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Journal of Scientific Exploration, Vol. 29, No. 2, pp. 189–204, 2015 0892-3310/15

EDITORIAL

One of the Commentaries in this issue is something of a departure for the *JSE*. The paper by Jaime A. Teixeira da Silva concerns retractions in scientific publications, a topic that has been receiving increasing attention in recent years, apparently coinciding with an increasing number of retractions over the same period. Because the SSE and *JSE* focus not only on specific (usually controversial or neglected) domains of scientific investigation but also on broader issues concerning the practice of science itself, I figured that the cluster of issues surrounding retractions might be of both theoretical and practical interest to *JSE* readers.

The sheer number of retractions is enough to give one pause. A recent survey by R. Grant Steen of the PubMed database from 2000 to 2010 identified 788 retracted papers (Steen 2011). For 46 of those papers, Steen was unable to find formal retraction notices. So his survey dealt with the remaining 742 papers for which he could obtain such notices. The reasons for retraction were broadly identified as fraud and error. The former included data fabrication and data falsification, and the latter included (among other things) plagiarism,[1] scientific mistake, and ethical issues (violations of accepted publication practices—for example, IRB [Institutional Review Board] violations). Steen found that the reason for retraction was more often error than fraud—73.5% as compared to 26.5%.

A later survey (Steen, Casadevall, & Fang 2013) examined the interval between publication and retraction for what strikes me as an astounding 2,047 retracted articles indexed in PubMed.[2] And the number of papers that *should* be retracted may well be greater than that. As Cokol, Iossifov, Rodriguez-Esteban, and Rzhetsky observe (2007), "Retracting a published scientific article is the academic counterpart of recalling a flawed industrial product" (p. 422). But

> . . . articles published in more prominent scientific journals receive increased attention and a concomitant increase in the level of scrutiny. This therefore raises the question of how many articles would have to be retracted if the highest standards of screening were universally applied to all journals. (Cokol, Iossifov, Rodriguez-Esteban, & Rzhetsky 2007)

Moreover, as Vedran Katavić noted, "the retracted articles do not die, but rather receive citations years and decades after their retraction, often by the authors themselves" (Katavić 2014:217). So one can easily see why da Silva is concerned about the consequences of all these retractions for

the downstream scientific literature. Katavić, in fact, supplies a stunning example of the extent to which retracted articles can infiltrate and leave traces in the media.

> On January 30, 2014, the scientific journal *Nature* published 2 papers by Haruko Obokata et al. detailing reprogramming of somatic into stem cells by an acidic bath. The journal's article metrics allow for some understanding of the impact these articles have attracted so far, before their inevitable retraction (at the time of writing this opinion piece, both papers are under investigation for fraud). Within approximately 50 days of publication, these two articles (taken together) have been tweeted about over 3300 times, appeared on more than 100 Facebook pages, picked up by 130 news outlets, cited a total of 30 times (which puts them above the 90th percentile of tracked articles of similar age across journals or in *Nature*), blogged about on at least 50 scientific blogs, and their web pages at the source through the nature.com journal platform have been viewed (HTML views and PDF downloads) more than 1,300,000 times total! (Katavić 2014:220–221)

Another piece of information I found especially startling was a presumably incomplete list of scientists with multiple retractions, some of them with truly amazing totals. Consider Table 1 provided by Katavić (2014:219).

TABLE 1
Some Authors with Multiple Retractions from the Last Decade

Name	Scientific field	# retracted publications
Yoshitaka Fujii	Anesthesiology	170
Joachim Boldt	Anesthesiology	90
Friedhelm Herrmann / Marion Brach	Neuroscience	94
Diderik Stapel	Psychology	50
Naoki Mori	Immunology	30
Jan Hendrik Schön	Physics	25
Shigeaki Kato	Biomedicine	20
Alirio Melendez	Immunology	20
Dipak K. Das (late)	Biomedicine	20
Silvia Bulfone-Paus	Biomedicine	13
Eric Poehlman	Biomedicine	10
Bengü Sezen	Biochemistry	9
Dirk Smeesters	Psychology	7

In addition to the specific concerns raised by da Silva, my own brief search of the relevant literature turned up the intriguing finding that "the probability that an article published in a higher-impact journal will be retracted is higher than that for an article published in a lower-impact journal" (Fang & Casadevall 2011:3856). The authors write,

> The correlation between a journal's retraction index and its impact factor suggests that there may be systemic aspects of the scientific publication process that can affect the likelihood of retraction. When considering various explanations, it is important to note that the economics and sociology of the current scientific enterprise dictate that publication in high-impact journals can confer a disproportionate benefit to authors relative to publication of the same material in a journal with a lower impact factor. For example, publication in journals with high impact factors can be associated with improved job opportunities, grant success, peer recognition, and honorific rewards, despite widespread acknowledgment that impact factor is a flawed measure of scientific quality and importance. . . . Hence, one possibility is that fraud and scientific misconduct are higher in papers submitted and accepted to higher-impact journals. In this regard, the disproportionally high payoff associated with publishing in higher-impact journals could encourage risk-taking behavior by authors in study design, data presentation, data analysis, and interpretation that subsequently leads to the retraction of the work. Another possibility is that the desire of high-impact journals for clear and definitive reports may encourage authors to manipulate their data to meet this expectation. In contradistinction to the crisp, orderly results of a typical manuscript in a high-impact journal, the reality of everyday science is often a messy affair littered with nonreproducible experiments, outlier data points, unexplained results, and observations that fail to fit into a neat story. In such situations, desperate authors may be enticed to take short cuts, withhold data from the review process, overinterpret results, manipulate images, and engage in behavior ranging from questionable practices to outright fraud. . . . Alternatively, publications in high-impact journals have increased visibility and may accordingly attract greater scrutiny that results in the discovery of problems eventually leading to retraction. It is possible that each of these explanations contributes to the correlation between retraction index and impact factor. Whatever the explanation, the phenomenon appears deserving of further study. The relationship between retraction index and impact factor is yet another reason to be wary of simple bibliometric measures of scientific performance, such as impact factor.[3] (Fang & Casadevall 2011:3856–3857)

Furthermore, according to Shi V. Liu, the high impact factor (IF) "for some journals is actually based—at least in part—on the high number of citations of their retracted papers. . . . Rather than removing these 'negative contributions' from the IF calculation, these journals have continued to use

their inflated IFs to promote their publications" (Liu 2007:792).

I should add that, among the many interesting observations in the passage quoted above from Fang and Casadevall, I found it refreshing to see the authors acknowledge that "the reality of everyday science is often a messy affair littered with nonreproducible experiments, outlier data points, unexplained results, and observations that fail to fit into a neat story." No doubt *JSE* readers (and authors) are all too aware of this, although that grubby reality is often ignored by critics of the research to which this *Journal* is devoted. (Katavić also has some pertinent observations on this topic.)

Because I felt that this general topic of retractions would be of considerable interest to *JSE* readers, I thought I might be able to stimulate commentaries on the Commentary by reaching out to various SSE stalwarts and some others, to see if they wanted to offer reflections of their own. Here are some of those responses: Their authors have allowed me to submit them for your further consideration.

From psychiatrist and psychoanalyst (and dissociation researcher) John O'Neil (personal communication, February 3, 2015):

> With increasing digitization, there's more and more automatic registering of what gets cited, so I assume that at some point in the future there may be some automatic tag that goes on all papers citing a retracted paper, and then some derivative tag that goes on all papers citing a paper that cites a retracted paper, etc. Though at that point the carbon-based units would need to take over for a little interpretation.
>
> Automatic tagging wouldn't suffice, of course, as a review paper might cite a retracted paper as an example of a retracted paper, and cite the retraction as well, I would assume, so then there would need to be some mechanism to have the 'tag' removed from that paper; otherwise the tag would mislead, and be carried into all the 'progeny' of the paper concerned. So the idea that a retraction [can] cause the retracted paper to cease to exist is nonsense. What happens instead is a published retraction by an author (or publisher), and this compromises the credibility of the paper.
>
> And then, of course, there's the forced retraction. Like Galileo (or whoever) retracting solid science under social, political, or religious pressure. So a retraction may be done to save one's skin. Or to please others (e.g., retractions of accusations of incestuous sexual abuse). So, retractions arising from a lack of moral fibre (to use some dated expression).
>
> So I think the author is onto an important point, but I also think the devil is in the details.

From my editorial predecessor, Henry Bauer, some characteristically trenchant comments (February 14, 2015):

> That retracted material continues to be cited and the retraction known is unquestionably a bad thing. However, it is whistling in the wind to call for systemic solutions: There is no mechanism by which solutions could be enforced.
>
> The problem arises in part from "publish or perish," nowadays more aptly "get grants continually or perish." That has led to a spate of online commercial publishers putting out hordes of journals whose only purpose is to allow grant-seekers to publish anything at all merely by paying "publication costs" (more at "Fake, deceptive, predatory Science Journals and Conferences," http://wp.me/p2VG42-29).
>
> If researchers were to be more scrupulous in checking what they cite, and peer reviewers were more conscientious, and editors, too, then the problem would not have reached its present proportions. That is water under the bridge. The issue da Silva addresses is simply one aspect of how science has become corrupted through excessive expectations and expansion, see "The Science Bubble" in *EdgeScience* #17, February 2014, http://www.scientificexploration.org/edgescience/edgescience_17.pdf.
>
> Of greater concern to me and others who try to get minority views published is retraction as a form of censorship, the retraction of articles that had been accepted after appropriate review but whose publication meets storms of protest from vigilante defenders of mainstream orthodoxy. See, for example, the story of the demise of the journal *Medical Hypotheses* for transgressing HIV/AIDS theory, Chapter 3 in my *Dogmatism in Science and Medicine* (McFarland 2012). More recently a literature review of the controversy over HIV/AIDS by Patricia Goodson survived the call for retraction with the editors compromising by changing it to an "Opinion" piece from the original "Hypothesis and Theory," though apparently its abstract has been removed from PubMed (article and comments at http://journal.frontiersin.org/Journal/10.3389/fpubh.2014.00154/full; protest is at http://journal.frontiersin.org/Journal/10.3389/fpubh.2015.00030/full; and publisher's statement at http://journal.frontiersin.org/Journal/10.3389/fpubh.2015.00037/full).

Finally, Michael Ibison (personal communication, February 14, 2015) contributed this:

> I wonder if in the future the 'static' paper will be a special case, the more common being a dynamic version subject to continuous revision. The latter is already under way at arxiv and researchgate. For this reason, when I have an interest in a recent journal paper I check out arxiv and elsewhere on the Web, sometimes finding a 'new and improved' and/or extended version. The journal paper might function as an 'advertisement' in such cases.

I have no particular ax to grind (yet) with respect to this general topic of retractions and their aftermath. However, I look forward to seeing whether

SSE members want to pursue the topic further, either with commentaries or correspondence submitted to the *JSE*, or perhaps at one of our conferences.

Notes

[1] One might wonder why plagiarism isn't considered fraud.

[2] See also the figures cited recently by Gasparyan, Ayvazyan, Akazhanov, and Kitas (2014).

[3] For further commentary, see, e.g., Cokol, Iossifov, Rodriguez-Esteban, and Rzhetsky (2007); Gasparyan, Ayvazyan, Akazhanov, and Kitas (2014); Gewin (2014); Katavić (2014); Liu (2007); Steen (2011); and Steen, Casadevall, and Fang (2013).

