



# JOURNAL OF SCIENTIFIC EXPLORATION

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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

I've been looking back on what's happened with the *JSE* since we parted ways with our former publisher, Allen Press, and switched to a more cost-effective and flexible online publishing system that allowed us to offer both print and electronic versions of the *Journal*. We were quite sure, when this happened, that the transition would reduce our production costs, and we figured that the savings could partially be passed along to readers by allowing us to increase the size of our journal issues—provided (of course) that we had enough acceptable submissions to sustain the increase.

I predicted at the time that we'd probably be able to add about 100 pages annually to the *JSE*. And I'm pleased to report that the prediction has so far panned out, by a comfortable margin. So I submit the following for your consideration.

Our last two volumes with Allen Press were Volumes 22 and 23. The former added up to 594 pages (a reasonable size for a journal), and the latter added up to 572. Now some previous volumes were larger. For example, Volume 13 (1999) was 724 pages; Volume 14 (2000) was 678; Volume 16 (2002) was 716; and Volume 18 (2004) was a whopping 812 pages. I don't pretend to know to what extent the US and global economic situations influenced our ability to produce somewhat larger journal volumes prior to Volume 22 (2008). Many things of relevance could have changed—and no doubt did change—between 2002 and 2008 (Volume 22). So I was particularly interested to see how the *JSE* grew beginning in 2010, when we produced Volume 24. It seemed to me that the *JSE*'s more immediate prior history might make for a more illuminating comparison, especially since both the US and global economy have been especially depressed and volatile since 2008. Moreover, I took over as Editor-in-Chief with Volume 23 in 2009, our last year with Allen Press, and I know first-hand how our publishing options were constrained that year by concerns over printing costs. Perhaps some of my predecessors as Editor-in-Chief would like to weigh in and shed further light on the matter. I'd welcome that.

Our first post–Allen Press volume, Volume 24, added up to 804 pages, and Volume 25 was 888. Our first three issues of the present year's volume already add up to 758 pages. That's almost 250 pages per issue for the current year, and an average of nearly 223 pages per issue since we left Allen Press. Granted, our recent issues have been bulked up somewhat by including a *JSE* index and announcements at the end. That probably accounts for, let's say, 15 pages per issue not devoted to scholarly material.

During the Allen Press era, announcements were always included in each issue, but an index was added only to the last issue of the year. So let's say that effective volume sizes for Volumes 24 and 25 should be reduced by 45 pages. By that count, Volume 24 was 759 pages and Volume 25 was 843 pages. And the first issues of our current Volume 26 should then be figured to be 713 pages. The average adjusted issue size from Volume 24 onward thus averages out to 210 pages. This is still a generous helping of material, by any reasonable standard, and it's a dramatic change from the average issue size of almost 146 pages for Volumes 22 and 23. And I believe it demonstrates how our new publishing arrangement has arrested what looked like a downturn in our ability to offer subscribers bang for their buck, and inaugurated a healthy trend of being able to provide both substantive and substantial issues. Our Managing Editor, Kathleen, and I figured initially that under the new regime we'd be able to increase issue size by about one article or 25 pages. Clearly, we've done better than that.

For those who are merely curious or who want to make a more detailed analysis, here's a list of the *JSE* volume sizes since the journal's inception.

Vol. 1:	196 pages (2 issues)
Vol. 2:	243 pages (2 issues)
Vol. 3:	219 pages (2 issues)
Vol. 4:	270 pages (2 issues)
Vol. 5:	270 pages (2 issues)
Vol. 6:	401 pages (4 issues a year from here on)
Vol. 7:	476 pages
Vol. 8:	588 pages
Vol. 9:	611 pages
Vol. 10:	626 pages
Vol. 11:	590 pages
Vol. 12:	654 pages
Vol. 13:	724 pages
Vol. 14:	678 pages
Vol. 15:	586 pages
Vol. 16:	716 pages
Vol. 17:	796 pages
Vol. 18:	739 pages
Vol. 19:	671 pages
Vol. 20:	675 pages
Vol. 21:	812 pages
Vol. 22:	594 pages
Vol. 23:	519 pages
Vol. 24:	744 pages
Vol. 25:	828 pages
Vol. 26:	758 pages (1st three issues)

Of course (as I noted earlier), our ability to sustain our current publishing bonanza depends on factors over which we have little control. Obviously, we need an ample supply of worthy submissions, and we also continue to rely on the energy and good will of those who spare their time to write book reviews for our sizeable and wide-ranging book review section. I doubt seriously that our book reviewers are motivated solely by the inducement of receiving a free review copy; it still takes time and energy to write a helpful and thoughtful review. The SSE is very fortunate to have such a conscientious and dedicated pool of reviewers, not to mention our remarkable Book Reviews Editor David Moncrief.

One problem with receiving many good submissions is that this often results in longer delays than we'd like getting an accepted paper out of the publishing queue and into print. It's great that we can now offer larger issues than before, but I don't want to increase issue size indefinitely. Submissions to the *JSE* come in waves, and I'm enough of a worrier to want to ensure, when we decide the table of contents for the next issue, that we always have sufficient material—or nearly enough—to fill the issue after that. Perhaps I worry too much about this. But our recent increases in journal size have undoubtedly changed subscribers' expectations, and I prefer to play it safe.

But *JSE* authors needn't despair over possible delays in seeing their work made public. A recent decision by the SSE Executive Council addresses this concern. The Council resolved to publish accepted research articles *online* as soon as possible after their acceptance—within a month. Now although that will suffice to get the research out into the world, it won't be possible to cite the papers fully until later. The actual page numbers of the articles can't be settled until the next table of contents is settled and my editorial gets written, and those matters will inevitably be later than the acceptance of the paper. So although we should be able to post a preliminary version of the formatted articles online, they will have to be cited still as “forthcoming” or “published ahead of print issue.”

...

One final matter. I'm pleased to welcome a new addition, James Spottiswoode, to my team of Associate Editors. I imagine James is already well-known to many long-time readers of this *Journal*. He's used his background in mathematics, and considerable knowledge of the physical sciences, to invent or develop numerous ingenious applications, and he also has an impressive history of innovative parapsychological research. His many areas of expertise will be put to very good use as Associate Editor.

**STEPHEN E. BRAUDE**

## RESEARCH ARTICLE

### Earthquake Triggering: Verification of Insights Obtained by Intuitive Consensus

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**Abstract**—Up until 1980 seismology was focused entirely upon data collection, the long-term study of tectonic processes, and limited surface-level measurements. Formal research on earthquakes was almost at a standstill despite the urgent need to discover reliable and measurable precursors in support of a system for short-term prediction. In the period 1975–1978 the author chose to interview eight intuitive experts who had proven their abilities in domains other than seismology. He asked them identical questions about the physical process involved in earthquake-triggering and associated precursors, and then compiled their consistent responses into a consensus. The accounts agreed well with one another and offered a number of insightful and possibly new directions for seismological research. Re-examination of these intuitive findings thirty years later, in the light of the many subsequent discoveries reported in mainstream geophysics journals, revealed that the expert intuitives had provided novel, significant, and strikingly correct information on earthquake-triggering and related precursors. This exemplary result suggests that skillfully applied intuitive inquiry could play a significant role in future seismological and geophysical studies, as well as in scientific research generally.

**Keywords:** earthquakes—intuition—intuitive consensus—expert intuitives—earthquake triggering—seismology—geophysics—prediction—verification—nuclear activity—precursors—tectonic plates—atmospheric electricity—electromagnetism—faults—earthquake lights—solar wind—thermal anomalies—solar activity—magnetosphere—ground gases—earth tides—planets—atmosphere—weather changes—animal behavior—human-caused earthquakes

#### Intuition—Another Way of Knowing

The discovery portion of science typically includes insight into the nature or concept of a problem being investigated, followed by methodical, rational exploration, formulation of hypotheses, and then verification of the hypotheses. Anyone mentioning science almost always refers only to the latter steps of the process: validating the hypothetical information according to consensus-based, rationally derived contemporary methodology. Alluding to the first step—the more intuitive part—in straight science means touching on a “taboo,” yet the history of science indicates that many major advances have been achieved from intuitive breakthroughs (Poincaré 1952, Koestler 1964, Harman & Rheingold 1984, Palmer 1998).



Many crucial ideas that led to the expansion of scientific knowledge relied upon intuitive insights. They arose out of sudden perceptions, subtle hunches, serendipitous associations, and even dreams, as the record of major discoveries convincingly reveals. These are mainly non-rational mental events, not explainable by contemporary models of the brain and mind and—except for transpersonal efforts—not even an acknowledged part of present-day psychology. They fall collectively into the category of intuition, or the direct reception of knowledge into the mind without the aid of reason, memory, or the senses (Vaughan 1979, Peirce 1997, Palmer 1998). Intuition has been observed to be a powerful source of new ideas, hypotheses, and understanding in many fields of knowledge as well as in various aspects of daily life. It exists in the human mind as an innate ability, and can be trained and developed into a refined skill through deliberate desire, intention, and effort, as demonstrated by many “expert intuitives” (Kautz 2005, Klimo 1987, Radin 1997, Shealy 2010, Schwartz & de Mattei 1990). These individuals have been able to access many kinds of new knowledge, including even highly specialized information not already known by anyone.

Since intuition is rarely mentioned in connection with the scientific discovery process, it is important to elaborate how it has been experienced historically and deliberately applied to generate new, accurate, useful information.

### ***The Nature of Intuition***

Intuition is popularly (and ambiguously) regarded as a flash of insight, a gut feeling, a “psychic hit,” and even an unconscious reasoning process. A much older tradition bespeaks of it as an innate human capacity (Kautz 2005, Palmer 1998). This kind of “direct knowing” was inherent in Greek philosophy (*nous*), Gnosticism, Eastern religions, and other early cultures as both a root belief and a common practice. It persisted over most of the world during the centuries to follow, up until the scientific revolution in the 17<sup>th</sup> century in the Western world. It then took second place to the empirical, sensually based, materialistic, and rational methodology of science, which became the favored means for gaining new knowledge about the natural world. Modern science has now become the accepted arbiter of validity for new knowledge from any source.

Intuition is not a favored topic for study within science, which regards it as too subjective for rational consideration and therefore allied with the superstitions of past generations. Today it is barely mentioned in psychology and psychiatry textbooks and has never been the subject of systematic study. This exclusion is historically understandable, and partially valid, because the metaphysical assumptions on which modern science is based insist on objectivity, measurability, repeatability, and certain presumptions about

causality. These assumptions are not fully satisfied by “phenomena” such as intuition (Barrow 1988, Harman & Clark 1994, Popper 1959, Sperry 1987). Thus, all that science can do with intuition is to verify empirically whether an alleged intuitive insight is or is not valid, according to its own accepted scientific criteria, and whether the insight might be explained through current physical understanding. Until the latter half of the 20<sup>th</sup> century, science was reluctant to do even this much.

### ***The Evidence for Intuition***

Several decades of careful parapsychological research have now verified firmly that (1) intuition actually exists as a mental capacity (Palmer 1998, Radin 1997, Vaughan 1979, Targ & Puthoff 1974) and (2) it contradicts one or more of the underlying assumptions of current physical science (just listed): objectivity, measurability, repeatability, and certain presumptions about causality. Deeper scientific exploration into the nature of intuition is difficult, therefore, and is not fully possible in view of these assumptions and other derived limitations of modern science’s means and models of investigation. A few recent studies seek to explain intuition within the latest models of human consciousness, as exemplified by several recent multidisciplinary international conferences on the subject.<sup>1</sup> Similar efforts seek a place for intuition within the various “theories of everything” that have emerged out of the paradoxes of quantum physics; for example, Bohm’s Implicate Order (Bohm 1980), Laszlo’s A-Field (Laszlo 2003), Pribram’s holographic model (Pribram 1987), and Hawking’s and Mlodinow’s M-theory (Hawking & Mlodinow 2010). These theories derive largely from the observation that both intuition and modern physics require the transcendence of ordinary conceptions of time and space, and the fluid, unilateral flow of events and information. While such speculative attempts are inspiring and suggestive of metaphors, none has yet found proof or won broad acceptance, even apart from their putative intuitive association. A satisfying scientific explanation of intuition is still lacking.

The classic Western philosophers—Descartes, Locke, Kant, and others—had their own notions of intuition, although most combined it with perception and intellect (Kenny 1997, Tarnas 1991). Freud had no use for intuition, but his follower Carl Gustav Jung considered it to be one of his four fundamental “psychological types,” along with sensing, thinking, and feeling (Jung 1971); the Myers–Briggs personality indicator utilizes these types (Myers et al. 1998). Philosopher Henri Bergson saw intuition as the essential ingredient of metaphysics and an evolved form of instinct that reveals the essence of things, apart from the symbols adopted for them (Bergson 2002). Michael Polanyi’s tacit knowing referred to unaware, contextual personal knowledge that a

person carries hidden in his mind (Polanyi 1966). Eminent neurobiologist Roger Sperry (1987) acknowledged intuition fully, and courageously assigned it to the right brain. In general, these philosophers and scientists sought to clarify the innate, direct-knowing quality of mind, apart from ordinary perception, intellect, senses, and brain. All were led to essentially the same definition of intuition as given above, though still not in scientific or familiar terms. The “direct knowing” capacity of intuition has always been an integral part of Eastern philosophy, which regards it as a valid and significant means for gaining deep knowledge, thus an alternative to classical science (e.g., Aurobindo 1993). Today limited systematic explorations of intuition are taking place within the humanistic and transpersonal subfields of psychology (Palmer 1998, Vaughan 1979, Walsh & Vaughan 1993).

### ***Intuition at Work***

The importance of intuition is most apparent today through the role it continues to play in creativity, the arts, humanities, and human interactions generally. Many psychotherapists and physicians are well aware of the important place of intuition in their practice. The most firmly established attributes of the human intuitive faculty are provided by the carefully conducted scientific experiments in parapsychology over the last century, as mentioned above. This work has shown conclusively that various kinds of specific information not accessible by ordinary means, not predictable in any real sense, and in some cases not known by any living human being can be accessed through intuition’s direct-knowing process (Mishlove 1975, Radin 1997, Targ & Puthoff 1977). Moreover, the individuals who have manifested this capacity—called here expert intuitives—are not obviously exceptional in any other way. Basic intuitive capacity appears to be natural, not supernatural, and virtually universal. In order to function, it need only to be enabled, like learning to walk and talk.

A ten-year research study at the author’s Center for Applied Intuition in the 1980s again showed that intuition as defined above is a genuine mental faculty. This work relied upon the services of several expert intuitives, and was applied practically in a dozen knowledge-dependent fields: recovery of ancient history and language, geophysics, nutritional science, archaeology, nuclear technology, medical problems, personal counseling, business consulting, and others (Kautz 2005, Grof & Kautz 2010). Intuition showed itself to be not only a significant facet of the human mind but also a practical tool for human endeavors that depend for their success on new information and knowledge—most especially in science.

There is no shortage today of expert intuitives. Most of them prefer to remain inconspicuous, however, and can be difficult to locate. Then

they must be carefully tested for expertness before being relied upon. The personal option remains open: Anyone may choose to develop his own intuitive capacities rather than relying upon experts.

While the existence issue for intuition has been settled, there remain many questions on the conditions under which accurate intuitive perception may take place deliberately and under control. For example: What are the limits on the types and depth of information that may be obtained intuitively? What factors govern its accuracy and clarity? How does intuition relate to familiar mental activities such as imagination, memory, dreams, learning, and cognitive function? Where does the new information come from? And what are the psychological/neurological mechanisms behind the intuitive process? While answers to most of these questions are not presently available, the same type of questions may be asked regarding other human capabilities such as reasoning, language, and creativity. We humans have learned to utilize these capacities effectively even though we cannot fully explain the physiological and brain processes involved and all their limitations. Similarly, as we wait for an acceptable explanation of intuition, we are free to develop and use it.

### ***Seismology: An Active but Slow-Moving Science***

Seismology, the subfield of geophysics concerned with earthquakes, began with the development of the seismometer. This simple device enabled the detection, recording, and eventual analysis of the heavy vibrations that propagate outward through the earth from the hypocenters and connected faults of earthquakes.<sup>2</sup> A global network of thousands of seismometers gradually evolved and generated sufficient data to permit detailed global maps of both the locations of earthquakes and the propagation of the seismic waves through the entire globe, which provided indirect information about the composition of the earth's interior. The first early earthquake theories soon evolved.

It became known by 1970 that major earthquakes are produced mainly at the boundaries of the dozen or so rigid tectonic plates that the thin crust of the earth comprises (Lay & Wallace 1995, Gubbins 1990, Lee, Jennings, Kisslinger, & Kanamori 2004). They float on the more plastic mantle of rock underneath, move slowly and unevenly from 1 to 6 centimeters per year, presumably in response to dynamic, circulatory convective mass movement within the earth. The quakes occur erratically along the boundaries when new plates are created where magma wells up from the mantle and solidifies, where existing plate edges are absorbed back into the mantle (subduction), or with internal cracks, or faults, where plates grind against one other and occasionally slip. Whenever the accumulated stress in these rock interfaces is suddenly released, a chain of ruptures takes place along the fault, producing an earthquake. This sudden release of energy, sometimes very great, generates

body waves, which propagate outward, shaking the ground over a wide area.

The motivation for seismological studies arises mainly out of the societal need to reduce the great cost in human life and property resulting from medium-to-large quakes. This need translates into two applications, namely earthquake engineering, the technology for designing earthquake-resistant structures (quite successful), and earthquake prediction or forecasting, the capacity to anticipate the catastrophic ruptures far enough in advance and with sufficient accuracy to allow a constructive and protective human response (not so successful). Earthquake-prediction research involves experimental and theoretical studies of both long-range causal mechanisms and the short-range earthquake-triggering process. The latter includes the search for specific precursory phenomena that might be continuously monitored through observation or instrumentation (Summary of Technical Reports 1980, Earthquake Prediction 1996, Geller 1997, Vogel 1979, Andriese 1980, Simpson & Richards 1981, Sykes, Shaw, & Schultz 1999, Kanamori 2003, Hough 2009).

It is not possible to make direct measurements of the buildup of stress along faults, which are relatively deep. There is therefore no way to know exactly where and when any particular point of high stress will rupture, and how much energy will be released when it does. These quantities can be estimated very roughly by analyzing the patterns of earlier quakes in one broad area, by measuring the strain along accessible faults, and by measuring variations in the propagation velocity of waves through deeper stressed areas. The results permit some long-range prediction of the time, location, and magnitude of future earthquakes. They show where building construction should be improved, but are much too crude and unreliable to allow short-range prediction. At the other extreme, short-term prediction (up to about a minute) is possible by transmitting the first indication of a large shock to communities within the radius of possible damage. High-speed trains may be slowed down,<sup>3</sup> nuclear reactors shut down, utilities turned off, and individuals in hazardous positions alerted. Similar warning systems are now in place around oceans to take advantage of the delay, up to several hours, before a tsunami wave arrives after an undersea earthquake.

While the local physical mechanism that triggers the release of stress is not understood, it is only reasonable to suspect from physical science that one or more precursors<sup>4</sup> ought to exist and be measurable in earthquake-prone areas, in order to enable short-term prediction—from a day to a few weeks, say—thereby allowing effective hazard-reduction measures to be taken (Rikitake 1975, Cicerone, Ebel, & Britton 2009). Despite various theoretical possibilities, laboratory experiments, and field studies, no reliable and broadly useful precursors have been identified and applied to date (2011) for prediction purposes.

**The Earthquake-Prediction Game**

The inability of seismologists to provide useful short-term earthquake prediction opens up the field to anyone who thinks he might do better. Since scientific knowledge is not necessarily required for discovery—only for validation of a claim or hypothesis—a useful earthquake precursor could be discovered accidentally by an amateur (even a psychic!) without a background in seismology. Every damaging shock brings into the media a flurry of announcements from persons who want to play the earthquake-prediction game. The public fascination with prediction of all sorts feeds this movement. Amateurs typically claim to have discovered a solution to the earthquake prediction problem in the patterns of eclipses, the number of notices for lost cats, occurrences of geomagnetic storms, unusual cloud formations, and the like.

Such claims never work out, of course, and even if a prediction is accurate it is worthless by itself. To be useful for practical public prediction, it must first be demonstrated to be consistently valid for a variety of types of earthquakes (shallow and deep, different fault types, a range of magnitudes, under land and water, etc.); it must lead to predictions reasonably precise in location and time; it must be readily observable or measurable with available instruments; and most important, it must be verified by scientists and supported by public officials who will stand behind it. Such validation requires a coordinated effort by specialists, can take years or decades to develop and verify, and is very expensive—well beyond the bright idea of a naïve amateur. Publicly useful short-term predictions must carry sufficient certainty to authorize the inconvenience and cost of major evacuation, shut-down of business districts, putting hospitals on alert, stopping trains, closing off large bridges, draining dams, and other hazard-reduction precautions. Moreover, they must initiate these various actions without inducing irrational public behavior (e.g., fear, disorientation, panic).

Most public predictions by amateurs are announced only after the shock, or are so loosely stated as to be unverifiable, or are incorrect even when timely and precise. Followup is rare: Failures don't make interesting news. We can only wonder why these persons are so zealous in announcing their shaky predictions to the media in the first place. The enthusiastic amateur who speaks out about his grand new solution to earthquake prediction is not only misleading his audience but is behaving irresponsibly. After all, it is very easy to make a prediction—anyone can do it. It is not so easy to predict correctly, significantly, and convincingly. (The very term *prediction* is ambiguous on this score.)

Predictions offered by seismologists are more credible but are infrequent, conservative, qualified, and apply only to small earthquakes and long-term possibilities. They are derived from extensive data, collected and analyzed for particular areas that have been studied for a long time. Stated as probabilities rather than certainties, they are not useful as warnings for immediate action. Because seismologists KNOW that they cannot predict earthquakes on the basis of their established seismological methods.

China, the scene of many tragic and damaging earthquakes, has a wide monitoring system in place and a few impressive predictions to its credit. One in 1975 resulted in the evacuation of Haicheng, a town of a million people, two days before a large shock destroyed the town (K. Wang, Chen, Sun, & A. Wang 2006). Only two thousand lives were lost. Still, the Chinese have had many more failures, both predictions without quakes and quakes without predictions. The search for a basis of practical prediction continues.



### ***The State of the Art in 1980***

Up until 1980 theoretical work on earthquake triggering was focused entirely on the behavior of stressed rock under the high temperatures and pressures believed to be prevalent at the depth of typical hypocenters, including especially the role of water in this unfamiliar environment. Findings were derived from wave-propagation records of seismometer data, extrapolation from laboratory and surface-level measurements on stressed rock, and a few relatively shallow bore-hole experiments. Several ground-level precursors that might be useful for prediction purposes were identified in particular areas: tilt, slip, elevation, and subsidence of the ground; micro-earthquake swarms; patterns of foreshocks; changes in well-water levels; seismic “gaps” without recent activity; and the release of radon and other ground gases. Other candidate precursors were reported for particular quakes, but their significance remained conjectural. They were not being tested or examined broadly: unusual animal behavior, the consequences of filling of dams, chemical changes in ground water, earth–tidal maxima, cloud formations, telluric electric currents, ground resistivity changes, air-pressure variations, and glows in the lower atmosphere.

By 1980 there was no precedent in seismology to suspect that above-ground factors could be causative or even indicative of earthquake triggering and changes in preliminary precursors (Vogel 1979, Summary of Technical Reports 1980, Adamo & Enns 1980, Andriese 1980, Simpson & Richards 1981, Rikitake 1981). Even later no specific hypotheses of above-ground effects or indicators were being investigated, and no scientific effort was directed into such possibilities (Earthquake Prediction 1996). Data that might have been relevant were poor and unconvincing. For example, abnormal animal behavior (see EQA section below, ***EQA—Abnormal Animal Behavior***) and atmospheric glows (EQE and EQL) had been anecdotally reported near earthquakes for decades, and could have been taken as a clue to novel atmospheric precursors, but they were generally assumed to be superstitions, insignificant, and not related to earthquakes.

On the political front, the U.S. national budget for earthquake research remained at a low level during the 1960s and 1970s (and is still low), despite the occurrence of large, damaging, and costly earthquakes in the U.S., and abroad. Not until 1977 was the National Earthquake Hazards Reduction Program established in the United States. Similar programs began in Japan (1964), China (1956), and the Soviet Union (partially in the 1960s, mainly in 2004). While foreign observations and research increased during the 1970s, the reports were not taken very seriously in the U.S. (only much later). Seismological research seemed to be moving as slowly as the tectonic plates themselves.

### **Purpose and Approach**

In order to spur progress on earthquake research, this author chose in 1975 to apply a method of multi-intuitive inquiry called intuitive consensus to generate new information on the triggering process and associated precursory phenomena. Similar intuitive efforts in other fields had achieved some impressive successes (Kautz 2005).

The inquiries were carried out in 1975–1978 with eight members of a team of “expert intuitives” at the Center for Applied Intuition, a San Francisco organization that was at the time investigating intuition and its applications.

The broad purpose of this early study was to demonstrate by example the validity and usefulness of properly conducted intuitive inquiry as a means for generating new knowledge, outside of the scientific approach. Specifically, it sought to identify new aspects of the earthquake-triggering process that would merit future research, and to provide relevant technical details in support of this research. It was believed that the intuitive findings, called here insights, if sufficiently credible as ideas and hypotheses, could open the door to their verification by accepted scientific methods of experimentation, validation, and proof, and eventually find subsequent application in research studies on earthquake triggering or even an actual prediction system.

This effort did not attempt to provide a full explanation of earthquake triggering, to predict particular earthquakes, or to predict what kind of earthquake research efforts would actually take place in the future, but only to generate findings that could be verified if future research were actually carried out. Nor did this study attempt to prove the existence of intuition, which has already been adequately carried out, or to show that intuitive information is always accurate and factual, which is neither true nor possible.

### **Method of Inquiry**

The original study comprised (a) definition of the main topic and sub-topics chosen for inquiry; (b) formulation of questions, based upon our understanding of the earthquake-triggering process available at the time (1975); (c) selection of expert intuitives; (d) execution of the inquiry sessions with them, including recording and transcription; (e) identification of agreeing responses (consensus); and (f) comparison of the consensus with already existing knowledge, leading to brief reports and eventually to this report.

Prior experience with intuitive inquiries had shown that the formulation



of the questions to be posed to the intuitives is critical to success. Namely, they should be specific, focused, clear, well-motivated (that is, not arising from curiosity alone), and without expectations, biases, or implicit assumptions. These guidelines were followed closely, conditioned only by the fact that the geophysical understanding of earthquake triggering at the time was very incomplete, and sometimes incorrect (as revealed later). In the absence of an overall physical model of the triggering process, and without knowing even the particular physical quantities (precursors) indicative of forthcoming quakes, the questions were necessarily more broad than deep and more exploratory than strictly focused. In retrospect, some seem today to be rather naïve. (It might be looked at as similar to asking for the cause of cancer, which can be recognized today as a meaningless question.)

Of the eight “intuitive experts” selected for participation, five were interviewed initially (1975) and three more two years later. None had prior formal training or experience with seismology or geophysics, or more than a typical public exposure to the subject. All were qualified because (a) they were experienced in intuitive work, having demonstrated their skills in prior inquiries on other topics, ranging from personal to historical to highly technical, and (b) these prior tasks revealed their individual answers to be responsive and self-consistent whenever the questions posed to them were clear and founded upon well-established knowledge. Their responses were also found to be accurate whenever independent validation was possible and was actually carried out.

The inquiry sessions with the intuitives were conducted independently with the same set of questions, a procedure that made it possible to compile and compare their responses. Previous investigations (Kautz 2005) had demonstrated that such consensual findings, created from a substantial majority of agreeing responses, reduces the probability of incorrect answers. Responses that were not in good agreement were excluded from the reported consensus, except as specifically noted here, but were retained for their suggestive value for any future inquiries.<sup>5</sup>

All interviews were conducted by the author. Special care was exercised to avoid unnecessary explanation to the intuitives beyond that needed to make the questions clear, and to prevent accidental leakage of the interviewer’s personal beliefs and expectations derived from his previous work in seismology.

### **Verification**

Insights generated through intuitive inquiry are initially unproven and must be regarded as hypothetical, just like new information from any source. Within our present societal paradigm, they must be validated by independent

means, usually scientific, before they can be regarded as substantiated and factual. To this end, the second phase of this study (2011) consisted of two steps, applied to the insights obtained in 1975–1977.

First, in order to validate that the insights had a genuinely intuitive source, it had to be shown that they were novel, meaning that they were not already known to the intuitives at the time of the inquiry. To establish prior ignorance of information is normally very difficult, but in a well-documented field such as seismology the flow of published technical information provides a timely, reliable, and thorough measure of the state of expanding knowledge over time, with only a year or two delay. By comparing the new information against the scientific record before 1980, say, we can be certain that the intuitives had no separate access to it through scientific or media channels.

Second, in order to show validity, the intuitive insights must be shown to be accurate in content, by comparing them with findings reported in scientific articles and books published after the inquiry. They should also not follow obviously and logically from what was known earlier. This published evidence was taken from scientific journals, and occasionally from less authoritative but substantially valid sources—for instance, when the latter contained acceptable observational or experimental data even though their interpretations or explanations were questionable, or when an explanation was credible though the data themselves were doubtful.

The degree of consensus among intuitives was high. Two early, inconspicuous publications (Kautz 1982, 2005) reported these early insights and compared them favorably with a few discoveries in geophysical research. It is quite certain now that these early publications did not stimulate ongoing seismological research, as originally hoped, even though (in retrospect) they could have done so if the seismological community had been open to such new ideas.

This present report arose out of thorough verification and substantiation, based upon published geophysical research between 1980 and 2011. The accuracy assessment was carried out in observable or potentially measurable terms, namely through the candidate precursors described by the intuitives. These are discussed separately in the following sections, abbreviated here for convenient cross-referral:

EQE—Atmospheric electric fields

EQM—Electromagnetic signals in the earth and lower atmosphere

EQI—Upper atmospheric and ionospheric field variations

EQTh—Thermal anomalies on the earth's surface

EQL—Earthquake lights, atmospheric luminescence

EQS—Solar activity, solar wind, geomagnetic field changes  
EQP—Planetary effects  
EQT—Earth tidal effects (solid and ocean, moon and sun)  
EQG—Ground gas emission  
EQF—Centrifugal forces inside the earth  
EQW—Weather changes  
EQA—Abnormal animal behavior  
EQN—Nuclear and other radiation  
EQH—Human activities

These fourteen candidate precursors are not of one type but varied. They seemed originally to be fairly distinct but turned out instead to be highly interdependent. It is still not fully known which are genuine rather than merely suggested; which are giving rise to which others; which are valid only in combination with others; and which are only indicative rather than causative of triggering. Some seem to be active only for particular kinds of earthquakes—locations, types, depths, magnitudes—or in the presence of new factors not yet discovered. Several cited by the intuitives were completely new, while others had already been explored in seismology but were not mentioned by the intuitives (and are not discussed further here). Finally, one precursor was found to require very costly instrumentation; it may contribute later to an understanding of triggering though it is not practically measurable for prediction purposes.

Earthquake research, including triggering and prediction, has for many decades been carried out in the US almost entirely by the U.S. Geological Survey (USGS), with more recent participation by the National Aeronautics and Space Administration (NASA), and supplemented by policy and overseeing bodies: the National Science Foundation (NSF), the National Research Council (NRC), and professional organizations such as the American Geophysical Union (AGU).<sup>6</sup>

### **General Description of Intuitive Findings**

Overall, it is impressive that several of the intuitives' insights, which at the time of the inquiry were unknown, unexpected, improbable, and contrary to existing theories, have since been verified by mainstream geophysical research. A number stated ambiguously were not fully assessable, but none of the consensual results have since been proven to be downright wrong.

We review first the broad intuitive findings that emerged from the inquiries. The full record of the consensus would occupy a book and would be very repetitive. A small number of typical and more eloquent excerpts are included in this article, to illustrate the flavor of the intuitives' typical

responses to the questions asked of them. They reflect only weakly the full breadth of content.

The intuitives explained that there is no single cause of earthquake triggering; rather, multiple, interdependent factors come together to create the trigger:

There is no one particular final [triggering] force except that which would be considered as the content [combination] of electromagnetic and vibrational energies upon the molecular level within the earth crust itself. . . . It is the combined forces of gaseous pressure, electromagnetic activity—which must be elaborated upon<sup>7</sup>—the pressures of centrifugal forces combined with the normal expansionary pressures of any heated matter. When all of these reach a critical point the final triggering of the quake [takes place through] the electromagnetic charge that has been built up. [KR]<sup>8</sup>

The trigger is not one thing but a combination—one factor in one case, another factor in another. Mainly, though, external energy is coming in from two sources, one external to the earth and the other inside of the earth, and there is an interaction between these two. [BR]

[What kind of forces are acting upon the rock at the point of fracture or sliding?] These are static pressures that come about from electrostatic, electromagnetic, and nuclear pressures. The electrostatic and electromagnetic forces act not directly on the epicenter [hypocenter?] but in the surrounding area, while the nuclear force acts directly on the epicenter [hypocenter]. [AA]

There is a combination of the forces within the earth itself . . . together with the electrical forces from the outside which are coming together and joining, even as you would see a discharge of energy between clouds. Here you have solid clouds. There will be an eruption when these two meet, an explosion. That is the mechanism of it, for the earth is charged and so are these forces. They are charges, particles of charged energy. [AA]

There are two forces which would account for triggering the final activities of quakes. In the centermost [part] of tectonic plates the centrifugal force of the planet in its axis of rotation combines with the thermal expansionary forces of the molten masses beneath the surface to [cause] movement along the inner structures, toward the outer surfaces of the tectonic plates, wherein there is the gaining of the highly charged magnetic and electromagnetic forces and the accumulation of both gaseous and aqueous forces. [KR]

Most surprising in these responses were the variety, types, and locales of phenomena that were said to be involved in triggering but which had never been considered or even suspected in seismological research. This prior research had focused entirely upon mechanical processes and

measurements in the crust of the solid earth and below it. It did not extend to physical energies above the earth's surface and certainly not to the farther reaches of the atmosphere and outer space. The intuitive version of the triggering process sounded as if it is as much electromagnetic and plasmic as it is mechanical and thermal. The most significant precursors, it said, were more likely to be found in the atmosphere, ionosphere, and space than in the ground.

We consider now in detail the intuitive responses, first those clearly verified by subsequent geophysical research (the next section, **Precursors Well Verified by Intuitive Insights**), then those only partially verified and therefore leading candidates for future research and verification (the following section, **Precursors Partially Verified by Intuitive Insights**), and finally those that are too vague to be verifiable (the last section **Candidate Precursors Not Verified**). They are classified according to precursor.

### **Precursors Well Verified by Intuitive Insights**

#### ***EQE—Atmospheric Electricity***

Prior to 1980 the static atmospheric electric field (about 100 V/m at the surface) and its daily variations were recognized but only partially understood (Bibliography: International Center for Earth Tides no date, Chalmers 1967, Vonnegut 1973, Anderson & Freier 1969). Sporadic luminescences such as ball lightning, "earthquake lights," and St. Elmo's fire (see subsection on EQL below) were presumably of electrical origin, but the high voltages necessary for ionization of the air had not yet been well explored and explained. The role of electricity in weather phenomena such as tornadoes, storms, clouds, and lightning was recognized, though not well understood, and except for lightning there was no recognition of strong electromagnetic activity in the atmosphere (see EQW below). While lightning was properly seen as a very powerful electric discharge between clouds and earth and among clouds, the physical mechanisms behind its occurrence were regarded as complex and mysterious (as are some aspects even today) (Orville 2009). Farther out in near-earth space, the ionosphere, and the earth's magnetosphere, while obviously electrical in nature, were seen as too far out to be related to lower atmospheric processes and certainly not to earthquakes.

In this historical context, the intuitives' statements about significant above-ground electrical activity before earthquakes were surprising, and at first not at all credible:

Researchers have come to understand that the triggering of electromagnetic forces that account for atmospheric conditions of storms, even the triggering of lightning, are grounding principles involving the electromagnetic fields of the earth, and not simply the aqueous and gaseous dynamics of the atmosphere itself. [KR]

High degrees of positive ions in the atmosphere . . . a release of negative ions close to the ground. . . . When all of these reach a critical point, the final triggering of the quake [occurs from] the electromagnetic charge that has been built up. [KR]

There is a general atmospheric disturbance involving highly charged air. [AAA]

There is a combination of the forces within the earth itself . . . together with the electrical forces from the outside which are coming together and joining, even as you would see a discharge of energy between clouds. Here you have solid clouds. There will be an eruption when these two meet, an explosion. That is the mechanism of it, for the earth is charged and so are these forces. They are charges, particles of charged energy. [AA]

After 1980 rapid advances in atmospheric physics led to an improved understanding of atmospheric electricity and lightning generally, and various data began to indicate their connection with earthquakes in particular. After a brief and early speculation (Pierce 1976), it gradually became known that electric charge was accumulating in the lower atmosphere and modifying the normal electric field prior to many earthquakes. This charge was found to arise from the emission of Rn out of the surface of the ground (already recognized—see EQG below), which was ionizing the air, and from compressed or impacted rock which generates positive hole charge carriers that ionize air molecules in the lower atmosphere. Both pre-earthquake charge production mechanisms have now been verified from experiments in the laboratory, and observations of electric field increases before earthquakes have been verified in several sites around the world (e.g., Jianguo 1989, Ifantis, Tselentis, Varotsos, & Thanassoulas 1993, Varotsos, Sarlis, Lazaridou, & Kaporis 1998,<sup>9</sup> Varotsos, Hadjicontis, & Nowick 2001, Freund 2002, Takeuchi, Lau, & Freund 2005, Freund, Takeuchi, Lau, et al. 2007, Freund, Kulahci, Cyr, Ling, & Winnick 2009, Pulinets 2009).

Other factors as well are apparently at work. Close-to-the-ground air ionization is probably responsible for the luminous phenomena sometimes observed (EQL) (Atmospheric Electricity 2011, Orville 2009). Aerosols from the ground tend to amplify the ground-level field strength (e.g., Tributsch 1978, Pulinets, Alekseev, Legen'ka, & Khagai 1997, Pulinets,

Boyarchuk, Hegai, Kim, & Lomonosov 2000, Sorokin, Yaschenko, Chmyrev, & Hayakawa 2006). Thunderstorms and other weather conditions play a part by moving charged water particles upward where conductance and temperature are higher, so that very high potentials (up to a gigavolt for a lightning strike) can build up.

It is still not known which of these production processes that increase the atmospheric electric field intensity is primary, and (at the moment) whether the charge accumulation is an integral part of the triggering process or is only ancillary to it. Still, there is now no doubt that electrical field increases in at least the lower atmosphere accompany many earthquakes and constitute a valid earthquake precursor.

This evidence validates the intuitive information on pre-earthquake electric fields. It also provides a mechanism that is consistent with the other atmospheric electrical phenomena mentioned above and discussed below—"earthquake lights" and St. Elmo's fire (EQL) and various weather effects (EQW)—and may explain them.

### ***EQM—Electromagnetic Energies***

Before 1980 the notion that electromagnetic energies in the atmosphere are significant before earthquakes was even weaker than for electric fields, which at least had the clue of luminescences and lightning. While the electric field effect is essentially static and moves fairly slowly, electromagnetism occurs at various frequencies and propagates as waves at the speed of light. The intuitives contradicted this mis-assumption about electromagnetic energies in the atmosphere before earthquakes:

... the content [combination] of electromagnetic and vibrational energies upon the molecular level within the earth crust itself. ... It is the combined forces of gaseous pressure, electromagnetic activity ... movement ... toward the outer surfaces of the tectonic plates, wherein there is the gaining of the highly charged magnetic and electromagnetic forces ... directly related to the earth's electromagnetic energies ... also the electromagnetic charges building up beneath the earth's surface. [KR]

The earth energy is less than 50 kilohertz. ... Ninety to ninety-five percent is electromagnetic, and yet there are other variations which also play a part in this. [LH]

The main energy for earthquakes comes from the central core of the earth ... is very powerful and can take any form ... it has a very high vibration. ... The electrostatic and electromagnetic forces act not directly on the epicenter, but in the surrounding area. [AA]



After 1980 the recognition that electromagnetic fields in the atmosphere were associated with earthquakes came from three sources: (1) ground-based and satellite measurements of electromagnetic energies of various frequencies (e.g., Gokhberg, Morgounov, Yoshino, & Tomizawa 1982, Larkina, Nalivayko, Gershenzon, Gokhberg, Liperovskiy, & Shalimov 1983, Parrot, Lefeuvre, Corcuff, & Godefroy 1985, Parrot 1990a, 1990b); (2) an accidental discovery of strong ULF signals in the area of the M7.1 Loma Prieta (California) earthquake of October 17, 1989 (the region was being monitored for another purpose)<sup>10</sup> (Fraser-Smith, Bernardi, McGill, Ladd, Helliwell, & Villard 1990, Campbell 2009); and (3) the recognition that stressed rocks can generate not only electrostatic but also electromagnetic radiation—essentially wideband noise over the range 10 Hz to 10 MHz (e.g., Warwick, Stoker, & Meyer 1982, Nitsan 1977, Martelli & Cerroni 1985, Ogawa, Oike, & Miura 1985, Cress, Brady, & Rowell 1987, Pulinets, Alekseev, Legen'ka, & Khagai 1997).

The first of these discoveries led to more than one hundred research reports on electromagnetic anomalies before earthquakes. They described magnetic and electromagnetic field measurements (nominally ELF,<sup>11</sup> usually taken as 0 to 300 Hz, through ULF from 300 Hz to 3 kHz, and a few reports for VLF at 3 to 30 kHz) near earthquakes in several parts of the world; laboratory tests on electromagnetic radiation from compressed rock, as just noted; postulated theoretical mechanisms as to where these signals are coming from, how they are being produced, and under what external conditions; and the detailed analysis and interpretation of observed data to ascertain which frequencies, how long before the shock, signal duration, signal quality, types of earthquakes, etc. (e.g., Parrot 1990b, Yoshino 1991, Park, Johnston, Madden, Morgan, & Morrison 1993, Stolorz & Dean 1996, V. Singh, B. Singh, Kumar, & Hayakawa 2006, D. Siingh, R. P. Singh, Kamra, Gupta, R. Singh, Gopalkrishnan, & A. Singh 2005, Hayakawa, Hattori, & Ohta 2007, Chauhan, O. Singh, Kushwah, V. Singh, & B. Singh 2009).

These various findings were not fully consistent, perhaps not surprisingly given the apparent complexity of the process. While the signals observed were often very strong, they were sometimes present without earthquakes and missing even for large quakes. Nor is it certain even today (2011) whether they participate in the triggering or are only indicators. They did show that electromagnetic energy is coming from both the ground and the ionosphere (or magnetosphere), and the signals are strongest and clearest near the epicenters and close in time to the largest earthquakes. The effect appears to be affected by weather conditions (storms, dryness, winds,



etc.) and the depth of the fault, as already suspected for the electrostatic field alone. In short, the electromagnetic phenomenon emerges as atmospherically complex and subject to influential and interacting factors still not fully identified.

The second discovery inspired the creation of a private “QuakeFinder” network<sup>12</sup> of 60+ detectors of ULF signals and other candidate precursors, along with telemetering and analysis equipment (Bleier, Dunson, Maniscalco, Bryant, Bambery, & Freund 2009, The Quake-Finder Network). Some potentially useful data are emerging.

The third discovery lends support to a more refined and multifunctional hypothesis, still unproven, in which the electrostatic charge emission from the ground into the lower atmosphere contributes to the ground–ionosphere electric circuit across the large annular waveguide around the earth (e.g., Pulinets, Alekseev, Legen’ka, & Khagai 1997, Pulinets 2004, Kazimirovsky 2002, Freund, Lazarus, & Duma 2010, Sorokin, Ruzhin, Yaschenko, & Hayakawa 2011). This hypothesis is developed further in the next section.

While there is now no doubt that electromagnetic energies are playing an important role in earthquake triggering, it is not yet known for certain whether they constitute by themselves a distinct causative precursor (Parrot, Achache, Berthelier, Blanc, Deschamps, et al. 1993, Johnston 2002). The many observations and measurements verify the intuitives’ broad statements, though a full understanding of the association is far from complete. Further intuitive inquiry would surely help answer these remaining questions.

### ***EQI—Ionospheric Changes***

Even more surprising were the intuitives’ claims that the ionosphere, the multilayered curtain of charged particles (though electrons are dominant) located from 100 to 500 km above the earth’s surface, undergoes measurable changes just before earthquakes and above their locations. It was almost inconceivable in 1980 how such a connection could exist, but the intuitives’ descriptions were clear and fairly specific:

[Describe these ionospheric changes that you mentioned.] In this case, there will be changes both in the pressure and in the particles themselves, such as there will be gases being changed from one state to another, or there will be a changing in the gases or pressures themselves. But as a whole, the ionosphere acts as a storage device, as a condenser, for the earthquake before it strikes. Lowered, it begins to release the energy, and then it rises again . . . within a few miles of the epicenter. It ascends as a balloon that has lost its baggage. There are changes taking place over a long period of time, but when the energy is released the changes take place very rapidly. [AA]

[What parameters of the ionosphere are relevant here?] It has something to do with the lower layer. The height and density . . . vary, anyway. And it will be changes that you detect in this lower layer that will help to indicate it. [What kind of changes?] I see almost silver, like aluminum foil or that type of thing, as though the way it reflects seems to be the changing factor that indicates what the change is going to be. [LH]

[The critical] focal point can be detected by an ionospheric disturbance, which will vary in size like a hole and travel over the atmospheric sheath like the shadow of the moon. You can detect it coming, and if it falls on a vulnerable spot, the earthquake occurs. [MA]

[Does the ionospheric activity you mentioned affect the earth directly or only through the weather, when triggering an earthquake?] Only by the weather. It creates the atmospheric and electrical discharge that completes or causes other changes. . . . The heat and dryness of the earth's crust is . . . the final catalyst after the pressure buildup has reached a certain point . . . even though the particular time of the occurrence is not dry per se. . . . The dryness seems to cause an electrical spark. [LH]

Unusual ionospheric activity before earthquakes was first detected by radio-sounding stations just before the M6.3 Hawaii earthquake of April 26, 1973,<sup>13</sup> then by another before the M9.2 Alaska earthquake of March 27, 1964 (Davies & Baker 1965, Moore 1964, Leonard & Barnes 1965).<sup>14</sup> The spate of scientific satellites launched in the 1980s opened up a new era of space observation of the earth environment, including the ionosphere at various sites, latitudes, and times of day. Following the first report of earthquake-ionosphere coupling (Gokhberg, Pilipenko, & Pokhotelov 1983), several satellites now have the capacity to monitor ionospheric changes, and DEMETER, launched in 2004, was used exclusively for detecting pre-earthquake ionospheric variations (e.g., Lagoutte et al. 2006, Sarkar, Gwal, & Parrot 2007). In 2004 NASA inaugurated the Global Earth Satellite System (GESS), a “twenty-year program to enable earthquake prediction,” using ionosphere measurements (EQI) as well as temperature (EQTh) and geodetic measurements, the magnetosphere (EQS), and other related earth-monitoring tasks from space (Solid Earth 2003).

These measurements, supplemented by others from ground-based VLF/LF radio transmissions, ionosphere-sounding stations, and geostationary GPS satellites, have shown conclusively that the Total Electron Content (TEC) of the ionosphere and the height of its lower E layer are frequently disturbed a few days before major earthquakes, and not excessively so at other times (e.g., Liperovsky, Pokhotelov, Liperovskaya, Parrot, Meister, & Alimov 2000, Liperovsky, Meister, Liperovskaya, Vasil'eva, & Alimov

2005, Chuo, Liu, Pulinets, & Chen 2002, Harrison, Aplin, & Rycroft 2010, Hayakawa, Kasahara, Nakamura, Hobara, Rozhnoi, Solovieva, & Molchanov 2010, Ouzounov, Pulinets, Alexey Romanov, Alexander Romanov, Tsybulya, Davidenko, Kafatos, & Taylor 2012).

The mechanism of this phenomenon is just beginning to be understood. It begins with the accumulation of atmospheric electric charge near the ground (EQE). This charge cloud rises up to the lowest level of the ionosphere, where it is amplified by the favorable conditions there and induces changes in the ionosphere's composition, pressure (electron density), and height. When strong enough, it can create a gap or hole, thereby reducing radio reflectivity (Sorokin, Yaschenko, & Hayakawa 2006). By participating in the global electric current back to the ground, it reacts downward as a kind of quiet lightning, possibly affecting the earth itself and helping to trigger an earthquake. Recent articles (Pulinets 2004, Gokhberg, Morgounov, & Pokhotelov 1995, Sorokin, Yaschenko, Chmyrev, & Hayakawa 2006, Freund 2007, Pulinets 2009) provide additional details on this overall hypothesis and speculate further on how the process might be taking place.

Unfortunately, local ionospheric behavior appears presently to be complex and erratic even under undisturbed conditions. Careful analysis will be required to relate the specific pre-earthquake variations to the specific location, type, timing, and size of the earthquakes (Singh, R. P. Singh, Kamra, Gupta, R. Singh, Gopalkrishnan, & A. Singh 2005, Karatay, F. Arikan, & O. Arikan 2010, Astafyeva & Heki 2009, Liperovsky, Meister, Liperovskaya, Vasil'eva, & Alimov 2005); and to distinguish these meaningful signals from the ever-present noise and other effects, both known and unknown: daily (day and night) changes, solar-induced geomagnetic/magnetospheric storms (see EQS below), lightning, man-made radio transmissions, nuclear explosions, and disturbances from space shuttles and rocket launches (Hayakawa, Kasahara, Nakamura, Hobara, Rozhnoi, Solovieva, & Molchanov 2010). As of 2011, these intricate analyses are still in progress.

In any case the accumulated evidence for an ionospheric precursor is sound, and it provides positive hope for its eventual use as a short-term earthquake predictor if it can be sufficiently localized in time and to the epicentral area. The intuitive information on the ionospheric precursor is therefore verified. The "storage" phenomenon mentioned therein has not been specifically reported in connection with earthquakes, though it could reasonably be expected from present-day geophysical understanding of atmospheric and ionospheric dynamics. New research and further intuitive inquiries are called for.

## Precursors Partially Verified by Intuitive Insights

### ***EQTh—Thermal Effects***

Before 1980 it was well known that the earth generates heat from its interior, more or less uniformly over its surface, but a little warmer below the oceans and above plumes and volcanoes, and a little cooler above plate boundaries. It is surprising that possible variations in this normal heat flow were never seriously investigated as a precursor. The intuitives were not hesitant to remind us of it, with comments such as:

The centrifugal force of the earth's rotation causes movement of the magmas along the cooling crust, [producing] rarefied or heated forces from the expansion of these molten materials seeking release into the atmosphere. . . . [They] combine with the thermal expansionary forces of the molten masses beneath the surface to [cause] movement along the inner structures, toward the outer surfaces of the tectonic plates. [KR]

The heat and dryness of the earth's crust is . . . the final catalyst after the pressure build-up has reached a certain point. [LH]

Recent infrared measurements from satellites have revealed increases in surface temperature up to 4°C one to two weeks before several major earthquakes, and a return to normal a few days afterward (e.g., Gorny, Salman, Tronin, & Shilin 1988, Saraf & Choudhury, 2005, Saraf, Rawat, Choudhury, & Das 2009, Ouzounov, Bryant, Logan, Pulinets, & Taylor 2006). In the absence of plausible mechanisms, however, these data are “hotly” contested.

It is nowadays well established that fissures, faults, mid-ocean ridges, and volcanoes provide for the upward convection of hot magma and the seepage of heated ground water. Convective transfer of this heat to the earth's surface, whether near earthquakes or not, is doubtful, however, because of the absence of clear heat transfer mechanisms and the large thermal inertia of intervening rock. If the satellite data turn out to be valid, another means of heat production may need to be found to explain them.

Fifty to 90% of the heat emanating from the earth is known to arise from the natural decay of the radioactive elements  $^{235}\text{U}$ ,  $^{238}\text{U}$ ,  $^{232}\text{Th}$ , and  $^{40}\text{K}$ , which exist in the crust and upper mantle but not at the high temperatures and pressures that prevail at greater depths ( $^{40}\text{K}$  may lie somewhat deeper). The balance of radiated heat is left over from the early formation of the earth, from the gradual sinking of the heaviest matter toward the core, the convection of soft magma upward, and the flexing of the earth due to lunar and solar tidal forces. All of these processes generate heat from gravitic

compression or frictional movement (Tronin 2002, Guo 2008), but none is able to account for the relatively rapid temperature rise observed before earthquake shocks and the drop soon afterward (Earthquakes 2003).

Theories of an alternative source of heat are based upon laboratory tests of the compressed rock itself, which may produce heat directly (Saraf et al. 2009); theoretical claims that ionization in the atmosphere may generate heat directly (Pulinets 2004); the possibility that positive hole recombination from compressed rock generates heat (Saraf et al. 2009, Freund, Takeuchi, Lau, Al-Manaseer, Fu, Bryant, & Ouzounov 2007), or gas or electromagnetic emission affecting thermodynamic processes in the atmosphere (Krasikov 2001, Pulinets 2004). Which (if any) of these mechanisms of local heat production before earthquakes may be responsible is still unresolved.

There remains the task of determining if the heat anomaly is consistently present before earthquakes, identifying its specific source and local expression, and verifying the precursory timing.

The intuitives' recognition of the existence of a thermal precursor is partially verified. Because of the relative ease by which ground temperature may be measured from space, this precursor shows some promise as a contributor to future earthquake prediction.

### ***EQL—Earthquake Lights***

The intuitives cited near-earth atmospheric luminescence as a valid though inconsistent precursor. Since ancient times, such glows in the sky have been reported anecdotally near the locations and times of earthquakes, but scientifically acceptable data were lacking. (e.g., Terada 1931, Ulomov & Malashev 1971, Derr 1973, Hedervari 1981).

Post-1980 reports of such pre-earthquakes luminescences are more wide-spread and better documented (Corliss 2001, Derr 2005, Freund 2003, St. Laurent, Derr, & Freund 2006, Lockner, Johnston, & Byerlee 1983, Heraud & Lira 2011). They confirm that the phenomenon is a genuine precursor, but the data are still not consistent and reliable enough to indicate the size and type of earthquakes they accompany. Indeed, the “lights” occur without earthquakes, and large earthquakes occur without the lights.

The extensive pre-earthquake electrical activity in the atmosphere, already validated above (EQE), offers a ready explanation for such earthquake lights, which could have only an electrical or possibly an electrochemical origin. It appears unlikely that earth gases (EQG) are also participating in these luminous effects, though this possibility cannot be ruled out at present (King 1986).

Pre-earthquake atmospheric luminescence is therefore a valid precursor,

just as the intuitives indicate, but it is too irregular to be useful by itself for prediction purposes. Nor is it likely to be helpful for studying the triggering process.

### ***EQS—Solar Activity and the Geomagnetic Field***

Might earthquakes be induced by solar activity? The intuitives imply that solar effects on the earth's geomagnetic field are indeed a causal part of the earthquake-triggering process, but it is difficult to imagine any such mechanism from known physical theory to support a claim of causality. The overall phenomenon is irregular and involves other factors as well, so it can be only contributory:

Gravitational forces from both the planets and heavier activities [affect] the release of many radiations from the surface of the sun. All these activities indeed are integrated with the phenomena. [KR]

Leaving aside a planetary influence for the moment (see EQP below), sunspots and other purely visible solar features have long been recognized and recorded, but the internal plasmic activity did not become known until after satellite measurements began in about 1980. Solar activity is now understood to be generating the sunspots as well as solar flares, solar wind, and other strong radiations that propagate outward into the solar system, somewhat irregularly and at various speeds. These emanations create the magnetosphere that surrounds the earth, a modulation of the natural geomagnetic field and the earth's electromagnetic environment with ionospheric changes, aurora, and disrupted radio transmissions, to name a few effects (Merrill, McElhinney, & McFadden 1996). They are responsible for some climatic variations and may also influence the weather and the atmospheric electrical phenomena already discussed (EQE, EQM). Under favorable conditions they may conceivably participate in the triggering of earthquakes. So a possible chain of cause and effect exists for allowing solar activity to be an earthquake precursor. Its credibility rests most strongly on the unknown effect of atmospheric electromagnetic fields on the stressed fault itself.

