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EDITORIAL

My reflections last issue on experimental replicability prompted some further thoughts on the subject. In particular, I wondered to what extent we should consider scientific expertise to be an art, or something more like a gift than a skill. In that previous Editorial I criticized a familiar view expressed as follows by Karl Popper: "Any empirical scientific statement can be presented (by describing experimental arrangement, etc.) in such a way that *anyone who has learned the relevant techniques* can test it" (Popper 1959:99, emphasis added). I noted that given the inevitable differences between original experiments and replication attempts—magnified in the behavioral sciences by many additional kinds of potentially relevant variables (such as well-documented experimenter effects), it may be unreasonable to expect success when replication attempts are conducted by someone other than the original experimenter. What I want to consider more closely now are the related questions: What are the relevant techniques? Can they be captured and conveyed by a mere list of procedures, like a recipe for baking bread? To what extent can these techniques even be learned?

When we consider what makes a good physician, psychiatrist, or clinical psychologist, we recognize that a key requirement is something that no mere recipe can capture adequately and that can't easily be taught (if it can be taught at all)—namely, having a "nose" so to speak for what matters—e.g., diagnostically relevant clues. Granted, education can help point one in the right direction, but it can't turn just anyone into a great diagnostician, or a great detective, any more than it can turn just anyone into a great human being. Indeed, one would think that another key requirement of these professions is the ability to relate successfully to others—that is, to have the kind and degree of sensitivity, empathy, or whatever exactly is needed, to understand what others are saying (e.g., to know what's *behind* their words), to know when they're dissembling or withholding information, to make them feel comfortable, supported, etc. And that, too, is something that's very difficult to teach (if it can be taught at all). Very likely, it requires native aptitudes that people simply either have or lack—the qualities in virtue of which some are especially good in relating to other people. To think that these qualities can be acquired merely through education is as foolish as thinking that through formal education alone one can learn to be compassionate, courageous, or witty—or more generally, that one can change deep features of one's character. Similarly, it would be astonishing (if not miraculous) if scientific expertise generally

and experimental expertise specifically (perhaps especially in the biological and behavioral sciences) didn't likewise require certain aptitudes or native capacities with which only some are fortunately endowed. And that may also include having a nose for what matters.

Although this bit of commonsense wisdom may frequently be overlooked, it's hardly a new observation. Perhaps the origins trace back as far as Plato's *Republic*. Plato was concerned with (among other things) what human excellence amounted to, and he noted that this must be answered relative to the different roles that a person can fulfill—for example, that of a teacher, parent, musician, military commander, boxer. A person isn't simply excellent *simpliciter*. That's why we can say that someone (for example) is a good teacher but a lousy parent. Plato also noted that we can evaluate someone qua (i.e. in the capacity of a) person—along some kind of moral dimension. Indeed, we can say that someone is a good person but a terrible teacher (an all too common phenomenon, in fact), or a good military commander but a lousy human being.

Now Plato had his own philosophical and political agenda in writing *The Republic* and so he didn't extend his observations in the direction that concern me here. But we can note that excellence in a person's various capacities might be related in intimate (perhaps even lawlike) ways to excellence in some other capacities. For example, it's likely that a scientist's personal qualities (e.g., character traits) could be a deciding factor in determining whether experiments succeed or fail, or whether theory-building and data-gathering are productive. And I don't have in mind only such relatively coarse measures as (say) whether a parapsychologist is a sheep (believer) or goat (non-believer or skeptic) (see, e.g., Wiseman & Schlitz 1997). Some examples will illustrate what I have in mind. (I'll confine my comments to work in parapsychology, but I encourage readers to find analogues in other areas of science.)

When I began my serious study of parapsychological research back in the 1970s, I was struck by the following episode at a conference of the Parapsychological Association. One of the presenters was Helmut Schmidt, an exceptionally creative and successful theoretician and experimenter. Helmut gave a talk in which he described his latest success in testing subjects' ability to influence the output of random number generators. Helmut's talk was given with his usual (and considerable) energy and enthusiasm. For example, he described in a very animated way how he encouraged his subjects to imagine themselves psychically *pushing* the RNG. And the word "pushing" he expressed with great emphasis and dramatic gestures. Following this presentation was a talk given by a young woman who had tried unsuccessfully to replicate one of Schmidt's earlier

experiments. I know from having talked to her that she was a very nice person. But her personality was so different from that of Schmidt, one could be forgiven for thinking that the two experimenters were members of different species. Helmut was charismatic, extroverted, enthusiastic, and dynamic. It was easy to see how he could have effectively encouraged his subjects to succeed. By contrast, this young woman was relatively lifeless, monotonous, and insipid. Her talk was given with an almost total lack of affect, and that wasn't just a matter of stage-fright; that was her manner of talking. So it was equally easy to see how she might have failed to inspire or excite her subjects. Similarly, perhaps the late John Beloff's notoriously poor track record in conducting or supervising successful psi experiments connects with his mild and quite understated personality, despite the fact that he clearly qualified as a sheep—that is, despite his demonstrated sympathy for psi research and his obvious conviction about the positive merits of the best cases.

Along the same lines, in both psi research and the behavioral sciences generally, experimental success might require, in addition to (or instead of) charisma, a supportive experimental personality that can make subjects feel safe or comfortable about participating in the experiment, and which can help them trust the experimenter. Many believe (as I do) that this is why Russell Targ (another low-key personality) has been so consistently successful in conducting remote viewing experiments. And clearly, only some people have that kind of character trait. Moreover, it may also be a matter of the way personality styles *fit* with one another. Even a generally supportive or encouraging person may rub some people the wrong way, if their personalities are broadly incompatible. That's one reason we can feel comfortable in life with certain people but not others.

Now you might think that psychologists especially should be keenly aware of these sorts of interactions and potential personality conflicts. I used to think so—at least I did early in my academic career, before I began to meet more and more psychologists and started attending their parties. At that point, however, I realized that my hosts often had almost no idea which people should be invited together to the same affair, and which people would almost certainly create friction when placed in a common environment. I could only wonder, then, how that ironic blindness might also affect their professional work—for example, their ability to relate to their subjects, or to select appropriate graduate assistants to interact with their subjects.

Not surprisingly, there has been some mainstream research on the personality correlates to successful experimentation in psychology. But those I've seen have been rather superficial, focused on such relatively rudimentary measures as, for example, experimenter need for social

influence, experimenter desire for control, subject need for social approval (see, e.g., Hazelrigg, Cooper, & Strathman 1991), and seldom rising above commonsense, very general conjectures and observations that probably never needed to be confirmed with the aid of precious research funds. Moreover, as far as the study cited above is concerned, given the authors' own experimental procedures, one can only wonder how they evaluated the relevance of their own personality traits in leading to their results. That is, one can only wonder about the wisdom of experimentally investigating experimental biasing—at least, in the absence of detailed and reliable information about the experimenters' own personalities. Personally, I suspect that experimentation is simply not the way to proceed here. Probably, there's much more to learn from keen and sensitive observers' careful and penetrating examination of both successful experimenters and also subjects who do well under a wide range of experimental conditions.

I mentioned earlier that scientists might need a "nose" (or perhaps "eye") for relevant data, and that in the absence of this ability their work might exhibit systematic deficiencies. This is a criticism I've lodged many times against the postmortem survival research of Ian Stevenson. Don't get me wrong; I believe Stevenson's work is monumentally important and valuable. However, as I've argued in detail (see, e.g., Braude 2003), Stevenson repeatedly treated the subjects of his case investigations as if they were psychological stick figures, with no depth or breadth of personality, and with no deep issues guiding their lives in the subtle ways most of us know from our usual life blunders and successes—for example, the cunning and often indirect or elusive ways we might repeatedly entangle ourselves in lethal relationships, or undermine our attempts to succeed professionally (for an exemplar of a more penetrating way to consider the behavior of both experimenters and subjects, see Eisenbud 1992). Consider, for example, the blatant clues about motivations and subject psychopathology Stevenson missed in the well-known case of Sharada (Braude 2003, Chapter 4). For all his many virtues, I'd say Ian was blind to much of what really deserved his attention. And as a result, he repeatedly underestimated the power of sophisticated and reasonable alternatives to the hypotheses of reincarnation specifically and survival generally.

Now if it's true that scientific success or failure sometimes hinges on the presence or absence of certain personality traits of the scientist and (in the case of experiments) is not simply a matter of following a recipe of procedures, what can be done about this? It seems unlikely that graduate programs in the sciences will suddenly—or ever—award advanced degrees only to students passing a battery of relevant psychological tests. And it seems equally unlikely that scientists will volunteer themselves for

psychological profiling, the results of which can be published alongside their research (for example, there's been very little enthusiasm for such a proposal offered in a parapsychological insiders' listserve to which I subscribe). In fact, I suspect that many (most?) scientists like to perpetuate the myth that they're especially objective observers and agents, and not the steaming, stinky cauldrons of fears, insecurities, flaws, and issues that afflict everyone else. Perhaps the most we can hope for is a rejection of Popper's simplistic statement about scientific expertise, a correspondingly more sophisticated assessment of experimental results, and a willingness to consider seriously the full range of variables (including character traits) that can affect experimental outcome. And more generally, we can perhaps hope for a greater appreciation of the fact that scientists, like other human beings, have both personalities and feelings, and that they're subject to the same grubby concerns and life issues that influence even the most mundane actions. Perhaps then we'll see a wider acknowledgment that scientific success and character traits are not neatly separable. And who knows, perhaps then we'll see a more sensible appraisal of replication attempts in areas of frontier science.

Stephen E. Braude

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RESEARCH ARTICLE

Use of a Torsion Pendulum Balance to Detect and Characterize What May Be a Human Bioenergy Field

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Abstract—Whereas the concept of bioenergy fields is thousands of years old, their existence has never been verified by scientific experiments designed to detect and measure them; so bioenergy fields have no scientific credibility. The instruments used for those experiments typically detect components of the electromagnetic spectrum. The experiments presented here utilize a detector that instead is sensitive to actual "pushing" forces that are capable of altering the momentum of a physical object such as a simple torsion pendulum balance that is suspended above a seated human subject. The experimental design includes a videocamera connected to a computer that can detect and measure the pendulum movements with high precision, and store this information in a data file for later analysis. Experiments show that the pendulum detects and measures substantial forces that drastically alter the motions of the pendulum when a subject is seated under it. The following effects are consistently observed with every subject in every experiment performed up to now: 1) Substantial shifts of the center of oscillation of the pendulum; shifts as large as 2.2 cm (7 deg) requiring a force that is equivalent to 45 mg are observed, 2) Many new frequencies of oscillation of the pendulum are introduced when a subject is present, 3) Dramatic changes in the amplitudes of oscillation of the pendulum are observed throughout the experiment; increasing, decreasing, and increasing again, in patterns that resemble chemical relaxation processes, 4) These shifts of the center of oscillation, the new frequencies of oscillation, and the changes in amplitudes all persist for 30–60 min after the subject has left the pendulum. This is inconsistent with the physics of a simple harmonic oscillator such as a torsion pendulum, which should return to simple harmonic oscillation immediately after any exterior disturbances are discontinued. After conducting control experiments to rule out effects of air currents and other artifacts, it is concluded that the effects are exerted by some kind of force field that is generated by the subject seated under the pendulum. We know of no force, such as one within the electromagnetic spectrum, that can account for these results. It may be that a conventional explanation for these surprising results will be discovered, but it is possible that we have observed a phenomenon that will require the development of new theoretical concepts. For now, it is important that other investigators repeat and extend our observations.

Introduction

Bioenergy and bioenergy fields have been central to the healing arts for thousands of years. However, efforts to establish the existence of these bioenergy fields using scientific instrumentation have so far been unsuccessful, so the existence of these fields is not accepted by mainstream science. This is understandable, because the scientific study of anything requires that the object of study be detectable, and that its properties can be measured, quantified, and characterized. One argument is that bioenergy fields do not exist. Another is that the instruments used to detect the biofields are inappropriate and therefore useless. An underlying assumption of heretofore biofield detection methods is that biofields consist of components of the electromagnetic spectrum and are photonic in nature. Suppose this assumption is incorrect, and that biofields do not consist of photons; in which case, previous attempts to detect these fields failed because of inappropriate detector designs. This work utilizes a detector of a completely different design. It is one that assumes that the bioenergy field, instead of being photonic, consists of a field that can exert an actual physical force; a force that can literally push against objects to alter their momentum. Since such a pushing force is likely to be small, a sensitive detector is required. A torsion pendulum, often called a torsion balance because of its ability to measure forces, was chosen because of its excellent sensitivity and simplicity. These qualities have been exploited by scientists for hundreds of years, a classic example being the accurate measurement of the gravitational constant by Henry Cavendish in 1797 (Cavendish).

Results

A depiction of the experimental setup is shown in Figure 1. The subject is seated beneath a hemispherical energy collector which is suspended by a short monofilament nylon fiber attached to a rigid support. As the detector oscillates by twisting back and forth, its motions are observed by a videocamera that is programmed to determine the position of the pendulum, usually at a rate of 10 measurements per sec, and this position information is stored in a data file together with the time of collection of each datapoint. Figure 2 shows the hemispherical energy collector, which is constructed of steel mesh, with a 1-cm white dot target attached to it, together with a screenshot during data collection of an experiment in progress. As the white dot twists toward the right, the data curve moves upward and then downward as the dot twists toward the left. Once the experiment is complete, the data file is used to chart and analyze the motions of the pendulum.

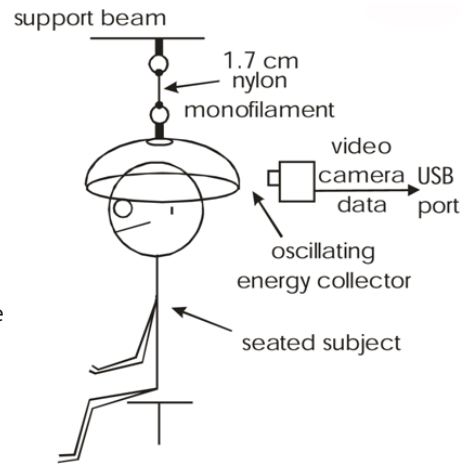

Figure 1. Subject seated under pendulum with video object-tracking camera. Pendulum components are shown in Figure 2. The camera was a ProScope Model HR2 fitted with a 1-10X lens.

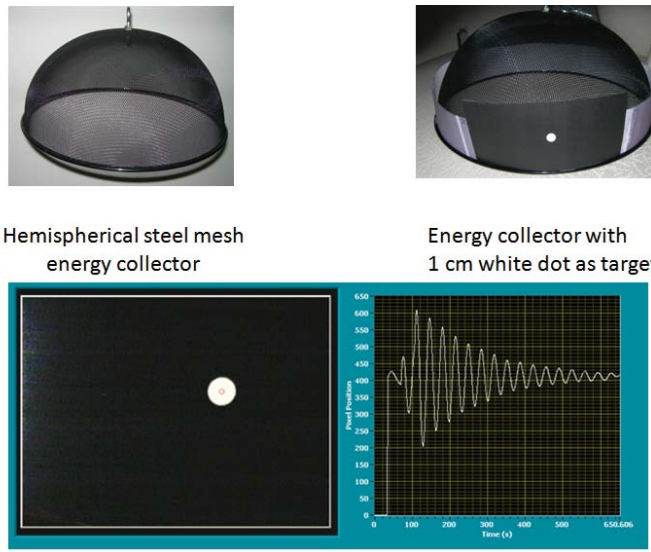

Figure 2. Components of the pendulum and data collection. The 15 x 35 cm steel-mesh hemispherical energy collector is shown on the upper left, and the 1-cm white dot target is shown on the upper right. Below is a screenshot of the data output while an experiment is in progress. It displays the position of the 1 cm white dot superimposed on a small red circle showing the calculated center of the white dot, and a graphical record of the position of the center of the white dot as the experiment progresses. The position of the center of the white dot is recorded into a data file that can be analyzed after the experiment is complete. The program for data collection was written by Irene He of http://www.hytekautomation.ca. The program can be obtained by contacting info@hytekautomation.ca.

Characterization of the Pendulum as a Classical Damped Simple Harmonic Oscillator

It is important to establish that the motions of the pendulum conform to the properties of a simple harmonic oscillator (*sho*). Figure 3 shows that it does. The graph displays two curves, one being the actual data profile obtained from the videocamera, and the other being an overlay of the theoretical curve predicted by the equation for a damped simple harmonic oscillator. This pendulum is highly damped because of air resistance and other frictional forces encountered during the twisting oscillations. Nevertheless, once a suitable damping coefficient is selected, the data curve and theoretical curve superimpose very well, even after oscillations have been highly damped. Despite the simplicity of the pendulum, its adherence to the ideal properties of a *sho* qualifies its suitability as a scientific instrument that can reliably detect forces that deflect it from its normal *sho* behavior. In its role as a torsion balance, this pendulum can also measure forces exerted against it. This requires calibration of the torsion constant of the fiber supporting the pendulum, also shown in Figure 3. The torsion constant is 2,240 dyne-cm/radian, and, using appropriate conversion factors, a force that is equivalent to 4.6 mg will deflect the pendulum by 1° of rotation. For this pendulum, a 1° rotation is equivalent to a displacement of 0.3 cm. This pendulum is therefore suitable to both detect and measure any twisting forces exerted against it by a subject sitting beneath it, if any such forces exist.

An important aspect of analysis of the motions of the pendulum is the use of the Fast Fourier Transform (FFT) to determine the frequencies of the twisting oscillations of the pendulum. The FFT analysis in Figure 3 shows that the pendulum oscillates with a frequency of 0.034 Hz, which is a period of about 30 sec.

Effects Exerted by the Presence of a Subject Seated Beneath the Pendulum

As shown in Figure 4, the pendulum detects and measures substantial forces when a subject is seated beneath the pendulum. Figure 4 shows a single continuous experiment during which the subject is seated under the pendulum during three separate time segments, each separated by a time period when the subject is absent. It is accordingly a triplicate experiment demonstrating the variation of effects exerted by the same subject during three closely spaced time intervals. Analysis of the three experimental segments reveals both consistent similarities and aspects that are different among these time periods. The behavior of the pendulum in the presence of the subject suggests the presence of an energy field above the subject.

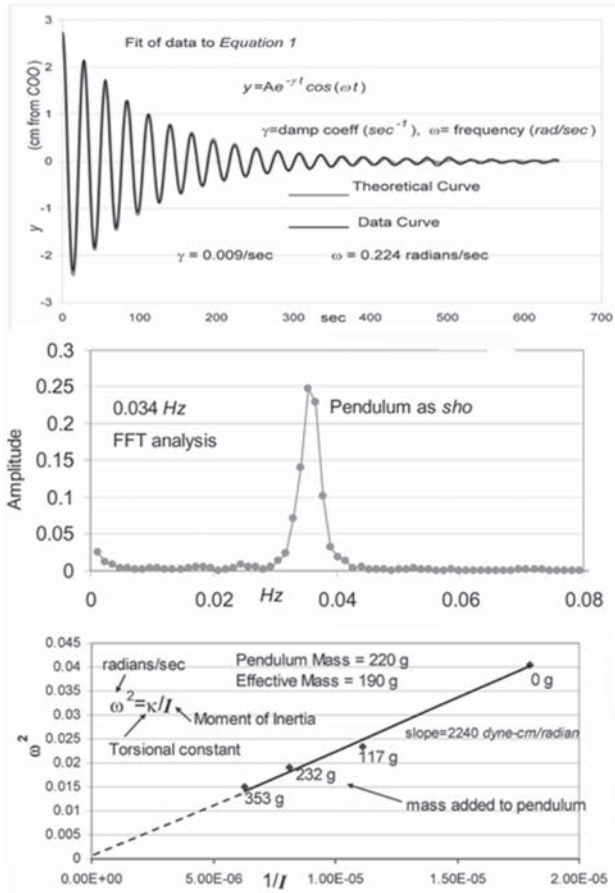

Figure 3. The pendulum behaves as a damped simple harmonic oscillator (*sho*). **(Top panel)** The black curve represents the datapoint measurements of the deflection of the pendulum from the Center of Oscillation (*COO*) taken at a rate of 10/sec. The red curve is the theoretical curve predicted by Equation 1 in which the values of ω (frequency) and γ (damping coefficient) are chosen to give a best fit to the data. The best-fit ω is 0.224 radians/sec, and the best-fit γ is 0.009/sec.
(Middle panel) FFT analysis of the data using the signal-analysis program, SIGVIEW, from sigview.com. It shows that the natural frequency of the pendulum, in the absence of a subject, is 0.034 Hz, which is equivalent to a period of 29.4 sec. SIGVIEW facilitates the use of signal-analysis principles as described by Lyons (2004).
(Lower panel) Determination of the torsional constant (κ) of the nylon support of the pendulum. The effects of adding masses to the outer rim of the pendulum on the ω of the pendulum are shown. The data are fitted to the equation shown in the figure, which gives a κ of 2,240 dyne-cm/radian, or 39 dyne-cm/deg of rotation. Using appropriate conversion factors, it was established that a force that is equivalent to 4.6 mg will displace the pendulum by 1° of rotation. For this pendulum a 1° rotation is equivalent to a displacement of 0.3 cm.

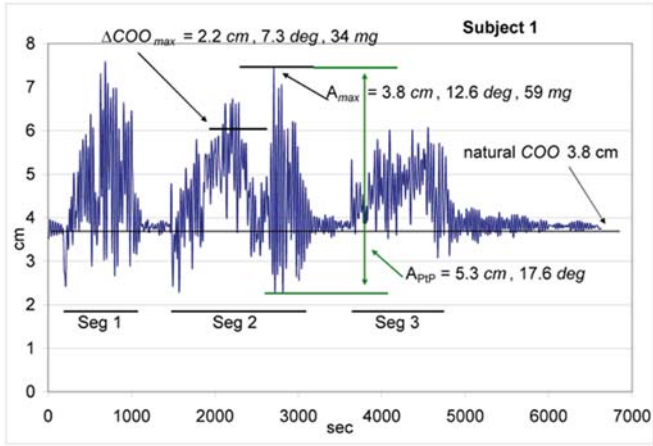

Figure 4. Patterns of oscillation of the pendulum when Subject 1 is present. The initial seconds of the experiment are twisting oscillations prior to Subject 1 being seated under the pendulum. The vertical cm axis represents movements of the pendulum in which positive values represent rotations in the clockwise direction as viewed looking downward from above the pendulum. Seg 1 is a period of time during which Subject 1 is seated under the pendulum, as are Seg 2 and Seg 3, respectively. When the Subject is present, the amplitudes of the oscillations and the Center of Oscillation (*COO*) of the pendulum both change dramatically, with the maximum Δ*COO* being indicated for Seg 2, expressed as cm, deg of rotation, and mg equivalent of force required to drive the rotation. A_{max} is the maximum amplitude of the displacement from the natural *COO* expressed as cm, deg of rotation, and mg of equivalent force. The vertical green arrow is the A_{PtP}, which is the largest peak-to-peak amplitude observed during the experiment, expressed as cm and deg of rotation. When the Subject departs from beneath the pendulum after each Seg, the pendulum reverts toward the natural *COO*, but it does not actually attain classical *sho* behavior until long after the Subject departs, as is shown in the post–Seg 3 region.

Moreover, the magnitudes of the forces being detected are substantial and easily measured, showing that it is the design of the detector that is crucial for the detection and characterization of the putative biofield.

Shortly after the subject is seated under the pendulum, which is oscillating as a low-amplitude classical *sho*, the pendulum begins to oscillate/twist with much stronger amplitudes. Moreover, within a minute or so, the pendulum begins to shift with respect to its natural center of oscillation (*COO*), and this *COO* undulates throughout Segs 1, 2, and 3. This deflection of the *COO* is substantial, and is at its highest during Seg 2, at which time the deflection is 2.2 cm, or 7.3 deg, which would require a force that is equivalent to 34 mg. This shift in *COO* does not occur in a haphazard or jumpy fashion, but

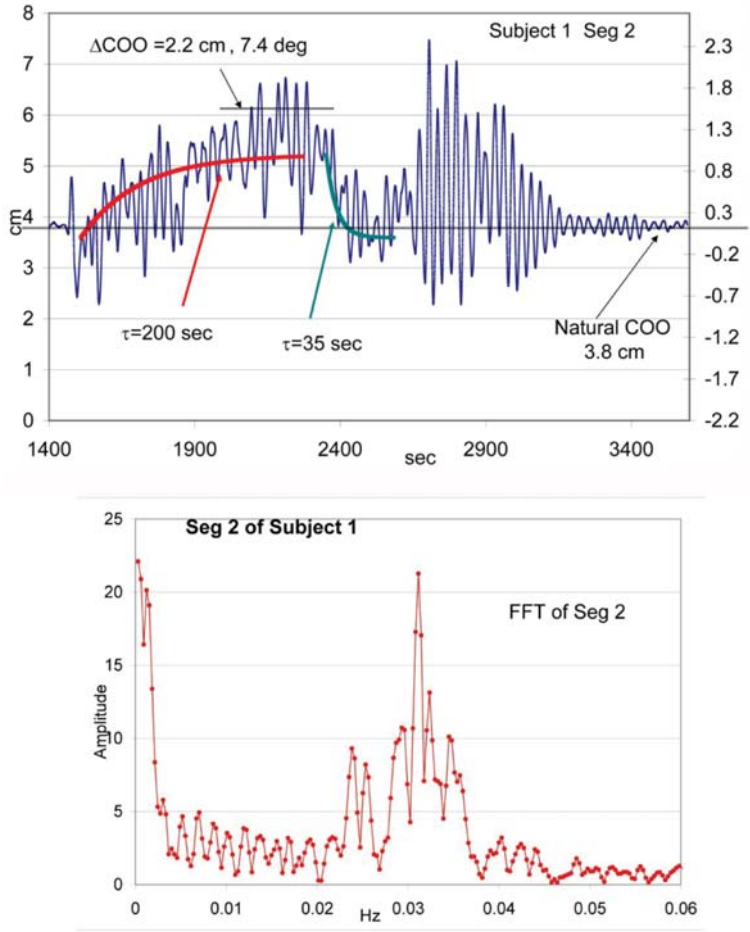

Figure 5. Focus on Seg 2 of Figure 4 data.
Figure 4 shows the results of three consecutive experiments with Subject 1 under the pendulum.
(Top panel) Focuses on Seg 2 data of Figure 4. The duration of Seg 2 is about 1,700 sec (28 min), and during that time the *COO* shifts 2.2 cm away from the natural *COO*. This corresponds to 7.3 deg of rotation, and based on the κ of the monofilament fiber would require a force that is equivalent to 34 mg.
(Bottom panel) FFT analysis of the Seg 2 data. SigView was used to analyze the frequency components that contribute to the oscillation data in the top panel. The major frequency peak is at 0.032 Hz, but there are other frequencies that are also significant. These significant frequencies encompass the entire range of 0.0–0.06 Hz (higher frequencies are small and are not shown). The frequencies that contribute to the Seg 2 profile are further analyzed in Figure 6.

occurs in a pattern that conforms to a chemical relaxation process with a corresponding relaxation constant, tau, (τ), where τ is the time it takes for an equilibrium to shift 1/*e* of the way from one position to another, and is analogous to a half-time reaction. Chemical relaxation curves are plotted on the data of Seg 2 (Figure 5), with values of 200 sec and 75 sec, respectively. That the data conform to chemical relaxation kinetics suggests that chemical principles may be useful in understanding the processes involved in mediating the shifts in the *COO*. Principles of chemical relaxations are described in Hammes et al. (1971).

Effects Exerted by the Subject on Pendulum Oscillation Frequencies

Whereas the pendulum oscillates with a single frequency when no subject is present, when a subject is present the pendulum oscillations show many new frequency components, as is shown by FFT analysis of Seg 2 in Figure 5. The largest amplitude frequency peak corresponds to the natural frequency of the pendulum, flanked by several large frequency amplitudes; and progressively smaller frequency amplitudes on both sides of the largest peak. This pattern of amplitudes is consistent with the pendulum resembling the behavior of a tuning fork that resonates most strongly with frequencies that are closest to the natural frequency. That many other frequencies that are quite distant from the natural frequency are also revealed suggests that their actual strength is substantial.

The FFT of the Seg 2 data establishes the existence of these frequencies during Seg 2, but it does not show how the strength of each individual frequency amplitude fluctuates during the Seg 2 data period. This fluctuation of amplitudes is shown in Figure 6 which employs the BandPass feature of FFT signal analysis (Lyons 2004), which allows discrete frequency ranges of the signal to be isolated and analyzed separately. The amplitude of each of the frequency ranges changes dramatically during Seg 2, with each one varying in a unique way. The actual pendulum oscillations constitute the sum of all the frequency components, according to the principles of constructive and destructive interference. The existence of many frequency components that vary among themselves and are undergoing constructive and destructive interference therefore accounts for the variability of the motions of the pendulum.

FFT Analysis Shows Substantial Differences in the Frequency Patterns of Segs 1, 2, and 3

FFT analysis of the pendulum oscillations in Segs 1, 2, and 3 are compared in Figure 7. Whereas one can see similarities, the differences among them

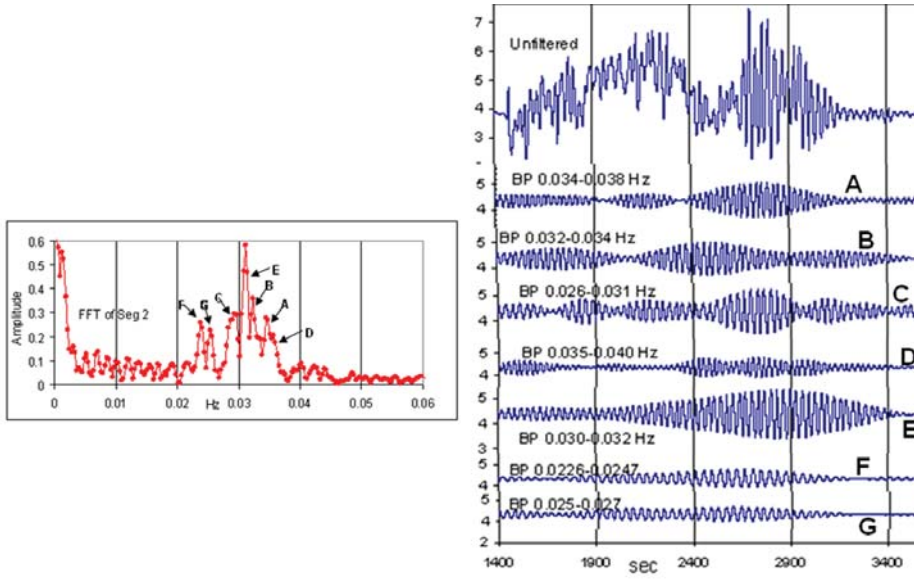

Figure 6. Frequency components that contribute to the oscillation profile of Seg 2.
(Right top panel) Shows oscillation data of Seg 2, including several min after the subject left the pendulum.
(Left panel) The FFT analysis of these data is shown.
Each of the A–G frequency peaks corresponds to a particular frequency range that is responsible for that peak. The contribution of each peak is assessed by applying a BandPass filter to the frequency range that is defined by the lowest amplitudes above and below each peak. For curves A–G, what is represented is the BandPass (BP) profile that represents the frequency contribution of that particular BP component to the top panel data, e.g., curve A is what is obtained by applying a 0.034–0.038 Hz BandPass filter to the top panel data.

are substantial; and in Seg 3 the natural frequency is greatly diminished, suggesting that the oscillations of the pendulum are dominated by the forces exerted by the subject, which override the natural properties of the pendulum.

Pendulum Motions Do Not Return to Normal until Long after the Subject Has Departed

Classical physics predicts that a *sho* that is being subjected to outside forces will immediately return to normal *sho* motion after the outside forces have been removed. Separate control experiments confirmed that this pendulum immediately returns to classical *sho* motion once outside forces such as

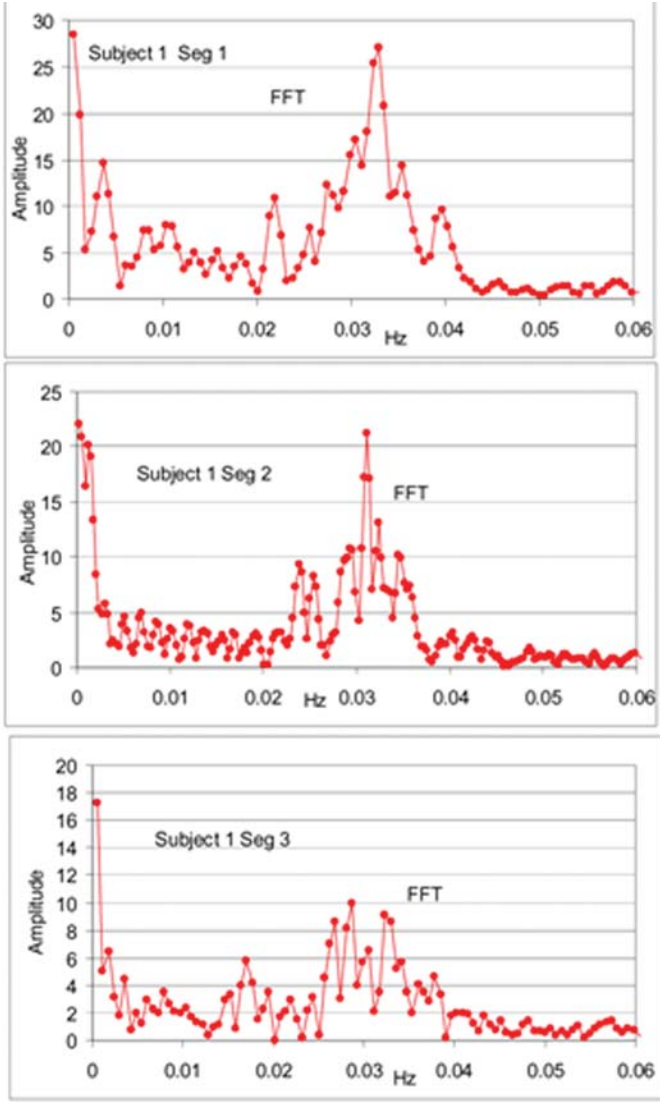

Figure 7. FFT analysis of Segs 1–3 of the Subject 1 data shown in Figure 4. The FFT profiles are aligned vertically so that differences between them can be compared. A complete analysis of the contributions of the frequency peaks in the Seg 2 FFT profile is shown in Figure 6.

puffs of compressed air have been eliminated. Inspection of the data profile in Figure 4 indicates that an immediate return to normal *sho* motion after the subject departs does not occur. This is especially evident at the end of Seg 3, when the non-classical motions of the pendulum continue to be observed long after the subject has departed. Figure 8 focuses on the time period after the subject has left the pendulum at the end of Seg 3. Two curves are shown, the red one being the expected oscillation pattern of the pendulum once the external forces have been removed, i.e. the behavior of the pendulum that is predicted from classical physics. The black curve shows the actual oscillations of the pendulum, which are very different from the expected (red) curve. Remarkably, both the deflection from the natural *COO* and anomalous amplitudes and frequencies are retained long after (30–60 min) the subject has left the pendulum. The intensities of these effects diminish with time, and the rate conforms to a chemical relaxation process with a τ of 600 sec. This is slower than the relaxation times when the subject was present, suggesting that the subject can accelerate the *COO* transitions, perhaps by catalyzing an energy-driven process, whereas the relaxation in the absence of the subject would be uncatalyzed and occur without an input of energy.

It needs to be considered that the effects observed with Subject 1 may be due to Subject 1 possessing unique abilities that result from a combination of natural talent and/or training. This possibility has been explored using many other subjects. Among more than a dozen subjects, all exert these effects, so it is neither necessary to be naturally talented nor to be trained. As an illustration, Figure 9 shows the results from the very first experiment with a new subject. In this experiment, there are shifts in the *COO* and new frequencies when the subject is present. There is also retention of the *COO* shifts and frequency components after the subject departs. Moreover, the τ when the subject is present is 200 sec, and the τ after the subject departs is 600 sec. A 200-sec relaxation was observed when Subject 1 was present (Seg 2). A 600-sec relaxation time was observed after Subject 1 departed after Seg 3. These are commonalities that suggest common explanations.

Based on many experiments using many subjects under many conditions, we are convinced that these effects are real, and not a result of experimental errors or artifacts. The most obvious artifact is air currents that are produced by the subject; a combination of body temperature, and body motion, which includes breathing. These issues were addressed by asking a subject to both suspend breathing as long as possible, and to breathe as shallowly as possible, and to reduce body motion to a minimum. These attempts had no apparent effects on the outcome of the experiment, in that substantial shifts in the *COO* and new frequencies of oscillation were

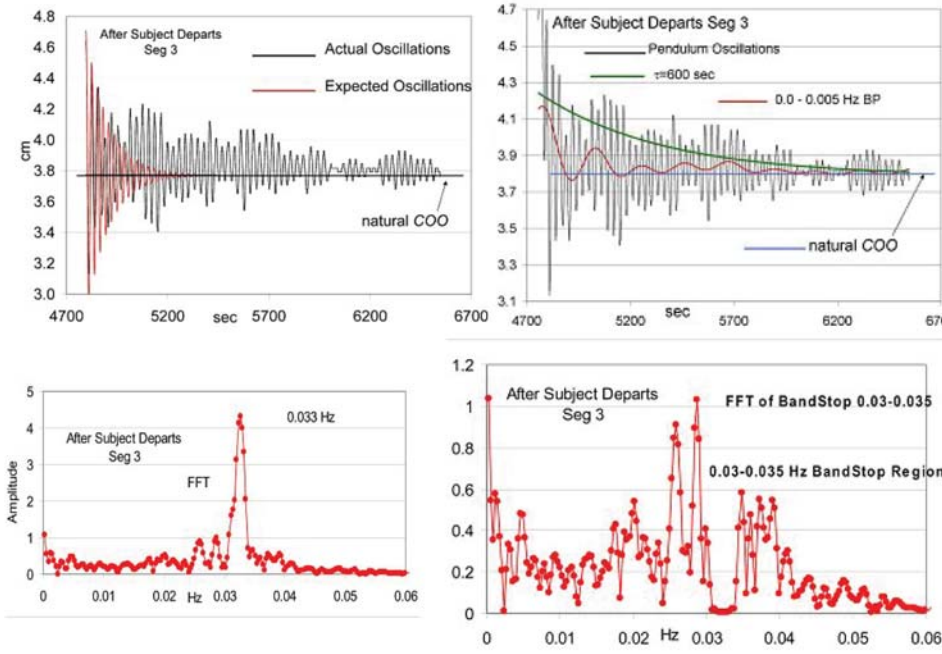

Figure 8. Analysis of region after the end of Seg 3 of Figure 4.
Subject 1 departed from the pendulum at the end of Seg 3 during a time at which Δ*COO* was still significant (~1 cm). Upon departure of the subject, the classical behavior of the pendulum should immediately resume. The pendulum did not resume classical behavior, and the deviation from expected was very large.
(Top left) The red curve shows the expected damped *sho* behavior, which is a theoretical curve calculated using the values of ω and γ obtained in Figure 3. The black curve shows the actual oscillations that were observed. It is noted that all persons left the area immediately after the end of Seg 3, so no person was in the vicinity of the pendulum during the time that these residual effects were evident.
(Top right) The green curve is the chemical relaxation curve with a relaxation time (τ) of 600 sec. The red curve is the 0–0.005 Hz BandPass frequency, which closely follows the midpoint of each oscillation during the approach to the natural *COO*.
(Lower left) FFT after Seg 3. The major peak corresponds to the natural frequency of the pendulum. Other frequencies are also present, especially the two peaks just below the main peak.
(Lower right) The continuing presence of these frequencies is made more evident by using the BandStop feature of SigView, which removes the major (0.03–0.035 Hz) fundamental pendulum frequency. Many frequencies remain, both above and below the fundamental pendulum frequency. The two frequencies immediately below the fundamental frequency have the largest amplitudes.