Stephen E. Braude

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RESEARCH ARTICLE

Modeling the Law of Times

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Abstract—The Law of Times is a constant pattern present in every catalog of Unidentified Flying Object (UFO) sightings that describes the number of sightings occurring at a certain hour of the day. It shows that most sightings occur at night, reaching a maximum at about 21 h, and sometimes a secondary peak at about 2–3 h, whereas for daylight hours the percent of cases is low. It has long been suspected that the decrease in sightings during the night is due to social factors such as people staying indoors and thus not being able to witness UFOs. An increase in sightings occurs at earlier or later hours in the same way that the sunset time varies during the year. Taking into account these features, this paper develops a mathematical model to reproduce the Law of Times. It is based on astronomical factors such as the altitude of the Sun and the Visual Limiting Magnitude that relates to the probability of a phenomenon being visible; and a second factor related to the social habits of the population, accounting for their availability to witness the phenomenon. These two factors alone can accurately reproduce the main peak at 21–22 h of the Law of Times.

Introduction

Ufology can be defined as the scientific study of anomalous aerial phenomena that have been traditionally referred to as Unidentified Flying Objects (UFOs). Kenneth Arnold's sighting in 1947 (Arnold) is usually considered the cornerstone of modern UFO phenomenon. From the very beginning of ufology, the compilation of UFO cases has been an important activity for looking for patterns that can characterize and hopefully eventually explain the phenomenon. As a result, a variety of catalogs exist on which statistical analyses can be done.

One of the first patterns was found by Jacques Vallée in 1966, when analyzing the time distribution of 200 landing cases in France (Vallée 1966). The pattern shows that only a small number of sightings take place during the day, while most of them concentrate in the evening hours, reaching a maximum around 21–22 h. We will refer to the 21 h peak as the main peak. This pattern has come to be known as the Law of Times, and was soon replicated by Vicente-Juan Ballester Olmos (Ballester Olmos & Vallée 1971)

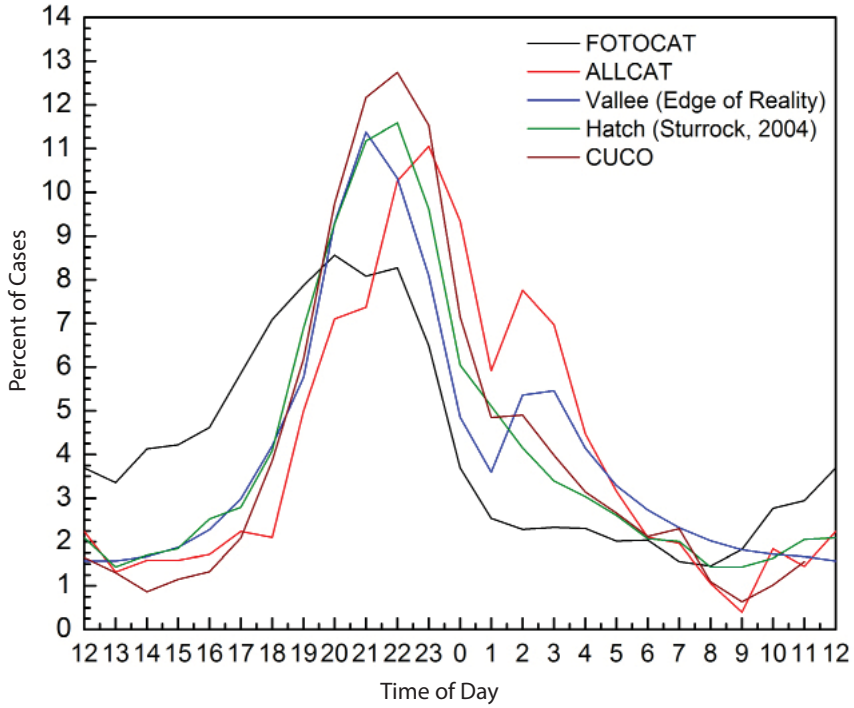

Figure 1. Law of Times for several catalogs.

and Ted Phillips (Swords 2010) using different catalogs, which confirmed the original discovery and led to the conclusion that a real phenomenon was taking place. Several other authors later reproduced this law (Pedersen 1978, Hynek & Vallée 1976, Gregor & Tickx 1980, Poher & Vallée 1975, Guasp 1973, Ballester Olmos & Fernández Peris 1987, Sturrock 2004, Ballester Olmos 2013, Rospars 2014), and even found evidence for it in pre-1880 UFO cases (Vallée & Aubeck 2010), which suggests that the same kind of phenomenon happened in earlier times.

Figure 1 shows several time distributions for a few catalogs. They have different scopes and geographical coverage: worldwide landing reports (VALLEE, extracted from Hynek & Vallée 1976); all kinds of reports worldwide (HATCH, extracted from Sturrock 2004); worldwide photographic records (FOTOCAT Catalog); landing reports in Spain and Portugal (ALLCAT Catalog); and all kinds of reports from Spain, Portugal, and Andorra (CUCO Catalog). They all show the same basic pattern with minor differences. Along with the main peak, in some cases a secondary

peak centered between 2 and 3 a.m. can be clearly seen. However, it seems to fade or hide, and can be only guessed at as a change in the downhill slope of the main peak in other catalogs.

By definition, a UFO is only a stimulus that could not be identified at the moment it was spotted. Whether it originated by misperception of a known object or phenomenon or is actually a phenomenon still unknown to science, is the goal of the investigation of these reports. Eventually, explanations based on known causes are found for many of these phenomena, making them Identified Flying Objects (IFOs). The question that arises then is whether there is any difference between the patterns of these two classifications. Figure 2 shows the UFO and IFO time distributions for FOTOCAT and CUCO catalogs, showing very similar patterns suggesting that both distributions are caused by similar factors if not the same factors.

Figure 3 shows two other specific catalogs, LANIB (LANdings in the IBerean peninsula), composed of UFO cases of high strangeness (as defined in Ballester Olmos & Fernández Peris 1971), and NELIB (NEgative Landings in the IBerean peninsula), composed of IFO cases. They show basically the same main peak, but a remarkable difference in the secondary peak for IFO cases. Just recently, other authors have found statistically significant differences for UFO and IFO distributions, including differences with respect to the main peak (Rospars 2014).

Poher–Vallée Interpretation of the Law of Times

After finding this characteristic pattern, it is only natural that interpretations were put forward to explain the nature of the graph and its relation to UFO phenomena. Perhaps the most widespread interpretation is that of Poher and Vallée, after the analysis of a database of close encounters (Poher & Vallée 1975). Their assumption was that these encounters occurred mostly at night, and the decrease of reports after the maximum was because of people staying inside at night, and not being able to witness the UFOs. That meant that there could be a significant amount of unreported landings.

This interpretation assumes that UFOs have a specific activity dependent on time that is modulated by a social factor determined by the probability of people being able to encounter UFOs. Poher and Vallée deduced that the UFO activity could be approximated to a gaussian curve, showing that the rate of potential encounters to the actual number of reports could be 14 to 1.

Thus, this interpretation takes into account two factors: a gaussian UFO activity centered at night, and a social factor determined by the time people are out of their homes and able to witness a UFO Close Encounter. This interpretation, however, does not explain the apparition of the secondary peak.

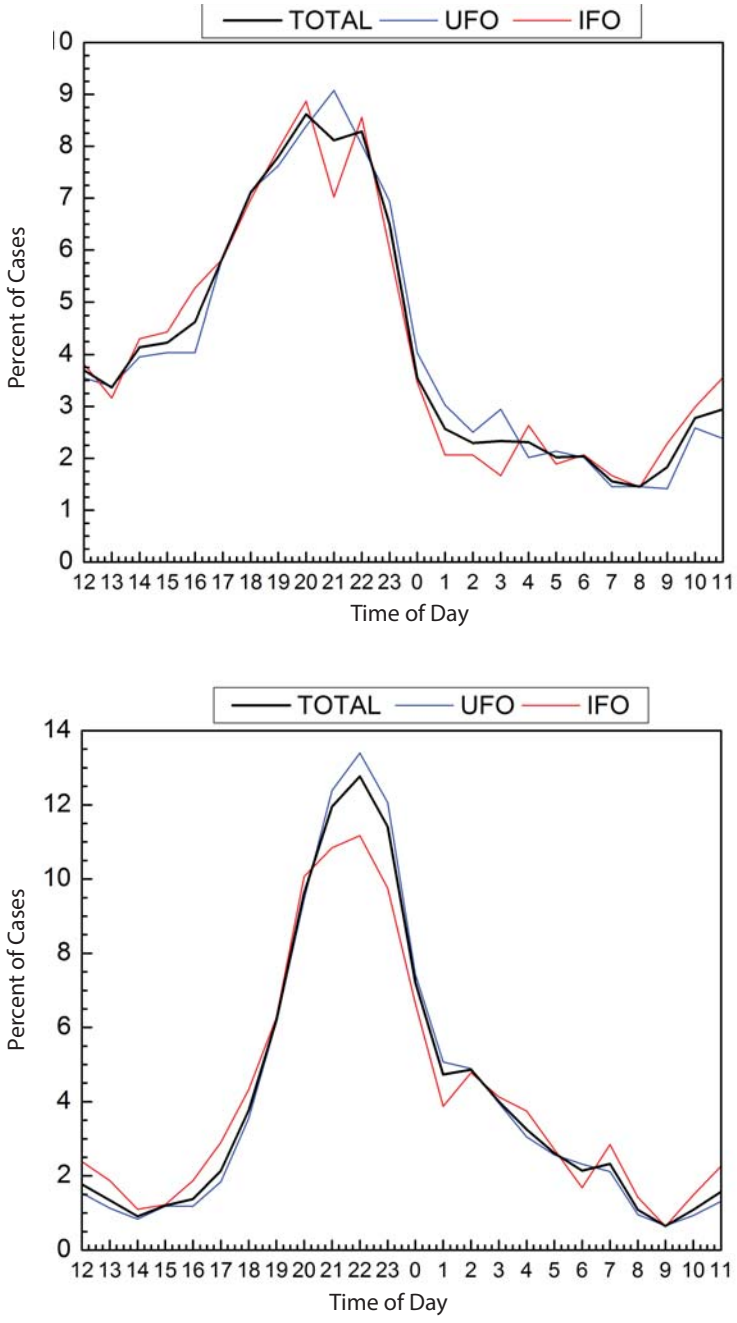

Figure 2. (Top) FOTOCAT (2,479 UFO cases and 2,247 IFO cases), (Bottom) CUCO (3,973 UFO cases and 1,547 IFO cases).

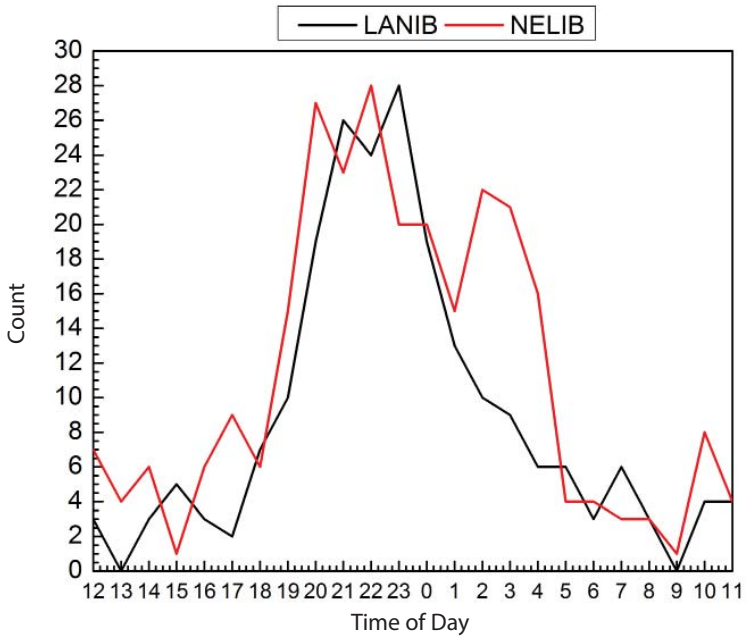

Figure 3. LANIB (213 UFO cases) and NELIB (273 IFO cases).