Better evidence for this putative sequence comes from a direct association of measured geomagnetic activity (such as geomagnetic storms) with the record of prior earthquakes and perhaps volcanoes. The results of this comparison are unfortunately unclear: Attractive evidence has been found both for and against such a correlation (Johnston 1997, Duma & Ruzhin 2003, Eftaxias, Balasis, Papadimitriou, & Manda 2009, Yesugey 2009), but neither argument is fully convincing.

One may also try to correlate solar activity directly with the historical record of earthquakes. The former is measured by the Sunspot (Wolf) Number (roughly, the total number of sunspots visible on a given day). The 300 years of records show a clear 11.1-year cycle, and an early comparison revealed a small increase in earthquakes during the minima of this cycle (Simpson 1967). Two recent studies agreed (Stothers 1989 [volcanoes] and Zhang 1998 [earthquakes]). The case is still being argued (Khain & Khalilov 2009, Casey 2010), and again the alleged correlation remains less than certain.

These tentative findings suggest that earthquakes are indeed related to sunspots and solar activity, through the geomagnetic field around the earth, but other still unknown factors also appear to be involved. The findings to date are weakly supportive of the intuitive statements but still not decisive. The issue remains open.

### ***EQP—Planetary Effects***

The intuitives cite the planets of the solar system as having an influence on earthquake triggering through their gravitational effect on solar activity and the resultant radiations which affect the earth. Again:

Gravitational forces from both the planets and heavier activities [affect] the release of many radiations from the surface of the sun. All these activities indeed are integrated with the [triggering] phenomena. [KR]

Seismologists have left the possibility of planetary influences on earthquakes to astrologers, if they ever took the matter at all seriously, and astrologers have responded with at least a dozen speculations on critical planetary configurations and even specific predictions of their own. None of the articles published in the astrological literature have been able to meet scientific criteria for validity. Either the statistics were misapplied, the theory or prediction was not sufficiently specific to be tested, verification by the astrologers or others was never actually attempted, or the expected quake never occurred (Tomaschek 1959, Dean 1977, Phillipson 2000). A few scientists and technical writers have done their part, too, with no better success (Johnston 2002, Harnischmacher & Rawer 1981, Gribbin & Plagemann 1975).

Planetary science provides no plausible mechanism for a direct causal effect from the planets upon earthquakes. The gravitational force of all planets combined is much too weak to be directly effective on the earth—less than one ten billionth of that of sun. An indirect influence may be possible, however. The combined force of the heavier planets moves the



center of mass of the solar system around inside the sun, and even outside of it as these bodies move in their orbits. Discoveries by Jose (1947) and Wood and Wood (1965) showed that sunspot occurrence is directly correlated with the rate of change of angular momentum of the sun about the center of mass of the solar system, which follows the 11.1-year cycle. This activity might then affect the convection of plasma inside the sun, influence the formation of sunspots, and modify the resultant radiations emitted by the sun, as described above (EQS). Since these radiated energies are known to distort the earth's geomagnetic field, the resultant near-earth storms may be involved as part of the earthquake-triggering process, as suggested in the previous section and as the intuitives say they do. Physical mechanisms have been proposed that would actually allow such an influence (Duma & Ruzhin 2003, Freund, Lazarus, & Duma 2010).

This long scenario would obviously have to be causal, not indicative. If it can be shown to be a valid precursor, it would allow at least some degree of predictability, simply because the motions of the planets are governed by fixed laws and their positions are perfectly predictable. Further research will be needed to complete the argument, for these effects must occur at just the right times, frequencies, intensities, and locales on the earth for the combined activity to be sufficient to trigger a local shock. Other less apparent factors may need to cooperate as well.

While this sequence from initial cause to final effect is partially speculative, and therefore not acceptable as a full explanation, the overall scenario is credible and partially supports the intuitives' claim. Their statements therefore stand as partially verified, the more so if the geomagnetic/magnetospheric influences (EQS) turn out to be valid and the electromagnetic fields can be shown to actually trigger the fault. This possibility merits further exploration. A study could benefit especially from additional intuitive inquiries.

### ***EQT—Solid-Earth and Ocean Tides***

Earth tides aid the triggering process. [AAA]

[Did you say tides in the crust of the earth?] They are responsible to a minor degree. [AA]

The forces of gravity would be minor upon the earth's crust. The alignment of both the moon and the sun [may] . . . cause an upset in the earth's own ability to balance these forces electromagnetically. [KR]

It has long been known that the gravitational pulls of sun and moon cause a twice-daily heaving of the earth's crust by up to half a meter, and of the oceans up to two meters, as the regions of highest gravitational stress



sweep over the earth's surface antipodally while the earth rotates beneath them. They peak at syzygy—the lineup of sun, earth, and moon—and when the moon is closest to the earth (perigee).<sup>15</sup> It is only natural to wonder if these forces, or possibly their spatial derivatives across the fault, might trigger already stressed portions on the verge of release. Amateur predictors are in their element with this one, since the forces are extraterrestrial and are at a maximum during the magic moment of eclipses.

One may readily check this candidate precursor by comparing the voluminous records of past earthquakes with the gravity forces from the sun and moon, which are readily calculated at the times and locations of each quake (e.g., Darwin 1962). Dozens of such studies have been carried out. Early findings were ambiguous, and some were in error due to neglecting the eccentricity of the moon's orbit (Cotton 1922, Simpson 1938, Tamrazyan 1967, Knopoff 1969, Shlien 1972, Mauk & Kienle 1973). Studies after 1980 revealed a definite but small and irregular triggering effect, just as the intuitives indicated (e.g., Bibliography: International Center for Earth Tides, Heaton 1982, Sue 2009, Zhao, Yanben, & Zhian 2000). The most recent of these revealed particular fault modes and areas under which the effects are most likely to occur, namely, when the earthquake is shallow, the tidal force lifts up the fault, or differential loading occurs across the fault from nearby ocean tides (Kilston & Knopoff 1983, Cochran, Vidale, & Tanaka 2004, Kansowa & Tatnall 2010, Tanaka 2010). The effect is not consistent, however, so it probably depends upon the particular state of the fault.

The intuitives' statements on the tidal precursor, obviously causal, are validated as stated, except for the comment about “electromagnetic balancing,” which is unclear. The overall effect is probably too unreliable to make it useful by itself for prediction purposes.

### **EQG—Ground Gas Emission**

Toward the outer surfaces of the tectonic plates, wherein there is . . . the accumulation of both gaseous and aqueous forces, . . . the building of gaseous forces, observable within at least a 50 to 100 mile radius of the quake. . . . There will be radon, xenon, some . . . argon and also increases of hydrogen and oxygen . . . in proportion as normally found in the aqueous state of water. [KR]

Many gases that are emitted from the earth's soil, some continuously, have been tested for their sensitivity to earthquakes. Rn, CO<sub>2</sub>, and CH<sub>4</sub> have shown the most significant co-seismic variations (King 1978, 1980, 1986, Voitov & Dobrovolsky 1994, Pulinets, Alekseev, Legen'ka, & Khagai 1997,

Zhou et al. 2010).  $^{222}\text{Rn}$  is produced naturally in the earth's crust from the radioactive decay of radium ( $^{226}\text{Ra}$ ) which seeps to the surface along water channels and through microfractures, faults, and volcanic structures. With a half-life of only 3.8 days, it soon decays, but not before creating a health hazard to humans who are exposed to too much of it.

Rn has been observed in surface water and in deep wells since 1966 at several locations in the world and has been explored for its possible association with earthquakes (Ulomov & Malashev 1971, Teng 1980, C. King, B. King, Evans, & Wei 1996, Igarashi, Saeki, Takahata, Sumikawa, Tasaka, Sasaki, Takahashi, & Sano 1995, Singh, Kumar, Zlotnicki, & Kafatos 2010). Its detection is not difficult, and measurements taken along known faults have shown large increases just before many major shocks. Reliable ongoing monitoring has proven difficult, however, because rainy weather, natural soil moisture, varying soil chemistry, and changing hydrologic conditions interfere with accurate measurement. The most recent attempts are encouraging but still not sufficiently uniform and consistent to provide reliable precursory information that might be useful for predictive purposes, even in well-monitored seismic areas. Moreover, it is still not clear if the increases in Rn concentration before earthquakes occur for all major events or if they also occur in non-seismic circumstances. And are they merely indicative of impending earthquakes or a prime contributing cause? The results from EQE suggest the latter.

Inflammable earth gases ( $\text{CH}_4$ ,  $\text{CO}$ , and  $\text{H}_2$ ) have also been proposed as an earthquake precursor (Wakita, Nakamura, Kita, Fujii, & Notsu 1980, Gold 1994, Singh, A. Kumar, Bajwa, Mahajan, V. Kumar, & Dhar 2010, Jones 2002). The intuitives agree. For example:

Metal deposits within the earth are concentrated in certain peculiar shapes and forms. When gases of a certain nature reach these they create an explosive effect that causes changes to come about. Now this . . . is a definite factor in some areas. [LH]

These gases are already known to accumulate in coal mines, where they have led to damaging explosions. They can also arise from ruptured gas lines, underground gas storage cavities, and seepage from natural gas wells. If ignited in seismic areas, such explosions could certainly induce slippage along faults. It may be difficult to detect them simultaneously with the quake unless they are large and close to the surface. There are no observational data that identify such natural gas explosions as the cause of an earthquake, though underground nuclear explosions are known to be capable of doing so.<sup>16</sup> The intuitive statement is therefore plausible, though it may not be relevant to a gas-related precursor.

While the intuitives' information on ground gases is broadly confirmed, more research will be necessary before the detection of Rn or other soil gases can be confirmed as a useful indicator or cause of triggering action, and especially as a useful precursor. Further intuitive inquiry could help to answer these questions.

### ***EQF—Centrifugal Forces Inside the Earth***

It has long been known that centrifugal forces, arising from the earth's steady rotation upon its axis, induce complex circulations in the plastic magma layer in the mantle. These motions are suspected of being responsible for driving the tectonic plates in their slow movement on the upper mantle and contributing thereby to earthquakes at a basic, global level (Lay & Wallace 1995). The intuitives confirm this suspicion and go on to explain further. One of them puts it this way:

There are two forces which would account for triggering the final activities of quakes. The centrifugal force of the earth's rotation causes movement of the magmas. . . . In the centermost [portion] of tectonic plates the centrifugal force of the planet in its axis of rotation combines with the thermal expansionary forces of the molten masses beneath the surface to [cause] movement along the inner structures, toward the outer surfaces of the tectonic plates. . . . The pressures of centrifugal forces combine with the normal expansionary pressures of any heated matter. When all of these reach a critical point, the final triggering of the quake [takes place]. . . .

When there is a concentration of the plasmic field, when it becomes exposed to the earth's magnetic field plus the earth's centrifugal force, [then there] is this critical force that causes a transference of energy from the kinetic level to the molecular level of the earth's stable crust. This is the setting-off factor that, in its own right, actually triggers the quake. [KR]

The claim for a role by the earth's static magnetic field (0.3–0.6 gauss at the surface, ~0.25 gauss at depth) is not presently seen as a contributing force in plate tectonics or lithosphere movement but only as an incidental consequence of the circular dynamo-like electric currents produced by convection in the earth's outer core, which consists mostly of molten iron.<sup>17</sup> These currents have a small retarding effect on the convection, thus upon the currents themselves, so the intuitive's statement is technically correct. But this is probably not what he meant.

Rather, his statement seems to be referring to magnetic forces large enough to assist the movement of magma upward to become a "molecular" (solid) triggering force in the crust, perhaps similar to the plume under a volcano. Accepted plate tectonics offers no support for such movement

beneath earthquakes except along plate boundaries. An alternative theory independent of plate tectonics proposes that other plumes, more widely spread, extend from the core to the crust and are influential not just for volcanoes but for earthquakes outside of plate boundaries (Morgan 1972, Foulger 2010). These plumes would be very hot, thus detectable by satellite (EQTh). This hypothesis remains to be confirmed for both volcanoes and earthquakes.

The intuitive description is therefore verified except for this last point, and again for the specific role of the earth's magnetic field as a regional triggering force. More data are required on the physical role of the earth's magnetism at the level of the magma, and especially how the centrifugal motion in the liquid core and mantle could induce regional seismicity.

### ***EQW—Earthquake Weather***

Weather changes are popularly believed to be indicators of forthcoming earthquakes (and numerous other unexplained events)—a muggy feeling in the air, strange winds, heavy storms, unusual cloud formations, etc.—but this widespread and enduring legend lacks adequate data to qualify it as a genuine precursor. Even if the data were valid, such changes are not sufficiently unique and consistent to signal a shock reliably.

There is some basis for accepting such influences as causal but only if they are indirect—that is, causal of intermediate phenomena which then induce the earthquakes. Numerous reports after 1980 speak of heavy rainfall saturating the ground (Costain & Bollinger 2010, Schultz, Kean, & Wang 2009); typhoons, which have initiated small earthquakes in Taiwan, presumably from the sudden drop in air pressure (Liu, Linde, & Selwyn Sacks 2009); and hurricanes and flooding that can trigger landslides and avalanches under certain conditions and may participate in triggering earthquakes as well (Larsen 1990, Schultz, Kean, & Wang 2009, Wdowinski, Tsukanov, Hong, & Amelung 2011).

The intuitives agree with some of these speculations and associated precursory mechanisms; for example:

There is often the attraction of certain cloud forms to areas of quakes, and even the stilling of the atmosphere. . . . observable cloud structures, bulbulous [bulbous?] and towering in nature, up to 50 to 200 miles from the center . . . a great release of quantities of water from the atmosphere. [KR]

The heat and dryness of the earth's crust is . . . the final catalyst after the pressure buildup has reached a certain point . . . even though the particular time of the occurrence is not dry per se. [LH]

Large cumulus and other unusual cloud patterns before particular shocks have often been reported from Japan and China (e.g., Wu, Li, & Liu 2009, Dicks 2008, Guo & Wang 2008), and the dryness of the crust is consistent with the favorable pre-earthquake atmospheric conditions involving electric charge buildup (EQE) and electromagnetic activity (EQM) (F. Freund, Kulahci, Cyr, Ling, Winnick, Tregloan-Reed, & M. Freund 2009). Moreover, the weather itself is now seen to be a dynamic electromagnetic process, interactive with familiar atmospheric thermodynamics:

Electromagnetic forces that account for atmospheric conditions of storms, even the triggering of lightning, are grounding principles involving the electromagnetic fields of the earth. They are . . . directly related to the earth's electromagnetic energies. [KR]

During the 1980s, satellite observations allowed the earth's weather to be monitored from space, and large computers enabled it to be modeled and forecast globally and accurately. Electromagnetic effects such as lightning, auroras, and the movements of ionospheric layers began to be better understood. The overall process turned out to be very complicated. There is much to be explained before the full relationship between earthquakes and the weather can be understood, even just looking for a practical precursor.

The intuitives' comments on weather phenomena associated with earthquakes are simple but partially supported by the knowledge and data gained in the last thirty years. While none of the information has been contradicted, most of it remains to be verified. At this point weather changes may become a contributing precursor, but their great variability makes it doubtful that they will turn out to be very useful as such.

### ***EQA—Abnormal Animal Behavior***

Many animals are known to possess physical senses not enjoyed by humans, both in kind and sensitivity: sounds, vibrations, thermal radiation, gases (smells), electromagnetic and magnetic fields, and surely others (Buskirk, Frohlich, & Latham 1981). It is reasonable to expect that they may be able to pick up subtle environmental clues related to forthcoming earthquakes. Hundreds of popular reports of observations of abnormal animal behavior near particular earthquakes have come from all across the world, from ancient Greece to news items every year in this century. A persistent folk legend had grown up around the possibility. Reports of observations accumulated up to 1980 left no doubt that the phenomenon has at least limited validity as a precursor (Lee, Ando, & Kautz 1976, Evernden 1976, Davis 1979, Kerr 1980, Lott, Hart, & Howell 1981), though solid data were missing. While

the intuitives confirmed this hypothesis, specifics were neither offered nor requested as to which kinds of quakes, animals, animal sensitivities, and perhaps other factors are behind the precursor.

Three surveys in later years sought to sort out the huge volume of accounts and try to identify those which were credible enough for scientific acceptance (Schaal 1988, Kirschvink 2000, Bhargava, Katiyar, Sharma, & Pradhan 2009). The main difficulty was that many of the reports arose only after the quake occurred and were therefore likely to be fortuitous recollections. Often the abnormal behavior itself occurred only after the quake. Most critical, they did not always distinguish the alleged abnormal behavior from ordinary animal behavior due to predators, rutting, storms, and fire sirens, for instance. Further research identified likely animal sensitivities (Buskirk, Frohlich, & Latham 1981) and explored some of the possibilities (Otis & Kautz 1981, Brown & Sheldrake 1997, Pararas-Carayannis no date) but led to no significant new options. See a recent report (Grant, Halliday, Balderer, Leuenberger, Newcomer, Cyr, & Freund 2011) on an indirect precursory possibility: toxic ground water.

The relatively few acceptable reports showed that the animal precursor is generally and widely valid though they revealed no useful pattern. There are just too many kinds of animals, earthquakes, and potential sensitivities to allow conclusions to be drawn about the underlying triggering process, let alone to serve as a useful precursor for prediction purposes. A major research effort would be needed to explore these many distinctions, and there would be no prior guarantee of eventual success.

The intuitives' information on abnormal animal behavior before earthquakes is therefore verified, though this precursor is not likely to be helpful by itself unless much research is carried out. Further intuitive inquiries could identify likely possibilities.

### **Candidate Precursors Not Verified**

#### ***EQN—Nuclear and Other Radiation***

Two intuitives spoke briefly of nuclear activity and radiation emitted inside the earth:

[What kind of forces are acting upon the rock at the point of fracture or sliding?] These are static pressures that come about from electrostatic, electromagnetic, and nuclear pressures. The electrostatic and electromagnetic forces act not directly on the epicenter [hypocenter?], but in the surrounding area, while the nuclear force acts directly on the epicenter. [AA]

The principal triggering action is coming from changes in the internal radiation which originates in the central core of the earth. ... It throbs and pulsates like a powerful human heart, and is continually changing its shape. These changes affect its radiation accordingly. [BR]

The main energy for earthquakes comes from the central core of the earth. This energy is very powerful and can take any form. ... This energy has a very high vibration. ... [It] is like atomic energy, and has some electrical and magnetic properties. [AA]

The term *radiation* is ambiguous since it can refer to any form of energy that is radiated, including thermal and electromagnetic waves as well as particle emission from nuclear decay. The “nuclear pressure” and “nuclear force” in the first excerpt may refer only to the radioactive decay in the crust and upper mantle which produces radon, as verified earlier for that precursor (EQG). The internal radiation in the earth’s central core (second excerpt) may be thermal only (EQTh). The vague terms *very high vibration* and *like atomic energy* (in the third) could also refer to thermal radiation. While these ambiguous intuitive statements have at least one valid interpretation, they are unfortunately not sufficiently informative to be verifiable.

Recent studies of anti-neutrinos emitted continuously from the earth have been helpful in determining its internal composition and source of heat emission. It is not yet known if the location and intensity of the anti-neutrinos are related to earthquakes in any way (except possibly for monitoring global heat generation [EQTh]), though the immense complexity and cost of the detection equipment (KamLAND and Borexino) precludes its use as a practical precursor that could be monitored regionally (Araki et al. 2005, Fields & Hochmuth 2006, Fiorentini, Lissia, & Mantovani 2007, Bellini et al. 2010).

Ionizing cosmic radiation is certainly impinging upon the geomagnetic field and ionosphere, parallel to the solar radiations already discussed (EQS), and could be playing a part in the ionospheric disturbances already verified (EQI) (Dorman 2004). The cosmic ray index has been found to be correlated with cloud formation at low altitudes, and the earth’s climate generally, though the claim has been contested (Svensmark, Bondo, & Svensmark 2009, Damon & Laut 2004). This influence was not mentioned by the intuitives except for a brief referral to an “upper energy” that affects the ionosphere. It could then be translated into infrared and propagated downward into the atmosphere. This phenomenon could be interpreted as cosmic radiation, but evidence is again lacking. The intuitives’ statements on nuclear and other radiation are unverifiable.



**EQH: Human Precursors?**

Since expert intuitives are able to provide detailed technical information on the earthquake-triggering process, might they also be able to predict earthquakes directly—that is, to be precursors themselves?

The answer is yes, but there are further conditions on this particular application of intuition because the prediction now becomes part of the event being predicted. Personal experience in collecting and evaluating intuitive earthquake predictions from both amateurs and experts shows that their efforts are sometimes remarkably successful, though their overall reliability and usefulness is small. It turned out that the intuitive acquisition of predictive information (of any sort) is easily limited or blocked unless the consequences of acquiring and utilizing it later are taken into account and respected. When the prediction would do more harm than good, because of consequences not foreseen, the flow of information can be blocked.

The expert intuitives offered further explanation for this blockage. First of all, they remind us that man already possesses both the intuitive and the physical capacities to be aware of when and where earthquakes are about to occur, and he is always free to make use of these faculties for his own benefit:

Unconscious material is admitted into consciousness according to the beliefs an individual holds about himself, his reality and his place in it. Those who want to use their own unconscious precognition of such an event will take advantage of it. . . . On other than conscious levels, simply as creatures, you are well aware of impending storms, floods, tornadoes, earthquakes and so forth. There are many hints and signs picked up by the body itself—alterations in air pressure, magnetic orientation, minute electrical differentiations of which the skin itself is aware. [JR]<sup>18,19</sup>

In other words, blockage is already taking place for almost everyone.

When intuitive earthquake predictions are intended for non-personal use, the reaction to the information is part of the prediction. Intuition can aid or retard the reception process. Since predictions for public use are not consistently accepted and acted upon equally by everyone, they can easily induce confusion and panic. To be useful they must be announced officially with scientific and governmental authority and with clear directions for evasive action, as noted earlier. Without this sanction, it is better not to release the prediction in the first place, or even seek it. A competent intuitive may not be able or willing to provide it (Kautz 2005).

For personal predictions, the intuitive process operates differently. The recipient is then free and responsible to choose whom he listens to and his own response. While the prediction can activate his fears and expectations, he is the only one who must deal with it. If he is the intuitive himself, he may find the information blocked. Even the most expert intuitives sometimes find it difficult to obtain reliable information about themselves. Like surgeons and psychiatrists, they know they can be blinded to their own issues and limitations.



Sometimes a prediction is not called for at all:

Natural disasters are brought about more at an emotional level than at a belief level, though beliefs have an important part to play for they generate the emotions. . . . Those in earthquake regions are attracted to such spots because of their innate understanding of the relationship between exterior circumstances and their own private mental and emotional patterns. [JR]<sup>20</sup>

That is, while one person will chose to live in a quiet locale which places few demands on his personal development, another will choose to live in an energetic environment with political unrest, wars, tornadoes—or earthquakes. Our private tremors tend to coincide with those of the earth! Persons inwardly seeking quiet and security will avoid such circumstances, while those seeking challenging drama will find themselves unconsciously gathering in earthquake-prone areas. This is just as some persons choose to join the military, enter the business world, or become involved in politics: They select what they deeply feel they need.

These individual choices can even help bring about the earthquake in the first place:

The qualities of such individuals . . . en masse affect the deep electromagnetic energy of the earth. . . . Obviously, there have been earthquakes where there are no people, but in all cases the origins are to be found in mental properties rather than exterior ones. . . . Your feelings have electromagnetic properties. . . . There are what I am going to call “ghost chemicals”—aspects of normal chemicals that you have not perceived so far—which are changed into purely electromagnetic properties. Energy is released that directly affects the atmosphere. [JR]

Geoscience is not yet aware of any such “ghost chemicals.”

Finally, and in a broader sense, man “creates” his earthquake experiences whenever he builds flimsy houses on ground susceptible to shaking, liquefaction, and slides, and constructs tall buildings covered with plate glass. He locates his cities (and nuclear reactors!) on shorelines subject to tsunamis. It is already established that quakes can be triggered from the creation of coal mines (Lovett 2010), dams and reservoirs (Gupta 2002), and geyser plants (Streepy 1996). It is also known that under favorable conditions small shocks can be turned on and off by pumping water into and out of wells near faults (Raleigh, Healy, & Bredehoeft 1976). Man explodes nuclear bombs underground (McEwan 1988) and extracts huge quantities of oil and gas out of the earth’s crust (Bibliography, no date), without serious concern for how these activities might affect stressed faults. As new precursors are found, still more human effects on earthquakes may be uncovered.

Humans, aware or not, are creating many of their own earthquakes.

### Conclusions and Implications

The past thirty years have seen the small, retiring subfield of seismology expand into its parent field of geophysics, thanks mainly to the wave of technological advances in space exploration and computation, and the global media awareness of costly and tragic earthquakes which has been enabled by modern global communications. At the same time the sheer complexity of the short-term earthquake prediction/forecasting problem has exceeded all earlier expectations and is now on the same scale as the problem of understanding the cause and treatment of cancer within the human body: much intricacy, no identifiable primary cause, many strongly interdependent factors, an interdisciplinary approach required (which the last generation of researchers are not equipped to handle), and no good clues on which of several possible approaches will most likely lead to a solution.

The most recent discoveries on the critical role of electrical activity (of many sorts) in the atmosphere and near-earth space, as anticipated by the intuitives thirty years ago, are adding their own fuel to this scientific explosion. They have contributed new portals for exploration, a deeper understanding of triggering, and several new precursors, but have not (yet) provided the specific directional clues that are so much needed now. Future intuitive inquiries hold this potential. The practical goal of short-term prediction still lies in the future, perhaps a distant one, and we are not even sure at this point if it can ever be reached.

Despite this complexity and expansion, we have learned that electrical activity in the ground, atmosphere, and space can no longer be neglected as part of the triggering process; that no one precursor is likely to be found sufficient by itself as measurable and reliable for short-term prediction; and that space exploration will continue to play a strong role in understanding the triggering process itself and which of the thirty or so potential precursors might be employed as measurable indicators for prediction purposes. Interested and experienced seismologists may be able to glean additional ideas from this article from the intuitive excerpts presented herein.

Nevertheless, the main issue in this paper was not seismology, despite its great interest and human importance, but rather the validation of a different and more powerful way of acquiring totally new information that can then be applied to any area, even outside of science, that is limited by a lack of relevant knowledge and understanding. This study has demonstrated, through an important example, that detailed, significant, and totally new knowledge may be obtained through suitably executed intuitive inquiry. By relying more heavily on intuitive methods in the future, the gateway to scientific discovery can be expanded widely. Examples taken from prior

research in other disciplines apart from seismology suggest that there are few if any limits on the depth and breadth of knowledge attainable by intuitive methods (Kautz 2005, Grof & Kautz 2010) so long as appropriate questions can be asked and the inquiry has a positive human purpose. A rich reservoir is waiting to be tapped.

### Notes

- <sup>1</sup> For example, the large annual meetings of the Association for the Study of Consciousness at the University of Arizona, now in their eighteenth year.
- <sup>2</sup> The hypocenter of an earthquake is the point within the earth where the rupture or slippage first begins; the epicenter is an imaginary point on the earth's surface, directly above the hypocenter. The region of strongest ground shaking may be neither of these, since it may arise from another portion of the same fault and depends in a complex way upon rock structures in the surrounding area.
- <sup>3</sup> The Japanese high-speed trains (*shinkansen*) operate at 210 km/hr, and are slowed to 70 km/hr after a very-short-term earthquake warning.
- <sup>4</sup> Let it be understood that the term *precursor* refers only to an advance signal or indicator that an earthquake is about to be triggered, or has been triggered. It need not be causal to the trigger, though in the investigation of the triggering process a candidate precursor should always be checked for a possible causal role.
- <sup>5</sup> Tape and transcript records are in storage in Sebastopol, California, USA. A preliminary report on intuitive inquiries on the earthquake-triggering problem appeared in *Psi Research* (Kautz 1982), a small journal no longer published, and a fuller report in Chapter 7 of the book *Opening the Inner Eye* (Kautz 2005).
- <sup>6</sup> Earthquake prediction research was effectively stopped in 1990 through legislation, promoted by Vice-President Al Gore, in favor of mitigation efforts. It has since been picked up again by several NASA projects centered around space observation, but there are no current (2011) USGS projects on prediction. Foreign programs on earthquake prediction in Russia, China, and Japan, with smaller programs in Greece, Turkey, and Italy, have bloomed in the last twenty years.
- <sup>7</sup> The reference to *electromagnetism* requires elaboration, for these forces include in their definition all known forms of radiation, from radio, infrared, light, ultraviolet, X-ray, and microwaves, to cosmic rays and even the fundamental particles of physics, which have both particle and wave properties. The distinguishing feature of all these forms of

energy is their wavelength (equivalently their frequency), which varies from many miles down to billionths of a millimeter and beyond. Static electricity and magnetism have zero frequency.

- <sup>8</sup> The bracketed initials [XX] indicate the contributing intuitives, who are more fully identified in the Acknowledgments.
- <sup>9</sup> The earthquake prediction claims made by this group (“VAN”) have been severely criticized (Lighthill 1996, Kagan 1997), but the instrumental measurements (“SES”) have not been questioned.
- <sup>10</sup> A later analysis (Campbell 2009) showed that the ULF signals were actually prevalent over much of Western North America at the time of the quake, not just near the epicenter. Fraser-Smith’s claim for a ULF precursor is therefore weakened but could still be valid with the precursor active over a much larger area.
- <sup>11</sup> The electric and magnetic components are effectively independent at these low frequencies and must be measured separately.
- <sup>12</sup> Not to be confused with the Quakefinder system of JPL (Jet Propulsion Laboratory), which seeks to automatically and accurately map regional ground displacements as measured by satellite (Stolorz & Dean 1996).
- <sup>13</sup> Newspaper report from Honolulu, April 1973, no longer retrievable.
- <sup>14</sup> This phenomena must be distinguished from acoustic gravity waves created directly from the earth’s vertical movement *during* the earthquake.
- <sup>15</sup> The term *syzygy* may refer to either an exact lineup of three planetary bodies, or merely a near lineup such as occurs at eclipses.
- <sup>16</sup> See, for example, [http://en.wikipedia.org/wiki/Underground\\_nuclear\\_testing](http://en.wikipedia.org/wiki/Underground_nuclear_testing)
- <sup>17</sup> Paleomagnetic traces near seafloor spreading played a major part in the development of the continental drift theory, now well accepted, though the earth’s magnetism is not seen as driving the spreading.
- <sup>18</sup> Roberts 1972.
- <sup>19</sup> Roberts 1972.
- <sup>20</sup> Roberts 1972.

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## RESEARCH ARTICLE

### **Audience Size Effects in Field RNG Experiments: The Case of Japanese Professional Baseball Games**

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**Abstract**—This study examined the association between the outputs of a true random number generator (RNG) and audience size during Japanese professional baseball games. We regarded the RNG as a signal detector of field consciousness and hypothesized that the number of signal sources may increase the ability of an RNG to detect signals. Experimenters and our assistants voluntarily obtained 76 samples from a total of 78 baseball games during the 2010–2011 baseball season. The effects of audience size at the stadium ( $M = 38970 \pm 6058$  SD,  $N = 78$ ) and TV audience ratings ( $M = 7.07 \pm 2.32$  SD,  $N = 23$ ) were examined in relation to the measurements of multiple Random Streamer and Psyeron RNG devices. RNGs set at remote locations ran simultaneously during the games. Our results showed a positive correlation between accumulated chi-squared statistics by Random Streamer and audience size at the stadium. Unexpectedly, identical RNG devices showed strong negative correlations between different machines, which suggested that their outputs canceled each other out. Finally, some future tasks are discussed.

**Keywords:** baseball stadium—MMI—PK—Rpg102—Rpg105—Tokyo Dome

### **Introduction**

A random number/event generator (RNG/REG) creates a physical random source in itself, and its output is essentially unpredictable and different from pseudo random numbers (PRNG), which require an initial seed. Recent field RNG/REG studies have reported that field consciousness affects RNG output during large events or when news is broadcasted worldwide (Nelson 2001, Nelson et al. 1996, 1998, 2002, Radin 1997, 2002, 2006).

Field consciousness seems to involve many psychological factors, including group emotion, focused group energy (Rowe 1998), and a coherent mind (Radin 2006). From the results of previous studies, it is possible to



assume that an RNG would be a signal detector despite its noisy outputs, whereas field consciousness would be regarded as the source of signals during events. In accordance with this assumption, it can be hypothesized that statistical biases are more detectable when strong signals, such as coherent group emotion, exist in the field (Shimizu & Ishikawa 2010, 2011). In contrast, it could be hypothesized that a large number of signal sources makes it easy for an RNG to detect biases as signals, even though each signal is weak. This possibility is suggested by the results of Radin (2006), who reported, based on analysis of data from midnight of Y2K and all New Year's Eve data from 1999 to 2005, that the anomalistic behavior of RNGs was observed by a "mass-coherent mind." As external variables such as audience size have no relationship to RNG outputs, audience size effects, if they exist, could no longer be an issue of RNG output qualities, and it might be concluded that it is simply an anomaly that an RNG detects field consciousness.

To date, however, there exists little systematic field research and few experiments that have directly investigated audience size effects. This is related to methodological issues confronting field RNG studies. It is difficult for researchers to control huge events or worldwide news and to determine exact audience size and the possibility that other factors might be involved.

### **Field RNG Experiment**

The current study focused on how signal quantity or audience size affects the outputs of an RNG. It was expected that the larger the audience is, the greater the statistical bias would be in the outputs of an RNG. To examine this hypothesis, a field experiment with a fixed event that enables us to measure audience size would be required. Additionally, the event must have particular locations and start and end times, in contrast to events such as broadcasted news, which pose difficulties in determining the endpoints. Recently, these kinds of field RNG experiments have been conducted by several researchers (Varvoglis 2006, Lumsden-Cook 2005a, 2005b, Shimizu & Ishikawa 2010).

It is important for a large-scale study to meet the following conditions. First, the event must be repetitive. Second, the audience size must be countable and must vary. This will allow an estimation of how RNG outputs and audience size are associated. Ideally, audience size always maintains a particular size. A large-scale repetitive sporting event fulfills these conditions.

Sporting events could be regarded as a way to detect field consciousness, considering that RNG outputs of some sporting events, such as soccer games showing statistical biases (Bierman 1996, Hagel & Tschapke 2004)

and American football games, which tend to produce chance results (Nelson et al. 1998, Radin 1997), have already been analyzed and reported.

For this purpose, we selected Japanese professional baseball games for our field RNG experiment because baseball is the most popular sport in Japan and typically has a large audience. The Tokyo Dome has a large audience capacity (a maximum of about 47,000), and audience sizes vary throughout each season. The game is also repetitive. Generally, about 70 games (half the number of total games in the season) are held at the home field during a regular season. It is predicted that RNGs run at the stadium during baseball games would show statistically biased outputs. The main hypothesis of this study is that outputs will reveal positive effects corresponding to the audience size at the stadium.

### ***TV Audience Ratings as a Second Factor***

Because Japanese professional baseball games are occasionally broadcast nationwide on a ground-based system, we can also consider TV audience ratings as a second factor indicating focus–attention quantity, assuming that the TV audience rating is related to and corresponds to the quantity of total nationwide focus attention or interest of many people.

It should be noted that the TV audience is distant from the stadium. As non-locality in psi phenomena has already been reported (Hagel & Tschapke 2004, Dunne & Jahn 1992, and the Global Consciousness Project), some might expect that an RNG can detect field consciousness without being influenced by distance. However, distance-dependent properties of RNGs have also been reported such that deviations in RNG outputs decrease with distance from the Institute of Noetic Science laboratory, where the RNG machines are located (Radin 2006). These findings are not inconsistent when we assume that non-local psi phenomena exist but are not completely free from distance effects.

One can rather assume that the TV audience is very large. In 2011, about 120 million people were living in Japan, and 42 million people live in the Kanto Area where the current study collected information. If the TV rating is 10%, then 4.2 million people in the Kanto Area will be focusing on the same program simultaneously (12 million in nationwide Japan). This size corresponds to 100 times the audience size at the stadium. It was expected that the TV audience rating may have sufficient influence on the RNG output due to its huge size, despite distance.

To examine these two kinds of audience effects, we set up RNGs at the ballpark and at locations remote from baseball stadiums, and we made them generate random numbers continuously. If the effects of audience size are dependent on distance, it would be expected that the effects of the stadium

audience could affect RNG outputs at the stadium while having small-sized effects on the remote RNGs. Additionally, TV audience rating effects, if observed, would have small effect sizes on all RNG outputs regardless of their location. If a huge TV audience size could make up for the increase in distance, all RNG output conditions would be affected by the TV audience ratings. The ratings were expected to directly affect not only experimental RNG outputs located at the stadium but also those located far from the stadium. In any case, it was expected that the audience size at the stadium would have positive effects on the RNGs located at the stadium.

### ***Using Multiple RNG Devices***

In the current experiment, multiple RNG devices were used to generate random numbers, based on the assumption that all the RNGs at the same location would be affected equally by field consciousness (or other potential factors). If so, the outputs of multiple RNG devices would be positively correlated, and an experiment with multiple RNG devices would have some advantages, as it could increase the chance of finding output biases from the viewpoint of signal detection.

## **Methods**

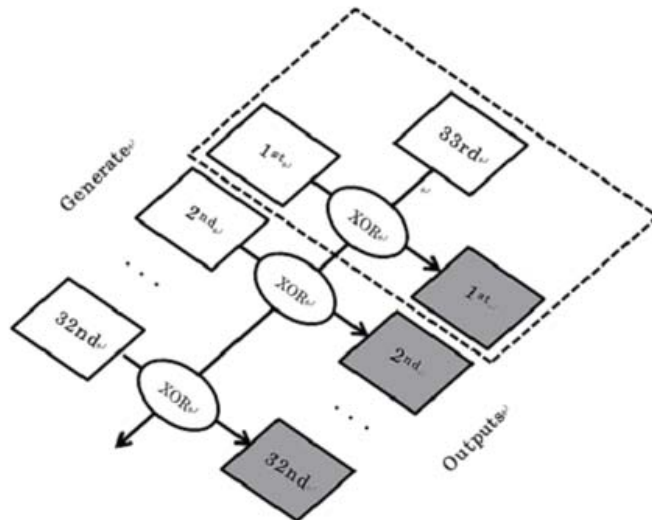
### ***RNG Hardware***

Five “Psyleron (REG-1)” and five “Random Streamer” devices were used as physical random number generators. Both types of RNG devices generated true random numbers with the physical sources.

The random source was a Psyleron device based on a field-effect transistor (FET), which has reverse-biased diodes with 10-nm doped gaps for electron tunneling, so that current flows opposite to the normal direction and forces electrons against the solid-state junction barrier. Electron tunneling allows a small current to develop past the barrier, and this is sampled to provide the random bits used in the experiments.

The Random Streamer<sup>1</sup> complies with Federal Information Processing Standardization (FIPS 140-2) regulations, using thermal noise as a random source. The IC tip has a pair of sensors, each of which is constructed by combining two FETs: one for a source of thermal noise and another as a noise amplifier. Next, the two signals from the sensors are modulated by other thermal noise. Two clock signals are put into delay flip-flop, which compares their time difference. Bit output (1 or 0) is decided based on whether one is faster than another. With these mechanisms, the device is not affected by environmental parameters such as external temperature or noise.

We have two types of Random Streamers, Rpg102 (four devices), and Rpg105 (one device). The Rpg102 has one random source, whereas the Rpg105 has 32 independent RNG sources that can generate outputs simultaneously. All Rpg105 outputs are bitwise-XORed (exclusive-OR) with a mask provided by RNG 33. However, we should note that the first



**Figure 1. Random number generation structure by Rpg105 (whole figure) and Rpg102 (within dashed closed line).**

Each random number generator (RPG100B) is represented by white rectangles in the figure. Gray rectangles are their outputs. Rpg105 has 32 independent RPG100B systems, which generate outputs of bits (1s or 0s), and the 33rd one is used only for XOR (exclusive or) between outputs by 32 RPG100B. Items shown within the dotted line are the same as the mechanism of the Rpg102.

part of its output is systematically the same as the Rpg102 (Figure 1). To analyze all the data from the Random Streamer equally, only the first outputs of the Rpg105 were used.

### Software Application

We developed two kinds of software to control the RNGs with the Visual Studio.NET framework version. The first application software was developed for Rpg105, and the other was designed to control Rpg102 and Psyleron at the same time. Since Rpg102 and Rpg105 were impossible to

use simultaneously in one machine, these two applications could not be used in the same machine. Both of the applications were programmed to generate random numbers at 512 bits per second, automatically recording them into a csv text file at two-minute intervals after the PC is started.

### **Procedure and Baseball Games**

The experimenter arrived at the stadium as long before the game as possible and started the notebook PCs attached to the RNGs before the start of each game. After starting the machines, he/she closed the covers to keep the monitors off to preserve the life of the batteries. One notebook PC controlled the Rpg102 and Psyleron devices (Figure 2) once we obtained



**Figure 2. Two RNG devices, Random Streamer (Rpg 105 and Rpg102) and Psyleron (REG-1), are shown connected to the PC.**

the Psyleron devices in early 2011. We used two notebooks simultaneously to generate as many random numbers as possible. Thus, two notebooks and four RNGs were used. The number of machines depended on the schedules of the 20 assistants who volunteered to take the PCs with RNGs to the stadiums. These time schedules were managed mainly by the first author.

A total of 78 baseball games in the Kanto area were observed as experimental field events from September 2010 to October 2011. The experimenter and our confederates went repeatedly to Japanese professional baseball games, including regular season games (67), the climax series (5), the Japan series (2), open games (2), and Eastern League games (2).

These games were held at the Tokyo Dome (70), Seibu Dome (3), Marin Stadium (2), and Meiji Jingu (3). Since two fixed season seats for the Giants were reserved throughout 2011, almost all of the observed games were at the Tokyo Dome. Both of the two season seats were bleacher seats located at the edge of the third floor behind the left pole (Figure 3), which allowed the watcher to relax, because there are no seats in front of or behind them, although sometimes the seats to the right were filled.

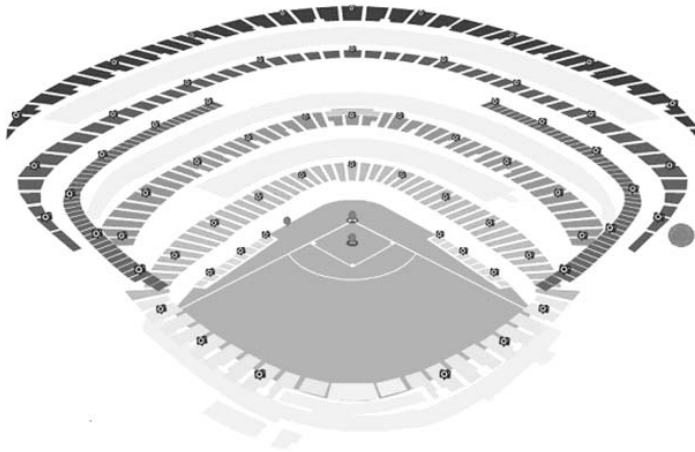


Figure 3. Location of season seats at the Tokyo Dome in 2011 (●).

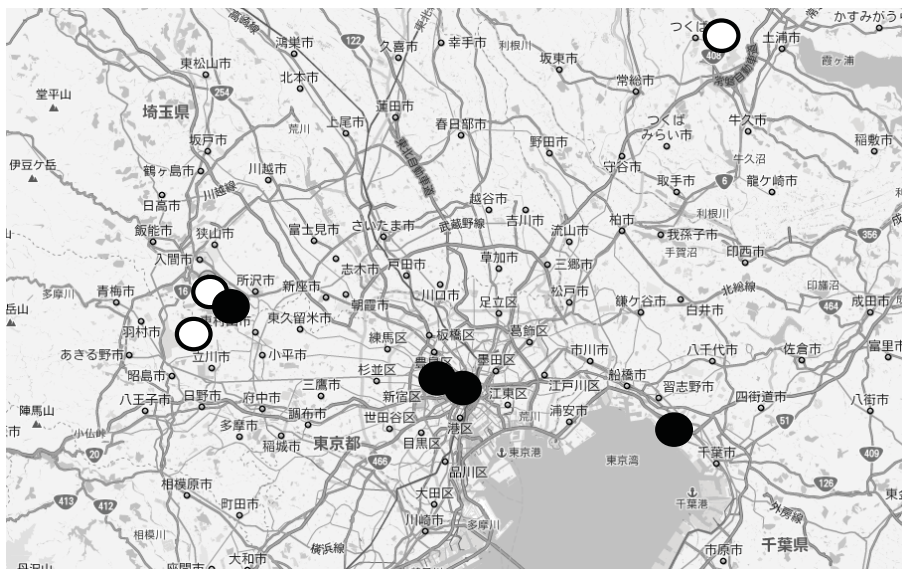


Figure 4. Location of baseball stadiums and remote PCs around Tokyo. Black circles represent baseball stadiums, and white circles represent remote PCs.



### Remote Client PCs and RNGs

For the remote condition, RNGs generated random numbers in Tokorozawa, Musashi-Murayama, and Tsukuba, all of which are in the Kanto area (Figure 4). At these remote PCs, RNGs always generated random numbers even though analyzed data were limited to the period when the game was in progress.

### Missing Data

Unexpectedly, we completely failed to generate random numbers during the entire game period for two games at the Tokyo Dome. Both cases were because our application failed to work due to PC troubles or software bugs. In another case, Psyleron could not be used because the experimenter found

**TABLE 1**  
Number of RNG Outputs Used in the Stadium and in Remote Locations  
in a Total of 78 Baseball Games

Device	Generation	Number of Games Observed	STADIUM		REMOTE LOCATION		
			First PC	Second PC	Musashi-Murayama	Tsukuba	Tokorozawa
Psyleron	single	46	25	21	67	64	35
	dual	21	17	21			
Random Streamer	single	55	34	21	57	64	43
	dual	21	21	18			
Number of RNG outputs			97	81	124	137	78
Total			178		339		
517							

Sum of Psyleron RNG outputs at the stadium is 84 (25+21+17+21), sum of Random Streamer outputs is 94 (34+21+21+18).

**TABLE 2**  
Estimated Pearson's Correlation Coefficients between Audience Size and z-Scores

Location	RNG	CZ					STOUFFER'S Z (SZ)					N
		Estimation	95% CI Lower	95% CI Upper	t-Score	p-Value	Estimation	95% CI Lower	95% CI Upper	t-Score	p-Value	
Stadium	Psyleron	-0.01	-0.22	0.21	-0.06	0.950	0.07	-0.14	0.28	0.67	0.507	84
	Random Str.	0.31	0.11	0.48	3.12	0.002*	-0.10	-0.30	0.10	-1.00	0.319	94
Remote Locations	Psyleron	0.01	-0.23	0.25			0.11	-0.13	0.34			67
	Random Str.	0.00	-0.26	0.26			0.03	-0.23	0.29			57
MUSASHI-MURAYAMA	Psyleron	0.08	-0.17	0.32			0.26	0.02	0.48			64
	Random Str.	-0.02	-0.25	0.21			-0.05	-0.27	0.19			73
TSUKUBA	Psyleron	-0.01	-0.34	0.33			-0.02	0.31	-0.36			35
	Random Str.	0.12	0.41	-0.18			-0.03	-0.32	0.28			43
TOKOROZAWA	Psyleron	-0.01	-0.34	0.33			-0.02	0.31	-0.36			35
	Random Str.	0.12	0.41	-0.18			-0.03	-0.32	0.28			43
Combined	Psyleron	0.02	-0.11	0.14			0.12	0.00	0.24			250
	Random Str.	0.13	0.24	0.01			-0.05	-0.17	0.07			267

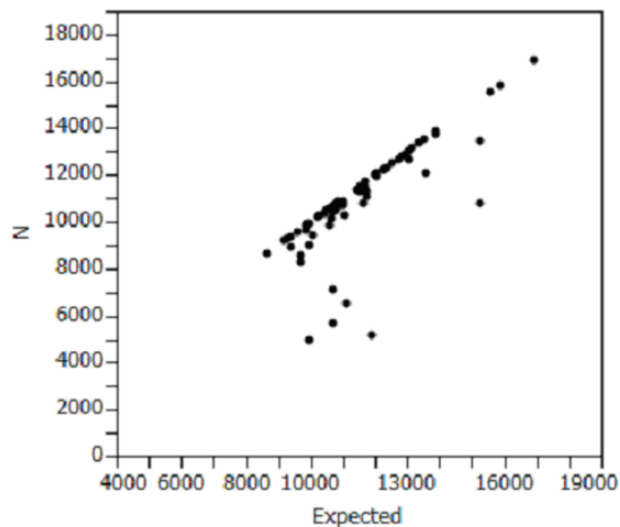
\* Significance after correcting for multiple testing ( $0.05/4 = 0.0125$ ).



that the USB port had broken just before the baseball game. As a result, the experimental condition included observations of 76 games (Random Streamer missed two and Psyleron missed three games). Using multiple RNGs, the number of RNG outputs collected at the stadium was greater than the total number of 76 games despite some missing data (Table 1). The experimental condition had a total of 178 independent RNG outputs, and the remote condition had 339 outputs from 78 games (Table 2).

However, the problem of missing data frequently occurred during games. We instructed our confederates to check the occasional blink of the Rpg102 devices to ensure that they were working so as to avoid stopping number generation due to unexpected issues. If they found that the devices had stopped during the game, the PC was restarted. The main reason for the machines to freeze seemed to be some kind of physical shocks to the RNGs. The second reason was a freeze in data processing. This is a problem related to the PCs and software application processing, and it was solved by maintenance. The third reason was delays in the games, as reported by our confederates. They reported at least two instances of long delays to the game. In addition, there were other occasions in which subtle delays occurred. One such reason was a dead battery.

As a result, the average coverage of expected trials within a game was 95.5% for the Random Streamer and 96.0% for the Psyleron REG-1 (Figure 5), excluding the two games that were completely missed at the Tokyo Dome.



**Figure 5. Expected trial numbers (sec) and actual numbers (sec) in games.** Some games failed to generate random numbers or generated fewer than expected.

### Data Processing and Analysis

To get information about audience sizes and lengths of the baseball games, we referred to the website of NPB (Japanese Professional Baseball) and the official website of the Yomiuri Giants. Average audience size was  $38,970.15 \pm 6,057.63$  for the 78 games. The average length of all the games was  $3.09 \text{ hours} \pm 0.40 \text{ SD}$  ( $11,141.8 \text{ sec} \pm 1445.0 \text{ SD}$ ). The Video Research Corporation provided information on ground-wave TV ratings during baseball games in the Kanto area from 2010 to 2011. This rating information was regarded as a lower limit of ratings, as the rating of satellite broadcast, cable TV, and radio listeners was not counted. For the recorded TV ratings, we used 23 games in our analysis. As some games were broadcast separately because of interruptions by other programs (short news broadcast, etc.), we integrated the two separate ratings into a whole rating for a game using the weighted mean. The average of TV ratings was  $7.07\% \pm 2.32 \text{ SD}$ .

Distances between the baseball stadium and remote client PCs were calculated by latitude and longitude using Geocoding API. These distances for machines at Tokorozawa were 33.1, 2.71, 59.3, and 34.3 km from Tokyo Dome, Seibu Dom, Marin Stadium, and Jingu, respectively. Those for Tsukuba were 53.9, 72.3, 49.8, and 58.5 km, and distances for Musashi-Murayama were 31.7, 3.62, 57.7, and 32.1 km, respectively. The average distance of machines operating under the remote condition was  $40.88 \pm 11.40 \text{ SD km}$  (total  $N = 339$  in the remote condition).

### Statistical Calculations

The RNG devices produce bits (1s or 0s) during real-time processing. All the outputs by the RNGs were converted into  $z$ -scores. When a RNG generated  $X$  bits per trial, where  $X$  is counted obtaining 1s,  $X$  was approximately binomially distributed, and standardized  $z$ -scores could be calculated from  $X$ . The  $z$ -score based on chance was calculated as follows:

$$Z_{\text{raw}} = (X - n\pi) / \sqrt{n\pi(1-\pi)} = (X - 256) / \sqrt{128}, \quad (1)$$

where  $\pi$  was 0.5, the probability of obtaining 1s,  $n$  was the total number of bits per second generated by the RNG, and  $X$  was the sum of 512 bits in a second.

Using the  $Z_{\text{raw}}$  scores from the time of game start to the end, time-accumulated chi-squared statistics were available, and standardized scores for each game (CZ for short) were calculated by

$$CZ_{\text{cumulative}} = \sum_{t=\text{start}}^{\text{end}} (z_{\text{raw}}^2 - 1) / \sqrt{2T}, \quad (2)$$

where  $T$  is game length (the number of trials; mean = 11,141.82 sec  $\pm$  1,445.0 SD). With these statistics, total  $CZ$  through all the games was:

$$\text{Total } CZ = \sum CZ_{\text{cumulative}} / \sqrt{G}, \quad (3)$$

where  $G$  means number of games observed. Additionally, Stouffer's  $z$ -scores ( $SZ$  for short) were calculated by:

$$\text{Stouffers } Z = \sum_{t=\text{start}}^{\text{end}} z_{\text{raw}}^2 / \sqrt{T}, \quad (4)$$

$$\text{Total } SZ = \sum \text{Stouffers } Z / \sqrt{G}. \quad (5)$$

All of the tests were two-tailed.

### Correlation Coefficients as Effect Size

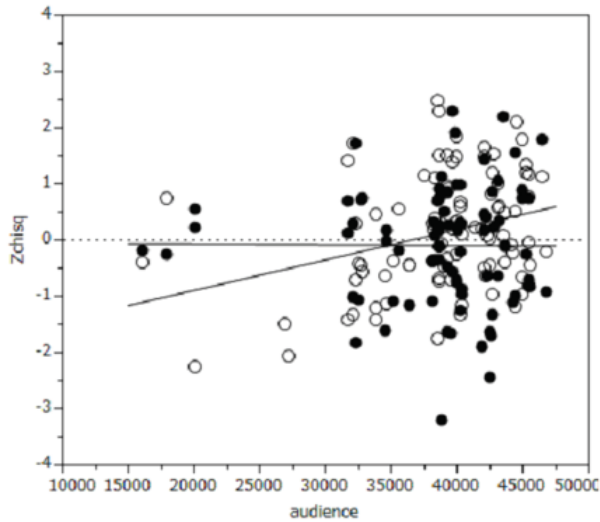
To examine our hypothesis, correlation coefficients were calculated under each condition and used as an indicator of effect size. Small, medium, and large effect sizes were  $r = 0.10$ ,  $0.30$ , and  $0.50$ , respectively. These values corresponded to  $d = 0.20$ ,  $0.50$ , and  $0.80$  (Cohen 1988). For the correlation coefficients,  $t$ -scores were calculated with  $G - 2$  degrees of freedom.

## Results

Unexpectedly, the total  $CZ$  and  $SZ$  scores during baseball games did not show significant biases for Random Streamer ( $N = 94$ , Total  $CZ = 1.33$ , Total  $SZ = .40$ ) or Psyleron ( $N = 84$ , Total  $CZ = -.80$ , Total  $SZ = .35$ ) devices. Statistics for both machines combined were also not significant ( $N = 178$ , Total  $CZ = .41$ , Total  $SZ = .53$ ).

### Audience Size Effects

Table 2 displays the outputs of the Random Streamers at the stadium, which showed medium-sized effects of audience size on  $CZ$  ( $r = .31$ ,  $N = 94$ ). As the  $z$ -scores of two devices and two kinds of statistics ( $CZ$  and  $SZ$ ) were tested simultaneously, a corrected significance level in the multiple tests was  $\alpha' = \alpha/4 = 0.05/4 = 0.0125$ . The correlation coefficient  $r = .31$  was significant ( $t(92) = 3.12$ ,  $p = 0.002$ ). No effects were found for the Psyleron  $CZ$  ( $r = -.01$ ,  $N = 84$ ;  $t(82) = -0.06$ , n.s.). Figure 6 shows scatterplots and regression lines for the devices. Stouffer's  $Z$  ( $SZ$ ) showed no significant results.



