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Volume 26, Number 1 2012

Editorial

5 Editorial — Stephen E. Braude

Research Articles

9 A Review of Sir William Crookes' Papers on Psychic Force with Some Additional Remarks on Psychic Phenomena — Masayoshi Ishida

43 The Implications of Near-Death Experiences for Research into the Survival of Consciousness — David Rousseau

81 Remote Viewing the Future with a Tasking Temporal Outbounder — Courtney Brown

111 Relativistic Variations in the Permittivity and Permeability of Free Space = Gravitation — Graeme D. Montgomery

Historical Perspective

117 The Psychic Sciences in France: Historical Notes on the *Annales des Sciences Psychiques* — Carlos S. Alvarado, Renaud Evrard

Obituary

141 Dr. Stuart Appelle, 1946–2011 — Thomas E. Bullard

Letters to the Editor

143 Response to Bousfield and LeBlond: Shooting Pipefish in a Barrel; or, Sauropterygian "Mega-Serpents" and Occam's Razor — Michael A. Woodley, Cameron A. McCormick, Darren Naish

146 Sir Arthur Conan Doyle's Resignation from the Society for Psychical Research — Leslie Price

147 Response to Leslie Price — Luca Gasperini

Book Reviews

149 Essay Review: Mysterious Minds, Irreducible to Brains? *Mysterious Minds: The Neurobiology of Psychics, Mediums, and Other Extraordinary People* edited by Stanley Krippner and Harris L. Friedman — Edward Justin Modestino

159 Essay Review: Bottazzi and Palladino: The 1907 Seances. *Mediumistic Phenomena: Observed in a Series of Sessions with Eusapia Palladino* by Filippo Bottazzi — Carlos S. Alvarado

169 *Matrix Energetics: The Science and Art of Transformation* by Richard Bartlett; and *The Physics of Miracles: Tapping into the Field of Consciousness Potential* by Richard Bartlett — Imants Barušs

177 *Among the Truthers: A Journey through America's Growing Conspiracist Underground* by Jonathan Kay — Henry H. Bauer

180 *The Moon and Madness* by Niall McCrae — Arnold Lieber

188 *The Moon and Madness* by Niall McCrae — Ramanujam Karthikeyan

191 *Glimpses of Eternity: Sharing a Loved One's Passage from This Life to the Next* by Raymond Moody with Paul Perry — Erlendur Haraldsson

194 *Glimpses of Eternity: Sharing a Loved One's Passage from This Life to the Next* by Raymond Moody with Paul Perry — Massimo Biondi

199 *Life After Life: The Investigation of a Phenomenon—Survival of Bodily Death* by Raymond A. Moody, Jr. — Lee Kinsey; Janice Miner Holden

203 *Reunions: Visionary Enounters with Departed Loved Ones* by Raymond Moody, Jr., with Paul Perry — Michael Grosso

207 *Recollections of Death: A Medical Investigation* by Michael B. Sabom — J. Kenneth Arnette

212 *Communicating Rocks—Writing, Speaking, and Thinking about Geology* by Peter Copeland — Constantin Cranganu

216 *Cuando Hablan Los Espiritus—Historias del Movimiento Kardeciano en la Argentina [When the Spirits Speak: A History of Kardecian Spiritualism in Argentina]* by Juan Gimeno, Juan Corbetta, & Fabiana Savall — Todd Jay Leonard

219 *The Psychopath Test: A Journey through the Madness Industry* by Jon Ronson — Tana Dineen

224 *Hidden Realms, Lost Civilizations, and Beings from Other Worlds* by Jerome Clark — Bill Ellis

SSE News

229 SSE Masthead
230 SSE 9th Biennial European Meeting; Parapsychological Assn. Call for Papers
231 SSE 31st Annual Conference
233 Index of Previous Articles in *JSE*
247 Order forms for *JSE* Issues, *JSE* Subscriptions, and Society Membership
250 Instructions for *JSE* Authors

Journal of Scientific Exploration, Vol. 26, No. 1, pp. 5–8, 2012 0892-3310/12

EDITORIAL

Composer and musicologist Nicolas Slonimsky published a fascinating and delightful book entitled *Lexicon of Musical Invective: Critical Assaults on Composers Since Beethoven's Time* (Slonimsky 1965). The book is a collection of what Slonimsky called "biased, unfair, ill-tempered, and singularly unprophetic judgments" (p. 3) about famous composers and their works. We find, for example, the *Gazette Musicale de Paris* on August 1, 1847, saying of Verdi, "there has not yet been an Italian composer more incapable of producing what is commonly called a melody." And this publication stuck to its guns. On May 22, 1853, it said of *Rigoletto* that it "lacks melody. This opera has hardly any chance to be kept in the repertoire."

The May 1804 issue of *Zeitung für die Elegente Welt* proclaimed

> Beethoven's Second Symphony is a crass monster, a hideously writhing wounded dragon, that refuses to expire, and though bleeding in the Finale, furiously beats about with its tail erect.

Amazingly, Beethoven appreciation still had a long way to go even more than a half century later. In 1857, thirty years after Beethoven's death, A. Oulibicheff said of Beethoven's style that it

> is nothing less than a violation of fundamental laws and of the most elementary rules of harmony—wrong chords, and agglomerations of notes intolerable to anyone who is not completely deprived of the auditory sense.

J. F. Runciman, in *The Saturday Review*, London, November 8, 1897, wrote the following about his "especial aversion, Johannes Brahms": "Brahms seems to appeal to those curious folks whose minds are made in bits carelessly joined or not joined at all." Despite the *especial* place of Brahms in his affections, in fact Runciman had plenty of aversion to spread around. In *The Saturday Review* of December 12, 1896, he wrote

> It is one's duty to hate with all possible fervor the empty and ugly in art; and I hate Saint-Saëns the composer with a hate that is perfect.

Moreover, like the *Gazette Musicale*, Runciman also stuck to his guns. In the February 19, 1898, issue of *The Saturday Review* we find him saying,

> Saint-Saëns has, I suppose, written as much music as any composer ever did; he has certainly written more rubbish than any one I can think of. It is the worst, most rubbishy kind of rubbish.

Most of the invective collected by Slonimsky was penned by critics. But composers are also represented generously. For example, Louis Spohr said of Beethoven that he "was deficient in esthetic imagery and lacked the sense of beauty." Tchaikovsky wrote in his diary on October 9, 1886, "I played over the music of that scoundrel Brahms. What a giftless bastard! It annoys me that this self-inflated mediocrity is hailed as a genius."

I don't think it's too much of a stretch to compare the intransigence, smugness, and lack of vision of these musical critics with the posturing of some who confidently criticize scientific innovation and exploration, or entire arenas of empirical investigation such as parapsychology and LENR (so-called "cold fusion"). Slonimsky himself saw the parallel, regarding it—not entirely correctly—as an illustration of non-acceptance of the unfamiliar. He may be right as far as the world of art is concerned. But I suspect that scientific shortsightedness, smug certitude, and easy recourse to cheap or dishonest dialectical tactics such as ad hominem arguments is a more complex matter. In any case, reading Slonimsky's book again gave me an idea I'd like to propose to some *JSE* reader (presumably someone with lots of spare time).

I think it would be both entertaining and instructive to compile a scientific book along the lines of Slonimsky's *Lexicon*. There are plenty of confident pronouncements by scientists and science writers illustrating clearly how humans are notoriously poor judges of the empirically possible. As Slonimsky noted, and as many readers of this *Journal* are all too aware, "The obscurantist opposition to progressive ideas in science is often made in the name of rational thinking and logic" (p. 29). Slonimsky also provided examples, many of which were new to me.

For instance, according Melanchton in his *Elements of Physics*,

> The eyes are witness that the heavens revolve in the space of twenty-four hours. But certain men, either from the love of novelty, or to make a display of ingenuity, have concluded that the earth moves. It is a want of honesty and decency to assert such notions publicly, and the example is pernicious.

Similarly, Dr. Nicolas Joly of Toulouse ridiculed Pasteur, writing, "It is absurd to think that germs causing fermentation and putrefaction came from the air; the atmosphere would have to be as thick as pea soup for that."

There's no shortage of similar examples. For example, Wilbur Wright commented,

> I confess that in 1901 I said to my brother Orville that man would not fly for fifty years. Two years later we ourselves made flights. This demonstration of my impotence as a prophet gave me such a shock that ever since I have distrusted myself and avoided all predictions.

Neverthless (and despite their success), in 1908 Orville said, "No flying machine will ever fly from New York to Paris."[1] Similarly, Lord Kelvin notoriously proclaimed in 1895, "Heavier-than-air flying machines are impossible." That same year, Thomas Edison seemed to agree:

> It is apparent to me that the possibilities of the aeroplane, which two or three years ago were thought to hold the solution to the [flying machine] problem, have been exhausted, and that we must turn elsewhere.

Lord Kelvin's confidence, in fact, like that of J. F. Runciman, extended more broadly. In 1897 he asserted, "Radio has no future." Earlier, in 1883, he was confident that "X-rays will prove to be a hoax." And speaking to the British Association for the Advancement of Science in 1890, he said, "There is nothing new to be discovered in physics now; all that remains is more and more precise measurement."

It's easy to continue. Apparently, Bill Gates once predicted, "We will never make a 32-bit operating system." In 1899, Charles H. Duell, Commissioner of the U.S. Patent Office, said, "Everything that can be invented has been invented."

You get the idea. My proposal (and hope) is that someone will do for scientific prediction and related fallible scientific judgments what Slonimsky did for music criticism. It would be good to have an organized collection on hand for those (probably inevitable) times when our capacity for self-criticism and humility seems to flag. In fact, I propose that this volume adopt one of Slonimsky's more clever innovations. In addition to a useful index of names and titles, Slonimsky's *Lexicon* includes what he called an "Invecticon, an Index of Vituperative, Pejorative, and Deprecatory Words and Phrases." So for example, we find such entries as these:

AGONY
Liszt, 112, 118
Scriabin, 172
Strauss, 185
Varèse, 213
Wagner, 247

COMMUNIST TRAVELING SALESMAN
Milhaud, 126

DEMENTED EUNUCH
Wagner, 239

HATEFUL FUNGI
Liszt, 111

OVERWHELMINGLY VULGAR
Ravel, 138

PUTREFACTIVE COUNTERPOINT
Bruckner, 80[2]

ZOO (feeding-time at)
Schoenberg, 156
Varèse, 214

ZOO (sleeping inhabitants of)
Webern, 250

So perhaps a *Lexicon of Scientific Invective* would include Dr. Walter Gross's 1940 statement,

> The so-called theories of Einstein are merely the ravings of a mind polluted with liberal, democratic nonsense which is utterly unacceptable to German men of science.

And perhaps that judgment would link to Invecticon entries under "ravings of a polluted mind" and "liberal democratic nonsense." Similarly, perhaps the term "absurd" will link to the following pronouncements (among others):

> The abolishment of pain in surgery is a chimera. It is absurd to go on seeking it . . . knife and pain are two words in surgery that must forever be associated in the consciousness of the patient. —Dr. Alfred Velpeau, French surgeon, 1839

> What can be more palpably absurd than the prospect held out of locomotives traveling twice as fast as stagecoaches? —*The Quarterly Review*, March, 1825

> Lee DeForest has said in many newspapers and over his signature that it would be possible to transmit the human voice across the Atlantic before many years. Based on these absurd and deliberately misleading statements, the misguided public . . . has been persuaded to purchase stock in his company . . . —a U.S. District Attorney, prosecuting American inventor Lee DeForest for selling stock fraudulently through the mail for his Radio Telephone Company in 1913

> The idea that cavalry will be replaced by these iron coaches is absurd. It is little short of treasonous. —Comment of Aide-de-Camp to Field Marshal Haig, at a tank demonstration in 1916

So, I hope that some enterprising (and probably otherwise idle) *JSE* reader will seize the opportunity to plug this hole in the literature of science. Initially, I was tempted to predict that it would be a big financial success, but now I'm not so sure . . .

Notes

[1] My thanks to Dick Shoup for this example.
[2] Actually, I agree with this one.

Stephen E. Braude

Reference

Slonimsky, N. (1965). *Lexicon of Musical Invective: Critical Assaults on Composers Since Beethoven's Time*. University of Washington Press. [first published in 1953]

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RESEARCH ARTICLE

A Review of Sir William Crookes' Papers on Psychic Force with Some Additional Remarks on Psychic Phenomena

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I dedicate this paper to Sir William Crookes, Mr. Daniel D. Home, and Professor Robert Hare.

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Abstract—A review was conducted of Crookes' lever experiments with the medium Daniel D. Home. The levers were 36″-long boards with a fulcrum at 1.5″ and 4.5″ from the left end in the first and second experiments, respectively, with a spring-balance suspension at the right end. In the first experiment, Crookes did not provide sufficient evidence to conclude that a psychic force exists. In the second experiment, direct contact between Home's hand and the lever was prevented by a water-vessel that had been set exactly over the fulcrum. If the vessel's center of gravity is assumed to have been accidentally off-center by even 5 mm to the right of the fulcrum, a force greater than 114 lbf (51.7 kgf) would have been required to be exerted on the water-vessel when Home's fingertips were dipped in the water to produce the observed increase in spring force (0.714 lbf: 324 gf); the recorded history of the spring force showed a pattern lacking the characteristics of oscillations. These are physically improbable. The analyses show that neither water sloshing nor external tremors can explain the experimental results. Furthermore, some well-known paranormal phenomena are reviewed with a discussion of possible impacts on some fundamental theories of current science.

Keywords: Psychical research—William Crookes—D. D. Home—lever experiments—psychokinesis—psychic force—spring balance—oscillation behavior—systematic errors—levitation—materialization—psychical knowledge

Introduction and Objectives

Journalist and historian Brian Inglis' extensive review of the history of psychical research was published in 1984. The author closed his book writing,

> If I have done nothing else, I hope I have done something to rehabilitate them [men of caliber in psychical research], at least in the eyes of their successors. (Inglis 1984:341)

So let us see the recent activities of their successors. Parapsychologist Dean Radin writes at the beginning of his book *The Conscious Universe*:

> The evidence is based on analysis of more than a thousand experiments investigating various forms of telepathy, clairvoyance, precognition, psychic healing, and psychokinesis. The evidence for these basic phenomena is so well established that most psi researchers today no longer conduct "proof-oriented" experiments. Instead, they focus largely on "process-oriented" questions like, "What influences psi performances?" And "How does it work?" (Radin 1997:6)

About 10 years before Radin's book was published, philosopher of science Stephen Braude became one, if not the first, academic to emphasize the importance of large-scale psychokinesis (PK) phenomena (also called macro-PK) recorded primarily before J. B. Rhine's initiation of laboratory-controlled parapsychology experiments in the 1930s. In his book *The Limits of Influence*, Braude (1986/1997) reviews large-scale PK phenomena that occurred during séances through the mediums D. D. Home, E. Palladino, and others. He argues against refutations offered by many skeptics. Let me call this a "macro-PK campaign." I do agree with Braude on the limits of influence of the laboratory-controlled small-scale PK (also called micro-PK) on mainstream science as well as on parapsychology. This is obvious when one considers the impact of macro-PK phenomena such as "levitation" and "materialization" on science. Indeed, no theory of psi has been put forward that can explain these. These two phenomena are obviously caused by conscious or unconscious human beings and are hence closely related to human consciousness—one of the most enigmatic subjects in current scientific research.

And if as Radin explains the existence of psi phenomena has already been well-established on the basis of more than a thousand experiments, why, then, any need for a review of Crookes' psi experiment with Home? I present the four-fold objectives of my work as follows:

(1) According to the book *Parapsychology in the Twenty-First Century* (Thalbourne & Storm 2005), mainstream parapsychologists still neglect large-scale PK. Hence, a scientific quantitative review of Crookes' lever experiments with Home would assist in the macro-PK campaign.

(2) Referring to Trevor Hall's accusation (1984/1962) of Crookes and the medium Florence Cook, psychologist Irwin writes in his textbook, *An Introduction to Parapsychology* (1989):

> Although there is no direct indication of any similar collusion between Crookes and Home this evidence [Hall's accusation], if confirmed, necessarily would undermine the integrity of Crookes and thereby raise doubts about the validity of all his psychical research including that involving Home. (Irwin 1989:24)

Hence, to an academic psychologist and parapsychologist, Braude's defense (1986) of Home's psi performance appears inadequate against Hall's

accusations. My review is intended to examine the lever experiments and the credibility of Crookes and Home.

(3) Home's psi performance displays phenomena ranging from PK to levitation to materialization of a hand. From my point of view, materialization phenomenon in authentic psychical research would have a great impact on current fundamental scientific theories, such as the Big Bang theory explaining the origin of our material world and the Darwinian theory of evolution explaining the origin of the human species. Hence, I personally feel that verification of the credibility of Home's psi performance is important. Moreover, Braude (2007) recently provided supporting (if not yet conclusive) data of paranormal materialization, in which a lady spontaneously produced a metal foil on her body; the foil *did not* dematerialize.

(4) However, the macro-PK campaign has had a limited influence on mainstream science. Why is this so? In my view, it is because the previous criticism still holds: "Unexplained cases are simply unexplained. They can never constitute evidence for any hypothesis" (e.g., see Gardner 1989:191). Hence, my final objective is to speculate about the possible impact of large-scale PK on mainstream scientific research.

The lever experiments were conducted in 1871 at Crookes' private laboratory (Crookes 1874) by William Crookes (1832−1919), Fellow of the Royal Society of London, with the medium Daniel D. Home (1833−1886). Crookes conducted two types of lever experiments. In the first one, Home directly touched one end of a 36″-long horizontal lever of mahogany board, which had a 1.5″-long flat foot (screwed to one end) supported on a table, while the other similar end was suspended from a spring balance. In the second experiment, Home dipped his fingertips into water in a glass vessel, the gravity center of which was set exactly over a knife-edge fulcrum screwed to a 36″-long mahogany board 4.5″ from one end, while the other end was suspended from a spring balance. In both experiments, it was ensured that Home did not do anything except that which was specified. Crookes reported that they observed and recorded scientifically unexplainable increases in the spring force in both experiments. These experiments were not new to Crookes, who stated:

> The references I now give afford an answer to the statement that these results must be verified by others. They have been verified over and over again. Indeed, my own experiments may be regarded merely as verifications of results already obtained and published by eminent scientific men in this and other countries. (Crookes 1874:27)

Crookes submitted his manuscripts on the experimental studies to the Royal Society of London in June 1871 and received technical comments on them from a reviewer. Although Crookes replied to the comments, his manuscripts were eventually rejected by the Committee of Papers of the Royal Society in January 1872. Crookes published his experimental papers in *The Quarterly Journal of Science*, his own scientific journal, in 1871. His experimental results were also published in 1874 in the form of a book reprinted from the Journal.

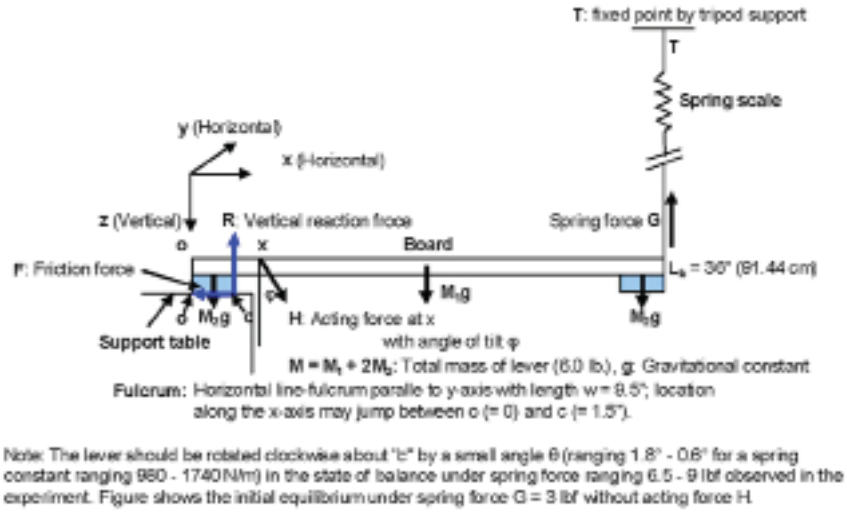
Many studies by skeptics on the psychical research of Sir William Crookes have been published in books, the most recent being those of Peter Lamont (2005) and Gordon Stein (1993), Ruth Brandon (1983), and Trevor Hall (1984/1962). Among them only Stein made somewhat technical comments on the lever experiments. Studies that present arguments for the defense of Crookes and Home have also been published (e.g., see Braude 1986/1997, E. Jenkins 1982). The former studies view Crookes' work with skepticism. However, to accept the criticisms of Crookes' lever experiments based on cursory reviews by scientists (i.e. reviewers of Crookes' manuscripts) or denouncements by skeptics without any scientifically quantitative basis is unsound and unfair. Hence, the primary objective of the present study is to show that the lever experiments were scientifically sound, even if the observed experimental results cannot be explained using Newtonian mechanics, unless some yet-unknown force or mechanism is assumed (see **Analyses of Crookes' Lever Experiments** section). In other words, the present study will reinforce the defenders' arguments for the credibility of the reported experimental facts. Furthermore, some well-known paranormal phenomena reported by Crookes and other researchers are reviewed with a discussion of possible impacts on some fundamental theories of current science (see ***Possible Implications of Large-Scale Psi Phenomena*** section).

Analyses of Crookes' Lever Experiments

Analysis of the First Lever Experiment

It should be noted that the results of the first lever experiment were presented on the basis of (1) the visual observations of the up-and-down movements of the index of the spring balance made by the experimenters and (2) the range of index movement recorded with an automatic register. The up-and-down movements of the index, however, were not recorded as a function of elapsed time since the start of the experiment.

(1) Specifications of the mahogany lever. The mahogany lever used in the first experiment is specified as follows (the x, y, z axes in Figure 1 represent the Cartesian coordinates for the lever):



Note: The lever should be rotated clockwise about "c" by a small angle θ (ranging 1.8° - 0.6° for a spring constant ranging 960 - 1740 N/m) in the state of balance under spring force ranging 6.5 - 9 lbf observed in the experiment. Figure shows the initial equilibrium under spring force G = 3 lbf without acting force H.

Figure 1. Schematic of the lever system in the first lever experiment.

Board: length L_b = 36" (91.44 cm), thickness t = 1" (2.54 cm), width w = 9.5" (24.13 cm);
Feet: screwed to the board at both ends, length c = 1.5" (3.81 cm) with the same width and thickness as the board;
The mass of the whole lever: 6 lb (2.722 kg)

On the basis of Crookes' statement: "As will be seen on referring to the cut (Fig. 3) [see Figure 2], the board was arranged perfectly horizontally" (Crookes 1874:15), we assume that at the start of the experiment, the left foot of the board was placed on the table, apparently supported uniformly by the surface of the table. The other end of the board was suspended by a string and a spring balance in series, the whole of which was suspended from a very firm tripod support. The normal weight of the board as so suspended was 3 lb (i.e. G_0 = 3 lbf without Home's presence). If, for now, we ignore force H in Figure 1, an analysis of the static balance of the lever shows: (1) if the fulcrum is at the left end (o) of the left foot, then the total mass of the lever $M = 2G_0/g$ = 6 lb and (2) if the fulcrum is at the right end (c), then $M \approx 1.0455 \times 2G_0/g$ = 6.273 lb, where g is the gravitational constant. Therefore, we assume that the fulcrum was at o (x = 0) at the start of the experiment with the spring force at equilibrium, G_{0o} = 3 lbf. If Crookes set the lever so that the fulcrum was at "c" initially, the spring force at equilibrium was G_{0c} = 2.87 lbf. Hence, the initial setting of the lever by

Crookes was more appropriate than setting it "perfectly horizontally."

The spring balance used in the first experiment would have been the same as or similar to that used in the second experiment. We can see a drawing of the spring balance used in the second experiment in Crookes' paper (Crookes 1874:34), which shows that the scale was rated for a maximum weight of 25 lb (11.34 kg) and had a minimum scale division of 1 lb. Hence, the weight measured by visually reading the scale index had an uncertainty in the reading of ±0.5 lb. On the basis of the total mass of the lever being 6.0 lb and the calculated total volume of the lever being 6,071 cm^3, the average density of the mahogany is calculated to be 0.448 g/cm^3.[1] This value is required in evaluating the mass of the mahogany lever used in the second experiment.

(2) Reported observations in the first experiment. The experiment began with the participation of Home, and Crookes described the experimental observations as follows (see Figure 2).

> Home placed the tips of his fingers lightly on the extreme end of the mahogany board which was resting on the support, while Dr. A. B. and myself sat, one on each side of it, watching for any effect which might be produced. Almost immediately the pointer of the balance was seen to descend. After a few seconds it rose again. This movement was repeated several times, as if by successive waves of the Psychic Force. The end of the board was observed to oscillate slowly up and down during the experiment.
>
> Home now of his own accord took a small handbell and a little card matchbox, which happened to be near, and placed one under each hand, to satisfy us, as he said, that he was not producing the downward pressure (see Fig. 3 [in Figure 2]). The very slow oscillation of the spring balance became more marked, and Dr. A. B., watching the index, said that he saw it descend to 6½ lbs. The normal weight of the board as so suspended being 3 lbs., the additional downward pull was therefore 3½ lbs. On looking immediately afterward at the automatic register, we saw that the index had at one time descended as low as 9 lbs., showing a maximum pull of 6 lbs. upon a board whose normal weight was 3 lbs. [. . .] I need scarcely add that his feet as well as his hands were closely guarded by all in the room. (Crookes 1874:14–15)

(3) Static balance of the lever in the first experiment. Crookes stated that in the experiment, they ensured that

> Mr. Home's fingers were not at any time advanced more than 1½ inches from the extreme end, as shown by a pencil-mark, which, with Dr. A. B.'s acquiescence, I made at the time. (Crookes 1874:15)

Either the right or left end edge of the 1.5″-long foot became the fulcrum of the lever.

After the submission of his manuscript in June 1871 to the Royal Society, Crookes received technical comments on the experiment. As acknowledged by

Crookes when quoting his paper and the comments made in a letter by Professor G. G. Stokes of the Royal Society, there were a couple of points to be clarified. These points were expressed as follows by Stokes, using a schematic figure of the relevant part of the lever.

> The breadth of the foot of the board was, I think, 1.5 or 2 inches, and the bell placed on it was, perhaps, 2 or 3 inches broad. (I can't carry the exact figures in my head.) Join the left edge of the top of the bell, *a*, with the right hand edge, *b*, of the base of the bell, and let *e f* be the joining line. Then we may suppose the fingers to have pressed in any direction short of the limiting line *e f*. Also as the board was rigid, the fulcrum for aught we know may have been at *c*. From *c* let fall a perpendicular *c m* on the line *e f*. Then the pressure of the finger may have acted at the distance, *c m*, from the fulcrum. Also, as the base lay flat on the table and both were rigid, for aught we know, an infinitesimal, and therefore imperceptible, tilt communicated to the table at the time of trying the experiment may have shifted the fulcrum from the edge *d* to the edge *c*, so that the weight of the hand may have acted by an arm longer than before by *c d*, which would have contributed to the result. (Crookes 1874:29)

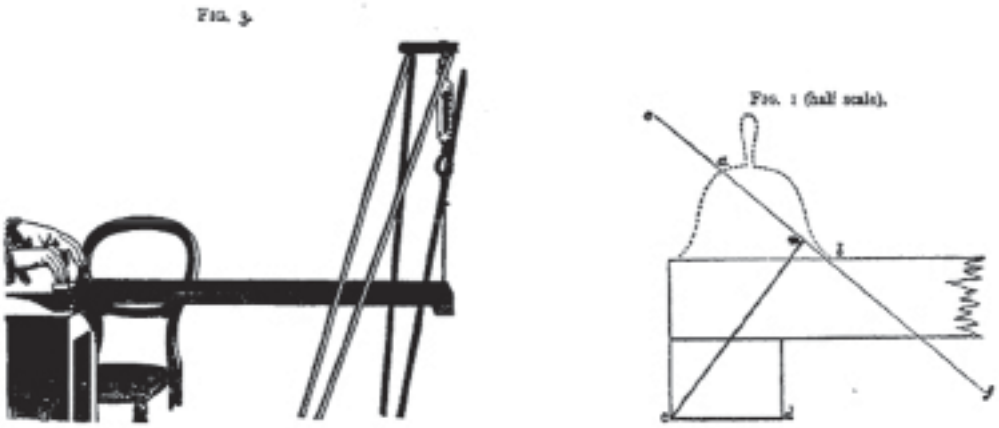


Figure 2. Setup of the first lever experiment and the sketch that accompanied Stokes' comment (Crookes 1874:15,29).

Crookes replied to Stokes' comment by calculating a moment balance of forces using the geometrical configuration shown in Figure 2 (the sketch on the right), writing "even if all your suppositions are granted" (Crookes 1874:30), which may imply that the geometry in Stokes' sketch above does not necessarily agree with the actual geometry in the experiment. That is, since

Crookes ensured that Home's fingers never advanced more than 1½ inches from the extreme end, as shown by a pencil mark, the right end of the handbell base was probably on the left side of the pencil mark, contrary to Stokes' sketch. However, in the following analysis, we use the relative geometrical size of the base of the handbell (estimated to be about 2″ in diameter) shown above. (The notations in the above reported figure are not used in the following, even though the same notation *c* appears).

Let us consider the static balance of the lever in Figure 1, assuming that an external force vector H is applied to the lever at a distance *x* from the left end with an angle of tilt φ from the vertically downward direction of the lever. Force vector H should be considered the sum total of the forces that Home exerted on the lever through his fingers. Because the spring force was observed to increase by 3.5 to 6 lbf, the right end of the lever should have fallen from the horizontal position. Hence, the fulcrum is located at c = 1.5″. The lever should rotate clockwise about the fulcrum by an angle θ, which depends on the spring constant, k_{sp}. Since the k_{sp} for Crookes' scale is estimated to be in the range of 980−1740 N/m,[2] the corresponding angle, θ, is estimated to be in the range of 0.6−1.8° (with the additional extension of the spring from equilibrium being in the range of 9−27 mm). $\varphi' \equiv (\varphi - \theta)$ is used in the following equations as the angle of tilt of acting force H from the z-axis in Figure 1. The static balance condition of the lever gives the following results.

$$H = \{(L_b - c) \times G - (1/2)(L_b - 2c) \times M \times g\}/f(\varphi'), \quad (1a)$$

$$f(\varphi') \equiv (x - c) \times \cos(\varphi') + 2t \times \sin(\varphi'), \quad (1b)$$

$$R = M \times g + H \times \cos(\varphi') - G, \quad (2a)$$

$$F \equiv \mu \times R = H \times \sin(\varphi'), \quad (2b)$$

where g and μ are the gravitational constant and coefficient of static friction, respectively; t is the thickness of the board; and M is the total mass of the lever. For the definitions of forces R, G, H, and $F = \mu \times R$, see Figure 1.

Equations 2a and 2b result in the following relationship between static friction coefficient μ and angle φ′:

$$\tan(\varphi') = \mu/\{1 - (M \times g - G)/R\}.$$

Because $M \times g - G$ ranges from −3 to −0.5 lbf and the reaction, R, is very large (>80 lbf) compared with $|M \times g - G|$ as long as force H is exerted close to the fulcrum, μ can be approximately expressed as follows:

$$\mu \approx \tan(\varphi'). \quad (2c)$$

Thus, the maximum angle of friction, $\delta = \tan^{-1}(\mu)$ (estimated as being less than 30°), is approximately equal to the angle of tilt, φ', when the left foot starts to slide on the support table. From Equation 1a, the required H to give G = 6.5 lbf decreases as φ' increases. This is shown in Figure 3. The results are summarized as follows:

(1) If H is acting within the range x = [0, c] (i.e. within the left side of the pencil mark) with an angle of tilt $\varphi' = 0°$ (i.e. strictly vertical), then no positive H (i.e. downward force) can give any increase in the spring force, G, from its equilibrium value, G_{0c} (2.87 lbf), when the fulcrum is at c (not at o).

(2) H acting at x = c with an angle of tilt $\varphi' = 30°$ is as low as 125 lbf (56.7 kgf).

(3) If H is assumed to be applied at x = 2″, 0.5″ to the right of the fulcrum as specified in the sketch (Figure 2) accompanying the comment from Stokes, the required H is about 250 lbf (113 kgf) for $\varphi' = 0$; it is about 87 lbf (39 kgf) for $\varphi' = 30°$.[3]

(Note: The φ' values at singularity in Figure 3 are the root of φ' in $f(\varphi') = 0$ for a given x within the range [0, c]. The negative solutions for H (i.e. the force pulls the lever upward) are spurious ones that give G = 6.5 lbf if the lever is fixed on the table with a freedom of rotation about the fulcrum at c.)

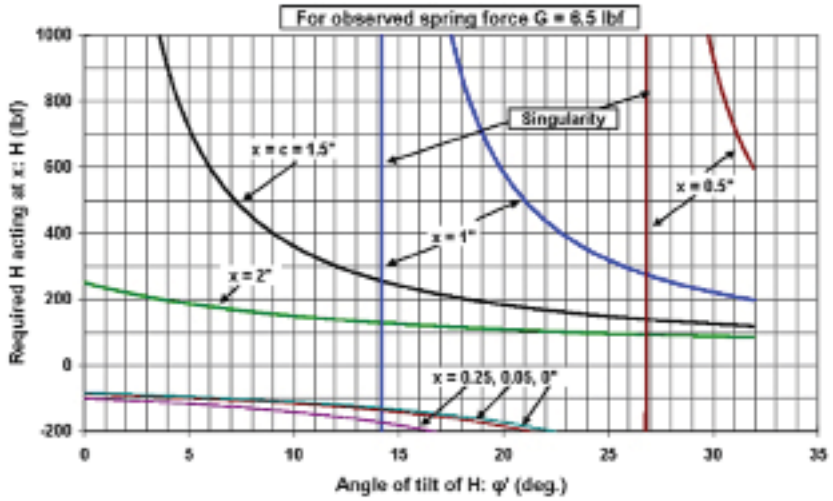


Figure 3. Solution of H for G = 6.5 lbf as a function of φ′ for x within [0, c = 1.5″] and at 2″.

Scientific common sense says that if the lever is being tilted downward about the fulcrum at c with a small angle, θ, from the horizontal plane (with a spring force $G > G_{0c}$), then downward force H exerted by Home always reduces G to its equilibrium value, G_{0c}, contrary to the observed change in the spring force. This appears to be quite a strange behavior of the lever caused by Home. It should be noted that these values of H are calculated on the basis of a static balance, assuming that the lever is not in motion. Actually, Crookes reported oscillations of the spring force from the initial equilibrium value.[4] Hence, the reported values of 3.5 and 6 lbf might have included overshoot reactions of the oscillating lever if the oscillation was of a physical nature. (In the second lever experiment, oscillations of a physical nature appear absent [see Figure 6 in the section ***Analysis of the Second Lever Experiment***].) However, the oscillations did occur because there were changes in the spring force caused by Home apparently touching the left foot of the lever.

The analysis in this section has shown that the experimentally observed increase in the spring force would have required a fairly large force H with a non-zero tilt angle φ′ (H > 125 lbf for dG = 3.5 lbf with φ′ <~30°) on the left foot of the lever. Although Crookes did not experimentally measure the force, H (its strength and direction), exerted by Home, it would have been small. The analysis above presents the possibility of explaining the experimental results based on Newtonian mechanics.

Analysis of the Second Lever Experiment

A lever experiment, in fact a series of experiments, was conducted apparently after it became clear that the scientific community was apathetic about the results of the first experiment. As shown in ***Analysis of the First Lever Experiment***, it is not possible to rule out the possibility of causing an increase in the spring force by pressing the left end foot of the lever in the first experiment, even though the force required to realize the observed increase in the spring force is not small. Hence, Crookes tried to eliminate the direct mechanical contact between the lever and Home's fingers by inserting a water vessel between the two in the second lever experiment. The experimental apparatus used in the second lever experiment is shown in Figure 4. A hemispherical copper vessel (N) was immersed in a glass water vessel (I). This copper vessel contained several holes that allowed water to flow between the two vessels, and was supported by a strong iron stand (L, 2″ from the board) on the floor. The vessels were placed 2″ apart.

This setup was a modification of that used by Robert Hare, M.D. (1781−1858), at the University of Pennsylvania in an experiment published in his book (Hare 1855). The double-vessel experimental apparatuses are compared as follows.

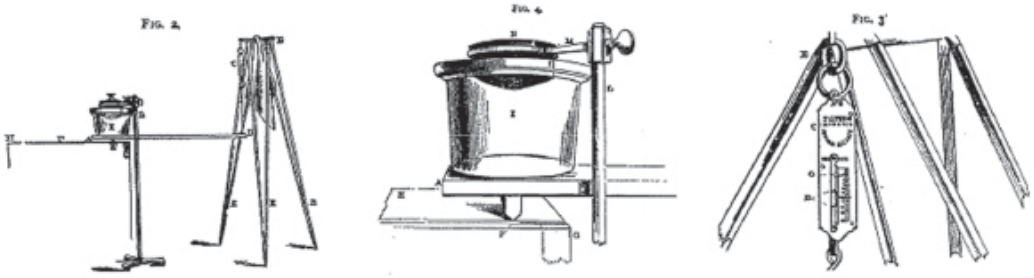


Figure 4. Apparatus of the second lever experiment (Crookes 1874:34–35).

(1) Hare's lever: a total length of 4′, fulcrum located 1′ from the left end, the right end suspended by a spring balance. The center of mass (COM) of the glass water vessel (diameter 9″ and height 5″) was off-center to the right of the fulcrum by 6″.

(2) Crookes' lever: a total length of 3′ (36″), fulcrum located 4.5″ from the left end, the right end suspended by a spring balance. The COM of the glass water vessel (diameter 9″ and height 7.5″) was set exactly over the fulcrum.

The second series of lever experiments with Home comprised four experiments in all: *Experiments* I through IV (*italics* are Crookes' [Crookes 1874:36−38]). Only *Experiment* I can be discussed from an analytical point of view because the other three experiments, in which Home did not mechanically touch the lever directly, not even via the water vessel, are very difficult to simulate on the basis of a physical model. In addition, critical discussions were provided only for *Experiment* I by Stokes. Crookes did not provide enough information on *Experiment* I for us to analytically simulate the experiment. However, we can estimate the necessary data from the figures of the experimental apparatus in his paper, which, Crookes suggested, were made on the basis of photographs (Crookes 1874:79). It should be noted that in *Experiment* I, the observed increases in spring force were fairly small—on the order of 5,000 grain-force (0.714 lbf = 324 gf)—compared with the 3.5−6 lbf observed in the first lever experiment and the 3 lbf and 7 lbf reported by Hare (Hare 1855:164−165). However, in the second series of lever experiments, Crookes recorded the time history of the change in the spring force in an excellent manner by recording the change in the location of the index of the spring balance on a smoked plate glass, which was moved horizontally by a clockwork system.

(1) Static balance of the lever system. Figure 5 is a schematic diagram of the apparatus used in *Experiment* I. The material density of the mahogany board is

assumed to be the same as that used in the first lever experiment. The length of the board, L_b, was 36″ and it had a wedge-shaped fulcrum screwed to it. The exact location of the fulcrum along the board was not described by Crookes (1874). In the following, the horizontal locations are defined as the distances from the left end of the lever in Figure 5. The location of the fulcrum is estimated to be at $a = 4.5$″ on the basis of Figure 4. The horizontal location of the COM of the water vessel is treated here as parameter b, although b will be very close to the fulcrum as described by Crookes ($b = a$ in Figure 5).

It is assumed that the outer diameter of the bottom of the glass vessel was 9″ (Crookes wrote only that the vessel diameter was 9″), and the geometry of the vessel is estimated from Figure 4. It is also assumed that the thickness of the glass vessel was 3 mm, except at the top rim of the vessel. The water height from the inner bottom of the vessel to the water surface in the hemispherical copper vessel was noted in the paper as 5.5″ (in the glass vessel) plus 1.5″ (in the copper vessel); thus a total of 7″.

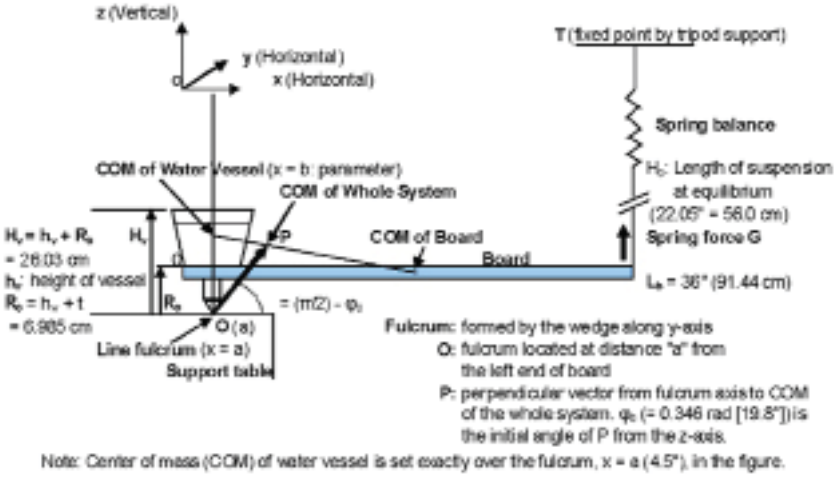


Figure 5. Schematic of the lever system in the second lever experiment.

The geometry of the mahogany board was the same as that specified in the first experiment. The geometry of the fulcrum wedge (assumed to be made of the same mahogany as the board) is estimated on the basis of Figure 4. The average density of the mahogany estimated in the section ***Analysis of the First Lever Experiment***, 0.448 g/cm^3, is assumed. Using these assumptions, the masses of the glass vessel (assumed to be a truncated cone), water, lever board, and fulcrum wedge are estimated as follows[5]:

Total glass volume = 977.7 cm^3
Water volume = 7688 cm^3
Total glass mass = 2.177 kg (assuming glass density = 2.227 g/cm^3)
Water mass = 7.688 kg
Total mass (water plus glass) of the water vessel W = 9.865 kg (21.748 lb)
Total mass of the mahogany lever $M = M_1 + M_2 + M_3 = 2.617$ kg (5.7694 lb)
(M_1: mass of board on the left side of the fulcrum = 0.314 kg,
M_2: mass on the right side of the fulcrum = 2.198 kg,
M_3: mass of the wedge fulcrum = 0.105 kg)

Using these data and assuming that the lever system was set in the static horizontal arrangement with no horizontal force acting, the static mechanical balance of the system in Figure 5 (without Home's presence) gives the spring force G and reaction R to the wedge as follows:

$$G = \{(b - a) \times W - (a/2) \times M_1 + (L_b - a)/2 \times M_2\} \times g / (L_b - a), \quad (3)$$

$$R = (W + M) \times g - G, \quad (4)$$

where

g: gravitational constant,
G: spring force suspending the right end of the board,
R: vertical reaction from the support table to the wedge, *a*
and L_b: 4.5″ and 36″, respectively,
b: x-axis location of the COM of the water vessel as a parameter.

The distance b – a is the off-center distance of the COM of the water vessel from the fulcrum. From Equation 3, if the COM is exactly over the fulcrum (b = a), then the spring force G_0 (before the participation of Home) does not depend on W and G_0 = 2.3736 lbf, and the reaction force R = 25.1435 lbf. These two forces together balance the total weight (27.517 lbf) of the water vessel and the mahogany lever as determined by Equation 4.

Crookes reported that he put the vessel exactly over the fulcrum (Crookes 1874:36). How did he set it exactly so? We usually achieve this using Equation 3 as follows. (1) First, set the lever almost perfectly horizontal without the water vessel on it, (2) record the location (G_0) of the index of the spring balance on a smoked glass plate using Crookes' automatic register, (3) place the water vessel (W) such that its COM is almost exactly over the fulcrum, and (4) adjust the horizontal position of the water vessel to exactly reproduce the same location (G_0) of the index of the spring balance as recorded without the water vessel.

Equation 3 then guarantees b = a; that is, the vessel is set exactly over the fulcrum. This is probably what Crookes did. Despite his statement, we assume that the vessel was accidentally set off-center to the right of the fulcrum by 2.63 mm; that is, b – a = 2.63 mm. Under this off-center condition, the spring force at balance changes from G_0 by $\Delta G = +0.0714$ lbf (500 grain-force = 32.4 gf = 0.318 N), which is 1/10 of the experimentally recorded 5,000 grain-force. The initial condition of *Experiment* I was described as follows (see Figure 4).

> The apparatus having been properly adjusted before Home entered the room, he was brought in, and asked to place his fingers in the water in the copper vessel, N. He stood up and dipped the tips of the fingers of his right hand in the water, his other hand and his feet being held. When he said he felt a power, force, or influence, proceeding from his hand, I set the clock going, ... (Crookes 1874:36)

Hence, the change in the spring force, dG(t), recorded by the index of the spring balance at time t since the start of the experiment can be defined by the following equation.

$$dG(t) = G(t) - G_{0H}, \tag{5}$$

where

G(t): actual spring force at time t,

G_{0H}: G(t) at the start of the recording with Home's fingertips dipped in the water (t = 0 s when Crookes set the clock going); G_{0H} may differ from G_0, which is the spring force at equilibrium before Home was brought into the room.

The small change in spring force, dG, was recorded using a special device on "a sheet of plate-glass which has been smoked over a flame." To represent him correctly, I quote his explanation of the recording system (in Crookes' Fig. 2 and Fig. 3 that are shown in Figure 4 of this paper):

> The following piece of apparatus is not shown in the figures. To the moving index, o, of the spring balance, a fine steel point is soldered, projecting horizontally outwards. In front of the balance, and firmly fastened to it, is a grooved frame carrying a flat box similar to the dark box of a photographic camera. This box is made to travel by clock-work horizontally in front of the moving index, and it contains a sheet of plate-glass which has been smoked over a flame. The projecting steel point impresses a mark on this smoked surface. If the balance is at rest, and the clock set going, the result is a perfectly straight horizontal line. If the clock is stopped and weights are placed on the end B of the board, the result is a vertical line, whose length depends on the weight applied. If, whilst the clock draws the plate along, the weight of the board (or the tension on the balance) varies, the result is a curved line, from which the tension in grains at any moment during the continuance of the experiments can be calculated. (Crookes 1874:34–35)

As written by Crookes in quoting his paper and comments given in a letter by the reviewer, Stokes, there were a couple of points to be clarified in the experiment. These points were expressed as follows by Stokes (Crookes 1874:29).

> (a) In your second paper the uncertainty as to the broad bearing is removed. But when the hand was dipped into the water the pressure on the base of the glass vessel (after a little time if the connecting hole be narrow) is increased by the weight of water displaced, and that would of course depress the balance.
>
> (b) I don't think much of mere tremors, for it would require very elaborate appliances to prove that they were not due to a passing train or omnibus or to a tremor in the body of one of the company.

Stokes' comment (a) is irrelevant if the initial condition of the experiment is carefully taken into consideration. The recorded force is the "change in spring force" from its initial value after the start of the experiment as defined by Equation 5. The effect of the buoyant force caused by the finger dipping on the static balance of the lever is included in G_{0H}, the initial value of the spring force, in Equation 5. What is not included in G_{0H} is everything that happens, including the effect of any change in the buoyant force, after the start of the experiment. The following reply of Crookes to Stokes' comment (a) above shows that $b - a \approx 0$ for Crookes' lever:

> . . . I have just tried the experiment of immersing my hand to the very utmost in the copper vessel (Home only dipped in the tips of his fingers) and the rise of the level of the water is not sufficient to produce any movement whatever on the index of the balance, the friction of the apparatus being enough to absorb the ounce or two thus added to the weight. (Crookes 1874:30)

The spring force G is given by Equation 3, which shows that if the gravity force on the water vessel mass, W, is increased by an amount $\Delta W \times g$, owing to the dipping of the fingertips for example, the change in G, (i.e. ΔG) can be calculated by (differentiating Equation 3 with respect to W).

$$\Delta G = (b - a)/(L_b - a) \times \Delta W \times g \quad (6a)$$

$$= O_{ff}/800.1 \times \Delta W \times g, \quad (6b)$$

with $O_{ff} \equiv (b - a)$ in millimeters.

It is assumed in Equation 6a or 6b that the addition of ΔW does not change the horizontal location of the COM of the water vessel. If ΔW is the weight of 3 cubic inches (49.16 cm^3) of water displaced by the dipped fingertips (Crookes 1874:76) (that is, $\Delta W = 49.16$ g = 758.7 grains and $O_{ff} = +2.63$ mm as discussed above), then ΔG is +2.5 grain-force (0.16 gf), which would be below the sensitivity of Crookes' scale. One may argue that an addition of ΔW shifts

the COM of the water vessel (W = 9.865 kg). A moment balance calculation shows that an addition of ΔW = 49.16 g within the diameter (~152 mm) of the hemispherical copper vessel never shifts the COM by more than 0.4 mm.

Conversely, to give the experimentally recorded quasistatic increase of ΔG = 5,000 grain-force in the spring force, Equation 6b requires a weight ΔW = 217.3 lb (98.5 kg) to be added to the water vessel under the off-center condition O_{ff} = +2.63 mm; if O_{ff} = +5 mm, then ΔW > 114 lb (51.7 kg). If O_{ff} = 0, then no value of ΔW (that does not change the horizontal location of the COM) results in a change in the spring force.

In Equation 6a, $(b - a)/(L_b - a)$ is the ratio of the arm lengths of the gravity force on the water vessel $W \times g$ and the spring force G. This ratio is almost equal to zero for Crookes' lever, while it is 1/6 for Hare's lever, as mentioned at the beginning of the section ***Analysis of the Second Lever Experiment***. Hare (1855) reported that his experimental subjects (mediums) dipped their hands in his hemispherical copper vessel and his spring balance showed an increase in the spring force of 3 lbf for Subject A and 7 lbf for Subject B (Hare 1855:164−165), which means that external forces of 18 lbf (8.16 kgf) and 42 lbf (19.05 kgf), respectively, must have been exerted vertically downward on the water vessel by the subjects. However, according to static hydraulics, Home in Crookes' experiment or Subject A or B in Hare's experiment could only exert a force corresponding to a reaction against the buoyant force they received when they dipped their fingertips or hands in the water, and these buoyant forces could never have exceeded 2.2 lbf (1 kgf) (for example, the total volume of both hands of the present author up to the wrists is about 560 cm^3). Hence, static mechanics concludes that it is impossible to exert such a magnitude of force on the water vessel by simply dipping fingertips or hands in the water.

The recorded experimental data or observations in Crookes' (the second lever experiment) and Hare's experiments mean that the increases in the spring force were very likely caused by the mediums, and to explain the results on the basis of Newtonian mechanics we need to assume the supposed "psychic force" (in Crookes' terms [1874:100−102]) or some other mechanism in the analysis. Similar reported results of increases in the spring force in Crookes' other experiments (*Experiments* II, III, and IV) with Home are consistent with this interpretation. In these other experiments, the water vessel, together with the iron stand, was removed from the system and Home was directed either to place both of his hands on the support table of the apparatus at P (in *Experiment* II) or to stand one foot (in *Experiment* III) or three feet (in *Experiment* IV) away from the lever with his hands and feet being firmly grasped by a bystander (Crookes 1874:37−38).

As discussed above, what is not included in G_{0H} in Equation 5 is any change in the spring force, including that caused by the change in the buoyant force on

Home's fingertips, which were possibly moving in the water, after the start of the experiment. Here, we are addressing dynamic mechanical effects, which were the subject of comment (b) made by Stokes. These dynamic mechanical effects are investigated in the following subsections.

(2) Simulation of dynamic behavior of the lever system.

(a) A model of small oscillations of the lever system. It is assumed that the lever and water vessel form a rigid body system for small oscillations about the fulcrum in Figure 5, although the water in the vessel is definitely not a rigid body (the effect of water sloshing in the vessel is discussed later). The rotation angle, θ, is defined as the angle between the fixed vertical z-axis and the axis that is fixed to the lever at the fulcrum at *a* and coincides with the z-axis at equilibrium. When the whole rigid body is rotated clockwise by angle θ, the vector, *P*, that defines the location of the COM of the whole system of the rigid body and is perpendicular to the fulcrum axis is also rotated by the same angle. The small oscillations of angle θ can be described by the following small oscillation equation of one degree of freedom:

$$\theta'' + 2\sigma\theta' + \omega_n^2\theta = \Sigma_i N_{ext,i}(t)/I_{yrt}, \quad (7a)$$

$$\omega_n^2 \equiv [k_{sp} \times A_0^2 - \mu g \times P \times \cos(\varphi_0)]/I_{yrt}, \quad (7b)$$

where

θ: rotation angle (rad);

ω_n: angular frequency of the natural oscillation of the system (rad/s);[6]

σ: damping coefficient of oscillation (1/s);

k_{sp}: spring constant (N/m), assumed to be either 980 or 1740 N/m;

A_0: initial arm length, A(θ = 0), of the spring force from the fulcrum axis (m), $A_0 = L_b - a = 0.8001$ m;

μ: total mass of the system (≡ $W + M_1 + M_2 + M_3$ in the static balance analysis in ***Analysis of the Second Lever Experiment***, ***(1)***) (kg), 12.482 kg;

g: gravitational constant (9.8 m/s^2);

P: distance from the fulcrum to the COM of the whole system (m), 0.2052 m;

φ_0: initial angle of vector P from the z-axis (rad), 0.346 rad (19.8°);

I_{yrt}: moment of inertia of the COM of the whole system about the fulcrum axis (kg·m^2), 1.023 kg·m^2;

$N_{ext,i}(t)$: time (t)-dependent moment (N·m) of the i-th external force (except for gravity and the spring force) about the fulcrum axis, where Σ_i indicates summation over i;

θ′, θ″: angular velocity and acceleration of the rotation angle θ, dθ/dt, and $d^2\theta/dt^2$.

The above data for P, φ_o, and I_{yrt} are calculated assuming that the COM of

the water vessel was set exactly over the fulcrum. The initial condition should be consistent with that of the experiment; it is assumed that the system was at equilibrium at t = 0 s, that is, $\theta = 0$ and $\theta' = 0$. The linear differential Equation 7a is solved numerically using the Euler–Romberg method.

The moment of the external force, $N_{ext,\,i}(t)$, depends on the arm length of each external force, $F_{ext,\,i}(t)$, from the fulcrum axis. Three external forces are considered in the following analyses:

(a) the force applied vertically downward on the spring suspension with arm length $A(\theta)$,

(b) the force caused by the sloshing of the water which is assumed to exert a horizontal force against the inner surface of the water vessel, assuming the arm length to be the height (H_v in Figure 5) of the water surface above the fulcrum, and

(c) the vertically downward force applied to the lever at an off-center location from the fulcrum—this force is supposed to simulate the effect of a change in the location of the COM of the water due to sloshing in the vessel.

(b) Simulation of oscillation behaviors in the second lever experiment. Two cases are simulated using the computer model: (1) pulling down of the right end of the lever to simulate the spring force history recorded in *Experiment* I and (2) the superimposing of external forces caused by water sloshing and external tremors onto the force in the first case.

***Case 1: Pulling down the right end of the lever to simulate the spring force history recorded in Experiment* I.**

A history of an external downward force $F_{ext}(t) = F_{psy}(t)$, produced from Fig. 5 in Crookes' paper in the form of a timetable, is exerted on the right end of the lever (see Fig. 5 in Figure 9A of this paper). The table data are interpolated with respect to time to obtain $F_{ext}(t)$ at any transient time. In the calculation, $k_{sp} = 980$ N/m and $\sigma = 1.0$/s were assumed. The large σ value is an effective damping coefficient, which takes into account the effect of water sloshing in the vessel (if the water in the vessel is assumed to be a rigid body, $\sigma \approx 0.3$/s).[7] The calculated results are shown in Figure 6 (the ordinate is shown upside down in accordance with the original Fig. 5 in Crookes' paper).

The purpose of this simulation is to show how the spring balance will respond if $F_{psy}(t)$ is applied to the right end of the lever, although $F_{psy}(t)$ itself is the experimentally recorded change in the spring force. An obviously strange feature concluded from Figure 6 is that the experimentally recorded change in the spring force, $F_{psy}(t)$, does not show the oscillation behavior calculated using the theoretical model. The damped oscillations calculated just after t = 2.5 s in Figure 6, which are caused by the fast increase and almost abrupt stop in the change in $F_{psy}(t)$ within a system period $T_p = 2\pi/\omega_n = 0.26$ s ($\omega_n = 24.32$ rad/s

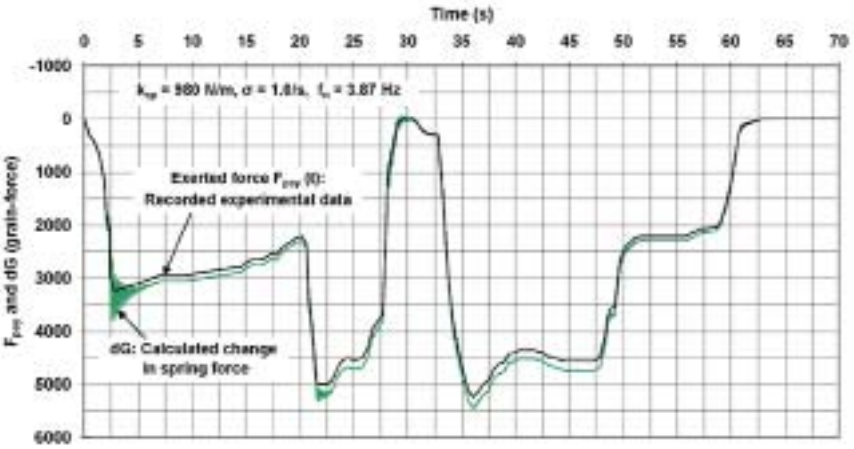


Figure 6. Case 1a: Simulation of the change in spring force in *Experiment* I (with k_{sp} = 980 N/m and damping coefficient σ = 1.0/s; exerted $F_{ext}(t) = F_{psy}(t)$).

for k_{sp} = 980 N/m), could have been recorded by the movement of the scale index if Home, who was judged to be a total charlatan by Stein (1993:97) and Martin Gardner (1989:xiv), somehow physically pulled down the right end of the lever in such a way as to result in $F_{psy}(t)$, using an almost invisible thread or "some clever mechanical arrangements, or legerdemain" (Crookes 1874:98) to fool the investigators. (We can see similar damped oscillations when we put a weight on a spring balance if the weight is within the maximum weight for the balance.) The damped oscillations are due to the inertia of the lever: The lever cannot abruptly stop at an equilibrium position for the temporally reached maximum force level (about 3,200 grain-force) because of its inertia. An equivalent mass, M_{eff}, of the lever system to a simple harmonic oscillator can be defined as $M_{eff} = k_{sp}/\omega_n^2$; the inertia is roughly that of a 1.66 kg mass for k_{sp} = 980 N/m. The recorded $F_{psy}(t)$ showed a pattern of variations in the spring force, but *the pattern lacks the characteristics of oscillations*, as if the lever system were no longer a harmonic oscillator. A small difference is seen between $F_{psy}(t)$ and the calculated response in Figure 6, and it is as large as 190 grain-force at t = 36 s; this difference can be explained by an increase in the arm length ($P \times \cos[\varphi_o] \times \theta$ in Equations 7a and 7b) of the gravity force caused by a small increase in the rotation angle, θ (≈ 0.0043 rad = 0.25°).

If the spring constant is as great as 1,740 N/m, the damped oscillations just after t = 2.5 s are a little less remarkable than the case with 980 N/m because the system period T_p is shorter or the inertia of the lever M_{eff} is less (ω_n = 32.66 rad/s, T_p = 0.19 s, M_{eff} = 1.63 kg for k_{sp} = 1,740 N/m).

The results of the analytical simulation suggest that the recorded change in the spring force in *Experiment* I was caused not by a natural external disturbance or a force pulling down the lever using a thread but by an unknown force in a very deliberate manner, apparently suppressing the oscillations of the system.[8] This reasoning is further justified by the following results for Case 2.

Case 2: Superimposing external forces caused by water sloshing and external tremors onto the force in Case 1.

In this simulation, external forces caused by water sloshing and external tremors are considered.

(a) Water sloshing. Stokes did not use the term "sloshing"[9] in his comment (b) on the second lever experiment. A possible effect of water sloshing was mentioned by Stein in his book, referring to Robert Hare's book (1855):

> Nevertheless, the possibility remains that some of the water could have been "sloshed" far enough off the fulcrum to have registered on the scale. (Stein 1993:96)

This comment by Stein on the possible effect of water sloshing is intended to claim that even if the water vessel was set exactly over the fulcrum, the horizontal location of the COM of the water vessel might have shifted "far enough off the fulcrum" when water sloshed in the vessel; hence, the effect would have registered on the scale.

The dynamic response of the lever system to the water sloshing is simulated in the following way. When speculating on the effect of water sloshing in the vessel, there is another point to consider. If the water mass sloshes in a direction perpendicular to the fulcrum axis, the impulse caused by water sloshing against the vessel wall gives an impulsive torque about the fulcrum. The arm length of the impulse could be provided by the height of the top of the vessel (H_v = 260.3 mm) from the support table in Figure 5.

How large will the impulse be? A very simple model is provided to give the possible maximum impulse of water sloshing in *Experiment* I (see Figure 7). Crookes reported that the depth of the water in the copper vessel was 1.5″. Suppose that there is a horizontal dividing surface in the glass vessel at a depth of 1.5″ (38.1 mm) from the water surface. The divider completely separates the top part of the water from the lower part. Now, let us forget the copper vessel. Suppose further that the top water is gathered to the left side of the top pool by a vertical divider that is parallel to the fulcrum axis. The total mass of the water in this shifted pool is estimated to be M_{up} = 1.657 kg, and the pool height becomes h_2 = 50.8 mm (corresponding to the estimated top of the vessel). Now, suppose that the vertical divider is removed suddenly, and the water is free to flow from the shifted left pool to the empty right pool. The water rushes to the inner surface of the vessel wall of the right pool and imparts an impulsive force.