Process Theory

Another not so widespread interpretation was given by Miguel Guasp, who proposed a UFO Process Theory (Guasp 1973). This theory was developed with the intent to provide a tool to work on catalogs and extract useful data about UFOs. The initial assumption was that real objects, coming from a point in outer space like Mars or any other planet, arrived at a specific point on Earth. But, during entry into the atmosphere, their trajectory changed depending on mission-specific variables in order to reach their final destination. Applying this theory to the 1968 wave in Spain (a small catalog of 29 cases), it was found that the distribution of one of these mission-specific variables ($|\alpha|$) resembled the Law of Times, and therefore, this parameter described some aspect of those objects.

Process Theory assumed that objects travelled to Earth using the shortest trajectory. Since there are more points outside Earth's orbit than inside, objects would be more likely to arrive on Earth on the dark side, i.e. at night. UFO activity would be again a gaussian-shaped curve, centered around midnight. The apparition of the secondary peak would be caused by the mission-specific variable α.

This interpretation relates UFO activity to the position of the Sun. But it does not take into account a social factor as Poher and Vallée did. However,

the math involved in this Process Theory shows a direct relationship of variable α with time, and thus it is normal that α distribution reflects the Law of Times. At this point, Process Theory questioned causality: Is the time distribution causing the distribution of α? Or was the Law of Times the effect of α distribution? The latter implies that α is a variable describing the object's behavior. But the former implies that if the time distribution can be explained in some other terms, then parameter α is only another description of the same phenomenon.

Influence of the Seasons

At least two works have analyzed the dependence of the Law of Times on the season of the year. In 1978, Per Pedersen, from the Scandinavian UFO Information center (SUFOI) (Pedersen 1978) used a catalog of 227 UFO cases in Denmark in 1975 to show that the rise of the main peak had a dependence on the season of the year. Plotting the Law of Times for winter and summer cases showed that sightings occurred earlier in winter than in summer. In winter the rise of the main peak started as early as 18 h, but in summer it was delayed to 21 h. This suggests a strong connection between sunset and the increase in UFO sightings. Some of the other features of the Law of Times could be related to social factors, such as the decrease of sightings as people go to bed. However, for the secondary peak at 3:30 a.m. there was no reasonable explanation that could be related to a social or astronomical factor, and it was thought to be solely related to UFO activity.

In 1980, Gregor and Tickx (Gregor & Tickx 1980) again showed a correlation between the main peak maximum and sunset for Belgian cases. For every month of the year, the main peak maximum always occurs later than sunset, but moves to earlier or later hours synchronously with the sunset. Among their conclusions was that UFO sightings were related to the elevation of the Sun relative to the horizon.

Standard Time and Daylight Saving Time

When calculating the time distribution of a catalog, bins are created to accumulate cases from a specific hour interval, and finally represented in a histogram. Catalogs usually register the official local time at the place of the sighting. However, in most countries official time changes for certain periods of the year. In the Northern Hemisphere, time is adjusted forward one hour from March or April to September or October or November in order to adjust human activity to daylight. In the Southern Hemisphere, the forward adjustment is from October to March. This adjustment is known as Daylight Saving Time (DST).

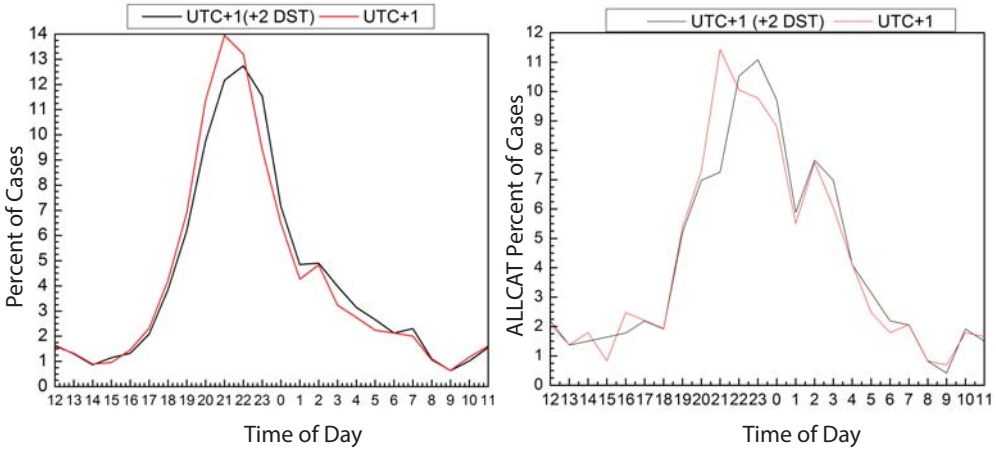

Figure 4. (Left) CUCO, and (Right) ALLCAT. Official time distribution, including DST (black) and standard time distribution (red).

However, not every country observes this adjustment, and not every country has always observed it. Moreover, in some countries, daylight savings varies across the time zones. As we just saw in the previous section, there is an important dependence on sunset in the Law of Times, and introducing a change in the official time due to social needs leads to artifacts or deviations like those shown in Figure 4. For both catalogs, we can see that the main peak maximum is shifted to a later time, and an abrupt change in the slope of the main peak rise can also be observed in one of them.

Therefore, it is important to correct the time of sightings to use only one daylight time for all of them. From here on, unless specified otherwise, the time distributions in figures will show the standard time after correcting the DST where needed.

Modeling of the Law of Times

In the past, some efforts were made to quantify the differences between catalogs. One of these efforts tried to define a "Satisfactory Law of Times" (Ballester Olmos & Guasp 1972) as a reference pattern to compare with; this "satisfactory curve" used parameters such as the main peak maximum, minimum, secondary peak maximum, and so on. Those are parameters that describe the curve, but have no physical meaning. But the development of a mathematical model capable of describing and reproducing time distributions based on known factors should allow for understanding of what the causes behind the features are, and understanding similarities and differences between distributions can shed some light on the factors influencing UFO phenomena.

What the work of Pedersen and Gregor are showing is that an astronomical factor is responsible for the increase in sightings over the time distribution. On the other hand, a social factor such as the time people go to bed for the night seems to be the cause of the decrease of sightings during the night, as proposed originally by Poher and Vallée. This leads us to think that the Law of Times, or at least its main peak, is only a consequence of both factors that describe the sighting conditions—*when an object is visible*, and *how many people are available to see it*. In the next sections we will derive a model using simple and reasonable assumptions based on physical and social parameters, and show that it qualitative and quantitatively reproduces the most basic features of the Law of Times.

Addition of Catalogs

Before starting the development of the model, it is interesting to deduce a mathematical property of catalogs that is independent of any model or distribution. We will refer to this property at some points in the next sections.

Given a catalog with a total of N reports, the Law of Times, $P(h)$, is constructed as the number of reports at hour h, $N(h)$, divided by the total number of reports:

$$P(h) = \frac{N(h)}{N} \tag{1}$$

Given n different catalogs, each having N_i reports, and individual time distribution $P_i(h)$, after joining all of them in a single catalog we obtain a new catalog with a total of $N_T = \Sigma N_i$ reports. The joint Law of Times of the new catalog is constructed as:

$$P(h) = \frac{N_1(h) + \cdots + N_n(h)}{N_T} = \sum_{i=1}^{n} \frac{N_i(h)}{N_T}$$

Let us multiply and divide each term of the summation by the number of reports of the i-th catalog:

$$P(h) = \sum_{i=1}^{n} \frac{N_i}{N_T} \cdot \frac{N_i(h)}{N_i}$$

The second term is exactly the definition for the Law of Times of the i-th catalog, $P_i(h)$. Therefore, the joint Law of Times is a weighted average of the individual time distributions, and its weight is the proportional contribution to the total catalog.

$$P(h) = \sum_{i=1}^{n} \frac{N_i}{N_T} P_i(h) = \left(\frac{N_1}{N_T}\right) P_1(h) + \cdots + \left(\frac{N_n}{N_T}\right) P_n(h) \quad (2)$$

This means that if we add two catalogs P_1 and P_2, one contributing 80% of reports, and the other 20%, the total time distribution will be 80% P_1-*like*, and thus more similar to P_1 than P_2.

Mathematical Model

Let us start by assuming that at hour h, on day d, there are an undetermined number of events N_e (h,d) happening. These events may have any origin: lights in the sky, meteors, direct reflection of sunlight on balloons, clouds, or planes; lights from a car, satellites, space debris re-entry, the moon, stars, planets, ballistic missiles . . . even a genuine flying saucer or any unknown phenomenon can be an considered an *event*. The nature of this event is not important at this stage, we just have to suppose that phenomena that emit or reflect light appear either in the sky or near ground level.

However, the brightness of the event, i.e. its magnitude, must be enough so that it is not eclipsed by atmospheric brightness. This means that there is a enough of a contrast between the event and the background to render it visible. Therefore, from the number of events N_e(h,d), only a fraction will have enough contrast to be visible to the naked eye. Let us define the number of visible events, N_v(h,d) as:

$$N_V(h,d) = N_e(h,d) \cdot P_V(h,d)$$

P_v(h,d) being the *visibility*, the probability of an event being visible at hour h, on day d.

But for an event to be witnessed, visibility is not the only condition. There has to be somebody present to see it. Thus, the number of witnessed events, N_w(h,d), depends on a *witnessing probability*, P_w(h,d), defined as the fraction of visible events that are actually witnessed:

$$N_W(h,d) = N_V(h,d) \cdot P_W(h,d) = N_e(h,d) \cdot P_V(h,d) \cdot P_W(h,d)$$

To construct the time distribution, we need to calculate the total number of witnessed events at hour h, N_w(h), adding all witnessed events at that hour,

$$N_W(h) = \sum_d N_W(h,d) = \sum_d N_e(h,d) \cdot P_V(h,d) \cdot P_W(h,d)$$

and finally divide by the total number of witnessed events, as defined in Equation 1:

$$P(h) = \frac{N_W(h)}{N_T} = \frac{N_W(h)}{\sum_h N_W(h)}$$

$$P(h) = \frac{\sum_d N_e(h,d) \cdot P_V(h,d) \cdot P_W(h,d)}{\sum_h [\sum_d N_e(h,d) \cdot P_V(h,d) \cdot P_W(h,d)]} \quad (3)$$

Equation 3 is a general expression that describes the Law of Times with three conditions: the presence of an event, its visibility, and the presence of a witness. The problem now is finding mathematical descriptions for the *event distribution* (N_e), *visibility* (P_v), and *witnessing probability* (P_w).

Event Distribution

From the three factors in Equation 3, event distribution is the one that can be related to UFO activity. It should reflect the probability of an event happening at hour *h* on day *d*. But, what should the event distribution for UFOs be? A catalog is composed of UFO reports that may have come from many different stimuli. That is why after an investigation, many UFO reports become IFO reports. Even so, UFO and IFO distributions are very similar, especially when there is a high count of reports (see Figure 2), suggesting that many still-unexplained reports may have usual causes.