**Figure 6. The effects of audience size on the z-scores of the accumulated chi-squares of outputs from RNGs.**

Circles are CZ, cumulative chi-squared statistics per game, standardized into z-scores. White circles were those of the Random Streamer. Black circles were those of Psyleron. A dotted line indicates chance level. A regression line shows a significant slope for the Random Streamer data, whereas the one for Psyleron was not significant.

Table 3 shows the correlations between TV ratings and detectable differences between the devices. Corrected significance level was  $\alpha' = 0.05/4 = 0.0125$ . Stouffer's  $Z$  for the Psyleron showed negative correlation coefficients for the combined location (stadium and three remote locations) ( $r = -.22$ ,  $N = 82$ ), despite the non-significant level ( $t(80) = 2.06$ ,  $p = 0.04 > 0.0125$ ).

### Multiple-Devices Measurements

The current field RNG experiment tested the validity of multiple RNG measurements. Sometimes our field experiment used dual PCs simultaneously, although usually two different kinds of RNG devices were used with one PC. Therefore, correlation coefficients were calculated for these three conditions: (1) different devices with a single PC, (2) the same kind of RNG device used with different PCs, and (3) different devices used with different PCs. Table 4 displays these correlation coefficients. As we were not interested in the PC difference, we tested the above three conditions/ RNG device differences. The total number of tests was 14 (see Table 4). The corrected significance level was  $\alpha' = 0.05/14 = 0.0036$ . The results showed significant negative correlations between identical RNG

devices using different PCs ( $r = -.54$ ,  $t(33) = 03.66$ ,  $p = 0.0009$ ). This does not suggest the advantages of multiple measurements that we had assumed before the experiment. A positive correlation was only found for different kinds of RNGs with a single PC, although it was not significant when multiple testing ( $r = .21$ ,  $N = 81$ ).

**TABLE 3**  
**Estimated Pearson's Correlation Coefficients ( $r$ ) for the TV Audience Ratings**

Location	RNG	CZ					STOUFFER'S Z (SZ)					N
		Estimation	95% CI Lower	95% CI Upper	t-Score	p-Value	Estimation	95% CI Lower	95% CI Upper	t-Score	p-Value	
Stadium	Psyleron	-0.15	-0.50	0.26			-0.26	-0.58	0.15			26
	Random Str.	-0.16	-0.50	0.22			0.27	-0.10	0.58			29
<b>Remote Locations</b>												
MUSASHI-MURAYAMA	Psyleron	0.22	-0.21	0.58			-0.38	-0.69	0.03			23
	Random Str.	-0.60	-0.86	-0.09			0.00	-0.53	0.53			14
TSUKUBA	Psyleron	-0.11	-0.51	0.32			-0.01	-0.43	0.41			22
	Random Str.	0.08	-0.30	0.44			-0.07	-0.43	0.32			28
TOKOROZAWA	Psyleron	0.07	-0.55	0.64			-0.30	-0.76	0.37			11
	Random Str.	-0.12	-0.67	0.52			-0.49	-0.84	0.16			11
Combined	Psyleron	-0.01	-0.22	0.21	-0.07	0.947	-0.22	-0.42	-0.01	-2.06	0.042	82
	Random Str.	-0.13	0.09	-0.34	-1.20	0.233	0.01	-0.20	0.23	0.13	0.898	82

**TABLE 4**  
**Estimated Pearson's Correlation Coefficients ( $r$ ) for Each Device Combination**

Devices		CZ					STOUFFER'S Z (SZ)					N
		Estimation	95% CI Lower	95% CI Upper	t-Score	p-Value	Estimation	95% CI Lower	95% CI Upper	t-Score	p-Value	
<b>TOTAL WITHIN PC</b>		0.21	-0.01	0.41	1.90	0.061	0.02	-0.20	0.24	0.22	0.830	81
	First	0.12	-0.19	0.41			-0.16	-0.44	0.16			42
	Second	0.38	0.07	0.62			0.24	-0.08	0.52			39
<b>Different Devices within PC</b>	Single PC	0.19	-0.10	0.46	1.31	0.198	0.03	0.31	0.87	0.17	0.865	46
	First	0.13	-0.28	0.50			-0.30	-0.62	0.11			25
(PSYLERON AND RANDOM STREAMER)	Second	0.30	-0.15	0.65			0.35	-0.09	0.68			21
	Dual PC	0.26	-0.08	0.55	1.58	0.124	0.02	-0.31	0.35	0.13	0.897	35
	First	0.23	-0.28	0.64			0.09	-0.41	0.55			17
	Second	0.45	-0.02	0.76			0.01	-0.46	0.48			18
<b>BETWEEN 2 PCs</b>												
<b>Same Devices</b>		-0.54	-0.74	-0.25	-3.66	0.001*	-0.16	-0.47	0.18	-0.93	0.357	35
	Psyleron	-0.55	-0.82	-0.10	-2.57	0.021	-0.39	-0.73	0.11	-1.63	0.124	17
	Random Str.	-0.54	-0.80	-0.09	-2.55	0.021	0.03	-0.44	0.49	0.12	0.906	18
<b>Different Devices</b>		-0.17	-0.48	0.17	-0.99	0.328	-0.19	-0.49	0.15	-1.11	0.276	35
	Pair 1	-0.55	-0.84	-0.03			0.09	-0.46	0.59			14
	Pair 2	0.02	-0.41	0.45			-0.38	-0.70	0.06			21

\* Significance after correcting for multiple testing ( $0.05/14 = 0.0035$ ).

Pair 1: First Random Streamer and second Psyleron. Pair 2: First Psyleron and second Random Streamer.

## Discussion

The current study conducted field RNG experiments repeatedly at baseball stadiums. Although our initial hypotheses were partially supported by our results, we found several unexpected results.

### **Audience Effects**

As expected, our results revealed that audience size had positive effects on the RNG outputs in *CZ* (cumulative chi-squares statistics) and no effects on the remote RNGs, which suggested some dependency of audience size effects on distance. When an RNG was run near the audience, the more signal sources there were, the more chances the RNGs had to receive signals.

This result essentially resembled that from a previous field RNG experiment at a movie theater using Rpg102 (Shimizu & Ishikawa 2010). However, the present audience size effect was quite small in comparison with that in the previous study, in which the number of people ranged from 19 to 70. It was suggested that *CZ* would be associated with density of audience at the experimental location, average distance of each person from the RNG, or strength of emotion evoked during the event.

In contrast, TV audience rates showed no effects on total RNG outputs, suggesting that the huge audience size did not make up for the decrease in distance. A larger size would be required to have sufficient influence on the RNG outputs. However, with respect to Stouffer's *z*-scores with the Psyleron, despite the small number of samples, it is possible that this would be significant if sample size were increased in the future. Some confirmation is required in the future using *SZ* scores from Psyleron outputs.

Unaccountably, qualitative differences between types of device in detecting audience size effects seem to exist. The Random Streamer was sensitive to audience size, whereas the Psyleron could detect TV ratings, suggesting that random bit sequences are physically dependent on the RNG bit-generation method. As mentioned above, the Psyleron is based on FET, whereas the Random Streamer uses thermal noise as a source.

Given that information on the audience was taken independently from the outputs of the RNGs, these significant correlations suggest that RNG could surely detect field consciousness. This is not a matter concerning the quality of the RNG outputs, but rather anomalistic phenomena caused by field consciousness.

### **Multiple RNG Measurements**

In the current field experiment, random numbers were generated with multiple devices simultaneously. If these multiple devices had been affected

by the experimental location equally, a positive correlation would be expected.

The results shown in Table 4 suggested that *CZ* rather than *SZ* was totally sensitive to field consciousness. It was also shown that different kinds of devices used with an identical PC have positive correlation coefficients (Table 4), which supports our prediction. This may result from the identical PC, which had the same location, CPU clock, and inner temperature. Some might indicate a possibility of spurious correlation between *CZ* and the audience size, both of which would be influenced by one of these environmental parameters. For instance, temperature might have positively increased them simultaneously. Of course, this seems improbable since RNG devices aren't influenced by such external parameters.

Several unexpected results were found. First, the chi-squared statistics for both devices showed a negative correlation between different machines that were the same kind of RNG (Table 4). The value of  $r = .50$  corresponds to a large effect size of  $d = .80$  (Cohen 1988). Importantly, when the signals became strong, the signals were not distributed equally among devices, but rather output from one machine canceled that from another, at least during a three-hour baseball game. Thus, a new issue to explore is how (and why) such canceling-out phenomena occur over such a long time span. Second, different devices operating with different machines also showed moderate negative correlations in *CZ* scores. These results suggest that multiple devices were not simply additive, but rather acted to cancel out each other. To make it clear which factors have positive and negative effects on the multiple devices, ideally we should have observed the behavior of the same kind of devices with an identical PC, although currently the two kinds of hardware devices could not run together in such a way. This becomes one of our future tasks.

### **Some Features of Baseball Games**

It was somewhat unexpected that neither the total *CZ* nor the total *SZ* showed significant results in the overall analysis. Possibly, the canceling effects between devices caused the absence of biases. However, this does not indicate the failure to detect field consciousness in the experiment for several reasons. First, this outcome is the same as the GCP results for football games in the World Cup championship in 2010, or American football games (Nelson et al. 1998, Radin 1997), which did not show significant results.

A possible reason is that sports events often evoke opposite emotions among audiences. Thus, baseball games might be an inappropriate venue to find biases in RNG outputs compared with something like movies, for which significant results have been found (Shimizu & Ishikawa 2010,



Shimizu & Ishikawa 2011). Similar to baseball or football games, it seems that audiences would not have a common empathetic mindset in horse races, car races, and combat sports, etc., because all of these are competitive. However, in contrast, shared emotion for a national team could construct high homogeneity.

Another reason for this failure to find biases is that these sporting events are not always exciting, which would cause no biases. Some games might drag out without scoring. Ishikawa (2004) has suggested in his post hoc analysis that the period of the inning in which scores were recorded could bias outputs of the RNG. The current study considered the period of one game in its entirety, which is about three hours, as a sample in the analyses. However, these wide ranges could have a variety of different effects on field consciousness. This means that, from the viewpoint of signal detection, signals have low homogeneity. Therefore, it would be an important future task to divide and elaborate innings according to some other conditions.

However, this raises another problem. It should be considered that the fewer accumulated samples one gathers due to limitations of range, the lower the power and reliability of the measurement become. Since the outputs of RNG are noise itself, after the sample is divided, the signal-to-noise ratio in the RNG outputs becomes low, which causes decreased power of analysis and reduced reliability in the measurements. The issue is somewhat of a tradeoff, as we have to change the window size to take in the accumulation of bits from RNG outputs. In further studies, we must increase the total sample by further repetitions of the field experiment.

### Note

- <sup>1</sup> Catalog or test results of Random Streamer IC (RPG100) are on the website of the FDK Corporation. In 2012, Rpg105 and Rpg102 were not produced or for sale.

<http://www.fdk.co.jp/whatsnew-e/release050930-e.html>

[http://www.fdk.co.jp/cyber-e/pi\\_ic\\_rpg100.htm](http://www.fdk.co.jp/cyber-e/pi_ic_rpg100.htm)

Random Streamer1M (Rpg105) and 24M(Rpg107): <http://www.fdk.co.jp/whatsnew-j/release060608-j.html> [in Japanese]

Rpg102: <http://www.fdk.co.jp/whatsnew-j/release041005-j.html> [in Japanese]

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## RESEARCH ARTICLE

### **Pranic Healing: Documenting Use, Expectations, and Perceived Benefits of a Little-Known Therapy in the United States**

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**Abstract**—The aim of this exploratory study was to examine client demographics and expectations, reasons for use, sensations during treatment, and perceived outcomes of Pranic Healing, an energy healing system lacking in scientific documentation but whose use in the general population is becoming more widespread internationally. This study consisted of a cross-sectional survey of adults (18+ years of age) receiving care from 12 Pranic Healing practices in four different states in the U.S. ( $N = 179$ ) completing online questionnaires. Closed-ended response sets were analyzed descriptively, while qualitative data were analyzed using content analysis. Reasons to use the therapy included physical, mental/emotional, and metaphysical issues, dissatisfaction with conventional care, and overall well-being. Expectations of care included enhanced abilities, cure or relief, or unsure. Respondents were more likely to cite a specific problem from which they needed alleviation than to cite overall health or wellness as their goal. Sensations experienced were reported to be of a relaxing nature and not, as was hypothesized, of an energizing nature. Results show that those who use Pranic Healing fit the sociodemographic profile of CAM (complementary and alternative medicine) users in the United States, that their reasons for use are not homogeneous, and that preliminary descriptive data from a selective subsample of respondents indicated that some respondents are experiencing positive outcomes attributed to this modality.

## Introduction

As defined by the National Center for Complementary and Alternative Medicine (NCCAM 2011a), energy-based medicine is a particular type of CAM (complementary and alternative medicine) that uses energy, both veritable and putative, to heal. Those therapies that use putative, or biofield, energy reflect the concept that every human being is infused with subtle energy—*qi*, *prana*—or a universal life force. Regardless of what it is called, the concept is still the same: Human beings have energetic as well as physical dimensions, and the health of one can be reflected in the other. Energy-based therapies attempt to manipulate this energetic dimension in order to affect the health of the individual (Forgues 2009).

Despite the fact that meta-analyses of energy-based therapy studies have found mixed results in terms of efficacy (Jonas & Crawford 2003, Wardell & Weymouth 2004, Vitale 2007, Jain & Mills 2010), energy-based therapies show increasingly widespread use among clients both in the U.S. and abroad (Barnes, Powell-Griner, McFann, & Nahin 2002, Pud, Kaner, Morag, Ben-Ami, & Yaffe 2005, Molassiotis et al. 2005, Jain & Mills 2010). Reflecting this greater demand for energy-based therapies, there has been an increased number of Western medical institutions that have begun to integrate forms of energy medicine into their conventional care (Miles & True 2003, Van der Riet 2011). As a result, medical training institutions have had to include such training in their offerings. For example, it has been reported that an increased number of nursing programs internationally now offer some form of energy-based therapies as part of their curriculum (Engebretson & Wind 2002).

As use of various types of energy medicine has spread, there has also been a global shift toward the creation of policies to govern the use of energy medicine and other CAM in individual nations. In 2001, the World Health Organization (WHO) conducted a global survey of national policies on CAM and found that more than half of the nations surveyed were in the process of establishing such policies (WHO 2001). In 2008, the WHO Congress on Traditional Medicine adopted the “Beijing Declaration,” calling for member states to integrate traditional and alternative medicine into national health systems (WHO 2011). One of the greatest obstacles found by WHO to establishing policies on CAM and meeting the goal of integrating CAM into national health systems is a lack of knowledge about many therapies that are currently being used worldwide (WHO 2001), particularly those that have healing mechanisms that are not easily understood or measured, or have been found to have mixed results in the scientific literature, such as energy-based medicine.

Even here in the United States there is an acknowledgment that we need a greater understanding of “‘real world’ patterns and outcomes of CAM use and its integration into health care and health promotion,” as voiced in NCCAM’s strategic objectives (NCCAM 2011b). In addition, NCCAM has stated as part of its strategic objectives that there is a need for “descriptive information examining the frequency of and reasons for CAM use in disease and symptom treatment and in promoting improved health and well-being” (NCCAM 2011b).

The current descriptive study attempts to explore client expectations, frequency and reasons for use, and perceived health-related benefits of Pranic Healing, a lesser-known energy healing modality that lacks scientific documentation but whose use is becoming more widespread internationally. It is hoped that the results of this study can have wider relevance for energy-based therapies.

### **Pranic Healing**

In the scientific literature, Pranic Healing has been alternately used as an example of a lesser-known culturally embedded ethnomedicine that can be used for counseling in traumatic situations such as natural disasters (Shah 2007); an example of an indigenous approach that focuses on the connectedness of the body, spirit, and mind, and on bringing about and maintaining a balance in the flow of energy that has implications for other types of counseling (Yeh, Hunter, Madan-Bahel, Chiang, & Arora 2001); an example of an alternative medicine with a spiritual component that makes it popular for palliative care in certain societies (Chaturvedi 2007); and an example of a type of religious movement with healing components (Beckford & Suzara 1994).

Although Pranic Healing is considered to be part of the “Yoga Vidya System of Knowledge” in India, most modern practitioners of Pranic Healing use a version of Pranic Healing that was formalized in the Philippines and includes elements from Chinese Traditional Medicine. According to Master Chang Kok Sui (Sui 2004), who formalized this modern version of Pranic Healing, Pranic Healers work with the layers of the subtle energy field that surrounds the physical body. Based on the premise that the biofield is a mold or template the physical body follows, the Pranic Healing practitioner begins to scan the biofield with his/her hands for areas of congestion or depletion of subtle energy that correspond to the presenting problem (whether physical or psychological). After having located and identified areas of concern, the practitioner sweeps the biofield with his/her hands in order to clean out the congestion, redistribute prana, and seal any holes in the field. In addition, the affected areas of specific health concerns are

treated locally via specific manual cleansing movements. Once the biofield is completely cleansed as determined by rescanning, the final stage begins which involves energizing the affected areas with fresh *prana* that is drawn from the atmosphere and projected on to impacted areas.

According to the Institute for the Inner Sciences, the primary institution associated with the organization and training of Pranic Healing practitioners, Pranic Healing is currently practiced in 49 different countries across six continents (The Inner Sciences 2011). In some countries, Pranic Healing is not only practiced but sanctioned by the government. According to Balasubramanian (2010), Pranic Healing is one of the alternative modalities that is currently practiced in India, and legally sanctioned in the sense of being recognized, regulated, and approved by the government.

India is not the only country to sanction the use of Pranic Healing. In a discussion of the normative, axiological, and ethical debates that surround the issues of the traditional and alternative medicine act and informal health economy in the Philippines, Lee Mendoza includes Pranic Healing in a list of healing modalities that are specifically endorsed by the Philippine Institute of Traditional and Alternative Health Care (Mendoza 2009). In Sri Lanka (Broom, Wijewardena, Sibbritt, Adams, & Nayar 2010), its legal use was framed as a public health issue when researchers found it among the types of TCAM (traditional, complementary, and alternative medicine) used by cancer clients before consulting with their doctors. Despite these reports of its growing use internationally, published scientific information on Pranic Healing is extremely limited.

In reviewing the scientific literature, very few studies were found that documented the specifics of use, efficacy, or effectiveness of Pranic Healing. A comprehensive search for published scientific articles on Pranic Healing was conducted using the following specific databases: PUBMED, PSYCINFO, Social Sciences Full Text, Sociological Abstracts, Evidence-Based Medicine Reviews, JSTOR, and CINHALL. In addition, the following meta-databases were used: WEB of SCIENCE, Academic Search Complete, and ScienceDirect. All databases were searched for the keywords “Pranic” and “Pranic Healing.” Results were not limited by publication date, methodology type, or language. The only requirement was that the study be published in a peer-reviewed journal.

The following databases yielded no results: Academic Search Complete, Sociological Abstracts, and Social Sciences Full Text. Among the other databases, not counting duplicates or abstracts from conference reports, a total of eleven articles was found. Of these eleven articles, only three



reported on scientific studies that directly studied Pranic Healing. Reference sections of eligible studies and other review papers were searched for additional studies, resulting in one more study for a total of four studies.

Among these four studies, the first was a case study of the effects of Pranic Healing on a breast cancer client (Tsuchiya & Motoyama 2007). The researchers attempted to measure changes in electrodermal conduction at specific acupuncture points on the client during four separate sessions of Pranic Healing. The researchers found positive evidence for changes in conduction for this particular client, including changes in directions and levels of energy according to the intention of the Pranic Healer.

The second study (Vrunda, Sundaram, Jaisri, & Das 2002) was a longitudinal study conducted in Bangalore on the effects of Pranic Healing on behavioral problems in juvenile females. The researchers found a positive effect after three months of Pranic Healing sessions in the reduction of overall behavioral problems in 22 juvenile females who had been committed by the courts to juvenile homes because of violence, but concluded that further research is needed to clarify the relationship.

The third study (Jain, Nagarathna, Nagendra, & Telles 1999) was a single-blind control study on the effects of Pranic Healing on musculoskeletal pain using 50 clients with chronic non-malignant continuous muscle pain of more than six months randomized into two groups. Researchers compared the immediate effect of Pranic Healing on chronic musculoskeletal pain with a placebo session of random hand movements over a two-day period. Researchers concluded that Pranic Healing when performed for 25 minutes in the standardized method by a trained healer is effective in reducing continuous chronic pain of musculoskeletal origin, as compared to placebo random hand movements for the same length of time.

Only a single study of utilizers or “adherents of Pranic Healing” was found in the literature (Beckford & Suzara 1994). The study, which examined Pranic Healing as a form of religious movement in the Philippines, was a qualitative study based on participant observation and interviews of 62 individuals who practiced Pranic Healing. Researchers found that contrary to many theoretical expectations, Pranic Healing had attracted relatively wealthy and well-educated followers who aspired to integrate their spiritual and therapeutic interests into their working lives as professionals or business people.

To our knowledge, there is currently no published scientific documentation of client expectations of, perceived experience of, reasons for, and perceived outcomes of Pranic Healing as a form of energy-based medicine.

## **Materials and Methods**

### ***Study Design***

This was an exploratory and descriptive study, using both qualitative and quantitative data from a survey of 179 Pranic Healing clients. Given that so little has been published on this modality, a pre-pilot focus group of practitioners of Pranic Healing was conducted for exploratory purposes in order to inform the development of later stages of the study. The purpose of the focus group was to collect information that would allow the researchers to contextualize the survey questions in the everyday reported experiences of the respondents. Among data collected were basic descriptions of typical healing sessions, common reports or reactions from clients during and following healing sessions, typical number of new clients seen per month, and use of other healing modalities during Pranic sessions. This information was used to create a survey questionnaire that would later be given to Pranic Healing clients and to inform the sampling protocols.

### ***Sampling Procedure***

There is no formal requirement for Pranic Healers to document their treatments and no informal source that defines the population of users of Pranic Healing in the United States. For this reason, study participants were recruited through Pranic Healing practitioners. Twelve certified Pranic Healers from four states (Oregon, Washington, California, and Florida) were chosen to participate in the study based on length of experience as healers, education, gender, background, and ability to generate a large enough number of new clients. Variation in the backgrounds of the practitioners was used to assure some variability in the type of client that each healer attracted.

Practitioners identified new clients eligible to participate in the study during a nine-month recruitment period. Eligibility consisted of: 1) age 18 years and older, 2) willingness to participate in the study, and 3) expected participation in more than one Pranic Healing session. Clients were informed of the study verbally by the practitioner before the healing session. If they were interested, they were given access to a dedicated computer in the practitioner's office that allowed them to go online at intake before their first healing session, read up on the study details, and read and sign the informed consent. After consenting to the study, clients were then directed to the online survey page, where they filled out an initial survey that took about 25 minutes to complete. Neither the clients nor the therapists received any financial remuneration for participating. The participant response rate was 89%, resulting in 179 completed baseline surveys.

### **Questionnaire**

The questionnaire was developed using questions adapted from validated instruments (Ryff & Singer 2006, Kahneman, Diener, & Schwarz 1999, Diener, Wirtz, Biswas-Diener, Tov, Kim-Prieto, Choi, & Oishi 2009, Keyes & Lopez 2002, Keyes 1998, Kopp et al. 2010) and questions created by the researchers based on focus group data. The questionnaire was pilot-tested for clarity, comprehension, and length on a sample of eight Pranic healers who were themselves users of this therapy, and then revised according to pilot findings. The first part of the questionnaire included questions on sociodemographic variables including age, ethnicity, marital status, education, occupation, and income. Also included in the sociodemographic section were questions on religious attendance and spiritual values. These questions were answered using fixed-response alternatives, usually in combination with one open alternative to be used if none of the given answer choices was suitable. Part two included questions on physical and mental/emotional health and ailments, use of types of alternative or complementary medicine, conventional medicine, medications and supplements, reasons for use, and use of various lifestyle practices for health-related reasons. Part three consisted of a combination of open-ended questions and fixed responses with an option to fill in alternatives if none of the given answer choices were suitable. This section covered use of and familiarity with Pranic Healing, reasons for use of Pranic Healing, expectations of Pranic Healing, and, among those who already used Pranic Healing, regularity of use, and sensations felt during Pranic Healing sessions. The open-ended questions were created to allow for a better understanding of Pranic Healing use within the context of the motivations and actual experiences of the average client.

### **Data Analysis**

Questions covering the following variables were analyzed descriptively: sociodemographics, physical and mental/emotional health, ailment type and number, use of CAM and conventional medicine, use of lifestyle practices for health-related reasons, familiarity with Pranic Healing, and regularity of use. Analysis for the following variables was based on questions with fixed-response and associated open-ended questions that required the respondent to elaborate on the fixed response: use of Pranic Healing practices outside of the practitioner setting, and sensations felt during Pranic Healing sessions.

Open-ended responses to the following questions on expectations for use and reasons for use were analyzed using conventional content analysis methods as described by Hsieh and Shannon (2005). Two members of

the research team independently read through transcripts of open-ended responses and created initial codes for all responses for these two variables, highlighting exact words from the responses that appear to capture key concepts. Codes were then sorted by each research team member into categories and subcategories based on how different codes were related. Next, the two research members compared their initial categories and subcategories, revising these until full agreement was reached, and a final list of categories and subcategories was created that captured the full range of responses. For these final categories, definitions for each and associated themes were chosen, along with exemplars from the text that supported each theme.

In order to examine how closely given responses for this study match current theoretical expectations for CAM outcomes, directed content analysis as described by Mayring (2000) was used to create general categories of expected outcomes. Directed content analysis uses existing theory to devise initial coding categories and their operational definitions (Potter & Levine-Donnerstein 1999). Data that cannot be immediately coded are analyzed later to determine if they represent a new category or a subcategory of an existing code. These findings can then be used as supporting or non-supporting evidence for, or to extend, current theory.

Responses were categorized using coding categories and operational definitions based on Schuster, Dobson, Jauregui, and Blanks' (2004) theoretical model of wellness outcomes for CAM, which is based on the understanding that certain concepts are common to most CAM modalities, including "high-level wellness," "the interpenetration of mind, body, and spirit," holism/individualism, self-healing, vitalism, the body as a bioenergetic system, and a focus on the natural/ecologic context (Goldstein 2000). This model proposes that health includes multiple domains, among them physical, psychological (mental, intellectual, emotional), social, and spiritual. Wellness is thus conceptualized as "a higher-order construct integrating these domains, drawing on individual self-perception" (Schuster, Dobson, Jauregui, & Blanks 2004).

## **Results**

### ***Patient Demographics***

Respondents ranged in age from 20 to 96, with a mean age of 49.5; 70.4% were female and 84.3% were white/Caucasian. Among respondents, 51.7 % were married. Fifty-nine percent were college graduates or had postgraduate education, and 48% described their main work activity as professional, technical (computer programming, engineering, etc.), or white collar, while

only 10.6% described their work activity as blue collar. More than half (57.4%) reported a household income of more than \$50,000 per year, and 32.3% reported a household income of more than \$75,000 per year. These characteristics make this sample of Pranic Healing users above average in terms of socioeconomic status (the median household income in the United States in 2009 was \$50,221, and those attaining a college education to the level of a bachelor's degree were 27.5% of the population) (U.S. Census Bureau 2011).

Given that Pranic healing had been described conversely as both a form of alternative medicine with a spiritual component (Chaturvedi 2007), and an example of a type of religious movement with healing components (Beckford & Suzara 1994), we asked respondents if they attended "church, synagogue, or temple regularly," and "How important is spirituality, religious beliefs, or metaphysical beliefs in your life?" The second question was answered on a four-point scale with answers ranging from "not at all important," "not very important" to "somewhat important," and "very important." Seventy-four percent of respondents responded that they did not attend church, synagogue, or temple regularly, but more than half (57.3%) responded that spirituality, religious beliefs, or metaphysical beliefs were "very" important in their lives. Another 29.2% responded that these beliefs were "somewhat" important to them.

### **Health Status**

Both physical and mental/emotional health were rated on a five-point scale ranging from "excellent" to "poor" using the following question: "Would you say that your (Insert) health at the present time is . . ." The question was asked twice, once for physical health and once for mental/emotional. Overall, respondents felt that their physical health was good, with 32% rating their physical health as "very good" or "excellent," 36% as "good," and only 18% as "fair," and 14% as "poor." Although respondents were just as likely to rate their mental/emotional health as good (31% "very good" or "excellent," 25.7% as "good"), a larger percentage of respondents were likely to rate their mental/emotional health as "fair" (28.5%) and "poor" (15.1%) than they were their physical health. The most common ailments were emotional disorders (30%), back pain (22%), neck pain (18%), stress (43.6%), and weight control issues (16.6%). Roughly 13.3% had a chronic condition.

### **CAM Use**

Among respondents, the most popular CAM practices used to treat ailments were: chiropractic (39.8%), energy healing (excluding Pranic)

(34.3%), acupuncture/OM (23%), massage (28.2%), homeopathy (18.8%), relaxation/meditation (16%), spiritual healing/prayer (15.5%), naturopathy (13.8%), and folk remedies/traditional healing (11.6%). The most popular CAM practices other than Pranic Healing used to promote health were: relaxation/meditation (25.4%), energy healing (excluding Pranic) (25.4%), spiritual healing/prayer (23.8%), fitness training (22.7%), massage (17%), chiropractic (14.4%), and acupuncture/OM (oriental medicine) (12.7%). Respondents used an average of 2.65 CAM therapies, not including Pranic Healing, to treat ailments, and 2.21 CAM therapies, not including Pranic Healing, to promote health.

### **Conventional Care**

Fifty-five percent of respondents admitted going to MDs or other conventional healthcare providers to treat physical ailments, and 19.9% used conventional care in the form of visits to their doctor to promote health. Twenty-one percent went to conventional mental health providers to treat mental or emotional ailments, and 13.8% went to conventional mental health providers, such as psychologists or psychiatrists, to promote health. Forty-four percent were currently taking prescription medications to treat ailments, and 23.2% were taking over-the-counter medications for the same reason. Thirty-one percent were taking multi-vitamins/minerals to promote health.

### **Pranic Healing Use**

Approximately half of the respondents (48.6%) had used Pranic Healing before this first visit to this new office. Among those who had used it before, the average length of time using Pranic Healing was 14.3 months (ranging from 1 month to 10 years). When asked how often they use Pranic Healing, more than one-third (36.7%) responded that they use it “only when I have pain or discomfort.”

### **Reasons for Using Pranic Healing**

One out of every four respondents reported more than one reason for using Pranic Healing. All of the reasons given by the respondents were reduced to six general categories: *physical issues*, *mental/emotional issues*, *social issues*, *metaphysical/spiritual issues*, *seeking alternatives to current care*, and *overall well-being*.

The first category, *physical issues* included both physical problems and enhancement/maintenance of physical state. Physical problems included both physical ailments that prevented functioning as well as specific diseases.

These included the subcategories of “physical pain” (25.7%), “specific disease” (26.9%), “decreased physical functioning” (9%), and “physical trauma” (2.9%) such as surgery or an injury. The following quotations from the respondent’s reports exemplify this category.

- ... Lyme disease, encephalopathy, and neuropathy ...
- ... severe sinus allergies related to environmental chemicals ...
- ... my left arm is steadily becoming weaker and the pain is increasing ...
- ... to reduce pain, heal my osteoporosis, gain energy, and help with weight loss ...
- ... Symptoms from a prior auto accident have returned; right arm pain and limited range of motion ...

The following are examples of the subcategory “physical enhancement or maintenance” (2.9%)

- ... facelift, wrinkle reduction ...
- ... increase HDL cholesterol ...

Like the category *physical issues*, the category *mental/emotional issues* included both problems and enhancements of the current healthy state. The subcategory mental/emotional problems included “stress” (18.1%); “negative affect” (18.7%) such as grief, anger, sadness, and emotional pain; “mental disorder/dysfunction” (12.87%) such as PTSD, addiction, phobias, and bipolar disorder; and “emotional trauma” such as sexual abuse and childhood traumas (2.9%). The following responses exemplify these categories.

- ... to deal with fear and pain ...
- ... I am seeking to lower my stress level at work. I consider the other “ailments” I selected to be minor ...
- ... to cope with PTSD and stress ...
- ... Ongoing (lifelong) emotional/behavioral complications due to perceived inability to handle my circumstances. Difficulty making decisions without self-depreciation and/or guilt ...
- ... anger, depression, need to stay focused on work, cannot become an emotional wreck, big job ...
- ... addiction, partner committed suicide ...

The following are examples of the subcategory “mental maintenance/enhancement” (4.1%)

- ... sharpen my mind and confidence before a big exam ...
- ... great focus ...



The third category, *social issues* (7.6%), centered upon concerns about family or finances as is exemplified in the following responses:

... need financial healing ...  
 ... difficulty in relationship ...  
 ... help with the pain of the disease and stress of living with alcoholic spouse ...

*Metaphysical/spiritual* issues (4.1%) as a category encompassed spiritual growth, purpose in life, and sense of oneness or groundedness. These are exemplified in the following responses:

... to really bring light into all areas of my life ...  
 ... enhancing spiritual practice ...  
 ... Pranic Healing is a way to harmonize with my innate oneness of being ...  
 ... Don't know where to go next in life, despite knowing I have a destiny ...

The fourth category, *seeking alternatives to current care* (4.6%), encompasses those who are dissatisfied with conventional care options, have not received relief from their condition from other forms of care, or are curious about the modality. These are exemplified by the following responses:

I am doing this to prevent further surgery and prevent the loss of my tongue. Medications have not always worked effectively. Pranic Healing was offered so I am giving it a try.  
 Been in therapy for 2 yrs and condition is not subsiding. Anxiety is getting worse and interfering with work.  
 ... No other form of treatment has helped me ...  
 ... Explore and learn more about this natural healing method ...

The final category was *overall well-being* (7.0%). These respondents referred to their overall health or overall well-being in their responses. The following are examples:

... Better health and well-being ...  
 ... to be whole well in mind body spirit ...

### ***Client Expectations***

Responses to the open-ended question "What are your expectations regarding Pranic Healing?" were coded into three categories. These categories were: *unsure*, *expectations of enhanced abilities*, *expectations of cure or relief*.

Those responses that fell into the category of *unsure* consisted of those who had never tried the modality before but were willing to try it and those

who had been referred by others (21.8%). This category is exemplified in the following responses:

- ... Completely open-ended. I have no expectations ...
- ... I heard this might help from someone at work ...
- ... I don't know ...

The second category, *expectations of enhanced abilities* (57.6%), consists of those respondents who expected Pranic Healing to either enhance their ability to cope or to accept their current adverse condition (15.3%); improve their current level of healthy functioning so that they experience enhanced physical, spiritual, or mental well-being (17.6%); or improve their body's innate ability to heal by aiding the body in healing itself or aiding in the recovery process (24.7%). The following exemplifies these subcategories.

- ... help with mental outlook to better personal situations ...
- ... to promote a healthier life living with MS. I am interested in working with the body to treat issues I am having ...
- ... improving sense of well-being ...
- ... Continued good health and well-being ...
- ... my doctor said it might help me to heal faster ...
- ... to experience a deep sense of peace and relaxation which allows my body to heal itself on whatever level is needed ...
- ... energy work is a good way to align the system which can help the body heal itself ...

Surprisingly, the third category, *expectations of cure or relief*, was the smallest (19.4%). This category included a range of responses, from those who expected Pranic Healing to cure them completely, as is exemplified in the following responses:

- ... to be completely free from chronic pain and fatigue and be able to carry a baby to full term without miscarriage ...
- ... I have faith that I will be healed ...

to those who simply want to feel better than they do at the current moment:

- ... I hope it helps so I won't have to keep taking so much medication ...
- ... To feel better ...

to those respondents for whom Pranic Healing was perceived as a last hope:

- ... healing in areas where other modalities have not been successful ...
- ... nothing is working please let it work ...

### Results of Directed Content Analysis

Responses for client expectations of Pranic Healing and reasons for use were analyzed jointly in order to create a proxy measure of patient expectations that was grounded in the clients' own experiences. Directed content analysis was used to compare these expected outcomes for respondents to current theoretical expectations of outcomes. Responses were categorized using coding categories and operational definitions based on Schuster, Dobson, Jauregui, and Blanks' (2004) theoretical model of wellness outcomes for CAM which proposes that health includes multiple domains, among them physical, psychological (mental, intellectual, emotional), social, and spiritual, and that wellness is "a higher-order construct integrating these domains, drawing on individual self-perception." Responses were coded into the following general categories: *physical well-being*, *psychological well-being*, *social well-being*, and *spiritual well-being*.

The general category of *physical well-being* (43.5%) included three subcategories: general physical state, physical functioning, and physical ailments. All of the responses that had been categorized as *physical issues* and its subcategories under "reasons for use" fell into this predetermined category.

Responses in the general category of *psychological well-being* (40.6%) fell into one of two subcategories: the theoretical model of eudaimonic well-being, which focuses on meaning and self-realization and defines well-being in terms of the degree to which a person is fully functioning (Ryff & Singer 2006); or the theoretical model of hedonic well-being, which emphasizes constructs such as happiness, positive affect, low negative affect, and satisfaction with life (Kahneman, Diener, & Schwarz 1999).

Responses in this general category of *psychological well-being* were more likely (23.9%) to be categorized as reflecting one of the six dimensions of eudaimonic well-being:

self-acceptance:	"... no more anxiety anger guilt ..."
purpose in life:	"... Give me sense of direction ..."
positive relationships:	"... high stress divorce ..."
environmental mastery:	"... mental stress, addiction ..."
autonomy:	"... get to feel better without seeing my doctor ..."
personal growth:	"... work the issues from youth ..."

rather than hedonic well-being (16.7%).

happiness:	"... be happy again ..."
positive affect:	"... emotional cleansing ..."
negative affect:	"... depression anger resentment fear sadness anxiety ..."

No responses reflected the subcategory of “satisfaction with life.” Responses that could not be coded into any of the above-mentioned subcategories were found to be related to the theoretical concepts of resilience (Keyes & Lopez 2002), which is defined as the capacity to prevail in the presence of adversity. This is exemplified by the following response: “. . . to return to my normal self and move on in my life without emotional trauma or loss of job . . .”

The *social well-being* category was based on the theoretical model of Keyes (1998), and included the domains of social acceptance, social actualization, social contribution, social coherence, and social integration. Responses in the general category of *social well-being* (8.7%) fit into only two of these five domains:

social actualization: “. . . financial healing . . .”  
 social integration: “. . . family problems . . .”

Responses which had previously been categorized as *metaphysical/spiritual* (8.4%) could be recoded into the general *spiritual well-being* category in the directed content analysis.

### **Results for Respondents Who Had Used Pranic Healing**

**Sensations During Use.** Respondents who had used Pranic Healing in the past (48%) were asked the following question: “Have you ever experienced any of the following sensations after a Pranic Healing session? (Please check all that apply)” and given a choice of 13 physical and psychological sensations commonly reported to practitioners by clients after healing sessions. The list was compiled using data collected during the practitioner focus group. Respondents were also given the option of writing in an answer if none of the choices was suitable. Most commonly reported sensations after a Pranic Healing session were feeling calm (56.7%), peaceful (49.6%), relaxed (49.0%), lighter (42.7%), more centered (37.6%), well-being (36.3%), optimistic (28%), and clearheaded (27.4%).

**Perceived Benefits.** Although we did not directly ask respondents about perceived benefits in the open-ended format, several respondents who had used Pranic Healing in the past reported their perceived benefits when answering the open-ended question about reasons for use. Although this limited set of responses ( $n = 6$ ) is highly self-selective and thus likely to be highly biased, it gives us a descriptive sense of some of the perceived benefits of this modality among its users. The following quotations exemplify these perceived benefits.

. . . I know that I look forward to having my pranic healing sessions. I always feel better after having a session overall . . .

- ... Feel better with PH on Effermal [ephemeral] level; overall feel better ...
- ... Pranic Healing relieves the stress which accumulates in the joints and throughout the muscles and nervous system of my body. It allows me to find some comfort while lessening the pain which is chronic from head to toe 7/24. It helps me in being able to function better during any given project or daily chores. It allows me to get out of bed and move around somewhat. ...
- ... I have experienced severe chronic pain on the entire right side of my body for years now, as well as uterine fibroid tumors which caused two miscarriages. No other form of treatment has helped me. With the Pranic Healing clinic, I noticed improvement in my level of pain. ...

### **Discussion**

The current descriptive study attempted to explore patient characteristics, expectations, frequency and reasons for use, physical and emotional sensations associated with healing sessions, and perceived health-related benefits of Pranic Healing as a CAM modality for which there is little scientific documentation but whose use is increasing internationally. Although the current study used criterion-based sampling rather than random sampling, sample demographic characteristics were comparable to the findings of various studies of CAM users in the U.S. (Wootton & Sparber 2001, Barnes, Powell-Griner, McFann, & Nahin 2004), and most recently Hawk, Ndetan, and Evans (2011), who in a secondary analysis of data from the 2007 National Health Interview Survey (NHIS) found that of the 4,416 respondents who had used some form of CAM in the past 12 months, respondents could generally be categorized as non-Hispanic White, middle-aged, college-educated women. The sample characteristics of this study also closely mirrored those of Beckford and Suzara (1994), who studied Pranic Healing as a religious movement in the Philippines and, contrary to their expectations, found their respondents to be of high socioeconomic status, slightly more likely to be female and be college-educated.

In terms of use, Broom, Wijewardena, Sibbritt, Adams, and Nayar (2010) noted in a footnote in their study of CAM practices and policy that individuals were more likely to use Pranic Healing to treat cancer than to enhance overall health. This use of Pranic Healing to assuage a specific physical or psychological problem rather than to enhance overall health was found to be true in the current study as well. Respondents were almost ten times as likely (80% versus 8.7%) to cite a specific problem that they needed to alleviate than to cite overall health or overall wellness as their goal in seeking out Pranic Healing.

Despite this, respondents did not necessarily expect Pranic Healing to cure their problem. From the results of the conventional content analysis of

expectations for treatment and reasons for use, it can be inferred that Pranic Healing is considered to be more of an aid to health and healing that has multiple possible general effects, and not a direct cure with a single specific effect. This was further exemplified by the comments on perceived benefits from a small subsample of respondents.

Results of the preliminary focus group showed that like some other biofield modalities, most notably Reiki, Pranic Healing has been used by practitioners in conjunction with conventional care methods as a form of integrative care. A brief review of Pranic Healing treatment locations and practitioner listings from several international and national Pranic Healing association organizational websites showed use in conventional hospitals in Australia, the United States, Ecuador, India, and Venezuela; and use as part of integrative medicine among medical doctors in 22 countries: Australia, Austria, Bosnia, Brazil, Canada, Colombia, Croatia, Costa Rica, Cuba, Ecuador, Ghana, India, Indonesia, Italy, Mexico, New Zealand, Norway, Philippines, Switzerland, United Kingdom, United States, and Venezuela (The Inner Sciences 2011, International Doctors for Pranic Healing 2011, Pranic Healers Association of Western Australia 2011, Canadian Pranic Healers Association 2011). This may also help to perpetuate a perception of Pranic Healing as an aid to health and healing rather than as a sole cure.

Sensations experienced after a Pranic Healing session were reported to be of a relaxing nature and not, as one would expect, of an energizing nature. This is in keeping with findings from studies of other energetic modalities such as Reiki (Wardell & Engebretson 2001), which found short-term changes in the physiological stress response immediately after healing sessions. These physiological and psychological sensations may be part of the reason why clients of Pranic Healing associate the practice with spiritual elements such as an “innate oneness of being” and “peace” and why resilience, with its implications for overcoming and dealing with stressful situations, emerged as a theme in the findings.

Expected outcomes for Pranic Healing were of divergent natures, and often contained multiple reasons with generalized expectations for any one respondent. This loosely fits the theoretical model for wellness outcomes espoused by Schuster, Dobson, Jauregui, and Blanks (2004), namely the concept of overall wellness as a higher-order construct that integrates multiple domains, among them physical, psychological (mental, intellectual, emotional), social, and spiritual. Specific theoretical subdomains within these four were not fully represented in the responses. It is unknown if these findings were due to selection bias or if they are true representations of the nature of Pranic Healing as a healing modality.

**Considerations of Bias**

Because criterion-based sampling was used as opposed to random sampling, it is possible that the sample was affected by selection bias. Approximately half of the sample was made up of clients who had used Pranic Healing in the past and were therefore more likely to have had positive experiences with Pranic Healing if they were continuing to use it. In this particular study, where the goal is to simply describe the characteristics of those who use this little-known modality, this is an advantage in that current expectations will likely be based on past experiences with the modality and would therefore be more realistic and more reflective of actual perceived benefits.

**Conclusions**

The current study aimed to examine patient expectations, reasons for use, patient demographics, sensations during treatment, and perceived outcomes of Pranic Healing, an energy healing therapy lacking in scientific documentation but whose use in the general population is becoming more widespread internationally. Results show that those who use Pranic Healing fit the sociodemographic profile of CAM users as described by the large representative quantitative studies of CAM use in the United States, that their reasons for use are not homogeneous, and that preliminary descriptive data from a selective subsample of respondents indicated that some respondents are experiencing positive outcomes attributed to this modality.

Expected outcomes and reasons for use by respondents could be categorized using a coding schema reflective of theoretical models wherein CAM is used to improve overall wellness or health or just one of the various dimensions that make up the perception of overall wellness. A longitudinal quantitative study would be necessary to examine these outcomes over time in order to better elucidate the dimensions of health and wellness that are perceived to change with use of this modality and the factors that may be associated with these changes.

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**Disclosure Statement**

The authors declare there are no competing financial interests.



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## RESEARCH ARTICLE

### A New Approach to Veridicality in Dream Psi Studies

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**Abstract**—A problem in spontaneous dream psi studies is validation of purported psi elements. Dreams rarely have enough specificity to satisfy critics that they point to paranormal knowledge of a specific event. This creates evidential hurdles to overcome when evaluating whether a dream contains psi-derived information such as scenes of future events or physically distant locations. In this study, the goal is to arrive at a reasonable method to establish that information derived from spontaneous dream experiences can be established as veridical. To accomplish this, a method for finding the equivalent of a target within a spontaneous experience is used to fix a specific locale and time for comparison. Adverse scoring criteria are used to address complaints about confirmation bias. The result is a psi-adverse method for scoring spontaneous experiences that are anchored to a specific locale and time. This method regularly produced significant *p*-values when used to assess 20 consecutive dream records, comprising 598 individual line items. These records were taken as a sample from a group of 3,305 dream records made over the past 22 years by the author.

**Keywords:** dream—veridicality—out-of-body—precognition—psi—pre-sentiment—spontaneous psi

### Introduction

Criticism of spontaneous cases of dream psi suggests that the number of uncontrolled variables is too great to generate a statistically valid means of evaluating their veridicality (Wiseman 2011:288). When elements of a psi experience do match a later event, they are sometimes criticized for being too generic to be reliably connected to the event in question. In both types of criticism, the problem is that there is no fixed target against which the purportedly psi dream elements may be compared. Laboratory experiments allow researchers to work with a known number of variables

by creating their own target sets, something that is theoretically impossible with spontaneous cases (Targ, Katra, Brown, & Wiegand 1995, Graff 2007, Bem 2011). Some researchers agree that spontaneous cases cannot be used effectively for evidential purposes:

[Louisa Rhine] made no effort to corroborate the experiences or to obtain additional details. This was not because she wanted to emphasize the personal meaning of the experiences over the evidential; rather, the Rhines' position was that spontaneous cases could not prove the existence of psi, but if patterns in sufficient numbers were noted, they might tell us something about the psi process that could be used in experiments. (White 1992)

A two-pronged strategy is used to address these concerns. The first step is to establish an anchor against which dream elements may be compared. Second, adverse criteria are used for selection and evaluation of purportedly psi elements to assuage concerns that the study is influenced by confirmation bias or other subjective factors. The results indicate the presence of psi, despite the use of adverse criteria.

### **Study History**

A very important but little used approach is the longitudinal study in which the experiences of a single person or group of persons are studied over a period of time. This method has probably not been used much because it requires someone who has had several experiences over a time period. . . . (White 1992)

The present study is based on data collected over the past 22 years as part of an informal longitudinal study of my own dreams. The study originated in a desire to prove that my dreams did not exhibit signs of psi, as I was told they did. Instead, the records quickly demonstrated the opposite. As of this writing, there are 3,305 records contained in the 31 journals that make up the database. No fewer than 230 of the records have demonstrated veridicality. The original purpose of the dream journals was satisfied after the third dream journal (DJ3) was complete, but I continue recording my dreams to the present day. Active checking of dream veridicality ceased after DJ3 but verifications were recorded afterward if it was convenient to do so. Because active verification ceased after DJ3 was complete, DJ3 was selected as the best example of a group of dreams checked for veridicality. There are 80 records contained in DJ3, of which 42 are veridical (Table 1). These records were split into 4 groups of 20 consecutive records. Records 1–20 were randomly selected for the study.

**TABLE 1**  
**Number of Veridical Records**

Records 1–20	Records 21–40	Records 41–60	Records 61–80	Total
12	7	16	7	42

**The Journals.** The variety of content in the 31 journals spans nearly the full range of psi experience. There are examples of OBE dreams, precognition, after-death communication, past life memories, psi training, healing, auras, telepathy, psychokinesis,<sup>1</sup> and less easily defined categories of a spiritual or religious nature. Although these more extraordinary dreams are numerous, they represent a minority of the dream records. At present count, approximately 10% of all records contain psi or other extraordinary content. The remaining dreams are not germane to this study, though examples do appear in the selection discussed here. Just as psi dreams do not easily suit expectations of critics who do not consider psi to be a plausible explanation for their content, the non-psi dreams recorded in these journals are in many ways unlike generally accepted theories about dream content.

**Non-Veridical Content.** Many dreams from the journal are not veridical but appear to be psi. The two most prominent categories that fit this description are after-death communication dreams that feature ghosts unknown to me or OBE dreams that similarly feature persons unknown to me. Another large category is dreams containing explicitly spiritual content. These dreams cannot be verified because they do not contain information related to physically connected events, or if they do, to events I have access to for verification purposes.

The remaining dreams are difficult to classify. They do not fit into any “normal” dream type easily but they do not appear to contain psi-derived information. These make up the bulk of the dream records in the journal and in this study. One popular theory is that most dreams are nothing more than “day residue.”

In a study that examined dream reports from undergraduates, researchers found that for the dreams most tied to a real-life event, the event had occurred either recently (1–3 days prior) or about a week earlier (5–7 days prior). The authors suggested that recent memories (day-residues) may appear in dreams because they are still mediated by the hippocampus, whereas the delayed memories may be in the process of transfer to the neocortex (MacDuffie & Mashour 2010).

While theory and research specific to dreaming are lacking, reason dictates that one's beliefs about dreams may be reflective of their waking life experiences (King & DeCicco 2009).

These quotes reflect the commonsense wisdom that many or most dreams are a composite of recent events from the dreamer's life. This "residue" is an innocent act of remembering recent but past events from one's own life, sometimes with added fanciful features. While this may be commonsense, the records used in this study do not support the theory. Examples of dreams that might contain day residue are so rare that even if all of them were legitimate examples of day residue, they would be much less common than every other type of dream, including any sub-category of psi.<sup>2</sup> If anything, day residue is disproved as a factor by the records used here.

### ***Spontaneous psi in Dreams***

Some exotic dream types contain information unlikely to have been acquired normally, but this is not always the case (Krippner & Faith 2001). A creative dream may lead to a surprising insight or the solution to a problem without resort to psi. The same is true of lucid dreams, dreams within dreams, and initiation dreams. Precognitive dreams and out-of-body dreams (OBE dreams) must contain psi content in order to inform the dreamer of distant places or future events. The question whether genuine examples of these dream types exist is highly controversial (Wiseman 2011).

Spontaneous psi is frequently reported to occur in dreams. In a table provided by Dr. Ian Stevenson (Stevenson 1970:2), he shows the results of an international survey with 9,319 respondents. Of these, 64.6% of self-reported psi experiences were dream-related. This was the largest source of psi material. The second most numerous category was "Waking Impression or Intuitive Experiences," accounting for 25.8% of the results. If a methodology may be found to validate spontaneous psi in dreams, the number of viable cases increases while potentially enhancing the richness of the content for qualitative examination and analysis.

#### **Definitions.**

Immediately I was in the spirit; and behold, a throne was standing in heaven. . . . Revelations 4:2 (Barker 1999)

If Biblical accounts of being "in the spirit" are counted as examples of an out-of-body experience (OBE), then historical OBE records date back thousands of years. Despite the history, there is no general agreement about what an OBE is (Alvarado 1989). Alvarado describes theories that

range from the purely biological, to full body/spirit separation as in the Biblical example above. He also references sources who consider an OBE as a type of dream. Robert Monroe differentiates his OBEs from dreams by describing them as a conscious experience, not a dream (Monroe 1977:26). Regardless of disputes regarding the true nature of an OBE, the term is used here to describe dreams of remote locations. This information is often veridical. When it is, it is a “veridical OBE” or an “OBE dream.”

Remote Viewing (RV) is similar to an OBE dream. The difference is that RV is a conscious exercise that does not include the sensation of dissociation common to OBEs. Because the body is unconscious during an OBE dream, conscious control of the RV-related aspects of the experience is not assumed, as it would be for RV. In both types of experience, information about a remote location is acquired via non-physical means.

Precognitive dreams purport to contain veridical data of future events. Dreams of this type have been reported in spontaneously occurring cases for thousands of years and remain quite common today (Stevenson 1970). Forced-choice testing provides statistical evidence to support the argument that precognition is a genuine phenomenon (Honorton & Ferrari 1989), but this type of testing cannot be applied to spontaneous cases.

**Laboratory Dream psi.** The Maimonides Dream Research Laboratory in New York used forced-choice testing to test for dream psi (Krippner & Ullman 1970). In forced-choice tests, targets are generated or selected before or after a subject begins to dream. Results of the dream are then compared to the target for indications of telepathy,<sup>3</sup> RV, or precognition. By testing for dream psi in a laboratory, some of the problems associated with spontaneous cases could be eliminated. According to a meta-analysis of the Maimonides dream studies, the data are suggestive of psi (Radin 2009).

In a different experiment, “dream incubation” was used to try to manipulate dreams to match a desired goal. The goal was to dream about material that would be found on the first few pages of any of three newspapers published after the dream (Graff 2007). That experiment allows for more flexibility than in the Maimonides studies, but its forced-choice design impairs reliability in at least one important dimension: If the intention to dream of an image from a specific newspaper on a specific day cannot be consciously controlled by the subject, the desired effect may not occur. The subject may have a legitimate precognitive or veridical OBE dream, but because it is not related to the target it is not counted. If there is a way to evaluate spontaneous cases where intention is not an issue, this type of data loss may be avoided.

**psi-Missing.** “psi-Missing” is when there is a conflict between one’s conscious objective and factors that inhibit its realization. This is generally



assumed to be unconscious and motivated by prior beliefs (Tart 2009). The concept is most commonly associated with the so-called “sheep–goat effect” where subjects who believe that psi is real (sheep) score higher than chance in psi tests, and those who do not believe in psi (goats) score below chance. In the context of this research, psi-missing includes the potential for a subject to miss a target due to an inability to consciously control a psi effect, regardless of beliefs regarding psi as an explanatory theory. This is important because forced-choice tests may suffer from this problem to a greater extent than is known, at least by this expanded definition. An example of this kind of psi-missing appears in record 12 of this study. In it, I went to sleep with the conscious intention of visiting an uncle of mine in an OBE. I did experience a veridical OBE that night, but not with the uncle I intended to visit. Instead I saw an acquaintance in a completely different part of the country. Another example occurs just after the 20 records used in the study. In it, I tried to visit a certain person in an OBE but failed. Instead, I had a very detailed dream that later proved to be precognitive. Spontaneous experiences should not have this problem because the conscious/unconscious dichotomy of forced choice testing does not exist. Instead, the unconsciously produced dream is compared directly to results, without regard for conscious goals.

### **Proposed Solution**

The examples used in this study utilize methodological controls to mitigate or eliminate the type of problems most often associated with spontaneous cases. Table 2 lists 6 major problem types associated with the evaluation of spontaneous psi in dreams and the controls applied.