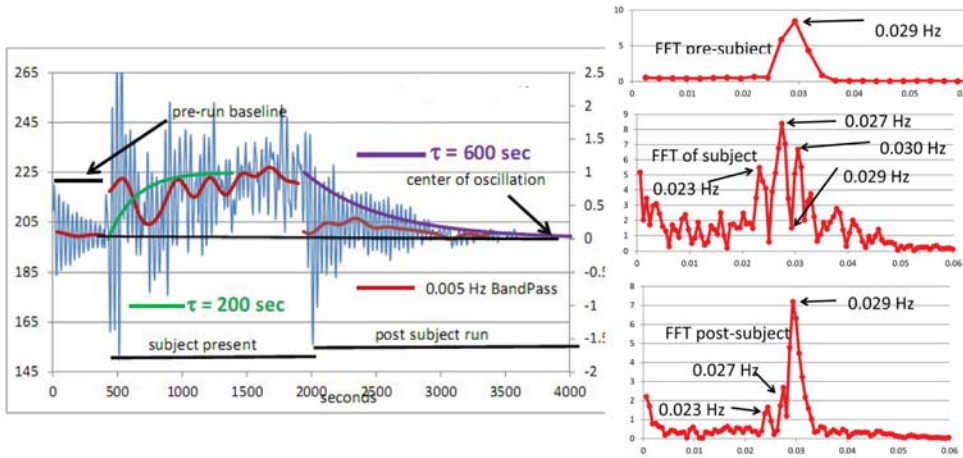

Figure 9. First experiment with a new subject.
(Left panel) The complete experiment is shown. The pre-subject baseline establishes natural *sho* behavior, and the natural *COO*. The Subject Present region is when the subject is seated under the pendulum. The post-subject region is after the subject has left the pendulum. The green curve during the time the subject is present represents a chemical relaxation process with a relaxation time (τ) of 200 sec. The red curve is the 0–0.005 Hz BandPass which closely follows the midpoint of each oscillation curve. The purple curve represents the chemical relaxation process with a relaxation time (τ) of 600 sec when Subject 1 is present.
(Right panels) FFT analysis of the three stages of the experiment.
(Top right panel) Shows the FFT prior to the subject being seated under the pendulum. The natural frequency is 0.029 Hz.
(Middle right panel) The FFT analysis while the subject is under the pendulum. Many frequencies are shown. The actual fundamental frequency of the pendulum does not appear, whereas other frequencies dominate.
(Lower right panel) FFT analysis after the subject has left the pendulum. The fundamental 0.029 Hz frequency has returned. However, the 0.027 and 0.023 Hz frequencies are still strongly present. It is clear that the effects of the subject on the pendulum persist long after the subject has left the pendulum.

observed throughout. Another experiment used a Presto-brand cooking pot with a lid that was placed under the pendulum so that the lid was in the position normally occupied by a subject. The cooking pot was heated to body temperature, and the effects on the pendulum were observed. Effects on the oscillations of the pendulum were negligible, and deviations from the natural *COO* were undetectable. These results of control experiments are available elsewhere (Hansen & Lieberman 2009).

A final concern is the possibility that the effects on the pendulum may be due to static electricity, in that static charges in the steel-mesh hemisphere could interact with static charges in the subject. This possibility was eliminated by performing experiments in which all of the components of the pendulum were connected to ground, and the subject was also connected to ground through wrist straps. The description of these experiments and the results are in the Appendix.

Discussion

These results suggest that a previously unknown human bioenergy field has been documented, which was made possible by designing a new detector of "pushing/rotational" forces that are capable of altering the momentum of physical objects, instead of the photonic detectors that have dominated in previous studies of bioenergy fields. The results appear to establish the existence, immediately above the human cranium, of a form of energy that can greatly influence the twisting motions of a torsion pendulum. Properties of the twisting force include the ability to deflect the center of oscillation of the pendulum, as well as the frequencies of oscillations. A particularly significant observation is that after the subject departs from beneath the pendulum, the effects on the pendulum are retained for a period of 30–60 min. In all of the experiments with the subject present and after the subject departs, the transitions of the position of the *COO* conform to the kinetics of chemical relaxation processes, suggesting that the principles of chemistry will be important in understanding the effects of the subject on the pendulum.

That the pendulum retains twisting-oscillatory qualities after the subject has left is reminiscent of phosphorescence, in which light can elevate electrons to an elevated state; and after removing the phosphorescent object from the light source into the dark, one can see light emanating from the object as the elevated electrons return to their ground states. The principles of phosphorescence are well-known and understood.

We hypothesize that what happens with the pendulum is similar in concept, but different in fundamental ways. It is accordingly hypothesized that the energy of the subject exerts an energy-driven effect on the pendulum that converts its atomic and molecular constituents toward higher-energy quantum states. The unusual thing is that these elevated quantum states must possess the ability to exert chiral forces that drive the pendulum to oscillate with a deflected *COO*, and also possess a kind of energy that can drive the pendulum to oscillate with new frequencies. Just as with phosphorescence, when the subject departs from the pendulum, these elevated energy states decay in a process that resembles a chemical relaxation process, which

proceeds until these elevated states are completely dissipated so that the pendulum eventually reverts to normal *sho* behavior. The experiments reported here utilized a steel-mesh energy collector. However, recent unpublished results show that similar effects are observed with a completely organic collector composed of coco-fiber. Explanations of these effects will therefore have to be inclusive of both kinds of materials.

We know of no quantum state that is capable of exerting these kinds of chiral and vibrational frequency effects. However, our results argue that these kinds of quantum states exist, and can be attained by the influence of the energy field above the subject. If so, it is very important that we study and understand this energy field. We believe that our pendulum will be a valuable component in this discovery process. While it is likely that conventional principles of chemistry, physics, and biology will provide an explanation of our results, it may be necessary to invoke exotic concepts such as quantum entanglement (Heyes, Sakuma, de Visser, & Scrutton 2009) and/or torsion fields (Kozyrev 1971).

The idea of bioenergy fields has long been a subject of derision because they have not been detectable using instruments that are sensitive to components of the electromagnetic spectrum. We have now introduced an entirely different type of detector; one which detects and measures physical forces, especially chiral forces, by means of a torsion pendulum, i.e. a torsion balance. This pendulum balance detects substantial forces on the pendulum; and the effects of these forces are highly unusual, as described in the Results section.

Unlike scientific experiments that require instrumentation that is highly complex, the pendulum described here is very simple. It is nothing more than a steel-mesh hemisphere that is suspended above the subject by a short strand of nylon monofilament. The only complexity is the measurement of the motions of the pendulum which requires a videocamera that observes the position of the pendulum over time, a computer, and appropriate software to collect the data. The data collection software is commercially available at: info@hytekautomation.ca. The simplicity of the instrumentation and the experimental procedures will allow experimenters with a wide range of interests and expertise to explore the phenomena that we have reported. In the tradition of the scientific method, it is hoped that these experimenters will test our observations by designing experiments to search for artifacts and alternate explanations of our observations. In view of the simplicity of the experiments, it is hoped that the results from other experimenters will arrive soon. We will facilitate these efforts in every way we can.

Ethics: The Institutional Review Board of the University of Maryland approved these experiments.

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Appendix
Ruling Out Static Electricity

In following through with the grounding experiments that were suggested by reviewers, we obtained important results. As you will see below, grounding of both the pendulum hemisphere and the subject had no effect on the results obtained from experiments. These experiments were done in response to concerns that the effects we were seeing might be due to static electricity, and to eliminate this possibility it was necessary to ensure that both the steel-mesh hemisphere and the subject were properly grounded.

To ground the steel-mesh hemisphere, we had to make some modifications to the equipment setup. Our original experiments were performed with the hemisphere being attached to a 1.7-cm nylon fiber support. Initial attempts to attach a grounding wire to the hemisphere resulted in the wire interfering with the oscillations of the hemisphere. The solution we chose was to substitute a copper wire in place of the nylon fiber. The copper wire was fastened to a steel eye-bolt, which was inserted into the steel mesh at the top of the hemisphere. Before attaching the eye-bolt, the mesh was thoroughly polished to brightness with emery paper, and the eye-bolt was secured with a nut on each side of the mesh, with steel washers squeezed against the mesh by tightening the bolts against the washers. Electrical conductivity between the eye-bolt and numerous locations around the surface of the hemisphere was confirmed with an ohm-meter. The other end of the copper wire support was similarly attached to the aluminum support that we normally use. A ground wire was then bolted to the aluminum support which was attached to the ground lug of a 3-prong plug, which was inserted into a grounded wall outlet.

A similar ground wire attached to a plug was used to attach to wrist straps of the type used by technicians to work with electrical equipment

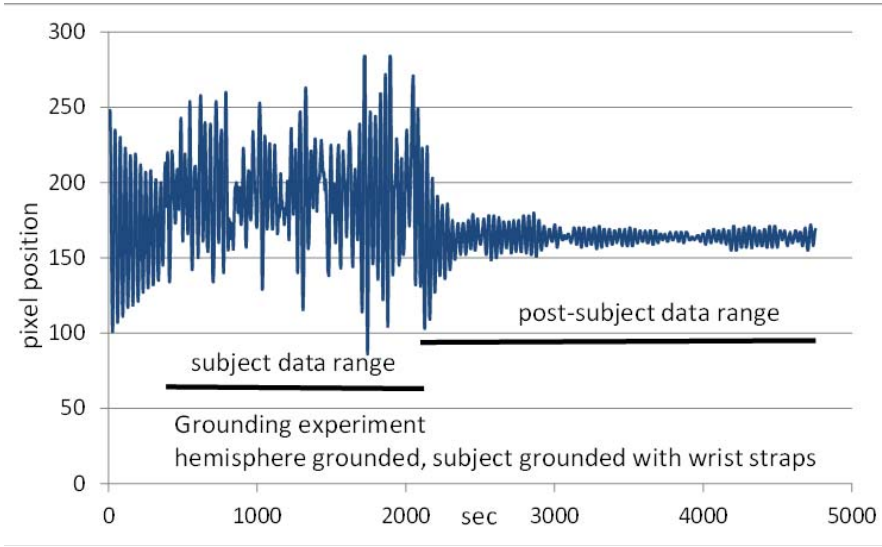

Figure 10. An experiment in which both hemisphere and subject were grounded.

that can be damaged by any static electricity that might be present on the technician. These wrist straps drain that static electricity to ground, so are appropriate for ensuring that the subject does not have a static charge. Total conductivity to ground was established by using the ohmmeter to show no resistance between the wrist straps and various points throughout the surface of the steel-mesh hemisphere.

It must be noted that the copper wire support had to be much longer than the original nylon support in order to obtain the normal 30-sec oscillation period of the hemisphere. The copper wire that gave this 30-sec period was 33 cm long and 0.32 mm in diameter, compared to the 1.7-cm, 0.7-mm dimensions of the nylon fiber. Whereas this produced the needed period, the damping coefficient was substantially smaller, so that when the pendulum was stimulated with a puff of air, it took substantially longer for the oscillations to damp down. This was due to the higher elastic coefficient of the copper metal compared with the nylon fiber. Fortunately, this did not have a significant effect on the results obtained from the experiments that were performed with a subject.

Figure 10 shows an experiment that was performed with both the hemisphere and the subject thoroughly grounded. The pendulum was stimulated in the usual way with a puff of canned compressed air and allowed to oscillate freely for 6 minutes in order to establish the natural frequency

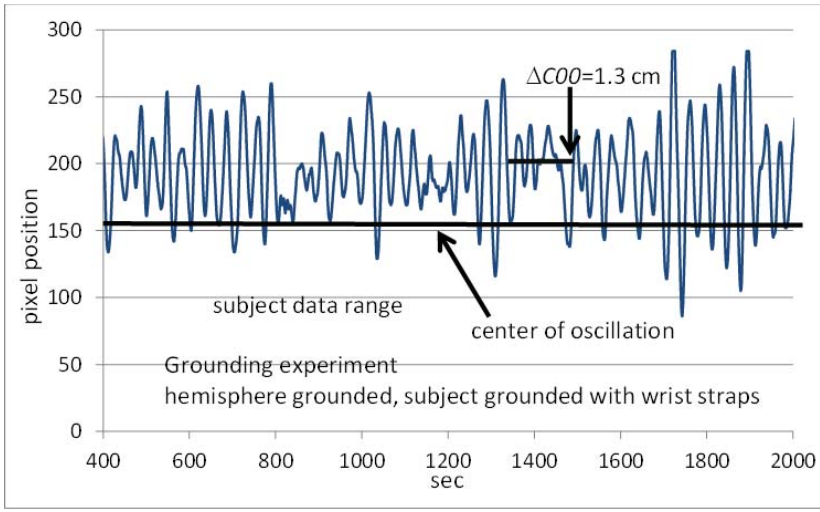

Figure 11. Pendulum oscillations during the Figure 10 time period "subject data range."

of oscillation of the pendulum. The subject then sat under the pendulum without touching it in any way. Data were collected with the subject present for about 30 minutes, whereupon the subject carefully left the pendulum and left the area. The time period when the subject was present is labeled "subject data range" and the black line shows the time period involved. The 43-minute time period after the subject left the pendulum is labeled "post-subject data range."

These data can be compared with data from the experiments in the main article in which neither the subject nor the hemisphere were grounded. They are qualitatively exactly the same, in which the "pre-subject data range" shows the conventional oscillations of the pendulum, the "subject data range" shows wild fluctuations in the oscillations of the pendulum, and the "post-subject data range" retains the anomalous oscillations for 30–60 min. To better view the oscillations that occur during the "subject data range," Figure 11 focuses on this time period.

We see the same kinds of patterns in the oscillations here as we consistently see in all of our experiments (see main article). Namely, there is a substantial shift away from the natural center of oscillation (*COO*), and there are many frequencies in addition to the natural frequency. Note that the shift away from the natural center begins immediately after the subject is seated under the pendulum, and continues to be shifted throughout the

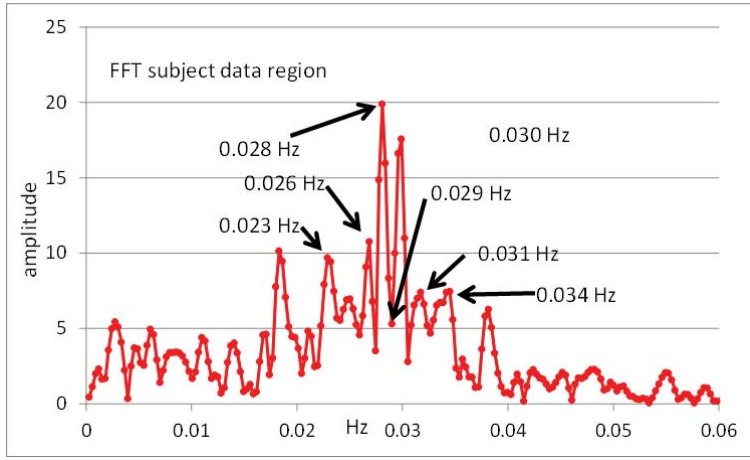

Figure 12. FFT (Fast Fourier Transform) analysis of Figure 11 "subject data range."

entire time the subject is present. The magnitude of this shift away from center is about 1.3 cm. The frequencies represented in this data range were determined by FFT analysis, shown in Figure 12.

This frequency pattern can be compared with the FFT analysis of the pre-subject data region (Figure 13), which shows just a single frequency peak, which is the natural frequency of the pendulum.

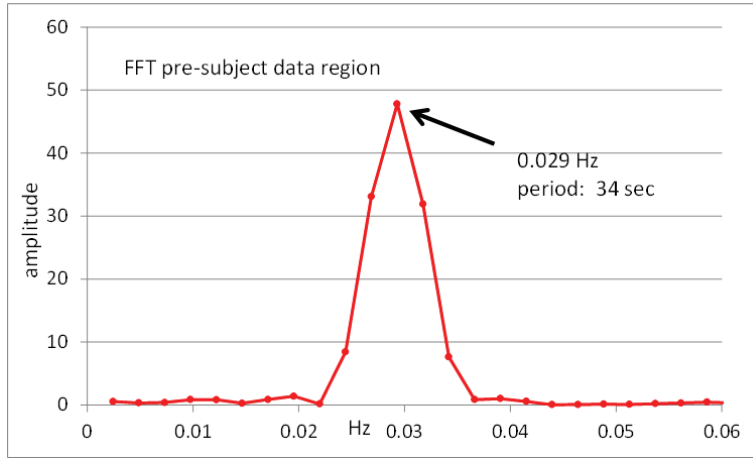

Figure 13. FFT (Fast Fourier Transform) analysis of Figure 11 oscillations in the pre-subject data range.

This experiment also shows that after the subject departs from the pendulum, the frequency patterns that were observed when the subject was present persisted after the subject departed, but at lower amplitudes. Figure 14 shows the FFT analysis after the subject departed from the pendulum.

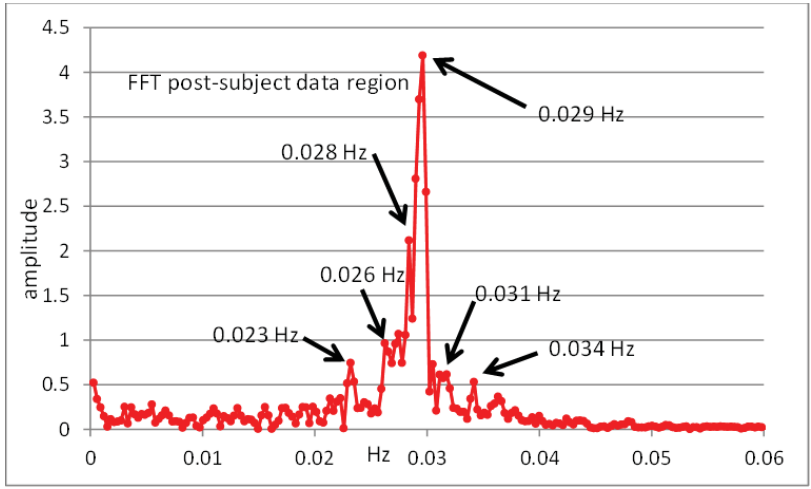

Figure 14. FFT (Fast Fourier Transform) analysis of Figure 11 oscillations in the "post-subject data range."

It is evident that the frequency patterns that were present during the time that the subject was present persist after the subject departs. This is what was observed in the experiments reported in the main article.

To show that this pattern of frequencies is unique to the presence of the subject, Figure 15 shows a run in which the same pendulum was stimulated by a puff of air, but no subject was present.

Figure 16 is the FFT analysis of this non-subject run.

We believe that these experimental results demonstrate that static electricity cannot explain the experimental results in our main article. The steel-mesh hemisphere is conductive throughout, so static charges should be distributed uniformly. Moreover, if charges were localized, it would be necessary for them to constantly move around to produce the observed results. The experiments in which both the pendulum components and the subject were grounded alleviate concerns about static charges, which should be dissipated through the ground connection, thus eliminating any static charge forces between the pendulum and the subject. Once everything has been grounded, there should be substantial differences between the grounded

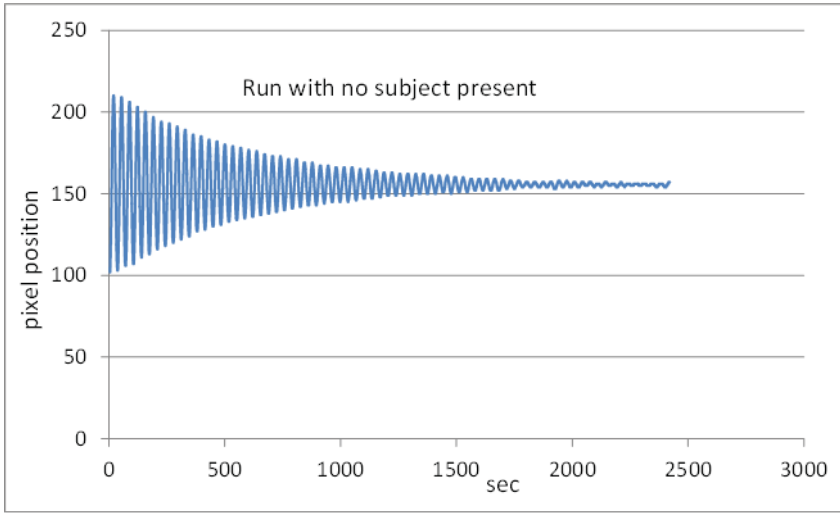

Figure 15. Pendulum oscillations in the absence of the subject.

results and the ungrounded results if static charge effects are significant. There were no significant differences in the phenomena observed in the grounded experiments versus the ungrounded experiments shown in the main article. We therefore conclude that static charges cannot explain our observations.

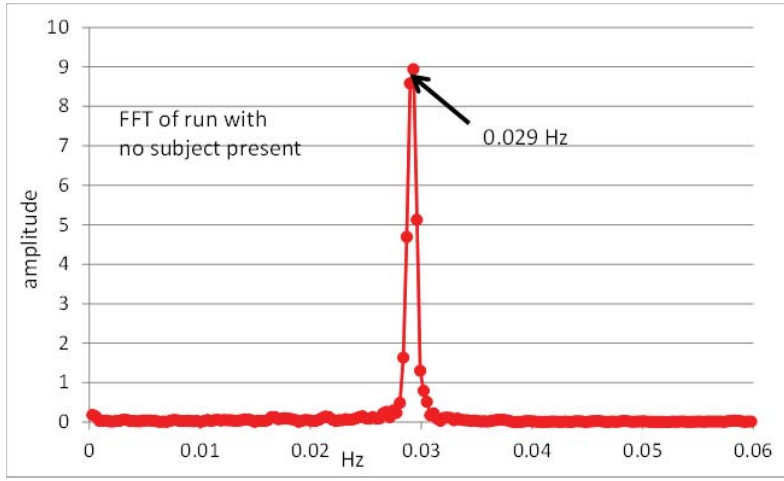

Figure 16. FFT (Fast Fourier Transform) analysis of Figure 15 data in which no subject was present.

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RESEARCH ARTICLE

Geometry of an Intense Auroral Column As Recorded in Rock Art

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Abstract—In 2003, Peratt demonstrated that rock art images worldwide bear a remarkable similarity to high-energy plasma discharge formations. In later papers, Peratt located the plasma discharge column in which all of these would have occurred at the Earth's South Pole. This article accepts the relation between the rock art images and the plasma formations, but concludes that the geometry of the reconstruction is incompatible with the global occurrence of the rock art images. As a corollary, the finer details of the reconstructed column must also be called into question. In particular, the reconstruction of the top cusp, the two upper plasmoids, and the filamentary sheath in a single column at the South Pole cannot be reliably deduced from the data as presented by Peratt. All evidence points to a worldwide distribution of the phenomena.

Introduction

Between 2003 and 2008, the American plasma physicist Anthony Peratt published three articles presenting evidence for a high-energy–density aurora as recorded in prehistoric rock art around the world.

In the first article, Peratt established a remarkable correlation between rock art image types and similar forms arising in high-energy plasma z-pinch discharges recreated in the laboratory and in particle-in-cell computer simulations of the same discharges (Peratt 2003). Peratt demonstrated that the rock art image types have similar forms worldwide, suggesting that these images were not random doodles or abstract carvings, as often is assumed by archaeologists, but representations of events visible in the sky above prehistoric man. The case Peratt made for the association of worldwide rock art and high-energy plasma events is impressive and entirely consistent with a similar suggestion, apparently unknown to Peratt, made by George Siscoe in 1976 (Siscoe 1976).

Peratt further observed that the millions of examined rock art figures share a preferred orientation. Using holographic computer software, this enabled determination of the likely location of the plasma events in the sky that inspired the ancient artists. The outcome of the initial stages of this research was presented in the second paper, wherein Peratt for the first time located his colossal plasma column of complicated morphology at the rotational South Pole (Peratt et al. 2007). Although Peratt presented this object as a straight, cylindrical structure extending into space from the Pole, he hinted that ongoing research revealed a significant easterly bend in the column (as seen from Australia), thereby explaining its worldwide visibility.

In the third article, Peratt reproduced the straight south-polar column from the previous paper, together with additional survey evidence for the postulated southern location, again hinting at evidence for a bend in the column (Peratt & Yao 2008). He also promised that further aspects of the column's evolution would be published elsewhere, but as no further papers have as yet been forthcoming, this remains, in brief, the situation to date.

The current article accepts the correlation between plasma events and a portion of rock art, but questions whether the proposed reconstruction of a single south-polar column as the event that inspired the worldwide rock art can be supported by the data presented in Peratt's published works.

Although Peratt conducted rock art surveys with a team, some of whom—including van der Sluijs—appeared as his co-authors, he was the actual author of all relevant publications and was solely responsible for the analysis of the data and the laboratory experiments. For convenience, Peratt's team will accordingly be referred to as Peratt.

Peratt's South-Polar Column

According to Peratt, rock art sites were surveyed in 139 countries. For each site or panel, photographs of the images themselves were supplemented with measurements of the geographic coordinates (latitude, longitude, and altitude) as well as the field-of-view (FOV) and the angle of inclination off horizontal of the southern—and sometimes eastern—skyline as viewed from the site. Peratt combined the survey data with the rock art images themselves to create a series of 'pixels', which were processed using holographic software to regenerate the original formation of which the images were supposedly accurate views from the locations concerned.

Peratt presented the results of the reconstruction in the form of a diagram showing a single plasma column with complicated morphology (Peratt et al. 2007:802, Figure 66; Peratt & Yao 2008:9, Figure 10; compare with the more generic diagram in Peratt 2003:1193, Figure 4).

In brief, the reconstructed column contains the following features:

- The column is located above the Earth's south rotational pole.
- A tall, narrow stem supports a wide cusp at the top. The tentative dimensions of the cusp are given as 50,000 km wide; the column is 701,000 km high (Peratt 2003:1211, Peratt et al. 2007:802). The cusp therefore subtends an angle of circa 4° for an observer on Earth. The stem of the column is clearly seen as being much narrower than the cusp, subtending a smaller angle at the observer's position.
- Below the cusp, but still in the upper part of the column, two "egg-shaped plasmoids" some two to three times the diameter of the narrow stem bulge out from the stem itself.
- Below the prolate plasmoids, the lower part of the stem surrounds nine small toroids in collinear arrangement along the axis of the stem.
- The whole column, including the top cusp, is contained within a funnel-shaped sheath formed of longitudinal filaments, which bulge out as they pass each of the upper plasmoids. These filaments are identified as Birkeland currents.

An adjacent diagram shows a conceptual view of the Birkeland currents flowing around the Earth (Peratt et al. 2007:802, Figure 67). The filaments form a narrow hollow cylinder extending into space above both the Arctic and Antarctic regions; the filamentary cylinder bulges out as it passes around the Earth itself.

Scale

Plasma configurations are scalable in principle, but it is unclear how Peratt determined the scale of his intense aurora. Today's aurorae are formed at heights above the surface of between 80 and 1,000 km, where the Earth's upper atmosphere interacts with inflowing electrons. Peratt offered a figure of 701,000 km for the "farthest limit of the reconstruction" (Peratt et al. 2007:802). The sheer scale of such a column militates against its interpretation as an aurora, exceeding the average upper limit of the Earth's ionosphere by a staggering factor of 700; it even dwarfs the magnetosphere, averaging circa 64,000 km in thickness, by a factor of 10.[1] Simply labeling the phenomenon 'intense', 'enhanced', or 'high-energy density' provides insufficient justification for the necessary expansion of the Earth's atmosphere, which raises a batch of other questions in itself. Peratt has stretched the application of the term *aurora* to extremes, for no stated reason.

Global Visibility of a Single Stationary Straight Column

Peratt postulated that a single auroral column coming into the Earth at the South Pole "was universally seen"; "what could be observed would depend on the observer's location on Earth and whether or not the entire column was visible or illuminated, or some portion of it, as in auroral displays today" (Peratt 2003:1203). Observers at northern latitudes recorded primarily the upper portions of the *z*-pinch; those in the far north, upwards of 50°, saw almost nothing of the plasma activity. In his published diagrams, Peratt modeled this column as a straight, cylindrical object, of varying though generally modest width (e.g., Peratt 2003:1193, Figure 4, Peratt et al. 2007:802, Figure 66 and Figure 67).

Like other art forms, rock art representative of instabilities in the inner shaft is apparently attested worldwide. A petroglyph showing a classic squatterman image—the so-called Pippi Stone—was found as far north as 69°, at the northernmost known prehistoric rock art site in the world.[2]

Yet even before addressing the question of what parts of the column were or could be seen from individual locations on the Earth, the global visibility of a single stationary column per se presents an insurmountable problem. It is inconceivable how a straight column located at the Earth's rotational South Pole could be discerned worldwide (Figure 1). A south-polar column must intersect the local horizon or it cannot be seen. It would not be visible from anywhere in the Northern Hemisphere, unless there was a negative angle of inclination of the southern skyline with a magnitude in excess of the northern latitude of the observer. Conversely, neither would such a column be visible from the Southern Hemisphere from any location where a positive angle of inclination with respect to the view exceeded the southern latitude of the observer. For magnetic south, the situation differs somewhat on account of the displacement of the geomagnetic poles from the rotational poles. Even so, a phenomenon occurring above magnetic south is visible only from viewpoints in that hemisphere of the Earth in which magnetic south forms the Pole.

In order to be visible at all from mid-northern latitudes, the cusp of the south-polar object would have to be enormously wide, extending so as to intersect the visible horizon for the latitude—and even then, the center of the column would remain invisible. Peratt never suggested that the column or any part of it extended to such width; as seen above, if the "dimensions" of the outer cusp region, presumably its diameter, measured 50,000 km, and the column attained a height of 701,000 km, this would subtend an angle of a mere 4° or so. A slim column such as Peratt envisioned, no matter how tall, could never be discerned from the Northern Hemisphere.

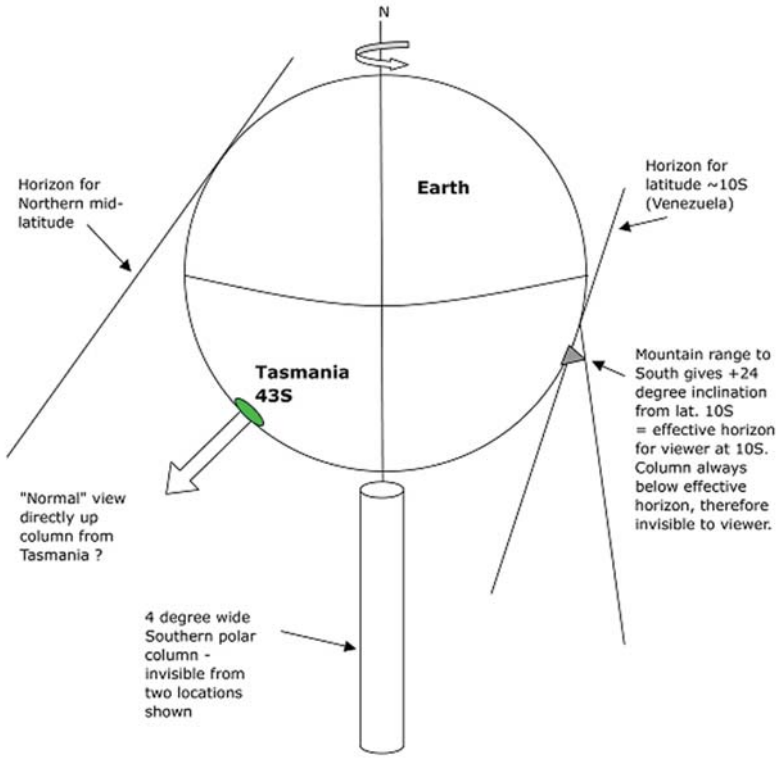

Figure 1. Visibility of a south-polar column relative to the horizon. There can be no visibility of anything below the horizon, or effective horizon, where there is an angle of inclination to a cut-off. Tasmania (43° South) apparently had a view directly up the column. © R. Johnson

In sum, Peratt's notion of a single, straight, and stationary column visible worldwide is untenable. If it is irreconcilable with the worldwide distribution of the pertinent rock art images, do Peratt's conclusions regarding orientation, perspective, and inclination fare any better?

Orientation

Other geometrical problems become apparent upon inspection of the southern orientation Peratt claimed for the column.

As Peratt's model evolved, the hypothetical enhanced aurora tacitly but abruptly shifted from the magnetic South Pole to the rotational one, over Antarctica. Initially, Peratt proposed that petroglyphs were carved in locations with a "line-of-sight to the Earth's magnetic poles and highly

conducting regions on the Earth's surface," which are "the criteria of an intense aurora today" (Peratt 2003:1199). In other words, at this stage Peratt was still discussing the evolution of plasma events in relation to today's auroral phenomena, with the lines of the Earth's magnetic field defining the paths of the incoming electrons. Between April and October 2005, Peratt changed direction without further comment. From then on, he would invariably locate the reconstructed column at the rotational South Pole. For example:

> . . . the light was observed totally from the direction of the south axial pole of Earth. (Peratt et al. 2007:801, Peratt & Yao 2008:2, cf. 6,8,11; compare with Peratt et al. 2007:779–780,796)

In Peratt's diagram (Peratt et al. 2007:802, Figure 67), it is unquestionably the geographic pole, not the magnetic one, that is directly below the column. Moreover, from October 2005 on, Peratt would correct compass measurements obtained in the field for the local magnetic declination, but had apparently not done so previously. At that stage, the rotational pole dominates his discussion of the survey data and the reconstruction, while the column's association with the magnetic pole is quietly relegated to the few occasions where Peratt touched on the physics of plasma (e.g., Peratt et al. 2007:797,799,800, Figure 61, 805). No explanation for the discrepancy is given.

If Peratt's survey data suggested an association of the plasma events with the rotational pole, it is surprising that the phenomenon observed by the artists did not follow the normal plasma behavior with electrons guided by the magnetic field lines, unless it were assumed that the magnetic and rotational poles coincided during the era concerned. However, if the data acquired during the first few years of the investigation had so unambiguously pointed to magnetic south, one wonders whether Peratt ever corrected these for magnetic declination to verify whether they are consistent with true south as well.

Peratt's treatment of directionality in rock art is further compounded by his indiscriminate conflation of two types of south. On one hand, Peratt's texts liberally employ the terms *south pole*, *south polar axis*, *south axial (pole)*, *south polar horn*, *true South Pole*, *south(ern) axis*, *south seeking pole*, and *south(ern) magnetic pole*. These refer to the Earth's rotational axis, its rotational poles, and its magnetic poles, all of which are definite geographic *locations* relative to the surface of the Earth. On the other hand, Peratt frequently used phrases such as *true south*, *south field-of-view (SFOV)*, *south FOV (SFOV)*, *southerly direction*, *due south*, *southern sky*,

south-facing, and *southwards*. This group relates to a *direction of view* from an observer's location on the Earth, as in surveyed data, and may conveniently be referred to as local south. The two groups of geodetic terms must be carefully distinguished.[3] As noted above, the celestial South Pole is always visible from the Southern Hemisphere, but remains below the horizon for viewers in the Northern Hemisphere.

Peratt seems to have been oblivious to this crucial distinction. With his equivocal use of terms, he apparently committed the logical error of equating a view toward local south with one that includes the celestial South Pole. According to Peratt's prevaricating diction, the south-polar column was observed at sites around the world—including in the Northern Hemisphere—in a portion of the sky oriented to "polar south" (Peratt et al. 2007:78, cf. 796). For example, Peratt wrote with respect to the column:

> Because of the latter's orientation at the south axis, all archaic petroglyphs have at least one polar south viewpoint. (Peratt & Yao 2008:4)

This one simple mistake may underlie the entire set of problems in Peratt's texts relating to field-of-view, inclination, and the visibility of a southern column from the Northern Hemisphere.