IFO catalogs show us that there is a high variety of stimuli: stars, planets, the moon, balloons, satellites, etc. For each of these stimuli an *event distribution* can be defined. For a planet such as Venus, an activity defining the probability of it being up in the sky should be maximum during the day, zero at night, and something in between at sunrise or sunset. For stars such as Sirius, its event distribution should show a high probability on winter nights and a low probability during winter days, but the opposite during summer (in the northern hemisphere). Stimuli such as planes and satellites are constantly crossing the skies, and therefore a constant activity may be assumed.

But even if each stimuli alone may have a specific time (or even daily distribution), when considering all of them simultaneously it seems that on any day, at any time, any event may happen. Since UFO catalogs may be composed of any event, a simple assumption is to think that the most general event distribution is a constant probability for any day and any hour. That is,

$$N_e(h,d) = N_0$$

After substitution in Equation 3, N_0 is a constant that can be extracted from the summations, and cancels out, resulting in an event-independent express

$$P(h) = \frac{\sum_d P_V(h,d) \cdot P_W(h,d)}{\sum_h [\sum_d P_V(h,d) \cdot P_W(h,d)]} \quad (4)$$

Therefore, a constant activity becomes some sort of *Null Hypothesis*, mathematically independent of the events. The assumption of a constant activity reflects the fact that UFO catalogs are composed of many different events, including potential IFO cases as well as potential genuine strange or unknown phenomena. However, as UFO cases are solved to become IFO cases, at some point if there is genuine UFO activity those cases would be the ones contributing the most to the catalog, and its own time distribution should reveal itself, as explained in the section "Addition of Catalogs."

For the next sections, we will continue the development of the model under the assumption of a constant activity.

Visibility

We defined visibility as the probability of an event having a contrast with respect to the background enough to be visible to the naked eye. Objects can either emit, reflect, or absorb light, and whether it is visible or not depends on the atmospheric luminosity, which is determined by the night/day cycle, geographical coordinates, and season of the year. For night time, it is easy to understand that events need a positive contrast, i.e. events must be brighter than the background. In this case, an appropiate variable to use is the visual magnitude.

On the other hand, during daytime the background is already bright, and objects can also be seen either due to a negative contrast (i.e. an object being darker than the background), or by color contrast (having a color different from the background). But how many *"non-luminous"* UFOs are usually reported? A statistical study by Poher (Poher 1976) showed that approximately 98% out of 458 cases could be classified as *"gleam," "luminous," "bright," "brilliant," "luminous with various terms,"* or *"reflects light"*; and only about 2% as "*non luminous."* This percent increases to about 6% (out of 31 cases) for daylight hours.

Similar values can be obtained from the CUCO catalog. Up to 947 entries (including UFO and IFO) have an explicit description that allows us to classify them as:

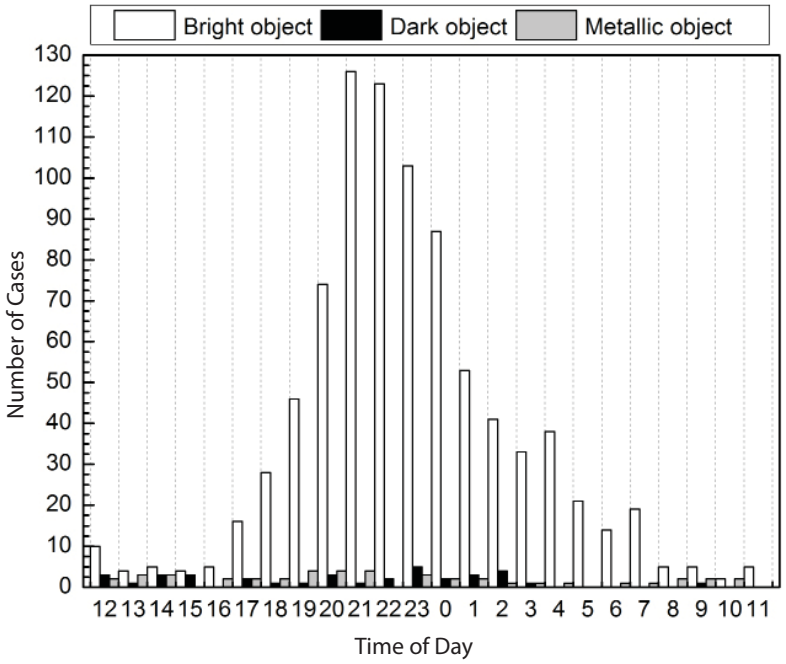

Figure 5. Time distribution for bright and dark cases in CUCO.

- Objects described as: luminous, bright, star-like, balls of fire, emitting or reflecting light of different colors (867)
- Objects described as: metallic, silvery, or reflective (44)
- Objects described as: non-luminous, dark, grey, black, shadow (36)

The first two can be easily grouped as bright objects creating a positive luminous contrast, and account for 96% of the total. The third one corresponds to non-luminous objects, and accounts for 4%. Regarding only daylight hours, non-luminous entries account for 9% of objects seen between 8 h and 19 h, whereas bright and metallic or reflecting objects represent 91%. But this increase in percentage is due to a decrease in the count of bright cases. The count of non-luminous UFOs is very low throughout the whole day, as can be seen in Figure 5.

We have omitted entries that might be included in *"bright"* or *"luminous"* due to context, but lack an explicit description about brightness or luminosity.

In any case, as the Law of Times shows, most events are reported at night, when visible objects are seen due to a positive luminous contrast. What we have classified as non-luminous events represent a very small

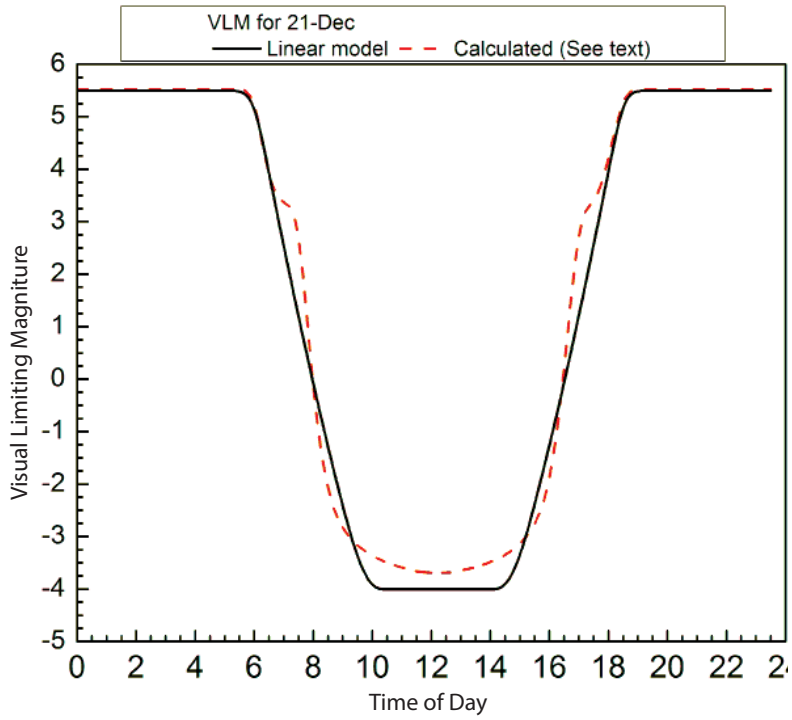

Figure 6. Modeled VLM for winter solstice (black line), and calculated from Vallee (1966) (dashed red line) at 40° N 0° E, UTC.

fraction of a whole UFO catalog, even for daylight hours when color contrast could be taken into account. Therefore, for our modeling, we will make the simplification of taking into account only events brighter than the background. We will use magnitude as the variable to describe the brightness of events.

Visual Limiting Magnitude (VLM) (Limiting Magnitude, http://en.wikipedia.org/wiki/Limiting_magnitude) is defined as the faintest magnitude that the naked eye can see. At night, this value is about 5.5. During the day, it is about −4. One must remember that the lower the magnitude, the brighter the object.

The transitions between daytime and nighttime create sunrises and sunsets, moments when the VLM will change between these two extreme values. Twilight (Twilight, http://en.wikipedia.org/wiki/Twilight) is the period of time when the position of the Sun changes from the horizon (altitude of 0º) to an altitude of −18º. Night starts when the Sun's altitude is below that value.

A simple model for the transition between daytime and night time is to

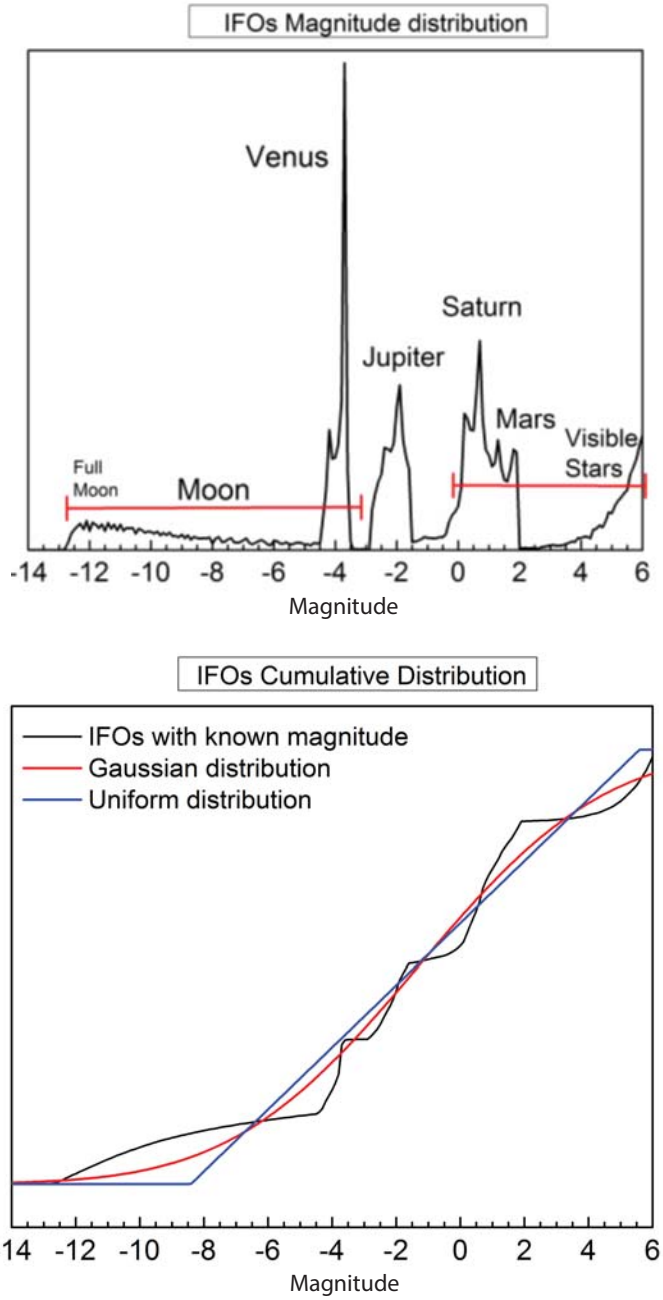

Figure 7. (Top) Magnitude Distribution for IFOs of known magnitude in CUCO, (Bottom) Cumulative Magnitude Distribution for IFOs of known magnitude in CUCO, and comparison with uniform and gaussian distributions.

consider that VLM changes from −4 to 5.5 linearly with the altitude of the Sun between 18º and −18º, and to take into account that light also vanishes while the Sun approaches the horizon during sunset (and vice versa during sunrise). Limiting Magnitude Calculations (http://cleardarksky.com/others/BenSugerman/star.htm) provides us with an online calculator for VLM, and we can see that our approximation is reasonable in Figure 6.