**TABLE 2**  
**Six Major Problem Types and the Controls Applied**

Problem	No statistical baseline	Law of large numbers	Affirmative bias	Subjective judging	Dependence	Weak or absent documentation
Control	Anchors	Dilution	Nothing censored	Adverse scoring	Dependence and redundancy check	Dated written record

**Records and Scenes.** 20 records from DJ3 are used. A “record” comprises all of the notes taken for the previous night’s dreams. Every item in the journal is written longhand and dated for the day of waking. These are transcribed later into digital format. Records are broken down into “scenes.” A “scene” as defined here is described as a “dream” in other

contexts. Scene content is usually internally consistent, but is thematically or temporally disjointed from other scenes in the same record. Scene breaks are designated by the “>” symbol at the beginning of each scene in the original record. Sometimes this symbol is used to separate scenes prior to remembering a full scene. When that occurs, a note is made in the journal to connect scenes that would otherwise be considered separate. Scenes are broken down into “line items.” Each line item is a short description of a person, place, thing, event, action, or characteristic of something in the dream. Table 3 shows a portion of one of the records used in this study to illustrate the difference between a record, a scene, and a line item.

**TABLE 3**  
**Part of One Record, to Illustrate Record, Scene, and Line Item**

Record Number	Scene Number	Line Number	Line Item
5	4	1	Stepmother and Dad at a Quality Inn
5	4	2	Impression that I am at hotel on vacation
5	4	3	Mention that we are in or just north of San Francisco
5	4	4	Pink neon sign in shape of coffin floats just below water

This table shows a portion of the coded record for record 5, scene 4. In this example, it related to a trip my stepmother and father took to San Francisco from Maine. I did not know of the trip until I called to verify the dream, nor was I aware of any reason that would compel them to leave Maine or to visit San Francisco.

The date for each item is recorded. There are no missing days for the 20 records, which cover the period May 1, 1990, through May 17, 1990. There are three naps during the period, each of which is counted separately.

**Validation.** All validations are made on or shortly after the date of the record. One exception in this group was verified eleven years after the original record was written. Validations are noted in the margin of the original record along with the date the validation was made, the method, and the witness involved, if any. Validation entries are of two principal forms. The most common are notes made of telephone interviews with the subjects of various OBE types of dreams. These phone calls usually take place later in the same day the journal entry is made. If visual information in the form of drawings exists, they were sent by fax or as a copy by mail to the subject. The protocol for OBE validation calls was the same for all of the examples in this study. In each case, the following steps were followed:

1. Subject identified from journal
2. Written material faxed if appropriate (usually if drawings were involved)
3. Phone call to subject made
4. I identified myself and the purpose of the call as follows:
  - a. "I have just awakened from a dream in which you played a part"
  - b. "May I read the dream to you from my journal?"
5. After reading the dream, I asked the subject if any part of it sounded consistent with recent activities or events in their life.
  - a. The most common result was that all confirmed elements related to the same day in the subject's life, and that it was recent. Most often, it was within 24 hours of the phone call.
6. The results of the call, whether verified, not verified, or denied, were recorded in the margin of the dream journal entry.

Precognition entries are made after I have encountered some event in waking life that reminds me of a specific dream. On returning home, I would review that one dream and was generally rewarded with multiple line item confirmations for various events from the same day.

**Anchors.** An anchor is a prominent line item from within the dream record. Like targets in forced-choice tests, line items from the dream record are compared against anchors for consistency. The number of items that match the anchor are counted as "successful" trials. Because the anchor is the statistical baseline, it is not counted as a trial. For example, in a record that contains four scenes, one of those scenes might have 15 line items. One of those line items is identified as an anchor, and the remaining 14 are compared to it. If it appears to be an OBE, the anchor is a person whom I will call for confirmation. For an item to be considered verified, it must be accepted by the subject as correct for a recent experience from their life. Each subsequent item must be consistent with the same event or events from the same day in the subject's life. In this way, each additional item compounds the complexity of the original verified item and successive items.

An example of an anchor is provided by record 4, scene 1, item 1; as shown in Table 4.

Precognitive anchors are a combination of a line item that matches a component of a specific event and the date it occurs on. Table 5 shows an example of a precognitive anchor.

Most dream records contain multiple scenes. Of these, some have multiple unrelated veridical scenes. When this happens, such as two OBE dreams involving two different people and locations, the record will contain more than one anchor, as shown in Table 6.

Record 5 has a total of 6 scenes, 3 of which contain enough items to be checked. Of these, 2 scenes contain veridical items. This is an example of

**TABLE 4**  
**An Example of an Anchor**

Line Item from Journal	Line Item Description after Verification	Anchor	Explanation
Wake up in dream, returning from NYC from seeing Barbara F	NA	1	Barbara F can verify or disconfirm, NYC location dependent on prior knowledge
This table shows the line item description, that it is identified as an anchor, and why it was chosen as an anchor. The NYC location is combined with the name of the anchor because it is her city of residence, something that was known to me and therefore dependent.			

**TABLE 5**  
**Example of a Precognitive Anchor**

Anchor	Description from Journal	Description after Verification	Discrepancy Explanation
0	First name is like "Jean-Michel"	First name is "John"	"Jean" is French for "John" and sounds very similar.
1	Someone tells me his last name is like "Kittez"	Last name is "Kitses"	A very close match for a highly unusual name. When I heard this name, I thought of this dream, making it the anchor.

The anchor for this dream is the most striking element of the dream, as is often the case for precognition anchors. Like OBE anchors, these are not counted as verified, though doing so is usually more adverse in the case of precognition than an OBE dream.

**TABLE 6**  
**Records with More Than One Anchor**

Record Number	Scene Number	Total Line Items	Possible Line Items	Anchors	Attempted Line Items	Verified Line Items	Verified Fidelity Value
5	1	17	15	1	15	13	10
5	2	1	0				
5	3	1	0				
5	4	7	7	1	5	3	3.75
5	5	1	0				
5	6	7	3	1	3	0	0

a record containing more than one anchor due to multiple veridical scenes. The anchors are two different people who were at that time about 350 miles apart (OBE anchors) and a trip to a museum (precognition anchor).

**Base Probability Value (Adverse Correlation).** Unadjusted or raw  $p$ -values are calculated based on an arbitrarily assigned Base Probability (BP). The purpose of the BP is to provide a probability estimate that is both extremely adverse and definitely wrong. If the  $p$ -value remains significant in favor of the psi hypothesis despite a BP that has a strong null hypothesis bias, it is unlikely that significant results are the result of affirmative bias. A range of BP values are used for each  $p$ -value, as a way to easily compare results and to test how robust each  $p$ -value is in different conditions.

In a binomial distribution test the exact probability is required to calculate a  $p$ -value. In spontaneous cases the actual probability is not only unknown, but cannot be known. This does not mean that nothing is known about the probability. If I have a dream about bee sting therapy (apitherapy) and my friend Ron on a specified date, as I did, it is fair to say that the probability he could verify the dream as pertinent to him on that date is not equivalent to a coin toss where only two options are possible. Therefore, a BP of .5 is adverse, wrong, and biased in favor of the null hypothesis. Despite that, this particular record (Record 13, scene 1) is significant at  $<.05$  when the BP is set to .5. The  $p$ -value is significant at  $<.01$  with a BP of .33. What is the probability that any specific person on a given day will have a meaningful connection to apitherapy? It would be difficult to estimate, but I can say that I'd never heard of it until my friend Ron confirmed the dream. To this day, over 21 years later, I have not encountered a second incident relevant to this subject in any context. With this in mind, it should be obvious that a BP of .5 is adverse, wrong, and strongly favors the null hypothesis. The fact that the resulting  $p$ -value remains significant in the direction of a psi explanation rather than the null hypothesis demonstrates how robust the data are in the face of adverse treatment. Table 7 shows the variables used to calculate three separate  $p$ -values for the scene, each of which uses a BP of .33.

**Selection.** To counter any concern about bias regarding selection, DJ3 was divided into 4 equal groups of 20 dream records and 1 was randomly selected for the study. Group 1 was selected. Significantly, group 3 contained more veridical records than group 1. Records numbered 1–20 here correspond to the first 20 consecutive records of DJ3. All line items are included, regardless of whether they have been verified. Line items are divided into four categories. They are:

**Valid line items.** The number of line items per scene, exclusive of redundant and anchor items is the number of valid line items. This is

**TABLE 7**  
**Variables Used to Calculate Three Separate  $p$ -Values for One Scene**

Valid Line Items	Possible Line Items	Attempted Line Items	Verified Line Items	Verified Fidelity Value	Time Lag	BPV	$p$ -Value Total Items	$p$ -Value Possible Items	$p$ -Value Attempted Items
10	8	8	8	6.25	1	0.33	1.86E-02	2.43E-03	2.43E-03

This table is an excerpt from record 13, scene 1. The dream concerns a friend who lived 2,900 miles away and bee venom therapy undergone by a friend of his on the day of the dream. It shows a total of 10 non-redundant, non-anchor items. 2 are not confirmed. The remaining 8 items are verified. The “verified fidelity value” is 6.25. This indicates that some items have been reduced in value due to discrepancies. The  $p$ -value is calculated based on the verified fidelity value rather than the number of verified items, to make the result more adverse.

equivalent to the number of trials in a binomial distribution.

**Possible.** Some line items literally cannot be verified. This can be for a number of reasons, but most come down to lack of access. If I have an OBE dream about the activities of a famous or unknown person, whether veridical or not, if I don’t have access to that person for confirmation the item cannot be checked.

**Attempted.** Some line items are possible but are not attempted. This has a number of causes, but the most common is discomfort relative to the content. An example from the study is related to a dream I had about a gynecologist that was connected to my stepmother. She confirmed that she had started working for a gynecologist, but I did not ask about several of the details (an IUD that had to be surgically removed from a red-haired patient) because I was uncomfortable discussing the subject with my stepmother.

**Verified.** Line items that are possible to check, have been checked, and have been corroborated by a witness or myself are “verified.”

**Independence.** Dependent and independent items occur in this study. To differentiate between them, items that are meaningfully dependent on another item are counted as redundant. Other items are dependent but not meaningfully so. This means that although one item may have caused the other, the causal event is not the only way the result could have happened. In a card selection task where the selected card is not replaced, subsequent draws are meaningfully affected by the missing card because that card may no longer be selected and its absence increases the likelihood that other cards will be selected. In the series of dreams discussed here this is much less likely to happen because of the nature of variables even when causal links exist.

Record 12, scene 1 describes a sequence of mutually dependent items. The sequence goes as follows:

- 1) A car with a smashed roof is described.
- 2) The roof was smashed by a tree branch.
- 3) The tree branch fell during a wind storm.
- 4) The car is one of two owned by the same person.
- 5) Both cars had their roofs crushed by tree branches.
- 6) The two cars were damaged in separate events.
- 7) Both events occurred in the same week.
- 8) Both events were instigated by separate wind storms on different days.

These items are not statistically dependent because:

- 1) Not all smashed auto roofs are smashed by tree branches.
- 2) Not all storms dislodge tree branches.
- 3) Not all dislodged branches strike cars.
- 4) The probability of any of these events happening again do not change after they have happened once.

An example of a statistically dependent item comes from the same record. It gives the name of the subject/anchor, Dr. David Ryback, and then in a separate item describes the location as Atlanta, Georgia. This is where Dr. Ryback resided at the time, so the location is associated with and dependent on the identification of the subject. For this reason, the location item was counted as redundant. Meaningfully, the dream described the events happening to someone known to Dr. Ryback but not Dr. Ryback himself. In other words, his car was unaffected by the same storms that affected those of his fellow tenant. If nothing else, this underlines the independence of the items listed in the dream.

#### **Fidelity and verification.**

Rarely do perceptions of external events find representation in dreams; when they do, the perceptual element is usually a minor part of the dream. In addition, the stimulus is generally distorted in some fashion. (Krippner & Ullman 1970)

The purpose of this study is to show that psi dreams can survive adverse treatment. For that reason, serious distortions are counted as not verified and nothing is given the status of a symbol that can stand in for something else. Incomplete descriptions are not counted as wrong, but inaccurate descriptions are. Table 8 provides an example of a verified but incomplete series of line items.

**Temporal.** The lag time between the date of the dream and of the verified event was originally used as a multiplier to dilute *p*-values. However, this had no impact on the significance of results, so this factor was discarded as a criterion. The reason it did not have a meaningful effect is that the majority



**TABLE 8**  
**Example of a Verified but Incomplete Series of Line Items**

Line Item Description from Journal	Line Item Description after Verification
Asked to pick up something called "Pahs" or "Paz" in exchange for papers.	This is Westmont College student newspaper <i>La Paz</i> .
"Pahs" or "Paz" is common item.	This newspaper is common at Westmont College.
Have to pick up "Pahs/Paz" in exchange for papers.	<i>La Paz</i> is the paper.

The description provided in the journal makes it clear that an unknown object (the "Pahz or Paz") is the subject. Although unidentified, its description was sufficient to obtain verification from the anchor subject of this dream scene, R. Anthony Askew. At the time of the dream I did not know how to contact Tony, that he had moved to Westmont College, or that there was a newspaper with this name anywhere in the world. The third item is counted as redundant, so it is subtracted from the group of verified items.

of items refer to the same day as the dream, yielding a multiplication factor of 1. When there is a difference, it is usually within a week of the dream. The precognitive dream from record 20, scene 1 was 4,136 days distant. That value would have seriously affected most records, but because there were 33.5 verified items out of 41 possible in that scene, the  $p$ -value was too small for the long lag time to affect significance.

**Population.** The estimated population of those who could conceivably verify line items is used to dilute the  $p$ -value. Table 9 shows 2001 population figures from a large study on world population (Demeny & McNicoll 2006). These were used as population values for dilution.

**TABLE 9**  
**2001 Population Figures**

Population	Estimated
World	6.15E+09
Western	7.39E+08
US	2.85E+08
My contacts	2.50E+04

The key to deciding which figure is used is access to validation. It is not reasonable to assume that a person in Malaysia could without great effort verify a dream about an unknown resident of Santa Barbara. However, it is reasonable to expect that almost anyone in the world could confirm a dream about the World Trade Center collapse in 2001. For these reasons,

the first example would be assigned a population value of 25,000, meant to represent an exaggeration of my total number of contacts, but the World Trade Center dream is assigned the world figure of 6.15 billion. The  $p$ -value is multiplied by the population value to make it larger and thus closer to null significance. Record 20 has the largest date and population value of any record in this study but it also has the lowest  $p$ -value due to the large number of verified elements.

### ***p*-Value Calculation Method**

For this study, actual probability is not known. For that reason a variable named “Base Probability” (BP) was used. The BP value is adverse. This means that the value is high enough that the assigned probability is expected to be both dramatically wrong and supportive of the null hypothesis. A range of BP values was used to test how robust the resulting  $p$ -values were. These ranged from .1 to .75.

The numbers of verified non-dependent items are counted as successful trials.  $p$ -values were calculated for the number of these items within a scene, per record, and in total across all scenes and records.

The number of trials is counted in three ways. These are: number of line items in total, number of possible line items, and number of attempted line items.

Table 10 shows a matrix of the variables used.

**TABLE 10**  
**Variables Used**

Successful	# Trials	Base Probability
Per scene	Total	0.1
Per record	Possible	0.2
Total	Attempted	0.25
		0.33
		0.5
		0.6
		0.75

From this, a binomial distribution  $p$ -value was calculated in MS Excel 2010 using the following formula:

$$=1-(\text{BINOMDIST}(\text{successful}, \text{trials}, \text{probability}, \text{TRUE}))$$

Table 11 is excerpted from one of the records in the study to show how the  $p$ -value calculation is made.

**TABLE 11**  
**How a *p*-Value Calculation Is Made**

Successful Verified independent	Trials			Probability	<i>p</i> -Values		
	Total	Possible	Attempt	Base	Total	Possible	Attempt
10	19	15	15	0.1	6.96E-08**	9.30E-09**	9.30E-09**
				0.2	7.56E-05**	1.25E-05**	1.25E-05**
				0.25	6.25E-04**	1.15E-04**	1.15E-04**
				0.33	7.36E-03**	1.65E-03**	1.65E-03**
				0.5	1.66E-01	5.92E-02	5.92E-02
				0.75	8.93E-01	6.86E-01	6.86E-01

### Results

Binomial distribution-based *p*-values were calculated for each of the categories of data separately. A range of adverse BP values were used to identify at what level a record achieved significance according to the criteria of the study. When all line items ( $n = 558$ ) are combined into a single group representing the total number of trials, including items that were never checked or were impossible to check, the *p*-value is significant at  $<.01$  if the BP value is set to .1. If only possible items are included ( $n = 315$ ), regardless of whether they were checked, the result is significant at  $<.01$  with a BP of .25. In both cases, those trials are the equivalent of adding significant numbers of unrolled dice to a dice-rolling trial. If only checked items ( $n = 256$ ) are used as the number of trials, the *p*-value is  $<.01$  with a BP of .33.

Table 12 illustrates a range of BP values used and the resulting *p*-values, for each of these scenarios. These *p*-values remain significant regardless of the category checked (Tables 15 and 16 in the Appendix). At the record level, it is clear that some scenes contain no verified items and others contain almost all verified items (and some have nothing but verified items). This pattern shows that nothing like a random distribution of verified vs. unverified items is present. 71 of 93 scenes contain 0 possible verified items. The remaining 22 scenes contain varying numbers of verified items, as shown in Table 13. The mean number of veridical items per scene is 7.5, and 23.5 for total number of line items per scene. This is significant at  $p <.05$  with a BP of .25, or 1:4. The most impressive result is scene 1 from record 20. This scene held up under extraordinarily harsh conditions.

**TABLE 12**  
**Range of BP Values Used and Resulting *p*-Values**

	LINE ITEMS					DILUTION		
	Success	Trials	Probability	<i>p</i> -Value	Significance	World Pop	US Pop.	Significance
<b>Total</b>	112.5	558	0.5	1.00E+00	NS	6.15E+09	2.85E+08	NS
	112.5	558	0.33	1.00E+00	NS	6.15E+09	2.85E+08	NS
	112.5	558	0.25	9.96E-01	NS	6.13E+09	2.84E+08	NS
	112.5	558	0.2	4.58E-01	NS	2.82E+09	1.31E+08	NS
	112.5	558	0.1	4.07E-13	<.01	2.50E-03	1.16E-04	<.01
	112.5	558	0.05	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
	112.5	558	0.01	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
<b>Possible</b>	112.5	315	0.5	1.00E+00	NS	6.15E+09	2.85E+08	NS
	112.5	315	0.33	1.53E-01	NS	9.40E+08	4.36E+07	NS
	112.5	315	0.25	1.16E-05	<.01	7.16E+04	3.32E+03	NS
	112.5	315	0.2	4.47E-11	<.01	2.75E-01	1.28E-02	NS, <.05
	112.5	315	0.1	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
	112.5	315	0.05	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
	112.5	315	0.01	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
<b>Attempted</b>	112.5	256	0.5	9.74E-01	NS	5.99E+09	2.78E+08	NS
	112.5	256	0.33	1.32E-04	<.01	8.15E+05	3.78E+04	NS
	112.5	256	0.25	2.15E-11	<.01	1.32E-01	6.12E-03	NS, <.01
	112.5	256	0.2	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
	112.5	256	0.1	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
	112.5	256	0.05	0.00E+00	<.01	0.00E+00	0.00E+00	<.01
	112.5	256	0.01	0.00E+00	<.01	0.00E+00	0.00E+00	<.01

This table shows the effect of adverse base probability values. It also shows that the number of successful line items in this study is enough to overcome adverse base probability values and the unfair addition of trials not conducted. The sections of Total line items and Possible line items both contain large numbers of trials not conducted. This can be checked by referencing the section Attempted line items where the actual number of conducted trials is used.

**TABLE 13**  
**Verified Fidelity Value**

		Frequency	Percent	Valid Percent	Cumulative Percent
<b>Valid</b>	0	70	75.3	75.3	75.3
	0.75	3	3.2	3.2	78.5
	1	2	2.2	2.2	80.6
	1.5	2	2.2	2.2	82.8
	2	1	1.1	1.1	83.9
	2.25	1	1.1	1.1	84.9
	2.75	1	1.1	1.1	86
	3	2	2.2	2.2	88.2
	3.25	2	2.2	2.2	90.3
	3.5	1	1.1	1.1	91.4
	3.75	2	2.2	2.2	93.5
	6.25	2	2.2	2.2	95.7
	9	1	1.1	1.1	96.8
	9.75	1	1.1	1.1	97.8
	10	1	1.1	1.1	98.9
	33.5	1	1.1	1.1	100
	<b>Total</b>	<b>93</b>	<b>100</b>	<b>100</b>	

75.3% of all scenes have 0 valid verified items. Adverse selection does reduce the verified fidelity value of individual items, but this cannot reduce a score to 0. If something is verified, not redundant, and not an anchor, it will receive a minimum fidelity value of .25.

The threshold between significance and non-significance for this scene is between a BP value of .71 and .72. At .5 it is significant at  $< .01$ , and at .34 it survives dilution against the US population. These results indicate that on an individual level, a group level (regardless of group type), and on average, these results do not support the null hypothesis. Table 14 details  $p$ -values for record 20, scene 1.

### Discussion

This paper seeks to demonstrate that it is possible to judge veridicality of spontaneous dream experiences by directly linking all of the line items in a

**TABLE 14**  
**Record 20, Scene 1**

LINE ITEMS					DILUTION			
Success	Trials	Probability	p-Value	Significance	World Pop	Significance	US Pop	Significance
33.5	41	0.71	6.04E-02	NS	3.71E+08	NS	1.72E+07	NS
33.5	41	0.7	4.58E-02	<.05	2.82E+08	NS	1.31E+07	NS
33.5	41	0.65	9.59E-03	<.05	5.90E+07	NS	2.73E+06	NS
33.5	41	0.34	1.60E-10	<.01	9.87E-01	NS	4.57E-02	<.05
33.5	41	0.32	2.49E-11	<.01	1.53E-01	NS	7.10E-03	<.01
33.5	41	0.3	3.37E-12	<.01	2.07E-02	<.05	9.61E-04	<.01
33.5	41	0.29	1.17E-12	<.01	7.20E-03	<.01	3.34E-04	<.01

Record 20, scene 1 is the most impressive of the group included in this study. The number of successful verifications is so high relative to the number of line item "trials" that only extremely severe treatment can reduce it to nonsignificance. Even if the number of successful trials is cut in half to 16.75, the result remains significant at  $p < .01$  if the BP is reduced to a still severe .25.

given scene to an item identified as an anchor. Anchors allow a dream event to be fixed to a place and a time, or to a specific person and time. Once this is done, the robust nature of some spontaneous psi dreams is able to weather even the harshest treatment. Clearly, if such results can be regularly found within records of spontaneous events, they are worth a second look.

If it were true that spontaneous records cannot be reliably correlated with any specific event, then laboratory experiments would have to be used to test for veridicality. However, the methodology used in this study does control for the same concerns at issue in traditional experiments and have the added benefit of not imposing such an unreasonable level of control that results are misleading or prevent the inclusion of more interesting content in the literature.

It is hoped that by showing that spontaneous dreams can be studied quantitatively, greater confidence in the study of spontaneous cases may be justified. This in turn may lead to serious study of other factors in dreams that extend well beyond the question of whether a given example is or is not a genuine example of psi. What of dreams that combine veridical content and spiritual content? Should one be accepted and the other rejected completely? Or can the validity of the veridical portion of the dream add credibility to other parts of it as well? The question of veridicality as it applies to a small subset of dreams, once answered in the affirmative, invites further, more

interesting questions. If the use of anchors and adverse probability values can be applied successfully in other studies, dreams that were hitherto simple “anecdotes” suddenly become accessible for serious study.

### Notes

- <sup>1</sup> There is just one example of this, where an attempt to disrupt electricity within a dream was rewarded with a near-simultaneous local blackout.
- <sup>2</sup> I intend to make a study on this subject later, but preliminary estimates are that potential examples of day residue exist in about 20 out of 3,305 records. If looked at from the scene level, it would be about 20 scenes out of approximately 12,000.
- <sup>3</sup> The Maimonides description of dream telepathy could be interpreted as including what I refer to here as an “OBE dream.” Because the purpose of this study is to suggest a methodology to evaluate psi content in spontaneous dreams, the difference between these definitions is less important than the fact that both are examples of psi.

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## Appendix A

Table 15 is a partial record from record 12, scene 1 (for space reasons, the complete record of this scene is not included, but only the first ten items).

**TABLE 15**  
**Record 12, Scene 1: Falling Tree Branches, OBE Dream Example**

Line Item #	Description from Journal	Description after Verification	Fidelity of Description	Verified Corrected for Fidelity	How Checked/Verified
1	Dr. David Ryback	Correct ID	Identical	ANCHOR Not counted	Call to Dr. David Ryback
2	Ryback in Southern city, probably Atlanta	This is verified, but I knew Ryback lived in Atlanta, so give it redundant = 1, fidelity = 0, and verified = 0.	Identical	ANCHOR not counted	Call to Dr. David Ryback
3	A car with smashed roof	Correct	Identical	1.00	Call to Dr. David Ryback
4	Roof smashed by tree branch	The morning of the dream, Ryback has conversation with colleague at his office where he describes how his two cars had their roofs caved in by tree branches.	Identical	1.00	Call to Dr. David Ryback
5	Branch fell during wind storm	Correct	Identical	1.00	Call to Dr. David Ryback
6	This is second time it has happened to this car.	Second time happened to same man, but different cars, both in same week. First was his car, then his wife's, 2 different wind storms.	Similar	0.75	Call to Dr. David Ryback
7	David is in nearby building.	His office is adjacent to parking lot with damaged car.	Identical	1.00	Call to Dr. David Ryback
8	Woman with David, possibly an assistant. Name is something like Rita or Rhoda —she may be Hispanic.	No record of this item being verified.	Consistent	0.00	Call to Dr. David Ryback
9	David is talking to someone else.	Yes, owner of damaged cars.	Identical	1.00	Call to Dr. David Ryback
10	Car in parking lot belongs to man in room with David or David.	Yes.	Identical	1.00	Call to Dr. David Ryback

Table 16 is a continuation of the partial record from record 12, scene 1.

**TABLE 16**  
**Record 12, Scene 1: Veridicality and Dilution Values for Line Items**

Record #	Scene	Line Item #	Date of Dream (Day I Wake Up)	Date Checked/ Verified	Date of Verified Event	Days between Dream & Event Checked	Anchor	Redundant	Verification Possible	Verification Attempt Made	Verified Items	Distance in Miles	Population Value
12	1	1	5/10/90	5/10/90	5/10/90	1	1	0	0	0	0	809	3.E+04
12	1	2	5/10/90	5/10/90	5/10/90	0	1	1	0	0	0	809	3.E+04
12	1	3	5/11/90	5/11/90	5/11/90	1	0	0	1	1	1	809	3.E+04
12	1	4	5/10/90	5/10/90	5/10/90	1	0	0	1	1	1	809	3.E+04
12	1	5	5/10/90	5/10/90	5/10/90	1	0	0	1	1	1	809	3.E+04
12	1	6	5/10/90	5/10/90	5/10/90	1	0	0	1	1	1	809	3.E+04
12	1	7	5/10/90	5/10/90	5/10/90	1	0	0	1	1	1	809	3.E+04
12	1	8	5/10/90	5/10/90	5/10/90	1	0	0	1	1	0	809	3.E+04
12	1	9	5/10/90	5/10/90	5/10/90	1	0	0	1	1	1	809	3.E+04
12	1	10	5/10/90	5/10/90	5/10/90	1	0	0	1	1	1	809	3.E+04

### **Record 20, Scene 1: 9/11 Precognition Example**

The following paragraphs provide an abbreviated sketch of this very detailed dream. For a full account, readers are encouraged to download a report from <https://sites.google.com/a/mundusvirtua.com/dreams/journals/written-journal-1>. Of the several documents listed, it is the one named [911\\_dossier\\_lowRes.pdf](#). The raw line item data used for this study is titled [JSE\\_V26-3\\_Data.xlsx](#).

### **Summary of Record 20, Scene 1**

In 1989 I had a very powerful dream that I did not immediately record. In it, I was warned to move out of Manhattan or be killed in a disaster that would occur in my neighborhood in the southernmost tip of the city. My long dream of May 17, 1990, from this group referenced the 1989 dream, inspiring me to type an account of the earlier dream for my records at that time. There is one error in that document and one significant omission. The error is that I transposed an item from the May 17 dream, and the omission is

simply something that I never thought was important enough to write down: As I looked at the fallen skyscrapers where the World Trade Center should have stood, I heard the echo of jet engines fading in the air. This sound was never recorded in writing, but it was mentioned often when I described the dream to others. Regardless, like every other record, the written record is the only thing considered for this study. After the May 17 dream, I had two more that referenced it. In one, I was happy I'd left Manhattan because my apartment (which was in the disaster zone) had been demolished. In the second, I was asked to provide some information about the May 17 dream. As I tried to come up with something to say in response, two billiard balls fell from a shelf, the "9" and "11" balls, in that order. Eleven years later and only two weeks before the 9/11 disaster, I had a powerful waking premonition as I looked at a faux NYC skyline at Legoland in Carlsbad, California. For some reason the fact that they hadn't built the World Trade Center as part of the tableau grabbed my attention and I was suddenly sure that the May 17 dream of 1990 would come to pass shortly. I made the prediction to my wife and daughter and pulled the journal out of my closet when I got home to look it over. Two weeks later, the World Trade Center collapsed. The May 17 dream has four scenes. Two are not veridical. One is an OBE that I verified with a friend later that day. The other is a view of destruction in Lower Manhattan that correlates with only one incident in the history of that city: the 9/11 attacks.

This dream was so vivid and frightening that I sent a copy of it by post to the Amazing Kreskin, and descriptions to various friends. In 1998, I posted it to the Internet on an early version of my website. Ironically, a physicist friend who knew of it, described it to psi-debunker the Amazing Randi as an example of a failed precognitive dream. This was a few years before the dream was realized.

Based on the factors just described, this dream sounds like a perfect example of a veridical precognitive dream. It was recorded and dated on the day of the dream as were all other related dreams, it was communicated to other people by post, telephone, personal conversations, and even the Internet before it happened, and it has many line items to compare with the later event. It does not, however, provide the kind of details found in OBE dreams. There is no named person to call and verify this with, nor enough information to have prevented it. Instead, it is a view of a group of details that are consistent with the 9/11 attacks and no other event in the history of the United States. This is something easily recognized in retrospect, but knowing the details of this dream could not have prevented the attacks. One dream gave me a direct personal warning: to move or die in the attack. I followed the advice by moving to New Jersey, little realizing that this attack wouldn't arrive for 11 years.

## *HISTORICAL PERSPECTIVE*

### **Distortions of the Past**

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**Abstract**—While no view of past parapsychological developments is free of problems, it is worthwhile to discuss how our accounts can be distorted, if only to be more aware of our working assumptions. In this address I will focus on the writings of parapsychologists, and particularly on some problems in these writings producing a distorted view of the past of the discipline. I argue that the past is distorted when we neglect the work of specific groups and individuals (such as lesser-known figures and women); when we see the past as a function of the present (neglect of unpopular ideas today, justification of research programs); and when we focus mainly on positive aspects of the study of psychic phenomena (neglect of critics and of examples of the rejection of the field). It is my hope that a consideration of these issues will assist us in expanding the reach of such writings.

**Keywords:** history of parapsychology—history of psychical research—great men history—justification history—presentism

This paper is an expanded version of an invited address that was part of the Outstanding Contribution Award the Parapsychological Association granted to me at the 2010 convention held in Paris, for which I thank the Association. While I have covered many topics during my career in parapsychology, perhaps my best-known work has been my papers about various aspects of the past literature of psychical research. This includes discussions of parapsychological terminology (Zingrone & Alvarado 1987), trends in the study of OBEs (Alvarado 1989b), the influence of the idea of survival of death on parapsychology (Alvarado 2003), ideas of human radiations (Alvarado 2006), and many others (e.g., Alvarado 1989a, 1993, 2009b, Alvarado, Biondi, & Kramer 2006). In this address I will not discuss the past literature proper, but will focus instead on some problems that produce an incomplete and unbalanced view of the past development of our discipline. These issues are important because, having a more complete

grasp of their subject, parapsychologists may improve their writings and may acquire a different sense of the complexity of factors behind their discipline. Furthermore, these new perspectives will affect the views of students of and newcomers to the field as well.

In summary, it is my hope that these comments—addressed to parapsychologists who write about aspects of the history of our field—will help us expand the reach of such writings.

### Prologue

Before starting to discuss my topic, it is important to recognize that the history of disciplines such as science and medicine is written by different types of scholars. The history of academic fields is mainly discussed by practitioners and by professional historians.<sup>1</sup> By practitioners I mean those individuals who are active members of the discipline in question, be they teachers, researchers, or something else, while professional historians are those who have been formally trained in history. Generally practitioners writing about past parapsychological developments focus on issues related to the practical use of the old literature, such as the use of these publications to generate or to guide current research and theorization (Alvarado 1982). Furthermore, they are concerned with the reality of the phenomena and the validity of related research findings and concepts, as well as with the issues of precursors and antecedents of current developments. In contrast, professional historians tend to avoid these issues, focusing instead on seeing the discipline in the intellectual and social context of its time, but generally without concern for the reality of psychic phenomena (Noakes 2008).<sup>2</sup>

The practitioner is generally engaged in disciplinary history which purpose is the crafting of a professional disciplinary identity based on currently perceived conceptual and methodological continuities coming from the past to the present day.<sup>3</sup> This is reflected, for example, in discussions about the life and contributions of past researchers in the field, including a variety of celebratory publications (e.g., Alvarado 2004, Rao 1982). These writings do much more than document the past, they legitimize the present. That is, they allow us, and each generation anew, to construct a developmental history of the field connecting past researchers and conceptual developments to present ones, and to defend the importance of current concepts and methods.

Like other disciplines, parapsychology has had many examples of histories written by workers in the field (e.g., Beloff 1993, Dèttore 1976, Parra 1990, Tischner 1960). Furthermore, in the past many researchers have presented historical accounts of parapsychology in chapters or sections of their textbooks (e.g., Broughton 1991:Chapter 2, Irwin & Watt 2007:Chapter

2, Richet 1922:16–42), and in overview articles (e.g., Alvarado 1989b, L. E. Rhine 1971).

Regardless of whether one is a practitioner or a trained historian, everyone writing about past developments is concerned with the issue of objectivity in presenting the past (for a discussion, see Newal 2009). That this problem is a relative one, and more an ideal than a reality, is clear in historian Peter Novick's (1988) designation of the issue as a "noble dream." This should not come as a surprise to anyone, particularly to those who have had some experience in crafting accounts of the past. Certainly many factors can affect our accounts, including personal interests and biases, the conceptual approach we use, and the sources and examples we select. As human beings we select aspects of the past, emphasizing some things and deemphasizing and neglecting others. An awareness of such selectivity has led historian of medicine Vivian Nutton to state in his book *Ancient Medicine* (2004): "History is an art of forgetting as well as of remembrance" (p. 1).

Historian of science Steven Shapin (1996/1998) has commented about such selectivity, arguing that this is inevitable in the work of historians, something that should make us skeptical about claims of "definitive" and "exhaustive" histories. In his words:

What we select inevitably represents our interests, even if we aim all the while to "tell it like it really was." That is to say, there is inevitably something of "us" in the stories we tell about the past. This is the historian's predicament, and it is foolish to think there is some method, however well-intentioned, that can extricate us from this predicament. (Shapin 1998:10)

In addition there are many blinders that produce incomplete and distorted views of the past. In other words, because of the complex nature of the enterprise, history is to some extent always distorted or changed in some way, depending on our looking glass as practitioners or as trained historians. Nonetheless, like scientists and their efforts to do the best study they can while being aware of the imperfections of methodology, those engaged in the study of the past need to always consider the above-mentioned problems, regardless of whether they are completely unavoidable. If anything, such awareness will allow us to be conscious of our assumptions and of the subjective nature of attempts to chronicle the past.

In this paper I will not focus on statements showing errors or ignorance about the past, nor on examples of how belief or skepticism about psychic phenomena affect historical accounts. Instead I will discuss the following problems found in the writings of parapsychologists: (1) Neglect of the work of specific groups and individuals; (2) Seeing the past as function of the present; and (3) Emphasis on progressive aspects.

## Factors Affecting Our Views of the Past

### ***Neglect of the Work of Specific Individuals and Groups***

Overall, and to quote from historian Christopher Hill's book *The World Turned Upside Down* (1972/1985),

... most of our history is written about, and from the point of view of, a tiny fragment of the population, and makes us want to extend in depth as well as in breadth. (Hill 1972/1985:16)

The following are examples of this problem.

***The "Great Man" Approach to History.*** One of the main ways in which history in general may be distorted is through what has been called the "great man" approach. As expressed by Scottish historian and writer Thomas Carlyle (1795–1881) in his book *On Heroes, Hero-Worship, and the Heroic in History*,

Universal History, the history of what man has accomplished in this world, is at bottom the History of the Great men who have worked here. (Carlyle 1840:3)

Ever since Carlyle, and even before, much of history has been written with emphasis on the exceptional and heroic qualities and work of a few individuals. The point here is not to deny that specific individuals—men such as Frederic W. H. Myers (1843–1901), Charles Richet (1850–1935), Albert von Schrenck-Notzing (1862–1929), and J. B. Rhine (1895–1980)—made important contributions to the development of the systematic and scientific study of psychic phenomena. In fact, we need more work about influential figures who distinguished themselves for their work, productivity, and leadership. But history also needs to include the less-prominent if only because the past is a collective construction and not only the product of the elites.<sup>4</sup> This implies that there were many other less-known figures whose work converged with the work of the better-known individuals, helping to achieve and create the work for which they are known today. They deserve our attention if we are interested in realistic views of the past. A case in point are the individuals surrounding and working with J. B. Rhine. In addition to J. Gaither Pratt (1910–1979) and Louisa E. Rhine (1891–1983), there were others such as Betty Humphrey (b. 1917) and Charles Stuart (1907–1947) (Mauskopf & McVaugh 1980).

Discussions of the Society for Psychical Research (SPR) have been dominated by emphasis on figures such as Frederic W. H. Myers, Henry



Sidgwick (1838–1900), and Edmund Gurney (1847–1888). While their work was essential, we also need to remember there were also other figures involved in the development and research conducted by the Society. For example, much more could be written about other important figures such as Richard Hodgson (1855–1905), Eleanor M. Sidgwick (1845–1936), and Alice Johnson (1860–1940), not to mention lesser-known figures.<sup>5</sup>

There is also a general tendency to refer to the scholars and scientists who formed the Council of the early SPR with no acknowledgement that some of them were spiritualists.<sup>6</sup> This was the case with Hensleigh Wedgwood (1803–1891), and of others such as E. Dawson Rogers (1823–1910), Morell Theobald (1828–1908), and William Stainton Moses (1839–1892).

Wedgwood, a philologist, was one of the vice-presidents of the early SPR, and one the authors of the first report of the Committee on Haunted Houses (Barrett, Keep, Massey, Wedgwood, Podmore, & Pease 1882). Furthermore, he participated in many SPR meetings (Meetings of Council 1885), and contributed cases to the spiritualist literature (Wedgwood 1881, 1883). Like other spiritualist members of the early SPR, Wedgwood fulfilled an important function in that he criticized the assumptions and methods of SPR researchers in their own publications (e.g., Wedgwood 1887).<sup>7</sup>



**William Stainton Moses**

Moses (see photo) is remembered today by many mainly as a medium. But he was also an early SPR vice-president and an active member who participated in such tasks as the collection of cases for the Society (Barrett, Moses, Podmore, Gurney, & Myers 1882). His writings show that he was also a serious student of psychic phenomena with a considerable knowledge of the literature on the subject (Moses 1889), as does his editorship of the spiritualist journal *Light* for several years. His studies of psychic phenomena appeared in his books about direct writing and mediumistic communications (Moses 1878, 1879) and in his detailed article discussions about phenomena such as apparitions of the living (Moses 1876) and materializations (Moses 1884).<sup>8</sup>

There is a similar need for expansion in discussions of early parapsychology in the United States of authors who focus on well-known individuals such as James H. Hyslop (1854–1920), William James (1842–1910), and Australian Richard Hodgson (e.g., Berger 1988). One hopes that

the scope of the history of American psychical research may be expanded to cover a variety of additional figures. One example was Hereward Carrington (1880–1958), known in his early career for his discussion of mediumistic and other forms of trickery (Carrington 1907).<sup>9</sup> Others include publisher and lexicographer Isaac K. Funk (1839–1912), physician Rufus Osgood Mason (1830–1903), and minister and writer Minot J. Savage (1841–1918) (Funk 1904, Mason 1897, Savage 1902).

Another important point is that the work of eminent individuals, no matter how important or innovative, was generally conducted in a context. Other individuals around the figures in question were important in creating an intellectual context that provided the opportunities, encouragement, and critiques that allowed the “great men” to conduct their work. This is seen in some writings about the history of parapsychology (e.g., Gauld 1968, Mauskopf & McVaugh 1980), and in other writings coming from the histories of science and medicine (e.g., Frank 1980, on prosopography see Clark 2003).

**SPR-Centered History.** Another distorting influence is the tendency to focus on the early research of the British SPR, while work conducted in places such as Italy, France, and Germany is barely mentioned.

In addition to what I have written above about researchers, emphasis on SPR material may blind us to the existence of different conceptual traditions, as seen in the following example. Partly because of the philosophical–psychological emphasis of the SPR, and because of the discovery of fraud with some mediums, their researchers paid less attention to physical mediumship than other groups. I made a comparison of articles about mental and physical mediumship published in the *SPR Proceedings* and in the French journal *Revue Métapsychique* for the period 1920–1930, which revealed that the *SPR Proceedings* had a higher proportion of papers about mental manifestations as compared with physical ones, while the French journal showed the opposite (Alvarado, Biondi, & Kramer 2006). Such trends, discussed by Inglis (1984), alert us to the existence of specific interests or styles in psychical research that characterize the mentality of research groups or countries.

**Women.** Closely related to this topic is the emphasis on men, to the neglect of the contributions of many women, a topic I have discussed before in another paper (Alvarado 1989a). It is common to mention prominent women such as Eleanor Sidgwick, Louisa E. Rhine (1891–1983), and Gertrude Schmeidler (1912–2009). But we neglect



Juliette Bisson



Fanny Moser

many others whose work was influential. Among them I may mention Lydia Allison (1880–1959), Juliette Bisson (1862–1956)<sup>10</sup> (see photo), Esther M. Bond (1913–1963), Laura Dale (1919–1983), Betty Humphrey (b. 1917), Fanny Moser (1848–1925), Helen Salter (1883–1959), Gerda Walther (1897–1977), Margaret Verrall (1859–1916), and Zoë Wassilko-Serecki (1897–1978) (see photo).

In my paper on women in parapsychology (Alvarado 1989a),

I argued that the issue is not merely one of saying that there have been women in parapsychology, but that their contributions need to be seen from their particular point of view. Because of women's position in society, they have frequently occupied positions of support and administration that are subordinate to those of men. This is what Margaret Rossiter referred to in *Women Scientists in America* (1982) as women's work in science. In turn, these aspects are generally ignored by parapsychologists who write about the history of their field. Furthermore, women's lack of opportunities, like those of minorities in general, connected to low prestige and difficulties in obtaining education, show that our past is gendered because the different sexes have enjoyed different opportunities and privileges in relationship to their intellectual development and work.

**Emphasis on English-Language Developments.** Another problem is the common practice of seeing our history through an Anglo-American lens. Over the years I have encountered parapsychologists whose view of the past is generally limited to American and British developments published in English, forgetting the contributions of many other groups.

One only has to see some of the writings of parapsychologists whose main language is English to realize that they generally ignore developments that have been published in other languages (e.g., Broughton 1991, Irwin & Watt 2007, Radin 2006, J. B. Rhine 1977). This is also seen in the history chapter of the *Handbook of Parapsychology* (Beloff 1977), which did not include a single reference that was not published in English, although it had a few translations of foreign works. While European developments



Zoë Wassilko-Serecki

Peter Mulacz

were included in the account, the emphasis was on British and American developments.

A considerable amount of the work of individuals such as Ernesto Bozzano (1862–1943), Gustave Geley (1868–1924), Cesare Lombroso (1835–1909), and Joseph Maxwell (1858–1938) has been published in languages other than English and tends to be neglected. Some argue that they do not know the relevant languages, but few seem interested in taking steps to solve the problem. In addition to learning languages, we can always collaborate with colleagues with knowledge of the necessary languages, something I have done in the past (e.g., Alvarado, Biondi & Kramer 2006, Alvarado & Nahm 2011). Unfortunately, and as has been my experience in the United States, sometimes the problem seems to go beyond languages, as seen in lack of knowledge of European works translated into English (e.g., Bozzano n.d., Maxwell 1903/1905). This suggests that the problem is not only one of a language barrier, but that there are wider cultural problems here affecting the writings of parapsychologists.<sup>11</sup>

Not knowing what has been published in other languages reduces our knowledge of the history of parapsychology, and produces incomplete, if not provincial, views of history. It also condemns us to follow particularly American, British, or other perspectives of the past, forgetting that, while there are international commonalities, there are also differences coming from different cultures, and that those collective differences, together with the similarities, are what form our history. Works written about developments in non-English language countries (e.g., Biondi 1988, Brower 2010, Parra 1990, Wolfram, 2009), as well as overviews that cover materials generally neglected in English-language works (e.g., Castellan 1955, Gutierrez & Maillard 2004), will assist us in spreading information necessary to correct this situation.

We may get some inspiration from the field of world history. This has been described by one of its representatives, Patrick Manning, in his book *Navigating World History* (2003), as historical work attempting to “portray the crossing of boundaries and the linking of systems in the human past” (p. 3).

While no one will put in doubt the historical importance of developments from English-speaking countries, it is important to bear in mind that those developments may be seen somewhat differently from the perspective of others in different countries. The SPR, to give an example, was known and was influential in France. Some of this work, such as that authored by Myers, was cited and translated in the *Annales des Sciences Psychiques* (e.g., Myers 1897–1900). While Myers was known in France (Alvarado 2010), his ideas seemed to have been less influential there than in England

and in the English-speaking world in general. The point here is that one cannot assume the universal equivalence of specific national events or contributions.

The situation is similar in the history of psychology. For example, Adrian C. Brock (2006:3) has stated in his anthology *Internationalizing the History of Psychology* that American psychologists sometimes assume that behaviorism was international, forgetting that most of its impact was felt in the United States. Following this trend of thought we may ask questions such as the nature of J. B. Rhine's influence in countries other than England and the United States (for example, see Parra 2010).

Although Rhine was influential in, say, Europe, the influence was not as intense as that felt in the United States. Some Europeans conducted research following Rhine's experimental procedures (e.g., Warcollier 1955) but this did not develop as much as it did in the United States where a community of researchers formed around Rhine, when a journal was founded, and when the research transcended parapsychological circles in critical examinations by psychologists (Mauskopf & McVaugh 1980). In addition, it is clear from the content of the well-known 1953 parapsychology congress held at Utrecht that by that time the parapsychological world had not become Rhinean. In fact, there were many who centered their work on theory, philosophy, and spontaneous cases (Alvarado 2009a).

### ***The Past as a Function of the Present***

Another problem is writing about the past in a presentist way, which includes the practice of presenting past developments basically as they relate to present needs, concerns, and ideas, and not on their own terms, as well as the interpretation of the past from the perspective of the present (Stocking 1965, Wallace 2008:37–41).<sup>12</sup> While this is understandable because it helps practitioners to build a professional identity, such emphasis can be problematic. For example, if the account in question focuses solely on work and ideas of the past that are similar to those of the present, we will end with an account that supposedly “explains” the present but that misses many developments important in past times. This may stop justificatory history but will not give us a study of what actually took place (if that is ever possible considering limitations of documentation and context). Parapsychology developed not only through ideas similar to present ones but also from the influence of dissimilar concepts.

***Unpopular Ideas Today.*** Ideas that are not popular or that are undesirable today tend to be neglected in looking to the past. For example, many in parapsychology today do not believe that psi phenomena have a physical basis in the sense that ESP and PK are explainable by the projection



**Asa Mahan**

of physical or biophysical forces from the human body. Consequently, the topic receives little contemporary discussion in the writings of parapsychologists about the past literature. But there is a large literature about such forces published before and during the initial development of psychical research (Alvarado 2006, 2008). For example, many wrote about physical mediumship in terms of force models. This was the case with American educator and clergyman Asa Mahan (1799–1889, see photo). He wrote about the phenomena of raps:

The physical systems of the individuals in these circles may be compared to a galvanic battery which is continuously, but more especially on occasions of the least extra excitement, developing this force. As soon as it is developed to a certain degree, in the organism of the rapping medium, it passes off to some object near, a chair, table, the ceiling, or floor, as the case may be, and produces, in passing into the object, the raps which have astonished the world so much. (Mahan 1855:129)

Other examples of ideas of force to explain physical mediumship were those of English chemist and physicist William Crookes (1832–1919) and Italian psychiatrist Enrico Morselli (Crookes 1874, Morselli 1908), who also wrote about materializations. The same may be said about the concept of ectoplasm (which for some was associated with ideas of force, see Alvarado 2006) and writings about physical ideas of ESP (Alvarado 2008). To ignore such ideas in our accounts of the development of the field because many do not believe today in physicalistic explanations of psychic phenomena may be consistent with some current views, but is a distortion of the historical record. I am not calling for a defense of these forces today, but to acknowledge the existence of a conceptual tradition generally ignored in historical accounts (e.g., Beloff 1993). Failure to do this may not impact on modern parapsychologists' work, but it results in an incorrect account of our history and in the elimination from the current record of aspects that contributed to the development of the field.<sup>13</sup>

We could also learn much from historians of science and medicine who study concepts believed today to be erroneous. This is important not only to get a more complete account of past developments, but to understand the



work and assumptions of past workers. Examples of this include studies of cosmology (Grant 1994), of the humors of Hippocratic and Galenic medicine (Nutton 2004), and other concepts such as the ether (Cantor & Hodge 1981).

Similarly, we also need to pay attention to more recent scholarship in the field of the history of science. There is much modern work whose authors have questioned traditional dichotomies of ideas in the development of science. A good example is the study of occult and mystical ideas in relation to the so-called scientific revolution during the seventeenth century. Part of the more recent literature on the topic presents those ideas as contributing factors and not as factors that had to be conquered in order for science to develop (for reviews see Applebaum 2005 and Henry 2002).<sup>14</sup> A similar situation exists in studies of the historical relation between Western religion and science. Different from ideas of conflict, such as those expressed by English-born chemist, historian, and physician John William Draper (1811–1882) in his book *The Conflict Between Science and Religion* (1874), many historians today believe in more complex interactions, some of which include ways in which religion helped science to develop. These views are evident in several recent works (e.g., Dixon, Cantor, & Pumfrey 2010, Ferngren 2002, Lindberg & Numbers 2003), developments that should make us cautious of seeing parapsychology as a simple conquest of the metaphysical or the occult in general, which would be a partial distortion of the past.

As I have argued in a previous paper, it is possible to see spiritualism, and more specifically, the concept of survival of bodily death, as a factor providing impetus for the development of psychical research (Alvarado 2003). Part of this is that such a concept provided psychical research with phenomena forming some of its subject matter (e.g., mediumship) and some of its conceptual agenda (survival of death). Furthermore, spiritualists had an empirical orientation that nurtured the idea that psychic phenomena could be studied collecting facts, as opposed to only accepting things by faith. Regardless of how their methods were evaluated, a good proportion of the spiritualistic literature emphasized the importance of empirical observations of mediumistic phenomena to form a belief, a value that came into psychical research as well. This was expressed by many spiritualists, among them American judge John W. Edmonds (1799–1874):

There was never . . . a religious creed promulgated among men, which so entirely eschewed blind faith, and so fully and always demanded the exercise of the judgment and the supremacy of the reason. (Edmonds & Dexter 1853:77)



Furthermore, Edmonds stated that the explanation for the phenomena was “capable of being found out by human research . . .” (p. 78).

**Justification of Research Programs.** The past is frequently used by scientists to justify the present. As historians Roy Porter and Mark Micale (1994) have stated in their anthology of essays *Discovering the History of Psychiatry*:

. . . for professional purposes, each generation of practitioners has written a history that highlights those past ideas and practices that anticipate its own formation and consigns to marginal status competing ideas and their heritages. (Porter & Micale 1994:5–6)

In fact, this is one of the main uses of history by practitioners (Graham, Lepenies, & Weingart 1983), and one seen in the writings of both J. B. Rhine (1977) and Louisa E. Rhine (1971). Their accounts of the development of the field present a justification and a defense of the uniqueness of their experimental research program consisting of a sequential account culminating in the research conducted at Duke University and one reducing the field to experimental approaches and to specific phenomena, namely ESP and PK.

Unfortunately such attempts to uphold the importance of the Duke University studies came with questionable rewrites of history. This was the case when Louisa E. Rhine discussed the forms of expression of spontaneous ESP experiences, arguing that before the 1950s “no particular attention had been paid to the manner in which psi was expressed” (L. E. Rhine 1971:46). A brief look at earlier work shows that the topic had been discussed in detail before (Gurney, Myers, & Podmore 1886). It is possible, and this is a speculation, that Louisa Rhine’s inaccurate statement was motivated by her present perspective in which the Duke work was seen as the most advanced and innovative development in the field. But this alerts us to be careful not to misrepresent previous workers in the field to support later views.

**Evidentiality.** The same may be said about discussions of the past based only on evidentiality. It is easy to dismiss many important issues from the past if we believe now that specific phenomena do not exist or are weak evidentially. This is also done with topics and phenomena some find embarrassing and threatening to the scientific status of modern parapsychology, such as the materialization phenomena of mediums, poltergeist reports, or the issue of survival of death. While this serves some present purposes of presenting high standards as researchers and respectable images of the field, it fails to represent the events present in our history and the beliefs of those who worked in previous eras.

**Phases and Stages.** Our need to classify history in periods or in stages is another source of distortion, but also a traditional way to see history. In his widely read and influential *Traité de Métapsychique*, Charles Richet (1922) (see photo) presented an influential classification consisting of the following periods: mythical (from antiquity to Mesmer), magnetic (from Mesmer to the Fox sisters), spiritistic (from the Fox sisters to William Crookes), and scientific (from Crookes on). Later authors continued with variant classifications (Castellan 1955, J. B. Rhine 1953, Sudre, 1956/1962, Xiong 2008), or had sections in their books about the movements of mesmerism and spiritualism (Beloff 1993, Broughton 1991, Irwin & Watt 2007). While these are useful heuristics to organize our past, and they make sense conceptually, they tend to lead us to believe in discrete sequential stages that were not completely so.<sup>15</sup>



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Take for example the case of mesmerism, generally presented as coming before spiritualism. While the heyday of the mesmeric movement was over by the middle of the nineteenth century, belief in magnetic action continued into the late nineteenth and the twentieth centuries (Alvarado 2009b, 2009c). This was evident in the writings of philosopher Émile Boirac (1851–1917), who collected his essays in his widely cited book *La Psychologie Inconnue* (1908). But several others also defended the existence of a magnetic force capable of inducing trance and other phenomena, among them Alexandre Baréty (1844–1918), Hector Durville (1849–1923), and Albert de Rochas (1837–1914) (Baréty 1887, Durville 1895–1896, de Rochas 1887). Consequently, mesmerism was more than a stage taking place before spiritualism. In fact, mesmerism coexisted with spiritualism and with psychical research at different time periods.



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The same may be said about spiritualism, which did not cease by the time psychical researchers from countries such as England, Germany, Italy, and France were conducting investigations. In fact, the movement was going strong and its influence continued into the twentieth century (e.g., Hazelgrove 2000), and it is still around today. Consequently, ideas of chronological



metabolized . . . by institutional medicine” (Méheust 1999b:11).