Additional confusion is caused by Peratt's concept of a "Cage" formed by individual Birkeland currents flowing around the Earth in the fashion of meridians. Under the heading "*Observations from the Northern Hemisphere*," Peratt interpreted a number of images from the Columbia River Basin in terms of the south-polar column with its "egg-shaped plasmoids" (Peratt et al. 2007:802), but, as noted above, the plasmoids could not possibly be visible at all at this latitude, while the filaments constituting the "Cage" would appear overhead and all around instead of at true south. On the Southern Hemisphere, meanwhile, Peratt invoked the Birkeland currents surrounding the Earth in order to account for the Nazca lines and similar features, misapplying medieval European and Chinese descriptors of the aurora borealis such as "'swords', 'spears', 'white vapor', 'like glossed silk penetrating it', and 'candles in the sky'" (Peratt et al. 2007:804, Figure 71), to some of which van der Sluijs had originally introduced Peratt. Apart from that, Peratt further linked the filaments encapsulating the Earth to "Vertical striped petroglyphs or vertical white-striped pictographs," as found "worldwide" (Peratt et al. 2007:804). Although this is an interesting proposition, the global visibility of the cables and their ostensible depiction in rock art and geoglyphs sits uncomfortably with Peratt's earlier claims that, for all petroglyphs, "the light was observed totally from the direction of the south axial pole of Earth." If there is any validity in Peratt's hypothesis

of a filamentary "Cage" represented in prehistoric art, Peratt ought to have evinced more clearly that it concerns a separate class of striped images to which the putative southern orientation does not apply. Instead, Peratt explicitly stated that petroglyphs representing the "Cage" had been included in the data survey (Peratt et al. 2007:781, Figure 2, Figure 40, 802), adding: "The characteristics of the Nasca–Palpa lines and geoglyphs differ in no way from the parameters determined for petroglyph locations worldwide" (Peratt et al. 2007:804).

Perspective

Additional difficulties arise when, based on the faulty model of a single stationary column, a consistent match is assumed between the geographic coordinates of terrestrial viewpoints and changing perspectives on the column.

The interpretation of individual rock art images and related art forms as local perspectives on a single celestial phenomenon requires a determination of scale, perspective, and temporal evolution. Peratt interpreted dotted circles and concentric circles as bottom-up renditions of a diocotron instability affecting the hollow outer sheets of the *z*-pinch (Peratt 2003:1209–1210,1212), while he derived 'ladders', 'caterpillars', 'birds on sticks', 'squattermen', 'Kokopelli', and many other forms from instabilities in the solid inner core of the lower segment of the auroral beam, viewed sideways or at an oblique angle (Peratt 2003:1193–1205). So far, so good—the matches between these respective instabilities and their petroglyphic correlates are indeed compelling. Problems appear when the geographic distribution of such designs is taken into account. If the hypothesized auroral column was stationary, as Peratt suggested, one would expect a geographic distribution of the two categories of 'hollow' and 'solid'-type instabilities, but both classes of perspective appear to occur wherever non-figurative rock art is found. Peratt determined that concentric designs—which are usually circular—occur between circa 59° North (as at Oslo, Norway) and 33º South (in South Australia), citing Stonehenge and petroglyphs from Australia, Arizona, and Oregon. At the same time, he adduced axial images from Australia, the southwestern United States of America, and "Europe", including Spain, Italy, and Tyrol. Indeed, illustrations of respectively an axial and a lateral perspective on Peratt's auroral column frequently appear at the very same sites, as could be demonstrated abundantly.

Peratt presumed that the worldwide concentric petroglyphs, geoglyphs, and related monuments represent views of one and the same phenomenon, with the viewing angle varying with latitude as one would expect. However, on closer inspection, this geometry is suspect. Peratt compared concentric

images from northern Arizona (35° North, 109° West) and the Columbia River Basin (45° North, 120° West) with Stonehenge (51° North, 10° West) (Peratt 2003:1209–1211). While the comparisons are impressive in themselves, calculations show that it is not possible to identify any location on Earth where the geometry of an auroral ring in even a greatly extended ionosphere would allow circular and tilted concentrics to be drawn in the places where they are actually found. Restricting the auroral ring to the South Pole, whether rotational or magnetic, simply exacerbates the problem. If concentrics indicate a local field-of-view up into a laminated column, they cannot all represent the same static column, wherever it be located, unless the dimensions of the Earth are ignored and the Earth is treated as a point particle. This hardly seems to be a realistic solution to the geometric puzzles.

Consistency in perspective would require that circular designs at locations directly below the assumed celestial phenomena give way to ovals in other places. Peratt expressly endorsed this when he professed that a petroglyph at the Columbia River Basin that shows rayed and dotted concentric circles, when compared with Stonehenge, "indicates a small obliqueness of observation as seen from the Columbia River" (Peratt 2003:1209–1210,1212). However, this statement is inconsistent with latitude: Stonehenge to the north may be more circular than the image from the Columbia River Basin, but so is the figure from Arizona to the south. As Stonehenge and Arizona are also separated by 99° longitude, it is impossible to find a single location for the column anywhere that can satisfy these geometries. Most certainly, a column at the South Pole cannot suffice.

Peratt also implied consistency in perspective when he compared an "ellipse" from Windjana Gorge (Western Australia; 17.6° South, 126.5° East, not West as Peratt stated) to a petroglyph from northern Arizona, for he "fitted" the ellipse to "the outer concentric of the Northern Arizona petroglyph" as he "digitally tilted" it "at an angle of 45.3°" (Peratt 2003:1209,1211, Figure 47). However, when the correct longitude of the Australian image is considered, the two sites are 128° apart on the great circle between them. A column vertically overhead at one site would not be visible at all at the other.

The expected neat geographic distribution of circular and oval designs is not found. Peratt's estimated distribution of concentric petroglyphs covers almost the entire inhabited part of the world and the southernmost limit may even have to be extended to 43° South, in keeping with what Peratt dubbed the "Tasmanian Paradox" or "why are the petroglyphs so dominated by circles," especially if this was "due to a geometry of FOV up into a concentric column" (Peratt 2003:797). As there is no known restriction on

ovals to the north or south of the boundaries for concentrics, it appears that ovals and circles both enjoyed a practically global distribution.

Peratt's claims on perspective founder in other respects, too. Peratt complemented the rayed circles from the Columbia River Basin with rayed concentric arcs from the same region (Peratt et al. 2007:803, Figure 68). A relationship of some sort between the latter two petroglyphs appears likely, but the differences must be accounted for, such as the omission of rays and the extension of the lowest circle into a 'neck' in the bottom image. As the images are geographically close, a different perspective on the same phenomenon is only plausible if they represent different stages in time for a moving—and evolving—object, but Peratt did not state this and insisted on a stationary column.

All of the above vitiates Peratt's proposed solution of a single stationary column. The only possible solutions to the distribution of the concentrics are that the auroral ring is allowed to change location over time and that there were different rings over a period of time.

Inclination

Based on in situ measurements, Peratt suggested that, between latitudes of circa 45° North and circa 25° South, there is always a cut-off in the direction of true south at an angle of inclination of +31°, above which no petroglyphs are found: All are located so that each has at least one south field-of-view with the constraint that no object to the south subtends an inclination off horizon to the observer of more than 31°; nearly all fall within the range of angles 24°–31° (Peratt & Yao 2008:9). Apparently, the column did not extend above the cut-off angle. A lower limit for angles of inclination is set by the carvers' apparent use of blinders. Blinders were required wherever the light of the column was too bright, that is to say, the lower part of the column. For latitudes between circa 45° North and circa 25° South, blinders blocked any light below an inclination of +24°: "The lower value assures that the bright synchrotron radiation at direct polar south is shielded from the observer's eyes. This can be a southern mountain range or a local boulder" (Peratt & Yao 2008:9).[4] Thus, for these latitudes Peratt defined three zones of visibility to the south: from 0° to +24°, where the column was seen, but too bright to be carved; from +24° to +31°, where the column was seen and carved; and above +31°, where the column was not seen (Figure 2).[5]

The geometry of the column becomes even more perplexing when these figures for a fixed inclination off horizon are taken on board. First, for any stationary column the zone of visibility, as defined by inclination off horizon, ought to vary commensurately with latitude, yet Peratt postulated the same rigid set of figures for all latitudes between circa 45° North and

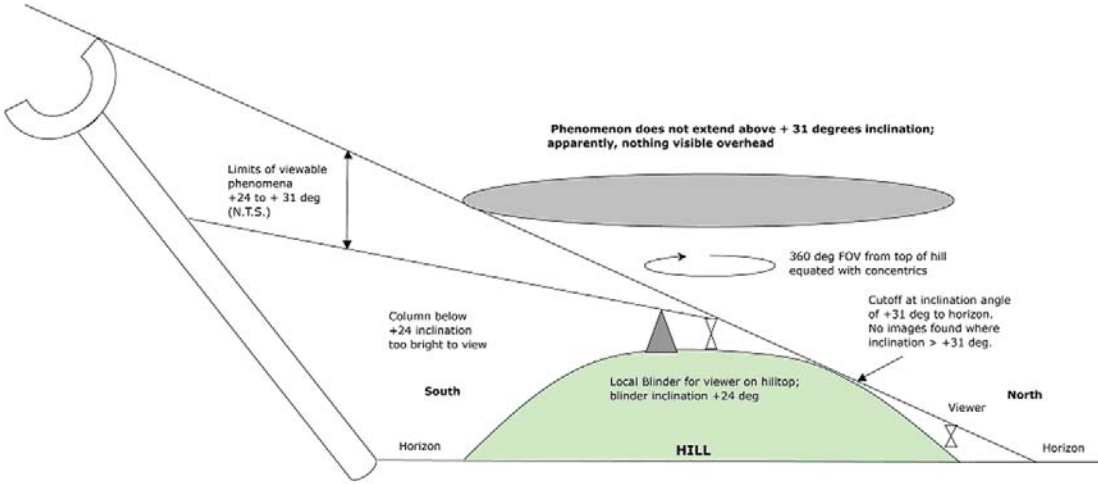

Figure 2. Effect of inclination cut-off on visibility.
Blinders and cut-off imply that all viewable phenomena were within +24° to +31° of the southern horizon. Concentrics are common on tops of hills, implying a 360° view within the above limits. © R. Johnson

circa 25° South. Second, if all petroglyph sites have a minimum inclination of +24°, no part of a narrow column of infinite length at the South Pole could have been seen at any latitudes to the north of 24° South, where most petroglyphs occur (compare with Figure 1). And third, in which portion of space were the phenomena occurring such that they never appeared at local inclinations above +31°, for observers between 45° North and 25° South (Figure 3)? Even if the top of the column was somehow visible at an inclination of +31° at any one northern latitude, the same point on the column would naturally appear higher than +31° at a more southerly latitude on the same meridian, and yet the same cut-off is still supposed to apply to the latter. This is inconceivable.

Summing up, it is impossible to find a single location for the phenomenon anywhere in space which can satisfy this geometry of visibility over the range of latitudes concerned.

In an enigmatic passage, Peratt explained how a rock artist's field-of-view on the celestial spectacle tends to change as one descends from a summit:

> Petroglyphs carved at the top of a hill or peak may provide a 0°–360° FOV, only one direction that the artist was sighting. . . . Well-drawn concentrics are often found in greater numbers at these locations, or high up on an escarpment.

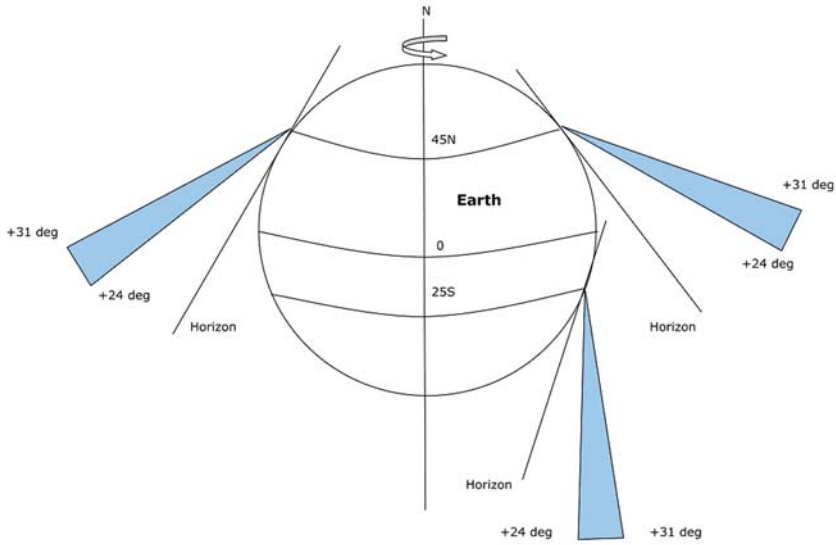

Figure 3. Limits of visible phenomena applied to different latitudes, using Peratt's window of inclination.
Not to scale. © R. Johnson

> Petroglyphs carved on the north side of a slope occupy an increasingly narrower portion of the compass with an FOV centered on 180° South as the distance from the peak increases. A null (void of markings) region is reached at an inclination of +24° to +31° downward from the peak whose location at which the artist used local blinders. This description is also applicable to petroglyphs carved on the east, west, or south slopes downward from the peak. (Peratt et al. 2007:796, paragraphing added) [all sic, MAS]

Typographical errors and confusing phraseology aside, this passage is riddled with obstacles.

The first few sentences seem to concern the *narrowing sector of the sky* visible from locations with petroglyphs as one descends a peak. At the top, the sky is visible in all directions; further down, the section of the sky visible from carved rocks occupies an ever narrower portion of the compass, closing in on true south. Because at lower elevations more obstacles block the view, the carvers were supposedly forced to select only those places where true south was still visible. Thus, petroglyphs at lower elevations are more diagnostic of the portion of the sky in which the column was seen than those at higher locations.

Apparently, Peratt then continued with a statement on *inclination*:

All petroglyphs occur in places where the local angle of inclination to the horizon is between +24° and +31°, but in mountainous terrain such places tend to be rarer at lower elevations. Thus, petroglyph fields often fizzle out toward the base of a peak in an area Peratt calls the 'null region'. On summits, blinders had to be distant mountain ranges, neighboring peaks, or a large rock on the peak itself (compare with Figure 2). Downslope, the peak itself might also function as a blinder.

With the final sentence, Peratt probably meant to say that the same two observations apply on all four sides of a peak.

If the above represents Peratt's views correctly, the following objections apply.

First, if a 'null region' is a petroglyph-free zone, one would not expect it to be *at* elevations where the skyline is inclined between +24° and +31°, but *below* such elevations.

Second, the argument that elevation correlates with restriction of petroglyphs to places with a view on true south is valid only on the assumption that the same stationary phenomenon was recorded in all cases. While this could be demonstrated more conclusively at lower elevations, it is hard to verify that the "one direction the artist was sighting" at peaks was always "180° south" when other directions were equally available. As the inclination was only ever measured for the southern field-of-view, it is not possible to compare data for other directions.

Third, Peratt's observation that "Well-drawn concentrics" concentrate on summits, if true, suggests that the auroral phenomenon was panoramic at such locations, being visible all around the horizon and up into the zenith. This meshes well with Peratt's contentions that such petroglyphs represent a view up into the hollow laminated column (Peratt 2003:1207–1212; cf. Peratt et al. 2007:797) and that the column formed a "Cage" surrounding the earth on all sides (Peratt et al. 2007:802, especially Figure 67). As the observer was situated 'inside' the cage, the filaments surrounding him or her would seem to converge at some latitude-dependent point in the sky, similar to the appearance of a 'starburst' pattern centered on the magnetic zenith as seen in an auroral corona today. For observers at latitudes between 31° and 90° South, the point of convergence would appear at a higher angle of inclination to the horizon than +31°, as Peratt seemed to allow. From vantage-points between 31° and 90° North, the convergence point above the South Pole would be invisible, below the southern horizon, but presumably the one above the North Pole would appear, again at inclinations exceeding +31° that *would* contravene Peratt's upper limit. And all observers, regardless of latitude, would see some filaments pass directly overhead, again contradicting Peratt's limits on inclination.

An alternative interpretation, apparently favored by Peratt, is that carvers at summits were only viewing south, sighting "only one direction" despite the panoramic views they enjoyed. The concentrics then also complied with the inclination limits of +24° to +31°, and Peratt's phrase "up into a concentric column" refers not to an appearance at the zenith, but to an oblique view into the column, whose base is directed toward the viewer. On this approach, the carvers' 360° field-of-view is not exploited, the enveloping "Cage" is rigidly distinguished from concentric petroglyphs, and the more general objection to visibility at northern latitudes, made earlier, applies: No part of a narrow south-polar column could have been seen at any latitudes higher than 24° South, including any concentrics. Moreover, as was also noted above, a perfectly circular perspective on concentrics can only be obtained for a wide range of latitudes if the column was mobile or if multiple columns existed.

No such considerations deterred Peratt from using the locations of concentric designs as 'pixels' in the reconstruction of a single south-polar column, as shown in several of his illustrations (e.g., Peratt et al. 2007:803, Figure 68; Peratt & Yao 2008:10, Figure 11).

And fourth, Peratt's statement regarding the application of the description to directions other than the north causes further confusion. In its context, the sentence makes no geometrical sense. Presumably, Peratt merely meant to say that the field-of-view at petroglyphs on the west, east, and south sides is also more narrowly oriented toward true south at lower elevations and that petroglyphs on these other slopes are also limited to places with a southern field-of-view within the stated range of inclinations from +24° to +31°. Even so, the sentence is awkwardly worded and easily induces the impression that each of the four sides offers a similar view on the south-polar column—which, of course, they do not.

In short, the extract quoted above epitomizes the apparent confusion about directionality and basic geometry that runs through all Peratt's papers.

Global Visibility of a Single Stationary Bent Column

Since December 2003, Peratt has been well aware of the complexities posed by perspective in relation to a straight column at the South Pole. Although he continued to portray the column as such in his diagrams, he also began to allude to a conspicuous bend in the auroral column, based on data to the south of mid-northern latitudes. For example:

> In South Australia, a bend in the plasma column far above the Earth was noted. Nearly normal to Antarctica, the column bends eastward as seen from Australia and presents an increasingly 'stretched' columnar profile for

> New Zealand and more so for South Africa. . . . At more southerly latitudes, the angle of inclination changes, as does the plane of the blinder, showing an eastward bend of the plasma column away from Antarctica. (Peratt et al. 2007:796,780)

Peratt did not conceal that the concept of this bend was introduced precisely to circumvent the problem of universal visibility and perspective-based distortion addressed above, for it concerned "a bend in the upper filament sheath that allows the upper plasmoids and column to be seen at northern latitudes" (Peratt et al. 2007:802), or rather "at the equator and both northern and southern latitudes" (Peratt et al. 2007:797).

Though "far above the Earth," the postulated bend must necessarily have been located beneath the "upper plasmoids and column" for them to have been rendered visible in the Northern Hemisphere. Thus, if computer models combining a particle-in-cell simulation with surveyed data had reliably dictated the morphology of the column as shown in Peratt's diagrams (e.g., Peratt et al. 2007:802, Figure 66 and Figure 67), the bend ought to have shown up there. Peratt conceded as much with his promise of a "higher resolution image showing the easterly curving of the auroral plasma column"; however, such an image would not be a matter of "resolution," as the bend would have manifested equally well in a low-resolution image. Arguably, said diagrams were generated by software programming that took the natural evolution of a plasma *z*-pinch into account, but not the measured orientation of petroglyphs.

Peratt presumed that the bent column was 'stationary' in space, while the Earth rotated underneath it. This inspired his comparison of the bent column to a mill-handle, the handle of a giant butter churn, and a giant spoon being stirred. For example: ". . . the column bent, swinging around the Earth as if a mill-handle, making images such as these visible to most places on Earth" (Peratt & Yao 2008:8). Rotation of the handle relative to the Earth is obviously necessary to allow the upper parts to be seen at opposite longitudes in the Northern Hemisphere. But this instantly invalidates the putative narrow orientation of *all* rock art to rotational or even magnetic south. To be sure, if an observer in the Northern Hemisphere would perceive the handle as the upper part of the hand of a clock sweeping around the southern sky from east to west, the movement of the hand might indeed with increasing latitude be restricted to an ever smaller section of the compass, centered on true south. However, someone in the Southern Hemisphere would see the handle as a giant arc passing overhead once a day and thus not only transgressing Peratt's boundaries for the angle of inclination, but also taking the column to positions all around rather than keeping it confined

to the south. Furthermore, the angle of inclination at which the top of any bent column would be seen should still be subject to the same latitudinal dependence as any other object in the sky; bending the column does nothing to salvage Peratt's rigid boundaries for the inclinations.

The only conceivable 'solution' to the widespread visibility of a bent column at latitudes ranging from circa 69° North to circa 46° South requires the sacrifice of all inclination data as well as the global restriction to true south and assumes that the top of the column was located at or above circa 21° South on the celestial sphere, so as to be visible above the horizon at circa 69° North. The visibility of the upper parts of the column would depend on the luminance relative to daylight, a point Peratt touched on inconclusively in 2003 (Peratt 2003:1194). On that occasion, he suggested that a scaled-up laboratory plasma might reach a peak luminance of 5 lumen per steradian per square meter, or 1/120th that of the full moon. Accordingly, the upper parts of the plasma column would have been visible only at night. This orientation of the bend, combined with its nocturnal visibility, suggests an association with the magnetotail in the same sector of the sky, whose dimensions are also more in line with the enormous scale of Peratt's column, at 701,000 km.

This scenario implies that the annual variation of the direction of the Earth's axis to the ecliptic must also be taken into account. Such variation might be supported by the coexistence of circular and oblique concentrics at the same locations, as noted above for the Columbia River Basin, which is otherwise inexplicable without recourse to a moving and evolving column. Yet the admission of annual variability in perspective precludes a straightforward use of survey data to recreate a single event from holographic pixels, as Peratt claimed to have made. Without knowing the time of year, the data are meaningless. The alternative, that the Earth's ecliptic plane was not yet tilted with respect to its equatorial plane, would require an intolerable degree of special pleading.

As an additional consideration, the notion of the column as a "mill-handle" seen to rotate around the Earth impairs the identification of the column with the mythological axis mundi. It apparently informed Peratt's repeated enquiries whether any human traditions presented the celestial column as an object moving along the horizon. The answer to that enquiry was far from straightforward; from the perspective of traditional cosmologies, the sky column was certainly not conceived as a cylinder passing along the horizon in the course of a day, but relevant recurrent themes might be the rocking of the nascent Earth prior to the fixative effect of the column (van der Sluijs 2011:I: 135–137), the comparison of the column to a spinning mill (van der Sluijs 2011:III:159–160), and the swaying of the

upper part of the sky column that preceded the final collapse (van der Sluijs 2011:IV:65,67–69,91,112). Although all such traditions impute some sort of repetitive motion to segments of the column, none portray the column itself as a mobile phenomenon, prone to the effects of the Earth's axial rotation.

Further details regarding the conjectured bend, as promised by Peratt, have never materialized. Perhaps this is because, upon reflection, no bend—of whatever magnitude or height—can resolve all the geometrical problems of visibility around the globe without completely undermining the detailed reconstruction of a southern column from the survey data. If the basic geometry of the reconstruction has to be modified to include a bend sufficient to allow the column to be seen around the world, how much credibility can be maintained for the claimed accuracy of the reconstruction of the straight column? In relation to Peratt's published diagrams and statements on the auroral column, this post hoc solution to the universality of the pertinent petroglyphs simply seems inadequate.

Number of Columns

Finally, in postulating a single south-polar column, Peratt repeatedly admitted that a corresponding plasma tube would be expected for the North Pole in theory, but the directionality he inferred from petroglyph data did not sustain that possibility (Peratt et al. 2007:797–798,805). Peratt predicted that the north-polar axis experienced impacts of hypervelocity protons—or protons moving at an extremely high speed—that were "not constrained and would shower the arctic region" at the same time that the Earth's south-polar axis was bombarded with a flow of relativistic electrons, tied to the Birkeland currents (Peratt & Yao 2008:1,11). No more information was supplied, but it is puzzling to find that one of Peratt's diagrams nevertheless complements the southern column with a northern one, the pair representing the incoming and outgoing segments of a single filamentary sheath enmeshing the Earth (Peratt et al. 2007:802, Figure 67). For the rationale for this, one searches Peratt's publications in vain, but the idea may have been that, aside from the "Cage" enveloping the Earth, only the southern column, formed of relativistic electrons, would have lit up, as only electrons—not protons—emit synchrotron radiation.

Conclusion

Peratt has made a very good case for identifying a large number of the images represented in rock art, geoglyphs, and other forms of art as high-energy density plasma discharges, such as might be seen if the aurora were increased by some orders of magnitude. However, the above analysis has

exposed some of the more salient geometrical discrepancies between Peratt's claimed reconstruction of a single plasma column located at the rotational South Pole and the apparent visibility of that phenomenon worldwide.

In summary, the postulated column above the South Pole would not have been visible in the Northern Hemisphere. It is not possible to find a single location on Earth or in the sky that satisfies the worldwide visibility of the phenomenon in accordance with the presented survey data. This undermines the entire claimed 'reconstruction'. The introduction of a bend in the column, hinted at in the later papers but never detailed, cannot resolve the geometric issues either and further calls into question the accuracy of the claimed reconstruction presented in the 2007 paper and duplicated in the 2008 paper.

If the location and the basic shape of the reconstructed column were not accurate, how much credence can the finer details of Peratt's reconstruction command? In particular, the top cusp, the two upper plasmoids, the internal toroids, and the filamentary strands in the reconstruction seem to be based on little more than approximations to various laboratory phenomena rather than on holographic pixels from which a single phenomenon can be deduced. It seems as though Peratt was not presenting a conclusion derived from a holographic reconstruction based on survey data, as he claimed, but rather a hypothesis which he could not, in fact, support.

The analysis presented here points unequivocally toward one conclusion: A large segment of rock art *was* inspired by high-energy *z*-pinch columnar plasma discharges, but these discharges must have occurred worldwide, not uniquely above the rotational South Pole, as Peratt declared. Building on the sound foundation of the similarities between many petroglyphs and high-energy density plasma discharge formations, the search is now on for an alternative interpretation of the sequence of events which could have inspired the creation of the rock art images worldwide.

A promising lead is the potential of *geomagnetic reversals and excursions*. The Earth's magnetic field is dominated by a dipole structure, but also includes weaker multipolar components, such as a quadrupole and even an octupole. During geomagnetic reversals and excursions, the dipole weakens while the north and south magnetic poles move to lower latitudes. As they approach the equator, the dipole field is superseded by the quadrupole. In reversals, the poles continue to wander until they have effectively swapped places; excursions, by contrast, may be seen as aborted reversals, in which the dipole regains strength and the north and south magnetic poles return to their original places.

Peratt had plausibly argued that each of the auroral ovals is actually the base of a column, defined by the funnel shape of the incoming magnetic

field lines and analyzed in plasma physics as a diocotron instability (Peratt 2003:1193, Peratt et al. 2007:798). His contention that enhancement of the aurora renders these columns themselves visible seems perfectly reasonable. Accordingly, geomagnetic reversals and excursions would be expected to feature visible plasma columns moving toward the equator along with the north and south magnetic poles. At a later stage, four or eight other columns would form above the quadrupolar and octopolar components of the field. Practically every part of the world would have a view of these moving and multiple columns at one time or other and from varying perspectives. The internal evolution of each column would follow the sequence of a plasma *z*-pinch, as modeled by Peratt and fellow plasma physicists. Dramatic weakening of the geomagnetic field, facilitating radical but temporary transformations in the structure of the field, thus seems to be a satisfactory key to the enigma of the distribution of plasma-related rock art forms. Whether such events and their causes can actually be identified in the palaeo- and archaeomagnetic records will be examined in a forthcoming study.

Notes

[1] Peratt's (2003:1192, cf. Peratt et al. 2007:797) indication that the magnetosphere at its widest measures 130,000 to 150,000 km exceeds the commonly cited figure of 10 to 15 Earth radii.

[2] Local museum at the rock art site of Hjemmeluft, Alta, Norway, personal observation by van der Sluijs, 26 March 2009.

[3] Ambiguous are *magnetic south*, *polar south*, and *axial south*, all used by Peratt, as well as the common terms *rotational south* and *geographic south*, as all of these are variously used with respect to the poles and in surveying contexts. The words *polar*, *axial*, *rotational*, and *geographic* are here used to indicate a contrast with *magnetic*.

[4] Compare with:

> At mid-latitude in the northern hemisphere, the angle of inclination for polar south at petroglyph locations will range from about +24° to +31°. . . . The southern hemisphere has the same inclination-blinder dependence as the northern hemisphere to about 25° S. At more southerly latitudes, the angle of inclination changes . . . (Peratt et al. 2007:780).

[5] At the northernmost latitudes, only the relatively feeble upper parts of the column were seen.

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RESEARCH ARTICLE

Did Modern Humans Originate in the Americas? A Retrospective on the Holloman Gravel Pit in Oklahoma

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Abstract—For decades, the dominance of the Clovis-first paradigm precluded the possibility of acknowledging a human presence in the Western Hemisphere before 11.5 ka. Yet there are a multitude of sites in the Americas with significant evidence for human occupation dating back to 200 ka and older. At two of these sites, Holloman in Oklahoma, and Hueyatlaco in Mexico, stone tools were found that indicate the possible presence of a lithic technology advanced beyond that found contemporaneously in Eurasia. Culturally modern humans may not have originated in Africa as is currently thought, but in America where evolutionary change was facilitated by geographic isolation. *Homo sapiens* could have re-entered Eurasia from America as early as 75 ka and spread rapidly, displacing archaic *Homo* species. The opening and closing of the Bering Land Bridge over the last several hundred thousand years may have functioned as the pacemaker of human evolution.

Keywords: Clovis, Out-of-Africa, evolution, Pleistocene, America

Introduction

In 1926, human artifacts were recovered from a gravel pit near Frederick, Oklahoma (Figure 1). The associated fossil deposits in the gravels were unmistakably of Pleistocene age. Even at this early date, the inference that humans occupied the Americas during Pleistocene time generated heated debate. After a few years, the owner of the gravel pit, A. H. Holloman, became disgusted with the controversy and closed the area in 1932. The site has remained closed since that time and has never been excavated (Branson 1955, Smith & Cifelli 2000).

For decades, archaeological dogma precluded the possibility of early human occupation in the Western Hemisphere. From 1965 through

1997, the predominant theory of human settlement in the Americas was the Clovis-first theory. The name refers to an archaeological site near the town of Clovis, New Mexico. By 1965, remains of stone tools from about six sites in the Great Plains and southwest United States had been carbon-dated to about 11,500 years before present. The narrative that developed and received wide acceptance was that these artifacts represented the first appearance of humans in the New World.

What made the Clovis-first theory so attractive was its parsimony. Clovis culture remains dated to precisely the same period that "for the first time in at least 15,000 years, an ice-free, trans-Canadian corridor opened up" (Haynes 1964:1412). It was a "striking relationship" that seemed to have extraordinary explanatory power (Haynes 1964:1411). The Clovis people were big-game hunters, and they spread rapidly across the continent (Meltzer 2004:539–540). Subsequently, any evidence that people might have occupied the Americas prior to Clovis times was routinely dismissed without serious consideration.

The Clovis-first theory collapsed in the late 1990s due to the accumulation of a weight of evidence documenting earlier occupation of the Americas. Yet the generally accepted date for first human settlement has been barely nudged back from 11.5 ka to 15 ka (Fagan 2005:71–96, Goebel, Waters, & O'Rourke 2008). The archaeological community continues to strongly resist the idea of older human occupation despite significant evidence to the contrary. An important site containing evidence of human presence in the Americas as early as 150 ka is the Holloman gravel pit in Oklahoma (ka is a kilo-annum, or a thousand years before present. Ma is a mega-annum, a million years before present). Although the Holloman site contains human artifacts cemented in situ with a Pleistocene faunal assemblage, the site has never been excavated.

The Holloman Gravel Pit

In the 1920s, A. H. Holloman operated a commercial gravel pit on a ridge approximately a mile (1.6 kilometers) north of the city of Frederick, Oklahoma. In 1926, Mr. Holloman discovered in the gravels what appeared to be human artifacts in the form of stone tools. A resident of Frederick, F. G. Priestly, wrote a letter to the editor of the journal *Scientific American* describing Holloman's finds (Cook 1927a, Branson 1955). The editor passed on this information to Harold J. Cook and J. D. Figgins. Descriptions of the Holloman site were published in 1927 by Cook in *Scientific American* and in separate articles by Cook and Figgins in *Natural History*.

The gravel deposits at the Holloman site were dated by Cook (1927b) and Figgins (1927) as being of Pleistocene age based on a distinctive fossil

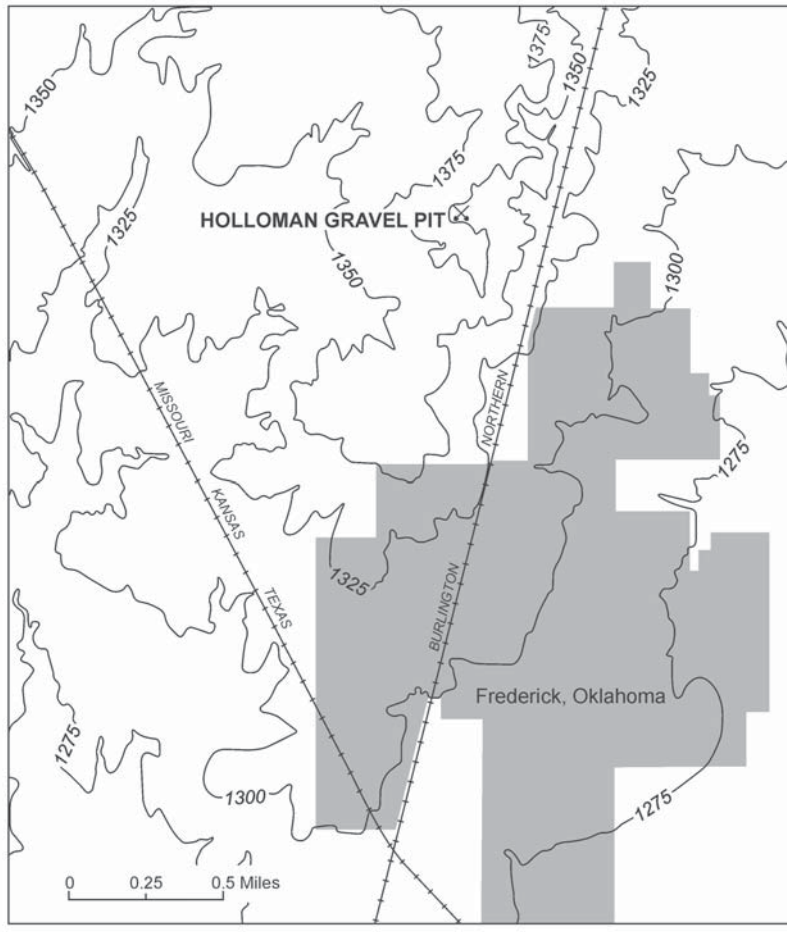

Figure 1. Location of the Holloman Gravel Pit near Frederick, Oklahoma. Contour lines show elevation (in feet) above sea level (1 foot = 0.3048 meters).

assemblage. The lowest member of the Pleistocene gravels was described by Figgins (1927:235) as "solidly cemented." Cook (1927b:247) concurred that the bottom layer was "generally cemented," and wrote that "it is in this bed that fossils are most abundant, and in it one flint spear point was found imbedded." Photographs of artifacts recovered from the Holloman pit were published by Figgins (1927:237–239). Five rounded stones recovered from the site were interpreted to be metates, implements used for food processing. Cook (1927b:247) concluded that the Holloman site provided "evidence of [human] antiquity" that was "clear-cut and conclusive."

Almost immediately, the artifacts and their supposed Pleistocene age became a controversy. Every possible argument, no matter how tenuous or speculative, was raised against the possibility of human occupation in Pleistocene time. Writing in *Science* in February of 1928, anthropologist Leslie Spier suggested that the artifacts described as "arrowheads or blades" had not been recovered in situ from the Pleistocene layers, but washed into the gravel pit from the surface. Spier dismissed the metates as "water-worn boulders" (1928a:160). Spier made these criticisms without either visiting the Holloman site or inspecting the artifacts.

Spier was immediately rebutted by Cook (1928) and Hay (1928). Cook (1928:371) pointed out that Spier had not seen the artifacts in question, while "no one who has seen them has questioned their authenticity as human artifacts." Cook also noted that the finding of metates did not necessarily imply agriculture. The stones could have been used by foragers to grind dried fruits and meat, as well as roots and plants.

Hay (1928:442) rejected Spier's claim that the artifacts had fallen into the pit from the surface, giving credence to Holloman's assertion that he had pried at least one "out of the hard conglomerate on the floor of the pit" using a tool. The debate in the pages of *Science* ended with a concession by Spier (1928b). After inspecting the rounded stones from the Holloman pit, Spier agreed they were "unequivocally metates." But Spier was not willing to concede the antiquity of the objects described as "arrowheads." He concluded these most likely were Holocene-age implements that had fallen or been washed into a pit "gullied by erosion along its margins" (Spier 1928b:184).

Given the controversy, Mr. Holloman was counseled on the necessity of preserving the in situ state of any future artifact he might find (Cook 1928). In September of 1928, Holloman found an artifact in the bottom layer of cemented gravel. The object was photographed in situ and copies of the photos were sent to Charles Gould (1929a, 1929b) at the University of Oklahoma in Norman. Gould and Leslie Spier inspected the site a few days later and satisfied themselves that the object had been recovered from a Pleistocene-age gravel. Oliver Hay described the artifact as an "arrowhead . . . 56 millimeters long and 38 millimeters wide" (1929:94). Spier, the skeptic, was now convinced that the artifact was of the same age as the gravel. He was quoted as conceding

> there can be no doubt that the artifacts occur in the pit near the basal portion, on the same level as the fossil remains . . . as the case stands, it looks very much as though the artifacts are of the same antiquity as the fossil animals. (Hay 1929:94)

Critics now took a new tact. Evans (1930a, 1930b) proposed that the artifacts found in the Holloman gravels were not autochthonous. He argued that the artifacts were found with Pleistocene fossils because both had been eroded, reworked, and redeposited together in Holocene time. According to Evans, at some time in the past the Holloman deposits had been laid down by the ancestor of the north fork of the Red River. Subsequently, the stream had been pirated by a river to the west, forming the present day north fork of the Red River, a tributary located 23 km west of the Holloman site.

Evans (1930a, 1930b) was rebutted by Cook (1931) and Sellards (1932). Cook pointed out that if the Holloman gravels represented a reworking of Holocene artifacts and Pleistocene fossils, they should also contain remains of Holocene animals. Yet "not one single bone found in these deposits . . . is referable to a modern species" (Cook 1931:162).