This model gives the *x*-component of the sloshing force, $F_{slosh\ 1} = 0.72$ N, whose moment is $N_{slosh\ 1} = H_v \times F_{slosh\ 1} = 0.187$ N·m with a duration of the impulse dt = 0.1 s. $N_{slosh\ 1}$ is assumed to be an alternating square impulse; it is a clockwise torque when water sloshes to the right and counterclockwise torque when water sloshes to the left. The sloshing impulse is assumed to start at $t_0 = 1$ s into the transience and to repeat several times with the same duration, dt = 0.1 s, and alternating plus and minus signs. It is assumed that there are calm periods of the same dt between impulses. Because the sloshing phenomenon eventually disappears, the impulse is multiplied by the function $\exp\{-(t - t_0)/\tau\}$ with an arbitrarily assumed $\tau = 2$ s. The period of sloshing is $T_{slosh} = 3 \times dt = 0.3$ s; $\omega_{slosh} = 2\pi/T_{slosh} = 20.9$ rad/s.

To be consistent with this water sloshing, the effect of the change in the location of the COM (oG_m in Figure 7) of the upper pool water with respect to the fulcrum should be considered, and this latter effect is far greater than the former. The COM, which is initially set exactly over the fulcrum, is shifted from the fulcrum by $oG_m = 33.4$ mm. The maximum torque of the latter effect is calculated as $N_{slosh\ 2} = oG_m \times F_{slosh\ 2} = oG_m \times (M_{up} \times g) = 0.542$ N·m. The arm length, oG, is assumed to change as $oG(t) = oG_m \times \sin[\omega_{slosh}(t - t_0)]$. This formulation is considered specifically for the case of Crookes' lever, in which the water vessel is located exactly over the fulcrum at equilibrium ($O_{ff} = 0$). $N_{slosh\ 2}$ is in phase with $N_{slosh\ 1}$.

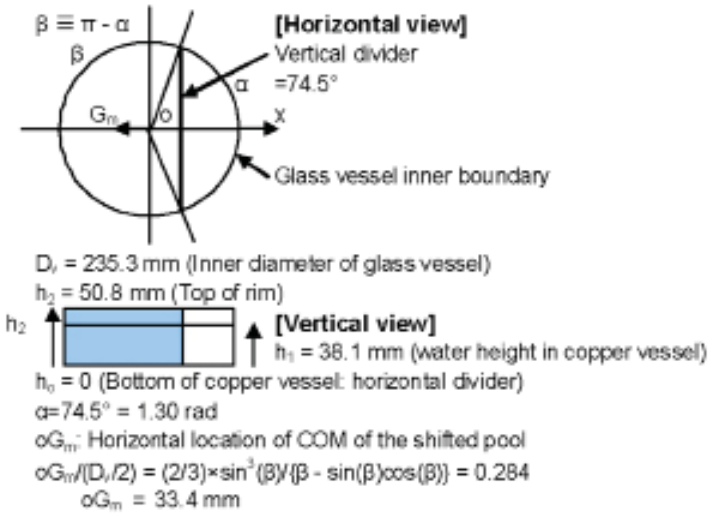


Figure 7. Schematic of the water-sloshing model with a shifted water pool above the bottom of the copper vessel.

Because the buoyant force on the dipped fingertips could never have exceeded 1 kgf, as discussed in ***(1) Static balance of the lever system***, any change in the buoyant force caused by moving fingertips could never have exceeded 1 kgf. Hence, $N_{slosh\ 2}$ (moment of 1.657 kgf) covers the effect of the change in the buoyant force on the lever; that is, we do not need to simulate the effect specifically.

(b) External tremors. An external tremor, F_{ext} (t), is assumed to be exerted vertically on the right end of the lever. Any F_{ext} (t) can be decomposed into a Fourier spectrum, regardless of whether the spectrum is discrete or continuous. The lever system responds to the external tremor on the basis of its own natural oscillation frequency, $f_n = \omega_n/2\pi$ (3.87 Hz). Hence, let us consider only the case in which F_{ext} (t) has the form of $F_{exo} \times \sin(\omega_{ex}t)$, with ω_{ex} as a parameter around the natural angular frequency of the lever system, ω_n. The amplitude, F_{exo}, can be arbitrarily assumed for Case 2. Suppose that an external tremor accidentally caused, not by Home, but by another person or an omnibus or train passing near Crookes' house resulted in an additional maximum extension of the spring as large as 1 mm. In this case, F_{exo} may be assumed to be $F_{exo} = k_{sp} \times 0.001$ N. F_{exo} is 0.98 N for k_{sp} = 980 N/m. F_{exo} should be the Fourier spectrum of $F_{ext}(t)$ at $\omega = \omega_{ex}$. The arm length of $F_{ext}(t)$ is A_0 (= $[L_b - a]$ = 0.8001 m in Figure 5). The torque of $F_{ext}(t)$ is $N_{ext}(t) = F_{ext}(t) \times A_0$.

Assuming external tremors of $F_{ext}(t) = 0.98 \times \sin(\omega_{ex}t)$ with two arbitrarily assumed frequencies ($f_{ex} = \omega_{ex}/2\pi$), f_{ext1} = 10 Hz and f_{ext2} = 5 Hz, the system response is calculated for the superposed forces. The first tremor is arbitrarily assumed to gradually start at t = 10 s and disappear at t = 17 s; the second is assumed to start at 40 s and disappear at t = 50 s.

The torques of these four external disturbances, $N_{ext,\,i}$ (t) in Equation 7a, as specified in ***(a)*** and ***(b)*** above are shown together with the torque of the force $0.964 \times F_{psy}(t)$ (with an adjusting factor of 0.964) in Figure 8A (the ordinate is shown upside down).

The calculated system response is shown in Figure 8B. The lever system has a natural frequency, f_n (= $\omega_n/2\pi$) = 3.87 Hz. The assumed fundamental frequency of water sloshing is f_{slosh} = 3.33 Hz; the frequency ratios f_{slosh}/f_n, f_{ext1}/f_n, and f_{ext2}/f_n are 0.86, 2.58, and 1.29, respectively. The first and third frequency ratios are relatively close to the resonance value (1.0), and hence these two disturbances are amplified in the system response, as seen in Figure 8B. One may argue that if the water sloshes in the vessel and the lever system responds to the impulse, then the natural angular frequency, ω_n, in the basic Equation 7b will no longer be constant but dependent on time t. This situation will be the case of "parametrically excited oscillation." However, even if such an effect is taken into account, the system behaves in an oscillating manner very different from the recorded history of the change in the spring force, $F_{psy}(t)$, in *Experiment* I.

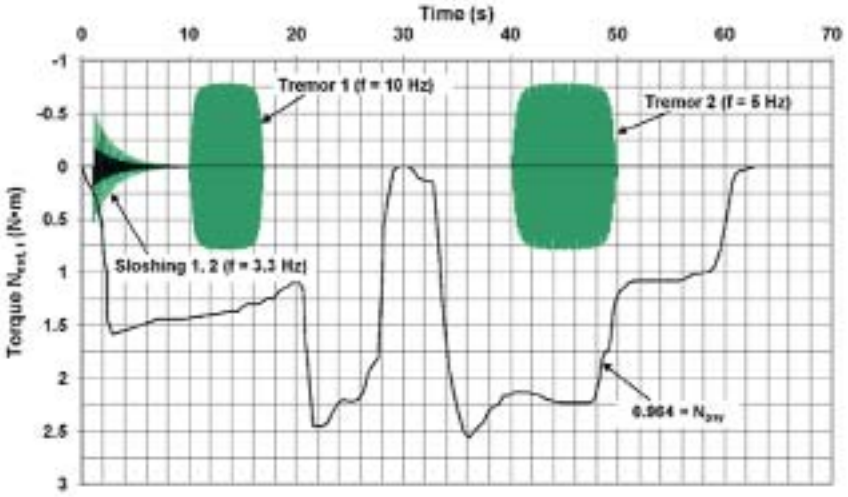


Figure 8A. Torques of external forces supposed in Case 2.

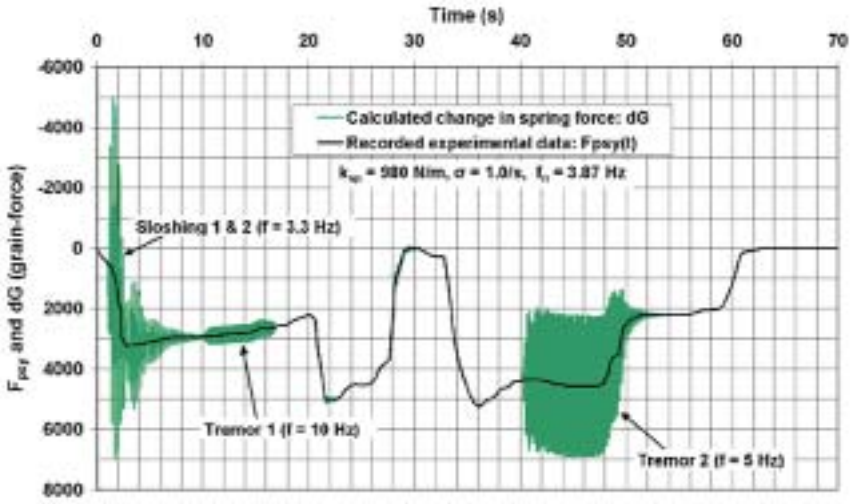


Figure 8B. Case 2: System response to the superimposed external forces/ torques in Figure 8A (with k_{sp} = 980 N/m and σ = 1.0/s).

Hence, Figure 8B provides our response to the comment of Stokes on the probable effect of external tremors and the comment of Stein on the probable effect of water sloshing in the vessel. If there had been such disturbances as speculated in Crookes' *Experiment* I, the effects would have appeared as calculated in Figure 8B. However, the recorded system response seems to have shown no effect from such disturbances.

If there is no force $F_{psy}(t)$, or, equivalently, if Home was not present at the scene of *Experiment* I, and only the speculated external disturbances exist, then the system response shows oscillations about the equilibrium, i.e. the baseline ($dG = 0$), caused by the sloshing and external tremors. Even if the maximum extension of the spring due to the external tremors becomes larger than the assumed 1 mm, the system response is basically the same, i.e. oscillations about the baseline.

Finally, all these theoretical analyses of the second lever experiment presuppose some uncertainties in the geometry and mass of the water vessel. These uncertainties affect the inertial moment of the water vessel. For example, the actual thickness of the glass vessel may have been 5 mm instead of the assumed 3 mm. This thicker glass vessel results in an approximately 4.6% increase in the total mass (W) of the water vessel containing water and a 4% increase in the total mass (μ) of the lever system. These increases result in a 6% increase in the total moment of inertia (I_{yrt}) and a 1.4% decrease in the natural angular frequency, ω_n. The biggest uncertainty in the analysis is probably in the spring constant k_{sp} (due to a lack of information in Crookes' paper [1874]). The natural angular frequency is increased by 34% (from 24.3 to 32.7 rad/s) if k_{sp} changes from 980 to 1740 N/m; hence, the parametric cases of k_{sp} have been mentioned in the above analyses. These analytical results correspond to what Crookes stated in his reply to Stokes' comment (b):

> You say "you don't think much of mere tremors," as if in the other experiments described in my second paper the movements of the apparatus were only of this kind. This is not the case; the quivering of the apparatus always took place before the index moved, and the upward and downward motion of the board and index was of a very slow and deliberate character, occupying several seconds for each rise and fall; a tremor produced by passing vehicles is a very different thing from a steady vertical pull of from 4 to 8 lbs., lasting for several seconds. (Crookes 1874:30)

Had Crookes shown the natural oscillation behavior, that is "the quivering," of the apparatus in a figure recorded with his system, his argument would have been more persuasive. The theoretical results presented above can be expected from the start without performing any calculation, if not on a quantitative basis. The reviewers of Crookes' papers could have at least given some constructive advice on ways to clarify any scientific ambiguities in his experiments for the acceptance of his papers. The conclusion of ***Analysis of the Second Lever***

Experiment section is that the speculations of Stokes and Stein cannot explain the recorded history of the change in the spring force in *Experiment* I. Although we cannot explain the exact procedure, we are compelled to conclude that Home exerted force $F_{psy}(t)$ on the lever system when he said "he felt a power, force, or influence" as he dipped his fingertips in water. *In my view*, Crookes' four experiments, *Experiments* I to IV, in the second series of experiments and the previously mentioned Hare's cases together probably demonstrate the existence of macro-PK. However, I expect many criticisms against this view of mine; hence, let me continue the discussions in the ***Discussion*** section and ***Possible Explanation of Crookes' Experiments?*** section.

Many researchers have defended Home's psychic abilities against various skeptical arguments to refute his cases. I hope this study enhances Home's credibility and stimulates mainstream scientists' interest in paranormal phenomena in general.

Discussion

(1) Control of experiments. The experimentalist Crookes wrote:

> In the meanwhile I trust that others will be induced to pursue the investigation in its scientific form. It should, however, be understood that, equally with all other scientific experiments, these researches must be conducted in strict compliance with the conditions under which the force is developed. (Crookes 1874:42)

From the above extract as well as from other quotations of Crookes in which he described how he prevented, with his collaborators, Home from performing any tricks in his experiments, I have no grounds to say that Crookes did not have sufficient control over his experiments with Home. Apparently, the fact that Crookes' (1874) descriptions are insufficient to recreate his experimental setup may invite criticism from the scientific community. Further, when one sees the recorded results (Figs. 5, 6, 7, and 8 in Crookes' paper [1874:37–38]; the first two figures are reproduced in Figure 9A and 9B in this paper), the figures reveal that the recording device was primitive. Nevertheless, the device was sensitive enough to record 5,000 grain-force and some abrupt changes in the spring force. One may evaluate Crookes' experimental setup as rough or primitive, and hence, one may say that the 5,000 grain-force (324 gf = 3.18 N) recorded by Crookes' device results in only a few millimeters of extension in his spring balance, which can be easily realized in conceivable ways such as external disturbances to the experimental setup. Three examples of such conceivable disturbances were *explicitly expressed* by Stokes and Stein, as discussed in Cases 1 and 2 in the section ***Simulation of Dynamic Behavior of the Lever System***, in which I rebut their arguments. It should be noted again that

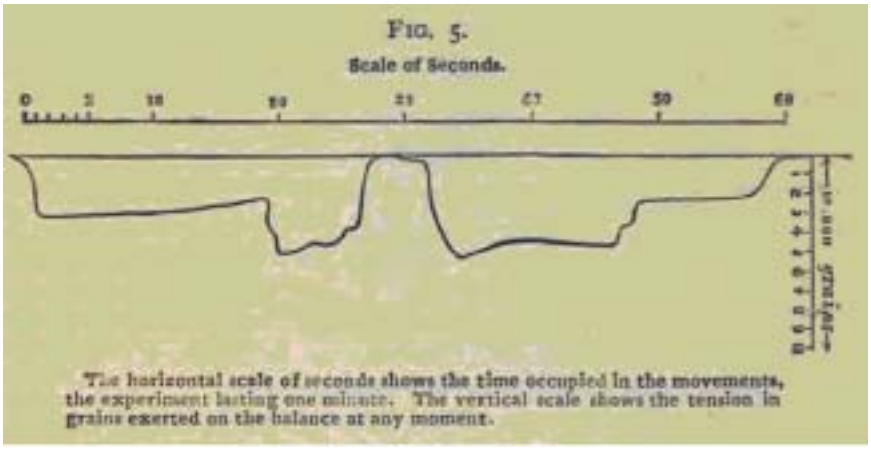


Figure 9A. Reproduction of Crookes' Fig. 5: Result of *Experiment* I (Crookes 1874:37).

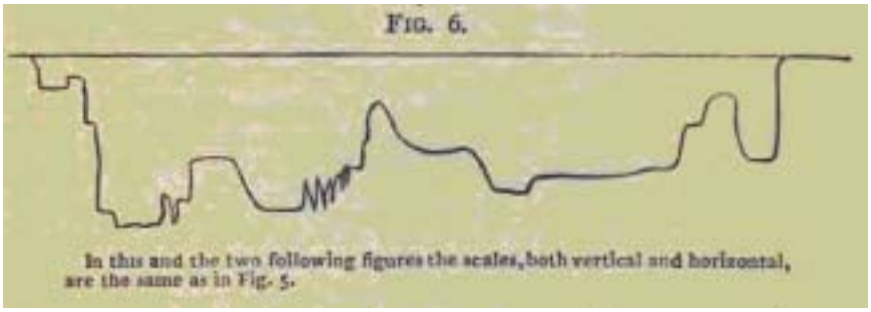


Figure 9B. Reproduction of Crookes' Fig. 6: Result of *Experiment* II (Crookes 1874:37).

the recorded $F_{psy}(t)$ showed a pattern of variations in the spring force, but *the pattern lacks the characteristics of oscillations*; any conceivable way to explain Crookes' results must realize *this feature*.

However, it is more than 140 years since the experiments of Hare and Crookes, and hence contemporary replication of the experiments is desirable to further confirm the validity of their experimental results. Some replication experiments, if not of *reproducible* nature, could be possible in the future; hence, the analyses in this paper have a meaning and are not futile.

(2) Non-reproducibility of Crookes' experiments. Obviously, Crookes could not *reproduce the same results* even in his own experiments, as he stated:

> In the case of Home, the development of this force [psychic force] varies enormously, not only from week to week, but from hour to hour; on some occasions the force is inappreciable by my tests for an hour or more, and then suddenly reappears in great strength. (Crookes 1874:40)

Crookes did not discard his results despite the non-reproducibility of the results, and obviously this earned him the contempt of the scientific community, as detailed in Crookes (1874:45–80). One cannot propose or standardize any cause-and-effect law on the basis of such phenomena. It has been considered that *scientifically significant physical phenomenon* is that which can be regularly reproduced by anyone who conducts the appropriate experiment in the way prescribed. This reinforces the previously mentioned critical statement: "Unexplained cases are simply unexplained. They can never constitute evidence for any hypothesis." However, this criticism is in conflict with the spirit of scientific research. If we continue to ignore the unexplained cases, we will never learn what is within them. Obviously, to search for the reason why the supposed mind–matter interaction such as that in Crookes' experiments is irregular and probably impossible to reproduce is one major theme for psi-theoreticians.

Additional Remarks on Psychic Phenomena

Possible Explanation of Crookes' Experiments?

Assuming that macro-PK really exists, as recorded in Crookes' second series of lever experiments, and that it is a result of the interaction between the human mind (or the brain) and inanimate matter, the next question is how the interaction could occur. Radin (1997) writes in Chapter 16: Theory: "This is not to say that there are no theories of psi, for actually there are many. They range from . . . (pp. 277−278)." However, I would like to introduce an idea to explain psi-phenomena in general, on the basis of some relevant discussions made so far in parapsychology studies.

Irwin introduced several specific theories and pre-theoretical ideas to explain psi phenomena (in the category of extrasensory perception [ESP] and micro-PK) in his introductory textbook on parapsychology (1989:Chapter 8). Among them, the idea of "pseudosensory models" (of ESP), which is obviously related to the idea of the "sixth sense," attracted my attention. Moreover, Radin writes about "What psi implies for various fields of scientific research" in his book *The Conscious Universe* (1997:Chapter 17). He asks questions about possible implications for biological research. Among them, "Are there secret senses we

have overlooked?" (Radin 1997:292) attracted my attention. Moreover, there is Charles Richet's "cryptesthesia" (for explaining ESP), meaning "*the human mind* [as a function of the brain] *has means of cognition other than our five poor senses*" (Richet 1923:600).These ideas suppose that the human brain has some yet-to-be found sensory functions other than the five senses.

My interest is about psychical knowledge that broadly explains psi phenomena (from ESP to macro-PK) on the basis of "inner senses" used in everyday life by our subconscious self, i.e. the inner self or inner ego. Some of the scientific basis of the significance of this psychical knowledge was described in Ishida (2010:Section 4.3). This psychical knowledge explains that we use our five outer (physical) senses as extensions of the inner senses; our physical senses allow us to perceive the physical world because they, as extensions of the inner senses, are finely attuned to the physical world. I refer to at least nine "inner senses," described primarily in Roberts (1997a, 1997b). Roberts summarized the nine inner senses in her book *The Seth Material* (2001/1970:Chapter 19), but in modest words, i.e. ignoring the more flagrant terms such as levitation, teleportation, and space travel. According to this psychical knowledge, the biological instinct is closely related to the sixth inner sense, which Roberts called "Innate Knowledge of Basic Reality." Regarding ESP, this psychical knowledge says:

> And while awareness of clairvoyance is fairly rare, it does exist; and though watered down in most instances, is a natural method of warning individuals of happening with which their own outer senses would not be familiar. It is a natural method of protecting the individual by giving him an inner knowledge of events. Without constant clairvoyance on the part of every man and woman, existence on your plane would involve such inner, psychological insecurity that it would be completely unbearable.
>
> [. . .] As telepathy operates constantly at a subconscious level, as a basis for all language and communication, so clairvoyance operates continually so that the physical organism can prepare itself to face its challenges. (Roberts 1997b:16–17, *ES2*/Session 44 on April 15, 1964)[10]

Hence, it is my expectation that parapsychologists gain greater insight into psi phenomena by unearthing cryptesthesia or secret senses that we may be unaware of. In my opinion, paranormal phenomena will never be understood as long as researchers confine themselves to a closed understanding of the physical dimensions. The above-mentioned inner self belongs to the nonphysical dimension. In my previous work (Ishida 2010), I stressed that physical dimensions are not a closed system, but are connected to nonphysical dimensions via a bridge of "consciousness," and it is via this bridge that the cherished law of conservation of energy is probably violated in the several well-known modes of psychological transition events, which are a blind

spot for physicists. According to the above-mentioned psychical knowledge, human ego has its root in the unconscious self, which belongs to nonphysical dimensions. To understand the paranormal phenomena in psychical research, we need a detailed knowledge of the abilities of the unconscious self, which have been extensively reviewed in Ellenberger's (1970) book *The Discovery of the Unconscious*. If I understand him correctly, this is one of the points that Jule Eisenbud emphasized in his book *Parapsychology and the Unconscious* (1983).

Possible Implications of Large-Scale Psi Phenomena

Levitation and materialization phenomena in psychical research, if verified as authentic, could have a great influence on mainstream science. Even the levitation phenomenon of macro-PK, which appears very close to reality because there are contemporary people (of the Transcendental Meditation organization) who are apparently practicing levitation in their own circle, could have a great impact on physics, if the mechanism of interaction between the mind and matter is explained. I would like to focus on the materialization phenomenon in this section to discuss its possible implications for mainstream science.

The temporal materialization and dematerialization of a human hand or an apparently living human form in psychical research is well-known since the initiation of psychical research in 1882. The materialization of Katie King in Crookes' séances (Crookes 1874:108−112) with the medium Florence Cook (conducted a few years after the lever experiments with Home) became notorious because of the alleged exposure of Miss Florence Cook as a fraud in her other séances (e.g., see Hall 1984, Stein 1993:43−48). However, materialization events are not specific only to Crookes' cases. Many similar events have been reported up until the early 20th century (e.g., see Carrington 1939, Beloff 1993), and to my knowledge events have been reported as recently as 1964 and 1968 (see Roberts 1997a:52−58, 1999:218−221, 2000:295−296). Many psychical researchers are strongly convinced of their experiences of materialization events; for example, Hereward Carrington (1880−1958) quoted Theodore Flournoy (1854−1920) as follows:[11]

> If such a phenomenon is authentic, it would be interesting to note the revolution which must necessarily follow in our biological ideas. Nature has taken upon our globe some hundred million years to transmute chemical substances into humanity; yet now it requires but twenty years to complete an adult; and *voilà!* by means of a young girl asleep behind a curtain it is possible, by reason of a species of partheno-genesis of a nature yet unguessed, to produce in two minutes a veritable Arab, of fine stature, with a beard down to his chin, walking, speaking, breathing as ourselves . . .

After quoting Flournoy, Carrington continued:

> It is hardly to be wondered at that biologists reject the very idea *a priori!* Nevertheless, there is much evidence in its favour, and I personally am quite convinced of the actuality of materialization. In saying this, however, it must not be understood that I accept the majority of phenomena which have been adduced in its favour; far from it. With few exceptions, every materializing medium whom I have ever seen has turned out, upon investigation, to be an arrant fraud. Nevertheless, such phenomena exist, and I believe that, in the presence of Eusapia Palladino, I have seen materializations of an unquestionably genuine character. I have seen, touched, and felt hands and portions of a living body which have occasionally melted within my grasp. It is my belief that similar manifestations have been seen by others, in the presence of such mediums as Home, Eva C., Willy and Rudi Schneider, etc. Genuine phenomena of the sort may be *rare*, but they are, in my estimation, undoubted. (Carrington 1939:77–78)

As reported by Crookes in his séances with Home (Crookes 1874:92–93), materialization phenomenon reported by psychical research had been known to scientists for at least 60 years before the discovery (in 1932) of the production of electron–positron pairs which manifest in cosmic ray showers in a cloud chamber for less than 1 ms before positron annihilation. The phenomenon of "materialization and dematerialization" in psychical research has nothing to do with the "production and annihilation" of matter and antimatter in physics. Besides, in Braude's more recent book, *The Gold Leaf Lady* (2007), he writes in Chapter 1 about the results of his field research conducted from 1988 to 1990 on a lady in Florida (with the help of psychiatrist and parapsychologist Berthold Schwarz, who informed Braude of the lady's peculiar abilities). The lady apparently spontaneously produces a metal foil on her body. The metal foil has a composition similar to a Dutch metal (an alloy of primary composition 84% Cu + 16% Zn, with a light golden color and excellent ductility). The important point is, if the materialization of the foil is authentic, that the materialized metal is a *real metal* alloy that *does not* dematerialize (hence, it can be subjected to chemical analysis). Although Braude appears confident of the authenticity of this phenomenon on the basis of his own observations, he appears very careful in drawing any scientific conclusions before much more study on this case is done. How these metal foils were produced by the lady, much like the materialization–dematerialization phenomenon, remains to be explained.

And, to quote the previous criticism: "Unexplained cases are simply unexplained. They can never constitute evidence for any hypothesis." However, the act of ignoring physical facts surrounding paranormal phenomena may be similar to the act of ignoring *systematic errors* in scientific experiments, and, from my point of view, the systematic neglect has been conducted intentionally (e.g., see Inglis 1984:Chapter 10) without realizing, or with realizing the threat of, their possible impacts on successful science. The possible impact of the findings of paranormal phenomena on current science is briefly speculated upon in this section to respond to this criticism. With reference to the "one

experience," mentioned in Carrington's quote above, *if and only if this experience is authentic*, would it cast serious doubts on (if not undermine) both (1) the *Big Bang* theory explaining the origin of our material world and (2) the *Darwinian theory of evolution* explaining the origin of the human species. I have the following explanations:

(1) According to the Big Bang theory, our material world is supposed to have originated entirely from pair-production of particles and antiparticles. Somehow, most of the antiparticles mysteriously disappeared from our observations, leaving behind largely ordinary particles from the pair-productions (through some yet-to-be-discovered strong CP violation mechanism or other mechanisms). This theory completely ignores the undeniable materialization phenomenon as recorded in psychical research.

(2) According to the Darwinian theory of evolution, biologically speaking, the human form can be produced only as an infant from a human female after approximately 9 months from impregnation whether or not the method is natural or artificial methods. This theory too ignores the undeniable fact quoted from Flournoy's writing above (i.e. "the revolution which must necessarily follow in our biological ideas").

(3) Using (1) and (2), we can conclude that these two fundamental theories, i.e. the Big Bang theory and the Darwinian theory of evolution, are *dogmatic and biased* because they conveniently neglect the undeniable facts ascertained in psychical research. The loopholes in the above-mentioned fundamental theories can be justified specifically because (a) modern physics, despite its rapid progress, is still grappling with unanswered questions (ten mysteries), including the above-mentioned asymmetry between matter and antimatter (see Kane 2004) and (b) the origin of life on Earth (long after the birth of our apparently life-friendly universe) still needs to be satisfactorily explained (see Dyson 1999, Davies, 1999, 2007) prior to the point that Darwinian evolution of life began all the way up to the human species equipped with ego-directed consciousness.

Such loopholes in the fundamental theories explaining the origin of the universe, life on Earth, developments of human species, and human consciousness may jeopardize further advancements in the field. I would like to close this paper quoting the above-mentioned psychical knowledge:

> Consciousness and matter and energy are one, but consciousness initiates the transformation of energy into matter. (Roberts 1997c:120–121, *Dreams,* Volume 1/Session 882 on September 26, 1979)[12]

Notes

[1] According to a science handbook, the density of mahogany ranges from 0.45 to 1.06 g/cm^3, depending on the moisture content (National Astronomy Laboratory Japan 2003). Mahogany with 11% moisture content has a density of 0.5 g/cm^3.

[2] This is based on a bold assumption. Assuming geometrical proportionality between Figs. 2 and 3 on p. 34 of Crookes (1874), the spring is estimated to expand 64 mm for the maximum weight of 25 lb. If this estimate is roughly correct, then k_{sp} is estimated to be 1740 N/m. The spring constant 980 N/m is for the spring balance (maximum weight 10 kg = 22.0 lb with maximum extension 10 cm) used in the mock-up experiments which were conducted by this author.

[3] The angle of tilt of line *e* − *f* from the vertical line in the quoted sketch is estimated to be $\varphi \approx 52.5°$ and the required H is 66.2 lbf (30.0 kgf) for the angle; however, the angle probably exceeds the maximum angle of friction of the system.

[4] The equation constants (in Equation 7b in the section ***Simulation of Dynamic Behavior of the Lever System***) of natural oscillation of the first lever experiment are: $A_0 = 0.8763$ m, μ (kg) = 2.7216, P (m) = 0.4208, $\varphi_0 = 84.805° = 1.48$ rad, and I_{yrt} (kg·m^2) = 0.6321. Estimated range of the spring constant k_{sp} of Crookes' scale is from 980 to 1740 N/m; corresponding range of natural oscillation frequency is from 5.5 to 7.3 Hz.

[5] One may wonder about the meaning of the detailed numbers estimated below when the uncertainty is definitely large; however, these are shown to record what data are used in the following analyses. The effect of uncertainty on the calculations will be discussed when necessary.

[6] A higher order approximation for ω_n^2 depends on the equilibrium length of the suspension (string plus spring balance in Figure 5), H_0 (22.05″ = 56.0 cm) and its natural length without load, J_0 (21.62' = 54.93 cm for k_{sp} = 980 N/m) as follows:

$$\omega_n^2 = [k_{sp} \times A_0^2 \{1 + (1 - (J_0/H_0)) \times (H_0 \times R_0 + R_0^2)/ A_0^2\} - \mu g \times P \times \cos(\varphi_0)]/I_{yrt}$$
$$\approx [k_{sp} \times A_0^2 - \mu g \times P \times \cos(\varphi_0)]/I_{yrt}.$$

The neglected term in the right side of the ω_n^2 expression above contributes (to ω_n) by less than 0.1% in Crookes' lever.

[7] The effective damping coefficient ($\sigma = 1.0$/s) was determined based on the results of a mock-up experiment conducted by this author for this study. The geometry of the board in the mock-up was approximately adjusted to that of Crookes' board. The total weight of the board was 2.9 kg (cf. 2.62 kg for Crookes' board). The plastic bucket geometry was an average outer diameter of 250 mm, thickness of 2 mm, height of 220 mm, and weight of 9.6 kg when filled with water; these values in Crookes' experiment are estimated to be 235.3, 3, 190.5 mm, and 9.86 kg, respectively. If $\sigma <$ 1.0, then the calculated damping oscillations in Figure 6 are prolonged.

[8] We can probably produce an F(t) history like $F_{psy}(t)$ without damped oscillations, for example, by holding the right end of the lever and carefully moving it up and down using hands, within 3 to 2 mm, for a k_{sp} varying from 980 to 1740 N/m, respectively. However, the lever is no longer a harmonic oscillator under such a condition.

[9] Mechanical interaction between a tank wall and sloshing liquid within the tank is a modern engineering problem, like that posed by a large oil tank in the event of an earthquake.

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[11] This quotation of Flournoy by Carrington was made from the former's writing (Flournoy 1911:220−222) based on the paper by Professor of physiology Charles Richet (1850−1935) published in *The Annals of Psychical Science* (October and November, 1905).

[12] From the book *Dreams, "Evolution," and Value Fulfillment, A Seth Book,* Volume One © 1997, Jane Roberts. Reprinted by permission of Amber-Allen Publishing, Inc. P. O. Box 6657, San Rafael, CA 94903. All rights reserved.

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RESEARCH ARTICLE

The Implications of Near-Death Experiences for Research into the Survival of Consciousness

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Abstract—It is generally supposed by psychical researchers that (a) evidence suggestive of consciousness surviving bodily death would always be compatible with the so-called "super-psi hypothesis," according to which living-agent psi is wholly responsible for the evidence, and (b) in terms of their respective scientific merits the super-psi hypothesis and the survival hypothesis are on a par. This has produced an explanatory impasse regarding survival evidence. In this paper, I argue that certain kinds of near-death experience cases challenge the premises and the parsimony of the super-psi hypothesis. I argue that from this base the general explanatory impasse between the super-psi hypothesis and the survival hypothesis can be broken.

Keywords: near-death experiences (NDEs)—super-psi—survival—consciousness—mind–body dualism

Introduction: The Super-Psi Challenge

In a recent essay in this *Journal*, Emily Kelly called for a revival in mediumship research, in the spirit of psychical research (2010). She pointed out that psychical research originated in the late 19th century in an attempt to answer first the broad question of whether mind or consciousness is solely a product of the brain, and second the narrower (but related) question of whether human personality survives bodily death (Kelly 2010:247). Both of these questions are still unanswered today.[1] Psychical research represents a distinct interest from that of parapsychology, which is concerned with the narrower objective of the experimental investigation of psi (Kelly 2010:279, note 1), "psi" being a generic designator for cognitive or influential capacities that are not mediated by the known bodily channels (Thouless & Wiesner 1948). Cognitive psi is also known as "extrasensory perception" or ESP, and influential psi is also known as psychokinesis or PK. The research interest in studying psi originated in

psychical research, on the premise that understanding the nature of psi would be of some help in answering the two core questions. At present there is very good evidence for the existence of psi (Braude 1986, 2002, Kelly, Kelly, Crabtree, Gauld, Grosso, & Greyson 2007, Radin 1997), but its nature is still unknown.

The question of whether consciousness can be accounted for in physicalistic terms is currently the subject of a very substantial debate in philosophy of mind, supplemented by research in neurology, psychiatry, and cognitive psychology. This debate is still very far from developing converging points of view (Crane 2001:viii, Heil 2004:129, Searle 2005:1,6). Jaegwon Kim has since the 1990s been arguing that the mainstream view is logically inconsistent (e.g., 2006:290–299), and some claim that this debate has reached an impasse (Burge 2010:236, note 4, Heil 2003:2). Kim has recently argued that the commitment to the world being "at bottom" exclusively physical in nature renders the puzzle of consciousness insoluble (Kim 2008:271). Carl Gillett similarly argues that the mainstream view, which he calls *Standard Non-Reductive Physicalism*, "is such that we cannot even imagine how it could be true" (Gillett 2010:27).

In this context, research into whether consciousness can survive the death of the body (hereafter, "survival research") may be of seminal importance, since it may show the way in which mainstream scientific premises have to be extended if the nature of mind and consciousness is to be understood. However, the survival question has largely disappeared from mainstream research, both philosophically and scientifically. As Stephen Braude reports, "only a handful of philosophers have taken both a serious and a well-informed interest in the conceptual problems of survival research" (2009:200). Early empirical investigation into the survival question was dominated by research into the phenomena of trance mediumship, and a vast amount of high-quality evidence was amassed in the first 50 years or so (Gauld 1982, Grattan-Guinness 1982, Griffin 1997). However, this research pretty much ground to a halt in the middle of the 20th century because researchers found themselves unable to adjudicate between the hypothesis that discarnate entities were involved in the production of mediumistic phenomena ("the survival hypothesis") and the alternative view that the psi of living agents was wholly responsible for it ("the super-psi hypothesis") (Cook 1987, Gauld 1961). This impasse still stands today (Kelly 2010:251). Braude recently (2009:209) reiterated his long-standing view (1992, 2003) that it may be impossible to defend the survival hypothesis against the super-psi hypothesis.

Some have argued that the super-psi hypothesis is unfalsifiable (e.g., Almeder 1992:51–53), but according to Braude it is not invulnerable. According to him it is not that nothing can count against the super-psi hypothesis, but that the evidence we have in hand is not good enough to break the impasse. Unfortunately, evidence of the right sort is unlikely to be forthcoming (Braude

2003:18–20). Braude has gone to some lengths to explain what sorts of evidence would be compelling (e.g., 2003:Chapter 9, 2005). He has argued that for cases sufficiently rich in detail we would be rationally warranted to rule against the super-psi hypothesis even though the evidence would still be compatible with the super-psi hypothesis (2003:17–18). However, he recently said that we still do not have any such cases (2010).

However, finding such cases is not necessarily the only way in which the super-psi hypothesis can be challenged. I will argue below that given the commitments of the super-psi hypothesis it is possible to see how there can in fact be counterexamples to it, and then go on to argue that certain NDE cases present such counterexamples.

The Super-Psi and Survival Hypotheses Compared

To set the stage for the discussion to follow, I will first explain the rationale behind the super-psi hypothesis, and then compare the main commitments of the super-psi hypothesis with those of the survival hypothesis. Following that I will show how the impasse between the hypotheses arises.

The Nature of the Super-Psi Hypothesis

In general terms, the super-psi hypothesis works like this. There is good evidence that people have informational and influential faculties that are not mediated by the known bodily channels, and that may be in principle beyond explanation in purely physicalistic terms. This is normally referred to as "psychic ability" or "psi" (for accessible overviews, see Braude 1986, 2002, Broughton 1991, Radin 1997). This capacity appears to be widespread (Cardeña, Lynn, & Krippner 2000:222–223, Haraldsson 1985) but typically very weak (Palmer 1979, Thalbourne 1994, 2004), especially under test conditions, where it is often undetectable (Irwin & Watt 2007:4,6,7). Nevertheless, certain individuals ("psychics" or "mediums") normally have strong abilities of this kind (see for example Barrington, Stevenson, & Weaver 2005, Méheust 2003), and *under special conditions*, typically crises, average people can have powerful psychic experiences, too (see for example Gurney, Myers, & Podmore 1886). The inference is drawn that most people, if not all, have potentially powerful psi, but that for most this ability is normally inhibited (or only used in inconspicuous ways), although it can manifest more powerfully (or more openly) under certain conditions, typically mortal danger or deep psychological stress. Just like any other faculty, the use and focus of this ability is determined by an agent's needs and interests. For this reason, Braude calls this the "*motivated* psi hypothesis" (2003:13). The implication is that if people have a sufficiently strong motivation, they can manifest psi powers to an extraordinary degree. This is typically called

"super-psi" in contrast to "ordinary psi," but as Braude argues it is really just the normal psi faculties somehow unleashed (or unmasked) by the agent's strong needs or interests.

The idea is therefore that if people's psi can manifest powerfully to collect information or cause effects that would serve their deep or intense needs, then if they really yearned to believe in survival of consciousness (e.g., in their grief over a deceased child), their psi could subtly obtain relevant veridical information from this-world sources, and present it to them in such a way as to give them the reassurance they are yearning for. The argument now goes that, granted this possibility, and given evidence ostensibly indicating survival of consciousness (e.g., apparent communication with a deceased person via a medium or in a dream), it is not possible to decide whether the origin of the experience lies in the super-psi of the needful agent or in the actions of a discarnate entity. This indecision is rendered the more acute by how difficult it is to identify people's "real" drives and motivations, since so many of these are unconscious and may even be contrary to what they themselves consciously think their needs and drives are (if they think consciously about them at all).

The only philosopher to have subjected this argument to substantive scrutiny in the light of a wide range of relevant evidence is Stephen Braude, in his book *Immortal Remains* (2003). He concluded that philosophical considerations and the evidential weight support a rational hope of survival, but not much more than that—he only grants "believing" with "little assurance but with some justification" that *some* survive for *some* time (Braude 2003:23,306).[2] The reason for his carefully caveated conclusion is that the super-psi hypothesis appears to be, on the wide range of evidence he considered, ineliminable and indefeasible. His analysis was focused, however, on the historically dominant areas of survival research, which is represented largely by cases of mediumship and ostensible reincarnation or possession (jointly six chapters), hauntings (one chapter), and out-of body experiences (OBEs) (one chapter). The more recent field of NDE research was only given just over five pages (as a subsection of the OBE chapter). Near-death studies is, however, a rapidly growing field of research, and I would suggest that developments in this area provide scope for a reassessment of Braude's conclusions.

The Super-Psi and Survival Hypotheses Summarized

The two competing hypotheses can now be stated more succinctly, and their commitments compared, as follows:

The Super-Psi Hypothesis: *All evidence suggestive of survival is the product of powerful subconscious psychic activity by living agents, mobilized and guided by deep-seated psychological needs.*

The super-psi theorist is obviously committed to the existence of psi, but as it stands the super-psi hypothesis does not commit the super-psi theorist either way concerning the existence of souls or survival. However, it does rule out a belief in discarnate souls that can interact with the world of the living ("discarnate interactionism").[3] So the super-psi theorist may in fact be sympathetic to the ideas of dualism and/or survival, but just not think that discarnate interactionism is the best explanation for the evidence suggestive of survival.

The Survival Hypothesis: *The best cases representing evidence suggestive of survival derive mostly from interaction between living people and the discarnate souls of formerly living persons.*

As it stands, the survival hypothesis commits the survival theorist ("survivalist") to the following:

1. Living persons are composed of a physical part ("body") and a supra-physical part (a "soul");
2. The soul survives the death of the body;
3. The soul is the essential bearer of the person's mental and psychic properties;
4. Discarnate souls ("spirits") can via their psychic abilities interact with living people and the ordinary physical world.

The survivalist is not in a position to regard the super-psi hypothesis as unlikely. On the survivalist model, discarnate persons are just souls, and therefore the commitment to discarnate interactionism entails that souls have psychic powers. By implication, the survivalist must then hold that incarnate persons have psychic powers because souls have them. Absent a theory about how psi operates, and how embodiment affects the soul's psychic powers, it is reasonable to suppose that living people could have psychic powers that are in principle of similar power to those of discarnate souls. The survivalist has therefore to be sympathetic to the idea that something like the process implied by the super-psi hypothesis is plausible, and may in fact sometimes occur, at least to some degree. And in practice this is apparently how things turn out—Sudduth has argued that in the case of mediumship evidence the power and complexity of psi demanded by discarnate interactionism (in accordance with the survival hypothesis) is of the same order as the living-agent psi required by the super-psi hypothesis, and he presented evidence that psi among the living is at least sometimes a component of mediumship evidence (2009:184,169).

Although the reasonable survivalist cannot reject the plausibility of the super-psi hypothesis, or that living-agent psi accounts for some of the evidence, they must nevertheless hold that the best explanation for the survival-suggestive evidence is that some interaction with discarnate souls is involved.

The Nature of the Impasse between the Survival Hypothesis and the Super-Psi Hypothesis

Theories have qualities philosophers of science call "theoretical virtues." These are qualities good theories typically have, such as explanatory power, falsifiability, parsimony, predictive powers, and coherency. These qualities are especially valuable in cases where we have to choose between theories with different underlying assumptions but which explain the data equally well (Harman 1965, Lipton 2000, 2004, Thagard 1978) (this is a common occurrence in science).

The impasse between the super-psi and survival hypotheses really exists not because they are equivalent in the virtue of explanatory power, but because when they are compared in terms of other theoretical virtues neither has a decisive overall advantage.

The super-psi–survival virtue tradeoff discussions given in the literature vary widely in depth and clarity, but the classic text is Braude (2003), supplemented by Braude (2009) and Sudduth (2009). Largely following these sources, the tradeoff seems (briefly) to play out like this:

1. **Empirical adequacy**: So far, the super-psi and the survival hypotheses both account for all the known data, so on empirical adequacy we have a draw (see, e.g., Braude 2003:20, but there seems to be widespread agreement on this).
2. **Ontological conservatism**: The super-psi hypothesis draws only on claims for psi powers and psychological mechanisms that there are already ample independent evidence for (Sudduth 2009:168), but the survival hypothesis assumes the existence of discarnate souls with psi powers, which we do not have independent evidence for outside the context of survival-suggestive evidence (Sudduth 2009:189). In terms of ontological conservatism, the super-psi hypothesis has the advantage.
3. **Explanatory simplicity**: The super-psi hypothesis produces very complex explanations, while the survival hypothesis produces very straightforward explanations. Advantage to the survival hypothesis (Braude 2003:216,305).
4. **Causal interference**: Causal exchanges are subject to modulation and interference by other causes operative at the same time. The super-psi hypothesis requires navigation of a very complicated causal nexus, but the survival hypothesis requires only that a single connection be made and hung on to. Braude regards the survival hypothesis as having the better chance of succeeding as far as causal interference is concerned (2003:305–306). Sudduth has pointed out that for some mediumship

cases the hurdle is similar (2009:179–181), but this is not enough to suggest that it is always similar, so the advantage to the survival hypothesis stands.

5. **Antecedent probability**: Antecedent probability of each hypothesis being right is about the same, since we have multiple lines of evidence for survival but also a very substantial database on living agent psi (Sudduth 2009:302–303).
6. **Plausibility**: The strength of psi required by either hypothesis is about the same (Braude 2003:20, Sudduth 2009:184), so the survival and the super-psi hypotheses are equally plausible.
7. **Actuality**: David Lund (2009:149–150) argued that the logical possibility of the super-psi hypothesis does not mean that that is what is actually going on in these cases, and nothing has been said to defend that super-psi is what is actually going on. However, the converse is also true: The survival hypothesis equally stands in need of a defense that something like discarnate interactionism is actually going on in these cases. Neither side seems able to do either without begging the question, so we have another draw. Sudduth has shown that some of the information in mediumship cases comes from psi among the living (Sudduth 2009:169, note 4,190), but this is not sufficient to show that this is superpsi at work or that nothing else is going on, so the draw stands.
8. **Minimal ad hocness**: Both the super-psi and the survival hypotheses appear to be compatible with all the data, but neither can be used to produce testable predictions (Braude 2003:18–19). Since they were each invoked specifically to explain the given dataset, and neither has explanatory or predictive value beyond the dataset they were set up to account for, they are both ad hoc hypotheses.
9. **Falsifiability**: Both the super-psi and the survival hypotheses are regarded as unfalsifiable in a strong sense, since they do not generate testable predictions. Braude has pointed out there may nevertheless be rational grounds for ruling against the super-psi hypothesis if we obtain sufficiently detailed cases (Braude 2003:17–19), and as Steven Hales has pointed out there may be rational grounds for ruling against the survival hypothesis, for example if we discover that mischievous but technologically advanced aliens are behind the survival-suggestive evidence (Hales 2001:342). However, these would be pragmatic decisions about specific cases and not a general refutation of the possibilities entailed by the respective hypotheses. Another draw.

There are many other virtues, but these are not normally brought into the

super-psi vs. survival discussion because neither hypothesis has any of them. I know of at least another 16 theoretical virtues that here play out in that way, for example neither hypothesis has any predictive power (since they are both ad hoc), neither fits into the mainstream scientific framework (because they are both committed to the existence of psi), both challenge fundamental scientific assumptions (again because of their commitment to psi), neither has theoretical depth (since we have no theory of either souls or psi), neither has relevance for areas of knowledge outside the survival-suggestive evidence, and so on.

In terms of the virtues mentioned, numbers 2 and 3 cancel each other out. Apart from number 4, everything else is a draw already. We can draw up a scorecard where we award a point to a hypothesis for each virtue it has, except when its rival is better in which case it gets a zero. From the list discussed above, the scorecard would appear as given in Table 1 below.

Table 1 shows that the survival hypothesis has a slight advantage, as Braude concluded in *Immortal Remains* (2003:306). However, this advantage is not worth very much. First, both hypotheses are weak, since in principle at least 25 criteria are at stake. In percentage terms, the score ratio is therefore something like 16%:20%. Second, as Braude and others have argued, the virtue of explanatory simplicity is somewhat suspect, since explanatory simplicity in one part of a theory is often later offset by complexity somewhere else

TABLE 1
A Simple Theoretical Virtues Scorecard

Theoretical Virtue		Score	
		Super-Psi Hypothesis	Survival Hypothesis
1	Empirical adequacy	1	1
2	Ontological conservatism	1	0
3	Explanatory simplicity	0	1
4	Causal interference	0	1
5	Antecedent probability	1	1
6	Plausibility	1	1
7	Actuality	0	0
8	Minimal ad hocness	0	0
9	Falsifiability	0	0
10–25	Unconsidered criteria	0	0
	Total score	***4***	***5***
	Possible score	*25*	*25*

(2003:301). If this is right, the ratio is 16%:16%. Either way we have an impasse (or something near enough) between two weak theories.

The evaluation given in Table 1 is of course debatable. The scoring was very coarse, the virtues were not weighted relative to each other, they are not completely independent of each other, and there are other virtues that have not been mentioned. The issue of weighting is a particular concern, since some theoretical virtues are more important than others, for instance virtues such as empirical adequacy and ontological conservatism are usually given more weight than for example explanatory simplicity. As things stand, applying such weightings would favor the super-psi hypothesis. On Sudduth's remarks, we might also regard number 4 as being closer to a draw than giving a clear advantage to survival hypothesis. The super-psi hypothesis would then be favored even more, and likewise the draw in number 7 could be regarded as a tradeoff giving a slight advantage to the super-psi hypothesis. Given what is at stake, and given the great uncertainties that remain, these slight differences are probably not enough to break the impasse. However, as things stand the super-psi theorist is probably entitled to feel more optimistic about the potential of the super-psi hypothesis to win out than the survivalist is entitled to feel about the prospects for defending the survival hypothesis.

Most researchers favor one of the hypotheses, but granted the virtue balance (or something near enough) they presumably derive their position from other grounds, such as religious convictions, intuition, personal experiences, different views on how to assign weights to different theoretical virtues, or even inadequate information. However, the objective for anyone in the debate must be to try to find a way of breaking the impasse in a *generally acceptable* way. Limited options are available for doing *that*. Possibilities include trying to find arguments or evidence that breaks the draw on some of the virtue tradeoffs, or introducing arguments around unconsidered virtues that may be differentiating (e.g., predictive power or vertical depth), or arguing for weighting the virtues so that the differentiating ones do not overall end up cancelling each other out. However, given that not everyone views the scorecard in quite the same way, and that in any case the variety and relative weights of theoretical virtues are not settled in the philosophy of science (Maxwell 2004), the impasse probably cannot be decisively broken without developing a range of discriminating advantages.

Falsifiability Reconsidered

As mentioned above, the super-psi hypothesis is widely regarded to be unfalsifiable in a strong sense, on the basis that it does not entail any predictions we can test. It is furthermore supposed that for all evidence suggestive of survival, it would always be possible to construct a super-psi–based theory that is compatible with the given evidence, since we do not know how powerful psi

could be, nor how subtle or covert it could be, and we cannot hope to fathom all the unconscious mediating processes that may be involved. On this basis it has been argued that perhaps the only way to rule against the super-psi hypothesis would be to find cases that are so rich in details that it would be unreasonable (perhaps even irrational) not to rule against it (Braude 2003:17–19).

However, in my view there is another way to challenge the super-psi hypothesis, by identifying cases that challenge its premises or ontological parsimony. The super-psi hypothesis is grounded in the conjunction of two very specific claims, namely that super-psi is motivated by deep-seated needs or intense emotions, and that it operates to produce compensatory or mitigating outcomes. This suggests that at least in principle it may be possible to find cases where psychological motivations needed to mediate super-psi are clearly absent, or where the outcomes are contrary to how things would be if super-psi were involved (e.g., the psychological stresses are not mitigated, and may even be enhanced). Note that such a counterexample would not count against the existence of super-psi, but only against it being the source of the survival-suggesting evidence in that given case. However, for these cases it may be impossible to develop explanations that maintain the super-psi hypothesis's neutrality about the existence of souls.

I will argue below that such counterexamples exist.

NDEs and the Super-Psi Hypothesis

Background

In my view, evidence that is crucially telling for this debate is becoming available from the new field of academic research called near-death studies that was triggered by the publication of Raymond Moody's book *Life after Life* in 1975. This research is centered on near-death experiences (NDEs). NDEs are "profound psychological events with transcendental and mystical elements, typically occurring to individuals close to death or in situations of intense physical or emotional danger" (Greyson 2000:315). However, qualitatively equivalent experiences occur under non-crisis conditions as well. Near-death experiencers (NDErs) typically report an out-of-body experience (OBE), a transition to another realm, and encounters with a being of light or a spirit guide. Often there is a life review, encounters with deceased relatives, a barrier or limit, and a decision to return to the body (Moody 1975, Zingrone & Alvarado 2009).

NDEs are not rare phenomena—survey studies indicate that the incidence may be about 4% of the general population (Gallup & Proctor 1982, Knoblauch, Schmied, & Schnettler 2001). Tens of thousands of cases have been collected by researchers. Three important case archives have been established: one at the Religious Experience Research Centre, based at the University of Wales Trinity

Saint David; one at the Division of Personality Studies of the University of Virginia; and one at the Near-Death Research Foundation. These archives hold approximately 4,000 case reports. More than 65 research studies involving nearly 3,500 NDErs had been published by 2005 (Holden, Greyson, & James 2009:7).

Mainstream neuropsychiatry appears to be stumped in terms of explaining NDEs (Greyson, Kelly, & Kelly 2009, van Lommel 2010:113–134), opening up the possibility that important discoveries may result from deeper investigation of NDEs. Of particular importance in this regard are the cases where people report having conscious experiences under conditions of cardiac arrest. Such experiences are reported by 10%–20% of the people who survive cardiac arrest (Greyson 2003, van Lommel, van Wees, Meyers, & Elfferich 2001, Parnia, Waller, Yeates, & Fenwick 2001, Schwaninger, Eisenberg, Schechtman, & Weiss 2002).

According to mainstream medical and philosophical paradigms, such experiences cannot happen. Cardiac arrest is a physiologically brutal event, which leads within 10–20 seconds to a state called clinical death, with no heartbeat, no breathing, no detectable electrical activity in the brain, and no brainstem reflexes (Fenwick & Fenwick 2008:206, Greyson 2010a). It is a very serious condition to be in, and only about 10% of people who suffer a cardiac arrest survive it (Ballew 1997, Nichol, Thomas, Callaway, Hedges, Powell, et al. 2008, Peberdy, Omato, Larkin, Braithwaite, Kashner, et al. 2008). Without medical intervention, cardiac arrest typically leads within five minutes to the onset of irreversible brain damage (Safar 1988), and within ten minutes to actual death (Kaplan 2007, Safar 1988). The signs of clinical death and actual death are the same; the difference is merely that patients in a state of clinical death can be revived with appropriate medical attention. For this reason, researchers sometimes refer to these two conditions as "reversible death" and "irreversible death."

If consciousness does persist during cardiac arrest, it would clearly be of great significance for the current academic debate about the nature of mind and consciousness, as many researchers have pointed out, for example:

> This conflict between neuroscientific orthodoxy and the occurrence of NDEs in conditions of general anesthesia or cardiac arrest is profound and inescapable . . . only when neuroscientists examine current models of mind in the light of NDEs will we progress in our understanding of consciousness and its relation to the brain. (Greyson, Kelly, & Kelly 2009:234)

Similar views are expressed by Kelly et al. (2007:421), Greyson (2007), and van Lommel (2010:158). These assertions are not lightly made, as explained below.

First, there is a substantial body of cardiac arrest NDE cases. By 2007, more than one hundred had been reported in the scholarly literature (Kelly et al. 2007:418), and many more have been reported since (Holden 2009, van Lommel 2010, Rivas & Dirven 2009).[4]

Second, the credibility of these reports is strongly reinforced by their high accuracy. In a recent review, Janice Holden has found that 90% of NDE reports of perceptual experiences during cardiac arrest or prolonged respiratory arrest contain no errors (2009:196). About 35% of these reports have been independently corroborated (Holden 2009). In contrast, Penny Sartori found that cardiac arrest survivors who did not report NDEs were unable to make accurate guesses as to what happened during their resuscitation (2005:292).

Third, some cardiac arrest NDE reports include very unusual incidents that occurred during the crisis, further strengthening their credibility, and also reinforcing the claim that these reports represent contemporaneous experiences (e.g., Cook, Greyson, & Stevenson 1998:388–390, van Lommel, van Wees, Meyers, & Elfferich 2001:2041, Moody & Perry 1988:18–19,19–20, Morse & Perry 1993:201, Ring & Cooper 1999:18–21, Ring & Lawrence 1993:226–227,227,227–228, Sharp 1995:3–16).[5]

Fourth, several cases include veridical reports of incidents that occurred beyond the range of the ordinary bodily senses, which substantially raises the stakes against finding orthodox interpretations (Cook, Greyson, & Stevenson 1998:388–390,392–395, Moody & Perry 1988:18,19–20,172, Ring & Lawrence 1993:226–227, Sharp 1995:3–16).

On the weight of the evidence cited above, most NDE scholars are now convinced that consciousness, identity, memory, and perception can function while the body is clinically dead (Holden 2010:363). But these functions must be the functions of something, and if not of the body then necessarily of something else. So this conviction amounts to a conviction that "minds" are distinct things aside from bodies, that is, a conviction that some kind of mind–body substance dualism is true. Substance Dualism is among the least popular mind–body models in current philosophy, largely due to a mistaken but widely held view that substance dualism equates to *Cartesian* Substance Dualism, which is not only a supernaturalistic doctrine but actually an incoherent one (see, e.g., Dennett 1991:35, Kim 2001). However, it is possible to frame substance dualism in a way that escapes the problems inherent to Cartesian Dualism, as some monists admit (e.g., Lycan 2009). For recent examples of Non-Cartesian Dualisms, see for example Beck (2008), Hart (2009), von Kutschera (2008), Meixner (2008), and Thompson (2008). It should also be kept in mind that such substance dualisms need not be ultimate theories, but may represent a contingent state of affairs grounded in an underlying Neutral Monism. Such models, which combine "functional dualism" with "energy monism," have

been proposed, for example by Mark Woodhouse (1996:Chapters 5,6) and Ervin Laszlo (2004:111–113).

Within the database of NDE cases, cardiac arrest cases are relatively rare, representing less than 1% of all documented cases. However, a useful connection can be made with the wider database, in the following way.

Phenomenological analyses indicate that there are no significant qualitative differences between the experience reports from the cardiac cases and NDEs triggered under different circumstances such as birth trauma, surgical emergency, accidents, suicide attempts, etc. (Fenwick & Fenwick 1995:158–159, Fox 2003:98–103, Greyson & Stevenson 1980, Greyson 1991, 2007). This suggests that all NDEs are mediated by a common mechanism, which can be "activated" in multiple ways.[6] This is a valuable connection to be able to make since it dramatically increases the phenomenological database we can draw on for theory-building.

The conviction that NDEs provide support for a dualistic view is reinforced by further evidence that suggests that individual "minds" can survive the irreversible death of the body by a significant period, as discussed in the next section.

NDEs as Evidence Suggestive of Survival

Persons who are dying while conscious often experience visions in which they report the presence of previously deceased family members or friends (Barrett 1926, Fenwick & Fenwick 2008, Kelly et al. 2007:390–391). Similar encounters occur in NDEs: About 21% of NDE accounts include reports of encounters with deceased persons (Greyson 2010b). In a survey of 74 NDE cases, Emily Kelly found 129 reports of encounters with spirits, mostly representing a previous generation (81%). Only 16% were from the same generation as the NDEr (e.g., siblings or spouses), and 2% were from the next generation (e.g., daughters or nephews) (Kelly 2001). Kelly has pointed out that while such experiences might conceivably be triggered by expectations of imminent personal death, this hypothesis seems less plausible when the visions include deceased persons other than the one(s) the experiencer would wish to see and/or visions of unidentified deceased persons (Kelly 2001). Even more challenging for the "expectancy hypothesis" are cases with special veridical attributes, such as the so-called "Peak in Darien" cases,[7] reviewed in some depth by Bruce Greyson (2010b). There are three kinds of these cases, namely:

(a) cases in which the person seen had died up to a week before the vision, although that death was apparently unknown to the experiencer;
(b) cases in which the person seen actually died at the time of, or immediately before, the vision, allowing no normal possibility for the

experiencer to have learned of the death;

(c) cases in which the deceased person seen was someone whom the experiencer had never known, but could afterward identify (e.g., from old photographs).

As Greyson reports, such cases are scattered throughout the literature. Although they are often inadequately documented, they are by no means rare. Fenwick and Brayne report six cases in a single recent study (Fenwick & Brayne 2011). Greyson reviews 28 cases: 15 examples of the first type (including two NDE cases, one of them a cardiac arrest case), nine of the second type (including four NDEs, two of which were cardiac arrest cases), and four of the third type (all four NDEs, two of them cardiac arrest cases) (2010b).

Of the 28 cases, ten also included encounters with persons who had been dead for years or decades (at least 16 encounters). Two of *these* cases were NDEs, one of which was a cardiac arrest case.

The veridical aspect of "Peak-in-Darien" cases suggests that these experiences cannot have a purely psychological origin, but must involve psi in some way. However, given the super-psi hypothesis presented above, it would appear undecidable whether these experiences really represent encounters with discarnate souls or are in fact merely need-serving hallucinations mediated by living-agent super-psi. The next sections will take a closer look at the forms the super-psi hypothesis must take to account for NDEs, and consider whether it is really the catchall solution it is generally assumed to be.