Boundary work was also shown by psychologists in relation to psychical research in the psychology congresses held between 1889 and 1905. While psychical research was discussed in the congresses, eventually it was rejected, as seen in the proceedings of the Fourth Congress held in Paris (Janet 1901) (see photo). Such process has been referred to as one of separation “between the acceptable and the unacceptable in psychology” (Le Maléfan 1995:624). This, like other examples of rejection from psychologists in the past (e.g., Coon 1992, Sommer In Press), represented attempts by psychologists to bolster their scientific reputation by pushing away what they regarded as undesirable and compromising from their field. This is an area in which we can find a continuity of purpose between older and more recent developments.

**Criticism.** The work of critics is also neglected in historical accounts authored by many parapsychologists (for an exception, see Zingrone 2010). My impression is that this neglect may be related in part to the fact that many parapsychologists feel beleaguered by critics, and believe that critics are basically destructive and negative in their work, contributing nothing or little to parapsychology. But while one may understand this reaction, we need to keep in mind that the history of the discipline is not formed solely by those who have produced positive evidence for the existence of psi. Instead it is formed from the interplay of a variety of factors and forces, among them the writings and arguments of critics. A history that explores only the achievements of those defending the existence of psychic phenomena is only half of a discipline. To understand the development of parapsychology research, we also need to study the writings of critics because they were part of the intellectual milieu in which concepts and methods developed. One such example is the criticisms of psychologists such as Joseph Jastrow (1863–1944), who frequently wrote to criticize psychical research in attempts to establish a difference between psychology proper and psychical research in terms of quality of evidence and training of practitioners (Jastrow 1889, see also Coon 1992). Other examples of critics include figures such as William Carpenter (1813–1885) and Pierre Janet (1859–1947), whose work did much to develop ideas of dissociation and automatic mental action (Carpenter 1877, Janet 1889). Regardless if the critics were interested in constructive criticism, or how parapsychologists feel today about their objections, their writings were influential at the time and affected the reception of work about psychic phenomena.

Of course the issue gets complicated when we recognize that we cannot always classify individuals neatly as proponents or as critics. Almost everyone in parapsychology is also a critic when it comes to specific

methodologies, phenomena, or concepts. Many figures from the past were both critics and proponents at the same time, depending on the topics of discussion. An example is the well-known SPR critic Frank Podmore who defended telepathy in his book *Apparitions and Thought-Transference* (1894). But he is also remembered for his skepticism about poltergeists and physical mediumship, as can be seen in his *Studies in Psychical Research* (1897). His approach contributed much to the critical mentality prevalent in the early SPR, although not everyone agreed with his analyses.

Those individuals who were negative overall in the sense of negating the existence of ESP and other phenomena were also part of the development of the field, a perspective recognized by professional historians but which does not seem to be shared by some practitioners. In any case, psychical research is not only a defense of phenomena, it is a critical approach to a group of phenomena with various implications about the nature of the mind. The same should be true in the case of its history.

### Concluding Remarks

In this paper I have discussed various practices that distort our views of past developments in parapsychology. I have focused on problems of the neglect of groups and individuals, seeing the past as a function of the present, and placing an emphasis on progressive aspects of the field. I have not discussed many other aspects or topics that are generally ignored by parapsychologists when they talk about the history of their field, and which are not strictly examples of distortion. This includes the recognition that our past is not only the research, theoretical, and methodological work conducted in relation to psychic phenomena. Our past includes issues other than what parapsychologists do. An example is the role of psychics and mediums in terms of their goals and life situations, as well as in terms of their relations to researchers and to other functions they have played in shaping the field (Alvarado 1993), and the study of particular cases from the past, (e.g., Hunter 2005). Both can offer more to our understanding of past developments than an analysis of their evidential value.<sup>16</sup>

In criticizing the writings of parapsychologists about the past of their discipline, we must remember that their goals are different from those of the trained historian. Theirs is not an attempt to do formal history or to document the above-mentioned wider aspects of the field. Their approach in writing about the past exists because it fulfills disciplinary needs and interests. But still, and regardless of their right to pursue their own agenda, we need to be aware that the end result also produces distorted views of the past of the discipline.

I am afraid this paper has taken the form of a list of complaints. But

my main purpose has not been to vent. Instead I want to caution fellow parapsychologists about selected problems limiting our views of the history of the field. These issues are not only related to parapsychology, but also to the way other professionals discuss the past of their disciplines, as seen in some of the histories of science written by scientists (Brush 1995, Graham, Lepenies, & Weingart 1983). However, and as stated at the beginning of this paper, no overview of past developments is free of problems, and this applies as well to the work of professional historians. The enterprise is always a subjective one involving selection of sources and events, not to mention interpretations of those materials,<sup>17</sup> something which determines our views of the past.

To conclude, it is my hope that my discussion of the strategies and practices that distort our views of the past will help parapsychologists to obtain a better understanding of the dynamics of their field, including a view of the range of factors involved and of the subjective nature of writing about the past. Such range is a constant reminder that the meaning and construction of the past is anything but simple.

### Notes

- <sup>1</sup> For discussions of aspects of these differences, see Brush (1995), Forman (1991), Reinhold (1981), Turner (1990), Windsor (2001), and various papers in the anthologies of Gavroglu and Renn (2007) and Söderqvist (1997). There are also other types of individuals who produce valuable writings, among them professional writers of different sorts.
- <sup>2</sup> Both approaches may be combined, and both have their own problems (Windsor 2001). For discussions of methodological and conceptual approaches in the study of the history of scientific disciplines, see Hessenbruch (2000), Krag (1987), Olby, Cantor, Christie, and Hodge (1990), and Wallace (2008).
- <sup>3</sup> On this topic, see Graham, Lepenies, and Weingart (1983). I have discussed aspects of this in relation to parapsychology (Alvarado 1992). It can also be argued that professional historians have their own agendas related to the concerns of their discipline (Windsor 2001).
- <sup>4</sup> For summaries of papers about such figures, see "Forgotten Pioneers of Parapsychology" (2007).
- <sup>5</sup> Gauld (1968) emphasized the work of men such as Gurney, Myers, and Sidgwick. But he also mentioned the contributions of many others.
- <sup>6</sup> Affirmations that some SPR leaders were scholars and scientists are not wrong, and some spiritualists may also be included in these categories. But such emphasis may have a function beyond the merely descriptive, that of enhancing the respectability and prestige of the organization. Of

course the fact that few parapsychologists mention the spiritualists may reflect their ignorance on the subject.

- <sup>7</sup> On Wedgwood, who was Charles Darwin's cousin and brother-in-law, see *In Memoriam* (1891).
- <sup>8</sup> On Moses, see Myers (1894/1895) and Podmore (1902:Volume 2, Chapter 5). Current views of SPR history are also centered on nineteenth-century developments. While there is no doubt this period was of basic importance, this leads us to neglect from the historical record the later contributions of individuals such as Theodore Besterman (1904–1976), Everard Feilding (1867–1936), Helen Salter (1883–1959), William Salter (1880–1969), H. F. Saltmarsh (1881–1943), and G. N. M. Tyrrell (1879–1952).
- <sup>9</sup> Carrington, born in St. Saviour, Jersey, one of the Channel Islands, developed his psychical research career in the United States (Tabori 1972:24–70, see also Alvarado & Nahm 2011).
- <sup>10</sup> I am grateful to Renaud Evrard for providing me with the dates for Bisson.
- <sup>11</sup> Some say defensively, but not completely mistakenly, that English is the current universal language for communication in science. But such comments reflect a myopic view about the complexity of our modern world.
- <sup>12</sup> Pickering (1984) has argued that scientists have frequently evaluated ideas from the past using current ideas of what is valid or not, what he refers to as retrospective realism:

Having decided upon how the natural world really is, those data which supported this image were granted the status of natural facts, and the theories which constituted the chosen world-view were presented as intrinsically plausible. . . . (Pickering 1984:404).

The explanation or determination of how something was developed or constructed in the past should not be based on current consensus in a discipline, but on a contextual study of the actions, meanings, and ideas at the time the initial work was conducted.

- <sup>13</sup> Interestingly, the old literature about forces has also been ignored in discussions of parapsychological theory (e.g., Irwin & Watt 2007:Chapter 8, Stokes 1987).
- <sup>14</sup> Actually, this trend has been going on for many years (Henry 2002).
- <sup>15</sup> The artificiality of chronological and conceptual headings frequently used by some to organize articles (e.g., Alvarado 1989b, L. E. Rhine 1971) should also be kept in mind.
- <sup>16</sup> Other topics parapsychologists may explore to enlarge the range of their



views of their field include issues such as attempts to popularize the field, the reception of research and ideas, interactions with other fields or disciplines, professionalization, and the role of overarching concepts such as vitalism and ideas of evolution in relation to psychic phenomena.

- <sup>17</sup> The use of particular concepts to guide historical analysis—issues such as modern views of gender relations, economics, and professionalization—may also be seen as a distortion of the past and as a validation of history as a profession.

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## ESSAY

### The Review Reviewed: Stop Publication Bias

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**Abstract**—This manuscript describes our past experiences with reviewers and the review procedures that are currently used in the medical sciences. We conclude that reviewers all too often are biased, whereas scientific discussion should be based on substantive comments and without prejudice. In our opinion, subjective arguments for rejection of manuscripts constitute a serious threat to evidence-based medicine. Since peer review should aim to facilitate the introduction into medicine of improved ways of curing, relieving, and comforting patients, a more objective review system with greater scope for the publication of divergent opinions is clearly needed to ensure that a literature search does not merely produce a plethora of articles with mainstream opinions. Our recommendations for a peer review system are: (1) No more anonymous reviewers; (2) The reviewer must concentrate initially on two questions: (a) was a real problem formulated in this manuscript? and (b) is the conclusion—if proven—relevant for practical situations?

**Keywords:** publication bias—evidence-based medicine—review

### Introduction

Having a good, well-structured idea is one thing; getting it published is something else entirely. In science, however, the first should—more or less automatically—lead to the second. It has long been recognized that scientific revolutions meet stiff resistance. The battle that the 2011 Nobel Laureate for Chemistry, Daniel Shechtman, had to fight for years against established

science is only a recent albeit shocking example. This phenomenon has already been described long ago by Thomas Kuhn (1970), and Juan Campanario (Campanario & Martin 2004, Campanario 2009) has published many examples of the resistance encountered by prominent physicists in gaining acceptance for their theories. If the problems encountered in the exact sciences are so great, what then are the prospects for medical science?

The Laureates of the 2005 Nobel Prize for Medicine had to infect themselves with the bacterium *Helicobacter pylori* in order to prove its role in gastritis and peptic ulcer disease. An idea borrowed from John Hunter, who in 1767 inoculated himself through the urethra with pus from a gonorrhoea patient. Why have we learned so little from the past? It seems that reviewers and their reasons for opposition will never change. And not just when it relates to paradigmatic changes, but even more so when it concerns more modest scientific findings. This means that other measures are required to ensure that medical science becomes more open to new and divergent ideas.

Another reason for tackling the problem of incomprehension and even opposition to such ideas is that it leads to publication bias. Publication bias is defined as “the tendency on the part of investigators, reviewers, and editors to submit or accept manuscripts for publication based on the direction or strength of the study findings” (Song et al. 2010). The first step toward the prevention of publication bias is to make the public aware of the sometimes detrimental effects of publication bias and the need for the results of all studies to be made accessible (Song et al. 2010).

We agree, of course, that evidence-based medicine (EBM), if correctly applied, is a highly logical and systematic approach to clinical practice. However, it is conditional on two constraints: (1) all evidence needs to be in the literature; and (2) all levels of evidence must be evaluated. The second of these often tends to be forgotten by many assessors for guidelines and systematic reviews. The main problem, however, and one which does not get much attention, is that not all evidence ends up in the literature. This is what we want to discuss here, because the validity of research synthesis based on published literature will be threatened if published studies comprise a biased selection of all the studies that have been conducted.

For what is the use of the usual literature studies in EBM if the literature on which they are based is a collection of mainstream works which seems to have come about more by “mutual admiration” rather than by peer review? Evidently, people prefer to see many investigators taking little steps along the beaten track rather than fewer investigators with divergent and sometimes pioneering ideas. That is a pity, since, as Proetz (1964) once wrote:

Perhaps at first glance it may seem immaterial whether science is advanced by a handful of geniuses in leaps and bounds, or by a million average citizens in creeps and budes, but there is a waste of both money and manpower, a spotty distribution of knowledge, and the necessity of having to weed out the errors and start over.

What is the point of weighing up the pros and cons if you can only find the pros because the cons have not been published? As Evans (1995) pointed out:

It is to use evidence in the manner of the fabled drunkard who searched under the street lamp for his door key because that is where the light was, even though he had dropped the key somewhere else.

We will show, also from our own experience, that the review process is all too often a subjective activity and that rejections are frequently made on improper and hardly scientific grounds. We do not get frustrated by well-argued rejections, and we are also well aware that journals get sent far more copy than they could ever publish. We are aware that even the examples given below are debatable. All we ask is that a scientific discussion be based on real substance and without prejudice.

We would argue that an appraisal by a reviewer requires a quite different strategy from the assessment of an already published article for a guideline, a critically appraised topic, or a systematic review. We consider that the review process should aim to publish a number of different insights in order to avoid ending up with *evidence-biased medicine*.

## **Manuscripts and Reviewers**

### ***Title and References***

The first place where things can go wrong in a manuscript appraisal is with the title and the references. The requirements for a title differ according to the journal. Should it be gripping or businesslike? How long should it be? Plenty of advice is available on this matter (Fraser 2008, Hall 2008).

Criticism of the title can even contribute to a rejection. We believe that it is up to the Editor to ask for a change of title if he thinks it necessary. It is not the task of the reviewer to make more than a minor comment on this.

Many journals ask authors to limit the number of references. Only cite relevant (and possibly contradictory) articles in order to place your story in its context, not to demonstrate how much you have read. Back in 1964, Proetz already wrote that there was far too much redundant name dropping and that it would be refreshing to read something in medicine without being



constantly interrupted by what Kussmaul and Rokitansky wrote about it in 1878.

Reviewing the contents on the basis of the title and references, as sometimes happens, is—in our opinion—like judging a wine on the basis of the label on the bottle.

### ***Length and Form***

How long or how short should a manuscript be? When have you given too little background information and when too much? Differing opinions on this matter should not form grounds for rejection. Of course, an article should be written in a short and concise style, but we believe that it eventually should be the task of the Editorial staff of a journal to judge this and, if necessary, make improvements.

Moreover, the length of a manuscript can give an impression of the underlying thought. A two-page commentary is not a trial and should not be judged as such. An article can sometimes be too long for the regular version of a journal, but this should not form a problem for a supplement.

Another factor is the form, the “personal” writing style. It is, of course, advisable for authors, especially if they are not native speakers, to have their manuscripts textually and linguistically checked by others. But the reviewer could also make suggestions for improvement, and journals could and should make a greater contribution. After all, some authors write better than others. But this is quite a separate issue from the significance of the message.

### ***Contents, Type of Investigation, and Level of Evidence***

Should all articles be subjected to the same appraisal? We think not. A case report or an opinion article is not a systematic review and should not be judged as if it were. We do not believe that the recommendations, as described in the CONSORT statement, are relevant here, and reviewers should realize that the overzealous application of rules, which were drawn up in connection with recommendations for the proper performance of randomized controlled trials (RCTs), to completely different sorts of investigation is quite absurd. Small-scale retrospective studies are recorded after the patient has left, and the procedures followed can no longer be changed. Needless to say, by then you no longer have all the relevant information available. Retrospective studies, opinion articles, and narrative reviews also have a practical role, especially for theoretical orientation. [There is no need here to mention the practical shortcomings of RCTs, since enough has been written on this subject in the past (Kaptchuk 2001, Rothwell 2005).]

In the final determination of the level of evidence needed for a guideline or systematic review, however, proper account must be taken of the methodology and the type of investigation.

Expert opinion is difficult to substantiate with systematic references, but is not without significance. Furthermore, it is important to note the difference between an expert who just makes an unsubstantiated statement and an expert opinion based on literature.

An expert who gives an opinion based on studies at a particular level of evidence can, in fact, be considered as giving an expert opinion at *that same level*. (Usually in EBM, systematic reviews and large trials are considered level 1, cohort studies level 2, case series level 3, and expert opinion as level 4. But an expert giving his opinion based on results from a level 1 trial or review might perhaps better be viewed as level 1 instead of level 4.) It is the ultimate combination of science and specialist know-how, in other words of EBM:

The practice of EBM means integrating individual clinical expertise with the best available external clinical evidence from systematic review. By individual clinical expertise, we mean the proficiency and judgement that individual clinicians acquire through clinical experience and clinical practice. (Sackett et al. 1996)

So why not assign a due level of evidence to this? If expert opinion is totally worthless, you might—for certain medical problems for which there is no other useful evidence available—just as well ask the gardener as ask a doctor with many years' experience.

### **Knowledge and Practical Use versus Methodology**

How do you get a divergent viewpoint, based on years of experience, published? Nowadays, many medical students are better able to judge the methodological quality of an article than its clinical relevance. Reviewers who are excellent in statistics, epidemiology, and conducting systematic reviews and guidelines have sometimes spent too little time gaining practical skills and experience. Doctors are not statisticians, and it is hardly possible to excel in both fields. Insufficient knowledge of pathology, anatomy, pharmacology, and pathophysiology, and also a lack of clinical experience, can lead someone to reject interesting studies on the grounds that they employ “poor methodology.” This quite apart from the fact that—according to statisticians—the statistics in many published articles often leave a lot to be desired and the statistical knowledge of many doctors is evidently inadequate (Ioannidis 2005).

This major problem also affects many systematic reviews and guidelines. Methodologically well-designed studies are presented, while the “flaws” in the definitions used in the Introduction and the Discussion cannot be recognized by non-experts, who are primarily the ones who carry out the literature search. However, just like Greenhalgh (2010), we believe that when interpreting a study, readers need to know how it relates to existing knowledge.

Many authors and reviewers interpret findings narrowly, failing either to identify previous studies or to place their findings in the context of previous studies.

Greenhalgh (2010) wrote that she was concerned that courses in EBM often concentrate too much on critical appraisal and apply insufficient critical evaluation to the other steps: “Yet if you have asked the wrong question or sought the wrong answers (from the wrong sources), you might as well not have read any papers at all.” That is like asking a restaurant dishwasher who has read a few cookbooks to prepare a meal. Although the meal might contain all the right ingredients, it is questionable whether it would taste very good.

We should like to take as an example the case in which it was recognized that a prospective study of the effects of Botox in the *corrugator supercilii* muscle was not a double-blind randomized trial, but the fact that Botox has an excellent and clearly visible effect on the face and therefore cannot be blinded was not recognized. In short, you only need to write that an effective randomization was applied and that independent-effect appraisers were employed, in order to be free from any criticism of the medical content.

In addition, it is worth quoting the long-standing Latin aphorism *ubi pus, ibi evacua* (“where there is pus, evacuate it”). There has not been much research into this matter during the last century, probably because this is something that every doctor is expected to know. On the other hand, plenty of research has been done into treatment by means of antibiotics. We now find that we often have to persuade colleagues to operate for example in cases of mastoiditis, even when it has caused meningitis. This is because—according to these colleagues—there is no evidence that an operation is useful, whereas there is evidence that antibiotics can often, eventually, solve the problem. Of course, an antibiotic therapy can have an added value, but we must not misplace the burden of proof. Although in cases with not very ill patients we have no objection to starting with a good second-best treatment, it is questionable whether antibiotics as a single therapy are as effective as the evacuation of the pus combined with antibiotics. The findings of a study into this subject should not only be based on mortality or ultimate cure, but should also take account of complications, morbidity,

length of hospitalization, the duration of antibiotic administration with the concomitant complications, and on the other hand the complications that could arise from an operation. Until genuine comparative evidence to the contrary is produced, we consider that expert opinion for evacuation—combined with antibiotics—will continue to be the gold standard. We would do better to base our decisions on the collective experience of thousands of clinicians treating millions of patients. (In other words: 50 million years of evolution and 50 years of research have demonstrated that mothers' milk is healthy for babies.) Of course, if the area of operation would involve excessive risk of operative complications due to anatomical circumstances, then the preferred treatment would shift to antibiotics. In that case, the treatment of second choice might then be the best option.

The same sort of problem is faced by Ridge (2010), to judge from his article "We show pictures, they show curves." The fact that radiotherapy and chemotherapy, possibly in combination, can constitute good modes of treatment in certain cases does not mean that surgical intervention is not equally effective in the case of tumors of the head and neck. Years of surgical experience by the real experts give results that cannot be expressed in simple graphs.

Another factor is that reviewers are sometimes unable to conduct certain operations themselves. There is no shame in this, since specialist operations, as the term itself implies, cannot be done by everyone. Unfortunately, these reviewers have the habit of attributing other people's good results to mere luck or to the placebo effect. Or even worse, the reviewer asks himself: "Surely the authors do not really intend to . . .?"

Such reviewers consider an endoscopic browlift, a microvascular decompression, or the severing (neurectomy) of the vestibular nerve as excessive techniques and the authors as "trigger happy." But just because you do not yourself perform a particular operation, you cannot leave patients to suffer lifelong pain, facial spasm, or dizziness while relatively routine operations exist, which—in experienced hands—could solve the problem. The decision as to the most appropriate treatment should be taken in the context of clinical practice and not by incompetent reviewers. After all, it is not the reviewer's task to judge what is onerous for the patient. This is a matter for individual doctors to discuss with their patients.

Another source of annoyance in some cases is reviewers' clear lack of general knowledge, although this is usually clothed in the suggestion that the concepts will be unfamiliar to the journal's readers.

Bradford Hill criteria? "Never heard of it." And these people have not even taken the trouble to find out that these are criteria drawn up by one of the foremost epidemiologists of the last century. How can you call yourself

a reviewer if you possess so little knowledge? How is it possible for a doctor to be unable to make any sort of link between smoking and cancer? And what sort of reviewer are you if you will not take the trouble to follow up references?

There is no shame in turning down a request for a review if you do not consider yourself sufficiently expert for the task. Unfortunately, this is not generally recognized.

And then there are the reviewers without an understanding of Bayesian statistics. No shame in itself, but no reason to reject an article; as if Bayesian statistics is something from another planet. They should ask a statistician to give an opinion on the subject if they do not understand it themselves. And if the readers and reviewers are really so ignorant, perhaps they should take things on trust.

### **Rules for Reviews**

Distinguishing genuine research from poor-quality endeavors of well-meaning amateurs is the primary task of reviewers in EBM (Greenhalgh 2010). Naturally, the goal is to aim at high-quality research, but this also—and above all—implies high-quality ideas or theories. Good research should be judged not only on the quality of the methodology, but also on the merits of the research goals and their relevance.

And do these well-meaning amateurs really have such bad ideas, or do they lack the extensive research facilities (including statistical and epidemiological advice) available to academic institutes? For it is a well-known problem that conducting research is becoming more and more difficult for a peripheral clinician (Warlow 2005).

Some time ago, we received a request to review a manuscript accompanied by the specific request to refrain from making comments that we would not like to receive ourselves. An excellent piece of advice that all journals would do well to adopt, since it is all too apparent that not all reviewers do this of their own volition. They could have read in *How to Write a Paper*: “Be kind—it’s a privilege, be helpful, and above all be fair and honest” (Hall 2008).

A reviewer recently insulted us with text such as: “undergraduate university education has passed the authors by,” “amateurish step backward,” “hidden agenda of discouraging some Dutch ORLs [otorhinolaryngologists] from doing excessive numbers of surgeries for personal gain,” and “would harm the high and growing reputation of the Journal.” If this reviewer had furnished some factual and substantiated comments, he might have demonstrated what worthless authors we were and would not have needed to

couch his opinions in such terms. In view of the fact that his comments had absolutely no bearing on our line of reasoning, references, or conclusion, they must have been purely a personal attack by a frustrated, arrogant colleague. An additional factor was that by accepting these comments, despite our complaint, the Editor was indirectly insulting the other reviewer, who had given it a good review.

We believe that a reviewer should no longer be allowed to give a personal opinion unless the anonymity of the review process is removed. Anything which you only dare say behind the veil of anonymity should not be said at all. Subjective, unsubstantiated opinions of reviewers are really not relevant. For example, we once had a reviewer who thought that he would have chosen different parameters in an appraisal system for the degree of facial dysfunction, but if this is not backed up by sound arguments, it is no more than an irrelevant remark. If a reviewer wishes to express a personal opinion, he should write his own article. At the most, he should confine himself to asking why something specific has not been included in the system and suggesting that we comment on this in the Discussion section.

Another point to be observed by a reviewer and which, in our opinion, should be included in the request for review is: no hair-splitting about relatively irrelevant matters. Try, rather, to imagine why a particular concept has been chosen. If someone mentions a follow-up period of 3–30 months, it is a non-issue to point out that it should be at least four months, all the more so if there is no evidence from the literature that four months is the gold standard and this applies to many patients in that situation in any case. One could perhaps take the results slightly less seriously or, preferably, ask the authors to explain why they chose for the one rather than the other. A rejection on the basis of this one month is quite illogical.

If authors have written a retrospective study, it is pointless to ask for prospective results. If relevant, the authors could be asked to comment on this in the Discussion. The possibility of carrying out a prospective study in the future will then remain open to any doubting reviewer—or ultimately the reader—if the results are called into doubt.

Back in 1964, Proetz stated that he considered the most important part of an article, i.e. what you should read first, to be the conclusion. If the conclusion is not worthwhile, then there is no point in reading the rest of the manuscript. Conversely, the more meaningful, innovative, or disparate the conclusion, the more reason there should be to publish. After reading the conclusion, the reviewers should first evaluate the medical content with respect to accuracy and relevance, and only then is the methodology worth appraising—if necessary in collaboration with statisticians, epidemiologists,

and/or other outsiders. We consider the first priority should be to present all opinions, from all levels of evidence. Only after this has been done—in this particular order—is it possible to rank the pros and cons, and only then will the public be well-informed about all the different opinions. This would also make it possible, in certain well-justified cases, to deviate from protocols drawn up on the basis of EBM.

We would prefer not to use formulas, such as the Fail-Safe Number (FSN), to correct for the possibility of not all the evidence being available (Orwin 1983). (And to think that Orwin used this FSN to correct for the fact that studies that do not demonstrate any effect might be more difficult to get published. The fact that controversial opinions are actively boycotted is particularly difficult to express in formulas.)

There is also a generally accepted opinion that the vast majority of research is published in low-impact journals, where peer review is undoubtedly less thorough (Greenhalgh 2010). In the field of otorhinolaryngology (ORL), we would make the point that we have read better review comments on articles we have published in *B-ENT*, a journal positioned somewhat lower in the journal ranking, than those given by many higher-ranked journals within the field. We would almost turn the argument around. The major journals ask big names to conduct their reviews, but these reviewers, with their often big egos, sometimes have difficulty in distinguishing between an objective review and their own personal opinion.

We consider that Editors would do well to take criticism of reviewers seriously and to be more open to asking for second opinions if prompted by justified questions from authors. Greenhalgh (2010) described decision-making by GOBSAT (good old boys sat around a table) for the purpose of arriving at a guideline. This process seems to be even worse when applied to the publishing of a journal. If complaints are made about a reviewer, it is not unusual to be fobbed off with remarks such as “the reviewer is one of the best minds in the business” or “the reviewer is this country’s leading expert.” As if this is of relevance when someone rejects a manuscript purely on account of prejudice and without sound arguments. Unfortunately, it seems that an Editor seldom overrules his Associate Editor. Whereas it is quite normal in legal proceedings for a higher court to overturn a decision, medical science continues to cling to an unshakable belief in a colleague’s infallibility. Even worse is when a request for a second opinion is rejected by the same Associate Editor who had examined the review in the first place, so that the case never gets to be examined by another person, possibly higher in the hierarchy. (In a legal setting, such a thing would be virtually inconceivable in the civilized world.)

Unfortunately, it also happens all too often that articles from leading



journals—chosen because of their titles and the impact factor of the journal—are cited inappropriately, i.e. where they are not specifically applicable, or are misused in practice. Editors should promote self-correction in science and participate in efforts to improve the practice of scientific investigation by publishing corrections, retractions, and letters critical of articles published in their own journal (Altman 2002). That applies, in our opinion, not only to errors in the article, but also—and in particular—if an article is repeatedly misused.

One of the most important messages in this manuscript is to point out to Editors and particularly reviewers that progress in knowledge is best achieved by debate. This is initiated by the publishing of divergent ideas and theories. Once such a divergent theory has been published, it can be objectively evaluated by colleagues in the same specialist field by means of a process of verification or falsification. If divergent ideas are (deliberately) written off, peers or confrères will never even have the opportunity to take cognizance of them.

In short, we believe that it should be made less problematical to get solidly substantiated, divergent opinions published. Our recommendations for a peer review system are:

- (1) No more anonymous reviewers
- (2) The reviewer must concentrate initially on two questions:
  - (a) was a real problem formulated in this manuscript?
  - (b) is the conclusion—if proven—relevant for practical situations?

If the answers are in the affirmative, the study should stand a good chance of being published. At this stage, the reviewer can, of course, ask critical questions about the methodology and request elucidation on ambiguities.

Peer review must aim to facilitate the introduction into medicine of improved ways of curing, relieving, and comforting patients. The fulfillment of this aim requires both quality control and the encouragement of innovation. If an appropriate balance between the two is lost, the peer review will fail to fulfill its purpose (Horrobin 1990). Or, as MacAuley (cited in Hall 2008) wrote: “What matters is originality, importance, and validity.” And, we would like to add, in that specific order!

### **Conclusion**

A more objective review system with greater scope for the publication of divergent opinions is needed to ensure that a search through evidence-based medicine does not merely produce an accumulation of articles with a

mainstream opinion and with a mainstream conclusion. The present, overly subjective system leads to publication bias.

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## BOOK REVIEWS

**An Introduction to the Psychology of Paranormal Belief and Experience** by Tony Jinks. McFarland, 2011. 273 pp. \$35.00 (paperback). ISBN 9780786465446.

Readers of Dr. Tony Jinks's first major literary offering to the world of parapsychology might be surprised that, in his Preface, he makes two salient points that are not to be taken lightly: First, he criticizes as "facile" and witless the skeptical assumption that paranormal beliefs and experiences can simply be attributed to drunkenness, stupidity, gullibility, or emotionalism, and second, he claims that he is not an expert writing for experts. Regarding the former comment, it does justice to the book to state that Dr. Jinks is not only accusing lay persons of making these casual assumptions, but he targets professionals (e.g., clinicians) who base those same inaccurate diagnoses on mainstream psychological theory. My experience from over a decade in the field suggests that this form of "professionalism" is not only generally manifest in the aims underpinning the *psychology* of parapsychology, but I also see a new form of radical skepticism surfacing that is bent on pathologizing, in a growing number of creative ways, those who profess paranormal beliefs, or avow paranormal experiences. I believe this trend is part of a bigger movement, and is a form of defense on the part of academia, and an indictment of the competitive times in which we live. While Dr. Jinks does not quite "go there" so to speak, in terms of fully addressing the sociodynamic forces that guide academia in its trajectory, one sees the writing on the wall as one journeys through the highly eloquent and erudite (one might even say entertaining) pages of Dr. Jinks's book. And if it isn't so apparent to the reader, the quote from another Australian parapsychologist, Professor Harvey Irwin, says it all when he points out that the "implicit objective" of psychological research into paranormal belief is to demonstrate "that believers . . . are grossly deficient in intelligence, personality, education, and social standing" (p. 62). Actually, nothing could be further from the truth (for evidence, see the review by Thalbourne, 2010).

Regarding Jinks's latter point, and having read the book assiduously from cover to cover, I feel the book is sufficiently "expert" in the way it is compiled of material drawn from diverse but relevant sources. Perhaps more importantly, the book takes an approach that is hitherto sidestepped by those hailed as experts in the area of paranormal belief and experience.

In that sense, this book is crucial to the field of parapsychology for taking issue and discussing oft-ignored facts to do with the assumed validity of the various constructs that define and depict paranormal belief and experience, and the taken-for-granted methodologies (including the measures) we use (see especially Chapter 2). The book hints at a need to address these key issues, and it is suggested that there will be no true progress for the field if they continue to be ignored, no matter what breakthroughs we may make outside the areas of criticism raised in Jinks's book. These points will become clearer as the review unfolds.

After an Introduction featuring some key terms (e.g., extrasensory perception = ESP, and psychokinesis = PK), the book is then divided into two main sections—what can be explained, and what cannot be explained. Part 1 deals with the “explicable.”

In Chapter 1, various ways of testing alleged psychic abilities are introduced. Jinks alerts us to the fact that the outcomes of these tests can be variously interpreted—first, in terms of the effects per se, and second, in terms of the possible *explanation* of those effects. For example, in the first case, telepathy (mind-to-mind communication) might be clairvoyance (“seeing” the target in the mind), or vice versa. However, in the second case, the scientific psychologist might think that terminology is inconsequential, and argue that all that has been demonstrated is fraud, leaking of information (a.k.a. sensory leakage), and a variety of other likely experimental design flaws. Jinks presents evidence that undermines these assertions. He then describes some effects well-known in parapsychological circles: psi-hitting (which he calls “psi-present”), psi-missing, the decline effect, and displacement. Jinks continues to play Devil's Advocate in this Chapter by presenting the skeptic's explanations for these effects. The problems with meta-analysis are also featured.

In Chapter 2, superstition and paranormal belief are outlined and contrasted, and Jinks ties in the concept of superstition with psychological theories going back to Pavlov and Skinner. Jinks is basically arguing, from the scientific psychologists' perspective, that many of our beliefs and behaviors (which are really responses) have nonsensical associations tracing back to one unrelated stimulus or another. Many of our unusual actions and thoughts come about as part of the conditioning process—we can be like rats or pigeons that behave in certain “superstitious” ways (e.g., turning on the spot without reason *coupled with* pressing a lever to release a food pellet). Such responses emerge merely because belief and/or action (relevant, or not) helps shape behavior if desired outcomes follow. The reader begins to see where Jinks is going in Part 1—he is setting the agenda of the scientific psychologist. But the scientific psychologist misses the

import of irrational thinking—it can actually have life-preserving value, and Jinks gives examples (e.g., see p. 59). Chapter 2 is also important because it points out the problems associated with measuring paranormal belief and experience.<sup>1</sup> Issues to do with the *validity* of paranormal belief questions are also raised in this Chapter.

In subsequent chapters on the manner and means by which we witness the paranormal, emphasis is given to illusions, hallucinations, neurological dysfunction, and induction of hallucinatory encounters, respectively, each of which are given as major (and normal) explications of paranormal belief and experience.

Chapter 3 considers paranormal experiences as possible illusions (misinterpretations of reality). An illusion needs an external source as a starting point for the mind/brain to go to work on. Jinks looks at the psychological factors that determine illusions, and phenomena such as electronic voice phenomenon (EVP, i.e. voices from the dead heard on electronic equipment such as radios and taperecorders) are explained in terms of *perceptual set* (the way we interpret new experiences based on past experiences). Jinks also describes an experiment showing a video of an allegedly psychic demonstration (i.e. a magic trick)—non-believers (so-called “goats”) remember seeing more sleight-of-hand and trickery than psi-believers (so-called “sheep”). Of course, in a completely different experimental design, we are left wondering what goats would “see” and recall if they witnessed a genuine paranormal demonstration. Logically, they should see sleight-of-hand and trickery that was never there! Being philosophically opposed to paranormal explanations, how else could they explain what they saw? Both experiments would prove that *everyone* is susceptible to illusion. Thoughtfully, Jinks then gives examples that touch on this point (see pp. 105–106).

Chapter 4 alerts us to the nature of hallucinations, and discusses the ways in which our minds construct reality purely from inner mental experiences that have nothing to do with the environment. Hallucinations intrude into our *running world model*. Escape-from-self, dissociation, fantasy-proneness, etc., are a few of the standard explanations for paranormal experiences ranging from ESP/PK and poltergeists to UFO sightings and UFO abductions. While the psychological and medical professions might generally be satisfied with these categorical explanations, Jinks again raises some rather challenging exceptions.

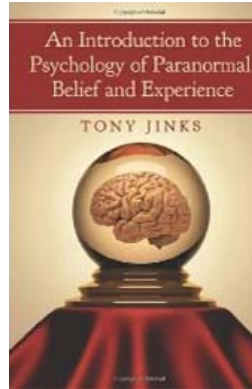
In Chapter 5, we advance to the topic of neurological dysfunction, which appeals to brain models as explanations for paranormal phenomena. The main topic in this chapter is epilepsy, which most people take to mean the grand mal type of disorder involving muscle spasms/convulsions and loss

of consciousness, but petit mal epilepsy also features, which is associated with the temporal lobe, and can even incorporate the limbic (emotional) system and cortical areas. Since the temporal lobe is a storehouse of a lifetime of experiences, the implications are profound. Any misfiring of neurons, insofar as it is in the temporal lobe, constitutes so-called temporal lobe epilepsy (TLE), which can mean images and memories are activated during TLE, but with no physical seizures. From here, it is a short step (or a giant leap) to propose that paranormal experiences are a symptom of TLE. The breadth of the experience can be characterized not only by visions, but by sounds, smells, tastes, heightened emotions, spatial distortions, floating, a sense of presence, and some physiological symptoms (e.g., changes in heart rate, respiration, and blood flow). For the experient it is all very real, but the observer would detect very little, if anything. This chapter is not only informative; it puts up a strong case toward understanding the nature of many *subjective* paranormal experiences, but I stress the word *subjective* in that neurological theories give us no explanation about the processes involved in objectively observable and demonstrated ESP and PK events. Even if we confine ourselves to *subjective* paranormal experiences, there is still the assumption that the seizures explain them, which only begs the question: What causes the seizures? This issue is taken up in Part 2.

In Chapter 6, Jinks investigates some of the hypothesized external sources of hallucinatory encounters, and these include magnetic, electromagnetic, and geomagnetic sources (e.g., our earth's core, our sun, the Milky Way Galaxy), and local earth energies that result from tectonic strain. These fields and forces affect our brains, particularly those areas (i.e. the temporal lobe) that produce subjective paranormal experiences. This is an intriguing chapter, but Chapter 7, which brings Part 1 to a close, is a more general and considered discussion and critique of such paranormal theories that appeal to tectonic strain, transcranial stimulation, and electromagnetism. These constructs are too complex and time-consuming to delve into here, and readers are encouraged to explore this area at their own leisure. However, it should be noted that mainstream commentators within the various professions (i.e. psychology, psychiatry, etc.) also dispute these and other explanations, though they usually do not drop the psychopathological causation that is often associated with claims of alleged paranormal experience. Nevertheless, the psychological theories set out in these sundry chapters appear to be viable in certain circumstances, and of course Jinks presents so much evidence that conventional psychological science may well be well-grounded in its claims when it comes to matters to do with paranormal experience and their causes (see Chapter 1).

Then we come to Part 2, which is not an attempt to explain the

*mechanisms* of paranormal experiences, but rather the *purposes* they serve. To do that, Jinks launches into the psychodynamic theories of Sigmund Freud, D. Scott Rogo, C. G. Jung, and others, and the reader will see that the purposes served have much to do with the psyche's attempt to heal itself by making conscious those events that are unconsciously causing deep mental discomfort. So, telepathy, ghosts, and UFO abductions are not real (at least, not necessarily), but are manifestations of the creative, dynamic unconscious working in symbolic mode, disguising deep moral dilemmas or traumas in a form more acceptable to the denying, repressive, sensitive, or neurotic ego. Breakthroughs and ultimate healing comes with revelation and recognition.



The various psychodynamic theories tend to be variations on each other, and one begins to see that the inexplicability of psi might not be so important for those who benefit and believe in it. But the scientist with a different set of questions is really forced to return to Part 1 and try to find answers there (Dr. Jinks says as much on p. 181). Perhaps those scientists should take a page from Part 2, for Jinks shows that even mainstream conventional science is riddled with complexity that not only defies common sense but also bamboozles the clear-cut processes of rational thought. These ideas are not the machinations of Dr. Jinks—major players in science feel the same way. For example, the physicist Richard Feynman stated “I think I can safely say that nobody understands quantum mechanics” (see p. 222). How are such statements supposed to win the hearts and minds of the lay public? How can we have a Physics (or topics in that field) that no one understands? Indeed, how can hard-line scientists dare to impress (force?) upon society ideas, notions, and even findings that bear the very hallmarks of magic and superstition leveled at, and usually confined to, the subject matter of parapsychologists? Perhaps all we need do is appeal to the notion of replicable evidence. Is demonstrable evidence that may or may not elicit strong effects more convincing than inconsistent evidence that produces only weak effects? Apparently not (readers should refer back to Chapters 6 and 7 to see that proponents of many explicatory theories are not dissuaded by lack of replication, or an incapacity of those theories to explain all the symptoms or facets of a paranormal encounter).

So, must we learn to live with only a partial knowledge and understanding of many phenomena that come under the scrutinous eye of scientists, simply because the universe is stranger than we can ever think



it is, or imagine it to be? It seems that the deeper we look, the more we investigate, the more unfathomable becomes the world of our experience. To help ground these speculative notions, Jinks's Chapter 10 (titled Transpersonal Psychology), which closes the book, offers some connecting thoughts on how parapsychology (and the psychology of parapsychology) is expanded as our understanding of Self expands.

In closing, one finds that a great deal of ufological and cryptozoological material is used by Dr. Jinks to illustrate the nature and manifestations of the underlying phenomenologies and the psychopathologies thereof. In part the reason for this is that most of the theories explaining paranormal phenomena are illustrated best using this material. One feels, though, that the author also has a penchant for using such cultural artifacts and motifs, but the subsequent limited treatment, in a similar way, of conventional parapsychological material (i.e. pure ESP and PK) indicates a lost opportunity to demonstrate both the relevance and inadequacy of the various claims made by the clinical/scientific professionals (notwithstanding the fact that Jinks does not completely overlook those topics). On the one hand, this limitation of the text might irk aficionados of parapsychology who regard and define their field as being *only* the study of ESP, PK, and life after death. On the other hand, it may be the case that the field has been rather restricted in its scope for too long, so that more focus on ufology and cryptozoology might be a welcome relief to a somewhat dry field driven by statistical findings and overly imaginative theoretical exploits.

Dr. Jinks's book is, nevertheless, an excellent in-depth introduction to the general topic of parapsychology, and goes beyond being a mere introduction to the twofold topic indicated in its title. More than that, it would stand up as a highly suitable introductory textbook for psychology and parapsychology students, even rivaling many such texts currently available. One reason being that it illustrates how complex the paranormal really is, but the main reason being that it takes an approach that weighs up both sides of the argument rather than endorses the practices of academic institutions that are only interested in furthering and perpetuating their rigid skeptical aims. I raised this concern at the start of my review. At the end of the day, I believe the problem hardnosed skeptics have with paranormal belief and experience per se (specifically referring to academic skeptics, not just lay skeptics) primarily stems from the goal of clinicians to normalize society, and it can only do this by first finding a problem. It does not require a careful reading between the lines of Jinks's book before one can see these issues rising to the surface—apart from Irwin's comment above, the issues are in fact embedded key topics of the book (for examples, I refer the reader to pp. 62–65, 73–78, 114–116, etc.).

Jinks is, however, careful not to drift from one of his more pragmatic aims of influencing parapsychology for the better—he usually (and diplomatically) couches his criticisms in methodological terms rather than outrightly expressing his disapprobation of conventional psychology *and* parapsychology. As things are, these fields could be seen as often deviating from their primary goal of furthering science for the sake of knowing and understanding more completely the nature of human beings (indeed, our very universe) irrespective of who or what those pursuits upset. That single enlightening fact, albeit controversial (if not divisive), is enough to convince this reviewer that this is a book well worth having on one's bookshelf.

### Note

<sup>1</sup> This topic formed the basis of a lecture given by Tony Jinks at the 3<sup>rd</sup> Annual AIPR Lecture in Melbourne, Victoria, Australia, in November 2011.

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**Perspectives of Clinical Parapsychology: An Introductory Reader**

edited by Wim H. Kramer, Eberhard Bauer, and Gerd H. Hovelmann.  
Bunnik, The Netherlands: Stichting Het Johan Borgman Fonds, 2012.  
320 pp. \$38.95 (paperback). ISBN 9789081835701.

This valuable compilation of 12 papers was initially presented at a conference held in The Netherlands in 2007, where 20 professionals from different countries gathered to address the different aspects and clinical needs of people suffering from exceptional or anomalous experiences. As a clinical psychologist who has worked with such clients for more than 35 years, I found this book to be an excellent contribution to a field that according to Tierney (1993) appears to be increasingly needed.

Numerous surveys conducted by contributors to this volume may partially explain why that need may be growing. Eyebrects and Gerding, for example, cite in their contribution, "Explorations in Clinical Parapsychology," that within the Dutch population, 63% of those surveyed believe in a person's ability to have contact with the deceased, and 40% of that group indicated they actually had had such an experience (p. 38). Parra, in his contribution, "A Group Therapy Approach to Exceptional Human Experiences," reports that among 392 undergraduates surveyed in Argentina, 66% indicated they experienced incidents of telepathy, while 50% reported ESP in dreams (p. 89). In Germany, Zahradnik & von Lucadou note in "A Counseling Approach to Extraordinary Experiences" that as many as 75% of those surveyed from the general population reported having had extraordinary personal experiences classified as paranormal (p. 119). Outside this volume, other investigations such as the one conducted in the United States by Gallup (2005) found that 73% of those surveyed believed in at least one of ten paranormal items presented in the survey.

Bauer and Schetsche (2003) suggest that these high levels of belief in the paranormal, particularly in the West, may in part be attributable to the popularity of mass media attention given to the paranormal: Television programs like *The Medium* and *Six Feet Under*; movies like *The Sixth Sense*, *The Matrix*, and *Paranormal Activity*; and books like the *Harry Potter* series.

This surprisingly high level of belief and familiarity with the paranormal might indeed contribute to why more clients may be showing up for psychotherapy following an exceptional experience. It also may be that similar to other previously considered "unspeakable" topics like child abuse or incest, disclosing exceptional or anomalous experiences may now seem safer to broach in therapy.

I find that clients are often frightened or confused when they broach anomalous or exceptional experiences in psychotherapy, particularly if the experience is the precipitant for seeking help. They're frequently apprehensive about how they'll be perceived. These fears are not ungrounded, because when broaching such concerns with a clinician who is uninformed or highly skeptical about paranormal phenomena, they may indeed find themselves prematurely judged as presenting psychopathology.

Anneli Goulding in her contribution to this volume, "Paranormal Beliefs and Experiences—Signs of Mental Health or Disorder," states,

People with paranormal experiences who are developing serious mental disorders need help to avoid severe mental breakdown. People with paranormal experiences who are not developing a mental disorder need help understanding their experience without being classified as disturbed. (p. 49)

Making this distinction is no easy task for the clinician, and other authors in this book address the need for the therapist to not only employ good clinical judgment, but also become familiar with the varieties of psi phenomena.

It is difficult to maintain a balance between employing good clinical judgment while holding a place for the plausible veracity of a client's exceptional experience. With regard to the later, Tierney, in his contribution, "Lessons from a Case Study: An Annotated Narrative," reminds therapists of their ethical responsibility to maintain an "evidence-based practice" when explaining to a client the complexities of psi phenomena. Upholding this standard might suggest that any therapist faced with a client reporting such phenomena sit lightly in the saddle of belief, go about a thorough assessment of the details of what is being reported, yet respect the phenomenological perspective of the client.

Sometimes, a client may present both a plausible experience and some evident psychopathology. And as Giovanni Iannuzzo states in his contribution, "Clinical Parapsychology and Parapsychological Counseling in Psychiatric Practice," "the presence of psi phenomena and the presence of a mental condition (or the beginning of one) is the main problem faced in a clinical practice" (p. 60).



Other papers in this volume present various strategies and approaches for understanding and treating individuals who report having experienced anomalous phenomena. Kramer, one of the three editors and first contributor to this book, points out in “Experiences with Psi Counseling in Holland,” that when treating such individuals, “We are not so much interested in whether the alleged paranormal experience is a real or pseudo psi phenomena. What is important is that the client experiences it as real” (p. 14). Again, Tierney further advises that we “separate therapeutic goals from the desire to collect evidence of psi,” and that we’re clear about our priority (p. 27).

However, even when giving appropriate priority to therapeutic goals while respecting the phenomenological perspective of the client, the therapist may find reasonable claims of anomalous phenomena *and* evidence of psychopathology. The greater clinical challenge then is discerning how the interactions of both the anomalous and the pathological have become entangled. When that occurs, it may not be sufficient to just provide supportive counseling with information about the paranormal to allay their anxiety. When plausible claims of the anomalous are accompanied by psychopathology, a more in-depth clinical and pharmacological intervention may be required to address the full scope of the problem.

Frequently, psychological problems that preceded or follow an exceptional experience may warrant a diagnosis, and the broader clinical challenge may require disentangling the exceptional experience from its intermingling psychological disturbances. An example of a pre-morbid clinical problem preceding an exceptional experience that would warrant a diagnosis might be a seriously depressed client who in attempting an unsuccessful suicide, has a near-death experience. Alternatively, an individual who was free of any psychopathology prior to an NDE, might present a diagnosable condition that arises as a result of the exceptional experience. With regard to this later example, Greyson and Harris (1987) and Furn (1987) found that some psychologically healthy individuals who had an NDE show lingering effects that may justify a diagnosis of depression. However, they note that their depression may be less severe than the more pathological type mentioned in the former example.

Other individuals who have experienced exceptional phenomena may also present clinical concerns such as PTSD, anxiety disorder, complicated bereavement, paranoia, or in extreme situations what might even temporarily appear to be a transient psychotic reaction. At the least, according to Harris Friedman (2012), the impact and attempt to integrate an exceptional experience may justify a diagnosis of an adjustment disorder in combination with the DSM-V Code “spiritual or religious problem.”

With regard to this clinical challenge of addressing the many facets

of such clients, Ahmed, in her chapter “Psychotherapeutic Approaches to Major Paranormal Experiences (MPE)” states

Clinical pictures centering around extraordinary experiences are unimaginably complex because of their multidimensional nature, exhibiting paranormal aspects, certainly, but also psychopathological, cultural, and a variety of other aspects. (p. 67)

I find that the more one investigates the field of Clinical Parapsychology, the more one discovers the need for it as both valid and challenging. It requires that the therapist be well-versed in the evidence-based research that does indeed exist on parapsychology, be a sound clinician capable of distinguishing a legitimate exceptional experience from psychopathology, and be capable of disentangling the interweaving of both (Pasciuti 2012). But due to some therapist’s pre-loaded attitudes and judgments of parapsychology, many clients presenting exceptional experiences are wrongfully diagnosed as exclusively pathological, and find the potential authenticity of what they present discounted before it is given full consideration.

Hovellmann, in the last paper in this volume, “Clinical Aspects of Exceptional Human Experiences: A Working Bibliography,” states, “there still is no agreed upon conceptual framework for the treatment of clients distressed by exceptional human experiences and for the meaningful discussion of clinical aspects of parapsychology” (p. 193). However, it is suggested that in order for therapists to be adequately prepared for the challenge such clients present, they become familiar with at least some recent works in the field of parapsychological research. He recommends books such as *Irreducible Mind: Toward a Psychology for the 21<sup>st</sup> Century* (Kelly, Kelly, Crabtree, Grosso, & Greyson 2007), and *Varieties of Anomalous Experience* (Cardeña, Lynn, & Krippner 2000). And for those who would like to sample from a broader selection of research and publications on parapsychology, he and his collaborators offer in this book, one of the most extensive and painstaking compilations of references: a 123-page bibliography that in and of itself is worth the price of admission.

I highly recommend this book to any psychotherapist who may occasionally work with clients presenting exceptional or anomalous experiences. It provides an excellent selection of papers from some of the leaders in the field, and will assist you greatly.

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**Pluralism and the Mind: Consciousness, Worldviews, and the Limits of Science** by Matthew Colborn. Imprint Academic, 2011. 300 pp. \$34.90 (paperback). ISBN 9781845402211.

*Can't we all just get along?*

—Attributed to Rodney King following his beating by Los Angeles police, May 1, 1991

Matthew Colborn's book on what might seem a topic radically unrelated to the above quote nevertheless might have used Rodney King's much-cited comment as its theme. In the areas of cognitive science and philosophy of mind, there is plenty of conceptual head-bashing going on as multiple views contend. The conflict is more acute than in typical disputes among philosophical positions. Where science stands in relation to this conflict of ideas lies in the advent of neuroscience transformed by the marriage of functional magnetic resonance imaging (fMRI) with the computer model of the brain. By finding that the activity of more or less discrete areas of the brain can be correlated with more or less imprecisely defined mental functions, and by assuming that the brain is a digital machine, the conclusion is drawn that the mind, self, and consciousness are now entirely within the purview of neuroscience. It follows that all other theories of the mind, and especially theories that appeal to spheres inaccessible to the physical sciences, are consigned to the trash heap. As a result, intentionality (meaning) must be cast out along with illusions such as freedom of will and spiritual aspiration.

Now along comes Matthew Colborn to resolve the issue, in at least a tentative manner. His thesis in *Pluralism and the Mind* is that to one extent or another all these conflicting theories must have something to offer. None is to be wholly denied. Therefore, "can't we all just get along?" What Colborn seeks to do is evaluate the merits of all sides and then suggest a kind of co-existence. In so doing he preludes his viewpoint with a carefully detailed account of the historical background of the differing theories (Chapters 1–4), philosophical considerations of ontology, epistemology, and concepts of causality (Chapters 5–7), and the limitations of physicalism (Chapters 8–10). Thus his book becomes a kind of well-tailored tutorial on the entire landscape. Eventually he comes to the question of a pluralistic viewpoint (Chapters 11–15), where in this reader's opinion to some degree he undermines his own view.

Neuroscientists advocating the theory of mind–brain identity (MBI) have often been accused of operating within an outmoded set of ideas such as the classic conceptions of causality. For example, W. T. Rockwell severely criticizes the theory that brain states "cause" mental states on

the grounds that this utilizes outmoded views of atomistic causality and intrinsic causal powers (Rockwell 2007:54–57). Raymond Tallis echoes this objection on the basis that the MBI theory fails to take into account the entirety of the biological system that is active in relation to mental activity (Tallis 2011:83). Colborn addresses this issue in some detail by bringing up the question of whether physical notions of causality can be applicable in the case of living organisms.

A central question is whether organisms possess a wider or different kind of causal agency than do inanimate objects . . . active self-maintenance and completion [of organisms is] described as . . . autopoieses. . . . Machines, by contrast, are allopoietic systems. These are not self-producing or autonomous but are built from individual components. (pp. 106–107)

Focusing on these and other central problems, Colborn delivers a comprehensive review. His book does not neglect even areas of thought that are sometimes ignored in such discussions, such as the possible contributions of Buddhist philosophy (p. 192 ff.).

Particularly of significance may be Colborn's account of the basic issue of the relation of theoretical science to reality itself. The situation here is eerily similar to that raised by advocates of creationism who condemn evolutionary theory as "just a theory." In this view, science does not describe reality, but just concocts "theories." Evolutionary scientists may take a hard line, i.e. to assert that evolution is a fact, not a theory (in the sense of whatever one happens to dream up). But on the other hand there is the Pragmatist view of scientific theory as either a continual approximation to reality, or justified simply "because it works." Colborn cites Nancy Cartwright on this most fundamental concern.

The reductionist programme suggests that everything should . . . be reducible to the "laws" of physics on the bottom tier, as psychology "should" be reducible to biology which should be reducible to chemistry which should be reducible to physics. . . . Cartwright rejects this picture of science, including the notion that there is a universal cover of law, instead adopting Neurath's picture of a patchwork of appropriate domains. (p. 96, citing Cartwright 1999)

This remedial view recalls the Pragmatism of Dewey, who also cites Neurath and at least 70 years ago severely criticized the syndrome of "selective preference" among scientists—the assigning of all reality to the procedures and findings of any one particular set of theories associated with any one particular science: in short, an illegitimate spilling over of science

into metaphysics at the expense of the full range of perspectives found in the experienced world (Dewey 1928). Proposals such as this—essentially related to the view of science found in the Pragmatism of Peirce, Dewey, and James—are certainly the wellspring for the pluralistic remedy proposed by Colborn. They are also ones that have essentially no existence within the circles of MBI theorists.