Cook's second objection was that reworking of the Pleistocene fossils would have destroyed them, yet they were intact. Cook cited especially a *Glyptodon* carapace, concluding "it is utterly impossible that any erosion could move the specimen without scattering the parts of the shell, or losing and destroying them" (Cook 1931:163).

Sellards (1932) also rejected Evans' (1930a, 1930b) claim of reworking. He concluded that the Holloman terrace as well as lower terraces east of the north fork of the Red River were of Pleistocene age. The geology and topography did not support the hypothesis of reworking. Sellards (1932) also reiterated Cook's observation that reworking would have destroyed the Pleistocene fossils, yet they were found intact.

By 1932 Mr. Holloman had closed the site, and he passed away in the 1970s (Smith & Cifelli 2000:7). A 1955 retrospective published by the Oklahoma Geological Survey concluded

> it is a scientific tragedy that the disagreement among observers and scientists caused all to cease collecting and observing the pit. (Branson 1955:100)

Discussion of the Holloman artifacts disappeared from the scientific literature. But there continues to be interest in the Pleistocene fossils. Meade (1953:459) described the fauna as Aftonian in age, "intermediate between the better-known Nebraskan age and Kansas age faunas." Subsequently, the term *Aftonian* was abandoned (Hallberg 1986). Most recently, Dalquest (1977) and Smith and Cifelli (2000) described the Holloman fossils as Irvingtonian age (1.9 to 0.15 Ma). Thus the lower Holloman layer from which human artifacts were recovered appears to be at least 150,000 years old (Bell et al. 2004:273). Age estimates based solely on faunal assemblages are necessarily imprecise. But up to the present time no better estimate has

been published. The Holloman gravel quarry is now abandoned and "filled with slump" (Smith & Cifelli 2000:7). In 2001, I visited the site and found it used only for cattle grazing.

Evidence of Pre-Clovis Occupation in the Americas

The Holloman pit in Oklahoma is not the only site in the Western Hemisphere from which substantive evidence of a human presence in Pleistocene time has been recovered. In the following summary, I list several of the more important sites (see also Goodman 1981:91–119). The list is not intended to be comprehensive, nor is this the place to enter into an extensive discussion of the relative strength or merits of the evidence from each location. The point is that the Holloman site is not unique: The scientific literature contains extensive evidence of a human presence in the Western Hemisphere in pre-Clovis times. Some of these studies have been published in preeminent peer-reviewed journals, including both *Science* and *Nature*.

Monte Verde, Chile: 12.5 to 33 ka

Monte Verde is the site that effectively falsified the Clovis-first paradigm (Dillehay 1986, 2000). For twenty years, lead investigator Tom Dillehay recovered extensive human artifacts at Monte Verde. Multiple carbon dates indicated human occupation at least as early as 12.5 ka. The excavations at Monte Verde were documented exhaustively in an authoritative thousand-page monograph (Dillehay 1997). A team of the world's leading archaeologists visited the site in 1997 and came to the conclusion that Monte Verde was an archaeological site older than 12.5 ka (Meltzer et al. 1997). There is also a lower layer at Monte Verde that dates to 33 ka (Dillehay & Collins 1988).

Alice Boër, Brazil: 14 ka

Carbon dating indicates a human presence in Brazil by 14 ka. Beltrao, Enriquez, Danon, Zuleta, and Poupeau (1986:211) concluded

> there are now at least five sites in Brazil at which evidence exists in favor of the presence of man more than 17 ka years ago.

Saltville, Virginia: 14.5 ka

The oldest horizon contains a bone tool dated at 14.5 ka (Goodyear 2005a).

Buttermilk Creek, Texas: 15.5 ka

At the Debra L. Friedkin site on Buttermilk Creek in Texas, 15,528 artifacts were excavated dating between 13.2 and 15.5 ka (Waters et al. 2011).

Cactus Hill, Virginia: 17 ka

Carbon dating of charcoal associated with artifacts yielded dates between 15 and 17 ka (Goodyear 2005a).

Great Plains, United States: 19 ka

Holen (2006) concluded that spiral fracturing of mammoth bones from sites in Nebraska, Kansas, and Colorado indicated the presence of humans on the Great Plains of North America at 18 to 19 ka.

Meadowcraft Rockshelter, Pennsylvania: 19.6 ka

The Meadowcraft rock shelter in Pennsylvania has been excavated since 1973. The "excavations are widely considered to represent state-of-the-art closed-site excavations" (Adovasio & Pedler 2005:24). Radiocarbon dates associated with artifacts range from 12.8 to 16.2 ka, and there is a single older date of 19.6 ka (Goodyear 2005a).

Pedra Furada, Brazil: 32 ka

Carbon dating of hearth charcoal associated with quartz and quartzite tools indicates humans were in Brazil by 32 ka (Guidon 1986, Guidon & Delibrias 1986, Guidon & Arnaud 1991).

El Cedral, Mexico: 31.9 to 33.3 ka

A bone tool and a chalcedony scraper were found in situ in layers dating, respectively, 21.96 ka and 33.3 ka. Charcoal from a hearth dated from 31.85 ka (Lorenzo & Mirambell 1986).

Burnham Site, Oklahoma: 35 ka

Fifty-five stone pieces "manifest[ed] attributes of having been flaked" (Wyckoff, Carter, & Theler 2003:296). The artifacts were found in a layer exhibiting minimal "disturbance and mixing," and both their vertical and horizontal distribution were consistent with being autochthonous (Wyckoff, Carter, & Theler 2003:300). Excluding the oldest and youngest dates, eleven ages obtained by various means yielded dates in the range of 22.6 to

46.2 ka. The most probable date for the artifact-bearing deposit was judged by the investigators to be 35 ka (Wyckoff, Carter, & Theler 2003:301–302).

Pendejo Cave, New Mexico: 37 ka

Human fingerprints, including some baked-on clay nodules, were found in layers dating to as old as 37 ka (Chrisman, MacNeish, Mavalwala, & Savage 1996).

China Lake, California: 42.4 ka

A mammoth tooth "in direct contextual association with two sophisticated finishing flakes" yielded a uranium date of 42.35 ka (Davis 1986:82).

Topper, South Carolina: 50 ka

Pre-Clovis excavations began in 1998 and yielded lithic artifacts from layers dating to 16 to 20 ka. Artifacts have also recently been recovered from a lower layer that is older than 50 ka, the limit of radiocarbon dating. Work at the Topper site is in progress, including dating by optically stimulated luminescence (Goodyear 2005a, 2005b, 2009, Waters, Forman, Stafford, & Foss 2009).

San Diego, California: 140 ka

San Diego contains, or contained, "many ancient sites" (Carter 1996:109). Many of these sites have been destroyed by development (Reeves, Pohl, & Smith 1986). The oldest site appears to be Texas Street, where "hearths and artifacts occur widely both laterally and in depth" (Carter 1996:109). The age was estimated by Carter (1957:320) to be "early last interglacial." According to the Devil's Hole chronology, the last interglacial began about 140 ka (Winograd et al. 1992).

Old Crow Basin, Yukon, Canada: 150 ka

Excavations yielded "flaked, polished and cut bones of mammoths and other large mammals" that were interpreted to be autochthonous artifacts dating to 150 ka (Irving, Jopling, & Beebe 1986:49).

Calico Mountains, California: 200 ka

This site, in the Mojave desert of California, has been described as "the best known example of the proposed evidence of very early man in the New World" (Simpson, Patterson, & Singer 1986:90). Among those who

interpreted Calico as evidence of an early human presence in the Americas was Louis Leakey (Goodman 1981:130–140). Critics dismiss the artifacts found at Calico as geofacts, rocks altered by natural processes (Haynes 1973). But when George Carter examined the artifacts, he concluded "I had no doubt that they had been man-made, for they had plural flake scars and no battering, such as occurs in nature" (Carter 1980:210). Uranium-series dating found "that the artifact-bearing deposits are about 200,000 years old" (Bischoff, Shlemon, Simpson, Rosenbauer, & Budinger 1981:576.)

Hueyatlaco, Mexico: circa 250 to 430 ka

The Hueyatlaco archaeological site at Valsequillo, Mexico, contains human artifacts in association with a Pleistocene faunal assemblage (Gonzalez, Huddart, & Bennett 2006). Unlike the Calico site, the artifacts cannot be questioned as geofacts, because they contain advanced forms such as bifacial projectile points. Uranium-series dates on bones from Hueyatlaco yielded dates suggesting an age of 250 ka (Steen-McIntyre, Fryxell, & Malde 1981). Analysis of diatoms indicates that the artifacts are likely autochthonous as "redeposition or reworking of sediments is highly unlikely" (VanLandingham 2010a:134). Diatom analysis established a minimum age of 80 ka for the artifacts (VanLandingham 2010a). A recent review concluded that the evidence of human presence at Hueyatlaco is older than 250 ka (Malde, Steen-McIntyre, Naeser, & VanLandingham 2011).

Toca da Esperanca, Brazil: 204 to 295 ka

The cave La Toca da Esperanca in eastern Brazil contains hearths and quartzite tools. The fact that the nearest quartzite outcrop is ten kilometers from the cave suggests that the tools are human artifacts. Uranium–thorium dating of associated animal bones yielded an age range of 204 to 295 ka (Lynch 1989:185). The site also contains a number of implements fashioned from bone (Beltrao & Danon 1987).

Clovis-First Theory as Paradigm

The Clovis-first theory is a classic example of what Thomas Kuhn termed a paradigm. Kuhn defined a *paradigm* as a "universally recognized scientific achievement that for a time provides model problems and solutions to a community of practitioners" (Kuhn 1996:x). Paradigms are a double-edged sword. They may become obstructive and dogmatic, but the adoption of a paradigm enables scientific activity to be focused, articulated, and defined. Thus "normal science" can function more efficiently for a time.

For more than thirty years, the adoption of the Clovis-first theory allowed archaeologists to focus their work on the elaboration of Clovis settlement in North America by addressing questions such as the geographical extent of Clovis culture, its propagation, and the details of Clovis lifestyle. Time and effort were reserved for strata most likely to yield evidence of human occupation. In general, any rock layer known to be older than 11.5 ka was ignored. Holen (2006:34) related that "a geologist informed the archaeologists that the deposits were older than 100,000 years old, at which point they ceased excavation."

The drawback to the adoption of a paradigm is that novelties and anomalies are suppressed because "they are necessarily subversive of [the paradigm's] basic commitments" (Kuhn 1996:5). Because "discovery commences with the awareness of anomaly," the normal scientific activity engendered by a paradigm ultimately runs its course and functions not so much to generate knowledge as to suppress its acquisition (Kuhn 1996:52).

Rarely has a paradigm become so dogmatic and obstructionist as Clovis-first. Any evidence that tended to falsify Clovis-first was questioned. Dogmatism masqueraded as skepticism. If any excuse could be found to dismiss data contradicting the ruling paradigm, they were rejected. There were two standards of evidence. One for evidence consistent with Clovis-first, another for observations that were inconsistent.

Archaeologist David Meltzer related being present in a group of archaeologists shown stone tools from Africa allegedly 2.3 million years old. No one even raised any question as to the authenticity of the objects as genuine human artifacts. Meltzer suddenly realized the contrast. "I'd been in rooms where artifacts on display from the pre-Clovis age sites of Monte Verde, Chile, and Meadowcraft, Pennsylvania, dated to 12.5 to 14.25 ka, respectively, triggered noisy debate" (Meltzer 2009:95). George Carter handed stone tools from the Calico site (200 ka) to archaeologists without telling them where they were from. The response was "that is an artifact . . . no one will deny that" (Carter 1980:35). Yet when the same individuals were handed the same artifacts and told they were from the 200 ka Calico site, the critics invariably insisted the objects they had previously identified as artifacts were geofacts.

Claims of pre-Clovis occupation in the Americas had to be "utterly unimpeachable in all respects" (Meltzer 2009:109). But of course no archaeological evidence is ever "unimpeachable." On the contrary, it is always open to interpretation and analysis of context. Charcoal deposits from hearths were said to result from naturally occurring wildfires. Simple stone tools were dismissed as geofacts. If a tool was sufficiently complex that it could not occur naturally, then it was not autochthonous but reworked. If

all of these arguments failed, then the method of last resort was to claim that artifacts had been fraudulently planted. Anyone who seriously maintained the possibility of pre-Clovis occupation in the Americas was subjected to ridicule and ostracism.

In that nearly everyone agrees humans initially entered the Americas through the Bering Land Bridge, it is surprising that much of the evidence for pre-Clovis occupation comes from South America. The probable reason is that South American archaeologists were not as bound by the Clovis-first paradigm as their North American counterparts. Unaware that pre-Clovis occupation was impossible, they went out and discovered it.

In a 1990 review of the evidence from South America, Thomas Lynch rejected all evidence for pre-Clovis occupation in South America, including Monte Verde. He concluded "there are no indisputable or completely convincing cases of pre-Clovis archaeological remains in South America" (Lynch 1990:27). But nothing in science is ever "indisputable." Science is not a foolproof system of deductive logic. Since Francis Bacon and the members of the Royal Society first elaborated experimental philosophy in the seventeenth century, the sciences have operated inductively (Deming 2012). Science constructs theories through induction based upon the set of observations available at the present time. As our observations increase through time in number, reliability, and precision, our theories change. The history of science is punctuated with the eventual adoption of theories once considered highly improbable. These include heliocentrism, continental drift, and the theory that peptic ulcers are caused by a bacterial infection. Before accepting the reality of pre-Clovis occupation in South America, Lynch demanded evidence that was "incontrovertible" (1990:28). He was oblivious to Karl Popper's warning: "if you insist on strict proof in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are" (1959:50).

There is a long tradition of denying human antiquity. The most infamous example of a short terrestrial chronology is Anglican bishop James Ussher's book *The Annals of the World Deduced from the Origin of Time* (1658). Ussher stated that the Earth had been created on the night preceding the 23rd of October, 4004 BC. Isaac Newton was also a young-Earth creationist (Deming 2012:234).

Even as nineteenth-century naturalists began to acquire an appreciation for the age of the Earth, they nevertheless insisted on a recent origin for man. Georges Cuvier gathered fossil bones by the thousands from the far corners of the Earth. But in his great four-volume monograph *Ossemens Fossiles* (1812) he concluded, "human bones have never been found as fossils" (Cuvier 1997:232).

In *Vindiciae Geologicae* (1820), William Buckland affirmed "the declaration of Scripture is positive and decisive . . . in asserting the low antiquity of the human race" (p. 23). When people began to find human fossils in association with extinct Pleistocene animals, Charles Lyell advocated a double standard of evidence. In the second volume of *Principles of Geology* (1833), Lyell preached "more than ordinary caution is required in reasoning on the occurrence of human remains" (p. 232). Among those who excavated human fossils in British caves was the clergyman John MacEnery. Years of field work convinced MacEnery that humans had been contemporaneous with extinct Pleistocene fauna. Yet MacEnery, the amateur, was convinced by Buckland, the professional, that he must be mistaken (Lyon 1970). If human bones were found in association with Pleistocene fossils, they could not be autochthonous (MacEnery 1859:50–51).

Geologists were unwilling to accept evidence of human antiquity until the theoretical framework changed. The publication of Darwin's *Origin of Species* in 1859 legitimized human antiquity. Subsequently, in 1863, Lyell published *The Antiquity of Man* wherein he confessed to having previously held an "extreme reluctance" to "accept the validity of evidence" for human antiquity (pp. 1–2). Once it became respectable to admit human antiquity, geologists suddenly "discovered" evidence that they had been summarily dismissing for the previous fifty years. Eldredge and Gould (1972:83) explained

> the expectations of theory color perception to such a degree that new notions seldom arise from facts collected under the influence of old pictures of the world.

Out of America?

The acceptance of Monte Verde as an authentic archaeological site dated to pre-Clovis time has pushed back the date of human entrance into the Americas to about 15 ka (Fagan 2005, Goebel, Waters, & O'Rourke 2008). But there is no logical or evidentiary reason to limit entry to this late date. Falsification of the Clovis-first theory opened a Pandora's Box of possibilities, and archaeologists have yet to come to terms with the implications. The Bering Land Bridge opened and closed repeatedly during the Pleistocene. It is entirely probable that humans migrated from Asia into the Americas not once, but several times during the Pleistocene (Meltzer 2009:199). Nor is there any reason for migrations to have been one-way (Goodman 1981).

One of the arguments invoked against the antiquity of artifacts from the Holloman site was that they appeared to be relatively modern. Spier

(1928a:160) noted that some of the artifacts resembled "modern Indian forms." Because the age of the cemented gravel in which these artifacts were recovered has been dated to the neighborhood of 150 ka, this suggests that the artifacts were not autochthonous. On the other side of the argument, we have the statement by Gould (1929a, 1929b) that he and others were satisfied that the artifacts had been recovered in situ from a cemented formation. Evans (1930a, 1930b) brought up the possibility of reworking, but this was rebutted strongly by arguments from Cook (1931) and Sellards (1932).

It is difficult to discern precisely how "advanced" the Holloman artifacts described as "arrow-heads" were. Implements recovered from Holloman have been scattered. Whether they are incompatible with stone tools typical of the Middle Paleolithic in Eurasia is undetermined. Stone technology may have been more advanced than has been previously recognized. Stone points that apparently functioned as spear tips were recently recovered from a site in Africa dating to 500 ka (Wilkins, Schoville, Brown, & Chazan 2012).

There is another possibility. Stone-working techniques in the Americas could have been more advanced than those of the same age in Eurasia. Holloman is not the only site in the Americas from which apparently advanced forms of great age have been recovered. At the Hueyatlaco site near Valsequillo, Mexico, artifacts were recovered that composed "a typological sequence ranging from edge-trimmed flake tools in the lower levels to well-made bifacial tools in the upper levels." The strata at Hueyatlaco are apparently older than 250 ka (Malde, Steen-McIntyre, Naeser, & VanLandingham 2011). Steen-McIntyre, Fryxell, & Malde (1981:15) concluded

> [We] are painfully aware that so great an age poses an archeological dilemma . . . if the geological dating is correct, sophisticated stone tools were used at Valsequillo long before analogous tools are thought to have been developed in Europe and Asia.

The presence of advanced stone tools in the Americas dating to circa 200 ka may have implications for our understanding of human evolution. The currently accepted view is that the genus *Homo* evolved from *Australopithecus* in Africa. The first human species appears to have been *Homo habilis* (circa 2.5–1.4 Ma). *Homo habilis* was followed by a succession of human species or subspecies whose categorization is necessarily somewhat subjective and overlapping. These include *Homo erectus*, *Homo ergaster*, *Homo heidelbergensis*, and *Homo neanderthalis*. All of these earlier species were eventually replaced by *Homo sapiens*. Archaic forms

of *Homo sapiens* first appeared circa 500 ka, with anatomically modern humans (AMH) in Africa circa 200 ka (Klein 2009, Tattersall & Schwartz 2009, Brauer 2008).

Homo is a highly mobile genus. Hominids were in the Republic of Georgia by 1.8 Ma and on the island of Java by 1.5 Ma. The oldest evidence of *Homo* in Europe is a jaw fragment from Spain dated to 1.2–1.4 Ma (Tattersall & Schwartz 2009:75–76). Hominids lived in China no later than 1.1 Ma (Klein 2009:351). An assemblage of flint tools recovered from the Happisburgh site on the east coast of Britain shows that humans were cold-adapted and living in northern Europe by 780 ka (Parfitt et al. 2010).

Although AMH appeared in Africa circa 200 ka, their behavior and culture did not differ from archaic forms of *Homo sapiens* or other species such as *Homo neanderthalis*. Culturally modern humans (CMH) first appeared circa 50 ka and rapidly spread throughout Eurasia. The sudden appearance of CMH has been described as "the most fundamental change in human behavior that the archaeological record may ever reveal" (Klein 2009:659). Yet it remains an event with no discernible cause. The genus *Homo* evolved slowly in Eurasia over hundreds of thousands of years. Whence discontinuity?

The currently popular theory that explains the sudden appearance of CMH at about 50 ka is called "Out-of-Africa." Out-of-Africa postulates that modern humans originated in Africa circa 60–50 ka and from there rapidly spread throughout the world, replacing other *Homo* species (Stringer & Andrews 1988, Higham et al. 2011). There seem to be two apparent reasons for selecting Africa as the origin of fully modern humans. The first is that the fragmentary fossil evidence indicates that by 500 ka the primary species in Africa, Europe, and Asia, respectively, were *Homo sapiens*, *Homo neanderthalis*, and *Homo erectus* (Klein 2009:739). The second reason is that Africa itself contains the highest degree of genetic diversity, and genetic differentiation increases with increasing geographic distance from Africa (Ramachandran, Deshpande, Roseman, Rosenberg, Feldman, & Cavalli-Sforza 2005). Out-of-Africa is regarded as the dominant, if not the only, acceptable theory that explains the origin of CMH. Ongoing research is mostly concerned with an elaboration of the theory, not a consideration of alternatives (Beyin 2011).

But there are several problems with the Out-of-Africa hypothesis. The evidence for the emergence of AMH in Africa is sketchy.

> [It] is truly remarkable . . . that if we look at the African record we find rather little that clearly foreshadows the distinctive morphology . . . [of] *Homo sapiens* today. (Tattersall & Schwartz 2009:82)

It is true that there is more fossil evidence for *Homo sapiens* in Africa than elsewhere, but this may well be because Africa is by far the place most people look for human fossils. What is not sought cannot be found.

Another problem with Out-of-Africa is that the sudden appearance of modern human behavior at 50 ka implies a significant evolutionary advance in neurological capacity. There is no evidence for any precursor in Africa. In other words, there is no discernible cause for the effect.

> The people who inhabited Africa between 100 and 60–50 ka may have been physically modern or near-modern, but they were behaviorally very similar to the Neanderthals and other non-modern humans. (Klein 2009:741)

A third objection to Out-of-Africa is that evolutionary changes in Africa would have likely been suppressed by gene flow. It is believed that evolutionary change results from the geographic isolation of a subset of a population. People in Africa were not isolated, and any putative evolutionary change would have been muted by gene flow, a process that "exerts a homogenizing influence" (Eldredge & Gould 1972:112). If there were not significant gene flow between Europe, Africa, and Western Asia in Middle Stone Age time (circa 250–50 ka), it would be difficult to explain why Mousterian stone technology was "remarkably uniform over vast areas" (Klein 2009:538).

> It is hard to imagine how this apparent cultural uniformity could have persisted without high levels of movement and mate exchange between groups. (Harpending, Sherry, Rogers, & Stoneking 1993:495)

A fourth problem with Out-of-Africa is that it implies that a species which evolved in tropical Africa rapidly displaced cold-adapted Neanderthals in northern Europe during the coldest part of the last Ice Age. For about 150,000 years, Neanderthals prospered in Europe while subjected to varying climatic extremes of glacial and interglacial conditions. CMH entered Europe at about 43–42 ka (Higham et al. 2011). Within a few thousand years of the appearance of CMH in Europe, Neanderthals became extinct (Pinhasi, Higham, Golovanova, & Doronichev 2011). Thus it seems that CMH "were better equipped technologically and culturally to deal with . . . severe glacial conditions" (Mellars 2006:934). Not all of Africa lies in the tropics, but nevertheless nearly all of the land area lies within 30 degrees latitude of the equator.

Finally, there is evidence that CMH colonized southeast Asia and Australia circa 60 ka, well before their entry into Europe circa 43 ka (Macaulay et al. 2005, Beyin 2011:3). It is bizarre that CMH migrating out of Africa would have entered Australia before Europe.

The presence of advanced stone tools at Holloman and Hueyatlaco suggests the possibility that CMH may not have evolved in Africa, but in the Americas (Goodman 1981). This theory has the advantage of providing a much higher degree of allopatry, the geographic isolation thought necessary for significant evolutionary change.

The Bering Land Bridge opens when sea level is about 50 meters below the present-day level (Elias, Short, Nelson, & Birks 1996). Reconstructions of global sea level over the past 470 ka suggest it was possible to walk from Asia to Alaska from 370–337 ka, 283–240 ka, 189–130 ka, and 75–11 ka (Siddall et al. 2003). Altogether, the Bering Land Bridge was likely open for about 200,000 of the last 500,000 years. Yet we are supposed to believe that *Homo sapiens* entered the Americas only 15,000 years ago, even though *Homo erectus* was in east Asia as early as 1.5 Ma. I suggest it is more likely that hominids moved back and forth over the Bering Land Bridge repeatedly.

There are many possibilities. Setting aside, for the moment, the question of earlier migrations, consider only the last 200 ka. Analysis of mitochondrial DNA suggests that AMH originated about 200 ka (Cann, Stoneking, & Wilson 1987). Because what scant fossil evidence is available places archaic *Homo sapiens* in Africa, it is believed that the woman who contributed this mtDNA lived in Africa. Suppose the conventional view is true, and that AMH originated in Africa—*this doesn't mean they necessarily remained there*. Estimates from DNA studies are imprecise, but they indicate that the indigenous people of southern Africa split from the rest of *Homo sapiens* anywhere from 90 to 157 ka (Behar et al. 2008, Gronau, Hubisz, Gulko, Danko, & Siepel 2011). The Bering Land Bridge was open from about 189–130 ka. For tens of thousands of years, it would have been possible for AMH to migrate out of Africa to eastern Asia and onward to the Western Hemisphere.

The critical period was the last interglacial. Temperatures were higher than during the Holocene (Andersen et al. 2004), and the Bering Land Bridge was closed for about 55,000 years due to flooding. Geographically isolated in the Western Hemisphere, humans would have had the opportunity to evolve into CMH without the muting influence of gene exchange with people in Eurasia. Once the Bering Land Bridge opened again circa 75 ka, a relatively small group or groups of CMH could have crossed back into Asia and spread southward into Australia and west to Africa and Europe. This would explain why CMH went through a population bottleneck in late Pleistocene time (Ambrose 1998). Thus the sudden appearance of CMH in Africa was due to a migratory influx, just as was the case for Europe.

Consider, by way of contrast, how implausible the standard Out-

of-Africa theory seems when viewed critically. AMH lived next door to Neanderthals in Europe for 150,000 years, sharing the same Mousterian technology. Then, deus ex machina, they changed suddenly into CMH and rapidly displaced Neanderthals in Europe. It is more plausible that CMH originated elsewhere and entered, as it were, as an invasive species.

There is nothing in this scenario that contradicts the possibility that present-day American Indians derived largely from an ancestral population living in Mongolia that entered the Americas during the last 20,000 years. The Bering Land Bridge was open from approximately 75–11 ka. There likely were multiple migrations of different groups of people back and forth. By "multiple migrations," I mean it is entirely plausible that there were as many as ten or twenty migrations in each direction. Five thousand years is a long time. Analysis of DNA indicates only degree of relatedness; it cannot discern migration directions or the number of migrations. People who enter at later times may displace people present at earlier times. Just because modern humans living in Europe have little common ancestry with Neanderthals does not indicate that *neanderthalensis* did not occupy Europe before *sapiens*.

Conclusion

An objection to the Out-of-America theory is that no early human remains have been found in the Americas. But in fact they have. Two "primitive looking" human skulls were recovered from Valsequillo, Mexico. Both were subsequently lost (Gonzalez, Huddart, & Bennett 2006:612, Lyons 2009, VanLandingham, 2010b). Despite this, no one looks for early human fossils in the Americas because their theoretical framework informs them that these do not exist. Human fossils are rare. Even in Africa, nearly a hundred years of searching have turned up only a handful of examples.

Our understanding of human evolution has been obstructed by some epistemological biases.

(1) There is a tendency to jump to premature conclusions on the basis of fragmentary evidence. Clovis-first is exemplary of this tendency. For decades, important evidence was ignored because it was inconsistent with a false theory. This debacle could have been avoided by adopting Chamberlin's method of multiple working hypotheses (1890).

(2) There is a failure to understand that the human archaeological and fossil record has not been assembled objectively, but partly reflects theoretical conceptions. An absence of evidence has been interpreted as evidence of absence. Yet an absence of human artifacts or fossils is surely biased by collecting strategies. Human fossils are found in Africa in part because this is where people look for them. The bias toward Africa dates

back to Darwin's observation that because Africa was home to chimpanzees and gorillas, "it is somewhat more probable that our early progenitors lived on the African continent than elsewhere" (1871:199). Yes, it seems likely that hominids originated in Africa. But with *Homo erectus* in eastern Asia by 1.5 Ma, subsequent human evolution must be considered on the stage of the entire world, including the Western Hemisphere.

As I write, what could be one of the world's most important archaeological sites sits abandoned and ignored. The Holloman site should be excavated. The Holloman site itself may constitute only a minuscule fraction of the area's potential. The site is located on a ridge about 800 meters wide that extends 12 to 16 kilometers to the north (Gould 1929a). This ridge likely exists because of the presence of Pleistocene gravels and cemented stream deposits that have proven relatively resistant to Holocene erosion. Thus the entire ridge may be underlain by Pleistocene deposits and represents a vast potential for discovery.

Excavation of Holloman and other American sites has the potential to illuminate our understandings of human origins. But if we do not look, we shall not find.

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RESEARCH ARTICLE

Experimental Birthmarks: New Cases of an Asian Practice

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Abstract—Experimental birthmarks involve a practice in several countries in Asia in which the body of a dying or recently deceased person is marked with a substance, most often soot, in the belief that when the individual is reborn, the baby will bear a birthmark corresponding to the mark. This is usually done with the expectation that the rebirth will occur in the same family as the deceased individual. A field study was undertaken in Thailand and Myanmar (Burma) to examine such cases. Eighteen cases were found in which a baby was born with a birthmark that corresponded to a marking made on the body of a deceased person; in six of these, the child also made statements that the family believed were related to the life of the deceased individual. Possible etiologies for these cases are explored.

Keywords: Birthmarks—experimental birthmarks—reincarnation

In cultures with a prevalent belief in reincarnation, people often interpret birthmarks and birth defects as evidence of a connection between a child and a deceased individual. In some Asian countries, the body of a dying or deceased person is sometimes marked in the belief that when the person is reborn, the baby will bear a birthmark that corresponds to the mark made on the body. The person marking the body often says a prayer that the dying person take the mark with him or her to the reborn body. Stevenson (1997) coined the term "experimental birthmarks" to describe this practice, and he reported that it occurred widely in Asia, with cases being found most readily in Thailand and Myanmar (Burma). Although Stevenson was the first person to assemble and report a group of these cases, several anthropologists and other writers about Asian cultures had earlier drawn

attention to them, sometimes mentioning individual cases (see Stevenson 1997:804 for sources).

Stevenson (1997) documented 20 such cases in Thailand and Myanmar from fieldwork that was primarily conducted at least 20 years before, and he reported that he and his associates had also studied 15 others. In all of them, the subject was born with a mark that informants said corresponded in location to the mark made on the deceased person.

Since the cases were generally 20 years old or older, we wished to learn whether more recent cases could be found and investigated. Though the practice appears to be disappearing in some areas of Thailand, we found 13 experimental birthmark cases in the northeastern region of Thailand and 5 more in Myanmar. While the custom of marking bodies may be more common in this area of Thailand than in some others, the villagers there report that it is only rarely done; they estimate that perhaps 1% to 4% of bodies are marked.

Methods

Cases were identified in various ways. Our interpreters had found some when they made inquiries prior to our visits. As these were word-of-mouth inquiries made to acquaintances, they only surveyed a limited number of people. Often, as we investigated one case, villagers would mention another case, which we would then investigate later.

Once a name was given to us, we endeavored to interview as many informants as possible. Most of the cases were in small villages, and we could find the families in question by simply going to the villages and asking for them. We then examined the children and sketched and photographed their birthmarks. We interviewed their families to verify that the marks were, in fact, present at birth. We also interviewed the individuals who had marked the deceased persons' bodies, and we had them show us how they had made the marks. In addition, we interviewed other witnesses to the markings whenever possible.

Results

We investigated 18 cases in which a child bore a birthmark that appeared to correspond to a mark made previously on a dying or deceased individual (Table 1). In 15 of these, the deceased individual was in the same family as the child. One of them involved a deceased sibling, while the others primarily involved grandparents. In 12 of the 18 cases, the deceased individual was at least 59 years old at the time of death. As for the cause of death, only one was violent, that being a gunshot wound. This is in contrast

TABLE 1
Characteristics of Subjects

Subject	Age at Interview	Deceased's Relation to S	Deceased's Age at Death	Mode of Death	Mark Seen by Mother	Spoke of Previous Life
1	7	Paternal grandfather	63	"Swollen brain"	No (may have heard)	No
2	9 (mark no longer seen)	Maternal grandmother	60s	Heart attack	Yes	No
3	5 months	Paternal grandfather	85	After surgery	No (but was told)	No
4	15	Maternal great-grandmother	83	Unrecorded	Yes	No
5	23	Maternal aunt	3	"Collapsed and died"	No (knew of it but not site)	No
6	4	Paternal grandmother	Elderly	Diarrhea	No (knew of it but not site)	No
7	20 months	Great-grandfather	Elderly	Unrecorded	No (may have heard)	No
8	3	Paternal great-aunt	Elderly	Unrecorded	No	No
9	17	Distant relative	21	Gunshot wound	No	No
10	18	Unrelated	Adult	Infection from wound	No (may have heard)	Yes
11	14	Brother	2	Fever	Yes	No
12	7 (marks no longer seen)	Maternal grandmother	68	Kidney disease	No (but knew of them)	Yes
13	Adult	Unrelated	42	Edema	No	Yes
14	2	Grandfather	63	Gout?	Yes	No
15	10	Paternal great-grandfather	67	"Old age"	No	No
16	2	Maternal grandfather	62	Cirrhosis	Yes	Yes
17	9	Paternal grandfather	77	Diabetes, stroke, heart attack	No	Yes
18	11	Unrelated	25	Malaria	No (but knew of it)	Yes

to general cases of children who claim to remember previous lives, 70% of which involve deaths by unnatural means (Tucker 2005).

In five of the cases, the mother of the child had seen the body after it was marked. In eight of the others, the mother had heard or may have heard of the marking but had not seen it. This leaves at least 5 of the 18 cases in which the mother of the child did not even know of the marking.

In 6 of the 18 cases, the child made statements about the life of the deceased individual that the family believed involved knowledge that he or she could not have acquired through normal means. These will be described further in the case reports below. We note that five of the other children were four years old or younger when examined, so they may have made statements subsequent to our investigations.

Following are four of the stronger case reports, which illustrate how the phenomenon usually progresses:

The Case of A. W.

A. W. is a girl who was born in Loei province, Thailand. Her maternal grandfather had died five years before. The details of his death are not clear, but he had suffered from gout. Just before his death, he could no longer walk and was apparently cachectic. He died in his home village, a neighboring village to A. W.'s home, when he was 59 years old.

One of his daughters decided to mark his body. She knew about this practice but did not follow a specific tradition. She said she wanted to know whether rebirth would happen. She made a mental wish that her father would take the mark with him whenever and wherever he was reborn. After making this wish, she marked his right leg above the ankle with soot from the bottom of a rice pot. She made the mark about two hours after he had died. She took her index finger and demonstrated to us using our interpreter's leg how she made the mark. When she had marked her father, a number of people from the village were present, but not all paid attention to what she was doing. A. W.'s mother told us that she had seen her sister make the mark on her father. The subject's paternal aunt also told us that she had seen how the body was marked.

After her father's death, A. W.'s mother dreamed more than ten times about him. The first dream occurred approximately seven days after he died. In this dream, he told her that he wanted to live with her again. This was the only dream with this kind of message.

A. W. was born in an uncomplicated delivery in the district hospital five years after her grandfather died. From the time she was born, she had a flat, hyperpigmented nevus on her right leg in a location that seems to be in good agreement with that of the experimental mark on her grandfather

Figure 1. Birthmark on A. W.'s right leg.

(Figure 1). It should be noted here that hyperpigmented nevi rarely occur at birth (Jacobs & Walton 1976, Pack & Davis, 1956, Pratt 1953), and they are found on the arms nearly twice as frequently as on the legs (Pack, Lenson, & Gerber 1952).

We interviewed A. W.'s mother; the deceased's widow and her new husband, who is the deceased's younger brother; two of A. W.'s maternal aunts, one of whom marked the body; A. W.'s paternal aunt; and a neighbor. They all agreed that the events recorded here occurred as indicated.

At the time of the interview, A. W. had said very little that could be related to a previous life. One possible exception was her objection to her mother's interest in gambling. The grandfather had often expressed disapproval of his daughter's gambling. A. W. also had one behavior that may relate to her grandfather: She stood while urinating approximately half of the time; Stevenson (1997) described other cases of girls who urinated while standing up claiming to remember previous lives as males.

The Case of N. N. W.

N. N. W. was born outside of Yangon, Myanmar, and raised by her maternal aunt and her husband. Her maternal grandmother had died of kidney disease at the age of 68, nine years before N. N. W. was born. One to two hours after she died, her daughter (N. N. W.'s aunt) made two marks on her body with soot. One was on the lateral surface of the left leg just proximal to the ankle, and the other was on the medial surface of the right leg on and distal to the ankle. The marking was observed by several people, including family members and a neighbor we interviewed, M. K. M. The subject's mother did not see the marks but knew they had been made.

Before N. N. W.'s mother became pregnant with her, she had three dreams in which her mother said she wanted to come live with her. In the dreams, N. N. W.'s mother initially said no, but N. N. W.'s grandmother became more insistent with each successive dream until her mother finally replied, "As you wish." One month later, she became pregnant. During the pregnancy, she had cravings for tea and cake, Indian spiced food, and milk—foods that she usually did not like. N. N. W.'s grandmother, whose father was Indian, had enjoyed Indian food.

When N. N. W. was born, she had birthmarks that corresponded to the two marks made on her grandmother's body. This was confirmed by her family and by the neighbor, M. K. M., whom we interviewed. She had no other birthmarks, and her two brothers had none. The two birthmarks had faded away by the time N. N. W. was 6 years old, so they were not present when we met her a year later.

