Volume 22, Number 4 2008

Editorial
457 Editorial 

Research Articles
459 Energy, Entropy and the Environment (How to Increase the First by Decreasing the Second to Save the Third) 
D. P. Sheehan
481 Effects of Distant Intention on Water Crystal Formation: A Triple-Blind Replication 
Dean Radin
Nancy Lund
Masaru Emoto
Takashige Kizu
Craig F. Buhler
Paul R. Burgess
Earl VanWagoner
Rupert Sheldrake
Pamela Smart

495 Changes in Physical Strength During Nutritional Testing 
Craig F. Buhler
Paul R. Burgess
Earl VanWagoner
Rupert Sheldrake
Pamela Smart

517 Investigating Scopesthesia: Attentional Transitions, Controls and Error Rates in Repeated Tests 
Rupert Sheldrake
Pamela Smart

Essays
529 Shakespeare: The Authorship Question, A Bayesian Approach 
P. A. Sturrock

539 An Anomalous Legal Decision 
Richard A. Blasband

Historical Perspective
543 Note on Charles Richet’s “La Suggestion Mentale et le Calcul des Probabilités” (1884) 
Carlos S. Alvarado

549 Hume's Syndrome: Irrational Resistance to the Paranormal 
Michael Grosso

Letters to the Editor
557 Letters to the Editor 

Book Reviews
561 What Happens When We Die, by Sam Parnia 
Erlandur Haraldsson

563 The Way of the Explorer: An Apollo Astronaut’s Journey Through the Material and Mystical Worlds, by Edgar Mitchell with Dwight Williams 
Roger D. Nelson

564 Inner Paths to Outer Space: Journeys to Alien Worlds through Psychodelics and Other Spiritual Technologies, by Rick Strassman, Slawek Wojtowicz, Luis Eduardo Luna, and Ede Frecska 
David Luke

569 Extraordinary Knowing, by Elizabeth Lloyd Mayer 
William Roll

573 On Being Certain, by R. A. Burton 
Mikel Aickin

576 The Deniers: The World-Renowned Scientists Who Stood Up against Global Warming Hysteresis, Political Persecution and Fraud. And Those Who Are Too Fearful To Do So, by Lawrence Solomon 
Joel Kauffman

579 The First Fossil Hunters: Paleontology in Greek and Roman Times, by Adrienne Mayor; Fossil Legends of the First Americans, by Adrienne Mayor; Thunderbirds: America’s Living Legends of Giant Birds, by Mark A. Hall 
Leigh Van Valen

583 Where Medicine Went Wrong: Rediscovering the Path to Complexity, by Bruce J. West 
Mikel Aickin

587 An Encyclopedia of Flying Saucers, by Vernon Bowen 
Jerome Clark

589 Further Books of Note
589  Articles of Interest
591  Journal Review
595  Reviewer Acknowledgment

Indices for Volume 22
596  Author Index
600  Keyword and Review Index

SSE News
603  SSE Executive Committee, Officers, and Council
EDITORIAL

I am once again impressed at the variety and quality of submissions to our journal: Entropy, Distant Intention, Nutritional Testing, and Scopesthesia – to mention only the research articles! (This is your chance to find out what “scopesthesia” means.) In addition, we have two essays (for one of which I am the guilty party), two contributions to our new Historical Perspective section (courtesy of Carlos Alvarado), three Letters to the Editor, and our usual very enjoyable collection of Book Reviews (courtesy of David Moncrief).

This Editorial is my swan song as Interim Editor In Chief of the journal. I am happy to report that Stephen Braude has been persuaded to assume the position of EIC effective January 1. We are very fortunate that Steve is willing to take on this assignment, and I congratulate our President, Garret Moddel, on his selection and powers of persuasion.

In addition to Carlos and David, I have been most fortunate to have a varied, able, and conscientious stable of Associate Editors: Stephen Braude, Courtney Brown, York Dobyns, Bernard Haisch, John Ives, Roger Nelson, Dean Radin, and Mark Rodeghier. I extend my heartfelt thanks to you all.

The Publications Committee [Henry Bauer, Bob Jahn (chair), and Garret Moddel] has proved to be another great asset. I have found it most helpful to be able to discuss policy issues with them. It has also been helpful – fortunately, on rare occasions - to be able to hand them a hot potato when an author was unhappy with an editorial decision, and wished to lodge an appeal with a higher authority. Thank you, Henry, Bob, and Garret.

Finally, I wish to acknowledge the highly professional and invariably good-humored support of the Allen Press staff – especially Kristen Jarboe and Joy Richmond (the Managing Editor team). Thank you, Kristen and Joy.

Energy, Entropy and the Environment (How to Increase the First by Decreasing the Second to Save the Third)

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Abstract—Energy is the lifeblood of civilization, but inexpensive, high energy density sources are rapidly being depleted and their exploitation is severely degrading the environment. This paper explores a radical solution to this energy-environmental dilemma. In the last 10–15 years, the universality of the second law of thermodynamics has fallen into serious theoretical doubt [1–3]. Should it prove experimentally violable, this would open the door to a nearly limitless reservoir of ubiquitous, clean, recyclable energy. If economical, it could precipitate paradigm shifts in energy production, utilization and politics. In this paper, recent challenges to the second law are reviewed, with focus given to one for which laboratory experiments are planned. Possible consequences of its violation for technology, society and the environment are explored.

Keywords: entropy—energy—second law of thermodynamics—climate change—environment—ecology—energy economy—famine

1 Introduction

Energy makes the world go round—physically, chemically, thermodynamically, industrially, economically, geopolitically. Current global consumption stands at roughly $1.5 \times 10^{13}$ W, equivalent to the output of about fifteen thousand large nuclear power plants, or comparable to detonating a WWII-style atomic bomb every five seconds. This figure is expected to grow 50% in the next 20 years. About 20% of the world economy is devoted to the discovery and extraction of fuels, and to the generation, distribution, and consumption of energy. Economies are defined by it; wars are fought over it; nations rise and fall by it.

Presently, energy is derived primarily from non-renewable oil, natural gas, coal, and uranium, and to a lesser degree from renewable hydroelectricity, solar, wind and biofuels. The burning of fossil fuels is implicated in environmental pollution, global warming, climate change, and the degradation of the biosphere, all of which are expected to worsen in coming decades [4]. Recently, the tightening of global energy supplies has been linked to food shortages, affecting hundreds of millions of humans worldwide.

In fact, we are surrounded by a virtually limitless reservoir of energy: thermal energy. The total thermal energy content of the Earth’s atmosphere is about
10^{24}\,\text{J};\) the oceans’ capacity is 500 times greater, and the Earth’s crust holds an order of magnitude still more. At civilization’s current rate of use, it would take millions of years to expend this amount, and even then, it is being replenished by solar radiation and the decay of radionuclides in the crust orders of magnitude faster than humanity could deplete it; in other words, the amount of thermal energy is effectively limitless. In magnitude, all the energy we could ever use already surrounds us; in form, however, it is largely beyond our reach – like a mirage in the desert – because of what is perhaps the most depressing law of nature: the second law of thermodynamics.

The second law has been called “the supreme law of nature” [5]. It governs our lives from the moments of our conception until our deaths; nearly every system in the universe, from an atomic nucleus to a galactic supercluster, is subject to it; the cosmos itself lives – and will eventually die – by it. Even the direction that time progresses, from past to present to future, has been attributed to it [6–9].

Among physical laws, arguably none is better tested than the second law. It has been verified in countless experiments for more than 150 years. Most scientists consider its universality beyond reproach; even to question it invites ridicule and ruin. Nonetheless, over the last 10–15 years, the second law has come under unprecedented scrutiny. More than 60 mainstream journal articles, monographs and conference proceedings have raised dozens of theoretical and experimentally-testable challenges to its universal status – more than the sum total during its previous 150-year history. From a Kuhnian perspective this suggests a paradigm shift might be on the horizon [10].

Given its central importance to the sciences, engineering and technology, in view of these recent theoretical developments, and in light of the current dilemmas facing world energy and environmental policies, it is timely to look ahead to possible changes that might result from second law violation. This paper briefly reviews recent second law challenges, and examines in detail one for which laboratory experiments are currently being mounted. Possible economic, geopolitical and environmental outcomes of second law violation are considered.

2 Second Law: Status and Challenges

2.1 Background

The second law was first enunciated by Clausius (1850) and Kelvin (1851), largely based on the work of Carnot 25 years earlier. There are several fine accounts of its history [11–14]. Once established, the second law settled down and multiplied wantonly¹; it has more common formulations than any other physical law. Some versions overlap, while others seem to be entirely distinct laws. Despite this Drinanian ambiguity, there is near universal agreement that, whatever it is, the second law is inviolable.

Here we introduce three standard formulations: two from classical thermo-
dynamics and a third from statistical mechanics. The first explicit and most widely cited formulation is due to Kelvin [15,16]:

**Kelvin-Planck:** No device, operating in a cycle, can produce the sole effect of extraction of a quantity of heat from a heat reservoir and the performance of an equal quantity of work.

In this, its most primordial form, the second law is an injunction against *perpetuum mobile* of the second type. Such a device could transform heat into useful work, in principle, indefinitely.

The second most cited version, and perhaps the most natural and experiential, is due to Clausius [17]:

**Clausius-Heat:** No process is possible for which the sole effect is that heat flows from a reservoir at a given temperature to a reservoir at a higher temperature.

In the vernacular: heat flows from hot to cold. A statistical formulation can be expressed in terms of entropy [16]:

**Planck:** For any spontaneous natural process, the entropy change of the universe is never negative; i.e., $\Delta S_{\text{universe}} \geq 0$.

Though simply put, these statements are profound because they assert that work (organized energy) degrades inexorably into a disorganized, less useful form: thermal energy (heat). They guarantee that heat is difficult to reorganize back into work – and even trying just wastes more energy and generates more entropy than if the effort had never been made. Dealing with the second law is a “no win” situation. In truth, the best strategy against its ravages seems to be to do nothing at all. ²

### 2.2 Recent Challenges

Although the question of second law universality has been considered settled by the wider scientific community for more than a century, there has always been an undercurrent of doubt, sustained by the unproven and admittedly incomplete foundations upon which it rests. In recent years this doubt has grown into a movement to examine more critically the second law foundations [18], integral to which has been the advancement of more than two dozen counterexamples to its universality by several research groups worldwide [1–3]. While *experimental* violation of the second law has not been claimed, experimentally testable challenges have been advanced, a number of which have laboratory corroboration of their critical underlying processes.

Second law challenges are diverse; they span classical and quantum mechanical regimes, range from nanoscopic to planetary in size, operate from above the melting point of steel down to a fraction of a degree above absolute zero. They make use of ideal gases, plasmas, semiconductors, superconductors, micro- and mesoscopic electrical circuits, chemical catalysts, and biologically-inspired structures. Perhaps not surprisingly, most inhabit physical regimes that were
unheard of when the second law was introduced by Clausius and Kelvin in the 1850s, but which are now routinely realized in the laboratory.

The modern second law movement began quietly 30 years ago with the seminal work of L. G. M. Gordon and J. Denur. Gordon considered theoretical chemical-mechanical systems apparently inspired by biological structures like cell membranes, molecular motors and ion channels. Through these he demonstrated that the principle of detailed balance was suspect [19–25]. Denur focused on the microscopic kinetics of ideal gases, which have traditionally been touchstones for second law universality, pointing out inconsistencies in the paradigm stance [26–30]. Most recently he has shown that at equilibrium, an ideal gas’ velocity distribution spontaneously becomes weighted in favor of low-velocity particles, owing to their commensurately greater flight time between wall collisions [30]. Effectively, the bulk gas becomes cooler than the walls – a problem for the Clausius form of the second law. Both Gordon and Denur remain active in the field today.

Beginning in the mid-1990s, the number and variety of challenges burgeoned as multiple research teams entered the field. Čápek et al. were the most prolific, posing a broad spectrum of quantum theoretic challenges [31–46]. Their models are noteworthy for their formal, foundational approaches; however, they are also difficult to connect to concrete physical systems, making it difficult to assess their prospects for experimental verification. Allahverdyan and Nieuwenhuizen have written extensively on the limits to various formulations of the second law in the quantum regime, particularly quantum coherence and entanglement [47–52]. Among their significant contributions is the spin boson model, by which two-level systems that are quantum mechanically entangled with a bath of harmonic oscillators can extract work from a heat bath. They have also suggested experiments on mesoscopic or nanoscopic electrical circuits interacting with a low-temperature heat bath, which could pose a violation of the Clausius form of the second law in the quantum regime.

Several challenges connected with superconductivity have undergone exploratory laboratory experiments. Keefe proposes a simple thermodynamic process based on the magnetocaloric effect in which a small superconducting sample is cycled through field-temperature space and performs net work solely at expense from heat from a heat bath [53–56]. Nikulov, et al., have conducted experiments on mesoscopic, inhomogeneous superconducting loops that are interpreted as supporting the existence of a so-called quantum force, which arises due to fundamental differences between classical and quantum states of electrons (or Cooper pairs) in a conducting (superconducting) loop [57–60]. Significantly, independent theoretical work, by J. Berger, supports their hypothesis [61,62].

Second law non-idealities associated with gases have been pursued by other workers besides Denur. Crosignani, Di Porto and Conti have theoretically and numerically investigated the dynamical evolution of a frictionless, adiabatic piston in a gas-filled adiabatic cylinder subject to the Langevin force [63–67]. In the mesoscopic regime the piston can undergo sizable fluctuations in position.
and display entropy decreases up to two orders of magnitude greater than those predicted from thermal fluctuations. The system also exhibits the disquieting property of failing to settle down to an equilibrium configuration.

Miller has theoretically analyzed gas cavities in the molecular flow regime in which anisotropic gas-surface interactions determine gas phase populations [68]. He finds that, under the standard constraints of particle flux, momentum and energy conservation, nonequilibrium steady-state gas phase populations are possible, depending both on cavity geometry and the nature of gas-surface interactions. From a theoretical standpoint, Miller’s is one of the most compelling cases yet made. Outside the academic sphere there are many more challenges, but these will be left to the reader to explore.

In light of these many challenges, the status of the second law is uncertain. On one hand, universality advocates (i.e., the broader scientific community) are unable to dispel the challenges, suggesting the law is likely either fundamentally flawed or incomplete. On the other hand, universality opponents, while having posed a number of theoretical challenges, have not yet delivered a decisive experiment to support their claims. Given the epistemic nature of physical law – that truth is ultimately decided by experiment rather than by theory – the burden of proof rests with the opponents. In the next section we review the largest class of experimentally testable challenges.

2.3 University of San Diego (USD) Challenges: Macroscopic Pressure Gradients (MPGs)

Over the last 18 years, a number of challenges have been investigated at USD, covering the fields of plasma, chemical, gravitational, biological and solid state physics [1–3,69–87]. Laboratory experiments have corroborated the key mechanisms upon which they depend. These have culminated in several micro- and nanoscopic solid state devices, one of which will soon undergo laboratory testing.

The USD challenges are joined by a common thread [72,86]. They exploit equilibrium MPGs, in particular, those found in the Debye sheaths at the edges of plasmas (electric field) [69–71]; nearby chemically active surfaces in low density gases (chemical potential field) [73,76,77,85]; in the curved spacetime around planets (gravitational field) [74,75,79,80]; and in the depletion regions of p-n junctions (electric field) [78,81–84,86]. They range in size from nanoscopic to planetary ($10^{-7}$–$10^{7}$ m), occur over more than an order of magnitude in temperature (100–2000 K), and over more than eight orders of magnitudes in pressure ($\sim 10^3$–$10^{-6}$ Torr).

Nearly all natural and technological processes are nonequilibrium in character and can be understood in terms of a working fluid moving under the influence of a macroscopic field expressible as the gradient of a potential. Examples are endless: water falling from the clouds under gravity; molecular hydrogen and oxygen combining in a fuel cell to form water; current in an electrical circuit.
Here potential gradient refers to any potential whose spatial derivative is capable of directing a fluid in a preferred spatial direction (i.e., $\nabla \Phi = -F$) and can transform equilibrium particle velocity distributions into nonequilibrium ones. (The Onsager relations embody this concept in the weakly nonequilibrium regime [88].) Directional, nonequilibrium particle fluxes are the hallmarks of standard work-producing processes.

While it is nonequilibrium MPGs that drive natural processes, equilibrium MPGs are also common. The crucial point is this: whereas nonequilibrium MPGs derive their energy from exhaustible free energy sources (e.g., nuclear reactions, sunlight, chemical reactions), equilibrium MPG derive their energy from purely thermal processes. Each USD system consists of a blackbody cavity surrounded by a heat bath, and a working fluid (e.g., gas atoms, electrons, ions, holes) in which an equilibrium MPG forms (e.g., gravitational field, electric field of a Debye sheath or depletion region). The following is a brief summary of the USD systems.

**Plasma** [69–71] Electrons and ions at a single temperature have different average thermal speeds ($kT/m^{1/2}$), owing to their different masses. In a sealed blackbody cavity, in order to balance thermal flux densities in and out of a plasma, the plasma resides at an electrostatic potential (the so-called plasma potential, $V_{pl}$) with respect to the confining walls. This potential drop occurs across a thin layer between the plasma and the blackbody walls, called the Debye sheath (thickness $\lambda_D$). Typical plasma parameters render plasma potentials up to several times $kT/q$ and gradients of order $\nabla V \sim V_{pl}/\lambda_D$. Sheath electrostatic gradients (electric fields) of the order of $10^3$ V/m are common. Although the sheath is thin (but still macroscopic) in the direction of the electric field, in the other two dimensions it can extend over arbitrarily large distances, making this a full, three-dimensional macroscopic potential gradient. The other systems share this trait.

**Chemical** [73,76,77,85] In a sealed blackbody cavity, housing a low-density gas (e.g., $A_2$) and two surfaces (S1 and S2) which are distinctly chemically reactive with respect to the gas-surface reaction (2A $\rightarrow A_2$), a chemical potential gradient can be supported, expressed as steady-state differential atomic and molecular fluxes between the surfaces.

**Gravitational** [74,75,79,80] All finite masses exhibit gravitational potential gradients (gravitational fields) that can direct working fluids (gases) preferentially along field lines. No thermodynamic processes are required to sustain this MPG.

**Solid State** [78,81–84,86] When n- and p-doped semiconductors are joined (forming a standard p-n diode) an electrostatic potential difference (built-in potential, $V_{bi}$) arises between the two regions, across the so-called depletion region (thickness $x_{dr} \approx 1 \mu$m). In the depletion region, a balance is struck between electrostatic and chemical potentials. The equilibrium electrostatic potential gradient scales as $\nabla V \sim V_{bi}/x_{dr}$, which for typical p-n diodes is on the order of $V/10^6 m = 10^6$ V/m. (The similarities between the plasma and solid state systems are not coincidental.) Applying these ideas to bio-membranes, a proposal has been made for a third category of life beyond the standard two – photosynthetic and chemosynthetic – that would rely on second law subversion: thermosynthetic life [87].

The above equilibrium MPGs and their working fluids possess all the required physical characteristics by which everyday nonequilibrium, free-energy-driven MPGs perform work in traditional thermodynamic cycles. Their potential
gradients are of sufficient magnitude and directionality to overcome thermal fluctuations and to perform macroscopic work. They differ from their non-equilibrium counterparts only in that they are generated and maintained under equilibrium conditions.

Work can be extracted from an equilibrium MPG system by allowing a fluid to cycle through the potential gradient. On one leg of its cycle the working fluid “falls” through the MPG and is transformed into a spatially-directed non-equilibrium flux, by which work is performed. On the return leg, the fluid and system returns to its original thermodynamic macrostate via standard thermal processes (e.g., diffusion or evaporation).

3 Solid State Challenge

3.1 Electromechanical Oscillators

We begin our examination of a specific laboratory-testable solid-state second law challenge with a closely related system that does not challenge the second law whatsoever, one that is familiar in dozens of everyday guises: an electromechanical oscillator.

Consider the simple electrical circuit depicted in Figure 1, consisting of a battery, a capacitor, a switch and ground. In Figure 1a, the capacitor is fully charged and in a stable, high-energy equilibrium state, while in Figure 1b, it is fully discharged to ground (which is assumed to have zero resistance) and in a stable, low-energy equilibrium state. Since there is no electrical resistance in the circuit, the charging and discharging are instantaneous. While it is straightforward to switch between these two equilibrium states, there is no practical mechanism for auto-switching or work extraction. In Figure 1c both shortcomings are resolved. Here is added a mechanical spring that doubles as a resistor. It simultaneously permits the top capacitor plate to discharge against the other – thereby acting as a switch – while also executing mechanical motion whereby mechanical work can be extracted from the moving upper plate, for example, by lifting a weight or running an electrical generator. Such electromechanical oscillators are ubiquitous in everyday life; battery-driven electric clocks and watches are common examples.

The device in Figure 1c, called the hammer-anvil, is a hybrid of well-known mechanical and electrical oscillators. The hammer (top capacitor plate) moves with respect to the anvil (lower plate). The electromechanics of the hammer is described by the following coupled pair of equations:

\[ F = F_{\text{diss}} + F_{sp} + F_{es} = m\ddot{x} = -\frac{1}{Q_m} x - k x - \frac{q^2}{2\varepsilon_0 A}. \]

Here \( x \) is the instantaneous excursion of the top plate from its equilibrium separation, \( A \) is the plate area, \( Q_m \) is the mechanical quality factor, \( k \) is the spring
constant, $\epsilon_0$ is the permittivity of free space, and $q$ is the instantaneous charge on the plates. The charge satisfies:

$$q = \left( V_o - \frac{q(x_g-x)}{\epsilon_0 A} \right) \frac{1}{R}; \quad q < q_{sat},$$

where $R$ is electrical resistance, and $\dot{q} = 0$ for $q \geq q_{sat}$. Here the right-hand side of (1) gives of the dissipative, spring, and electrostatic forces, respectively. In (2) $q_{sat}$ is the maximum (saturated) charge on the plates, $x_g$ is the equilibrium gap width between plates, and $\epsilon_0 A(x_g-x) = C_o$ is the capacitance.

Two independent time constants characterize this system: one electrical ($\tau_e \sim R C_o$) and one mechanical ($\tau_m = 2\pi \sqrt{m/k}$). This system is electromechanically unstable: if the charged capacitor plates electrostatically draw together and electrically discharge, the attractive electric field collapses, the spring retracts the plates, the plates recharge on time scale $\tau_e$, and the cycle can repeat. If the hammer’s mechanical oscillation time constant $\tau_m$ is comparable to the circuit’s electrical time constant $\tau_e$ and if the quality factor, $Q_m$, is sufficiently large, then the system can execute resonant, sustained electromechanical oscillation, converting the electrical energy of the battery into mechanical energy. Laboratory scale models of this oscillator have been co-built by the author and silicon-based microelectromechanical system (MEMS) versions, incorporating cantilever springs, have been demonstrated by others [89].

Ideally, this resonant electromechanical oscillator cycles between the two equilibrium states (Figure 1a and b) and some mechanical energy can be siphoned off in the process, so long as the extra load does not damp the oscillator beyond its ability to self-discharge. (Work load is modeled as an additional damping term in (1).) Clearly, this oscillator derives its energy from the battery; as it runs down, the electric field in the capacitor subsides and eventually the electromechanical oscillation cannot be sustained. This system also complies...
completely with the second law since work (chemical energy in the battery) is steadily degraded into heat via mechanical-aerodynamic-electrical damping.

It is crucial to notice that the mechanical part of the oscillator, the part that performs work, is oblivious to the source of the electric field that drives it. It could originate with a battery – as is the case here – or from a less familiar source, say the built-in potential associated with the depletion region of a p-n diode. Herein lies the rub.

3.2 Solid State Electromechanical Oscillator

In this section, a solid state version of the traditional resonant electromechanical oscillator is described that, in theory, undermines the universality of the second law. We begin with a review of pertinent solid state concepts.

3.2.1 Intrinsic Bias. The depletion region of a standard p-n diode represents a minimum free energy state in which bulk electrostatic and diffusive forces are balanced. Typical depletion regions are narrow, ranging from 10 µm for lightly-doped semiconductors to 0.01 µm for heavily-doped ones. The potential drop across a region, the built-in potential \( V_{bi} \) is given approximately by [90]:

\[
V_{bi} = \frac{kT}{q} \ln \left( \frac{N_A N_D}{n_i^2} \right)
\]

Here \( kT \) is thermal energy; \( q \) is electric charge; \( n_i \) is intrinsic carrier concentration (for silicon \( n_i \approx 1.2 \times 10^{16} \text{ m}^{-3} \) at 300K); and \( N_{A,D} \) are acceptor and donor concentrations. For dopant concentrations \( N_A = N_D = N = 10^{21} \text{ m}^{-3} \), one has \( V_{bi} \approx 0.6 \text{ V} \), a typical value.

Consider the horseshoe-shaped p-n diode with a vacuum gap (Figure 2a). At the left junction (J-I) is a regular p-n diode depletion region, while at the right (J-II) there is a vacuum gap. (Notice the similarities between Figures 1 and 2.) The built-in potential of the depletion region \( V_{bi} \) will be expressed across the vacuum gap; this can be argued either via energy conservation (Kirchhoff’s loop law) or via Faraday’s law. Numerical models using semiconductor device simulators (e.g., Silvaco International – Atlas) corroborate this gap electric field [78,84], as have numerous condensed matter experiments.

Although \( V_{bi} \) is small, the gap width \( x_g \) can also be made small such that the electric field – and therefore the electrostatic pressure – can be sizable. For example, for \( V_{bi}=0.6 \text{ V} \) and gap width \( x_g = 3 \times 10^{-8} \text{ m} \), the gap field is \( E \approx V_{bi}/x_g = 2 \times 10^7 \text{ V/m} \) and pressure \( P \approx (\varepsilon_o/2)E^2 \approx 10^3 \text{ Pa} \). The open-gap configuration is a high-energy equilibrium state. If the gap is closed (Figure 2b) a new equilibrium state is created, characterized by a new depletion region. The net energy released by gap closure, between Figure 2a and 2b, can be shown to be:

\[
\Delta E \approx \frac{16 \varepsilon_o^2}{qN^2} \left\{ \frac{kT}{q} \ln \left[ \frac{N}{n_i} \right] \right\}^3 \left\{ \frac{1}{x_g} - \frac{2}{3} \eta \left( \frac{2 \eta \varepsilon_o}{N q} \left( \frac{kT}{q} \right) \ln \left[ \frac{N}{n_i} \right] \right)^{-1/2} \right\},
\]
where $\eta$ is the dielectric constant ($\eta_{\text{silicon}} = 11 - 12$). In principle, this energy difference $\Delta \mathcal{E}$ can be used to perform useful work. A more detailed account of the gap electric field is presented elsewhere [78,84].

This structure constitutes an intrinsically-biased capacitor since no external voltage source is used, this in contradistinction to the traditional externally-biased capacitor in Figure 1. In principle, the intrinsic capacitor can store electrostatic energy indefinitely, purely by thermal means.

3.2.2 Work Cycle. Now consider the following thermodynamic work cycle (Figure 3), appropriate to closing and then reopening the vacuum gap in Figure 2c. (Compare this with Figure 1c.) The ordinate is the electrostatic force $F_{es}$ on the gap face (area $A$), which is:

$$F_{es} = \frac{\epsilon_0}{2} E^2 A = \frac{\epsilon_0}{2} \left[ \frac{V_{bi}}{x} \right]^2 A.$$

The abscissa is the instantaneous width of the vacuum gap $x$. The cycle proceeds counterclockwise: $a \rightarrow b \rightarrow c \rightarrow a$. The cycle in Figure 3 runs as follows:

(a $\rightarrow$ b): The gap closes quasi-statically, performing work, $\int_{x}^{0} F_{es} \, dx$. Since $V_{bi}$ across the vacuum gap is fixed by the left depletion region (J-I), in principle, the electric field, energy density and total energy increase at J-II during gap closure. In theory, the work integral diverges, but in practice it does not because the gap electric field saturates at a finite value ($e.g., E_{\text{max}} \approx 2 \times 10^7$ V/m for silicon which, as expected, is below its dielectric strength). Quantum tunneling of charge across the gap might occur in the last several angstroms of the stroke, allowing some pre-contact discharge.

(b $\rightarrow$ c): The apposing gap faces of J-II make contact ($x = 0$), precipitating rapid and irreversible electron-hole recombination at the gap faces, formation of a depletion region in the bulk like the one at J-I, and elimination of the gap electric field. The diode

---

**Fig. 2.** Electrical solid state equilibria for p-n diode, (a) High-energy state with open p-n junction, (b) Low-energy state with closed p-n junction, (c) Auto-switching resonant electromechanical oscillator (hammer-anvil). Slashed areas indicate depletion regions.
system falls from its high-energy equilibrium (Figure 2a) to its low-energy equilibrium (Figure 2b).

(c → a): The route taken to complete the cycle determines the net work output. If the gap opening is performed quasi-statically, then the cycle retraces in reverse its original path exactly (c → b → a), in which case the area enclosed by the cycle is zero and no work is performed. (This is also the path taken if the semiconductor were replaced with perfect conductors.) Along other paths (i–iv) non-zero area is enclosed and, thus,
net work is performed. Of these, only path (iv) is unrealistic since it indicates no recharging during gap opening, hence presumes instantaneous opening. Path (iii) is physically realistic for a semiconducting hammer-anvil and corresponds to near maximum work extraction.

It is emphasized that only with semiconductors can this cycle realistically have positive gain; neither perfect conductors (e.g., metals) nor nonconductors will work. Perfect conductors discharge and recharge instantaneously, making a cycle of zero area, while nonconductors might allow a single discharge, but would take an infinite time to recharge, therefore they are not feasible for a continuous work cycle. Semiconductors, on the other hand, can spontaneously develop the necessary built-in potential, discharge, and then thermally recharge in a sufficiently long time to allow for mechanical motion of the faces (cases (i–iii) in Figure 3).

Second law universality is theoretically challenged by the steady-state operation of this device. This can be shown by pitting the first law of thermodynamics against the second. Let the universe consist of the hammer-anvil (plus its work extraction apparatus), plus a surrounding heat bath. Let the system settle into a thermodynamic steady-state (actually a steady-state nonequilibrium). If the hammer-anvil performs steady-state work, but remains in a thermodynamic steady-state (neither heating nor cooling), then the energy of the work it performs must come from somewhere other than itself. Since the first law demands that energy (heat + work) must be conserved universally, this leaves the heat bath as the source of energy. But a heat bath does not perform work; it provides only heat. Thus, by logical exclusion, the work performed by the hammer-anvil operating in its thermodynamic steady-state must come solely from the heat bath. This violates the Kelvin-Planck formulation of the second law. This device can also be shown to challenge any other standard formulation of the second law.