The Super-Psi Hypothesis in Relation to NDEs

The NDE evidence contains not only cases strongly suggestive of survival (e.g., "Peak in Darien" cases) but also cases strongly suggestive of mind–body dualism (e.g., lucid consciousness and veridical perception during cardiac arrest). Some "Peak in Darien" cases are also cardiac arrest cases, so carry both suggestions.

As discussed above, the super-psi hypothesis is neutral with regard to whether dualism and survival is true or not, rejecting only discarnate interactionism. In this way the super-psi hypothesis rejects that the survival-suggestive evidence counts in favor of the existence of souls (dualism) or their endurance beyond the death of the body (survival). It is an important theoretical virtue of the super-psi hypothesis that it can be used to construct explanations for the survival-suggestive evidence that do not involve claims about the existence, endurance, or interaction capabilities of souls. In order for the super-psi hypothesis to preserve its ontological conservatism when constructing explanations for NDE cases, it must therefore be set up in such a way that it blocks not only the inference to survival but also the inference to dualism.

This can be done, but it is severely constraining. It has been suggested (but not by super-psi theorists, as far as I know) that the cardiac arrest NDEs might involve precognitive or retrocognitive impressions that occur just before or just after the arrest period, such as would create the impression of continuity of consciousness during the cardiac arrest. In this scenario the subjects really are completely unconscious during their cardiac arrest, just as orthodox medical models would suggest. However, they would not be able to tell, since they have no experience of unconsciousness, and the experience they *did* have reflected the events that actually occurred during the arrest period. The experience would be convincing to them, but in fact it would be a false impression created as a psychological coping mechanism that deals with their fear of dying. By choosing the timing of the living-agent psi in this way, neither dualism nor survival is required to explain the data.

There is good evidence that people can have powerful psychic experiences during times of stress or danger (Gurney, Myers, & Podmore 1886, Sidgwick, Johnson, Myers, Podmore, & Sidgwick 1894), and we have good evidence for the existence of both precognition and retrocognition (Radin 1997), so this does not seem a far-fetched hypothesis. However, on closer consideration this proposal must be refined even further, in the following way.

The first thing to note is that the people who have NDEs are in no way exceptional individuals before their NDE. There appear not to be any personal or demographic factors that would make one more prone to having an NDE. As a group, NDErs appear to be psychologically healthy individuals and to be similar to comparison groups in age, gender, race, occupation, religion, religiosity, education, and mental health (Gabbard & Twemlow 1985, Greyson 1991, Irwin 1985, Ring 1980a, Sabom 1982). NDErs are not unusual in terms of measures of intelligence, neuroticism, extroversion, trait and state anxiety, and relevant Rorschach measures (Locke & Shontz 1983). The evidence also indicates that as a group NDErs are, before their NDE, no more prone to psychic experiences than the average population (Greyson 2003, Kohr 1982, 1983, Sutherland 1989). Some studies have suggested that NDErs remember their dreams more often, and are adept at using mental imagery (Irwin 1985), and have good short-term memory (van Lommel 2004). However, the nature of these traits suggests that these individuals report more NDEs because they are better able to remember and describe their experiences, rather than that these traits make them more prone to having an NDE.

For the present argument, the important implication is that NDErs are neither psychologically nor psychically unusual. They have the same motivations and fears people typically have, and do not have unusual psychic abilities (at least before their NDE). If NDEs are powerful precognitive or retrocognitive events, then they are indeed super-psi phenomena, that is, produced via the normal psi

of ordinary people put powerfully into play in service of a deep need or fear, such as fear of dying.

Fears related to dying are plausible as typical triggers for such super-psi events. 34% of Americans who have a close brush with death report associated "experiences" (Gallup & Proctor 1982). This is higher than Americans' admission of fear of death as such, which ranges from about 20% for late teens to under 7% for those over 65 (Pinkus, Richardson, & Amet 2000), but lower than the number who fear a painful death (67%), or leaving loved ones behind (65%), or dying alone (43%) (Yankelovich Partners 2000).

If cardiac arrest NDEs actually occur precognitively or retrocognitively, this raises the question of just when they do occur. There is a good case for ruling out the transitional phases during which consciousness is lost or restored. At the onset of a cardiac arrest there is a very rapid transition to unconsciousness, and at normal body temperatures cerebral iso-electricity is reached in about 11 seconds. It is implausible that this rapidly declining brain activity can support the rich coherent narrative experiences afterward reported. After resuscitation, the recovery to full consciousness is slow and confusional, as it takes time for blood pressure to rise back to a level where the brain is properly oxygenated. This confusional state is very different from what is needed to support the clear coherent narrative structure of NDEs (Fenwick & Fenwick 2008:206–210). For present purposes, the implication is that if cardiac arrest NDEs are mediated by super-psi, they have to happen outside these transitional phases. However, if a retro-cognitive event occurred after the confusional recovery phase, there would be confused memories, but memories nonetheless, of this confusional period between the arrest and the retrocognitive event, which would destroy the illusion that the NDE occurred simultaneously with the arrest. A precognitive event immediately before the transition to unconsciousness would be convincing, however, since there is no intervening memory to give away the temporal asynchrony, and the alignment between the contents of the experience and the actual events would make them seem synchronous. So the overwhelming likelihood is that if NDEs are super-psi–mediated experiences then they are precognitive episodes. This has the implication that the NDE must be triggered in relation to deep fears or interests that the subject already had *before* the event, and not by concerns that are raised by the recognition that such an event has occurred or the actual shock to the system the event produces. The NDE therefore serves to provide reassurance in relation to these pre-existing concerns, not event-triggered new concerns.

In sum, a super-psi hypothesis that can explain cardiac arrest NDEs while remaining neutral about dualism and survival must be based on precognitive super-psi motivated by antecedently existing concerns. As I argued above, it is reasonable to suppose that the same sort of mechanism underlies all NDEs,

since they are phenomenologically consistent across different contexts. The implication is that the super-psi hypothesis would in general view NDEs as precognitive experiences motivated by antecedently existing concerns.

NDEs of Young Children

The NDEs of children have been much studied (e.g., Atwater 2003, Morse & Perry 1990, Sutherland 1995), and they are phenomenologically very similar to adult ones (Long & Perry 2010:137, Sutherland 2009:89). Children report them from all ages, and some even retrospectively report NDEs that occurred to them before they were able to talk (Sutherland 2009:92–93).

It has been suggested, for instance by Elisabeth Kübler-Ross, that young children do not understand death in the same way as adults do, and hence do not have death anxieties (1997). Titus Rivas has recently suggested that because children do not fear dying, they would not have the motivation needed to trigger a super-psi episode to help them cope with a life-threatening crisis (2010:358–359). Unfortunately things are not so straightforward. Children do have *some* understanding of death, since for example the deaths of grandparents and pets (their own or others') have to be explained to them (Badham & Badham 1984:80). So, they can probably recognize a death event, even if they do not fear it. More importantly, their parents will have been training them from the outset to keep themselves safe, and this would involve training them to recognize and avoid mortally dangerous events. It is therefore very plausible that children would be very aware of the distress that their own deaths would cause their parents, and this deep need to please, or at least not distress, their parents, could, in the face of an impending physical crisis trigger an experience of continuity of consciousness, whereby they can (afterward) reassure their parent that they were not actually dead or dying. If this were the case, one would expect the experience to take the form it does for adults, which it does. This proposal is akin to what Braude calls the *multiple process* super-psi hypothesis sometimes posed in relation to mediumship evidence (2003:11). Given the present context, call this the *child* super-psi hypothesis.

There is an alternative version of this. Perhaps the child's super-psi is not in play, but the *parent* precognitively senses the imminent danger, their super-psi collects the veridical information about the future event, and their super-psi stimulates the child's hallucinatory experience of continuity of consciousness and veridical perception, so the child can reassure the parent (afterward) that they were not actually dead or dying. If *this* were the case, one would also expect the experience to take the form it does for adults, as it does. This proposal is akin to what Braude calls the *magic wand* super-psi hypothesis sometimes posed in relation to mediumship evidence (2003:11). Given the present context, call this the *adult* super-psi hypothesis.

If the adult super-psi hypothesis was on the right track, preverbal children should not have NDEs, since they would be unable to assuage their parents' fears until many months or even years later. If such cases existed, the adult super-psi hypothesis would be a non-starter, since the parents' needs are *not* served by the child's experience, since they cannot know of it (until much later).

If the child super-psi hypothesis was on the right track, children would not have NDEs until they are old enough to understand what a mortally dangerous event is, and to understand the deep concern their parents feel for their safety. If cases predating this developmental stage existed, the need or fear required to activate the child's super-psi would not be in place. These children would also be in the preverbal group, so once again the parents' needs will not be served by the child's experience.

Surprisingly, there *are* such cases, for example the following 15: (Atwater 1996:12–13, 2003:9–10,69,69 (bis),236–238, Fenwick & Fenwick 1995:183–184, Herzog & Herrin 1985, Ring & Valarino 1998:107–108,108–112, Serdahely & Walker 1990:177–183, Serdahely 1995:178,187, Sutherland 1995:82–83,136–141, Walker, Serdahely, & Bechtel 1991:194). All of these are preverbal cases (ruling out the adult super-psi option), and ten of them are from the first nine months of life (thus also ruling out the child super-psi option). Research supports the credibility of such cases, as follows.

Cases involving preverbal children appear to be consistent in features, depth, and complexity with the NDE reports of postverbal children and adults. Survey studies have shown that adult retrospective accounts of childhood NDEs and contemporary pediatric accounts of NDEs are indistinguishable (Serdahely 1991:223), conformant with a persistent finding that "unlike ordinary memories or dreams, NDE memories do not seem to be rearranged or altered over time" (Morse 1994:142). Furthermore, age seems to have no bearing on the complexity of NDEs, even for preverbal children (Fenwick & Fenwick 1995:182–183, Sutherland 2009:92,93). Phenomenologically, the NDEs of preverbal children are very similar to NDEs generally (Sutherland 2009:92–93). Some of the preverbal cases even have veridical elements, for instance in the case of Mark Bots, who spontaneously reported (at age 5 years) an NDE he had during a cardiac arrest at age 9 months. He reported seeing the doctors and nurses working on him, and seeing his grandmother wandering around the hospital corridors looking for his mother, as his family later confirmed did happen (Ring & Valarino 1998:112).[8]

Cases such as these strongly challenge the motivated psi hypothesis.

Three of the cases cited above were frightening experiences, and such cases further strengthen the challenge to the super-psi hypothesis. The case described by Peter and Elizabeth Fenwick involves a girl who remembers an

NDE after pulmonary arrest at birth. After a typical "material plane" OBE, she "somehow" found herself in a dark space where discarnate beings were trying to grab at her, before experiencing a tunnel leading to the light, and upon reaching the light her experience suddenly terminated. The fright of experiencing "other side" beings trying to grab at her left her with a lifelong fear of death, which she describes as "an awful affliction to have to bear" and a "burden that I carry around every day." Clearly neither the child nor the parents had the motivation to induce a precognitive hallucination that would *increase* death anxiety for the child. This experience does not even serve the nominal purpose of reassuring that death is not the end of consciousness, since for the child this is now a terrifying prospect, and the parents could not get the report for several years to come.[9]

In these cases, the facts are incompatible with the super-psi hypothesis, as the motivation needed to engage or direct the super-psi is absent, and the outcome is sometimes contrary to what would be expected if super-psi had been involved.

NDEs of Humanists and Atheists

If NDEs are precognitive events, and serve to assuage fears of death by providing convincing reassurance that death is not the end of existence, we should expect that atheists would not have such experiences, or at least not typical ones. The relevant point here is of course not just that atheists reject the idea of God but some may reject the idea of an afterlife. Such persons might nevertheless still fear death because they have much they want to achieve in *this* life, but evidence of an afterlife would undermine their fundamental conviction that this life, and what they achieve in it, is important because there is no other life. So if they did have NDEs, the point of the experience would be to reassure them that their condition was not really as serious as it seemed at the time, hence their continuing consciousness during that time. Something like a veridical this-world OBE would achieve this. What the experience cannot include is content that undermines their basic assumption that there is no "other world," no consciousness beyond irreversible death, and no lucid consciousness during severely compromised brain functioning. Experiences suggesting the existence of an after-world and discarnate beings would undermine the belief system they are most fundamentally invested in, and thus be contrary to their pre-NDE needs, and thus not plausibly due to super-psi.

Studies show that atheists *do* have NDEs, and are about as likely to have them as non-atheists and agnostics. Moreover, their NDEs have the same phenomenology that NDEs have in general (Holden, Long, & MacLurg 2009:118, Ring 1980b:4). Cultural or religious suppositions do affect how the experiences are interpreted, but there are clear similarities. For instance,

Buddhists might experience the "being of light" as Buddha, and Christians as Jesus, while atheists typically just encounter a light that is a living person (Badham & Badham 1984:86). Atheists' pre-existing beliefs do not, therefore, prevent them from experiencing apparent encounters with after-world places and discarnate beings, including beings that are apparently divine. For such cases, it is hard to see how super-psi can be in play. Since such an experience actually *undermines* their fundamental beliefs and the worldview they want to promote, it is not serving their pre-NDE needs.

It is just about possible to imagine, however, that while atheists might not want or believe in an afterlife themselves, they will be aware of the distress their demise would cause people close to them, who might not be atheists. Just as children may have "experiences" that reassure their parents, so atheists might, in the face of a mortal threat, have "experiences" that would reassure *their* loved ones. However, this alternative is not merely far-fetched; it is completely hopeless in the case of feisty atheists who are particularly clear-minded about their non-expectation of an afterlife, as one might find among atheist philosophers. And we have at least one vivid example of such a case, the cardiac arrest NDE in 1988 of the well-known logical positivist A. J. Ayer. He was formerly the Grote Professor of the Philosophy of Mind and Logic at University College London (1946–1959), the Wykeham Professor of Logic at the University of Oxford (1959–1978), president of the Aristotelian Society (1951–1952), and president of the British Humanist Association (1965–1970). He was also a dedicated hedonist and serial seducer, travelled in the most glamorous social circles, and had many famous friends. He was, however, "remote," it seemed, "from some of the more ordinary human emotions" (Rogers 1999), which would suggest that he was unlikely to have a hallucinatory experience contrary to his own beliefs just to please someone else.

In June of 1988 Ayer had a cardiac arrest while in the hospital recovering from pneumonia. Although he was 77 at the time, he was clearly in complete possession of all his faculties, even after his resuscitation (Cash 2001). He was connected to a heart-rate monitor at the time of his cardiac arrest, which lasted for four minutes. The attending physician Dr. Jeremy George later reported that on that same day, having finished his rounds, he returned to Ayer's bedside: "I came back to talk to him. Very discreetly, I asked him, as a philosopher, what was it like to have had a near-death experience? He suddenly looked rather sheepish. Then he said, 'I saw a Divine Being. I'm afraid I'm going to have to revise all my various books and opinions'" (Cash 2001). Three months later, when Ayer wrote an article about his experience for *The Sunday Telegraph*, he was apparently reverting to atheism again, concluding "My recent experiences have slightly weakened my conviction that my genuine death, which is due fairly soon, will be the end of me, though I continue to hope that it will be"

(1988a).[10] Two months later, his remission was apparently complete, and he wrote in an article for *The Spectator* that the most probable explanation for his experience was that his brain had continued to function even though his heart had stopped (1988b). He was not rejecting that he had experiences during that time, but promoting a particular interpretation of them, namely that he was simply hallucinating. This will not do, however. He could not have been having a synchronous hallucination, since having experiences at all during this time would be incompatible with his medical condition. It cannot have been precognitive super-psi either: If his subconscious needs were directing the experience, divine beings would not have featured in it, since their existence is contrary to his deeply held beliefs, and such an experience would subvert his life's work.

Publicly, he tried to trivialize his experience, but privately things had changed. His wife, Dee Wells, said: "Freddie became so much nicer after he died. He was not nearly so boastful. He took an interest in other people" (Cash 2001). The writer Edward St. Aubyn said Ayer told him that he had had a "kind of resurrection" and that he had recently started to notice scenery. In France, on a mountain near his villa, he said, "I suddenly stopped and looked out at the sea and thought, my God, how beautiful this is . . . for 26 years I had never really looked at it before" (Cash 2001). Near the end of his life, he spent more and more time with his former BBC debating opponent, the Jesuit priest and philosopher Frederick Copleston. According to Dee, "They got closer and closer and, in the end, he was Freddie's closest friend. It was quite extraordinary" (Cash 2001). This sort of transformation is not usually associated with cardiac arrest or hallucinatory episodes, but it is well-known in NDE cases. These include a more caring and compassionate attitude to others, a greater interest in spirituality, a sense of having been "reborn," and a greater appreciation of nature (Noyes, Fenwick, Holden, & Christian 2009). It is very likely that Ayer's intellectual pride motivated him to compromise his public account of his NDE (Cash 2001). What is not open to serious doubt is that (a) he did have a cardiac arrest, (b) he did have an NDE that suggested the continuity of his consciousness, and (c) he did not secretly desire any reassurances about his mortality on his own account or on behalf of others.

The precognitive super-psi hypothesis cannot account for his experience because the motivation needed to engage or direct the super-psi was absent, and the outcome was contrary to what would be expected if super-psi was involved.

This is consistent with what we find overall regarding the NDEs of atheists and agnostics. As noted above, their disposition before their NDE apparently does not impact the likelihood of them having an NDE or the likely content of it. On the whole atheists and agnostics respond in the same way to NDEs as others do, and come away openly convinced of survival and with a heightened

interest in spiritual matters (e.g., Ring 1984, Sutherland 1990; for a detailed case discussion, see the Wren-Lewis case in Bocking 2006). Given the facts of these cases, it cannot credibly be claimed that need-motivated super-psi is responsible for these experiences.

Dualism versus Survival

In cases such as these, involving very young children or committed atheists, the facts are incompatible with the super-psi hypothesis. Similar arguments could be developed in relation to other kinds of NDE cases, such as certain kinds of harrowing NDEs, young children who have "other world" experiences contrary to their parent's religious perspectives, and cases where NDE-like experiences occur in non-crisis circumstances such as during meditation, yoga, shamanic drumming, or even while listening to music.

If the precognitive experience super-psi hypothesis does not apply in these cases, the implication is that NDEs occur contemporaneously with their physiological contexts. Absent precognitive super-psi, the NDEs that involve veridical OBEs associated with cardiac arrest periods then entail that persons must have a "part" that is not only capable of lucid consciousness while brain functions are severely compromised, but has competent perceptual capacities independently of the physical body. By implication, some kind of soul–body dualism is implied, in which the soul is the ultimate bearer of the person's consciousness and rationality, and also has powerful psi abilities (for a broader discussion and analysis of this argument, see my paper, Rousseau 2011).

These are momentous claims, both ontologically and scientifically. However, substance dualism and consciousness during cardiac arrest are not enough to establish that consciousness can survive the irreversible death of the body. Until the issues are pointed out to them, most people (including most philosophers) assume that substance dualism entails survival, and that survival entails immortality. However, the situation is not so simple. It might be the case that the mind depends for its existence as a structured whole on the body existing as a structured whole, in which case it will not last much past the irreversible death of the body. Nature provides us with many examples of compound systems that have such dependencies, e.g., parasites and symbiotes. And just as there is a world of difference between consciousness surviving for minutes and surviving for years or centuries, there is a world of difference between something that is contingently enduring and something that is in principle everlasting. On these points, further evidence needs to be considered.

I did earlier present NDE evidence suggestive of souls surviving beyond the irreversible death of the body, in particular the so-called "Peak in Darien" cases, which occur both as NDEs and in other contexts. Some "Peak in Darien" NDEs are also cardiac arrest cases. Such cases might answer the empirical

question just posed, but first a new version of the super-psi hypothesis has to be overcome. The soul–body dualism just inferred entailed that the soul has powerful psychic abilities during the NDE OBE.[11] This opens the door to a new kind of super-psi hypothesis, as follows.

The super-psi theorist might concede dualism at this point, but not yet give up their neutrality regarding survival, and thus still reject the existence of discarnate interactionism. It is now conceded that during the OBE people really are conscious, and their psi faculties are operating powerfully, so they can observe local and remote events, read minds, and so on. The argument would now go that the actual NDE serves as a powerful reminder of the NDEr's mortality, even in the non-cardiac cases, since most NDEs are triggered by some kind of physiological trauma or threat. This fear of dying then powerfully engages their psi to gather veridical information from this-world sources including the memories of other living persons, and based on this it stimulates a hallucinatory experience that is incorporated into their NDE. The hallucination itself provides them with a reassuring "encounter" in the form of a meeting with deceased spirits, and sometimes it even includes information new to them which they can afterward verify by normal means, thus reinforcing the conviction that these were "encounters" with enduring spirits. However, once again it is all just a psychological coping mechanism to assuage their fear of actual death.

This dualistic super-psi hypothesis works very like the precognitive super-psi hypothesis: Deep fears motivate the focussing of psi faculties to produce experiences that assuage the fear. It is different from the precognitive super-psi hypothesis in that the sufficient fear and the veridical hallucination are postulated to occur simultaneously with their physiological contexts.

NDEs and the Dualistic Super-Psi Hypothesis

It is interesting to look at NDE cases from the point of view of looking for evidence that something like what the dualistic super-psi hypothesis asserts is going on.

It is notable that most NDErs find the OBE realistic and vivid, in the same way that waking consciousness is. For some it even seems more real and more vivid than waking consciousness (Kelly et al., 2007:386, note 16). People reflexively interpret this experience of being lucid while apparently separate from their bodies as meaning that their bodily death will not be the end of their consciousness. It is very clear from their reports, and survey studies bear this out. In Sutherland's study of 51 NDErs, she found that 84% had some fear of death before their NDE, but this dropped to 2% afterward (1992). Margot Grey found a drop from 63% to 0% (1985), and Anja Opdebeeck found a drop from 45% to 0% (2001). At the same time, belief in an afterlife increased from 38% to 100% in Sutherland's study, 25% to 96% in Opdebeeck's, and 22% to

92% in Cassandra Musgrave's study of 50 NDErs (1997). Compared to these widespread changes, only 20% of NDErs experience encounters with spirits, of which perhaps half are long-dead. Clearly the driver for these changes in conviction is not the experiences of spirit-encounters, but rather the experience of personal consciousness dissociated from the body.

This is not really a surprise, because although the idea that dualism entails survival, and that survival entails immortality, is unwarranted, it is incredibly widespread. I have never come across anyone unfamiliar with the academic literature on survival, who did not make this automatic connection between dualism, survival, and immortality. And many people who *are* familiar with the literature seem to make the same connections anyway, or at least speak as if such views do not stand in need of a defense. The statistics quoted above suggest that the number of NDErs to whom it occurs that their experience does not entail long-term survival is very low, if not actually zero.

In this light, the dualistic super-psi hypothesis is shown up to rest on a philosophical esotericism—average people do not have this concern during their NDE OBE. Whatever their death-anxieties might have been *before* their NDE, once their OBE starts the motivation to focus their psi on constructing a reassuring hallucinatory encounter with spirits of the deceased is simply not there, because by then, as far they are concerned, they have already discovered that death is not the end.

From this it is clear that the dualistic super-psi hypothesis is a non-starter. By implication, the experienced encounters with what presents as deceased spirits cannot be dismissed as psychic fabrications, but can instead be regarded as experiences that are as objective as the out-of-body perception of the physical environment. Some of these encounters involve what appears to be the spirits of the *long* dead, so from these NDE cases we can infer that there is some manner in which personal consciousness can indeed survive the irreversible death of the body for a significant period (many decades at least), and that, whatever the true nature of this continuant, it has the causal means needed to interact with living persons.

The Problem of Recalcitrant Evidence

Every scientific theory encounters "recalcitrant evidence" from time to time, and it is rarely a reason to reject the theory. It might reasonably be retained anyway because it may have something going for it in terms of other theoretical virtues, or there may be some hope of modifying its auxiliary hypotheses in such a way that it would bring the problematic data back into its explanatory scope, or there may simply be no known viable alternatives. It is on such considerations that Ptolemy's planetary model, Galen's heart model, and Newton's theory of

universal gravitation were all retained, despite challenging evidence, until more competent alternatives were available.

The present situation is, however, hardly comparable.

First, in the present instance we do not merely have evidence that the super-psi hypothesis cannot explain: In addition, its premises are contraindicated, for the motivations it relies on are demonstrably absent, and there are outcomes that are actually *contrary* to the super-psi hypothesis's expectations. So in relation to NDE evidence the super-psi hypothesis is not only baffled, it is also undermined and falsified.

Second, the super-psi hypothesis does *not* overall have clear advantages in terms of other theoretical virtues, as was shown above.

Third, it was all along an ad hoc theory (just like the survival hypothesis), so hopes to adjust the super-psi hypothesis's premises and auxiliaries to make it viable again would amount to a scientifically suspect intention to "fix" the theory's explanatory power by adding ad hocery to ad hocery. Ad hocness is a theoretical weakness, so such actions are likely to just offset the gain in explanatory virtue.

Last, it is not as if there are no useful alternatives, since the survival hypothesis *does* accommodate all the evidence and in other ways it is not on balance inferior to the super-psi hypothesis. Some have argued that ontological parsimony should have a high weighting, and that on this basis the super-psi hypothesis would then still have merit, since it is the less ontologically extravagant. This is a moot point, but I will in any case show below that on this criterion the advantage of the super-psi hypothesis has been overstated, which carries the implication that weighting this virtue will not help the super-psi hypothesis.

On these considerations the super-psi hypothesis can fairly be considered to have been eliminated as a potential explanation for NDE phenomena.

NDEs and the Case for Survival

Having dismissed the challenge of the super-psi hypothesis in relation to NDEs, we are warranted to infer from NDE evidence support for dualism, survival, and discarnate interactionism. NDE evidence can now be analyzed for clues about the nature of the soul and its postmortem condition and capacities. However, NDE evidence is only one kind of survival-suggestive evidence among several, and it may be suspected that the super-psi hypothesis can still stand against the other sorts of survival-suggestive evidence, placing this other evidence out of bounds as further source material for the just-mentioned model development. Given the potential value of this other evidence in this regard it is important to consider how the theoretical virtue tradeoff has shifted in the light of the super-psi hypothesis's failure in the NDE arena.

A Revised Virtue Tradeoff between the Super-Psi Hypothesis and the Survival Hypothesis

I will here consider the theoretical virtues in the same order as before, but discuss some of them at longer length.

Empirical Adequacy

The kinds of NDE cases discussed above, and arguably NDEs as a class, cannot plausibly be explained by the super-psi hypothesis (even in a weakened form that accepts dualism). This breaks the draw previously declared in empirical adequacy. However, it is true that in general scientific theories do not explain all the data in their domain of application (Braude 2003:18–19), so it is not immediately obvious that this is a serious problem for the super-psi hypothesis with regard to other kinds of survival-suggestive evidence. For *that* to be the case, the super-psi hypothesis also has to fare worse in a wide range of other virtue criteria. But this does not diminish the point that in the present virtue category the survival hypothesis has an advantage over the super-psi hypothesis, so that overall there is no longer a draw.

Ontological Conservatism

It was said earlier that the survival hypothesis is at a disadvantage here, because it assumes the existence of discarnate souls with psi powers, which are entities we do not have independent evidence for outside the survival-suggestive evidence. In principle, there cannot be such evidence in a direct way, since any evidence produced would immediately be classified as survival evidence and therefore would not be "independent evidence." However, it may be possible to argue for the likelihood that such entities exist, on the basis of independent evidence for each of the components of the claim that discarnate psi-capable souls exist. This can be done for example by presenting evidence that living persons have souls, that the souls of living persons have psi powers, and that souls do not depend on their bodies in an essential way. Each of these steps would reduce the apparent extravagance of the survival hypothesis's ontological claims, and if all three claims can be sustained then the survival hypothesis's position would be a reasonable one.

Such evidence can be obtained from NDEs and other sources. NDEs are rich phenomena that involve much more than just putative encounters with discarnate souls—in fact spirit encounters occur in only 20% of NDEs. Without taking the events involving spirit encounters into account, it can be argued that on the failure of the super-psi hypothesis in the case of cardiac arrest OBEs, these incidents are best interpreted as evidence for the existence of souls: The

person's persistent lucid mental functioning in the face of deeply compromised brain functions show that minds are distinct from brains, and the person's psychic observations during the arrest period shows that what is continuing to function constitutes much more than just a mind. This is therefore evidence not only for the existence of souls but of souls with psychic powers. There is even some evidence of NDE OBErs interacting with people in normal states of consciousness (e.g., Greyson & Bush 1992:223), and of course souls can interact with their physical bodies under normal circumstances, so overall the evidence suggests that the souls of living people have, at least in principle, the sorts of interactionist powers that are ascribed to spirits under the survival hypothesis.

The evidence for the existence of souls is bolstered by other evidence, from outside the context of NDEs, for competent mental functioning in the face of severely compromised cerebral conditions. These include cases of so-called "terminal lucidity," where mental faculties are recovered before death despite severe brain disorders such as Alzheimer's and schizophrenia (about 50 published cases, see Nahm & Greyson 2009, Nahm, Greyson, Kelly, & Haraldsson 2011, Nahm 2009), cases of normal (or even above normal) mental functioning despite severe brain developmental disorders such as hydranencephaly (about 80 cases referenced and 20 described, see for example, Baudoin 1996, Berker, Goldstein, Lorber, Priestley, & Smith 1992, BUVFC 1982, Duyff, Davies, & Vos 1996, Feuillet, Dufour, & Pelletier 2007, Lewin 1980, Lorber 1965, 1978, 1983), and cases of serious brain degeneration without significant concomitant cognitive deficits (one case published but many more apparently known, see Archer, Schott, Barnes, Fox, Holton, et al., 2005, Melton 2005).

The way in which this evidence for the existence of souls arises is interesting in relation to the question of whether souls can survive the death of the body. In the NDE cases we have not only mental functioning distinct from brain functioning, but lucid mental functioning concomitant with deeply compromised brain functioning. In fact, 80% of NDErs reported that during their NDE their thinking was at least as clear as usual (45% "clearer than usual"), 74% reported that the speed of their thinking was unimpaired (37% "faster than usual"), 65% reported their logic was unimpaired (29% "more logical than usual"), and 55% reported no decline in the control over their thoughts (19% "more control than usual") (Kelly et al., 2007:386, note 16). This is the opposite of what we would expect if the mind depended on the brain in an essential way—it looks more as if the brain inhibits or constrains the mind in some way, rather that supporting it. Psychic powers are also greatly enhanced during the NDE OBE, suggesting that psychic powers are likewise inhibited rather than facilitated by normal embodiment. The inverse relationship between psychic ability and arousal of the nervous system has of course often been noted outside the context of NDEs (Kelly 2007:603–607, Myers 1891:638). If the soul is the

seat of the person's mental and psychic faculties, and these work better when the functioning of the body is inhibited, then this can be taken as evidence suggesting that the connection between the soul and the body is a contingent one, and not an essential one.

We can therefore argue, without bringing putative spirit encounters into account, that there is evidence for the existence of souls, for souls having psi powers, and for souls not being essentially dependent on bodies. In this light the claim that the survival hypothesis's ontology is extravagant cannot be sustained, and the tradeoff against the super-psi hypothesis is reduced to a draw (or something near enough).

Explanatory Simplicity

As before, the survival hypothesis provides simper explanations, so the survival hypothesis still has the advantage.

Causal Interference

As discussed earlier, the survival hypothesis appears to have the advantage here. No change.

Antecedent Probability

It was previously argued that both hypotheses are equally probable, since there are multiple lines of evidence for survival, but also ample evidence for powerful living-agent psi. However, since we now have grounds to reject the super-psi hypothesis for NDE cases, the antecedent probability that discarnate interactionism is behind the other kinds of survival-suggestive evidence is increased. It would certainly seem arbitrary to claim for example that NDE-based "Peak in Darien" cases are due to discarnate souls but other "Peak in Darien" cases occurring in for example deathbed visions or dreams are equally likely due to super-psi. In terms of antecedent probability, the survival hypothesis now has the advantage.

Plausibility

As stated before, the strength of psi required by each hypothesis is the same, so on those grounds they are equally plausible. However, this argument glosses important evidential considerations from NDEs and other kinds of psychical research cases. I have recently argued that the NDE evidence suggests that although people in principle have powerful psychic abilities (because souls have them), embodiment and physiological arousal typically inhibits the expression of these powers (Rousseau 2011). A similar point was in fact previously argued by

other researchers from Myers onward, who pointed out an inverse relationship between the strength of psychical experiences and the level of activation of the nervous system (Kelly 2007:603–607, Myers 1891:638). The implication of these observations is that absent significant physiological calming or severe mind–body dissociation, living-agent psi is unlikely to be available for use in a very powerful way. In those cases where survival evidence is obtained without deep trance or severe dissociation (e.g., hauntings, apparitions, children who remember previous lives, organ memory cases, and certain poltergeist cases), the inherent plausibility of the super-psi hypothesis is therefore diminished. Since the survival hypothesis covers all the case contexts, but the super-psi hypothesis is less plausible in some contexts, the previously assessed draw regarding this virtue is replaced by an advantage to the survival hypothesis.

Actuality

It was previously argued that although both hypotheses are plausible, evidence has not been presented to show that either mechanism is actually in play in the survival-suggestive evidence. However, since discarnate interactionism has now been shown to be the most likely explanation for "Peak in Darien" NDE cases, we have good grounds for asserting that something like this is also going on in other kinds of survival-suggestive evidence. The previously scored draw is converted to an advantage in favor of the survival hypothesis.

Minimal ad Hocness

Both hypotheses were previously ad hoc, so this was a draw. Ad hoc theories are sterile, having no explanatory or predictive power beyond the data they were invoked to explain. However, the failure of the super-psi hypothesis in the case of NDEs means that the NDE evidence can now be analyzed in order to develop a rich model about the nature of souls. If such a model had consequences beyond the survival-suggestive evidence, it would defuse the charge of ad hocness against the survival hypothesis. I have been developing such a model, and a part of it was recently published (Rousseau 2011). The published parts of this model *do* have consequences beyond the survival-suggestive evidence, for example it explains why psi is normally weak but can sometimes function powerfully, and why NDErs so often are more psychic after their NDE than before it. It shows how psychic phenomena are related to mystical ones. The model potentially has relevance for understanding certain aspects of dyspraxia, conversion disorders, psychopathy, and autism. It enables new kinds of claims to be made, such as that PK is an emergent capacity of the soul–body system, while other psychic abilities are inherent capacities of the soul as such. The model makes some novel predictions, such as that the soul is a naturalistic substance and not a

supernatural one, and that certain kinds of soul–body dissociation carry certain health risks. On these grounds, the failure of the super-psi hypothesis in the NDE arena can be regarded as having liberated the survival hypothesis from being an ad hoc theory to a scientific one, since its dualistic assumptions are no longer ad hoc but grounds for a scientific model supported by evidence. Overall, the advantage has shifted in favor of the survival hypothesis.

Falsifiability

I have argued that the super-psi hypothesis is falsifiable in relation to specific kinds of cases, and have presented NDE cases that do this. This suggests that in principle it is possible to find counterexamples in the other kinds of survival-suggestive evidence, too. This improves the super-psi hypothesis's score on this virtue. I have also indicated in the discussion of the previous virtue that the failure of the super-psi hypothesis in the case of NDEs has led to a dualistic model that has explanatory and predictive value beyond NDEs, and makes claims about the inherent abilities and limitations of souls. In consequence, there is now scope to develop a survival model with testable predictions, so in principle the survival hypothesis's falsifiability has also improved. Therefore, there is still a draw in this virtue (but it is now one–all rather than zero–all).

A New Virtue Scorecard

Without applying weightings to the virtues, a new scorecard based on these revisions would appear as in Table 2.

Table 2 shows not only that the impasse has been broken, but that the survival hypothesis has been considerably strengthened as well: On a percentage basis the score ratio is now something like 8%:36%. It is clear from Table 2 that the balance is now in favor of the survival hypothesis in most virtue categories (seven out of nine), and that the super-psi hypothesis arguably no longer has a distinct advantage in any category. If weightings were to be applied, the survival hypothesis would score even higher. The virtues of empirical adequacy, ontological conservatism, and limited ad hocness arguably weigh much more than the others, and one of these is a draw while the other two favor the survival hypothesis. If the work that I have published elsewhere were taken into account as far as the unconsidered criteria are concerned, the survival hypothesis's score would be even higher (e.g., in terms of predictive power, integration with other areas of knowledge, and practical utility). Now that dualism, survival, and discarnate interactionism have become accessible to scientific modelling, the prospects are there for the survival hypothesis to be developed on the basis of NDE and other evidence, and thus for the survival hypothesis's score to rise even higher.

TABLE 2
A Revised Scoring of the Theoretical Virtues

	Theoretical Virtue	Score (Original)		Score (Revised)	
		Super-Psi Hypothesis	Survival Hypothesis	Super-Psi Hypothesis	Survival Hypothesis
1	Empirical adequacy	1	1	0	1
2	Ontological conservatism	1	0	1	1
3	Explanatory simplicity	0	1	0	1
4	Causal interference	0	1	0	1
5	Antecedent probability	1	1	0	1
6	Plausibility	1	1	0	1
7	Actuality	0	0	0	1
8	Minimal ad hocness	0	0	0	1
9	Falsifiability	0	0	1	1
10–25	Unconsidered criteria	0	0	0	?
	Total score	***4***	***5***	***2***	***9***
	Possible score	*25*	*25*	*25*	*25*

Conclusion

I have argued above that NDE evidence escapes the challenge presented by the super-psi hypothesis, and that this allows an inference to be made in favor of dualism, survival, and discarnate interactionism. I have also argued that the failure of the super-psi hypothesis in this arena shifts the balance between the theoretical virtues of the rival hypotheses strongly in favor of the survival hypothesis. The implication is that when considering other kinds of survival evidence where the survival and super-psi hypotheses equally well explain the data, it is warranted to rule in favor of the survival hypothesis on the basis of these other theoretical virtues. In consequence, the non-NDE survival-suggestive evidence is broadly available to be analyzed for clues about the nature of souls, the mind–body relationship, and survival, and new research projects may reasonably be defined with a hope of finding answers (or better answers) to these important questions.

Notes

[1] In the mainstream debate there is much controversy over how the relationship between mental phenomena and brain states should be understood, and this is still very much an open issue. However, the working assumption is that the relationship is an essential one, so that in consequence the survival of consciousness is ruled out a priori. This assumption is, however, challenged by a growing body of evidence from psychical research and NDE studies.

[2] On the whole, advocates of the survival hypothesis radically underestimate what a huge step in metaphysical commitments, compared to mainstream views, this conclusion represents.

[3] The term "discarnate interactionism" was coined by Michael Sudduth (2009:170).

[4] The cardiologist Fred Schoonmaker claimed to have 55 such cases from his own research (Audette 1979), but these cases have not yet been published.

[5] For example, in one case the patient had an out-of-hospital acute massive heart attack, and was brought in unconscious and cyanotic. An attending (male) nurse removed the patient's dentures before attaching the ventilation mask. After he recovered, he asked for his dentures but no one knew what had become of them. The patient reported that he had observed what happened from his OBE state. He recognized the nurse in question and said that he had placed the dentures on a shelf on the crash cart, where they were then found (van Lommel, van Wees, Meyers, & Elfferich 2001, Smit 2008). In another case, the patient had an OBE in which she drifted outside the hospital room and saw a Christmas tree on the balcony below the window of her room. This was in February (Cook, Greyson, & Stevenson 1998:389).

[6] Peter and Elizabeth Fenwick make essentially the same point in relation to a comparison they made between the phenomenology of NDE-OBEs and "spontaneous" OBEs, saying "If the phenomena seem the same, even though they occur in different circumstances, then the chances are that all OBEs have an underlying common mechanism" (1995:37).

[7] They are called "Peak in Darien" cases after the name of an 1882 book by Frances Power Cobbe which contained such a report.

[8] These cases appear to be continuous with a much larger dataset concerning the memories and cognitive capacities of prenatal and perinatal children. For more on perinatal memory cases, see Chamberlain (1983, 1988). See Ring & Valarino (1998) for an independent consideration of such evidence and a positive conclusion (pp. 114–117). See also the *Journal of Prenatal & Perinatal Psychology and Health* (now in its 20th volume).

[9] For postverbal children, for whom we have a much bigger dataset, frightening NDEs occur in about 3% of cases (Atwater 2003). The motivational psi hypothesis fails here, too.

[10] Ayer's irreversible death occurred nine months later.

[11] I have recently argued (Rousseau 2011) that it appears that a person's psychic ability is enhanced during cardiac arrest because during that time the integration between the soul and the body is disrupted, and that this makes available psi capacity that is normally engaged in soul–body integration.

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RESEARCH ARTICLE

Remote Viewing the Future with a Tasking Temporal Outbounder

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Abstract—This study uses remote viewing in a predictive manner within the context of a novel experimental design to describe 11 target events spread out over a year, each of which occurs approximately one month after the remote-viewing sessions are completed. The study was conducted at The Farsight Institute using 12 highly experienced remote viewers who were trained in the use of four remote-viewing methodologies that are the same as or derived from those previously used by the United States military for espionage purposes. While prediction using remote viewing has a long and spotted history, the current investigation is aimed at enhancing our understanding of the remote-viewing phenomenon by utilizing a temporal outbounder approach to tasking in order to improve the description of future events. In this design, the tasker is located in time after the remote-viewing sessions are completed and after the occurrence of the chosen target event. Exploiting one of the largest bodies of remote-viewing data ever collected using military-related viewing methodologies, this study finds strong support for the hypothesis that experimental designs utilizing a temporal outbounder as a tasker greatly enhances the accuracy of remote-viewing descriptions of future events. The causal mechanism for why this might occur is left to be determined by future research.

The use of remote viewing to predict notable public events has a long and spotted history. Failures at using remote viewing to predict the future are abundant although under-reported in the scientific literature, a recognized phenomenon tied to the undesirability for researchers to publish negative results. Contrarily, remote viewing current and past events is not nearly as problematic, and the use of structured remote-viewing methodologies of the type developed by the United States military has been shown to be relatively effective as data-collection platforms (see especially Brown 2006, McMoneagle 2000, Puthoff 1996, Targ 1996, Puthoff & Targ 1979). The current study uses a new experimental design to investigate a novel approach to tasking that greatly enhances the accuracy of remote viewing in describing future events.

This is a process-oriented study of remote viewing. I do not attempt to "prove" in this setting that remote viewing is a real phenomenon. In my view, this has already been accomplished elsewhere (see especially Utts 1991, 1996). The accumulated statistical evidence presented in the literature of this field would have been broadly accepted long ago for a less controversial subject. Rather, my purpose here is to present the results of an extensive remote-viewing experiment that uses an innovative design that helps shed light on the process of the remote-viewing phenomenon itself.

Remote Viewing as a Data-Collection Platform

Remote viewing is a mental process of nonlocal perception based on psi that has the capability of extending the range of normal human perceptions through both space and time. Remote viewing is always conducted "blind," which means that the viewer has absolutely no prior knowledge of the nature of the target. The United States military exploited the phenomenon using trained remote viewers for many years until the official programs became publicly known and had to be closed down in the 1990s. It is publicly unknown if still-secret remote-viewing programs exist today within the military, although many assume this is highly likely.

Participating in this study were 12 remote viewers who were trained extensively (usually for well over a decade) in the use of structured remote-viewing data collection using methodologies that are either identical to or derived from those utilized by the United States military. With few exceptions, most of the remote-viewing training for these remote viewers was accomplished (long before the current experiment began) under the leadership of Glenn Wheaton who leads the Hawaii Remote Viewers' Guild (HRVG) and Lyn Buchanan who leads a group using methods known as Controlled Remote Viewing (CRV). Until their retirement from the military, Glenn Wheaton worked in Intelligence within what was essentially a decentralized movement (popularly known as the First Earth Battalion) located within the U.S. Army's Special Forces branch, whereas Lyn Buchanan worked as a remote viewer in an official program now known popularly as the Star Gate Program within the Defense Intelligence Agency (DIA).

Civilian research into the remote-viewing phenomenon is now quite advanced, and an up-to-date report of the science of remote viewing (including an extensive review of the extant scientific literature on the subject) can be found in the volume *Remote Viewing: The Science and Theory of Nonphysical Perception* (Brown 2006). Readers wanting an overview of the extant scientific literature on remote viewing are encouraged to locate it in this source, and a redundant review is omitted here for brevity. The book also presents an in-depth investigation into an enigma commonly known as the "displaced target

phenomenon" that plagued decades of past research into the remote-viewing process. This phenomenon (sometimes called "cross-cutting psi channels") occurs when remote viewers are instructed to remote view a target from a list of five targets, one real (chosen by a random process) and four decoys. The results of such experiments invariably result in some remote-viewing sessions that contain excellent descriptions of one of the five targets, but not necessarily the so-called "real" target chosen by the random process. The book resolves this mystery by identifying the mechanism controlling the remote viewer's focus of perception under this and similar situations. It is theorized that the analysis process involving the five potential targets has the capability of psychically contaminating the perceptual data-collection process. The connection between this and the current study is explained further below.

It has long been known that remote viewing can be used to describe events across time (see, for example, McMoneagle 2000, 1998, 1993). However, remote viewing future events has a more spotted history than remote viewing past events. For example, in a now declassified official DIA report written for Dr. Jack Vorona on 15 October 1987 (Sun Streak Report—Third Quarter CY 87 [1987], hereafter cited as simply "Vorona's report," http://www.farsight.org/demo/Multiple_Universes/DIA_project_P_results.pdf), the success of military remote viewers at describing future events hovered between 13% and 18%, and these percentages were considered in this report to be "weak correlations" between the remote-viewing data and the targets. Although often anecdotal, many other reported attempts to use remote viewing to describe future events performed similarly. In one well-documented experiment conducted at The Farsight Institute, remote-viewing data were obtained that appeared to describe a future major earthquake in the area of Los Angeles International Airport (LAX) in 2008, and an analysis of the data published on the website for the Institute in June 2008 suggested that the most likely date for the earthquake based on the characteristics of the remote-viewing data was the last few days of July 2008. Indeed, on Tuesday, 29 July 2008, there was a notable 5.4–5.8 magnitude earthquake in Los Angeles. This exactly coincided with the predicted date of the earthquake, although the magnitude of the quake was lower than expected.

Yet another way in which remote viewing is used to describe future events is within the context of associative remote viewing experiments. In such experiments, targets are assigned to certain possible future outcomes. For example, the Eiffel Tower may be the target if the Dow Jones Average is to go up at the end of a given week, whereas a location in Death Valley may be the target if the Dow Jones Average is to go down at the end of that same week. Remote viewers do the sessions blind, and a judge evaluates whether or not the sessions resemble the Eiffel Tower or Death Valley. If the session images resemble the Eiffel Tower, then, presumably, someone involved with

the experiment purchases stocks in the hope of earning money by the end of the week. If the sessions resemble Death Valley, then that same person may sell stocks in the hope of avoiding a loss at the end of the week.

Although opinions will inevitably vary, in my view associate remote-viewing experiments have a history of widely varying outcomes. At best, sometimes there is a short string of successes, and the experimenters spend years unsuccessfully trying to replicate the briefly positive results. The results are never consistent over time, and successes inevitably are much more rare than the failures. For example, in one of many associative remote-viewing experiments conducted at The Farsight Institute, 128 remote-viewing sessions were used with binary choices that led to a lottery prediction (Brown 2006:122–124). Despite extraordinary controls that included having an automated and sophisticated computer program analyze the remote-viewing data, the result was a failure in both quantitative and qualitative terms. Bluntly put, no one has ever become a millionaire based on associative remote viewing.

It is important to understand that I do not rely on Vorona's DIA study, associative remote-viewing attempts, or any other specific study as a formal control group in the current context. I discuss the physical difficulty of using a traditional control group in the current context further below. Yet some things are just so well-known they do not need to be formally structured; predicting future events using psi-based processes has never been reliably accomplished previously. (Here I am addressing only detailed descriptions of future events, not statistical evaluations of presentiment involving randomized trials, as reported by Bem [2011] and Radin [2006].) In this study however, the description of future events is shown to be unambiguously reliable (using any reasonable standard) when the experimental design involves a temporal outbounder and highly trained remote viewers. Nonetheless, while I sometimes do compare the current results to other attempts at predicting the future in the discussion below (such as with respect to Vorona's DIA study), these comparisons are of an informal nature only.

To develop this experimental design, the traditional tasking of remote-viewing sessions needed to be restructured. In traditional remote-viewing research, tasking (i.e., determining the target for a remote-viewing session) is done prior to conducting the session. For past (historical) and concurrent (at the same time that the viewing takes place) targets, this structure is outlined in Figure 1.

The trouble with remote viewing a future target is that the tasker must select an event with certainty that is in the future of the remote-viewing session. To do this, the tasker (and potentially the analyst) needs to have experienced the target event to be certain that it did indeed happen. Obviously this requires that the tasker must determine the target choice after the remote-viewing session has

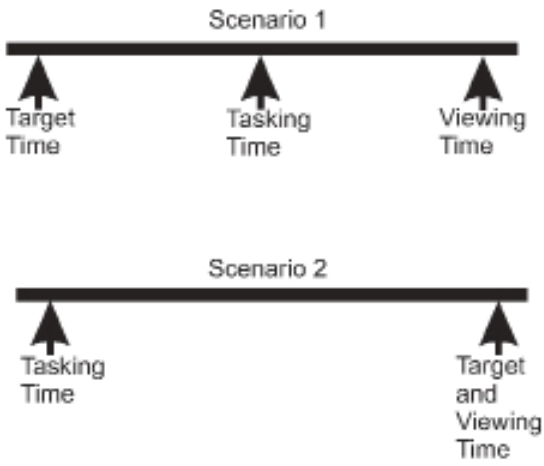

Figure 1. Traditional remote-viewing tasking scenarios for past and concurrent events in which the tasking time precedes the viewing time.

been conducted. This can happen in one of two ways. If the future event that is to be predicted is the tasker's choice of a target (that is, not the physical event itself that is described in the remote-viewing session, but the choice of which physical event is to be perceived), then the target event can be in the past as long as the tasking is done after the sessions are already completed. This scenario is shown as scenario 3 in Figure 2. Scenario 3 in Figure 2 involves the future

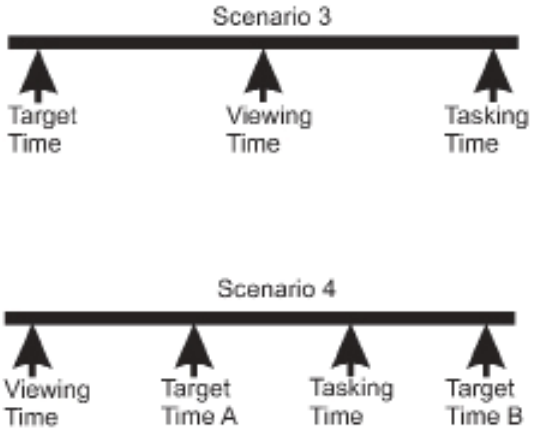

Figure 2. Alternate remote-viewing tasking scenarios in which the tasking time occurs after the viewing time.

tasking of past (historical) events, and the choice of the past event is determined after the remote-viewing session is completed. Obviously, the tasker must be totally blind with respect to the remote-viewing data for this to work.

More relevant to the current study is Scenario 4 in Figure 2, where the physical target event occurs after the remote-viewing session is conducted. There are two possibilities. With "target time A," the sequence of events is (1) viewing time, (2) target time, and (3) tasking time. In this situation, the remote-viewing data should describe the future event, but the choice of which event is determined by someone in the future of that event. We can call that future tasker a "temporal outbounder," in the sense that the tasker is someone in the future who has experienced a future event. The second possibility in Figure 2 involves "target time B," and this has the following sequence of events: (1) viewing time, (2) tasking time, and (3) target time. With this scenario, the viewing time still precedes the tasking time, but the tasker has not yet experienced the future event. This latter scenario is obviously not optimal for the current experiment since there can be no certainty that the tasker and viewer will experience the event.

This experiment is structured using Scenario 4 and tasking time A, as shown in Figure 2. To do this, an elaborate, publicly verified setup was maintained in which the remote-viewing data were collected first, followed by a period in which an undetermined target event would happen, and then another period in which the tasker would choose the target event from the set of any publicly known events that happened in the middle period. In practical terms, this was broken down into three consecutive months. To develop an example using the months of February, March, and April, the remote-viewing sessions would be conducted during the month of February. March would be a waiting period within which the eventual target event would be located. In April, the tasker would choose a target from any event that happened in the month of March.

One goal of this experiment was to have the public participate in the study by verifying the experimental process. To do this, all remote-viewing sessions were transmitted by the viewers (via fax or as email attachments) to a central office. All sessions were then encrypted using secure 256-bit encryption available in the Winzip compression software. The encrypted sessions were then placed on the website of The Farsight Institute for the public to download at the end of each viewing month. The public had one month to download the encrypted sessions while everyone waited for the third month in the sequence to arrive so that the tasker could pick a target. Sometime during the third month in the sequence, the tasker would pick a target from the set of any events that happened during the second month of the sequence. The target was then posted on the website of The Farsight Institute together with the passwords needed to decrypt the remote-viewing data.

For this experiment, two taskers participated in this project, Glenn Wheaton and Lyn Buchanan, and they alternated as taskers for a series of 11 experiments that lasted for one year. All viewers were instructed not to show or discuss any of their data to either Glenn or Lyn. These instructions were conveyed repeatedly to all viewers by myself, Glenn, and Lyn, and they were followed without violation throughout the entire project. Moreover, none of the HRVG viewers had any contact with Lyn, none of the CRV viewers had any contact with Glenn, and none of the SRV viewers had any contact with either Lyn or Glenn.

The entire three-month sequence was repeated in a rolling fashion from February 2009 through March 2010, and the public was encouraged to download and save each set of encrypted sessions for each of the experiments. Thus, the first experiment in the series began in 2009 with February as the viewing month, March as the month where the target event would occur, and April as the month when the tasker picked the first target from the set of all events that happened in March. The second experiment involved the months March 2009, April 2009, and May 2009 in a similar fashion. The third experiment involved the months April 2009, May 2009, and June 2009, and so on for the remainder of the experiments through March 2010.

Some readers may wonder when viewer feedback and session-evaluation times occur with respect to Figure 1 and Figure 2. In virtually all situations involving any of the scenarios in Figure 1 and Figure 2, viewer feedback and evaluation times occur after the final elements identified in the timelines shown in the figures. In some research, viewer feedback occurs after the sessions are evaluated by one or more judges. In other situations, viewer feedback occurs as part of the session-evaluation process. This latter situation is especially relevant when formal session "closing" procedures are utilized, which is the case with this current research. In such a situation, the viewers are the first individuals to evaluate their sessions with respect to the actual targets. The underlying rationale and application of such a closing process with respect to the current study is explained fully below.

Evaluating Remote-Viewing Data

Remote-viewing studies can be basically broken up into three periods. In the early period, the remote-viewing phenomenon was studied in its most basic form. That is, experiments were designed in which remote viewers were assigned targets chosen by human taskers, and the remote-viewing data were often evaluated subjectively by the experimenters. Sometimes these studies involved the use of spatial outbounders who went to physical locations selected from a collection of, say, three possible locations. The task of such experiments was for the remote viewer to describe the physical location of the outbounder in

real time. Sometimes these procedures involved targets that were selected from a larger pool of potential targets, and statistical evaluations were made of the results based on often complex coding schemes. Pioneering (indeed, landmark) early studies at Princeton Engineering Anomalies Research (PEAR) Laboratory and Stanford Research Institute (later, SRI International) can be located within this period. This is, indeed, the approach used in Puthoff and Targ's seminal work involving spatial outbounders and remote viewing by Pat Price, Ingo Swann, and Hella Hammid (Puthoff & Targ 1976).

The second (middle) period of remote-viewing research has a clear starting point, which is the publication of a blistering review by Hansen, Utts, and Markwick (1992) of some of the early period's work. The criticisms focus in part on the use of human taskers (often outbounders capable of picking the target location), the lack of randomness in the selection of targets (such as selection from a pool without replacement), and insufficient isolation of the outbounders from the perceivers, as well as data-coding issues. A central part of their argument essentially states that any experimental design that employs human taskers in almost any manner exposes the study to targeting biases since it cannot be guaranteed that the remote viewers will not have picked up subconscious cues from the taskers as to the nature of the target. This is particularly important when there has been some direct contact between the taskers and the perceivers, although the issue can still arise if intermediaries who have contact with both the taskers and the perceivers act as subconscious informational leaks. With respect to target selection, part of the recommended "solution" is to use computer programs or other non-human methods of randomly selecting targets with replacement from a pool of diverse potential targets.

One of the procedures to emerge from this second period involves a method aimed at making the analysis of remote-viewing data more objective. This method is to have a list of, say, five potential targets, one real and the others decoys. The real target would be chosen by a computer or other random dynamic process. The remote-viewing data would then be evaluated by a panel of, say, three judges who would be blind as to the chosen real target. Their job was to compare the data with the list of five targets to determine which target resembles the data best.

As mentioned previously, this process led to the emergence of the "displaced-target" phenomenon, in which the remote viewers would often produce data that clearly described one or more of the decoy targets rather than the "real" target. Targ and Harary (1984) suggest that the targets on the list cannot be fully separated psychically, and thus they are placed in a "psychic bubble" from which viewers draw their perceptions. This phenomenon led to years of researchers bemoaning the apparent fickle nature of remote viewing. (See especially Hyman 1996, Jahn 1982, Targ 1999:89.) Indeed, statistical

techniques were developed and sometimes applied in an attempt to control for the lack of independence across the target pool, often referenced as the "stacking problem" (Thouless & Brier 1970).

In general, during the second period of remote-viewing research, the overall solution to the problem of collecting and analyzing remote-viewing data was to follow the general guidelines of Hansen, Utts, and Markwick (1992) to remove the human element from the experimental design as much as possible. The problem with the "solution" is that the implicit assumption was made that the remote-viewing phenomenon could be isolated with respect to causality in classical terms. In practical terms, it was assumed that the computer program that chose the "real" target could actually do this. That is, it was assumed that the focus of the perceptions of the remote viewers when they conducted their sessions could be constrained by the target choice of the computer. It is important to note that this assumption was simply posited; it was never tested to see if the computer itself was the actual constraining mechanism. Bluntly put, many researchers during the second period of remote-viewing research did not fully appreciate the nonlocal nature of the remote-viewing phenomenon itself. Indeed, they did not fully understand what makes a target a target.

Subsequent research (Brown 2006) indicates that the focus of perception of a remote viewer is crucially dependent on the thoughts of the person who analyzes the remote-viewing data for the first time. While there is insufficient space here to entirely recount the research that establishes this aspect of the remote-viewing phenomenon, the essence of the argument is that a telepathic connection between the remote viewers (when they do their sessions) and the analysts (when they compare the remote-viewing data to the targets on the list) essentially creates or establishes the focus of perception of the remote viewers. In practice, this telepathic connection creates the target for the experiment. The classical methods of isolating causality fail; neither time nor space act as separators between the remote viewers and the analysts.

The remote-viewing sessions are being done with the intent of satisfying the informational needs of the analysts, and the thoughts of the analysts at the time that they are analyzing the remote-viewing data indeed determine the focus of perception of the remote viewers. With respect to the "pick a target from the list of five" procedure, since the judges have all five targets on their minds when they compare the remote-viewing data with the targets to determine the best fit, any of the five targets are fair game for the perception of the remote viewers. In a very real sense, the judges are playing the unwitting role of outbounders in a period-one style remote-viewing experiment. The only known way to circumvent the problem is to utilize a spatial outbounder who is physically located at a target site at the time that the remote viewing is being done. With that approach, the dominant telepathic connection is with the spatial outbounder

in a situation of competing mental attractors (the analysts vs. the outbounder).

Thus, it is impossible to separate in classical terms the viewing from the analysis of the viewing. The time separation of the two events cannot causally separate the two events given the nonlocal nature of the remote-viewing phenomenon itself. Indeed, the computer program (or other random dynamic event) that supposedly chooses the "correct" target as mentioned in the "solution" above is irrelevant in constraining the focus of perception that the remote viewers experience. The experimental design itself is, in fact, corrupting the collection of the remote-viewing data, producing what one might call a telepathically induced "perceptual leak." This leak is not a minor issue; it is central to the remote-viewing phenomenon itself. In experiments using this procedure, a slight extra mental emphasis was usually placed on the so-called "correct" target due to extra mental focusing over time by the judges (especially post-target feedback), and this often allowed experimentalists to obtain statistical significance across trials despite the perceptual corruption. But these results were far less consistent than they would have been had the experimental design used been more appropriately structured to match the actual perceptual process inherent with the remote-viewing phenomenon.

Some researchers may object to some of my characterizations of early period remote-viewing research. For example, in the seminal study mentioned earlier by Puthoff and Targ (1976), a complex system of blind judging was utilized to evaluate nine remote-viewing trials, and the results were significant by any reasonable standard. Some researchers may argue that this demonstrates that blind judging procedures work well as traditionally configured, and that these methods were not limited to the second period of remote-viewing research. However, details matter, and one must remember that all of those trials in the Puthoff and Targ report involved a spatial outbounder. The remote viewers were tasked with perceiving the correct target as it was being concurrently perceived by the outbounder who was physically located at the target location. From the current perspective, this established a telepathic connection between the viewers and the outbounder that dominated any subsequent mental activity of the blind judges. The intent of the viewer was, in fact, to perceive what the outbounder was perceiving, not to satisfy the informational needs of the blind judges. Indeed, my argument in this report is that the use of a tasking temporal outbounder parallels the use of a spatial outbounder in controlling the focus of perception of a remote viewer, thereby making the description of a future event more accurate. Of historical interest, the original name for this project was "The Multiple Universes Project," which reflects the desire to test for a causal mechanism behind the success in using a temporal outbounder as a tasker for future-based remote-viewing sessions. However, no attempt is made here to address the causal mechanism underlying the phenomenon.