The journey Colborn sets out for the reader takes us through key issues such as this, including the question of the nature of memory (p. 168), the validity of “folk psychology” (p. 182), and the elimination of telic factors in biology (p. 197 ff.). This provides a picture of the multiple ramifications springing from the claim that neuroscience has co-opted all other explanations of the fundamental nature of the human being.

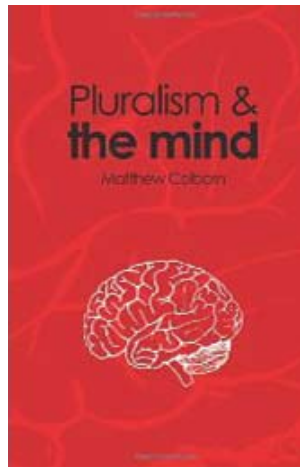
So is this the proper role for science? Is neuroscience the final arbiter of all human questions? Colborn generously does not claim to have reached any “firm conclusions,” but he feels that from the broad perspective he has painted “a few comments are in order” (p. 212).

A pluralistic option would be to resist the adoption of a “universal” theory at all . . . bits and pieces of both mainstream and heterodox theories may well prove useful for deepening our understanding.

But this brief nod to a pluralistic solution is not at all Colborn’s final assessment. In his concluding chapter he engages in a more philosophically (and scientifically) satisfying analysis. Like any philosopher worth his or her salt, Colborn now asks the *necessary questions* and seeks at least tentatively to answer them.

1. *Could* we construct a one-size-fits-all theory of consciousness?
2. *Should* we construct such a theory to the exclusion of others?

Colborn’s answer to the first question is: Yes, but possibly at an unacceptable cost. What he actually appears to be saying is that such a theory would not satisfy the overall scientific demand for adequate explanation of all the phenomena in question, but instead would “work” only in the sense that the range of the explained phenomena must be severely limited.



The short answer to question (1) is probably affirmative . . . provided one is willing to reduce “consciousness” to functions, objects, “information-processing,” or maybe novel physical processes, and provided one is willing to actively suppress or to subsume alternative models and/or modes of knowing.

What Colborn is diplomatically saying is that the answer to (1) is simply *no* on the grounds that such a theory is unacceptably related to the testimony of experience. This conclusion becomes more and more inevitable as one moves through the course of his insightful penetration of problems that arise within single-focus theories and in contrast to the multiplicity of counter-theories and issues. Assuming Colborn is correct in his assessment of (1), far more interesting is his discussion of question (2).

As far back as his Chapter 11, Colborn touched upon the question of an ethical and even perhaps *moral* issue surrounding the impulse to turn all reality over to a single theoretical viewpoint. There he cited Feyerabend.

Feyerabend also advanced arguments for the primacy of everyday experience, and held that it was possible to dissent from a scientific view if one felt that it diverged significantly from one’s own personal experiences of the world. This may be necessary, for example, if one finds the “objective” world promulgated in the name of science *dehumanizing*. (p. 182, citing Feyerabend 1978 [my italics])

Here in his final chapter he questions

whether the supremacy of one faction would be either good for science or good for the populace at large, who . . . will also have to live with the ramifications of a predominant theory. (p. 267)

There is something more here than simply saying that any point of view, no matter how whimsical, is equally “good” or just saying that the domination of a particular science, no matter how functionally useful, is “bad” per se. Lurking beneath this is a more uncomfortable observation—one which Raymond Tallis did dare to articulate in his recent *Aping Mankind*. There Tallis states outright his opinion that domination of the human self-image by MBI theory and the computer model of the brain is politically, socially, and psychologically dangerous (Tallis 2011).

Throughout his ostensible advocacy of mediation and pluralism, Colborn engages a style which I have called “generous.” Yet more than once, as in the quote from Feyerabend above, ideas emerge which suggest that an absolute pluralism in the absence of ethical and moral considerations is going too far. In his Chapter 13 on the topic of free will, Colborn cites

neuroscientist Michael Gazzaniga's book *The Ethical Brain*, reporting Gazzaniga as suggesting legal reform based on brain science (Gazzaniga 2005).

If the legal system was reformed as Gazzaniga and others suggest, this would place a significant amount of power into the hands of the consultant neuroscientists and simultaneously reduce the power of the "lay person" (who is, after all, effectively an—often malprogrammed—robot in these conceptions). This would be part of the wider trend of the appropriation of mental health management from private individuals to various experts. (p. 230)

Responding to these and similar concerns, Colborn strikes at the very heart of the matter.

The models and ideologies that issue from institutional science form part of a hierarchical and bureaucratic society that . . . often favors personal uniformity and internal cohesion over diversity. . . . I personally find it very suspicious how well the vision of the human produced by cognitive science fits with the agenda of a consumer society. (p. 280)

So for Colborn, in the last analysis, pluralism is a good thing; but it must not be an uncritical pluralism that says "anything goes" but a critical pluralism: a pluralism of ideas tempered by an understanding of good and evil and a sense of the primacy of human experience.

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**The Neurology of Consciousness: Cognitive Neuroscience and Neuropathology** edited by Steven Laureys and Giulio Tononi. Academic Press, 2008. 423 pp. \$104 (hardcover). ISBN 9780123741688.

This information-packed volume, without doubt a landmark event in the developing neuroscientific study of consciousness, deserves the attention of anyone interested in this subject. It is a sequel and companion to an earlier collection, *The Boundaries of Consciousness: Neurobiology and Neuropathology*, also edited by Steven Laureys (2005), which contains the proceedings of a 2004 conference sponsored by the Association for the Scientific Study of Consciousness (ASSC). Published initially as a special issue of *Progress in Brain Research* (volume 150), *Boundaries* has been reissued in paperback and appears on the ASSC website as one of its official titles. Both volumes include many prominent figures in contemporary neuroscience, and the two volumes together constitute about as clear, comprehensive, and authoritative a picture as one can find anywhere of the current state of mainstream neuroscientific thinking about consciousness and the brain. Like classical physics of the late 19<sup>th</sup> century, it is a picture of great power and beauty; nevertheless, just as discordant phenomena such as black-body radiation and the photoelectric effect presaged the rise of quantum mechanics, this book, precisely because of its clarity, reveals signs of trouble ahead.

The two volumes overlap considerably, with many topics and authors carried directly over from the first to the second. The new volume, however, in addition to updating various topics covered previously, shows greater signs of editorial and doctrinal control, with a narrower range of subjects treated in more deliberately organized fashion. Whereas *Boundaries* contains 40 chapters contributed by 87 authors, ordered according to no discernible principle, the present volume contains 28 chapters by 39 authors, organized into four main sections. I will begin by summarizing, in necessarily telegraphic fashion, the main content of these chapters.

Section I, "Basics," consists of six chapters. The first, by Antonio Damasio and Kaspar Meyer, titled "Consciousness: An Overview of the Phenomenon and of Its Possible Neural Basis," sketches the general picture that will be developed in detail in subsequent chapters. Consciousness is produced by the brain, has a nested multilevel structure that emerges in the course of biological evolution, and can be studied both behaviorally, from the third-person perspective of an external observer, and introspectively, from the first-person perspective of the subject within. Alterations of consciousness and behavior produced by particular types and locations of

brain injury are particularly valuable in revealing brain structures associated with normal conscious functioning, and this results in a methodological emphasis, echoed throughout the book, on in-depth  $n = 1$  neurological investigations of patients with rare forms of injury. Consciousness per se, as distinguished from its occurrent contents, seems to require integrity of a distributed system of midline structures including the upper brainstem, thalamus, and medial cortical areas including in particular frontal, posterior, and cingulate cortex. Chapter 2, by Hal Blumenfeld, provides an excellent description of neurological examination techniques and their deployment in differential diagnosis of the major forms of impaired consciousness including brain death, coma, vegetative states, minimally conscious states, and associated conditions such as the terrifying locked-in syndrome in which patients can be fully and normally conscious but deprived of all or nearly all motor function and unable to communicate. In brain death, all cortical, subcortical, and brainstem function has been irreversibly lost, and only spinal-cord reflexes remain. Stages of recovery—ranging from comatose states in which patients are totally unresponsive and unarousable, to vegetative states with sleep/waking cycles and brainstem reflexes present and eyes occasionally opening but without signs of purposeful or meaningful response to external stimuli, to minimally conscious states with inconsistent but clear indications of consciousness—occur in conjunction with progressive functional restoration of the activation systems in the upper brainstem and diencephalon and their associated cortical targets. Blumenthal correctly emphasizes the difficulties of these diagnostic distinctions and provides numerous hints and guidelines for making them correctly. He also points out in this connection the increasing use and potential diagnostic value of modern neuroimaging procedures. These in turn form the subject of Chapter 3 by Steven Laureys, Melanie Boly, and Giulio Tononi, which provides a brief but generally accurate introduction to the main imaging techniques and associated image-processing and analysis procedures. In a magnificent Chapter 4 titled “Consciousness and Neuronal Synchronization,” eminent German neuroscientist Wolf Singer outlines theory and evidence linking conscious states to the formation of large-scale distributed neural assemblies united by shared neuroelectric activity transiently synchronized at frequencies extending into the gamma range. This picture is common to all forms of contemporary “global workspace” or “global neuronal workspace” theory, of which Singer is a principal architect. An excellent Chapter 5 by Geraint Rees develops that picture in greater detail in the context of visual consciousness, seeking to identify differences in terms of neural activity between identical stimuli that do, versus do not, reach conscious awareness. In general, activity in functionally specialized

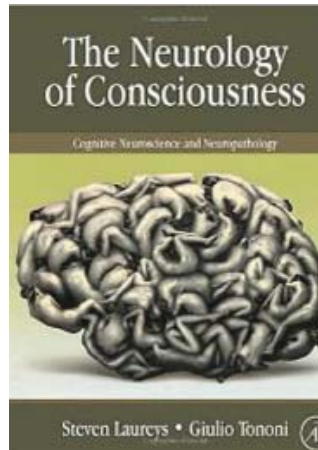


areas of the visual system corresponding to stimulus properties such as color, shape, and motion in depth is necessary but not sufficient for awareness of those properties. In keeping with the global workspace picture, visual awareness occurs when the received stimuli are also able to ignite larger-scale patterns of synchronous neuroelectric activity reciprocally linking widely distributed cortical zones including frontal cortex. Chapter 6 by Naotsugu Tsuchiya and Christof Koch is still more specialized in character, focusing on the relationship between consciousness *per se* and selective, top-down visual attention, and arguing on the basis of various kinds of psychophysical and neurophysiological observations that these are distinct phenomena.

Section II, “Waking States, Sleep, and Anaesthesia,” consists of four chapters. Chapter 7 by Marcus Raichle and Abraham Snyder, titled “Intrinsic Brain Activity and Consciousness,” develops a theme first introduced in Chapter 1 by summarizing important recent work by themselves and others on the structure of background activation in the awake but resting brain—the so-called “default mode network”—which appears to be closely associated with the sense of self at the root of one’s first-person conscious perspective and with consciousness *per se* (versus specific contents of consciousness), and which accounts for a large proportion of the brain’s energy consumption. This network includes in particular midline cortical structures of frontal and parietal cortex whose activities are correlated among themselves but anti-correlated with those of structures activated under particular task conditions. Individual moments of conscious experience with all their specific contents are thus portrayed as being somehow realized upon this more fundamental “backbone” system. In Chapter 8 Giulio Tononi ably summarizes the current state of knowledge concerning normal sleep and dreaming plus associated topics such as daydreaming, lucid dreaming, sleepwalking, and narcolepsy, emphasizing overall patterns of brain activity associated with these various conditions. Chapter 9 by Claudio Bassetti provides a more specialized and clinically oriented review of one particular sleep-related phenomenon, somnambulism. The final chapter of this section, by Michael Alkire, provides an authoritative review of the effects of general anesthetics on brain and consciousness. Although much remains to be learned, anesthetics clearly produce their effects by disrupting operation of the global workspace—i.e. by preventing the precise temporal coordination of neuroelectric activity across large-scale thalamocortical networks that is deemed necessary by contemporary systems neuroscience for the occurrence of conscious experience.

Section III, “Coma and Related Conditions,” contains eight chapters. These are mainly clinical in orientation and with one possible exception noted below broadly consistent with the general global-workspace picture

developed in earlier chapters. Chapter 11 by G. Bryan Young deals with coma, and covers precipitating conditions plus issues surrounding differential patient diagnosis, prognosis, and management. Chapter 12 by James Bernat presents his widely accepted concept of human death as brain death—irreversible cessation of the clinically relevant functions of the brain—contrasting this formulation with various alternatives and outlining the required diagnostic and confirmatory tests that are used to determine its occurrence. Chapter 13 by Adrian Owen, Nicholas Schiff, and Steven Laureys focuses on the vegetative state and the potentially vital contribution of neuroimaging techniques such as PET and fMRI to differential diagnosis. Of particular theoretical interest, however, is their claim to have demonstrated, using such techniques, the presence of conscious awareness in patients who are truly vegetative by the standard behavioral and neurological criteria; I will return to this subject later. Chapter 14 by Joseph Giacino and Nicholas Schiff covers minimally conscious states, which have only recently been distinguished from vegetative states and understandably present many of the same difficulties of differential diagnosis, prognosis, and care. These authors emphasize again the increasing role of neuroimaging studies in clarifying anatomical and functional differences between these disorders, and in identifying possible neural correlates of the considerable fluctuations in functional competence that commonly accompany both of them. Chapter 15, by five authors including Steven Laureys, deals with the locked-in syndrome and is strongly clinical in orientation; one point of special interest, however, is that such patients typically show normal or only mildly abnormal EEGs and evoked potentials, unlike persons suffering the more severe disorders of consciousness treated above. Chapter 16, by Pietro Petrini, Eric Salmon, and Paolo Nichelli, discusses degenerative disorders such as Alzheimer's disease and frontotemporal dementia, emphasizing the degree to which the progressive destruction of particular aspects of mind and consciousness mirrors the progressive destruction of anatomical and functional connectivity among disease-specific cortical areas in different classes of patients. Chapter 17 by Andrea Kubler surveys recent work on brain–computer interfaces (BCI), by means of which paralyzed or locked-in patients without usable motor function can interact with the external environment through self-regulation of selected aspects of brain activity.



Accomplishments are so far rather modest, but they are of enormous value to those who need them, and there appear to be possibilities of extending the basic approach to more severely impaired patients such as those in minimally conscious states. In Chapter 18, which concludes this section, Joseph Fins outlines scientific and ethical challenges posed by work with patients suffering from disorders of consciousness, particularly in light of our currently primitive level of understanding, and presents his “pragmatic” approach to managing these challenges in typical neuropalliative care settings.

Section IV concerns “Seizures, Splits, Neglects, and Assorted Disorders,” and contains no fewer than 10 chapters. The first of these, by Hal Blumenfeld, surveys the main forms of epileptic seizure (absence, generalized tonic-clonic, and complex partial seizures), all of which produce impairments of consciousness despite seemingly wide differences in etiology, outward form, and accompanying electrophysiology. Relying on recent advances in both electrophysiology and other neuroimaging modalities, he is able to show that these seizures in fact share a common pattern of disrupted activity in key elements of a global network associated with normal consciousness, including upper brainstem and medial thalamus, anterior and posterior interhemispheric connections, and frontal and lateral association cortex. He also underscores the interesting fact that the excessive and abnormal neural synchrony associated with seizure activity, like the *absence* of such large-scale synchronous interactions in coma and vegetative states, is incompatible with normal consciousness. Chapter 20 by Michael Gazzaniga and Michael Miller attempts to summarize decades of work on split-brain subjects, and to explain the astonishing fact that the linguistically competent left hemisphere in such patients shows no awareness that the right hemisphere has gone missing, despite its demonstrated superior capabilities for certain specialized perceptual and cognitive tasks. They do this by assimilating the split-brain case to other known forms of anosognosia such as left-side neglect in patients with strokes of the right parietal region. They conceive of the general situation as one in which an “interpreter” located somewhere in the left hemisphere—a conscious person, essentially—is constantly making up a story about its world based on all the information presently being funneled to it from a vast number of specialized brain modules. Where this interpreter is located and how it operates is not specified. They also offer some speculations about the probably impoverished but conscious experience of the disconnected right hemisphere. I should probably confess in passing that I have always found the split-brain work rather confusing and ultimately uncertain in terms of its bearing on mind/brain relations; see also Nagel (1979:Chapter 11). Chapter 21 by Lionel Naccache on the neurology

of visual consciousness describes how recent studies of neuropsychological syndromes such as blindsight, visual agnosia, and so on, together with associated experimental studies in healthy subjects of the principles they suggest, have led to development of the global neuronal workspace theories of consciousness advanced by himself and various colleagues (e.g., Baars 1997, Crick & Koch 2003, Dehaene & Naccache 2001, Edelman & Tononi 2000, Engel, Fries, & Singer 2001, Varela, Lachaux, Rodriguez, & Martinerie 2001). Chapter 22, by Patrick Vuilleumier, concerns the neurophysiology of “hysteria” or conversion disorder, and asks whether the relatively few neuroimaging studies that have so far been carried out have shed any new light on the central enigma as to how ideas in a patient’s mind can produce circumscribed and physiologically improbable effects, such as glove anesthesia, in that patient’s body. Indeed, it is noteworthy that this diagnosis is usually applied only after recognized organic or physiological causes of the presenting symptoms have been ruled out. The main generalization from neurophysiological studies to date seems to be that the problems arise somehow within high-level attentional and control systems associated with orbitofrontal and medial prefrontal cortex rather than within the lower-level perceptual and motor processes themselves. Whether this approach will yield anything more than speculative translation of old psychodynamic concepts into modern neurophysiological ones remains to be seen. Chapter 23 by Olaf Blanke and Sebastian Dieguez covers out-of-body and near-death experiences. In the *Boundaries* volume these topics had been covered separately, with Blanke writing the OBE chapter from a relentlessly reductive-physicalist point of view and Chris French, widely regarded as a moderate skeptic, writing on NDEs in critical but open-minded fashion. French’s replacement by Blanke immediately struck me as an ominous development, and indeed this new combined chapter proved to be profoundly unsatisfactory for reasons I will explain shortly. Chapter 24 by Bradley Postle, titled “The Hippocampus, Memory, and Consciousness,” reviews (with special reference to the celebrated patient Henry M.) the highly selective effects on various types of memory as well as imagination and consciousness that result from injury to structures in and around the medial temporal lobe. Chapter 25 by Chris Butler and Adam Zeman surveys three clinical syndromes of transient amnesia—transient global amnesia, transient epileptic amnesia, and psychogenic (hysterical) amnesia—and what is presently known about their etiology and impacts on memory and consciousness. Chapter 26 by Paolo Nichelli examines effects of various forms of aphasia on working memory, inner speech, error monitoring, and consciousness, using Alan Baddeley’s multicomponent model of working memory as a theoretical framework. Chapter 27, “Blindness and

Consciousness,” by Pietro Pietrini, Maurice Ptito, and Ron Kusters, surveys a variety of clever neuroimaging and transcranial magnetic stimulation (TMS) studies in blind and sighted humans, as well as some neurosurgical studies with animals, to explore issues related to sensory overlap and substitution, cortical plasticity, and the abstract or “supramodal” character of cortical representations of the perceptual world. With regard to the latter, for example, parts of the dorsal or “where” pathway in the visual system are activated by apparently moving *auditory* stimuli as well as similarly moving visual stimuli, and this occurs in congenitally blind or early-blind subjects as well as sighted ones, indicating that the auditory effect is not mediated by visual imagery. Visual cortex in congenitally blind persons can become involved in a host of perceptual and cognitive tasks not normally associated with visual cortex, and this appears to occur primarily by unmasking and reinforcement of preexisting connections rather than generation of novel ones.

Chapter 28 by Giulio Tononi and Steven Laureys, titled “The Neurology of Consciousness: An Overview,” appears as the final chapter of Section IV, but it could easily have constituted a separate Section V on its own. It is by far the longest chapter in the book, masterfully summarizing information presented in earlier chapters and integrating it with a still larger range of neurological and neurobiological research and theory. To indicate its impressively rich and thoughtful contents, I can do no better than to quote their own Abstract:

First, the chapter reviews the evidence suggesting that consciousness can be dissociated from other brain functions, such as responsiveness to sensory inputs, motor control, attention, language, memory, reflection, spatial frames of reference, the body, and perhaps even the self. The chapter then summarizes what has been learned by studying global changes in the level of consciousness, such as sleep, anesthesia, seizures, and vegetative states. Next, it asks what can be said at this point about the role of different brain structures in generating experience. Then dynamic aspects of neural activity are discussed, such as sustained vs. phasic activity, feedforward vs. reentrant activity, and the role of neural synchronization. The chapter ends by briefly considering how a theoretical analysis of the fundamental properties of consciousness can complement neurobiological studies. (p. 376)

The specific “theoretical analysis” referred to here is the information integration theory of Tononi (2004) and subsequent refinements, but as the authors acknowledge this is one variant of a larger class of global neuronal workspace theories having a great deal in common. Tononi thinks that his theory has certain advantages over the others (p. 405), but I am skeptical of this and in any case what matters more for present purposes is the deep

*commonalities* among them. Specifically, despite differences in detail, all hold that the kinds of complex, unified experience that constitute normal waking consciousness require, as a necessary condition of their occurrence, an intact brain capable of sustaining large-scale cooperative neuroelectric interactions linking widely distributed regions of the thalamocortical network at frequencies extending into the gamma range. Furthermore, they all agree that these neurophysiological conditions are abolished in largely parallel ways under a variety of conditions in which consciousness is severely attenuated or abolished, including coma and vegetative states, deep sleep, adequate general anesthesia, and cardiac arrest. This general picture constitutes the central take-home message of the book, and it has continued to develop and flourish in the more recent neurobiological literature (e.g., Dehaene & Changeux 2011, Edelman, Gally, & Baars 2011, Siegel, Donner, & Engel 2012).

Having described as best I can in such short compass the great strengths of this book, let me turn now to some defects, starting with minor ones. The book was typeset in India and printed and bound in China, and although this process undoubtedly helped keep the price down it somehow resulted in an unusually large number of misspellings and grammatical oddities. Production people or perhaps even the editors themselves could also have done more to improve numerous sentences written in non-English (especially German) syntax, and to fix missing or incomplete references. I also wish they had not used numerical referencing, which saves space but makes it much harder to locate work by particular authors.

A slightly more serious issue concerns the wide variation among chapters in terms of clinical versus theoretical emphasis. Some chapters, particularly in Sections III and IV, are sometimes so densely packed with clinical jargon as to be nearly unintelligible to someone not specifically immersed in the corresponding medical specialty.

Most serious by far are issues related to doctrinal control. This is a book totally committed a priori to the standard mainstream view that mind and consciousness are generated, without residue, by physical processes occurring in brains. Thus for example Laureys declares in his Preface (p. ix) that it “tackles one of the biggest challenges of science; understanding the biological basis of human consciousness.” A little further on he says without qualification that “You are your brain,” ironically citing Wilder Penfield in this context without mention of Penfield’s well-known dualist sympathies. Similarly, Allan Hobson in his Prologue (p. xi) proclaims that “Consciousness, like sleep, is of the Brain, by the Brain, and for the Brain. A new day is dawning.” Dissenting views and evidence potentially threatening to the classical physicalism giving rise to these triumphal proclamations are



apparently not to be tolerated, and in fact are systematically and I presume deliberately excluded from the book.

The exclusions take two main forms. The first involves editorial decisions about what topics to include in the book and what to leave out. Widely shared and increasing philosophical doubts about physicalism, for starters, are nowhere mentioned, and the “hard problem” is never confronted (Chalmers 1996, Koons & Bealer 2010). I was also disappointed, although not surprised, that states of consciousness represented in the book range almost exclusively *downward* from normal waking consciousness, with scarcely a mention, let alone discussion, of things such as mystical or even psychedelic states.

Much more distressing, however, is the treatment within the book itself of evidence discordant with its central point of view. There are a number of relevant examples, but I will focus here on two in particular. The first concerns the claims made in Chapter 13 and elsewhere about conscious experience occurring in vegetative states. This claim was initially advanced by Owen, Coleman, Boly, Davis, Laureys, and Pickard (2001), who carried out fMRI studies with a female victim of traumatic brain injury from a car accident. This patient strictly satisfied the current diagnostic criteria for persistent vegetative state (PVS), including in particular total behavioral unresponsiveness. The key finding was that when she was asked to imagine playing tennis, or walking around in her own house, distinct patterns of cortical “activation” appeared that strongly resembled those produced by the same instructions in normal awake subjects. The investigators argued, and continue to argue, that this demonstrates conclusively the presence of conscious awareness in otherwise totally unresponsive persons. They have subsequently discovered several additional cases of similar type (Monti, Vanhaudenhuyse, Coleman, Boly, Pickard, Tshibanda, Owen, & Laureys 2010), but it is worth mentioning in passing that all of these involve traumatic brain injury rather than the sort of global anoxia resulting from cardiac arrest.

Several things need to be said about this. First, I do not find their argument compelling, either logically or evidentially: logically, because we do not know enough about the limits of unconscious processing to be so confident that conscious awareness necessarily accompanied the fMRI effects seen in her brain; evidentially, because we are told very little about the baseline condition of that brain in terms of physical damage, metabolic status, and neuroelectrical output both resting and stimulus-evoked. It certainly doesn’t help that Steven Laureys was apparently duped by “facilitated communication” into thinking that the Belgian PVS patient Rom Houben had really been fully conscious during the 23 years following



his car accident (Boudry, Termote, & Betz 2010). What would be really convincing, of course, would be for the patient herself to wake up and report remembering the experiments, but so far as I know that hasn't happened and is unlikely to happen. The more fundamental and interesting point, however, is this: If that patient (and others like her) can be more or less fully conscious under conditions in which the global workspace has really been functionally degraded in the devastating manner characteristic of vegetative and comatose states, then global workspace theory as currently formulated is *false*. That important possibility goes unnoticed in Chapter 13 or anywhere else in this book.

The same possibility arises, of course, in any other condition in which the global neuronal workspace becomes functionally disabled, and this leads to the second and more important example, the chapter by Blanke and Dieguez on OBEs and NDEs. By way of background, let me say first that in Chapter 6 of *Irreducible Mind* (Kelly, Kelly, Crabtree, Gauld, Grosso, & Greyson 2007) we made an explicit argument of the type just indicated, and did so in the deliberately chosen context of OBEs and NDEs occurring in conjunction with deep general anesthesia and cardiac arrest. We made that choice specifically because it is relatively straightforward in such cases, as compared with cases of the sort above that involve traumatic brain injuries of incompletely known, uncertain, and probably fluctuating sorts, to demonstrate that complex, profound, and life-transforming experiences can and sometimes do occur under conditions in which the global neuronal workspace has been unambiguously disabled, and in which contemporary mainstream neuroscience therefore decrees that no experience of any sort, let alone *that* sort, should be possible. This conflict is head-on, profound, and in my opinion inescapable (see also Holden, Greyson, & James 2009, van Lommel 2010).

Blanke and Dieguez, however, do not see it that way, and although they certainly know of this argument they do not deign even to mention it, let alone discuss it. Instead, they try desperately to evade it. Their chapter amounts to an update of Blanke's original unsatisfactory treatment of OBEs in the *Boundaries* volume, now extended to cover NDEs in parallel reductive fashion. For them the conventional production model of brain/mind relations is proven, axiomatic, unquestionable. In forcing OBEs and NDEs into that framework, however, they necessarily do violence to the data at a number of critical points. In discussing cases occurring under general anesthesia, for example, they try to evade the disabling of the global workspace by making the astonishing suggestion (p. 308) that all such patients must in fact have woken up during the surgery due to inadequate anesthesia. Now it is undoubtedly true that there have been some patients

who woke up during surgery and then had an OBE or NDE in connection with that experience, but in general these are very different states with radically different phenomenologies and sequelae and very little overlap. Surgical awakenings are in the vast majority of cases highly traumatic events characterized by total paralysis, fear, panic, pain, and helplessness, with a legacy of resentment and high rates of post-traumatic stress disorder; only a tiny minority include OBEs or NDEs, and it is surely little wonder that one would do anything possible to escape such terrifying circumstances if means were available! All of this is heavily documented in our chapter in *Irreducible Mind*. Blanke and Dieguez even ignore the main message of a case which they themselves cite approvingly (Lopez, Forster, Annoni, Habre, & Iselin-Chaves 2006), one involving a teenage boy who during one operation had an unpleasant awakening, and during another a typical NDE without awakening. The authors of that paper, as well as the boy himself, clearly recognized that inadvertent surgical awakenings and NDEs are utterly different phenomena that need to be clearly differentiated; why can't Blanke and Dieguez?

Moving on to NDEs occurring in conjunction with cardiac arrest, they make another astonishing and in my opinion scientifically indefensible move. Specifically, they assert (on p. 315 and elsewhere) that in studies such as that of van Lommel, van Wees, Meyers, and Elfferich (2001), we really don't know whether patients were unconscious or what sort of condition their brains were in. Why not? Because full neurological and EEG examinations could not be performed *on those particular individuals* under the demanding conditions of resuscitation. With all due respect, that is pure evasion: The presence of cardiac arrest or ventricular fibrillation is determinable with complete reliability from EKG records, which van Lommel et al. had for all of their patients, and the consequences of such events for cerebral physiology are also known in detail and with high reliability from a large number of previous experimental studies in both animal and human subjects. These consequences are spelled out in considerable detail both in our chapter and in van Lommel (2010), among other places, and they are unquestionably tantamount to total disabling of the global neuronal workspace during the period of arrest.

The only remaining way to try to circumvent this argument is to argue that the experience actually occurred at another time, usually either before full arrest or during recovery from it. This move is blocked by subjects' verifiable reports of events occurring during the period of unconsciousness, but of course Blanke and Dieguez give no credence to any such reports, or to anything paranormal for that matter. Cases such as that of Pam Reynolds are not even mentioned in their chapter, unlike Chris

French's in the *Boundaries* volume. They also totally fail, again unlike French, to come to grips with the hyper-reality, enhanced mentation, and transformative impact characteristic of deep NDEs. Instead, like many previous reductively inclined commentators, they wear on at length, with abundant but largely irrelevant citations of neurological literature, about alleged phenomenological similarities and hence "links" between NDEs and sundry other syndromes of known etiology. We've seen this approach before, and we and many other commentators have pointed out numerous deep flaws in it, but to hear Blanke and Dieguez tell it there's only one scientifically proper way to think about the matter, and that's *their* way, as if no such problems had been discovered. This is not genuine science, in my opinion, but rather a quasi-religious scientistic defense of a dogmatically held a priori opinion. I think Chris French himself also underestimated the challenge posed to current neuroscientific thinking by OBEs and NDEs, but he at least was willing to look at actual evidence and think about its possible meaning. Blanke and Dieguez are apparently incapable of that.

What should be the governing principle here was stated forcibly by Francis Bacon (1620/1960) at the beginning of the scientific era: "The world is not to be narrowed till it will go into the understanding . . . but the understanding to be expanded and opened till it can take in the image of the world as it is in fact" (Bacon 1960:276). What is ultimately at stake here is correct interpretation of the intimate mind-brain correlation that everyone agrees holds under normal conditions of conscious mental life. From a historical point of view it is perfectly natural to interpret this correlation exclusively, as current mainstream thinking does, in terms of production of the mental by the physical, and it is certainly appropriate and desirable that this point of view be developed as vigorously and fully as possible. The volume under review does that for the most part in a sophisticated, wide-ranging, and up-to-date way, and for that reason I strongly recommend it. But in the examples cited I think we also see facts that are not readily accommodated by the standard view, and that could make better sense in terms of an alternative and broader interpretation proposed by James (1900) and Myers (1903), among many others, according to which mind normally operates in close coordination with the brain as a sensorimotor organ, but has features and capacities that cannot be explained in conventional physicalist terms. It is only by honestly and squarely facing potentially decisive facts of the sorts described above, and many others like them (Kelly et al. 2007), that we can hope eventually to arrive at the correct view, whatever that might be. As Myers himself cautioned, "Our notions of mind and matter must go through many a phase as yet unimagined."

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**Psicologia e Neurociência: Uma Avaliação da Perspectiva Materialista no Estudo dos Fenômenos Mentais [Psychology and Neuroscience: An Assessment of the Materialist Perspective in the Study of Mental Phenomena]** (second edition) by Saulo de Freitas Araujo. Juiz de Fora: Federal University of Juiz de Fora (UFJF), 2011. 94 pp. \$20. ISBN 9788585252809.

*Psicologia e Neurociência [Psychology and Neuroscience]* is a book that offers interesting and fruitful contributions to current studies involving the mind–brain question. Starting from a critical analysis of the materialist perspective in the light of the philosophy of mind, psychology, and neuroscience, it promotes a debate on the language used in the description of mental phenomena and scientific experimental procedures for studying these same phenomena.

The book is derived from the Master's degree work of the author, Saulo Araújo de Freitas (Professor, Department of Psychology, Universidade Federal de Juiz de Fora (UFJF), Brazil). First published in 2003 by the UFJF press and currently in its second edition (2011), the work is divided into six chapters logically linked, addressing the issue of materialism from its foundations to its application to the study of mental phenomena. The second edition (which was used to prepare this review) also presents an addition: a historical investigation of materialism prior to the twentieth century.

In general, the book questions the optimistic promise of materialism and its supporters to reach in the near future a complete understanding of all mental processes through the progress of neuroscience research. Among other things, the author criticizes the deletion of the autonomy of the subjective dimension in the investigation of human mental life. Reduced or reformulated in objective terms (the brain), subjectivity would be lost in this process. The intention of materialism in explaining subjectivity (first person) through the objective perspective (third person) has not achieved success over all of its historical attempts. However, the advocates of materialism, a kind of “messianic promise,” argue that, one day, all mental phenomena will be explained by the progress of neuroscience.

The book's chapters are arranged in a progressive way, which architecture goes from the conceptual foundations of materialism (its constituent bases) to its operative culmination in the “eliminative materialism” of Paul and Patricia Churchland, in the philosophy of mind. The author thus shows a kind of conceptual embryology of materialism applied both to the philosophy of mind and to the neurosciences and its impact on the current constitution of the future of psychology.

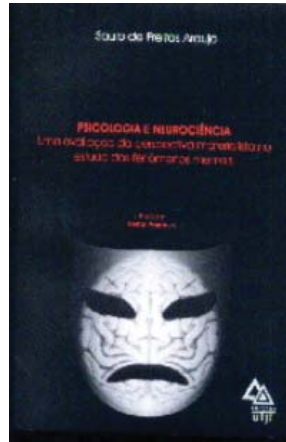
The first chapter weaves elaborations about “folk psychology” (the psychology of common sense). One of its main features is the use of popular vocabulary for the expression of or attempts to describe various states and mental phenomena. From this perspective, many of the existing psychological theories are supported by this popular vocabulary. One of the proposals of “eliminative materialism” (a position taken by Paul and Patricia Churchland) is precisely to eliminate this popular language and replace it with objective, scientific terms, supported mainly by neuroscientific studies. Words such as desire, will, belief, motivation, etc., would be eliminated in a truly future scientific psychology. Folk psychology, devoid of ontology, is not scientific.

In the second chapter, the author presents the theory of identity, created in the late 1950s by Place, which is the first systematic attempt to define a materialist ontology for psychological concepts. Continuing in a dense and complex argumentation, in general the theory of identity, distant from a dualistic stance, says that consciousness and mind (and all its processes and attributes) can be identified as particular sets of brain processes. Although the theory of identity has been further complemented by the work of D. Armstrong in the 1970s, it still has maintained a continuity between the language of science and the language of common sense, making it difficult to create a genuine materialist theory of mind. In fact, the proposal for a complete break with popular language came only with the emergence of eliminative materialism.

The third chapter introduces a first formulation of eliminative materialism, which occurs with Feyerabend (1963). Based on the Theory of Identity, this first formulation affirmed the importance of the study of brain processes to an understanding of mental phenomena, and its proposal was to eliminate all the “mentalist” vocabulary of psychological theories. It advocated the creation of a physiological theory for the investigation of mental processes. The term *eliminative materialism* was created by Rorty (1970), and its main idea was to eliminate from scientific psychology not only mental terms, but also popular vocabulary and common sense, replacing them with concepts generated by physiology and neurology. In the past, demons were accused of causing behavioral disorders in individuals (“the subject is possessed by a demon”), but we know now that there are mental disorders that cause such disturbances. Thus, the belief in demons is no longer needed. For Rorty, the same will occur in the study of sensations, for example. Once neurology advances, mention of feelings will be replaced by discourse on the brain. However, the most radical materialistic proposal appeared with the Churchlands, where not only sensations but all mental processes are considered as products of the brain.



In the fourth chapter, the eliminative materialism of the Churchlands is presented, and the central idea of this approach is to eliminate (and not merely replace or reduce) the entire vocabulary of folk psychology. This view attempts to refute the dualism of mind–brain and argues that only the brain is real and that mental terms are a kind of popular fiction that should be eliminated in a future scientific psychology. Thus, terms such as *belief*, *desire*, *will*, *fear*, *intent*, etc., would be eliminated. The eliminative materialism of the Churchlands represents the culmination of the materialist perspective in the philosophy of mind.



The fifth chapter continues with a critical view of the proposed hypothesis (eliminative materialism). The central point lies in what the author called “the paradox of elimination.” That is, to eliminate the popular vocabulary (folk psychology) of a future scientific theory of mind, you must use the folk psychology to realize such a construction. The interpretation of neuroimaging and people’s subjective reports can only be expressed in terms of folk psychology, which is the vocabulary available. Thus, to eliminate folk psychology and then build a vocabulary based on neuroscience, folk psychology would need to be used. This is the “paradox of elimination.” Another criticism reminds us of the complexity of the human brain. Will we ever be able to faithfully reproduce the human neuronal apparatus in a computer? This constitutes a difficulty for the eliminativist program. After all, if we cannot reproduce the complexity of the human brain, how can we understand it in its entirety?

In the sixth chapter, the author weaves in an overview of the future of a scientific psychology. In the twentieth century, the materialist proposal (identification, reduction, or even parallelism between mental functions and neurological brain function) failed to solve the mind–brain problem. Although folk psychology has not been eliminated, it is necessary, however, to develop a specialized vocabulary for the study of the mind. Then it could progress on its own ground of psychology in a scientific way. Research in neuroscience should be complemented by analysis of social and cultural contexts that influence trends, opinions, and behaviors (neuroscience + social psychology).

Finally, focusing on the materialistic proposal, the author states the “messianic promise,” that “one day, everything will be explained by progress in neuroscience,” is a repetition of old hopes that never materialized.



He also draws a distinction between scientific practice and worldview. Science depends on correct methodology, logic, and conceptual coherence. Materialism, in turn, is a worldview that can be adopted by scientists. In other words, science and materialism are not synonymous. According to the author, the materialistic view attempts to suppress the autonomy of the subjective dimension of human beings, reducing them to the objective dimension.

It is true that neuroscience progresses every day, but will this progress be able to completely explain all mental phenomena and their complex network of relationships, trends, and expressions? The materialists are betting yes. Saulo Araujo de Freitas, with this book, puts in doubt certain aspects of this proposal. An interesting book, dense and complex, it is designed to shed light on current debates about mind–brain issues, but it is also directed to students of anomalistic psychology, considering that since the time of the Society for Psychical Research (London), and on through the works of the Rhines at Duke University and up until today, the mind–brain problem is still one of the most intriguing of all.

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**Shakespeare Suppressed: The Uncensored Truth about Shakespeare and His Work** by Katherine Chiljan. San Francisco: Faire Editions, 2011. 448 pp. \$35.00 (paperback). ISBN 9780982940549.

In 2011, the film *Anonymous* directed by Roland Emmerich focused worldwide attention upon what has long been suspected to be a literary and historical conspiracy—the Shakespeare authorship question. Particularly during the last century, scholars from diverse parts of Europe and America have emerged in increasing numbers to voice their doubt that William Shakespeare was ever more than a front for the true author. Mainstream scholarship has largely responded with silence to these protests, but this has only served to sharpen interest, and to reveal the absence of any probative evidence that is sufficient to establish Shakespeare’s authorship. As Hugh Trevor-Roper pointed out,

he has been subjected to the greatest battery of organized research that has ever been directed upon a single person. Armies of scholars formidably equipped, have examined all the documents which could possibly contain at least a mention of Shakespeare’s name. . . . And yet the greatest of all Englishmen, after this tremendous inquisition, still remains so close to a mystery that even his identity can still be doubted. (Trevor-Roper 1962)

The author, Katherine Chiljan, is a historian who graduated from UCLA, and who can lay claim to more than twenty-five years experience associated with the problem of Shakespeare’s authorship. During that time, she has debated the problem at the Smithsonian Institution, read papers at conferences in both the US and the UK, and served as editor of the quarterly *Shakespeare Oxford Newsletter*. It is against this background that her book seeks to piece together a jigsaw puzzle depicting the man she calls “the great author”: a man whom she believes to be responsible for writing the works of Shakespeare. It is a daunting task, mainly because there are so many important pieces missing from the puzzle. Whereas the mainstream academic can write from the high ground, and give good accounts of Shakespeare’s life and business transactions, into which he or she inserts at regular intervals of convenience the author’s works, according to their supposed dates of composition, this approach is not open to the unconventional scholar. Chiljan’s first task is therefore to undermine the evidence supporting Shakespeare, in order to create an opening for the true author to appear.

This task occupies two of the five sections, which complete the book. As the relevant chapters unfold, probing questions are repeatedly raised

concerning the viability of evidence which conventional scholarship never addresses, and which are likely to unsettle long-standing beliefs about Shakespeare. Chief among these concern the several references to a play called *Hamlet*, which includes a performance staged by Henslowe. Yet, the earliest reference to the play occurred at a time coinciding with the recent arrival of Shakespeare in London. To add to this oddity, we are reminded of the absence of a single letter or manuscript in the hand of the author; even though he wrote at least 36 major plays, 154 sonnets, 2 narrative poems, and several smaller pieces. Questions are also raised concerning the origin of the author's exemplary education, which allowed him to write so knowledgeably, and which stands out in contrast to that of a non-specialist, writing without previous theatrical experience.

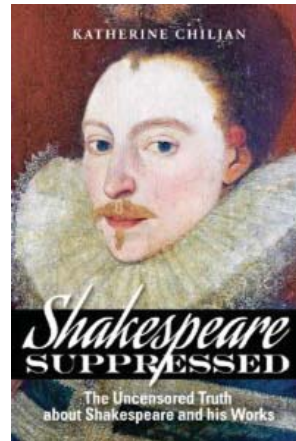
After commenting upon several statements in the Sonnets that contain autobiographical sentiments unconnected with Shakespeare, and then indicating an array of quotations that mirror those written by the great author, but which are too early for acceptance by orthodoxy, Chiljan begins to piece together a picture, from which parts are either missing or unclear. Robert Greene's *Groats-Worth of Wit* (1592) presents a challenge to all who undertake the task of explaining the letter it contains. The epistle was addressed by Greene to the three writers with whom he had dined, shortly before his death. Who were these three? What reason drew the four writers together for a banquet that Greene, for one, could not afford? He died soon afterward, begging the wife he abandoned to pay his two carers, for "had they not succoured me, I had died on the streets." Chiljan does not seek an answer to these questions. Her focus is upon the identity of the man referred to by Greene as *Shake-scene*, the "upstart crow." Chiljan is certain this referred to Edward Alleyn, an actor considered to be second only to Richard Burbage. In 1583, at the age of sixteen, Alleyn was already listed among the Earl of Worcester's Players. He probably first trod the boards at an earlier age, learning his art by playing female roles. Chiljan therefore seeks evidence for Alleyn as Greene's "upstart crow." Such evidence could seem appropriate were it not for the doubt it carries with it. In 1592, Alleyn was a respected actor: not an "upstart crow." He had begun his acting career long before Burbage, even though he was the elder man by only sixteen months. Another difficulty occurs from Chettle's subsequent apology, which he was forced to give for having published defamatory remarks against two persons. One of those offended was Kit Marlowe, who recognized certain atheistic comments that were directed at him, and he made his displeasure known to Chettle. The other offended party ought to be Alleyn, the upstart crow. But this does not fit Chettle's apology, in which he recognized the offended party's "facetious grace in writing." Since Alleyn was not a writer, Chiljan

is compelled to suggest this was intended for George Peele who, she alleges, had taken some minor offence. This places Peele as the third man at the table, while Alleyn, who had been greatly maligned, receives no apology.

But Tom Nashe admitted he too was present at the banquet, and when he later wrote to Gabriel Harvey, he made a coded reference to the third man as “Will Monox” (an anagram of M. Will Oxon.—Oxon. being the conventional Latin abbreviation for Oxford, hence the need for not naming him in a letter). This would explain why Chettle spoke of many titled people having written to protest his publication of Greene’s letter. It is therefore curious that Chiljan did not pursue this lead; especially, when Nashe provocatively suggested to Harvey that Will Monox is known by “his great dagger”—a satirical hint to Harvey, to help him unscramble the anagram, and ensure he recognized the Sword of State carried by the Earl of Oxford (Edward de Vere) on ceremonial occasions.

In Part III, the book concentrates upon two cornerstones that continue to support Shakespeare’s authorship, the First Folio and the Stratford monument. Much has been said before concerning the ambiguities, untruths, and unlikely circumstances that brought the First Folio into being, and these bear repeating. In addition, Chiljan offers new light upon these doubts, including a deeper look at the involvement of the Herbert family (de Vere’s son-in-law Philip and Philip’s brother William) in bringing this project to a conclusion.

The Stratford monument is an essential part of the Shakespeare mystery. Its enigmatic inscription, issuing a challenge to passersby, has been carved below a bust which, today, bears no resemblance to the etchings made after visits to Stratford by Dugdale, Betterton, and Thomas: visits that spanned more than a century. Chiljan therefore suggests the original bust was actually that of John Shakespeare; and that after his son’s death, Pembroke and Jonson appended the present inscription beneath the existing bust.



IVDICIO PYLIVM GENIO SOCRATEM ARTE MARONEM,  
 TERRA TEGIT, POPVLVS MÆRET, OLYMPVS HABET  
 STAY PASSENGER WHY GOEST THOV BY SO FAST;  
 READ IF THOV CANST, WHOM ENVIOVS DEATH HATH PLAST,  
 WITH IN THIS MONVMENT SHAKSPEARE: WITH WHOME,  
 QVICK NATVRE DIDE: WHOSE NAME DOTH DECK Ys TOMBE,  
 FAR MORE THEN COST: SIEH ALL, Yt HE HATH WRITT,  
 LEAVES LIVING ART, BVT PAGE, TO SERVE HIS WITT.

But for what reason? If it was to satisfy visitors who wished to pay their respects to the grave of the man believed to have been the great author, the plan would have quickly dissolved into farce, when visitors learned from local residents that the bust was actually Shakespeare's illiterate father, and had no connection to the inscription beneath it. Pembroke and Jonson were surely too intelligent to have failed to see the impracticality of this plan. Could one envisage a suitable epitaph to Beethoven set beneath a statue of his abusive, alcoholic father? The only reason for pursuing it is that it could explain why the original figure was nursing a bag of wool, instead of the present pen and paper. One imagines a more introspective approach would have served the author better, especially since this was touched upon when Chiljan queried why Mount Olympus was named instead of Mount Parnassus, the home of poets. It is now known this exchange of mounts allows the letter count of the only inset line on the inscription, together with mÆRET instead of MAERET, to total 34; thus providing the missing key to solving the monument's challenge, which identifies Edward de Vere as Shakespeare. When the sixaine is copied onto a 34-column grille, known as a Cardano grille, which had been in use since its invention in 1550 for concealing secrets in an otherwise innocent-looking text, it reveals a perfectly grammatical sentence, arranged in three clusters, which reads: "So Test Him, I Vow He Is E. De Vere As He Shakspeare: Me I. B." These initials in reverse are the same as those used by Ben Jonson in the First Folio.

There is also the damage to the monument incurred in 1645, during the English Civil War, when the church billeted troops. Chiljan suggests the bust was altered in 1691, when the damage to the chancel was repaired. But Charlotte Stopes recorded that repairs to the monuments were carried out at this time by descendants of the deceased, and among the names recorded, there is no mention of repairs paid for by Shakespeare's friends or relatives. Moreover, Stopes also reported that as late as 1730, Dr. Thomas reprinted Dugdale's book of *Antiquities of Warwickshire* [1656], which included the original copy of the bust and woolsack. In the Preface, Thomas stated he had made personal visits to the locations mentioned in the book, to check for accuracy before republishing it.

Part IV commences by examining the plays written by Ben Jonson for characters that appear to coincide with Shakespeare, according to the low opinion the author held of this man. It is an interesting and thoughtful exposition, which includes a similar search for equivalent characteristics in the Parnassus plays (written about 1598–1601 and mentioning many writers of the day, including Shakespeare). Chiljan then turns to the Shakespeare plays to add further evidence to her findings. This task is dealt with by

blending her interpretations with a unique view of how the great author used his genius to leave a lasting mark of his authorship in significant parts of what he wrote. *Hamlet*, *The Winter's Tale*, *Henry IV* (Part 2), and *As You Like It* figure prominently in supplying the required evidence.

Following on from this is an excellent analysis of the satirical booklet published in 1594, *Willobie His Avis*. Chiljan has a good grasp of the interplay between the anonymous author and his subjects, and one can see why the unknown author's first open recognition of Shake-speare [sic] by his full name; viz, "And Shake-speare, paints poor Lucrece rape" is an embarrassment to conventional biographers. It suspiciously hyphenates the unhyphenated name used by the author of *Lucrece* (1594), when he addressed Henry Wriothsley in the preface to his poem. Also embarrassing to orthodoxy, the Willobie poem introduces two characters by their initials, W. S. and H. W. in a "loving comedy." The former is described as "the old player"; Oxford was then 44 years of age, with a theatrical reputation at Court. The latter is referred to as the "new actor." Wriothsley was 21 when this was written. W. S. then tutors H. W. in the art of courtship; addressing him with a familiarity impossible for the real William Shakespeare, but not for Oxford. "Well met, friend Harry, what's the cause / You look so pale with Lented cheeks?"

Chiljan then builds upon this with a thoughtful display of the innuendo and allusions revealed in the writings left by the great author's contemporaries: writers who shared his secret. One of these was Nashe. It is only now that Chiljan makes known Nashe's reference to "Will Monox" and "his great dagger." The lure of combining these two references with Nashe's further mention of "Gentle Master William" and his "dudgeon dagger" was too obvious to miss, and Chiljan correctly identifies their joint meaning to be a covert reference to the Earl of Oxford aka Master William Shakespeare. But Nashe told Harvey that both he and Will Monox were at the banquet held shortly before Greene died. This places Oxford as one of the diners and no doubt the host who paid for the meal.

Chiljan, however, has already identified the diners as Greene, Nashe, Marlowe, and Peele. It is an unfortunate faux pas, but this should not be allowed to outweigh the book's positive aspects.

No book advocating an alternative author to Shakespeare can fail to offer a reason for the author's secrecy. Part V is devoted to the provision of evidence in support of the proposal that the Sonnets are a dialogue between father and son; that is between Oxford and Southampton, whose birth mother is said to be Queen Elizabeth.

After the Sonnets were published for the third time in the 18th century, the gender of Shakespeare's love had reverted back to a male. Thus began

more than a century of debate concerning the homoerotic content of the verse. The proposition that the relationship between poet and youth was paternal would therefore disperse any inference of sodomy, and offer a possible explanation for concealing Oxford's identity. But the paternity of Southampton is a well-trodden path, and Chiljan can only repeat what others have said before. By the careful selection of phrases from the Sonnets to support her theory, a positive picture can evolve. It is only when one realizes that the negative side has been purposely omitted that a more balanced outlook unfolds. Would a father speak to his son as the poet does in Sonnet 20, as "The Master Mistress of my passion;"? Passion then meant, "a mental state opposed to reason; a powerful and controlling emotion, such as lust." Also, when speaking of the youth's mother, Chiljan repeats the poet's words—"Thou art thy mother's glass, and she in thee / Call back the lovely April of her prime:" but neglects to quote the poet when he wrote:—"Dear my love, you know / You had a father: let your son say so." If Oxford had been the youth's father, he would have said—"You *have* a father." Also omitted is the Countess of Southampton's will, in which her choicest items were bequeathed to her son, rather than to her husband, who received the bulk of what remained. Chiljan also uses the Phoenix and Turtle by Robert Chester in the 1601 poem *Love's Martyr*, as metaphors for the love between Elizabeth and Oxford. But Chester was a Catholic, using the separated lives of Anne and Roger Line as an allegory for the phoenix, the Catholic faith rising from the ashes of the Reformation, and the biblical metaphor of a turtle dove, for the Holy Spirit returning from exile. Then again, if Oxford and Elizabeth had begotten Southampton, the politically, sensible solution would have been for them both to marry after the death of Oxford's first wife. This would have legitimized Southampton, and the absence of a natural heir to the throne would have been resolved. Alas, this straightforward resolution is left unexplored.

The book is an excellent source for factual material, but some theories chosen to weave them together into a consistent whole inevitably contribute toward unintended consequences.

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**Exploring Frontiers of the Mind–Brain Relationship** edited by Alexander Moreira-Almeida and Franklin Santana Santos. New York: Springer, 2012. xxix + 243 pp. \$129.00. ISBN 9781461406464.

The paradigm shift in physics that came with the establishment of quantum mechanics in the last century has implications for all the sciences, but that fact has been remarkably slow to sink in, perhaps in part because physicists themselves have not been able to agree on what it means. The one thing that seems incontrovertible is that quantum reality is qualitatively different from the everyday, observable macro-reality in which we commonly operate. At the level of the latter, Newtonian mechanics works well and supports a materialistic perspective, but underlying our quotidian world is a strange probabilistic sub-atomic world in which particles are waves and waves are particles and nothing is definite until it is viewed by an observer (or so some claim).

Perhaps it is not an observer, but *something* is causing wave functions to collapse and convert probabilities into actualities. Could that something be consciousness? That is the \$64,000,000 question. If the answer is yes, does that imply a kind of substance dualism, consciousness and matter as co-equal constituents of the universe, neither reducible to the other? Or could consciousness be primary and ultimate reality non-dual in the Indian sense? Physicist Henry Stapp (2009) considers these questions and concludes not only that the orthodox von Neumann interpretation of quantum mechanics points toward consciousness as primary (and ultimate reality as non-dual) but also that there is nothing in modern physics that rules out the possibility that our personal consciousness survives bodily death. That, he says, is an empirical question which cannot be settled by appeal to “a presumed incompatibility of such phenomena [i.e. phenomena which suggest survival] with our contemporary understanding of the workings of nature.”

In *Exploring Frontiers of the Mind–Brain Relationship*, Alexander Moreira-Almeida and Franklin Santana Santos have given us a set of papers that address a range of mind/body issues. The collection is an outgrowth of a symposium of the same title held at the University of São Paulo (Brazil) in 2010 and is published in Springer’s *Mindfulness in Behavioral Health* series. It includes 11 chapters arranged in four parts: Philosophy and History; Physics; Functional Neuroimaging; and Human Experiences as Promising Lines of Investigation of Mind–Brain Relationship, with a Preface and a Conclusion by the editors and a Foreword by psychiatrist and geneticist C. Robert Cloninger. The chapters are designed to be read independently, but the earlier ones build toward the later so that there is an overarching

thesis as well (p. xix). Many of the contributors are luminaries in the fields on which they report, and the book is aimed principally at academics and clinicians. Its strengths are its interdisciplinary approach, its appreciation of the spiritual dimensions of consciousness, and its balance of laboratory and field research findings. It does not shy away from the survival question but confronts it head-on with empirically based chapters on near-death experiences, end-of-life phenomena, mental mediumship, and cases of the reincarnation type, exactly the sort of material Stapp believed was needed to arrive at an answer.