3.2.3 Solid State Hammer and Anvil. The rigid silicon horseshoe (Figure 2c) is not practical for work extraction, but realistic physical embodiments have been proposed [81,87] and laboratory studies of them are in progress.

Consider the electromechanical device depicted in Figure 4, a solid state torsional version of the hammer-anvil discussed previously. The top piece (all p-type semiconductor), which consists of an oscillator mass, two flexible torsional springs and the surrounding ledges, rests on an n-type base, thus forming a p-n diode. Comparing Figures 1c and 2c, the top-center p-semiconductor mass acts as the hammer in Figure 4; likewise, the lower stationary n-semiconductor in Figure 4 acts as the lower, fixed anvil. The spring is replaced by two torsional fibers.

For long, thin fibers (\(w_f, t_f \ll l_f\)) and for small angular displacements (\(\theta \ll 1\)), a linear torsional constant can be defined: \(\kappa = (3/10)(S/l_f)(w_f^3 t_f^3)/(w_f^2 + t_f^2)\) where \(l_f, w_f, t_f\) are length, width and thickness of the torsion fiber, and \(S\) is its shear modulus (\(S_{\text{silicon}} = 7.9 \times 10^{10} \text{ N/m}^2\)). The device’s depletion region imposes its built-in voltage across the vacuum gap between the p-type hammer and the n-type anvil base, as in Figure 2c. The mechanical frequency of the torsion
oscillator is given by $\omega = \sqrt{\frac{\kappa}{I}} = 2\pi f$ where $I = (1/12)ML^2$ is the moment of inertia of the oscillating mass about its axis of rotation, with $M$ its mass and $L$ its total length. The fiber must be given an initial twist to kick-start the oscillation.

The electric field in the hammer-anvil gap provides negative electrostatic pressure that drives and sustains the mechanical oscillations. It has been shown elsewhere [2,87] that, for sustained oscillation, three criteria must be met. (These criteria are also met by the macroscopic electromechanical resonator in Figure 1.)

(I) The electrical and mechanical time constants must be comparable to achieve electromechanical resonance ($\tau_e \sim \tau_m$).

(II) The hammer’s mechanical energy gain per cycle must equal or exceed its mechanical dissipation per cycle, otherwise the oscillation will damp out.

(III) The torque retracting the hammer after contact with the anvil must exceed the maximum torque exerted by van der Waals and electrostatic attractive forces, otherwise the hammer will stick to the anvil.

For Criterion I ($\tau_e \sim \tau_m$), the electrical time constant $\tau_e$ for the hammer-anvil p-n junction should be on the order of the inverse-slew rate of a comparably-sized p-n diode. This is typically $10^{-6} - 10^{-8}$ sec for micron-size silicon diodes, corresponding to frequencies of $f_e \sim 1$–100 MHz. Mechanical resonant
frequencies for cantilevers in excess of $10^9$ Hz have been achieved. Since $f_m$ can be made comparable to $f_e$, the first criterion can be met. Alternatively, a resistor can be inserted between the hammer and anvil to establish a resistor-capacitor (RC) circuit. In this case a physically larger, lower frequency oscillator is possible, still satisfying $\tau_e \sim \tau_m$.

Regarding Criterion II, NEMS-MEMS oscillators have documented Qs as high as $Q \sim 10^5$ in vacuum [94]. This implies that a small energy gain per cycle (e.g., $\sim 10^{-5}$ total mechanical energy) should be sufficient to sustain oscillation. Numerical simulations verify that this condition also can be met for the torsional oscillator.

Finally, regarding Criterion III, it has been shown [2,84] that with sufficiently stiff cantilevers, the van der Waals and electrostatic forces can be overcome, while satisfying the other two criteria.

In all, detailed analysis indicates that high-Q MEMS-NEMS torsion oscillators and linear cantilevers, in principle, can achieve self-sustained resonant oscillation, utilizing intrinsic bias from a p-n depletion region. Simulations indicate a broad, physically realistic and experimentally accessible parameter space in which the torsional hammer-anvil should be viable that is squarely within the current art of MEMS fabrication.

The torsion oscillator we will investigate experimentally (Figure 4) will be relatively slow ($f \sim 10^4$ Hz) and large ($L \sim 10^{-2}$ m); nonetheless, its hammer-anvil gap distance must be minute ($x_{gap} \sim 10^{-7}$ m), thus requiring extremely fine mechanical tolerances ($x_{gap}/L \approx 10^{-5}$). Phosphorus and boron doping will be in the $N@10^{21}$ m$^{-3}$ range, resulting in 0.6 V built-in voltages and electric fields near that of silicon’s dielectric strength.

3.2.4 Practical Considerations. The useful work derived from second law devices (SLDs) can take many forms; proposals include mechanical, chemical, gaseous pressure gradients, osmotic pressure; temperature gradients, light and electrical. The most immediately convenient will probably be electrical, although chemical possibilities might also be competitive [85,87].

For the electrostatically driven hammer anvil devices, the theoretical maximum power density ($P$) should scale as $P \sim \rho_e f$, where $\rho_e$ is the electrostatic energy density ($\rho_e = (\epsilon/2)E^2$ (J/m$^3$)) and $f$ (Hz) is the oscillation frequency. Taking $E$ to be the dielectric strength of silicon ($3 \times 10^7$ V/m) and the frequency to be the maximum of state-of-the-art NEMS resonators ($f \sim 5$ GHz), one finds that the maximum theoretical power density for electromechanical SLDs should be a staggering $2 \times 10^{13}$ Wm$^{-3}$. In other words, one cubic meter of SLDs could, in theory, supply civilization’s power requirements. Realistically, however, one must consider: (a) the device’s mechanical support volume not devoted to vacuum electric fields; and (b) heat transfer into the SLDs, both of which will greatly reduce this maximum theoretical power density. With regard to (a), based on advanced SLD designs, the actual fractional volume of an SLD array devoted to electric fields will likely be $\leq 10^{-2}$, thus lowering $P$ to $\leq 500$ GWm$^{-3}$. 

472 D. P. Sheehan
Consideration (b) is even more limiting. To review, there are three types of
heat transfer: convection, conduction, and radiation. Under their anticipated
operating conditions it can be shown that convection will probably dominate
heat transfer for SLDs. Consider fluid flow with velocity $v$ through one face of
a matrix of SLDs and let the temperature drop be $\Delta T$ between the fluid’s
entrance and exit. The net output power flux density ($\text{Wm}^{-2}$) for the matrix is
given by

$$F = \rho v C \Delta T - \frac{1}{2} \rho v^3,$$

(6)

where $\rho$ is the mass density of the fluid and $C$ is its specific heat. The first term
is the SLD output power derived from the conversion of heat (see cycle in Figure
3) and the second is the kinetic energy vested in the fluid, assumed to
be supplied by the SLDs. The condition for positive flux density is that:
$v \leq \sqrt{2C \Delta T}$. The maximum flux density with respect to velocity ($\partial F/\partial v = 0$)
occurs at $v_{\text{max}} = \sqrt{(2/3)C \Delta T}$. For water with $\Delta T = 10$ K, this is $v_{\text{max}} = 160$ m/s,
and for air with $\Delta T = 100$ K, $v_{\text{max}} = 230$ m/s.

For a water-powered SLD matrix with a $1 \text{ m}^2$ intake ($v = v_{\text{max}} = 160$ m/s, $\Delta T = 10$K, $C = 4.2 \times 10^3 \text{ J[kgK]}^{-1}$), Eq. (6) predicts $P \cong 5 \times 10^9 \text{ W}$, the equivalent
output of several modern nuclear power plants. As a convecting fluid, air has
advantages over water in being easier to handle and more ubiquitous, and it also
allows greater temperature variations $\Delta T$; on the other hand, it has lower density
and specific heat ($C_{\text{air}}/C_{\text{water}} \approx 0.2$). For air, letting $\Delta T = 100$ K and $v = v_{\text{max}} = 230$ m/s, Eq. (6) predicts $P = 2 \times 10^7 \text{ Wm}^{-2}$. Notice that the power flux density
for the SLD can be several orders of magnitude greater than those possible by
wind or solar, which are fundamentally limited by wind speed or the solar
constant, as well as by their availability during the hours of the day and by the
season.

To be viable for large-scale power production, microscopic SLDs, like the
torsional hammer-anvil, must efficiently transduce their energy to macroscopic
scales. Analysis indicates that massive series-parallel arrays operating in unison
should be possible. The detailed engineering and economics are beyond the
scope of this paper, however, several transduction mechanisms suggest
themselves:

**Piezoelectricity:** For SLDs with flexing mechanical structures, like the double-
cantilever or torsional versions of the hammer-anvil, piezoelectric elements might be
built into the device, or perhaps the entire device might be fabricated from a piezoelectric
material, with output electrodes situated at locations of maximum mechanical strain.

**Thermoacoustics:** Here, SLDs’ mechanical vibrations would generate sound
waves in a background gas, by which thermoacoustic heating and cooling could be
used to run a heat engine. Thermoacoustic motors and refrigerators are commercially
available.

**Faraday Induction:** In this scenario, microscopic magnets would be affixed to the
hammer. Via Faraday induction, an ac electromotive force (emf) is induced in a nearby
coil via time-changing magnetic flux coupled through it. While this scheme would
require substantial ancillary magnetic and electrical hardware – especially compared with
the piezoelectric scenario for which the mechanical and electrical elements could be the same structure – the high frequencies typical of NEMS are advantageous since induced emf is proportional to the time rate of change of magnetic flux.

4 Ramifications of Second Law Violation

The second law is so ingrained in human experience that its violation should lead to unexpected and counterintuitive results. Consider SLDs at home. For example, a household SLD power generator might consist of a tube about the size of a coffee can. On one end could be a fan to draw the air through the tube over a series of baffles – like a radiator – packed with millions of SLDs. The SLDs convert atmospheric heat into electricity, some of which powers the fan, but the vast majority of which is available to run household appliances and utilities. For modest, self-generated air flow (5 m/s) and modest heat recovery ($\Delta T \approx 20$ K), Eq. (6) predicts that this coffee-can-sized generator should produce between 1 and 2 kilowatts of electricity nonstop – roughly enough to power an average US household.

On the road and in the sky, SLD automobiles and planes could run on air, taking in air at the front, passing it through internal SLD baffles, converting heat into electricity for electric motors, and finally exhausting colder air out the back. They would consume no fuel and produce no pollution, aside from trailing plumes of cold air. In principle, almost any technological device could be redesigned to be energy self-reliant. Further, since nearly all electricity is eventually degraded into heat, it can be recycled again and again. Homes, businesses and industries could become energy self-sufficient. The power grid would become superfluous.

Thermal energy should be superior to almost any other energy resource. First, the terrestrial thermal energy reserves in the atmosphere, ocean and crust alone exceed by orders of magnitude all presently exploited energy reserves combined (coal, oil, gas, uranium) and are exceeded only by the potential energy of thermonuclear fusion of light elements, a prospect still decades away from commercial viability. Thermal energy is also ubiquitous so SLDs should operate anywhere, anytime. Second, unlike any other energy source, it should be completely recyclable and renewable; in this sense, thermal energy is effectively limitless.

Third, thermal energy is clean, green energy. Aside from the products of their manufacturing, SLDs should create no chemical wastes and no pollution since they consume no material fuel, only heat. Fourth, in principle, their power flux densities are compatible with virtually any modern mechanical or electronic device, from lightbulb to locomotive. (Only for the most power-intensive systems, e.g., rockets, would they be infeasible.)

If they prove economically competitive, SLDs could precipitate a shift in the world’s energy paradigm. Unlike traditional energy sources, thermal energy does not require discovery and extraction since it is found in abundance everywhere.
Large generation plants or transmission infrastructure would be unnecessary since heat-to-electricity conversion could be accomplished locally. Energy storage (e.g., batteries, flywheels) would be unnecessary for all but the highest power applications. Furthermore, unlike other renewable energy sources (e.g., solar, hydroelectric, or wind) SLD can support very high power densities. And, thermal energy is not simply renewable, it is perpetually recyclable.

The short-term economic and political impacts of cheap and abundant SLDs could be dire. Vast personal, corporate and national fortunes in mineral wealth would be wiped out. Middle Eastern energy empires would collapse as oil and gas became nearly worthless, their use restricted largely to plastics, fertilizers, lubricants and asphalt. The energy exploration, extraction and delivery industries would implode; gas and oil wells, coal mines, tanker fleets and gas stations would be idled; pipelines, refineries, power plants and power grids would be scavenged for spare parts. The economic clout and political leverage derived from energy resources would largely vanish, restructuring economic and political landscapes across the globe, for instance, those between gas-rich Russia and energy-poor Europe.

After these shocks, the economic, political and ecological benefits of SLDs could be profound and salutary. The release of the world economy from the constraints of limited and expensive energy should be invigorating. Energy-shackled economies, like India and China, could flourish. Cheap energy should reduce the cost of virtually all products. The costs of recycling material resources like metals, plastics and paper should also be reduced.

Inexpensive energy should help unlock other critical resources, for instance, possibly allowing widespread desalination of seawater and its pumping over long distances to thirsty lands and populations. Most recently, the world has experienced a tight coupling between energy and food markets, resulting in global shortages in basic foodstuffs like rice, wheat and corn, affecting hundreds of millions of humans. If energy can be made sufficiently inexpensive, these two markets should decouple, thereby stabilizing food supplies. (Of course, cheap energy should also reduce the cost of producing and transporting food, as well.)

Eliminating these energy-related shortages should, in turn, reduce political and economic tensions leading to war and civil strife. The necessities for military interventions to control energy reserves would end; armies could come home. Politically and militarily, there would be one less critical resource to fight over.

Pollution from fossil fuel burning and nuclear fission could be eliminated. Land scarred and ecosystems maimed by civilization’s thirst for energy could be left to heal. (It has been suggested that greenhouse gases might be scrubbed from the atmosphere, but these proposals are energy-intensive. SLVs might be employed here since scrubbing would now be an energy-neutral proposition.4)

Of course, the virtues of this technology could become a vice if taken to extremes. Abundant, inexpensive energy would lift a fundamental constraint on humankind’s exploitation of Nature. Mining, fishing and logging could be conducted non-stop, further stripping the world’s natural resources and
accelerating environmental destruction. Wars could be conducted by tanks, ships and planes without need of refueling. The fault of these dangers rests, of course, not in the technology but in ourselves.

At present the immediate specters of global food shortages, climate change, pollution, ecosystem destruction and species extinctions, driven largely by humanity’s thirst for energy, seem to require rapid and radical solutions. If the second law can be violated in an economically and ecologically viable manner, then I believe it should be pursued vigorously.

5 Outlook

The energy paradigm under which civilization has traditionally operated but which now threatens the environment and civil society – that free-energy sources are absolutely required – is now being challenged. The experiment described in this paper is merely a test of principle. If successful, hopefully it will inspire more efficient, higher power density and economical versions.

Certainly, any experimental violation of the second law would fundamentally alter the landscape of physics and the pure sciences, but its potential for positive societal change is perhaps even more profound.

In the next several years, as laboratory SLDs are tested at USD and other institutions, the heat will be turned up on the second law. If successful, they promise to change humankind’s relationship to energy perhaps as fundamentally as it was by the taming of fire 400,000 years ago.

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References


Notes

1 Čapek and Sheehan list 21 different versions in [2], without exhausting the possibilities.

2 When he was younger the author argued with his mother that he shouldn’t clean his room so as to spare the universe further disorganization. He was unsuccessful. Perhaps he should have appealed to his father, a physical chemist.

3 This hammer-anvil is a relative of well-studied NEMS and MEMS (Micro-Electro-Mechanical Systems/Nano-Electro-Mechanical Systems) cantilever oscillators. Cantilever oscillators have many proven and potential applications, including as accelerometers, motors, clocks, sensors (e.g., temperature, pressure, electronic charge, magnetic fields, environmental contaminants, microbes), beam steerers, choppers, and modulators, computing elements and switches [91–93]. Cantilevers are usually driven by AC electrical signals whose frequencies are commensurate with their mechanical oscillation frequencies, but DC signals can also drive them.

4 In the near term, SLDs might actually exacerbate global warming by replacing energy devices whose emitted particulates cause \( \text{solar dimming} \) and the \( \text{Twomey indirect effect} \), both of which probably offset the warming effects of greenhouse gases.
Effects of Distant Intention on Water Crystal Formation: A Triple-Blind Replication

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Abstract—An experiment tested the hypothesis that water exposed to distant intentions affects the aesthetic rating of ice crystals formed from that water. Over three days, 1,900 people in Austria and Germany focused their intentions towards water samples located inside an electromagnetically shielded room in California. Water samples located near the target water, but unknown to the people providing intentions, acted as “proximal” controls. Other samples located outside the shielded room acted as distant controls.

Ice drops formed from samples of water in the different treatment conditions were photographed by a technician, each image was assessed for aesthetic beauty by over 2,500 independent judges, and the resulting data were analyzed, all by individuals blind with respect to the underlying treatment conditions.

Results suggested that crystal images in the intentionally treated condition were rated as aesthetically more beautiful than proximal control crystals (p = 0.03, one-tailed). This outcome replicates the results of an earlier pilot test.

Keywords: intention—water—consciousness

Introduction

Can one person’s intention affect another person’s health from a distance? A growing number of clinical studies have investigated this question. Some of them provide positive evidence1, others do not2. To help study this question under more stringent laboratory controls, investigators have also explored whether one person’s intention can affect another person’s nervous system from a distance3. From those studies the evidence is clearer. From a meta-analytic perspective the original question can be answered with a tentative yes4. Tentative, because while the evidence is statistically significant and repeatable, the observed effects are small in magnitude, nontrivial to replicate, and theoretical explanations remain speculative.5

Because of the complexities associated with studying human health and physiological responses, still other investigators have aimed towards further simplification by asking whether intention affects properties of water. This remains relevant to the question about health because the human body consists
of 70% to 90% water, depending on age. Evidence from those studies supports the hypothesis that intention affects properties of water, but like many of the empirical studies in this domain, most of the experimental reports have appeared in specialty journals and have gone unnoticed by most medical researchers.

One exception that has elevated the question about intention and water from the obscure to the infamous is the claim that water exposed to or “treated” by positive intentions results in frozen water crystals that are aesthetically more pleasing than similar crystals formed from “untreated” water. In an earlier pilot experiment we tested this claim under double-blind conditions and found evidence in favor of the “intentional hypothesis” (p = 0.001). The present study was a replication attempt conducted under triple-blind conditions.

Method

Water Sample Preparation

In preparation for the experiment, the first author (D.R.) purchased six plastic bottles of Fiji brand commercial bottled water, the same type of water used in the pilot study. D.R. randomly assigned (using a tossed die) the bottles with labels A through F, and then the second author (N.L.) took the bottles to the laboratory and randomly selected (again with a tossed die) two bottles as the treated samples, two as “proximal” controls, and two as “distant” controls.

N.L. noted the resulting assignments and placed two copies in separate envelopes which remained sealed until after the analyses were completed. She retained one envelope and the other was stored in D.R.’s desk. Then she entered a double steel-walled, electromagnetically shielded room (Lindgren/ETS, Cedar Park, Texas, Series 81 Solid Cell chamber) at the Institute of Noetic Sciences (IONS) in Petaluma, California, where she placed the two treatment bottles on top of a small table and the two proximal control bottles under that table. The shielded room acted as a convenient, limited-access location in which to leave the bottles during the experiment.

N.L. then took a digital photo of the treatment and proximal control bottles, and placed the two remaining bottles (distant controls) in a Styrofoam box and stored them on top of a bookshelf on another floor of the laboratory building. D.R. edited the digital photo of the bottles in the shielded chamber to reveal just the two treatment bottles, then emailed the photo to M.E. and T.K. They used this photo as a visual aid for three groups that would later direct their intentions towards those bottles.

Throughout the experimental setup, N.L. was instructed to handle each of the water bottles in the same way, and to hold them about the same length of time. During the intention periods all bottles remained in their originally placed locations and were not disturbed. The third and fourth authors (M.E. and T.K.) knew in advance that there would be treated and distant control bottles in this
study, but they were not informed about the existence of the proximal controls until after all distant intention treatments had ended.

The comparison of principal interest in this study was the average (blindly rated) aesthetic differences of frozen water crystals obtained using the treated vs. proximal control samples. This is because those two conditions were located close to each other in the same environment, and because the proximal control was not influenced by M.E. or T.K.’s prior knowledge of its existence. That is, to take seriously the hypothesis that intention plays a role in this experiment, we felt it was necessary to constrain who knew about the potential targets of intentional influence. By analogy with a quantum optics system, in which the knowledge one has of the path that photons take through a double-slit apparatus influences the behavior of those photons, we speculated that knowledge of the experimental conditions in this test might influence what was ultimately measured. Thus, to provide some control over the distant intentions in this study we required a comparison condition that was unknown to M.E., to T.K., or to the groups of “distant intenders.” This was provided by the proximate control. The distant control was retained in this study primarily because we used a similar control in the previous study, so M.E. and T.K. would have expected it.

*Intentional Treatments*

On May 20, 2006, in Graz, Austria, M.E. led a group of about a thousand people in a prayer of gratitude directed towards the water in the IONS laboratory, some 5,700 miles away. M.E. showed the audience where the IONS laboratory was located in relationship to Graz through a sequence of images from the Google Earth global mapping application. Then he showed the digital photo of the treatment bottles inside the shielded chamber with the words of an intentional “prayer for water” overlaid on the photo. After explaining the photo and purpose of the experiment, M.E. led the group in speaking aloud the words of the prayer for about five minutes. M.E. led a second group of 450 people in a similar exercise on May 23, 2006, from Nuremberg, Germany, and then a third group of 500 people from Munich, Germany on May 24, 2006.

The day after the third group sent their intentions, N.L. retrieved all six bottles from the laboratory. Then she and D.R. (who remained blind to the bottles’ conditions) wrapped the bottles in separate sheets of aluminum foil and placed all six bottles in a box. That package was placed inside a larger box, cushioned with foam peanuts, and mailed to M.E.’s laboratory in Tokyo. At this point D.R. informed M.E. and T.K. about the existence of the proximal controls. Like D.R., M.E. and T.K. remained blind to the conditions of the six bottles throughout the crystal formation and statistical analysis phases. N.L. was not involved in the study again until after all data had been collected and analyzed, whereupon she broke the blinding code.
Crystal Analysis

Upon receiving the six bottles, T.K. blindly examined water samples from each bottle according to the following procedures:

1) From each bottle, a drop (approximately 0.5 ml) of water was placed into each of 50 Petri dishes, and a lid identifying the bottle’s randomly assigned letter (A–F) was placed on each dish. Thus there were 50 water drops tested from each bottle.

2) Each dish was then placed on a tray in a random position in a freezer maintained at −25 to −30 degrees C for a minimum of three hours. The random placements helped to ensure that potential temperature differences within the freezer would be randomized among the dishes.

3) T.K. later removed the dishes from the freezer, and in a walk-in refrigerator (maintained at −5 degrees C) he took a photo of the apex of each resulting ice drop using a stereo optical microscope at either 100× or 200×, depending on the presence and size of a crystal. Based on the results of the earlier experiment, some water drops were not expected to produce any discernable crystals.

4) All 300 resulting photographs, from all six bottles, were then emailed to D.R., each identified with a bottle assignment letter A–F, and a within-bottle sample number from 1 to 50.

Aesthetic Assessments and Analysis

To provide blind, subjective assessments of the aesthetic beauty of the water crystals, D.R. created a website to allow individuals to judge each crystal photograph on two factors. The factor of principal interest was beauty, meaning that the picture was aesthetically pleasing in some way. A second, exploratory factor was interest, meaning that the picture was notable in some way. In both cases the rating choices ranged over a seven-point scale, from “not” to “very,” e.g. “not beautiful to “very beautiful.” Each participating judge viewed and rated 50 photos, randomly selected out of the 300 available photos, and presented one at a time in a newly randomized order.

We asked judges to rate both beauty and interest because prior research on aesthetic judgments, in realms ranging from fine art, to faces, to commercial product design, suggests that numerous factors influence aesthetic preference. They include figural goodness, figure-ground contrast, stimulus repetition, symmetry, and prototypicality. Such factors suggest that asking for a single rating of aesthetic beauty may not be sufficient to capture individuals’ full assessments of the photographs of frozen water. Whether the factor of interest was the best possible variable to use for this purpose was unknown, and was thus considered exploratory.

To test the hypothesis that the crystals in the intentional condition would be rated as more beautiful on average than the same crystals in the proximal
control condition, a mixed, hierarchically nested variance components analysis of variance was employed\textsuperscript{13}, where treatment condition was a fixed effect, and the two bottles used per condition and 50 crystals sampled per bottle were both random effects (see Figure 1).

**Image Contrast Analysis**

In addition to the subjective assessments, we also used image processing software (Matlab 7.0.1 Image Processing Toolbox, The Mathworks, Inc., Natick, Massachusetts) to generate an objective score of image “contrast” for each of the 300 photographs. Contrast in this context refers to the proportion of black vs. white in an image. This was a useful metric because when crystals appear on the apex of frozen water drops, they tend to rise up beyond the surface of the drop, partially because ice expands when it freezes and also because water crystals grow out like branches on a tree. When a microphotograph is taken of such crystals, the narrow field of focus tends to separate the white-appearing crystal from the darker background, thus increasing the image’s overall contrast. When no crystal is present, the surface is flatter and the image has a more uniformly gray appearance (see Figure 2). We predicted that these contrast values would be

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**Fig. 1.** The intentional hypothesis was tested using a hierarchically nested variance components analysis, with treatment condition as a fixed effect, and bottles within condition and samples within bottles as random effects.
Fig. 2. Example of images with high (top [a]) and low (bottom [b]) contrast. The left image shows a crystal formation, the right does not.
correlated with the average ratings of aesthetic beauty, and thus that contrast in the treatment condition would be higher than in the proximal control condition.

**Results**

**Analysis of Crystals**

Subjective assessment ratings were collected online for one month. During that time, 2,579 people had each assessed 50 randomly selected images, for a total of 128,950 assessments and an average of 430 beauty and interest ratings for each of the 300 images. These average ratings, in the form of point values, formed the dependent variables in the subsequent analyses. Assessment data from individuals who started to evaluate images but stopped before finishing all 50 were excluded from further analysis.

Figure 3 shows the average assessments and 95% confidence intervals for average ratings of aesthetic beauty for each image. Images 1–100 correspond to the distant control condition, 101–200 to the proximal control condition, and 201–300 to the treated condition. The grand average rating for beauty was 1.77 (on a scale of 0–6), thus most of the images were not regarded as particularly beautiful. Of the 300 images, 270 obtained average beauty ratings greater than 1.0. This subset of images was examined in a secondary analysis because it was more likely to contain crystalline shapes, which was of main interest in this experiment. That is, the intentional hypothesis was not that more crystals would form due to intention, but rather that crystals that did form would appear to be more beautiful in the treatment condition vs. the proximal control condition.
The grand average rating for interest was 2.51. The correlation between average ratings of beauty vs. interest was highly positive \((r = 0.86, t = 29.1, N = 300, p = 0)\). The correlation between beauty and normalized image contrast was also positive \((r = 0.30, t = 5.35, N = 300, p = 8.97 \times 10^{-8})\).^{14}

**Analysis 1: Aesthetic Beauty**

The treatment condition resulted in a significant, albeit weak main effect \((p = 0.03; \text{Table 1; Figure 4})\). When interest was used as a covariate of beauty, the main effect for condition was no longer significant \((F[2,293] = 3.03, p = 0.20)\). The latter is not too surprising given the strong correlation between beauty and interest variables. For the subset of 270 trials with beauty > 1.0, the results remained significant \((p = 0.04; \text{Table 2; Figure 4})\).

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The grand average rating for interest was 2.51. The correlation between average ratings of beauty vs. interest was highly positive \((r = 0.86, t = 29.1, N = 300, p = 0)\). The correlation between beauty and normalized image contrast was also positive \((r = 0.30, t = 5.35, N = 300, p = 8.97 \times 10^{-8})\).^{14}

**Analysis 1: Aesthetic Beauty**

The treatment condition resulted in a significant, albeit weak main effect \((p = 0.03; \text{Table 1; Figure 4})\). When interest was used as a covariate of beauty, the main effect for condition was no longer significant \((F[2,293] = 3.03, p = 0.20)\). The latter is not too surprising given the strong correlation between beauty and interest variables. For the subset of 270 trials with beauty > 1.0, the results remained significant \((p = 0.04; \text{Table 2; Figure 4})\).
The pairwise comparison of principal interest—treated vs. proximal controls—supported the intentional hypothesis for all trials ($t_{198} = 1.67$, $p = 0.05$, one-tailed). The same comparison was somewhat stronger for the subset of trials where beauty > 1 ($t_{168} = 2.32$, $p = 0.01$, one-tailed). The distant control condition resulted in slightly more beautiful crystals than the intentional condition when considering all trials ($t_{198} = 0.77$), and slightly less beautiful for the subset where beauty > 1 ($t_{168} = -0.14$).

**Analysis 2: Image Contrast**

Normalized image contrast scores resulted in a nonsignificant main effect across the three conditions for all trials ($p = 0.25$; Tables 3 & 4; Figure 5), but a pairwise comparison between the treated vs. proximal controls showed suggestive effects for both all trials, $t(198) = 1.85$ ($p = 0.03$, one-tailed), and for the subset of trials where beauty > 1, $t(168) = 1.55$ ($p = 0.06$, one-tailed). The distant control comparisons were nearly identical to the proximal controls.

**Discussion**

This experiment found a modestly significant difference ($p = 0.03$) in blind ratings of subjective aesthetic beauty of crystals formed from water samples “exposed” to distant intentions vs. proximal and distant control samples. The comparison of main interest confirmed, weakly, that the treated water crystals were rated as more beautiful, on average, than the proximal controls ($p = 0.05$, one-tailed). A similar analysis using objective ratings of image contrast was not significant when comparing across the three conditions, but a planned

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comparison between the treated and proximal controls again showed a modest difference in favor of the intentional hypothesis ($p = 0.03$).