Other examples from the early period of remote-viewing research of experiments utilizing both blind judging and spatial outbounders are numerous. For example, statistical significance was found in two such remote-viewing experiments conducted by Bisaha and Dunne (1979, 2002). Readers interested in similar examples where blind judging procedures were used with experiments involving spatial outbounders might find the following reports of interest: Dunne and Bisaha 1979, Hastings and Hurt 1976, Schlitz and Gruber 1980, 1981, Schlitz and Haight 1984, Vallee 1988.

The Third Period of Remote-Viewing Research

We are currently in the beginning of the third period of remote-viewing research. In this period, the argument has been made (although it may not yet be fully accepted by the broader scientific community) that the experimental design in which "blind" judges evaluate sessions with respect to a small pool of targets is fatally flawed and needs to be abandoned entirely. It cannot be fixed by minor adjustment. Indeed, current research indicates that it is impossible to entirely remove the human element from the tasking and analysis aspects of the remote-viewing perceptual process. The key is to isolate the tasking and analysis processes such that the complaints made of the early period of remote-viewing research can be addressed without compromising the data-collection process itself. One solution is to place both the target and the tasking process in the future of the time when the remote-viewing sessions are done, as done in this study. This way the viewers can clearly be completely blind on a conscious level as to the nature of the target since it is in the future of the viewing time. Moreover, the pool of possible targets is essentially infinite for all practical purposes.

However, there is still the matter of isolating the influence of the initial analyst in the evaluative process. Since it has been argued (Brown 2006) that the thoughts of the analyst who first compares the remote-viewing data to the actual target can crucially affect the focus of perception that is experienced by the remote viewer, the experimental design for this study involves having the remote viewers themselves "close" their own sessions. Closing a session is the process in which the temporal loop between the time when the remote-viewing session is completed and the data are first analyzed is finalized. Closing a session essentially "seals" the data from telepathic contamination in the subsequent analysis process. The exact process of closing as performed in this study is described further below.

Having the remote viewers close their own sessions resolves another issue that was never fully understood during the first or second periods of remote-viewing research. Research conducted at The Farsight Institute over the past 15 years (summarized in Brown 2006) indicates that any extraneous thoughts

that the analyst may have about the viewer can also compromise the quality of the remote-viewing data. For example, if an analyst believes that men are better remote viewers than women, the remote-viewing data obtained by the male remote viewers will, on average, be better than those obtained by the women. It does not matter that the analyst has been separated from the viewer in classical terms of time and space. It does not even matter that the analyst is unknown to the viewer. The viewer is collecting the perceptual data to satisfy the informational needs of the analyst, whoever that person may be, and the thoughts of that person (any and all thoughts) constrain the perceptual focus of the viewer. Other analyst-held beliefs similarly telepathically introduce prejudices and other oddities into the data-collection process and need to be eliminated.

Emphatically, the remote-viewing phenomenon is based on nonlocal thought. Thoughts matter, and extraneous or leading thoughts cannot be fully isolated in time and space. The extraneous and leading thoughts need to be controlled in a manner that is not time- and space-dependent. Intermediate target possibilities evaluated by blind judges introduce biases and corruption in the data-collection process and must be avoided. The judges are unwittingly closing the sessions on all of the target possibilities, thereby creating erroneous foci of perceptions for the remote viewers. All of these problems are ameliorated or eliminated entirely when the remote viewers close their own sessions.

In practical terms, this means that for all of the remote-viewing sessions collected in this study, after the tasker chose a target, the target identity was first revealed individually to each remote viewer. Each remote viewer would then close his or her own session by carefully comparing each page of the session data with the actual target. This ensured that the focus of perception for each remote viewer was constrained by his or her own thoughts during each session closing. Indeed, when each session was posted to the web in encrypted form, the posting process was entirely mechanical using unopened PDF files, and only the remote viewer knew the content of the session. Thus, no human was allowed to see any of the remote-viewing data prior to the time when each remote viewer reported that the session was closed. Meticulous records were kept to ensure that these procedures were followed without violation. Only after a session was closed by a remote viewer were others allowed to see the session data and compare them with the actual target.

The Issue of a Traditional Control Group

It is now appropriate to ask if a traditional control group can be used in the current context to evaluate data involving descriptions of future events. This is a clear situation in which traditional approaches to experimental designs conflict with the remote-viewing process being evaluated. The primary element

being evaluated here is the use of a temporal outbounder (located in the future) to task the remote-viewing sessions. A control group must have everything else the same except for that single element. To construct an appropriate control group, one would have to utilize an all-knowing tasker at the time that the remote-viewing sessions are being done (or prior to their being done) who could accurately and perfectly predict an unanticipated future event 100% of the time. This is clearly not possible, and if it could be done, there would be no need to use remote viewing to describe the future events in the first place.

The best that can be done in terms of a control group is to utilize less formal comparisons, such as to compare the current results with relatively unsuccessful attempts at associative remote viewing, such as experiments described above and conducted at The Farsight Institute (Brown 2006:122–124). Also, informal comparisons can be made with respect to predictions of the future as described in reports such as the one prepared by the DIA for Dr. Vorona. The comparisons are not perfect, but they are nonetheless useful.

One of the important differences between the current experimental setup and associative remote viewing attempts is that the latter utilize only a small set of potential targets. In the current study, the future events are drawn from an infinite collection of future possibilities, many of which are entirely unpredictable using traditional means. Obviously such events could not be anticipated by the tasker in advance. Many of the targets involved in this study are of that nature, such as a tornado that rips through Meno, Arkansas, or the appearance of a spiral anomaly in the skies over Norway, or the malfunction of a major prop involved in an Olympic ceremony.

The fact that the current study draws its targets from an infinite set of future possible events adds emphasis to the current results. That is, from a probabilistic perspective, associative remote viewing using only a small set of potential targets should be much easier to do than what is done in the current study, and so the comparison with associative remote viewing strengthens the current results greatly. Thus, even though the comparison is not entirely appropriate, the statistical probabilities run strongly against the ability of the current experimental setup to succeed. The fact that the experiment does succeed so well is quite remarkable from a probabilistic point of view.

Nonetheless, I fully recognize that some, and perhaps many, researchers may wish to persist with the creation of a formal control group in a followup study related to this report. We must recognize that there will always be differences in opinion as to how to proceed, and I offer my views here as only one such opinion. I suggest only that future researchers attempt to address these concerns as best as they may see fit. In short, since it is not possible for a tasker to know of a future and unexpected event, the most likely solution to the problem will be to utilize an associative remote viewing design related to some future event

that is partially predictable. For example, let us say that an associative remote viewing design is developed with respect to a sporting event such as a football game. It is partially predictable since it can be assumed that the sporting event will in fact take place, and that there will be a winner in the competition. A rule could be devised stating that if team X wins the competition, then the remote viewers should perceive target A, and if team Y wins, then the remote viewers should perceive target B.

The problems with this approach should be obvious, and I offer no solutions to them here. The design does not come close to paralleling that used in the current study where no association is made between an external event and a target. This violates the principle in which a control group should involve one and only one thing different from the experimental group. Also, the associative remote viewing design ignores my previous argument as to what makes a target a target, which I argue has nothing to do with whatever rule someone may configure. The focus of perception for a remote viewer is dependent upon a telepathic connection between the viewer at the time of the viewing and the person conducting an analysis or closing process in which the session data are compared for the first time with the actual target. With associative remote viewing, the analysis is conducted prior to the decision event (such as the outcome of the sporting competition), which adds deep levels of telepathic conflict to the desired outcome of the remote-viewing experiment. Also, the rule of target association applied in associative remote-viewing experiments is not independent of this telepathic conflict. In my view, in any such future study utilizing an associative remote-viewing design as a control group, any appearance of a displaced target (that is, a good remote-viewing description of the wrong target given the associative rule) should be immediately recognized as a perceptual leak in the experimental design. The problem should not be ignored, nor should it be relegated to an anomaly involving a lack of independence across the target pool that is dealt with using statistical methods that essentially cover up an unacceptable design flaw.

With this said, I nonetheless offer a prediction. In future studies using a control group structured with an associative remote viewing design, the accuracy of remote-viewing sessions utilizing such an approach should be, on average, lower than that achieved using an approach employing a temporal outbounder as the tasker as has been done in this study. The manifestation of displaced targets in the associative remote-viewing results alone should guarantee this outcome, at least on average.

The question remains as to how the remote-viewing sessions in this study are to be evaluated. Clearly the process of having “blind” judges compare the sessions to small pools of targets, one real and the others decoys, is not a viable option for the reasons given above. Nor is it possible to construct a traditional

control group. There are three alternative approaches to session analysis that are possible. The first is for a post-closing analyst to evaluate each remote-viewing session with respect to the actual targets, and to rate each session according to the accuracy and depth of the target descriptions. This approach is useful since any subsequent reader can return to the original data to confirm the rating, and one does not have to rely on the one-time rating of "blind" judges who were unwitting participants in the data-collection process itself.

While some researchers do support using blinded judging in all situations (e.g., Hansen, Utts, & Markwick 1992), I have long pointed out (Brown 2006) weaknesses in their favored approaches to judging, many of which are mentioned above. But more generally, blinded judging is most appropriate in "proof-of-psi" investigations involving trials with a great deal of noise within the perceptual data. As mentioned previously, this is a process-oriented study, not a proof-of-psi study, and the structure and requirements of process-oriented studies differ from proof-oriented studies. One of the primary reasons for using highly trained remote viewers in studies such as the current one is that their data contain much lower levels of noise. Their data are also typically very rich in descriptive detail and highly unambiguous, often raising the judging process to the level of "obvious," blind or otherwise.

There is no scientifically justifiable reason why non-blinded judging should not be used in carefully applied situations that address process-oriented questions. For example, if all non-blinded scoring in the social sciences were eliminated, the fields of political science, sociology, anthropology, psychology, and others would not be recognizable. In the social sciences, we teach our graduate students how to score all sorts of data competently (violence scores for inner city riots, interview scores, newspaper content scores, etc.). We write peer-reviewed books about scoring procedures. The National Science Foundation spends millions of dollars each year supporting research that involves non-blinded scoring. Even survey research typically requires face-to-face interviews with respondents that involve the non-blinded scoring of data. The key criteria used in these fields are that the scoring process has to be fully explained, and the data must be publicly available for independent verification and replication using the same or alternate scoring criteria. Followup studies can then be done which employ alternative judging procedures to further test the initial results, including blinded judging when possible and/or appropriate.

One problem with using a standardized evaluative rating (blinded or otherwise) is that it does not allow for the identification of sessions that clearly and unambiguously describe unique elements in a target. These unique elements are important to the evaluation of remote-viewing data because some readers may think that a session can have a high rating simply due to the existence of standard elements that appear across many targets. A sensitive scoring process

applied in a consistent manner can resolve this, but readers are left with the task of checking the scoring personally. Until this check is done, readers may wonder if the scoring is compromisingly subjective. A solution to this is discussed immediately below.

Unique Element Portrayals

The second approach to analyzing remote-viewing data resolves this issue by identifying unique target elements that have been unambiguously described in a remote-viewing session. This is easy for others to check, and it is an entirely objective measure of session evaluation. Little or no interpretation is required on the part of the analyst with respect to this, and it is referred to as "Unique Element Portrayal," or UEP. In practice, a remote-viewing session is marked as containing a description of a unique target element by the placement of a UEP marker for the session. If a session has a UEP marker, this signifies that the session contains at least one description that unambiguously describes a unique element in the target.

A unique target element is some important target component that is not a normal element in other targets. For example, flat land would not be a unique target element since many targets are located on flat land, and the same could be said of blue sky. However, something much more specific with, say, a unique shape, purpose, or energy would be a possible unique target element. For example, if the target is a space satellite, the clear description of a space satellite together with an unambiguous, detailed, high-quality sketch would be a Unique Element Portrayal, and a session having such an unambiguous description would be signified with a UEP marker.

Unique Element Portrayals often involve highly specific sketches of some element of the target, although a highly specific verbal description can also qualify. Indeed, the ability of extensively trained remote viewers to obtain such descriptions is the primary reason for utilizing such viewers in a project such as this. Without such viewers, research utilizing UEP markers in the manner described above would not be possible.

Remote-viewing sessions that have a high rating as per the first method mentioned above (also, see "clarity scores" below) in addition to having been marked as containing at least one UEP are normally considered unambiguous evidence of remote viewing. Such sessions should normally satisfy the judging concerns of all reasonable people as indicating that the remote viewers have provided accurate descriptions of the given target.

A third method of analyzing remote-viewing data that does not run into the problems mentioned earlier with respect to the "pick a target out of five" process is to code each session "post-closing" with respect to a detailed collection of set characteristics and then compare this numerical dataset with a similar

numerical dataset for the given target. Statistical significance is found when comparing this session-target correspondence with similar correspondences that are obtained when comparing the remote-viewing session to a large pool of other targets. In practice, this is a complex process involving a large assortment of statistical evaluations, and it is used extensively in the report by Brown (2006). The software to conduct such analyses (the SAM program) is available for free from the website of The Farsight Institute (http://www.farsight.org). However, this analysis process is not included in the current report for reasons specified below.

This study employs the first two approaches to analyzing remote-viewing data as described above. That is, each session is compared with the target in a post-closing setting and rated with respect to the accuracy of the target descriptions. The method of rating is described below. Additionally, evidence of unambiguously accurate remote viewing is indicated by the identification of sessions with UEP markers. A number of examples of Unique Element Portrayal are also shown below to further clarify their unambiguous nature.

The third approach involving statistical evaluations of the sessions with respect to a large pool of targets is not used here because of (1) concerns of brevity, since such an evaluation would add a substantial new layer to the current report, and (2) little additional benefit would be gained from such a statistical analysis that is not already easily apparent with the application of the first two approaches in combination. Since all of the original data are available on the website for The Farsight Institute (http://www.farsight.org), interested readers can themselves confirm any of the session evaluations by examining the data in detail. Additionally, the objective nature of the UEP identifications as described below should unambiguously clarify for any reader the accurate nature of the remote-viewing perceptions obtained in this study.

I am a mathematician by training and vocation, and I teach statistics for a living, relying on them for a great deal of my work. But statistics are not a panacea. One does not need advanced statistics to conclude that a truck has just plowed into your garage. Statistics are obviously needed when dealing with remote-viewing data that are severely "fuzzy." For example, this is the motivation for the attempt by May, Utts, Humphrey, Luke, Frivold, and Trask (1990) to create a coding scheme for remote-viewing data using fuzzy set technology that is designed to enable computer-based analysis. While I have expressed serious concerns with how they operationalize their scheme (Brown 2006:113–116), the point to be made here is that such approaches are a necessary consequence of working with relatively poor quality remote-viewing data. When the quality of the data are poor, researchers need all sorts of tools to test for evidence of psi functioning, and statistical approaches are entirely appropriate in this respect.

But the entire purpose of working with highly trained remote viewers in the first place is to improve the quality of the remote-viewing data such that the evidence of psi functioning is self-apparent. Indeed, if evidence of psi functioning with respect to remote viewing remains dependent upon complex statistical approaches to be discerned, then remote viewing would never become of practical use. The fact that the military once relied on remote viewers for practical espionage purposes (and many suspect that a new secret program most likely still exists), suggests that it is possible for highly trained viewers to provide high-quality data that can be understood in a self-evident manner. The current availability of a pool of highly trained civilian remote viewers (albeit still a small pool) changes the psi-based research map in this respect.

The clarity scores described below in combination with the UEP identifications produce unambiguous self-apparent evidence of psi functioning. Many examples are offered in the discussion that follows. Anyone left unconvinced with such evidence will not be swayed by any statistical analysis, regardless of the tests used or the *p*-values obtained. These matters should become clear beyond dispute as the data are presented below.

Clarity Scores

All remote-viewing sessions in this study have been evaluated using "clarity scores." Clarity scores evaluate sessions with respect to known and verifiable characteristics of the target. Clarity scores can range from 0 to 3, and they convey the following meaning:

3: The known and verifiable target aspects are described exceptionally well with few, minor, or no decoding errors.
2: The known and verifiable target aspects are described well. There may be some notable decoding errors.
1: The known and verifiable target aspects are described minimally. There may also be significant decoding errors.
0: The known and verifiable target aspects are described very poorly or not at all.

Roughly comparing this coding scheme to the criteria used in Dr. Vorona's Sun Streak DIA report referenced previously, for a single session, a clarity score of 1 would be similar to a "weak correlation" for that session with respect to the remote-viewing data and the target, a clarity score of 2 would be a "moderate correlation," and a clarity score of 3 would be a "strong correlation." Alternatively, when quantitatively comparing the number of sessions in a collection that describe a target well (that is, a "hit"), the proportion of hits as reported to Vorona would be approximately comparable with the proportion of sessions in the current study having clarity scores of 2 or greater. Again, Vorona's report describes a hit rate of 13% to 18% of total sessions and describes this as a "weak correlation."

Decoding errors occur when a remote viewer perceives something that is real at the target, but the description of this perception is not entirely correct. For example, if someone describes a city with tall skyscrapers as a mountain range, that is a decoding error. The perception is correct in terms of the target's peaked topography, but the characterization of it as a mountain range is incorrect. Also, if a person places trees or animals in a barren natural landscape, that is a decoding error. The perception of a natural landscape is correct, but the conscious mind added things that it thought would be normal for a natural landscape. Remote-viewing training focuses on minimizing decoding errors, and experienced remote viewers can often become quite proficient with this. Different remote-viewing methodologies address this issue in different ways, although there is considerable overlap in theory.

For this study, 86 remote-viewing sessions were conducted across 11 highly diverse targets, with 12 participating remote viewers using four remote-viewing methodologies (HRVG, CRV, SRV, and TDS). This is one of the largest remote-viewing studies of its kind using structured remote-viewing methodologies that are the same as or derived from those developed by the United States military. Again, the targets for this study are highly diverse, and they are listed in Table 1.

TABLE 1
The Targets Chosen for the Multiple Universes Project

Targets for All Experiments	Target Month*	Tasker
Lighting the Cauldron at the 2010 Olympic Games	February 2010	Lyn Buchanan
The Norwegian Spiral Anomaly	December 2009	Glenn Wheaton
The Macy's Thanksgiving Day Parade	November 2009	Lyn Buchanan
NASA's LCROSS Mission to the Moon	October 2009	Glenn Wheaton
An Oktoberfest at Holloman Airforce Base	September 2009	Lyn Buchanan
The Edinburgh Military Tattoo in Scotland	August 2009	Glenn Wheaton
The Estonian Laulupidu (Song Festival)	July 2009	Lyn Buchanan
Paragliding Landings Event at the World Games in Turin, Italy	June 2009	Glenn Wheaton
The Landing of the Space Shuttle Atlantis	May 2009	Lyn Buchanan
A Tornado in Mena, Arkansas	April 2009	Glenn Wheaton
The Launch of the Kepler Mission	March 2009	Lyn Buchanan

* The remote-viewing sessions were completed prior to the target month, and the target was chosen by the tasker the month after the target month. Each target event happened during the target month. Thus, three sequential months were blocked off as follows: 1) sessions completed, (2) target event happens, (3) target is chosen from the set of anything that happened during the second month.

The months within which the target events took place are also listed in Table 1, together with the name of the tasker for each target. All of the remote-viewing data for these experiments (scans of all original session pages), summaries of all session results, plus detailed descriptions of all of the targets (including images and video), can be found at the following url: http://www.farsight.org/demo/Multiple_Universes/Multiple_Universes_Experiment.html

Target Criteria

The taskers for this project were given clear instructions regarding how to pick targets for this project. The goal was to maximize the amount of meaningful information that is available for each target, which is related to the concept of "Shannon Entropy" as discussed by May, Spottiswoode, and James (1994) as well as Watt (1988). (See also May, Spottiswoode, & Faith 2000.) Here, preferred targets are ones with dramatic visual appeal or activity. In addition to more general guidance to target specification (see "The Prometheus Protocols" on the website for The Farsight Institute for full details, http://www.farsight.org/SRV/Prometheus_Protocols.pdf), the taskers were given the following explicit instructions:

1. All targets must be events that take place during an assigned month. There should be significant activity at the target site. Thus, Tiger Woods golfing is not a good target since there is not much activity with such a target (i.e., just people standing around on generally flat land).

2. Targets should involve a large geographical area with significant topological variety. Thus, a birthday party in someone's backyard would not be an appropriate target. On the other hand, an eruption of a volcano would be an appropriate target.

3. Most targets should be external, in the sense that targets should not be small things embedded in a structure or other macro environment. For example, someone placing a flower pot on a kitchen table is not an appropriate target for the Multiple Universes Project. Exceptions to this rule would be targets that involve large events with significant activity inside of large structures, such as sporting or musical events.

4. All acceptable targets for the Multiple Universes project must be verifiable to a worldwide audience using normal Internet sources. Thus, a clipping from a local newspaper about some event would not be acceptable.

5. Targets should be newsworthy, in the sense that the general public should have a natural interest in the target.

6. Targets do not need to be found on Earth. Targets off-planet are acceptable as long as the target locations and events are verifiable using normal Internet news sources.

7. One last (and crucially important) point. The target must be chosen by the tasker without ANY input or assistance at all—of any type—from anyone else. The tasker/targeter must not communicate any information about the target to anyone prior to the target's selection. This includes spouses, best friends, workmates, or absolutely anyone else.

Results

A wide-angle view of the project results organized by targets is presented in Table 2. In Table 2, the total session clarity scores for each of the targets and all sessions are listed, as well as the percent for each clarity score. For example, with respect to the "Lighting the Cauldron at the 2010 Olympic Games" target, four sessions had clarity scores of 3, two sessions had clarity scores of 2, and three sessions had clarity scores of 1, for a total of nine sessions. There were no sessions with clarity scores less than 1. In terms of percent, 66% of the sessions had clarity scores of 2 or 3, which is a sound performance for psi-based data.

TABLE 2
Clarity Score Totals for All Sessions by Target (raw:percent)

Targets for All Experiments	3	2	1	0	Total # of Sessions
Lighting the Cauldron at the 2010 Olympic Games	4:44%	2:22%	3:33%		9
The Norwegian Spiral Anomaly	3:50%	1:17%	2:33%		6
The Macy's Thanksgiving Day Parade	1:17%		4:67%	1:17%	6
NASA's LCROSS Mission to the Moon	6:86%		1:14%		7
An Oktoberfest at Holloman Airforce Base	2:22%	2:22%	3:33%	2:22%	9
The Edinburgh Military Tattoo in Scotland	2:25%	1:13%	5:63%		8
The Estonian Laulupidu (Song Festival)	1:17%	1:17%	3:50%	1:17%	6
Paragliding Landings Event at the World Games in Turin, Italy	1:13%	1:13%	6:75%		8
The Landing of the Space Shuttle Atlantis	2:25%	3:38%	2:25%	1:13%	8
A Tornado in Mena, Arkansas	6:60%	2:20%	2:20%		10*
The Launch of the Kepler Mission	4:44%	5:56%			9
Totals	32	18	31	5	86
Percent	37%	21%	36%	6%	100%

* The total number of sessions done for this target was 11. One session was removed from this pool because of an accidental closing error, thereby invalidating the session.

Summarizing the results for all targets at the bottom of Table 2, 37% of all sessions had clarity scores of 3, 21% had clarity scores of 2, 36% had clarity scores of 1, and 6% had clarity scores of zero. Thus, more than half of all 86 sessions (58%) had clarity scores of 2 or 3. When compared with the description

in Vorona's report of weak correlations between remote-viewing sessions and future targets of 13% to 18%, the current results are remarkably strong. It is worth reminding the reader that the assertion of this study is that the difference between the results for the two projects is fundamentally related to the use of a temporal outbounder as the tasker.

This conclusion can be further enhanced by examining the clarity scores when organized by viewers, as is presented in Table 3. Table 3 includes the raw clarity scores for all viewers, as well as the associated percentages. There will always be performance variations among viewers. Remote viewing is both

TABLE 3
Clarity Score Totals for All Viewers (raw:percent)

Remote Viewers for All Experiments	3	2	1	0	Total Number of Sessions
Daz Smith (CRV)	5:45%	2:18%	4:36%		11
Pat Sage (CRV)		1:100%			1
Dan Chevalier/Romferd (CRV)	2:33%	1:17%	3:50%		6
Rising Sun Touch (CRV)	1:50%		1:50%		2
Apollo 1969 (CRV)		3:100%			3
Dick Allgire/Viewer 212/Allergic Kid (HRVG)	9:75%	2:17%	1:8%		12**
Debra Duggan-Takagi (HRVG	4:40%	2:20%	3:30%	1:10%	10
Sita Seery (HRVG)	2:25%	1:13%	5:63%		8
Maria (HRVG)	4:36%	2:18%	2:18%	3:27%	11
Anne (HRVG)	2:17%	4:33%	6:50%		12**
Michele (HRVG)*			6:100%		6
Athena (SRV)	2:67%			1:33%	3
Athena (TDS)	1:100%				1
Totals	32	18	31	5	86
Percent	37%	21%	36%	6%	100%

* Michele was a new viewer who had just begun her training in the HRVG methodology. She used a portion of the methodology.
** These viewers did more than one session for some targets.

skill- and talent-based, like any other performance. For example, one listens to a piano recital by Lang Lang because of a desire to hear beautiful music played by a virtuoso. Part of Lang Lang's performance is based on talent, and part is based on learned skill. To some extent, one can substitute skill for talent, and talent for skill. But when high levels of both talent and skill are combined, consistently sublime music is the result.

From Table 3 we can identify viewers who consistently achieve relatively high levels of remote-viewing performance, and this can offer significant theoretical purchase with respect to the central hypothesis for this study. Consider the viewers who contributed ten or more sessions for this project. In particular, note that the results for Dick Allgire are remarkably strong. Indeed, nine of his 12 sessions (75%) had clarity scores of 3, and 92% of his sessions had clarity scores of 2 or greater. Similarly, note that Daz Smith conducted 11 sessions for this project, and 63% of his sessions had clarity scores of 2 or greater. Also, 60% of Debra Duggan-Takagi's ten sessions had clarity scores of 2 or greater, and 50% or more of the sessions conducted by Maria and Anne had clarity scores of 2 or greater. Thus, while we do see variation across viewers, the fact that even one viewer (for example, Dick Allgire) can have such unambiguous results offers support for the central hypothesis of this report.

Sessions with Unique Element Portrayals

Not all sessions with clarity scores of 3 had Unique Element Portrayals. And while it is possible for a session with a clarity score of 2 to have a UEP, it is unlikely to happen in practice. Usually, a viewer needs to have a deep level of target contact in order to have a UEP, and it is almost a rule that a session with a UEP will nearly certainly contain a great deal of additional correct information such that it obtains an overall clarity score of 3. In this experiment, no sessions with clarity scores below a 3 contained a UEP. Thus, we need focus only on those sessions with clarity scores of 3 which also contain at least one description that is a compelling description of a unique element in the target.

Table 4 summarizes all sessions with clarity scores of 3 which also have at least one UEP. Among all 32 sessions with a clarity score of 3, 27 of those sessions also contain at least one UEP, or 84%. In comparison with psi research using minimally trained or untrained viewers (which involves the bulk of the extant remote-viewing studies originating from the second period of remote-viewing research as identified previously), this is an exceptionally high number, and this is another indicator of the value of extensive and long-term training in a structured remote-viewing methodology.

It is difficult to appreciate the unambiguous nature of UEPs without actually looking at the data themselves. At this point, it is useful to show some examples of remote-viewing data for sessions with both a clarity score of

TABLE 4
Unique Element Portrayal (UEP) for All Sessions with Clarity Scores of 3 by Target

Targets for All Experiments	Number of UEP	Number with Clarity 3	% UEP	Total Number of Sessions
Lighting the Cauldron at the 2010 Olympic Games	4	4	100%	9
The Norwegian Spiral Anomaly	3	3	100%	6
The Macy's Thanksgiving Day Parade	1	1	100%	6
NASA's LCROSS Mission to the Moon	4	6	67%	7
An Oktoberfest at Holloman Airforce Base	2	2	100%	9
The Edinburgh Military Tattoo in Scotland	1	2	50%	8
The Estonian Laulupidu (Song Festival)	1	1	100%	6
Paragliding Landings Event at the World Games in Turin, Italy	1	1	100%	8
The Landing of the Space Shuttle Atlantis	2	2	100%	8
A Tornado in Mena, Arkansas	4	6	67%	10
The Launch of the Kepler Mission	4	4	100%	9
Totals	27	32		86
Percent	31%	37%	84%	100%

3 and a UEP, and for this purpose remote-viewing sketches are particularly helpful. For the sketches shown below, accurate verbal descriptions accompany these sketches within the sessions. Nonetheless, most readers will see that the sketches themselves make the point. Examples of some verbal UEPs are offered following the discussion of the sketches. It is not possible to present all UEPs (sketches or verbal) here for reasons of space. But all of the data are available for inspection on the website for The Farsight Institute, and readers are encouraged to examine all the data for themselves.

Figure 3 is a sketch drawn by Dick Allgire of the tornado entering Mena, Arkansas, which is the target event that occurred during April 2009 for this project. Figure 4 is a sketch of the internal structure of the tornado, taken from the same session as the sketch in Figure 3. From another session, this time

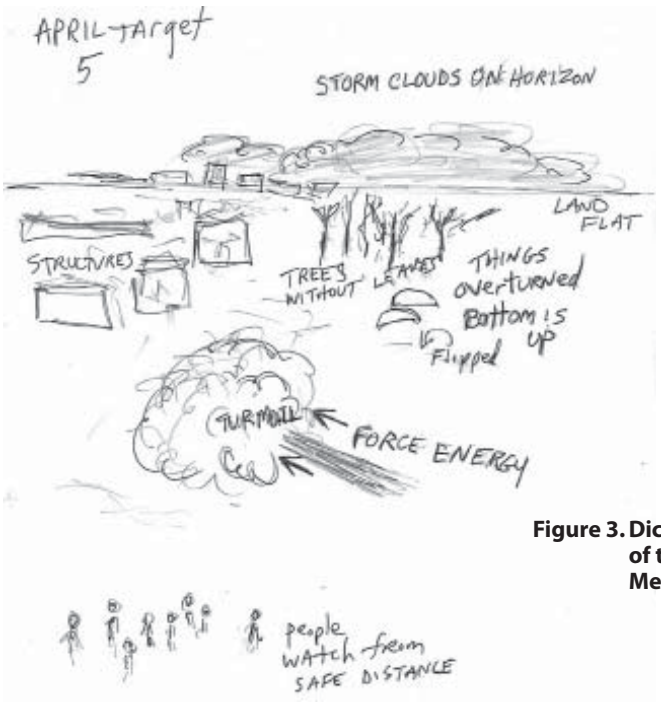

Figure 3. Dick Allgire's sketch of the tornado in Mena, Arkansas.

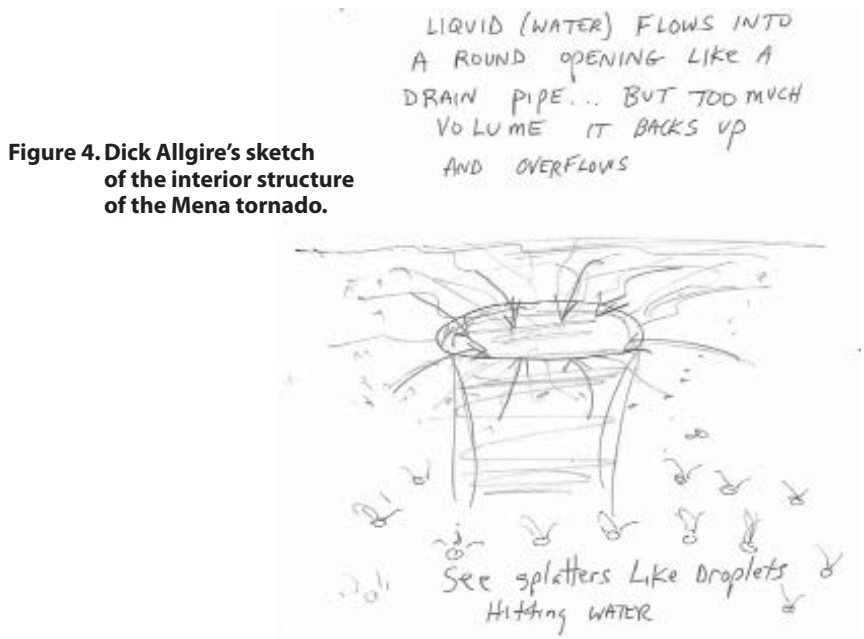

Figure 4. Dick Allgire's sketch of the interior structure of the Mena tornado.

describing NASA's LCROSS Mission to the Moon, Figure 5 is one of Allgire's sketches of the LCROSS satellite side by side with a NASA artist's representation of the actual satellite. These are unambiguous remote-viewing descriptions of these targets, and the verbal descriptions that accompany these sessions are equally accurate.

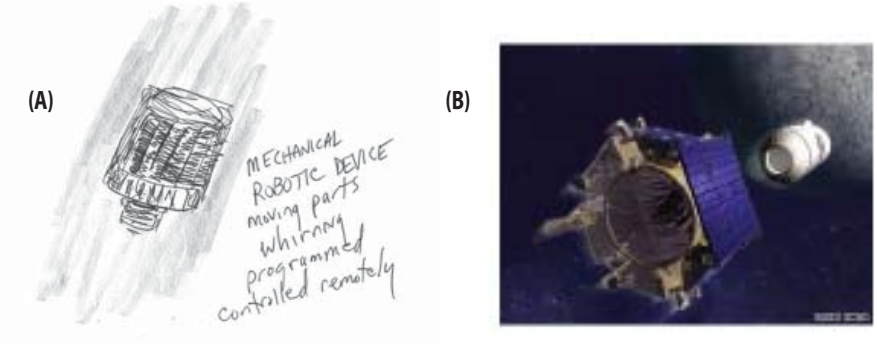

Figure 5. Dick Allgire's remote-viewing sketch of NASA's LCROSS satellite (A), together with an artist's representation of the actual satellite (B).

Figure 6 is one of Debra Duggan-Takagi's sketches of the Kepler launch in Cape Canaveral, Florida. This is a very detailed sketch showing the coast, the energy released by the launch itself, and many of the vehicles that are deployed to observe and track the launch. Note that she is able to identify the purposes

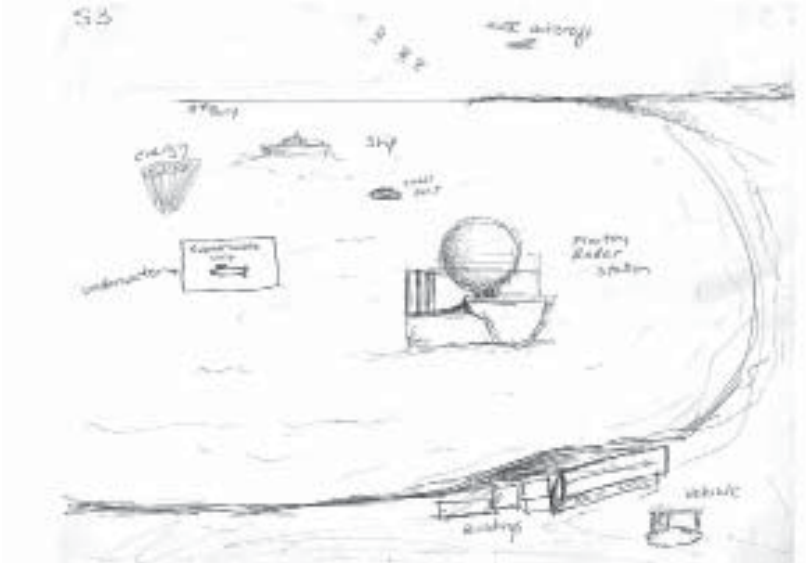

Figure 6. Debra Duggan-Takagi's sketch of Cape Canaveral, Florida, during the launch of the Kepler mission.

of many of the vehicles, such as a floating radar station, a submarine, various aircraft, and other structures.

Sometimes the remote-viewing sessions shed light on some mysterious aspects of a target. For example, the spiral anomaly that occurred above Norway in December 2009 caused quite a stir in the media and on the Internet, with some people speculating that the anomaly could be an alien spaceship or wormhole. The remote-viewing sessions for that target show no esoteric content, and the data seem to affirm the official position that the anomaly was a result of a Russian missile test gone astray. Athena has an excellent session for that target, and one of her sketches of the anomaly is presented here as Figure 7. Daz Smith also draws a good sketch in his session of the circular ring structure of the anomaly, and this is shown in Figure 8.

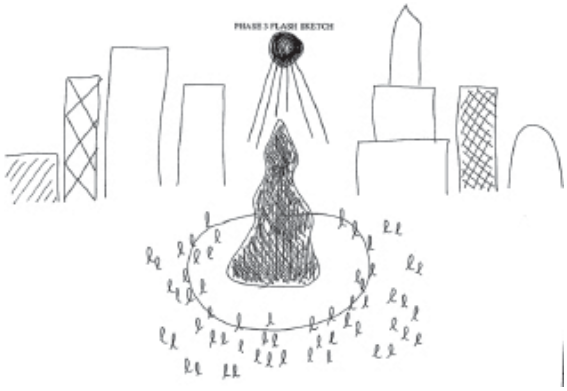

Figure 7. Athena's sketch of the spiral anomaly above Norway in December 2009.

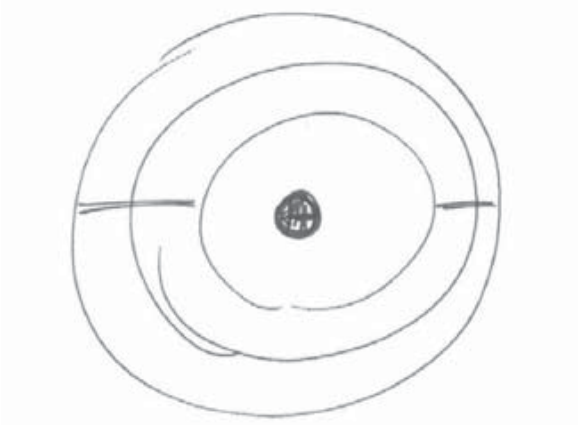

Figure 8. Daz Smith's sketch of the circular ring structure of the Norwegian spiral anomaly.

Again, some UEPs are verbal. For example, Rising Sun Touch correctly identifies the launch of the Kepler Mission from Cape Canaveral as a missile launch. Daz Smith correctly identifies the tornado in Meno, Arkansas, as a tornado. Sita correctly notes that the LCROSS mission to the Moon involves a spinning metallic object crashing into wet rocks. Dick correctly notes that the three pillars supporting the Olympic Cauldron (accurately sketched) during the opening ceremonies of the 2010 Olympics Winter Games are both a structure and a machine. Romferd correctly notes that this same machine is partially broken (the fourth pillar did not rise). Again, while it is not practical to include a complete list of UEPs here, scans of all remote-viewing sessions (all original data in convenient PDF format) plus brief session summaries for this project are maintained on the website for The Farsight Institute.

Discussion

Let us be clear that we do not yet understand the physics involved with the remote-viewing phenomenon. There are obvious hurdles to overcome. This is not the place to resolve the issue of what constitutes physical reality. Just as physicists have spent decades arguing about string theory, scientists can spend additional decades trying to fit the results presented in this and similar studies into a more cosmologically complete theoretical framework. We need not be dismayed that our current set of theories about physical reality are incomplete. In truth, we already knew that they were incomplete when science posited the divide between the quantum and classical realms. How can the classical realm work on one set of relativistically guided principles while the quantum realm from which the classical realm arises works on a completely different set of principles? Simply put, Philipp von Jolly's advice to Max Planck that he should not go into physics because everything important has already been discovered is as incorrect now as it was in 1874. This is clearly not the time to celebrate how much we know, but rather to look forward to how much we still have to discover.

The results from this study should give us new insight with which we can shape our future theories of time and physical reality. That these ideas may be much different from what we have previously envisioned should not worry us as much as the possibility of continuing to think incorrectly about that reality. These results encourage the idea that remote-viewing experiments can be configured in such a manner as to shed light on the nature of time. This supports the notion that we are on the cutting edge of a new understanding of the nature of existence that will potentially yield significant changes in how we view the temporal evolution of events.

Acknowledgments

This research could not have been accomplished without the help and support of Glenn Wheaton and Lyn Buchanan. They actively encouraged their viewers to participate in the project as unpaid volunteers. Glenn and Lyn are retired military practitioners of remote viewing. Glenn came out of the Intelligence wing of the U.S. Army's Special Forces branch. He was involved in what was essentially a decentralized "movement" within the military that eventually became popularly known as "The First Earth Battalion." Lyn retired from the U.S. Army's Intelligence and Security Command (USAINSCOM), and he now leads a group of viewers who perform Controlled Remote Viewing (CRV). These two groups developed separate approaches to remote viewing. Both Glenn and Lyn have spent the last 15 years teaching remote viewing as it was done in the military to a significant number of enthusiastic students, and they trained most of the viewers who participated in this project. I must also give a great deal of thanks to the remote viewers themselves. They volunteered their time for this project, as they have volunteered their time for many of my science projects over the years. Some of the viewers desire to be identified by "tags" rather than their real names, possibly due to the continued controversy associated with this new field. Thus, in terms of those tags, the viewers who participated in this project are Daz Smith (CRV), Pat Sage (CRV), Rising Sun Touch (CRV), Apollo 1969 (CRV), Dan Chevalier/Romferd (CRV), Dick Allgire (HRVG), Debra Duggan-Takagi (HRVG), Maria (HRVG), Anne (HRVG), Michele (HRVG), Sita Seery (HRVG), and Athena (SRV and TDS). This research was conducted at The Farsight Institute, which is not affiliated with any other institute or university.

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RESEARCH ARTICLE

Relativistic Variations in the Permittivity and Permeability of Free Space = Gravitation

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Abstract—This paper describes gravitation in terms of electromagnetism, arguing that the dielectric properties of free space: permeability μ_o and permittivity ϵ_o are not fundamental constants but vary in accordance with Einstein's relativity, adjusting the metrics of *length* and *time* so that all observers measure the speed of light *C* and the gravitational constant *G* as fundamental constants; that inertia is the resistance to the change in an accelerating particle's relativistic mass, as it responds to an applied force, keeping its mass-to-energy ratio constant; that curved space is a form of mass/energy where gravitational rest mass and relativistic mass manifest themselves as energy gradients in the dielectric properties of the space that surrounds a particle, such that a slow-moving heavy rest mass particle is equivalent to a fast-moving light rest mass particle.

Keywords: gravitation—electromagnetism—permeability—permittivity—relativity

Equations:

- $E = mC^2$ (1)
- $C = 1/\sqrt{(\epsilon_o \mu_o)}$ (2)
- $m / E = \epsilon_o \mu_o$ from (1) and (2) (3)
- $E = hv$ (4)
- $m / hv = \epsilon_o \mu_o$ from (3) and (4) (5)

Introduction

One aspect of Einstein's relativity tells us that the metrics of *length* and *time* vary so that all observers measure *C* and *G* as fundamental constants. Despite the widespread acceptance of relativity, some authors continue to argue that gravitation can be explained in terms of varying dielectric properties of space resulting in a varying refractive index and speed of light. In addition, attempts have been made to argue that *G* varies in accordance with *Mach's principle* which suggests that the gravitational force is a function of the average mass

density in the universe and is affected by distant masses. In 1921, Wilson (1921) made one of the earliest attempts to explain gravitation in terms of varying dielectric properties of space:

> . . . matter is believed to be composed of electrical charges, tends to move through the ether in the direction in which the specific inductive capacity and permeability of the ether increase most rapidly and that this is the cause of gravitation.
>
> It has always been difficult to believe that the ether in a gravitational field contains less energy per unit volume than the ether at a great distance from matter.

Contemporary authors continue to build on Wilson's electromagnetic approach to gravitation:

> . . . the language of classical optics is as suitable as that of Riemannian geometry for the study of electromagnetic phenomena in a gravitational field. (Felice 1971)
>
> . . . the velocity of light *C* in the Lorentz transformation and elsewhere is replaced by the velocity of light in a medium of variable refractive index . . . (Puthoff 2002)
>
> . . . a gravitational field is assigned a variable refractive index . . . (Boonserm, Cattoen, Faber, Visser, & Weinfurtner 2005)
>
> . . . with a variable velocity of light or a variable vacuum refractive index induced by the gravitational matter. (Ye & Lin 2008)

On the subject of a varying gravitational constant, Dicke (1957, 1961) discusses gravitation without a *principle of equivalence*:

> . . . one should not infer that the gravitational acceleration observed in a laboratory moving at high velocity relative to distant matter in the universe would be constant, for such has never been observed.
>
> The motion of the Earth relative to the distant matter . . . could be a factor influencing the locally observed gravitational acceleration. . . .

While all of the aforementioned papers approach the subject differently, using different mathematical techniques to describe the properties of the vacuum, their proposals share in common a varying value of *C* which is diametrically opposed to relativity. Before arguing that it is possible to vary the dielectric properties of space without violating the tenets of relativity, it is instructive to consider the following thought experiment:

Clearly, if a particle could travel at an infinite velocity, the universe would be infinitely big and the particle would be everywhere at once, colliding with every other particle simultaneously, resulting in a universe without cause and effect. An upper limit to velocity creates order out of chaos and leads to the evolution of life. But an upper limit to velocity can be meaningful only if all observers, particle masses included, measure exactly the same value and agree that it is a

fundamental constant, otherwise there is no upper limit and the infinite velocity problem remains. Given that observers travel at various velocities when they make their measurement, relativity tells us that the metrics of *length* and *time* vary for each observer as a function of their velocity with respect to *C*, to ensure they all measure the same value of *C*. In addition, relativity tells us that when we accelerate a particle we add energy to the particle's rest mass such that the mass increases toward infinity as it approaches *C*, thereby preventing the particle from traveling faster than *C* and that this additional mass has an equivalent form of energy in accordance with Einstein's equation (1). It therefore seems reasonable to suggest, that in order to have a universe with cause and effect, leading to the formation of fundamental particles which combine to form living creatures, that there has to be a maximum velocity, which we all agree on and if it is not *C* it would have to be some other *constant* value.

Accepting that *C* is a fundamental constant we might argue that if *C* and *G* are fundamentally linked then *G* must also be a fundamental constant. To argue that for any given *constant* value of *C* there is a correspondingly unique value of *G,* we consider the following thought experiment consisting of a *fast* and *slow* universe. In the fast universe, the value of *C* is a trillion times its actual value and we see from Maxwell's equation (2) that the product $\epsilon_o \mu_o$ has to be much lower in order to obtain this value of *C*. In addition the *m/E* ratio in equation (3) must also be lower as it is equal to $\epsilon_o \mu_o$. We apply a force to a particle and plot its relativistic mass as a function of its velocity with respect to *C* noting that it takes much more work/energy to reach an infinite mass compared to a particle in our universe. We assume that inertia is much less in the *fast* universe and argue that due to the *principle of equivalence* gravity must also be less which leads us to intuitively suspect that *C* and *G* are fundamentally linked.

We consider the *slow* universe, where *C* is one trillionth of its actual value and note that the product $\epsilon_o \mu_o$ is much higher, as is the m/E ratio. Once again we plot the particle's relativistic mass as a function of velocity with respect to *C* noting that inertia and gravity are much greater in the *slow* universe compared with ours. While in principle any pair of values of *C* and *G* might lead to the evolution of life, most physicists would agree that by virtue of the relationship of these constants to others in nature, as discussed by Barrow, Tipler, and Wheeler (1988) in terms of the *anthropic principle*, it is unlikely that life would evolve in the *slow* or *fast* universes described above. We can, however, conclude that there must be some intermediate universe where the value of *C, G,* and $\epsilon_o \mu_o$ are such that life can form and that this is the universe we live in. From the above thought experiment we see that for every value of *C* there appears to be a correspondingly unique value of *G* which leads us to think that inertia, gravitation, and the dielectric properties of space are related to the change in the relativistic mass of an accelerating particle as it maintains a constant *m/E* ratio with respect to *C*.

We see that the mass/energy ratio on the left hand side of equation (3) is representative of Einstein's relativity while the dielectric *constants* of Maxwell's electromagnetism occupy the right hand side, and we wonder if relativity and electromagnetism could actually be two sides of the same coin. We now revisit the thought experiment to consider the physics of what happened when we applied a force to accelerate the particle. First we injected energy into the particle by applying a force, thereby increasing E in equation (3). We know that the product of the dielectric properties of space on the right hand side of equation (3) must remain constant in order to not violate C and consequently the mass must have increased in order to keep the m/E ratio constant. We now consider what mechanism allowed the force to do the work that injected mass/energy into the system and we look to the dielectric properties of space on the right hand side of equation (3) realizing that their product can remain constant while the dielectric values can change reciprocally. Permittivity can decrease as permeability increases or vice-versa, allowing the applied force to introduce energy by doing work on the dielectric properties of space and thereby changing their values and storing energy. We know from electronics that energy can be stored in a capacitor when a voltage is applied to two conductors that are separated by a dielectric, suggesting that the storage of energy in the dielectric properties of a vacuum, in the presence of a mass or fast velocity, may not be unreasonable.

In a universe populated with moving masses, space can never be perfectly flat and Maxwell's equations are not the complete story. Maxwell's equation (2) describes flat space, making no predictions as to the bending of light or the changing of its frequency in the presence of a mass. Given that light does bend as it passes a mass leads us to ask how a photon senses the curvature of space if space has no properties which can be curved. A particle gaining energy in a gravitational field leads us to wonder where the gravitational potential energy was stored before it was absorbed by the particle as kinetic energy. An atomic clock knows how much to slow down in a gravitational field or inertial reference frame. Because the photon, particle, and clock are interacting with space, then the properties of *space* must store the energy and control the metrics of *length* and *time*. Clearly *space* knows how much energy to impart to a particle and how much it must slow down an atomic clock as a function of the velocity of the reference frame and gravitational intensity with respect to C and G. Moreover, one part of space must be able to differentiate its properties from another part of space such that it agrees with the inverse square law ($1/R^2$). In all the aforementioned examples the properties of space seem to play a fundamental role, and unless space has some other properties that we are not aware of then it must be the changes in permittivity and permeability that store the energy and adjust the metrics of *length* and *time* such that an atomic clock dilates and a ruler measures the Lorentz contraction. Maxwell's equations have the electric

and magnetic vectors at right angles, resulting in the speed of light propagating at right angles to both alternating vectors and in a straight line with no work being done. If the electric and magnetic vectors of free space are distorted such that their attributes are changed by the space in which they are imbedded, work may be carried out on a photon's frequency as it interchanges energy with the distorted properties of curved space and this may be permitted by equation (5).

But why should the dielectric constants of space vary when most textbooks and physics dictionaries list them as fundamental constants? It is interesting that in Maxwell's equation (2) there are supposedly three fundamental constants with no variables. Because permeability and permittivity contain the units of *length* and *time*, it is arguable that these so-called *constants* should vary in accordance with relativity, otherwise relativity becomes questionable. The units of permittivity (farads per meter) are a measure of the resistance of a dielectric material to the passage of an electric flux in response to an applied electric field. The units of permeability (volt seconds per amp meter) provide a measure of the generation of a magnetic field from a changing electric field. If relativity is correct then the dielectric properties of space must be variables.

Modern quantum physics tells us that space is a sea of virtual particles which are constantly being created and destroyed, as discussed by Urban, Couchot, and Sarazin (2011), and this agrees with the proposal that curved space can store energy. If curved space can store energy then space must be an equivalent form of mass with properties such as linear and angular momentum, and it should not be surprising that particles can be created from the energy–mass stored within space curvature. Another thought experiment allows us to create a recipe for creating a menu of particles from the dielectric properties of curved space: Take a quantized volume of spinning curved space and polarize its dielectric properties to point inward or outward so that it is either negatively or positively charged (a zero-charged particle would result from zero polarization). Allow the particle's surface to introduce a discontinuity, in proportion to its rest mass, with the dielectric properties of the surrounding space so that an energy gradient is formed which decreases in accordance with Newton's gravitational inverse square law. Create a second particle in close proximity to the first particle and take note of any forces that result between the particles. We might find that there are two distinct forces between the particles. The first (electromagnetic) force might result from the two charged particles trying to orient the dielectric properties of their surrounding space in the opposite sense to each other. The second (gravitational) force might arise from the discontinuity that each particle forms between its surface and the dielectric properties of the surrounding space, resulting in two overlapping inverse square energy gradients. The overlapping energy gradients cause an overall asymmetry which space tries to reduce by accelerating the two particles toward each other

in order to reduce its asymmetrical curvature. The particles appear to attract because the curvature of space that each particle introduces to the space between the particles cancels out, allowing the curvature of space on the opposite sides of the particles to reduce curvature by accelerating the particles toward each other, thereby imparting kinetic energy to the particles. Similarly it is the dielectric curvature of space that is increasing as it decelerates the masses as they move away from each other, thereby absorbing and reducing their kinetic energy. This explains why a light rest mass particle traveling at a high velocity can have the same dielectric values as a slower moving heavier rest mass particle and why inertial and gravitational mass lead to the *principle of equivalence*. Although the recipe for particle creation, developed from our thought experiment, is somewhat oversimplified, it illustrates how quantum physics could create particles and forces simultaneously from the dielectric properties of space.

There is the possibility that enormous amounts of energy can be stored in the dielectric properties of space, and with infinitesimally small distances quantum physics may impose restrictions on position, momentum, time, and distance as a consequence of the presence of Planck's constant in equation (5). There are a number of hypothetical, but as yet unproven theories associated with gravitation: extra dimensions, zero point energy; vacuum energy; dark energy and the graviton. The proposal presented above does not postulate any new physics but simply argues that if permittivity and permeability vary in accordance with relativity, in the presence of mass or an inertial reference frame, then energy can be interchanged between particles and space, adjusting the metrics of *length* and *time* so that all observers agree that C and G are fundamental constants and this explains why masses appear to attract each other.

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HISTORICAL PERSPECTIVE

The Psychic Sciences in France: Historial Notes on the *Annales des Sciences Psychiques*

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Abstract—This paper is an overview of aspects of the French journal *Annales des Sciences Psychiques* (*ASP*, 1891–1919). The *ASP* was founded by Charles Richet and Xavier Dariex. The development of the journal was assisted both by the prestige and influence of Richet as a scientist and of Félix Alcan as a publisher. For the nineteenth-century period the journal emphasized cases and experiments over theories. Much of this was about spontaneous telepathy and physical mediumship. Some of the authors included in the pages of the *ASP* were Émile Boirac, Ernesto Bozzano, Albert de Rochas, Giovanni Battista Ermacora, Paul Joire, and Julian Ochorowicz. The journal provided a forum in France to argue about standards in psychical research, discuss controversies, and to bring in information on the topic from foreign countries. This included translations of the work of members of the Society for Psychical Research, such as Frederic W. H. Myers. In 1908 the *ASP* was affiliated with the Sociétè Universelle d'Études Psychiques, becoming its official publication. The *ASP* was important in establishing standards and in providing a forum for the development of psychical research in France.

Keywords: *Annales des Sciences Psychiques*—French psychical research—history of psychical research—mediumship—Charles Richet

The March 1891 issue of the *Journal of the Society for Psychical Research* announced the forthcoming appearance of a French periodical—the *Annales des Sciences Psychiques* (*ASP*)—to discuss psychical research "under the strictest conditions obtainable by accuracy and care" (*Annales des Sciences Psychiques* 1891:46). The *ASP*, discussed in recent writings (e.g., Alvarado, Biondi & Kramer 2006:67, Lachapelle 2011:88–91, Monroe 2008:207–209), represented an important effort in France to record and disseminate observations

and research related to psychic phenomena. In the current paper we will present additional information about the journal focusing on its content, orientation, publisher, and reception. While the journal was published between 1891 and 1919, in some parts of the paper we will focus on its nineteenth-century period. It is our hope that our discussion of the *ASP*, which is meant to present a panoramic view of the topic, will inform *JSE* readers of aspects of the history of French psychical research that may be difficult to study due to language barriers.

French Interest in Psychic Phenomena

By the time the *ASP* appeared, there was already a long history of French interest in mesmerism and spiritism.[1] These developments were instrumental in defending the existence of, and in popularizing, ideas such as the action of animal magnetism and spirits of the dead.

Physician and popular science writer Louis Figuier (1819–1894) summarized developments from antiquity to the mid-nineteenth century for the French public in his four-volume work *Histoire du Merveilleux dans les Temps Modernes* (1860). Figuier represented the skeptical position, arguing both for humankind's innate love and need for the "marvelous," and for explanations of magnetic and spiritistic manifestations assuming that the "lights of physiology and of medicine suffice" to explain the supposed mysteries (Figuier 1860, Vol. 1:XI). Others such as philologist Emile Littré (1801–1881) similarly reduced phenomena such as table turning and possession to a variety of manifestations showing the "weakening of the nervous system" (Littré 1856:871). In later years philosopher (later physician) Pierre Janet (1859–1947) discussed mediumship as an example of dissociation, as seen in his influential monograph *L'Automatisme Psychologique* (1889).[2]

But others took a different approach, explaining phenomena through forces said to be directed by discarnate (Kardec 1863) or human agency (De Gasparin 1854). Later in the century there was much positive interest and research on mental suggestion at a distance, as seen in physiologist Charles Richet's (1850–1935) classic paper in which, in addition to his celebrated use of statistical evaluation, he reported experiments with various methodologies, and discussed conceptual issues (Richet 1884, see also Alvarado 2008b). Richet, one of the founders of the *ASP*, became known during the nineteenth century for his important experimental studies of clairvoyance and related phenomena (Richet 1888, 1889) (see the photograph of Richet).[3] Other observers documented examples of distant mental influences such as the induction of trance (Janet 1886), and the effects of drugs at a distance (Bourru & Burot 1887).

The impact of the mesmeric movement continued during the late part of the century, as seen in the writings of several individuals defending the reality of

animal magnetism (Alvarado 2009). A good example of this line of work was Alexandre Baréty's (1844–1918) *Le Magnétisme Animal: Étudié Sous le Nom de Force neurique Rayonnante et Circulante dans ses Propriétés Physiques, Physiologiques et Thérapeutiques* (1887).

Charles Richet

From the early nineteenth century on there was a need to have periodicals for the discussion of unexplained phenomena (Alvarado, Biondi, & Kramer 2006). Examples of French responses to this need were the *Journal du Magnétisme* (started in 1845), and the *Revue Spirite* (started in 1858). In later years French physicians and others recorded their observations in scholarly and scientific journals such as the *Revue Philosophique de la France et de l'Étranger* (starting in 1876), and in the *Revue de l'Hypnotisme Expérimental & Thérapeutique* (starting in 1887, later the journal changed its title). The creation of the *ASP* provided French researchers with a specialized forum of discussion, and one modeled to some extent on the influential *Proceedings of the Society for Psychical Research*, published in London since 1882 (Alvarado, Biondi, & Kramer 2006:65–66).

The *Annales des Sciences Psychiques*

Beginnings and Founding Principles

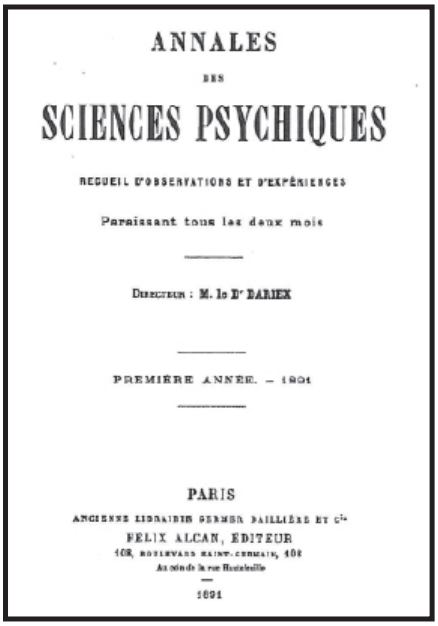
An important force behind the *ASP* was the above-mentioned Charles Richet. Although Richet was well-known for his scientific research in physiology, among other areas, he also was interested in scientific journalism and in the popularization of science. According to one biographer:

> Richet immersed himself in this journalistic undertaking with great enthusiasm. He immensely enjoyed his work as a editor and, according to his grandson, Gabriel, he was as enthusiastic about journalism as about science. (Wolf 1993:56)

Richet had considerable experience as an editor since he edited the *Revue Scientifique* between 1880 and 1902. This was a semipopular science journal addressed to both educated laymen and scientists.

The *ASP* appeared in 1891, and was the joint project of Richet and Xavier Dariex, an opthalmologist at the Hôpital des Quinze-Vingt in Paris. In some ways the *ASP* was a continuation of Richet's interest in disseminating scientific work in the pages of the *Revue Scientifique*, but this time the emphasis was psychical research. However, the initiative did not come from Richet himself but from Dariex. Initially, Richet was reluctant to get involved in the journal. He did not seem to have much regard for Dariex, whom he described as "a small

man, thin, timid, hesitant, and lacking in charm" (Richet, unpublished Mémoires, quoted by Wolf 1993:59). But collaboration ensued and the first issue of the journal appeared in 1891 (see photograph of title page).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TITLE PAGE OF THE *ANNALES DES SCIENCE PSYCHIQUES*

Before the journal was published, Richet had been the general secretary of the Société de Psychologie Physiologique. This society had the famous neurologist Jean-Martin Charcot (1825–1893) as President, and its purpose was the examination of psychological phenomena, among them those of hypnosis and psychical research, following the model of the London-based Society for Psychical Research (SPR). But the Société de Psychologie Physiologique did not fully achieve its agenda and disappeared soon after having organized the International Congress of Physiological Psychology in 1889, the first international meeting of psychology.[4]

The *ASP* was another attempt by Richet to develop French psychical research. He hoped that the journal, similar to the publications of the SPR, would serve as a repository of observations of "so-called occult facts" that had supportive evidence (Richet 1891:4). In 1890 Richet founded the Société des Annales Psychiques with Dariex to publish the *ASP*.