Finally, we also have to take into account that VLM depends not only on hour of the day, but also on geographical location and the day of the year.

After calculating the VLM, the next step is to assign a value for the visibility probability. Let us suppose that whenever an event appears, it has a random magnitude following a certain distribution. Since we are only looking for events brighter than the background, we can define *Visibility* as the probability of an event having a magnitude equal to or lower (brighter) than the Visual Limiting Magnitude at the time, day, and location where the event appears.

Thus, we need to know the magnitude of UFOs. This, however, presents a problem, because the perceived magnitude of an event by witnesses is a totally subjective description. On the other hand, once UFOs are identified and become IFOs, sometimes they can be related to objects with a known magnitude, or magnitude distribution. We have looked for such IFO cases in CUCO, and found the distribution shown in Figure 7. The magnitude distribution of IFOs with known magnitude is shown on Figure 7 Top. Planets show a distribution that can be approximated to a gaussian distribution. The moon, however, extends in a wider range. The distribution for the magnitude of stars was obtained from the Bright Star Catalogue (Hoffleit & Warren 1991), and can be approximated to a growing exponential.

However, these IFOs represent only 18.5% of all the IFO cases in CUCO. Many IFO cases were caused by lights of planes, reflections, satellites, spatial debris re-entry, bollides, and other stimuli for which a certain magnitude or magnitude ranges could be guessed, but not determined with certainty. For the total distribution, some basic assumptions have to be made.

Experience tells us that the multiple measurement of a single variable almost always yields a gaussian distribution around a mean value with a certain standard deviation. Such is the case of the visual magnitudes of Venus or Jupiter in Figure 7. But when several of them overlap, they form a new distribution that can cover a wider range. That is the case of Saturn and Mars' magnitudes.

What should the distribution of all possible events look like? The first approximation is to think that even if each event could show a gaussian distribution, all together would form a uniform distribution between a

maximum and minimum value. On the other hand, we can assume that, even if all possible events would cover a wide range of magnitudes, there would be a higher percentage of them around a mean value, and approximate to a gaussian distribution. We can in fact see in Figure 7 that magnitude values are centered between −4 and 2.

In any case, our definition of visibility implies that it is not the distribution itself but the Cumulative Distribution that we must look at (Figure 7 Bottom), to consider only objects with a magnitude greater than a given VLM for a determined day and time. Figure 7 also shows the approximations for uniform and gaussian distributions. From here on, we will assume a gaussian distribution for a magnitude of events that can be parametrized by a mean value μ and a deviation σ and expressed by:

$$P_V(h,d) = \int_{-\infty}^{VLM(h,d)} \frac{1}{\sqrt{2\pi\sigma^2}} e^{-\frac{(v-\mu)^2}{2\sigma^2}} dv \approx \frac{1}{2}\left[1 + \mathrm{erf}\left(\frac{VLM(h,d)-\mu}{\sqrt{2\sigma^2}}\right)\right] \quad (5)$$

In summary, $P_v(h,d)$ is a rather complex function that has to be calculated in several steps:

- Calculate the elevation of the Sun with respect to the horizon, depending on latitude and longitude of the location, day of the year, and standard time.
- Calculate VLM as a function of the elevation of the Sun.
- Calculate P_v as the probability of an event having a magnitude brighter than VLM.

Taking into account the annual periodicity of night/day cycles helps to simplify and accelerate calculations, as the summation can be done over the days of the period of interest. For instance, if the interest is in reproducing a catalog covering several years, the summation of days can be done only over the 365 days of a single year.

$$P(h) = \frac{\sum_{d=1-Jan}^{31-Dec} P_V(h,d)\cdot P_W(h,d)}{\sum_h\left[\sum_{d=1-Jan}^{31-Dec} P_V(h,d)\cdot P_W(h,d)\right]} \quad (6)$$

But the summation can also be done over a single month to have a *Monthly Law of Times*:

$$P(h) = \frac{\sum_{d=1-Jul}^{31-Jul} P_V(h,d)\cdot P_W(h,d)}{\sum_h\left[\sum_{d=1-Jul}^{31-Jul} P_V(h,d)\cdot P_W(h,d)\right]} \quad (7)$$

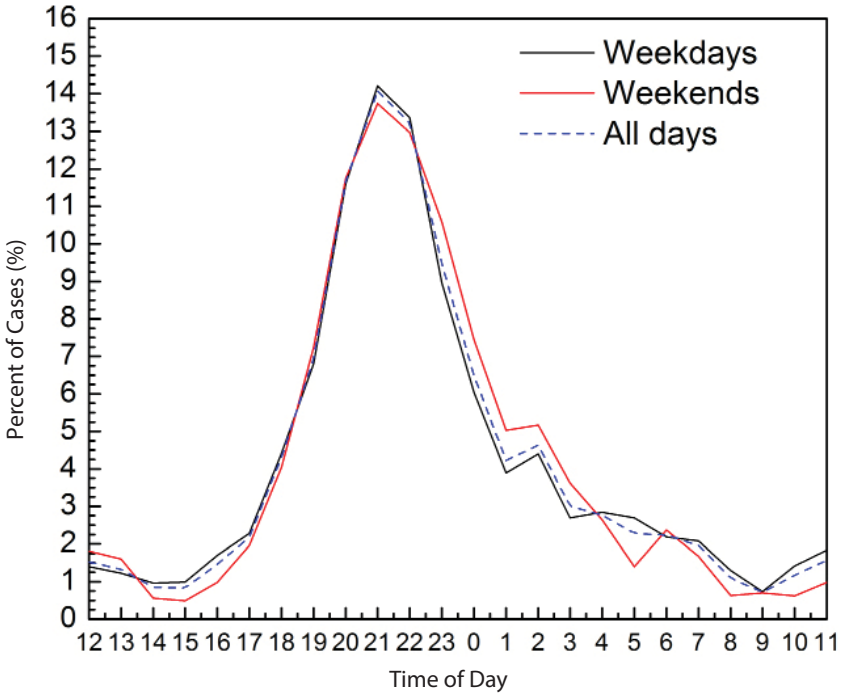

Figure 8. Law of Times for weekends, weekdays, and every day.

Other periodicities such as seasonal periods also can be considered to construct a *Winter Law of Times*, a *Summer Law of Times*, and so on. . . . The possibilities the model offers are flexible enough to analyze different situations.

Witnessing Probability

This is the most difficult factor to model, since it is meant to represent a social habit. The most intuitive idea is to represent the fraction of the population that is awake as a function of time. The more people who are awake, the higher the probability that somebody can witness an event.

The easiest assumption is to consider that sleep habits are the same every day. However, we can find differences between weekdays and weekends. Figure 8 shows the time distribution for weekdays and weekends for CUCO. The rise of the main peak starts at the same time for both, but the decrease shows a small but clear difference, which can be attributed to people going to bed later on weekends.

TABLE 1
Day of the Week Distribution for CUCO

Day	Number of cases	Percent of cases
Monday	579	12.6 %
Tuesday	615	13.4 %
Wednesday	627	13.7 %
Thursday	649	14.1 %
Friday	688	15.0 %
Saturday	726	15.8 %
Sunday	709	15.4 %

It is also interesting to note that the daily distribution shows a higher count of cases during weekends than expected for a uniform distribution (χ^2 = 26.26, *p*-value = 0.0002, see Table 1). However, the total Law of Times, when taking into account all the days, is almost a replica of the weekday time distribution. Even if during weekends UFOs are seen more than expected by chance, they only account for 31%, while 69% of cases are seen during weekdays. Thus, when taking into account all cases, the time distribution of weekdays weights more than that for weekends. As explained in the section "Addition of Catalogs," the total distribution is a weighted average of both distributions, and so we can consider that the *Go-to-Bed* time averages accordingly.

The same argument can be applied to monthly distributions. Summer months may have people going to bed later because of longer days, and people enjoying their holidays. But we can also consider that the total time distribution is a weighted average of all the months. Therefore, we can model a constant habit throughout the year, but assuming that it will represent an average, while the actual habits can be different for each month or day of the year.

Thus, Equation 4 can be then rewritten with P_w independent of the day, and removed from the summation on *d*:

$$P(h) = \frac{P_W(h)\cdot\sum_d P_V(h,d)}{\sum_h[P_W(h)\cdot\sum_d P_V(h,d)]} \qquad (8)$$

To model P_w, we will suppose a normal distribution for the *Wake-Up* time of the population, and another normal distribution for the *Go-to-Bed* time.

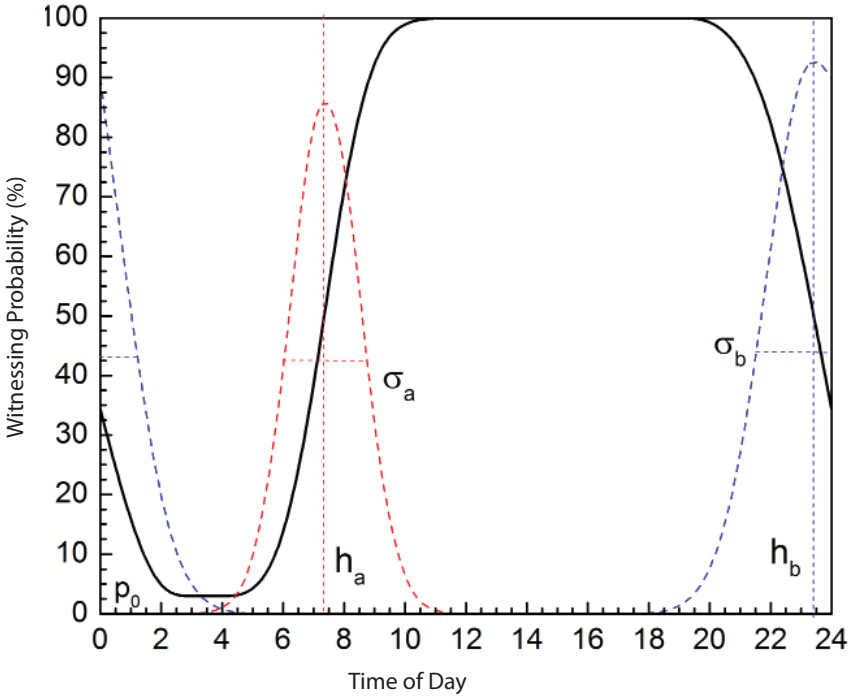

Figure 9. (Solid black line) Witnessing Probability, (Red/blue dashed lines) Wake-Up and Go-to-Bed distributions.

$$P_{w-u}(h) \propto e^{-\frac{(h_a-h)^2}{2\sigma_a^2}}$$

$$P_{g-b}(h) \propto e^{\frac{(h_b-h)^2}{2\sigma_b^2}}$$

- h_a : Mean Wake-Up time.
- σ_a : Standard deviation of Wake-Up time.
- h_b : Mean Go-to-Bed time.
- σ_b : Standard deviation of Go-to-Bed time.
- p_0 : Minimum percentage of the population that is awake at night.