N. N. W. began talking at about 18 months of age, and she made a number of statements related to the life of her grandmother. She asked about a mortar that the grandmother had owned, and when her uncle hurt his knee, she said that medicine should be pounded in the mortar and put on his knee. The grandmother also had a shell that she had used during ceremonies. Others in the family did not use shells, but during a ceremony N. N. W. asked about her shell. Both of these questions came before she was two years old, and as a youngster, she frequently talked about the previous life. For example, she asked about her money and jewelry. Her grandmother had apparently been quite well off. The family developed financial problems after her death, and N. N. W. once asked why the family had spent her money. When she was being spanked, she would ask, "Why do you not respect your mother?"

The neighbor, M. K. M., was known to N. N. W.'s family as "Ma Win Kyi." N. N. W.'s grandmother, however, had called her "Daw Win Kyi," and so did N. N. W., even though no one else around her did. She also called her parents and her aunt and uncle by their given names. Children who

claim to remember previous lives frequently call adults by familiar names as adults would do; in Myanmar, this is considered quite rude, as respect for older persons is deep in that culture (Stevenson 1983).

During World War II, the grandmother had lived in Tavoy, Burma, with one of her cousins whom she called "Baby," being the only person to do so. That cousin later lived with N. N. W.'s family for a time, beginning when N. N. W. was five years old. N. N. W. also called her "Baby" and once said to her, "Please shut your ears because the English bombers will drop the bombs." Her family interpreted this as a reference to the bombing of Japanese soldiers in Burma by the English during World War II.

Her family felt sad hearing her talk about the life of her grandmother, so family members tried to discourage her from such talk. They fed her eggs for a time in the belief, common in Myanmar (Foll 1959, Khaing 1962), that this would make her forget about the previous life. The statements became less frequent as she became older, and by the time we met her she generally talked about the previous life only when she was angry or sad. Two days before our meeting, however, she did say to her female first cousin, who was visiting, "You look like my son." The family stated that the cousin does, in fact, closely resemble the grandmother's son (the cousin's father), but he had not accompanied her on the visit.

Along with the statements, N. N. W. had one habit that reminded her family of her grandmother: She would eat with one leg hiked up in her chair. She and her grandmother were the only two in the family to do that. This is similar to a behavior of the subject of a Sri Lankan case, Sujith Lakmal Jayaratne, reported by Stevenson (1977). When Sujith drank, he drew his legs up, as had the person whose life he remembered.

When we interviewed N. N. W. at age seven, she did not say a great deal. She did report a memory of a group photograph being taken. Her family produced a group photograph that included her grandmother. N. N. W. did not identify anyone in the photograph, but she then said she remembered another photograph that had been taken in a particular room of the house (to which she pointed). Her family reported that a group photograph that included her grandmother had, in fact, been taken in that room 25 years before. The photograph had been given to other family members in Tavoy more than 20 years earlier, and N. N. W.'s immediate family members had not thought of it for many years. (The photograph that we saw had been taken prior to the one that N. N. W. discussed, and perhaps its age contributed to N. N. W.'s inability to recognize anyone in it.)

In addition to N. N. W., we interviewed her mother, her aunt, her uncle, and M. K. M., the neighbor. They were all convinced that N. N. W. was her grandmother reborn.

The Case of K. H.

K. H. was born in Meiktila, Myanmar. His father was a merchant and his mother a homemaker. At the time of the interview, he was an only child.

K. H.'s maternal grandfather died 11 months before K. H. was born, at the age of 62. He was apparently an alcoholic, and he died of cirrhosis of the liver. Prior to the ceremony of his burial, his body was marked by a neighbor. She used charcoal from the underside of a pot and touched his left arm with her finger. At the ceremony, many people, including K. H.'s mother, other family members, and neighbors, saw the mark.

When K. H.'s mother was pregnant with him, she and her sister both had dreams of a man coming to them. In her dream a man said, "I want to live with you." K. H.'s mother recognized the man as her father. K. H. was then born in Meiktila Hospital in an unremarkable delivery. At birth, he was noted to have a birthmark on his left arm in the same place where his grandfather's body had been marked. It was reported to be similar in size and color to the marking, though it has subsequently faded somewhat since birth (Figure 2).

Author J. K. conducted the initial interview when K. H. was approaching two years of age. The marker was present, and she demonstrated how she had made the mark. The location she indicated corresponded to that of the birthmark on K. H. At that time, K. H. had not said anything related to a previous life. By the time we returned four months later, however, he had made several statements that the family interpreted as indications that he was the rebirth of his grandfather.

K. H. called his grandmother "Ma Tin Shwe," a name that only his grandfather used for her. Other children called her "Daw Lay," or "Auntie," and her children called her "Mother."

During the interval between our interviews, K. H. was taken to his grandfather's house for the first time. When he met the maid there, he called her "Sein Sein." His grandfather had called her this, but the family stated that K. H. had not been told her name. While at the house, K. H. was asked to pick out the picture of Aung San Suu Kyi, the leader of the democracy movement in which both his grandfathers had been involved. He was able to pick out her picture from among the other pictures there, but given the admiration that the family had for her, it is unclear if this was the first time he had seen a picture of her.

The family also reported that K. H. never called his mother "Mother." Instead, he called her "War War Khine," as his grandfather had done, while others called her "Ma War." K. H. did not make any statements about a previous life to us, and his family reported that he talked about the previous life only when he wanted to, not when he was questioned.

Figure 2. Birthmark on K. H.'s left arm.

The Case of P. S.

P. S. was born in a village of the Sakon Nakhon province of Thailand. He was the third of four children. One of the children was a boy who had died with a fever the year before P. S. was born. He was two years and nine months old at the time of his death. Ten minutes after he died, his maternal grandmother marked his body. She used soot to make a spot on his left jaw, and both parents saw the marking. Six months after that, P. S.'s mother became pregnant, and she then gave birth to him in an uneventful delivery at home. At birth, P. S. had a dark brown spot on his left jaw. Near the spot was a larger area of increased pigmentation that was only faintly visible at the time. As he became older, the spot matching the experimental mark faded somewhat, though it was still clearly visible when he was interviewed. Meanwhile, the larger area became darker, but the original spot was still slightly more prominent (Figure 3). His father had a birthmark on one of his thighs, but there were no other birthmarks in the family.

P. S. was 14 years old when Author J. K. interviewed him. He had not made any statements related to a previous life, and the birthmark was the

Figure 3. Birthmark on P. S.'s face.

only indication for his parents that he was his brother reborn. This case is similar to others J. K. has studied (Keil 1996) that demonstrate a family's belief that an infant is the rebirth of a particular previous personality does not necessarily lead the child to talk later about that person's life.

Discussion

With regard to potential etiologies for experimental birthmarks, the possibility that the correspondence of the marks is purely coincidence certainly has to be acknowledged. The likelihood of a chance correspondence is made less likely when families look for matches only among babies born into the same family. It is also questionable when only one body in the extended family had ever been marked in this way and when this custom is seldom practiced in the community.

There is also the possibility that after a random birthmark occurs, family members' faulty memories about the location of the experimental mark could lead them to think that the birthmark corresponds to it much

more closely than it actually does. It must be noted, however, that several people often see the body after it has been marked. This explanation would thus require faulty memories on the part of several people, perhaps involving peer pressure conformity effects that can sometimes be seen in groups (Asch 1956).

There is also the problem of statements or behaviors by the children that appear to relate to the life of the deceased. While some of the apparent connections may have been misinterpretations or over-interpretations by the families, others seem more difficult to dismiss. One example is N. N. W.'s statements to us about a photograph from 25 years ago. There are also objective behaviors, such as A. W.'s tendency to urinate standing up, that require explanation.

These objections suggest that etiologies other than chance warrant consideration. One would be a psychosomatic theory that is similar to the concept of *maternal impression*. Maternal impressions involve the effects on the fetus of sights that a mother witnesses, and they were a serious topic for discussion in medical journals until about 100 years ago. Some authors argued that a stimulus during and perhaps even before pregnancy that had a significant psychological impact on the future mother, such as the sight of a deformed individual, could cause a corresponding birthmark or birth defect on the child (Ballantyne 1891–1892, Dabney 1890, Drzewiecki 1891). In the West, the concept came to be seen largely as superstition for two reasons. First, when it became known that the nervous systems of the mother and fetus were not connected and that their circulations were separated by the placenta, the fetus was regarded as sufficiently encapsulated to shield it from any temporary physiological and emotional disturbances its mother might encounter during pregnancy (Warkany 1959). Second, during several months of pregnancy any woman may experience a large number of impressions. If her child is born with a birthmark or birth defect, she may then select a suitable impression to explain the defect. In other words, with a prevailing view that a fetus could not be modified by its mother, any apparent correspondences between maternal impressions and birthmarks or birth defects were explained as being due to chance. Today, maternal impression is rarely discussed in modern medical journals other than for historical purposes. Some authors, however, have published reports that attempt to keep the concept alive as one that may have validity (Farkas & Farkas 1974, Stevenson 1992, Williams & Pembroke 1988).

The theory of maternal impression differs from experimental birthmarks in that maternal impressions were usually assumed to be associated with surprising and often terrifying impressions experienced by the future mothers. In countries with a tradition of experimental birthmarks,

however, these negative experiences are seldom regarded as a necessary or even favorable condition for the transfer of markings. In some ways, the experimental birthmark process is more similar to other phenomena of physical changes produced by suggestion. Examples include hypnotic subjects who develop skin reactions matching previous wounds or even burns suggested by hypnotists, as well as individuals, often monks, who develop stigmata, skin markings matching the wounds of Christ. Numerous such case reports were reviewed by Stevenson (1997), and it appears that in susceptible individuals suggestion can have specific and localized physiological effects. As for experimental birthmarks, the question of how the suggestion of a birthmark in a mother's mind would be transmitted to the skin of the fetus remains unanswered, but so does the question of how a suggested injury is transmitted to the skin of a hypnotized subject. While the psychosomatic mechanism for such a process remains unexplained, we now know, of course, that some substances can cross the placenta, and we have evidence that at least in a general way a mother's emotional state can affect the fetus (Glover 1997, Lou et al. 1994).

If one accepts the premise that maternal impression can be considered as an etiology for experimental birthmarks, then there is still the question of whether it provides a satisfactory explanation for all of the details of the cases. Mothers of the subjects saw the marks on the deceased in only five of the cases. They heard, or may have heard, of the markings in eight others, but in at least two of these they did not know the site of the markings. That leaves at least five cases in which the mother did not even know of the mark on the deceased. Thus, although there are some cases in which the maternal-suggestion hypothesis provides a possible explanation that deserves serious attention, there are others in which the evidence for it is quite weak.

Another explanation is that experimental birthmarks represent a phenomenon of consciousness. There are two types of consciousness-mediated processes to consider. The first would be one in which the prayers and wishes of the mourning family effected the development of the birthmark. Several double-blind studies in the medical literature have provided preliminary evidence that intercessory prayer or distant healing can have positive effects on the health of others with conditions such as heart disease and AIDS (Astin, Harkness, & Ernst 2000, Byrd 1988, Harris et al. 1999, and Sicher, Targ, Moore, & Smith 1998). This is consistent with more than 800 experiments in the parapsychological literature suggesting that consciousness can affect physical systems (Radin & Nelson 1989). Even these provide little basis for the idea that a prayer at a funeral could influence the fetal development of a child born months or years later, but they suggest that the possibility should not be rejected out of hand.

The other consciousness-related explanation involves what the villagers believe: that there is a continuation of the consciousness of the deceased individual in the child born with the birthmark. While this possibility may be the most speculative, it should be noted that Stevenson collected more than 2500 cases of children who appear to remember previous lives (Stevenson 2001) and more than 200 cases of children with birthmarks that correspond to wounds or other marks on the body of the identified previous personality (Stevenson 1997). Taken in that context, the six cases in our series in which the child made statements related to the life of the deceased individual indicate that this explanation warrants consideration.

Conclusions

In summary, these cases of experimental birthmarks may well represent a heterogeneous group. At this point, it is not clear if all of the cases arise merely from coincidence, but some have features that strongly suggest otherwise. Whether these cases represent a psychosomatic phenomenon, a consciousness-mediated one, or some other process, they at least deserve more study. After our investigations, we learned of 20 more cases: 14 in Thailand and 6 in Myanmar. If more cases are studied, it should be possible in the future to conduct statistical analysis of particular features that will allow for a better understanding of the processes involved.

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COMMENTARY

A Critical Response to David Lund's Argument for Postmortem Survival

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Abstract—In *Persons, Souls and Death,* David Lund (2009) presents a cumulative case argument for postmortem survival based on the ostensible explanatory power of survival in relation to data drawn from psychical research. In this paper I argue that the survival hypothesis does not satisfy at least two necessary explanatory criteria accepted and deployed by Lund. First, the data that the survival hypothesis ostensibly explains are not otherwise improbable, as much if not all of the data may be adequately accounted for in terms of psychic functioning among living agents—the LAP hypothesis. Here I argue in considerable detail that Lund's criticisms of the LAP hypothesis, like those leveled by many other survivalists, are significantly defective. Second, the survival hypothesis does not lead us to expect the data Lund outlines, so it fails with respect to predictive power. Since the "best explanation" is one that leads us to expect what is otherwise improbable, the survival hypothesis is not the best explanation of the data that Lund considers.

Introduction

In *Persons, Souls and Death: A Philosophical Investigation of an Afterlife* (Lund 2009), philosopher David Lund presents an argument for postmortem survival informed by reflections in the philosophy of mind and the data of psychical research. Like many recent treatments of the survival question among philosophers (Almeder 1992, Braude 2003, Griffin 1997, Paterson 1995), Lund assesses the case for survival as a cumulative case argument based on several different strands of observational evidence collected from the domain of psychical research: data from near-death experiences, apparitional experiences, cases of the reincarnation type, and mediumship. He also provides substantial engagement with a range of questions in the philosophy of mind as a prelude to his evaluation of the evidential force of data collected from psychical research.

In the course of his exploration Lund argues four main points concerning the survival hypothesis:

(I) the antecedent probability of the survival hypothesis is not too low.
(II) the survival hypothesis is the best explanation for the range of empirical data drawn from near-death experiences, apparitional experiences, cases of the reincarnation type, and mediumship.
(III) the survival hypothesis is more probable than not.
(IV) belief in the survival hypothesis is a rational belief.

Like many other defenders of the survival hypothesis, Lund makes his case for the rationality of belief in survival by assigning this belief a certain evidential or conditional epistemic probability[1] on the basis of its possessing certain explanatory virtues and its not being an antecedently unlikely hypothesis. In essence Lund argues for (IV) on the basis of (III), and he argues for (III) on the basis of (I) and (II).

The focus of this paper will be Lund's argument for (II), though at points I will touch on the logical connection between (II) and (III). Since Lund's argument for (II) depends on arguments that attribute explanatory merit to the survival hypothesis and the alleged failure of competing explanations to achieve the same level of explanatory efficacy, I will critically discuss not only what Lund says on behalf of the alleged explanatory power of the survival hypothesis but also his criticisms of what he regards as the strongest explanatory competitor to survival, the appeal to psychic functioning among living persons. My exploration of the survival hypothesis and the logic of inference to best explanation will show that Lund underestimates the difficulty of attributing superior explanatory power to the survival hypothesis over the living-agent psi alternative. Consequently, Lund does not give us a very good reason to believe that (II) is true. In fact, I will also argue that Lund's own criticism of the appeal to living-agent psi contributes to a case for supposing that (II) is false. I hope these criticisms will bring clarity to the points at which survival arguments in general are most vulnerable to defeat and therefore attention to the liabilities that future survival arguments must overcome if they are to succeed.

The General Structure of Lund's Argument

Lund makes it clear at the outset of his book that we cannot have, nor should we expect to have, epistemic certainty about survival, nor are the arguments for survival conclusive or compelling (Lund 2009:7, 217–218). In this respect his position exhibits a modesty not displayed by some prominent writers on postmortem survival who maintain that the evidence for survival is so strong that not believing in survival is irrational (Almeder 1992:62, 1996:507–509). Lund takes the position that there can nonetheless be grounds for rational

belief in survival. These grounds amount to a cumulative case probabilistic argument for survival (Lund 2009:127, 212, 217). The case is *cumulative* because the conclusion that some people survive biological death is inferred from several different and independent lines of evidence that individually add weight to the survival hypothesis: data from near-death experiences (Lund 2009:114–128), apparitional experiences (Lund 2009:129–152), cases suggestive of reincarnation (Lund 2009:153–180), and mediumship (Lund 2009:181–203). It is a *probabilistic* argument because Lund does not claim that the data logically entail survival, but rather the data collectively confer likelihood or probability that survival is true, specifically that the balance of probability favors the survival of human consciousness beyond death. Relative to the evidence Lund outlines, the survival hypothesis is more probable than not (Lund 2009:215–218).

According to Lund, the survival hypothesis acquires a probability or likelihood of being true based on its ostensible explanatory power and its not being an antecedently unlikely hypothesis.[2] What is required here is the widespread, though arguably problematic, principle that propositions may acquire degrees of probability based on their explanatory efficacy, and by virtue of their level of probability they acquire epistemic credentials of various sorts (e.g., rational, justified). Survival allegedly "accounts for" or "explains" the data Lund presents in much the same way food poisoning might explain Jack's symptoms of illness that developed a few hours after eating a bacon cheeseburger because food poisoning can plausibly be construed as the *cause* of his symptoms (Lund 2009:125, 142–144, 149–152, 211–218). The survival hypothesis postulates the postmortem continuation of the individual person as a distinct center of consciousness as the cause of the data Lund presents.

The survival hypothesis not only explains the data in Lund's view, but it provides the *best* or *most plausible* explanation from a narrow range of explanatory competitors that postulate something other than a postmortem self as the cause of the data (Lund 2009:137, 213–217). Hence, the survival hypothesis allegedly has explanatory virtues not shared by alternative hypotheses or has such virtues to a greater degree than its competitors. These fall into two classes. "Naturalistic explanations" postulate purely natural laws that describe the physical and mental activity of human beings (Lund 2009:112, 120, 135–136, 167–170). These include the general appeal to coincidence or fraud (in mediumship), hallucinations (in apparitional experiences), cryptomnesia and paramnesia (in cases of the reincarnation type), and various psychological and physiological processes or mechanisms (in near-death experiences). "Paranormal explanations" postulate psychic functioning in living agents in the form of extra-sensory perception (ESP),

psychokinesis (PK), or some combination of the two (Lund 2009:156, 163, 170–171, 184).

Whether survival is the best explanation of the data depends of course on the application of criteria of explanatory efficacy. Lund does not provide a detailed or systematic account of explanatory virtues, but we can partly infer his position here from how non-survival explanations allegedly fail to be plausible or good explanations. With reference to both the naturalistic and paranormal hypotheses, one of Lund's frequent criticisms is that these hypotheses do not *fit* the observational data. By this he means that these hypotheses either do not lead us to expect the data or they lead us to expect something that is actually incompatible with the data. For example, Lund argues that while some naturalistic explanations of NDEs postulate causes that would lead us to expect some of the phenomenal features of NDEs, some of the postulated causes lead us to expect experiential features that are incompatible with their actual phenomenology, and none of the natural causes leads us to expect veridical experiences of the sort reported in NDE cases (Lund 2009:112–116). In the case of paranormal explanations of NDEs, Lund argues that, while paranormal explanations might lead us to expect some of the veridical features of NDEs, nothing we know about living-agent psi leads us to expect the vivid, rich, and detailed phenomenology associated with such experiences (Lund 2009:121–126). This strategy is repeated for each strand of ostensible survival evidence.

So following typical accounts of inference to best explanation, Lund sees what is often called *predictive power*[3] as at least a necessary component of explanation. A good hypothesis leads us to expect our observational data, and it does not lead us to expect anything incompatible with our observational data. Second, though, Lund contends that non-survival explanations must in some sense be ruled out prior to accepting survival as the best explanation (Lund 2009:177). This is why Lund devotes considerable space to criticisms of alternate hypotheses. It would seem that Lund is committed to the explanatory power of a hypothesis being partly a function of its leading us to expect phenomena that are otherwise not to be expected. This would not be the case if there were nearby explanatory competitors with high predictive power in relation to the same data. Technically stated, the *prior probability of the data* must be fairly low. Finally, Lund frequently invokes *simplicity* as a virtue of the survival hypothesis (Lund 2009:215). Even where other hypotheses account for the data, they do so as more complex hypotheses, and this counts against their plausibility. So (II)—Lund's central claim—amounts to the more specific claim that the survival hypothesis is a relatively simple hypothesis that leads us to expect a suitably robust range of observational data that are otherwise quite unlikely.

The Living-Agent Psi Hypothesis

Lund recognizes, rightly in my view, that the nearest explanatory competitor to the survival hypothesis is the appeal to psychic functioning among living persons (hereafter, LAP for "living-agent psi"). Lund provides a fairly detailed examination of this exotic hypothesis throughout his book and attempts to show that it is explanatorily inferior to the survival hypothesis (Lund 2009:118–128, 142–151, 171–173, 203–204, 212–215). Like many other survivalists, Lund explicitly accepts the reality of LAP (Lund 2009:207, 213–214). However, he maintains that, as an explanation of the data drawn from psychical research, it is in crucial respects inferior to the survival hypothesis.

Lund identifies three defects in the LAP hypothesis.

> (a) LAP as currently understood and ostensibly established in parapsychology from an analysis of phenomena outside the context of cases suggestive of survival does not account for the full range of survival data (Lund 2009:120–123, 125–127, 171–177).
>
> (b) The only version of the LAP hypothesis that properly accounts for the full range of data requires adopting what is often called the "super-psi hypothesis," but this hypothesis lacks independent support since it involves postulating psi of a considerably greater degree and refinement than ordinary psi (Lund 2009:149–150, 212–214).
>
> (c) The super-psi hypothesis is a highly complex hypothesis compared to the survival alternative, and simplicity is preferred to complexity in choosing among hypotheses (Lund, 2009, pp. 142, 152, 215).

So Lund presents a kind of dilemma for advocates of the LAP hypothesis. LAP is either a hypothesis for which there is independent support but which cannot account for the data or it is a hypothesis that can account for the data but at the cost of being an overly complex hypothesis for which we have no independent support. On the first horn of the dilemma, the LAP hypothesis may be antecedently plausible or probable but lacks explanatory merit. On the second horn of the dilemma, the LAP hypothesis has explanatory merit but its antecedent plausibility or probability is significantly lowered. Hence, the LAP hypothesis fails as an explanatory competitor to the survival hypothesis. In this section and the next I want to focus on (a) and (b) to dissolve this dilemma and undercut Lund's argument for supposing that survival is the best explanation of the data. In the section **The Predictive Power of the Survival Hypothesis** I will build on considerations explored here to rebut Lund's contention that survival is the best explanation of the data.[4]

Essential to Lund's overall argument is a notion of "ordinary" LAP. This is a concept of LAP informed by experimental research and the analysis of spontaneous phenomena outside the laboratory. Based on paradigmatic cases of LAP drawn from these sources, ordinary LAP involves some fairly clear characteristics that function as constraints on the explanatory efficacy of the LAP hypothesis. The argument is an old one urged by survivalists against appeals to LAP (Dodds 1934:160). However, before critically examining Lund's reasons for believing that ordinary LAP does not do the necessary explanatory work, we should first explain the idea of ordinary LAP and why some parapsychologists have maintained that it poses a challenge to the survival hypothesis.

The Conception of Ordinary LAP

The conception of so-called ordinary LAP depends largely on data associated with qualitative and quantitative experimental research typically conducted in laboratory settings, as represented for example in ganzfeld, remote viewing, and random number generator experiments which have tested for telepathy, clairvoyance, precognition, and PK. Some of the results from this experimental history are worth noting since they inform us about the characteristics of ordinary LAP.

The data collected from forced-choice tests[5] (e.g., card-guessing and random number generator experiments) indicate a statistically significant above-chance selection of fixed and limited targets by experimental subjects, as well as positive correlations between the intentions of experimental subjects to alter various kinds of output from random number generators (RNGs) in particular ways and actual changes in their output (Braude 2002:64–101). If such data are evidence for LAP, they at least provide evidence that some people are capable of acquiring knowledge of simple images on cards (through telepathy and/or clairvoyance) and causally influencing presumably otherwise random physical systems. While these may seem like fairly weak effects, the data from some RNG experiments are compatible with interpretations that involve more radical manifestations of psi, ranging from living agents having direct causal influence over the past (retroactive PK) to their successfully using multiple psi processes that combine PK and highly refined precognition (Braude 2002:68–78). Since precognition itself raises the specter of the future affecting the past (to account for some person at present time knowing what will happen at some future), it may be necessary to postulate a very powerful clockwise ESP and PK, one that involves psychic access to highly detailed information and influences on large-scale events (Braude 1997:233–253). Moreover, the experimental data also provide good evidence that PK success is independent

of task complexity. PK appears capable of influencing target systems of varying types and complexities (where this includes the complexity of the experimental design), and it is efficacious even when subjects are blind to the target and details of the RNG mechanism, as well as when subjects do not even know that they are involved in a PK experiment (Kennedy 1978, Stanford 1977:338–342, 370–374).

Free response experiments seem to provide more direct evidence for LAP of broader scope, potency, and refinement. In the dream laboratory at Maimonides Medical Center, a decade-long run of experiments tested subjects for telepathy and clairvoyance during their dream states (Ullman & Krippner 2002, Sherwood & Roe 2003). In these experiments many subjects scored significant "hits" by providing descriptions of their dream content that corresponded thematically and often in specific details to randomly selected pictorial targets, typically in the form of paintings or art-prints. Telepathy-specific experiments involved agents, sometimes at a great distance from the subject, who focused on the target and attempted to "send" the image to the subject during their REM state. The results suggest that in altered states of consciousness detailed imagery in a narrative format mediates telepathic or clairvoyant interactions. In ganzfeld experiments subjects have achieved significant hits with static and dynamic targets (ranging from pictures to movies) during a waking but sensory-restricted state (Honorton 1985, Bem & Honorton 1994). In the STARGATE remote viewing program, subjects in normal states of consciousness have produced accurate and sometimes detailed verbal descriptions and drawings of large outdoor targets at a great distance (including large and small buildings, underground facilities, and natural settings), with and without any ostensible sender (May 1996, Targ 1996, Puthoff 1996). Where our ordinary conception of LAP draws on data from free-response experiments, ordinary LAP entails the telepathic, clairvoyant, and perhaps even precognitive acquisition of information about complex and dynamic targets, and it is often mediated by detailed mental imagery.

While many parapsychologists wish to limit claims about LAP to what has been ostensibly established in the above kinds of experimental contexts, Braude (1997) has provided what I regard as a compelling case for including spontaneous case data.[6] These are significant in that they both reinforce the general conclusions drawn from experimental research and further extend our conception of the potency and refinement of LAP. Many such cases provide ostensible demonstrations of a wide range of large-scale PK effects, including knocks and raps, apports, levitations, and materializations. We find these not only in the older physical mediumship of D. D. Home and Eusapia Palladino (Braude 1997), but also similar phenomena in modern

RSPK cases (Roll 2004) and modern controlled sitter-group situations, such as those conducted by Kenneth Batcheldor (Batcheldor 1966, 1984) and Alan Robert George Owen (Owen & Sparrow 1977).[7]

Similarly, documented cases of veridical apparitions of the living provide evidence that living persons are capable of impressive psychic accomplishments. Some living persons have reported the perceptual experience of some other living person at a location where the apparent's body was not located (Hart 1956, Broad 1962:147–152, 167–189). In some of these cases, the apparent has formed an intention to appear to a particular person, while in other cases the apparent has an out-of-body experience in which she experiences herself traveling to particular places and acquiring knowledge of the happenings at the location where she is perceived. As Lund notes (Lund 2009:134), it is hard to avoid the conclusion that these are instances of LAP. If they are, though, we have living agents who are capable of experiencing themselves moving through regions of physical space to specific locations where they acquire information that would be possessed by people at those locations using their senses. These would be cases of clairvoyance involving the subjective sense of being outside one's body and knowledge derived from detailed imagery of physical environments at locations remote from one's physical body. In cases where other people perceive the apparent, the apparition must either be a quasi-physical entity, the knowledge of which arises by the use of the ordinary senses of the perceivers, or it must be a mental image. On the former interpretation, the apparent must have the capacity to produce a temporary physical or quasi-physical substance, which sufficiently resembles herself, in some region of space away from her physical body. This is a clear example of a large-scale PK effect produced at a specific location, combined with clairvoyantly acquired information about the location where the effect is produced. On the latter interpretation, the apparent must have the capacity to causally influence the minds of some other person at a great distance, resulting in a temporary, interactive mental image of sufficient resemblance to herself, and to telepathically or clairvoyantly acquire information about the environment at the location where she is experienced by the perceivers.

The Prima Facie Explanatory Relevance of the Ordinary LAP Hypothesis

On the basis of the above experimental, semi-experimental, and spontaneous case data, we can begin to see why some parapsychologists have maintained the explanatory relevance of ordinary LAP to data allegedly suggestive of survival. Some comparison and contrasts with the survival hypothesis will be necessary to develop this.

(1) There is independent evidence for LAP that is broad in magnitude and very potent (including both small-scale and large-scale phenomena), as well as refined in its operation (often combining multiple psi processes and resistant to task complexity).

As E. R. Dodds pointed out in the early twentieth century, the LAP hypothesis appeals to a kind of causal agency and cognitive functioning for which we have independent evidence and agents (embodied ones, as opposed to discarnate ones) whose existence is not antecedently in question, even if it requires an expansion of the antecedently known boundaries of the causal and cognitive powers of human agents (Dodds 1934:156). In this way advocates of the LAP hypothesis emphasize epistemic conservatism: Adopt hypotheses that fit with background knowledge in the precise sense of involving agents and causal processes for which we have independent support. It is better (from the epistemic point of view) to postulate entities and processes whose existence is independently known than appeal to novel ones, since—all other things being equal—the antecedent probability of the former is higher.

(2) LAP provides an explanation of the veridical features of the data.

To see why (2) is true, consider why the veridical features of the data are suggestive of survival. In cases of mediumship and ostensible reincarnation, some living agent has knowledge that—due to its highly specific, systematic, and private nature—a particular formerly living person was uniquely situated to possess. To be "uniquely situated" with respect to some body of knowledge is to be in a position with respect to this knowledge that no one else is in, or at least to be better situated with respect to the knowledge than any other person would be. This is obviously not the case for individual bits of knowledge about the deceased, as many other people will have that kind of knowledge. The knowledge in view here is a body of knowledge that forms a coherent narrative of significant aspects of the deceased person's life and personality. Call this knowledge K. Since psychological continuity—continuity of a person's various mental states (intentions, thoughts, memories)—is an important indicator of personal identity, the continuation of K is evidence that the person, with whom K was originally associated, has continued to exist, either as a re-embodied living person or a discarnate entity utilizing a medium to communicate with living persons. The same principle seems operative when considering the *prima facie* force of the veridical features of apparitional experiences and NDEs as evidence for survival.

So survival inferences from veridical features of the data depend on the following sort of premise:

(SV) There is some living agent, A, who has knowledge K, where K is such that some deceased person D is uniquely situated to be the source of K.

(SV) makes it clear why the survival hypothesis has apparent explanatory power over data associated with cases of mediumship and cases of the reincarnation type. If the veridical features of such data are linked to the deceased in the way indicated by (SV), then these features of the data are not very likely to occur unless survival is true. In other words, (SV) entails that the prior probability of the veridical features of the data is low. Since the explanatory power of a hypothesis is a function of high predictive power and the low prior probability of the data, (SV) boosts the explanatory power of the survival hypothesis. Now the prima facie appeal of the LAP hypothesis is that it tells an alternate story about how K *could* have been acquired solely as the result of paranormal cognitive processes in embodied, living agents. This story seems to erode the otherwise maximally tight connection between the deceased and the stock of accurate and detailed information about the deceased that is communicated in the better survival cases. In essence the LAP hypothesis rebuts the contention that some deceased person or the temporarily disembodied consciousness of a living agent is *uniquely* situated to be the source of K. By rebutting (SV) in this way, the LAP hypothesis is a kind of defeater or doubt-maker for the inference for survival, inasmuch as that inference depends on the truth of (SV).[8]

We can illustrate this by considering how the LAP hypothesis operates to defeat the inference for survival from the data of mediumship. The medium is a living agent who possesses K, but we can know this fact only because someone other than the medium has verified the medium's claims about the deceased. But this process of verification requires that facts about the life of the deceased be known or knowable by people independently of the medium's testimony. This in turn requires an accessible source of the relevant information, e.g., other living agents having the information or the information being available in documents. But in that case the medium might have acquired K by telepathically or clairvoyantly accessing these sources. In other words, the deceased person D is not so uniquely situated with respect to K if living agents have psychic functioning and the information that constitutes K is psi-accessible. Moreover, there is no compelling reason to suppose that the information that constitutes K is not psi-accessible once we postulate even ordinary LAP and observe that we simply are not warranted in stipulating any clear-cut boundaries for its magnitude or efficacy, a point to which I will return below in the section ***Response to Lund's Criticisms of the Ordinary LAP Hypothesis.***

(3) There is prima facie evidence that in some instances mediumistic claims, ostensibly originating from the deceased, are actually the product of telepathic interaction with the minds of sitters.

There are at least two kinds of considerations in support of (3). First, there are cases where the medium's highly specific claims about the deceased are actually false, but where these incorrect claims correspond to incorrect beliefs held by the sitters (Myers 1889–1890:568–571, 581–583, Podmore 1910/1975:165–166). Since the claims in question concern highly specific matters about which the deceased is unlikely to have been mistaken, and it is not surprising that agents other than the deceased would have been mistaken, we have evidence that the correspondence between the medium's false claims and the sitter's false beliefs is the product of telepathic interaction between their minds. Moreover, it seems implausible to suppose that the medium's telepathic acquisition of information from the minds of the sitters would take place only on occasions where the sitters entertained false beliefs about the deceased. So it seems reasonable to infer that at least some of the medium's veridical claims about the deceased should also be the product of telepathy with the sitters.

Second, there are cases where the content of mediumistic communications seems to correspond in a striking way to matters recently and randomly experienced or mentally entertained by the sitters. For example, in some sittings the medium spontaneously introduces the name and other identifying details of a deceased person but the person happens to be related to a living person whom the sitter has only recently randomly encountered or who may have through chance coincidence been on the mind of the sitter (Salter 1926:69–72). When the claims of mediums relate to fortuitous aspects of the sitter's very recent experiences, it seems that the medium is simply tapping into the sitter's recent memory to guide the narrative of the sitting, rather than this being evidence that a deceased person has highly impeccable timing for showing up at a sitting with precisely this sort of information. More persuasive along these lines are cases where obviously fictitious communicators or controls appear at séances, but their identities happen to correspond in some way to what sitters were thinking about prior to the séance (Sidgwick 1915:85, 297ff, 437–448). Because of their highly specific or idiosyncratic nature, it seems implausible to suppose that these latter kinds of correlations would be merely fortuitous. In that case, though, we have prima facie evidence that the medium not only has telepathic interaction with sitters, but she sometimes presents or constructs (ostensibly deceased) personalities from telepathically derived information from the minds of the sitters. It seems unlikely that telepathy with sitters would only

operate when the personalities entertained by sitters were fictitious. So it is plausible that on different occasions the names and characteristics of deceased family members and friends would also enter into the medium's mind through telepathic interaction.

(4) Contextual features of paradigmatic cases of LAP have characteristics that significantly resemble other important features of the data.

If the conception of the LAP hypothesis includes contextual features of paradigmatic cases of LAP, then the LAP hypothesis does not merely cover the acquisition of accurate and detailed information about the deceased but also the manner in which this knowledge is often acquired or conveyed. As explained above, we have evidence that living agents sometimes exercise clairvoyance during dynamic out-of-body experiences, and this parallels both the phenomenal and veridical features of NDEs. Moreover, data from cases of apparitions of the living (and dying) provide evidence that living agents can produce full-blown apparitions of themselves to other living agents (through PK or telepathy), sometimes with clairvoyantly acquired information about the environment in locations where their apparitions are perceived. While such apparitions are of the living and not the dead, once we grant that living agents can psychically produce accurate, lifelike, and seemingly localized representations of themselves that are experienced by other living persons, LAP can account for most of what stands in need of explanation in cases of apparitions of the dead. The remaining question as to why some apparitions are of the deceased will be addressed below in the section ***Response to Lund's Criticisms of the Ordinary LAP Hypothesis***.

From the point of view of explanatory efficacy, it is highly relevant that (3) and (4) show us that LAP can *mimic* important strands of survival evidence. So we can say that while (2) informs us that LAP provides an explanation of the veridical features of the data, (3) and (4) each leads us to expect the presentation of such veridical features through an *appearance* of survival. The appearance of survival is generated by structural similarities between survival data and the data that informs our ordinary conception of LAP, at least where the latter draws on data from spontaneous cases. The gap between ordinary LAP and what would be true if the data were the product of discarnate persons seems to converge at crucial points. This further reinforces the point raised above (in connection with (2)) that ordinary LAP boosts the prior probability of the veridical and some of the phenomenal features of the data, as well as their joint occurrence. In this way, the ordinary LAP hypothesis reduces the explanatory force of the survival hypothesis. Important strands of the data do not seem surprising or unlikely if the survival hypothesis is false.

Lund's Criticisms of the Explanatory Force of the Ordinary LAP Hypothesis

We are now in a position to assess Lund's criticisms of the ordinary LAP hypothesis, an essential part of his contention that the survival hypothesis is the best explanation of the data. Recall that Lund maintains that (a) ordinary LAP is explanatorily defective since it does not explain important features of the data, and (b) this can be remedied only by adopting a super-LAP hypothesis that requires postulating a degree or magnitude of psi for which we have no independent evidence. In this section I will argue that (a) and (b) are both false.

Three Explanatory Defects in Ordinary LAP

As Lund sees it, the data in need of explanation include features that are not found in paradigmatic cases that inform our conception of ordinary LAP. As noted earlier, survivalists have long opted for this strategy in arguing against the explanatory efficacy of appeals to LAP (Ducasse 1961, Dodds 1934). Lund assumes that if the data have qualities that ordinary LAP does not, an appeal to the latter does not serve to explain the former. Lund identifies at least three aspects of survival cases that manifest this incongruity: veridical features, phenomenal features, and skill-set features.