It should be noted that the distant controls were judged as being slightly (nonsignificantly) more beautiful than the treated samples when considering all trials, but nevertheless for the comparison of main interest (treated vs. proximal controls) the difference was in alignment with the previously reported pilot test. The present experiment extended the earlier test design by including five new features to address potential alternative explanations. They included (a) using a proximal control condition to eliminate environmental differences between the treated and control samples, (b) placing Petri dishes in random positions in the deep freezer to average out any systematic temperature differences in the freezer, (c) employing a triple-blind design to control for expectation biases on the part of the photographer, judges, and data analyst, (d) including image processing to objectively characterize the images, and (e) analyzing all images rather than just those judged by the photographer to contain crystals.

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**Fig. 5.** Average normalized values for contrast for all 300 images, and for the subset of 270 images where average rating for beauty $> 1.0$, with one standard error bars.
These design elements excluded obvious environmental differences and conventional subjective biases as plausible explanations for the observed results, and the combined results of the two experiments appear to exclude chance as an explanation (unweighted Stouffer $Z = 3.34$, $p = 0.0004$). At first blush this seems to imply that distant intention influenced water crystallization properties in accordance with the hypothesis. However, as in any experiment involving intention, the intentions of the investigators cannot be cleanly isolated from those of the nominal participants and this in turn constrains how one should properly interpret the results.

In addition, there were many uncontrolled degrees of freedom in this experiment which may have allowed “unintended intentional” effects to creep in. They all involve human decisions, e.g. selecting six specific bottles of water from a huge population of available bottles, randomly assigning those bottles to three conditions, selecting and preparing the water drops, placing the water drop samples inside the freezer, searching for and photographing ice crystals on the frozen water drops at different magnification levels, choosing one of a large possible set of image processing algorithms to provide an objective measure of image contrast, and so on. The challenge for future tests of this kind is to find ways of reducing these degrees of freedom without imposing such severe constraints on the design that the effect of interest is either quenched out of existence, or that the experiment becomes so expensive to conduct that it doesn’t take place at all.

Notes


10 The analysis reported here differs from the original plan in response to a reviewer’s comments, so an argument could be made that this study, as reported, is actually double-blind.


13 Statistica 7.0, StatSoft, Tulsa, OK, Variance components and mixed model ANOVA/ANCOVA analysis, in a hierarchically nested design.

14 Normalized contrast values were formed as $z = (c - m)/s$, where $c$ was the raw contrast value for a given image, $m$ was the mean of all raw contrast values, and $s$ the standard deviation of all raw contrast values.
Changes in Physical Strength During Nutritional Testing

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Abstract—The ability of a patient to effectively stabilize (lock) and thus prevent rotation of his/her joint against forces applied by one of the authors (C.F.B.) appears to vary with the nature of the substance with which the patient is in proximity. C.F.B. believes the patient’s nutritional needs can be evaluated by determining which of several substances is most effective in strengthening the weakened patient. The purpose of this study is to determine why such changes in physical strength occur. Bottles of pills were placed one after another on the supine patient’s abdomen and chart recordings of force vs. time were made for the most effective strengthening agent and for an inactive placebo substance as the patient attempted to resist shoulder flexion. In the first set of trials, the tester was unblinded and the patient blinded. These same substances next were placed in opaque cloth bags so that both the tester and the patient were blinded and charted again. Finally, recordings of force vs. time again were made for the strengthening agent and the placebo substance, with the tester unblinded and the patient blinded. The tester, C.F.B., favored a signature hypothesis in which some electromagnetic emanation from the beneficial substance made the patient strong. Another author, P.R.B., favored a mental influence hypothesis, whereby C.F.B. made the patients strong through a subconscious mental influence reflecting his belief that a particular substance would be beneficial. When C.F.B. was unblinded, the patients were weak (yielded) in every placebo trial and were strong (locked the joint) in every trial with the active agent. When C.F.B. was blinded, patients again were weak with one compound and strong with the other; but C.F.B. correctly identified the active and placebo substances in only 8 of 27 patients. These findings support the mental influence hypothesis. Neither C.F.B. nor the patients were consciously aware of any mental interaction. When strong, individual patients could hold against peak forces on average 20–90% higher than the forces that caused them to yield when weak.

Keywords: motor function—nutritional testing—mental influence—electromagnetic signature
Manual muscle testing has long been a part of patient evaluation. It is used by physiatrists, orthopedists, neurologists, chiropractors, osteopaths and physical therapists, among others. The patient is positioned so that the muscle or muscle group to be tested causes trunk or limb rotation in a particular direction. The tester then applies a counterforce as the patient makes a maximal effort to produce the desired rotation. Beasley (1956, 1961) recognized patient motivation increased when enough force was applied by the tester to cause the limb to rotate in the direction opposite that intended by the patient. This provides a defined endpoint for both the patient and tester.

Beasley stressed the desirability of making quantitative measurements of the forces produced by the patients. More recent studies (Burgess & Jones, 1997; Jones & Burgess, 1998) showing subjects can produce different amounts of force at a given effort level (effort-force rescaling) underscore the importance of making quantitative force measurements.

In the 1960s, manual muscle testing began to be used in unexpected ways (Diamond, 1979; Goodheart, 1966, 1976; Hawkins, 2002; Walther, 1976). For example, it was reported that simply holding a presumably harmful substance like lead in the hand made almost all subjects weak when any of a number of muscles were tested, whereas subjects were strong when holding a presumably beneficial substance like vitamin C. The tests most commonly were done using the shoulder joint, with the tester pressing down at the wrist on the outstretched arm of the standing patient, the starting position of the arm being parallel to the floor. The patient held the substance with the other hand and whether s/he knew what was being held made no difference to the outcome (Hawkins, 2002).

This led to the hypothesis that muscle testing could be used to assess the nutritional needs of patients, what substances they might be allergic to, the appropriateness of medications taken, etc. If it were true that patients would be weakened by holding substances harmful to them and strengthened by holding beneficial substances, the medical and scientific impact would be considerable.

Using quantitative force measurements, one of the authors (C.F.B.) has verified that the strength of a blinded patient can vary with the nature of the substance with which the patient is in proximity. The goal of this experiment is to determine why such changes in motor function occur.

C.F.B. favors the hypothesis that different substances have distinctive electromagnetic signatures that change the patient’s strength. This proposition is called the signature hypothesis.

Another author (P.R.B.) thinks that C.F.B. changes the patient’s strength with a subconscious mental influence that reflects C.F.B.’s beliefs, desires and expectations. This is called the mental influence hypothesis.

To test these hypotheses, blinded patients who were weak following ‘diagnosis’ of a medical problem were evaluated with different nutritional substances until one was identified that produced a strong response (the active or
strengthening substance). Another substance was identified that did not improve the strength of the already weak patient (the placebo substance).

The signature hypothesis predicts that when C.F.B. is blinded, the active substance will strengthen and the placebo substance will not. The mental influence hypothesis predicts one substance will strengthen and the other will not, but C.F.B. will not be able to reliably identify the active and placebo substances. The mental influence hypothesis makes this prediction because C.F.B. knows in advance he will be given both substances to test on each patient. In order for his signature hypothesis to be verified, he must find that one substance strengthens and the other does not. However, he will not know which is the active substance and which is the placebo.

Methods

Patient Selection

Of the 27 patients who participated in this study, only 2 were not currently being seen at the ChiroMAT Clinic. Any actual or potential patient was considered eligible. They were asked if they would like to be in an experiment designed to test how nutritional substances influenced motor function. In exchange, they would receive a nutritional evaluation and a nutritional supplement free of charge. All agreed and were scheduled to appear on 1 of 2 days (see Procedure).

The participants ranged in age from 17 to 79 years (mean 49.9). Fourteen were women and 13 were men. All but 2 were right-handed. One right-handed patient with a painful right shoulder was tested on the left. All other patients, including the 2 who were left-handed, were tested on the right. The Institutional Review Board of the University of Utah has determined this experiment to be exempt because it does not expose the patients to anything other than regular chiropractic care.

Measuring Strength and Stamina

Manual muscle testing was used in this study to evaluate the nutritional needs of patients. One of the authors (C.F.B.) did all of the evaluations. The patient was supine with the pronated right arm held straight upward at a right angle to the frontal plane. Force was applied to the patient’s wrist by the examiner with a force transducer (Hoggan FET System) interposed between the examiner’s hand and the patient’s wrist. The force was applied at right angles to the same place on the back of the wrist (±1 cm), thus tending to flex the shoulder with constant leverage. The patient was instructed to keep the back of the shoulder flat on the table and the elbow straight and locked while making a maximal effort to resist shoulder flexion. Any attempt by the patient to shift the body out of the test position during the application of force should be seen as an inappropriate response and the test should be discarded. This is a critical component of any muscle test as it indicates recruitment and leads to increased
adaptive strength and faulty results. The patient was alerted in advance of each trial that force was about to be applied.

Muscular strength was measured after a bottle of pills of the nutritional substance to be tested was placed on the patient’s abdomen. The bottle, as it was moved to and rested on the abdomen, was out of the patient’s line of sight; the back of the patient’s head and shoulders always was against the examining table during muscle tests. Since C.F.B. did not tell the patient what supplement he was testing, the patient was blind.

Figure 1 is a record of force vs. time obtained with the tester unblinded and the patient blinded. The active agent was on the patient’s abdomen during set A and the placebo substance during set B. When the placebo was present (set B), the patient yielded in every trial, the shoulder flexed about 10° and the force declined. When the active substance was present, the patient was able to lock the joint, even though the peak force was higher, and did not yield in any trial. The numbers above the tracings are peak force values in pounds, with those in parentheses indicating the values relative to the mean of set B. The numbers below the tracings are for the areas enclosed by the force vs. time tracings. One unit = 0.75 lb-sec. The force-time areas were measured from a baseline (upper horizontal line) that was equal to 10% of the mean peak force value in set A. The numbers in parentheses indicate the values relative to the mean area in set B. When the active substance was present, the mean peak force was higher by a factor of 1.28 and the mean force-time area was greater by a factor of 3.20.
and the placebo substance during set B. The way in which the patient’s forces are measured is designed to emphasize the improvements in strength that can occur in the clinic. When the patient is weak, sufficient force is produced by the examiner to flex the patient’s shoulder 10–15°; i.e., the patient yields in every trial (set B). When the patient is strong, the force is increased to a higher level, but not so high that the patient yields, and is sustained for about 2 sec, at which time the examiner releases the force (set A). This has the advantage that it is less likely to injure the patient, but has the disadvantage that it underestimates the patient’s ability to produce force because the tester terminates the trial before the patient yields.

In order to evaluate these force measurements, it is helpful to understand something of the biomechanics of strength testing and the design of the force transducer. The force acting back on the force transducer from the patient’s wrist equals the force with which the examiner is pressing against the wrist. Gravitational effects on the transducer are small and can be ignored. The only forces relevant for flexion of the arm at the shoulder are those that act at right angles to the wrist in the direction producing shoulder flexion torque. The force transducer accurately measures this force by having a side-load rejection ratio of more than 100 to 1 and by being insensitive to the point on the contact surface where the force acts.

The next question is what features of the patient’s force response should be measured? Two have been chosen: the peak force and the force-time area. The peak force is a widely used measure of physical strength and has been shown to be reliably evaluated with hand-held dynamometers (Bohannon, 1995, 1997a; Brinkmann, 1994; Hsieh & Phillips, 1990; Mulroy et al., 1997; see Sapega, 1990, for a general discussion of force measurement in the clinic and Bohannon, 1993, for a list of the 132 studies in which hand-held dynamometers had been used up to 1993). The precision of hand-held dynamometry compares favorably with fixed dynamometers (Brinkmann, 1994; Magnusson et al., 1990; Stratford & Balsor, 1994) and hand-held dynamometers display good consistency when a patient is repeatedly tested (Bohannon, 1986, 1997a,b; Hosking et al., 1976; Hsieh & Phillips, 1990) so long as there is a well-defined endpoint for the test and the tester can easily break the stabilized patient (Agre et al., 1987; Byl et al., 1988; Mulroy et al., 1997).

The force-time area is the area under the force-time curve and is influenced by both the amplitude of the force and its duration. It has been used in the past as a global measure of motor output (Nicholas et al., 1978; Ryan & Agnew, 1917). The force-time areas were measured with a planimeter.

Both peak force and force-time area measures have a straightforward interpretation when the patient resists displacement (Figure 1, set A). When the patient yields and the arm moves (Figure 1, set B), some of the force measured by the transducer is that required to produce angular acceleration of the limb. What is important in the present context is that these inertial forces add to the force measured in the placebo trials and so work against the hypothesis that
therapy strengthens the patient. It is especially important to minimize Type I error when doing research that is potentially controversial and no attempt has been made to correct either peak force or force-time area measurements for inertial forces.

Another factor increasing the measured force during the placebo trials is the well-known increase in muscular force that occurs when a contracting muscle is stretched even though there has been no increase in the neural drive to the muscle. Animal studies have shown that these increases can be as great as 10–20% of the prestretch isometric force (Joyce et al., 1969). Once again these forces increase the placebo break values and so work against the hypothesis that therapy strengthens. No attempt was made to correct either peak force or force-time area measurements for these eccentric enhancements in muscular force.

Stretch reflexes also add to the muscular forces produced during the joint rotation that occurs during the placebo trials. These forces would be reduced for a given fusimotor drive during the trials in which the patient resists displacement. Stretch reflex force also works against the hypothesis that therapy strengthens and no attempt has been made to estimate or correct either peak force or force-time area measurements for these forces.

In summary, it can be argued that peak force and force-time area measurements of the placebo trials (Figure 1, set B) should be restricted to the portion of the trial before the limb begins to move. This would ensure that the placebo measurements are comparable to the measurements of the trials with the active agent, which cover only times when the patient has control of the load and is successfully resisting displacement. Another argument for this approach is that the patients were instructed not to let their arm be displaced and to make a maximal effort to prevent this. How much they should resist after they lose control of the load was not specified. Although patients were not encouraged to resist after movement began, their behavior will have varied with their interpretation of the task and their motivation. As a consequence, whether the force peaks as movement begins or later may vary from patient to patient. In any case, once the force begins to fall in a continuous fashion, it is safe to assume that movement is underway. Hence, measurements of force-time area in the placebo trials should be restricted to the time between when the force exceeds threshold and when it peaks.

The counter-argument to these ideas is that to minimize Type I error, the methods used should always work against the hypothesis that the patient is strengthened by therapy. For this reason, we have included the entire placebo force response, including the declining phase of the force, in the force-time area. (Note that a threshold is set below which the force is not measured in order to exclude baseline fluctuations; see Figure 1.) The effect of including the declining phase of the force is to increase the placebo force-time areas and thus reduce the magnitude of any increase of the active-agent trials over the placebo trials. This increase in the placebo trials is somewhat diminished because some
of the patient’s muscular force has to be diverted to support the arm against gravity as the shoulder flexes and so is not registered by the force transducer. No correction has been made for this gravitational effect.

A 2-tailed unpaired t test (one that does not assume equal variance) was used to evaluate the statistical significance of changes in the magnitude of peak forces and force-time areas. Lines were fit to data points using linear regression analysis.

Procedure

The experiment was done on 2 different days; 10 patients were tested on the first day and 17 on the second.

Patients arrived at the clinic at an appointed time. They read a form describing the possible risks and benefits of participating in the experiment, and all indicated their willingness to participate by signing the form.

The patient was escorted to an examining room where C.F.B. and P.R.B. waited. The patient’s motor status was evaluated by C.F.B. and all patients tested strong on arrival. C.F.B. then searched for some indication of an incipient or actual health problem using methods honed over many years of practice. It is beyond the scope of this paper to describe these methods in detail. Suffice it to say that from the perspective of Western medicine, they would not be expected to change physical strength.

Within 2–10 min, every patient was diagnosed with a medical issue that was revealed when s/he tested weak. Typically, 4–6 bottles, each containing a different nutritional supplement appropriate for the diagnosed medical condition, were placed one by one on the patient’s abdomen until the most effective strengthening agent was found. A bottle containing a placebo substance also was identified, which, when placed on the patient’s abdomen, did not improve the patient’s ability to resist shoulder flexion; i.e., the patient remained weak. The patient’s strong response with the strengthening bottle on the abdomen was then charted. The patient’s weak response was next charted with the placebo bottle on the abdomen. Three trials were done with each substance. These tests constitute the first part of the nutritional experiment. The patient was blinded and the tester was unblinded.

In the second part of this study, the tester (C.F.B.), P.R.B. and the patient were blinded. The proper dosage of the strengthening agent was determined first by opening the pill bottle, placing the inverted bottle cap on the patient’s abdomen, adding 1 pill to the bottle cap and testing shoulder flexion to see if the patient was as strong as with the entire bottle of pills. If necessary, another pill was added and the test repeated. The maximal dose for this group of patients was 6 pills; the minimum was 2.

P.R.B. took a glass bottle holding the appropriate dose to another room and waited outside the closed door while another individual (the blinder) placed enough placebo pills into an identical glass bottle so that the 2 bottles had
a similar weight. The bottles (9.5 mm tall, 3.5 mm diameter, 68 gm weight capped and empty) were capped and placed into 1 of 2 cloth bags (15 mm wide, 10 mm high, with a zipper along the top, 17.7 gm weight). One bag was black and the other, tan. Whether the active agent would be in the black or the tan bag was predetermined using a random number table. Randomization was violated near the end of the assignment so that the active agent was distributed between the black and tan bags as equally as possible.

The 2 bags were weighed (Sartorius PT120 portable electronic balance). The amount of placebo was adjusted until the bags weighed almost the same. The largest difference in bag weight was 0.7%; the mean difference was 0.1%.

A different bottle was used to hold the strengthening substance for each patient. The same bottle was used for the placebo substance for all but 1 patient. Only 1 patient required a different placebo and a different bottle was used for that patient.

When the blinding was complete, the blinder opened the door to her room while holding both bags in the other hand and gave them to P.R.B. He returned to the examining room with the bags. The active agent was in the black bag for 19 patients and in the tan bag for 18 patients. C.F.B. always found one bag to strengthen the patient and the other not to strengthen. For 15 patients, he found the tan bag to strengthen. The remaining 12 patients were strengthened by the black bag. These outcomes were charted with 3 trials for each bag. These tests with everyone in the examining room blinded constitute the second part of the nutritional experiment.

In the third part of the experiment, the strengthening and placebo bottles used in the first part were retested with C.F.B. unblinded and charted with 3 trials for each.

The patient was given the bottle of strengthening pills minus those in the cloth bag. The cloth bags were returned to the blinder by P.R.B. Then P.R.B. brought the next patient to the examining room, etc. The average duration of a session was 14 min; the shortest was 8 min and the longest was 22 min.

C.F.B. was not told whether he had accurately identified the active and placebo substances until all patients had been tested. The person analyzing the data did not know whether C.F.B. had been correct or incorrect in the blinded part of the experiment.

Summary of Procedure

1. Patient’s motor status is evaluated. All patients test strong.
2. Incipient or real medical problem is revealed when patient tests weak.
3. Appropriate nutritional substances are sequentially placed on patient’s abdomen until one is found that makes the patient strong.
4. Placebo substance is identified that leaves patient weak.
5. Chart recordings of force vs. time are made for strengthening and placebo substances with C.F.B. unblinded and the patient blinded.
6. Step 5 is repeated with both C.F.B. and the patient blinded.
7. Step 5 is repeated with C.F.B. again unblinded and the patient blinded.

Normalization of the Data

The experiment consisted of 3 parts for each patient. In each part, the patient was tested with a strengthening substance and a placebo substance, and 3 records of force vs. time were made for each substance (Figure 1). In the first and third parts (tester unblinded, patients blinded), the patients always were able to resist flexion of the shoulder joint when the strengthening substance was present and always yielded in the presence of the placebo. In the second part (both tester and patients blinded), the patients also were strong and weak, but C.F.B. correctly identified the active agent in only 8 of the 27 patients.

There were 6 sets of records for each patient. Each set consisted of 3 trials: a weak and a strong set for the first part, a weak and a strong set for the second part, and a weak and a strong set for the third part. The 3 parts were normalized separately. Using patient 1 to illustrate the method, the individual peak-force values in the weak and strong trials of the first part were divided by the mean value of the peak forces when the patient was weak (Figure 1). This allows each peak force in the weak and strong trials of the first part to be expressed relative to the mean value of the weak trials. The relative mean for the weak trials, therefore, will equal 1; and the relative mean for the strong trials will be larger than 1 by a factor that indicates the magnitude of the strengthening effect. This puts all patients on the same footing regardless of strength. The force-time areas of patient 1 in the first part were normalized in the same way (Figure 1). This procedure was repeated on the data of patient 1 for the second and third parts. This allows the relative changes in peak forces and force-time areas for patient 1 in all 3 parts of the experiment to be compared. The data for the other 26 patients were normalized in the same way.

Results

Comparison of Unblinded and Blinded Data

Figure 2 shows the data from the first part of the experiment (tester unblinded, patients blinded). In this figure, the force-time area for each trial has been plotted as a function of the peak force for the same trial. Open circles indicate placebo tests where the patient yielded in every trial. Filled circles are for the strengthening substance; the patient held in every trial. That the patient did not yield increases the force-time area for a given peak force and magnifies the effect of increases in peak force on the force-time area, increasing the slope of the relationship between force-time area and peak force. In the presence of the strengthening substance, the mean peak force increased by a factor of 1.49 ($p = 9.22 \times 10^{-35}$, unpaired t test, 2-tailed) and the mean force-time area by a factor of 4.28 ($p = 1.14 \times 10^{-37}$).
Figure 3 shows data from the third part of the experiment (tester unblinded, patients blinded) similarly plotted and using the same bottles of strengthening and placebo substances. In the third part, the mean peak force increased by a factor of 1.50 in the presence of the strengthening agent ($p = 5.75 	imes 10^{-31}$, unpaired $t$ test, 2-tailed) and the mean force-time area increased by a factor of 3.97 ($p = 1.07 	imes 10^{-31}$). The mean peak force and force-time area values for the placebo tests are both 1.00 because the data were normalized. The standard deviations are $\pm 0.087$ and $\pm 0.16$ respectively. Linear regression analysis gave a slope of 1.45 for the placebo data with a correlation coefficient of 0.81, and a slope of 3.78 for the active substances with a correlation coefficient of 0.66.

Figure 4A shows the data in the second part of the experiment (both tester and patients blinded) of the 8 patients for whom C.F.B. correctly identified the strengthening and placebo substances. The mean peak force increased by...
a factor of 1.58 in the presence of the strengthening agent \((p = 2.02 \times 10^{-11})\) and mean force-time area increased by a factor of 4.37 \((p = 2.54 \times 10^{-12})\).

Figure 4B shows the 19 patients for whom C.F.B. incorrectly identified the strengthening and placebo substances. Here the mean peak force increased by a factor of 1.52 in the presence of the placebo substance \((p = 3.61 \times 10^{-19})\) and the mean force-time area increased by a factor of 4.16 \((p = 2.51 \times 10^{-22})\). The peak-force and force-time-area increments were not significantly different depending on whether the substances were correctly or incorrectly identified \((p = 0.35\) and 0.53 respectively).

In Figure 5, the data for the correct and incorrect judgments in the second part of the experiment have been combined for comparison with the first and third parts of the experiment. Mean peak forces increased by factors of 1.49, 1.53 and 1.50 in the first, second and third parts respectively. The mean force-time areas increased by factors of 4.28, 4.22 and 3.97. None of these differences were statistically significant.
Fig. 4A. These are the 8 patients for whom C.F.B. correctly identified the placebo and active substances in the second part of the experiment (both tester and patients blinded). Again, there were 3 trials per condition with the patient yielding in every placebo trial and holding in every trial with the active agent. The mean peak force increased by a factor of $1.58 \pm 0.25$ when the active
The data for all 3 parts of the experiment have been combined in Figure 6A. The open circles indicate weak responses (trials where the patient yielded), which were always associated with the placebo when the tester was unblinded (first and third parts of the experiment); this also was true for 8 of the patients when the tester was blinded (second part of the experiment). The other 19 patients in the second part were weak in the presence of the active substance. The filled circles indicate strong responses (trials where the patient resisted joint rotation), which always were associated with strengthening substances when the tester was unblinded and in 8 of the patients when the tester was blinded. The other 19 patients were strengthened in the presence of the placebo when the tester was blinded. The patients when strong had peak forces 51% greater on average than when they were weak \( (p = 8.92 \times 10^{-8}) \) and the average force-time area was greater by 316% \( (p = 3.12 \times 10^{-10}) \).

Figure 6B shows the data in Figure 6A after averaging the weak and strong trials for each individual subject. Nine trials were averaged to obtain each of these mean values. The mean peak force and the mean force-time area equal 1 for each subject when weak because the data were normalized (large open circle). The filled circles indicate the mean values of the strong trials for each subject. The overall strengthening effect is robust when viewed patient by patient; individual patients, when strong, can stabilize peak forces on average 20–90% higher than the forces that caused them to yield when weak.

**Discussion**

Several hypotheses may be pertinent to our experiments.

**Biomechanical Hypothesis**

This hypothesis proposes that the strength of the patients does not change, but that patients when seemingly weak are caught off guard or somehow overwhelmed by the tester and that patients are treated with restraint when agent was present \( (p = 2.02 \times 10^{-11}) \) and the mean force-time area increased by a factor of 4.37 ± 1.25 \( (p = 2.54 \times 10^{-12}) \). In the placebo trials, the means of the peak forces and force-time areas equal 1.00 because of normalization; the standard deviation is 0.079 for peak force and 0.13 for force-time area. The slope of the placebo trials is 1.46 (correlation coefficient 0.89) and the slope of the active-agent trials is 4.47 (correlation coefficient 0.89).

Fig. 4B. These are the 19 patients for whom C.F.B. incorrectly identified the placebo and active substances in the second part of the experiment (both tester and patients blinded). Here the patient yielded in all 3 trials when the active substance was present and held in all 3 placebo trials. The mean peak force was greater by a factor of 1.52 ± 0.30 when the placebo substance was present \( (p = 3.61 \times 10^{-19}) \) and mean force-time area was greater by a factor of 4.16 ± 1.52 \( (p = 2.51 \times 10^{-22}) \). The means of the peak forces and force-time areas in the active-agent trials equal 1.00 because the data were normalized; standard deviations are 0.084 and 0.17 respectively. When the patients were weak, the slope of the line is 1.65 (correlation coefficient 0.79). The slope for the patients when strong is 4.30 (correlation coefficient 0.84).
seemingly strong. Three things are important for this hypothesis. Was force applied to the patients when 'weak' with greater leverage or with the shoulder in a less favorable position? Did the trials of the patients when weak begin before they were ready? Were the patients when weak subjected to forces that built up more rapidly or to higher levels?

Great care was taken to apply the force at right angles to the same place on the patient’s wrist in every trial. This was not difficult because of the obvious anatomical landmarks at the wrist. Accurate placement within ±1.0 cm can be achieved and produces an error of ±2.2% in muscular force estimation for a patient with an arm of 46 cm from shoulder to wrist. Almost all patients had arms longer than 46 cm. The patients were watched carefully to be sure that the shoulder was flat against the examining table and remained there during the tests. The patient’s elbow was straight and locked in the extended position. Patients were not charted unless these conditions were met. The patients were always alerted by being told to resist just before the force was applied. Every chart recording used in this report was examined for evidence that the rate of rise

Fig. 5. The data in Figures 4A and B have been combined as there were no statistically significant differences between their peak forces and force-time areas. Patients when weak yielded in every trial and patients when strong held in every trial. The mean peak force is greater by a factor of $1.53 \pm 0.28 (p = 4.59 \times 10^{-29})$ in the strong trials and the mean force-time area is greater by a factor of $4.22 \pm 1.44 (p = 4.22 \times 10^{-31})$. The means for the weak trials are 1.00 with standard deviations of 0.082 and 0.16 for peak force and force-time area respectively. The slope of the line for the weak trials is 1.60 (correlation coefficient 0.81); and for the strong trials, the slope is 4.33 (correlation coefficient 0.85).
of the force was higher when the patients could not resist displacement. Figure 1 is typical; the tendency is for the rate of rise of the force to be greater when the patients were able to resist displacement. Figure 6B illustrates that individual patients when strong could hold against peak forces on average 20–90% greater than caused them to yield when weak.

**Social Interaction Hypothesis**

There was no observable change in C.F.B.’s manner that might explain whether a patient was weak or strong. P.R.B. accompanied C.F.B. whenever he was with patients and C.F.B. was uniformly agreeable and professional in his conduct. At no time were the patients when weak subjected to intimidation or distraction, nor were the patients when strong selectively encouraged.

**Mechanical Interaction Hypothesis**

According to this hypothesis, C.F.B. applies force in subtly different ways to the patient depending on whether he wants the patient to yield or resist. C.F.B. trains the patient to recognize these subtle mechanical cues so the patient will know what C.F.B. expects. Neither C.F.B. nor the patient is consciously aware of the existence of these mechanical cues or of the training process.

This hypothesis faces 3 major difficulties. (1) Naïve patients may test weak, for example, and a few seconds later test strong after an intervention thought to be beneficial. There is no time for training in such cases. (2) Even if the patients could be trained to yield or resist based on subtle mechanical cues, there is no reason for them to yield at lower forces than they can resist without yielding. Patients make a maximal effort in every trial, and this is particularly noticeable when one is forced to yield. As discussed in Methods, for a given motor drive patients would be expected to yield at higher forces than they can successfully resist. Thus this hypothesis fails to account for the observation that patients can be weakened and strengthened by C.F.B. (3) A careful search of the patient’s force records yielded no evidence of a uniformly different way in which C.F.B. applied force when the patient was weak vs. strong. There is a tendency for the force to rise more rapidly when the patient is strong (see Biomechanical Hypothesis and Figure 1), but there are cases where the forces rise at similar rates and a few where the force rises more rapidly when the patient yields.

**Placebo Hypothesis**

This hypothesis attributes strengthening to a placebo response by the patient. It does not apply to these experiments because the patients did not have a placebo response; they always were weak when the placebo substance was present in the first and third parts of the experiment (tester unblinded, patients blinded). It is difficult for patients to develop the expectations needed for a placebo response when they cannot predict whether they will be weak or
Fig. 6A. The data from all 3 parts of the experiment (Figures 2, 3 and 5) have been combined, since there were no statistically significant differences between their peak forces and force-time areas. Patients when weak yielded and patients when strong held in every trial. The mean peak
strong while being tested. From the point of view of the patients, the tester controls their physical strength.

Signature Hypothesis

This hypothesis proposes that a substance held in the hand or resting on the abdomen improves a patient’s ability to resist joint rotation because the substance has an electromagnetic emanation that is strengthening. This hypothesis predicts C.F.B. when blinded will be able to accurately identify substances that strengthened the patient or had no influence on the already weak patient.