The percentage of the population that is awake can be calculated as the Cumulative Distribution of the Wake-Up distribution minus that of the Go-to-Bed distribution:

$$P_W(h) = p_0 + (1 - p_0) \cdot \left[\int_0^h P_{w-u}(h')\, dh' - \int_0^h P_{g-b}(h') dh' \right]$$

$$P_W(h) \approx p_0 + (1 - p_0) \cdot \left[\frac{1}{2} \left(1 + \mathrm{erf}\left(\frac{h - h_a}{\sqrt{2\sigma_a^2}} \right) \right) - \frac{1}{2} \left(1 + \mathrm{erf}\left(\frac{h - h_b}{\sqrt{2\sigma_b^2}} \right) \right) \right]$$

A minimum number of the population may remain awake at night, p_0. We can think that this value represents people working at night, or awake for any other reason. Figure 9 shows P_w, as well as the meaning of the parameters.

Qualitative Analysis of the Model

Once $P_w(h)$ and $P_v(h,d)$ have been modeled, the Law of Times is related to the multiplication of both functions: A peak is formed right after sunset.

During daylight hours, most of the population is awake and there is a high probability of witnessing an event. However, the Visual Limiting Magnitude is as high as −4, meaning that only very bright events are visible. Therefore, a low percentage of cases can be reported during those hours. The opposite reasoning is valid during most of the nighttime: A VLM of 5.5 makes even faint events visible, but the fraction of people awake is low, again yielding a low percentage of reported cases.

The main feature of the Law of Times is the 21−22 h peak. It is the consequence of an increase in visibility due to sunset, as well as still having a high percentage of the population awake (Figure 10-1). The combination of both factors causes the peak to reach its maximum value (Figure 10-2). Then, as people go to bed, there is a decrease in reported events (Figure 10-3).

As shown previously, the main peak moves to earlier or later hours depending on the day of the year. It is possible to reproduce this behavior with our model. Figure 11 has been produced using Equation 8, varying only the month over which the summation in d is done, while all other parameters remain unchanged. The qualitative behavior is essentially the same as that shown by Pedersen (1978) and Gregor and Tickx (1980).

We have also analyzed a list of UFO sightings for California, taken from NUFORC (National UFO Reporting Center, http://www.nuforc.org/), with 9,225 cases. No particular revision was made in order to reject cases because of duplication, hoaxes, or for any other reason. The list was used as

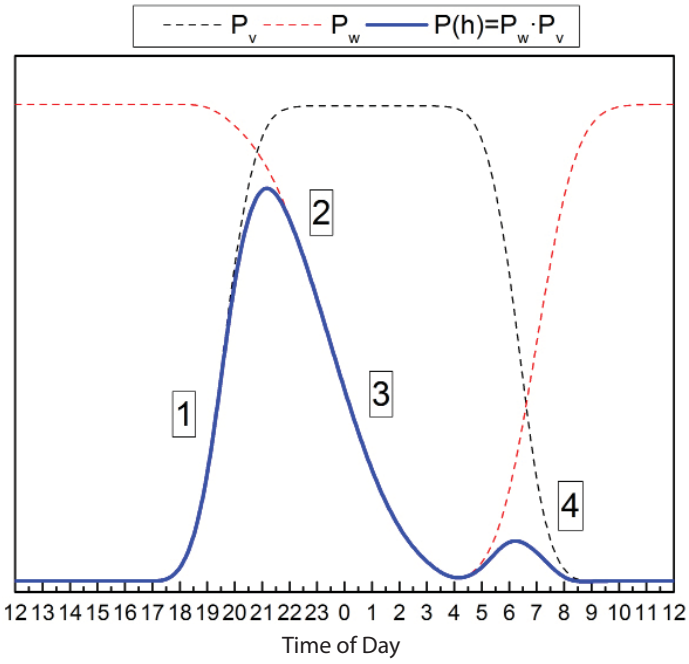

Figure 10. Law of Times as the multiplication of PV and PW. Main features are: 1–peak rise, 2–peak maximum, 3–peak decrease, and 4–morning peak/valley.

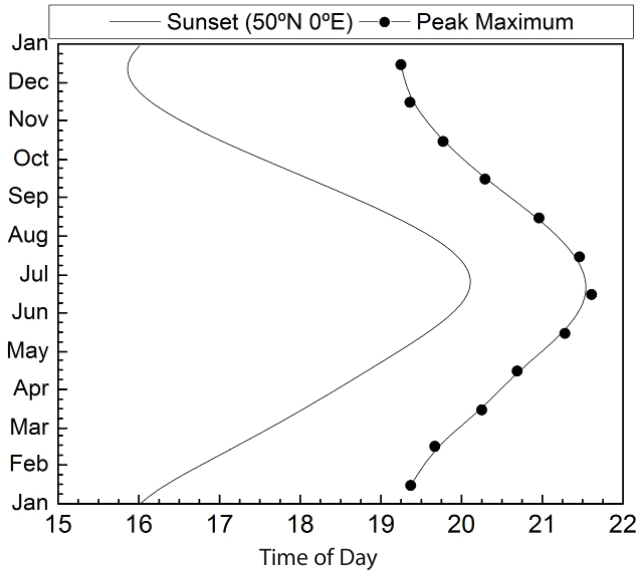

Figure 11. Shift of main peak position as sunset changes throughout the year. Calculated at 50° N, 0° E (UTC).

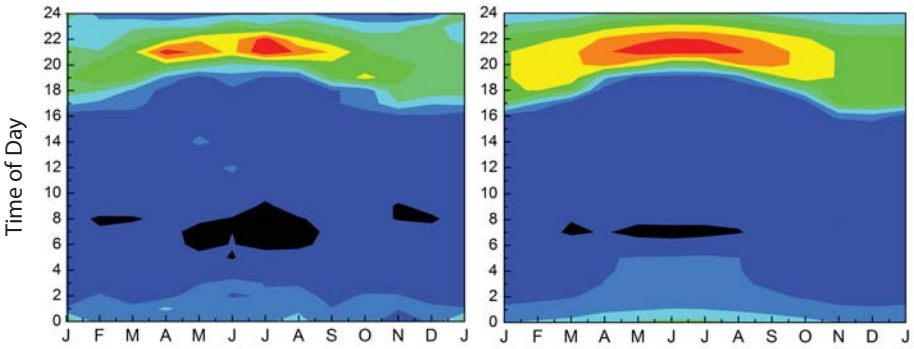

Figure 12. (Left) Monthly Law of Times in California (Source: NUFORC), (Right) Monthly Law of Times from Model.

it is. There are enough entries to construct monthly histograms with about 700 cases per month (Figure 12 Left; No correction for Daylight Saving Time was done). Colors map the percentage of cases, with black and blue for the lowest values, and yellow and red for the highest values. Figure 12 Right shows a reproduction of the Monthly Law of Times using the model.

The data allows us to verify another feature: the increase in percentage of cases of the main peak during summer months. This increase in percentage is not to be confused with an increase in the absolute number of cases. It can be understood by thinking that in summertime nights are shorter. As observations are more frequent at night time, in summer they tend to be grouped in a shorter time frame before people go to bed. On the contrary, in winter, with longer nights, observations can be scattered over a longer time frame. Hence the increase in peak maximum value (in percentage) during summer months.

The model indicates that a fourth feature could exist in time distributions, which should appear at the moment when P_w and P_v cross again at sunrise (Figure 10-4). The model predicts either a small peak or a valley:

- If people wake up when it is still dark, the percentage of cases should increase, creating a *morning peak*. This situation would be typical of winter.
- If sunrise starts before a significant number of people wakes up, a small *morning valley* would be observed. This situation could happen in summer.

This last feature could be difficult to reproduce in the model, since it is near the daylight baseline, although in Figure 12 there seems to be a small morning valley during summer.

Application to Catalogs

CUCO

CUCO (CUCO Catalog) compiles all kinds of UFO sightings in Spain, Portugal, and Andorra. It contains 5,220 entries (including UFOs and IFOs) in which the hour of the sighting is registered. For a direct comparison of the model and the Law of Times for CUCO, it is necessary to assign values to all the parameters that we have seen during the development of the model. These parameters are a priori unknown to us. However, we will see that some of them can be fixed, and for others an approximate value can be guessed. After that, a fit of the model to the time distribution can be done, and then we can analyze whether the final value of the parameters is reasonable or not.

Witnessing Probability parameters. We modeled a social factor accounting for the availability of people witnessing a visible event as a proportion of people awake. In 1975, Poher and Vallée used French statistics about working populations not at home for a similar purpose. Another way of having an estimate of human activity is looking at the electrical demand for the country. The electrical demand is low during the night when most people are sleeping. It rises in the morning as people wake up and get ready for work, and as industries begin demanding energy. The demand remains high throughout the day until people go to bed again.

Figure 13 shows the electrical demand in Spain for two different weeks in winter and summer. Despite the difference in the absolute value of the demand for power, the shape of both curves is quite similar. We can use this curve as a first approximation to Witnessing Probability. The parameters for $P_w(h)$ that best correlate with power demand are shown in Table 2.

TABLE 2

Parameters for P_w That Maximizes Correlation with Energy Demand (Figure 13)

Parameter	Value
Wake–Up time (h_a)	7:23 h
Wake–Up deviation (σ_a)	0.99 h
Go-to-Bed time (h_b)	0:03 h
Go-to-Bed deviation (σ_b)	1.39 h
Awake population at night (p_0)	3 %
Correlation coefficient	0.97

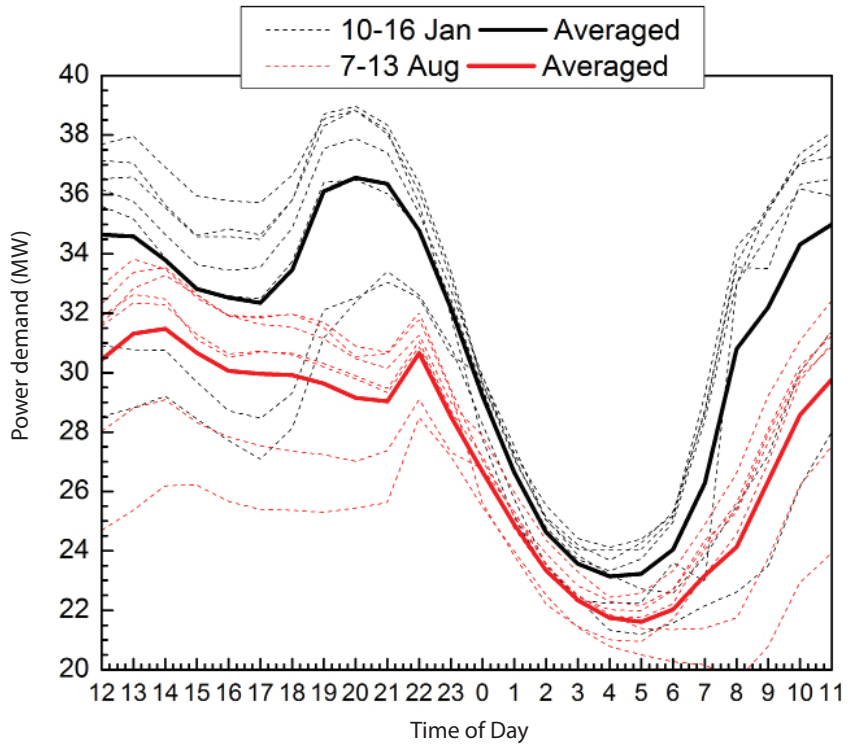

Figure 13. Averaged electrical demand in two weeks of 2013 (Red Eléctrica Española).

Geographical coordinates and time zones. Geographical parameters for the model are latitude (ϕ) and longitude (λ) of the location of the sighting. These parameters, along with the local time and time zone, are used to determine the altitude of the Sun, and thus some dependence of the time distribution on them is expected, especially regarding λ: Given two different locations with the same local time, sunset occurs later in the one to the west, and hence, the main peak rises later than in the other location to the east.