First, Lund argues that while it is true that ordinary LAP might explain how a living person acquires intimate and detailed knowledge about the life of a formerly living person, there are more fine-grained features of the veridical aspects of the data that ordinary LAP cannot explain because ordinary LAP does not have these features. His main illustration of this concerns the quantity and diffusiveness of detailed information presented in data drawn from mediumship and ostensible reincarnation cases. If such information were acquired through LAP, living agents would have to tap into multiple sources and integrate the information from these sources into a coherent narrative. But there are no paradigmatic cases of ordinary LAP in which the information possessed by one mind has been drawn from multiple other minds or remote locations and synthesized into a single seamless narrative (Lund 2009:184–186, 188, 191, 193–194, 197–198).

Second, there are phenomenal features of the data that are not present in cases of ordinary LAP. In near-death experiences, subjects have a distinct sense of being outside their bodies (Lund 2009:121, 125), and they typically perceive the physical environment from a particular position in space above their body (p. 123). Ordinary telepathy and clairvoyance, though they involve the acquisition of knowledge about states of affairs external to the subject, are not accompanied by this kind of perceptual imagery. Moreover,

Lund contends that the clarity and accuracy of perceptions during out-of-body experiences exceeds the degree of clarity and accuracy in ordinary cases of clairvoyance (Lund 2009:121). Mediums acquire their information about the deceased in a way that seems to them like it is originating from the deceased with whom they are interacting. In data suggestive of reincarnation, subjects not only have knowledge of some formerly living person, but they have this knowledge in the form of memorial experiences. Paradigmatic cases of telepathy and clairvoyance do not involve this (Lund 2009:172–173). With respect to apparitional experiences, ordinary LAP does not involve the creation (through ESP or PK) of apparitions of a third person who appears to a particular perceiver, so ordinary LAP cannot explain communicating apparitional experiences of the deceased, whether these are experienced in deathbed scenarios or elsewhere (Lund 2009:25).

Finally, data drawn from both reincarnation cases and mediumship involve a variety of skills (e.g., linguistic, musical, literary) associated with a formerly living person, but ordinary LAP involves the transfer of information not the transfer of skills (Lund 2009:176–177, 193). It can explain knowledge *that* something is true, but not knowledge *how* to do something that requires learning and practice. Children who remember past lives, for example, do not simply have knowledge of the lives of formerly living persons, but they sometimes display many of their musical, artistic, or linguistic skills. Similarly, mediumistic data often include the medium's exhibiting detailed information about the deceased through facial expressions, tone, and vocabulary and sentence structure characteristic of the deceased person, as well as other personality features. We have no parallel to this in paradigmatic cases of ordinary LAP.

Response to Lund's Criticisms of the Ordinary LAP Hypothesis

One of the difficulties with Lund's procedure for ruling out explanations in terms of ordinary LAP is that the boundaries of ordinary LAP are not as clear as Lund suggests. For example, if we turn to random number generator experiments, the way in which some of these experiments provide evidence for LAP is compatible with different stories about what specific psi processes are being utilized and the requisite degree or magnitude of psi. As indicated earlier, the statistical data can be interpreted in ways that permit, and may even demand, a fairly powerful and refined sort of LAP whose success is resistant to the typical limitations of task complexity.

Drawing conclusions about the boundaries or limits of LAP based on what we take to be paradigmatic cases of psi can be tricky for another reason. It is not immediately clear what to say about cases exemplifying characteristics not present in our current paradigmatic set of psi cases. Do

they represent an entirely different phenomenon such as survival or are they simply cases of LAP that exhibit properties not found in what we have antecedently accepted as paradigmatic cases of LAP? In other words, when we come across cases that resemble psi in certain ways but also include novel features, why should we not regard such cases as providing evidence for the expansion of the domain of LAP, especially when such a view would be compatible with theorizing about psi based on the experimental data. This matter can be very sneaky, for when Lund asks for "independent evidence" for super psi, it is natural to ask whether any such evidence could be presented that would not be regarded by Lund as evidence for survival.

Now these are just two preliminary methodological concerns, but there are substantial problems too. Lund's contention that some characteristics of survival data are not found in paradigmatic cases of LAP is mistaken at several points.

Lund attributes apparitions of the living to LAP (Lund 2009:131–134), but he argues that since we have no cases of LAP where a person creates an apparition of another person who is deceased, this characteristic of apparition-of-the-deceased cannot be explained by ordinary LAP. To illustrate one of the concerns mentioned above, note that if LAP *did* have this characteristic, it would be a case that is phenomenally indistinguishable from apparitions of the dead that Lund takes to be evidence of survival. Moreover, notice that Lund has described the relevant characteristic as an apparition of a person distinct from one's self, not the more generalized description of *apparition of a person*. But there's no evidence that suggests that the psi needed to produce an apparition of one's self is any less potent or refined than the psi needed to produce an apparition of another person (dead or alive). Claiming that we have *no* evidence that LAP can produce apparitions of other persons is a lot like saying that we have no evidence that a particular artist is capable of painting a picture of other people because the artist's known works only include impressive self-portraits. If a person has the ability to produce a lifelike apparition of himself using LAP, it seems implausible to argue that LAP *cannot* account for apparitions of the deceased, unless of course one has independent evidence that the LAP needed for the latter is radically different in kind from the LAP needed for the former.[9]

Moreover, the conclusion that apparitions of the dead are ESP or PK productions by the living is entirely compatible with Lund's own endorsement of Hornell Hart's conclusion (Lund 2009:134) that apparitions of the living and the dead are so similar in their characteristics that they should be regarded as belonging to the same kind of phenomenon. Since apparitions of the living involve the consciousness of the apparent being

the cause of the apparition, Lund infers that apparitions of the dead must involve the consciousness of "the deceased" apparent being the cause of the apparition. But this would be evidence for survival only if we had good reason to believe that "the deceased" caused the apparition at some point after death. Given the evidence for telepathic deferment (i.e. a delay between the time a telepathic stimulus occurs and when the subject actually experiences it), there is no way to adequately ensure that an apparition experienced at some particular time after the death of the apparent was in fact generated by the apparent *after* his death. Nor is it clear why the symmetry between cases of apparitions of the living and the dead require the conclusion that the consciousness *of the apparent* be the cause of the apparition, rather than the consciousness *of some living agent*.

Lund's alleged explanatory defects of the LAP hypothesis with reference to the phenomenal features of NDEs are equally suspect. As indicated earlier, in free-response tests for clairvoyance and telepathy, target hits are often mediated by imagery with varying degrees of detail and vividness. Also, cases of reciprocal apparitions include cases where subjects have a vivid sense, even perceptual experience, of being outside their bodies, together with other worldly imagery and perceptions of places, people, objects, activities, and events in this world. Since Lund mentions such cases and regards them as instances of LAP (Lund 2009:132–135), his claims about the inadequacy of the LAP hypothesis for accounting for these features of NDEs seems mistaken.

Lund also claims that ordinary clairvoyance and telepathy do not include instances where information is drawn and integrated from multiple sources, and yet in some mediumship cases the medium's knowledge, if the result of LAP, would have to have done precisely this, for at the time of the sittings no single source contained all the information communicated through the medium (Lund 2009:194–199). This has often been proposed as a serious problem facing the LAP hypothesis since it seems to demand super psi (Braude 2003:36–38, 82–84, 93–94, Gauld 1982:59–60, 68–73).

However, there are two problems with Lund's argument at this juncture.

First, Lund's general claim is contradicted by experimental research that provides evidence that subjects have successfully carried out ESP tasks involving the integration of information from multiple targets. For example, subjects have successfully carried out blind matching ESP tasks in which they have matched two unknown cards, as opposed to simply identifying a single unknown card (Kennedy 1995). While such experimental evidence is clearly not on the same level as the better mediumistic evidence, it is nonetheless highly relevant to the plausibility of accounting for that evidence in terms of LAP. Indeed, it is worth noting that with reference to

the Runki Case—a mediumship case involving multiple sources to confirm the medium's veridical claims—the principal investigators (Erlendur Haraldsson and Ian Stevenson) cautioned against a survivalist interpretation on the grounds that living agents (in spontaneous cases) have performed "remarkable feats" of psychically deriving and integrating complex information without any participation from purported discarnate persons (Haraldsson & Stevenson 1975:57).[10] I mention this in part because Lund himself appeals to the Runki case as being especially problematic for the LAP hypothesis because of what it would have allegedly involved in the way of the gathering and synthesizing of information from diverse sources (Lund 2009:195–199).

Second, Lund frequently mentions a concern about the "complexity" of the kind of psi that would be needed to account for survival data. This concern seems rooted in the assumption that LAP operates in a way analogous to ordinary information processing, proceeding in a step-by-step manner, gathering and then organizing information. Lund thinks that psi would have to move through discrete steps or stages: selecting, organizing, and integrating information. It is as if LAP would have to operate like a librarian trying to reconstruct a physical card catalogue after the cards had been scattered throughout a city by a hurricane and mixed together with tens of thousands of other pieces of paper (Lund 2009:174, 199). However, as explained earlier and illustrated by blind psi experiments, the experimental data suggest that LAP is not bound by the constraints of ordinary information processing (Foster 1940, Kennedy 1980). LAP seems resistant to many of the limitations that characterize task complexity. Lund's contention that LAP must become super LAP to account for "multiple source" cases seems to depend on a false premise about how psi is related to task complexity. More generally stated, we are not warranted in supposing that what is obscure, difficult, or complex from the vantage point of normal information gathering and organizing would present similar challenges to psi.

Finally, in the case of mediumship Lund contends that ordinary LAP does not involve the presentation of information in the form of subjective impressions of having originated from discarnate persons. However, this is not correct. As explained in connection with the section *"(3) There is prima facie evidence that in some instances mediumistic claims, ostensibly originating from the deceased, are actually the product of telepathic interaction with the minds of sitters"* above, in instances where mediums have made claims that are most plausibly the product of telepathy with sitters, they have had no less a subjective impression that the information was originating from the deceased with whom they were ostensibly communicating. And we will see shortly why LAP, once situated

in its broader psychological landscape, would lead us to expect situations where LAP is conjoined with the subjective impression that information is originating from a person distinct from the medium herself.

So Lund has significantly overstated the explanatory deficiencies of the ordinary LAP hypothesis, and some of his reasoning seems to depend on unwarranted assumptions about the limits of LAP or otherwise questionable inferences from the data. Nonetheless, we can grant Lund that at least *some* of the characteristics he attributes to the data are not found in cases that inform our conception of ordinary LAP, of these the two most important are the first-person character of veridical claims in ostensible reincarnation cases and the skill-set data Lund notes for both reincarnation-type cases and data from mediumship. It will of course be highly relevant whether the survival hypothesis can account for any of this, a topic to which I will return in the section **The Predictive Power of the Survival Hypothesis** below. For the moment I want to explore what can be said in defense of the LAP hypothesis in relation to such recalcitrant data.

Recalcitrant Data and the LAP Hypothesis

It is important to remember that according to Lund the survival hypothesis is the best explanation of the data only if it is a relatively simple hypothesis that leads us to expect a suitably robust range of observational data that are otherwise quite unlikely. The LAP hypothesis can defeat the satisfaction of this condition by leading us to expect or rendering unsurprising a significant *portion* of the data, even if it does not account for all the data. The latter would arguably be required if we are to be justified in claiming that the LAP hypothesis is clearly *superior* to the survival hypothesis as an explanatory competitor. But there is no need for such a strong claim in order to challenge the survival hypothesis. Even if we suppose that the ordinary LAP hypothesis is not the best explanation of the data, it might nonetheless reduce the conditional epistemic probability of the survival hypothesis, maybe even significantly enough to prevent the case for survival from being more probable than not.[11]

To see how this works, we only need to recall that explanatory power is a function of both the predictive power of the hypothesis and the prior probability of the data. With respect to the latter, the explanatory power of a hypothesis is inversely proportional to the value assigned to the prior probability of the data. A good explanation for some range of observational data is one that renders the data probable and where the data are otherwise improbable. The more likely our observational data are, whether or not our hypothesis is true, the less our hypothesis actually explains, even if our hypothesis leads us to expect our data. Where the epistemic probability of

the hypothesis is conditioned by its explanatory power, the net result is a diminished epistemic probability for the target hypothesis.

Now the prior probability of the data is just the probability of that data conditioned on our background knowledge—how likely are the data given everything else we know (independent of the survival hypothesis)?[12] Arguably, the strength of survival arguments against the usual naturalistic counter-explanations (e.g., fraud, coincidence, malobservation) is that central features of the data, such as the way in which the data exemplify veridical features, are still improbable given the usual naturalistic suspects. However, unlike the naturalistic explanations, LAP appears to render significant domains of the relevant data probable or unsurprising: living agents having veridical apparitional experiences, detailed and systematic knowledge of deceased persons they have never met, veridical out-of-body experiences, and the occurrence of physical phenomena (and some mental phenomena) with the appearance of having originated from discarnate entities. Since the explanatory power of the survival hypothesis depends on these domains of data being improbable apart from the truth of the survival hypothesis, the fact that LAP makes them probable results in a diminished explanatory role for the survival hypothesis. This is true, even if the survival hypothesis also renders such data probable, and even if LAP does not render other survival data probable.

Finally, it is worth adding here a point that is easily overlooked in survival literature. The case for the explanatory superiority of the survival hypothesis would face a formidable challenge even if we did not know or could not show that the LAP hypothesis rendered the data probable. The survivalist is trying to show that the survival hypothesis is the best or better explanation of the data, but as we have seen this requires that the survivalist argue that the data are otherwise quite surprising. So the survivalist is in the rather difficult position of having to argue that the data, ostensibly rendered probable by the survival hypothesis, are *not* probable given the LAP hypothesis. However, it is exceedingly difficult to see how this can be shown without having to argue that the efficacy and magnitude of psi have rather clearly defined boundaries and limits. Lund nowhere argues this point, and—given our present state of ignorance about psi—I do not see how this *can* reasonably be done.

A Robust LAP Hypothesis without Super Psi

Up to this point my argument has assumed that the LAP hypothesis has limited predictive power. While it can account for veridical features of the data, it does not render probable data concerning skill-set features and the first-person character of the veridical features of the data in cases of

alleged past-life memories and some cases of mediumship. There is no doubt that LAP of unlimited scope and refinement would account for these data (and indeed everything!), but this is explanatory overkill and overlooks more sensible options that equally, if not more effectively, diminish the explanatory force of the survival hypothesis.

Since Pierre Duhem (1861–1916), it has been widely acknowledged in the philosophy of science that single hypotheses rarely have observational consequences. The testable consequences of hypotheses are the result of logical deductions from bundles of statements, typically a central hypothesis conjoined to various independently testable auxiliary assumptions. Even my own account of the explanatory power of the LAP hypothesis (in the section **The Living-Agent Psi Hypothesis**) relied on auxiliary assumptions. For example, I assumed that LAP is linked to contextual features of paradigmatic cases of psi so that we should expect both veridical and phenomenal features of ordinary psi functioning in non-survival contexts. Now in order to account for the apparently recalcitrant features of survival cases, we need simply to expand the content of the LAP hypothesis by adding the right sort of non ad hoc and independently testable auxiliary assumptions.

Motivated Psi Hypothesis

It seems to me that no survival theorist has done a better job at this than Stephen Braude, who has developed what he has called a *motivated* psi hypothesis. In this hypothesis, psychic functioning is integrally related to a person's larger psychological life, for example, a person's needs, goals, and interests, whether these be conscious or not (Braude 2003:13–14, 23–29). So the LAP hypothesis must be considered in conjunction with various auxiliary assumptions drawn from general and special psychology that illuminate the possible psychodynamics in which psychically acquired information about other (deceased) minds is embedded. The LAP hypothesis will therefore cover considerably more than the veridical features of survival data. This has highly relevant consequences for the explanatory power of the LAP hypothesis, especially over otherwise recalcitrant data. Sadly, Lund nowhere mentions Braude's *Immortal Remains* (2003), in which the motivated LAP hypothesis is systematically developed and its explanatory merits compared to the survival hypothesis, but Braude's motivated psi hypothesis adequately circumvents the kinds of problems that Lund believes render the LAP hypothesis implausible.

Consider first data from mediumship. Since a motivated psi hypothesis appeals to some living agent's psychological needs, the fairly widespread human interest in personal survival becomes explanatorily relevant for why the data should take the form of "survival evidence." Sitters typically have

a powerful and conscious interest in communicating with their deceased loved ones, and there is little doubt that mediumistic phenomena often meet their fundamental need for assurance that a loved one is still alive, for them to still connect with the person in some way, or to have assurance that their own life will not terminate with death. And many mediums have an overriding interest in offering comfort to sitters, and the appearance of the survival of a loved one provides just such a comfort. Such motivations would lead us to expect the content of much ostensible spirit communication, such as providing evidence that establishes the deceased person's identity and relaying messages that comfort family and friends.

Of course, needs may be covert and unconscious, and not even related to any interest in survival.[13] For example, when formerly living personalities in cases of the reincarnation type belonged to a higher caste, it is reasonable to ask whether the desire for increased social or financial status is a motivating factor in living agents identifying themselves with a former personality. And while it may be implausible to attribute such motivations to children with ostensible past-life memories, it remains a reasonable supposition for their parents or other family members. And of course interest in higher social class and its direct benefits is only one of a multitude of possible efficacious motivations for (unconsciously) simulating survival evidence. Others could include relieving parental or family responsibility, guilt, or anxiety over children born with physical or mental abnormalities or who develop negative character traits, both of which easily lend themselves to karmic interpretations in eastern cultures. Nor need the psychic agent with the relevant motivations be restricted to the family of the current personality, but the relevant psychic agent(s) might be family members of the former personality.[14]

Lund's Appeal to Discarnate Motivations

Lund is aware of the relevance of motivational factors in attempting to explain survival cases, but he appeals to motivation as a reason for preferring the survival hypothesis to the LAP hypothesis for some cases. For example, Lund argues that with respect to drop-in communicators—discarnate spirits who appear uninvited at séances but who are not related to the medium or any of the sitters—it seems that they have better reason to manifest than the medium has for engaging in psychic sleuthing that results in the acquisition of information about their particular life history (Lund 2009:195–199). When children claim to remember past lives, it seems that the child has less of a reason to identify himself with the formerly living person than the formerly living person would have to reincarnate (Lund 2009:175–176).

The intuitive obviousness of this in any particular case depends largely

on the range of motivations one is willing to entertain as plausible, as well as the scope of the relevant psychic agents. In cases of children who claim to remember past lives, Lund begins by restricting the pool of potential psychic agents to the children themselves, and he then argues that the negative social consequences for children who identify themselves with formerly living persons would override any personal motivation for identifying with them (Lund 2009:173–174). However, there are no obvious social stigmas or other negative social ramifications attached to many cases of children who remember past lives. Moreover, particular needs can function as powerful motivations in behavior even where the pursuit of satisfying such needs has negative social consequences. We need only think of the negative social consequences of associating with particular people, having a particular occupation, or identifying oneself with a particular religious group. Finally, in cases where children claim to remember past lives, there is no good reason to restrict the pool of relevant psychic agents to the children. The motivated agents may be family members or friends, either of the child or the former personality. Hence, even where there are negative social consequences for children who claim past lives, such consequences can easily be outweighed by the stronger needs or interests of other people.

In the case of drop-in communicators, Lund says that it seems inexplicable why without any apparent motive a medium would select one particular communicator as opposed to another and psychically acquire information about him, whereas the communicators seem to have good and often overt reasons for communicating (Lund 2009:195–197). Now for the two cases Lund describes, the Harry Stockbridge case and the Runki case, no actual reason is provided for supposing that the communicators actually had better reason for communicating. This is just asserted, without any analysis of the psychodynamics of the relevant sittings. Lund appears to be relying on Alan Gauld's account of these two cases (Gauld 1982:68–73), but Gauld provides no specific reason for favoring the motivations of the alleged deceased persons in either case. He only notes that, in connection with the Stockbridge case, the communicator indicated a wish to help one of the sitters who was also a military serviceman.

First, it is important not to exaggerate the extent to which drop-ins provide us with anything special here. Haraldsson and Stevenson point out (Haraldsson & Stevenson 1975:34) that *many* drop-in communicators provide no reason for their appearance, and they often disappear just as quickly as they appeared, leaving sitters with little if any knowledge of even their actual identities. So drop-ins as a class of communicators do not seem particularly special with respect to supplying us with clear-cut motives that outweigh the motivations that might plausibly be attributed to living

agents. In the absence of any stated motive, we are certainly not adequately situated to judge that discarnate persons have better reason to communicate on some particular occasion than mediums have for psychically acquiring information about them and unconsciously constructing their persona using such information.

Second, ruling out relevant and plausible motivations in living agents is only as effective as our abilities to grasp subtle and complex psychodynamics in particular situations. Motivations behind behavior are frequently not apparent to the subject or onlookers, even to those with the appropriate nose for detecting it. Attempting to uncover potentially psi-guiding psychodynamics in a group context is more difficult, though of course not impossible. There certainly *are* drop-in cases that seem susceptible to a kind of psychological deconstruction in terms of motivated psi once we dig beneath the psychological surface, as Jule Eisenbud did in his analysis of the Cagliostro case (Eisenbud 1993:227–243, Braude 2003:39–43). Moreover, the widely acknowledged fictitious nature of the controls of many mediums who nonetheless provide detailed and highly accurate information about the deceased is evidence that motivated psi is unconsciously guiding the manifestation of different personae in mediumistic settings, even though we sometimes cannot specify what needs or interests are at work. Therefore, we cannot treat the absence of evidence for relevant motivations among living agents in particular cases as evidence of their absence. While such cases do not provide compelling evidence that all drop-in cases are best explained in terms of motivated LAP, they do render drop-in phenomena less surprising than they would be in the absence of motivational considerations.

Third, suppose we agree with Lund that in *some* cases ostensible discarnate persons have a reason to communicate with the living that appears to outweigh any interests or needs that can be reasonably attributed to the medium (or sitters) as the alternate source of the discarnate persona. Unless the alleged motivations of the discarnate person are reasons to communicate with the particular medium, we are saddled with a similar problem. Many reasons for "communicating" with the living will not sufficiently explain why and how the communicator selects one particular medium as opposed to another from among the potentially thousands that exist to be the recipient of biographical snippets. To use one of Lund's own examples, an agitated Runki communicator shows up through the medium Hafsteinn Bjornsson in Iceland in the 1930s and says (for over a year) he wants his missing leg. But Runki's "unfinished business" underdetermines the selection of the medium Hafsteinn Bjornsson, in much the same way we might imagine that living-agent needs or interests served by the appearance of survival underdetermine Hafsteinn Bjornsson's selection of Runki. By contrast, in

the Harry Stockbridge case the alleged discarnate person provided a fairly specific reason for showing up, namely that he wanted to help a sitter who, like him, was a military serviceman. But if Harry is so motivated because of something he and a sitter have in common, living agents *could* psychically access this information and *would* if a convincing lifelike representation of Harry most effectively serves needs best met by an appearance for survival.

These sorts of issues reveal why it is difficult to determine whether an ostensible discarnate agent has a more sensible motive for showing up than what we might attribute to the living agents in such cases. Survivalists may point out that there may be ontological constraints on discarnate psi that limit or direct a motivation to communicate through mediums, a kind of otherworld to this-world filter. Perhaps Hafsteinn Bjornsson is the best or only *available* option to aid Runki in the retrieval and burial of his missing femur, or maybe the medium is just where Runki's discarnate psi fortuitously connects him. However, once we are willing to make these charitable accommodations to the survival hypothesis, there is no good reason for not extending the same charity to a motivated LAP hypothesis. Drop-in communicators may simply be the result of psychic sleuthing (by the medium or sitters) that is filtered or otherwise influenced by factors beyond the control of the medium and sitters. The sleuthing is interest-driven, but without any particular discarnate person in mind.

Dissociative Phenomena and Unusual Skills

While motivation is crucial to the directedness of psi processes, and hence to the LAP hypothesis, leading us to expect the appearance of survival, dissociative phenomena are of considerable importance as well. First, we have evidence that dissociative states are psi-conducive (Zingrone & Alvarado 1997), so needs that are served by the appearance of survival might be best met as the result of dissociative states. Second, we have good reasons for believing that the fictitious controls and communicators of trance mediums are dissociated aspects of the medium (Braude 2003:33–35, 56, Gauld 1982:114–118). If the conscious or unconscious needs of the medium (or sitters) are best satisfied by an appearance of survival, then—given the psi-conducive nature of dissociated states—the medium's making veridical claims about deceased persons during dissociated states would not be surprising. There are also some interesting similarities between the communicators and controls of mediums and alters in cases of dissociative identity disorder (DID), which arguably constitute evidence that the phenomena are closely related (Braude 1995:218–240).

However, the most relevant aspect of dissociative phenomena is that they provide illustrations outside the context of survival of the sudden

manifestation of novel skills without prior learning or practice. Recall that Lund claims that LAP cannot account for the skills displayed in the better cases of mediumship and ostensible cases of reincarnation, for example, the speaking of a new language, artistic or musical abilities, and refined literary skills. First, according to Lund, LAP can generate only knowledge-*that* something is true not knowledge-*how* to do something. Second, Lund argued that since the skills manifested in survival cases are skills that are developed through practice, their presence in living agents who have never engaged in the practice is very surprising. It is considerably less surprising if we regard the living agent either as a reincarnation of a formerly living person (who retains skills developed through practice in a former life) or a medium being controlled by a discarnate spirit (who retains the skill).

In response to Lund's position, it is highly relevant that dissociative phenomena are commonly linked to the sudden manifestation of novel cognitive and behavioral patterns, including unusual and impressive linguistic, artistic, and musical skills (Putnam 1989, Ross 1997). In DID cases, alters manifest, in addition to radically different personality traits, skills not previously manifested in the person and which typically require learning and practice before their initial manifestation. The linguistic, artistic, and musical skills manifested by ostensible reincarnation subjects and by trance mediums are significantly similar in kind to what is exhibited in abnormal psychology, and this fact renders their appearance in survival cases less surprising. Nor is it the case that living agents acquire such skills through LAP. There is no good reason to believe that skills in survival cases have been *transferred* or *acquired*, only that novel skills are suddenly *manifested* without any obvious antecedents. A dissociative psi hypothesis, then, attempts to explain the data of trance mediumship and cases of the reincarnation type in terms of dissociation, which in turn facilitates potent and refined psychic functioning, as well as the manifestation of latent and impressive skills (Braude 2003:101–132). Where an agent's (conscious or unconscious) needs are best met by the appearance of survival, the psychic functioning facilitated by dissociation will lead us to expect a confluence of dissociative characteristics and the appearance of survival.

In fairness to Lund, he does note that while the LAP hypothesis by itself does not account for the recalcitrant features of ostensible reincarnation cases and mediumship, it may be supplemented with a theory of "subconscious impersonation," a position that Lund believes deserves to be taken seriously (Lund 2009:173–177, 191–193). But he rejects this move for the following reasons.

(i) Subjects with alleged past-life memories exhibit a behavioral pattern of identifying themselves with a former personality,

but—as noted earlier—Lund maintains that psychically acquired information about a formerly living person does not lead us to expect that the subject would personalize the information in the form of memorial experiences.

(ii) LAP would not explain the patterns of recognizing relatives of the formerly living person, as we find in reincarnation cases.

(iii) While impersonation can affect how skills are exercised, it cannot explain how they come to be initially possessed, so neither LAP nor impersonation explain the relevant skills in the survival cases.

(iv) In both reincarnation cases and mediumship, living agents do not merely imitate the deceased; they carry on lengthy conversations "in character," but this requires harnessing an extremely powerful ESP to make the persona seem convincing to many different people who knew the formerly living person.

Since I have already noted that a subject's identification with a formerly living person is explicable in terms of the motivations of some living agent, (i) may be quickly dismissed. (ii) may also be quickly dismissed. While Lund wants to understand the recognition of people as a kind of skill and thus not the sort of thing that can be acquired by LAP, sadly he provides no supporting argument for this claim. I see no reason why LAP cannot result in the identification of persons known to the formerly living person. As for (iii), Lund is correct that neither LAP nor impersonation explains how skills are acquired, but what we know from cases of dissociative phenomena, hypnosis, child prodigies, and savants, is that high-level skills, which most people must develop through practice, are latent and emerge suddenly in some subjects. As indicated earlier, there is no reason to suppose that the skills in survival cases are *acquired* through LAP or in any other way. (iv) seems to depend on questionable assumptions about task complexity (addressed earlier in the paper) and limits on the functionality of personae that are generated as dissociated aspects of the self. On the "impersonation" side of it, the alters in DID cases have no difficulty carrying on protracted conversations with other parties, and people "in character" induced through hypnosis do the same. On the "veridicality" side of it, we need only to remember that obviously fictitious controls and communicators are most likely dissociated parts of the medium, but they are capable of delivering impressive amounts of accurate information over long periods of time, for example Mrs. Piper's "Phinuit" control and Mrs. Leonard's "Feda" control (Gauld 1982:32–44, 114–118). We should also exercise caution in the weight we afford to human testimony to the convincing nature of mediumistic impersonations, as there are profoundly subjective factors that shape such assessments.

Now the basic problem in Lund's treatment of the LAP hypothesis supplemented with "unconscious impersonation" is what he does not discuss. He neither describes nor even mentions dissociative phenomena or their link with psi and the emergence of novel cognitive and behavioral skills.[15] Similarly, he does not consider the relevance of the manifestation of impressive skills in prodigies and savants to the discussion of the skills manifested in survival cases. This failure to get beneath the psychological surface of survival cases results in treating the LAP hypothesis in its least plausible forms and thereby missing the ways in which the case for survival is challenged by LAP and our background knowledge from the field of psychology.

To summarize: The plausible motivational aspect guiding psi functioning, the characteristics of dissociative phenomena, and paradigmatic cases of rare cognitive abilities outside cases of survival each leads us to expect different aspects of the range of data adduced in support of survival. It is also highly relevant that a number of these psychological factors are intimately related to each other, for example, dissociative states are psi-conducive, and subjects experiencing stronger dissociative states manifest unusual abilities that resemble the abilities of savants and prodigies. It is difficult to resist the conclusion that we have here a way of accounting for *all* the main features of survival data, and in a somewhat unified or integrated manner, including data that prove to be recalcitrant under a very narrow construal of the LAP hypothesis. The motivational–dissociative aspects of the robust LAP hypothesis also show us that a robust LAP hypothesis leads us to expect, not just individual bits of data taken in isolation from each other but the confluence of several central features.[16] This is precisely why the debate between the survival hypothesis and LAP alternatives is unresolved.

It is worth clarifying at this point that I am not arguing that a psychologically robust LAP hypothesis is the best explanation of survival data, only that the above considerations render implausible Lund's contention that the only explanatorily adequate LAP hypothesis would have to be a super-LAP hypothesis. Whatever difficulties we might attribute to the robust LAP hypothesis, its ability to render unsurprising most, if not all, of the central features of the survival data in a way that is not transparently ad hoc and dependent on untestable assumptions is not among them.

The Predictive Power of the Survival Hypothesis

I take it that my defense of the appeal to LAP in the prior two main sections shows that a crucial component of "best explanation" is not adequately satisfied in the case of the survival hypothesis, that is, its ability to lead

us to expect phenomena *that are otherwise improbable*. (Recall that the explanatory power of a hypothesis is inversely proportional to the prior probability of the data.) In this section I turn attention to the survival hypothesis itself and explore its explanatory merits (independent of the LAP alternative), for another necessary condition of survival being the best explanation of the data is that it does some explanatory work: It must account for the data and in a way that avoids the explanatory deficiencies of its nearest explanatory competitor.

Simple Survival Hypothesis Is without Predictive Power

An essential aspect of the ostensible explanatory power of the survival hypothesis is its alleged ability to "account for" or "lead us to expect" the body of data Lund surveys in his book. Lund, of course, is not the first survivalist to contend that the survival hypothesis succeeds in this regard. Robert Almeder has strongly insisted on the predictive power of the survival hypothesis (specifically in relation to data suggestive of reincarnation) on the grounds that reincarnation has specific deductive consequences, which he believes are confirmed in part by the testimony of some people to have systematic memory of past lives.[17] However, I think the contention that survival (whether as discarnate entities or reincarnating souls) has predictive power in the required sense does not withstand logical scrutiny.

Contrary to what we might naturally suppose, simply postulating the survival of human persons does not by itself entail or make probable the data that survival is adduced to explain. This is true even if we understand a person to be what Lund contends in the first half of his book: an immaterial subject of mental states possessing various causal powers. Postulating the continuing existence of such a person after death does not have the relevant sort of predictive power since it does not lead us to expect a world in which there are any observational phenomena brought about by such persons, much less the specific observational phenomena that constitute the data Lund outlines.[18]

First, there is the general problem that postulating a surviving immaterial person does not logically entail or even make probable that such persons possess the causal powers or mental states in their postmortem state that would lead us to expect there being any kind of observational data brought about by such persons for the purpose of providing evidence of their survival. After all, there is no contradiction in supposing that (i) immaterial persons survive death but—in the absence of a functioning brain—do not exhibit any mental states or exert causal influence on our world,[19] (ii) some persons survive death as conscious beings, desire and intend to communicate, but lack the power to communicate, (iii) some persons survive death as

conscious beings, possess the power to communicate, but lack the desire and/or intention to communicate, or (iv) some persons survive death as conscious beings but lack the power, desire, and intention to communicate. There is not even a probabilistic inconsistency involved in any of these scenarios. Nor can we deduce from a simple conception of the survival of consciousness anything about the specific mode of survival, whether as a disembodied person or a reincarnated self.[20]

Second, even if we grant the survival of an immaterial conscious self with the requisite power, intentions, and knowledge to communicate with living persons in our world, it is incredibly difficult to see how any of this would lead us to expect the particular bits of observational data that Lund outlines. Lund argues that LAP explanations of NDEs cannot explain why subjects would view their environment (including their bodies) from an elevated position above the body as reported in NDEs. But Lund nowhere argues why the survival hypothesis should lead us to expect this either, and I fail to see how it can be a deductive or probabilistic consequence of postulating a surviving conscious immaterial self. Why should we expect a surviving self to have continuing perceptions of the empirical world after death, as opposed to being causally isolated from the physical world? And even if we could extrapolate the continuing perception of this world, we cannot derive any prediction about the specific location from where a surviving immaterial self will observe this world. And there is no expectation as far as I can see that such entities would experience deceased relatives in their afterlife environment.[21] So what does it actually mean to say that survival "accounts" for these particular data? It is pretty hard to say. And something similar must be said for immaterial persons becoming re-embodied again (as data from reincarnation assume), taking executive control of a medium's body to speak or write messages (as the data from trance mediumship assume), or appearing in apparitional forms (as apparitions of the dead assume).

Constructing a Robust Survival Hypothesis

Obviously the survival hypothesis needs exactly what the LAP hypothesis needs: a suitably robust range of auxiliary assumptions that will conjointly entail or render probable the data. Survivalists typically operate with auxiliary assumptions, but their fairly covert employment of such assumptions only masks what ultimately proves to be a serious liability for survival arguments. To see this, I will explicitly spell out some minimal necessary auxiliary assumptions for the survival hypothesis.

One is what I will call *discarnate interactionism*. This circumvents one wave of obvious objections. Since the survival hypothesis posits persons

as the cause of observational datum, it is a species of personal explanation. Such explanations attempt to explain some observational data as the effect of causal powers exercised by intelligent agents guided by mental states in the form of certain beliefs, desires, and intentions. Someone who argues in favor of the hypothesis that Jack stole $150 from Lisa's desk drawer is attempting to explain the disappearance of Lisa's money from a particular location within a certain range of time in terms of the actions of a particular person. This requires auxiliary hypotheses about the extent of Jack's causal powers (he had the ability to steal the money) and his having the appropriate mental states to guide the exercise of his causal powers (e.g., beliefs about the whereabouts of the money and how to remove it), and his having the desire and intention to steal the money. For the data associated with mediumship, apparitional experiences, and NDEs, the persons who are supposed to be causally responsible for the observational data are *discarnate* persons who have the requisite causal powers and mental states (in the form of beliefs, desires, and intentions) to bring about the relevant data.

Hence, we need something like the following auxiliary hypotheses:

[A1] At least some discarnate persons possess the power, desire, and intention to communicate with the living.
[A2] At least some discarnate persons possess empirical knowledge of events taking place in our world after their death.

[A1] and [A2] conjointly constitute the discarnate interactionist hypotheses. Successful communications require not only that discarnate persons initiate causal chains terminating in observational phenomena in our world, but that they are aware of what is happening in our world (either the mental states of living persons or physical events), otherwise they cannot properly have communications with a responsive element. However, since the surviving persons in view are ex hypothesi discarnate, the discarnate interactionist hypothesis entails a *discarnate psi hypothesis*.

[A3] At least some discarnate persons exhibit efficacious psychic functioning in the form of ESP and PK.

The conjunction of the simple survival hypothesis and [A1], [A2], and [A3] constitutes at least the makings of a fairly robust survival hypothesis. Now inasmuch as Lund seems to acknowledge all three of these auxiliary hypotheses (Lund 2009:102, 144), he may be seen as advocating a robust survival hypothesis, though he does not explicitly acknowledge the particular relevance of this for ascertaining predictive consequences.

Problems Facing a Robust Survival Hypothesis

Nonetheless, several serious problems remain.

First, the auxiliary assumptions I have introduced differ in a crucial way from the auxiliary assumptions adopted in scientific reasoning and that arguably also characterize the LAP hypothesis: They are not independently testable. Fundamentally, these assumptions presuppose that we already know something about what it either is like to survive death or what it would have to be like (for purely conceptual reasons), and I dare say we are not in the position to make this kind of judgment with sufficient accuracy. While discarnate interactionism posits persons with powers, desires, and intentions that approximate those found in embodied persons (and in this sense fits with our background knowledge of persons), we simply do not know whether any immaterial person who survives death will exhibit this degree of psychological continuity with their prior existence as embodied persons, much less retain or have enhanced exotic cognitive and causal powers that are at best obscurely understood in living persons in this life.