While the term "psychic sciences" was used in the journal, "psychical research" also appeared in the text frequently. This was probably an attempt to separate the field, and the *ASP*, from French spiritism. In fact, writing many years later, Richet (1922) argued that, like the SPR *Proceedings*, the *ASP* established a proper "balance between the credulity of the spiritist journals and the blind ignorance of the compendiums of official psychology" (p. 37).

The *ASP* addressed a more popular audience than the one targeted by more academic journals such as the influential *Revue Philosophique de la France et de l'Étranger*. While the journal included articles written by known figures from psychology and from other fields, the contributions were shorter in length and less scholarly than those presented in the *Revue*.[5] The *ASP* was open to everyone, a situation reflecting the status of psychical research. As stated by Lachapelle (2011):

> Psychical research would be a science in its presentation, and its rigor, but it would be a different kind of science. It would be accessible and inclusive: an open enterprise. Anyone could subscribe to the *Annales*, which was aimed at a broad audience rather than a restricted group with specific disciplinary credentials. Psychical phenomena could be observed or experienced by everyone, and readers were encouraged to participate by reporting their own psychical experiences to the *Annales*.[6] (Lachapelle 2011:86–87)

Dariex (1891) discussed how to produce a reliable report about psychic phenomena and asked readers to prepare written reports of psychic phenomena. However, Dariex warned them to be aware of problems such as the unreliability of memory.

The first issue of the journal opened with a letter Richet (1891) addressed to Dariex. Such an introduction was important for at least two reasons. First, Richet's association with the journal probably gave respectability to the enterprise and attracted both subscribers and writers. This was the case because Richet was very prominent during his lifetime. Not only did he belong to French high society, but he was a recognized figure in areas such as physiology and literature, not to mention psychical research (Carroy 2004, Wolf 1993).

Second, Richet set the tone for the new journal. He acknowledged the many methodological issues involved in the study of psychic phenomena, thus presenting the *ASP* from the beginning as an attempt to contribute to solving this problem. Most previous work, he argued, consisted of "incomplete facts, defective observations, ridiculous experiences, rough and weak attempts . . . ." (Richet 1891:2). While he believed that conventional explanations could not account for all claims of psychic phenomena, he cautioned readers that the journal would contain only raw facts. Theory, he stated, was simply premature. Richet believed that, while it was easy to propose theories, it was hard to establish a fact well. Theoretical explanations would come later, as more facts were established.[7] Consequently, experiments and observations of cases would be emphasized.

Following the example of the SPR, and their *Proceedings*, Richet stated that the *ASP* would report "all the serious observations" addressed to them about "so-called occult facts" (Richet 1891:4). He listed the following topics to be discussed in the journal: telepathy, lucidity, presentiments, movement of objects, and objective apparitions, or those apparitions that were unaccountable by hallucination. This included "photographs of phantoms" and collectively perceived apparitions (p. 6). However, Richet recognized that the classification was incomplete.

Dariex (1891) commented on the topic along similar lines:

> Our ambition . . . is to contribute to throw a bit of light on the profound darkness that still surrounds psychic phenomena, and to encourage their study. . . . (Dariex 1891:10)

Like Richet, he emphasized research over theory:

> We will not be spiritists, swedenborgeans, nor theosophists, nor occultists. We will be modest researchers of facts and very humble worshipers of the truth . . . (Dariex 1891:29–30)

In a later communication Dariex (1894) argued that the purpose of the journal was the search for truth through science. He expressed the hope that others would follow the *ASP* in the "cult of truth" (p. 2).

Reports of Cases, Experiments, and Seances

An editorial note stated that the *ASP* started with mental phenomena but would include physical manifestations later as new evidence for the phenomena accumulated (Dariex 1892a:64). Indeed, and as mentioned below, later volumes included other topics, such as more séance reports.

The first year of publication showed much activity. An analysis of the first volume of the journal, published in 1891, revealed that out of 48 articles 40% were reports of cases of such phenomena as telepathy and apparitions. The rest consisted of performances or tests of telepathy and clairvoyance (19%), book reviews and notices of publications (19%), editorial comments (10%), discussions of methodology (6%), theory and concepts (4%), and séance reports (2%).

Many of the papers were reports of cases. One of the most remarkable cases published was lawyer M. G. Morice's (1892–1893) account of an auditory haunting. Other contributions—some of which appeared in later years—included presentations and discussions of cases of "bilocation" (Bozzano 1911), premonitions (Desbeaux 1899), telepathic hallucinations (Hallucinations Télépathiques 1891), presentiment (Héricourt 1895), and faces seen on a wall (Maxwell 1905b).

There were also papers about telepathy and clairvoyance experiments. German physician and psychical researcher A. von Schrenck-Notzing (1862–1929) reviewed recent studies on the topic (Schrenck-Notzing 1891). Others presented observations of psychic dreams (Ermacora 1895), and mental suggestion at a distance (Joire 1897), among other topics. Richet's pupil Jean-Charles Roux (1872–1942) (1893) conducted card studies, and Richet (1893b) analyzed them statistically in a note on chance and probability.

Writing from the United States some years after the period considered here, Hereward Carrington (1880–1958) pointed out that most reports about Italian medium Eusapia Palladino (1854–1918) were to be found in the *ASP* (Carrington 1907:11). The journal, indeed, was required reading at the time to be able to evaluate Palladino's mediumship. The report of the famous Milan Commission,

formed by Alexander Aksakof (1832–1893) and others such as Carl du Prel (1839–1899) and Giovanni Battista Ermacora (1869–1898), Richet, and Cesare Lombroso (1835–1909) appeared in the journal (Aksakof et al. 1893). The studies were held in 1892 and involved the use of instruments such as balances to weigh the medium. The report included phenomena observed under light and in darkness. These consisted of movements of the table while the medium was touching it, and movements of objects at a distance. Table 1 lists many papers about Palladino that were published in the *ASP*.

TABLE 1
Examples of Papers about Eusapia Palladino Published in the *Annales des Sciences Psychiques*

Reference	Topic
Ciolfi (1891)	Seances
Lombroso (1892)	Theoretical ideas
Aksakof et al. (1893)	Seances
Richet (1893a)	Seances
Dariex (1896)	Seances
Ochorowicz (1896)	Fraud and dissociation
Sabatier et al. (1896)	Seances
De Rochas (1897)	Seances
Bottazzi (1907)	Seances using instrumental recording
Morselli (1907)	Variety and reality of phenomena
Imoda (1908)	Action on an electroscope
Lombroso (1908)	Seances and clinical observations
De Fontenay (1908)	Photo of materialized hands
De Rochas (1908)	Materialized hands

Physical phenomena were also discussed by Dariex (1892b, 1893, 1900). In the later paper he noted that the supposed agent behind the phenomena was a servant suspected to be a medium. This servant had a delicate health and suffered from gastro-intestinal problems of possible nervous origin.

Theoretical Papers

While theory was not emphasized, it was discussed more frequently than the journal founders recognized. For example, Albert de Rochas (1837–1914) mentioned his interest in concepts of force, a topic he had discussed before the

ASP was founded (e.g., De Rochas 1887). He argued for the existence of a fluidic body capable of leaving the physical body to produce physical phenomena:

> 1. There is in man a spiritual and fluidic body destined to serve as intermediary between the soul and the material body . . .
> 2. This fluidic body, which during life is the mold of the material body, can under some circumstances momentarily abandon the material body, and be perceived, even at a distance, by some of the human senses.
> 3. Some persons enjoy the property of secreting the substance that forms the fluidic body in fairly great quantity and with enough density for this substance to be perceived in the form of a luminous cloud. . . . (De Rochas 1895b:125)

Some ideas of emanations believed to come from the body of mediums were briefly discussed in papers about Palladino (e.g., De Rochas 1897, Lombroso 1892). Polish philosopher Julian Ochorowicz (1850–1917) speculated on nervous forces, dissociation, and the ideas of the circle to explain Palladino's phenomena (Ochorowicz 1896).[8]

Others discussed similar ideas to explain telepathic manifestations (e.g., Baudoin 1900, A. Denis 1895, Goupil 1897, Héricourt 1891). Léon Denis (1846–1927) wrote that:

> Thought vibrations may propagate through space like light and sounds and may affect other organisms that are in affinity with the one manifesting. The psychic waves, like hertzian waves in wireless telegraphy, propagate at a distance. . . . (L. Denis 1900:291)

There were other theoretical discussions, such as a review of theories of premonitions (Ermacora 1899). Finally, psychologist Théodore Flournoy (1854–920) presented speculations about mediumistic communications. He referred to the "subconscious imagination of the medium, working through recollections or latent worries" (Flournoy 1899:19).[9]

Philosopher Émile Boirac (1891–1917) presented a classification of psychic phenomena, which he called "parapsychic" (Boirac 1893). He divided them into those that could be explained by known principles, and those that required other explanations. The first group included "psychopathy" (mental and physiological effects of suggestion and the manifestations of hysteria) and "cryptopsychism" (or intelligent subconscious action expressed through automatism). The second order were phenomena consisting of action on matter or on biological organisms under the name of "psychodynamism." Boirac believed these effects were caused by a force different from the known forces of nature. When the action took place at a great distance the phenomena were called "telepsychic," such as cases of telepathy. Finally, "hyloscopy" included phenomena where matter seemed to exert an influence on humans. This included

agents such as "atmospheric currents," and magnetism from the ground and from magnets, metals, and other substances.

Overviews and Controversies

As seen in Table 2, there were papers on many other topics. Such variety shows that the *ASP* was open to practically all topics of psychical research.

The *ASP* also presented notices and reviews of the work of the SPR and other foreign societies.[10] Regarding the SPR this included a long review of Richard Hodgson's (1855–1905) writings about medium Leonora E. Piper (1857–1950) (Mangin 1898), reprints of cases of telepathic hallucinations (Hallucinations Télépathiques 1891), and papers about various phenomena such as physical mediumship (Myers 1896), poltergeists (Podmore 1897–1898),

TABLE 2
Various Topics Discussed in the *Annales des Sciences Psychiques*

Reference	Topic
De Rochas (1894)	Perceptions of "effluvia" during the hypnotic state
Boirac (1895)	Exteriorization of sensibility
Boirac (1896)	Induction of trance at a distance
Boucher (1895)	Healing case at Lourdes
De Rochas (1895a)	Exteriorization of the double
Erny (1900)	Case of premonition
De Rochas (1901)	Levitation of human body
Regnault (1901)	Role of vasomotor system in religious healing
Petrovo-Solovovo (1902)	Seances with Sambor
De Rochas (1902)	Long fasts
Joire (1903)	Case of crystal vision
Lombroso (1904)	Thought-transference
Waller (1905)	Crystal visions
Wijk (1905)	Rapping poltergeist
Durville (1908)	Projection of double
Duchatel (1914)	Psychic photography
Osty (1914)	Psychometry
Geley (1916)	Self-premonition of death
Lodge (1917)	Personal conviction of survival of death
Flammarion (1918)	Seeing the future and free will

and clairvoyance (Sidgwick 1891–1892). Furthermore, one of Frederic W. H. Myers' (1843–1901) multi-part papers about the subliminal mind was also translated and published in the *ASP* (Myers 1897–1903).

In fact, Myers received much attention in the journal, something that helped to spread his ideas in France (e.g., Mangin 1903, 1904, Review 1903). When Myers died, two obituary notices of him appeared in the *ASP* (Erny 1901, Richet 1901). Richet made it clear that he admired Myers, and pointed out that the man combined the "faith of the mystics" with "scientific sagacity and precision" (Richet 1901:173).

In addition the *ASP* provided a forum for the discussion of controversies and differences of opinions. A good example was Ochorowicz's (1896) strong critique of Richard Hodgson and other SPR researchers' investigation of Palladino at Cambridge. Another was the polemics about Richet's (1905b) observations of materializations (e.g., Maxwell 1906).[11]

Also interesting, and showing differences of opinion about conceptual and methodological issues that are part of every serious discipline, were Bozzano's (1905a) defense of critiques of the mediumship of William Stainton Moses (1839–1892), and critiques about such topics as the views of Frederic W. H. Myers (Mangin 1903), Camille Flammarion (1842–1925) (Erny 1900), and Enrico Morselli (1852–1929) (de Vesme 1908).

Frequent Authors

Some individuals were frequent authors in the journal. Richet was prolific, as seen in Table 3. French physician and jurist Joseph Maxwell (1858–1938) also contributed several articles on various topics, among them the work on the automatisms of Michel Eugène Chevreul (1786–1889), a haunting, and intuition (Maxwell 1904, 1905b, 1913). Another frequent contributor, Italian scholar Cesar de Vesme (1862–1938), wrote many papers about such topics as specific mediums, publications, and the phenomena of phantom armies and battles (see, respectively, de Vesme 1905, 1908, 1915–1916).

A remarkable series of multi-part papers was published by the above mentioned Julian Ochorowicz. The papers were about the physical phenomena of Polish medium Stanislawa Tomczyk. Ochorowicz included in the papers many observations about telekinesis, the photographic detection of "fluidic" hands and forces, and speculations about a variety of forces believed to exteriorize from the medium to produce phenomena (Ochorowicz 1909a, 1909b, 1910, 1911, 1911–1912, 1912). Another example was Italian student of psychic phenomena Ernesto Bozzano (1862–1943). He published several papers about topics such as animals and psychic phenomena (Bozzano 1905b), deathbed visions (Bozzano 1906), symbolism and psychic phenomena (Bozzano 1907), "bilocation" (Bozzano 1911), and premonitions (Bozzano 1912–1913).

TABLE 3
Examples of Papers published by Charles Richet in the *Annales des Sciences Psychiques*

Year of Publication	Topic
Richet (1891)	Introduction to the journal
Richet (1892a)	Reply to a critic
Richet (1892b)	The future of psychology
Richet (1893b)	Statistical analysis of an ESP test
Richet (1900)	Case of a musical prodigy
Richet (1901)	Obituary of F. W. H. Myers
Richet (1902)	Case of near-death experience from antiquity
Richet (1903)	Case of premonition
Richet (1905b)	Materialization phenomena
Richet (1905c)	Automatic writing in Greek
Richet (1908a)	Call for premonition cases
Richet (1908b)	Incredulity
Richet (1914)	On reason and science
Richet (1919)	Lucidity

The *Annales* and the Félix Alcan Publishing House

Up until 1904 the *ASP* was published by Félix Alcan, a well-known French publishing house that produced and disseminated countless works in philosophy and in the natural and social sciences (Tesnière 1990, 2001). In addition to the *ASP* the press published several journals such as the *Annales de l'École Libre des Sciences Politiques*, the *Bulletin de la Société de Psychologie Physiologique,* the *Revue Historique*, the *Revue de Médicine*, and the *Revue Philosophique de la France et de l'Étranger*.

Alcan published several series of books. Some of them were the Bibliothèque de Philosophie Contemporaine (Library of Contemporary Philosophy), which in addition to philosophy included psychiatry, criminology, and sociology, as well as the Collection Historique des Grands Philosophes (Historical Collection of the Great Philosophers), the Bibliothèque d'Histoire Contemporaine (Library of Contemporary History), and the Bibliothèque Scientifique International (International Scientific Library). Many eminent authors were represented in these collections. Among the Frenchmen, and in addition to Charles Richet and

Pierre Janet, were Gilbert Ballet (1853–1916), Henri Bergson (1859–1941), Alfred Binet (1857–1911), Charles Féré (1852–1907), Paul Janet (1823–1899), Etienne J. Marey (1830–1904), and Hippolyte Taine (1828–1893). Foreign translated authors included Alexander Bain (1818–1903), John Stuart Mill (1806–1873), Arthur Schopenhauer (1788–1860), Herbert Spencer (1820–1903), James Sully (1842–1923), John Tyndall (1820–1893), and Wilhem Wundt (1832–1920).

In addition, the press published psychical research books. This included abridged translations of Gurney, Myers, and Podmore's *Phantasms of the Living* (1886, 1891), and of Myers' *Human Personality and Its Survival of Bodily Death* (1903, 1905). There were, of course, other works on the topic published by Alcan that were originally written in French (e.g., Flournoy 1900, Gyel 1899).[12]

The prominence of the publisher helped bring attention to the *ASP*. Alcan advertised the journal widely in the catalog of its works included at the end of many books.[13] In addition, there were advertisements of the journal in English-language publications such as *Mind* (Advertisement 1896), and the *Philosophical Review* (Advertisement 1898).

Citations of the Annales

In France, the journal was criticized for having the word "science" in its title, because the *ASP* was seen as a publication "which contains but tales" (Verdier 1892:707). However, the critic recognized that the editors would perform a good service for science if they could uncover the laws behind telepathy and other phenomena.

Most books on psychic phenomena published in France after the founding of the *ASP* cited or mentioned the journal. Examples include the publications of figures associated with spiritism and psychical research (e.g., Bois 1907:22,175,369, Delanne 1897:60, Flammarion 1900:159,160, Maxwell 1905a:16,93,170). Even critics paid attention to the journal (e.g., Grasset 1908:21,22,52,57). Attention to the *ASP* continued even after the journal had ceased publication (e.g., Richet 1922:19,735, Warcollier 1921:40,55,128,184).

While the *ASP* was very important in France, it also received some attention in other countries. In England it was mentioned in the *Journal of the Society for Psychical Research* (*Annales des Sciences Psychiques* 1891). In the United States a writer in the *Monist* showed some reservations about the cases reported in the journal (Arreát 1891). Some years later another writer in the same publication considered that, like the *Proceedings of the Society for Psychical Research*, the *ASP* presented a "record of the highest distinction for observations of this kind" (Vaschide 1902:282).

"The French," wrote someone in charge of news on the journal *Medical Record*, published in New York, "have joined in the investigation of ghosts, telepathy, haunted houses, etc., and have begun the publication of a journal called *Annales des Sciences Psychiques* in which to record their work. . . . We trust that their work will be fruitful" (News of the Week 1891:46).

Several writers paid attention to the nineteenth-century issues of the *ASP*, as did Frederic W. H. Myers in his *Human Personality and Its Survival of Bodily Death* (1903: Vol. 1:533, Vol. 2:136,212). Some, like Andrew Lang (1894:159), Edmund Parish (1897:85), and Frank Podmore (1894:80–81,19–120,140–142) cited cases from the *ASP* in their writings. William James (1894) reviewed an 1893 issue of the *ASP* in the *Psychological Review*, bringing the publication to the attention of American psychologists.

Issues of the journal were listed in some library catalogs (e.g., *A List of Periodicals* 1897:9). It was also included in several bibliographies (e.g., Bibliographie der Psycho-Physiologischen 1898:353–354, Fletcher 1891:283). Articles from the journal were listed in the *Revue Scientifique*. The *ASP* was featured in the journal among the publications of disciplines such as biology, geography, physiology, and zoology in a section about the main periodicals presenting original essays (Bibliographie: Sommaires 1891:223). All of this shows that the *ASP* was known beyond psychical research circles.

One can only speculate on the impact the journal had on specific individuals, particularly those involved in psychical research. One individual about whom we have information is Italian student of psychic phenomena Ernesto Bozzano. He wrote in an autobiographical essay:

> In the year 1891 . . . I had my initiation into metapsychical research, Professor Ribot, director of the *Philosophical Review* [*Revue Philosophique*], having sent me the first number of "The Annals of Psychical Science," to read and send him my judgment upon. I read: Therein was talk of telepathy, of clairvoyance, and of telekinesis. I was scientifically scandalized! I wrote in this tone to Professor Ribot. He answered, exhorting me to re-read and reflect more quietly, for he saw that the existence of the facts could not be denied. In deference to the Director of the *Revue Philosophique*, I re-read, thought it over, and decided to acquire works of this nature. Result: the complete defeat and demolition of my *misoneism*. (Bozzano 1924:153–154)

A Look into the 1901–1919 Period

During the twentieth century the role of the *ASP* in French psychical research grew and changed in quality. Dariex wrote in 1901 that the journal, initially created to examine psychical phenomena with impartiality, and to present them to the scientific world with credibility, had succeeded in its mission by careful consideration of phenomena with little theoretical discussion. The *ASP* was now ready to enter a new phase of existence:

> We will not abandon this line of conduct and the *Annales des Sciences Psychiques* will continue to be what they have been; but now that the goal has been accomplished, now that we have done all that we could to bring attention to these phenomena and their study, we will abandon our strictness—what some impatient minds would say is our exclusiveness—and we will widen our program. (Dariex 1901:2)

In 1904, the *ASP* incorporated the *Revue des Études Psychiques* and began to appear every month (as opposed to every two months). Dariex was the only person listed as director but it was stated that Cesar Baudi De Vesme (1862–1938), formerly director of the *Revue*, became editor-in-chief of the *ASP* (Dariex 1904).[14] A new editorial committee was formed. It consisted of Camille Flammarion, Marcel Mangin (1852–1915), Joseph Maxwell, and Albert de Rochas for France. In addition, the following men represented other countries: William Crookes (1832–1919), Cesare Lombroso, Enrico Morselli (1852–1929), Julian Ochorowicz, Francesco Porro (1861–1937), and Albert von Schrenck-Notzing. However, Richet's name continued to appear on the cover of later issues as one of the directors of the journal.

An English edition of the *ASP* appeared between 1905 and 1910 (Crabtree 1988:302). The cover of the first bound volume (January–June 1905) lists De Vesme as editor, but in subsequent years the editor is listed as Laura I. Finch.[15] Dariex and Richet are listed as Directors and there is an editorial committee of eleven individuals, among them the above-mentioned De Rochas, Lombroso, Maxwell, Ochorowicz, and Schrenck-Notzing. While there was overlap between the French and the English editions, they were not identical. The order of the articles varied and the sections of news and comments were also somewhat different.

In 1908, the *ASP* became the official publication of the Société Universelle d'Études Psychiques (SUEP), a society founded in Lille in 1901 by physician Paul Joire (1856–1930) to study psychical phenomena scientifically (La fusion . . . 1914). The journal included many accounts of conferences, meetings, and research related to the SUEP (e.g., Actes de la Société 1908a, 1908b).

With this new format, the *ASP* became more popular and accessible for the French general public. Its pages now had evocative pictures and illustrations, news, and a more varied content. Lachapelle (2011) has argued that such changes led to a loss of the original purpose of the journal. She wrote:

> Gone were the days when the journal limited most of its activities to the sober discussions of telepathic occurrences; it had become a truly popular enterprise. The more accessible it became, however, the less likely it was to incorporate the scientific world. Ultimately, the creators of the *Annales* failed in the mission they had set out for themselves. (Lachapelle 2011:91)

While it is true that the topics were more varied and that there were many illustrations, Lachapelle's perspective is somewhat exaggerated. Even reports of sensational phenomena, such as papers about materializations and their photographs in the cases of mediums Marthe Béraud (Richet 1905b) and Eusapia Palladino (de Fontenay 1908), were attempts to learn about these phenomena. In other words, the phenomena, and some of the illustrations, may have been sensational in appearance, but the intention of the authors was a serious and critical one, even if many were not convinced. While it is possible to criticize methodology, it is problematic to equate seriousness and scientific outlook on the basis of "sober discussions of telepathic phenomena" when the subject matter of the field also included physical phenomena that appeared incredible to many. Was the journal supposed to ignore some topics just because they seemed sensational to some, or because they were difficult to investigate scientifically?

Regardless of methodological and interpretation aspects, and of the illustrations, several twentieth-century authors presented various attempts to learn from the phenomena. In addition to the above-mentioned work by Bozzano and Ochorowicz, we may mention publications about premonitions (Geley 1916) and lucidity (Osty 1914). The investigations of Ochorowicz (1910, 1911, 1911–1912, 1912), in which he tried to determine empirically the characteristics of a force he believed was exteriorized by medium Stanislawa Tomczyk, represent one of the most systematic observations published in the *ASP*.

The *ASP* lost Dariex, its founding editor, after his having been involved with the *ASP* for twenty years. He was replaced by De Vesme. In later years, during World War I, the journal's activities were considerably slowed down and only a few issues appeared between 1914 and 1918. The last issue was published in 1919. At that time, the Institut Métapsychique International (IMI), which had just been created, took over the affairs of the *ASP* and the SUEP (De Vesme 1924).

The transition from the *ASP* to the *Revue Métapsychique* (*RM*), the journal published by IMI, was difficult because IMI wanted to have a high intellectual content with articles and experimental reports written by the elite researchers.[16] The place left to popular science, more prominent in the *ASP*, was reduced. It was suggested that readers of the *ASP* would subscribe to the *RM,* with no competition between them. But De Vesme (1921–1922) tried to negotiate with the board of IMI a merging of the *ASP* and the *RM* in which he would obtain some financial compensation. Although by this time the *ASP* was no longer published, De Vesme affirmed that he would start its publication again because it was his "moral duty."

Concluding Remarks

Like so many other journals dedicated to psychical research, the *ASP* belongs to a previous generation and thus has been forgotten by many current students of the subject. The same may be said about the work of authors represented in the journal such as Bozzano, Boirac, Dariex, de Rochas, Ermacora, Ochorowicz, and Richet, among many others.

Our comments by no means represent all that can be said about the journal. Certainly more can be written about its content. This not only includes a discussion of the topics emphasized, but also of the methodologies and approaches represented, and the affairs of the SUEP. In addition, there is more to say about the contributors. Even the papers of such a frequently discussed figure as Richet have not been studied in detail (see Table 3).

The journal, or its editors, did much to inform French readers about foreign developments. For example, there were many news reports about the SPR, and translations of articles written by SPR members. In fact, it may be argued that the *ASP* was one of the vehicles by which British psychical research was spread in France.

It is a matter of speculation how much influence the *ASP* had on the development of French psychical research. One author has stated that the journal assisted in the process of bringing together "scattered and non-centralized researches" about psychic phenomena (Marmin 2001:157). Another was more emphatic and perhaps overstated her case when she said: "Psychical research officially began in France with the founding of the *Annales des Sciences Psychiques* in 1891" (Lachapelle 2011:86). There is no question that the journal stimulated and supported the field in France by providing a forum of discussion separate from spiritist and occult periodicals. In addition, the *Annales des Sciences Psychiques* was one of the factors that assisted the development of later French studies of psychic phenomena, when the former "science psychique" was called "la métapsychique," showing the continuous influence of Richet.[17]

Notes

[1] For reviews see Brower (2010), Edelman (1995), Lachapelle (2011), Méheust (1999a, 1999b), Monroe (2008), and Plas (2000).

[2] On conventional explanations of mediumship in France, see Le Maléfan (1999), and, to some extent, Alvarado, Machado, Zangari, and Zingrone (2007). On Janet, see Ellenberger (1970) and Carroy and Plas (2000).

[3] For general studies of Richet see Estingoy (2003), Lewer (2006), Schneider (2001), and Wolf (1993). His involvement with psychical research has been discussed by several authors (e.g., Brower 2010:Chapter 4, Le Maléfan 1999:85–88,273–278, Lachapelle 2011, Monroe 2008:208,211,253, Plas 2000:54–55,89–93,99–109,125–126,150–154, Tabori 1972:98–132).

[4] On the Societé, see Plas (2000). Regarding the Congress, see Alvarado (2006a, 2010b) and Nicolas and Meunier (2002).

[5] The *Revue* was founded by philosopher and psychologist Théodule Ribot (1839–1916). In addition to papers about topics such as philosophy, sociology, and education, the journal represented the new empirical psychology in France (Bertolini 1991, Nicolas 2002:113–118). The *Revue* carried important papers about psychic phenomena during the nineteenth century, such as those of Janet (1886) and Richet (1884).

[6] In England SPR leaders asked the general public to send them cases, as seen in the pages of *The Times* (Advertisement 1883, Gurney, 1883). They also issued instructions to conduct experimental work (To Members 1883).

[7] To some extent, such an attitude toward theory was followed by Richet in later publications (e.g., Richet 1922). But this does not mean that Richet was completely theory-free, as one of us has discussed before (Alvarado 2008a, 2008b).

[8] This paper has been summarized elsewhere (Alvarado 2010a). On the topic of magnetic, psychic, or vital forces used in the past to explain psychic phenomena, see Alvarado (2006b).

[9] Flournoy (1900, 1911) is well-known for having developed these ideas further in later publications.

[10] SPR work was also disseminated in France through the translation of works (Gurney, Myers, & Podmore 1891, Myers 1905), and through discussions of investigations in other publications (Delanne 1897:91–93,143,145–149, Gibier 1891:36–39, Richet 1884:633–635).

[11] This was probably the most controversial episode in Richet's career in psychical research. For an overview, see Le Maléfan (2002).

[12] E. Gyel was the pseudonym of French physician and psychical researcher Gustave Geley (1868–1924).

[13] Some examples include Berthelot (1891), Binet (1892), Lefèvre (1893), and Proal (1892).

[14] The *Revue* was the French version of the Italian *Rivista di Studi Psichici*, which was founded in 1895 by Giovanni Battista Ermacora and by Giorgio Finzi (Alvarado, Biondi, & Kramer 2006:67–68). During 1902–1903, the *Revue* was the French version of the *Rivista*, and it ceased publication in 1903 (we owe these last two points to Massimo Biondi). On De Vesme, see Warcollier (1938).

[15] Finch was said to be the Mme. X who produced automatic scripts for Richet (1905c). See Bozzano (1932:46) on her psychic abilities. See Fodor (n.d./1966:61).

[16] The first volume of the *Revue Métapsychique* appeared under the title *Bulletin de l'Institut Métapsychique International*.

[17] Richet (1905a) presented the term *métapsychique* in his Presidential Address to the Society for Psychical Research. While he admitted there was a precedent for the use of the term (Richet 1905a:13,footnote), it was popularized by his influence. His later and extremely influencial *Traité de Métapsychique* (1922), published by Félix Alcan, also helped to popularize the term in later years. The term was adopted by the Institut Métapsychique International (founded in 1919), and used in its journal. Many French authors used the term in articles and books (e.g., Maxwell 1907, Sudre 1926).

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Journal of Scientific Exploration, Vol. 26, No. 1, pp. 141–142, 2012 0892-3310/12

OBITUARY

Dr. Stuart Appelle, 1946–2011

Stuart Appelle pursued two intellectual careers with distinction. He was a professor of psychology at the State University of New York College at Brockport, an experimental psychologist who published numerous professional papers in his specialty of tactile perception. Later he assumed various administrative positions at SUNY Brockport, among them chairman of the Department of Psychology and Dean of the School of Letters and Sciences. In his conventional academic pursuits, his life was full and successful, but he devoted considerable time to another career that carried him out of ordinary academia into the realm of anomaly research. Since I knew him in this second capacity I will emphasize it even though his interest in UFOs composed only one corner of a much larger whole.

As early as 1972 while still a grad student, Stuart criticized a sociological "status inconsistency" explanation for UFO sightings in an article published in a scholarly journal (Appelle 1971). His participation in UFO research began in earnest in the late 1980s as alien abductions rose to prominence as a topic of dispute. Psychological explanations for the nature of these experiences and the distress of people reporting them proliferated, but he saw considerable bad science and stepped in as a critic of speculative theories that paid little heed to the evidence. In a benchmark paper published in the *Journal of UFO Studies* he reviewed with systematic care the entire range of solutions, assessing such proposals as hypnotic suggestion, false memory, fantasy-proneness, and sleep anomalies for their strengths and exposing their shortcomings. He concluded that the conventional answers offered so far are insufficient to explain abductions and called for research that truly reckoned with the evidence (Appelle 1995/1996). A further development of his criticisms and arguments appeared in a chapter of *Varieties of Anomalous Experience*, published by the American Psychological Association in 2000 (Appelle, Lynn, & Newman 2000). His co-authors were both prominent skeptics of abduction but he carried his case for the inadequacy of current research and theory throughout the chapter, thereby defending abduction as a potentially anomalous phenomenon in the mainstream scientific literature. He further defended the subject as worthy of scientific study in presentations at professional meetings.

If the theoretical treatment of abductions presented a chaotic landscape, the practices of case investigators gave even further cause for concern. Stuart viewed the often undisciplined, sometimes potentially harmful efforts of investigators as a key challenge to progress and crusaded to replace the prevailing amateurism of the field with scientific standards. He became a dedicated advocate for both improvements in investigative techniques and adoption of a professional code of ethics in the treatment of human subjects. He participated in the Ethics Committee of the Abduction Study Conference held at MIT in 1992, which drafted comprehensive guidelines based on current standards for clinical research and therapeutic practice (Ethics Committee 1994).

Alongside his contributions to UFO research and practice, Stuart also served for some 20 years as a board member of the Center for UFO Studies. A regular contributor of articles to the *Journal of UFO Studies*, he assumed editorship in 1995 and continued the high standards set by his predecessor, Michael Swords, for the only peer-reviewed journal dedicated to ufology.

With his sudden death on June 27, 2011, at age 65, UFO research lost a rare proponent who not only excelled in his academic profession but also committed his expertise and rigor of thought to reform the study of UFOs. His critical acumen set a welter of psychological hypotheses about the subject into perspective, and his advocacy of high standards guided a chaotic field toward scientific discipline. None of his efforts brought him gold or glory: In fact, amid general academic disapproval, only his curiosity and devotion to inquiry served as his reward. In addition, he has the enduring thanks of those of us who worked with him. More than anything else, Stuart Appelle was an admired colleague as well as a valued friend, and for those reasons we will miss him dearly.

Thomas E. Bullard

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Journal of Scientific Exploration, Vol. 26, No. 1, pp. 143-147, 2012 0892-3310/12

LETTERS TO THE EDITOR

Response to Bousfield and LeBlond: Shooting Pipefish in a Barrel; or, Sauropterygian "Mega-Serpents" and Occam's Razor

In their response to our recent article (Woodley, Naish, & McCormick 2011), Bousfield and LeBlond (2011) argue on the basis of purported morphological and behavioral differences that the case for Hagelund's juvenile "Cadborosaurus" being a pipefish is weakened into triviality. We note several major problems with their response and feel that their dismissive tone indicates a biased and unscientific approach to the investigation of this subject. First, note that Bousfield and LeBlond (2011) thought that, by attempting to dismiss our pipefish identification, they had completed their task of critiquing our paper. While their "critique" is grossly inadequate in any case, note that they made no mention of the assorted additional fish taxa also found by us (Woodley, Naish, & McCormick 2011) to be more similar to the Hagelund specimen than the specimen was to their "Cadborosaurus" construct.

Second, Bousfield and LeBlond's (2011) arguments involve gross misrepresentation of Hagelund's account, demonstrating unyielding bias toward their pet "mega-serpent" hypothesis and a total unwillingness to explore alternate interpretations. Their most alarming claim is that Hagelund's specimen held its head out of water for 5–10 minutes, ruling out pipefishes, which only engage in this behavior briefly. However, Hagelund never gave an estimated duration for his encounter, let alone this particular behavior; prior to the specimen's capture it was visible only due to a "small vee of wavelets" at the water's surface, meaning that it did not hold its head out of water for the entire encounter. While there is an ambiguous area behind the eyes and in front of the "flipper-like feet" in Hagelund's drawing, nowhere did Hagelund claim that a "neck" was present (contra Bousfield & LeBlond 2011)—in fact the spacing there is consistent with Bay Pipefish morphology. Furthermore, Hagelund never used the description "large jaws" (contra Bousfield & LeBlond 2011), only describing the mouth as "open[ing] slightly" and illustrating the mouth-line terminating well anterior to the eyes, both of which are again compatible with a pipefish. Bizarrely, Bousfield and LeBlond (2011) regard the dorsal fin of pipefishes as "pronounced" when it is actually a transparent, often hard-to-spot structure which takes up a small portion of total length. Bousfield and LeBlond (2011) pointlessly cite a "very elongate post-vent tail region" as a

detail of dissimilarity between Hagelund's specimen and a pipefish despite the fact that Hagelund never described or illustrated a vent!

Hagelund clearly conflated the "spade-shaped tail" with the "tiny flipper-like fins" in his description and illustration of the caudal region. Contra Bousfield and LeBlond (2011), there is no indication from Hagelund's text or illustration of a tail separate from the fins nor is the orientation of the fins (horizontal or vertical) directly mentioned. Hagelund initially confused the specimen for a sea snake and referred to it as "eel-like," which implies lateral undulation and a vertical (fish-like) tail, contra the claim of our critics. Our analysis (Woodley, Naish, & McCormick 2011) specifically noted that the description of teeth is problematic, but we again note the 18-year gap between Hagelund making his observations and recounting them in his book. Recollection of any memory after such a long interval will surely be prone to distortion.

Third, the crux of Bousfield and LeBlond's (2011) argument appears to rest on an interpretative mangling of Occam's razor, which they take to imply that explanatory preference should be given to their singleton identity theory of Caddy (i.e. that most encounters can be interpreted as evidence for the existence of a single entity, specifically an extant sauropterygian of some kind [e.g. Bousfield & LeBlond 1995]) over the hypothesis that encounters with creatures of highly variable morphology and behavior (see Figure 1) should be interpreted first and foremost as encounters with a diversity of known animals.

Sober (2000:32) in his *Philosophy of Biology* would appear to disagree with this interpretation of Occam's razor, stating instead that "The overall plausibility of a hypothesis is a function both of its likelihood relative to *present* observations and its *antecedent* plausibility [italics in original]." What this means in practice is that if a drunk man reports having seen a pink elephant, it is true that we can account for his observation by positing the existence of a pink elephant. However, refusal to posit such an entity does not violate Occam's razor, *because pink elephants are not antecedently plausible*. Like the pink elephant, the idea that there exist extant sauropterygians corresponding to the descriptions of Caddy is not even remotely antecedently plausible, as has been repeatedly argued elsewhere (Bauer & Russell 1996, Naish 2001, Staude & Lambert 1995, Woodley, Naish, & Shanahan 2008). Extant sauropterygians aside, the idea that one type of animal is being reported across Caddy encounters also does not appear to be antecedently plausible (we refer again to Figure 1). We (and the reader) are therefore under no obligation to give explanatory preference to Bousfield and LeBlond's preferred explanation for Hagelund's baby "sea serpent": In point of fact, Occam's razor obliges us to give preference not just to our proposed explanation, but to *any* explanation with similar antecedent plausibility (i.e. the idea that it may have been a poacher or sturgeon or some such other) over that preferred by our critics.

Figure 1. A comparison of all known drawings of caddy indicates a diversity of morphologies with few consistencies across encounters (by Cameron A. McCormick).

On these grounds the entire substance of the Bousfield and LeBlond (2011) objection is negated. To work within the framework of science, Bousfield and LeBlond must operate in line with another well-known principle, i.e. the Sagan standard, or the idea that extraordinary claims require extraordinary evidence, and thus must present evidence of a sort that reduces the antecedent implausibility of their preferred singleton hypothesis. Subjective and erroneous interpretations of drawings and eyewitness testimony made almost two decades after an alleged encounter do not advance their cause.

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Darren Naish

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Sir Arthur Conan Doyle's Resignation from the Society for Psychial Research

Luca Gasperini's tribute to Bozzano is very welcome in "Ernesto Bozzano: An Italian Spiritualist and Psychical Researcher" in JSE 25:4 (Gasperini 2011).

Bozzano's friendship with Conan Doyle would merit further exploration.

As Gasperini notes, Conan Doyle resigned his membership in the Society for Psychical Research in 1930, though he was an ordinary dues-paying member and not an honorary member. He did not take with him 77 other members. The passage cited from Mauskopf and McVaugh (1980) does indeed say that six members and one associate resigned in support of Doyle, and that 77 others resigned in the year, but there is no reason to reject the statement made by the Society for Psychical Research in their annual report: "A great majority of those who gave any reason for their resignation mention financial stringency, long illness, residence abroad, etc." (Annual Report . . . 1931).

Recent research, cited by Gasperini, has shown that the Millesimo phenomena which Sir Arthur defended in 1930 were even more doubtful than was suggested at the time (Biondi 2009).

Doyle went on to resign angrily as president of a leading Spiritualist body (London Spiritualist Alliance) shortly before his death in July 1930 (Gaunt & Price 2006).

Yours faithfully,

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Response to Leslie Price

In the first place, I would like to thank Mr. Price for his close examination of Conan Doyle's resignation, because he shows me some particulars of the history of psychical research that, honestly, I didn't know enough about. And, above all, I would like to apologize for my mistake.

The letter written by Mr. Price, furthermore, taught me another thing: We can't thoroughly rely on the Bozzano correspondence, in particular that with Gastone De Boni, which is, as I wrote (Gasperini 2011), the most important of his correspondence that we can still read nowadays.

For example, in a letter to De Boni dated April 1943, Bozzano wrote that Conan Doyle defended him and his reports of the Millesimo phenomena and that more than 200 associates resigned in support of Conan Doyle (Letter from Genoa, 15 April 1943, unpublished, in Bozzano & De Boni 1930–1943). We know that Conan Doyle actually defended Bozzano against Besterman's criticisms (Doyle 1930, Bozzano 1930), but we also know, thanks to Mr. Price, that many fewer people than 200 resigned (I had found a more reasonable number (77) in Mauskopf & McVaugh (1980), and, ingenuously, I took it).

The history of Bozzano is still quite unexplored; another reason to esteem the letter from Mr. Price.

Yours faithfully,

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ESSAY REVIEW

Mysterious Minds, Irreducible to Brains?

Mysterious Minds: The Neurobiology of Psychics, Mediums, and Other Extraordinary People edited by Stanley Krippner and Harris L. Friedman. Santa Barbara, CA: Praeger, 2009. 219 pp. $44.95 (hardcover). ISBN 9780313358661.

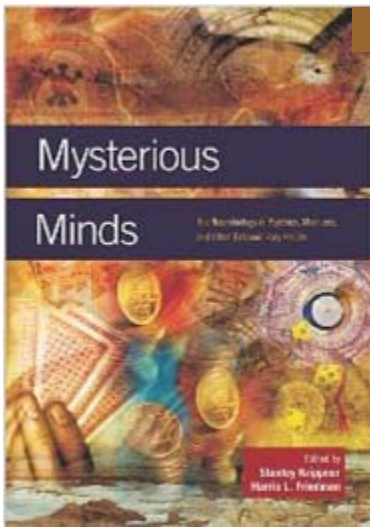
This book presents a wide range of anomalous experiences and events and their possible neural correlates within a framework that is not necessarily materialist-reductionist based. Here psi-related *experiences* refer to the subjective; whereas psi-related *events* occur in a scientific setting with objective measures. These anomalies are presented using a broad spectrum of viewpoints. One end of the continuum is expressed in Allan Combs' Foreword, stating that the standards that psi research is expected to meet are far stricter than most mainstream science could meet, possibly due to a mindset similar to scientism. The other extreme is expressed in James Alcock's chapter (Chapter 2) titled "The Parapsychologist's Lament" which portrays in detail the body of difficulties in the field, but ends with conclusions—e.g., failure to replicate, and that neural correlates of psi cannot be possible because these things have not been proven to exist.

Chapter 1 by William Roll and Bryan J. Williams addresses the intersection of quantum theory, neurobiology, and parapsychology. This commences with a review of the electrophysiology of psi, including psi-related contingent negative variation and slow cortical potentials, EEG/fMRI correlation studies of remote stimulation, neural correlates of overt psi performance, fMRI/EEG of presentiment, and temporal lobe sensitivity in ESP experiencers. Quantum entanglement is used to explain how non-locality may be involved in psi. From this, theories of mechanism are suggested. Retrocognitive and precognitive information cross into conscious awareness and psychokinesis is suggested to involve entropy and negentropy. Next, poltergeists are described as a form of macro PK (recurrent spontaneous psychokinesis) which takes place when a witness is not directly observing the object moving. This is justified by the quantum Zeno effect. Micro PK, or changes in RNG to a non-random output, is presented here as being correlated with alpha brain activity. Quantum physics may very well be the key to explaining how psi occurs, but, at this point,

mapping psi onto quantum entanglement and other quantum effects is merely an analogy and not a direct mechanism of action of psi.

Chapter 2 contains Alcock's negative appraisal of the field, as previously mentioned. He claims that there is a lack of replicability in psi studies. Ironically, the very next chapter by Caroline Watt and Harvey Irwin provides ample evidence that this is not the case, with an emphasis on "independent replications" in their review. Next, Alcock claims that "ad hoc constructs" like an experimenter effect are unfalsifiable. To me, this claim is reminiscent of Freudian ego-defense mechanisms. It may be that Alcock's own bias against evidence of psi is unfalsifiable to himself. Following this, Alcock makes the bold claim that without a theory of the mechanism of action involved in psi and robust evidence that psi exists, we cannot link deviations in objective measures to specific hypotheses. It is not clear that this is the case. Objective observations and correlations, even without a specific mechanism of action, are valid. Hypothesis building may indeed lead to further research which may elucidate the mechanisms of action involved in psi. Alcock ends his chapter with, "there can be no 'neurobiology of ESP and PK' until first one can be sure that there is ESP and PK" (p. 42). This appears to be in contradiction with even the most extreme of viewpoints against psi, that psi is merely an internally generated hallucination of the brain. Even in that case, there should be neural correlates of such human experience worthy of study. The chapter ends with ". . . what it would take for parapsychologists, if there is no psi, to accept that conclusion" (p. 41). This assumes that all parapsychologists are biased in the opposite direction of diehard skeptics. It is unclear that any body of evidence for psi would ever meet Alcock's criteria for existence. He is clearly indoctrinated in scientism. As my friend Julia Mossbridge once said to a skeptical colleague, "I'm sorry; I did not realize you were religious about science. I thought you were an empiricist and valued evidence."

Chapter 3 is a review by Watt and Irwin of the laboratory evidence for psi. This is broken down into two types of research, proof-oriented and process-oriented studies of ESP. Next, definitions based on Rhine (1934) are given for precognition, telepathy, and clairvoyance. Meta-analysis of a pool of studies is presented as a useful tool to show how many null studies would be needed to counteract studies with positive results and thus support the null hypothesis of no psi. This is helpful to demonstrate that anomalous results significantly different from chance are indeed consistent across studies. Of course, considering the conflicting results between various meta-analysis publications, this is making the assumption that all meta-analyses show a significant deviation from chance or significantly large effect sizes. Such discrepancies suggest that the biases of the people conducting a meta-analysis can affect the outcome and results. Interestingly, despite the tendency of the use of meta-analysis to shoot down the



file drawer effect, Watt and Irwin state that ". . . proof-oriented ESP experiments have not succeeded in establishing the reality of ESP" (p. 48). As in the Alcock chapter, the reason given for this statement is lack of a specific mechanism of psi action. Surely in the early days of scientific discovery, correlations and subsequent theory building were essential to unraveling the mysteries of mechanisms of action. Particularly relevant to this topic is the fact that most modern-day neuroimaging is merely correlational by design, which does not necessarily reveal causation. Without true causation, where is the mechanism of action? Next, there is an overview of independently replicable patterns seen in psi performance such as the sheep–goat effect (psi-missing correlating with negative attitudes toward psi, psi-hitting correlated with positive attitudes toward psi), position effects (clustering of hits at the beginnings or ends of runs and displacement forward or backward by trials, both of which are associated with negative attitudes), and decline effects. This is followed by a review of electrophysiological and neuroimaging studies, some with selected and some with unselected subjects, suggesting that reduced physiological arousal is psi-conducive.

Chapter 4 by Adrian Parker considers psychokinesis (PK) within a dualist and other perspectives. One other perspective is panpsychism. This is defined here as ". . . the unique property of neural matter to provide the means for producing consciousness. . . " (p. 67). Next, evidence of PK is presented. Much of this is from subjective experiences of objects moving on their own, or macro-PK. Some has come from laboratories such as PEAR (Princeton Engineering Anomalies Research Laboratory) using random number generators (RNG) to study "micro-PK." The positive results seen at PEAR have not been replicated with significant results or similar effect size according to Parker. Additionally, other studies have shown psi-missing attributed to micro-PK within micro-PK protocols. Such varying results might be caused by mixing gifted PK subjects with the average person who does not have such ability. That would explain why such a small effect is seen. Interestingly, based on a meta-analysis by Radin and Ferrari (1991) of dice-throwing studies, there is a very small but significant and consistent effect of PK with intention. This appears to be universal and is not present in control studies. This suggests that this small but

universal effect is not due to an error. Subsequently, Parker describes a PK-conducive psychological state involving ". . . suspension of one's critical ability and dissociation from one's ordinary sense of identity" (p. 75). This is followed by cases of poltergeists involving disturbed adolescents with apparent temporal lobe abnormalities. The conclusion of the chapter includes an introduction of the filter theory, attributed to F. W. H. Myers, about how much information processing takes place outside of conscious awareness.

Chapter 5, by Joan Hageman and colleagues, is titled "The Neurobiology of Trance and Mediumship in Brazil." The chapter commences with a detailed list of "methodological pitfalls" in such studies. These include materialist interpretations (assuming region-specific brain activity as the origin and cause of such experiences), secondhand hearsay, causality/directionality within a proposed mind–brain interaction, superficially clustering subjects by their subjective experience, ignoring the holistic viewpoint, theories that do not include a mechanism of action or are not all-encompassing of phenomena, and finally using novices instead of expert subjects. This is followed by an overview of electrophysiology/neuroimaging studies of various trance and religious experiences including a table for comparison. Common findings include an increase in brain activity in the frontal lobes during trance. Next, they present two original studies conducted in Brazil with mediums. In the first study, two mediums and a control were used. To avoid motion-induced artifacts in the autonomic (hand temperature, EMG, heart rate, and skin conductance level) and central (EEG) measures, ". . . mediums were instructed to imagine incorporating a spirit (rather than to engage in direct incorporation)" (p. 97). This fact alone means that the results obtained do not necessarily reflect what occurs in the physiology of a medium when he/she actually does the regular process. Results from the two mediums were paradoxical, combining sympathetic activation in the ANS with relaxation in the CNS. Study two employed EEG (with no mention of montage or number of channels) with nine mediums, all free of known mental illness and use of indigenous ritualistic substances, doing their usual practices of mediumship.

> EEG findings did not show a consistent pattern among the mediums, but ranged from absolutely normal electrical activity to some degree of EEG slowing. (p. 104)

This was determined by comparing the baseline with the trance states. Most noteworthy, none of these mediums showed any evidence of epilepsy. This chapter mainly shows that more and better research is needed in this area to elucidate neural correlates of trance and mediumistic experiences.

Chapter 6 by Norman Don is a presentation of his group's research which includes unconscious physiological measures associated with psi. In a number

of the studies presented, behavioral performance on psi tasks was at chance level, but physiological responses to targets were different than responses to non-targets, providing evidence of unconscious physiological differences associated with precognition and clairvoyance, depending on the design. Following this, Don presents his EEG research with a professed psychic, Olof Jonsson. Jonsson claimed that he experienced three unique states (CD 1–3) which he associated with psi, CD 2 and CD 3 being the prime states for this. In the analysis, CD 3 was contrasted with CD 1. CD 3 with psi-hitting was associated with right hemispheric dominance shown by EEG power and 40 Hz occipital activity. Using Stepwise discriminant analysis, a model was developed that allowed for correct classification of CD 1 and CD 3 and classification of correct versus incorrect behavioral psi responses with the incredible claim of ≥90% accuracy. Based on this 40 Hz gamma activity, Don and colleagues decided to go back to reexamine the data in their ERP studies of unconscious psi. As expected, they found that there was ". . . significantly more 40 Hz EEG activity for target card imagery than for non-target imagery" (p. 118). This was demonstrated in the right frontal and posterior regions. Don and colleagues have also seen similar gamma activity in ayahuasca users during Brazilian rituals, trance surgeons during a "possession trance," and even those claiming to have been abducted by extraterrestrials during self-initiated altered states of consciousness involving hyper-vigilance. This was often with simultaneous random event generators giving non-random output. Additionally, Don was inspired to examine the hits and misses in behavioral data. He created a table of this which resulted in what appeared to be a wave-like oscillating pattern. This was visible even though the behavioral results were at chance, suggesting the answers were random. However, despite there being no periodicity in the data, spectral analysis revealed peaks that were significant. Don termed these anomalies "correlation waves." In further research, the peaks in these wave patterns tended to correspond to hits. Don interpreted this to represent a leak from the unconscious mind to consciousness based on placement of the trial within the run. Furthermore, Don claimed this was found in both precognition and clairvoyance experiments. Based on this, Don made a statement that this ". . . suggests that psi is woven into the very fabric of nature" (p. 125). On further speculation, Don ties this together with the gamma waves and correlation waves. While the information provided in this chapter is intriguing, there are many questions which remain unanswered to this reader. For instance, it is not clear from this chapter if the differences seen between target and non-target averages are statistically significant. Furthermore, the explanation of "correlation waves" is unclear. Finally, connecting gamma brain waves and correlation waves with psi and the fabric of the universe seems a bit premature. Presumably, many of

these issues would become clearer upon reading the original papers. However, a chapter such as this should be complete within itself.

Chapter 7 by Vernon Neppe is an attempt to present the neurobiology of "subjective paranormal experiences" (SPEs). Oddly enough, none of the published research by Varela and colleagues about neurophenomenology is included in the chapter. Out of body experiences (OBEs) and near-death experiences (NDEs) are discussed in relation to the supposed induction of similar experiences via brain stimulation, and in relation to traumatic insults and pathologies such as narcolepsy and temporal lobe sensitivities. Some of this appears to suggest that "REM intrusion" into normal consciousness may be associated with SPEs. Next, Neppe transitions awkwardly into a review of fMRI studies of ESP, commencing with an appropriately scathing review of Moulton and Kosslyn (2008) and their Harvard press release. Ironically, as Neppe reports, one pair of the subjects in this study appeared to show above chance on ESP scores, and trials of correct ESP responses were associated with unique brain activity changes (reductions in brain activity in temporal lobes and other areas). Moulton and Kosslyn dismissed this as artifact. In contrast to his previous assertions, Neppe proceeds to say that subtle differences associated with psi may not be easily seen in fMRI due to the low signal-to-noise ratio. There is substantial evidence against Neppe's claim that subtle differences may not be seen in fMRI. Many event-related fMRI experiments have discerned parametric subtleties in responses and successfully deconvolved overlapping hemodynamic response curves even to very brief stimuli (for instance, refer to Dale and Buckner 1997). Next, Neppe proceeds with a brief survey of previous and generally more successful fMRI studies of ESP-related anomalies. Then there is a review of the temporal lobe's potential involvement in SPEs via the use of the "Neppe Temporal Lobe Questionnaire" (NTLQ) and the "Inventory of Neppe of Symptoms of Epilepsy and the Temporal Lobe" (INSET) for possible temporal lobe symptoms (PTLS). Following this, Neppe provides a possible neuropsychological basis for SPEs involving frontal lobe inhibition or inhibition of the "cortical filters." Obviously, research needs to be conducted to validate this theory. The chapter ends with Neppe's own phenomenological classification of SPEs. Throughout this chapter, Neppe references his own prior work without clear explanations of meaning and definitions, using his own often idiosyncratic language. Such a chapter should be complete within itself and not require one to read all of Neppe's work to make sense of it. Terms of this ilk include "dimensional biophysicist" (apparently replacing parapsychologist), parafamiliar, metafamiliar, queryfamiliar, and Neppe's Anomalous Multiaxial Event System (NAMES).

Chapter 8 by Morris Freedman is simply titled "Psi and the Brain." Freedman points out that despite the large body of literature on psi, there has

been a lack of focus on the potential neural correlates or specific brain loci correlated with authentic psi. Here authentic psi is differentiated from Neppe's SPEs and limited to lab experiments with objective measures including REG and neuroimaging. Freedman elaborates that there are many brain regions that have been theorized to be involved with objective psi, e.g., reticular formation of the brain stem and right hemispheric occipito–parietal regions, in contrast with SPEs which appear to involve the temporal lobes. One theory given about the reason for the elusiveness of psi is that we have developed, evolutionarily, a filter to inhibit the overwhelming surge of psi-related input we receive at every moment. Without such a filter, we would be overwhelmed and unable to focus on information vital for our survival. If we could discover the nature of such a filter, and find a way to inhibit it, this might uncover the neural correlates that mediate authentic psi and improve access to strong psi phenomena. As in the previous chapter, there is mention of reducing self-awareness and frontal lobe activity, perhaps via altered states of consciousness that may be intertwined with this filter. Freedman further explains that neuropsychological patients with frontal lobe lesions or disorders with known deficits in frontal lobe functioning could be key to examining this theory. He then presents a pilot study using patients with pre-existing frontal lesions. A random event generator (REG) was used with a visual stimuli representing the outputs. Only one patient, one with a left frontal lesion, was able to significantly alter the REG output, and thus the visual display, toward the right hemifield with directed intention. One possible explanation given is that an intact right frontal lobe is required to sustain attention to complete the task at hand. Thus, right frontal lesion and bilateral lesion patients were unable to accomplish this. Freedman rightfully suggests that further research is needed to verify the importance of left frontal lobe inhibition in psi tasks. Note that transcranial magnetic stimulation (TMS) might be used to create reversible functional lesions in human subjects, e.g., to test this theory.

Chapter 9, by David Luke and Harris Friedman, focuses on the neurochemistry of psi and SPEs. Psychedelic psychotherapy using LSD has yielded much evidence of psi-related SPEs. Furthermore, self-reports of psychedelic users has also yielded a high percent of such experiences using the Anomalous Experiences Inventory. It is not clear if this is showing a cause and effect relationship between the use of psychedelics and such experiences. It may be that people who choose to use psychedelics do so because they believe in SPEs and are trying to induce them. Therefore, belief and corresponding interpretation of experience may have more to do with SPEs in these people than do the psychedelics themselves. Alternatively, these substances may be inhibiting frontal lobe activity and self-awareness of the body, leading to dissociation and inhibition of a possible psi filter. The authors review seventeen

studies involving the use of psychedelics in the lab with psi tasks using a forced-choice paradigm. The majority of these were exploratory and lacking methodological rigor such as a control condition, etc., and they did not yield significant results. Only one such study (van Asperen de Boer, Barkema, & Kappers 1966) demonstrated that psilocybin produced psi-hitting significantly greater than chance using a Zener-card–like task. Free-response experiments have yielded much better results than forced-choice protocols. As expected, Luke and Friedman report that subjects in these studies were "high" and thus it was difficult to get them to focus on a laboratory experiment. Osis (1961) and Wezelman and Bierman (1997) are examples of studies also showing significant results. Again, as in previous chapters, a model of the brain as a filter to psi is suggested. Psychedelics, as Huxley (1954) put it, may ". . . override the 'reducing valve' of the brain" (p. 167) and thus permit psi to emerge into conscious awareness. Subsequently, Luke and Friedman present several neurochemical models of psi and SPEs based on psychedelics and their central mechanisms of action. This includes a disruption (via antagonism of 5-HT_{2A} receptors) of the cortico-striato-thalamo-cortical circuit, the β-carboline and tryptamine model via MAO inhibition as via DMT (dimethyltryptamine) and ayahuasca, or endogenously via pineal secretions, the DMT model again with endogenous involvement of the pineal, and a ketamine model of NMDA-receptor binding, blocking glutamate and eliciting dissociative experiences. As the authors readily admit, this chapter is highly speculative. The conclusion of the chapter is that there are four possible models of psi and SPEs: (1) the brain acting as a filter, (2) the β-carboline and tryptamine model, and the (3) DMT and (4) ketamine models. However, one possibility, as mentioned by the authors, is that all of these models interact, intersect, or even overlap. Psychedelics and ketamine may work via different mechanisms to inhibit the frontal lobe function where such a psi-filter may exist.

The book ends with a Postscript by Stanley Krippner and Harris Friedman, emphasizing a "costly signaling theory" (CST). In accordance with CST, such a signal between people (perhaps telepathy) would need to be ". . . reliable, authentic, and fairly impervious to fakery" (p. 191). However, this is not how psi appears to work. Furthermore, the explanation of this theory in the context of psi is not very clear here. Following this is an overview of the book's various chapters, and excessive references to Gazzaniga. Mirror neurons are presented as binding people. Could this be a neural interface of telepathy via some non-local or quantum entanglement-like mechanism? At this point, that is mere conjecture. The Postscript ends with a non-materialist explanation of consciousness in which a mind may exist outside of the brain.

In sum, this book provides a wealth of information and varying opinions of relevant studies of SPEs and psi; some contesting the existence of psi,

whereas others provide evidence in favor of it. The studies presented are a bit selective and uneven in quality. This is all within the context of possible neural correlates of psi and related SPEs. Suggested neural correlates for psi and related SPEs include slow cortical potentials, gamma waves (40 Hz), and temporal lobe abnormalities as shown in EEG; fMRI correlates; reduced physiological arousal, as measured by psychophysiology and neuroimaging, shown to be conducive to psi; the model of the cortex (possibly the frontal lobes) acting as a filter for unconscious psi; and psychedelic models of psi including serotonergic and glutamatergic systems possibly inhibiting the frontal lobes and thus the filter of psi. Several chapters mention the need for theories of mechanism of action of psi and related experiences while ignoring the evidence for psi. Other chapters supply metaphors of quantum mechanics without a direct mapping onto phenomena. In order to progress in this field, testable hypotheses of mechanisms of action are needed. Some theories may help divulge a mechanism of action of psi, whereas others may not. If theories are not supported by evidence, this does not discount the possibility of psi. However, it remains clear that until a mechanism/or mechanisms of action for psi can be supported by objective evidence, leading to predictions of outcome and replicability, this debate will be ongoing.

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ESSAY REVIEW

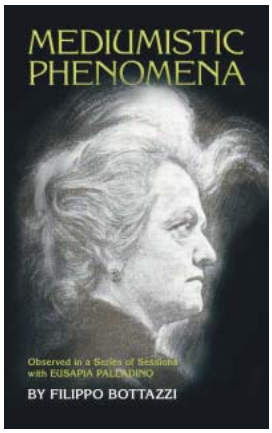
Bottazzi and Palladino: The 1907 Seances

Mediumistic Phenomena: Observed in a Series of Sessions with Eusapia Palladino by Filippo Bottazzi, translated by Irmeli Routti and Antonio Giuditta. Princeton, NJ: ICRL Press, 2011. 201 pp. $12.95. ISBN 9781936033058.