Mental Influence Hypothesis

This is a broad category of interaction that includes both conscious expressions of the will and subconscious mental manifestations of beliefs, desires, attitudes and expectations. This hypothesis predicts C.F.B. will find patients to be weak with one of the blinded compounds and strong with the other; this is the only outcome allowed by the signature hypothesis, which he favors. The mental influence hypothesis also predicts C.F.B. will not be able to reliably identify the blinded substances.

The active agent always strengthened the patients when the tester was unblinded, but the active agent strengthened only 30% of the patients when the tester was blinded. In 70% of the trials, the placebo strengthened the patients.

This result is clearly supportive of the mental influence hypothesis. The chi-square test can be used to determine the likelihood this result would have occurred by chance alone. Once the outcome with the active agent is specified, the outcome with the placebo also is known; therefore only the results with the active agent were tested.

The sample consisted of 27 patients; each was tested while blinded in both parts 1 and 3 of the experiment with the tester unblinded. These patients were strong in 54 out of 54 tests when the active agent was present. When the tester also was blinded, 8 patients were strong and 19 were weak with the active agent.
These data were entered into a $2 \times 2$ contingency table. The chi-square is 45.8, $p < 0.0001$, and the mental influence hypothesis is supported.

The accuracy of the blinded tester differs from what would be expected if he were as likely to select the active agent as the placebo ($p = 0.034$, binomial test, 2-tailed). It is not known why in our study the blinded tester correctly identified the active agent only 30% of the time.

There was no awareness on the part of C.F.B. or the patients that any mental interaction had occurred either when C.F.B. was unblinded or blinded. It is for this reason that the mental influence is considered to be subconscious. For C.F.B. consciously to will a particular patient to be strong or weak would have been inappropriate because he believed that he simply was evaluating the motor status of the patient.

It is not known how one person’s mind can act on another person’s motor function. The mental influence hypothesis is consistent with an earlier study in which the ability of subjects to hold against the muscular forces of an experimenter apparently was influenced by the intentions of the latter (Burgess & Wei, 1984). It is only a modest extension of the interpersonal mental influence hypothesis to an intrapersonal version, which states that one’s own mind can influence one’s own motor behavior. We have been unable to find any paper in the peer-reviewed scientific literature that provides evidence for a direct mental influence of one person on the physical strength of another.

**Other Related Studies of Motor Function**

There have been a number of studies in which the more controversial aspects of muscle testing have been examined. They can be divided into those with force measurements and those without. Without force measurements, there is no way to rule out the biomechanical hypothesis discussed above. If the tester is simply overwhelming the patients/subjects when they are found to be weak, and is unaware of this due to effort-force rescaling (Burgess & Jones, 1997; Jones & Burgess, 1998; see also Hyman, 1999), muscle testing outcomes that at first seem surprising are relatively easily explained.

Studies in which force is measured and subjects are found to be strong and weak for no ‘known’ reason pose a challenge to orthodox science, even when the experiments are unblinded (Caruso & Leisman, 2000; Chorbajian et al., 1988; Diamond, 1979; Monti et al., 1999; Omura, 1979; Scopp, 1978). The explanation that has been favored so far is that the subject/patient has been changed in some way by the substance with which s/he is in contact, the lie just told, the reflex (acupuncture) point pressed on, etc., and that these patterns of strength and weakness can be used for medical diagnosis and lifestyle recommendations (Diamond, 1979; Goodheart, 1966, 1976; Hawkins, 2002; Walther, 1976). Another possibility, the one supported by our findings, is that the beliefs, desires, attitudes and expectations of the practitioner determine whether the patient/subject is weak or strong.
Which of these 2 alternatives is correct can be established by a research design in which a series of tests is done to establish that blinded patients are reliably weak under one set of conditions and reliably strong under another set of conditions. Once these nonintervention controls are completed, the tester is blinded and the tests are repeated.

Several carefully done experiments have not studied the ability of subjects to resist joint displacement at the hands of a tester, but have used the subject’s ability to produce handgrip or elbow-flexion force as an indicator of motor performance (Arnett et al., 1999; Braud, 1989; Keating et al., 2004; Kendler & Keating, 2003; Radin, 1984). In this design, the tester has been replaced by an experimenter who presents various substances to the subject who then makes a maximal effort against a dynamometer. Unfortunately, no unblinded experiments were reported showing which substances were reliably associated with strong and weak responses. After blinding both the experimenter and the subject, substances thought likely to weaken and strengthen the subject had similar effects. These results are consistent with the mental influence hypothesis, but this hypothesis would only be relevant if consistent weak and strong responses were obtained for particular substances when the experimenter was unblinded.

The mental influence hypothesis explains how perceptually dramatic changes in one’s ability to resist joint rotation can occur that may or may not be correlated with particular stimuli, depending on whether the tester is blinded. However, our experiment was done at 1 clinic by 1 tester with a small group of patients. More studies need to be done before the generality of the mental influence hypothesis can be accepted. Perot et al. (1991) gave subjects a form of muscle manipulation aimed at unloading muscle spindle receptors within the muscle and thereby creating weakness. The effect was present after blinding the tester. This may mean that the mental influence of the manipulator, who remained in the room, was influencing the subjects. Alternatively, this procedure may actually produce muscle weakness by spindle unloading.

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References


Investigating Scopesthesias: Attentional Transitions, Controls and Error Rates in Repeated Tests

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Abstract—The sense of being stared at, or scopesthesias, was investigated experimentally with participants working in pairs. Two participants were tested repeatedly and the effect of attentional transition was investigated. In some tests, in the pre-trial period the starer stared at the staree, who was blindfolded, and in others the starer did not stare during the pre-trial period. Their overall hit rate in these attentional transition tests was 52.8% (2,800 trials; \( p = 0.002 \)), but there was no significant difference in hit rates between the two kinds of test. Participants were given trial-by-trial feedback, so if there was any learning, there should have been a progressive increase in hit rates. This did not happen. The participants also took part in a control test in which there was no staring at all. In these tests hit rates were at chance levels, indicating that other forms of ESP, such as telepathy and clairvoyance, could not account for the results in scopesthesias tests. There were only 3 recording errors in 2,800 trials (0.1%), and two of these cancelled out, leaving a net error rate of 0.04%.

Keywords: scopesthesias—sense of being stared at—attentional transitions—response bias—error rates—feedback

The sense of being stared at is well known. Most people claim to have turned around to find that someone was looking at them; most people also claim to have caused other people to turn round by looking at them (Sheldrake, 2003). Scopesthesias is a newly coined scientific term for this phenomenon (Carpenter, 2005), which is also referred to in the research literature as “unseen gaze detection” (Wiseman & Smith, 1994), “staring detection” (Braud, 2005), “non-visual staring detection” (Sheldrake, 2005b) or “remote staring detection” (Baker, 2005).

The simplest tests for this phenomenon involve people working in pairs. One person, the staree, sits with her back to the other, and usually wears a blindfold. In a randomized sequence, the starer either stares at the back of the staree’s neck or looks away and thinks of something else. The beginning of each trial is signalled by a click, beep or bell. In over 30,000 trials of this kind the overall hit rate was 55%, very significantly above the mean chance expectation of 50%. The hit rates were also significantly above chance in several studies in which
starers and starees were separated by windows or one-way mirrors (for a review, see Sheldrake, 2005a).

It is a general principle of sensory physiology that the senses detect changes or differences. Perhaps the same principles apply to scopesthesia. In all the tests conducted so far, before the trial the starer was not looking at the staree. Then in looking trials, he started looking at her, while in not-looking trials he continued not looking. Thus, at the beginning of looking trials there was a change in his attention, and in not-looking trials there was not. In the tests described in this paper we explored whether starees were more sensitive when the starer changed from not looking to looking, or vice versa. In half the tests, all 20 of the trials in the test began with the starer looking. Then, as a signal was given to the staree, the starer either looked or did not look according to a randomized schedule. Thus, in the looking trials there was no change, and in the not-looking trials there was a transition of attention. In the other tests, as in standard staring experiments, the situation was opposite: in looking trials there was a change of attention from not-looking to looking, while in not-looking trials there was no transition.

Most experiments on scopesthesia have tested participants only once or twice. In the present series of investigations, the same participants were tested repeatedly. This enabled us to find out whether or not there were any changes over time in the staree’s hit rates. In these tests, starees received immediate feedback after each trial as to whether their guesses were correct or not. Their hit rates would increase if they were improving with practice, or decline if they were becoming bored with the testing process or losing their ability to make accurate guesses for any other reason.

We also carried out control tests to find out what patterns of guessing occurred in the absence of scopesthesia. Control experiments enable several fundamental questions to be explored. Are above-chance results in scopesthesia tests due to some ability other than scopesthesia? If so, what? First, significant positive results may depend on a detection of subtle sensory cues. Second, the staree might pick up the starer’s intentions telepathically, rather than through staring itself. Third, the staree might be picking up the starer’s written instructions by clairvoyance. Fourth, in tests in which trial-by-trial feedback is given, starees might be picking up the answers precognitively.

If positive results in scopesthesia tests depend on any of these other kinds of information transfer, then when scopesthesia is eliminated in control experiments, the hit rates should still be above chance.

One way to carry out control experiments would be to tell the starees that these are standard staring tests and that they will be stared at, or not stared at, in a random sequence. In fact, they are not looked at in any of the trials.

We did not want to base an experiment on deception. Instead, in our control tests, the starer and staree sat with their backs to each other. The starer did not look at the staree at all, and the staree knew that this was the case. The staree was asked to guess what instruction the starer was receiving in each trial. Using
standard randomized instruction sheets, the starer looked at the instruction, signalled the beginning of the trial to the staree by means of a standard mechanical click, and the staree then guessed whether the instruction was “look” or “no”. The staree received immediate feedback as to whether the guess was correct or not. We analysed the results of tape-recorded experiments to find out how frequently errors occurred in the recording of data.

Method

Participants

There were two participants in these tests, who took turns at being starer and staree: Pam Smart (PS) and her 14-year-old niece, JM, who was paid for her participation. These participants had already taken part in standard staring tests and were familiar with the general procedure.

Tests

The tests took place in JM’s family’s house with both participants in the same room, sitting about 2 metres apart. The staree was blindfolded. Each test consisted of 20 trials and was conducted in accordance with a randomized instruction sheet. There were 20 different randomized sheets altogether, and the sequence of looking and not-looking trials was determined by a random number generator. These 20 sheets were used repeatedly, but in a different order each time. Some sheets had equal numbers of looking and not-looking trials, while others had unequal numbers. Thus, by chance in some sets of data there were unequal numbers of looking and not-looking trials.

The participants carried out a series of between 14 and 16 tests in a session, and in each test there were 20 trials. After one 20-trial test was completed, the participants changed roles. The sessions occurred at roughly weekly or two-weekly intervals. The dates of these sessions are given in Table 1. For the tests on attentional transitions there were 10 sessions, followed by 3 sessions of control tests. All sessions began at 4 pm, apart from the session of February 15, which began at 1 pm.

Just before the beginning of each trial, the starer looked at the instruction sheet and read the instruction “look” or “no”, then signalled the beginning of the trial by means of a mechanical clicker, which gave a sound of standard intensity. For the tests on stimulus transitions, each trial was divided into two parts, the first lasting 3 seconds. The beginning of the second part of the trial was signalled by another click. A device used for training dogs emitted these clicks: the first was produced by pushing in a metal flange, and the second by releasing it 3 seconds later.

There were two kinds of test: in “looking tests”, during the first part of each trial, the starer looked at the staree, and then, 3 seconds later, as the second click
was sounded, followed the randomized instruction “look” or “no”. These were designated L tests. In the second kind of test, designated N (“not-looking”), during the first part of each trial the starer did not look at the staree, and then after 3 seconds either looked or continued not looking in accordance with the randomized instruction. Thus, in L tests, at the second click the starer either continued to look or changed to not looking; in the N tests at the second click the starer either continued not looking or changed to looking. The staree knew whether the test was an L or an N test.

PS determined at random, by the toss of a coin, whether the first test in a session was L or NL, and then each staree alternated between L and NL tests throughout the session. The starees knew whether they were taking part in an L or N test.

The staree guessed out loud “looking” or “not looking” within 10 seconds of hearing the second click and received immediate feedback as to whether this guess was correct or not. The starer recorded the result on the instruction sheet and proceeded to the next trial.

In the control tests, the starers and starees sat with their backs to each other. As usual, the staree was blindfolded and just before each trial the starer looked at the instruction sheet to see if the trial was “look” or “no”, and signalled the beginning of each trial by means of a mechanical clicker. Unlike the dog clicker used in the stimulus transition tests, this clicking device emitted a standard single click. The starer did not look at the staree at any stage during the test. The staree was asked to guess what instruction the starer had received and made this guess within 10 seconds of the trial beginning.

### Error Detection

All attentional transition tests were tape-recorded so that the pattern of clicks and responses could be evaluated independently at a later date in a “blind” fashion. This evaluation was carried out by Kayleigh Allenby (KA), who did not know either of the participants and lived 200 miles away. KA listened to the tapes of the trials, noting down the trial number and date and then recording
what guess the staree made in each trial. While Rupert Sheldrake (RS) had the original score sheets, PS then entered the guesses recorded by KA on duplicates of the original score sheets for each test. RS then compared these score sheets with the originals so that discrepancies could be detected. When such discrepancies were found, RS listened to the tape recording of that test to determine whether there was any error in the evaluation of the tape by the evaluator. If there had not been then the discrepancy was due to a recording error by the starer.

Scoring and Statistics

As usual in staring tests, the number of correct and incorrect guesses in looking and not-looking trials were tabulated separately, along with the total for each test (Sheldrake, 2000). As in previous research, the totals were also evaluated by means of the sign method, with scores of 11 or more out of 20 given a “+” sign, scores of 9 or less a “−” sign and scores of 10 an “=” sign. The advantage of the sign method is that it gives an equal weighting to each test. The chance expectation was that 50% of the guesses would be correct, and also that the number of + signs would be equal to the number of − signs, ignoring the number of = signs. The null hypothesis was tested using the binomial test. For comparisons of data from tests under different conditions the 2 × 2 Chi-squared test was used.

Results

Attentional Transition Tests

The overall hit rate for all the attentional transition tests was 1,477/2,800, or 52.8% (p = 0.002). By the sign method the results were 69 + 34 − 23 = (p = 0.0005).

For the L tests, in which the staree was looked at during the first 3 seconds of the trial, the hit rate was 52.9% (p = 0.01) or 39 + 23 − 13 = (p = 0.02) (Table 2).

For the N tests, in which the staree was not looked at during the first 3 seconds of the trial, the hit rate was 52.5% (p = 0.03) or 36 + 16 − 13 = (p = 0.004). The hit rate for the L tests was significantly greater than for the N tests on the basis of scores (p = 0.03) but not on the basis of signs.

The two starees had slightly different hit rates (Table 1): overall, PS scored 53.2% (p = 0.01) or 37 + 21 − 12 = (p = 0.02) and JM scored 52.3% (p = 0.04) or 38 + 18 − 14 = (p = 0.005). These differences between the two starees’ scores were not statistically significant.

In the L tests, the hit rates in looking trials were slightly higher than in not-looking trials, 53.5% and 52.3%, respectively. In the N tests, the reverse was the case, with 52.4% in looking and 52.7% in not-looking trials. Overall, the score
was slightly higher in looking trials (53.0%) than in not-looking trials (52.5%), but these differences were not statistically significant.

In most previous staring experiments, the total number of ‘‘looking’’ guesses was greater than the total number of ‘‘not-looking’’ guesses; in other words, there was a response bias in favour of ‘‘looking’’ (Sheldrake, 2005). In these tests, the total number of ‘‘looking’’ guesses was 757 (looking/hits) + 651 (not-looking/misses) = 1,408/2,800, or 50.3%, not significantly different from the chance level of 50%. However, this average figure conceals a striking difference between the two starees. PS guessed ‘‘looking’’ in 53.2% of the trials, while JM did so in only 47.4% of the trials, a significant difference (p = 0.002).

Control Tests

The results of these control tests are shown in Table 3. The overall hit rate of 49.3% was not significantly different from the chance level of 50%, nor were the scores of the individual participants: JM’s hit rate was 48.5% and PS’s was 50.0%.

Both participants scored below the chance level in looking trials and above the chance level in not-looking trials. This effect was due to a response bias whereby both of them guessed ‘‘not looking’’ more often than ‘‘looking’’: only 42.2% of JM’s guesses were ‘‘looking’’, while 46.2% of PS’s guesses were ‘‘looking’’. Overall, the percentage of ‘‘looking’’ guesses was 44.3%.

Changes with Time

The hit rates in the 10 sessions of attentional transition tests and for the 3 sessions of control tests that followed them are shown in Figure 1.

### Table 2

Scores in Staring Tests Where Trials Began with 3 Seconds Looking (L) or Not Looking (N) with 2 Starees (PS and JM). The Numbers of Hits and Misses Are Shown for Looking Trials, Not-Looking Trials and Totals

<table>
<thead>
<tr>
<th>Staree</th>
<th>Test</th>
<th>Looking Hit</th>
<th>Miss</th>
<th>Not looking Hit</th>
<th>Miss</th>
<th>Totals Hit</th>
<th>Miss</th>
<th>Hit %</th>
<th>Signs</th>
</tr>
</thead>
<tbody>
<tr>
<td>PS</td>
<td>L</td>
<td>215</td>
<td>156</td>
<td>178</td>
<td>171</td>
<td>393</td>
<td>327</td>
<td>54.6</td>
<td>21 + 11 = 4</td>
</tr>
<tr>
<td>PS</td>
<td>N</td>
<td>189</td>
<td>158</td>
<td>163</td>
<td>170</td>
<td>352</td>
<td>328</td>
<td>51.8</td>
<td>16 + 10 = 8</td>
</tr>
<tr>
<td>JM</td>
<td>L</td>
<td>189</td>
<td>195</td>
<td>212</td>
<td>184</td>
<td>401</td>
<td>379</td>
<td>51.4</td>
<td>18 + 12 = 9</td>
</tr>
<tr>
<td>JM</td>
<td>N</td>
<td>164</td>
<td>163</td>
<td>167</td>
<td>126</td>
<td>331</td>
<td>289</td>
<td>53.4</td>
<td>20 + 6 = 5</td>
</tr>
<tr>
<td>Totals</td>
<td></td>
<td>404</td>
<td>351</td>
<td>390</td>
<td>355</td>
<td>745</td>
<td>655</td>
<td>53.2</td>
<td>37 + 21 = 12</td>
</tr>
<tr>
<td>PS</td>
<td></td>
<td>353</td>
<td>358</td>
<td>379</td>
<td>310</td>
<td>732</td>
<td>668</td>
<td>52.3</td>
<td>38 + 18 = 14</td>
</tr>
<tr>
<td>JM</td>
<td></td>
<td>404</td>
<td>351</td>
<td>390</td>
<td>355</td>
<td>794</td>
<td>706</td>
<td>52.9</td>
<td>39 + 23 = 13</td>
</tr>
<tr>
<td>N</td>
<td></td>
<td>353</td>
<td>321</td>
<td>330</td>
<td>296</td>
<td>683</td>
<td>617</td>
<td>52.5</td>
<td>36 + 16 = 13</td>
</tr>
<tr>
<td>Grand totals</td>
<td></td>
<td>757</td>
<td>672</td>
<td>720</td>
<td>651</td>
<td>1,477</td>
<td>1,323</td>
<td>52.8</td>
<td>75 + 39 = 26</td>
</tr>
</tbody>
</table>

Note: The signs indicate the number of tests in which the hit rate was 11/20 or more (+), 9/20 or less (−) or 10/20 (=).
There was no systematic trend in the data, neither a regular improvement from session to session nor a regular decline. In the first three attentional transition tests and in the control tests the fluctuations for the two starees moved in similar directions, but in most other sessions they moved in opposite directions.

Within each session, each staree took part in 3 or 4 L and N tests. The average data testwise are shown in Figure 2. Again there was no clear trend. In the L tests the hit rates were higher in tests 3 and 4 than in the first 2 tests, but this was not the case in the N tests.

Error Rates

The number of errors in recording the data was determined from tape recordings of all the attentional transition tests. Out of a total of 2,800 trials, there were 5 discrepancies. Of these, two were owing to errors made by the evaluator in writing down the guesses on the tape recording. Three were errors made by the starer in noting down the staree’s guesses, an overall error rate of 0.1%. All three errors were in not-looking trials; two were false positives and one a false negative, giving an overall error of just one false positive, giving a net error rate of 0.04%.

Discussion

This is one of the first studies on scopesthesia in which the same participants have been tested repeatedly. A surprising result was that there was no obvious tendency for hit rates to increase or decline (Figure 1). Since the starees were receiving trial-by-trial feedback, their hit rates might have been expected to improve with practice, but this was not the case. In the first 3 sessions and in the control sessions, the fluctuations were similar with both starees, suggesting that external factors may have influenced both similarly, but there was no similar pattern in the other sessions. Within sessions, there was a tendency for hit rates
to increase in L tests but not in N tests (Figure 2). The lack of systematic trends suggests either that there was no tendency to improve with practice or that any such tendency was offset by a countervailing tendency, such as boredom.

A second surprise was that the number of errors in recording the data was so low. The net error rate of 0.04% was negligible.

The fact that hit rates were at chance levels in the control tests shows that the above-chance hit rates in the scopesthesia tests cannot be ascribed to telepathy, clairvoyance or precognition, or to any subtle sensory cues that were common to both kinds of test.

There was surprisingly little effect of attentional transition, with very similar overall results from L and N tests. The attentional transitions within these two kinds of tests did not result in higher hit rates than the trials in which there was no transition; indeed, there was a tendency for the reverse to be the case. In the L tests, all of which started with the starer looking at the staree, there was no transition in the looking trials, because the starer simply continued to look. The

---

Fig. 1. Changes in hit rate with time in sequential sessions. Sessions 1–10 were for attentional transition tests and Sessions 11–13 for control tests. The dates of these sessions are given in Table 1.
transition occurred in the not-looking trials when the starer stopped looking. Yet
the looking trials gave a slightly higher hit rate than the not-looking trials.
Conversely, in the N trials, all of which started with the starer not looking at the
staree, not-looking trials involved no transition and gave a slightly higher hit rate
than looking trials.
This lack of effect of the transitions is not what we expected. It implies either
that scopesthesia differs from other senses in not responding to changes or
differences or that the tests we carried out were too insensitive to detect them or
were inappropriately designed. Probably the best way to detect such transitions
would not be to signal when they occur, as we did, but to create a situation in
which the transitions occurred unpredictably, and to monitor people’s response
to them physiologically, for example, by the galvanic skin response.
Overall, the pattern of results differed from the typical pattern in staring
experiments, where there are usually above-chance hit rates in looking
trials, around 60%, and chance-level hit rates, around 50%, in not-looking trials
(Sheldrake, 2005). Here, the score was only slightly higher in looking trials
(53.0%) than in not-looking trials (52.5%), and the difference was not
statistically significant.
The usual pattern in standard staring experiments could arise because of a response bias in favour of saying “looking” rather than “not looking”. In the absence of scopesthesia, a 5% bias would give a 55% hit rate in looking trials and a 45% rate in not-looking trials, with an overall average of 50% (Schmidt, 2001). If staring detection occurred in both kinds of trial at 5% above chance, then the hit rate would be 60% in looking trials and 50% in not-looking trials, as observed.

In the attentional transition experiment described in the present paper, there was no significant overall response bias: 50.3% of the guesses were “looking”. Thus, in looking trials, taking into account the response bias, the hit rate was 2.7% above chance, and for not-looking trials it was 2.8% above chance, not significantly different. However, the two starees had significantly different response biases. PS’s response bias followed the more common pattern in that it was in favour of looking. With her response bias of 3.2% in favour of “looking”, the hit rate in looking trials of 56.3% was 3.1% above this chance level. In the not-looking trials her response bias would give a chance hit rate of 46.8%; the actual hit rate of 50.0% was 3.2% above this. JM’s response bias went in the opposite direction: only 47.4% of her guesses were “looking”, or, in other words, her response bias was -2.6%. Her hit rate in looking trials was 49.6% and in not-looking trials was 55.0%, which were 2.2% and 2.4%, respectively, above the chance level expected on the basis of her response bias.

These results closely fit a simple model for the probability \( P \) of a hit in looking and not-looking trials:

\[
P(\text{hit}/\text{looking}) = \frac{1}{2} + b + s \\
P(\text{hit}/\text{not-looking}) = \frac{1}{2} - b + s
\]

where \( b \) is the response bias, positive when the percentage of looking guesses is greater than 50%, and \( s \) is the effect of scopesthesia, with equal contributions in looking and not-looking trials.

In the control tests, both starees showed a response bias in favour of saying “not looking”, with “not-looking” guesses making up 55.7% of the total. This bias may well have reflected the fact that they both knew that in these tests they were never being looked at.

The fact that both participants served as starees in over 70 tests each makes them unusually experienced, and the results in these experiments may not be representative of naïve participants with little or no previous experience. This is something that only further research can reveal.

A possible problem with the attentional transition test described here was that the starers gave two signals to the starees in each trial using a mechanical clicking device: the first signal indicated that the pre-trial period had begun. In the L tests this meant that the starer was looking at the staree; in the N tests she was not. The second click indicated the beginning of the randomized trial in which the starer would either be looking or not looking. The starer estimated the 3-second interval between the two clicks. This raises the possibility that she
might have given subtle cues by unconsciously varying the length of the interval. However, the starees themselves were not aware of any differences of this kind. But perhaps they picked up subtle cues unconsciously. Unfortunately, we were unable to resolve this question definitively by a precise timing of the click interval trial by trial because the tape recordings were inadvertently discarded.

Starees might also have been influenced by other subtle cues, such as slight sounds from the starer as she turned her head. Another possible flaw was that we reused the same 20 randomized sheets and, hence, it is conceivable that starees might have unconsciously remembered the randomized sequences after they were exposed to them repeatedly. If so, the feedback they received should have enabled them to improve their scores very considerably with practice. But this did not happen. In future experiments, possible auditory cueing should be minimized, either by the use of sound-proof windows separating the participants or by the use of ear plugs or headphones. Also, a fresh randomization procedure should be used for each test.

Because the double-click procedure is potentially capable of introducing artefacts, it should be avoided in any further research on attentional transitions. A better method would be to use an electronic beeper that emits two beeps with a 3-second interval between them. An even simpler procedure would be for the starer to give just one signal at the beginning of the trial, having looked for 3 seconds previously in the L tests, or not looked in the N tests.

Acknowledgments

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References

ESSAY

Shakespeare: The Authorship Question,
A Bayesian Approach

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Abstract—Bayesian probability theory can be helpful in organizing the multiple evaluations required in analyzing complex problems that involve the comparison of several hypotheses on the basis of several datasets. The problem of deciding the authorship of the Shakespeare literary material falls under this heading. We here discuss just one aspect of this major problem: whether or not the available evidence indicates that “William Shakspere,” of Stratford-upon-Avon, was a writer. We consider twenty-four known writers who lived in England at the same time as Shakspere. For each of these writers, and for Shakspere, we follow Price in considering whether or not there exists evidence in each of ten categories relevant to the literary profession. We find that there is evidence conforming to at least three categories for each comparison author, but none for Shakspere. We evaluate the probability, based on this information, that Shakspere was a writer similar to the twenty-four comparison writers. According to this analysis of Price’s data, we find that there is only one chance in 100,000 that Shakspere was a writer. These considerations support the heretical view that Shakspere was not the author of the Shakespeare material.

Keywords: Shakespeare—statistics—probability

1. Introduction

It is generally – but not universally – assumed that the plays and poems associated with the name “Shakespeare” were written by a man who was born and raised, and died and was buried, in Stratford-upon-Avon in the county of Warwickshire in the West Country of England, ninety miles northwest of London. For recent accounts of the orthodox “Stratfordian” position, and for references to supporting material, one may refer for instance to Bryson (2007) and Honan (1998). The available records refer to the “Stratford” person variously as “Shackespere,” “Shackspeare,” “Shackspere,” “Shagspere,” “Shakespere,” “Shakespere,” “Shapsere,” “Shakspeare,” “Shaxper,” “Shaxpere,” and “Shexpere,” as well as “Shakespeare.” It is convenient to follow Price (2001) in referring to the man from Stratford as “Shakspere,” reserving the name “Shakespeare” for
the person or persons who individually or collectively produced the "Shakespeare" literary corpus.

Price’s (2001) “Chart of Literary Paper Trails” lends itself to statistical analysis. This chart compares personal and literary records left by twenty-four known Elizabethan and Jacobean writers during their lifetimes, together with notice at death as a writer within twelve months of the writer’s demise. This chart comprises ten “categories of evidence” (1). For each category, Price follows historical and biographical practice (see, for instance, Altick & Fenstermaker, 1993: esp. 49; George, 1909: 48–49; Kendall, 1985: xiii; Williams, 2003: esp. 58) in requiring that the evidence be (a) contemporaneous, (b) personal, and (c) related to the relevant profession – in this case, a literary life. Each category is reviewed for each of the twenty-four known authors and for Shakspere.

The comparison authors are listed in Table 1. The earliest (Gabriel Harvey) lived from 1550 to 1630, and the last (William Drummond) lived from 1585 to 1649. William Shakspere lived from 1564 to 1616. The average years of birth and death of these comparison authors was 1567 (standard deviation 10 years).