The first chapter, by Brazilian philosopher Saulo de Freitas Araujo, is entitled “Materialism’s Eternal Return: Recurrent Patterns of Materialistic Explanations of Mental Phenomena.” Araujo observes that although materialism often is equated with science, the scientific method is about hypothesis testing. Materialistic scientists do not offer a way to prove their materialistic assumptions right or wrong but simply accept them as givens, so their position really is a metaphysical one and their materialism is promissory (Popper). Araujo shows how the rhetorical strategies used by materialistic neuroscientists today have been deployed since the 18th century periodically in support of similar views. U.S. philosopher Robert Almeder next considers “The Major Objections from Reductive Materialism Against Belief in the Existence of Cartesian Mind–Body Dualism.” The defense of Cartesian dualism is curious, given that other dualisms face fewer conceptual challenges (see Rousseau 2012:54 for a concise review). In “Psychic Phenomena and the Mind–Body Problem: Historical Notes on a Neglected Conceptual Tradition,” U.S. historian of parapsychology Carlos Alvarado documents the contributions made to the discussion of mind/body dualism by those who have studied psychic phenomena such as ESP, apparitions, and mediumistic communications.

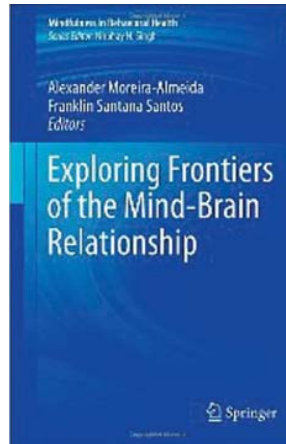
These three chapters compose Part I of the book. If they seem to signal a dualistic approach to the mind/body problem, that expectation is decisively dispelled in the two chapters of Part II. In “No-Collapse Physics and Consciousness,” British physicist Chris Clarke presents an interpretation of quantum mechanics diametrically opposed to the standard one Stapp endorses. Building on the heterodox speculations of Sir Roger Penrose, he suggests essentially that the wave function collapses on its own, without the involvement of consciousness. In “The ‘Quantum Soul’: A Scientific Hypothesis,” U.S. physicians Stuart Hameroff (who has collaborated with Penrose on several papers) and Deepak Chopra suggest that consciousness originates at the point at which quantum activity scales up to the level of classical mechanics and biological systems. The “soul” in this conception is reduced to “quantum information” (p. 90). The authors believe that their

model can explain out-of-body and near-death experiences “and conceivably an after-life” (p. 86). “A dualist perspective,” they write, “may not be necessary” (p. 90).

Part III, however, returns us to dualism. These two chapters are the first directed to clinicians. “The Neurobiological Correlates of Meditation and Mindfulness” by U.S. medical student Jesse Edwards, Brazilian psychologist Julio Peres, and U.S. physicians Daniel Monti and Andrew Newberg is a detailed and seemingly comprehensive review of the research domain. Although materialistic neuroscientists take this sort of data as proof that the brain produces consciousness, the authors see it in terms of “reciprocal interactions between the mind and body” (p. 107). This point of view is developed further in the following paper, “Functional Neuroimaging Studies of Emotional Self-Regulation and Spiritual Experience,” by Canadian neuroscientist Mario Beauregard. That we are able to intentionally regulate our emotional states shows that we are more than our brains, Beauregard submits. He summarizes the findings to date in this promising area, in which he has played a leading role. He extends his discussion to mystical experience but, regrettably, does no more than note the quantum neuroscience he has been a party to developing along with Stapp and neuropsychologist Jeffrey Schwartz (Schwartz, Stapp, & Beauregard 2005).

Part IV takes up case studies of phenomena that imply a separation of mind and body and suggest that some aspect of the human being may survive bodily death. None of the four chapters explicitly address Hameroff and Chopra’s quantum soul model, but readers who have come through the book from the beginning will reach them with it in mind and it seems appropriate to assess them in relation to it.

British physician Peter Fenwick asks “Can Near-Death Experiences Contribute to the Debate on Consciousness?” The literature on the near-death experience (NDE) has become so large and varied that reviewing it adequately in a brief space is nigh impossible and Fenwick is right to narrow his focus to the theoretical question, but even so this chapter is superficial in its coverage and specialists will find it more irritating than illuminating. Fenwick concludes that NDEs provide less support for mind/body dualism than for a field theory consciousness within a “transcendent reality” (pp. 160–161), although the two positions are not entirely incompatible (it is



all in how one looks at the problem, a Hindu would say). The next chapter, which Fenwick co-authored with book co-editor, Brazilian physician, and geriatric specialist Franklin Santana Santos, is a discursive review of end-of-life issues, aimed at medical-care workers. It does not take a stand on the mind–body question. Neither of these chapters offer anything inconsistent with the quantum soul model, as I understand it.

“Research on Mediumship and the Mind–Brain Relationship” by Brazilian psychiatrist and book co-editor Alexander Moreira-Almeida furnishes a good introduction to mediumship studies, ranging over both historical and contemporary sources. Moreira-Almeida argues in favor of mind–body dualism and survival, implicitly challenging the quantum soul model. I agree with much that he says. However, I must take issue with his handling of the Sumitra case as one of mediumship (p. 201). Sumitra fell ill and appeared to die, but revived with a different personality who called herself Shiva (Stevenson, Pasricha, & McLean-Rice 1989). Sumitra never returned and Shiva continued to possess Sumitra’s body until the end of her life (Mills & Dhiman 2011). One might call this a case of possession, although the possession is not mediumistic. It might best be characterized as “permanent possession” or “replacement reincarnation” (Matlock 2011:801).

We now come to the final substantive chapter, “Cases of the Reincarnation Type and the Mind–Brain Relationship,” by Icelandic psychologist Erlendur Haraldsson, one of the most accomplished field researchers in parapsychology. Haraldsson has studied more than 100 reincarnation-type cases, mostly in Sri Lanka and Lebanon, but this contribution is a disappointment. Instead of providing a general introduction to or overview of reincarnation studies such as Moreira-Almeida supplies for mediumship, Haraldsson emphasizes his own work. He describes four cases (all reported elsewhere) at length. While these serve to show the reader what this research is about, they give a very incomplete resume of the findings of reincarnation studies over the last half century (see Matlock 2011, and for an older but more comprehensive review Matlock 1990).

Haraldsson makes the important point that reincarnation-type cases consist of more than claims to have lived before. They include birthmarks and phobias related to the previous persons’ deaths. However, birthmarks are but one example of a large class of physical features, including birth defects and internal diseases, that appear to be transmitted from one life to another in these cases. Phobias, similarly, belong to a large class of behavioral traits linking the subject and the previous person, especially striking where there are differences of sex, caste, religion, or ethnicity. There are other recurrent features also, such as announcing dreams, which Haraldsson mentions

but effectively downplays. Moreover, I see no need to surrender to the materialists on the issue of memory (pp. 215, 229). There is a good deal of evidence that memory is not in fact stored in the brain, and neuroscientists such as Penfield have repeatedly challenged the materialistic view that it is (Kelly et al. 2007).

I have given special attention to the deficiencies of this chapter because of its place in the book. Several authors mention reincarnation in relation to the mind–brain dilemma and the book builds anticipation for it. This chapter could (and in my view should) have addressed some key issues more thoroughly and directly. Even more than mediumship, reincarnation puts the quantum soul model to the test. The behavioral and physical features of reincarnation-type cases suggest that conventional ideas of “mind” in relation to “body” may be too constrained. It may be better to think in terms of an animistic spirit/body dualism (Matlock 2011) than a mind-body dualism. “Intermission memories,” accounts of events between lives (Sharma & Tucker 2004), pick up where NDEs leave off, and like announcing dreams suggest an active role for a discarnate agent in effecting reincarnation (Matlock 2011). The quantum soul model does not anticipate and appears ill-equipped for the notion of discarnate agency, but, significantly, directed attention plays a central role in the quantum world and its interface with neural systems in the orthodox interpretation of quantum mechanics (Schwartz, Stapp, & Beauregard 2005). We may be closer than we realize to a quantum biological theory of survival and reincarnation, but this is more likely to be based on the orthodox interpretation of Stapp (2009) and Schwartz, Stapp, and Beauregard (2005) than the speculations of Penrose, Clarke, Hameroff, and Chopra.

The editors address dualism in their Conclusion. They appear to think that their book is an argument for dualism and that it presents a non-reductionistic theory of mind (p. 234). They do not seem to appreciate—or in any event do not acknowledge—that Clarke’s model is not dualistic and that Hameroff and Chopra’s idea that consciousness originates at the transition between the quantum and classical realms is as reductionistic as the standard materialistic view that consciousness is an epiphenomenon of brain activity. Had they wanted to advance an interactional dualistic thesis, the editors would have been better off going with Stapp for the physics contribution and allowed Stapp and Beauregard to explore further the principles laid out by Schwartz, Stapp, and Beauregard (2005). At a minimum, it would have been useful to have had this point of view aired, to give the reader an alternative to the quantum soul model.

This book is admirable in conception and valuable in bringing together data and perspectives that are not normally found between the same covers.

As in many edited volumes, the contributions are uneven, and there is an unacknowledged tension between dualism and reductionism, albeit reductionism of a post-modern sort. The editors might have done more to overcome these shortcomings. They could have exercised more control over content, and they could have addressed the tension between dualism and reductionism in their introductory and concluding chapters. Had they done the latter, the book would have become more explicitly a dialogue between different points of view, and possibly more successful. But I do not want to leave the impression that this is a bad book. It is not bad, though it could have been better.

The book is printed on acid-free paper and nicely bound, but its small print is conducive to eye strain, the index is light and poorly constructed, and there are an astonishing number of grammatical, spelling, and formatting errors in the text, and incomplete citations in the reference lists. Buyers have the right to expect more for the price.

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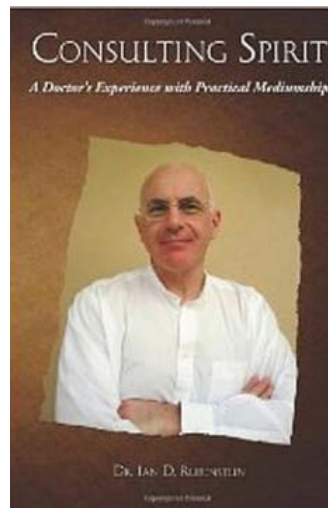
**Consulting Spirit—A Doctor's Experience with Practical Mediumship** by Ian D. Rubenstein. Anomalist Books, 2011. 280 pp. (paperback). \$15.95. ISBN 9781933665559.

A British general practitioner has written a book about his sortie into the world of spirits (known to the initiates as “spirit”), mediums, telepathy, psychic circles, ghosts and poltergeists, and all manner of matter that cannot be touched but only felt. A concatenation of coincidences nudges him gradually until his life becomes a tableau where the supernatural has become natural, yet business as usual is still transacted.

These are not quite C. J. Jung’s “meaningful coincidences,” which were structured through a parallel world of archetypes, a collective unconscious, and a subtle universal order, although there are similarities. Dr. Rubenstein’s happenstances, such as two teenagers trying to steal his car and all that ensues, are rather described as impish intrusions from the spirit world: spirit guides and dead relatives trying to get his attention.

And get it they do. By the book’s end, the doctor is a competent medium, and has added spirit contact to his armamentarium for grief-counseling selected patients. He is aware that many people are not receptive to the paranormal, but is surprised at how many of his patients are accepting, particularly when they know their doctor is.

This I can relate to, as I am a practicing family doctor (GP, general practitioner) with some interest in the paranormal. My medical practice, like Dr. Rubenstein’s, is quite conventional. I wrote a book in 2006 (Bobrow 2006) about paranormal phenomena in medicine, and although I never routinely mentioned this to my patients, some found out about it. Those who mentioned it to me were quite accepting of such events, and, for many, their lives and health were affected by these events, often adversely. I found it satisfying to be able to counsel an occasional patient that a “near death” type experience can occur in 10% of the population and doesn’t represent psychosis, or to work with a diabetic woman who admitted to multiple personality disorder and whose sugar I could never control because, it turned out, most of her alter personalities





were “not diabetic.” But while I became a better listener, Dr. Rubenstein became a participant, consulting with spirit for at least some of his patients. I get the impression that his is still a conventional medical practice but that the skills he developed as a medium are put into use occasionally.

*Consulting Spirit—A Doctor’s Experience with Practical Mediumship* describes, in easily readable prose, an unwitting journey from the humdrum to the bizarre. The book is broken into many short chapters and can be read comfortably and quickly. Despite his naïveté and skepticism at the book’s beginning, Dr. Rubenstein does describe an experience during his teenage years which turns out to be a forerunner of things to come. It’s a bit like discovery favoring the prepared mind. Healthcare practitioners might have some interest in this book. Those who practice mediumship would probably find it even more interesting. Try it.

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**Wenn die Dunkelheit ein Ende Findet: Terminale Geistesklarheit und andere Phänomene in Todesnähe [When Darkness Comes to an End: Terminal Lucidity and Other Phenomena around Death]**

by Michael Nahm. Crotona Verlag, 2012. 286 pp. €17.95 (hardcover). ISBN 9783861910244.

Michael Nahm, biologist and forestry scientist, is also known as a researcher and author of many publications about spontaneous paranormal experiences in current and past times. His current book *When the Darkness Comes to an End* is valuable, not only because it is an impressive case collection of the wide range of phenomena before and at death, but because it is much more than that. It discusses possible approaches to explanations for as-yet-unexplained phenomena. This concerns *light*, which has a double focus, one meaning a clarity of mind and the other meaning the unusual lights observed around a death. The terminal lucidity part of the subtitle (discussed in Chapters 1–4) refers to the first group of experiences, a sudden mental clarity shortly before death. The second part of the subtitle (discussed in Chapters 5–8) covers OBEs, NDEs, deathbed visions, and mysteriously acoustic and visual phenomena such as unusually beautiful music, lights, and fog. Further attention is given in the book to the aftereffects of those experiences on the mind and body. They can affect the personal worldview and attitude to life, but also lead to unusual bodily changes, spontaneous healing, and even healing in the context of hypnosis. The emphasis of the book is on cases of persons with serious psychological diseases and organic brain damage who have a sudden remarkable comeback of mental alertness shortly before the end of their life.

The book builds bridges between historical and new case reports and literature, integrates old reports by showing the same or similar basic patterns, and covers the huge field of experiences in the context of dying and death. It integrates several unique topics, which are, so far as I know, not described or even discussed in the current scientific context, presumably because they have been dismissed as anecdotal stories. Nahm's book finishes with a philosophical treatise on the nature of a "background reality" and on the value of a positive attitude toward death (Chapters 9–10). Seen as a whole, the text is a true pleasure for the reader, written in a very comprehensive style with accuracy in details and references—a rare combination in scientific literature. Many original case reports, both old and new, are cited and bring life to the matter. Michael's maxim: The reports should be taken as face value, even if some of them might have natural explanations or be in a way deceptive, since as a majority they could

speak for themselves. The book is rounded off with a useful international bibliography of not fewer than eleven pages.

Chapter 1 introduces "Terminal Lucidity." Nahm establishes that the topic has been known for centuries although it has only recently, thanks mainly to Peter Fenwick, caught the attention of science. In Germany his book is a first-ever on this topic. An overview is made of the medical pioneer work on the frequency of historical cases where the foregoing mental confusion is followed by the occurrence of spontaneous lucidity. Such a case is that of the "bear man," who had not spoken for 52 years, but imitated the behavior of a bear and only regained his speech when he became seriously ill. Recent cases are also reviewed, for instance that of a five-year-old boy who came back from a three-week coma and could finally speak to his family, the day before he died.

In the second chapter, he takes up the two major contrasting "Explanatory Approaches to Terminal Lucidity": reducing them to biochemical processes in the brain versus the impossibility of reducing them. The latter reductionist model was favored during the time of the German *Romantik* by Gotthilf Heinrich von Schubert and later by the renowned authors Carl du Prel, Friedrich Happich, and Wilhelm Wittneben.

The third chapter on "Mysterious Medical Results in the Brain" branches out, discussing the implications of "massive brain injuries and surgeries," hydrocephalus, and Savant syndrome, as well as infant prodigies. A section on "The Human behind the Scenery" argues for the importance of hemispherectomy, strokes, and other damage to the brain for a theory of the mind-body relationship. The question is raised whether the brain is the producer or filter for the content of consciousness. The case of the Keulen twins of Potsdam, Germany, who apparently are able to gain knowledge about the content of books without reading them, leads Nahm to see the door opening to a huge field of research and to speculate that if there are more such cases, the exact 1:1 relation between mind and brain, where brain is the producer of consciousness as the reductionist theory asserts, is deficient, while the filter theory would support the reality of ESP as the *modus operandi*.

"Unexplained Bodily Changes" around death are the focus of Chapter 4, a diverse chapter which additionally covers changes due to hypnosis and to multiple personalities along with "Healing and Improvement of Physical Symptoms Shortly before Death." Many of the previously forgotten cases along with contemporary case material (such as Penny Sartori's) are rather striking. Nahm introduces several other strange bodily phenomena along with their spontaneous improvement. One such case concerns a woman who was able to get rid of several painful warts under her feet by directing healing

light onto them during her lucid dreams. A more well-known case is the teenager who had skin that was like fish scales, a rare genetic disease (ichthyosis), which could be remarkably reduced by hypnotic suggestion. Michael deserves credit for pointing out that in this context “the sudden whitening of the hair” (leukotrichia) is a common enough phenomenon for research. He illustrates this with a case given by a former director of the medical university clinic in Frankfurt am Main. The conclusion from all this is that the mind or some psychological factor has a clear determining influence. In short, mind reigns over matter. The philosophers of the era of the German *Romantik* got it right after all.



Chapter 5 gives an overview of “Near Death Experiences” including OBEs which among these topics is the most known area of research internationally, while Chapter 6 focuses on the “Visions around Death,” the so-called deathbed visions. Michael Nahm is forthright in relating his own experiences including a personal OBE in which he perceived accurate details of the surroundings. He has also experienced about 100 lucid dreams. Nahm gives his reasons for rejecting the biochemical hallucinatory theory and thereby questioning the 1:1 relationship between mind and brain. For him there are too many facts that seem to contradict this view, and which fit rather better into the filter model.

Less known and discussed than other NDE phenomena is the occurrence of “Mysterious Music around Death,” which forms the theme of Chapter 7. Michael has collected 47 cases of this sort dating back to Gregor the Great (540–604), but in 31 cases the “supernatural” music, as it is often described, was heard by several persons, sometimes independent of each other and at different times, but nevertheless at the same deathbed. This further leads Nahm to support the Romantic idea, that here there may be “aspects of the otherwise hidden reality penetrating the awaking consciousness of humans and connecting especially those who are very near to each other” (p. 232).

Chapter 8 about “Mysterious Lights and Fog around Death” focuses on some neglected aspects of the literature. Here we are concerned not with the so-called light at the end of a tunnel or the being of light associated with NDEs, but rather the lights seen by others around dying persons.

Nowadays those lights can still be seen in various forms, such as foggy clouds or flickering air, dark shadows or shining haze at the time of death, most often emanating from the lower part of the body. Michael Nahm's collection consists of 113 such cases plus 29 special cases in which bright halos were seen around the head. This fascinating phenomenon, which was understood in traditional folk language as the soul leaving the body, up to now unjustly has been completely ignored by science.

Chapters 9 and 10 on "The Background Reality of Being (*Sein*)" and "Death as a Friend" is a well-reasoned, philosophical discussion of the physical and biological aspects of the universe, including an excursion into the difficulties of evolutionary biology (Chapter 9), and suggestions on how to come to terms with death (Chapter 10). Here again the orthodox biochemical explanation runs into difficulties. Nahm shares the view of Georg Friedrich Daumer (1872) when he expresses his incomprehension at the far-fetched hypothesis of mainstream science since it seems those unexplained phenomena "obviously belong together and have a common ground" (Daumer). Last thoughts are given to ethical implications of the themes of the book and in appealing to the "human behind the scene" (p. 273) by emphasizing the deeper meaning and individual task in the life of every person, albeit often unknown. The final message is: Make friends with death, but be patient and take life as it is.

*Wenn die Dunkelheit ein Ende Findet* promises an inspiring reading, offering hard data as well as a philosophical treatise stretching out to the unknown. The philosophers of the *Romantik* would have liked it and I hope many others today will also.

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**Debunking Delusions: The Inside Story of the Treatment Action Campaign** by Nathan Geffen. Jacana Media (South Africa), 2010. 236 pp. \$24 (paperback). ISBN 9781770097810.

This book's lead title seemed to make it a natural for review in the *Journal of Scientific Exploration*; but it is the subtitle that properly describes the contents: It deals almost exclusively with South African controversies, about HIV/AIDS in particular and medical matters more generally.

Still, there are points of general interest. When a belief does not correspond to reality, the believers can go far astray in their actions and their recounting and explicating of events. So it is with this book, which is based on the mistaken notion that a retrovirus designated HIV causes fatal illness—AIDS—by destroying the immune system. Readers should also beware of the frequently used but entirely misleading terms “AIDS denialist” and “AIDS denialism”: No one denies the existence of AIDS. What is denied is that AIDS is a new syndrome and that HIV is its cause.

“Cognitive dissonance” refers to the fact of human psychology that makes it essentially impossible for true believers to recognize—to *see*, to *take in*—evidence that falsifies their belief. The classic description was by Festinger, Riecken, and Schachter (1956). Cognitive dissonance is rampant among those who have accepted HIV/AIDS theory, and this book illustrates it.

Mention in the Acknowledgements (p. x) of a “partner of nearly two decades” whose given name is masculine indicates that Geffen is gay. AIDS was at first uniquely troubling for gay men because of its apparently unique association with them—it had at first been named GRID for gay-related immunodeficiency. It was natural, then, for gay activists to welcome the notion of a viral threat to everyone rather than ascribing the illnesses to aspects of unwise “fast-lane” lifestyle exemplified by a small subgroup of gay men exulting in the “gay liberation” of the 1970s. Still, the evidence was rather clear from the beginning that injudicious and promiscuous use of recreational and prescription drugs was the prime “risk factor” for contracting AIDS (Lauritsen 1993). Geffen notes that in South Africa homosexuality was still illegal when AIDS appeared in the United States, and the puritanical ambience was such that even mention of condoms was frowned upon (pp. 18–19). All this makes it understandable that Geffen will have been a prime candidate to swallow HIV/AIDS theory whole. For a dozen years or more now he has been active in the Treatment Action Campaign (TAC) whose aim is to make antiretroviral medication available as widely as possible.

I can sympathize with Geffen also when he describes some of the blatant quackery that exploits the fear of AIDS (pp. 3–10), and the book critiques appropriately and in detail one exemplar, Tine van der Maas (p. 106 ff.), of the rather numerous charlatans who capitalize on the panic about HIV to peddle quack remedies. But it is the unwarranted hysteria over “HIV” that led many frightened, gullible people to turn to quacks. Quack theories spawn quack remedies.

For Geffen and his ilk, the tragedy is that the path to hell is paved with good intentions, and cognitive dissonance effectively screens that path from the truth, no matter how obvious the clues may seem to others. Thus Chapter 2, “What We Know about Aids” (British usage), begins with an epigraph from Harold Jaffe, a venerable HIV/AIDS researcher, referring in 2008 to the “global epidemic”: Even though it had been obvious by then, for many years and including in all official datasets, that there never has been a global epidemic. The prevalence of positive HIV tests is well under 1% in every region of the world except those populated by people of a particular subset of African ancestry: sub-Saharan Africa, the Caribbean, and small areas such as Washington, D.C. The racial preference of HIV for such ancestry is in itself a strong clue that HIV tests do not detect a sexually transmitted retrovirus (Bauer 2007).

Within Chapter 2, “The Origin of Aids” repeats the absurdly incredible tale that HIV crossed to humans from chimpanzees in West Cameroon, probably early in the 20<sup>th</sup> century, and “was not widely present in South Africa until at least the mid-1970s.” In the meantime it had supposedly jumped to Haiti, and from there to the United States, where for the first time it caused actual illness. But for 30 years it has not spread within the United States outside the initially affected groups, remaining at the same level of about 1 million; but it is exceptionally prevalent among African Americans all over the country. In Haiti, the prevalence has also remained steady for more than two decades, at roughly 5%. And since the 1990s, southern Africa has been the epicenter of both HIV and AIDS—not West Cameroon or Central West Africa where it was all supposed to have started. Geffen mentions the dramatic changes in demographics (p. 18) but suggests no possible reason for it. This scenario is simply not believable as the course of a sexually transmitted disease. Yet Geffen believes it, which induces him to believe as well that “recent swine and avian flu outbreaks also show that for a virus to cross from animal to human is not unusual” (p. 14).

Cognitive dissonance affects HIV/AIDS believers severely when it comes to medications. Like many others, Geffen is disingenuous about the first AIDS drug, AZT (nowadays called ZDV, zidovudine), by admitting that antiretroviral treatment before 1996 was “not particularly effective”



(p. 19); in reality the data indicate that AZT directly killed about 150,000 “HIV-positive” people in the United States alone (Bauer 2008) during the decade that it was the typical monotherapy. Geffen’s TAC displayed cognitive dissonance again quite recently when it protested against a planned trial comparing stavudine to tenofovir because the former was said to be distinctly more toxic—even as the toxicity of the latter is well-established in the mainstream literature, in particular that it causes kidney failure (Bauer 2011). “Labour and breastmilk” are indicted for infecting 60,000 babies annually, even as several studies have shown that exclusively breastfed African infants are less likely than others to become “HIV-positive” (Bauer 2012).

Few people feel able to assess technicalities of the research literature for themselves, and so they have to rely on the views of specialists. The tragedy of HIV/AIDS began when some of the most competent specialists, who from the outset recognized the flaws in HIV/AIDS theory, were brushed aside by those who exercised power within government agencies, notably the National Institutes of Health and the Department of Health and Human Services. Geffen simply parrots what official sources say. For example he cites Gallo’s four papers in *Science* as showing clearly “that Aids was caused by a newly discovered retrovirus”: But those papers do not even claim to do that, they found retroviruses in only 26 of 72 AIDS patients and 18 of 21 who had “pre-AIDS” (Gallo et al. 1984).

It is one thing to understand the circumstances that predisposed Geffen to accept HIV/AIDS dogma and thereafter to suffer corollary cognitive dissonance; it is something else to excuse the presentation of rumors and shibboleths as though they were established fact. I would categorize as inexcusable Geffen’s assertions, with no sources cited (pp. 22–23), that:

- Healthcare workers have been infected with HIV and then developed AIDS. There are no such cases of that certified in the literature.
- “HIV [has been] photographed regularly using electron microscopes.” No.
- There is a correlation between viral load and health. No (Rodriguez et al. 2006).
- It is understood how HIV supposedly destroys the immune system. No:

It is not clear how much of the pathology of AIDS is directly due to the virus and how much is caused by the immune system itself. There are numerous models which have been suggested to explain how HIV causes immune deficiency. (The Pathogenesis of AIDS 2009)

After three decades, no consensus has been achieved on how HIV can possibly do what it's claimed to do.

Geffen is utterly out of order when he writes (p. 23), "It is fair to say that there is no other infectious disease whose cause has been confirmed as thoroughly as HIV." The writer is not a doctor, nor a medical researcher, nor a biologist, nor a historian or sociologist of medicine. He makes this unequivocal sweeping claim purely on faith in the knowledge that others claim to have. And without even citing specific primary sources.

So Chapter 2 is best ignored. However, Geffen knows whereof he writes in Chapter 3, a history of TAC. One cannot quarrel with TAC's campaign to make necessary drugs available at reasonable price, including allowing generic drugs to be imported, beginning with the antifungal fluconazole. Since then the concern has been with antiretroviral drugs. HIV/AIDS devotees believe those drugs to be beneficial, but HIV/AIDS dissidents believe that they do more harm than good. Most of Chapter 3 describes the confrontations between TAC and the Government over this issue.

Chapter 4, "Tradition and Science," begins with anecdotes of individuals who did not benefit from traditional healers but responded well to antiretroviral drugs. Traditional African medicine is discussed in reasonable fashion, in the context of placebo and such comparable Western traditions as homeopathy; including an acknowledgment that some traditional herbal remedies can actually be harmful, and that some regulation of traditional healers would be a good thing. Geffen makes the revealing statement (p. 96) that he is a great fan of Wikipedia and is "no longer too snobbish to reference" it, underscoring how unreliable his sourcing is.

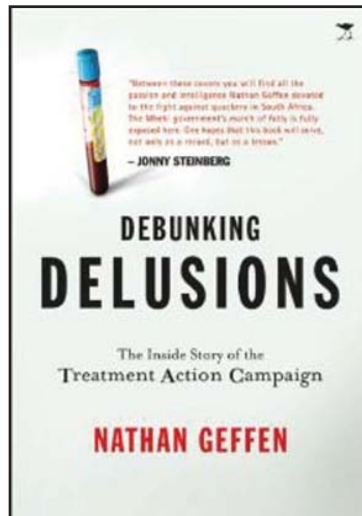
Chapter 5 begins by contrasting two "HIV-positive" individuals, one using antiretroviral drugs and the other a nutritional approach. Geffen acknowledges that this is no sort of scientific trial—but then contradicts himself immediately by asserting that "science does tell us . . . the most likely outcome," implying that the outcome actually means something scientifically. Chapter 6 then attacks Dr. Matthias Rath, a fully qualified Western physician who worked with Linus Pauling and has an evidentiary basis for recommending nutritional supplements. Geffen does demonstrate that Rath is often involved in legal disputes—but also that he wins some of them. The crux is that Geffen does not question diagnoses of "AIDS" and "HIV-positive" whereas many better-qualified dissenters from HIV/AIDS dogma recognize that "AIDS" might reflect malnutrition or a variety of actual illnesses and diseases that might have nothing at all to do with "HIV." Some physicians (for example, Juliane Sacher (2006) and Claus Koehnlein (Duesberg, Koehnlein, & Rasnick 2003) in Germany) have had better success treating "AIDS" patients by specifically targeting their manifest

conditions, compared to other physicians who relied solely on antiretroviral drugs. An advertisement by Rath showed a bottle of AZT with its skull-and-crossbones warning of toxicity; Geffen characterizes this as misleading because it was “an experimental bottle of AZT. . . . This was a liquid form of AZT not distributed to patients. . . . [and] used by a research company called Sigma-Aldrich” (p. 136). This is massive ignorance on an important matter: There is no liquid form of AZT, though one can make solutions of it; there was nothing “experimental” about the sample; AZT was *made* by Sigma-Aldrich, which is a well-known supplier of chemicals, and though it doubtless does research pertinent to its manufacturing business, it isn’t a “research company.”

Chapter 7 goes into great detail about TAC’s legal actions against Rath. Again there is the cognitive dissonance against recognizing that antiretroviral drugs are known to cause heart disease as well as other organ failures (Guidelines, no date). There is much about the maverick lawyer and maverick HIV/AIDS dissident Anthony Brink, and Geffen shows that he fails to see himself as others see him: His debate with Anthony Brink was “like taking candy from a baby” (p. 134), yet under this reviewer’s reading of the transcript, it reveals no victor.

Chapter 8 concerns itself with questions of controlling drugs and complementary supplements in South Africa, and Chapter 9 summarizes TAC’s success in changing official policy, through targeting supporters of the party in power, wooing the media, and campaigning at the grass roots in the community, which Geffen describes as “treatment education.” He also credits such education with bringing better adherence to taking antiretroviral drugs. In the United States, compliance with “HIV/AIDS” medication is a hot issue because the “side” effects of the drugs are so debilitating. Geffen repeats claims of lives lost because antiretroviral drugs were not made available, even though those claims have been soundly debunked (Duesberg et al. 2011).

The depth of Geffen’s passion shows when he calls for “denialists” to be brought to account (pp. 199–200), an unfortunate indication of vindictiveness when earlier he strives to emphasize selflessness and high-



mindfulness. The book will interest primarily people already interested in HIV/AIDS matters or in the social and political struggles in South Africa's nascent democracy. Readers should be aware that the approach is explicitly partisan and fails even to cite in the Bibliography central works by people with differing views, for instance Robert Root-Bernstein (1993) (not to speak of Peter Duesberg (1996)), or reports that show the facts in a different light, as those by South African journalist Rian Malan (2001, 2003).

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**The Woman Who Changed her Brain: Unlocking the Extraordinary Potential of the Human Mind** by Barbara Arrowsmith-Young. London: Square Peg, 2012. ISBN 9780224095181. New York: Free Press, 2012. ISBN 9781451607932. 268 pp. Kindle e-book. ISBN 9781451607956.

This book describes the difficulties experienced by Barbara Arrowsmith-Young of growing up with severe learning difficulties; the means whereby she found the techniques, not to live with those difficulties, but to actually address and resolve them; and how she has brought those techniques to children and adults through her 35 Arrowsmith Schools now established in various parts of Canada and the U.S.

Barbara grew up in Peterborough, Ontario, where for the first 26 years of her life, she “lived in a fog.” She could make no sense of the relationship between the hour and minute hands of a clock, so could not tell the time. She could not add or subtract double-digit numbers; had difficulty reading; got the wrong words for common objects; could not tell the difference between the right and left hands; was accident-prone; kept getting lost; and could not tie her shoelaces.

Barbara could however remember (parrot) accurately the 9 o’clock news and was evidently gifted with a remarkable memory and sense of determination. These abilities got her through school and university. The change came in graduate school when she happened across Alexander Luria’s 1972 classic *The Man with the Shattered World: The History of a Brain Wound* which described the cognitive deficits of a brain-injured soldier from WWII. Barbara identified with the soldier among whose difficulties was the inability to tell time following his brain injury. Around the same time she came across some work by Mark Rosenzweig (Rosenzweig, Love, & Bennett 1968) which demonstrated that the brains of rats could physically change in response to stimulation. If it was possible for rat brains to change, then maybe it was possible for human brains to change also.

She created flash cards with pictures of clocks and studied every day for up to twelve hours each day, gradually making the tasks more complex (like adding second hands and day hands) and demanding faster response. She does not say how long she worked at this but it appears to have been several months. Eventually the fog cleared, she not only could tell time but points of logic, grammar, and math now made sense. Now she could suddenly understand, not merely parrot, the TV news programs.

Not all her difficulties were resolved by the clock exercises, so she developed new cognitive exercises to address each difficulty in turn based on knowledge of the functional anatomy of the brain. These exercises are now routinely used in the Arrowsmith program.

Since individual parts of the brain are involved in multiple activities, a defect in one part may have multiple consequences. Therefore, the first step is to isolate which part is causing the manifest difficulty through psychological testing. These component cognitive deficits are described on the Arrowsmith website (Arrowsmith 2012) and in the book. There is sufficient detail that it would be possible to isolate which of the 19 brain functions isolated is in deficit for a relative or acquaintance, though probably not as accurately as with the formal tests.

For example, the facility for “motor symbolic sequencing” is involved in reading, writing, and speaking. People with impairments to this facility often misread texts, their handwriting is poor, and copying text is slow and inaccurate. Spelling of the same word can vary from instance to instance. They often make apparently arithmetical errors but in reality they are making motor errors due to thinking the right answer but writing another. In severe cases they may have difficulty communicating ideas because they ramble and leave out much information, making it difficult for others to follow.

There are some hints also of the kind of exercises that could resolve the various difficulties, but clearly attendance at one of the Arrowsmith schools is advised.

One of the exercises for the above symbolic sequencing deficit is to trace Chinese (or other foreign) characters with the right (dominant) hand while wearing an eye patch over the left eye. The idea is to stimulate the left supplementary motor area responsible for eye–hand coordination. The several motor symbolic sequencing disabilities mentioned above tend to resolve without being directly addressed.

The exercises are arranged in sequence to gradiently address the difficulty. The tasks must be neither too easy nor too hard so that the student has to, and is able to, make a conscious effort to engage with the task. Once the difficulty is resolved, the change is permanent.

The average student is usually enrolled for 3 to 4 years, but progress is maintained through the period so if a student is unable to complete the three-to-four year program, they nevertheless benefit for every year they are in the program. In one study (Lancee 2005) of 79 students aged between 5 and 19 (average 11) who were assessed on 15 standard educational attainment tests for reading, writing, comprehension, and arithmetic, 29 had between 1 and 7 scores below the 25<sup>th</sup> percentile, the lower end of the normal range. These students were considered to have the *least* severe learning difficulties. The average attainment in this group was at the 15<sup>th</sup> percentile at entry but after one year the average was at the 41<sup>st</sup> percentile. Even the 10 most severely affected students at intake (all 15 test scores below the 25<sup>th</sup> percentile) with

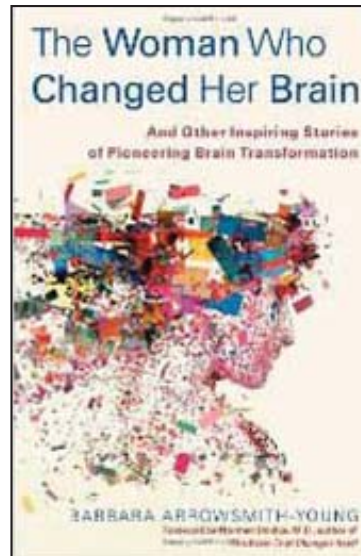


average attainment at the 5<sup>th</sup> percentile had average attainment at the 25<sup>th</sup> percentile after 3 years. Age, gender, and IQ do not evidently affect the rate of progress significantly.

It must be stressed that these cognitive exercises are not the same kind of thing as the computerized “brain-training exercises” that are the current fad and evidently form the basis of a multi-million dollar industry. A six-week study (Owen et al. 2010) of 11,430 participants who “brain-trained” several times each week showed that there was no evidence that the training led to any improvement in untrained tasks or any general improvement in cognitive functioning. Improvement was observed on the specific tasks trained only. Evidence that some exercises aimed at “surface abilities” over a six-week period are ineffective is not evidence that some exercises aimed at specific “atomic abilities” for several hours per day over several months to several years are not effective.

Cognitive learning difficulties can lead for those affected to social isolation and exploitation, thus compounding their difficulties. Emotional and arousal problems that may arise through this mechanism are not addressed by the Arrowsmith program. Of course emotional and arousal problems can occur through psychosocial mechanisms in those not affected by learning difficulties. A wholly different approach to these appears to be needed.

What is interesting about the efficacy of these cognitive exercises is what they say about the philosophy of mind. Since they are based on an understanding of the correlations between brain anatomy and brain function, they may seem to give physicalists no cause for concern and even vindication of their “mind is brain” stance. But as we are constantly reminded, “correlation does not prove causation.” The puzzle in philosophy of mind is how physical processes can give rise to consciousness. There have been numerous “solutions” to this puzzle, including the idea that consciousness has no causative powers and influences the workings of the body (and brain) with the same power as “the steam-whistle which accompanies the work of a locomotive engine . . . without influence upon its machinery” (Huxley 1912). This stance known as “epiphenomenalism” is





commonly held in some philosophical (Pauen, Staudacher, & Walter 2006), neuroscientific (Soon, Brass, Heinze, & Haynes 2008), and psychological (Wegner & Wheatley 1999) circles. It is flatly contradicted by the fact that the Arrowsmith exercises are done with *conscious* effort. It may not be psychokinesis, but the evidence is that the brain is changed by the continued effort of a conscious mind. At least that is the simplest and most direct explanation.

The Arrowsmith program deserves to become known and applied worldwide. I hope the organization Barbara Arrowsmith-Young has put together will be able to cope with the demand and the inevitable criticism and conflict that will follow.

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**The Aztec Incident: Recovery at Hart Canyon** by Scott and Suzanne Ramsey, Dr. Frank Thayer, and Frank Warren. Mooresville, NC: Aztec 48 Productions, 2012. 217 pp. \$24.95 (paperback). ISBN 9780985004606.

One scarcely knows where to begin. Perhaps with this quote from a June 19, 1951, letter—reprinted in these pages (90–91)—written by *San Francisco Chronicle* editor Paul G. Smith to *Variety* entertainment columnist and author Frank Scully: “Frankly, I recall that when I first saw your book I thought you were merely having fun with your readers.” The book, the already scandalous *Behind the Flying Saucers*, which Henry Holt had issued the previous September, was a marketplace success but a disaster in every quarter that did not involve commerce. Even so prominent an early UFO proponent as Major Donald Keyhoe, the first outsider to investigate Scully’s claims of a 1948 saucer recovery near Aztec, New Mexico, rejected them as absurd and fanciful. When I read Scully’s book in junior high school, my impression—even as a naive adolescent—was the same.

In fact, though they circulated freely through the larger society, because of the Scully association, tales of UFO crashes were spurned by mainstream ufologists until the late 1970s. Around that time, a respected colleague, the late Leonard H. Stringfield, began collecting what he called “crash/retrieval reports” from mostly anonymous sources with whom he privately communicated.<sup>1</sup> In 1980 the first major book on the subject, *The Roswell Incident* by Charles Berlitz and William L. Moore, saw print. Other books, mostly though not exclusively focused on Roswell, followed (and an Air Force refutation followed them in the late 1990s, succeeded by refutations of the refutation, and so on in continuing loop to the present).

Inevitably, Scully’s narrative—at least in a cleaned-up version that did not incorporate the dead Venusians of the original—would get a second look. The first book-length treatment was William S. Steinman and Wendelle C. Stevens’s *UFO Crash at Aztec* [1987], a work notable only for its levels of paranoia (high) and coherence (low). The second is the new *The Aztec Incident*, based on what we are told is a \$500,000 investment in research expenses and more than two decades’ worth of inquiry.

First, so that future authorial references will be clear, the crowded byline is courtesy of a writing novice’s error that no experienced author would have committed. There is only one author—Scott Ramsey—who refers to himself in the first person throughout. The other three, who participated in one way or another in accumulating the material that made the book possible, ought to have been relegated to the credits, and not represented as co-authors. Thus, in what follows, I refer to the real author in the singular.

Since there is much panning and little praising in the comments that follow, let's start on the most positive note circumstances render available. *The Aztec Incident* reprints some of the private correspondence, never before seen as far as I know, of the principal figures in the episode. As one who has written at length on the history of the UFO controversy in all its dimensions, including its less lucid moments, I like that. The off-stage voices, I have found, are often illuminating.

Here, however, the revelations are modest. One never imagines for a moment that Scully *appreciated* the efforts of investigative reporter J. P. Cahn (who memorably uncovered the confidence swindle behind *Behind the Flying Saucers* in a couple of hard-hitting, entertainingly documented *True* articles),<sup>2</sup> but it is interesting to read this record of his grievances against Cahn's hard-charging approach. And who can blame Scully? Though as late as 1984 Cahn remarked that he had always liked Scully personally, clearly the affection was not destined to be reciprocal. At the end of the job, Cahn had exposed Scully as—in the most charitable interpretation—a fool.

Unfortunately, one thing *The Aztec Incident* does not address—cannot address by its very purpose, which is to turn dross into gold—is to what degree Scully was a party to the hoax. To his death in 1964, Scully professed his confidence in what his informants, whose probity he endorsed in the face of overwhelmingly contrary evidence, had reported about the crash in New Mexico along with others, less detailed, in Arizona, Maine, and elsewhere in the late 1940s.<sup>3</sup> My supposition, for which I make no larger truth claim than I can glean from observation of his behavior over the years, is that Scully was initially gulled into acceptance of the yarns, then grew eventually to grasp that he'd been bamboozled. By that time, he was sufficiently invested in the bamboozlement that he felt he could not disown his book and the attendant controversy; if it took whopper-forging to sustain his otherwise untenable position, then smalltime grifter Leo A. GeBauer—top magnetic authority “Dr. Gee” in *Behind the Flying Saucers*—would become, years later, a composite figure representing not GeBauer but some of the leading magnetic scientists in America. (In reality, a waitress had given GeBauer the nickname “Dr. Gee,” according to GeBauer's widow, and Scully merely borrowed it for the book.) In other words, Scully was complicit in the hoax. The only remaining question is if that complicity happened sooner or later.

Obligingly, Ramsey devotes an eye-glazing chapter (Chapter 4: Dr. Gee and the Mystery Men) to profiles of eight leading magnetism-studying scientists of mid-century America. “Without a doubt,” he insists (p. 51), “they possibly knew or worked with Silas Newton, a man of science himself.” Only a book as rhetorically hapless as *The Aztec Incident* could cram “without a doubt” and “possibly” into the same pronouncement

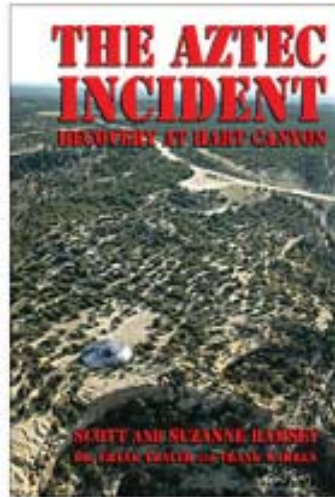
without betraying the faintest cognitive dissonance, and then proceed from there to characterize lifelong swindler Newton not only as a “man of science” but as a major one at that, sharing his purported colleagues’ access to the U.S. Government’s classified extraterrestrial technology. Having pronounced as much, Ramsey feels no obligation to provide a fragment of actual evidence that links these eminent scientists to Newton. For that matter, he fails even to document his repeated assertion that Newton was a significant figure in the oil industry.

It is Newton who was the intellectual author, if that’s the phrase, of the Aztec legend. His stories would almost certainly have been forgotten months after their concoction if not for Scully. In the consensus-reality version, here highly condensed and necessarily incomplete, is how *Behind the Flying Saucers* came to be:

The print record—no prior press references to the described event, said to have taken place on March 25, 1948, have ever been located and are almost certainly nonexistent—begins with Scully’s *Variety* column of October 12, 1949, where he reports having learned from unnamed “scientists” of two saucer retrievals, one in the Mohave Desert, the other in the Sahara. The latter vanishes from the story hereafter, but in Scully’s account the scientists examined the American ship (intact but for a small hole in a port window), presumed to be from Venus and housing 16 human-like midgets—all dead and “charred black”—clad in 1890s-style clothing. The ship, it turned out, flew along “magnetic waves.” All of its dimensions are evenly divisible by nine.

*Behind the Flying Saucers*, published 11 months later, mentions two Arizona crashes but provides few details beyond the allegation that the bodies were identical to those found at Aztec and that the alien mathematics appeared nine-based.

It developed that Newton and GeBauer had imparted these tales to Scully in August 1949. GeBauer had shown Scully parts from the saucer, among them a tubeless “magnetic radio.” It is generally assumed that the location for the story has its origins in a trip GeBauer took early that same month to Hart Canyon near Aztec—a small town in the northwestern Four Corners part of New Mexico—to demonstrate his alleged oil-detection



device (the sort of thing known derisively in the industry as a “doodlebug”) to locals. Hart Canyon would evolve into the location where the ship came down and was recovered.

As Cahn and—much later and in considerably more detail—William L. Moore<sup>4</sup> would determine, Newton and GeBauer had devoted their lives (the smart and polished Newton more lucratively than the relatively slow-witted GeBauer) to various confidence operations, many involving oil-finding schemes. Characterized wryly by Moore as “the type of character best avoided by anyone with money in his pocket,” Newton got into trouble in the 1930s in New York, Kansas, and California for assorted shady dealings. “Newton’s tactic in every case was to suck in additional investors,” Moore wrote, “and pay off the complaining party with the money raised—in exchange, of course, for the dropping of charges against him.” When he died in Los Angeles in 1972, Newton had 40 legal claims filed against him based primarily in fraudulent oil and mining schemes. Two years earlier, he had been indicted for grand theft.

The saucer story was intended to draw the interest of the well-heeled, who would soon learn that GeBauer’s doodlebug (the “magnetic radio”), in reality made up of ordinary mechanical parts, was a product of extraterrestrial technology. In other words, if not for Scully’s broadcasting the story to a national and international audience, it would have been no more than another of Newton/GeBauer’s ephemeral efforts to separate fools from their hard-earned.

In attempting to rehabilitate the Aztec “case,” Ramsey falls into the fatal error of defending the indefensible, namely Scully, Newton, and GeBauer, rather than conceding their manifest deficiencies and drawing up an Aztec episode that is not so fundamentally dependent upon their being who they clearly weren’t. From one way of viewing it, Ramsey’s approach is ill-considered. From another, his book wouldn’t exist without *Behind the Flying Saucers* and all it brought into the world. There’s little else outside Scully’s pages, and even there, there isn’t much. One thinks of Woody Guthrie’s famous crack: “That stew was so thin even a politician could have seen through it.”

Ramsey’s defense is unlikely to sway any but guile-free readers. To any critics Ramsey responds with the self-serving words of Scully, Newton, and GeBauer, presented as the equivalent of divine revelation standing unshaken against the darkly driven contrary charges of Cahn, portrayed invariably as pursuing a “petty vendetta” motivated by pure “envy,” or else—and what else?—doing the dirty work of some sinister intelligence agency. To anyone immune from Aztec fever, Cahn emerges as an old-fashioned, aggressive shoe-leather reporter of a type sorely missed in this

era of celebrity journalism. If Moore is mentioned, it is so briefly that I missed it in the extensive notes I took during multiple readings of *The Aztec Incident*. The back pages that should have been devoted to an index are taken up with irrelevant photographs of historic Aztec.

Affirmation of unswerving faith in Scully's severely flawed sources is not quite all of Ramsey's book, however. After half a million dollars and more than two decades, he has his own evidence to put forward. That evidence, he boasts, makes the Aztec recovery "true beyond argument." Or maybe not.

First, however, it must be stressed that for as long as they have been asked about it, Aztec residents have with virtually one voice denied that anything like a UFO retrieval happened there on March 25, 1948, or on any other date. That includes the man who was newspaper editor during the period, the 1948 county sheriff, the son who succeeded him in that office (all of whom actively and fruitlessly sought out local informants), the family that owned the property, and other longtime residents.<sup>5</sup> They first heard of an extraordinary UFO incident through the publicity surrounding Scully's claims or its revival in subsequent decades. This contrasts tellingly with Roswell residents, to whom an incident many tied to the crash of an unknown object—however conflictly interpreted—was widely known. No one has to prove that *something* happened in the Roswell area in July 1947.

The book opens with Ramsey's two claimants to first-person experience at the site. Both contradict the original—Scully—account in notable ways. Newton's drawing of the craft, shown to a University of Denver class to whom he lectured notoriously on March 8, 1950, depicts, in researcher Joel Carpenter's words, "a bizarre contraption that . . . resembled a can on top of a [spinning] saucer."<sup>6</sup> The alleged witnesses, by contrast, speak of a disc with a dome on top and a corresponding one on the bottom. In Scully's account as related by Dr. Gee, it took a team of scientists *two days* to break into the craft, whereas in Ramsey's version it took a few hours for locals to gain entry well before the arrival of official personnel. (In both stories a pole poked through a small porthole opening manages to push a door handle, exposing the craft's interior.)

There are two, and only two, named persons who speak from what is represented as first-hand experience. One, Doug Noland, was interviewed by Ramsey after a "series of strokes." The other, Ken Farley, since deceased, was "dying of a respiratory disease." Ramsey has their alleged experiences occurring on the Scully-approved date of March 25, 1948, without ever explaining how they remembered it with such precision decades after the alleged fact. One can only suspect an editorial insertion into the narrative, hardly the first one.



Even as these narratives would have us believe that dozens of civilians congregated at the site, independent testimony to that effect is hard to come by. Ramsey's rhetoric is slippery enough to mislead a careless reader, one who notices other names in the testimony and is lulled into thinking they amount to verification. A police officer said to be present has "since been identified as Manuel Sandoval"—even in the absence of any testimony from Sandoval (presumably dead or otherwise unavailable; clearly, he was never interviewed) pertaining to the event. Noland's friend Bill Ferguson "died long before we got involved in our research" (p. 5). Later (p. 201) Ramsey casually remarks that Ferguson "revealed his Aztec knowledge to very few people" while offering no reason to think, in the first instance but for Noland's testimony, that Ferguson possessed such "knowledge" and, in the second, that Ferguson told *anybody at all*.

Two other informants claim to have participated in aspects of the recovery operation. One is identified only as "George," for whom Ramsey vouches, which—all else considered—does not reassure. In any event, his story of a large operation run out of Roswell's Walker Air Force Base lacks any supporting evidence. Such supporting evidence, Ramsey notwithstanding, certainly does not come from Fred Reed.

Ramsey writes that in April 1948—take notice of the date—Reed's military "team was dispatched for a 'crash clean-up' as Fred would describe it to me years later [in 1999]." The clean-up, at the Hart Canyon site, was to be of anything left by the craft (which he later learned was a UFO) and by a subsequent military presence at the site. But this was not the story—as Ramsey does not inform his readers—that Reed provided in a strikingly different account just a few days before he faced questions, perhaps seriously leading ones, from the "investigator." Here are Reed's words as expressed in a March 27, 1999, letter to the Aztec newspaper:

Today, my wife and I . . . went out to the site of UFO crash in late 1948 [note: not exactly March 25] in Hart Canyon. . . . The aliens had built stone cairns marking the path from the oil field road to the crash site. These cairns are still in place today. The trees around the crash site open to the south, which is a typical distress signal for extraterrestrials.

The area looked basically as it had in 1948 when the OSS [Office of Strategic Services, which disbanded in 1945] sent our group there. . . . We had heard rumors that a UFO had crashed there. But it did not look like a crash site. And we had heard that army personnel had rushed in there and cleaned up the site. But it did not look like a clean-up site either. . . .

So what it boiled down to was this: No UFO crash. Instead, the UFO landed there for some specific intent to place (bury?) some instrument or thing there. Then they got into their saucer and flew away.



While failing to mention that his “witness” (whose eccentric beliefs about aliens and their ways also go missing) had flagrantly contradicted the testimony he solicited from him, Ramsey effects his own (unacknowledged) clean-up. Knowing, one infers, the OSS reference to be unsupportable, he revises Reed’s resume so that “he had worked for the OSS . . . back in the early 1940s, [and] was now working for the military.” In *The Aztec Incident* everything that fails to serve the narrative either undergoes revision or gets dropped into the memory hole.

Among other reported witnesses is a pastor, Solon Brown. Brown allegedly confided to a church officer and the officer’s young son (the latter is the source of the story) that Brown had seen dead aliens and a saucer at Hart Canyon on—Ramsey would have it, again without justification—March 25, 1948. Ramsey located the minister’s son, also a pastor, who said he had never heard his father talk about such an experience, though he had expressed interest in press accounts of the Roswell event in the same general period. An Air Force man who supposedly participated in the Aztec coverup in 1948 confided as much to a fellow Air Force member, an Aztec native, in England in the 1960s. The informant, Donald “Sam” Bass, cannot be found. Experienced investigator Kevin D. Randle learned that the claim related here that Bass was killed in an accident while serving in Vietnam cannot be verified in military records.

In Ramsey’s judgment of his own work, he has established that an Aztec recovery occurred and nobody can any longer argue otherwise—unless, I suppose, on the payroll of a sinister intelligence agency. Ramsey’s credulity is awesome and bottomless. In a passing aside (p. 203), he outs himself as a member of that small army of far-right cranks who discern a conspiracy to conceal President Obama’s birth certificate, apparently to protect his true identity as a Kenya-born socialist Islamic jihadist. In fairness, Ramsey is not *always* impossible to take seriously. Earlier in the book (p. 31) he acknowledges that in high school he “was never a superior student” and that he has always been “disappointed in how history is taught.” To those assertions, if to no others, *The Aztec Incident* offers compelling testimony.

### Notes

- <sup>1</sup> Stringfield died without ever revealing their identities. To the extent that subsequent investigations were possible, none seemed to lead anywhere, leaving only speculation about the informants’ motives.
- <sup>2</sup> “The Flying Saucers and the Mysterious Little Men” and “Flying Saucer Swindlers,” September 1952 and August 1956 issues of *True*, respectively.
- <sup>3</sup> A secret confessional diary/memoir allegedly composed by Silas Newton professes uncertainty about Scully’s true attitude. The late ufologist Karl

T. Pflock claimed to have examined it under peculiar circumstances, though no one else has seen it or been able to verify its existence. See Pflock's "What's Really Behind the Flying Saucers? A New Twist on Aztec," *The Anomalist* 8 (Spring 2000), 137–161.