In 1908 a student of psychic phenomena stated that Italian medium Eusapia Palladino (1854–1918) had been mentioned thousands of times in the reviews of psychic topics. About her mediumship, we were told, "now there exists an entire library" (Morselli 1908, Vol. 1:132). The book commented on here is part of that library.

Palladino showed mental phenomena such as communications said to come mainly from her spirit control John King, but her performances consisted mostly of physical phenomena such as movement of objects, imprints on clay, luminous effects, raps, touches, cold winds, and materializations. While she belongs to the old days of psychical research, her presence still lingers among us, as seen in defenses of the reality of her manifestations (e.g., Braude 2007), in historical overviews (e.g., Blondel 2002), and in papers that have been published in relatively recent issues of the *JSE* (Alvarado 2011, Giuditta 2010).

As I have argued before, Palladino's case was important for the development of psychical research in various ways (Alvarado 1993, 2011). Historically speaking, the medium's performances were a factor that led researchers to produce methodological and conceptual developments, and the sensational character of her phenomena and the publicity around them created various images about mediumship. But such performances also provided much evidence for the reality of physical phenomena. In fact, she convinced many influential researchers of the genuineness of her manifestations, as seen in overviews such as Carrington's *Eusapia Palladino and Her Phenomena* (1909). This was the case with Alexander Aksakov (1832–1902), Hereward Carrington (1880–1958), Albert de Rochas (1837–1914), Everard Feilding (1867–1936), Camille Flammarion (1842–1925), Oliver Lodge (1851–1940), Cesare Lombroso (1835–1909), Joseph Maxwell (1858–1938), Enrico Morselli (1852–1929), Frederic W. H. Myers (1843–1901), Charles Richet (1850–1935), Albert F. von Schrenck-Notzing (1862–1929), and Julian Ochorowicz (1850–1917).



In 1907 Italian physiologist Filippo Bottazzi (1867–1941) joined the ranks of investigators of Palladino who became convinced of her phenomena. By this time Bottazzi's scientific career was established, his having won several awards and important university appointments. He went on to become even more eminent in later years, although it has been argued that he has been somewhat forgotten (Stanzione 2011). His work on Palladino, first published in Italian in the *Rivista d'Italia* (Bottazzi 1907a), was translated and published in the same year in French (Bottazzi 1907b) and English (Bottazzi 1907c). The studies received much publicity in several European countries, as seen in magazines and journals published in Italy (Lombroso 1907), Austria (S 1907), and France (Van Thieghem 1907). There were also many discussions of the seances in the United States, as seen in writer Hamlin Garland's (1860–1940) book *The Shadow World* (1908) and in historian and writer Gustavus Myers' (1872–1942) *Beyond the Borderline of Life* (1910), not to mention many articles in magazines such as the *American Review of Reviews* (The Progress of the World 1907). Somewhat later Bottazzi (1909) presented a similar account of the seances in a book entitled *Fenomeni Medianici Osservati in una Serie di Sedute Fatte con Eusapia Paladino*, which recent translation is the topic of this review.

Mediumistic Phenomena is the result of neurobiologist Antonio Giuditta's interest in the seances Bottazzi had with Palladino in 1907. His work has been presented to members of the Society for Scientific Exploration both in a paper delivered at the Eighth European SSE Meeting held in Italy in August of 2009 and in an article published in the Society's *Journal* (Giuditta 2010). The book was translated by Giuditta, together with Irmeli Routti.

It consists of a report of eight seances conducted in Bottazzi's laboratory in which a variety of instrumental studies were made. Many of the seances were attended as well by physicians and scientists. Some of them included Gino Galeotti (professor of general pathology), Tommaso De Amicis (professor of dermatology and syphilograph), Oscar Carpa (professor of physics), Luigi Lombardi (professor of electrotechnology), and Sergio Pansini (professor of medical semiotics). There were also others who joined some of the seances, among them engineer Emmanuela Jona, senator Antonio Cardarelli, and Bottazzi's wife. Her full name, which is not mentioned in the report, was Annunziata Fabbri.

By the time Bottazzi entered the scene there had been a long history of studies of physical mediums and of Palladino in particular, not to mention a rich Italian history of the subject, ably documented by Massimo Biondi (1988). But Bottazzi admitted in his Introduction that he "had read little or nothing of" (p. 4) mediumistic phenomena. He stated that he had heard of the studies of Richet and others and that he had been impressed by Barzini's articles on Palladino published in the Italian newspaper *Corriere della Sera*. The articles led him first to a state of doubt and then to being interested in studying the topic himself. This was a reference to journalist Luigi Barzini (1874–1947), who popularized the medium in his articles, works that were collected in a book (Barzini 1907/1984).

Bottazzi stated that not all the phenomena witnessed by him and his collaborators were included in the book. For example, in the account of the first seance he stated: "Given the little relevance of the phenomena observed during the first session, the sequence of their appearance is not worth describing. I will summarize in few words the results obtained" (p. 39). Later Bottazzi said: "Not caring about the precise sequence of observed events, I prefer to describe them briefly" (p. 45). Nonetheless the book contains many descriptions such as the following.

At one point during the fifth seance a switch that was connected to a lamp was moved around and thrown on the seance table by an "invisible hand." The light was then turned on and off several times. Later on:

> The switch was placed on the table. Eusapia said: "Look how it is moving." We all fixed our gaze on the small object and we saw that it rose a few millimeters above the table top, oscillated and vibrated, as if invaded by an interior quiver. Eusapia's hands, held by Galeotti and me, were at least thirty centimeters away from the switch. (p. 113)

Regarding one common phenomena, table levitations, Bottazzi wrote in his account of the fourth seance:

> We obtained a levitation lasting about 10 seconds at a height of 30–40 cm and a shorter but higher one while Palladino was the only one standing up. Finally, at the end of the session, an additional levitation occurred that lasted several seconds while all of us were standing up at Palladino's request. . . . Sometimes we tried all together to lower it by pressing its surface with our hands, but without success. It yielded and lowered a little but as soon as we let go our hands it rose up again. (p. 89)

An interesting phenomenon, and one reported frequently by previous investigators of Palladino, was that of synchronisms. As Bottazzi explained:

> Any mediumistic event was almost always occurring simultaneously with movements of one or more parts of the *medium*'s body. . . . (p. 62)

For example, during the second seance:

> The table started moving by steps, every pull perfectly corresponding to pressures and pulls made by Palladino's hands on our hands (mine and Pansini's). . . . Every pull of the small table corresponded in perfect synchrony with a push by Eusapia's leg against Jona's knee and with the contraction of her thigh muscles. (p. 46)

Bottazzi stated that the synchrony between actions showed "a common point of origin," the will of the medium (p. 127).

Interestingly Mrs. Bottazzi seemed to attract phenomena such as touches. In answer to the question if she had mediumistic powers, the medium's spirit control John King answered in the affirmative. As her husband wrote about the third seance:

> The curtain swelled around her several times, like hugging. She was unceasingly touched, fondled (she said it felt like a cat climbing up her right arm toward her shoulder), tapped on her shoulder with something like the open palm of a hand (and we all heard the blows), and she was the one who saw the largest number of apparitions. (p. 60)

Several of the instruments used produced graphic recordings that were presented by Bottazzi to show the objectivity of the manifestations:

> The telegraph key was struck several times. . . . We all clearly heard the typical sounds of energetic, quick hits. To certify that it was not an illusion, or a collective hallucination, the second trace from the top . . . shows three groups of signals and two isolated beats in between them. (pp. 71–72)

Other devices produced graphic recordings as well (see photos such as those presented in figures 4, 13, and 15 in the book). There are also brief descriptions of failures to obtain effects on the instruments (e.g., those described on pp. 77–78).

Similar to previous observers, Bottazzi reported some physiological observations of the medium after the seance:

> It is noteworthy that after every session Palladino had considerable hyperalgesia (exaggerated sensitivity to pain) on her hands, especially on their back side. She said it felt like burning, as if her hands had been immersed in lye for a long time. In fact, her hands were always red and hot, and the subcutaneous veins appeared full of blood. (p. 132)

Other observations published before the current ones referred to weakness. According to De Rochas (1896), after seances

> [the medium] was completely exhausted and nearly unconscious; her face showed fatigue, suffering; she even seemed older. She could hardly stand and had the most complete apathy; we were obliged to hold her under her arms to take her to the dining room. . . . (p. 19)

In addition, and as done by other researchers (Courtier 1908), analyses were made of the content of the medium's urine, before and after the sixth and seventh seances. With regard to the sixth seance, Bottazzi stated:

> Comparison of the two samples of urine showed that the one taken after the session was considerably more concentrated. It had a higher specific weight, higher osmotic pressure and electric conductivity. Total nitrogen and albumin were also increased.
>
> Kidneys seemed to produce more concentrated urine during the sessions. Despite the presence of albumin and sugar, values of osmotic pressure and electric conductivity of the urine differed little or not at all from the normal levels. Microscopic examination never showed the presence of kidney cells nor cylinders. This was a strange case of chronic albuminuria without definitive sign of nephritis.
>
> The observation of strong urine acidity and abundant content in uric acid was remarkable. Some uric acid crystals were already present shortly after urine was collected. Their number increased enormously, and the layer they formed with time became macroscopically visible while the urine remained acid. Eusapia was undoubtedly a subject of clearly arthritic character, a uricemic person. (pp. 151–152)

Relevant to these results, and as I have summarized elsewhere (Alvarado 2011), Palladino suffered from diabetes and died of nephritis.

Synchronic phenomena and observations such as the following led Bottazzi to speculate that the medium produced projections from her body such as "invisible hands." According to his report of the seventh seance:

> I saw a human hand of natural color, and I felt with my hand the fingers and the back of a lukewarm, muscular, rough hand. *The hand vanished, and my eyes saw it retreat, describing an arc of a circle. As if entering back into Palladino's body.* (pp. 165–166, italics in the original)

Interestingly, Bottazzi states that during the eighth seance Galeotti saw two left arms in the medium. He presents in his book what I presume is his recollection of Galeotti's statement during the seance:

> I see two identical left arms. One is on the table and is the one Mrs. Bottazzi is holding, the other seems to come out from Eusapia's shoulder, to approach Mrs. Bottazzi, touch her, and then return back and melt into Eusapia's body, vanishing. (p. 180)

Such observations led to ideas about a "splitting of . . . physiological personality" (p. 198) consisting of limbs or complete figures emanating from the medium's body. Bottazzi believed that with these hands

> [the medium] felt form, consistency, cold and hot, hard and soft, humid and dry, exactly the same way she would feel by touching and feeling with her physical hands. . . . (pp. 117–118).

Furthermore, Bottazzi wrote:

> *Mediumistic phenomena are not mere hallucinations of those attending sessions known as spiritualistic sittings. They are biological phenomena depending on the MEDIUM's organism. If they are such, they occur AS IF they are operated by the extensions of natural limbs or by additional limbs stemming out of the MEDIUM's body, and returning and dissolving into it after variable time. During those periods they reveal themselves by the sensations they elicit in us as limbs in no essential way different from natural limbs.* (p. 201, Bottazzi's italics)

The book is a useful contribution in that it presents in English a difficult-to-obtain book about the medium in question. Contemporary readers will appreciate having a translation of it. While a translation of the original Italian report (Bottazzi 1907a) had been published in the *Annals of Psychical Science* (Bottazzi 1907c), the present translation of the 1909 book has more details, particularly of the eighth seance in which three photographs are included that did not appear in the *Annals* paper. The instrumental and physiological tests show the scientific spirit in which some mediumistic research was conducted in the old days, and serve as a reminder of Italian scientific interest in mediumship, a topic that includes the work of other individuals such as Cesare Lombroso and Enrico Morselli (Iannuzzo 1986).

Furthermore, the book may be evaluated focusing on two different issues. I am referring to evidential considerations as well as issues of historical context. From the evidential point of view it is to be regretted that Bottazzi's initial introductory comments about the medium (pp. 2–11) do not include mention of Palladino's well-known propensity to commit fraud. He mentioned that someone asserted that the medium "is almost always resorting to fraud" (p. 4), but this is quite different from conveying the well-known fact that she was known to commit trickery. The reader has to wait to get to pages 124 and 187 for an admission that others found her in trickery. Bottazzi stated there was no fraud detected in his seances, and he mentions a suspicious, but inconclusive, observation (pp. 122–123). The issue of fraud was mentioned throughout the report (pp. 62, 84, 124), and we are presented with many assurances that the medium's hands were controlled or that they were far from where the phenomena took place (pp. 68–69, 75, 90–91).

Bottazzi would have been more successful in conveying conviction if his report had been more complete and if he had included actual accounts of phenomena and descriptions of controls from his collaborators, as opposed to the summaries he presents in the book. This is not to say that some of the

observations recorded in his book are not impressive or that Bottazzi was a naïve observer. In his words:

> She is operating with her legs and feet! But when her legs are stretched over my knees, and her feet are propped against Jona's knees, or are held by Scarpa under the table, how could these feet drag out a table or a chair from the mediumistic cabinet? (p. 192).

Filippo Bottazzi

Bottazzi went on to list a variety of effects inconsistent with the medium's well-known repertoire of tricks, effects that were similar to those reported by previous observers.

On another issue, documents such as *Mediumistic Phenomena*, be they translations or reprints of old work, are generally presented in context. That is, background information is provided about personalities, investigations, and other events to help contemporary readers understand the document in question because they are generally too removed in time from its original appearance. Unfortunately there is little about this in the book. The meager three pages about Palladino presented at the beginning of the book (pp. v–vii), and the two references presented (p. vii) are not enough to convey to contemporary readers who are not familiar or are only slightly familiar with the medium information about her importance for psychical research. There could have been a more detailed discussion and a bibliography of the investigations of Palladino conducted both before and after 1907, or of the topic of spiritism and mediumship in Italy (Biondi 1988). Furthermore, readers would have welcomed more information about individuals mentioned (e.g., Barzini, p. 4, Flammarion, p. 18, Morselli, p. 189, de Rochas, p. 195) and about the reception of Bottazzi's work (see the opening comments of this review).

I also believe that the book would have been more effective if it would have included information about the existence of a tradition of concepts of force related to physical mediumship relevant to Bottazzi's speculations. Such ideas were prevalent in the literature published before and after the report and consist of a variety of biophysical speculations of projections from the bodies of mediums (Alvarado 2006), including several specifically proposed to account for Palladino's phenomena (Alvarado 1993). An example was Oliver Lodge's (1894) concept of "temporary prolongations" emanating from Palladino's body. Lodge wrote:

> I myself have been frequently touched by something which might most readily be described as such a prolongation or formation, and have sometimes seen such a thing while it was touching another person. But the effect on an observer is usually more as if the connecting link, if any, were invisible and intangible, or as if a portion of vital or directing energy had been detached, and were producing distant movements without any apparent connexion with the medium. (Lodge 1894:335)

In addition to Lodge, others reported observations of materialized "prolongations" that were consistent with Bottazzi's ideas (Morselli 1908, Vol. 1:198,212). For example, Albert De Rochas (1896) wrote:

> M. Maxwell was invited by Eusapia to watch and had the visual sensation of a forearm and a hand. He saw in front of him, on the strip of the wall illuminated by the crack of the door, the silhouette of a hand and arm that were above the head of M. Sabatier. They appeared to him on several occasions to descend and to raise as if to touch the head of M. Sabatier who said that at that moment he felt various touches. The forearm felt was long and thin. He did not see the continuity with the arm because it was lost in the shadows. . . . (De Rochas 1896:292)

While a more scholarly presentation of the work would have improved the value of this book, this publication is a welcome addition to the literature. If anything, it helps us to recover a piece of the generally forgotten literatures of physical mediumship and of the history of psychical research.

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BOOK REVIEWS

Matrix Energetics: The Science and Art of Transformation by Richard Bartlett. Atria Books, 2007. 208 pp. $22.95 (hardcover). ISBN 9781582701639.

The Physics of Miracles: Tapping into the Field of Consciousness Potential by Richard Bartlett. Atria Books, 2009. 287 pp. $24.99 (hardcover). ISBN 9781582702476.

I have read the first book, *Matrix Energetics*, four times and the second book, *The Physics of Miracles*, twice. Just after reading the first book for the first time, one of my students emailed me to say that she had been diagnosed with cancer. I offered to do remote healing for her using the techniques described in the book, to which she agreed. The apparent results were sufficiently promising that I decided to attend an instructional seminar taught by Richard Bartlett in Miami. Since becoming involved with this healing modality, I have attended the Level 1 seminar three times, Level 2 twice, and Levels 3 and 4 once each. Completely independently of Richard Bartlett, I completed a formal experiment in remote healing using techniques derived from Matrix Energetics (ME) and am currently on a second experiment in which there are statistically significantly greater changes in self-reported energy levels at the time that I am actually doing remote healing compared to when I am not. So, based on my experience with ME, I think that the subject matter of these books should be taken seriously and I recommend both of them.

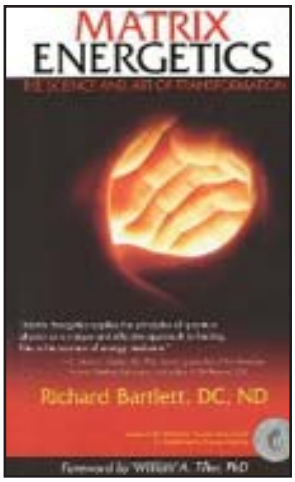
The substance of ME is trivially simple. It boils down to the idea that reality, including physical manifestation, is more plastic than we usually think and can be changed by intending it to be different. Well, there are lots of books that say the same sort of thing, none of which make that sound particularly likely to happen. What kept me reading the first of these two books was the significance that Bartlett attaches to Rupert Sheldrake's notion of morphic fields. A morphic field is a template that physical structures or events can follow (Sheldrake 1988), and it has increasingly seemed to me that something like the presence of morphic fields is needed to explain why physical manifestation takes the shape that it does (Barušs 2010). Bartlett contends that the problem with approaching illnesses, such as cancer, as *problems,* is that that puts one into the morphic field of the illness with the attendant limitations on the possible outcomes that go along with that illness. For beginners, the advice is simply to get out from

underneath the morphic field of whatever problem presents itself. Those who are more advanced can acknowledge a particular problem and stay open outside of its morphic field at the same time. "It is kind of like a split or dual mind" (Bartlett 2009:81).

By itself, the acknowledgment of the presence of morphic fields might not seem to have got us very far. But Bartlett combines that with the notion of superposition in quantum theory. For him, reality remains in an indeterminate state until an observation is made at which time the observer can influence the outcome. Bartlett does not consider the nuances of that contention, for example, of whether it is the selection of what to measure (Kochen & Specker 1967) or the collapse that one affects (Barušs 1986, 2008a, 2008c), or the dismissal of the need for collapse altogether due to decoherence (Polkinghorne 2002). For Bartlett, the contention is simply that there are multiple scripts of what can happen and we can select from among those possibilities. Or, better yet, according to Bartlett, we can allow some deeper wisdom within or outside of ourselves to select for us, since we might not be the best judge of what is optimal for ourselves or someone else.

There are also techniques that one uses in the course of doing ME, such as "two-point," "timetravel," "archetypes," and so on. These are described in the first book *Matrix Energetics*. When doing "two-point" for example, I would find a point on a person's body or somewhere else by noticing what I notice; place one hand on that point; find a second point that seems to me to be related in some way to the first, again by noticing what I notice; imagining that the two points are connected; and then "'doing nothing'" (Bartlett 2009:80). All of the techniques are essentially variations on playing with one's imagination in whatever way seems appropriate for oneself. And indeed, as pointed out in the Level 4 training seminar in San Diego on February 26–28, 2011, being creative with these techniques is thought to enhance their effectiveness.

We still do not seem to have gotten anywhere. Someone shows up with cancer. I deliberately forget about the fact that she has cancer, I open up to the possibility that things could be different in her life, I do any of a number of techniques that involve imagining things in my mind, and I make sure I do not intend that any specific outcome occur. *How could that possibly work?* Well, let us return to that question in a moment. Let us consider a more practical question first, *Does that work?* The first answer to the second question is that we do not know. There have been no outcome studies. In fact, Bartlett disparages the use of "innumerable double-blind studies"—the point being rather to seek "new ways to see and to be" (Bartlett 2009:28). The second answer is that there are lots of examples of apparently miraculous healing given in the books, and Bartlett says: "I myself have witnessed the disappearance or dissolving of tumors and other 'medical miracles'" (Bartlett 2009:29). But he follows



that up with "Does this *always* happen? I wish it were so. If I could heal cancer or anything else with a reasonably reliable certainty, I would do it full-time . . ." (p. 29). But this is precisely where science could be of assistance. If there are cases in which broken bones reset, tumors disappear, scoliosis straightens out, and so on, then those can be compared to cases in which such dramatic healing does not take place and an effort can be made to determine the relevant parameters.

If we assume for a moment that radical transformation actually occurs at least in some cases, then let us return to the first question: *How could that possibly work?* I think that there are several logical possibilities. The first is that ME has its own morphic field; one that continues to grow with each book that Bartlett writes and every seminar that he teaches. "With enough individuals sustaining the consciousness of our special case reality, such as Matrix Energetics, it functions consistently and reliably" (Bartlett 2009:48). So, if I am doing ME, I am drawing on the resources that are delimited by ME, and the idea is that those surpass anything that I could access on my own. And the expectation of the occurrence of radical transformation is embedded within the ME morphic field, increasing the likelihood of anomalous healing.

The second reason that ME could possibly work, I think, has to do with the degree of commitment with which one makes imaginary changes. Bartlett emphasizes the significance of this in a case in which he "became" the Brazilian healer John of God. Bartlett says that he looked at a picture of John of God and

> Assuming resonance with his morphic field, I reached up to [the patient]'s nose and "saw" and felt myself as John of God. Without hesitation, in my mind's eye I inserted John's surgical clamp right up the man's nose and into his brain, just as I had seen in the video [of John of God]. Note, I didn't say that I imagined it; that would not be enough. Instantly [the patient] slumped into unconsciousness. (Bartlett 2007:79)

Of course, for this to work, what goes on in our minds must necessarily have such intimate connections with physical manifestation as to allow for such mental events to have concurrent physical consequences. Which brings us full circle to the initial contention that intention can affect physical manifestation. And I actually think that that is likely on both theoretical and empirical grounds (Barušs 2009). So the third reason that ME could possibly work is because reality is structured in such a way as to allow it to work.

The first book is more practical in that it provides detailed instructions for

doing the ME techniques, whereas the second book spends more time providing a theoretical rationale for why such techniques might work, as well as taking on other subject matter such as invisibility and levitation.

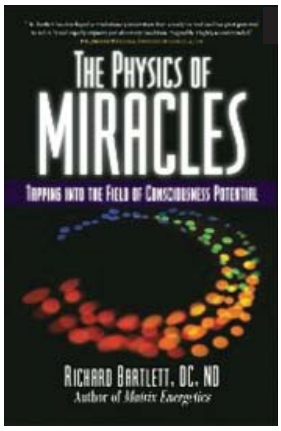
The first book sits well with Bartlett's training as a chiropractor and naturopath and his experience with healing. I feel that Bartlett is on fairly firm ground here and that the first is the stronger of the two books. In venturing a bit deeper into physics in the second book, Bartlett's footing is less secure, and greater care could have been taken with fact-checking. For example, Bartlett gives an example of a complex conjugate number that is obviously not a complex conjugate number (Bartlett 2009:62). This is not surprising, perhaps, because he tells us "I am neither a mathematician nor a physicist" (Bartlett 2007:xix) and "I flunked algebra" (Bartlett 2009:52), but then why not have a mathematician or a physicist proofread the manuscript before it goes to print?

There are similar sorts of factual errors in both books. For instance, Bartlett keeps saying that everything is made of photons (Bartlett 2007:xix,46, Bartlett 2009:111). There are, of course, lots of other elementary particles, such as electrons, quarks, gluons, and so on, which do not reduce to photons (Sudbery 1986). And there are errors not just with mathematics and physics but also with neuroscience. For instance, Bartlett keeps referring to the "left brain" and the "right brain" with the implication that the "left brain" has a more rational processing style and typically dominates the "right brain" which has a more imaginative manner of processing. For instance,

> *By cultivating the habit of asking powerful, mind-altering questions, you are training your right brain to respond to the signals from your subconscious.* (Bartlett 2009:13)

The idea that the left and right cerebral hemispheres have their own distinctive streams of consciousness arose from Roger Sperry's studies of a small number of patients with epilepsy who had had some of their brain commissures severed to varying degrees, thus somewhat isolating the cerebral hemispheres from one another (e.g., Teng & Sperry 1973). By 1980 it had become clear that, in the intact brain, the cerebral hemispheres do not act as units and that there are no differences in processing style between the two hemispheres (Yates 1980, Barušs 1990). In other words, there are no differences between the "left brain" and the "right brain" aside from some small functional asymmetries, so that the expressions "left brain" and "right brain" only have metaphorical value, if any at all.

In *The Physics of Miracles* Bartlett introduces the notion of a *torsion field*, which he then uses as another way of explaining apparently miraculous physical occurrences. As I understand what he is trying to say, the idea is that James Maxwell's original quaternion formulation of electromagnetism accounted for



gravity as well as electromagnetic phenomena and that Oliver Heaviside's subsequent reformulation of Maxwell's work using vector algebra pared away a lot of the content of the original formulation, including the explanation of gravity. Apparently in the original work, physical reality is conceptualized as being four-dimensional by the addition of a scalar potential to the three spatial dimensions. According to Bartlett, this potential "can be projected into the vacuum" causing stress that is "linked" to torsion fields (Bartlett 2009:120, cf. Hoagland & Bara 2009) which "interact with the spins of particles" such as protons and neutrons (Bartlett 2009:144). Bartlett defines a "torsion field" as "The quantum spin of empty space, the large-scale coherent effects of the spin of the particles in the virtual sea" (Bartlett 2009:246, cf. Swanson 2010) and ends up saying things like: "You can imprint your thought forms onto a scalar wave and deliver the product at a distance" (Bartlett 2009:123). And *"**This technology of consciousness resides right within the torsion field of our heart linked to our bioplasmic energy fields**"* (Bartlett 2009:151). What are we to make of this?

To make sense of this torsion field stuff, let me just make a few preliminary comments. Maxwell said in fact: ". . . I cannot go any further . . . in searching for the cause of gravitation" (Maxwell 1865:493), so that, whatever other valuable insights might have been lost from his original work, it does not appear as though a unified field theory was one of them. These "torsion fields" appear unrelated to the usual definitions of "torsion" in differential geometry (e.g., Millman & Parker 1977) or to Albert Einstein's one-time notion that electromagnetic phenomena could be modelled by such torsion in a general theory of relativity (Sauer 2004). And what any of this might have to do with consciousness is highly speculative, given that there is no scientific agreement on what consciousness even is (Barušs 2008b).

Nonetheless, quaternions, invented by William Hamilton (1843/1944/1945), can be conceptualized as numbers in four-dimensional space (Hathaway 1897), and it is always fun to think of ways in which phenomena could be explained by the introduction of additional spatial dimensions. What and how anything actually gets explained is in the details, and before long one can become mired in minutiae that no longer have anything to do with the phenomena one sought to explain in the first place, as evidenced by string theory (Smolin 2006). In this case we have the additional notion of torsion, but at that point things already start to get confusing. Bartlett wants torsion to arise from spinning macroscopic objects as well as the quantum spin of virtual particles. But it is not at all clear

how those could have anything to do with one another. I mean, in what sense do virtual particles rotate? And to the extent that elementary particles of any sort would be involved, they cannot be bodies actually rotating about axes given that they have no substructures that would allow for such rotation. In fact, elementary particles do not have any continuous temporal existence, nor, by some accounts, any spatial extension. And even if we can resolve those sorts of problems, there are plenty more before we can get to the contentions made by Bartlett, such as,

> Torsion fields create a hyperdimensional geometry, which has access to extradimensional realities. When you master the unified field of the heart, you can actually fold space–time locally. (Bartlett 2009:144)

Bartlett's discussion of these matters comes across as muddled. And once quaternions, scalar waves, torsion, bioplasmic fields, the vacuum, and so on get shuffled around sufficiently, then it seems to me that we can "explain" pretty much anything. Having said that, I feel that Bartlett moves through this material intuitively, creating stepping stones for himself that allow him to rationalize what it is that he sees happening even if these stepping stones have only a tenuous relationship to notions in conventional science.

> If it is more useful to you [to recreate your reality with your imagination and insight], then it doesn't matter what science says, since the scientists seem to be making it up anyway. (Bartlett 2007:62)

Bartlett realizes that all explanations are ultimately fictional. And he explicitly gives himself the freedom to entertain his own fictions, as in the following example: "In Matrix Energetics, we teach a mythology that states that we are made up of photons" (Bartlett 2009:77). However, and this is my point, further down the same page he intimates that the consensus view is that all elementary particles are made up of photons, which is certainly not the case. Similarly, I think he wants at least some of the hyperdimensional torsion field stuff to refer to actual events within physical manifestation. And it is clear that Bartlett wants scientists to take his work seriously:

> From a purely scientific perspective, if you are a physicist, you will see this material quite differently. Please don't let that stop you from considering some of these concepts. (Bartlett 2009:11)

But then it would be helpful, for the sake of those with a background in science, to represent the scientific fictions accurately and to be sensitive to what does and what does not have empirical support.

Thus far in this review, I have emphasized the substance of the subject

matter of these books and deemphasized Bartlett's various experiences. But we can flip that over so that prominence is given to the experiences and read these books as "The Magical Adventures of Richard the Weird Healer" in the course of which he has developed techniques for radical transformation along with a rationale for understanding them. My favorite feats are the "'Superman Event'" (Bartlett 2009:3) whereby a hallucination of George Reeves dressed up as Superman showed up and helped Bartlett to successfully treat a girl with lazy eye; the near-death experience in which Bartlett crashed his car after hitting black ice near Butte, Montana; and the use of "Rees's Harmonic crystal technology 'thingies'" (Bartlett 2009:195) for healing his patients. Bartlett writes the way he talks, with much punning and humor. He is aware of the fact that laughter is disarming and helps to create a space within which radical transformation can occur, so that he "learned how to make instant changes in both physical and emotional conditions in a fun and playful way" (Barlett 2007:156). Bartlett reminds me of a Sufi *rind*, whose knowledge of deep aspects of reality is concealed by buffoonery that protects what he has to teach from those who are preoccupied with the surface of life. Indeed, Bartlett appears to delight in playing up the outrageous nature of some of his exploits. For example: "Resolved to wing it, I reached into Dr. Rees's bag of tricks and settled upon a brightly colored plastic doodad" (Bartlett 2009:196–197). So, these books are fun to read. But beyond the beguiling narrative with its smattering of scientific lacunae, they offer promising insights into the nature of reality that I think it would benefit all of us to consider carefully.

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Among the Truthers: A Journey Through America's Growing Conspiracist Underground by Jonathan Kay. HarperCollins, 2011. xxiii + 340 pp. $13, hardcover. ISBN 9780062004819.

Any book that seeks to assess the rights and wrongs of many controversial topics is likely to lack credibility on some of them. When those topics include matters of science, the author had better have a good understanding of how science works. Jonathan Kay unfortunately does not, and his treatment of several subjects is unwarrantedly brief and misleading—perhaps because he regards Wikipedia entries as reliable, comparable to Snopes (p. 241), "particularly those relating to controversial subjects" so long as Wikipedia's "corps of dedicated editors" don't relax their control (p. 247); but it is precisely on controversial matters that Wikipedia and its editors are quite unreliable. This book is a frustrating mixture of interesting material and wrong-headedness. Incredibly, quotation marks imply direct quotes in the absence of any source citations or bibliography, for instance from Shermer on pp. 26–27; or "one Protestant propagandist" on p. 35; or "one scholar's analysis" (p. 69) showing that 40% of the *Protocols of Zion* was "lifted word for word" from Maurice Joly's *Dialogues in Hell between Machiavelli and Montesquieu*. I would also have liked a citation for Kay's assertion that "pluck" is synonymous with "chutzbah" (p. 189).

The "Truthers" are those who adduce a variety of circumstantial evidence to deny that it was an Al-Quaida operation that felled the Twin Towers; they include the group, Scholars for 9/11 Truth and Justice. Kay wanted to discover why "these people" had crossed the line from healthy concentration on a subject to conspiracist political philosophy or worldview (pp. 7–8). Throughout, it seems to me, Kay describes as conspiracism what CSICOPers call pseudo-science and promiscuous defenders of mainstream beliefs call denialism. Those who entertain the possibility of beliefs that Kay deems unfounded are thereby labeled not just wrong but fatally wrongheaded to a degree that verges on mental unbalance.

I found flawed logic and factual mistakes galore. For example, Kay refers to "Steven Jones, a famous Brigham Young University physicist renowned for his work with cold fusion" (p. xxii)—but Jones is neither famous nor renowned for that work, indeed his contribution to cold-fusion studies was weak and little.

Kay cites the well-known phenomenon of extremists moving from one side to the opposite, but insists that this reveals a "fundamentally conspiracist vision of society" (p. 31).

America has been more hospitable than Europe to "intellectual outsiders—oddballs, dissidents, heretics, fussy autodidacts, and skeptics—the sort of men whom we would now call 'cranks'" (p. 32); "the American Enlightenment set

loose a million eccentrics to sweep away the dogmas inherited from Europe" (p. 190). I don't believe the evidence for that is obvious. Kay's exemplar is Ignatius Donnelly, and there have certainly not been anything like a million Americans of Donnelly's ilk. At any rate, this hospitable-ness supposedly allowed emergence of

> America's unique brand of conspiracism . . . the imagining of a complex organizational chart linking all of America's power centers, from media companies to drug makers to the CIA, to one central, all-controlling secular Antichrist. (p. 33)

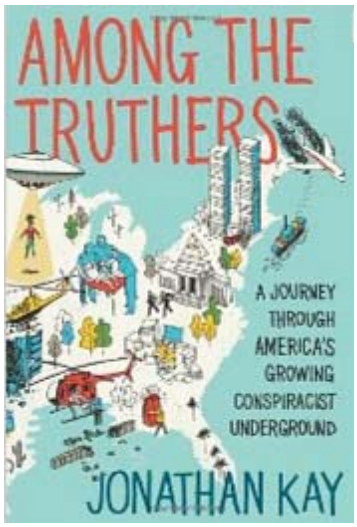
At the same time, "anti-Semitism, a European pathology that formerly had been comparatively mild in America" became embedded in America in the populist movements of the late 19th century and remained "a fixture of American political life until the 1940s" (p. 37); one Europe-inherited dogma that apparently was *not* swept away.

An encounter with one Truther gave Kay "obvious" reasons why there are no crank women, only men. But Martin Gardner (*Fad and Fallacies in the Name of Science*) named a couple of quite prominent female cranks — Annie Besant, Helena Blavatsky—and some of the most prominent mediums have been women. Cranks are typically "math teacher, computer scientist, chess player, or investigative journalist" (p. 191)—after the book has just described a number of cranks who were none of those things.

The lack of evidence- or logic-based argument in this book is illustrated when it cites 16 questions Bertrand Russell asked about the Kennedy assassination (pp. 44–45) without bothering to explain what the proper answers are or why the questions might be ill-founded. The assertion that Kennedy was assassinated for failing to implement Operation Northwoods is cited scoffingly and without letting readers know what that Operation was; and since there is no index entry for it, I was left wondering for another 60 pages, until p. 106.

The fairly serious charge of plagiarism is made against Aldous Huxley, George Orwell, and Ayn Rand for appropriating the ideas of Yevgeny Zamyatin's 1922 novel *We*. Another blithely thrown-out assertion is that in the 1960s aliens were reported as looking like fetuses because pregnant women were now seeing such images via ultrasound (p. 56). "One of the most famous AIDS conspiracy tracts" (p. 80) is one that I had never heard of despite reading about HIV/AIDS fairly intensively for years. "Many UFO conspiracists" believe AIDS is part of an alien agenda to clear humankind away (p. 81) and "Many UFO buffs" believe there are subterranean farms where aliens milk humans of vital fluids. I've known quite a number of ufologists and have read a reasonable amount in UFO books and periodicals without encountering those beliefs.

Conspiracists include not only UFO buffs and 9/11 Truthers but also Tea Partiers (Chapter 4) and many others. Kay overgeneralizes ad absurdum. "In



every society preceding the American Revolution . . . a man's life largely was governed by factors beyond his control," but now "life's losers have no one to blame but themselves" and so it's a relief to blame instead some dark conspiracy (p. 140). Although they are apparently all "losers"—and "Only a small minority . . . seemed out-and-out insane" (p. 181) including Ron Hubbard (p. 183)—conspiracists exist in eight subtypes, set out in Chapter 5, "A Psychological Field Guide": midlife crisis; failed historian; damaged survivor; cosmic voyager; clinical conspiracist; crank; evangelical doomsayer; firebrand.

Yet in the midst of questionable stuff and worse, Kay offers some useful insights, for instance of the degree to which the media shape public opinion, particularly in the postwar years of only three television networks with barely differing approaches (p. 94). That cranks are often people who have been frustrated in a career (p. 192) does have some evidentiary basis. I can agree with Kay also that political correctness seems able to foster conspiracy theorizing (p. 278); and, what might seem obvious, that "Not all conspiracy theorists are anti-Semitic" (p. 289). But then immediately comes the assertion that "all conspiracy movements—all of them—attract anti-Semites."

Kay's summarizing final chapter decries "AIDS denialism," and cites approvingly the good work of groups like the James Randi Educational Foundation and Michael Shermer's Skeptics Society. Like those and their ilk, Kay calls for "an anticonspiracist curriculum," evidently not understanding that education means helping individuals learn to think for themselves, whereas any "anti" or "pro" curriculum describes an intention to indoctrinate.

This book joins a large genre of "skeptical" works written by *pseudo*-skeptics, those who are skeptical only about the ideas that others have, never about their own.

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The Moon and Madness by Niall McCrae. Charlottesville, VA: Imprint Academic Philosophy Documentation Center, 2011. 177 pp. $34.90 (paperback). ISBN 9781845402143.

I

In a paper titled "The Effect of Cosmic Influences on Physiological Phenomena" by the Danish Nobel Laureate Svante Arrhenius (1898), lunar rhythms in records of births, deaths, menstruation, and epileptic seizures were documented. Since then, more than two hundred papers have appeared in peer-reviewed scientific journals purporting to investigate lunar influence on human life. I undertook a comprehensive review of this literature in 1970, prior to the publication of my initial research report (Lieber & Sherin 1972), and I have maintained an active interest in subsequent lunar studies up to the present time. The quality of the offerings is uneven, to say the least. There are informal surveys of data covering one or two years which should not have been accepted for publication since their results are meaningless. There are opinion pieces based on cursory literature reviews that are gratuitous. Several papers that present statistical treatments of the research of others, including some large scale meta-analyses, often turn out to be one-sided polemics. So-called replication studies are often misnomers because they utilize variables and/or statistical tests that differ from those of the study they presume to replicate. There are very few prospective studies of humans to be found. Prospective research is expensive and cumbersome. The majority of published research studies are retrospective and correlative.

A distinctive publication pattern is readily discernible upon perusal of this literature. Each study showing a positive correlation between one or more behavior variables and one of the several lunar cycles (synodic, anomalistic, sidereal, ecliptic, daily lunar transit) is soon followed by a "replication" study, which invariably fails to confirm the original positive results. Apparently there are a number of *impartial* researchers who seem consciously or unconsciously committed to undoing any evidence of lunar influence. Why this should be so makes for interesting philosophical conjecture. Lunar studies are time- and space-sensitive. They require carefully selected quantifiable behavior variables and reliable statistical measurement. Slight alteration in any of these parameters can obscure the findings of a study designed to detect small effects. So it is quite easy to "disprove" a positive result.

All this has led to the preponderance of conflicting findings duly noted by the author, Niall McCrae, who usually manages to remain impartial in his reporting of the opposing dynamic forces in this challenging area of research. Though McCrae does not list any academic credentials, he implies a background in nursing administration. It was here that he first encountered the traditional

body of folklore, beliefs, and observational data that aroused his interest in learning about a possible lunar influence on hospitalized mental patients. His later work as a researcher in an academic department of psychiatry helped him to understand the modus operandi of psychiatrists as researchers. Along the way he became an insightful writer who is able to penetrate to the core of his subject, separate the wheat from the chaff, and distill the essence of what is important. His writing style is colorful and cohesive, with touches of irony and humor.

I will attempt to summarize the manuscript section by section in hopes of inducing the reader to examine in depth his richly detailed and well-referenced excursions ranging from the ancient past to the development of astronomy, cosmology, medieval asylums, psychiatry, and contemporary research. I must first acknowledge my own theoretical bias in favor of the gravitational hypothesis. The title of my second book (Lieber 1996, *How the Moon Affects You*) implies as much. Many researchers become influenced by what they perceive as the weight of the evidence—both their own and that of others. It is unlikely to affect my objectivity in assessing this author's contribution.

II

McCrae's Introduction reads like the executive summary of a business plan. He provides a brief and succinct overview of the material upon which he elaborates in the nine chapters of his text. This is a useful device for those who are too busy or may otherwise be disinclined to peruse the entire text. Additionally, the section summaries which follow may provide the stimulus to delve into certain chapters for greater detail to complement the introductory overview.

The first three chapters set the stage for the author's *fairly* comprehensive review of the world literature on lunar studies over the past century. My own review uncovered more than a dozen contemporary references that were not included in his review, but this does not necessarily detract from the essence of his findings and conclusions, which I believe accurately reflect the current state of affairs in this field. Here he traces the origins of our curiosity about possible influences of the sun and moon acting in concert on the social, cultural, and biological aspects of our civilization. His odyssey courses from ancient times to the middle ages and on through the Age of Enlightenment, culminating at the end of the eighteenth century. Eminent early physicians such as Aesclepius, Hippocrates, Galen, Paracelsus, and Richard Mead subscribed to the belief that extraterrestrial (including lunar) forces played a prominent role in a number of human maladies, especially madness and epilepsy, which during earlier times were not viewed as separate conditions.

Chapter Four details the development of psychiatry as a specialty of medicine. Defining lunacy for the purposes of English jurisprudence in the

mid-eighteenth century, Sir William Blackstone cited the general belief that changes of the moon influenced periodic episodes of psychosis in otherwise lucid individuals. At that time, large public sanctuaries such as Bethlehem Hospital (Bedlam) in London and Gheel in Belgium housed the insane in less-than-therapeutic conditions. During the early part of the nineteenth century, the public asylum movement spread across Europe and the U.S. The medical administrators of these institutions met periodically to share their observations and treatments, thus forming the nucleus of organized psychiatry. Germany became the leader in academic training and systematized diagnostic classification. Toward the end of the century, two movements took root, each traveling in a different direction. Psychoanalysis grew and influenced the intellectual discourse of the twentieth century, while experimental psychology laid the foundations for quantitative psychometric measurement of mental functioning. The last section of the chapter consists of a concise summarization of these important movements in the field of mental health.

In Chapter Five, the author attempts to referee between what appears to be opposing forces—those researchers whose results favor the lunar hypothesis and those whose results favor the null hypothesis. Following adoption of the scientific method by psychiatric researchers in the 1950s and 1960s, a flurry of papers began to appear in scientific journals, reflecting a renewed interest in testing the lunar hypothesis. The decade of the 1970s saw a lively controversy ignited in the literature which persists to the present day. McCrae tabulates the studies, both pro and con, examines the roots of controversy, deplores strident critiquing by some authors of the work of others and strives to avoid taking sides. He correctly infers that flaws in methodology account for many of the discrepant findings.

In Chapter Six the author strives to comprehend what influences our attitudes with regard to possible lunar effects on human behavior. Citing the results from several large-scale surveys published during the 1980s, he notes that belief in a lunar influence is commonly reported by a significant percentage of the general public, university students, nurses, doctors, police personnel, crisis workers, and emergency technicians. Among workers in mental health facilities, this belief is pervasive. He examines the possible role of folklore, fairytales, astrology, religion, cultural beliefs, self-fulfilling prophecy, as well as psychological constructs such as locus-of-control and attribution theory. Rather than speculate on which among them most impacts our thinking, he suggests the following: The era of the Internet provides people with a broad spectrum of ideas and readily accessible knowledge with which to form their own ideas and judgments as to the credibility of a concept.

Chapter Seven summarizes the research on biological rhythms, the biological clock, and the luminosity hypothesis. The latter infers a possible

full-moon effect on the behavior of the deranged mediated by bright moonlight. Some studies show a full-moon predilection for disturbed behavior, and others show increased seizure activity among epileptics. A case is made for the full moon resulting in disruption of body rhythms leading to sleep disturbance, which is known to induce manic episodes in patients with bipolar disorder (the modern-day equivalent of the medieval lunatic). A majority of the patients in the old asylums were likely to be bipolars and/or epileptics. Might this account for the long-held belief that mental disturbances blossomed at the full of the moon? It is noted that the luminosity hypothesis does not account for the reported findings of increased behavioral disturbance at new moon or other points in the synodic cycle.

Chapter Eight presents a noteworthy synthesis of the evidence for an *indirect* influence of the moon upon terrestrial geophysics, and subsequently upon biophysics and human behavior. The medium by which these phenomena transpire are the pervasive electromagnetic fields that bathe all terrestrial organisms and participate in all periodic processes and perturbations occurring in our solar system. A considerable body of research began to appear in the mid-twentieth century that explored how the human organism may be impacted by naturally occurring electromagnetic fluctuations in the ambient terrestrial environment. These forces, along with ion showers and cosmic radiation, emanate from our sun and are modulated on their earthward journey by lunar gravity. They may cause weather disturbances, disruptions in the transmission of radio waves, and disturbances in human physiology and biochemistry. Careful investigations of each of these intermediary variables reflect the imprint of solar and lunar cycles. The fact that much of this research, including some important applications in contemporary medicine, has been overlooked by the orthodox scientific establishment reflects a general failure to transition from linear thinking to systems thinking in the health sciences. McCrae has summarized the essential discoveries made by most of the relevant researchers in this area. He has, however, omitted some important material produced by Russian workers such as Dubrov (1972) and Presman (1970). These and other Eastern European scientists were early proponents of the importance of terrestrial geomagnetism in the regulation of life processes and the unraveling of enduring mysteries such as extrasensory perception and the biological compass. The author, who strives throughout to maintain a neutral stance, summarizes thus:

> with evidence of a small but statistically significant correlation between geomagnetism and mental disturbance, and monthly perturbations in magnetic activity caused by the moon, an indirect lunar influence on behavior via geophysical forces is in principle a reasonable hypothesis.

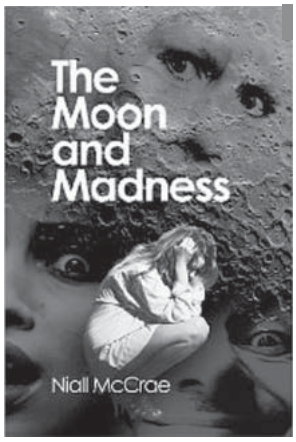
In the final chapter the author attempts to resolve his ambivalence with

regard to the existence and relevance of lunar effects on mankind. He does this by exploring the tenets of various philosophers of science during the past half-century in a futile effort to elicit clues that might inform his judgment. He would like to conclude with his statement that the bulk of research on lunar variables fails to refute the null hypothesis. However, his basic sense of fair play and a nagging feeling that there may be more to learn does not allow him to trivialize the modicum of positive studies. He feels compelled to allow for the plausibility of an indirect gravitational hypothesis. He compares the conflicting results of lunar studies with those of contemporary studies of antidepressant drugs. The latter are, in fact, no less conflicting and both leave their readers in a quandary as to how they should be interpreted. McCrae also bristles at the thinly veiled diatribes of skeptical authors Campbell and Beets, and Rotton, Kelly, and Culver, and he chides them for their strident critiquing of others' work. His conclusions from his perusal of much of the relevant world literature of the past fifty years are as follows:

1. the research findings are conflicting, hence they are confusing,
2. most of the studies consist of retrospective data correlation,
3. methodology is at a rather primitive stage of development,
4. replication studies are rarely found,
5. statistical reviews often become polemics,
6. lunar studies continue to appear despite caveats from some authors,
7. the argument for further study remains compelling.

III

My interest in lunar study began with a body of hearsay similar to that encountered by McCrae. It was supplemented by my personal observations on the wards of an acute-care psychiatric hospital and in the psychiatric emergency room of a large municipal hospital. As a psychiatric resident wishing to initiate a research project, I had to satisfy the criteria of a demanding department chairman, who was also an established researcher. I teamed with an experimental psychologist and a prominent academic statistician in developing a foolproof methodology. We then measured lunar timing of several behavior variables over long time periods and in large numbers of subjects from two widely separated locations. Over the next eight years I published three research studies and a book, and my findings were widely disseminated to the general public by the media. During the same period, a rash of conflicting results were reported by other workers. In an attempt to head off the inevitable confusion, I called for standardization of methodology for all future lunar studies (1978a) This effort was ignored, however, and to this day there remains a glaring need to establish clarity rather than the uncertainty that still prevails.



One might inquire why lunar studies are relevant at all. They are largely ignored by the scientific establishment and have been denounced by a small group of fanatical doubters among the research community. I believe there are three factors that address their relevance—the heuristic, the practical, and the philosophical. I will comment briefly on each.

The heuristic value of lunar studies is self-evident. The thrill of unmasking elemental forces that impact upon the growth, development, and functioning of the human organism is matchless in the overall scheme of discovery. Confirmation of ancient observational wisdom has also proved gratifying. The value of legend and folklore in shaping the direction of contemporary investigations should not be overlooked. For example, curiosity about the centuries-old practice of farmers applying moldy cheese to infected wounds led Fleming to the discovery of penicillin.

Applied lunar knowledge has never been systematized, and interest in practical applications has been sporadic. There is no documented evidence that gravitational forces consistently affect any given individual in a predictable manner. Nonetheless, there are surgeons who avoid new and full moon scheduling of elective surgery, there are event promoters who will not book a rock concert at these times, there are police departments that alter staffing patterns and alert their officers to be more cautious during these "high-risk" periods, and there are Wall Street firms that base their investment patterns on the coincidence of the synodic and anomalistic cycles. Almost every psychiatrist has seen patients who claim to be a "moon person." The wiser among them know better than to ridicule the concept. I used to encounter clusters of treated bipolar patients who simultaneously called in to report symptom recurrences during times of gravitational stress. Usually simple reassurance that this should pass in a few days would suffice, but I had occasion to adjust medication for some of these patients. Similar experiences were not uncommon among my colleagues, however I doubt that they attributed these recurring case clusters to geophysical stressors.

There are a number of fascinating philosophical issues concerning our subject, discussions of which could easily fill a separate manuscript or produce another volume. I will limit my present focus to three topics which I deem as timely. One of them is fortuitous and has the potential to alter the direction of science.

- The concept that all terrestrial organisms, including man, are cosmic resonators, reflecting the harmony (or discord) of the spheres dates back to W. F. Peterson (1947). Peterson noted that this idea was, in fact, quite old, having been popular among ancient philosophers. He studied three medical students who were identical triplets over a three-year period and was able to detect solar and lunar rhythms in multiple physiological and biochemical parameters, which he also correlated with weather variables. He used the term *cosmobiology* to describe his approach. There are many studies of plants, animals, sea life, humans, weather variables, and terrestrial geomagnetism that seem to embody this concept. Also there is an aesthetic fit between the idea of terrestrial organisms resonating with the cosmos and my theory of Biological Tides.

- It is interesting to speculate on the root causes of the antagonism toward lunar studies found among certain writers. I suspect there are two main fears that underlie their misgivings. Some workers are reluctant to confront the percept that there may be limitations to their free will or to mastery of their own fate. If one were to acknowledge the possibility that forces beyond our control might influence our biology and behavior, then apparently we do not possess as much freedom as we had imagined. This idea is, of course, anathema to those who need total control over their environment.

  If one accepts the possibility of a gravitational influence on human biology, then he must also recognize that a variable relevant to the design of many research experiments has been overlooked. This could alter, or even invalidate, years of research results. One begins to comprehend why some may harbor the need to disprove positive findings.

  Only those are free who are able to acknowledge the limitations of their freedom, and of their methods.

- Thirty-four years ago I alerted readers in my book (Lieber 1978b) that lunar time should be incorporated into the design of biological and behavioral experiments. Results from my research combined with a working familiarity with the literature on biological rhythms led me to an awareness that lunar rhythms coexist with solar rhythms in biological functioning. Therefore, lunar time must be an integral component of biological time. The late Professor Frank A. Brown from Northwestern University agreed with this contention. Lunar time is out of synch with solar or calendar time, advancing by 50 minutes daily across the solar

time spectrum (hence, the lunar month is about a day and a half shorter than the solar month). The difference between solar and lunar time parameters constitutes a variable that must be controlled for in the design and conduct of biological and behavioral studies. Failure to account for this difference when designing a research methodology dooms the ensuing study to inevitable replication failure. Needless to say, this admonition, which was reiterated in the revised edition of the book (Lieber 1996) was ignored, and incorporation of lunar time into research designs did not occur. On December 2, 2011, an article appeared on the front page of *The Wall Street Journal* titled *Scientists' Elusive Goal: Reproducing Study Results*. One of medicine's dirty secrets was revealed: Most results, including those appearing in tier-1 peer-reviewed journals, cannot be reproduced! The extent of this problem throughout the literature of science and medicine is vast. That same week, the journal *Science* ran a series of articles attempting to explore possible contributing factors such as flawed methodology, increased competition among authors and journals, publication bias, data fudging, the prioritizing of positive over negative studies, etc. In the end, the investigators and the editors really don't know why most studies cannot be replicated. Their search for an answer came up short. Science has finally awakened to the reality that research results simply are not replicable at our present level of understanding. This confirms my 1978 prediction. The time may be ripe for a new level of understanding based on an old principle. Perhaps scientists will now revisit this long overlooked viewpoint, as it may embody part of the missing answer.

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The Moon and Madness by Niall McCrae. Charlottesville, VA: Imprint Academic Philosophy Documentation Center, 2011. 177 pp. $34.90 (paperback). ISBN 9781845402143.

Mathi—Tamil word with two meanings, *knowledge* and *moon*
(Tamil is one of the ancient languages of the world, spoken in South India)

The word *Mathi*—how it got two meanings is unknown to me. Interestingly, many traditional and cultural practices associated with the moon are still in practice in South India. These two things came to mind when I first sighted this book. The Moon and its charisma allows no one to remain silent: Poets describe it, artists paint it, numerous folklore tales and legends continue.

The author has provided an overall review of lunar studies that have been done through the years, and brings to the lay reader a critical analysis of the chaos, obfuscations, and intricacies relevant to the influence of the Moon. It is a lively presentation of the various beliefs and ideas over the entire history of philosophy, science, and medicine from ancient civilizations to the modern age. It takes us on a journey to learn about the knot that ties our lives with the cosmos.

There are nine chapters, a Foreword by Dr. Paul Crawford (University of Nottingham), and an Introduction. In the Preface, Niall McCrae gives us an account of his acquaintance with the influences of the Moon. In the Introduction, he describes the current position of scientific knowledge regarding the acceptance of traditional knowledge about the Moon. The first half of the book covers knowledge of the Moon in primitive civilizations, and how that later transformed after the Renaissance and the Reformation. The second half of the book is devoted to an in-depth analysis of lunar studies and to alternative thinking in science about the Moon. There is a strong non-acceptance of traditional knowledge in Western science. Most scientific journals and scientists strongly reject the lunar influence without having a questioning mind about it. Before starting an experiment one should have an open mind, which is the spirit behind this book.

The Moon was worshipped as a deity in early civilizations, and many fantasies were associated with Moon. The Moon was strongly correlated with astrology, body functions, and mental health. When astrology was studied by European physicians, they drew astrological charts but never bothered with the treatment aspects, even when patients were in critical condition. With the help of the telescope, the Moon was described as having rough terrain, valleys, and mountains, and as a result a paradigm shift occurred. With the advancement of reason, philosophy, medicine, and science, the previous fantasies associated with the Moon were abolished.

Initially, alienists (physicians who pronounced on the mentally ill) were introduced to treat lunatics in asylums. They were the first people who observed the

mentally ill and devised methods to assess human behavior. The author has serious questions about how the present practice of psychiatry is able to determine human behavior based on assessment. The author critically argues for the incorporation of human experience and behavior which is widely ignored in the current practice of psychiatry. All remember Spencer's terminology "survival of the fittest" but forget Wallace's theory in which survival was determined by the suitability of the environment. The incorporation of the social sciences and a positivistic approach would enable improvements in the treatment of psychiatric patients. McCrae's viewpoints in this area are geared toward the benefit of mankind not just science.

McCrae's view is that a psychiatric disorder should be treated as a social disorder and not as a mere biochemical aberration. He also explains the controversies and politics related to drug trials and the need to regulate them. Psychiatry should not be victimized for the optimum operation of Pharma companies. Psychiatrists should consider the knowledge of nurses since they spend more time with patients and know their behavior well. Current psychiatry neglects the knowledge and experience of nurses and individual patients. It provides for more priority to be given to quantitative research than to qualitative research. How psychiatry treats fellow human beings and how that deviates from a humanistic approach is a deep concern of this book.

Jürgen Aschoff (co-founder of chronobiology) opposed the development of specialized symposia and journals. Science should not be compartmentalized and specialized since most of the specialists never interact with each other. McCrae believes difficulties in science should be shared so that they can be rectified and lead to proper scientific research methods. This book urges the need to develop a unified science.

Most correlations in lunar studies have not been replicated. After a huge number of studies, there is no clear satisfactory answer proving or disproving lunar influence on humans. The book substantiates statistical errors and technical errors correlated with the influence of the Moon. It seriously asks for a new way of approaching studies of the Moon and its influence on human beings. The final chapter provides a broad view on the problems, defects, flaws, and requirements in lunar studies. The author wants to see a methodology in future studies that takes account of all previous studies' flaws and defects.

In concluding remarks, the author asks that more trustworthy lunar inquiries be done with an open-minded approach crossing professional and department lines. The evidence so far begs for a more consistent and developed methodology. This book is a shot in the arm to those who are "tired of lunar research" and encourages young investigators to pursue lunar studies. Most

of the beliefs from two thousand years ago and before have declined and diminished as science has evolved. But the possibility of lunar influence still persists in the minds of people as well as in the scientific community. It still fascinates lots of researchers—What is the reason behind this? McCrae never provides a conclusion about lunar influence, leaving the question to the reader, but he believes that further investigation on lunar research is warranted.

Earth-generated circadian rhythms is in our genes. Is there any impact of the rotation and revolution of the Moon also in our cells and genes? What could be the adaptation of human physiology and behavior to the Moon? Does it have a common effect on all of us, or does it influence only a small group of the population? These were the questions I asked before constructing a hypothesis on lunar influence. Initially, I started my research on the clock gene polymorphisms associated with schizophrenia and bipolar affective disorder. During visits to the mentally ill, nurses and administrative staff told me of the association of lunar influence with mentally ill patients. Initially, I didn't believe it as I had never heard of such a thing. Later on after frequent visits, I witnessed the behavioral changes in emotionally unstable people associated with lunar phases.

The human brain is the most complex organ produced in the process of evolution. Since emotions vary among people, could anyone establish a list of standard protocols to qualify or quantify human emotions? Probably not.

Two possible factors, gravity and light, have been mentioned as the effecting agents of the Moon. Normal light beams do not affect the sleep of normal persons but do affect the sleep of those with sleep disorders. There are reports that light conditions could affect all primates' sleep behavior. So perhaps emotions can be elevated in certain people at the Full Moon and the New Moon. Lunar influence depends not only on the individual but also on time (my speculation). Obviously, more studies should be done.

Difficulties associated with lunar studies include:

1. Lunar studies will not fetch any project grants.
2. People should be monitored continuously 24-7.
3. There is a need to use a lunar day schedule.
4. Long-term studies are needed, since the same lunar position will not recur and so results are not easily reproducible.
5. There is a need to check for influences at the New Moon, Full Moon, and the last quarter. Looking at only one time of the month would cloud the results.

Perhaps if we can puzzle out the cause and effects of lunar influence, we would be better able to treat psychiatric patients as well as other patients.

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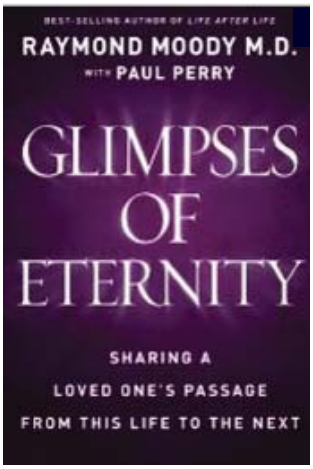
Glimpses of Eternity: Sharing a Loved One's Passage from This Life to the Next by Raymond Moody with Paul Perry. New York: Guideposts, 2010. 183 pp. $19.95 (hardcover). ISBN 978824948139.

Glimpses of Eternity is a highly interesting book on a fascinating topic. It contains dozens of accounts of what the authors call shared-death experiences. These accounts were related to Moody over the years, often at conferences and book signings, or told by colleagues, friends, or hospital staff. The book is not describing the results of a systematic scientific research project, but it brings to light many interesting rare cases of the kind that little attention has been paid to, in part perhaps because of how exceptional they are. One wonders whether many of these cases are a residue of deathbed-vision cases and near-death experiences that the authors were until now hesitant to publish because they appeared so incredible. Or, could the features of some of these experiences have been somewhat exaggeratedly reported to the authors? Or, were these accounts jotted down after lectures and book-signings without getting back to the persons who reported them to check their correctness? These are methodological limitations and questions to which some readers will want answers. There is much room for further studies of deathbed-visions and end-of-life experiences, among them to corroborate some of the more exceptional experiences found in this book.