We have adopted [A1], [A2], and [A3] in this context only because without them the survival hypothesis would not have any predictive consequences. But it is all too easy to add assumptions to a hypothesis so that the new set of statements jointly entails our observational data. "There is an invisible old man who lives in my garden" does not generate much if anything in the way of observational consequences, but if I conjoin it to "invisible men attract blonde women who wear red shirts," the conjunction of the two statements leads me to expect my observational datum of having been visited by a large number of blonde women wearing red shirts. But of course the auxiliary assumption cannot be independently tested. The challenge is to find statements that are independently testable and that lead us to expect observational data once added to a central independently plausible hypothesis. When observable perturbations in the orbit of Uranus did not fit with what was predicted by Newtonian celestial mechanics, scientists postulated that there was another planet (Neptune) beyond Uranus exerting gravitational influence on Uranus and affecting its orbital path. Scientists did not postulate a novel kind of entity to account for the data, and they postulated something whose existence could be (and eventually was) confirmed by independent tests. Similarly, the robust LAP hypothesis outlined above appeals to our ordinary concept of psi and various facts about human psychology as its stock of auxiliary assumptions to account for the same kind of data that the survival hypothesis can account for only once we have adopted untestable auxiliary assumptions.

Second, the auxiliary assumptions I have introduced above are, however necessary for generating predictive consequences, nowhere nearly

sufficient. The robust survival hypothesis would not lead us to expect the particular modes of communication presupposed by the data of psychical research. At best what the robust survival hypothesis leads us to expect is that there should be *some* phenomena caused by discarnate persons (for the purposes of communicating with the living), but it does not predict with any discriminating detail what these phenomena should actually look like, or when or where they should occur. It is true, of course, that *if* we assume significant psychological continuity, this would lead us to expect that the content of communications would include details about the afterlife and the attempt to assure the living that their loved ones had survived death (and hence content should have markers of the identity of the communicators), but this is a far cry from expectations about how such messages would be delivered. If discarnate spirits can move objects, turn on televisions, turn house lights on and off, and produce apparitions, why not spell out their name with rocks in my garden or send me an email? It is possible of course that some modes of communication may be easier than others for discarnate persons or they may have personal preference for communicating in one particular way, but we do not know enough about the afterlife to make determinations about any of this in a reliable manner.

Lund criticized the LAP hypothesis for not being able to account for the fact that some living persons possess information about the deceased in the form of apparent memories, as if they had lived such lives. But this fact is certainly not explained by the survival hypothesis as Lund has developed it. Nothing in the robust survival hypothesis above leads us to expect that living, embodied persons will have past-life memories, for there is nothing in the robust survival hypothesis that entails or makes it probable that discarnate persons will ever become re-embodied again, much less carry retrievable memories with them. To get this, the survival hypothesis will have to adopt a highly specific doctrine of karma or endow psychological attachments to the physical world with a degree of causal efficacy sufficient for bringing our individual consciousness back to this world. The latter assumption endows living agents with something akin to super PK, and neither assumption would, without further specification, lead us to expect details about who would reincarnate, when the individual would reincarnate, or where and under what new bodily identity the person would reincarnate. And I am even less persuaded that these auxiliary assumptions could be tested in any reasonable way. As for the so-called "impersonation skills" exemplified in trance mediumship and the various linguistic and artistic skills exemplified in some cases of the reincarnation type, nothing in the robust survival hypothesis entails such observational consequences, though much in abnormal psychology does.

If survivalists wish to maintain that the survival hypothesis can be treated as a scientific hypothesis because it makes actual predictions, they need to do more to show this in a way that is commensurable with actual scientific reasoning. The reason why predictive power is important in the sciences is because fairly *precise* predictions can be made, in the hard sciences with quantitative and mathematical accuracy. In 1705 astronomer Edmond Halley proposed that the sun and a previously observed comet formed an approximate Newtonian system. One of the crucial tests for this hypothesis was Halley's prediction about the future time and location of the appearance of the comet. The prediction was deduced from Newtonian celestial mechanics together with descriptions of three past observations of the position of the comet going back 150 years. However, Halley's predictions were very specific ones. Given the Newtonian model and the past positions and velocities of the comet, Halley predicted the same comet, with a specific orbital path, should reappear again in December 1758, which of course it did and was named Halley's comet. Halley's prediction was not the vague prediction that some comet or other would appear between 1705 and 1758, or that the same comet would appear again at some point between 1705 and 1758. Halley predicted a comet with a specific orbital path to appear within a 30-day period 53 years in the future. Clearly, if survival is anything like a scientific hypothesis, survivalists must show that fairly specific predictions can be made from it together with a set of independently testable auxiliary assumptions.

In the final place, the criticisms leveled here prove fatal to one of Lund's earlier arguments against the LAP hypothesis. Lund argued that the LAP hypothesis cannot account for all the data unless it is adjusted to a super-LAP hypothesis, but he rejects this hypothesis on the grounds that it involves postulating a degree of psi for which we have no independent evidence. Lund's reliance on a principle of independent support is a two-edged sword in this context because the survival hypothesis is in exactly the same position as the LAP hypothesis. In its simple form, the survival hypothesis cannot account for all the data since it has little if anything in the way of predictive consequences. In a robust form it may account for the data, but only at the expense of having to conjoin itself to a large number of auxiliary assumptions that are not independently testable. Moreover, the situation is even more dire if—as Stephen Braude and I have argued elsewhere (Braude 2003, Sudduth 2009)—the psi powers needed by discarnate persons are at least equal to those needed by living agents to produce the same observational data. If we are to reject super-psi explanations because they posit a degree of psi for which we have no independent evidence, how sensible is it to maintain that survival is a superior explanation of the data when it involves

postulating persons we have no independent reason for supposing exist and attributing to them powers Lund has himself acknowledged we have no independent reason for supposing exist? The survival hypothesis is no more plausible as an explanation than the super-psi hypothesis if each suffer from exactly the same defects.

Conclusion

The focus of this paper has been David Lund's contention that postmortem survival is the best explanation of data drawn from the field of psychical research. Lund bases this claim on the ostensible explanatory virtues of the survival hypothesis and the alleged explanatory deficiencies of its nearest explanatory competitor, the living-agent psi hypothesis. By way of criticism, I have argued modestly that Lund has not presented a very strong case for supposing that survival is the best explanation of the data he surveys. In the latter part of the paper, I have argued a stronger case, namely that—given Lund's own criteria for explanatory virtue—we have good reason for supposing that the survival hypothesis is not the best explanation of the data.

My central argument for the stronger claim has involved presenting reasons for supposing that the survival hypothesis does not satisfy necessary criteria for explanatory power. Roughly stated, the best explanation must be a hypothesis that, together with independently testable auxiliary assumptions, leads us to expect observational data that are otherwise severally or jointly improbable. First, since most of the data ostensibly explained by the survival hypothesis are at least equally explicable by a carefully nuanced motivated living-agent psi hypothesis (that incorporates our knowledge of dissociative phenomena and rare cognitive gifts), the survival hypothesis attempts to account for data that are *not* otherwise improbable. Second, the explanatory virtues of the survival hypothesis can be purchased only at the cost of proclaiming explanatory success on the basis of confirmations grounded in vague predictions and the adoption of typically unstated auxiliary assumptions that cannot be independently tested.

Since Lund's positive assessment of the evidential probability of survival (as being more probable than not) depends on the survival hypothesis being the best explanation of the data, it follows that Lund has not succeeded in showing that survival has this favorable epistemic probability. Moreover, since Lund claims that survival is a rational belief because it is more probable than not, it follows that Lund has not shown that belief in survival is a rational belief. Of course, it does not follow that belief in survival is not a rational belief. Lund has just not provided a sufficiently good reason to think so. Indeed, nothing I have argued in this paper entails that a successful

evidential case for survival cannot be constructed, only that doing so will require more carefully addressing the formal problems facing attempts to make such arguments. There are plenty of data on which to reflect. What is needed is greater clarity and rigor in the process of reflection, and it may well be time for survivalists to radically rethink the logical framework in which survival arguments are developed.[22]

Notes

[1] "Epistemic probability" is the probability that some belief or proposition is true relative to some body of evidence (in the form of other beliefs or propositions). For example, we can speak of the likelihood that Jack committed the robbery given that his fingerprints were found on the safe, he had a particular motive, and he was seen there about the time of the robbery. This kind of probability should be distinguished from "factual probability" (including "physical" and "statistical" probability) that is a function of objective features of the physical world (e.g., its laws and structure). For example, the factual probability of drawing a black ball from a sealed box containing nine black balls and one white ball is .9 (almost certain), whereas its epistemic probability will vary depending on the evidence one has about the color and number of the balls in the box.

[2] Considerations from philosophy of mind and cognitive science, such as physicalist theories of mind or data allegedly showing the dependence of consciousness on a functioning brain, are frequently used to argue that the antecedent probability of survival is low. In the first part of his book, Lund attempts to refute such arguments and thereby show that the antecedent probability of survival is not low or that arguments purporting to show otherwise are logically defective.

[3] My use of "predictive power" here and elsewhere in the paper does not assume that the predictive consequences of a hypothesis were formulated prior to the time when the confirming observations were made.

[4] To clarify the dialectical structure of the arguments here, *undercutting* Lund's argument for (III)—the survival hypothesis is more probable than not—involves showing that we do not have good reasons to believe that (III) is true, whereas *rebutting* (III) involves providing good reasons for believing that (III) is false. Since (III) is a premise in Lund's argument for supposing that survival is a rational belief, it follows that, for two independent reasons, we lose our reasons for supposing that his main conclusion is true.

[5] In "forced-choice" experiments, subjects must make a selection from among a small number of known candidate targets (say, one of five cards), whereas in "free response" experiments (below in the text) subjects are

asked to describe targets without being given any potential candidates (say, simply describe the imagery they experienced during a dream state or while in the ganzfeld).

[6] Inasmuch as Lund accepts spontaneous exhibitions of LAP (Lund 2009:131–135), he is likely to be more sympathetic to a more liberal range of phenomena that are suggestive of the nature of LAP.

[7] It is sometimes argued that we cannot justify appeals to the physical phenomena associated with D. D. Home and Eusapia Palladino as evidence for LAP since Home and Palladino claimed to be communicating with discarnate spirits who might have been responsible for the phenomena. However, there are important similarities between phenomena associated with older physical mediumship and more recently documented physical phenomena in modern RSPK and sitter-group situations that are better interpreted as cases of LAP. We have good reason to believe that human agents are, individually or jointly, causing physical phenomena, even where there is ostensible contact with discarnate entities. For example, in the Bindelhof Group in the 1930s, Batcheldor's sitter-group experiments in the 1960s, and the Philip Group in the 1970s the ostensible discarnate spirits do not exhibit sufficient autonomy from the sitters themselves, as we would expect from some distinct center of self-consciousness (Pilkington 2006:202–226). These "personalities" often end up relaying messages to sitters that correspond to the ideas or wishes of the sitter-group participants. In the Philip Group sittings, the participants intentionally created the "Philip" personality by collaborating in the production of a fictional biography prior to this alleged spirit being conjured by the group. For a good summary of connections between physical mediumship, sitter-group experiments, and RSPK, see Roll (1982:212–226).

[8] As will be explained in some detail in the section **Motivated Psi Hypothesis**, the *prima facie* appeal of the LAP hypothesis is greatly strengthened when motivational factors are introduced that explain why LAP *would* tap into veridical information relating to deceased persons and in a way that presents such information as ostensibly arising from the deceased. In that case, the LAP hypothesis will actually lead us to expect that living agents will possess veridical information about other minds, including the deceased, as the result of psychic functioning among living agents. This would *significantly* increase the prior probability of the veridical features of the data and so significantly reduce the explanatory force of the survival hypothesis. We will shortly examine this more robust understanding of the LAP hypothesis.

[9] As mentioned above, the evidence for living-agent PK drawn from spontaneous cases not only involves physical phenomena characteristic of the

great physical mediums of the nineteenth century, but the sitter-group experiments mentioned above involved the production of physical phenomena that give the appearance of having been produced by discarnate persons. This is significant evidence for the *externalizing* of LAP effects, that is, psi effects taking a form in which they have the appearance of having been produced by an autonomous agent.

[10] Gauld (1982:131–136) discusses the performances of E. Osty as illustrative of high-level LAP, including the apparent derivation of veridical information from multiple sources.

[11] Survivalists have a tendency to exaggerate what is claimed on behalf of appeals to LAP as an explanatory competitor, sometimes maintaining that critical appraisals of survival evidence involve attributing superior explanatory power to LAP. For example, Ian Stevenson committed this mistake in his assessment of Braude's defense of "super-psi" (Stevenson 1992:145). See also Braude's response (Braude 1992:151).

[12] Technically stated, the background knowledge will include the disjunction of all hypotheses that lead us to expect our data.

[13] "Unconscious" psi effects are well-established in experimental psi research. See Stanford (1977).

[14] Stephen Braude provides a fairly detailed development of these possibilities in connection with particular cases (Braude 2003, especially Chapter 6).

[15] One exception: The term *dissociation* appears in a lengthy endnote (Lund 2009:220) in which Lund discusses matters related to the philosophy of mind covered in the first half of his book.

[16] This is an important point, as the survivalist might contend that it is not enough to show that for each essential datum (d), there is some hypothesis (h) that renders d unsurprising. For example, h1 might render d1 unsurprising, h2 might render d2 unsurprising, etc. It does not follow that a single event in which d1 and d2 both occur together is unsurprising. A particular weather pattern might render a particular meteorological phenomenon probable on a given day of the week, and another weather pattern might render another meteorological phenomenon likely on another day of the week. This does not tell us that it would be unsurprising to witness both meteorological phenomena together on any given day of the week. For this we would need a hypothesis that would lead us to expect the joint occurrence of otherwise diverse or independently occurring phenomena.

[17] See Almeder (1996).

[18] Prominent survivalists have insisted that a necessary condition for a good explanation of physical phenomena is that it must have "some test impli-

cations by way of providing deductively specific predictions of sensory experience" (Almeder 1996:504).

[19] This point is sometimes missed because survivalists sometimes assume that a surviving soul must exhibit conscious states, but this is not true, at least not a conceptual truth. The functioning of a soul, which results in conscious episodes, might depend on a functioning brain (even if its existence does not) in much the same way that a lightbulb depends on electrical current to give off light (even if its existence does not). See Swinburne (1986:176, 310).

[20] Almeder (1996:497–498) is thus incorrect when he says that we *know* antecedently what would count as evidence for reincarnation because of our intuitions about personal identity. We have no more reason to suppose that a reincarnating soul would have memories of its past life than lack these, unless we assume a fairly contentious thesis about "personal identity," namely that it consists in the continuity of memory. Moreover, as a technical point, "past-life memories" are not observational data. The observational data would be the testimony people provide that they have such memories. But in that case, it is not possible to directly confirm the alleged prediction.

[21] H. H. Price (1953) presented an account of surviving immaterial persons in which they do not have continuing perceptions of this world, but exist in an image world constructed from their pre-mortem memories and desires. On Price's model, telepathic interaction (in the form of projected telepathic apparitions) between discarnate minds *could* provide a means for discarnate persons to communicate with and experience other deceased discarnate persons in the afterlife. But this is merely one conceivable theoretical possibility from among a number of others.

[22] I would like to thank Stephen Braude for his comments on an earlier draft of this paper.

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OBITUARY

Jack Houck (1939–2013)

With great regret the SSE recognizes the passing of one of its Dinsdale Award recipients, Jack Houck. Born George B. Houck, Jack, as he was better known to his many friends, was raised in Mechanicsburg, Pennsylvania. He attended the University of Michigan where, in 1961, he received a Masters of Aeronautical and Astronautical Engineering degree. For more than four decades he was a researcher, theoretician, experimentalist, engineer, and analyst of anomalous phenomena. His most significant contributions to the field came with his direct contact with more than 20,000 people and with his providing them a firsthand, demonstrable experience of psychokinesis.

In 1981 Jack Houck developed the concept and protocols for psychokinesis metal bending (PKMB) parties. Rather than simple demonstrations by a talented individual who professed special skills, Jack proved that the process could be taught to anyone who was prepared to attempt it. For many participants, the PKMB experience that he provided proved to be a life-changing event. Importantly, his interests and explorations took him into many other controversial topics such as remote viewing, firewalking, spontaneous germination of seeds, EEG biofeedback, and healing with human energy.

Like many other members of the SSE, Jack delicately, and successfully, bridged the dichotomous fields of science and psi research. From 1961 until 2003 he worked as an aeronautical engineer with the Douglas Aircraft Company, which became McDonnell Douglas and then Boeing. Jack held many positions of increasing responsibility. As a rocket scientist he initially was involved in missile defense studies, and in 1972 he was selected as one of the authors of the Strategic Arms Limitation Treaty (SALT). For more than two decades Jack simultaneously managed a number of extremely sensitive defense and intelligence programs. During that period he formed the Advanced Research group that engaged in evaluating data of foreign rockets and associated weapons systems. Later he became involved as a risk management expert on large, classified space systems. In his final position he performed as the risk manager for all of the company satellite systems.

While most of his efforts in researching unexplained events were conducted outside his traditional work assignments, there were times when

the circumstances overlapped. In preparation for development of the SALT agreement with the Soviet Union, he became familiar with the possibilities afforded by remote viewing and he later began conducting experiments in the field. Awareness of his interests reached the top of the organization and in 1980 James McDonnell, then chairman of McDonnell Douglas, asked him to run a remote viewing experiment. In response Jack conducted fully judged, double-blind experiments using latitude and longitude coordinates for targets all over the world. It was those experiments that led him to develop a conceptual model of paranormal phenomena.

An inveterate researcher, Jack meticulously collected information at all of his PKMB events and amassed an enormous collection of data on macro-PK observations. What Jack uniquely and thoroughly documented was the fact that the human mind can and does impact physical matter. He worked with established material scientists to conduct analysis and documentation of microscopic changes that occur to the materials during the PKMB. He subjected a wide range of materials to the process and observed and reported the differences. Concerned with practical application of the extended human capabilities he observed, Jack also explored their use in healing illnesses.

It was the legendary PK parties that brought Jack and me together. Attending one of his early events in Alexandria, Virginia, with my boss, Major General Bert Stubblebine, we observed a renowned psychic, Anne Gehman, have a fork drop 90 degrees with NO physical force applied. That was a pivotal point for our relationship with Jack. His formula could be replicated and was used to educate and influence military intelligence officers for years to come.

Even when faced with severe personal physical challenges, Jack continued to carry forward his efforts in providing others with personal experiences in anomalous phenomena. Through scientific publications and a personal website Jack selflessly made his experiments and analysis available to other researchers and the public.

The work performed by Jack in development of theory, experimentation, and analysis covering a wide range of extraordinary observations was exemplary. What set Jack apart was his willingness to take great personal risks and transform theory into personal experiences that could be demonstrated by anyone willing to participate. Again, for many these were life-changing experiences. His positive influence on the field of consciousness studies has been immense; but more importantly he was able to attract the attention of average people in ways that no other researcher has done.

Among those surviving Jack are his wife Dr. Aleda "Jean" Houck, daughter Linda McGregor, and son David Houck. His works continue on and can be viewed at www.jackhouck.com

John Alexander

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OBITUARY

Ted Rockwell (1922–2013)

Theodore "Ted" Rockwell was an amazing man, great friend, and a Full Member of the SSE. While best known to the world as a nuclear energy pioneer, he was intensely interested in spiritual matters and psychic research. For decades he successfully straddled both worlds and loved to "comfort the afflicted, and afflict the comfortable." The latter meant he frequently raised questions that often tended to bother conventional scientists.

For more than 65 years he worked in nuclear technology, and was a founding officer of the nuclear engineering firm MPR Associates, Inc. (he was the R), and of Radiation, Science, and Health, Inc. During World War II, Ted worked at the Manhattan atomic bomb project in Oak Ridge, Tennessee. Continuing with the U.S. Navy in a civilian capacity after World War II, from 1949 to 1964 he worked at Naval Reactors Headquarters. The last 10 years of that assignment Ted served as Technical Director of Admiral Rickover's program to build the nuclear Navy, including the design, building, and commissioning of the *USS Nautilus*, the world's first nuclear-powered submarine. He also served as Director of President Eisenhower's Atoms for Peace Program. There he was selected as one of the 13 official U.S. presentation volumes at the 1958 Atoms for Peace Conference in Geneva. Ted was instrumental in declassifying much of the relevant nuclear technology, and building the world's first commercial atomic power station at Shippingport, Pennsylvania. Those efforts led to the World Nuclear Association Award for Distinguished Contribution to the Peaceful Worldwide Use of Nuclear Energy.

A member of the National Academy of Engineering, Ted was a Fellow of the American Nuclear Society and recipient of its first Lifetime Contribution Award, now known as the *Rockwell Award*. Nationally he received Distinguished Service Medals from both the U.S. Navy and the

U.S. Atomic Energy Commission. Ted was the only non-medical member of the Advisory Group on the National Artificial Heart Program (1966) and a member of the Advisory Council, Princeton University Department of Chemical Engineering (1966–1972). From 1965 to 1968, he was a Research Associate with the Johns Hopkins School of Advanced International Studies (in connection with nuclear proliferation research). He was Chairman of the Atomic Industrial Forum's Reactor Safety Task Force (1966–1972) and Consultant to the Joint Congressional Committee on Atomic Energy (1967). Sigma Xi selected him as the first Distinguished Lecturer sponsored by the National Academy of Engineering.

Ted held several patents, including one listed in "a selection of [27] landmark US atomic energy patents from all the patents issued to date." His extensive writing includes several books and many technical papers. Many of them have been translated and published in foreign languages including German, Dutch, Russian, Chinese, Japanese, and Korean. Demonstrating versatility, he also wrote three one-act plays that were professionally produced as a staged reading at Washington's Source Theater.

While maintaining a high profile in the field of nuclear energy, Ted actively pursued his interests in spirituality and psi phenomena. Ted introduced me to another pioneer, Cleve Backster, leading to the replication of Backster's work in primary perception. Ted also attended several of the key psychokinesis metal bending (PKMB) parties, lending his credibility in our attempt to influence senior officials as to the reality of psi phenomena. Ted was also instrumental in setting up an expedition including our families, as well as C. B. "Scott" Jones, to experiment with dolphins in their natural habitat in Bahamian waters. These experiments included attempted telepathic influence on the behavior of the pods we encountered and they appeared to have positive results. The self-imposed protocols dictated that we refrain from physical contact although allowed the wild dolphins to initiate interactions at extremely close proximity (less than an inch).

Always inquisitive, Ted participated in supporting several consciousness exploration organizations including the U.S. Psychotronics Association which studies the science of mind–body–environment relationships, an interdisciplinary science concerned with the interactions of matter, energy, and consciousness. The power of prayer was of interest to him and he followed closely the experiments of Spindrift Research which explores "consciousness and prayer with scientific methods." He also served on the Science Advisory Board of the National Institute for Discovery Science. Privately he amassed an extensive collection of psi-related publications which he graciously donated to the Bigelow Collection in Las Vegas, Nevada.

John Alexander

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BOOK REVIEW

Quirks of the Quantum Mind by Robert G. Jahn and Brenda J. Dunne. ICRL Press, 2012. 274 pp. $19.95. ISBN 978-1936033034.

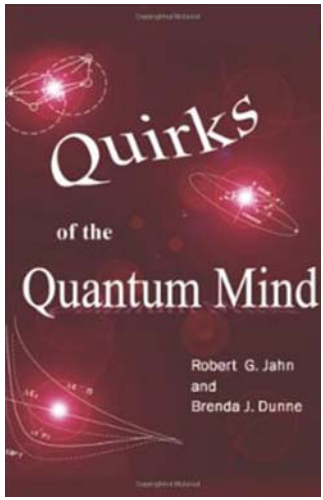
Quirks of the Quantum Mind accomplishes something that no other book to my knowledge has done. Using extensive data from the authors' multiple decades of unique field research, *Quirks* synthesizes a "metaphor" which serves as a model for understanding consciousness-related anomalies. The book coherently presents a parallel between the principles of quantum mechanics and the psychic behavior observed in the research done at the Princeton Engineering Anomalies Research Laboratory.

The authors do not presume to create a "theory" connecting quantum mechanics with consciousness. The authors steadfastly stick to the notion that quantum mechanics may serve as a *metaphor* for psychic phenomena. Yet they also make clear that the formalism of quantum mechanics itself is ultimately a reflection of the way we think. They point out that

> common concepts of physical theories, such as mass, momentum and energy, electric charge and magnetic field, the quantum and the wave function, and even distance and time, are not more than useful organizing strategies consciousness has developed [for organizing its world].

This motivates the metaphor and shows us that we should not treat the quantum formalism as more fundamental than the psychic formalism. This approach successfully opens the reader's mind to a state of curiosity, without actually violating the conservative physicist's boundary between physics and consciousness.

The consciousness/quantum mechanics metaphor suggested is consistent with the properties of the vast collection of data collected by the authors (as well as experiments performed by others). In the metaphor, there exist "consciousness eigenfunctions" which are not "constructed by some grand superposition" of the individual atoms in a measuring device. Rather, they "represent its aesthetic, functional, or even anthropomorphic character as perceived by the operator . . . " The authors are able to account for the factors that seem to affect psi experiments. Examples include the importance of not only the objective properties of a measuring device but also the *perception of the experimenter* of the *qualities* of the measuring device, as well as the subtle qualities of attention necessary for positive results in psi experiments.



The suggestions in this book, though stated in a non-threatening manner, would be considered controversial to mainstream interpretations of quantum theory. There are aspects of the suggested metaphor that go farther than may seem reasonable in drawing parallels between quantum theory and consciousness. For instance, in addition to creating a consciousness version of the wave function, the authors go completely through the formalism of quantum mechanics and derive metaphorical meanings for every quantity in the formalism. An example of this is the interpretation that, with regard to the "consciousness wave function," the quantum numbers for "spin" and "orbital angular momentum" correspond to the degree of cognitive vs. emotional qualities in an individual.

However, in order to pave a way forward through the briar patch of current theoretical quantum theory, we need to be open-minded to courageous new ideas. Given the laborious effort made by the authors to gather relevant data on this subject, it is reasonable (if not a matter of obligation) that they would provide such a framework within which the data are consistent. Although they suggest that there should be ways to gather predictions and verify the model, the authors are careful not to make claims as to the analytical correctness of their metaphor. They focus rather on the suggestive nature of the parallels between quantum mechanics and psychic experiments. Their suggestion is that it may be possible to create a model for psychic behavior that takes significant guidance from the quantum formalism.

Quirks is impressive in its scope, translating many basic laws of physics into "consciousness space." Yet it remains unpretentious and humble in its approach. It appears to have been written for a technical audience, in that the language used is often formal and sometimes cumbersome for the casual reader.

In the effort to make sense of the growing body of solid psychic research, *Quirks of the Quantum Mind* has made a significant and meaningful contribution.

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BOOK REVIEW

The Discovery of the Sasquatch: Reconciling Culture, History, and Science in the Discovery Process by John A. Bindernagel. Courtenay, BC, Canada: Beachcomber Books (www. beachcomber.com), 2010. 325 pp. $49, paper. ISBN 978-0968288719.

This book's subtitle acknowledges the complexity of the task that anomalistics faces. Important aspects of the evidence come from times past, which makes it necessary to consider the reliability of the sources and how to interpret them *in light of the cultural environment* in the pertinent eras. The present and past states of science are obviously important, including why science has chosen not to look into what seems to us worth looking into; and that again calls for an understanding of how science affects and is affected by culture, in the present and in the past. All these things are discussed in relation to Sasquatch, in the body of the book and also in the substantial and insightful Foreword by Leila Hadj-Chikh.

The general phenomenon of resistance to genuine novelty is often remarked, but this book goes much further than the generality by rooting out quite specific reasons for mainstream science's resistance to the existence of Sasquatch; and by pointing to the unspoken assumptions that underlie those reasons. Perhaps the central issue is that popular culture takes Sasquatch to be a primitive relative of humans, *Homo*, whereas Bindernagel identifies Sasquatch as one of the great apes.

Critics will often claim that if Sasquatches existed, "we"—our science and our conventional wisdom—would have known it by now. Surely a hunter would have shot one, for instance. Hadj-Chikh points out, however, that the human-like appearance attributed to Sasquatch would make hunters hesitant to shoot one; and a shot Sasquatch would not necessarily die at once or in the vicinity; and moreover a hunter might well wish to hide the fact of an inadvertent shooting. Not many people approved of Grover Krantz's resolve to shoot a specimen if given the chance. Similarly with the critics' rhetorical query, why haven't "we" found traces: There are indeed reports of possible traces in addition to the footprints and tracks that gave Sasquatch the common name of Bigfoot.

Hadj-Chikh reminds us that Europe's Paleolithic cave paintings were at first taken to be hoaxes because the contemporary mainstream view of human evolution and history did not allow for such sophisticated art so long ago.

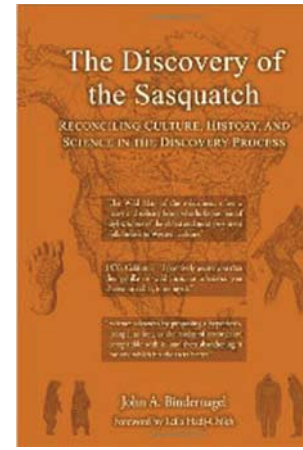
Just as with Nessies (the Loch Ness Monsters), there are seemingly excellent reasons why Sasquatch could not exist: It tends to be nocturnal whereas apes are diurnal; it is reported from temperate zones whereas the great apes live in the tropics and sub-tropics; there are no pertinent fossils in Sasquatch's reported habitats. But Hadj-Chikh reminds us here how very improbable we *Homo sapiens* are, deviating so greatly in so many respects from our hominoid cousins. If we, why not Sasquatch?

In Chapter 1, Bindernagel addresses the notion of Sasquatch as a cultural phenomenon based on legends and myths of wild men as well as the deliberate perpetration of modern hoaxes. The first point has not carried any weight with me after Dmitri Bayanov pointed out that if such creatures exist, there would certainly have grown up a wealth of folktales and the like about them. On the second point, hoaxes are in a sense irrelevant to the actual evidence: Hoaxers will do their thing quite independently of the existence of Sasquatch.

Chapter 2 surveys some literature about general issues in questioning established knowledge. Then Bindernagel turns to the evidence for Sasquatch: eyewitness reports (Chapter 3), historical reports (Chapter 4), and recent accounts (Chapter 5)—which raise the additional complication that wide publicity about Bigfoot is likely to bias what people now believe they have seen. Also addressed here is the relatively large number of reports from mid-western and eastern regions, which Bindernagel—following John Green's earlier work—suggests should not be automatically dismissed as too unlikely.

Chapter 6 takes up the issue of tracks, in considerable detail and with comparisons against human and ape anatomy. While critics sometimes assert that various casts of footprints show too much variation to be credible, Bindernagel cites Colin Groves: 19th-century classification of great apes had to deal with the fact of anatomical variability almost as great as in humans. Also dealt with here is other physical evidence: signs of foraging for ground squirrels; twisted saplings; possible beds; and possible scat. A very general basis for pooh-poohing cryptic beings is that the conventional wisdom actually knows very little about the enormous range of the natural behavior of animals; for example, that Baird's tapirs prefer to defecate in water, so a lack of obvious authentic Sasquatch scat is not necessarily decisively negative evidence.

Of course it is not the evidence itself but its interpretation that is crucial. In Chapters 7 and 8, Bindernagel points out that a great variety of reported Sasquatch characteristics—anatomical and behavioral—do not appear at all odd if they are compared with those of gorillas rather than humans. For me this is the single most persuasive point: The popular view of Bigfoot



as a "wildman," a close cousin of humans, is quite misguided; almost everything about Sasquatch appearance and behavior is quite plausible for a great ape. I think this adds considerable plausibility to the eyewitness reports: Despite the conventional wisdom's prejudice that Bigfoot is a sort of humanoid, reported appearance and behavior have nevertheless been often described in terms that might well describe an ape—the eyewitnesses are *not* describing what they expected to see.

In Chapter 9, Bindernagel considers how the evidence supports or does not support not only the hypothesis that Sasquatch is a real great ape but also the hypothesis that it is a cultural phenomenon inspired by legend and sullied by hoaxing. Interesting is that after the discovery of gorillas, reports of Sasquatches began to reference gorillas as well as "wildmen." Fascinating is that some Sasquatch reports, for example meat-eating or projectile-throwing, were found only later to have counterparts in ape behavior.

Chapter 10 is about the importance of theoretical approaches in combination with empirical ones, followed (Chapter 11) in natural fashion by an analysis of the discovery of Sasquatch as a process, a complex one, and not a single event, as Thomas Kuhn suggested is the case with any really novel discovery. Chapter 12 again follows naturally with the citing of Barber's classic paper, "Resistance by scientists to scientific discovery" (Barber 1961), and notes of such resistance in the discovery of the various apes as well as other phenomena like that of the okapi and several medical conditions: geometric patterns associated with migraines, Tourette's syndrome, and alien-limb syndrome.

Then there come detailed discussions of the inadequacy of four common conventional explainings-away of Sasquatch claims: misidentified bears (Chapter 13), hallucinations or imagined entities (Chapter 14), myth (Chapter 15), and hoax (Chapter 16). Philosophy of science regards parsimonious explanations as preferable to others, and Bindernagel points out that the Sasquatch hypothesis is far more parsimonious than the others, albeit perhaps superficially less plausible.

Chapters 17–19 look into reasons why discovery of the Sasquatch has been hindered. It identifies specific points in line with the general analyses of delayed discovery ventured long ago by Gunther Stent (Stent 1972) and discussed more recently in Ernest Hook's edited volume (Hook 2002). That

mainstream science has not taken an interest amounts to a vicious circle: The investigation is left largely to amateurs, thereby lacks the discipline of professional approaches, thereby enhances the mainstream's tendency to write the whole thing off, especially since the media like to publicize the hoaxes and the antics of the craziest extremes among Bigfoot hunters. Further, labeling Sasquatch-seeking as "cryptozoology" with the implicit aim of gaining scientific status may be counterproductive given that mainstream pundits label cryptozoology as a whole as pseudo-science. The inability to connect Sasquatch convincingly to known species is an obvious barrier to acceptance, just as centuries of empirical experience left Western explorers unprepared for the fact of Australian black swans.

Perhaps above all, the mainstream's disdain means that most professional scientists in the relevant fields are simply unaware of the evidence. Thus they readily presume that, because the evidence for Himalayan yetis is slim to non-existent, the same must be true for any similar creature elsewhere. This illustrates a very general point in anomalistics: No matter how satisfactory general principles for studying anomalies may be, ultimately each specific investigation must succeed or not on the basis of idiosyncratic efforts (Bauer 2013). The Bigfoot hunters who claim yetis and almas and other reported "wildmen" as adding to the plausibility of Sasquatch are not actually making it more plausible but rather, in the eyes of many, significantly *less* plausible.

That anomalistics is inevitably multidisciplinary brings in difficulties categorized by Hook (Hook 2002) as "interdisciplinary dissonance," research in one disciplinary approach being inhibited by clashes with what is accepted in some other field. Once again we are reminded of important discoveries that had been long delayed: the prevention of scurvy by citrus fruit, and the prevention of fatal infections at birth advocated by Semmelweis. And in the Epilogue we are reminded that chemist–philosopher Michael Polanyi extrapolated his personal experiences to describe issues of paradigm shift in a similar way as Thomas Kuhn did a few years later.

This book is eminently worth the attention of all anomalists, for many lessons pertinent to all investigations of improbable claims as well as for a convincing demonstration that Sasquatch, interpreted as a great ape rather than a humanoid "wildman," is far from implausible.

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BOOK REVIEW

Demystifying the Out-of-Body Experience: A Practical Manual for Exploration and Personal Evolution by Luis Minero. Llewellyn Publications, 2012. 403 pp. $19.99. ISBN 978-0738730790.

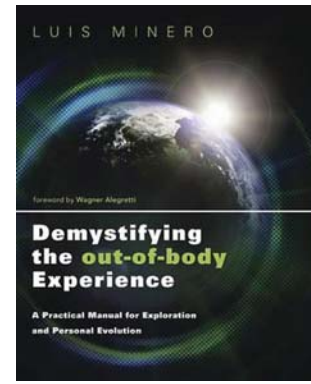
I was very pleased to learn that Luis Minero had written a book on the out-of-body experience (OBE). I bought it sight unseen, and I was not disappointed. It was back in 2002 that I first came across the author's name in a correspondence to the *Journal of Conscientiology* (Minero 2002a), the peer-reviewed journal of the International Academy of Consciousness (IAC).

In that letter, Minero analyzed with great accuracy, clarity, and thoroughness, the so-called "mind-split" hypothesis, proposed by author Robert Bruce to explain some of the puzzling factors of the OBE phenomenon, such as memory loss and dual consciousness (Bruce 1999). I still remember how impressed I was when I read Minero's step-by-step refutation of Bruce's hypothesis, not only for the care and lucidity of his analysis, but also for his natural, didactical style and the balance with which he was able to strongly criticize weak points of some of the ideas presented, and at the same time give full credit to other innovative aspects of Bruce's work.

I rediscovered these same intellectual qualities in other writings of Minero, such as his suggestive essay on *lucidocracy* (Minero 2002b), a political proposal contemplating the possibility of a government system based on lucidity, with the main goal of facilitating the fulfillment of one's potential or life mission (existential program).

I would start by saying that the book's title may possibly seduce the hasty reader in error. Indeed, the *demystification* in question is not the usual one, consisting in reducing the entire OBE complex of phenomena to a mere hallucination produced by the subject's physical brain, when his or her sensory inputs are altered in some way. Minero, quite to the contrary, considers OBEs as experiences describing real projections of the human consciousness through objective subtle bodies (vehicles of manifestations), which can exist independently of the physical body.

The book's demystification is, therefore, of a very different kind: It is about those more mystic-like and folkloristic aspects that have been historically associated with the OBE phenomenon, mostly based on



immature, emotional, or superstitious thinking, and which have little to do with a more mature understanding of this fundamental topic.

A very important point to be emphasized: One can use a sound, scientific approach to the OBEs, without necessarily reducing these experiences to a mere phenomenon of autoscopy, i.e. the experiences of seeing one's physical body from an out-of-body perspective.