<table>
<thead>
<tr>
<th>Author</th>
<th>DOB</th>
<th>DOD</th>
<th>I-1</th>
<th>I-2</th>
<th>I-3</th>
<th>I-4</th>
<th>I-5</th>
<th>I-6</th>
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<td>—</td>
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<td>y</td>
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<tr>
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<td>1634</td>
<td>—</td>
<td>—</td>
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<td>y</td>
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<td>y</td>
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<td>y</td>
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<td>1619</td>
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<td>y</td>
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<td>y</td>
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<td>y</td>
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<td>1632</td>
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<td>y</td>
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<td>—</td>
<td>y</td>
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<td>1631</td>
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<td>y</td>
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<td>—</td>
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<td>—</td>
<td>y</td>
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</tr>
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<td>1649</td>
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<td>y</td>
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<td>—</td>
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<td>1579</td>
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<td>—</td>
<td>—</td>
<td>y</td>
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<td>—</td>
<td>—</td>
<td>y</td>
<td>y</td>
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<td>Robert Greene</td>
<td>1558</td>
<td>1592</td>
<td>y</td>
<td>—</td>
<td>y</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>y</td>
<td>y</td>
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<tr>
<td>Gabriel Harvey</td>
<td>1550</td>
<td>1630</td>
<td>y</td>
<td>y</td>
<td>—</td>
<td>y</td>
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<td>y</td>
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<tr>
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<td>1641</td>
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<td>—</td>
<td>y</td>
<td>—</td>
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<td>y</td>
<td>y</td>
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<td>1637</td>
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<td>Thomas Lodge</td>
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<td>1625</td>
<td>y</td>
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<td>—</td>
<td>—</td>
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<tr>
<td>John Lyly</td>
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<td>1606</td>
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<td>—</td>
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<td>—</td>
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<tr>
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<td>1564</td>
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<td>y</td>
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<td>y</td>
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<td>—</td>
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<tr>
<td>John Marston</td>
<td>1576</td>
<td>1634</td>
<td>y</td>
<td>—</td>
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<td>y</td>
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<td>y</td>
<td>y</td>
<td>y</td>
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<tr>
<td>Philip Massinger</td>
<td>1583</td>
<td>1640</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
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<td>1627</td>
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<td>y</td>
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<tr>
<td>Anthony Mundy</td>
<td>1560</td>
<td>1633</td>
<td>—</td>
<td>—</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
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</tr>
<tr>
<td>Thomas Nashe</td>
<td>1567</td>
<td>1601</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>—</td>
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<tr>
<td>George Peele</td>
<td>1556</td>
<td>1596</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>—</td>
<td>—</td>
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<tr>
<td>Edmund Spenser</td>
<td>1552</td>
<td>1599</td>
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<td>y</td>
<td>—</td>
<td>—</td>
<td>y</td>
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<td>—</td>
<td>y</td>
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<td>1557</td>
<td>1592</td>
<td>y</td>
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<td>—</td>
<td>—</td>
<td>y</td>
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<tr>
<td>John Webster</td>
<td>1578</td>
<td>1632</td>
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<td>—</td>
<td>y</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>y</td>
<td>y</td>
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</tr>
<tr>
<td>William Shakspere</td>
<td>1564</td>
<td>1616</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
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<td>—</td>
</tr>
</tbody>
</table>

Note: For twenty-four comparison authors and for Shakspere, this table lists the date of birth, the date of death, and whether there exists evidence conforming to each of ten items related to the profession of writer (‘y’ for Yes, ‘—’ for No).
and 1620 (standard deviation 18 years). As far as chronology is concerned, this seems a reasonable comparison set.

For each of these authors, and also for Shakspere, we note in Table 1 whether or not there is “paper-trail” evidence for each of the ten categories. These categories are specified in Table 2, where we note the number of comparison authors for which such evidence has been found. We note from Table 1 that, for every comparison author, we have at least three items of relevant literary evidence. For Shakspere, by comparison, we have none. We now examine this discrepancy statistically.

### 2. Analysis

For each category, we proceed as follows: We determine whether or not there is evidence relevant to that category for each of the $N$ ($N = 24$) comparison authors. Suppose we find such evidence for $K$ of those authors. The problem then is to determine the probability of finding comparable evidence for the “test” author, Shakspere, on the assumption that he belongs to the same group as the comparison authors. We may regard this as a sequence of $N + J$ trials. We are given the result for the first $N$ in this sequence (the comparison authors), and we wish to estimate the probability $P$ of getting a positive result for the $(N + J)th$ trial (i.e. for Shakspere). We find (see Appendix) that this is given by

$$P = \frac{K + 1}{N + 2},$$

which is known as the Laplace “rule of succession” (Howson & Urbach 1989; Jaynes 2003). These estimates are listed in Table 3.

### TABLE 2

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<thead>
<tr>
<th>Item</th>
<th>Yes</th>
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<tbody>
<tr>
<td>1</td>
<td>Evidence of education</td>
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</tr>
<tr>
<td>2</td>
<td>Record of correspondence, especially concerning literary matters</td>
<td>14</td>
</tr>
<tr>
<td>3</td>
<td>Evidence of having been paid to write</td>
<td>14</td>
</tr>
<tr>
<td>4</td>
<td>Evidence of a direct relationship with a patron</td>
<td>16</td>
</tr>
<tr>
<td>5</td>
<td>Extant original manuscript</td>
<td>10</td>
</tr>
<tr>
<td>6</td>
<td>Handwritten inscriptions, receipts, letters, etc., touching on literary matters</td>
<td>15</td>
</tr>
<tr>
<td>7</td>
<td>Commendatory verses, epistles, or epigrams contributed or received</td>
<td>21</td>
</tr>
<tr>
<td>8</td>
<td>Miscellaneous records (e.g. referred to personally as a writer)</td>
<td>24</td>
</tr>
<tr>
<td>9</td>
<td>Evidence of books owned, written in, borrowed, or given</td>
<td>9</td>
</tr>
<tr>
<td>10</td>
<td>Notice at death as a writer</td>
<td>9</td>
</tr>
</tbody>
</table>

Note: Columns 3 and 4 list the number of comparison authors for which there is (“yes”) or is not (“No”) such evidence. (A “Yes” represents one or more qualifying pieces of evidence.)
As we see from Table 1, the Shakspere entries are remarkable in that they show no evidence for any of the ten categories that we have found to be characteristic of most of the comparison authors. The question is whether or not this discrepancy is significant. The usual procedure, in statistical analyses, is to compute the probability that a certain result may have occurred by chance. We see from Table 1 that the probability that there might by chance be no evidence for category 1 is 0.31; for category 2 it is 0.42 etc. The probability that all ten categories are unrepresented purely by chance is the product of these ten estimates, which is found to be $10^{-5}$. If we expect evidence relevant to Shakspere to conform to the evidence we find for the twenty-four comparison authors, there is only one chance in 100,000 that the results concerning Shakspere would have occurred by chance.

One may be justifiably concerned that the above estimate may depend critically on the precise selection of comparison authors. The best response for this concern would be for other Shakespeare scholars to develop independent “charts of literary paper trails.” However, one can to some extent judge the dependence of the result on the selection process by using the bootstrap procedure (Efron & Tibshirani, 1993). For each bootstrap simulation, and for each cell in Table 1, we enter a value selected randomly (with replacement) from the column to which the cell belongs. We have carried out 10,000 such bootstrap simulations, and we show in Figure 1 a histogram of the resulting estimates of the logarithm (base 10) of $P$. We see that our estimated value $10^{-5}$ is very close to the peak of the histogram, indicating that our probability estimate is not sensitive to the precise selection of comparison authors.

<table>
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<td>0.69</td>
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<td>0.58</td>
<td>0.42</td>
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Note: For each of ten items, columns 2 and 3 list the number of comparison authors for which there is (“Yes”) or is not (“No”) such evidence. Columns 4 and 5 list the probability that there would or would not be such evidence for a twenty-fifth author, on the assumption that the authors all have similar habits, which result in the creation of literary paper trails with common characteristics including probability of survival. It may be noted that published evidence, such as a personal commendatory verse, has inherently greater probability of survival than does, for instance, a handwritten letter or a manuscript.
The probability of obtaining the results of an experiment or test on the basis of an assumed “null hypothesis” (in this case, the hypothesis that Shakspere was in most respects similar to the comparison authors) is known as a “P-Value.” It is generally recognized that this quantity should not be interpreted as the probability that the null hypothesis is false. (See, for instance, Sturrock, 1997; Utts, 1996.) However, the latter quantity may be estimated by means of Bayesian procedures (Sturrock, 1973, 1994). To use the Bayesian approach, one must consider a complete set of hypotheses, such that one and only one of the hypotheses must be true. For the present problem, clearly one hypothesis could be

\[ H1. \] Shakspere wrote the Shakespeare material (plays and poems), and one may complete the set by adopting

\[ H2. \] Shakspere did not write the Shakespeare material.

We consider the two statements:

\[ S1. \] Shakspere was a writer, and
\[ S2. \] Shakspere was not a writer.

We need to evaluate the probability of each of these statements on the basis of the evidence and on the basis of each hypothesis.
We have found that $P(S1|E) = 0.00001$. Since one of $S1, S2$ must be true, $P(S2|E) = 0.99999$.

If $H1$ is correct, he must have been a writer, and one might therefore expect that he would have the same “paper trail” as the comparison authors. This proves to be a key decision. If we make this assumption, then $P(S1|H1) = 1$.

If Shaksper is assumed not to be the author of the Shakespeare material, we can be noncommittal and assume that it is even odds whether or not he was a writer, i.e. $P(S1|H2) = 0.5$. Then, since $S1$ and $S2$ are mutually exclusive, $P(S2|H1) = 0$ and $P(S2|H2) = 0.5$.

We can now calculate the post-probabilities of $H1$ and $H2$ from

$$P(H_i|E) = \frac{\sum_k P(S_k|H_i)P(S_k|E)}{\sum_j P(S_j|H_j)P(H_j|E)}$$

where $P(H_i|E)$, $i = 1, 2$, are the prior probabilities.

If we adopt the non-committal values $P(H_i|E) = 0.5$, we find that the post-probabilities are $P(H1|E) = 7 \times 10^{-6}$ and $P(H2|E) = 1 - (7 \times 10^{-6})$.

Note that, in the present context, the evidence $E$ comprises Price’s (2001) “Chart of Literary Paper Trails,” nothing more and nothing less. To get an “absolute” post-probability, one would need to convolve the probability of the hypotheses on Price’s evidence with the probability of Price’s evidence on some (non-existent) “absolute” database.

### 3. Further Hypotheses and Discussion

This very strong result hinges on the key assumption that $P(S1|H1) = 1$, which rests implicitly on the assumption that if Shaksper was a writer, he would have footprints similar to those of the comparison authors. The assumption needs careful consideration. If one can plausibly argue that, for instance, Shaksper had some strong incentive to hide the fact that he was a writer, we can no longer conclude from our statistical analysis that he was not a writer. This then leaves open the possibility that he might have been the author of the Shakespeare works. One way to cope with this possibility is to divide $H1$ into two sub-hypotheses:

$H1.1$. Shaksper was a writer and did not hide the fact,

and

$H1.2$. Shaksper was a writer but hid the fact (as best he could).

Shaksper would not have been able to suppress all the items of evidence listed in Table 2, but he might (for unknown reasons) have taken steps to
suppress those that he could. This seems an unlikely prospect for Shakspere, which could be reflected in a low prior probability for $H1,2$.

If we conclude that Shakspere was not the author of the Shakespeare material, we are left wondering who was. We could then proceed to allow for more options, such as:

- **$H1$.** Shakspere was the sole author of the Shakespeare material.
- **$H2$.** Shakspere produced the Shakespeare material in collaboration with another commoner, or with more than one commoner.
- **$H3$.** Shakspere produced the Shakespeare material in collaboration with at least one member of the upper class\(^{(3)}\), and possibly one or more other commoners.

One must also consider the possibility that Shakspere had no part in the writing of the Shakespeare material. This may be broken down into the following possibilities:

- **$H4$.** The Shakespeare material was written by one or more commoners, excluding Shakspere.
- **$H5$.** The authorship of the Shakespeare material involved at least one member of the upper classes, possibly in collaboration with others, but excluding Shakspere.

Various specific proposals have been made which are special cases of the above hypotheses. The basic Stratfordian position corresponds to $H1$. The proposals for Ben Jonson and Christopher Marlowe are special cases of $H4$. The proposals for Francis Bacon, Edward de Vere, Earl of Oxford, and Mary Sidney, Countess of Pembroke, are special cases of $H5$. Hypotheses $H2$, $H3$, $H4$, and $H5$ allow for possible collaborations\(^{(4)}\).

In pursuing this topic, the first requirement would be to assess or supplement Price’s (2001) “Chart of Literary Paper Trails,” on which our estimates have been based. An ideal procedure would be for several scholars to agree on a list of comparison authors, and a list of categories of evidence, and then for each scholar to make his or her own assessment of whether or not evidence for each category exists for each comparison author and for Shakspere. One would then obtain a list of $P$-Values, one for each scholar, which could be converted into post-probabilities for the proposed hypotheses. These post-probabilities could if necessary be combined using Bayesian procedures.

It would be most desirable to evaluate other relevant evidence, which we refer to as “items,” such as (a) chronological analysis, comparing the known history of Shakspere and the dates of first mention of the plays and poems; (b) content analysis (as indicative of knowledge of other languages and other countries, and of the interests and pastimes of commoners and of the nobility, etc.); and (c) textual analysis, comparing samples of the Shakespeare material and the writing of specified candidates. We could evaluate each hypothesis on the basis of each “item,” and then combine the judgments using procedures described elsewhere (Sturrock, 1973, 1994).
Notes

1 Price (2008) explains that her list of just ten categories represents a convenient packaging of diverse pieces of evidence. For instance, Price can list over thirty pieces of evidence for Drayton and over twenty for Chapman, down to five each for Fletcher and Kyd. Hence a checkmark for evidence in a particular category for a particular author may represent a number of separate pieces of evidence. For instance, Price collapses over twenty records concerning Marlowe’s presence and education at Cambridge into a checkmark for one category of evidence (Evidence of education). For recent updates, see http://www.shakespeare-authorship.com/resources/errata.asp.

2 If Christopher Marlowe’s murder was merely a staged event to save him from the not-so-tender mercies of the Court of Star Chamber, he would have had the best reason in the world to keep a very low profile.

3 The term “upper class” is used to connote a member of a noble family, or any person with a title (such as Bacon, who was knighted in 1603).

4 Ms Price advises me that Shakespeare editors and scholars have long known that other hands were responsible for parts of Pericles, Henry VIII, and parts of the Henry VI trilogy, to name the best known Shakespeare “collaborations” (in quotes, since the nature of such collaborations remains elusive.) More recent scholarship has succeeded in identifying or confirming specific collaborators in those and other plays in the canon, as well as finding Shakespeare’s hand in plays attributed to others or published anonymously. The process of analyzing texts has been facilitated by the Chadwick-Healy database Literature Online, which provides scholars with tools to compare and quantify vocabulary, function words, syntax, prosody, stylometry, parallel passages, and other linguistic features. These techniques have been discussed in book form by Vickers (2002) and by Jackson (2003). Both books provide the interested student with helpful bibliographies. Recent journal articles include “Shakespeare and the Quarrel Scene in Arden of Feversham” by Jackson (2006), and Vickers (2007), in which Vickers makes the case for the presence of the hand of Thomas Nashe.

Acknowledgments

The author is indebted to Diana Price for generously reviewing and commenting on earlier versions of this article, and to Henry Bauer, Elliot Bloom, Jeffrey Scargle and Sarah Webster Goodwin for helpful discussions.

References

APPENDIX

Suppose that we begin with a completely open mind, and assume initially that $P$ can have any value between zero and unity, with a uniform distribution in that range. We denote the probability that $P$ is in the range $p$ to $p + dp$ by $Q(p|Z)dp$, where the symbol "$Z$" indicates that we have zero relevant information. The likelihood of getting a positive result (a "Y") for any trial is $p$, and the likelihood of getting a negative result (an "N") is $1 - p$. Hence the likelihood of getting $K$ positive results and $(N - K)$ negative results is

$$L(K|N; p) = p^K(1 - p)^{N-K}. \tag{A.1}$$

By Bayes theorem, we may obtain the "post-probability-distribution-function" for $p$, given $N$ and $K$, as follows:

$$Q(p|N, K) = \frac{L(K|N, p)}{\int_0^1 dp' L(K|N, p')} \tag{A.2}$$

Based on this information, the probability of getting a positive result for the $(N + 1)th$ trial is

$$P = \frac{\int_0^1 dp Q(p|N, K)p}{\int_0^1 dp Q(p|N, K)} \tag{A.3}$$

This is found to have the value

$$P = \frac{K + 1}{N + 2}. \tag{A.4}$$
An Anomalous Legal Decision

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Abstract—In 2003 the Veterinary Medical Board (VMB) of the State of California brought a complaint against Dr. Gloria Dodd for what they alleged was false advertising for the use of homeopathic remedies and the use of radionic devices for the diagnosis and non-local treatment of animals. Unsatisfied with Dodd’s compliance the VMB brought the case to court in 2006. There, through in absentia testimony as to Dodd’s abilities and my testimony as to the evidence for non-local man-machine interaction and remote viewing and the failure of the complainants to rebut our evidence Dodd was almost completely vindicated. Full vindication came with our appeal to a superior court, which permitted Dodd to continue to practice without the need to personally examine her animal patients. If sustained through appeal by the VMB the case might serve as a precedent for all practitioners of non-local healing.

Keywords: radionic treatment—Veterinary Medical Board—legal decision

In the winter of 2003 the Veterinary Medical Board (VMB) of the State of California brought a complaint against the veterinarian Dr. Gloria Dodd of Gualala, CA, for what they alleged was false advertising in her claims that homeopathic remedies and non-local diagnosis and non-local radionic treatment could help sick animals. Radionic treatment usually involves the practitioner utilizing a “black box” as an intermediary device for establishing non-local contact with the patient. In essence the box consists of a well which holds something such as a drop of blood on a paper strip, a hair, or a sample of saliva or of some other substance of a previous living nature from the patient. The surface of the box also holds a rubber strip set onto a Bakelite frame which is screwed into the surface of the box, numbered dials, and metal wires connecting all the aforementioned items. The practitioner lightly rubs the rubber strip with a couple of fingers while simultaneously thinking of the various possible causes of the ailment. By rotating the dials, which correspond to certain “frequencies”, one can fine-tune whatever one is thinking of. When the finger spontaneously sticks to the strip, this is a “yes” response. In this way, one can make a diagnosis and figure out which “frequencies” of “energy” can be used to broadcast to the patient to cure his problem. Radionics is very much like dowsing, where one must think various thoughts regarding the target. When resonance is established
the dowsing branch or L-rod will spontaneously move, thus signaling a “yes”. Another similar device is in the practice of medical radiesthesia where a pendulum provides “yes” or “no” types of answers to thought questions. In my opinion based upon anecdotal experience with all of the devices mentioned above, the devices serve to support the practitioner’s belief system in establishing a state of resonance with the patient.

Dr. Dodd claimed that she had complied with previous demands of the VMB to stop advertising the way that she had, but the VMB thought otherwise and requested a hearing before the courts of the state with the intention of revoking Dr. Dodd’s license to practice. Through their evaluation of the records of Phido, a canine patient of Dr. Dodd, the agents of the VMB concluded that, “Its a smoke and mirror power of magic type of practice” . . . “bizarre” . . . “no medically scientific basis for the idea that she can detect disease in a patient who is 3000 miles away . . . ludicrous that she can broadcast therapy across the same distance . . . claims defy basic established principles of physics”

The case was heard in mid-winter of 2006 at the Oakland, CA court before Administrative Law Judge Steven C. Owyang, a Chinese gentleman. While I had originally been pessimistic about the outcome of the case based on my knowledge of previous similar cases, when I saw that Judge Owyang was Chinese I thought that we might have a chance since the judge well may have been familiar with Chinese medicine and the functions of “chi”, which, in the Chinese system, is a kind of “vital fluid” or “force” operating on a non-mechanical “subtle” level. Hans U. Stucki was the attorney representing Dr. Dodd. In absentia expert witnesses and satisfied lay clients of Dr. Dodd testified to the efficacy of Dr. Dodd’s assistance to them in the treatment of some very sick, refractory animals. As a putative expert in the knowledge of the non-local transmission of information I testified in person. I had been referred to Mr. Stucki by Brenda Dunne of the PEAR laboratory in Princeton, NJ. Brenda had been contacted by Mr. Stucki because of PEAR’s well-known research on man-machine local and non-local interactions. In my testimony I suggested that the operative agent in non-local healing was of a non-physical nature, something like chi. None of our testimony or any of the cases were rebutted by the complainants. I believe that the complainants thought that the case would be a slam-dunk in their favor because Dr. Dodd’s mode of practice and beliefs were so thoroughly different than those of the official position of the VMB and that rebuttal would not be necessary because the judge would readily see how ludicrous Dr. Dodd’s method of practice was, and rule for the VMB without argument.

To my surprise and our great pleasure Judge Owyang appeared to ignore accepted “theory” and go for the clinical evidence which favored Dr. Dodd. I left after testifying and before the other clinical evidence for Dr. Dodd was presented. Mr. Stucki called me at home and reported that after all the evidence was presented Judge Owyang turned to the complainant and stated that Dr. Dodd seemed to be doing as good a job as traditional veterinary practice in caring for
her patients. In his decision, several months later, Dr. Dodd was placed on probation with the proviso that she rectify a few minor problems relating to her practice and that she establish physical contact with her clients in her practice.

Dr. Dodd through Mr. Stucki appealed the proviso that Dr. Dodd had to make physical contact with her clients given that the nature of her practice made this exceedingly difficult, especially since many of her animal clients lived so far away and were so seriously ill and could not travel. The appeal was heard in Superior Court of CA before Judge Gary Nadler and on May 16, 2008, Dr. Dodd was granted a stay of the prior administrative order. The grounds for the stay were that its granting would, first, not cause suffering of the public interest and second, in view of the fact that Judge Owyang made a clear and unequivocal finding that Dr. Dodd should be permitted to continue to practice her profession and in view of the fact that her work was non-local in nature, that it would be “internally inconsistent” with the ruling to force her to physically exam her patients. As Judge Nadler stated, “To strictly impose the physical examination requirement where no violation based on past practice has been determined is to effectively prevent Petitioner from practicing her profession and, further, to deny her clients the opportunity to select the apparently efficacious alternative treatment modalities which she employs in part”.

Of course the VMB could not let it go, so on May 24th they submitted an order of remand questioning in many ways whether testimony in the original hearing by the defense was by “qualified experts”. A final hearing on Dr. Dodd’s overall appeal is coming up, once again before Judge Nadler in Superior Court. This hearing will involve issues virtually identical to those previously decided by Judge Nadler on the Motion for Stay which he granted. If the decisions of Judges Owyang and Nadler are sustained then Dr. Dodd will be free to practice as she always has. This could also be a precedent for those of us practicing alternative forms of medicine which involve non-locality.

Notes

1 Division of Investigation of VMB, Case #2004-09-1097, 2/16/2005, p.4.
Note on Charles Richet’s “La Suggestion Mentale et le Calcul des Probabilités” (1884)

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Abstract—In 1884 French physiologist Charles Richet published an article on “mental suggestion” in the Revue philosophique de la France et de l’étrangère that is an early classic of experimental parapsychology. This work is generally remembered for the use of statistical evaluation of ESP experiments. Nonetheless, Richet discussed other issues as well that are generally neglected. This included reanalyses of previously published thought-transfer studies, and discussions of topics such as the place of mental suggestion in science, study participants, and the relationship of mediumship to the unconscious mind. Furthermore, in this paper Richet wrote about a variety of issues related to ESP such as its various features, targets, motor automatisms, its unconscious nature, and theoretical issues.

Keywords: Charles Richet—mental suggestion—ESP—French psychical research—mediumship

A historian of psychology referred to an 1884 attempt “to make mental suggestion a scientific subject by the use of probability calculations . . .” (Carroy, 2004:226). This was a reference to one of the most important nineteenth century reports of experimental ESP tests. Published by French physiologist, physician, and psychical researcher Charles Richet (1884), the paper was entitled, “La Suggestion Mentale et le Calcul des Probabilités.” It appeared in the December 1884 issue of the Revue philosophique de la France et de l’étrangère, a journal that published later several examples of “sleep” induced at a distance, such as the famous work of Pierre Janet (1886a,b) and the observations of others (e.g., Beaunis, 1886; Dufay, 1888).

Richet defined mental suggestion in the paper as the “influence that an individual’s thought exerts over a specific sense, without an appreciable exterior phenomenon on our senses, over the thought of a nearby individual” (Richet, 1884:615, this, and other translations, are mine). The term mental suggestion was widely used in France to mean the active sending of thoughts, images, and other effects including commands to induce trance at a distance (for overviews see Ochorowicz, 1887/1891; Plas, 2000:87–109).
Richet’s paper received both sympathetic (Gurney, 1884) and unsympathetic (Franklin, 1885) contemporary commentaries. Later discussions on the statistical aspects of the paper include those of historians (Hacking, 1988:438–439) and parapsychologists. Among the latter, J. B. Rhine said that Richet deserved “credit for the first use of the mathematics of chance in evaluating results of telepathy tests . . .” (Rhine, 1947:16). Jahn and Dunne (1987) affirmed that Richet’s statistical approach “strongly influenced the subsequent analytical strategies” (Jahn and Dunne 1987:41) of parapsychology and other disciplines. In this note I briefly discuss additional and generally neglected contributions of Richet in his 1884 paper.

Reanalyses of Previous Thought-Transference Experiments

In addition to the statistical analyses of his own data, Richet’s work presents an early example of the statistical reanalyses of previously published experimental studies (pp. 633–634). For example, in some trials of thought-transference studies published by the Society for Psychical Research (Barrett et al., 1882) it was found that there were five consecutive hits. Richet argued that there was one chance in 52 to select the proper card from a deck of playing cards. The successful guessing of a second card was associated with a “probability . . . of 1/52 × 1/52 . . . and, in consequence, to state exactly five times the suit of a card, the probability is . . . 1/16.680.235 . . .” (p. 633). This, Richet believed, showed that chance did not account for the results. Regarding other tests in the same report in which eight hits were reported to occur, Richet stated that the probability was “1/52^8 = 1/7 164 938 643 456” (p. 634). In Richet’s view this was equivalent to selecting the single black ball from the remaining 7,164,938,643,455 white balls in an urn.

Targets

Richet not only used playing cards to test for mental suggestion. He also utilized “photographs of paintings, of statues, of antique objects, of scenes, of diverse topics” (p. 635). Richet commented that the photographs “certainly strike the imagination in a way that is more powerful than a simple playing card” (p. 635). There were also tests in which hidden objects and letters served as targets.

Mental Suggestion and the Unconscious

Richet believed that mental suggestion acted on the “unconscious faculties of intelligence” (p. 639). The person receiving the message was not aware of the fact. But such information could manifest through weak unconscious movements. This is what led him to use the dowsing rod and table tilting, as discussed below.
Motor Automatisms

In addition to guessing tasks, Richet used motor automatisms to obtain responses from his participants. He reported tests in which the response was produced through movements produced by table turning. To accomplish this Richet had the persons on the table (designated as C, D, and E) sitting with their backs to two individuals, who sat on a separate table with a board displaying the alphabet. An electric battery was connected to a bell and to the legs of a table so that when any leg was raised the bell would ring. Two other participants were designated as A and B. The person designated as A moved his finger along the alphabet and, when the bell rang (meaning that one of the table’s legs rose), B wrote down the letter where the finger was resting. This took place without the table tilters knowing the identity of the letter. Some “quick and repeated movements indicated that the word or the phrase were finished” (p. 653).

To the surprise of C, D, and E, “the word had a meaning, the phrase had a significance” (p. 653). In some tests someone asked for a specific name or reply. Many responses had letters that were either before or after the correct letter in terms of their position in the alphabet and that had the same number of letters, responses that Richet tried to quantify.

Tests were also done using dowsing rods. In one group of tests several pictures representing objects, animals, and persons were placed on a table. Someone concentrated on a picture, while another tried to select the picture using the rod and its movements as responses. The rod was also used in other places and for other tasks, such as trying to find objects placed on a shelf in Richet’s library.

Features of Mental Suggestion

Richet noticed the faculty was “very capricious, wandering, uncertain” (p. 616). It manifested “in different degrees with different individuals” (p. 616). Richet referred both to target displacement and declines in the subject’s performance. The displacements seemed to be related to consistent confusions of one target for another.

Participants

Good results were obtained with adults “in good health, not hypnotized, nor hypnotizable . . .” (p. 632). Most of his tests were done with “non sensitive persons, such as my friends and myself . . .” (p. 632). Richet tested himself repeatedly. He posited that two participants in his studies, Mlle. B. and Mme H., were “very sensitive to magnetism” (p. 635), meaning that they were hypnotizable. These individuals were said to have obtained in some tests 22 hits out of 54 trials, where mean chance expectation was 10.

Tests performed with table tilting were done with five of Richet’s childhood friends. They were described as educated and intelligent, and lacking in mystical tendencies. Two of them, Gaston Fourier and Henri Ferrari, were said to be
mediums. While Richet and the others could not cause the table to move, his two friends could. It should be pointed out that both Fournier and Ferrari participated in many of the above mentioned tests which Richet said were conducted with non-sensitive persons.

**Explanation of Mental Suggestion**

Richet said that, regarding the explanation of mental suggestion: “Theory, explanation, is currently quite impossible” (p. 618). Nonetheless, while acknowledging that the process behind such phenomena was an unknown one, he speculated. Richet mentioned the possible existence of a force emitted by an agent, “such that the vibration of the thought of an individual influences the vibration of the thought of a nearby individual” (p. 617).

He further wrote that if a candle could “produce a very clear light at night at 200 meters from us, it seems absurd that at three or four meters of distance cerebral activity shows no action on close-by objects” (p. 668).

**Mental Suggestion and Science**

From the beginning of the paper Richet let his readers know of the controversial and improbable nature of mental suggestion. He said that the topic at hand was different from the “facts commonly admitted by science” (p. 609). The results of mental suggestion tests are “improbable facts; but their improbability is entirely relative; in the sense that none of them contradict the known facts, acquired by science” (p. 615).

In addition to warning his readers about the incredible nature of the phenomena, he cautioned them to keep in mind the “insufficience and impotence of current science” (p. 609) both to explain many facts of nature as well as mental suggestion.

**Mediums**

On a different topic, Richet also discussed mediums in terms of the unconscious mind. “In reality,” he wrote, “all the intelligent manifestations attributed to the spirits are due to an individual that is unconscious and active at the same time” (p. 650). But such individual (the medium) was not aware of this.

Richet speculated that these persons were in a state of hemisomnambulism in which part of the brain accomplishes some operations, produces thoughts, receives perceptions without the awareness of the self. The consciousness of this individual persists in its apparent integrity: all the very complicated operations are accomplished outside of consciousness, without the voluntary conscious self apparently feeling any modification (p. 650).

In this view a medium was a person showing “partial unconsciousness, a faculty by which part of its intelligence, of her memory, of her will, operates out of consciousness . . .” (pp. 650–651).
Concluding Remarks

Although Richet published several influential discussions throughout his career about mental suggestion, and what he later called lucidity and the sixth sense (e.g., Richet, 1888, 1889, n.d.), his 1884 paper is still considered a classic of early experimental parapsychology. Clearly, Richet had more to offer in this paper than card guessing tests and statistical analyses, the latter being of considerable importance. He presented information about reanalyses, targets, motor responses to targets, features of mental suggestion, and participants’ characteristics, and speculated that mental suggestion acted unconsciously, and that it may be related to the emission of unspecified vibrations.