In shared-death experiences, as the authors define them, the observer of a dying person suddenly finds himself seeing the deceased persons that are generally visible only to the dying, namely, he shares the deathbed visions of the dying person. Or the observer, usually a relative, finds himself outside his body and observes how the dying person leaves the body or is hovering over the dead body, both of them looking down at the dead body. Or, the dying person suddenly appears to a relative elsewhere in the same house who then rushes to the deathbed to learn that the person has just died. There are experiences at the deathbed where the relative sees a light, presumably as the dying person also sees it. Sometimes more than one relative perceives the light.

The last kind of case reminds me of the experience that my colleague Karlis Osis told me about that happened to him at his home in Latvia in his youth. An aunt was very sick in bed as he was resting in a nearby room. His room then became filled with "living light." Then the door opened and a relative announced, "Auntie just died." "My experience and her death were surely simultaneous" (Osis 1987). This was the underlying reason for his interest and later research into deathbed visions.

Other instances involve hearing music, sometimes described as "the most beautiful and intricate music I have ever heard." There are also cases of shared life-reviews, distortions of the surrounding space ("change of geometry"), and



"encountering unworldly or 'heavenly' realms." In one place the authors describe shared-death experiences as a "picture window into the afterlife."

All these accounts are anecdotal, the reviewer does not recall mention of attempts to validate a case by interviewing a second witness. One misses here the thoroughness that we find in the classic *Phantasms of the Living* by Gurney, Myers, and Podmore (1886). While the actual investigation of cases of this kind has been neglected, they have been discussed here and there over the years, such as recently by Alvarado (2006) and Nahm (2011).

Moody and Perry do not bother readers with many numbers or statistics. Nor are there references, footnotes, or citations, not even for quotations from well-known sources. This is a popular mass-market book written for the general reader, and indeed very well written. The text is highly edited and easy to read, the book is light in the hand and has large letters comfortable for the eyes. Ideal for bedtime reading, and very thought-provoking.

One wonders why Moody does not write up some of his cases for peer-reviewed journals. One also wonders what "with Paul Perry" really means. How do Moody and Perry work together? Who writes what? Some readers would like to know this, especially when a book is published in the U.S. where ghostwriting is so common.

For this reviewer the book was a highly interesting read. It brought back memories of cases from Karlis Osis and my survey of deathbed-visions (Osis & Haraldsson 1977), also of cases that have since been reported to me by various people, as well as of similarities to some cases in my study of apparitions of the dead, or ADC as it is sometimes referred to nowadays (Haraldsson 1988–1989, 2009). It also brought to mind parallels I have found in reports of mediumistic phenomena, in particular of Indridi Indridason (Gissurarson & Haraldsson 1989, Haraldsson 2011).

This book is more personal than previous books by Moody. He describes his experience at the deathbed of his mother where the room seemed to change shape and "four of six of us felt as though we were being lifted off the ground." His sister saw that their deceased father had "come back to get her" (p. 49). At one place he writes "death opens a portal to the 'other side'. By now I had faith that such a portal exists" (p. 44).

Moody also writes:

> Rather than try to explain how a phenomenon so great as shared death experience takes place, I most often like to observe the sense of wonder and serenity on the face of a person who is telling what it is like to accompany a loved one into a heavenly realm. It is then, when I see the total joy in their face, that I relax about trying to explain these experiences and accept them for the wonderous experiences that they are. In short, I drop my scientific skepticism, quit asking, 'Are we there yet' and just enjoy the view that is provided for me by the people that offer their case studies. (pp. 178–179)

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Glimpses of Eternity by Raymond Moody with Paul Perry. New York: Guideposts, 2010. 183 pp. $19.99 (paperback). ISBN 9780824948133.

Shared Death Experiences: A New Class of Anomalous Experiences

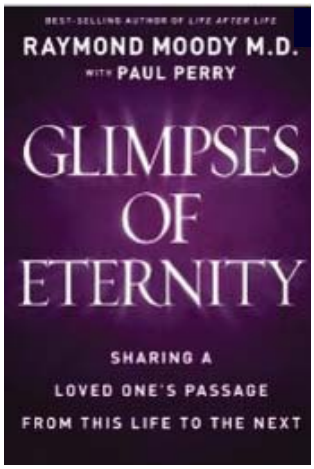
For the originality of his first works, Raymond Moody gained considerable celebrity more than 35 years ago; since then he has tried to reach the same success again by following different routes. After the publication of volumes which did not receive the same public favor—such as those on the healthy effects of laughter and on the therapeutic power of psychomanteum-induced hallucinations—he started a new research path with *Glimpses of Eternity*, focusing on a kind of anomalous experience, the *shared death experience*, which was never systematically treated by the psychological nor parapsychological literature.

A Shared Death Experience (SDE) occurs when someone who is watching at the bedside of a dying person experiences abnormal and unexplainable feelings unrelated to those of the dying person. Sometimes an "energy flow" or a blurred "mist" from the body of the dying person is perceived. Out-of-body experiences are also reported, as well as the mental vision of the dying person or of moments of his/her life, similar to the life reviews reported in Near-Death Experiences (NDEs). Any of the elements may occur separately or together.

According to Moody, a thorough analysis of the reported SDEs allows us to define at least seven identifying elements: *geometrical change* in the space that the dying person and the witness occupy, the vision of a *mystic light*, the perception of harmonious and comforting *music*, *out-of-body experiences*, a shared *life review*, contact with a *supernatural space*, *mist*. However, different classification criteria for the materials presented may be conceived. For example, elements such as the *energy shock* after contact with the person, a *tunnel* ending in light, and the identification of an insuperable *boundary* may be also included. However, Moody's work represents only the first stage of research on this issue, so probably it is too early for establishing fixed modalities for selecting and analyzing relevant experiences.

A very remarkable detail is that only the assisting person, who is in good physical and psychological condition, has had this kind of abnormal experience. In this element—which Moody aptly outlines—lies the difference between SDEs and NDEs, and this prevents the explanation of the phenomenon by brain function decline or the effects of drugs and other psychoactive substances, as has been described sometimes for NDEs.

In his work, Moody reports or cites 50 SDE cases he came in contact with, but only 41 of them are detailed enough to be analyzed. Out of all the reported experiences, 6 (15%) involved more than one person assisting



someone's death, while in the other 35 cases (85%) the experience occurred to one person, who subsequently informed the author. As regards people who experience an SDE, here referred to as "percipients," 60% were women and 40% men. And the number of deaths among men was similar to that among women. This differs from classical parapsychological experiences, where higher rates of women are registered among the percipients and higher rates of men among the dying persons (Schouten 1979, 1981, 1982). Relations between percipients and dying persons are of some interest: In 60% of cases, they are close family members (parents, partners, children, siblings), in the other cases they are connected by other family relationships (i.e. grandparents and grandchildren) or by professional/social connections (e.g., doctor–patient). Again, these data partially differ from those related to other kinds of parapsychological experiences (Williams 2011), but the lack of reference data on how often people die on their own, close to their loved ones or to acquaintances, makes it impossible to give statistical significance to the "familiarity" variable.

Notwithstanding Moody's small sample, data about the *nature of death* occurring in relation to SDEs are more definite. In 9 cases no precise information exists, 26 deaths (81% of the evaluable sample) occurred after a disease, and only 6 cases (19%) were sudden and unexpected deaths. Such data are opposite to every other kind of parapsychological experience, from telepathic impressions and postmortem apparitions to reincarnation-like experiences (Haraldsson 2003, 2009, Rinaldi & Piccinini, n.d., Stevenson 1990). To better understand the phenomenon, it is useful to search for information about the circumstances when an SDE occurs. From the reported accounts, it appears that a high number of people who experienced an SDE had sat up by the dying person's bedside for a long time (even days) and that SDEs mostly occur during the night, probably when the percipient is tired, sleepy, or asleep. Furthermore, it appears that in cases of sudden death the SDE is mostly characterized by definite and recognizable apparitional images, whereas, when the end arrives after a disease, coenesthetic alterations, undefined sensations, visions of the afterworld, and life reviews are mostly reported. Although Moody does not outline these difference, which have to be verified on larger samples, they are very interesting and may orient us toward explanations.

After describing the main features of SDEs, Moody focuses his attention

on a number of historical reports "of the same kind", i.e. crisis and postmortem apparitions. This is one of the weaknesses of his work, since SDEs as he defines them in the first chapters are quite different from what is commonly considered parapsychological apparitions. According to Moody, the anomalous perception that a relative or an acquaintance is dying or has died apart from the percipient represents *anyway* a shared death experience; but such a conceptual extension is not appropriate, for at least three reasons. The main trait separating SDEs from crisis apparitions is the *distance* between the percipient and the dying person, nonexistent in the first case, remarkable and essential in the second one. Also, the involved *death* appears to be different: In SDEs it mostly occurs after a disease, in the other cases it is often sudden and unexpected. Finally, SDEs are *qualitatively* different from classical apparitions, since they include elements such as shared *life review* or perception of transcendent voices and scenes, which are not reported in apparitional experiences. More, it seems important to highlight that, different from what Moody states, the deathbed visions he found, for example in the work of William Barrett (1926), do not have the fundamental feature of being experienced by healthy people near to a dying person, as it happens in SDE cases.

Nevertheless, Moody's proposal to analyze historical documents is an interesting one, since reports of SDEs, overlooked at the time of publication, may be found and could both corroborate current observations and increase the number of cases to be analyzed. From this perspective, the first aim of the research could be the large series of psychical and spiritualist journals of the nineteenth and early twentieth centuries, when readers' accounts were published. It could also be of interest to reanalyze some case collections, such as those included in the works of William Harrison (1879), Gurney, Myers, and Podmore (1886), Joy Snell (1920), Eleanor Sidgwick (1922), Ernesto Bozzano (1943, 1947), and other authors. In a recent review of the anomalous experiences that appear to be induced or favored by death, Michael Nahm (2011) showed how "psychical stories" preserved in historical archives could be fruitfully reexamined by using modern classification and analysis criteria.

Moody considers SDEs as perceptions or contacts with a real afterlife. He believes that experiencers really enter a supernatural dimension and that, as a consequence, their reports should be considered as *true stories*, more reliable and sound than those provided by NDErs, because they involve sane and healthy persons. It is almost needless to say that this is the second weakness of his work, since to follow him the reader has to be yet inclined to believe in the afterlife and in transcendent phenomena. Really, if SDEs were real contacts, they should be more common and, chiefly, they should represent the same supernatural realm, whereas there are as many descriptions as there are people who provide them.

Since in many cases, at the moment of the experience, the percipients were at the bedside of a dying or a dead person, the stories seem to have a psychological dimension. Prolonged sensory deprivation, cognitive and perceptive alterations occurring during the night, poor sleeping, and negative feelings appear to be the natural ground for this kind of experience. At the same time, SDEs have a deep impact on the persons involved, often leading to existential change and to a reassuring conviction of the survival of bodily death.

Due to their *transforming* effect and to the other characteristics yet mentioned, SDEs should constitute an interesting field of inquiry for psychologists and parapsychologists. On the one hand, psychology could explain how and why SDEs originate, the reason behind their different phenomenological aspects, the consequences they exert on the psychological structure of percipients, which are the more common after-effects, and so on. On the other hand, parapsychology should understand, for example, how and why those experiences are *shared* in that way, how they are related to the beliefs of perceivers, whether they are associated with other anomalous experiences, why some people have them, especially when close relationships with the dying are lacking, and why the percipients may experience them only once and not every time they are in similar situations with the dying.

For all of this, appreciation is to be expressed to Moody, who proposed a new research field that, even if it probably will not have the same public favor as NDEs, still is an interesting area for inquiries, both for the general knowledge it could produce and for the answers it could give to the people involved in SDEs.

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Life After Life: The Investigation of a Phenomenon—Survival of Bodily Death by Raymond A. Moody, Jr. New York: HarperOne, 2001. 193 pp. $14.99 (paperback). ISBN 9780062517395. Kindle edition $10.99 (e-book). [Original work published 1975]

Raymond Moody, often referred to as the father of near-death studies, first published his groundbreaking book *Life After Life* in 1975. After receiving his doctoral degree in philosophy, the young Moody decided to pursue his medical degree in psychiatry to study the philosophy and ethics of medicine. During his time in medical school, Moody encountered people whose accounts of extraordinary experiences after clinical death and subsequent resuscitation seemed amazingly similar to each other. The commonality ended with the stories; the storytellers themselves seemed diverse and independent from each other. Moody's initial investigation into what he termed "near-death experiences" (NDEs) culminated in his pioneering, controversial, and best-selling work (Moody 1975) that, as of 2001, had sold more than 13 million copies (Holden, Greyson, & James 2009a:3).

In that book, Moody described investigating 150 cases of NDEs and interviewing 50 of the people who claimed to have had them. He found a pattern of experiences involving non-ordinary consciousness during what seemed to be temporary physical death. He surmised that NDEs are extraordinary experiences that seem to transcend medical and psychological explanations. However, attempting to remain unbiased, Moody presented his findings without judgment and entertained several possible explanations for the experiences. He acknowledged that in the course of interviewing people who claimed to have had an NDE, he found himself believing the intensity of their convictions: that their experiences, although ineffable and beyond scientific explanation, were real.

Summary of Content

In the first half of his book, Moody described 15 common elements of NDEs, of which a near-death experiencer (NDEr) might experience one or more or all. He offered case examples by quoting NDErs whose experiences illustrated the various elements such as moving rapidly through a dark tunnel, being out of body, encountering one or more beings of light, and experiencing a life review. He noted the profound feelings of peace and love that seemed to permeate most experiences. He also catalogued typical NDE aftereffects that included a renewed vigor for life, appreciation for meaningful relationships, revitalized spirituality, and loss of fear of death.

In the second half of *Life After Life*, Moody discussed some historical accounts of, offered answers to frequently asked questions about, and provided possible explanations for NDEs, as well as offering his personal impressions.

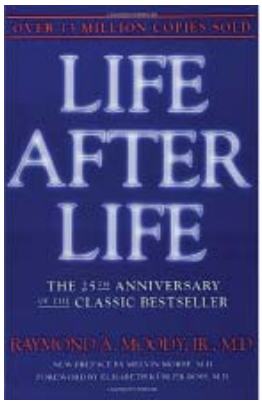
Possible historical accounts of NDEs exist in the Bible, the Tibetan Book of the Dead, and the writings of Plato and Emanuel Swedenborg. Although elements of these accounts are strikingly similar to some elements of NDE accounts, Moody acknowledged that they offer little in the way of proof that NDEs are real experiences. In the last sections, Moody entertained possible explanations for NDEs, drawing from the realms of natural science, psychology, and the supernatural. He argued that neither science nor psychology offers a comprehensive explanation for the phenomenon and again asserted that NDErs believe in the reality of their experiences. Moody concluded with his personal impression that although his work was not proof of life after death, he was convinced that an explanation beyond the natural sciences was possible.

Analysis and Evaluation

Moody's introduction of NDEs to the world survives as a groundbreaking exploration of a previously unexplored phenomenon that opened a new field of near-death studies. Thirty-seven years later, near-death experiences remain a topic of scientific research into the nature of consciousness at death, the effect of NDEs and similar experiences on experiencer's lives, the possibility of life after death, and the implications of NDE-related phenomena for meaning and purpose in life. A recent comprehensive, critical review of NDE-related literature for the 30 years following publication of Moody's book yielded the conclusion that most of the original findings he reported in *Life After Life* have been substantiated by subsequent research from around the world and representing several cultures (Holden, Greyson, & James 2009b). Moody's initial work has garnered substantial attention from both supporters and critics who continue to debate the nature of NDEs and substantial gratitude from NDErs who finally have a term to label their experiences and research to legitimize it. Though critics have found Moody's work fundamentally flawed in design due to his unempirical investigative approach, near-death scholars continue to seek to improve Moody's original research design with investigation characterized by rigorous scientific method (Holden, Greyson, & James 2009b). However, near-death researchers often acknowledge the difficulty in studying a phenomenon that transcends the usual parameters of space and time.

Although most of Moody's conclusions have withstood repeated scrutiny, one notable exception deserves mention. In *Life After Life*, Moody asserted that NDEs resulting from attempted suicide are generally unpleasant and are characterized by some form of punishment in the afterlife. His speculation for this finding was that life is God's gift to humankind; that suicide is an insult to God, the giver of life; and that distressing NDEs are a form of punishment for violating God's gift. Further research has contradicted his assertion on two accounts. First, with regard to pleasurable and/or distressing contents, NDEs

resulting from suicide attempts have not been found to differ consistently from NDEs resulting from other circumstances—and in any case, NDErs almost never report feeling "punished," per se (Zingrone & Alvarado 2009). Second, researchers specifically investigating distressing NDEs have found no difference in their frequency related to near-death circumstances, leading investigators to conclude that anyone can have a distressing NDE (Bush 2009).



Conclusion

In *Life After Life,* Moody offered readers a framework of common elements and aftereffects of NDEs that, by and large, have withstood the test of subsequent scientific investigation (Holden, Greyson, & James 2009b). Following his seminal work, near-death researchers continue to investigate NDEs for their potential to enhance understanding of various death-related phenomena and consciousness in general. His work also has inspired subsequent scholarly attention to issues such as how healthcare providers can most competently serve NDErs in the aftermath of their experiences (Foster, James, & Holden 2009). Anyone interested in the possibility of life after death will find Moody's first book a refreshingly honest account of a beginning scientist's discovery of a phenomenon largely unexplored and unexplained. Those looking for quantifiable explanations of NDEs might find this qualitative exploration frustratingly inconclusive. When either scholars or lay people ask for introductory references on NDEs, we recommend *Life After Life* third on the list, after *Lessons from the Light* (Ring & Valarino 1998), which we consider the "heart" of NDEs, and *The Handbook of Near-Death Experiences: Thirty Years of Investigation* (Holden, Greyson, & James 2009b), which we consider the "head" of NDEs. Since 1975, Moody has published several other resources on NDEs and related phenomena, including sole-authored books and videos and co-authored books. We believe that *Life After Life* will likely be not only Moody's most frequently used resource but also a widely read classic in the field of near-death studies, and justifiably so.

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Reunions: Visionary Encounters with Departed Loved Ones by Raymond Moody, Jr., with Paul Perry. New York: Villard Books, 1993. 211 pp. $6.99. ISBN 0679425705.

Raymond Moody, known for launching modern near-death studies with his book, *Life After Life* (1975), may be credited with other innovative work. His 1993 book, *Reunions*, is a popular account of how to induce visionary experience of one's "departed loved ones." In near-death experiences (NDEs), one seems to encounter such departed ones, often with notable therapeutic effects. Dr. Moody, trained in psychiatry, began to explore ways of inducing these visions as a tool for grief therapy. He recalled methods of ancient Greek oracles and seers, attempting to create a modern *psychomanteum*, a kind of "theater of the mind." As director of this theater, Moody would help the performers (seekers of a vision) enter a suitable state of mind. He provided them with a comfortable, relaxed, almost completely dark environment. As a method for stimulating visions, Moody settled on using a speculum: a crystal or glass ball, or glass of clear liquid such as ink, blood, wine, or water, or a mirror; in fact, just about any shiny surface or transparent substance might do the trick. What was needed was a focus for gazing that was hypnotic and suggestive, conducive to dreaming and free association. Gazing at perceptually ambiguous objects like crystal balls or mirrors—in the right mood and with the right lighting—could stimulate fantasies, dreams, visions.

Moody's theater of the mind is designed to lend itself to visionary experience; the author abstains from theorizing the nature of the envisioned entities. His main concern as maverick psychopomp and psychiatrist (in Greek, "soul-doctor") was to facilitate a visionary encounter with a departed loved one, and to measure success in terms of the therapeutic benefits. Surprised and delighted, Moody found that his visionary theatrics were successful in (a) producing real hallucinations and (b) causing (via self-report) beneficial effects with his subjects.

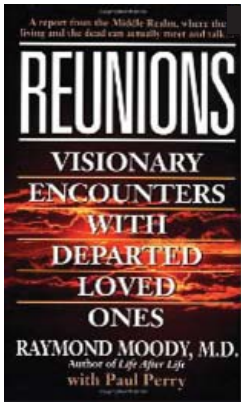
The book's seven chapters cover three main topics. The first talks about crystal and mirror gazing, followed by one that highlights the history of crystal gazing. The next chapters describe Moody's therapeutic experiments and enthusiastically presented results. It also describes Moody's own experiment with the mirror, in which an apparition of his grandma popped out and hung around for a while; the experience, he says, convinced him of the reality of the other world. Elsewhere, however, he seems to express the view that life after death is not amenable to scientific inquiry. His own experience, as he reports it, appears like nothing more than a powerful, self-induced hallucination. This is not to say that all crystal-caused visions are devoid of paranormal content; but as far as I could see, in none of Moody's cases, his own included, did I find

anything paranormal. But for Moody this was not the point of the experiments, for which dwelling on such questions might ruin the therapeutic benefits. Moody's unspoken assumption seems to be: If the experiment makes the subject feel good, it qualifies as a success. The scientific quest for proof would inhibit something else Moody regards as important: the humorous, wonder-inspiring, fun-and-entertainment value of psychical phenomena. In defense of Moody, according to Skeat's etymological dictionary, the Sanskrit root of the word miracle is *smi*—related to *smile*. Miracles make us smile with wonder, which Plato said was the mother of philosophy. (Do people antipathetic to anomalies and "miracles" generally lack a good sense of humor?) In any case, Moody would prefer to leave the humorless, grim task of assessing evidence, calculating probabilities, and qualifying unto death one's conclusions to the grunts and kobolds of parapsychology.

Moody's procedure was to invite a person motivated to have a visionary reunion to join him for a day at his home in Alabama. From the description, the preparation for the show in the theater of the mind consisted of two main things. The first was to get the client to unwind mentally and physically, and to put him into a state of relaxed expectation. Moody's psychomanteum was a collage of devices used to elicit visions from the subliminal mind. Central was the use of darkness, candlelight, and some form of "shew-stone"—a mirror. To suggest the sense of being dislocated in time, all timepieces were removed. One brought and dwelled on mementos of the departed one hoped to see. Like the masters of Baroque Rome, music and art are deployed in service to the induction of spiritual vision. Moody used prints of the Surrealists, also featuring posters of Donald Duck and a life-size statue of a wooden Red Indian, along with other oddments and antiques; the aim was to create a mood of playful fantasia, psychically permissive and mentally fluid.

In terms of inner preparation, Moody encouraged his client to talk about the deceased target person, the feelings, motives, and needs for seeking the vision. Clients were invited to bring photographs and personal effects that might facilitate an encounter. The author quotes some very positive and enthusiastic accounts of the visions experienced, and there is enough reportage to suggest that the experiments were emotionally effective with some of his client-performers. How long and how deep the alleged therapeutic impact we are not told.

Moody is clearly excited by his work with crystal gazing, but seems intent to isolate it from other forms of divination. He states, for instance, that what he is about has "nothing to do with mediumship and séances" (p. xix). Actually, crystal gazing is just one of many forms of mediumship and divination. *Séance* is just a French word for "sitting," and the cornerstone of Moody's psychomanteum is sitting in the dark in front of a crystal or mirror, gazing and waiting for an image, ghost, apparition, or other hallucination to



show up. As with mediumship, so with crystal gazing, it's done in the dark, because the darkness shuts out the distracting visual world and moreover is sleep-and-image conducive; medium and scryer try to bypass reason and communicate directly with some discarnate or higher intelligence. Crystal gazing is one of many ways people have discovered for trying to do this; the general procedure is to divert the conscious mind and allow the unconscious to bubble up freely without impediment. The diversion may occur through crystal gazing, invoking a mediumistic control, native vision questing, chanting or prayer or repeating a mantra, fasting, prolonged wakefulness, immobility (as with the pillar saints), ingestion of psychoactive substances, and so on and so forth.

Even modern gadgetry has vision-producing potential. Moody points out that peering through a telescope toward the starry cosmos may induce visions, and suggests that we return to the art of contemplating nature as a way of inducing visions. Of course, low-tech induction works as well. Jacob Boehme was gazing at sunlight reflected on a pewter dish when he suddenly beheld a transformative vision. Spinoza made his living as a lens grinder and might often have been lost in thought as he focused on the lenses he spent his time finely grinding.

The last three chapters move from a report on mirror-gazing experiments to discussion of mirror gazing as an intuitive skill to learn as an instrument of self-discovery. Cultivating the gazing art itself—the contemplative life—is recommended. In the Indian school, we find the idea of *vipassana* meditation; practicing pure awareness without object, without fixation on anything. Moody sees the skill necessary to open one's visionary eye as playing a role in our spiritual development. Crystal gazing may be thought of as practice for a more contemplative, less analytic and scientifically suspicious, approach to the world at large. Once in possession of more contemplative skills, we may learn to divine much that is hidden in everyday life. This is similar to André Breton's surrealistic plan to occupy the real with the surreal, and superpose the space of dreams upon the space of ordinary life—an intriguing, even disturbing idea of *metaphysical* revolution.[1]

In the final chapter, Moody discusses possible future uses of mirror gazing. Grief therapy has already been noted; we don't know how effective the psychomanteum was for those who came with grief issues; Moody's treatment of everything in this book is sketchy. There is, however, a genuine potential to explore. The difficulty with the idea of grief therapy is this: Everything about Moody's method involves using suggestion to induce a vision of the deceased. Will this not weigh against the belief that the encounter was an authentic visit

from a real deceased person? And wouldn't the therapeutic value depend on *really* believing he had met the departed love one? How much benefit could a grief-stricken person obtain from a vision he knew was deliberately induced?

Other suggestions for application are offered. Suppose, for example, we could hook up a crystal gazer to devices monitoring brain behavior? What would the brain of the active visionary tell us? Of course, there have been neurophysiological studies of meditative states, which might be the place to look for hints on how to proceed with crystal gazers. For another conceivable use, Moody would have us consider hauntings; a competent crystal gazer present at the investigation of haunted houses might conceivably be instructive. (In modern-day ghost hunting, mediums are in fact often brought on the scene to comment.) Moody thinks groups of crystal gazers performing conjointly in some necromantic project might produce some interesting effects, but begs off from trying it himself, preferring to experiment with one person at a time.

Yet another suggestion, crystal-mediated mental skills might be a powerful adjunct to the study of history; the visionary experiences of great historians like Arnold Toynbee and Edward Gibbon furnish examples (p. 195). As clairvoyant visitors to the other world, a new form of historical science would become possible (it would be nice to catch up with Shakespeare in the next world and find out if he really wrote his plays).

Moody's mirror-gazing studies taught him to revere imagination and mark the limits of reason. Crystal gazing, a technique for ransacking the treasure-trove of the subliminal mind, permits a veritable re-enchantment of the world. With it in hand, we can understand the secret of Aladdin's Lamp and follow Alice *through the looking glass*. Indeed, we are told, if we follow the mystery of scrying, we are led back to the fount of all magic and mythology.

This is a suggestive book, especially for its emphasis on the need for experimentation with altered states of consciousness; but, allowing for the author's natural ebullience, he overrates the novelty of his findings (there is no word of predecessors such as Frederic Myers and Goodrich Freer)[2] and exaggerates the implications for science and the understanding of history of his undoubtedly worthy observations on this age-old phenomenon.

Notes

[1] A metaphysical revolution would involve alterations in the fundamental sense of what is real. To this kind of subversive, the real would be treated as unreal; the unreal as truly real.

[2] See Besterman, T. (1965). *Crystal-Gazing: A Study in the History, Distribution, Theory and Practice of Scrying*. New Hyde Park, NY: University Books.

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Recollections of Death: A Medical Investigation by Michael B. Sabom. New York: Harper and Row, 1982. 224 pp. 3 illustrations. ISBN 0060148950.

The phenomenon now known as the Near-Death Experience (NDE), and experiences closely related to it, have in fact been known to exist since antiquity, as evidenced by passages from *The Tibetan Book of the Dead*, the writings of the ancient Egyptians, and passages from Plato's *Republic*. But in modern times the NDE was established as a phenomenon by and received its name from Raymond Moody, M.D., Ph.D., with the publication of his book *Life After Life* in 1975. Moody was the first to collect cases of the NDE and establish a pattern among the various reports from NDE experiencers (NDErs). His study, however, was qualitative in design and was found to be unconvincing among most in the medical and academic arenas.

To put the study of the NDE on a more quantitative and scientific basis, Kenneth Ring, Ph.D., conducted a psychological study of the NDE, which he published in his book *Life at Death* in 1980. The results of this study confirmed Moody's work, especially in terms of the individual phenomena that constitute what are now called the *core features* of the NDE. Nonetheless, these two works sparked more controversy than anything else, and the scientific and medical communities maintained their skepticism about the NDE, even questioning whether the reports were fabricated or somehow subconsciously suggested by publicity about the phenomenon. Michael Sabom, M.D., was one such person.

It was during the same period as Ring's initial research that Sabom, a cardiologist, began his research into the NDE, yielding his book *Recollections of Death* in 1982. Sabom had read Moody's work and was skeptical of Moody's dualistic interpretation of the data—that the human mind or personality survives the death of the physical body. In Chapter One of his book, Sabom describes the origins of his interest in the NDE. Even though he was a member of a Christian church, he initially greeted Moody's work with a typical scientific skepticism, an "indoctrinated scientific mind," as he put it (p. 3).

In an effort to demonstrate that the NDE was fictional, Sabom conducted a medical investigation of the NDE at the University of Florida Medical School teaching hospital and later at the Atlanta Veterans Administration Medical Center. Sabom describes his methodology in Chapter One. He and his coworker, Sarah Kreutziger (a psychiatric social worker) eventually assembled a sample consisting of 116 cases. The interview method used was similar to Ring's methodology and the same types of data were collected. It is important to point out that Sabom's patients, as Ring's, did not know that the researchers were interested in the NDE until after the patients had given their initial accounts.

Sabom's analysis of the data confirmed the generality of Moody's NDE features. Several of these core features are described in Chapter Two, along with examples from Sabom's own data. In analyzing his data, Sabom classified NDEs as autoscopic (viewing one's own body from an external perspective), transcendental (entering a different reality), and composite or combined (having autoscopic and transcendental elements). The autoscopic category is described in Chapter Three, again accompanied by examples from Sabom's dataset. Chapter Four describes the transcendental and combined categories, also with illustrative cases.

Sabom statistically analyzed the data, as described in Chapter Five, for correlations between frequency of the NDE and several variables. He found no significant correlations for age, gender, race, location of residence, size of home community, years of education, occupation, religious background, frequency of church attendance, prior knowledge of the NDE, or type of near-death crisis event (cardiac arrest, coma, or accident). Also, the content or features of the NDE did not vary significantly with these variables. Thus, Sabom's *first* major contribution to NDE research was this important replication and verification of the work of Moody and Ring.

Sabom made a *second* major contribution by critically comparing the core NDE phenomena with other psychological experiences proposed to explain the NDE in terms of materialistic models (discussed in Chapter Ten). In accordance with the philosophical and scientific principle of Occam's Razor, parsimony requires that the inadequacy of materialistic explanations be demonstrated before other models can be proposed. Sabom made significant progress toward this goal by comparing the key features of the NDE with those of other psychological phenomena, such as autoscopic hallucinations and reactions to anesthetics, which had been proposed in the literature as NDE explanations. He considered a wide variety of medical and psychiatric syndromes that could possibly explain the features of the NDE. For each syndrome, he showed that the characteristic phenomena differed from those of the NDE. For example, temporal lobe seizures involve negative affect, whereas those who experience at least one stage of the NDE overwhelmingly report positive affect. In this manner, Sabom showed that no materialistic model proposed thus far could account for the content of the NDE.

But perhaps most important was the *third* major contribution of Sabom's work: investigation of operating room (OR) NDEs. His descriptions of these cases are presented in Chapters Six and Seven. Suppose one could arrange for unique, purely visual stimuli to be present at the time of an NDE. If the NDEr could, after the experience, accurately describe these stimuli without previous knowledge of them, and if this accuracy could be documented, then a

materialistic interpretation of the NDE would be strongly challenged. While not designed as such, OR NDEs provide a very close, naturalistic approximation to the visual experiment described above. The patient is unconscious before being brought into the OR, and typically is unfamiliar with the personnel, equipment, or specific procedures used during surgery. Thus, if a patient has an NDE under these conditions, the opportunity exists to compare the patient's recall of the details with the medical records and with standard medical practices. This experiment is not ideal, since a patient might be familiar enough with medicine to reconstruct the events fairly well, or may be able to hear under anesthesia and infer visual details from these perceptions. Even with these flaws, the OR presents one of the best chances to evaluate the NDE objectively.

Sabom recognized this opportunity. He identified 32 cases of autoscopic NDEs occurring in operating rooms, and accessed the medical records for these cases. Comparing the NDE interview data with these records (which the patients had never seen), Sabom found that in every case, the NDEr's report was consistent with the medical records, equipment, and procedures. Of course, the NDEr typically offered more detail than appeared in the records, but where the records were explicit, the interview data were consistent.

It is impossible merely to summarize the reports of these patients while also demonstrating their stunning detail and accuracy. Thus, I provide an extended quotation of Sabom's work as an example, found below. This was the NDE of a retired air force pilot who had a cardiac arrest while in the hospital following two heart attacks, reported to Sabom in 1978 (five years after the incident). Within his account, the NDEr described the behavior of the meter on the defibrillator which was being used to revive him. I note that doctors and medical staff typically do not attempt to revive a living person, so we can be assured that this patient actually was dead, although not irreversibly so. Thus, there is no concern as to whether the patient heard the medical team at work or could somehow see their actions from a bodily perspective. This is the case with all the examples that Sabom describes in Chapters Six and Seven.

Operating Room NDE of a Retired Air Force Pilot (S) (pp. 99–101)

S: I had the arrest the following morning after the night I had my second heart attack. . . . I think I was sleeping. It was two or three in the morning. . . . There was no feeling in myself that I was even having an arrest. I wouldn't even have known it unless all the people came around. I think I was probably asleep when the thing arrested. The first thing I remember was hearing Code Blue [another term for Code 99] on the intercom and I remember everyone running in. . . .

A: Do you remember any of the other details that went on in the room?

S: I remember them pulling over the cart, the defibrillator, the thing with the paddles on it. I remember they asked for so many watt-seconds or something on the thing, and they gave me a jolt with it.

A: Did you notice any details of the machine itself or the cart it was sitting on?
S: I remember it had a meter on the face. I assume it read the voltage, or current, or watt-seconds, or whatever they program the thing for.
A: Did you notice how the meter looked?
S: It was square and had two needles on there, one fixed and one which moved.
A: How did it move?
S: It seemed to come up rather slowly, really. It didn't just pop up like an ammeter or a voltmeter or something registering.
A: And how far up did it go?
S: The first time it went between one-third and one-half scale. And then they did it again, and this time it went up over one-half scale, and the third time it was about three-quarters.
A: What was the relationship between the moving needle and the fixed needle?
S: I think the fixed needle moved each time they punched the thing and somebody was messing with it. And I think they moved the fixed needle and it stayed still while the other one moved up.
A: Did the moving needle ever pass the fixed needle?
S: I don't think so, but I don't specifically remember.

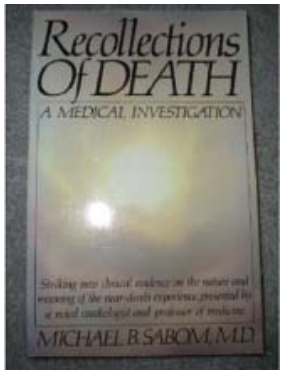
This account is given in Chapter Seven, accompanied by several other equally impressive cases. Sabom's commentary on the case above was especially telling and compelling:

> I was particularly fascinated by his description of a "fixed" needle and a "moving" needle on the face of the defibrillator as it was being charged with electricity. The movement of these two needles is not something he could have observed unless he had actually seen this instrument in use. . . . This charging procedure is only performed immediately prior to defibrillation, since once charged, this machine poses a serious electrical hazard unless it is correctly discharged in a very specific manner. Moreover, the meters of the type described by this man are not found on more recent defibrillator models, but were in common use in 1973, at the time of his cardiac arrest. (p.104)

Thus, this patient's report was incredibly detailed and amazingly accurate. Combined with the other such cases that Sabom reported, there is a strong argument to be made that while the bodies died, the people did not.

In order to compare his results with a control group, Sabom interviewed a group of surgical patients who did not report NDEs, and asked them to describe their ideas as to how resuscitations were conducted. Analysis of these reconstructions revealed at least one major error in each account. This contrasted sharply with the NDEr accounts, none of which contained any errors. Also, the descriptions of the control group lacked the clarity and detail of the NDE accounts. It thus would appear that these NDErs truly witnessed OR events they should never have seen.

In the remainder of the book, Sabom discussed potential materialistic explanations of the NDE (as described earlier in this review) and the implications of the NDE for various fields. Space prohibits further discussion



of these chapters, but I believe the overall point is clear: Sabom demonstrated to his own satisfaction (and mine as well) that the human personality *does* survive the death of the physical body. This is in the immediate and literal sense. How long that personality persists, and where it goes after leaving the body, were issues that Sabom's study did not and could not address. But the materialistic paradigm—that the brain generates the mind, that the mind is what the brain does—is completely inconsistent with Sabom's findings, as well with all other empirical NDE studies before and after his book was published.

To date, no one has demonstrated that these empirical findings are flawed in any way. Some do continue to attempt to disprove the validity of the NDE as a true experience, but they argue on a theoretical level only. Curiously, these skeptics continue to work on debunking the NDE while simultaneously ignoring the empirical data and replicable results that exist in the literature, especially Sabom's work. These skeptical researchers typically reduce the rich qualitative accounts of the NDE to an overly simplistic caricature of the core features, then explain away each individual feature out of context, and with a mosaic of theories rather than a comprehensive one. Such efforts reek of intellectual dishonesty, and do nothing to undermine the validity of the NDE.

Even more disconcertingly, many of those who believe as I do that the NDE destroys the materialistic paradigm appear to have neglected or forgotten Sabom's crucial work. In my estimation, *Recollections of Death* should have gone most of the way toward dismantling materialism and giving new directions for research into consciousness and the mind–body problem. The book should not stand on its own, of course, since replication in science is a vital step in testing a theory. But such replications have been performed, and Sabom's original work has been supported by later research. It is well past time to acknowledge the crucial nature of his book and investigate its implications.

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Communicating Rocks—Writing, Speaking, and Thinking About Geology by Peter Copeland. Pearson, 2012. 176 pp. $38.47 (paperback). ISBN 9780321689672.

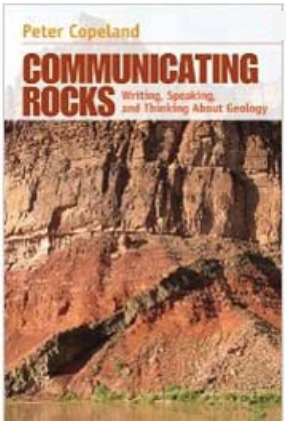
No style of writing is so delightful as that which is all pith,
which never omits a necessary word, nor uses an unnecessary one.
—Thomas Jefferson (1743–1826)

Writing, speaking, and talking publicly about geology and, by extension, about science, may be a daunting task for some of us (called scientists). The major fear, in my opinion, is that our audience might be not prepared to understand our lingo and thus we will end up barking up the wrong tree. Additionally, scientists tend to be more cautious speaking or writing to audiences outside their own field because they are fully aware of how easy is to make a fool of oneself when venturing publicly beyond one's own group. On the other hand, the students taking our geology classes are trying to develop excellent writing skills to publish reports and to solicit grants from government, industry, and private foundations. They are also interested in mastering speaking skills necessary in future job endeavors.

Being capable of writing and speaking clearly and persuasively about geology (i.e., science) is a task every undergraduate and graduate student (and not only they) should tackle earnestly if they want to communicate to others the results of their work. That's why I read with interest the recent book written by geology professor Peter Copeland. Being myself a professor of geology and teaching for thirty years in universities in both Romania and the U.S., I took a special interest in what he had to say about "balancing science with writing and speaking," and making "the case that one cannot have the former without the latter."

The book is balanced, going from "Communication Equals Thinking" (Chapter 1) to "Written Communication" (Chapter 2) and "Oral Communication" (Chapter 3) to end with "Writing is Hard" (Chapter 4). The author uses some of his own research and teaching experience to illustrate "types of written communication" (the abstract, the research proposal, the research paper, the review paper, etc.). He then presents extensively "problem words and concepts" (e.g., common confusions surrounding words such as accuracy, precision, uncertainty, and responsibility; or hypothesis, theory, and law). Useful hints and examples are offered in the "Oral Communication" chapter (know your audience, give a road map, stick the dismount, etc.)

Overall, the content of the book was interesting, and I agree with the



author's assessment of many of the issues discussed there. About others, I have reservations. Here is an incomplete list.

From the moment I moved from Romania to the U.S.., back in 1993, I was appalled by how little Latin and Greek are known here. Whenever I heard or read that "mass media *is* . . . ," or "data *is* . . . ," I cringed. At the University of Oklahoma I tried to continue my Romanian academic experience and teach my American students some basic Latin, Greek, or French in addition to geology classes. After a couple of semesters, some student evaluations started to read like this: "I paid for this class to be taught geology, not Latin or Greek." No comments.

Is there a solution to this issue? Peter Copeland quotes Patricia T. O'Connor (1996), who argues that *data* has joined a group of technically plural Latin and Greek words very few recognize as thus. Other linguists I talked to told me that there is little hope: The spoken language imposes the norm. If more and more people are going to use "mass media *is* . . . " or "data *is* . . . ," little can be done to stop it (maybe by making the study of Latin and Greek mandatory?).

Completely agreeing with what Copeland wrote about the above topic, I was puzzled to find on page 100 the following: "The data has [*sic*] not constrained the history of the reason . . . ." Is there a typo here?

Remaining in the realm of foreign languages, I noticed that the author misspelled twice (page 52 and page 54) the French word *voilà* (he wrote *viola*). Any explanation?

On page 64 Copeland writes: ". . . strictly speaking, *heat* is a verb and shouldn't be used as a noun." I respectfully disagree: My second Ph.D. thesis (1997) was titled *Heat Flow in Oklahoma* (Cranganu 1997), and I still do not see any verb meaning in *heat flow*. The *American Heritage College Dictionary* (fourth edition, 2002) lists for *heat* 14 definitions as a noun and only 5 definitions as a verb.

On pages 62 and 63, after the definition of *half-time*, the author writes nine equations that have little to do with half-time ($t_{1/2}$), a term applied to radioactive elements. The most important equation ($t_{1/2} = \ln 2/\lambda$) is missing.

On page 30 it says: "*Accuracy* is *not* how close a measured value is to the actual (true) value." Well, I have to disagree again and—if I am allowed—I would like to quote from my Geostatistics course:

> *Accuracy* is determined by bias.
> Bias is error that is the same for every measurement. For example, a scale that always gives you a reading that is too low.
> The smaller the bias, the more accurate the measuring process.
> The bias in the measuring process is the difference between the mean measurement μ and the true value:
>
> $$\text{Bias} = \mu - \text{true value}$$

On the other hand, Figure 2.2 on page 34 is a good illustration of the concepts of accuracy, precision, and repeatability (reproducibility).

On page 96, discussing a sentence with (possibly) eight errors, the author writes:

> 2) *Metrics*. Not a noun. [*sic*!]

If metrics is not a noun, then what it is? I noticed earlier (page 69) that he decried the use of *metric* as "another unfortunate example of the nounification of adjectives." I did a rapid search online to see whether metric(s) is a noun and here is what I found:

From the *Merriam-Webster Dictionary* (2011): Metric

> **1** *plural* **:** a part of prosody that deals with metrical structure
> **2:** a standard of measurement <no *metric* exists that can be applied directly to happiness—*Scientific Monthly*>
> **3:** a mathematical function that associates a real nonnegative number analogous to distance with each pair of elements in a set such that the number is zero only if the two elements are identical, the number is the same regardless of the order in which the two elements are taken, and the number associated with one pair of elements plus that associated with one member of the pair and a third element is equal to or greater than the number associated with the other member of the pair and the third

On page 105, Copeland proposes an alternative to the ways in which the electromagnetic energy from the Sun interacts with an object:

> When electromagnetic energy from the Sun strikes an object, the energy can be reflected, adsorbed [*sic*], or transmitted.

Although I agree with many other alternatives he proposed in the book, this time he should have stuck with the uncorrected version [absorption]. Simply because **adsorption** is the adhesion of atoms, ions, biomolecules, or molecules of gas, liquid, or dissolved solids to a surface. The electromagnetic energy from the Sun is **absorbed**, not **adsorbed**.

On page 89, writing about split infinitives, the author states:

> The infinitive forms of English verbs come as a two-word package. . . . Somewhere, somebody decided that these pairs should never be split apart. . . . This is a rule in Latin, but it has no correspondence in English.

What rule in Latin is he referring to? I know Latin, but I am not aware of such a rule, because the Latin verbs do not come as a two-word package (for example, *to love* in English = *amo* in Latin)

Finally, I have one more comment: Copeland quotes O'Connor (1996) twice (page 48 and page 64), but he did not list her work in the References.

In similar situations, my former Romanian students (quoting Horace) used to tell me: "aliquando bonus dormitat Homerus . . . "

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Cuando Hablan Los Espiritus—Historias del Movimiento Kardeciano en la Argentina [**When the Spirits Speak: A History of the Kardecian Movement in Argentina**] by Juan Gimeno, Juan Corbetta, and Fabiana Savall. Buenos Aires: Editorial Dunken, 2010. 384 pp. US$20. ISBN 9789870248163.

Throughout human history, the common necessity to create myths, stories, urban legends, systems of thought, and beliefs associated with the possible survival of one's personality after death has excited all cultures without exception. (p. 17)

This quotation by Dr. Alejandro Parra begins the Prologue that introduces the book *When the Spirits Speak—A History of the Kardecian Movement in Argentina* [*Cuando Hablan Los Espiritus—Historias del Movimiento Kardeciano en la Argentina*]; it very succinctly summarizes a human need to know what is beyond the worldly life we humans experience here on the earth plane. Is there something more? What happens to the soul essence of the person once the transition known as death occurs? Can spirits return to communicate with those left here on the earth plane?

These are a few of the questions raised and answered by coauthors Juan Gimeno, Juan Corbettta, and Fabiana Savall in this very detailed and well-documented academic book which tackles head-on the idea of life after death and the long and colorful history surrounding the belief systems connected with Spiritism and Modern Spiritualism. Most importantly, the history of Spiritism, as prescribed by Allan Kardec (1804–1869)—a French educator (and later, author) who gained widespread acclaim and notoriety as the founder of Spiritism—is the focus of this book.

It is interesting to note that it was not until Kardec was in his fifties that he become interested in spirituality, and this was only after witnessing some "spirit tapping" (a phenomenon where Spirits make contact with humans by making tapping sounds) which had gained in popularity in the United States with the advent of Modern Spiritualism. Although he was not a professionally trained scientist, he decided to do his own research and subsequently wrote a number of books that are still in circulation today, most notably *The Spirits' Book* and *The Book on Mediums*.

This is the beginning of the Spiritism movement that this book is about. Although Modern Spiritualism and Spiritism are as closely related as a parent and a child, one primary difference in the early years of both movements was the fact that Spiritism openly embraced the idea of reincarnation and Modern Spiritualism was much slower to do so (today, a number of Spiritualists, as well as Spiritists, actively embrace the idea of reincarnation in their spiritual beliefs).

When the Spirits Speak offers readers a very thorough and complete examination of spirit communication from its early beginnings to modern-day times in Argentina. The authors meticulously, and in painstaking detail, research the movement's highs and lows throughout its long history. It is quite obvious that the authors are passionate about this topic because as I read through the book I could feel their devotion and love for the subject. It offers an interesting viewpoint that seems almost nostalgic (similar to what one might encounter in an autobiography) and almost wistful for the golden age of Spiritism.

The authors make some rather grandiose claims at the beginning of the book which purport to prove scientifically the existence of Spirits and subsequently Spiritism's attraction over other mainstream or traditional religions, offering debates between Christian and Catholic orthodox leaders. I would have liked to have read more about the process of these debates, however, in order to come to a more concrete conclusion regarding that which was being debated. The issue, I suppose, is that in any religion "faith" is the cornerstone from which all other beliefs are made. For people who have experienced spirit communication personally, no amount of debate or arguments for or against it will ever be enough to convince them about what they otherwise know internally, from an intuitive vantage point. "Faith," and their experience with this faith, is what make people followers of any religion or movement.

Will science someday prove the existence of spirits inhabiting an "other" worldly realm that can communicate with those left behind? Perhaps. But until it does, people must follow their own personal faith and belief system based on personal experience in the religious tradition which they follow. Spiritism and Modern Spiritualism are no different. For those who have experienced spirit communication personally, it allows them to have an unshakeable faith that is experiential. Who knows? Perhaps science will someday finally catch up with Spiritism and Modern Spiritualism, confirming what many people over the eons have already realized to be true intuitively.

If I were to have a criticism of the book, which I really do not, it would be that I would have appreciated more contrastive information in its historical review of Spiritism in order for the reader to make his or her own conclusions about the movement of Spiritism. This is a minor point, however, when considering all the historical detail which is included, as well as the academic

referencing of primary and secondary sources. Perhaps in the future, should the authors have a followup work to this book, it might be nice if other doctrines were included in order to offer a contrastive review with other belief systems.

Although I am not a native Spanish speaker, I was easily able to read the book, as it is written in a very down-to-earth manner, allowing non-experts to understand and follow without difficulty the information contained within it. Some academic books get bogged down in using technical terms which limit the understanding to specialized experts. Not this book—it allows novices interested in the topic to grasp easily the points and suppositions.

I certainly recommend this book to people who are interested in spirit communication and its history. It would make a nice addition to anyone's existing library as it fills a hole in published materials on the subject of the history of Spiritism in Argentina.

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The Psychopath Test: A Journey through the Madness Industry by Jon Ronson. Riverhead, 2011. 288 pp., $25.95. ISBN 9781594488016.

To say that *The Psychopath Test* is a weird book is an understatement. Riveting, from beginning to end, it reads like a nonfictional variation of *Alice in Wonderland* with the author, Jon Ronson, taking on the role of Alice. The people he encounters and the events he describes seem way too far-fetched to be true; one must keep reminding oneself that it's all for real.

In that sense the book is similar to Ronson's earlier bestseller *The Men Who Stare at Goats* (Ronson 2005), which was turned into a Hollywood movie starring George Clooney. Reading that book, and watching the movie based on it, helped me to appreciate this author's unusual style.

He once said that the idea of *The Men Who Stare* project was to explore "the apparent madness at the heart of U.S. military intelligence" (Ronson 2012). In the case of this *Psychopath Test* adventure, it might be said that the idea is to explore the apparent madness at the heart of the madness industry.

It begins with Ronson meeting in a Bloomsbury coffee shop with a neurologist who shows him a strange package she had received in the mail. Postmarked Gothenburg, Sweden, it contains an expensively produced book titled *Being or Nothingness*—suggestive of Sartre. The cover is a reproduction of M. C. Escher's *Drawing Hands*. Of the 42 pages, half are blank, 21 have text (sometimes just a cryptic sentence), and one of them has words cut out. The same package, he is told, had been sent anonymously to a number of accomplished academics, in various disciplines, who are intrigued, become convinced that it is meant to relay to them some important message, and struggle to figure out the puzzle. When none of them can solve it, they turn for help to Ronson—a non-academic who is known for his investigative journalistic talents.

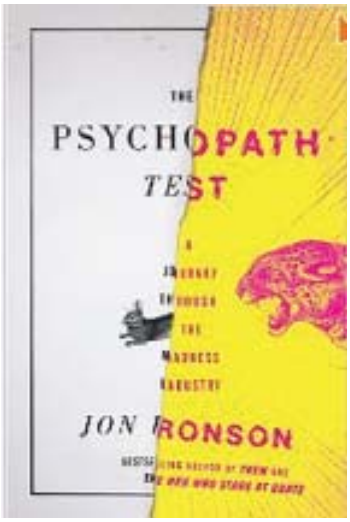
Very quickly, his sleuthing takes him on a trip to Sweden and to the door of a seemingly prominent psychiatrist. His wife answers and Ronson learns a few things about this man, including that he is away for a few days seeing patients at a clinic. He then spends several days waiting in his hotel room watching TV shows that he can't understand (because they are in Swedish) before he returns to the house for what seems to have been a very brief meeting with this strange man. Concluding, on the basis of what I'm not sure, that he is the source of the book, Ronson returns to London and informs the neurologist that the mystery is solved. She, on hearing this news, says: "That *Being or Nothingness* thing . . . Are you *sure* it was all because of one crazy man?" (p. 32). Wondering what kind of man does something like this—something that so dramatically disturbs other people—Ronson veers off into an exploration of the meaning of "psychopathy."

To this point in the book, the story had seemed to be about the *Being or Nothingness* book, and, while there had been references to psychopathy, these seemed to be "in passing." Ronson had talked about how his first meeting with the neurologist coincided with the threat of a lawsuit against him for calling someone a psychopath. When he encountered a researcher who specialized in studying psychopaths, he had expressed some curiosity and had mentioned recalling claims made that it was psychopaths who ran the world. But the book had seemed to be about the *Being or Nothingness* mystery; so, it was a bit disorienting when that mystery was so quickly solved and the focus shifted, via the puzzle of psychopathy, into madness.

Soon I was feeling, I think, the way Alice must have felt when she fell down the rabbit hole to encounter Wonderland's cast of unusual (and quite possibly mad) characters. While I can't possibly convey the feeling, I will introduce a few of these characters—just to give the flavor of the encounters and to give a sense of what the experience of reading this book is like.

I'll start with Tony. Scientologists, active in fighting the power wielded by psychiatrists, arrange for Ronson to visit Tony, a fellow who, having been convicted of Grievous Bodily Harm and, facing a 5–7 year prison sentence, had decided to act delusional in order to be hospitalized instead of imprisoned. "The minute he got admitted to Broadmoor . . . and saw what a hell hole he'd got himself into, the symptoms just vanished" (p. 57). A host of psychiatrists assessed him and agreed he was not mentally ill. But they decided that he suffered from psychopathy, thus declaring him dangerous and condemning him to an indefinite hospital stay. Twelve years later, he remains incarcerated with an indeterminate sentence. Everything he does to prove he is not mentally ill is seen as further evidence of his psychopathic personality. In his meeting with Ronson, Tony appears, well, sane enough.

Then, there's Elliott Barker—a Canadian psychiatrist who had worked at Oak Ridge Hospital for the Criminally Insane in Ontario in the 1960s. After investigating nude psychotherapy in California, R. D. Laing's therapeutic community in London, and the use of psychedelic drugs, Barker invented a raw, naked, LSD-fueled treatment program for psychopaths. Participants spent 11 days and nights in what Barker called the Total Encounter Capsule, exploring and exposing their deepest emotions and urges. While initial opinions were positive, evidence gradually accumulated showing that if anything, it made psychopaths worse. Eighty percent of those who went through the program re-offended while only 60% of those who didn't re-offended, introducing the still-popular notion that psychopaths are both sick and untreatable. After looking at the material written about Barker's treatment program, Ronson interviews one of the former patients who claims that what the "treatment" did for him was teach him to manipulate better. Then he converses by email with one of Barker's



former colleagues who says that he'd always thought that Barker had his heart in the right place but that psychopaths are just born that way. He refers to Robert Hare whom Ronson had first heard mentioned in his brief encounter with the psychopath researcher way back when he was just starting to delve into the *Being or Nothingness* mystery.

Hare turns out to be the pivotal character in the book. A Canadian psychologist, he is the undisputed, internationally renowned expert on psychopaths. His research spanning three decades, and his writings, including the classics *Without Conscience* and *The Disturbing World of Psychopaths among Us*, form the basis of contemporary notions about psychopaths. Hare's test, *The Psychopathy Checklist—Revised* (*The PCL-R*), is the most widely used instrument for identifying them. It's based on the idea that there are 20 characteristics (e.g., "Glibness and Superficial Charm"; "Shallow Affect") by which a psychopath can be identified. Hare's *PCL-R* is now used routinely in prison systems where it is used to identify the most dangerous, and most prone-to-re-offend, prisoners.

But Hare's influence is much more widely spread. It has served to bolster a widespread impression that these predators are not just in prisons but rather roaming anywhere and everywhere, and that among those who hold powerful positions in industry, business, and government they may be especially plentiful—and especially dangerous. "Corporate and political and religious psychopaths ruin economies," Hare states, "They ruin societies" (p. 112).

Ronson e-mails Hare and they agree to meet for a drink in a hotel bar in rural Pembrokeshire, West Wales. They get along well and Ronson ends up registering, at an only slightly reduced fee, in Hare's three-day workshop where he learns to identify psychopath traits in others' speech and behavior, and to score the Checklist. By the third day, he feels that he has "new powers" as a certified "psychopath-spotter." Ronson writes,

> Everyone in the field seemed to regard psychopaths (as) inhuman, relentlessly wicked forces, whirlwinds of malevolence, forever harming society but impossible to identify unless you're trained in the subtle art of spotting them as (he) was now. (p. 114)

Ronson imagines applying his new powers to his friends and associates, and teaches his wife to administer the Checklist. It's no surprise that she quickly

"identifie(s) lots of people we know as psychopaths" (p. 169).

Now, Ronson takes us on the next lap of this adventure as he goes about applying his new "psychopath-spotter" powers.

We meet "Toto," a former Haitian death-squad leader, responsible for countless atrocities and now incarcerated for mortgage fraud. We learn that he wants everyone to like him and later learn that he wants to be liked only so that he can more easily manipulate. Then, there is Al Dunlap, who is well-suited to his career in shutting down failing factories and firing employees by the hundreds. With his cool, grandiose personality and extensive trophy collection of ferocious, predatory animals, he appears to score high on the *PCL-R*. Next we meet Charlotte, who may well be drawing on her psychopathic traits in her job of booking guests for the Jerry Springer TV show, and David Shayler, an MI5 spy who became a 9/11 conspiracy theorist, promoting the idea that the World Trade Center plane crashes were the product of government-projected holograms. None of these characters is actually scored as a psychopath; however, Ronson's comments on how they present or behave suggest that they'd be likely to be given high scores on the *PCL-R*.

Eventually, we come to accept, as Ronson seemingly does, that these people are, to differing degrees, psychopaths. And there are moments even, in later encounters with Hare, when Ronson seems to be wondering whether Hare himself might be a psychopath or, at least, dangerous because of the "power" bestowed upon the psychopath-spotters he trains and certifies.

Some nagging questions—ones that come to nag the author and eventually the reader too—are:

- Should "psychopathy" really be recognized as a mental illness? At present, it is not officially a mental illness but it is thought of already that way and could one day be officially proclaimed a psychiatric diagnosis.

- Is the *PCL-R* a reliable way to identify psychopaths? Or is it a dangerous weapon, especially when put in wrong hands?

- Are there effective therapies, as Barker believed, or are psychopaths untreatable—a different species as Hare contends?

- Should we be defending ourselves by identifying people we encounter as psychopaths? Or does believing that we can identify them just make us paranoid and in danger of seeing everyone through a darkened lens?

Ronson summarizes this dilemma writing:

> Some months passed, during which I solved the *Being or Nothingness* mystery, met the Scientologists and Tony in Broadmoor, attempted to prove (with mixed results) Bob Hare's theory that psychopaths rule the world, and became uncomfortably conscious of the fact that being a psychopath-spotter had turned me somewhat power-crazed. Actually, I now realized I had been a somewhat power-crazed madness-spotter for twenty years. It is what we journalists do. It is why I had taken to being a psychopath-spotter with such aplomb. I was good at spotting the diamonds of craziness amid the gloom of normality because it's what I've done for a living for twenty years. There can be something quite psychopathic about journalism, about psychology, about the art of madness-spotting. (p. 205)

In the last few pages, the *Being or Nothingness* mystery resurfaces as Ronson himself becomes the recipient of the strange package that, way back at the beginning, had so intrigued and puzzled the academics. I didn't entirely understand what this meant—or what the message of the book, presumably sent by the Swedish psychiatrist, might actually be. But, I accept this note of uncertainty as a suitable ending.

Perhaps, Ronson wants to leave us wondering—not knowing for sure whether "psychopathy" is something or nothing, an abnormality or a shade of normality, a scientific finding or a well-marketed idea, a useful concept or a harmful idea. Assuming that he does want to leave us in this state of confusion, I will say that of all the books I have read that claim to expose or critique psychological notions and to question society's tendency to go along with them, *The Psychopath Test* may well be the best. That's because the author, very much like Alice when she falls down the rabbit hole, starts off without an agenda, sets out on an adventure, doesn't judge or condemn the characters he meets, catches himself when he jumps to conclusions, and imparts to the reader an appreciation of wonder.

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Hidden Realms, Lost Civilizations, and Beings from Other Worlds by Jerome Clark. Detroit: Visible Ink Press, 2010. 352 pp. $24.95 (paperback). ISBN 1578591759.

This volume is the latest in a series of valuable reference works by Jerome Clark. A central figure in the UFO community since the 1970s, his objectivity and concern for verifiable facts has gained him respect from all sides of this contentious topic. His major work, *The UFO Encyclopedia* (Clark 1990–1998), pulls together widely scattered ephemeral data about the most significant incidents, investigators, and theories involved in this complex phenomenon. Since then, Clark has produced similar compendia, dealing with material that, arguably, is not as central to understanding the physical nature of UFOs themselves, but still deserves attention.

Hidden Realms, Lost Civilizations, and Beings from Other Worlds is such a work. A self-proclaimed "agnostic" of any theory that provides a simple explanation of UFOs, Clark insists that serious research needs to place highest value on verifiable physical evidence. This involves resisting the psychological pressure that would discount some reports in order to save a cherished hypothesis. This is true, he argues, whether the cherished hypothesis considers UFOs as natural phenomena or as evidence that extraterrestrials are contacting humans. Therefore, this volume is a challenging one for Clark to compile, as it collects and discusses the most dubious and physically impossible claims that have in some way impacted UFO research.