- <sup>4</sup> See Moore's "Crashed Saucers: Evidence in Search of Proof," especially pp. 133–154, in Walter H. Andrus Jr. and Richard H. Hall, Editors (1985), *MUFON 1985 UFO Symposium Proceedings*, Seguin, TX: Mutual UFO Network.
- <sup>5</sup> See Moore (1985:147–148). Also Mike McClellan, "The UFO Crash of 1948 Is a Hoax," *Official UFO*, October 1975, pp. 36–37, 60–64, and William E. Jones and Rebecca D. Minshall, "Aztec, New Mexico—A Crash Story Reexamined," *International UFO Reporter*, September/October 1991, pp. 11–15, 23. Ramsey says that the son of the owners of the Hart Canyon property in 1948 refused to speak with him (p. 199), but in 1991 that man, Jack Dunning, told Jones and Minshall that, in their paraphrase, "his father [the now-deceased Harold] knows nothing about such a crash, though they are both aware of the rumors, having met [Aztec crash advocate William] Steinman when he came to Aztec" (p. 15).
- <sup>6</sup> See Cahn (1952:19) for the similar drawing Newton later provided for the *True* writer.

### Acknowledgments

I would like to thank Kevin Randle and Joel Carpenter for their generous assistance in the research on which this review draws.

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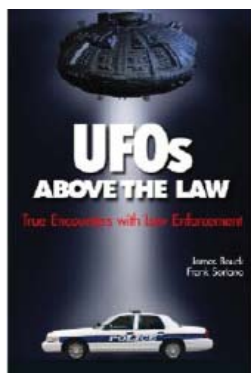
**UFOs: Above the Law, True Encounters with Law Enforcement**  
by James Bouck and Frank Soriano. Atglen, PA: Shiffer Publishing,  
2011. 192 pp. ISBN 9780764339202.

No man is above the law, so they say. But what about non-man, that is non-humans or aliens, be they extraterrestrial or interdimensional or whatever? Are they “above the law?” According to this book they are, literally, as when a police officer looks up and sees a UFO that appears to be an AFC (alien flying craft) above him.

Before you say “That is silly because there are no AFC so they couldn’t appear ‘above the law,’” you should read this book and see how many times law enforcement personnel have reported UFOs that seem to be AFCs. According to Edward Condon, director of the so-called “scientific study of UFOs” carried out at the University of Colorado at the request of the Air Force in the latter 1960s, only poor observers report UFOs. If this were true one would expect that UFO reports from police would be as scarce as hair on a billiard ball because law enforcement officers are trained to be good observers. However, as demonstrated in this book, police are as likely to report UFO sightings as are people in other jobs and professions.

Momentary break for “Truth in Reviewing.” Before continuing I should disclose that the book was written because of me. Well, that may sound a bit “proddish,” but the fact is that I suggested to Frank that he should write a book such as this. It was back in 1998 while I was investigating a videotaped sighting by Frank (which is described in the book) that Frank started telling me of sightings by other law enforcement officers and corrections officers (e.g., prison guards) with whom he was acquainted. From my study of the history of UFO sightings I knew that law enforcement officials had reported sightings, but, so far as I knew, these sightings had not been collected into a book. So I mentioned it to Frank. He thought it would be a good idea, so during the following decade he and Jim Bouck collected reports and wrote the book. It was a long time coming, but it is now here, a compendium of police and corrections officer sightings from around the world. I should point out that I had no part in writing the book and I have not given them a “blurb” to use in advertising. And they have had no part in this review.

Police officers understand the importance of maintaining credibility. They are trained to observe and report events. Whether the events are brief and “frantic” or of some duration, the officers involved should remain “calm, cool, and collected.” They realize that in criminal cases they may be called to testify about criminal activities they have witnessed and their testimony could result in fines, prison sentences, or even death of the perpetrator. What



they report may be contested by defense attorneys who would grab onto anything that could reduce the credibility of the officer's testimony. Therefore police avoid doing things that might reduce their credibility . . . and one of the things that reduces credibility is reporting a UFO sighting, especially a sighting that might be an AFC. So, although a search of the literature will turn up police reports, they are mostly those that made it into the press or news media, usually without the expressed permission of the officer(s) involved. But there are many sightings that have not been reported because

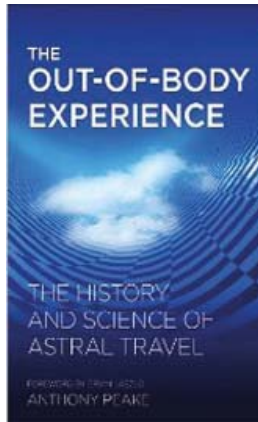
of the credibility issue. Both types of sightings, previously reported and previously unreported, are discussed in this book.

The first few chapters serve as an introduction to the subject and describe the approach used by the authors regarding their selection of reported sightings. They begin with a few famous non-police sightings and military sightings and then get to the "good stuff": a selection of 38 sighting reports spanning the years from 1964 (Socorro, New Mexico) to 2008 (Stephenville, Texas). In their discussion of the Laconia, New Hampshire, sighting of August 1974, you can almost ride along with the police cruiser as the sighting develops through the transcribed recording of the conversation between the dispatcher and the officers. Then come thirteen reports from other countries (Argentina, Brazil, France, Japan, United Kingdom) followed by fourteen police "chase" cases. Through a transcript of conversations, the reader can follow all the action that occurred during the April 24, 2001, police "chase" in Warren County, Ohio. The next chapter contains a selection of police officer cases investigated by the late reporter Bob Pratt followed by a chapter on corrections officer sightings. This is followed by chapters on subjects that will be familiar to those who have studied the subject of UFOs: government activities, astronaut reports, sightings by famous people, and alien abductions. Then come chapters that recount sightings by the authors themselves, followed by their conclusion. They believe that the fact that law enforcement and corrections officers have reported seeing craft-like flying objects, similar to what has been reported by other people, increases the probability that at least some UFOs are in fact AFCs. It is likely that, as a result of this publication Frank and Jim will receive more reports from officers of the law, leading to the publication of a second book or a revision with additions to the first book. If the latter happens, I hope the errors in editing can be corrected.

**BRUCE MACCABEE**

### Further Books of Note

**The Out-of-Body Experience: The History and Science of Astral Travel** by Anthony Peake. Watkins, 2011. 240 pp. \$14.95. ISBN 9781780280219.



In *The Out-of-Body Experience*, Anthony Peake provides a history of astral travel with a focus on Bob Monroe's documented experiences. He includes remote viewing, near-death experiences, lucid dreaming, and other psychic phenomenon as closely related to the classic OBE (out-of-body experience). Most fascinating is Peake's theory on how to explain astral travel scientifically. He considers several models of physics combined with more recent studies in consciousness and neuroscience to extrapolate an intriguing look at how our perceptions merge with so-called reality.

**SKIP ATWATER**

**The Righteous Mind: Why Good People Are Divided by Politics and Religion** by Jonathan Haidt. Pantheon, 2012. 419 pp. \$28.95 (hard-cover). ISBN 9780307377906.

This book has had much favorable publicity, including author interviews on radio and TV. Those snippets indicated that anomalists might derive useful understanding from the book, seeing as we're divided from mainstream science by something like ingrained ideologies. But I found the book immensely disappointing, for after 300+ pages, the conclusions amount to this:

- We are heavily influenced by gut feelings—hardly a revelation; I was nurtured intellectually on Freud's scenario of interactions between id, ego, and superego, and the general idea of conscious and unconscious or subconscious minds.



- People of different backgrounds or cultures tend to differ on many issues.
- We tend to regard those who differ with us as being not only wrong but even immoral.
- People who have established points of meaningful commonality are better able to discuss other matters agreeably.

I don't know how long ago it was that I reached those conclusions without wading through a textbook that charts in considerable detail the development of the field of moral psychology and the author's own participation in it, which is what this book is. The academic slant, which includes abstraction and a tendency to over-

generalize, is illustrated for example by:

the human mind is designed to 'do' morality. (p. xii)

an obsession with righteousness (leading inevitably to self-righteousness)

is the normal human condition. (p. xiii)

human beings—but no other animals—. . . produce large cooperative groups, tribes, and nations without the glue of kinship. (p. xiii)

I seem to have read about, and seen documentaries of, cooperative behavior among large groups of ants, bees, fish, wolves, chimpanzees. . . .

Haidt admits at the outset that his conclusions are nothing new:

I'll draw on the latest research in neuroscience, genetics, social psychology, and evolutionary modeling, but the take-home message . . . is ancient. (p. xvi)

So people interested in those intellectual fields will undoubtedly find much of use in the book, and it is written quite accessibly, albeit in didactic style ("I will show . . ." and the like).

I seem to be in a very small unappreciative minority. There are about 80 rave reviews on amazon.com and only a few that express something like my rather negative reaction. Potential readers should sample those reviews as well as this one before deciding whether or not they want to read the book themselves.

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## Article of Interest

**On the Reality of the Quantum State** by M. F. Pusey, J. Barrett, and T. Rudolph, *Nature Physics*, 8, 476–479, May 2012. doi:10.1038/nphys2309.

### Is the Quantum Wave Function Real?

*Reality? What a concept.*

—Robin Williams

#### Introduction

The conflict between all we know about the physics of quantum systems and what we say or believe is real about them is brought forward dramatically with the concept of the quantum wave function (QWF). Is the QWF ontic or merely epistemic? Here I review and clarify through original examples the recent work of M. F. Pusey, J. Barrett, and T. Rudolph who have formed a novel theorem to decide on the ontology or epistemology of a QWF based on a hidden variable (HV) theory dating back to the mid-20th century.

In a remarkable remark, physicist E. T. Jaynes once stated:

We believe that to achieve a rational picture of the world it is necessary to set up another clear division of labor within theoretical physics; it is the job of the laws of physics to describe physical causation at the level of ontology, and the job of probability theory to describe human inferences at the level of epistemology. The Copenhagen interpretation scrambles these very different functions into a nasty omelet in which the distinction between reality and our knowledge of reality is lost. (Jaynes 1989)

I shall use the adjectives *ontic* and *epistemic* to modify a number of nouns such as physics, beliefs, observables, and reality as most of us currently understand these things. Hence ontic reality is what we accept as real and “out there” objectively independent of anything we have to say, believe, or know about it. Epistemic reality, on the other hand, is what we accept as real and “in here” subjectively dependent on what we think, know, or believe is either ontic or epistemic reality.

Into this omelet we now add some new ingredients, or perhaps better said, we give the omelet another flip in the frying pan. Is the quantum wave function (QWF) epistemologically or ontologically real? In a recent *Nature* review, E. S. Reich (2012) discussed the latest work (the article under review here) of three physicists: M. F. Pusey, J. Barrett, and T. Rudolph



(PBR). PBR, basing their work on a number of previous epistemic vs. ontic considerations dating all the way back to the Einstein–Bohr debate at the 1927 Solvay conference in Brussels and continuing with the 20th- and 21st-century work of many others, notably Bell, Bohm, Caves, Fuchs, Harrigan and Spekkens, Kochen and Specker, Norsen, and others, once again throws down the gauntlet of uncertainty by attempting to provide an ontic view of the QWF, something that even Bohr most likely was not ever considering. Jaynes even pointed out that the famous Bohr–Einstein debate was actually never resolved in favor of Bohr at Solvay in 1927—although common thinking even among physicists is that it was—when you consider that the two physicists were not discussing the same physics. Bohr was only thinking about epistemic physics while Einstein was considering only ontic physics. Hence while Bohr believed quantum physics was certainly epistemically complete (like classical thermodynamics), Einstein was equally correct in believing that quantum physics wasn’t ontologically complete (like Newtonian mechanics).

The conflict between all we know about the physics of quantum systems and what we say or believe is real about them is brought forward dramatically with the concept of the QWF. Is the QWF ontic or merely epistemic? To decide on the ontology or epistemology of a QWF, an old argument known as the hidden variable (HV) theory dating back to the mid-20th century is revisited. This theory was probably most emphasized by David Bohm (who formulated from standard quantum physics an ontic QWF that influenced a real particle). Later it was revisited by Bell, in his famous no-go theorem involving a QWF describing two quantum-entangled separated particles à la Bohm’s version of the Einstein, Podolsky, and Rosen (BEPR) paradox. BEPR showed that such a QWF could not be local (measurements made on one particle at one spacetime location could influence and change the QWF and therefore the outcome of measurement on the other particle at a distant (spacelike) spacetime location simultaneously). Bell’s theorem shows that any hidden variable theory must involve nonlocal influences at the ontic level, regardless of what you think of the QWF. Hence one might conclude from Bell’s famous HV theorem (à la Einstein) that QWFs are epistemological rather than ontological since two observers could have different beliefs about the quantum state of their respective spacelike separated particles.<sup>1</sup>

Quantum physical HV theories all have one thing in common: They all have ontic definite-valued hidden states underlying the QWF. A specification of these HVs should reveal the results of a measurement of any property or observable.<sup>2</sup> So the question is what would one need to do to HV theory to make the QWF ontological? This is precisely what PBR do by making

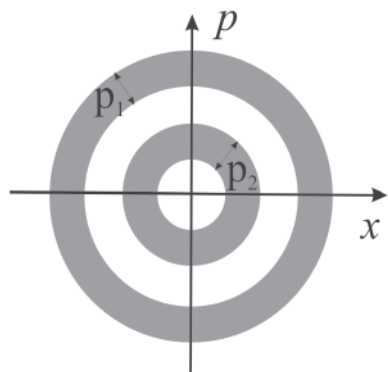
a particular assumption: If a specification of an HV uniquely determines a QWF, then the QWF is ontic. If, on the other hand, specification of an HV does not uniquely determine a QWF, the QWF is said to be epistemic.

As an epistemic example, in the first version of their paper (Pusey, Barrett, & Rudolph 2012), PBR consider a classical case of flipping a biased coin in one of two distinct ways. In the first way the coin has a probability  $p_1$  of coming up heads while in the second way the probability for heads is  $p_2 \neq p_1$ . If the coin is flipped and then observed any number of times, regardless of the results obtained, we cannot know for certain by which method the coin was flipped, although the observed frequency of heads resulting could provide a clue, provided we knew that the same preparation was used each flip. Not knowing this, the result, heads, could have been obtained with either mode of flipping. Hence we cannot assign uniquely either probability  $p_2$  or  $p_1$  and these probabilities remain epistemic although the unobserved method of flipping need not be so.

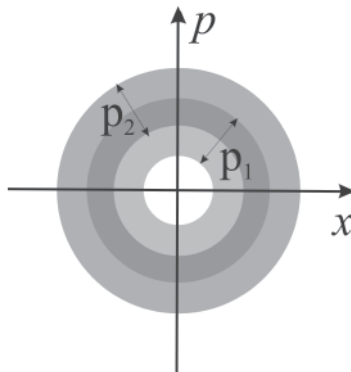
In another epistemic example, used by Reich in her review (Reich 2012), consider a die prepared in a manner that shows the value 2 with a predicted probability of  $\frac{1}{3}$ . We cannot know if the die was prepared in such a way that only prime numbers (2, 3, or 5) were allowed to show, or if only even numbers (2, 4, or 6) were allowed to show. Each distribution has the number 2 in common, so the distributions are conjoint and epistemic.

### Classical Physics Epistemics

Let me now give you a simple example of the difference between ontic and epistemic reality taken from classical physics. Consider a ball with mass  $m = \frac{1}{2}$  attached to a spring with spring constant  $k = 2$ . Such a system is known as a simple harmonic oscillator (SHO)—stretch or compress the spring and the SHO “springs” into motion with the ball having momentum  $p$  and a position  $x$  relative to its unstretched or uncompressed 0 position, and constant energy  $E = p^2 + x^2$ . I’ll use a single variable  $\lambda$  to denote the ontic pair  $(p, x)$ . Suppose that someone unknown to us stretches the spring an unknown initial distance,  $x_0$ , within a range  $1 \leq x_0 \leq 2$  or in a second range  $3 \leq x_0 \leq 4$ . If you think of a two-dimensional space with orthogonal coordinate axes,  $p$  and  $x$ , the above energy equation describes a circle contained within one of the two sets of concentric thickened circles centered about the coordinate origin. Such a space is a simple example of what is called a phase space which in general has  $n$  dimensions of  $ps$  and  $xs$ . Every point on a circle provides a momentum and position of the ball which, even if not observed, hence hidden, are ontic variables. At no time do the different sets of circles have common points of overlap.



**Figure 1. Disjoint epistemic probability distributions in phase space for a SHO (see text.)**



**Figure 2. Conjoint (overlapping dark grey) epistemic probability distributions in phase space for a SHO (see text.)**

We can think of the thickened circles as disjoint probability distributions,  $p_1(\lambda)$  and  $p_2(\lambda)$ , of positions and momenta—disjoint because we never have any  $\lambda$ s in common—the thick circles are concentrically nested (see Figure 1). Each  $\lambda$  may be a uniformly distributed (over time) HV satisfying the SHO energy equation (and the Liouville equation in phase space if we didn't know the energies which govern the temporal evolution of more complex distributions involving more SHOs). However, as I said, these simple distributions would be disjointed. Hence  $p_1(\lambda) \cdot p_2(\lambda) = 0$  always since each  $\lambda$  uniquely determines its own distribution. Consequently if there was a state  $\alpha_1$  associated with  $p_1(\lambda)$  and a state  $\alpha_2$  associated with  $p_2(\lambda)$ , then specification of the value of  $\lambda$  would uniquely determine which state,  $\alpha_1$  or  $\alpha_2$ , we would be in. We could, although it is clearly not necessary, view the  $\lambda$ s as HVs and declare the states as ontic since each  $\lambda$  uniquely determines  $\alpha$ .

Suppose we now reconsider the initial preparation of the SHO. At  $t = 0$ , that unknown someone simply decides to stretch the spring a certain distance,  $x_0$ , an amount in the range,  $1 \leq x_0 \leq 3$ , and lets it go.<sup>3</sup> We would then find a thick ring band of different energy possibilities in the phase plane. Or if the unknown person prepares the SHO in the range  $2 \leq x_0 \leq 4$  and lets it go, we would then find a second thick ring band of possibilities. The two circular bands now form overlapping concentrically nested distributions (see Figure 2). Now we have the two distributions,  $p_1(\lambda)$  and  $p_2(\lambda)$ , overlapping. Then  $p_1(\lambda) \cdot p_2(\lambda) \neq 0$  in the overlapping area  $2 \leq x_0 \leq 3$  and each  $\lambda$  no longer uniquely determines its own state. A specification of  $\lambda$  in

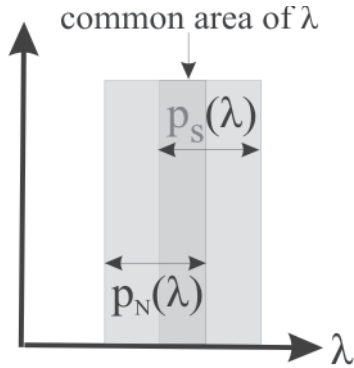
the overlapping probability distribution could indicate we were in either the  $\alpha_1$  or  $\alpha_2$  state and that would make the states epistemic.

PBR's proof is based on a contradiction that arises between the probability predictions of quantum physics when QWFs are considered to be ontological (their respective HV probability distributions are disjoint) and the same predictions based on epistemic QWFs (their respective HV probability distributions are conjoint). They consider this contradiction in a series of ever increasingly complex arguments that includes a calculation eventually involving  $n$  identically prepared and uncorrelated independent states as well as noise considerations. Accordingly, whenever QWFs of observables are governed by disjoint distributions of ontic HVs, these QWFs are uniquely determined and must be ontic even though their respective distributions are epistemic (similar to arguments made in statistical mechanics). Thus if the states of a quantum system are specified by QWFs which are determined by disjoint epistemic distributions over ontic variables, the QWFs are as ontic or real as any observable in physics. On the other hand, if such distributions governing these QWFs are conjoint, that is they have values of ontic HVs in common, the QWFs are epistemic or merely represent knowledge (probabilities) of observables in question.

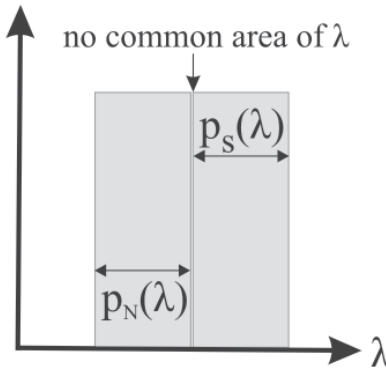
### Simple Quantum Physics Ontology and Epistemology

Before we look at PBR's argument, I want to explain a little more about why overlapping probability distributions lead to a contradiction in the quantum physical predictions. Consider for simplicity a top-hat probability distribution,  $p_\psi(\lambda)$ . We shall be looking at two special cases  $\psi = N$  and  $\psi = S$  (you can think of these states as polar opposites) associated with orthogonal QWFs,  $N$  and  $S$ , respectively (that is  $\langle N|S \rangle = 0$ ), which have a common overlapping area of an HV,  $\lambda$  ( $\lambda$  could also indicate a set of HVs). A common  $\lambda$  means simply that both  $p_S(\lambda) \neq 0$  and  $p_N(\lambda) \neq 0$  as shown in Figure 3.

**First Case:** Now consider the probability of obtaining a measurement of  $N$  and suppose that this probability depends only on the HV  $\lambda$ . We can write it as a conditional (Bayesian) probability,  $M(N|\lambda)$ . To obtain the total probability,  $P(N|\psi)$ , that is to get the probability for result  $N$  for any QWF,  $\psi$ , we must calculate  $P(N|\psi) = \int M(N|\lambda)p_\psi(\lambda)d\lambda$ . That is, we multiply the probability of obtaining a result for a given  $\lambda$  by the distribution function,  $p_\psi(\lambda)$ , specific to the chosen QWF,  $\psi$ , and integrate over all  $\lambda$ . From the Born Rule of quantum physics,  $P(N|\psi) = \langle \psi|N \rangle \langle N|\psi \rangle$ .



**Figure 3. Conjoint top hat (overlapping) epistemic probability distributions for orthogonal quantum physics states.**



**Figure 4. Disjoint epistemic probability distributions for orthogonal quantum physics states leading to ontic states  $|N\rangle$  and  $|S\rangle$ .**

**Second Case:** Next consider a measurement of  $S$  which is also given by a probability,  $M(S|\lambda)$ , which is also clearly dependent only on HV  $\lambda$ . Now suppose we wish to obtain the probability of getting the result,  $S$ . To obtain the total probability  $P(S|\psi)$  for getting the result  $S$ , we must have  $P(S|\psi) = \int M(S|\lambda) p_\psi(\lambda) d\lambda$ . And again from the Born Rule:  $P(S|\psi) = \langle \psi | S | \psi \rangle$ .

Now if  $M(S|\lambda)$  and  $M(N|\lambda)$  are the only probabilities for values obtained by measurements, and since there are only two such values possible, then clearly  $M(S|\lambda) + M(N|\lambda) = 1$ . There can be no other result possible and this must hold for every  $\lambda$  value. In plain language, specifying  $\lambda$  must lead to unity probability when all possible results of a measurement are taken into account with ontic variable  $\lambda$  specified. For example,  $\lambda$  could be a simple option,  $\lambda_q$  or  $\lambda_d$ , for an unseen biased coin—use a quarter or use a dime. Using a quarter, suppose  $M(H|\lambda_q) = .25$  and  $M(T|\lambda_q) = .75$ , or using a dime suppose  $M(H|\lambda_d) = .65$  and  $M(T|\lambda_d) = .35$ . In each HV option, dependent on the value of  $\lambda$ , head (H) and tail (T) are clearly orthogonal results after a toss of the coin. Again, as in the other coin example, after many such observations we could only guess the HV of the coin was a dime or a quarter because of the relative frequencies of heads to tails appearing provided we knew that just one type of coin was used each time. Otherwise we would never know which coin was used.

However, as simple as is this N or S case, it leads to a contradiction

with the Born Rule of quantum physics that arises when you put  $\psi = S$  in the First Case, and  $\psi = N$  in the Second Case. Since  $S$  and  $N$  are orthogonal (they cannot both occur),  $\langle S|N \rangle = 0$ . Hence in the First Case we get  $\langle \psi|N \rangle \langle N|\psi \rangle = \langle S|N \rangle \langle N|S \rangle = P(N|S) = \int M(N|\lambda) p_S(\lambda) d\lambda = 0$ , and in the Second Case,  $\langle \psi|S \rangle \langle S|\psi \rangle = \langle N|S \rangle \langle S|N \rangle = P(S|N) = \int M(S|\lambda) p_N(\lambda) d\lambda = 0$ . If these integrals are to be zero, then the integrands have to be zero for every value of  $\lambda$  because both  $M(N|\lambda)$  and  $M(S|\lambda)$  as well as  $p_S(\lambda)$  and  $p_N(\lambda)$  are positive functions. Therefore, in particular, these integrands have to be zero in the overlapping region. But given that both  $p_S(\lambda) \neq 0$  and  $p_N(\lambda) \neq 0$  in the overlapping region, that is we have overlapping distributions in  $\lambda$  space (see Figure 3), these results can only occur if both  $M(N|\lambda) = 0$  and  $M(S|\lambda) = 0$ , which contradicts  $M(S|\lambda) + M(N|\lambda) = 1$ .

Hence for this simple orthonormal case, we cannot have both  $p_S(\lambda)$  and  $p_N(\lambda)$  possessing nonzero values for any common  $\lambda$ . In brief, they cannot have overlapping hidden variables. This means that a specification of  $\lambda$  leads to a unique  $\psi$ , either  $S$  or  $N$  (as in the quarter/dime example above), and we can therefore take it that  $p_S(\lambda)p_N(\lambda) = 0$ , so in both cases either  $p_S(\lambda)$  or  $p_N(\lambda)$  must be zero. PBR might call this a necessary step to proving that a QWF is an ontological function, but this proof only includes orthogonal QWFs,  $|N\rangle$  and  $|S\rangle$  as indicated in Figure 4. To be both necessary and sufficient one would need to show that the probability distribution  $p_N(\lambda)$  for  $|N\rangle$  and any other probability distribution  $p_\psi(\lambda)$  for a QWF  $|\psi\rangle$  cannot have any overlap even if  $\langle N|\psi \rangle \neq 0$ .

### More Complex Quantum Physics Ontology and Epistemology

In the above case we only considered orthogonal QWFs,  $N$  and  $S$ , and found them to be ontic. Can we make the argument that  $\psi$  is real in any case including nonorthogonal situations? To fully answer the query in the title of this review, we would need to look at the case when possible quantum states,  $\alpha$  and  $\beta$ , are not orthogonal. One might think that since two such QWFs,  $|\alpha\rangle$  and  $|\beta\rangle$ , do overlap, i.e.  $\langle \beta|\alpha \rangle \neq 0$ , one might find no contradiction in having both  $p_\alpha(\lambda) \neq 0$  and  $p_\beta(\lambda) \neq 0$ . Hence both  $\alpha$  and  $\beta$  could be epistemic and still satisfy the Born Rule of quantum physics.

PBR dispel that possibility by first considering nonorthogonal states of the same simple system as above that is prepared with compass directions  $|N\rangle$  or  $|E\rangle$ , where  $|E\rangle = (|N\rangle + |S\rangle) / \sqrt{2}$ ,  $|W\rangle = (|N\rangle - |S\rangle) / \sqrt{2}$ . Here we have  $\langle N|S \rangle = \langle E|W \rangle = 0$ , respectively orthogonal, but  $\langle N|E \rangle = 1/\sqrt{2}$ , hence

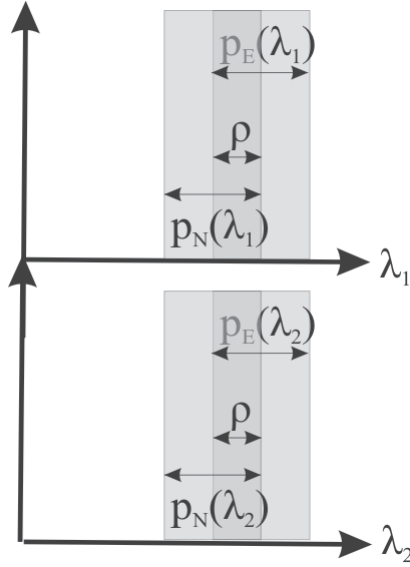
N and E are not orthogonal.<sup>4</sup> We shall again assume that the QWF,  $|\psi\rangle$  (either  $|N\rangle$  or  $|E\rangle$ ), is dependent on an HV distribution  $p_\psi(\lambda)$ , similar to what we did in the orthogonal case above. One can recognize these “directional” states as *spinors*, i.e. spin  $\frac{1}{2}$  states, wherein  $|N\rangle$  means spin up in the z direction,  $|S\rangle$  means spin down in the z direction,  $|E\rangle$  means spin up in the x direction, and  $|W\rangle$  means spin down in the x direction.

The system is to be prepared in one of two ways such that one preparation produces  $|N\rangle$  with unity probability  $P(N|N) = \int M(N|\lambda)p_N(\lambda)d\lambda = 1$ , arising from an epistemic  $p_N(\lambda)$  distribution, while a second kind of preparation produces  $|E\rangle$  with unity probability,  $P(E|E) = \int M(E|\lambda)p_E(\lambda)d\lambda = 1$ , arising from epistemic distribution  $p_E(\lambda)$ . The aim: If a specification of  $\lambda$  yields a specific QWF,  $|\psi\rangle$ , orthogonal or not to any other QWF,  $|\phi\rangle$ , then  $|\psi\rangle$  must be ontic and therefore an objective real “thing” “out there” independent of any observer. So, accordingly, in the case involving states  $|N\rangle$  and  $|E\rangle$ , in spite of the nonorthogonality of these states, the two distributions  $p_N(\lambda)$  and  $p_E(\lambda)$  must be disjoint,  $p_N(\lambda)p_E(\lambda) = 0$ , as shown in Figure 4, only substitute E for S.<sup>5</sup>

On the other hand, if  $\lambda$  lies within a region where  $|N\rangle$  and  $|E\rangle$  have conjoint distributions, i.e.  $p_N(\lambda)$  and  $p_E(\lambda)$  overlap so that  $p_N(\lambda)p_E(\lambda) \neq 0$ , then  $|\psi\rangle$  cannot be ontic and must be epistemic as shown in Figure 3 (again substitute E for S).<sup>6</sup> In brief, an epistemic  $|\psi\rangle$  results in a contradiction with the prediction of quantum physics just as we saw in the above N and S orthogonal case.

To clarify their argument, I will follow PBR with a slight change of notation. PBR have us consider a quantum physical situation in which two such identical, but separate, preparations  $|\psi_1\rangle$  and  $|\psi_2\rangle$  are independently made using HVs,  $\lambda_1$  and  $\lambda_2$ , wherein both HVs lie within identical HV spaces; we have essentially two copies of the same hidden variable space. Consequently these preparations result in the uncorrelated joint quantum state  $|\psi_1\rangle|\psi_2\rangle$ , since they are produced from independent HVs. It is important to realize that PBR assume that both  $\lambda_1$  and  $\lambda_2$  lie within corresponding, respectively, identical HV spaces. Thus each separate space of HVs contains an identical range,  $\rho \geq 0$ , over which probability distributions are conjoint. Consequently each preparation produces its own corresponding HV  $\lambda_i$ , resulting in identical overlapping probability distributions of  $|N\rangle$  or  $|E\rangle$ , wherein,  $p_N(\lambda_1)p_E(\lambda_1) \neq 0$  and  $p_N(\lambda_2)p_E(\lambda_2) \neq 0$ , provided  $\lambda_1$  lies within the overlapping range,  $\rho$ , and  $\lambda_2$  lies within the same





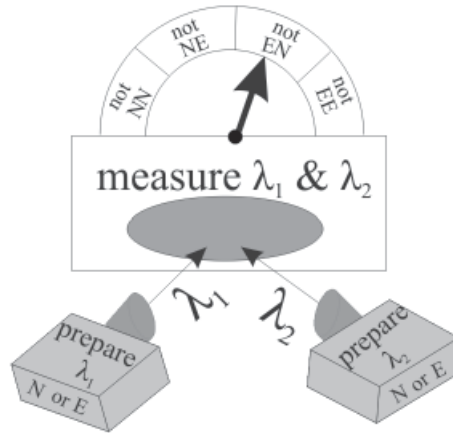
**Figure 5. Conjoint top hat (overlapping) epistemic probability distributions for two identical systems with non-orthogonal quantum physics states.**

correspondingly identical overlapping range,  $\rho$ , as shown in Figure 5.

That is, both systems are prepared in such a manner that we cannot uniquely determine  $|N\rangle$  or  $|E\rangle$ . PBR also assume the probability distribution functions,  $p_N(\lambda_i)$  and  $p_E(\lambda_i)$ , are the same for  $i = 1$  or  $2$ . Since these are independent preparations, both  $p_{\psi_1}(\lambda_1) \neq 0$  and  $p_{\psi_2}(\lambda_2) \neq 0$  whenever  $\lambda_1$  and  $\lambda_2$  are each found in the same range,  $\rho$ . In Figure 5 we are essentially duplicating the scenario shown in Figure 3 for each copy.

So after preparing the joint system with both  $\lambda_1$  and  $\lambda_2$  in their corresponding conjoint  $\rho$  ranges, we obtain the following epistemic (possible) results for  $|\psi_1\rangle|\psi_2\rangle$ :  $|N\rangle|N\rangle$  or  $|N\rangle|E\rangle$  or  $|E\rangle|N\rangle$  or  $|E\rangle|E\rangle$ . All we need now is to specify the basis for making a measurement of the joint system. Suppose now that the two systems are brought together and measured using (projected onto) the following orthonormal entangled base states:

$$\begin{aligned} |1\rangle &= (|N\rangle|S\rangle + |S\rangle|N\rangle) / \sqrt{2}, \\ |2\rangle &= (|N\rangle|W\rangle + |S\rangle|E\rangle) / \sqrt{2}, \\ |3\rangle &= (|E\rangle|S\rangle + |W\rangle|N\rangle) / \sqrt{2}, \text{ and} \\ |4\rangle &= (|E\rangle|W\rangle + |W\rangle|E\rangle) / \sqrt{2}. \end{aligned}$$



**Figure 6. Experimental preparations and measurements of ontic hidden variables possibilities.**

These four states are maximally entangled and orthogonal ( $\langle ij | j \rangle = 0$ , unless  $i = j$ , and then  $\langle ij | i \rangle = 1$ ). Consequently the probability for obtaining a result,  $i$ ,  $P(i|\psi_1\psi_2)$ , given that the joint wave function,  $|\psi_1\psi_2\rangle = |\psi_1\rangle|\psi_2\rangle$ , can be expressed in a similar manner as for the simple case above. Following the above example and the Born Rule, we have for the joint probability,  $P(i|\psi_1\psi_2) = \langle \psi_1\psi_2 | i \rangle \langle i | \psi_1\psi_2 \rangle = \iint M(i|\lambda_1, \lambda_2) p_{\psi_1}(\lambda_1) p_{\psi_2}(\lambda_2) d\lambda_1 d\lambda_2$ , where the probability of obtaining a joint measurement,  $M$ , of state  $|i\rangle$  now depends on two HVs,  $\lambda_1$  and  $\lambda_2$ , and we write it accordingly as a conditional (Bayesian) probability,  $M(i|\lambda_1, \lambda_2)$ . Consequently, we cover all of our four bases and find for any chosen pair of HVs,  $\lambda_1$  and  $\lambda_2$ ,  $M(1|\lambda_1, \lambda_2) + M(2|\lambda_1, \lambda_2) + M(3|\lambda_1, \lambda_2) + M(4|\lambda_1, \lambda_2) = 1$ . This says that the probabilities of obtaining a result for  $i$ ,  $1 \leq i \leq 4$ , now depends on both given  $\lambda_1$  and  $\lambda_2$  values. Change those values and the individual  $M(i|\lambda_1, \lambda_2)$  may change, as in the case of the quarter and dime; but they will always sum to unity regardless of whether or not the chosen values of  $\lambda_1$  and  $\lambda_2$  fall within the ranges of  $p \geq 0$ .

The question is: What are the probabilities of the results of measurement using (projecting onto) these entangled base states according to the Born Rule of quantum physics? It isn't too difficult to see that there are four cases in which we get predictions of zero probabilities—the result of a measurement will be to not find a specific result as shown in Figure 6 (based on PBR's Figure 2).

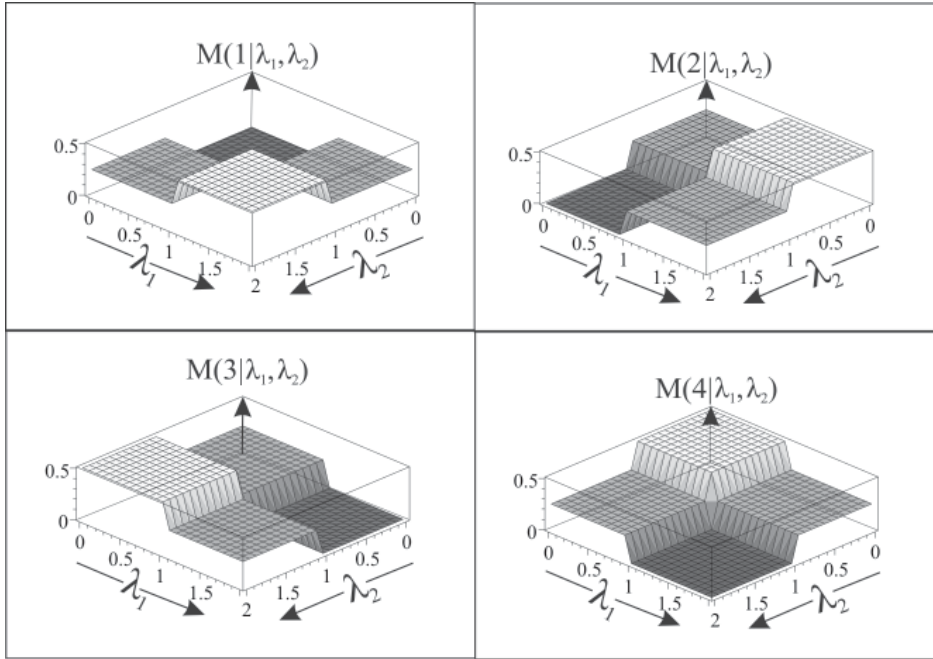
As we see next, this fact leads to a contradiction if  $\lambda_1$  and  $\lambda_2$  fall within the ranges of  $\rho$ , thus producing non-vanishing probability distributions. It is here where the independence and conjointness of the two individually overlapping probability distributions,  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) \neq 0$ , play their roles.

In the first case,  $P(1|NN) = \langle NN|1 \rangle \langle 1|NN \rangle = 0$ , as can be seen by inspection. Therefore,  $\iint M(1|\lambda_1, \lambda_2)p_N(\lambda_1)p_N(\lambda_2)d\lambda_1d\lambda_2$  must be 0. But since  $\lambda_1$  and  $\lambda_2$  have non-vanishing probability distributions,  $p_N(\lambda_1)p_N(\lambda_2) \neq 0$ , it follows that  $M(1|\lambda_1, \lambda_2) = 0$ . A similar line of reasoning applies to  $P(2|NE) = \langle NE|2 \rangle \langle 2|NE \rangle = 0$ , where  $p_N(\lambda_1)p_E(\lambda_2) \neq 0$ , and for  $P(3|EN) = \langle EN|3 \rangle \langle 3|EN \rangle = 0$ , where  $p_E(\lambda_1)p_N(\lambda_2) \neq 0$ , and finally for  $P(4|EE) = \langle EE|4 \rangle \langle 4|EE \rangle = 0$ , where  $p_E(\lambda_1)p_E(\lambda_2) \neq 0$ . Remember we are assuming that  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) \neq 0$ , corresponding to  $\lambda_1$  and  $\lambda_2$  falling within the ranges of  $\rho$  and these are the only cases of concern.

Therefore we would conclude for these particular values of  $\lambda_1$  and  $\lambda_2$ , within the ranges of  $\rho$  where  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) \neq 0$ , in each of the vanishing probabilities,  $P(i|\psi_1\psi_2) = 0$ , we must have  $M(1|\lambda_1, \lambda_2) = 0$ ,  $M(2|\lambda_1, \lambda_2) = 0$ ,  $M(3|\lambda_1, \lambda_2) = 0$ , and  $M(4|\lambda_1, \lambda_2) = 0$ , which contradicts the equation:  $M(1|\lambda_1, \lambda_2) + M(2|\lambda_1, \lambda_2) + M(3|\lambda_1, \lambda_2) + M(4|\lambda_1, \lambda_2) = 1$ , which is valid for all values of  $\lambda_1$  and  $\lambda_2$ . The only way out of the contradiction is, of course, to deny that the non-vanishing probability distributions, where  $\lambda_1$  and  $\lambda_2$  are within the supported “overlapping” ranges of values of  $\rho$ ,  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) \neq 0$ , can ever occur. Thus  $P(1|NN) = 0$  implies that  $p_N(\lambda_1)p_N(\lambda_2) = 0$ ,  $P(2|NE) = 0$  implies that  $p_N(\lambda_1)p_E(\lambda_2) = 0$ ,  $P(3|EN) = 0$  implies that  $p_E(\lambda_1)p_N(\lambda_2) = 0$ , and  $P(4|EE) = 0$  implies that  $p_E(\lambda_1)p_E(\lambda_2) = 0$ . In each case it's necessary and sufficient that only one of the pairs of  $p_{\psi_i}(\lambda_i)$ s need vanish to rule out any overlap and thus rule in that all such  $\psi_i$ s are ontological. Having either  $p_{\psi_i}(\lambda_i)$  vanish means  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) = 0$ , and consequently since both  $\psi_1$  and  $\psi_2$  are either N or E then the condition  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) \neq 0$  is equally ruled out for each  $\psi_i$ . Thus for any pair of nonorthogonal  $\psi_i$ s, the Born Rule of quantum physics cannot be satisfied, if their respective HV probabilities overlap.

### Simple Illustration of the BPR Theorem for Two Non-Orthogonal States

Of course, it could be that for most values of  $\lambda_1$  and  $\lambda_2$ , outside the range of  $\rho$ , or indeed if  $\rho = 0$ , the condition  $p_{\psi_1}(\lambda_1)p_{\psi_2}(\lambda_2) = 0$  need not arise to have  $P(i|\psi_1\psi_2) = 0$ , and for these cases no contradiction arises. To further clarify the argument consider Figure 7, where I show a possible set of conditional



**Figure 7. Three dimensional views of quilted, stepped, conditional measurement probabilities,  $M(i|\lambda_1, \lambda_2)$ , consistent with disjoint top hat probability distributions for two identical systems with non-orthogonal quantum physics states.**

measurement probability distributions,  $M(i|\lambda_1, \lambda_2)$ , consistent with nonoverlapping top-hat probability distributions shown in Figure 5 with  $\rho = 0$ . Each conditional measurement probability distribution consists of a quilt of four patches with  $M(i|\lambda_1, \lambda_2)$  being constant in each patch and  $i \in (1, 4)$ . The darkest patch has  $M(i|\lambda_1, \lambda_2) = 0$ , the light grey patches have  $M(i|\lambda_1, \lambda_2) = .25$ , and the nearly white patch has  $M(i|\lambda_1, \lambda_2) = .50$ . One can see by inspection that  $M(1|\lambda_1, \lambda_2) + M(2|\lambda_1, \lambda_2) + M(3|\lambda_1, \lambda_2) + M(4|\lambda_1, \lambda_2) = 1$  for any pair of values,  $(\lambda_1, \lambda_2)$ , in the quilt. So long as  $\rho = 0$ , we never see any contradiction arising with the Born Rule because the disjoint probability distributions,  $p_{\psi_1}(\lambda_1)$  and  $p_{\psi_2}(\lambda_2)$ , are consistently defined within the same boundaries as the quilted measurement probabilities,  $M(i|\lambda_1, \lambda_2)$ . It is only when  $p_{\psi_1}(\lambda_1)$  and  $p_{\psi_2}(\lambda_2)$  exceed those quilted boundaries that contradictions arise as indicated next.

If we have  $\rho > 0$ , then these measurement probabilities,  $M(i|\lambda_1, \lambda_2)$ , lead to contradiction with the Born Rule. To see this in each of the four cases, let us again consider our conjoint top-hat probability distributions, as shown in Figure 5 such that,  $p_N(\lambda_1) = p_N(\lambda_2) = 1/(1+\rho/2)$  in the  $\rho$ -extended range, when

$0 \leq \lambda_1 \leq (1+\rho/2)$  and  $0 \leq \lambda_2 \leq (1+\rho/2)$ , respectively, and 0 elsewhere. And similarly for  $p_E(\lambda_1) = p_E(\lambda_2) = 1/(1+\rho/2)$  in the  $\rho$ -extended ranges,  $(1-\rho/2) \leq \lambda_1 \leq 2$  and  $(1-\rho/2) \leq \lambda_2 \leq 2$ , respectively, and 0 elsewhere. Consequently we have the normalized probabilities,  $\int p_N(\lambda_i) d\lambda_i = \int p_E(\lambda_i) d\lambda_i = 1$ , for  $i = 1, 2$ .

**Case 1.** Let us now examine the first case where  $P(1|NN) = \langle NN|1 \rangle \langle 1|NN \rangle = \iint M(1|\lambda_1, \lambda_2) p_N(\lambda_1) p_N(\lambda_2) d\lambda_1 d\lambda_2 = 0$ , according to the Born Rule. There is no problem for  $0 \leq \lambda_1 \leq 1$  and  $0 \leq \lambda_2 \leq 1$ ; we simply have on this patch of the  $\lambda$ -quilt,  $M(1|\lambda_1, \lambda_2) = 0$ . However, in the overlapping ranges,  $1 < \lambda_1 \leq (1+\rho/2)$  and  $1 < \lambda_2 \leq (1+\rho/2)$ ,  $M(1|\lambda_1, \lambda_2) = .5$ , and consequently  $P(1|NN) = \rho^2/[8(1+\rho/2)^2] \neq 0$ , in contradiction of the Born Rule.

**Case 2.** A similar line of reasoning applies for  $P(2|NE) = \langle NE|2 \rangle \langle 2|NE \rangle = \iint M(2|\lambda_1, \lambda_2) p_N(\lambda_1) p_E(\lambda_2) d\lambda_1 d\lambda_2 = 0$ , according to the Born Rule. Here we again have no problem for  $0 \leq \lambda_1 \leq 1$  and  $1 \leq \lambda_2 \leq 2$ . On this patch of the  $\lambda$ -quilt,  $M(2|\lambda_1, \lambda_2) = 0$ . However, for  $1 \leq \lambda_1 \leq (1+\rho/2)$  and  $(1-\rho/2) \leq \lambda_2 \leq 1$ , we have  $M(2|\lambda_1, \lambda_2) = .5$  and consequently  $P(2|NE) = \rho^2/[8(1+\rho/2)^2] \neq 0$ , as in the first case, in contradiction of the Born Rule.

**Case 3.** A similar line of reasoning applies for  $P(3|EN) = \langle EN|3 \rangle \langle 3|EN \rangle = \iint M(3|\lambda_1, \lambda_2) p_E(\lambda_1) p_N(\lambda_2) d\lambda_1 d\lambda_2 = 0$ , according to the Born Rule. Here we again have no problem for  $1 \leq \lambda_1 \leq 2$  and  $0 \leq \lambda_2 \leq 1$ . On this patch of the  $\lambda$ -quilt,  $M(3|\lambda_1, \lambda_2) = 0$ . However for  $(1-\rho/2) \leq \lambda_1 \leq 1$  and  $1 \leq \lambda_2 \leq (1+\rho/2)$ , we have  $M(3|\lambda_1, \lambda_2) = .5$  and consequently  $P(3|EN) = \rho^2/[8(1+\rho/2)^2] \neq 0$ , as in the first case, in contradiction of the Born Rule.

**Case 4.** A similar line of reasoning applies for  $P(4|EE) = \langle EE|4 \rangle \langle 4|EE \rangle = \iint M(4|\lambda_1, \lambda_2) p_E(\lambda_1) p_E(\lambda_2) d\lambda_1 d\lambda_2 = 0$ , according to the Born Rule. Here we again have no problem for  $1 \leq \lambda_1 \leq 2$  and  $1 \leq \lambda_2 \leq 2$ . On this patch of the  $\lambda$ -quilt,  $M(4|\lambda_1, \lambda_2) = 0$ . However for  $(1-\rho/2) \leq \lambda_1 \leq 1$  and  $(1-\rho/2) \leq \lambda_2 \leq 1$ , we have  $M(4|\lambda_1, \lambda_2) = .5$  and consequently  $P(4|EE) = \rho^2/[8(1+\rho/2)^2] \neq 0$ , as in the first case, in contradiction of the Born Rule.

Of course, in each case, in the limit where  $\rho \rightarrow 0$ , no contradiction arises and the correct results for the measurement probabilities are obtained. Thus, for example, from the top right-hand corner of Figure 7 dealing with measurements projected onto the  $|2\rangle$  state we find:

$$\begin{aligned} P(2|NE) &= \langle NE|2 \rangle \langle 2|NE \rangle = \iint M(2|\lambda_1, \lambda_2) p_N(\lambda_1) p_E(\lambda_2) d\lambda_1 d\lambda_2 = 0, \\ P(2|NN) &= \langle NN|2 \rangle \langle 2|NN \rangle = \iint M(2|\lambda_1, \lambda_2) p_N(\lambda_1) p_N(\lambda_2) d\lambda_1 d\lambda_2 = .25, \\ P(2|EN) &= \langle EN|2 \rangle \langle 2|EN \rangle = \iint M(2|\lambda_1, \lambda_2) p_E(\lambda_1) p_N(\lambda_2) d\lambda_1 d\lambda_2 = .50, \text{ and} \\ P(2|EE) &= \langle EE|2 \rangle \langle 2|EE \rangle = \iint M(2|\lambda_1, \lambda_2) p_E(\lambda_1) p_E(\lambda_2) d\lambda_1 d\lambda_2 = .25, \end{aligned}$$

all consistent with the Born Rule leading to unity probability when summed. Similar results follow for the other measurements projected onto the  $|i\rangle$  state, with  $i = 1, 3$ , and  $4$ .

### Discussion

To prove or disprove whether or not any general QWF  $|\alpha\rangle$  is ontic is quite an accomplishment even for a limited HV, but a clever approach as taken by PBR. To establish that a given  $|\alpha\rangle$  is ontic, you have to construct an argument showing that for any other QWF,  $|\beta\rangle$ , even when  $\langle\beta|\alpha\rangle \neq 0$ , it is always possible to find such a contradiction as shown above. They use  $n$  identically prepared and uncorrelated independent QWFs (I looked at  $n = 2$ ) generating a QWF,  $|\Psi\rangle = |\psi_1\rangle|\psi_2\rangle \dots |\psi_n\rangle$ , where each QWF is either  $|\alpha\rangle$  or  $|\beta\rangle$ .  $|\Psi\rangle$  is projected onto an entangled QWF measuring device (a combination of various gates and other devices used in quantum computers called a measurement circuit) that jointly measures the  $n$  systems in such a manner that there is always at least one of the  $2^n$  QWFs predicted with zero probability. Indeed this is a very clever idea as one can nearly always show<sup>7</sup> that  $|\Psi\rangle$ , being a product of independent QWFs, must consist of independent ontic states.

On the other hand, if a measurement of a state with zero probability ever occurs (e.g., corresponding to an EN measurement when a not-EN state was prepared, as indicated in Figure 6), indicating a violation of the predicted quantum probabilities, does that indicate Einstein was right after all and quantum physics is ontologically incomplete?<sup>8</sup>

Could this be proven experimentally? All one would need to do is show that the condition of never finding a zero probability case in any the  $2^n$  possible cases would possibly do it. Suppose that indeed one were to find all (measurement) projections onto such entangled base states devices never occurring with zero probability.<sup>9</sup> According to PBR the epistemic nature of QWFs in violation of quantum physics would be established. Einstein would emerge victorious and we would need a new physics beyond quantum physics.

In summary we have a logical proof here: For two or more QWFs the Born Rule (TBR) implies disjoint HV probability distributions (DPD),  $\text{TBR} \rightarrow \text{DPD}$ . However DPD does not necessarily imply the Born Rule  $\sim(\text{DPD} \rightarrow \text{TBR})$ . They are not equivalent. The important statement of PBR is that conjoint probability distributions (CPD) violate the Born Rule,

( $\text{CPD} \rightarrow \sim\text{TBR}$ ). That means CPD make the quantum state unknown and hence epistemological. CPD mean the quantum state is not fixed by a determination of the HV. A given HV will produce more than one quantum state possibility—hence the quantum state is epistemological. Since  $\sim\text{CPD}$  is the same as DPD and CPD implies a negation of the Born Rule,  $\text{CPD} \rightarrow \sim\text{TBR}$ ; reversing the logic we get  $\text{TBR} \rightarrow \sim\text{CPD}$  so  $\text{TBR} \rightarrow \text{DPD}$ .

Let me add a few more comments of my own here. I believe that until the ontology/epistemology issue is fully resolved (although readers may believe it already resolved after reading this review), we still have the “measurement problem” that stimulated such considerations as given by PBR, Bell, Bohm, and many others. We also still have the nonlocality issue to deal with. Perhaps PBR can resolve this issue. Ontologically speaking, what does it mean to have nonlocal influences? What does it mean to have an observer effect (collapse of the QWF)? Does the PBR solution resolve these problems?

Consider the effect of observation on an ontic QWF. Does a human being alter the QWF simply by making an observation? If the QWF is ontic then we have a real observer effect—observation (including nonlocal) indeed alters the QWF and therefore reality. That would mean that mind is inextricably tied into matter; they are truly entangled, and such a finding could lead to breaking discoveries in the study of consciousness. On the other hand, if the QWF proves to be epistemic in violation of the Born Probability Rule, observation is simply the usage of the Bayesian approach to probabilities wherein new information simply changes what we know, but leaves reality unscathed—at least what we mean by ontic reality. I hope that PBR and others continue this line of research. The next frontier may indeed not be space but will be the mind.

### Notes

- <sup>1</sup> Indeed Einstein did make this conclusion based on the EPR argument. However, it is not a conclusion of Bell’s theorem and certainly not Einstein’s conclusion based on Bell’s work because he was dead at the time. In fact, Bell’s theorem rather stymies this line of argument, since it says that you will still have nonlocal influences even if the wave function is epistemic, so this move does not solve the problem of nonlocality.
- <sup>2</sup> One may need to allow for the fact that measurements might be fundamentally noisy or stochastic and demand only that HVs specify probabilities for any measurement outcome.



- <sup>3</sup> In this SHO example (with  $m = \frac{1}{2}$  and  $k = 2$ ), assuming  $t = 0$ , the spring is stretched to a distance,  $\sqrt{E}$ , we get  $x = (\sqrt{E})\cos(2t)$  and  $p = (-\sqrt{E})\sin(2t)$ . The point in the phase plane rotates clockwise around the circle completing the cycle in the period of  $\pi$ . The probability density is simply a constant,  $dP/dt = 1/\pi$ , for all such circles regardless of the energy. Indeed that's why spring clocks work.
- <sup>4</sup> This sounds peculiar since clearly the directions are perpendicular. However, perpendicular in space does not necessarily mean the same thing as orthogonal in quantum physics. For those who know a little quantum physics: Two quantum states  $\alpha$  and  $\beta$  are orthogonal if and only if  $\langle\alpha|\beta\rangle = 0$ .
- <sup>5</sup> That is, there is no overlap of these probability distributions, so we have  $p_N(\lambda)p_E(\lambda) = 0$ . So this means either  $p_N(\lambda) = 0$  or  $p_E(\lambda) = 0$  for all  $\lambda$ .
- <sup>6</sup> Here there is an overlap, so  $p_N(\lambda)p_E(\lambda) \neq 0$ . So that means both  $p_N(\lambda) \neq 0$  and  $p_E(\lambda) \neq 0$  for  $\lambda$  within the overlap region.
- <sup>7</sup> PBR also carry out an error analysis to complete their proof.
- <sup>8</sup> Such a violation would tell us that it is possible, i.e. not in conflict with experimental results, that the wave function is epistemic.
- <sup>9</sup> Matt Leifer in an email to me pointed out that from any epistemic HV theory, you can always construct one that is ontological and gives exactly the same predictions. Such an argument is given in M. Schlosshauer and A. Fine, "Implications of the Pusey–Barrett–Rudolph no-go theorem," <http://arxiv.org/abs/1203.4779>. Consequently Leifer doesn't think it is possible to establish that the QWF is epistemic purely by experiment.

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FRED ALAN WOLF

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