Richet’s views of vibrations were consistent with ideas of human radiations to explain ESP, ideas that preceded and that were discussed around the time he was writing (Alvarado, 2006). Richet continued discussing these ideas in later years. As he wrote in the late 1920s: “The sixth sense is that one which gives us knowledge of a vibration of reality, a vibration which our normal senses are unable to perceive” (Richet, n.d.:224).

The involvement of non-conscious levels of the mind in telepathy and mediumship was an idea that started being discussed during the nineteenth century by writers with different conceptions about these hidden levels of the mind. Some had postulated that table-tilters produced the movement with their own hands but without their own awareness (e.g., Chevreul, 1854/1992), and that genuine (and veridical) mediumistic phenomena could take place thanks to unconscious reflex actions of the brain (Rogers, 1853/1856). Earlier in 1884 before Richet’s paper was published, there were discussions in publications of the Society for Psychical Research on both the subconscious aspects of motor automatisms and telepathy (Barrett et al., 1884; Myers, 1884). Richet (1886) himself discussed the topic two years later, but his discussions were never as detailed or attentive to the concept of a self-reflective subconscious mind as those of Myers (1884, 1885).

Richet’s paper is not only a testament to his creative talents, but also provides us with a fascinating view of concepts and methodology that were developed further in later years.

References


Rogers, E. C. (1856). *Philosophy of Mysterious Agents, Human and Mundane; or, the Dynamic Laws and Relations of Man*. Boston: J. P. Jewett. (Original work published 1853)
Abstract—One of the obstacles to progress in psychical research is irrational resistance to the phenomena. Among eighteenth-century Enlightenment writers, one type of resistance was evident that has persisted until present times. To illustrate, the present paper looks at David Hume’s discussion of miracles in his *An Enquiry Concerning Human Understanding* (1748/1955). Hume’s essay actually lays out a good case for some extraordinary events reported about the death of the Jansenist Francois de Paris—phenomena produced by the so-called “convulsionaries of St. Medard.” The contradiction is resolved by Hume himself, who naively reveals what motivates him to deny the overwhelming testimony he reviews: namely, his fear of validating religion. This paper notes the same pressure to deny “miracles” in another eighteenth-century writer, Edward Gibbon; Gibbon, however, unlike Hume, yields to the pressure of evidence and admits one startling instance of a well-documented preternatural event. A third figure from the same century is cited, a rationalistic *Promotor Fidei* of the Catholic Church, Prosper Lambertini, who, ironically, may be cited as having advanced the cause of the scientific investigation of psychic phenomena. The lesson from history is not to be seduced by stereotypes: an empiricist can deny and distort facts; a religious believer can be critical and objective.

Keywords: David Hume—miracles—convulsionaries of St. Medard—Edward Gibbon—Prosper Lambertini

Introduction
Sheer ignorance aside, some people resist the idea of the paranormal. There are many interesting reasons why they might. Telepathy, for example, could be seen as implying the loss of one’s inner privacy; if we accept telepathy, we would have to admit that other people might be able to “read” our minds, snoop on our inmost secrets. I once recoiled from a chance to speak face to face with Padre Pio for that very reason. People might resist psychokinesis because it suggests that black magic or sorcery might be effective, a frightening thought. Others might flee the afterlife hypothesis because it would force them to revise their assumptions and attitudes toward life. People might just resent having to admit they were wrong about a worldview they were deeply invested in.
David Hume’s essay on miracles exhibits a distinctively political form of resistance to the paranormal, an age-old polemic against repressive religion (Hume, 1748/1955). His essay calls attention to a contemporary outbreak of truly strange phenomena, a series of events enmeshed in a bitter controversy between Jansenists and Jesuits. On May 3, 1727, deacon François de Paris, a Jansenist known for his sanctity, died; on that day the first healing miracle ascribed to him was reported. The deacon’s gravesite became the scene, lasting for years and spreading to the environs of Paris, of other bizarre, incredible, and “miraculous” phenomena. Supernormal healings were widely reported, perhaps overshadowed by a different class of often grotesque physical phenomena, designed, however, to demonstrate the power of spirit over matter. Innumerable witnesses observed and wrote about these surreal happenings. Pierre Mathieu’s valuable study is available in French (Mathieu, 1864/2006), and for a taste of the marvels in English, the report of the arch skeptic Eric Dingwall is entertaining, shocking, and contains a good bibliography (Dingwall, 1962). To see the phenomena of St. Medard in their historical context as driven by people in the grips of apocalyptic passion, read the account of Ronald Knox. Knox is by no means a friendly observer, and describes the holy deacon who was the immediate cause of the outbreak as a man with an “instinct for starvation and squalor and complete self-obliteration” (Knox, 1950: 375). One thing all these writers, including Hume, agree upon: the convulsionaries of St. Medard behaved in astonishing fashion; their performances seemed to defy normal explanation. Even the enemies of Jansenism, the Jesuits, were forced to yield to the facts as attested by innumerable witnesses. Hume’s reaction to what were being called “miracles” is what concerns us here.

**Miracles and David Hume**

It was the age of revolution and the *philosophes*, and the new materialism had political consequences. According to the new view, the frequent enemy of science was organized religion, so one must pay heed to religious superstition, or what was taken to be superstition, and always be ready to extirpate for the sake of truth, progress, and freedom. As John Donne said in a famous poem, all “coherence” was gone with the rise of the new cosmology. A new coherence was rising, and it meant getting rid of the mysterious, the mystical, the supernatural; the specter that haunted Epicurus from classical times still had to be exorcised. Above all, miracles were not “coherent” with materialism. A new form of intolerance sprouted a new set of inquisitorial tentacles: “miracles” became a code word for anathema, and were pegged as the enemies of scientific and political progress.

Miracles were so disturbing to David Hume that he published his essay on the subject in 1748, hoping to devise an argument to “be an everlasting check to all kinds of superstitious delusion, and consequently, will be useful as long as the world endures” (Hume, 1748/1955: 118). Our author begins with the reasonable
Humean claim that experience is our only guide to the truth of matters of fact. Facts may be rare, ambiguous, elusive; or obvious, repetitive, and overwhelming. “A wise man, therefore, proportions his belief to the evidence” (Hume, 1748/1955: 118). For witnesses to a phenomenon, “the evidence, resulting from the testimony, admits of a diminution, greater or less, in proportion as the fact is more or less unusual” (Hume, 1748/1955: 120).

Notice that Hume did not say, like some modern anti-psychists, that “extraordinary claims require extraordinary evidence,” which is nonsense. The claim that Joseph of Copertino levitated is extraordinary; the evidence for his levitations was not extraordinary at all, but consisted of a large number of people who saw with their own eyes the saint rise into the air, in the same way they would have attested to the fact that a bird had entered the church and flown about (Bernini, 1752).

Hume hoped to make an argument that would once and for all silence anyone with a “miracle” claim. “A miracle is a violation of the laws of nature; and as a firm and unalterable experience has established these laws, the proof against a miracle, from the very nature of the fact, is as entire as any argument from experience can possibly be imagined” (Hume, 1748/1955: 122). The first dubious assumption here is that all the laws of nature are known; moreover, it is un-Humean and unempirical to assume that any “law” is based on “unalterable experience.” With a little sleight of hand, Hume rules out, a priori, any event that cannot be comprehended under the known laws of a given epoch. But this is a formula for squelching scientific progress. If we follow Hume, we would have to discard every new phenomenon that was inconsistent with well-established patterns of past experience. We learn what is matter of fact solely by experience, and can’t be absolutely sure that the sun will rise tomorrow, however well established an induction. It contradicts Hume’s premises to claim to know in advance that no new experience might violate a “law” based on customary experience.

Zeal to combat superstition and “enthusiasm” seems to have deformed Hume’s reasoning, blinding him to matters of fact that clashed with his beliefs and political passions. However, Hume places before his readers the evidence they need to refute his claim about the phenomena of St. Medard (see also Radner, 2003): “There surely never was a greater number of miracles ascribed to one person, than those, which were lately said to have been wrought in France upon the tomb of Abbe Paris, the famous Jansenist, with whose sanctity the people were so deluded. The curing of the sick, giving hearing to the deaf, and sight to the blind, were everywhere talked of as the usual effects of the holy sepulcher. But what is more extraordinary; many of the miracles were immediately proved upon the spot, before judges of unquestioned integrity, attested by witnesses of credit and distinction, in a learned age, and on the most eminent theatre that is now in the world. Nor is this all: A relation of them was published and dispersed every where; nor were the Jesuits, a learned body, supported by the civil magistrate, and determined enemies to those opinions, in
whose favour the miracles were said to have been wrought, *ever able distinctly to refute or detect them*” (Hume, 1748/1955: 132; italics added).

Hume adds a lengthy footnote providing further details in support of the extraordinary claims, including some useful bibliographical references: “Many of the miracles of Abbe Paris were proved immediately by witnesses of the officialty or bishop’s court at Paris, under the eye of cardinal Noailles, whose character for integrity and capacity was never contested even by his enemies.” The new archbishop did not favor the Jansenists, but Hume reports that “22 rectors or curés. of Paris, with infinite earnestness, press him to examine those miracles, which they assert to be known to the whole world, and indisputably certain” (Hume, 1748/1955: 132–135).

Hume then criticizes (we are still in his footnote) the Molinist (Jesuit) party for unfairly repudiating the case of Mademoiselle le Franc, but who “soon found themselves overwhelmed by a cloud of new witnesses, one hundred and twenty in number, most of them persons of credit and substance in Paris, who gave oath for the miracle.” Again, to underscore this: one hundred and twenty witnesses, most of whom were creditable persons and all who testified under oath that something inexplicable had occurred.

We are told of a “Mons. Heraut, the *Lieutenant de Police*, whose vigilance, penetration, activity, and extensive intelligence have been much talked of. This magistrate, who by the nature of his office is almost absolute, was invested with full powers, on purpose to suppress or discredit these miracles; and he frequently seized immediately, and examined the witnesses and subjects of them: *But never could reach anything satisfactory against them*” (Hume, 1748/1955: 136; again, italics added).

And neither does David Hume reach anything satisfactory against the reports he has before him about the extraordinary happenings at St. Medard. What then does he conclude from all this? Returning to the main body of his text, we read: “And what have we to oppose to such a cloud of witnesses, but the absolute impossibility or miraculous nature of the events, which they relate? And this surely, in the eyes of all reasonable people, will alone be regarded as a sufficient refutation” (Hume, 1748/1955: 137). And so, all the experiences of numerous, highly credible witnesses—placed in the balance with Hume’s belief of what is impossible—are reduced to nothing!

How to explain this slippage into self-contradiction, this reversion to pig-headed dogmatism? Hume was bewitched by a word, *miracle*, which he defines as something “absolutely impossible.” Because witnesses and participants of the Medard phenomena used the word *miracle*, which for Hume meant “violation of the laws of nature” and “absolutely impossible,” reports of them became in his eyes automatically incredible, no matter the quantity or the quality of the testimony.

Hume reveals his deepest anxiety when he writes, “…we may establish it as a maxim, that no human testimony can have such force as to prove a miracle, and make it a just foundation for any such religion.” Here it is plain what
Hume’s phobic reaction is all about; he is afraid of making a miracle the “foundation for any such religion” (Hume, 1748/1955: 137). Hume fears the possible exploitation of “miracles” by some religion, e.g., Roman Catholic. If Hume had dispensed with the politically charged terminology of “miracle,” and if he were less fanatical about his opposition to religion, it might have been easier for him to regard the strange phenomena more objectively.

Rather than make an argument that forever silenced all miracle claims, Hume showed how a person in the grips of a dominant idea, however great a thinker and congenial a human being, may suffer from intellectual blind spots and blatant self-contradiction. Hume had the information before him about what was happening with the so-called “convulsionaries” (in fact, not all the striking phenomena were accompanied by convulsions). Intellectual honesty forced him to present a fair account of events he concluded were based on a delusion. He described the facts and the testimony in support of them accurately, but refused to credit them as true. A common effect, call it Hume’s syndrome, we may define as a state of involuntary negative hallucination with regard to seeing or acknowledging facts that appear to disrupt one’s cherished worldview.

One shouldn’t treat Hume’s syndrome lightly. Bayle, Voltaire, Paine, Hume, Jefferson, and other Enlightenment stars all associated prodigies, wonders, miracles, and the supernatural with political and intellectual backwardness and oppressiveness (Earman, 2000). It is well known, for example, that Thomas Jefferson produced his own emended version of the New Testament; he accomplished this feat on his own in a few evenings when he had spare time. He deleted all references to miracles; Jefferson did believe Jesus was the greatest social philosopher of the ancient world.

Hume’s syndrome continues to animate irrational resistance to the paranormal. No doubt there are some factual reasons lurking behind this resistance, but there is a difference between rational caution and hysterical rejection. Occultists, spiritualists, psychical researchers are traditionally attacked, ostracized, and sometimes demonized by the scientific elite and by organized religion. Irrational resistance appears in many shapes and guises; there is probably more than a touch of it in all of us.

**A Confession of Edward Gibbon**

Another eighteenth-century writer critical of Christianity was the historian Edward Gibbon. But Gibbon, unlike Hume, had the courage to admit that not all crows are black. He is bound to have read Hume’s essay, with whom he enjoyed a warm friendship, and he might not have wanted to appear as stubbornly inflexible as his friend. Gibbon published *The Decline and Fall of the Roman Empire* in 1776, a book religionists attacked for its hostile stance toward Christianity. Gibbon lays bare impostures of piety, as he thought, the dream of Constantine and the story in Eusebius about a cross appearing in the sky during battle that led to Roman conversion to Christianity.
Busy lambasting all the “pious frauds,” Gibbon interrupts his narrative and writes: “Yet the historian who views this religious conflict with an impartial eye, may condescend to mention one preternatural event, which will edify the devout, and surprise the incredulous” (Gibbon, 1891: 600). Tipasa, a maritime colony of Mauritania, known for the zealous orthodoxy of its people, had fought off the Donatists and the Arians. Most of the Catholic inhabitants had fled by boat to the coast of Spain from the onslaught of a heretical bishop. Hunneric, who was in Carthage, sent a military dispatch to Tipasa, and the rebellious Catholics were arrested. Brought before the inhabitants of the province, their right hands were cut off and their tongues cut out.

The sequel was attested by the African bishop, Victor Vitensis, in a publication two years after the event; apparently, they whose tongues had been excised continued to speak. In Tipasa, about seventy individuals who had their tongues ripped out continued to produce articulate speech. Gibbon accepted the historical testimony for these strange events and used the word “miracle” to describe them, quoting Victor: “If anyone should doubt of the truth, let him repair to Constantinople, and listen to the clear and perfect language of Restitutus, the subdeacon, one of these glorious sufferers, who is now lodged in the palace of the emperor Zeno, and is respected by the devout empress.” According to this report, the phenomenon was ongoing, there were multiple witnesses, and the witnesses were credible.

Gibbon states that he was astonished to find “a cool, a learned, and unexceptional witness, without interest, and without passion,” the Platonic philosopher Aeneus of Gaza, who described his observations: “I saw them myself: I heard them speak: I diligently inquired by what means such an articulate voice could be formed without any organ of speech: I used my eyes to examine the reports of my ears: I opened their mouth, and saw that the whole tongue had been completely torn away by the roots; an operation which the physicians generally suppose to be mortal” (Gibbon, 1891: 601).

Gibbon refers to further evidence for this astonishing report in an edict issued by Justinian, an account in Marcellinus’s Chronicle of the times, and one of the dialogues of Gregory the Great. The enemies of Catholic orthodoxy, he notes, are prevented by an “incurable suspicion” from accepting the most “plausible evidence” for any Catholic miracle. Why? As Hume observed, to admit such “miracles” would authenticate Catholic orthodoxy and empower the dreaded Papism. Gibbon, who was anything but a Papist, saw this; still, he fought off Hume’s syndrome and chose to include in his history an account of at least one thing as outré as the events at St. Medard.

A Pope Critical of Miracles

Let me round this off with a comment on another eighteenth-century scholar, one who may be said to have consciously, and without condescension, introduced a more rational attitude to the study of “miraculous” phenomena. It
is ironical that this person was Prosper Lambertini, who was not only a Papist but also a Pope. Before becoming Pope Benedict XIV, he served twenty years (1702–1722) as Promotor Fidei, otherwise known as the Devil’s Advocate. The role of the Devil’s Advocate was to cross-examine witnesses and critically examine evidence for miracles and heroic virtue. The legalistic jousting was part of the beatification and canonization process. Admired for his tolerance, humor, and practical scientific rationalism, Lambertini’s job was to determine if an alleged miracle (paranormal) withstood critical scrutiny. Based on his experience as Devil’s Advocate, he published a four-volume study, Of the Beatification and Canonization of the Servants of God (1734–1738).

Unfortunately, an adequate translation into English is lacking; the best summary is available in Renée Haynes’s book about this Enlightenment pope (Haynes, 1970). What we find, unlike Hume, is a willingness to confront the entire range of human experience. Lambertini drew on common sense, history, knowledge of human nature, and the widest possible compass of recent scientific knowledge to determine if a claimed phenomenon could be explained in terms of known natural science. So comets and falling stars were treated as intelligible in terms of the new astronomy. There is much discussion of what today we call altered states of consciousness in the production of supernormal cognition. Since witnesses often claim that saintly persons emanate or are surrounded by preternatural light, he provides a detailed discussion of unusual natural luminosities. To determine what counts as normal for a period of time one can go without food or drink, he assigns Dr. Beccari the task of collecting all the known information on the subject, and doubts if preternatural fasting is proof of divine influence. Likewise, he reviews all the available data describing the natural circumstances in which a dead body may remain incorrupt, and here concludes there are cases that defy scientific explanation, but denies they are proof of holiness. Without using contemporary terminology, he clearly understands that some healings are psychogenic or psychosomatic and therefore not miraculous. He fully grasps the role of imagination and its possible pathological or therapeutic effects on the body. Without using current lingo, he grasps the concept of placebo. He sets up almost impossible criteria for “miraculous” healing, demanding that healings be sudden, complete, and lasting before they can begin to qualify as miraculous. Lambertini must be counted, with Robert Boyle and Joseph Glanvill, as an early pioneer in the scientific study of supernormal phenomena.

**Conclusion**

History is full of surprises and contradictions: a philosopher of genius, and radical exponent of experience, is blinded by his assumptions and cashiers whole dimensions of experience. On the other hand, an embodiment of “Papism” is fair, rational, and objective in his treatment of the same outlaw phenomena. So much for stereotypes. Hume’s syndrome pervades all walks of life and all points
of view. Irrational repression of the paranormal—of the different, the unexpected—can become so ingrained as to become a disposition. We tend to see what we expect to see and what confirms or is consistent with our worldview. Regardless of status, education, or accomplishment, an extra effort is necessary to see the gorilla in the room with us.

References


This is an informative, thoughtful, and engaging book about research on Near-Death Experiences focusing on the author’s attempt to study them systematically. For anyone who wants to know where NDE research stands today, this is a book to read, clearly written, entertaining at times, and a personal account by a young British physician specializing in pulmonary and critical care medicine.

Sam Parnia designed a prospective study of NDEs in patients that suffered a cardiac arrest at the Southampton General Hospital in England. He describes the difficulties he had when setting up his study, and how he wanted to test the out-of-body aspect of NDEs by hanging up 150 boards near the ceiling in wards, emergency areas and a resuscitation room. On their upper side were printed various images only visible from above and near the ceiling.

Within a year Parnia found 63 patients who survived a cardiac arrest (Parnia et al. 2001; Parnia & Fenwick, 2002). Four of them brought back from their coma memories that could be defined as NDEs according to the Greyson Scale, and two people experienced NDE-like features but scored too low on the scale to be defined as NDE. These experiences were feeling very peaceful, experiencing light, and seeing a tunnel and a relative who had passed away, all typical features of earlier NDEs studies. Not every person experienced all these features. None of Parnia’s patients described an out-of-body experience that is sometimes reported in NDEs, and hence its validity could not be tested by the numerous boards that were so painstakingly set up. The sample proved too small.

Other prospective studies conducted around this time (Greyson 2003; Van Lommel 2001; Schwaninger 2002) revealed that relatively few cardiac arrest patients do experience NDEs, namely 10, 12 and 24 percent respectively in these studies, a much lower proportion than orginally expected.

Parnia states that this was the first attempt to test the out-of-body state in this way. That is true for patients near the brink of death, but Charles Tart and Karlis Osis both made decades ago attempts to test self-induced OBEs in a similar manner and with some success.

The fact is—as this reviewer sees it anyway—that most aspects of the NDE are sometimes experienced without coma and without critical illness although in many cases death may loom around the corner, such as in deathbed-visions. In the study that Karlis Osis and I conducted in the seventies, there were many fully conscious patients who—shortly before they died—suddenly had experiences of great peacefulness and joy, of seeing relatives who had passed away, and who had experiences of otherworldly “lightful” beings (Osis & Haraldsson, 1986). If
we look at the 16 items of the Greyson Scale so widely used to determine NDEs, we find overlap with ecstatic/religious experiences as William James describes them. Some of James’s cases might even reach the critical number of seven to count as NDEs although they are not occurring near death, and they can certainly be a point of touch with what appears to be a reality beyond our ordinary conscious awareness, or as Karlis would have said, “glimpses of the world to come”.

As in death-bed visions, NDEs tend to occur primarily when the risk of death seems high, and Parnia’s finding that resuscitated patients that experience an NDE are significantly more likely to die within a month than patients without NDE, is a reminder of that.

Parnia attempts to test some theories that have been advanced to explain the NDE state, such as lack of oxygen to the brain. His sample was too small to generalize widely, but oxygen levels were higher in those who survived cardiac arrest than in those who had no NDEs. This comes as no surprise to this reviewer. When At The Hour of Death (Osis and Haraldsson 1986) appeared in Swedish a physician reviewed it for a major Swedish newspaper stating that deathbed-vision could be explained by lack of oxygen. Shortly afterwards I received a letter from a physician working for the Norwegian Air Force. He pointed out that aviation medicine had made many studies about the effect of lack of oxygen in pilots. He advised me to write to a friend who headed an institute for aviation medicine for the US Air Force and ask him what evidence there was that pilots experiencing lack of oxygen had hallucinations of persons before they fall into coma. In all the studies not one incident was found. The hallucinations that sometimes occurred were in the periphery of the visual field and consisted of geometrical forms, were not of persons living or dead, or imagery of the kind found in NDEs.

“What happens when we die” deals also with various theories of consciousness and how consciousness may emerge from activity of the brain. How can the paradox that the NDE presents be explained? “How can there be such clear and lucid thought processes when the brain is at best severely disrupted, and as far as we can measure, not functioning?”

It was my late colleague Karlis Osis’s dream to see a prospective study made of NDEs and deathbed-visions. Now we have four of them for NDEs and to my knowledge no one has described them as eloquently as Sam Parnia in this book.

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References


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This is a very readable book that gives, as the long title indicates, one man’s view of the complex joining of two worlds—the materialistic and scientific on the one hand, and the ephemeral realms of mind and experience on the other. Edgar Mitchell has an ease with physics and science that give his language and descriptions transparency that is good for the general reader as well as for techies. He is a very human fellow, whose life history (the framework for this book) includes triumphs like the Apollo 14 moon landing followed by his epiphany on the journey back to the earth he now sees as one with and fully connected to the universe. But he also honestly recounts his personal hurt when the board of the Institute of Noetic Sciences he founded decided to oust him from the president’s role during a period when research seemed less important at IONS.

Ed’s direct experiences are complemented by careful experiments that are, from a retrospective view, all designed to test and discover the nature of the central issues he ultimately describes in a dyadic model of mind and matter. He wants to solve the problems epitomized by the confrontation of duality and monism in philosophy, and he argues forcefully for his own model. I think there remains a lot of work to do to make it a fully functioning explanatory framework, but it does go a step beyond mere naming to suggest what can be done.

Mitchell cuts through verbal thickets where metaphors escape their role and confuse our thinking, using common sense in his simple but technically correct descriptions of quantum mechanical conundrums, among many difficult-to-translate scientific questions. He succeeds, for example, in making it clear why consciousness is an important aspect of QM, and shows how the conceptual structures for the material and the mental complement each other. He talks about the energy environment “and its patterns, which we call information,” laying the foundation for his dyadic model in which these separate and complementary aspects are seen as unitary in a fundamental sense. There is structure and
patterning in the material (energy) of the universe, and this makes all the difference. No rock or tree, no water or air exists without structure, and certainly no life. The material rest or remainder is a pile of bricks with no architect. It awaits the patterning that we represent formally as information, which creates all we observe and all we can imagine.

As a researcher in one of the edge sciences, I appreciate that Mitchell takes on the sort of scientific chauvinism which accepts amazingly unlikely things like the many worlds conception of “entire universes created by casual observation,” but not good evidence for psychokinesis or resonance of consciousness, which “can be observed and verified and do not violate conservation laws.” He is quite clear that such attitudes not only disrespect science itself, but risk missing the interesting bits while filling in the blanks.

Mitchell likes to turn an idea on its head, and this talent for independent thinking has served him well. His book is a kind of scientist’s report of a shaman’s world, with gems of wisdom from both. I suppose it is fair to regard the book as an instantiation of his dyadic model—if the reader is willing to look behind the veil of disbelief that seems to cripple so many who choose science as their way, it is possible he or she will find, as the shaman does, more than meets the eye.

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I was particularly keen to read this text having read Rick Strassman’s (2001) earlier book, DMT: The Spirit Molecule, in which he documented his extraordinary medical research administering the potent psychedelic neuro-chemical, dimethyltryptamine (DMT), to human volunteers. After receiving intravenous injections of DMT, Strassman’s participants reported a range of exceptional phenomena from entity encounters and alien abduction-like experiences to near-death-like experiences. The Spirit Molecule documented the whole experimental process whereby over 60 participants received a combined total of 400 doses of DMT. It concluded with the theory that the near-death experience (NDE) is caused by the action of DMT in the pineal gland, where Strassman speculates it is made because DMT is known to occur
naturally in the human body. The book currently under review, *Inner Paths to Outer Space*, is the natural sequel to that book in that it considers the DMT-induced entity encounters and alien abduction-like experiences from Strassman’s research in further depth, particularly in the contexts of quantum physics, science fiction and shamanism, proposing that access to alien worlds in outer space occurs in the inner space of the psyche.

Having four authors, three of whom hold medical degrees and one a PhD, the book could almost be an anthology, but it has just enough continuity in the chapters to read like a single text, the backbone of which consists of the earlier human DMT research of Rick Strassman, who contributes four of the 12 chapters. The first three of Strassman’s chapters essentially précis *The Spirit Molecule*, though this time without detailing the lengthy bureaucratic process he had to endure in attempting to conduct the research, and thankfully this time *Inner Paths* has an index as well as a notes section and references. These three chapters map out the bizarre territory of the DMT scenario, drawing on a number of examples of entity encounter experiences, especially of the alien kind, which are loosely compared to spontaneous alien abduction experiences. As in the previous book, it is pointed out that both DMT entity encounters and alleged alien abductions are experienced as being “more real than real” and, for instance, that both may feature alien operations and the insertion of probes.

Compelling as this cursory comparison is, a systematic analysis of the correspondences between these two experience syndromes would have been well received at this point in *Inner Paths*, because a more thorough analysis is still wanting – although Hancock’s (2005) recent book made some attempt at this. Nevertheless, despite the apparent similarities, the classic “greys” themselves are absent from DMT experiences, as Barus (2003) has pointed out. What is conspicuous from the examples of drug-induced experiences presented in *Inner Paths*, however, is the prevalence of contact with insectoid entities, particularly praying mantis-like beings, although it is not pointed out that these also frequently occur in abduction experiences. This strange fact, had it been mentioned, alongside the rest of the similarities between DMT and abduction experiences might indicate that we are truly dealing with the same entity encounters – one spontaneous, or perhaps hypnotically-induced, and the other drug-induced. Although, alternatively, the constructivist argument would suggest that the chemically-induced experiences have been influenced by the parallels drawn to alien abduction experiences extant in the public domain, especially those occurring since the most outspoken psychedelic commentator of the last 30 years, Terrance McKenna, reported contact with insectoid aliens on psilocybin a few decades ago (McKenna & McKenna, 1975). Nevertheless, the universal prevalence of particularly praying mantis-like aliens in DMT and other tryptamine reports seems worthy of more detailed investigation, as it seems to better support a kind of perennial philosophy rather than a constructivist argument – because this specific mantis feature appears not to have been widely
popularised in the psychedelic literature and yet it has been widely reported. Unfortunately, none of this is discussed in *Inner Paths*.

Aside from his three integral chapters, Strassman also adds a valuable chapter at the end of the book that acts as a fairly comprehensive guide on how to conduct psychedelic group experiences safely and meaningfully. This subtext of the book, as a guide to experiencing psychedelic states, happens to compose a fairly substantial theme, as the second of Luis Luna’s chapters also offers a guide to what might be expected when taking ayahuasca, the Amazonian jungle decoction that gives extended DMT experiences. The other of Luna’s chapters offers a fascinating, but condensed, biography of his extensive involvement in the anthropological and ethnobotanical investigation of ayahuasca-use in South America. Both of his chapters offer wonderful insights into the indigenous use of the DMT-rich brew, such as how the shamans introduce other plants to the ayahuasca so that their visions may reveal the healing properties of the added plant. Of particular interest to parapsychology, it is well known that ayahuasca was once called “telepathine” because of its apparent telepathic, precognitive and clairvoyant-inducing properties, but we also discover from Luna how shamans of the Shuar tribe take the decoction to also create the future, not just see it. Furthermore, it is described how alien abduction-like experiences also occur on ayahuasca, as we might expect, as well as out-of-body experiences, glossolalia, entity encounters and ostensible shapeshifting and past-life experiences. Given the similarities between experiences on DMT and ayahuasca, it is no surprise then that the chapter by Slawek Wojtowicz on psilocybin- and psilocin-containing “magic” mushrooms also recounts NDEs and science-fiction-sounding alien entity encounter experiences with this substance, because we learn that the active molecule in these fungal substances, psilocin (4-hydroxy-dimethyltryptamine), is a very close chemical relative of DMT.