This effect can be seen in CUCO. We have constructed the Law of Times for two different regions: eastern Spain (728 cases) and western Spain (1,134 cases). Figure 14 shows these two regions and their respective Law of Times; a time delay can be seen for the western region, as predicted.

In any catalog, or subcatalog, sightings do not happen at a single geographical point but in several different places, creating a geographical distribution. For our western and eastern distributions, the latitude range is about the same in both regions, from 37º to 43 º N. But in longitude, the eastern region covers a range from 1.6º E to 2.3º W, whereas the western ranges from 3.7º to 7.4º W. For such distributions, mean values and variances

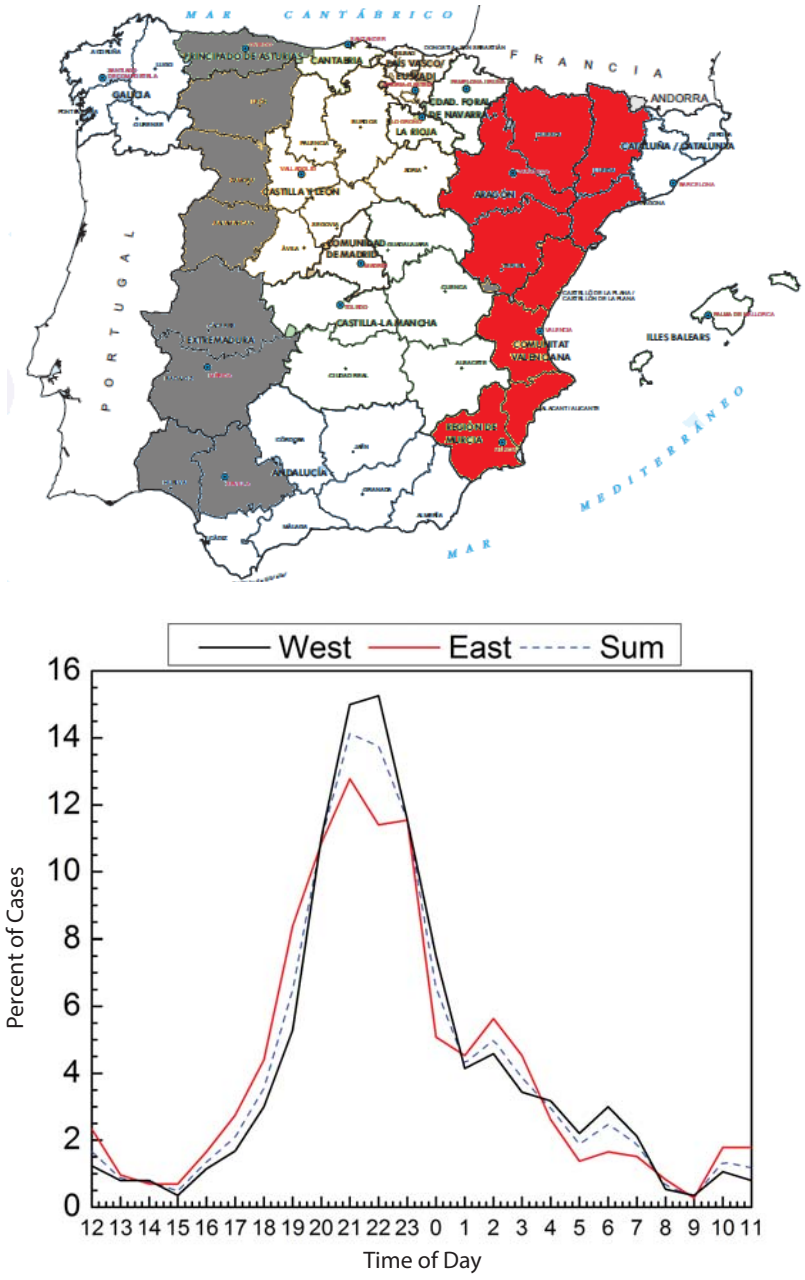

Figure 14. (Top) Selected east and west regions of Spain, (Bottom) Time distributions for east and west regions of Spain in CUCO.

can be defined: $\lambda_{East} = 0.4º \pm 0.8$ W for the eastern zone, and $\lambda_{West} = 6.1º \pm 0.5$ W for the western zone. They both cover less than 4º, and most of the values concentrate in a range of 1º.

If we join both eastern and western distributions into a single one, we will obtain a total time distribution that averages both distributions with their correspondent weight factors; and we obtain a time distribution that is centered on a mean longitude of:

$$\lambda_{Mean} = \frac{278}{278+1134}\lambda_{East} + \frac{1134}{278+1134}\lambda_{West}$$

$$\lambda_{Mean} = 4.9°W$$

That means that for a catalog of a wide country like Spain, we can set the geographical coordinates to fixed values representing the average latitude and longitude of the sighting locations.

However, an issue arises when we have to consider regions using different time zones. If we compare the west of Spain distribution, and the Portuguese distribution (452 cases), we can see that the main peak for Portugal seems to rise earlier than the peak for western Spain; that is Portugal seems to be to the east of western Spain (Figure 15).

Time zones exist to adjust the local time to daylight. Earth rotates at an angular speed of 15º/h. That means that in two places 15º apart in longitude, the relative position of the Sun will be the same with a difference of one hour. For western places, the Sun sets later, as we saw in our recent example of western and eastern distributions. For that reason, local time is adjusted adding or subtracting hours depending on the geographical location, creating time zones. Coordinated Universal Time (UTC) is the time reference, taken at 0º longitude.

Ideally, each time zone should cover 15º. UTC time zone spans from 7.5º W to 7.5º E; UTC + 1 spans from 7.5 E to 22.5º E, and so on. However, countries may use a time zone because of other factors (political or economic, for instance). For this reason, part of western Europe is included in the CET time zone (Central European Time, UTC + 1), when, because of their longitude, we should use the WET (Western European Time, UTC) time zone. Such is the case of France and Spain, while Portugal, and Great Britain, being at about the same longitudes, observe the WET time zone. Furthermore, some countries use more than one time zone. Canary Islands (Spain) observe WET time, while Madeira and Azores (Portugal) are in UTC-1 and UTC-2 time zones. The United States is divided into 5 different time zones and Russia uses up to 7 time zones.

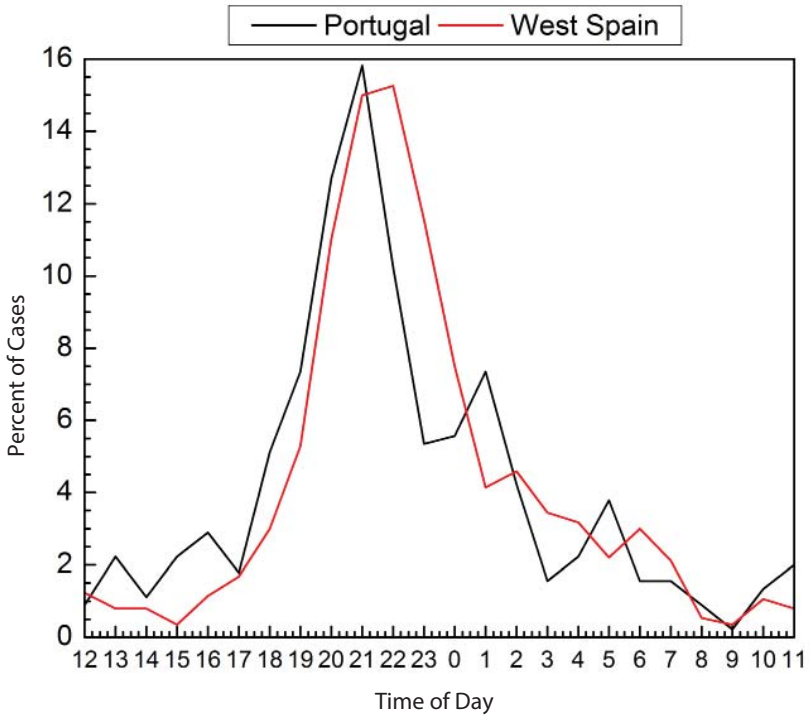

Figure 15. Time distribution of Portugal and western Spain as defined in Figure 14.

On the other hand, two places in about the same geographical location (like western Spain and Portugal), but with an hour of difference, means that even if the Sun sets in the same UTC time, local times are different. If we did not take into account the time zone, it would look as if the Sun had set earlier in one place than the other, as if it was to the east. Therefore, if we consider Portugal and Spain being in the same time zone, we have to correct Portugal's longitude 15º to the east and assume their local time belongs to the CET time zone, or, conversely, correct Spain's longitude 15º to the west, and consider its local time as observing a WET time zone. After that, we can calculate the λ distribution and its average value to introduce it in the model.

For the generalization of this correction, we can define a *Longitude Z*, (λ_Z), as the longitude at which local time can be considered with no UTC offset. It can be calculated as:

$$\lambda_Z = \lambda - 15 \cdot \Delta_{\mathrm{UTC}}$$

where λ and Δ_{UTC} are the real longitude and UTC offset of the place of sighting.

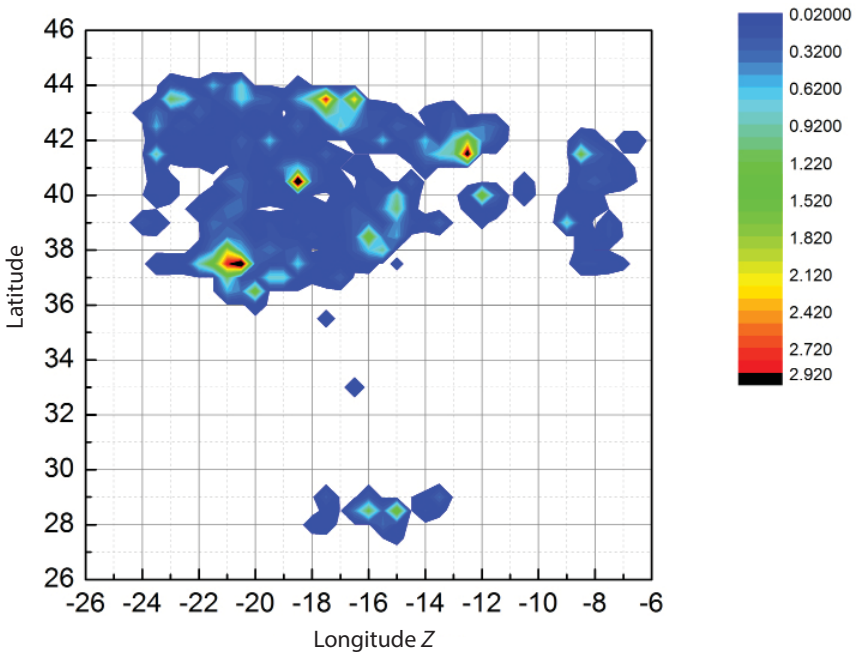

Figure 16. λ_z distribution of CUCO, including Spain, Portugal, Andorra, Canary Islands, Madeira, and Azores. Color maps the percentage of cases.

If we apply this transformation to CUCO, we obtain the distribution in Figure 16. We can see that Portugal appears to the east of Spain. But we can also see that Canary Islands do not suffer any change, since they are already using a UTC + 0 time zone, and appear in their real position in the same longitude ranges than *moved* Spain. The average coordinates for the geographical distribution of CUCO are ϕ = 40.35º N, and λ_Z = 17.98º W, considering a UTC time zone for all the locations in the catalog: Iberian peninsula, Ceuta, Melilla, Andorra, Canary Islands, Madeira, and Azores.