Minero stresses in many passages and chapters of his manual that the OBE is much more than this. OBEs are described as complex, highly articulated experiences, involving para-matter of a non-ordinary kind, obeying para-physical laws, different from the physical laws obeyed by the ordinary physical matter today studied by physicists.

OBEs are not characterized as the mere exteriorization of one's awareness to one's bedroom: To Minero, OBEs are about exploring physical and extraphysical environments, meeting other (more or less evolved) extraphysical (disincarnated) consciousnesses, providing assistance to intraphysical (incarnated) and extraphysical (disincarnated) beings, with the possibility of working with teams of more advanced and organized consciousnesses, which are referred to as extraphysical helpers.

Demystifying posits that OBEs are about understanding the process of death from a broader point of view, i.e. from a viewpoint that considers our biological vehicle as just one among different vehicles we can use to manifest, intelligently and self-consciously, in different existential dimensions.

Minero describes how individuals have used the OBE as inspiration for re-examining life's purpose, one's potential, or personal existential directives or priorities. Lucid projectors, those who experience OBEs frequently, often describe observing extraphysical individuals planning what they want to do when they acquire a physical "suit." That is, projectors may observe the "intermission," as Dr. Jim Tucker calls it: the period between two physical incarnations.

Minero suggests that the phenomenon of the OBE acquires all its meaning and potentiality when its theoretical study and practical experimentation is motivated by a genuine desire for achieving greater integral maturity (holomaturity), i.e. a condition of inner development that is not limited to the attributes developed in the ordinary physical world, or even just this lifetime.

The volume, which is not only an instruction manual for self-exploration of the OBE phenomenon, also acts as a guide for stimulating personal evolution. It is organized into seven well-thought-out chapters. The first one introduces the basic concepts of the proposed scientific disciplines of *projectiology* and *conscientiology*, as coined by Waldo Vieira, M.D. (Vieira 1994, 2002). The second one deals mainly with the subject of subtle energy (bioenergy, orgone, chi, biofield), and the importance of its mastery to obtain sufficiently controlled, frequent, and lucid OBEs.

Chapter 3 introduces a multi-vehicular (multiple-body, holosomatic) structure of human consciousnesses and the characteristics of many extraphysical environments described during projections. Consciousness, in this context, is seen not as a property of self, not as the physical body or any other perceived "body," but as a synonym for the self. In this work, rather than having or experiencing consciousness (awareness), one *is* a consciousness: a novel use of the word by Minero and his colleagues.

Chapter 4 describes the different stages one may go through during an OBE. This is the chapter where the reader will find, very scrupulously described and logically organized, many different techniques one can use to achieve a lucid OBE. Chapter 5 considers many possible interactions and forms of communications projectors, including the possibility of simultaneous or joint projections, whereby two or more individuals describe meeting while they have OBEs at the same time.

Chapter 6 includes suggestive neologisms, such as *holomaturity*, *assistentiality*, *evolutionary intelligence*, and *cosmoethics*, and finally, in Chapter 7, the author investigates compelling hypotheses, always considering them from the OBE perspective: *existential program* (life mission), *existential seriality* (reincarnation, death-rebirth cycle), *intermissive courses* (the training that a consciousness possibly takes to prepare itself for rebirth), and many others as well.

The extreme care with which the book is written can be seen in the details. The volume is equipped with a very useful Glossary, with the explanation of the most important neologisms used. At the end of each chapter, there is a practical summary of the key points that have been developed, and throughout the book one can find a number of text boxes, identifiable by specific icons, providing complementary information to the text, in the form of definitions, recommendations, firsthand OBEs, challenging questions, etc. Last but not least, the book is very carefully illustrated, with professional drawings that considerably facilitate the understanding of the topics covered.

To recapitulate, this is a professionally written text. Per its aim, it is an

introductory textbook, which thanks to its pedagogical style will appeal to a wide audience. It is also, I believe, a book that will prove to be instrumental to all those scientists interested in the study of consciousness from an integral perspective, and who sincerely wish to move from the level of pure speculation, or research of third-person accounts, to that of lucid self-experimentation (first- and second-person perspective research).

Indeed, as Minero rightly emphasizes in his Preface, direct experiences should be the first step toward a more mature understanding and study of OBEs and allied phenomena. And his volume is certainly a precious tool that can be used by scholars of all kinds to take a first step in that direction. This will help create a more ample, consensual basis for the discussion of the reality of the OBE, considering also that there is a small, but growing number of scholars who take seriously the importance of firsthand experience when the subject of the study is . . . oneself.

Now, while it is true that today's predominant scientific approach remains quite cold regarding disciplined self-study and self-experimentation of consciousness, it is also true that the current scientific debate is increasingly based on experimental evidence, so that more and more researchers are starting to become more open to the possibility of engaging in first-person investigation of the hypothesis of the multi-dimensional nature of consciousness. When these researchers look for a reference manual, written in a sincere, and yet non-reductionist style, they will find in Minero's book a valuable companion. Surely, from now on, it will be the book I will recommend to those who ask me for a highly readable and professionally written reference on the subject.

Let me conclude by observing that, in the same way a beginning student of, say, quantum mechanics, has to undertake a long journey of study to obtain firsthand understanding, including acquiring all the necessary preliminary knowledge in physics and advanced mathematics, so too with regard to the possibility of reaching a direct understanding of the OBE phenomenon, which also requires a considerable amount of personal investment to develop those preliminary abilities described in this work (for example, the control of bioenergy). Without this discipline, it remains quite difficult to achieve sufficiently lucid, meaningful, frequent, recalled OBEs. The more individuals who can reach this degree of mastery, the more experiments can be repeated and reproduced.

To quote Minero:

> In this current world of fast, easy solutions and short-term fixes, there are still no substitutes for personal effort, will, perseverance, and patience.

And the subjective and intersubjective investigation of the full multidimensional content of the OBE phenomenon posited in this work is no exception.

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BOOK REVIEW

Mind and Cosmos: Why the Neo-Darwinian Conception of Nature Is Almost Certainly False by Thomas Nagel. Oxford University Press, 2012. 130 pp. $24.95 (hardcover). ISBN 978-0199919758.

The subtitle of this surprisingly brief volume by Thomas Nagel presages something more, and something less, than what at a glance it may seem to promise. In such a confined space as a mere 128 pages, coming from such a noted philosopher, one might expect that Nagel has consolidated and refined a highly focused, decisive argument against the prevalent materialist–reductionist account of mind and its place in nature. Those of a materialist view will not likely be concerned, since philosophical objections seldom seem to have much effect on that paradigm. On the other hand, those who feel deeply that something is amiss in the reductionist account might be a bit disturbed when they realize the import of the word *almost* in the subtitle. Is Nagel hedging his bets?

That puzzling "almost" is easier to understand, however, when we reach the concluding summary, which might better have been placed right up front on page 1:

> Philosophy has to proceed comparatively. The best we can do is to develop the rival alternative conceptions in each important domain as fully and carefully as possible, depending on our antecedent sympathies, and see how they measure up. That is a more credible form of progress than decisive proof or refutation. (p. 127)

And this is what Nagel sets out to do most brilliantly. But there is another phrase in this philosophically subtle paragraph that belies any impression that Nagel himself is uncertain about the topic. That is his reference to "our antecedent sympathies." Nagel's own sympathies are clearly present throughout and are firmly negative when it comes to materialistic reductionism. But he is not dogmatic about it, and this produces quite another kind of argument. Nagel argues most compellingly against the materialist view by first delving deeply into every nook and cranny of the multiple possible theories about mind and its relation to the cosmos, then inviting the reader to understand, and hopefully share, his own profoundly personal and philosophically careful conviction that reductionist theories lead to a dead end.

In his chapter on values, for example, after having admitted that some options he has been detailing which are contrary to the materialistic view are "offered merely as possibilities and without positive conviction," he explains what he *is* convinced of.

> What I am convinced of is the negative claim that, in order to understand our questions and judgments about values and reasons realistically, we must reject the idea that they result from the operation of faculties that have been formed from scratch by chance plus natural selection, or that are incidental side effects of natural selection, or are products of genetic drift. (p. 125)

In other words, what Nagel *himself* is convinced of after exhaustive and informed study of the various options, and after consideration especially of what it is to be a human being living in a world of "values and reasons," as well as consciousness, selfhood, and meaning, is that (exactly as the subtitle says) the reductionist conception of nature is "almost certainly false." It is almost certainly false, because dumping all those important aspects of what it is to be human into a trash heap is not only unacceptable, it is a profound misconstrual of the natural world.

What is really entailed by Nagel's "almost" is that while the failure of the reductionistic paradigm seems clear, the success of the most likely alternative theory is, in our current state of knowledge, still beyond reach. That does not mean, however, that there may not be an alternative theory offering more promise of success than does the prevailing paradigm. This paradigm is the orthodox view, and as Nagel points out "any resistance to it is regarded as not only scientifically but politically incorrect" (p. 5). In opposing this view, Nagel uses an end-run strategy. He mounts a hypothetical argument, the "argument from the failure of psychophysical reductionism," which means working from the premise that such reductionism *is* false, and seeing what must result from that assumption (p. 15).

The assumption of the argument is not arbitrary. Nagel believes that there are empirical reasons to adopt a skeptical view with respect to the reductionist program. He is working from a basis of informed skepticism. And as he puts it, that skepticism has to be rather strong.

> For a long time I have found the materialist account . . . hard to believe, including the standard version of how the evolutionary process works. The more details we learn about the chemical basis of life and the intricacy of the genetic code, the more unbelievable the standard historical account becomes. . . . It seems to me that, as it is usually presented, the current orthodoxy about the cosmic order is the product of governing assumptions that are unsupported, and that it flies in the face of common sense. (p. 5)

This not at all indeterminate position sets the overall tenor of the book. On the one hand, Nagel delves into the empirical reasons for adopting a skeptical view, throwing in a couple of powerful logical reasons as well. On the other hand, he explores possible alternatives to the materialist view and which of the alternatives, in his opinion, is the most probable one. At times his argument is rather involved. I find that on a first reading the overall organization of the book is not immediately clear, at least not until the closing chapters. A second and even a third reading, however, reveal jewels of careful thought that in this reader's opinion are not only rewarding but are a significant contribution to the discussion.

One of the primary reasons against the reductionist view which Nagel cites repeatedly is that the application of the criterion of "fitness" to such experiential factors as consciousness, cognition, and value simply does not work. These three factors represent, in fact, the division of chapters in the book. After an Introduction and overall survey of issues and alternatives, Chapters 3, 4, and 5 discuss those topics in that order. In this limited space I do not attempt to represent or evaluate the multitude of arguments literally crammed into the book. All are challenging and all are carefully set forth. I will however summarize some of that material and then turn to what I feel is the most important contribution of the book.

When Nagel refers to consciousness as an acknowledged feature of the world, he tends to use the phrases "subjective appearances" or "subjective experience" (pp. 35–36). He does not intend "subjective" here to imply a Berkeleyan subjective idealism, the view that only appearances are experienced rather than objective reality. In clarification, he provides "the aspect of mental phenomena that is evident from the first-person, inner point of view of the conscious subject" (p. 38). He makes the point that identification of such experiences with a physical brain state constitutes a serious logical error, citing an argument made by Max Black (pp. 39–41).[1]

In this discussion, Nagel distinguishes between a *constitutive* and a *historical* explanation of consciousness (p. 54). The attempt to identify a subjective experience with a brain state is an example of a constitutive explanation of consciousness. A historical explanation would be a demonstration of how some evolutionary theory would explain how consciousness could be the eventual result of a process of natural evolution.

Nagel, however, makes a very interesting and definitive move; he does not dispute evolution as such, but only neo-Darwinian evolution as allied with physical science and based on chance mutation and survival of the fittest. This important diversion plays a role in his discussion of alternative theories.

As a proper philosopher, Nagel thrives on the making of important distinctions. Along with the distinction between constitutive and historical explanations, he employs a distinction between "external" and "internal" explanations. These distinctions are applied to a further distinction among three main theories of mind in relation to cosmos. (So as the reader can intuit, things tend to get rather complex.)

The two external theories are the materialistic and the theistic theories. They are "external" because in those theories the driving force in evolution derives from an external source: the operation of chance mutation under physical laws in the first case, and the intentions of a divine creator in the other (p. 21 ff.). Nagel finds both of these theories lacking as a means of accomplishing a *transcendent* self-understanding, which would mean a comprehensive understanding of ourselves, including our most salient features such as consciousness, cognition, and values, as natural expressions of the cosmos. (I will come to a discussion of the third proposed alternative momentarily.) In the three chapters that follow, Nagel employs yet another distinction, that between emergent explanation of consciousness and reductive explanation. The reader, then, can anticipate quite an array of alternatives and evaluations of each.

In this endeavor, Nagel employs a large-scale set of general criteria against which he finds the two "external" theories, in whatever manifestation, lacking. Essentially, these criteria stem from the nature of "our own existence."[2]

> Our own existence presents us with the fact that somehow the world generates conscious beings capable of recognizing reasons for actions and belief, distinguishing some necessary truths, and evaluating the evidence for alternative hypotheses about the natural order. We don't know how this happens, but it is hard not to believe there is some explanation of a systematic kind—an expanded account of the order of the world. (p. 31)

In order to get hold of this strong criterion, which echoes Nagel's previously quoted reference to common sense, it is important to realize that all these things Nagel cites as facts of human existence have been increasingly *denied* existence in the halls of cognitive science—or, as philosopher/physician Raymond Tallis put it recently, by those addicted to "Neuromania" and "Darwinitis" (Tallis 2011:40, McDaniel 2011).

Common sense, which Nagel cites as important, is regularly dismissed as a false "folk psychology."[3]

In contrast, it is precisely this move—denying the existence of what the current paradigm cannot explain—that Nagel takes as empirical evidence that the materialistic explanation fails. It is worthwhile to note the difference between the way Nagel makes his appeal to experience as the criterion and the way Tallis expresses it. Nagel, in his concise 128-page essay, speaks largely in terms of general categories of experience, such as the category of our ability to reason or the category of our belief in objective truths about moral and ethical matters. Tallis, on the other hand, utilizes his 358 pages to house much more detailed descriptions of what that experience, with its unfathomable and perhaps ineffable depths, actually is—an experiment which indeed every person can carry out as he or she goes about in daily life (e.g., Tallis 2011:75–80). It is his reliance on these facts of experience that Nagel fundamentally appeals to in his remark on "our antecedent sympathies." Nagel does however include a brief account of the sorts of experience Tallis recounts in more detail, citing the "incredible riches" of experience, including "beauty, love, pleasure, knowledge, and the sheer joy of existing and living in the world" (p. 120). In effect, the challenge to the reader is this: "Look closely at your life—and then tell me you can agree that you are not a self but a machine devoid of free will, consciousness, knowledge, and value."

Summing up his initial overall perspective at the end of his first chapter, "Antireductionism and the Natural Order," Nagel cites "the respective inadequacies of materialism and theism" which he has dealt with briefly in that chapter and which he will pin down in more detail in the following chapters. Despite these inadequacies of present theory, he argues for the impossibility of giving up the task of understanding, with the hope that the future may lead to "an expanded but still naturalistic understanding that avoids psychophysical reductionism" (p. 32). At this point, Nagel makes a statement that many, and particularly the vast majority of physical scientists, will perceive as scandalous. It is an expression of the third possibility, which is an internal, rather than external, theory.

> . . . such an understanding would be to explain the appearance of life, consciousness, reason, and knowledge . . . as an unsurprising if not inevitable consequence of the order that governs the natural world from within. That order . . . will not be explainable by physics and chemistry alone. An expanded, but still unified, form of explanation will be needed, and *I expect it will have to include teleological elements.* (pp. 32–33, my emphasis)

Here we get down to the bottom line of Nagel's book. In denial of the

dogma of standard scientific practice against any explanation that dares to suggest a purposive impulse in the natural world, Nagel proposes that an expanded evolutionary theory must involve a teleological factor; but not a teleology resulting from the inscrutable intentions of a supernatural creator. Rather it must be what Nagel calls a *natural teleology*, coming from within the cosmos rather than coming from either the will of a divine creator or the action of an inadequate set of physical laws which preclude the telic factor. It would assert that directionality of evolution leading to the development of life and consciousness must belong *internally* to the natural world at every stage of its existence, from the Big Bang onward.

Here, then, is where Nagel steps in where angels fear to tread. So powerful is the bias against any explanation of evolutionary development that includes a teleological factor, that Nagel may expect a cold welcome from those committed to the current paradigm. And this propels him into initiating some discussion of how the process of the evolution of life and consciousness can involve a teleological factor without assuming a single telos or goal—in other words, the theory is not a theory of extremely predetermined goals, but yet one of purposiveness in nature: cosmological directionality without a closed conclusion.

Nagel's essay into this treacherous realm is not extensive. He cites an important analysis by Roger White to the effect that a confusion exists when it is assumed that since the intentional theory must be rejected, no alternative account of evolution remains but the mechanistic one (p. 90). Following up on this point, he provides a brief foray into the question of what a "natural teleology" would be. It would have to be distinct from appeal to the operation of chance, from external supernatural intention, and from blind physical law (p. 91). Is such a conception of teleology in nature possible? Nagel returns here to his guiding principle of careful philosophical exploration as well as his view that whatever the answers to the evolutionary dilemma are, they will not be those of the standard paradigm and they will eventually be discovered.

> A naturalistic teleology would mean that organizational and developmental principles of this kind are an irreducible part of the natural order, and not the result of intentional or purposive influence by anyone. I am not confident that this Aristotelian idea of teleology without intention makes sense, but I do not at the moment see why it doesn't. (p. 91)

In making this move, Nagel is walking on a philosophical and a scientific tightrope between the other alternatives. But his contribution to the discussion is significant for two reasons. The first is that it emerges from a strongly argued skepticism as to the value and likely success of

the materialist–reductionist approach as well as a general rejection of supernatural explanations. Many will agree with him that the actual nature of human experience constitutes an empirical reason for rejecting the former, and many others, including most scientists, will agree with him that creationism will not fly.

The second reason is that he has framed the way to, and re-opened the topic of, a teleological factor in providing a transcendent internal understanding of who, what, and why we are. With respect to this last reason, its importance, in my view, is that Nagel does not write from a base within those philosophical genres where this same subject of teleology in evolution and in the nature of life has already been put forward, but from within a genre of philosophy where such things are generally avoided like the plague. The evidence of this apparent neglect is that literally none of those thinkers whose views might be relevant (but whose views have been in recent times universally excluded from mainstream philosophical thought) are mentioned or included in the paucity of the Index in the book.

That said, I wish to devote the final paragraphs of this review to the latter, with whom Nagel has actually more in common than one might think. Uppermost in this respect is Nagel's strong view to the effect that there must be a *continuity* in the evolution of consciousness from the earliest stages of the cosmos, i.e. from the moment of the Big Bang. In other words, living things have some degree of consciousness all the way back to the origin of life, and the laws of nature must have contained that potentiality throughout the course of time. This affirmation of continuity throughout the course of evolution such that the existence of consciousness in ourselves testifies to its presence, potential or actual, over the play of cosmic time really places Nagel's tentative conclusions within the context of those past but presently *persona non grata* philosophers who agree with him and who place continuity at the heart of their own transcendent internal understanding of mankind.

For the sake of brevity, I will mention only a few of those individuals whose ideas seem not to have found their way even into a footnote in Nagel's book. Offhand I would mention Henri Bergson (1911), American Pragmatists such as John Dewey (1929) (strongly influenced by Bergson), the Jesuit philosopher Pierre Teilhard (1955), and more recently Hans Jonas (1966), Professor of Philosophy at the New School for Social Research in New York City from 1955 to 1976. All these individuals, each in their own way, impinge on the issue of natural teleology raised by Nagel, and in this writer's opinion each should be assessed and re-evaluated in terms of what they may provide for the discussion.

Bergson explores the nature of the continuity of time in living

existence, which speaks to a different conception of time than that of the laws of physics. Dewey argues for continuity in the development of cognition over the course of evolution and at the same time insists that the salient features of experience cannot be denied by theory at the peril of impoverishing our self-understanding into a dead end of eternal dualism. Teilhard, while always under fire for his apparent view that there is a fixed goal of evolution in the dispensation of the Second Coming, nevertheless asserts in no uncertain terms that if consciousness is present in humankind, it must be present in potential or actual form from the beginning of time; and further that development must never come to absolute closure but must remain always open for further understanding—a *telos* more consistent with what Nagel feels has to be the case. And Jonas engages in a lengthy and detailed critique of the difference between "purpose" in a mechanism (i.e. the purpose built into its mechanical design) and the nature of purpose in biological teleology (Jonas 1966, Fifth Essay, especially p. 126).

Bergson, of course, is accused of insupportable Vitalism. Dewey's efforts seem to many to be antiquated and (unjustly) to smack of a form of behaviorism. The value of Teilhard's overall theory is weakened by the appearance of its seemingly intentionalistic character despite the fact that his "Omega" telos is strangely non-supernatural in certain ways. As far as Jonas's work goes, his analysis is concise, pointed, and accurate, plus he speaks in a language more comfortable to those working within the contemporary philosophical genre.

In avoiding reference to these other views, Nagel achieves a valuable separation of his analysis from the sorts of knee-jerk criticisms to which they have been subjected. Yet I would suspect that elements from the views of these and similar thinkers must, in the event, necessarily fructify Nagel's search for a viable articulation of his desire for a "natural teleology."

Notes

[1] In making this distinction between the Berkeleyan subjectivity and his own point, Nagel is in a position similar to that of Kant, who also has been interpreted almost universally as advocating a kind of subjective idealism despite his efforts to make clear that this is not his position at all (cf. Friedrich 1949:xxix).

[2] There is a close parallel here between what Nagel refers to as "our own existence" and the concept of *experience* as articulated within the philosophical position of John Dewey's philosophy of pragmatism:

> If experience actually presents esthetic and moral traits, then these traits may also be supposed to reach down into nature, and to testify to some-

> thing that belongs to nature as truly as does the mechanical structure attributed to it in physical science. (Dewey 1929:2)

It is notable that there is not a single reference to Dewey or to the pragmatists in Nagel's account.

[3] For a description and criticism of these views, see Will Wilkenson: http://enlightenment.supersaturated.com/essays/text/willwilkinson/church-landdebunked.html

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BOOK REVIEW

Resurrecting Leonora Piper: How Science Discovered the Afterlife by Michael Tymn. Guildford, United Kingdom: White Crow Press, 2013. 232 pp. $16.99. ISBN 978-1908733726.

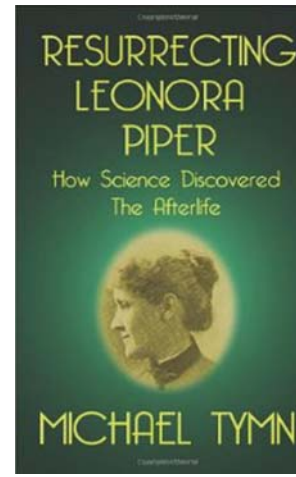
This is a book that offers many of the recorded transcripts of Leonora Piper's mediumship experiences with various sitters starting in 1885 when William James began taking an interest in her. According to the author, it is an

> attempt to explain the dynamics of her mediumship, including the difficulties associated with it, and to offer some of the best evidence for survival of consciousness after death that came from her mediumship. (p. xv)

The sessions with Mrs. Piper took place in both England and the United States over a period of 25 years. They numbered in the hundreds and were observed and recorded by many of the most distinguished scientists and academicians of the day, including Richard Hodgson, William James, Sir Oliver Lodge, Frederic Myers, and Professor James Hyslop. The intent was to offer as much evidence as possible to either support or refute the nature of mediumistic communications and whether they truly come from discarnate entities.

The vigor and amount of time and energy that went into examining Mrs. Piper was unprecedented. Many of the most distinguished scholars, most of them initially intent on exposing Mrs. Piper as a fraud, took their research very seriously and subjected Piper to the highest standards of vigorous scrutiny. In addition to determining whether conscious fraud was involved, the researchers were also interested in determining whether Piper got her results through telepathy or "super-psi."

Chapter 1 provides a brief review of Mrs. Piper's early life and how she came to be a trance medium. After providing some startlingly accurate information about the son of a certain Judge Frost, word soon got out about her abilities and she began doing sittings for friends and relatives with some success. Soon William James heard about her and had a number of sittings with her in addition to arranging others with friends, relatives, and associates. He ended up so impressed that he labeled Mrs. Piper his "white crow" (p. 14), reflecting his often repeated quote that "in order to disprove the assertion that all crows are black, one white crow is sufficient."



Chapters 2 and 3 describe Richard Hodgson's beginning involvement with observing and recording Mrs. Piper's sessions and her evolution in becoming a much sought after research subject by some of the world's most esteemed scientists at that time. Hodgson, one of the main researchers, continued sitting with her on the average of three times a week for 18 years between 1887 and 1905 (p. 196). His initial skepticism, like all the others', turned into a firm belief that she was not a fraud, and he suggested that leaders of the Society for Psychical Research (SPR) invite her to England for further observation and testing. It was at that time that a Dr. Phinuit became her spirit entity "control," which involves the taking over of the body of an entranced medium. Much of the rest of the book consists of recorded transcripts of her sessions with various sitters and evidence which attempts to substantiate the existence of survivalism.

As one reads the transcripts of sitting after sitting, the question of how Mrs. Piper could have known so many detailed and accurate facts becomes more and more compelling. With the possibility of fraud removed, how could Mrs. Piper receive such intimately detailed and largely accurate information from deceased friends and relatives of the sitters? That is why much of the evidential offerings directly or indirectly deal with whether Mrs. Piper received her information through some form of telepathy, either personal or cosmic. There are many sittings that seem to refute those possibilities. They include details the sitter did not or could not have known about. Here are just a few:

— George Pellew, one of Mrs. Piper's other deceased controls, provided information to a sitter, John Hart, about a conversation Pellew had had with the 15-year-old daughter of some friends of Hart about "God, space, and eternity" that Hart did not know about but which was later verified. (p. 66)

— James Hyslop's deceased father asked him what he remembered in a conversation they had one evening in a library about the father's description of the Bible including a discussion they had about hypnotism, apparitions near the point of death, and Swedenborg. Hyslop remembered all of it but could not recall discussing Swedenborg. However, when he talked to his stepmother about it, she remembered it well because she did not know about Swedenborg and discussed the latter with her then-still-living husband after Hyslop left for the day. (p. 100)

— Another sitting also involved Hyslop's deceased father asking about his old horse, giving the horse's name, Tom. The father indicated that an old friend had moved west and that he had had a dispute over putting an organ in their church with a second friend. As Tymn states, "the latter two facts were outside the scope of mental telepathy as Hyslop knew nothing about them, although he later checked with relatives and found them to be true." (p. 101)

More evidential sittings:

— A deceased brother said that he could hear his sister playing the piano. Hodgson, who was alone with Mrs. Piper and taking notes, recorded the time as 11:26 a.m. and sent a telegram to the parents after the sitting, asking if the daughter had been playing the piano that morning. The mother replied by telegram that her daughter had been playing between 11:15 and 11:30. Normally, she would have been in school at that time but bad weather kept her at home. (p. 113)

— A sitter named Robbins heard from a control named Rector. He introduced her to a deceased physician who gave her some advice on her health. The latter told her that he formerly lived in Boston, but had died in Paris a year or two earlier. Robbins later confirmed that a physician by the name given her had lived on Beacon St. in Boston and had died in Paris the preceding September. (p. 133)

— One especially evidential message came through another medium. Mr. Lodge's son Raymond had been killed on the battlefield in Ypres on September 14. Approximately two weeks later, Lodge and his wife saw a medium named Mr. A. Vout Peters. At that time, the deceased Raymond came through and referred to a group photograph in which he was holding a walking stick. Neither Lodge nor his wife could recall any such photograph. Then, during a later sitting with another medium, a Mrs. Leonard, they asked Raymond about the photograph. Raymond communicated that it was a group photo of his army unit, that he was sitting down while others were standing, and the person behind him was leaning on him. Four days after that sitting, Lodge and his wife received a letter and photograph from the mother of one of Raymond's fellow officers. Raymond was sitting, with a walking stick across his legs and the arm of the man behind him resting on his shoulder. The photo had been taken three weeks before Raymond's death. (p. 190)

An important evidential source at the time called cross correspondences involved automatic writing mediums receiving messages in the privacy of their homes. There were no sitters or researchers present. Basically, cross-correspondences involved fragmentary messages coming through two or three mediums, which when joined together formed a coherent message. It was a scheme devised by the not-yet-discarnate Frederic Myers before his death but to take place after his death (p. 174).

One of the more simple cross correspondence experiments involved SPR researcher John Piddington communicating with Myers through Mrs. Piper asking Myers to attach a sign to any message he might send through another medium, suggesting a circle with a triangle in it. Such a sign came through the automatic writing of a medium 12 days after Piddington's request. Then the same sign came through another medium although the triangle was not in the circle. (p. 176)

In summarizing, Tymn states that

> the fact that information unknown to (people like) Hyslop but later verified as true was communicated seemed to rule out simple person-to-person telepathy. As for a more cosmic telepathy—one in which the medium taps into minds and memories anywhere in the world or into some cosmic computer and then relays the information back to the sitter in a conversational manner—Hyslop felt that there was no adequate scientific evidence for such a theory and that it represented a process far more incredible than spirits. (p. 104)

Tymn concludes that many of the facts communicated were not recorded anywhere, and at times the sitters were unaware of things told to them and had to verify them as facts through others. In addition, Mrs. Piper also spoke or wrote in foreign languages she did not know. She also did trance writing as well as trance voice, and Hodgson observed that a sitter's deceased sister communicated by having Piper write with one hand with George Pellow communicating through the other while Phinuit was talking—all simultaneously on different subjects (p. 75). Tymn concludes by saying that

> with all of the foregoing hypotheses ruled out, at least highly unlikely, it would seem that the spirit, or spiritistic, hypothesis makes the most sense. (p. 200)

He also notes that it is unfortunate that people today remain largely unaware of the extraordinary sessions of Mrs. Piper and the unparalleled research of people such as Hodgson, Lodge, Hyslop, and James in examining them. As with diamonds waiting to be unearthed, the world will gain immeasurably from the eventual sparkle of so much brilliant and profound evidence regarding the survival of consciousness after death.

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BOOK REVIEW

Telephone Calls from The Dead: A Revised Look at the Phenomenon Thirty Years On by Callum E. Cooper. Tricorn Books, 2012. 193 pp. £8.99. $18. ISBN 978-0956759726.

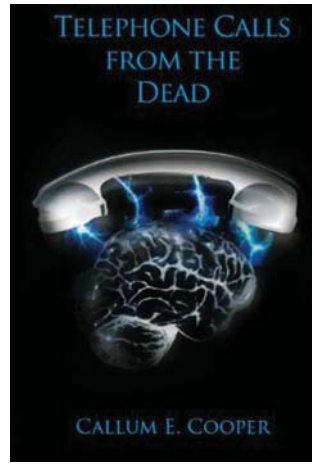
Telephone Calls from the Dead deals with a topic rarely touched upon since Rogo and Bayless published their classic *Phone Calls from the Dead* (1979). This book gives a review of how this field has developed, presents a number of old and new cases, and deals at length with various explanations of the phenomenon and its technical aspect. And it is very readable.

The book reviews collections of case reports of spontaneous phone calls from the dead, primarily Bayless and Rogo's 50 cases suggestive of anomalous communication that were collected in the 1960s. They divided the cases into a few categories, such as simple calls where the dead caller says only a few words and is unresponsive to questions, which brings the call to an end. Second, prolonged calls that involve a conversation, and third, answer calls where living persons make a call to someone they do not know has died and yet they get an answer. There are also cases involving disconnected telephones. This fascinating anecdotal material has undergone considerable investigation and scrutiny.

During the 1980s, some 40 Italian cases were collected by Massimo Biondi. After interviewing the receivers of the calls and additional witnesses, Biondi concluded that 20 of these cases still appeared to be inexplicable.

The author describes attempts to construct equipment (*psychophones*) to communicate with the dead. This was mostly done by persons long forgotten by our generation, such as Francis Grierson and F. R. Melton. Among them was no less a person than the great inventor Thomas Edison. Many of the researchers involved in telephonic communication with the dead believed that the dead were somehow able to manipulate the electrons down the telephone line to produce the calls. Hence it was essential for investigators to compile an instrument made of extremely sensitive components.

The subject of electronic voice phenomena (EVP) and instrumental transcommunication is mentioned and the contributions of Friedrich Jurgensen and Konstantin Raudive briefly described. The author argues for keeping the phone call phenomenon and the electronic voice phenomenon apart and he treats them as separate anomalies. There were also mixed cases, and inexplicable malfunctions of telephones or telephone systems



that border on poltergeist (such as in Bender's Rosenheim case, which is not mentioned). The author also takes up the more recent subject of anomalous voicemail and text messages which he finds closely related to the electronic voice phenomena.

The bulk of the book deals with the phone call phenomena and gives various analyses of the characteristics of the cases. It is very interesting to note that most of the people who had these experiences had never heard about such a thing as phone calls from the dead.

One chapter looks at the question of whether psychologial factors can explain the phenomena, and also at the obvious weaknesses of these cases and various potential sources for errors. Still, some residua of cases seem to remain inexplicable.

Cases of contacts with the dead can take many forms. Apparitional forms are the most common, as shown in my recent book *The Departed among the Living: An Investigative Study of Afterlife Encounters* (2012). Cooper's book brings to our attention another and more rare form (telephone contact). He should be complimented for taking up this almost forgotten subject in a thoughtful and thorough manner.

Cases of this kind appear to be extremely rare. Hopefully the readers of this Journal will bear with me if I end this review with a few words about a case that happened in my family. One day the phone rang and my wife went to the phone. It was a call from a relative in Copenhagen who had recently died in his nineties, and whom both of us had visited on several occasions. He greeted her in his usual affectionate way and started to say something, but then his voice slowly faded away. There could be no mistake about his voice, my wife told me. His voice was so easy to recognize. I remember that for days after this incident my wife brought it up again and again. She was so deeply impressed by this extraordinary phone call.

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ARTICLE OF INTEREST

Bird Origins Anew by Alan Feduccia. *The Auk—An International Journal of Ornithology*, *130*(1), January 2103, pp. 1–12.

In *Dogmatism in Science and Medicine* (Bauer 2012), I mention a number of fields in which the mainstream position is dogmatic to the n^{th} degree and constitutes a monopoly. That is accompanied by suppression of other views: Dissenters are excluded from conferences, from publishing in leading journals, and from funding of research, and they are labeled "denialists," with pejorative association to those who deny the Holocaust (Furedi 2007). To the already long list of fields mentioned in the book, I can add the dogma described in this article, that birds are derived in a particular way from a particular line of dinosaurs. The circumstances are uncannily similar to those facing minority views concerning string theory, extinction of dinosaurs, HIV/AIDS theory, the hypothesis of human-caused global warming, etc.:

> [T]he current mantra . . . has become an unchallengeable orthodoxy: Birds are living maniraptoran theropods. . . .
>
> [T]hose who offer contrary evidence are subjects of ridicule and no longer considered scientists. . . . [O]nly supporting evidence will be recognized, while contradictory evidence is ignored or explained away. . . .
>
> [A]ll conclusions are based on the *fact* [emphasis in the original] that "birds are living dinosaurs". . . .
>
> Lack of citation has become a common but disturbing mechanism of censorship. . . .
>
> The current orthodoxy of flight origins, involving massive exaptation, stretches biological credulity and is practically non-Darwinian.

[Current dogma requires that flight was "learned," acquired in some way, by creatures accustomed to roaming the ground, which seems massively improbable. By contrast, the now-minority view that used to be mainstream is the highly plausible idea that powered flight was achieved by extrapolation of near-flight behavior in creatures long used to gliding downward from high in trees. "Exaptation" means that characteristics evolved for a particular purpose are coopted to serve a different purpose. It is difficult to see which characteristics of land-roaming creatures could be adapted to flight, but easy to see in the case of species that had become accustomed to gliding.]

> Attempts to silence any opposition to the current unchallengeable orthodoxy are seen in the lack of citation of contrary views . . . , and polemical and ad hominem reviews that are substituted for evidence. . . .
>
> [We] are typically accused in ad hominem fashion of not understanding cladistic methodology and, therefore, of not being scientists. But we emphatically do understand the essence of the methodology, and that is the problem— . . . the fragility and very tenuous nature of cladistic analyses.

Part of Feduccia's argument concerns the validity of cladistic approaches to discovering or proving ancestry. Cladistics groups species according to large numbers of characteristics, using computers to discern similarities and lineages. As with computer modeling, this approach depends on what is fed into the computer, in this case which characteristics to encode and how to weight their significance. Feduccia points out that no amount of descriptive morphological data used in cladistic analysis can compete with, let alone supersede, genetic analysis. One reason, enough in itself, is the phenomenon of convergent evolution: Quite distinct genetic lineages have led to species that look somewhat alike and behave somewhat alike, because those features happen to suit a particular environment—for example, several Australian marsupials came to look and behave rather like certain non-marsupial mammals elsewhere. Therefore morphology and behavior cannot be relied on for inferences about ancestry. By contrast, genetic analysis is a direct way of demonstrating ancestry which could be invalidated only by some most improbable series of mutations. [Hull (1988) has described in fascinating detail the history of cladistics, as an example of the social processes at work in scientific activity. It's a marvelously informative book that everyone interested in scientific activity could read with profit.]

So absurd are some of the assertions and speculations by mainstream dogmatists about avian evolution that they have been pilloried by Creationists, no less; Feduccia observes that "It is chilling to contemplate that the Creationists may be the ones to sweep our own house clean."

Another interesting point in Feduccia's article concerns neoteny ("Peter Pan evolution"), the phenomenon whereby the adults of some species resemble the infants of another species. For instance, Feduccia notes that the flightless birds (ostrich, kiwi, etc.) evolved from flighted ancestors by neoteny: "They are all big chicks" and thereby "closely resemble, albeit superficially, the theropod dinosaurs." Similarly, human adults are much more like chimpanzee babies than they are like chimpanzee adults; we humans are neotenous apes.

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