Having laid out the basic background and phenomenology, however, it is not until we come to Ede Frecska’s three chapters about halfway into the book that we arrive at any concerted effort to account for the ontology of these exceptional tryptamine-family-induced experiences. His first chapter draws the basic distinction between scientism and the culturally relativistic approach to explaining the phenomena, and while he purports to offer a neutral argument, he clearly falls on the side of the latter – but not without good argument. Offering a number of examples of shamanic divination – explained either in the consistent terms of the action of spirits, or from a number of varying sceptical perspectives – Frecska deftly demonstrates that Occam’s razor is a double-edged sword for sceptics because under this rubric the consistent shamanic perspective has far greater parsimony than the numerous sceptical explanations.

Frecska then uses such further logical gymnastics to springboard into his own dichotomous conception of quantum-based psychology, in which the ordinary perceptual-cognitive-symbolic mode is contrasted with the direct-intuitive-nonlocal mode. The latter mode being one in which quantum processes
supposedly occurring in the brain’s microtubule system engage a state of interconnectedness that allows for parapsychological phenomena to occur. The implication being that DMT can activate such states, and this harks back to Karl Jansen’s (1999) earlier idea that Bell’s theorem arose in synchrony with the use of the psychedelic anaesthetic, ketamine, allowing humans to directly experience nonlocal space-time through the dissociative effects of this molecule.

Frecska then dazzles us with some further intellectual backflips, such as a poetic comparison between the mediumistic effect of channelling and the tunnelling effect in physics (although itself completely unexplained), because both involve the location of information where it would not ordinarily be expected. Yet, despite his obvious depth of knowledge on the possible quantum processes of consciousness, Frecska tends to assume the poetic appeal of his notions of local and nonlocal perception at the expense of acknowledging his theory really is just that, a theory, and is not actually substantiated by either physics or psychology thus far. Nevertheless, misquoting Einstein, imagination probably is more important than truth, and the 24 beautiful, futuristic colour plates of chemically-induced sci-fi landscapes and beings, by artists Pablo Amaringo, Karl Koefed, Robert Venosa, Martina Hoffmann and the author Slawek Wojtowicz, indicate that this book is as much, if not more, for a psychedelic sci-fi audience as a scientific one.

Frecska’s following chapter, however, goes a complicated conceptual step further and attempts to account for the alleged interspecies communication that occurs between shaman and plant by introducing the concepts of topological geometrodynamics, a concept so complex and yet so casually introduced into the text that the reader’s understanding must by necessity become the hostage of their imagination. As a psychologist rather than a physicist I would have found such intellectual quantum leaps exhausting had it not been for some of the more down to earth twists and turns in this chapter as support for the possible consciousness of plants, such as Darwin’s apparent conception of the root structure of plants as a neural network. This is no doubt a thorny point of departure from Darwinism for most modern scientists, but Frecska also confronts us with the fact that underground mycelia networks among single mushroom organisms can span 11,000 acres or more, and certainly have more interconnections than neurons in the human brain. Are these mushrooms in some sense conscious? Shamanic wisdom among those that consume psychedelic fungi for their input on the interspecies communication argument would say so.

Perhaps the most unsatisfying chapter of the book is Frecska’s third, which takes a perplexing tangent into the possibility of earthly paleo-contact with ancient alien entities by rummaging through a range of archaic Middle Eastern texts, including the Bible. After shakily building up this rather New Age idea over nearly 30 pages, Frecska casually knocks it down again at the end of the chapter by suggesting that such antediluvian alien encounters were actually DMT-induced entities experienced “... through nonlocal, extradimensional connections within the multiverse” (p. 254), whatever that means exactly.
I had mixed feelings about Frecska’s chapters, especially his last one, because on the one hand he made some observations highly contiguous with my own recent investigations into the ontology of DMT-induced entity encounters (Luke, 2008), such as the consideration of the Enochian “watchers” – the fallen angels – as possible DMT entities, and yet I feel he could have made a more concerted effort to offer some ontological speculations about the reality of these DMT entities, given that so much of the book is concerned with them. Rather we are left with the feeling that these entities are merely drug-induced, albeit by drugs that are naturally present in our brains and which may be able to help us access nonlocal information. But, does this imply a neurotheological-like reductionism for the existence of these entities, or a support for the perennial philosophy, or something else altogether? The authors don’t really speculate much on this, unfortunately, although in their defence neither do they ever promise that they will.

Finally, the last couple of chapters by discuss some other related aspects of the speculated DMT-alien matrix, such as the late John Mack’s research with hypnotically-recalled abduction experiences and Weiss’ past life regression therapy. Connecting the possible past with the possible future, Wojtowicz also discusses some of the apocalyptic future visions that can be found within the altered states literature, specifically that of the anthropologist Hank Wesselman and writer Gary Renard, who both foresaw a fair amount of planetary doom and gloom round the corner. Though some of it, especially that concerning the supposed “Westernercide” forthcoming from the new Iranian premier, might be best kept quiet.

In conclusion, however, I found Inner Paths to be a highly stimulating and worthwhile read, even though I was a little disappointed that the reality of the aliens wasn’t probed a lot more and that the insectoids weren’t satisfactorily dissected. I also think that, without a more detailed analysis of the discussed phenomena, Strassman might seem to be trying to have it both ways by proffering DMT as the cause of both NDEs and alien abductions. He might be right of course, but what then of the differences between aliens and NDEs, or can we expect a UFO waiting for us at the end of the tunnel of light when it’s our turn to do the mortal coil shuffle? Furthermore, Frecska also throws sleep paralysis into the DMT mix, but we might be more cautious of heralding DMT as the ultimate paranormal chemical catalyst, at least until further research can be done, because any one molecule that explains everything essentially explains nothing. In any event this book raises many fundamental questions about the nature of reality that have barely been asked in the scientific community, let alone answered, and I strongly urge all researchers of consciousness to read it.

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References


This absorbing book is the odyssey of Elizabeth Lloyd Mayer, a psychologist into the land of psi. The journey began with the search for a harp that had been stolen from her daughter in Oakland, California. The harp was a valuable instrument carved by a master and loved by the girl for its special sound. Mayer checked with the police, instrument dealers, and so on, all to no avail. A friend then suggested contacting a dowser and got her the phone number of Harold McCoy, president of the American Society of Dowsers. Since all else had failed, Mayer called McCoy. After a moment, he said the harp was still in Oakland, and if she sent him a street map, he would locate it for her. She did and McCoy called back and gave her the address where the harp would be found. Mayer posted flyers in a two-block area around the house, offering a reward for the harp’s return. Some days later a man called and said he had seen a flyer and that his next-door neighbor had shown him the harp, the caller offering to get it back to her. She was instructed to meet a teenage boy at a certain time and place and thereby received the harp. As Mayer drove home, “I had the thought, This changes everything” (p. 3).

Mayer was then teaching psychology at the University of Berkeley, was doing research on female development, was a training and supervising analyst at the American Psychoanalytic Association, and much else. She had a practice of her own and began listening more attentively to her patients. A deeply troubled woman, who had earlier insisted that she never dreamt, told her that she’d dreamed of Mayer going to Arizona, which was true, and which no one knew about. Mayer’s curiosity opened a floodgate to other such dreams that the woman had learnt to keep to herself (her parents had beaten her and called her crazy when she told them about her dreams). Mayer’s acceptance changed her
profoundly and positively. In turn, the patient’s dream took on “an enormous resonance” (p. 8) for Mayer. The result was a long-term research adventure that took three forms: talking with colleagues and professionals who, like herself, had experienced things that went against their beliefs; interacting with practitioners like McCoy; and talking to prominent psi researchers, such as Larry Dossey and Robert Jahn. Readers of this Journal may know this group, so I shall limit myself to the parts where Mayer explores less familiar territory.

A colleague said, “I woke up in the middle of the night like I heard a shot; the next day I found out it was exactly when my patient took a gun and tried to kill herself” (p. 5). Mayer was fascinated by the eagerness of her colleagues to share personal stories with her however weird they seemed, and then realized that people needed to integrate experiences that did not fit the accepted worldview. Another told her, “I was on a bus and all of a sudden found myself smelling the perfume my brother’s ex-wife used to wear. When the bus stopped, she got on. I hadn’t smelled that perfume or seen her in 30 years” (p. 6). Incidents like this caused Myers and a colleague to start a discussion group at the American Psychoanalytic Association, for people with anomalous experiences of their own or their clients. The stories came pouring in. A child psychologist related that a four-year-old girl, who was engaged in play therapy, suddenly turned and said, “Your brother is drowning—you have to save him!” (p. 14). It was October 2nd, the anniversary of her brother’s death by drowning years before.

Mayer was unable to decide which of two women to choose for a job, when a friend said he had just received an accurate diagnosis of a medical condition from Deb Mangelus. Mayer called Mangelus, mentioning neither her name nor dilemma, but Mangelus immediately homed in on the two women, “One is fiery, playful, someone you can have fun with. She has trouble with words. Maybe she’s not always reliable . . .” She paused, “The other person is different . . . She’s very responsible. Dutiful. Orderly. The funniest thing is happening . . . I keep seeing her hands and they’re clasped in her lap. I simply can’t get her to unclasp her hands” (p. 44). Mayer got “the same sudden sensation of my world shifting in some irrevocable way,” as the night of the harp. She said the first woman “seemed like she’d be enormous fun to work with, though her writing samples were terrible and I wondered how she’d handle details. I’d been less drawn to the other woman. She seemed great on details, but . . . struck me as boring. Even more to the point was this: The second woman had managed to sit through our entire-two-hour interview holding her hands firmly clasped in her lap.” When Mayer heard Mangelus, she thought, “She knows me better than I know myself; she’s gotten ahead of me in my own life” (p. 63).

Mayer did not know what the neurobiological basis might be for this type of knowing until she read a book by Andrew Newberg et al. (2001) that included studies of meditators. During moments of deep meditation or prayer, the posterior parietal lobe of their brain was unusually inactive. This is the part of the brain that orients us in relation to the environment. Mayer explains, “During
the subject’s moments of deepest meditation and prayer, what stopped firing were all the signals that tell us where to locate the boundaries that separate us from everything that isn’t us” (p. 65). This was consistent with Mangelus’s belief about her ability, “I know what I know about the other person because I go where they are. I draw on how connected we all are so I really am seeing with their eyes when I read them” (p. 67f). Mayer spoke with three other intuitives, John Huddleston, Helen Palmer, and Ellen Tadd, who had impressed her with their explanation of how they did it. Huddleston: “It’s ordinary because it’s just there, all that information about the other person. All you have to do is get yourself out of the way. We’re all connected, that’s the point. We don’t know it most of the time because we think we’d rather feel separate.” Palmer: “You read the other person accurately because you are them; you known them from the inside because you’ve stopped being separate” (p. 67). Tadd: “It’s a state of oneness, really–from that oneness you get a very profound knowing” (p. 68).

Mayer then turned to psi researchers. Physicist Harold Puthoff said he had invited Ingo Swann, the artist and psi sensitive, to Stanford University to test his ability. A physics professor, Arthur Hebard, had installed a magnetometer in a vault beneath the floor. Putthoff asked Swann to affect it, with the result that the rate of decay of the magnetic field in the magnetometer was doubled, to Hebard’s astonishment. Hebard then asked Swann to stop the field change, which Swann did for about 45 seconds. Swann then drew a sketch of the complex interior of the apparatus. The CIA got wind of the tests, and a 24-year, $20 million project was initiated to explore remote viewing by this and the Defense Intelligence Agency. This did not happen in a vacuum but was a Cold War spin-off. It was known that parapsychology was funded by Soviet security services, and the US needed to know whether and how psi might serve its own intelligence needs. This resulted in 266 papers by Puthoff and his associates, which seemed to provide conclusive evidence that remote viewing occurs and that it can be used to gather military information. The program ended in 1995, as the cold war wound down.

In 1988, in the midst of the government-mandated psi research, the National Research Council of the National Academy of Sciences wrote a report where it announced that it had found “no scientific justification from research conducted . . . for the existence of parapsychological phenomena” (pp. 130f). The NRC had commissioned Harvard psychologist, Robert Rosenthal to examine the evidence. But he and his associate, Monica Harris, reported that “the ESP ganzfeld studies regularly meet the basic requirements of sound experimental design” (p. 121) and that it would be “implausible” to suggest the results were due to chance. How did the NRC deal with the Harris-Rosenthal paper? They simply omitted it from their report.(2) Mayer examines other facets of the psi controversy, including a remark by a reviewer for a physics journal who rec-
ommended that an article about psi be rejected because “This is the kind of thing that I would not believe in even if it existed” (p. 133).

Mayer has a new way of understanding those who reject psi categorically versus those who accept it as a matter of course. She compares the two perspectives to a tool used by Gestalt psychology to illustrate their point that any experience gains meaning because it is part of a unified whole. The tool consists of drawings that represent different objects depending on how you look at them, such as a picture of a white chalice against a black background, or two profiles in black against a white black background, if you shift your point of view. Mayer’s point is that it is impossible for us to organize our perceptual field such that we can see both at the same time; you have to see one, then the other. With respect to psi, the problem is that the researcher is committed to the perspective of rational scientific thinking, which is incompatible with the mindset that governs psi. This problem is compounded by fear that arises from the likelihood that the border between self and other, including the researcher and his or her subjects may disappear in psi.

Mayer relates the figure-ground relationship of gestalt imagery to the concept of complementarity (introduced by quantum physicist, Niels Bohr) according to which two descriptions of nature may both be valid but cannot be observed at the same time. She brings in quantum entanglement where two subatomic particles that were previously connected will continue to intersect regardless of how distant they are from each other, a principle that has since been extended to objects on the macroscopic level. In other words, there may be a physical basis to the correlation seen, for instance, in remote viewing between the impression of the viewer and the remote scene.

As Mayer was struggling to capture the sense of knowing that she had been told about, she suddenly recalled an incident that happened long before her journey began. Her husband’s aunt had given him a gold watch that was too showy, so he gave it to Mayer’s sister, who was saying with them. She wore it daily, but was 17 and careless and would leave it at various places, until one day she couldn’t find it. The sisters sought everywhere, but it was gone. “At that point something happened that was unlike anything I’d ever experienced. I was standing... near the door of my husband’s study. I walked into his study: deliberately, intentionally, but with no awareness of volition on my part. It was as if I was watching myself in a slow-motion film. I walked straight to a closet... I’d entered maybe twice... over the course of our entire marriage” (p. 58). She reached in, and at the back corner of the closet, behind shoes and boxes, her hand went to a small leather case where she found the watch. She showed it to her sister. Then she returned it to the closet to “save face for everyone.” When her husband came home, she mentioned her sister’s panic, her husband saying he had found the watch in the bathroom and wanted to teach the girl a lesson before returning it to her. Mayer’s odyssey, which had lasted 15 years, five years longer than Odysseus’, had come full circle.
Mayer died on New Year’s Day, 2005, shortly after completing Extraordinary Knowing.

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Notes


2 The full story came out in an article by Daryl Bem and Charles Honorton in 1994 in Psychological Bulletin, a publication by the American Psychological Association. Bem was a social psychologist at Cornell, and Honorton, did the ganzfeld experiments that Harris and Rosenthal had praised. Bem and Honorton said that Rosenthal had told them that the NRC chair had asked him to delete the parapsychology section of the paper, and that he had refused to do so. Nevertheless it was omitted.

3 A more familiar example may be the drawing of an old hag, who becomes a young woman from another perspective.


Cognitive scientists spend a lot of time attempting to name the processes that go on in our minds (or brains, or both). The task is daunting because the tool that they use to make the description is the same as the object they are trying to describe. Inevitably, this seems to produce self-reference loops and insoluble philosophical puzzles.

In this book, the process that Burton wants to address he calls the feeling of knowing. This is a sense that we have about some particular fact or situation which transcends both evidence and rational thought. It is more like the primordial senses of sight, hearing, touch, taste, and smell, in that when we receive a message from one of these five senses, we are unable to deny its veracity. In effect, Burton argues that we have another sense, which he should
call the sense of knowing but instead calls the feeling of knowing. When this feeling is activated, our brains are not capable of denying it, that is, not capable of disbelieving it. His basic purpose is to point this out to us, and argue that just as with our basic sensations, a feeling of knowing is not something we choose, but something that happens to us.

There are sound biological reasons for Burton’s view, many of which he cites throughout the book. It is very common to see biological processes that have both fast and slow versions. The fast version is not usually terribly smart, but there is always a way in which it makes sense. The slow version is more deliberative, almost cognitive, and provides the possibility of correcting the excesses of the fast version. For example (not one of Burton’s) the immune system has a rapid non-specific response to a non-self entity that invades the body, and this response is very general, but not particularly effective. The slower immune response recognizes the chemical configuration of the invader, and mounts a very much more elaborate and targetted retaliation, which ultimately is much more effective. It does not seem to be an enormous leap to imagine that the brain (or nervous system) includes the same kinds of biological processes. A feeling of knowing would be something triggered very rapidly, and leading to immediate action, of the kind that would cause one to elude a predator, or to avoid being hit by an oncoming car. Such a capacity would have an obvious survival advantage, but perhaps only if paired with a slower, more rational process that weighed alternatives and considered chances, when immediate action was not required.

If this argument makes sense, then (as Burton suggests) we should have a visceral sense of knowing that can in some ways compete with our slower and more reasoned ways of knowing. Although he gives references to experiments that shed light on this hypothesis, they are fleeting, and more of them would have been welcome. He does point out, however, that in general when people are faced with a contradiction between their feeling of knowing and their slower rational responses, they inevitably pick the former. This should not be interpreted in some judgmental way, but rather in the same spirit in which we observe that optical illusions cannot be willed away (because they are created by the nerves behind the eye, before the signals are sent to the brain).

Burton’s distinction leads us to think about how knowing and the feeling of knowing relate to each other. There does not seem to be any problem when they coincide. Oddly, knowing without having the feeling of knowing can be demonstrated in physiological experiments (like blindsight). Here the person has the sense that they do not know something, but when questioned they do better than someone who genuinely does not know it. Almost certainly, the reversed situation is more frequent, in which the person has the feeling of knowing when in fact they do not know. We have all engaged in political arguments with know-it-all family members, rendering experiments in this area all but redundant.

The feeling of knowing concept has important implications for anomalies research. There is certainly no lack of examples in which a conventional feeling
of knowing has blocked scientific progress. Anomalists, as well as historians of science more generally, might be tempted to ascribe this to blindness, or arrogance. I think Burton would say that we have to recognize that the feeling of knowing is a genuine neurobiological phenomenon, that we do not really understand how it arises, and it is not under the control of the individual who has it. We would better take the attitude that as with any other human trait, we must at least sympathize with the bearer. The other side of the coin, of course, is the anomalist who cannot shake the feeling of knowing some particular phenomenon that conventional science rejects. Although Burton again does not account for how this feeling arises, he would regard it as a genuine mental state.

In the later parts of the book, Burton tends more and more into areas in which his own feelings of knowing may mislead him. He mounts a few strange little attacks, involving Andrew Weil (in which Burton reveals that, as a physician, he does not know the definition of alternative medicine) and Richard Dawkins (in which Burton equates the belief in the absence of a creator with a philosophy of pointlessness). Burton seems to have a feeling of knowing that there is something wrong in Dawkins’s view of the universe, but he does not seem to have a feeling of knowing of his feeling of knowing this, or at least not one that he can express to us. He then continues to spin this off into thin ruminations about the existence of God that do not really draw on his central theme of feeling of knowing in any informative way.

My attitude is that Burton is right when he identifies a feeling of knowing as something worthy of our attention, and I would be more than willing to imagine that it plays an important role in many parts of human life. For me it was, therefore, disappointing when Burton did not develop the consequences of this insight in any illuminating or convincing way. How do feelings of knowing arise, and how do they interact with slower rational beliefs? Are feelings of knowing as fixed as Burton implies, or can they be modified, and if so, how? Is it possible for those who have feelings of knowing to recognize that that is what they have, and do some people have the abilities to see these things more clearly, and perhaps alter what happens to them? If a feeling of knowing is an event that is imposed on someone from outside, then is there any sense in which we can hold them responsible for these kinds of beliefs, or the actions they engender? None of these (or other) questions are even hinted.

Burton is strongest when he brings in experiments, explains to us what they might mean, and uses them to support his idea of a sense of belief. He is weaker, and wanders about a bit too much in directionless speculation, when he tries to apply his basic idea in wider realms. While it is fine to ask questions, it is better to distinguish good questions from bad ones, and try to give some idea of what kind of knowledge (or belief) we would need to resolve the former, and in the later parts of the book this is what Burton mostly fails to do. However, the fact that a good idea, worthy of development, has not yet been fully developed, is no criticism of it. The implications of Burton’s thinking about the nature of some
kinds of beliefs may turn out to be fundamental for understanding the brain/mind in several distinct ways, and for that reason his book is well worth reading.

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At first The Deniers ... might appear to be about characters opposite in philosophy to that of SSE members. Not so. The deniers are highly qualified mainstream scientists in geology, meteorology, climatology, physics, and astronomy, often professors in respected positions and recipients of awards and prizes.

A columnist with the National Post of Toronto, Lawrence Solomon is an author of other books, an environmentalist and an activist. In the latter role Solomon strives to save the world’s rain forests and prevent nuclear power expansion. He works for an environmental group called Energy Probe. Despite these credentials, he has written an unusually accurate work about climate change.

To discredit dissenters from his views on climate catastrophe, the term “deniers” was coined by Al Gore et al. to place them in the same category as Holocaust deniers. Other forms of slander and intimidation are exposed by Solomon. The book was inspired by a bet by a climate “warmer” or alarmist that he could name three areas of climate science that were settled. Solomon showed that a credible dissenting scientist could be found to refute each one, winning the bet.

Subtopics of climatology were discussed in The Deniers ... along with the findings of one or more experts in each area. The 29 CVs of the “deniers” showed that they were usually more qualified than the alarmists making the doomsday claims. Several deniers are or were members of the Intergovernmental Panel on Climate Change, whose reports are normally presented as gospel by climate alarmists, media reporters and many politicians, but exposed as misleading or worse, at least in parts, by the deniers, and also by entire books and reports (Michaels, 2004; Singer, 2006, 2008).

Examples are given of punishment of deniers, no matter how good their science was, just for disagreeing and thus threatening the alarmists. Loss of
research funding, dismissal from expert panels, loss of office or status in scientific bodies as well as character assassination are all revealed.

Based on expert opinion, Solomon shows evidence that: (1) The 1990s were not the warmest decade in 1000 years, the period from 1100–1440 being warmer; and the evidence that the 1930s were warmer than the 1990s was given for the Arctic region, utterly uncorrelated with industrial CO2 emissions. (2) Storms are not more frequent or more violent in the last 20 years, but were probably most so in the 1940s in the last 110 years. (3) The Antarctic peninsula (2% of the area of the continent) has lost ice, but the rest of Antarctica is cooler since 1957 and has gained ice. (4) Global warming of about 0.5°C in the 20th century followed equal warming in each of the previous three centuries, an utter disconnect with the claimed CO2 levels in the air, which are not correlated with warming—the central dogma of climate alarmism. (5) Unusual even for climate realists, Solomon noted that CO2 levels were higher than now in pre-industrial times (p. 91), and mentioned Ernst-Georg Beck’s 2007 review of 90,000 direct chemical assays, but without the solid findings that those levels were over 420 ppm in 1823 and 1942, and the same as now in 1858. (6) Solomon showed that the ice core data for CO2 levels used by warmers was hopelessly unreliable. (7) Climate modeling was shown to be badly flawed mostly because it does not model cloud behavior. (8) Several solar effects were shown to account for the warmings and coolings of the last 400 years. These include changes in the output of the sun, changes in the distance of the earth from the sun, and changes in the sun’s ability to deflect cosmic rays from the earth. More cosmic rays mean more clouds, and lower temperatures, as in the Little Ice Age of 1600–1800. There were other aspects as well, all in agreement with a recent review in JSE (Kauffman, 2007).

On the other hand, Al Gore is taken to task for misinformation on temperatures, CO2 levels, storm frequency and severity, warming as a spreader of infectious disease, and misinterpreting the positions of his Harvard professor, Roger Revelle. An article in Cosmos in 1991 by Revelle and Prof. S. Fred Singer (Meteorologist, University of Virginia), “What to Do about Greenhouse Warming: Look before You Leap,” was seen by Gore as a threat to his intransigent climate positions. Gore tried to show that Revelle had become senile. Through another Harvard scientist, Justin Lancaster, Gore tried to have Revelle’s name removed from a proposed reprinting of the article, and accused Singer of using Revelle’s name over Revelle’s objections. Singer sued Lancaster, and with overwhelming evidence, won. “Quite recently, Lancaster retracted his retraction, claiming he had only issued the retraction in the first place because of the financial strain of the lawsuit.” (p. 197) Of course, this sort of fracas discredits many climate alarmist politicians and scientists and is discrediting all of science. It shows why so many climate realists (“deniers”) are professors emeritus like me with not much to lose.

On the downside, Solomon mentions water vapor as a greenhouse gas, but not
that it is by far the most important one. He cites a claim that ethanol production requires 1,700 L of water per L of ethanol produced, as though the water is changed to other molecules, and will not fall as rain somewhere else. Also, he does not seem to understand that the nuclear reactor that failed at Chernobyl, Ukraine, was an inherently unstable type never built outside of the former USSR or its satellites. He mentioned a reactor failure in Ontario, PA, which I could not locate. The only one I know of in PA was on Three Mile Island, which did not kill or injure anyone (p. 212). He equates reactors with nuclear device (bomb) proliferation, even though the CANDU reactor type from his native Canada does not need enriched uranium-235. He calls hydroelectric dams and nuclear plants “grandiose government-backed relics of yesteryear” without providing any steady, large-scale alternatives. On the other hand, Solomon sees environmental havoc from the new designation of “carbon” as a currency (p. 210).

Except for its last 3 pages, The Deniers . . . is highly recommended for its unique approach, solid climate science and some astute environmental understandings. It has many graphs, extensive citations and an index.

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References


Paleontology is supposed to have begun with Steno (Niels Stensen) in 1666, who hypothesized how shark teeth and seashells came to occur in rocks on mountains. Or maybe a century earlier with Conrad Gesner, whose 1565 book De Rerum Fossilium (On Objects that Are Dug Up) correctly identified fossil shark teeth. Or maybe even earlier with Leonardo da Vinci, who anticipated Steno in his private notebooks.

Adrienne Mayor, in her two wonderful books reviewed here, shows how myopic this perspective is. In classical antiquity, as well as by inferences made and transmitted in diverse nonliterate cultures, fossils were discovered and interpreted as once-living organisms.

Mayor is a folklorist of classical antiquity. In the course of her research for these books she visited a wide variety of places and talked with a wide variety of people. She even participated in some paleontological field work. Some previously lost collections were found from her efforts. She has managed to uncover a remarkably large amount of information that was previously unknown in a scholarly context, and to reinterpret a great deal more.

Her books are well written and are accessible to the general reader. There are many illustrations and some maps, and Mayor documents her statements by copious and detailed notes at the end of the books and a substantial bibliography. There are nevertheless occasional errors of various kinds, but they are all minor in this context. Mayor has made appreciable efforts to retrieve the names and other information on Native Americans associated with fossils.

The natural world doesn’t come to us with labels, and different cultures have interpreted fossils within their own cultural frameworks. ‘There were giants in those days.’ Yes, but not giant people. Bones and teeth of mammoths and other large animals have commonly been regarded as human by people unfamiliar with their living relatives, or who were otherwise constrained, even 19th-century farmers. About 15% of bones of supposed murder victims in FBI records are nonhuman. It’s outside Mayor’s purview, but relics venerated as remnants of Christian saints are commonly nonhuman. American mammoths were first recognized as elephantine by African slaves about 1725.

Mayor’s remarkable entry into the subject was with griffins. In classical Greece, at least from the 12th century BCE, they were taken to be real animals, with bodies
resembling those of lions and beaks (and later wings) like eagles’. They guarded gold in a far-off eastern land. The lore came from the Scythians, who lived in central Asia. The Greek poet Aristeas visited them about 675 BCE, as did Herodotus about 450 BCE as well as others later.

Eastern Scythians did mine gold from the foothills of the Altai Mountains around the western border of Mongolia and China; ‘Altai’ even means ‘gold’. Just east of the mountains is the late Cretaceous Djadokhta Formation, where white bones of the dinosaur *Protoceratops* are conspicuous in the red strata.

*Protoceratops* had four feet with big claws, and a large beak convergent on that of raptorial birds. Its ‘frill’, a backward and upward extension of the skull, was initially depicted somewhat accurately (even on a tattooed Scythian mummy), but it was later interpreted as a pair of wings. It’s quite clear that it was the original griffin.

Similarly, although with less complete evidence, the one-eyed cyclops that Odysseus killed in a cave was based on the skull of a dwarf elephant, the opening in the skull for the trunk calling to mind conjoined orbits. These elephants are found especially in caves in Sicily and Malta. Mayor didn’t originate this interpretation, but she has an original drawing of a cyclops based on Greek sculptures, next to one of a skull of a dwarf elephant.

Not all fossil mammals were interpreted as human. In the fifth century BCE bones on the island of Samos were recognized as belonging to extinct animals. Christian worldviews largely eclipsed the possibility of extinction until Cuvier about 1800. (Cuvier himself recognized classical paleontology, but his recognition was forgotten.) Even the religious liberal Thomas Jefferson, who had fossils in the White House and actually described a partial ground sloth (although he thought the large claw was from a lion) didn’t accept extinction, and one of his hopes for the Lewis and Clark expedition to the Pacific was that they would find living representatives.

The Greeks collected fossil bones for shrines (one had 2 tons of them) and stole them in war. Some Paleolithic people took fossils into their dwellings and graves, even a tooth of the extinct elephant *Stegodon* in the Congo. A vase from the sixth century BCE shows Heracles shooting arrows at a skull in a cliff: monsters came from the rocks. The killings of monsters by Zeus and Heracles were in places with large fossil bones. At this time Xenophanes found fossil fishes on Malta and on mountains and realized that the sea had once been there, and about 100 BCE Plutarch took fossil shells on mountains to be proof that all land was once sea. This view was apparently generally accepted then. (Did Steno read them? Leonardo didn’t, as he lacked education.) The emperor Augustus established a museum on Capri for large fossil bones. Theophrastus wrote a treatise on fossil fishes, now lost, about 300 BCE.

In the first century BCE Manlius placed the giants at a time when the mountains were still being formed, before humans. Native Americans had a similar perspective.
Native Americans are much more diverse than the classical Greeks, but for many of them fossils represented remains of the eternal struggle for natural balance among forces of the sky, water, and earth. They were things of awe and respect. Many land animals, such as mastodons and giant bison, were grandfathers of their corresponding smaller living representatives, but they themselves were killed by lightning from the sky. According to the Zuni, human ancestors resembled large salamanders. The greatest battles were between thunder birds, who threw lightning, and water monsters. Large bones being found in lakes and marshes, it was easy to interpret them as from aquatic creatures.