The book's flashy title, unfortunately, obscures the actual content of this reference work. In fact, it is divided into three sections, none of which clearly lines up with the three elements of the title. The first, "Earth's Secret Places," handles traditions that derive from the American tradition of spiritualism. Since the 19th century, mediums have recorded alternative histories of world civilizations, via automatic writing or dictated messages from spirit guides. These often describe cultures that attained spiritual and technological heights unknown to the modern world, but then were destroyed in some global calamity. Clark surveys spiritualistic accounts of the world of Lemuria (a culture that occupied a continent now covered by the Pacific Ocean) as well as the distinctively American version of the ancient Greek realm of Atlantis, largely generated by the 20th-century Kentucky seer Edgar Cayce. Traditions deriving from the "hollow earth" theory generated by John Cleves Symmes, Jr., (and popularized by Edgar Allen Poe) are also included. An especially useful chapter sums up the career of Richard S. Shaver, a former mental patient whose writings told sensational tales of demonic deros and benevolent teros in subterranean passageways underneath the earth.

A second section, "The Alternate Solar System," focuses on the tradition of "contactee" lore, in which individuals claim to have met aliens from other planets in the solar system. These are, interestingly, placed in the context of early astronomical thinking, in which many respected scientists argued that complex life was indeed possible on all of these locations and even claimed to have detected physical evidence of intelligent life there. However, most of the section summarizes the stories of people like George Adamski who described long-term relationships with visitors from other planets and published complex accounts of extraterrestrial journeys with the aliens in their spacecraft. Some of the contactees' accounts may be, like Shaver's stories, complex personal delusions, but a number of them were no better than confidence men, using the strong cultural interest in UFOs generated in the 1950s to market their fictions to a credulous or at least intrigued audience.

The final section, "Between this World and the Otherworld," is the most miscellaneous, dealing with personal experience stories and news accounts of increasingly bizarre events. After a brief survey of encounters with fairies and elves, mostly (but not exclusively) drawn from historical documents, Clark adds chapters on more recent bizarre experiences. These include celestial visions of ghostly armies marching across the skies, airborne dragons and giant snakes, and a rash of news accounts of "airship" sightings from 1897, a time when no practical form of human-controlled flying machine yet existed. Clark quotes from a range of previously unexamined news stories from the late 19th and early 20th centuries to show that such events were widely reported in the American media as unusual but not incredible.

The book, however, is more suggestive than truly informative. In an Introduction, the agnostic Clark calls these phenomena "experience anomalies," that is, "something not quite wholly real and something not quite wholly dreamed up . . . experienced vividly in ways that resist both prosaic explanation and lazy categorization" (p. xiii). In this, he follows the radical skepticism of Charles Fort, whom he credits in his Acknowledgments, who compiled four influential books from [then] recent news accounts that presented as fact events that scientific minds of his day "damned" as unworthy of notice. Clark has indeed also been an important figure in the contemporary scene of "forteans," amateur observers and compilers of paranormal and extraordinary claims in the press and, when possible, among direct observers. However, many forteans work from very limited goals, finding the material itself intriguing but not wanting to delve deeply into possible explanations.

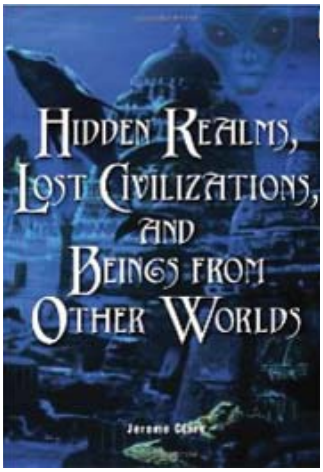
For it is clear that there is a wide diversity of explanations. Some, clearly, are clever hoaxes, intended to pull an audience's leg for a while. Some of the elaborate news stories from the 19th and early 20th centuries are told with a straight face but were intended as practical jokes. Their original audiences

would have read them as "windies," reacting to contextual details that now are difficult or impossible to reconstruct. Others, as Clark frankly admits at times, were perpetrated dishonestly and with mercenary intent, hoping to increase a magazine's circulation or even to swindle people wanting to gain proof of extraterrestrial realms. Still others seem likely to be, as debunkers would suspect, misidentifications of natural events. The stories of ghostly armies and twisting serpents in the night sky, for example, suggest the lights generated by an unusual and unfamiliar display of the aurora borealis during a geomagnetic storm, as seen through eyes undimmed by modern night-time light pollution.

But much of the material is not as easy to explain away, and, by the same token, equally challenging to explain. This in fact points to the key limitation of Clark's work: While he stops short of denouncing the topics of his book in their entirety as unscientific nonsense, he also does not suggest any constructive way of viewing what they actually represent. His Introduction suggests some paths: He concedes that these experiences are subjective, present only in the memories and imaginations of tellers and audiences. And the fact that they are *shared* experience, rather than hallucinations unique to solitary deviants, suggests that they are "supernatural and recognizable" (p. xiv). That is, the narratives represent realities that other people can accept as plausibly real. And this in turn signifies that the stories, however impossible as science and fact, nevertheless connect up with culturally constructed beliefs and myths that a significant portion of American culture would be predisposed to accept as human experience.

The stories Clark summarizes may contribute nothing to the scientific examination of what UFOs actually are in the mundane world, but they do, as Fort intuited decades ago, challenge the official scientific conception of consensus reality, as allegedly sane people construct it with the help of their senses and the belief language that culture provides them. Clark is especially hard on the contactees, because their incredible stories, backed by equally unconvincing photographs of spacecraft, have tended to stereotype genuine investigators of "close encounters" as soft-minded mystics. He summarizes George Adamski's complex dealings with benevolent aliens from the planet Venus, but with obvious reluctance, calling one tour of the solar system in alien spacecraft "remarkably tedious" and noting that some researchers (himself presumably included) saw him as "a shameless con man" (p. 118). But Adamski and other prophets of otherworldly saviors could instead be seen as proponents of an emergent American mythology that aims to replace the primitive Mesopotamian cosmology of *Genesis* with one that takes full advantage of recent scientific and technological perspectives on the universe.

So it is disappointing that the book has relatively little to say about possible theories of the actual dynamics of anomalous experience. One



wishes that at least one section could survey some of the academic research, especially in folkloristics, that might bear on the issue of how and why ordinary people could find themselves experiencing alternative realities, or entertaining those reported by others. He notes that in a recent book on the topic, "two prominent academic folklorists remark with no small hint of exasperation, 'It should be possible to believe one's informants without believing their explanations'" (p. 195). However, Clark does not identify or cite the two academics in question,[1] so the reader is unable to follow up on the remark or its larger intellectual context. To gain this, readers could consult the important work of folklorist Thomas E. Bullard, who has suggested an academic way of seeing experiential anomalies in terms of cultural myth-making. Bullard's recent book, *The Myth and Mystery of UFOs* (2011) covers many of the topics Clark summarizes in a much more analytic and insightful way. In so doing, he picks up a number of threads that were initially started by Brenda Denzler in her pioneering work, *The Lure of the Edge: Scientific Passions, Religious Beliefs, and the Pursuit of UFOs* (2001). And in noting the overlap between fairylore and encounters with space aliens, Clark might well have noted the provocative work of folklorist Peter Rojcewicz on the topic.[2] And overall he would profit from the important theoretical work of Jeffrey J. Kripal, who in works such as *Authors of the Impossible: The Paranormal and the Sacred* (2010) argues that participation in anomalous experience is a fundamental element in human consciousness and a cornerstone of religious activity.

The book is a good value, given that it is printed on glossy paper and has been produced carefully, with no visible typos or misspellings. While quotes are not cited or footnoted, each section is given a brief bibliography, and the Index, happily, is comprehensive. A number of production features are annoying, though. There are frequent illustrations, but many of them derive from clip art and show generic images that are, at best, tangentially related to the text. The most interesting are covers of science fiction journals from the Mary Evans Picture Library, a vast and intriguing compilation of paranormal art.

Overall, this book makes a good addition for a researcher who is interested in delving into the complex world of the spiritualist/contactee/folkloristic tangents of the UFO world. Still, one wishes the material had been presented for the sake of what it teaches us about the mythological possibilities of the human mind and the American experience, and not just for its own funky content.

Notes

[1] They are Lizanne Henderson and Edward J. Cowan, in *Scottish Fairy Belief: A History* (East Linton, U.K.: Tuckwell Press, 2001), p. 12. Henderson and Cowan continue,

> The stance taken in this study is that it is irrelevant whether or not fairies existed; what matters is that people believed in the reality of the phenomenon. The folklorist is thus interested, as should be the historian, in the "reality of the supranormal experience and not in the reality of paranormal phenomena."

In the last sentence, Henderson and Cowan are quoting UCLA folklorist Donald Ward ("The Little Man Who Wasn't There: Encounters with the Supranormal," *Fabula*, *18* (1977): 216).

[2] Notably his articles "Between One Eye Blink and the Next: Fairies, UFOs, and Problems of Knowledge," in *The Good People: New Fairy Lore Essays*, edited by Peter Narvaez (New York: Garland, 1991), pp. 479–514, and "Fairies, UFOs, and Problems of Knowledge," *Mythos Journal*, *5* (Fall 1995):69–80. Rojcewicz began his folkloristic career in 1980 when he was contacted by a sinister Man in Black in the library of his graduate school and told that "Flying saucers are the most important fact of the century." Far from a crank, he has enjoyed a distinguished academic career and currently serves as Vice-President of Academic Affairs and Dean of Faculty at Antioch University Seattle.

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June 20–23, 2012, Boulder, CO, Millennium Hotel

Bonfire of the Paradigms

Challenging Paradigms in the Health Sciences **Richard Olree**
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Challenging Paradigms in the Biological Sciences **Thomas Dykstra**
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Call for Contributed Papers*: Deadline is April 17th
SSE members are invited to contribute talks for presentation at the meeting both on topics related to the three subthemes and also on other topics. Titles and abstracts should run between 300–500 words and will be sent to Thomas Dykstra (dykstralabs@yahoo.com). The program will be listed at scientificexploration.org and emailed to registrants in May.

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Program Chair: Thomas Dykstra **Local Host**: Dominique Surel.

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Banquet: Saturday night's banquet will close the meeting.

**Contributed papers related to themes of the meeting will be included in the same session as the relevant invited talk when possible. Contributors should indicate what subtheme they wish to present in and preference will be given to those that fit. Because the number of talks may be larger than can be accommodated in oral sessions, some papers may be selected by the Program Committee to be presented as posters.*

A general requirement is that the contributed talks must be science based, especially if it is not obvious from the title. The proposed topic should cover an area not adequately covered in mainstream science. Preference will be given to Full Members in the Society. Non-members may submit a talk for consideration, but only if they are sponsored by a Full Member of the Society. The selection criteria additionally may include date of receipt of abstract as well as relevance to the themes of the conference and to topics generally covered at past SSE meetings. Contributed talks will be given 20 minutes. All speakers will be contacted about the option of being videotaped. If permission is granted, the talk may be eventually placed on the SSE website and as a YouTube video for public viewing.

Index of Previous Articles in the *Journal of Scientific Exploration*

Vol: No	Article	Author(s)
1:1	A Brief History of the Society for Scientific Exploration	P. Sturrock
	Aterations in Recollection of Unusual and Unexpected Events	D. Hall et al.
	Toward a Quantitative Theory of Intellectual Discovery (Esp. in Phys.)	R. Fowler
	Engineering Anomalies Research	R. Jahn et al.
	Common Knowledge about the Loch Ness Monster	H. Bauer
	An Analysis of the Condon Report on the Colorado UFO Project	P. Sturrock
1:2	The Strange Properties of Psychokinesis	H. Schmidt
	What Do We Mean by "Scientific?"	H. Bauer
	Analysis of a UFO Photograph	R. Haines
	Periodically Flashing Lights Filmed off the Coast of New Zealand	B. Maccabee
2:1	Commonalities in Arguments over Anomalies	H. Bauer
	Remote Viewing and Computer Communications—An Experiment	J. Vallee
	Is There a Mars Effect?	M. Gauquelin
	Raising the Hurdle for the Athletes' Mars Effect	S. Ertel
2:2	UFOs and NASA	R. Henry
	The Nature of Time	Y. Terzian
	Operator-Related Anomalies in a Random Mechanical Cascade	B. Dunne et al.
	Evidence for a Short-Period Internal Clock in Humans	T. Slanger
	Three New Cases of Reincarnation Types in Sri Lanka with Written Records	I. Stevenson et al.
3:1	Arguments Over Anomalies: H. \?Polemics	H. Bauer
	Anomalies: Analysis and Aesthetics	R. Jahn
	Trends in the Study of Out-of-Body Experiences	C. Alvarado
	A Methodology for the Objective Study of Transpersonal Imagery	W. Braud/ M. Schlitz
	The Influence of Intention on Random and Pseudorandom Events	D. Radin/J. Utts
	Case of Possession Type in India with Evidence of Paranormal Knowledge	I. Stevenson et al.
3:2	New Ideas in Science	T. Gold
	Photo Analysis of an Aerial Disc Over Costa Rica	R. Haines/J. Vallee
	Three Cases of Children in Northern India Who Remember a Previous Life	A. Mills
	"Signatures" in Anomalous Human–Machine Interaction Data	D. Radin
	A Case of Severe Birth Defects Possibly Due to Cursing	I. Stevenson
4:1	Biochemical Traumatology/Plant Metabolic Disorders in a UFO Landing	M. Bounias
	Return to Trans-en-Provence	J. Vallee
	Analysis of Anomalous Physical Traces: 1981 Trans-en-Provence UFO Case	J. Velasco
	Physical Interpretation of Very Small Concentrations	H. Bauer
	Luminous Phenomena and Seismic Energy in the Central United States	J. Derr/ M. Persinger
	Photo Analysis of an Aerial Disc Over Costa Rica: New Evidence	R. Haines/J. Vallee
	A Scientific Inquiry into the Validity of Astrology	J. McGrew/ R. McFall
	Planetary Influences on Human Behavior: Absurd for a Scientific Explanation?	A. Müller
	Five Arguments against Extraterrestrial Origin of Unidentified Flying Objects	J. Vallee
4:2	Using the Study of Anomalies To Enhance Critical Thinking in the Classroom	M. Swords
	Observations of Electromagnetic Signals Prior to California Earthquakes	M. Adams
	Bayesian Analysis of Random Event Generator Data	W. Jefferys
	Moslem Case of Reincarnation Type in Northern India: Analysis of 26 Cases	A. Mills
	Electromagnetic Disturbances Associated with Earthquakes	M. Parrot
	Extrasensory Interactions between Homo Sapiens and Microbes	C. Pleass/N. Dey
	Correlation between Mental Processes and External Random Events	H. Schmidt
	Phobias in Children Who Claim To Remember Previous Lives	I. Stevenson
	A Gas Discharge Device for Investigating Focused Human Attention	W. Tiller

	Radio Emissions from an Earthquake	J. Warwick
5:1	The Cydonian Hypothesis	J. Brandenburg et al.
	Cases in Burma, Thailand, and Turkey: Aspects of I. Stevenson's Research	J. Keil
	Effects of Consciousness on the Fall of Dice: A Meta-Analysis	D. Radin/D. Ferrari
	The Wasgo or Sisiutl: A Cryptozoological Sea-Animal	M. Swords
	The Extraterrestrial Hypothesis Is Not That Bad	R. Wood
	Toward a Second-Degree Extraterrestrial Theory of UFOs	J. Vallee
	Low-Frequency Emissions: Earthquakes and Volcanic Eruptions in Japan	T. Yoshino
5:2	Eccles's Model of Mind–Brain Interaction and Psychokinesis	W. Giroldini
	Ball Lightning and St. Elmo's Fire as Forms of Thunderstorm Activity	A. Grigor'ev et al.
	Social Scientific Paradigms for Investigating Anomalous Experience	J. McClenon
	Count Population Profiles in Engineering Anomalies Experiments	R. Jahn et al.
	Children Claiming Past-Life Memories: Four Cases in Sri Lanka	E. Haraldsson
6:1	Can the UFO Extraterrestrial Hypothesis and Vallee Hypotheses Be Reconciled?	W. Bramley
	Learning for Discovery: Establishing the Foundations	R. Domaingue
	On the Bayesian Analysis of REG Data (Response from W. Jefferys)	Y. Dobyns
	Electrodynamic Activities and Their Role in the Organization of Body Pattern	M. W. Ho et al.
6:2	Review of Approaches to the Study of Spontaneous Psi Experiences	R. White
	Survival or Super-Psi?: Interchange Responses	I. Stevenson/S. Braude
	The Psychokinesis Effect: Geomagnetic Influence, Age and Sex Differences	L. Gissurarson
	Are Reincarnation Type Cases Shaped by Parental Guidance?	S. Pasricha
6:3	Heim's Theory of Elementary Particle Structures	T. Auerbach
	Better Blood through Chemistry: A Laboratory Replication of a Miracle	M. Epstein
	The Gauquelin Effect Explained? Comments on Müller's Planetary Correlations	S. Ertel
	The Gauquelin Effect Explained? A Rejoinder to Ertel's Critique	A. Müller
	Ball Lightning Penetration into Closed Rooms: 43 Eyewitness Accounts	A. Grivor'ev et al.
	A Series of Possibly Paranormal Recurrent Dreams	I. Stevenson
6:4	Experiments in Remote Human/Machine Interaction	B. Dunne et al.
	A Low Light Level Diffraction Experiment for Anomalies Research	S. Jeffers et al.
	A New Look at Maternal Impressions: An Analysis of 50 Published Cases	I. Stevenson
	Alternative Healing Therapy on Regeneration Rate of Salamander Forelimbs	D. Wirth et al.
7:1	Accultured Topographical Effects of Shamanic Trance Consciousness	P. Devereux
	Mainstream Sciences vs. Parasciences: Toward an Old Dualism?	G. L. Eberlein
	Existence of Life and Homeostasis in an Atmospheric Environment	S. Moriyama
	A Guide to UFO Research	M. D. Swords
7:2	Non-Causality as the Earmark of Psi	H. Schmidt
	Adequate Epistemology for Scientific Exploration of Consciousness	W. W. Harman
	Puzzling Eminence Effects Might Make Good Sense	S. Ertel
	Comments on Puzzling Eminence Effects	J. W. Nienhuys
	A Systematic Survey of Near-Death Experiences in South India	S. Pasricha
	The Willamette Pass Oregon UFO Photo Revisited: An Explanation	I. Wieder
7:3	Near Death Experiences: Evidence for Life After Death?	M. Schröter-Kunhardt
	Analysis of the May 18, 1992, UFO Sighting in Gulf Breeze, Florida	B. Maccabee
	Selection Versus Influence in Remote REG Anomalies	Y. Dobyns
	Dutch Investigation of the Gauquelin Mars Effect	J. Nienhuys
	Comments on Dutch Investigations of the Gauquelin Mars Effect	S. Ertel
	What Are Subtle Energies?	W. Tiller
7:4	Explaining the Mysterious Sounds Produced by Very Large Meteor Fireballs	C. S. L. Keay
	Neural Network Analyses of Consciousness-Related Patterns	D. I. Radin
	Applied Parapsychology: Studies of Psychics and Healers	S. A. Schouten
	Birthmarks and Birth Defects Corresponding to Wounds on Deceased Persons	I. Stevenson

	The "Enemies" of Parapsychology	R. McConnell
8:1	Survey of the American Astronomical Society Concerning UFOs: Part 1	P. Sturrock
	Anatomy of a Hoax: The Philadelphia Experiment Fifty Years Later	J. Vallee
	Healing and the Mind: Is There a Dark Side?	L. Dossey
	Alleged Experiences Inside UFOs: An Analysis of Abduction Reports	V. Ballester Olmos
	What I See When I Close My Eyes	R. Targ
8:2	Survey of the American Astronomical Society Concerning UFOs: Part 2	P. Sturrock
	Series Position Effects in Random Event Generator Experiments	B. Dunne et al.
	Re-Examination of the Law of Conservation of Mass in Chemical Reactions	K. Volkamer et al.
	The 'Genius Hypothesis': Exploratory Concepts for Creativity	E. Laszlo
8:3	Survey of the American Astronomical Society Concerning UFOs: Part 3	P. Sturrock
	Strong Magnetic Field Detected Following a Sighting of an UFO	B. Maccabee
	Complementary Healing Therapy for Patients with Type I Diabetes Mellitus	D. P. Wirth
	Report of an Indian Swami Claiming to Materialize Objects	E. Haraldsson
8:4	Scientific Analysis of Four Photos of a Flying Disk Near Lac Chauvet, France	Pierre Guérin
	A Linear Pendulum Experiment: Operator Intention on Damping Rate	R. D. Nelson
	Applied Scientific Inference	P. A. Sturrock
	The Mind-Brain Problem	J. Beloff
9:1	Unconventional Water Detection: Field Test of Dowsing in Dry Zones: Part 1	H. Betz
	Digital Video Analysis of Anomalous Space Objects	M. Carlotto
	The Critical Role of Analytical Science in the Study of Anomalies	M. Epstein
	Near-Death Experiences in South India: A Systematic Survey	S. Pasricha
	Human Consciousness Influence on Water Structure	L. Pyatnitsky/ V. Fonkin
9:2	Unconventional Water Detection: Field Test of Dowsing in Dry Zones: Part 2	H. Betz
	Semi-molten Meteoric Iron Associated with a Crop Formation	W. Levengood/MJ. Burke
	Experiments on a Possible g-Ray Emission Caused by a Chemical Process	V. Noninski et al.
	The Effect of Paranormal Healing on Tumor Growth	F. Snel/ P. van der Sijde
	Psychokinetic Action of Young Chicks on the Path of an Illuminated Source	R. Peoc'h
	Eddington's Thinking on the Relation between Science and Religion	A. Batten
	Two Kinds of Knowledge: Maps and Stories	H. Bauer
9:3	Experiments on Claimed Beta Particle Emission Decay	V. Noninski et al.
	Assessing Commonalities in Randomly Paired Individuals	T. Rowe et al.
	Anomalously Large Body Voltage Surges on Exceptional Subjects	W. Tiller et al.
	Six Modern Apparitional Experiences	I. Stevenson
	Viewing the Future: A Pilot Study with an Error-Detecting Protocol	R. Targ et al.
	Could Extraterrestrial Intelligences Be Expected to Breathe Our Air?	M. Swords
9:4	Decision Augmentation Theory: Applications to Random Number Generators	E. May
	Extrasensory Perception of Subatomic Particles & Referee Interchange (Dobyns)	S. Phillips
	North American Indian Effigy Mounds	A. Apostol
	A Holistic Aesthetic for Science	B. Kirchoff
10:1	An Assessment of the Evidence for Psychic Functioning	J. Utts
	Evaluation of a Program on Anomalous Mental Phenomena	R. Hyman
	CIA-Initiated Remote Viewing Program at Stanford Research Institute	H. Puthoff
	Remote Viewing at Stanford Research Institute in the 1970s: A Memoir	R. Targ
	American Institutes for Research Review of the STAR GATE Program	E. May
	FieldREG Anomalies in Group Situations	R. Nelson et al.
	Anomalous Organization of Random Events by Group Consciousness	D. Radin et al.
10:2	Critical Review of the "Cold Fusion" Effect	E. Storms
	Do Nuclear Reactions Take Place Under Chemical Stimulation?	J. Bockris et al.
	Claimed Transmutation of Elements Caused by a Chemical Process	V. Noninski et al.

	Selection versus Influence Revisited: New Methods and Conclusions	Y. Dobyns
	Illegitimate Science? A Personal Story	B. Maccabee
	Anomalous Phenomena Observed in the Presence of a Brazilian "Sensitive"	S. Krippner et al.
10:3	Mass Modification Experiment Definition Study	R. Forward
	Atmospheric Mass Loss on Mars and the Consequences	H. Lammer
	Exploring Correlations between Local Emotional and Global Emotional Events	D. Bierman
	Archetypes, Neurognosis and the Quantum Sea	C. Laughlin
10:4	Distance Healing of Patients with Major Depression	B. Greyson
	Cases of the Reincarnation Type: Evaluation of Some Indirect Evidence	J. Keil
	Enhanced Congruence between Dreams and Distant Target Material	S. Krippner et al.
	Recent Responses to Survival Research (Responses by Braude & Wheatley)	R. Almeder
	Toward a Philosophy of Science in Women's Health Research	A. Lettieri
11:1	Biased Data Selection in Mars Effect Research	S. Ertel/K. Irving
	Is the "Mars Effect" Genuine?	P. Kurtz et al.
	Fortean Phenomena on Film: Evidence or Artifact?	R. Lange/J. Houran
	Wishing for Good Weather: A Natural Experiment in Group Consciousness	R. Nelson
	Empirical Evidence for a Non-Classical Experimenter Effect	H. Walach/ S. Schmidt
	Consciousness, Causality, and Quantum Physics	D. Pratt
11:2	Anomalous Cognition Experiments and Local Sidereal Time	S. J. P. Spottiswoode
	Evidence that Objects on Mars are Artificial in Origin	M. Carlotto
	The Astrology of Time Twins: A Re-Analysis & Referee Interchange (Roberts)	C. French et al.
	Unconscious Perception of Future Emotions: An Experiment in Presentiment	D. Radin
	A Bayesian Maximum-Entropy Approach to Hypothesis Testing	P. Sturrock
	Planetary Diameters in the Surya-Siddhanta	R. Thompson
	Science of the Subjective	R. Jahn/B. Dunne
11:3	Accessing Anomalous States of Consciousness with Binaural Beat Technology	F. Holmes Atwater
	The "Mars Effect" As Seen by the Committee PARA	J. Dommanget
	Astrology and Sociability: A Comparative Psychological Analysis	S. Fuzeau-Braesch
	Comparison between Children with and without Previous-Life Memories	E. Haraldsson
	Did Life Originate in Space? Discussion of Implications of Recent Research	A. Mugan
	Correlations of Random Binary Sequences with Pre-Stated Operator Intention	R. Jahn et al.
	The Hidden Side of Wolfgange Pauli: An Encounter with Depth Psychology	Atmanspacher/ Primas
11:4	Topographic Brain Mapping of UFO Experiencers	N. Don/G. Moura
	Toward a Model Relating Empathy, Charisma, and Telepathy	J. Donovan
	The Zero-Point Field and the NASA Challenge of Create the Space Drive	B. Haisch/A. Rueda
	Motivation and Meaningful Coincidence: Further Examination of Synchronicity	T. Rowe et al.
	A Critique of Arguments Offered against Reincarnation	R. Almeder
	The Archaeology of Consciousness	P. Devereux
12:1	Gender Differences in Human/Machine Anomalies	B. Dunne
	Statement Validity Analysis of "Jim Ragsdale Story": Roswell Implications	J. Houran/S. Porter
	Experiment Effects in Scientifi c Research: How Widely Are They Neglected?	R. Sheldrake
	Roswell—Anatomy of a Myth	K. Jeffery
	A Different View of "Roswell—Anatomy of a Myth"	M. Swords
	Critique of "Roswell—Anatomy of a Myth"	R. Woods
12:2	Physical Evidence Related to UFO Reports	P. A. Sturrock et al.
	Empirical Evidence Against Decision Augmentation Theory	Y. Dobyns/R. Nelson
	Cases of Reincarnation in Northern India with Birthmarks and Birth Defects	S. Pasricha
	Can the Vacuum Be Engineered for Spaceflight Applications? Overview.	H. E. Puthoff
	Four Paradoxes Involving the Second Law of Thermodynamics	D. Sheehan
	The Paranormal Is Not Excluded from Physics	O. Costa de Beauregard

12:3	Estimates of Optical Power Output in Six Cases of Unexplained Aerial Objects	J. Vallee
	Analyses in Ten Cases of Unexplained Aerial Objects with Material Samples	J. Vallee
	Do Near-Death Experiences Provide Evidence for Survival of Human Personality	E. Cook et al.
	Anomalous Statistical Influence Depends on Details of Random Process	M. Ibison
	FieldREG II: Consciousness Field Effects: Replications and Explorations	R. D. Nelson et al.
	Biological Effects of Very Low Frequency (VLF) Atmospherics in Humans	A. Schienle et al.
12:4	The Timing of Conscious Experience: Causality-Violating	F. A. Wolf
	Double-Slit Diffraction Experiment of Investigate Consciousness Anomalies	M. Ibison/S. Jeffers
	Techno-Dowsing: A Physiological Response System to Improve Psi Training	P. Stevens
	Physical Measurement of Episodes of Focused Group Energy	W. Rowe
	Experimental Studies of Telepathic Group Communication of Emotions	J. Dalkvist/ Westerlund
	Strategies for Dissenting Scientists	B. Martin
13:1	Significance Levels for the Assessment of Anomalous Phenomena	R. A. J. Matthews
	Retrotransposons as Engines of Human Bodily Transformation	C. A. Kelleher
	A Rescaled Range Analysis of Random Events	F. Pallikari/E. Boller
	Subtle Domain Connections to the Physical Domain Aspect of Reality	W. A. Tiller
	Parapsychology in Intelligence: A Personal Review and Conclusions	K. A. Kress
	Dreaming Consciousness: More Than a Bit Player in the Mind/Body Problem	M. Ullman
13:2	The Effect of "Healing with Intent" on Pepsin Enzyme Activity	T. Bunnell
	Electronic Device-Mediated pH Changes in Water	W. Dibble/W. Tiller
	Variations on the Foundations of Dirac's Quantum Physics	J. Edmonds
	Do Cases of the Reincarnation Type Show Similar Features over Many Years?	J. Keil/I. Stevenson
	Optical Power Output of an Unidentifi ed High Altitude Light Source	B. Maccabee
	Registration of Actual and Intended Eye Gaze: Correlation with Spiritual Beliefs	G. Schwartz/ L. Russek
	Real Communication? Report on a SORRAT Letter-Writing Experiment	I. Grattan-Guinness
	What are the Irreducible Components of the Scientific Enterprise?	I. Stevenson
	Anomalies in the History of Relativity	I. McCausland
	Magic of Signs: A Nonlocal Interpretation of Homeopathy	H. Walach
13:3	Second Sight and Family History: Pedigree and Segregation Analyses	S. Cohn
	Mound Configurations on the Martian Cydonia Plain	H. Crater/ S. McDaniel
	Geomorphology of Selected Massifs on the Plains of Cydonia, Mars	D. Pieri
	Atmosphere or UFO? A Response to the 1997 SSE Review Panel Report	B. Maccabee
	An Unusual Case of Stigmatization	M. Margnelli
	Methuselah: Oldest Myth. or Oldest Man?	L. McKague
	Analysis of Technically Inventive Dream-Like Mental Imagery	B. Towe/ Randall-May
	Exploring the Limits of Direct Mental Influence: Two Studies	C. Watt et al.
13:4	Experimental Systems in Mind–Matter Research	R. Morris
	Basic Elements and Problems of Probability Theory	H. Primas
	The Significance of Statistics in Mind–Matter Research	R. Utts
	Introductory Remarks on Large Deviations Statistics	Amann/ Atmanspacher
	p-adic Information Spaces. Small Probabilities and Anomalous Phenomena	A. Khrennikov
	Towards an Understanding of the Nature of Racial Prejudice	Hoyle/ Wickramasinghe
	Clyde Tombaugh, Mars and UFOs	M. Swords
14:1	Investigating Deviations from Dynamical Randomness with Scaling Indices	Atmanspacher et al.
	Valentich Disappearence: New Evidence and New Conclusion	R. Haines/P. Norman
	Protection of Mice from Tularemia with Ultra-Low Agitated Dilutions	W. Jonas/D. Dillner

	The Correlation of the Gradient of Shannon Entropy and Anomalous Cognition	Spottiswoode/Faith
	Contributions to Variance in REG Experiments: ANOVA Models	R. Nelson et al.
	Publication Bias: The "File-Drawer" Problem in Scientific Inference	J. Scargle
	Remote Viewing in a Group Setting	R. Targ/J. Katra
14:2	Overview of Several Theoretical Models on PEAR Data	Y. Dobyns
	The Ordering of Random Events by Emotional Expression	R. Blasband
	Energy, Fitness and Information-Augmented EMFs in *Drosophila melanogaster*	M. Kohane/ W. Tiller
	A Dog That Seems To Know When His Owner Is Coming Home	R. Sheldrake/ P. Smart
	What Can Elementary Particles Tell Us about the World in Which We Live?	R. Bryan
	Modern Physics and Subtle Realms: Not Mutually Exclusive	R. Klauber
14:3	Plate Tectonics: A Paradigm Under Threat	D. Pratt
	The Effect of the "Laying On of Hands" on Transplanted Breast Cancer in Mice	Bengston/Krinsley
	Stability of Assessments of Paranormal Connections in Reincarnation Type Cases	I. Stevenson/J. Keil
	ArtREG: A Random Event Experiment Utilizing Picture-Preference Feedback	R. G. Jahn et al.
	Can Population Growth Rule Out Reincarnation?	D. Bishai
	The Mars Effect Is Genuine	S. Ertel/K. Irving
	Bulky Mars Effect Hard To Hide	S. Ertel
	What Has Science Come to?	H. Arp
14:4	Mind/Machine Interaction Consortium: PortREG Replication Experiments	Jahn/Mischo/ Vaitl et al.
	Unusual Play in Young Children Who Claim to Remember Previous Lives	I. Stevenson
	A Scale to Measure the Strength of Children's Claims of Previous Lives	J. B. Tucker
	Reanalysis of the 1965 Hefl in UFO Photos	Druffel/Wood/ Kelson
	Should You Take Aspirin To Prevent Heart Attack?	J. M. Kauffman
15:1	The Biomedical Significance of Homocysteine	K. McCully
	20th and 21st Century Science: Reflections and Projections	R. G. Jahn
	To Be Or Not To Be! A 'Paraphysics' for the New Millennium	J. E. Beichler
	Science of the Future in Light of Alterations of Consciousness	I. Barušs
	Composition Analysis of the Brazil Magnesium	P. A. Sturrock
	Does Recurrent ISP Involve More Than Congnitive Neuroscience?	J.-C. Terrillon/ S. Marques Bonham
15:2	The Scole Investigation: Critical Analysis of Paranormal Physical Phenomena	M. Keen
	Bio-photons and Bio-communication	R. VanWijk
	Scalar Waves: Theory and Experiments	K. Meyl
	Commentary: On Existence of K. Meyl's Scalar Waves	G. W. Bruhn
	Cases of the Reincarnation Type in South India: Why So Few Reports?	S. K. Pasricha
	Mind, Matter, and Diversity of Stable Isotopes	J. P. Pui/A. A. Berezin
	Are the Apparitions of Medjugorge Real?	J. P. Pandarakalam
	Where Do We File 'Flying Saucers'? Archivist and Uncertainty Principle	H. Evans
	The Bakken: A Library and Museum of Electricity in Life	D. Stillings
15:3	A Modular Model of Mind/Matter Manifestations (M5)	R. G. Jahn/B. J. Dunne
	The Speed of Thought: Complex Space–Time Metric and Psychic Phenomenon	E. A. Rauscher/ R. Targ
	Failure to Replicate Electronic Voice Phenomenon	I. Barušs
	Experimental Study on Precognition	Vasilescu/Vasilescu
	Unexplained Temporal Coincidence of Crystallization	Constain/Davies
15:4	The Challenge of Consciousness	R. G. Jahn

	Anomalies and Surprises	H. H. Bauer
	Earth Geodynamic Hypotheses Updated	N. C. Smoot
	Unexplained Weight Gain Transients at the Moment of Death	L. E. Hollander, Jr.
	Physico-Chemical Properties of Water Following Exposure to Resonant Circuits	C. Cardella et al.
16:1	Can Physics Accommodate Clairvoyance, Precognition, and Psychokinesis?	R. Shoup
	The Pineal Gland and the Ancient Art of Iatromathematica	F. McGillion
	Confounds in Deciphering the Ramey Memo from the Roswell UFO Case	J. Houran/ K. D. Randle
	The Pathology of Organized Skepticism	L. D. Leiter
	Aspects of the Wave Mechanics of Two Particles in a Many Body Quantum System	Y. S. Jain
	Microscopic Theory of a System of Interacting Bosons: A Unifying New Approach	Y. S. Jain
	Unification of the Physics of Interacting Bosons and Fermions	Y. S. Jain
	The Pathology of Organized Skepticism	L. D. Leiter
16:2	Arguing for an Observational Theory of Paranormal Phenomena	J. M. Houtkooper
	Differential Event-Related Potentials to Targets and Decoys in Guessing Task	McDonough/Don/ Warren
	Stigmatic Phenomena: An Alleged Case in Brazil	S. Krippner
	The Case for the Loch Ness "Monster": The Scientific Evidence	H. H. Bauer
	What's an Editor To Do?	H. H. Bauer
16:3	M*: Vector Representation of the Subliminal Seed Regime of M5	R. G. Jahn
	Can Longitudinal Electromagnetic Waves Exist?	G. W. Bruhn
	Development of Certainty about the Deceased in Reincarnation Case in Lebanon	Haraldsson/ Izzeddin
	Manifestation and Effects of External Qi of Yan Xin Life Science Technology	Yan et al.
	Face-Like Feature at West Candor Chasma, Mars MGS Image AB 108403	Crater/Levasseur
	A Search for Anomalies	W. R. Corliss
	Common Knowledge about the Loch Ness Monster: Television, Videos, and Film	H. H. Bauer
16:4	Relationships Between Random Physical Events and Mass Human Attention	D. Radin
	Coherent Consciousness and Reduced Randomness: Correlations on 9/11/2001	R. D. Nelson
	Was There Evidence of Global Consciousness on September 11, 2001?	J. Scargle
	A Dog That Seems To Know When His Owner Is Coming Home	D. Radin
	An Investigation on the Activity Pattern of Alchemical Transmutations	J. Pérez-Pariente
	Anomalies in Relativistic Rotation	R. D. Klauber
	The Vardøgr, Perhaps Another Indicator of the Non-Locality of Consciousness	L. D. Leiter
	Review of the Perrott-Warrick Conference Held at Cambridge 3–5 April 2000	B. Carr
	Wavelike Coherence and CPT Invariance: Sesames of the Paranormal	O. Costa de Beauregard
	Why Only 4 Dimensions Will Not Explain Relationships in Precognition	Rauscher/Targ
17:1	Problems Reporting Anomalous Observations in Anthropology	C. Richards
	The Fringe of American Archaeology	A. B. Kehoe
	Rocks That Crackle and Sparkle and Glow: Strange Pre-Earthquake Phenomena	F. T. Freund
	Poltergeists, Electromagnetism and Consciousness	W. G. Roll
	AIDS: Scientific or Viral Catastrophe?	N. Hodgkinson
17:2	Information and Uncertainty in Remote Perception Research	B. J. Dunne/R. G. Jahn
	Problems of Reproducibility in Complex Mind–Matter Systems	H. Atmanspacher
	Parapsychology: Science or Pseudo-Science?	M.-C. Mousseau
	The Similarity of Features of Reincarnation Type Cases Over Many Years: A Third Study	I. Stevenson/ E. Haraldsson
	Communicating with the Dead: The Evidence Ignored. Why Paul Kurtz is Wrong	M. Keen
	Purported Anomalous Perception in a Highly Skilled Individual: Observations, Interpretations, Compassion	G. E. Schwartz/ L. A. Nelson/L. G Russek

	Proof Positive—Loch Ness Was an Ancient Arm of the Sea	F. M. Dougherty
17:3	Radiation Hormesis: Demonstrated, Deconstructed, Denied, Dismissed, and Some Implications for Public Policy	J. M. Kauffman
	Video Analysis of an Anomalous Image Filmed during Apollo 16	H. Nakamura
	The Missing Science of Ball Lightning	D. J. Turner
	Pattern Count Statistics for the Analysis of Time Series in Mind–Matter Studies	W. Ehm
	Replication Attempt: No Development of pH or Temperature Oscillations in Water Using Intention Imprinted Electronic Devices	L. I. Mason/ R. P. Patterson
	Three Cases of the Reincarnation Type in the Netherlands	T. Rivas
17:4	Testing a Language-Using Parrot for Telepathy	R. Sheldrake/A. Morgana
	Skin Conductance Prestimulus Response: Analyses, Artifacts and a Pilot Study	S. J. P. Spottiswode /E. C. May
	Effects of Frontal Lobe Lesions on Intentionality and Random Physical Phenomena	M. Freedman/S. Jeffers/K. Saeger/
	Physical Phenomena	/M. Binns/S. Black
	The Use of Music Therapy as a Clinical Intervention for Physiologist	D. S. Berger/
	Functional Adaptation Media Coverage of Parapsychology and the Prevalence of Irrational Beliefs	D. J. Schneck/ M.-C. Mousseau
	The Einstein Mystique	I. McCausland
18:1	A Retrospective on the *Journal of Scientific Exploration*	B. Haisch/M. Sims
	Anomalous Experience of a Family Physician	J. H. Armstrong, Sr.
	Historical Overview & Basic Facts Involved in the Sasquatch or Bigfoot Phenomenon	J. Green
	The Sasquatch: An Unwelcome and Premature Zoological Discovery?	J. A. Bindernagel
	Midfoot Flexibility, Fossil Footprints, and Sasquatch Steps: New Perspectives on the Evolution of Bipedalism	D. J. Meldrum
	Low-Carbohydrate Diets	J. M. Kauffman
18:2	Analysis of the Columbia Shuttle Disaster— Anatomy of a Flawed Investigation in a Pathological Organization	J. P. MacLean/ G. Campbell/ S. Seals
	Long-Term Scientific Survey of the Hessdalen Phenomenon	M. Teodorani
	Electrodermal Presentiments of Future Emotions	D. I. Radin
	Intelligent Design: Ready for Prime Time?	A. D. Gishlick
	On Events Possibly Related to the "Brazil Magnesium"	P. Kaufmann/ P. A. Sturrock
	Entropy and Subtle Interactions	G. Moddel
	"Can a Single Bubble Sink a Ship?"	D. Deming
18:3	The MegaREG Experiment	Y. H. Dobyns et al.
	Replication and Interpretation Time-Series Analysis of a Catalog of UFO Events: Evidence of a Local-Sidereal-Time Modulation	P. A. Sturrock
	Challenging Dominant Physics Paradigms	J. M. Campanario/ B. Martin
	Ball Lightning and Atmospheric Light Phenomena: A Common Origin?	T. Wessel-Berg
18:4	Sensors, Filters, and the Source of Reality	R. G. Jahn/ B. J. Dunne
	The Hum: An Anomalous Sound Heard Around the World	D. Deming
	Experimental Test of Possible Psychological Benefits of Past-Life Regression	K. Woods/I. Barušs
	Inferences from the Case of Ajendra Singh Chauhan: The Effect of Parental Questioning, of Meeting the "Previous Life" Family, an Attempt To Quantify Probabilities, and the Impact on His Life as a Young Adult	A. Mills
	Science in the 21st Century: Knowledge Monopolies and Research Cartels	H. H. Bauer
	Organized Skepticism Revisited	L. D. Leiter

19:1	The Effect of a Change in Pro Attitude on Paranormal Performance: A Pilot Study Using Naive and Sophisticated Skeptics	L. Storm/ M. A. Thalbourne
	The Paradox of Planetary Metals	Y. Almirantis
	An Integrated Alternative Conceptual Framework to Heat Engine Earth, Plate Tectonics, and Elastic Rebound	S. T. Tassos/ D. J. Ford
	Children Who Claim to Remember Previous Lives: Cases with Written Records Made before the Previous Personality Was Identified	H. H. Jürgen Keil/ J. B. Tucker
19:2	Balls of Light: The Questionable Science of Crop Circles	F. Grassi/C. Cocheo/ P. Russo
	Children of Myanmar Who Behave like Japanese Soldiers: A Possible Third Element in Personality	I. Stevenson/J. Keil
	Challenging the Paradigm	B. Maccabee
	The PEAR Proposition	R. G. Jahn/B. J. Dunne
	Global Warming, the Politicization of Science, and Michael Crichton's State of Fear	D. Deming
19:3	A State of Belief Is a State of Being	Charles Eisenstein
	Anomalous Orbic "Spirit" Photographs? A Conventional Optical Explanation	G. E. Schwartz/ K. Creath
	Some Bodily Malformations Attributed to Previous Lives	S. K. Pasricha et al.
	A State of Belief Is a State of Being	C. Eisenstein
	HIV, As Told by Its Discoverers	H. H. Bauer
	Kicking the Sacred Cow: Questioning the Unquestionable and Thinking the Impermissible	H. H. Bauer
19:4	Among the Anomalies	J. Clark
	What Biophoton Images of Plants Can Tell Us about Biofields and Healing	K. Creath/ G. E. Schwartz
	Demographic Characteristics of HIV: I. How Did HIV Spread?	H. H. Bauer
20:1	Half a Career with the Paranormal	I. Stevenson
	Pure Inference with Credibility Functions	M. Aickin
	Questioning Answers on the Hessdalen Phenomenon	M. Leone
	Hessdalen Research: A Few Non-Questioning Answers	M. Teodorani
	Demographic Characteristics of HIV: II. How Did HIV Spread	H. H. Bauer
	Organized Opposition to Plate Techtonics: The New Concepts in Global Tectonics Group	D. Pratt
20:2	Time-Normalized Yield: A Natrual Unit for Effect Size in Anomalies Experiments	R. D. Nelson
	The Relative Motion of the Earth and the Ether Detected	S. J. G. Gift
	A Unified Theory of Ball Lightning and Unexplained Atmospheric Lights	P. F. Coleman
	Experimenter Effects in Laboratory Tests of ESP and PK Using a Common Protocol	C. A. Roe/ R. Davey/P. Stevens
	Demographic Characteristics of HIV: III. Why Does HIV Discriminate by Race	H. H. Bauer
20:3	Assessing the Evidence for Mind–Matter Interaction Effects	D. Radin et al.
	Experiments Testing Models of Mind–Matter Interaction	D. Radin
	A Critique of the Parapsychological Random Number Generator Meta-Analyses of Radin and Nelson	M. H. Schub
	Comment on: "A Critique of the Parapsychological Random Number Generator Meta-Analyses of Radin and Nelson"	J. D. Scargle
	The Two-Edged Sword of Skepticism: Occam's Razor and Occam's Lobotomy	H. H. Bauer
20:4	Consciousness and the Anomalous Organization of Random Events: The Role of Absorption	L. A. Nelson/ G. E.Schwartz
	Ufology: What Have We Learned?	M. D. Swords
21:1	Linking String and Membrane Theory to Quantum Mechanics & Special Relativity Equations, Avoiding Any Special Relativity Assumptions	M. G. Hocking

	Response of an REG-Driven Robot to Operator Intention	R. G. Jahn et al.
	Time-Series Power Spectrum Analysis of Performance in Free Response Anomalous Cognition Experiments	P. A. Sturrock/ S. J. Spottiswoode
	A Methodology for Studying Various Interpretations of the N,N-dimethyltryptamine-Induced Alternate Reality	M. A. Rodriguez
	An Experimental Test of Instrumental Transcommunication	I. Barušs
	An Analysis of Contextual Variables and the Incidence of Photographic Anomalies at an Alleged Haunt and a Control Site	D. B. Terhune et al.
	The Function of Book Reviews in Anomalistics	G. H. Hövelmann
	Ockham's Razor and Its Improper Use	D. Gernert
	Science: Past, Present, and Future	H. H. Bauer
21:2	The Role of Anomalies in Scientific Exploration	P. A. Sturrock
	The Yantra Experiment	Y. H. Dobyns et al.
	An Empirical Study of Some Astrological Factors in Relation to Dog Behaviour Differences by Statistical Analysis & Compared with Human Characteristics	S. Fuzeau-Braesch/ J.-B. Denis
	Exploratory Study: The Random Number Generator and Group Meditation	L. I. Mason et al.
	Statistical Consequences of Data Selection	Y. H. Dobyns
21:3	Dependence of Anomalous REG Performance on Run length	R. G. Jahn/ Y. H. Dobyns
	Dependence of Anomalous REG Performance on Elemental Binary Probability	R. G. Jahn/ J. C. Valentino
	Effect of Belief on Psi Performance in a Card Guessing Task	K. Walsh/ G. Moddel
	An Automated Online Telepathy Test	R. Sheldrake/ M. Lambert
	Three Logical Proofs: The Five-Dimensional Reality of Space–Time	J. E. Beichler
	Children Who Claim to Remember Previous Lives: Past, Present, & Future Research	J. B. Tucker
	Memory and Precognition	J. Taylor
	AIDS, Cancer and Arthritis: A New Perspective	N. Hodgkinson
	Online Historical Materials about Psychic Phenomena	C. S. Alvarado
21:4	Synthesis of Biologically Important Precursors on Titan Sam	H. Abbas/
	Is the Psychokinetic Effect as Found with Binary Random Number Generators Suitable to Account for Mind–Brain Interaction?	D. Schulze-Makuch/ Wolfgang Helfrich
	Explorations in Precognitive Dreaming	Dale E. Graff
	Climate Change Reexamined	Joel M. Kauffman
	Franklin Wolff's Mathematical Resolution of Existential Issues	Imants Barušs
	From Healing to Religiosity	Kevin W. Chen
22:1	Theme and Variations: The Life and Work of Ian Stevenson	Emily Williams Kelly/ Carlos S. Alvarado
	Ian Stevenson: Recollections	Kerr L. White
	Reflections on the Life and Work of Ian Stevenson	Alan Gauld
	Ian Stevenson and Cases of the Reincarnation Type	Jim B. Tucker
	Ian Stevenson and the Modern Study of Spontaneous ESP Experiences	Carlos S. Alvarado/ Nancy L. Zingrone
	Ian Stevenson's Contributions to Near-Death Studies	Bruce Greyson
	Ian Stevenson's Contributions to the Study of Mediumship	Erlendur Haraldsson
	Where Science and Religion Intersect: The Work of Ian Stevenson	Edward F. Kelly/ Emily Williams Kelly
	The Gentle American Doctor	M.M. Abu-Izzeddin

	Professor Ian Stevenson—Some Personal Reminiscences	Mary Rose Barrington
	Ian Stevenson: A Recollection and Tribute	Stephen E. Braude
	Ian Stevenson and His Impact on Foreign Shores	Bernard Carr
	Ian Stevenson: Gentleman and Scholar	Lisette Coly
	The Quest for Acceptance	Stuart J. Edelstein
	Ian Stevenson: Founder of the Scientific Investigation of Human Reincarnation	Doris Kuhlmann-Wilsdorf
	Remembering My Teacher	L. David Leiter
	Comments on Ian Stevenson, M.D., Director of the Division of Personality Studies and Pioneer of Reincarnation Research	Antonia Mills
	Ian Stevenson: Reminiscences and Observations	John Palmer
	Dr. Ian Stevenson: A Multifaceted Personality	Satwant K. Pasricha
	A Good Question	Tom Shroder
	The Fight for the Truth	John Smythies
	Ian Stevenson: A Man from Whom We Should Learn	Rex Stanford
	Ian Stevenson and the Society for Scientific Exploration	Peter A. Sturrock
	Ian Stevenson's Early Years in Charlottesville	Ruth B. Weeks
	Tribute to a Remarkable Scholar	Donald J. West
	An Ian Stevenson Remembrance	Ray Westphal
22:2	Meditation on Consciousness	I. Ivtzan
	An Exploration of Degree of Meditation Attainment in Relation to Psychic Awareness with Tibetan Buddhists	S. M. Roney-Dougal/ J. Solfvin/J. Fox
	Thematic Analysis of Research Mediums' Experiences of Discarnate Communcation	A. J. Rock/J Beischel/ G. E. Schwartz
	Change the Rules!	R. G. Jahn/ B. J. Dunne
	Proposed Criteria for the Necessary Conditions for ShamanicJourneying Imagery	A. J. Rock/S. Krippner
	"Scalar Wave Effects according to Tesla" & "Far Range Transponder"by K. Meyl	D. Kühlke
	How to Reject Any Scientific Manuscript	D. Gernert
22:3	Unusual Atmospheric Phenomena Observed Near the Channel Islands, United Kingdom, 23 April 2007	J.-F. Baure/ D. Clarke/ P. Fuller/M. Shough
	The GCP Event Experiment: Design, Analytical Methods, Results	P. Bancel/R. Nelson
	New Insights into the Links between ESP and Geomagnetic Activity	Adrian Ryan
	Phenomenology of N,N-Dimethyltryptamine Use: A Thematic Analysis	C. Cott/A. Rock
	Altered Experience Mediates the Relationship between Schizotypy and Mood Disturbance during Shamanic-Like Journeying	A. Rock/G. Abbott/ N. Kambouropoulos
	Persistence of Past-Life Memories: Study of Adults Who Claimed in Their Childhood To Remember a Past Life	E. Haraldsson
22:4	Energy, Entropy, and the Environment (How to Increase the First by Decreasing the Second to Save the Third)	D. P. Sheehan
	Effects of Distant Intention on Water Crystal Formation: A Triple-Blind Replication	D. Radin/N. Lund/ M. Emoto/T. Kizu
	Changes in Physical Strength During Nutritional Testing	C. F. Buhler/ P. R. Burgess/ E. VanWagoner
	Investigating Scopesthesia: Attentional Transitions, Controls and Error Rates in Repeated Tests	Rupert Sheldrake/ Pamela Smart
	Shakespeare: The Authorship Question, A Bayesian Approach	P. A. Sturrock

	An Anomalous Legal Decision	Richard A. Blasband
23:1	A New Experimental Approach to Weight Change Experiments at the Moment of Death with a Review of Lewis E. Hollander's Experiments on Sheep	Masayoshi Ishida
	An Automated Test for Telepathy in Connection with Emails	R. Sheldrake/ L. Avraamides
	Brain and Consciousness: The Ghost in the Machines	John Smythies
	In Defense of Intuition: Exploring the Physical Foundations of Spontaneous Apprehension	Ervin Laszlo
23:2	Appraisal of Shawn Carlson's Renowned Astrology Tests	Suitbert Ertel
	A Field-Theoretic View of Consciousness: Reply to Critics	D. W. Orne-Johnson/ Robert M. Oates
	Super-Psi and the Survivalist Interpretation of Mediumship	Michael Sudduth
	Perspectival Awareness and Postmortem Survival	Stephen E. Braude
23:3	Exploratory Evidence for Correlations between Entrained Mental Coherence and Random Physical Systems	Dean Radin/ F. Holmes Atwater
	Scientific Research between Orthodoxy and Anomaly	Harald Atmanspacher
23:4	Cold Fusion: Fact or Fantasy?	M. E. Little/S. R. Little
	"Extraordinary Evidence" Replication Effort	M. E. Little/S. R. Little
	Survey of the Observed Excess Energy and Emissions in Lattice-Assisted Nuclear Reactions	Mitchell R. Swartz
24:1	Rebuttal to Claimed Refutations of Duncan MacDougall's Experiment on Human Weight Change at the Moment of Death	Masayoshi Ishida
	Unexpected Behavior of Matter in Conjunction with Human Consciousness	Dong Shen
	Randomized Expectancy-Enhanced Placebo-Controlled Trial of the Impact of Quantum BioEnergetics and Mental Boundaries on Affect	Adam J. Rock Fiona E. Permezel
	A Case of the Reincarnation Type in Turkey Suggesting Strong Paranormal Information Involvements	Jürgen Keil
	Questions of the Reincarnation Type	Jürgen Keil
	How To Improve the Study and Documentation of Cases of the Reincarnation Type? A Reappraisal of the Case of Kemal Atasoy	Vitor Moura Visoni
24:2	Importance of a Psychosocial Approach for a Comprehensive Understanding of Mediumship	E. Maraldi, F. Machado, W. Zangari
	Investigating Mental Mediums: Research Suggestions from the Historical Literature	Carlos S. Alvarado
	Advantages of Being Multiplex	Michael Grosso
	Some Directions for Mediumship Research	Emily W. Kelly
	Parapsychology in France after May 1968: A History of GERP	Renaud Evrard
	Remy Chauvin (1913–2009)	Renaud Evrard
24:3	Anomalous Magnetic Field Activity During a Bioenergy Healing Experiment	Margaret M. Moga William F. Bengston
	Further Evidence of the Possibility of Exploiting Anticipatory Physiological Signals To Assist Implicit Intuition of Random Events	Patrizio E. Tressoldi M. Martinelli Laura Scartezzini Stefano Massaccesi
	Fire in Copenhagen and Stockholm. Indridason's and Swedenborg's "Remote Viewing" Experiences	E. Haraldsson Johan L. F. Gerding
	Soal's Target Digits: Statistical Links Back to the Source He Reported After All	Roderick Garton

	Common Paranormal Belief Dimensions	Neil Dagnall Andrew Parker Gary Munley K. Drinkwater
	The 1907 Psychokinetic Experiments of Professor Filippo Bottazzi	Antonio Giuditta
24:4	Psi in a Skeptic's Lab: A Successful Replication of Ertel's Ball Selection Test	Suitbert Ertel
	Anticipatory Alarm Behavior in Bengalese Finches	Fernando Alvarez
	The Daniel Experiment: Sitter Group Contributions with Field RNG and MESA Recordings	Mike Wilson Bryan J. Williams Timothy M. Harte William J. Roll
	Field RNG Data Analysis, Based on Viewing the Japanese Movie *Departures* (*Okuribito*)	Takeshi Shimizu Masato Ishikawa
	The Healing Connection: EEG Harmonics, Entrainment, and Schumann's Resonances	Luke Hendricks William F. Bengston Jay Gunkelman
	Laboratory Psi Effects May Be Put to Practical Use	James Carpenter
25:1	Are There Stable Mean Values, and Relationships between Them, in Statistical Parapsychology?	Wolfgang Helfrich
	Exploring the Relationship between Tibetan Meditation Attainment and Precognition	Serena Roney-Dougal Jerry Solfvin
	A Faulty PK Meta-Analysis	Wilfried Kugel
	Karhunen-Loève Transform for Detecting Ionospheric Total Electron Content (TEC) Anomalies Prior to the 1999 Chi-Chi Earthquake, Taiwan	Jyh-Woei Lin
	Eusapia Palladino: An Autobiographical Essay	Carlos S. Alvarado
	Mental Health of Mediums and Differential Diagnosis between Mediumship and Mental Disorders	Adair Menezes, Jr. Alexander Moreira-Almeida
25:2	Objective Analyses of Real-Time and Audio Instrumental Transcommunication and Matched Control Sessions: A Pilot Study	Mark Boccuzzi Julie Beischel
	Measurement Controls in Anomalies Research	Walter E. Dibble Jr. William A. Tiller
	Hessdalen Lights and Piezoelectricity from Rock Strain	Gerson S. Paiva C. A. Taft
	Retroactive Event Determination and the Interpretation of Macroscopic Quantum Superposition States in Consistent Histories and Relational Quantum Mechanics	Sky Nelson
	Thoughts about Thought Bundles: A Commentary on Jürgen Keil's Paper "Questions of the Reincarnation Type"	Michael Nahm Dieter Hassler
	Reply to the Nahm and Hassler Commentary on Jürgen Keil's Paper "Questions of the Reincarnation Type"	Jürgen Keil
	The Desire for the Development of Flight: A Recurrent Theme for Advanced Civilizations?	B. Reiswig D. Schulze-Makuch
25:3	Reflections on the Context of Near-Death Experiences	Michael Nahm
	An Important Subject at the Institut Métapsychique International: Jeanne LaPlace	Guilio Caratelli Maria Luisa Felici
	A Baby Sea-Serpent No More: Reinterpreting Hagelund's Juvenile "Cadborosaur" Report	M. A. Woodley D. Naish C. A. McCormick

	Avian Formation on a South-Facing Slope Along the Northwest Rim of the Argyre Basin	Michael A. Dale George J. Haas James S. Miller William R. Saunders A. J. Cole Susan Orosz Joseph M. Friedlander
	On Wolverines and Epistemological Totalitarianism	Etzel Cardeña
25:3	Reflections on the Context of Near-Death Experiences	Michael Nahm
	An Important Subject at the Institut Métapsychique International: Jeanne LaPlace	Guilio Caratelli Maria Luisa Felici
	A Baby Sea-Serpent No More: Reinterpreting Hagelund's Juvenile "Cadborosaur" Report	M. A. Woodley D. Naish C. A. McCormick
25:4	Revisiting the Ganzfeld Debate: A Basic Review and Assessment	Bryan J. Williams
	The Global Consciousness Project: Identifying the Source of Psi	Edwin C. May and S. James P. Spottiswoode
	Reply to May and Spottiswoode's on Experimenter Effect as the Explanation for GCP Results	Roger Nelson
	Reply to May and Spottiswoode's "The Global Consciousness Project: Identifying the Source of Psi"	Peter Bancel
	The Global Consciousness Project, Identifying the Source of Psi: A Response to Nelson and Bancel	Edwin C. May and S. James P. Spottiswoode
	Alien Visitation, Extra-Terrestrial Life, and Paranormal Beliefs	Neil Dagnell, Kenneth Drinkwater, Andrew Parker
	Anomalous Switching of the Bi-Stable Percept of a Necker Cube: A Preliminary Study	Dick J. Bierman
	Color Distribution of Light Balls in the Hessdalen Lights Phenomenon	Gerson S. Paiva Carlton A. Taft
	On Elephants and Matters Epistemological: Reply to Etzel Cardeña's Guest Editoral "On Wolverines and Epistemological Totalitarianism"	Neal Grossman
	Response to Neal Grossman's Reply "On Elephants and Matters Epistemological"	Etzel Cardeña
	Ernesto Bozzano: An Italian Spiritualist and Psychical Researcher	Luca Gasperini
	Obituary: In Memory of William Corliss	Patrick Huyghe
	Letter: Pipefish or Pipedream?	Ed L. Bousfield Paul H. LeBlond

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