The Sioux recognized four kinds of thunder birds as well as many kinds of water and land monsters. Mayor suggests that one of the thunder birds was based on pterosaur fossils and two others on large birds such as condors and the Cretaceous *Hesperornis*. She may be right, but here and elsewhere the bases for some of the creatures and phenomena are less well established than she was able to do for classical Greece.

Some origins, though, do seem clear. The Iroquois, at least, knew Big Bone Lick, on the Ohio River downstream from Cincinnati. After one of them reported it they sent another party to verify it. Some of Jefferson’s bones came from here. In Sioux country thunder beasts lived especially in badlands, where large mammals such as Eocene brontotheres and Cretaceous *Triceratops* occur relatively frequently. Cretaceous mosasaurs were very probably one source for water monsters. Belemnites, elongate and pointed cones from Cretaceous squids, are commonly black. They were taken to be missiles associated with lightning.

The high plains are a rather dry region. The Pawnees, living in Nebraska and Kansas, interpreted large bones as from giants who died in a flood (and water monsters cause floods) and then sank into mud, which eventually became rock. Others regarded the giants as burrowers, living mostly where they died. Conspicuous and large helical burrows occur in some Miocene rocks, and paleontologists misinterpreted them for decades. But the Sioux had realized that they were formed by small beavers, as paleontologists eventually found too.

The Abenaki, of the Algonquin group, and the Iroquois had an oral record of a very large animal with a very tough hide, which strode through 8-foot snowdrifts and had a peculiar prehensile extra arm extending from its upper body. It’s unclear whether this is a memory of living mammoths or was derived from individuals preserved in permafrost.

The Thunderbird book is a quite different kettle of catfish. It gives evidence, of varying quality but none conclusive, for the existence today of giant predaceous birds in North America and elsewhere. The more common (or least rare) resembles the eagle-like *Teratornis*, supposedly extinct a few thousand years ago. The other is a giant owl such as *Ornimegalonyx*, of similar vintage; Hall calls it bighoot.

The evidence for these creatures is mostly reports of sightings, supplemented
by some Native American lore. Documentation of the evidence is excellent, and Hall has managed to come up with a remarkable diversity of sources. He is sometimes skeptical but is clearly a believer. He oddly reverses the burden of proof, saying that doubters should provide evidence that the reports are false.

When I was a student at Miami University, too long ago, a few boys from the next dormitory said that they saw a monster of some kind bounding up out of the ravine behind the dormitories and disappearing into the night. This managed to get into the newspapers and wasn’t refuted in print. I knew the boys, and, yes, it was a prank. Despite this, someone I mentioned the event to thought that it might actually have been real. Wishful thinking, of one kind or another, is surprisingly common.

Some of the reports are of this quality, although not necessarily thereby false. A few are much better, and their cumulative effect can be persuasive, if one is willing to be persuaded. They are often, but not always, mutually reinforcing in their descriptions. People who report such crazy things, accurately or inaccurately, can be subject to ridicule, and there is indeed a reluctance by most scientific naturalists to investigate, partly for this reason. Such considerations aren’t evidence, but they can affect what statisticians call prior probabilities and thereby somewhat influence conclusions. Whether such influence should be accepted is unclear to me, as a similar argument could be made for any nonsense claims.

Bigfoot reports are more common, and some of the apparent footprints provide a partly different kind of evidence. Here, though, the subject is well known and is therefore more easily amenable to deliberate or inadvertent copying. Such influence is less likely for most of the scattered megabird reports. Overall, the cumulative quality of evidence seems rather similar for the two cases.

Neither bigfoot nor megabirds are ecologically impossible, although any megabird population must be quite small and therefore vulnerable to extinction. It would be good to keep an open mind, but not so open that anything can enter unimpeded.

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This book focuses on one-dimensional biosignals, that is, time series of measurements commonly made in physiology or medicine. Analysts of such signals tend to fall into several different camps. One large group concentrates on the conventional interpretation, that a biosignal consists of a message plus noise, with noise being described in probability terms. They are concerned with using the message to model the underlying biological process, often in order to design therapeutic strategies. Another perhaps smaller but growing group of analysts consists of those who look at a biosignal as the result of a deterministic process, involving the ideas of complexity, chaos, fractals, and so on. West’s book is clearly in the second camp, but rather than address issues separating “probabilists” from “determinists”, he trains his guns on the primary medical use of such biosignals, which is to determine “normal ranges” and define diseases as systematic departures from these ranges.

Along with his fellow determinists, West believes that biosignals contain more information than is generally thought. What the probabilist sees as noise, West sees as an opportunity to get a deeper understanding of the mechanisms that produce the signals. The tool he wants to use for this purpose is the so-called “fractal dimension”, a measure of the roughness of the biosignal, which was defined by Hausdorff and improved by Besicovitch in the first half of the past century. The basic idea is that a reasonably smooth curve (one that has a derivative, for example), plotted on a graph, is a one-dimensional object, no matter what sensible definition of “dimension” we use. One of the discoveries credited to Mandelbrot (although invented, but not developed systematically, much earlier) is the notion that some curves are so irregular that they have a higher, fractional dimension (between 1 and 2 in the case under consideration). One of the several competing definitions of “fractal” is a curve whose dimension (in the Hausdorff-Besicovitch sense) exceeds its usual topological dimension.

In the first two chapters, West introduces the role of probability in the study of disease, and recalls the interesting combat between Bernoulli and d’Alembert, which concerned whether probabilities should be interpreted in terms of groups (Bernoulli) or individuals (d’Alembert). West’s purpose is to indicate that Bernoulli’s victory had important consequences for how clinical medicine and epidemiology subsequently developed, and that perhaps the current volume in some sense digs up d’Alembert’s arguments for reconsideration. He reviews some of the biosignals he intends to study, which include heart rate, respiration, and temperature (he adds gait, gut measurements, and neurons later in the penultimate chapter). His major point is that conventional science concentrates on the averages of these biosignals over various time periods, and ignores the possibility that their shapes or patterns within the time periods might contain even more valuable information.
The next two chapters are in some ways a diversion from his central mission. West spends considerable time discussing probability distributions with tails that are thicker than the Normal (or Gaussian) distribution, beloved of statisticians. His basic message appears to be that the history of applied probability advanced by adopting the law of “errors”, developed for the purposes of scientific measurements (mainly by Gauss), as if the same rules would hold in areas such as human behavior (mainly by Quetelet). Thus the quantifiers in the soft sciences, and in medicine, ignored the very real possibility (and to West, a certainty) that exceptional observations would be made in ordinary circumstances far more frequently than predicted by the Normal distribution. West lays very heavy emphasis on “inverse-power laws”, that is, an association between 2 variables that is linear when their logs are plotted against each other (a situation I will call “log-log-linear”). Such relationships occur quite frequently in a widely scattered variety of fields, and West evidently takes this to mean that they are important because they are ubiquitous.

The real message of the book then starts to appear in Chapter 5. West concentrates on a sort of scale-invariance to characterize fractals, which is linked to the earlier discussion by the fact that probability distributions with inverse-power tails are invariant under scale transformations. He goes on to introduce allometric relationships, rather casually given the large part they play in the remainder of the book. West concentrates on cases in which a time-sequence of values is available, the sequence is divided into non-overlapping, contiguous groups each of the same size, and a new sequence of sums (one for each group) is computed. He computes the variance of the original sequence, and then the variances of each of the sequences of sums, for groups consisting of 2, 3, 4, and so on, observations per group. Finally, he plots the log variances against the log average sums, computes its slope S, and then the fractal dimension $2 - S/2$. He applies this methodology to single cases of heart rate, breathing rate, gait, body temperature, gut function, and neurons, in each case computing a fractional dimension and concluding that the biosignal has fractal structure. In the final two chapters West discusses cases in which diagnosed pathologies can be detected by his fractal analysis, with heart rates dominating, if for no other reason than that more money and effort has been put into this area than the others. He speculates that therapies based on fractal principles will be more beneficial than the simplistic control-based therapies that dominate medical thinking. One of the more interesting conjectures here is that fractality is related to the ability to respond adaptively to a changing environment, which would account for the observation that diseased individuals (in cardiology, at least) seem to show either too much or too little irregularity.

I will couch my criticisms of the book in terms of my purpose for reading it, which was to see whether it would be appropriate for graduate students needing an introduction to the potential role of complexity theory in biomedical research. From this perspective, the book is narrow in several different ways. West’s concentration on the log-log-linear phenomenon leaves out many of the other approaches that have been used on biosignals. In fact, it leaves out some of the
considerations in estimating fractional dimension altogether: see Carr and Benzer (1991). Aspects of complexity, other than fractals, get very short mention. The book gives the impression of being more interested in the log-log-linear approach than it is in understanding how the biosignals might be generated. Mathematical formulas have been assiduously avoided throughout, forcing the definitions and explanations to become vague, or opaque, or occasionally misleading. Even further, assertions are frequently made with no demonstration, no real suggestion why they should be true, and no further reading for the student to follow-up on.

This latter aspect is particularly concerning, in that the dominant analytic method (log-log-linear plots) is never fully explicated. West asserts, but does not argue in favor of the connection between allometry on the one hand and fractals on the other. In computing variances of aggregated sums, he seems not to realize that for correlated data (which certainly includes biosignals), the sample variance of the sums does not estimate their population variance. In fact, what he computes for a group of size m, and calls a variance estimate, is actually an estimate of \(m^5 \sigma^2 (r_m - r_n) n - m\), where \(n\) is the number of observations in the original sequence, \(\sigma^2\) is the variance of an individual observation, and \(r_m\) is the average (including the diagonal) of all the correlations in an \(m\)-by-\(m\) diagonal block of the correlation matrix. (Technically this requires the time-series to be weakly stationary, but a corresponding, more complex formula holds in the general case.) Thus, West’s method is based on an index that in a round-about way captures how fast correlations between observations drop off as they are further apart in time. It seems reasonable to expect that a conventional correlogram would do a better (and more understandable) job of this, but correlograms are not to be found in this book.

There is a large amount of space given over to the material on probability distributions with tails thicker than the Normal. West gives no actual examples of distributions with inverse-power tails, and he implies that the only examples are the symmetric stable distributions of Levy. First, a ratio of 2 independent Gamma variates has an inverse-power tail, and secondly, except for the Normal, all Levy distributions have infinite variance, which makes it hard to explain how they can apply to physiological measurements that are, by their very nature, bounded.

For students it is often important to have good definitions of terms, and good examples that they can examine to cement their understanding. I would have welcomed improvements in both of these areas. For example, when it first appears, a fractal is defined as a “non-analytic” curve. Defining something by what it is not tempts confusion, and in any case this would create a problem for anyone who knew the mathematical definition of “analytic function”. West does go on to give a few illustrative examples, but given the central place that the concept of fractal holds for him, it is odd that he did not take the time to explicate some of the many simple examples that have been well-presented in existing books and websites (such as Barcellos, 1984). Similarly, the definition of fractal dimension could certainly have been developed in a satisfactory manner, as in Kinsner (2005), for example.
As an aside, I found myself having some problems with the notion that inverse-power laws are ubiquitous, in biomedicine or anywhere else. There is an interesting, if antagonistic, literature on a presumed power law relating metabolic rate and body size, but even the critics of this specific value do not question that there is some power law (which might vary between species, for example). I performed the numerical experiment of generating a standard Normal probability density $p(x)$, and then plotting $\log p(x)$ against $\log(x)$ for extreme values of $x$. I had no difficulty finding cases which, over a suitably selected range, were exceedingly linear (correlations about 0.999). (Recall that West implies that log-log-linearity in the tails invalidates the Normal distribution approach.) It is thus disturbing how many of the presumed examples of log-log-linearity are based on narrowly selected ranges in the extreme of a distribution, where potentially almost everything might look log-log-linear. This is not, it is to be emphasized, a criticism of West’s book, but of the researches upon which it is based.

In the end, I concluded that the book was not appropriate as required reading for students, although given enough available time it could be optional. Despite this, I enjoyed reading it myself, since West is a very good writer, and has the rare ability to make complicated material understandable in language that most of us understand. The book is also filled with the kinds of by-ways and historical references that are too often left to the side.

Finally, I believe that the book is not titled correctly. It should have been something like “What physiology has ignored; discovery of the role of fractal signals.” I say this because therapeutic medicine makes virtually no appearance in the book, no evidence is presented that medicine has gone wrong, “rediscovering” complexity implies that it had already been discovered but somehow lost, and the book is about one-dimensional fractal signals, which is not the same as complexity.

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References

We can thank Robert and Ryan Wood, father and son, for publication of this curiosity, which came to light in the course of their ongoing inquiry into UFO cover-ups. The Woods have focused much of their investigation on deeply controversial documents of disputed provenance, alleging a highly secret government agency, Majestic 12, said to oversee studies of extraterrestrial wreckage and bodies. Most of my ufologist colleagues—I, too—harbor grave doubts, but the authenticity of An Encyclopaedia of Flying Saucers is not open to dispute.

Three hundred thirty-three pages of the manuscript arrived in the mail one day in June 1999, the recipient a Bear Lake, California, man, Timothy Cooper, who was also supplying the Woods with numerous supposed MJ-12 documents which he averred had arrived anonymously. The return address on the package indicates that the manuscript was mailed from FREEDOM OF INFORMATION/PRIVACY OFFICE, US ARMY INTELLIGENCE AND SECURITY COMMAND in Fort Meade, Maryland. In subsequent investigation the Woods determined that the listed author, Vernon Bowen, had really existed, though he is now deceased. An advertising writer who worked for a Manhattan agency, Bowen lived in Old Greenwich, Connecticut. His son Patrick provides the introduction to this, the first publication of the book. He confirms that it was put together between 1957 and 1961.

Owing—so one infers from the written evidence—to an unabashedly worshipful view of the military, Bowen submitted the manuscript to the U.S. Air Force for clearance. To the best of anybody’s knowledge, the Air Force, presumably out of indifference, kept the manuscript and never got back to Bowen. It showed up decades later, with cryptic annotations (not included here but mentioned by Dr. Wood in an afterword) supposedly composed by cover-up insiders. Fortunately, the version here is as originally written, though several pages of the manuscript are missing. They are not, however, missed.

Books identified as UFO encyclopedias began appearing in the 1970s. The first of any real merit were both issued in 1980, Ronald D. Story’s Encyclopedia of UFOs and Margaret Sachs’s The UFO Encyclopedia. Others appeared in later years. My own multivolume UFO Encyclopedia went through two editions (the first in three volumes, 1990, 1992, 1996, the second revised and expanded in two volumes, 1998). So Bowen’s bears the distinction of being an early attempt, though it’s less an encyclopedia than an annotated guide to the public UFO literature up to 1960. In some ways it reminds me, at least broadly, of M. K. Jessup’s The UFO Annual (1956), notwithstanding obvious differences, one of them that Jessup’s book is more readable and entertaining. I suspect that Bowen would have a hard time finding a commercial publisher.

Nobody seems to know why Bowen, author of several children’s books,
elected to devote so much attention and labor to this manuscript. To the outsider, a mountain’s worth of effort may look like a molehill’s worth of results, at least from a strictly critical point of view. Still, it is a genuine oddity, not least because of Bowen’s apparent love-hate relationship to his subject, evident throughout but expressed explicitly in a blizzard of distance-establishing scare quotes at the commencement of the foreword:

I have never seen a “flying saucer.” I don’t know anything about “flying saucers.” I am not a “flying saucer” expert. I am not a member of any “flying saucer” club. Furthermore, I do not intend to be. I have never gone to a “flying saucer fan” convention. I don’t intend to. I am not a “flying saucer” fan. I don’t intend to be.

In the unlikely event that the reader has missed the point, Bowen goes on in that vein for yet more sentences. It soon becomes apparent that this book might have been a better one if Bowen had immersed himself in the UFO subculture of the 1950s. He gets no closer to it than Fate magazine. Beyond that, his sources are the New York City metropolitan newspapers and popular magazines.

At some point I realized that I had read nearly every item he cites as I was researching my own UFO encyclopedia. The material is not without interest or significance as a measure of how the idea of flying saucers was passing through the culture. Unfortunately, Bowen, no intellectual, not even a notably thoughtful lay observer, has little if anything of consequence to pronounce about such matters. Coverage of both UFO sightings and the emerging UFO subculture in mainstream publications was hit and miss, consistently superficial. To understand what was going on in those days (at least in America), one needs to read, on one side, C.S.I. Bulletin, The U.F.O. Investigator, and The A.P.R.O. Bulletin, where sightings were investigated and analyzed, and on the other, Saucer News and The Saucerian Bulletin, which chronicled the outlandish, mystical-religious aspects of the movement.

By the end Bowen has persuaded himself that UFOs are top-secret aircraft, a notion that more informed students of the phenomenon had abandoned years earlier and which practically nobody has revived since.

If not much in itself, Encyclopaedia will be welcomed by scholars seeking to document the UFO phenomenon’s social history, to which the book is a small footnote. An additional item of curiosity, of course, is the circumstance under which it surfaced.

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**FURTHER BOOKS OF NOTE**


This is a revised edition with two new chapters of a book with a similar title by Noreen Renier with Naomi Lucks, published by Berkley Books/Penguin, 2005, which I reviewed in *JSE 20*, no. 1, Spring 2006, pp. 135–141.

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**ARTICLES OF INTEREST**


Suppose our world were exactly as it actually is today, except that every person in the world, with very few exceptions, believed the Earth to be flat. I don’t mean that people had debated it, and had decided that the Earth is flat; I mean that they believed it so deeply that the question could not arise: that anyone who raised any alternative seemed a nut.

In such a world you would no doubt, every so often, see a *ScienceNOW Daily News* article entitled something like, “Airline Fuel Tables Get Spooky,” describing how a new audit of airline records, once again, for the *n*th time, revealed the “spooky” fact that the using of Zweistein’s famous equations in setting airline routes had verified that going a much *longer* distance resulted in *less* fuel use; and, even spookier, took *less* time than going the shorter direct route across (flat) Earth. (Of course the word flat would be absent from the report, for that went without either saying or thinking.)

And suppose you were one of the few people who had awakened to the fact that the Earth is *not* flat, and that Zweistein’s famous equations are merely trivial spherical trigonometry? And then, suppose that you were invited to write an Articles of Interest piece on “Airline Fuel Tables Get Spooky”—what would you do?

That is precisely the position that I find myself in. In that mythical alternative world, the problem was that almost everyone adhered to a fundamental view of the nature of the world that was simply wrong. The problem in *our actual* world is exactly the same: the overwhelming majority of people, including, I fear, the
overwhelming majority of PhD physicists, adhere to a fundamental view of the nature of our world that is simply wrong.

The difficulty is reality. Almost everyone thinks that the world is real. But we know that the world is not real. While science cannot establish that something is true, science can establish that something is not true. In fact, that is the essence of science—that hypotheses are falsifiable. Note: not verifiable, falsifiable! And it is a matter of fact that we have verified that the possibility that what you are observing is a real world, can, and must, be rejected. Sir Arthur Eddington and Sir James Jeans recognized this immediately when quantum mechanics was discovered in 1925. Einstein realized it too, but feeling that it could not be true, he spent the rest of his life trying to break quantum mechanics, with complete lack of success. Anton Zeilinger and his colleagues have recently experimentally demonstrated that reality can be ruled out (Nature 446, 871, 2007).

I have created an illustration of the famous John Wheeler delayed choice experiment (http://henry.pha.jhu.edu/unreal.html). What this experiment shows, is that Schrödinger’s cat’s history is determined by your observation: “If you find a dead cat, an examination by a veterinary forensic pathologist would determine the cat to have died eight hours ago. Your observation not only creates a current reality, it also creates the history appropriate to that reality” (Rosenblum and Kuttner, Quantum Enigma, Oxford, 2006). This is where evolution comes from! The most recent experimental verification of the delayed-choice result is by V. Jacques et al. (Science, 315, 966, 2007).

Quantum mechanics is not spooky, and is not even slightly mysterious. No more than spherical trigonometry! Newton’s $F = ma$ follows very simply from Schrödinger’s equation, which, in turn, can be derived assuming simple symmetries (Henry, R. C., 1990, Am. J. Phys. 58, 1087; Shapiro, M., 2008, J. Phys. A: Math. Theor. 41, 175303).

What is spooky, of course, inconceivably spooky, is our own existence (that is, our minds’ existence) and the fact that we make observations. Physics does not even address these questions. The universe being purely mental begs the question of other minds—I resolve it, now, by belief in God—I dropped my atheism in 2004; not easily, but decisively.

It is also spooky that Galileo was able to educate the world to understand that the Earth goes around the Sun (and what could be spookier than that?), yet physicists today have utterly failed to inform the public to understanding the purely mental nature of the universe, with all that that implies for the meaning of human existence. That is a tragedy, and it should be rectified. I wish I knew how.

Wilhelm Reich, the physician, psychoanalyst, and scientist, was an extremely original and prolific investigator of a variety of scientific disciplines all bearing on the nature of emotions, and a mass-free “energetic” substrate in humans, the atmosphere, and the cosmos that Reich called “orgone energy”. During his 60 years of life he published over a dozen books and numerous articles describing his discoveries mostly in his own press. After Reich’s death in 1957 articles on orgonomy, mostly of a clinical nature, were published in the Journal of Orgonomy, the organ of the American College of Orgonomy and Annals of the Institute of Orgonomic Science. Since 1989 James DeMeo’s Pulse of the Planet is the only journal devoted almost exclusively to articles on orgonomic science and allied scientific findings rather than clinical articles. Pulse is published sporadically, there being five issues published to date. The last, Heretic’s Notebook, was released in 2002. The journals are described as the literary organ of DeMeo’s Orgone Biophysical Research Laboratory in Ashland, OR. Anyone with an interest in Reich’s work and/or our environment will find the entire set well worth study.

James DeMeo received his doctorate in geography from the University of Kansas. His doctoral dissertation title “On the Origins and Diffusion of Patrism: the Sharasian Connection”, based on Reich’s observations on desert formation, was published in 1999 as Saharasia: The 4000 BCE Origins of Child Abuse, Sex Repression, Warfare and Social Violence, in the Deserts of the World. Since then DeMeo has focused his considerable talents of observation, experimentation, and analysis to the replication of the scientific findings of Reich, informing others about them through Pulse, conducting workshops at his laboratory in Oregon and giving public lectures on Reich’s and his own work in the U.S. and abroad. DeMeo is one of a handful of responsible scientists in the world investigating Reich’s non-clinical work and utilizing Reich’s invention, the “cloudbuster” to modify the weather, which he has done, often in collaboration with others, in varied locales in the U.S., Europe, and Africa. In the various issues of Pulse he informs us of the results of these many activities.

Each issue of Pulse consists of formal, scholarly articles replete with extensive bibliographies and of briefer personal notations on changes in the climate, eco-systems throughout the world and advances in science. Volume 1:1 published in 1989, for example, has four major articles, “Cloudbusting”, “New Approach to Drought”, “Response to Martin Gardner’s Attack on Reich and Orgone Research in the Skeptical Inquirer”, and “Postscript on the Food and Drug Administration’s Evidence against Wilhelm Reich”, all by DeMeo, and an original article by Yoshio Kato, Head of the Department of Aerospace Science at Tokai University, Japan on “Recent Abnormal Phenomena on Earth and Atomic Power Tests”. There is also an extensive section on planetary climate
features and unusual phenomena, and briefer research reports, including DeMeo’s work in weather modification.

Volume #4, published in 1993, is dedicated to Reich and Reich’s early work. It contains three classic articles on orgonomy by Reich, translated by Barbara Koopman, M.D. Ph.D. These are: “The Basic Antithesis of Vegetative Life Functions”, “The Orgasm as an Electrophysiological Discharge”, and “Experimental Investigation of the Electrical Function of Sexuality and Anxiety”. These are the basic experiments and theoretical constructs that eventuated in Reich’s later discovery of orgone energy, not to be missed by anyone seriously interested in this subject. While these papers have since been reprinted in other formats, the wonderful paper by Ellen Siersted, “Wilhelm Reich in Denmark”, has not. Siersted was a patient and student of Reich and well documents this time when Reich was separating himself out from psychoanalysis, discovered muscular armoring, and was beginning his experimental investigations in the bioenergetic basis of the emotions. There are many photographs of Reich with his colleagues, his girlfriend, Elsa Lindenberg, and his children, Lore and Eva.

Other authors in this issue are Lois Wyvell, who did organizational and secretarial work for Reich; R. D. Laing, the British psychologist; Mitsuru Katagiri, Dean of Academic Affairs, Kyoto Seika University; Aileen Smith; and Matthew Appleton, a houseparent at the Summerhill School, Suffolk, England. Wyvell writes on “The Jailing of a Great Scientist in the U.S.A., 1956”; Laing writes on “Why is Reich Never Mentioned?”; Katagiri and Smith on “Three Mile Island Revisited”, reprinted from Kyoto Review; and Appleton writes on “Summerhill at 70”. Summerhill is the radical school devoted to self-regulation, founded by the educator, A. S. Niell, who was one of Reich’s best friends. These are all informative well-written articles having in common serious critiques of how society constrains the spontaneity and creativity of its finest minds and pollutes its own nest.

Heretic’s Notebook, volume 5 of Pulse, from here forward referred to as notebook, is a thicker version of the usual Pulse, and features many more articles by different authors than the usual Pulse. The articles are a varied lot, ranging over the entire spectrum of ergonomic thought and work. They include Eva Reich’s (Reich’s daughter) work on infants and children; DeMeo’s update of his original research on the origin of patriarchy in Saharasia; a lecture on ergonomic functionalism given in Berlin by Myron Sharaf, the author of Fury on Earth, an excellent biography of Reich, and the editor of some of Reich’s journals; “CSICOP, Time Magazine, and Wilhelm Reich” by John Wilder; “Childbirth as a Sexual Process” by Matthew Appleton; and “Giordano Bruno’s Philosophy” by Carlo Albini.

Articles on bions (primitive non-living microscopic vesicles with many of the qualities of life) and biogenesis include Professor Bernard Grad’s (a former professor of biology at McGill University and research associate of Reich) “Studies on the Origin of Life: The Preparation of Primordial Cell-Like Forms”,

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“Some Observations on Reich’s Experiment XX” by Maxwell Snyder, “The Sanal Theory of Bong Han Kim: Bion-like Processes in Acupuncture in Biology” by Dong Chul Kong and Hyun-Wong Kim, and “Bion-Biogenesis Research and Seminars at OBRL—Progress Report”.

The section on Orgone Biophysics includes several articles on Dayton Miller’s ether-drift experiments, “A Fresh Look” and “Reconciling Miller’s Ether Drift with Reich’s Dynamic Orgone” by DeMeo and “The Experiments of Dayton C. Miller and the Theory of Relativity” by Maurice Allais. Courtney Baker, a physician and physicist, describes his invention, a new method of objectifying mass-free orgone manifestations in the atmosphere using his variation of an electroscope, in an article entitled, “Orgonomy: A New Detector”. He clearly obtains anomalous results in testing the device in different energetic situations and terrains. My own article, “The Implications of Current Consciousness Research on Orgonomic Theory” describes results of my experiment using a Random Event Generator (REG) while patients in Reichian (orgone) psychotherapy were strongly emoting. There, as reported previously in JSE, I found striking correlations between the kind of emotional expression, sorrow, anxiety, anger, and the direction of REG output, and I ask the reader to entertain hypotheses on possible causal mechanisms for this phenomenon, including Reich’s mass-free orgone energy theory and Robert Jahn and Brenda Dunne’s views on resonance in living systems. Nicolas Nikolaidis describes the use of L-rod dowsing rods in establishing the size and strength of the orgone energy charge around accumulators and other orgone devices. This technique in skilled hands holds much promise for those wishing to objectify orgone fields. Using scintillation chambers Dave Marett measured the directionality and intensity of muon (secondary cosmic radiation) flux. He found a west-east asymmetry, a wave function, peaking at sunrise and sunset, and a diurnal flux. He offers an orgonotic explanation for these anomalous findings, suggesting, in accord with Reich, that a component of cosmic radiation may have a terrestrial origin. In a well-controlled experiment DeMeo studied the effect of an orgone energy accumulator on the growth of mung beans in Pyrex dishes. The orgone-charged beans showed highly significant 34% increase in sprout-length albeit with what appears to be a lower sugar content. Victor Milian, a physicist working with others at the Chemical and Nuclear Engineering Department at the Polytechnic University of Valencia, Spain, found decidedly anomalous changes in electronic temperature sensors exposed to a radioactively excited orgone energy accumulator, anomalous decreases in radioactive decay of RA-226 and Cs-137 when placed inside an orgone accumulator, confirming in many respects observations made by Reich in his original oranur experiment.

On the basis of his work using his invention, the “cloudbuster”, to modify the weather, Reich developed the discipline “cosmic orgone engineering”. This section of notebooks on cosmic orgone engineering contains six articles by DeMeo. DeMeo, who has a Ph.D. in Geography from the U.K., obtained his master’s degree on the basis of his controlled studies of the use of the

There is also a report on the analysis of unusual objects alleged to be in the atmosphere recorded on infrared film during cloudbusting. The use of stereo-photography revealed the forms not to be in the atmosphere, but in the camera. The issue closes with brief progress reports of ongoing work at DeMeo’s laboratory and book reviews of DeMeo’s book, noted above, Saharaasia, of James Martin’s Wilhelm Reich and the Cold War, and of Peter Robbins’ Left at East Gate, A First-Hand Account of the Bentwaters-Woodbridge UFO Incident, its Cover-Up, and Investigation. The section on “In Memoriam” honors the passing of therapist Robert D. Morris, therapist Michael B. Rothenberg, sociologist Louis Hochberg, and sociologist, educator, and biographer of Reich, Myron Sharaf.

There are very few of us carrying on Reich’s experimental research in life energy. On behalf of my colleagues I thank James DeMeo not only for his excellent work in the field, but for making available to us and the public such a fine literary organ where we can publish and our work be available to colleagues and others interested in orgonomy. I know it is created with much personal and financial sacrifice and this makes the effort so much more worthwhile.

RICHARD A. BLASBAND

Readers are encouraged to submit for possible inclusion here titles of articles in preferably peer reviewed journals (typically, which do not focus on topics about anomalies) that are relevant to issues addressed in JSE. A short commentary should accompany. The articles may be in any language, but the title should be translated into English and the commentary should be in English.