Some countries have a UTC offset that makes local time synchronized with their natural solar time. That means that a longitude translation to the UTC time zone will yield values between 7.5º E and 7.5º W. For those one hour ahead of their solar times, like Spain, λ_Z will be between 22.5º and 7.5º W. Finally, the behavior of any catalog (local, regional, or worldwide) will be as though the world has been compressed into a region from 22.5º W to 7.5º E and can be described in terms of this *Longitude Z*.

CUCO time distribution. Figure 17 shows CUCO and the result of the model. The model fits the main peak very well, using the same parameters that were estimated and calculated in the previous sections. Only those for

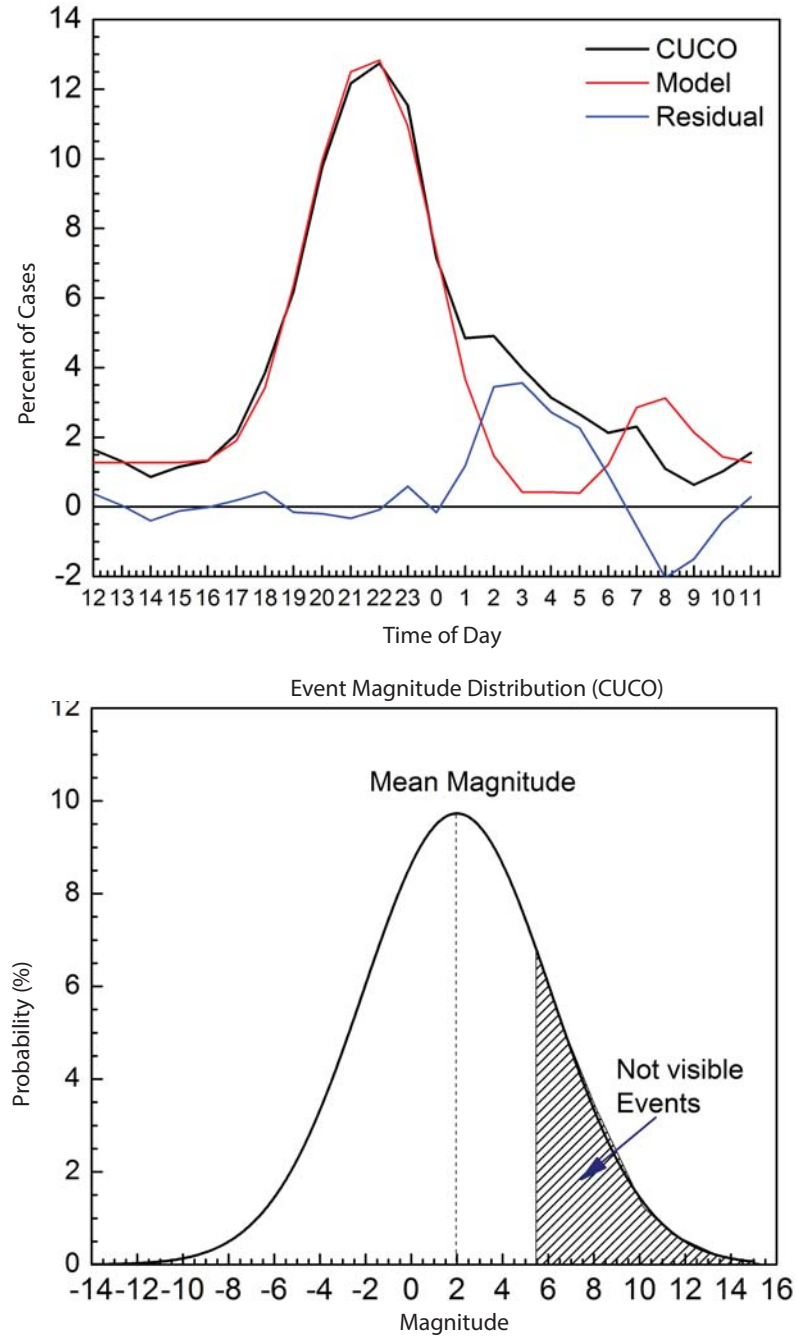

Figure 17. (Top) Law of Times for CUCO, and result of the modeling, (Bottom) Distribution of Event Magnitude for CUCO.

the magnitude of events (mean magnitude and standard deviation of events) were free to vary to fit the model.

On the other hand, cases near sunrise and morning hours are overestimated—at least for this catalog. The expected morning peak simply vanishes in the data, and the only way to reproduce it would be increasing the Wake-Up time to later hours, to values that would make no sense. This means that this parameter is not a good descriptor for the sunrise and morning features, and changes need to be made to Witnessing Probability, or a new factor needs to be introduced into the model.

The secondary peak is not reproduced either. The residuals show that it represents about 13% of cases in CUCO.

The only parameters that were varied for fitting the model were the mean magnitude of events, and its standard deviation. As the model does not reproduce all the features of the data, only the main peak was used to optimize the parameters (from 12:00 p.m. to 01:00 a.m.), yielding a coefficient of determination of $R^2 = 0.99$.

The Influence of Technology: FOTOCAT

The other catalog we worked with is FOTOCAT (FOTOCAT Catalog). It is a catalog composed of photographs and video footage, which means there is an important technological factor in this catalog, which we can see by looking at the time distributions shown in Figure 1.

We worked with a set of 2,247 IFO cases. These were originally classified into 7 categories:

- Hoax: fakes and manufactured flying saucers.
- Camera and film-related: development flaws, lens flares, artifacts related to the camera . . .
- Aerospatial: aircraft, condensation trails, balloons, helicopters, satellites, reentries, airborne debris . . .
- Meteorological and geophysical: clouds, mirages, ball lightning . . .
- Astronomical: bolides, stars, planets . . .
- Biological: bugs, birds, persons . . .
- Miscellaneous: automobiles, debris, ground lights . . .

We can see from this classification that some of the explanations are solely dependent on technology: development flaws, lens flares, flying-by birds or bugs, blurred objects . . . . They do not depend on visibility and are only seen after taking the image (i.e. they were not seen initially by the photographer, and hence not photographed on purpose). On the other hand, we have cases that are basically in the scope of our model: planes, satellites,

distant lights, clouds . . . . Those are events likely to have been seen by the photographer, and so photographed on purpose. The main contributions to IFO cases are Aerospatial (~33%), followed by hoaxes (~22%) and technology-related explanations (~13%).

But, we can define a broader classification of IFOs as follows:

- Hoax: This is the same category as the previous Hoax category (512 cases, 25.4%).
- Accidental image: Composed of the previous categories 2 and 6 (camera and film-related; and biological). This category joins cases of UFOs most likely not seen at the moment of taking the images: film flaws, flares, flying-by birds or bugs . . . . These cases are not covered by the model developed in this work (500 cases, 24.8%).
- On-purpose image: Composed of the previous categories 3 (Aerospatial), 4 (Meteorological and Geophysical), and 5 (Astronomical). This category joins events that were likely to be seen, and hence photographed on purpose. They also are cases covered by the model developed in this work (1,005 cases, 49.8%).

Most cases from category 7 (Miscellaneous) have been reclassified into *Accidental* and *On-Purpose* new categories. Duplicated cases also have been removed, for a total of 2,017 unique IFO cases.

Let us look at the individual time distributions of each of these categories. Figure 18 shows the Law of Times for Accidental Images and Hoaxes. As expected, they do not follow the usual time distribution. It is interesting to compare the equivalent categories of IFO cases in CUCO. Technology-related IFOs (only 36 cases in CUCO) seem to follow a distribution similar to FOTOCAT. The very low count of cases produces high peaks that should be considered noise. But for hoaxes (227 cases in CUCO), the distributions are different, CUCO's resembling the usual Law of Times.

Technology-related IFOs in CUCO and Accidental Images in FOTOCAT confirm that the use of technology introduces a factor different from those used in this work. The probability of having flares when making a photograph, development flaws, blurred objects, or bugs, etc. . . . should be directly related to the number of images taken. More photographs means more opportunities to have a flaw or an artifact. It is obvious that more photographs are taken during daylight than during the night because of available light, but also because more people are awake—this time both factors are synchronized.

On the other hand, Hoax time distributions show that technology influences the way hoaxes and fakes are made. If we think of how in

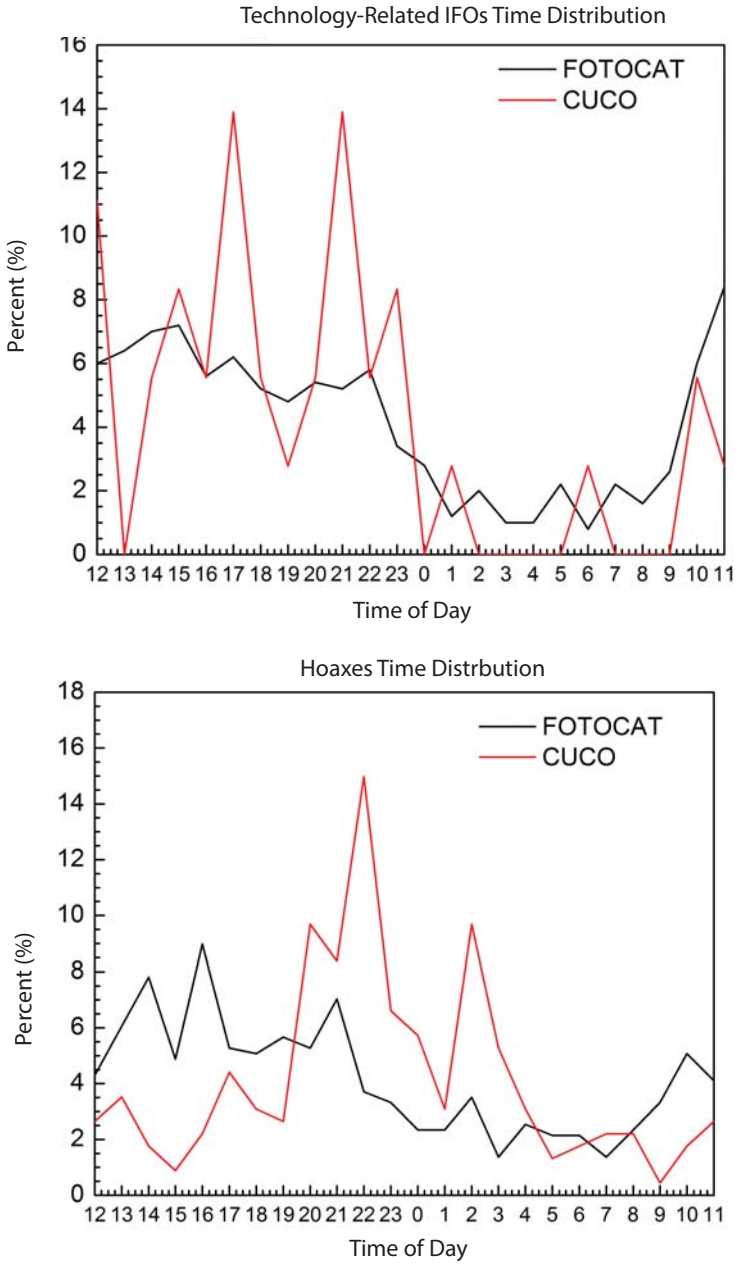

Figure 18. (Top) Law of Times for each IFO category, (Bottom) Comparison between total IFO cases, On-Purpose cases, and CUCO.

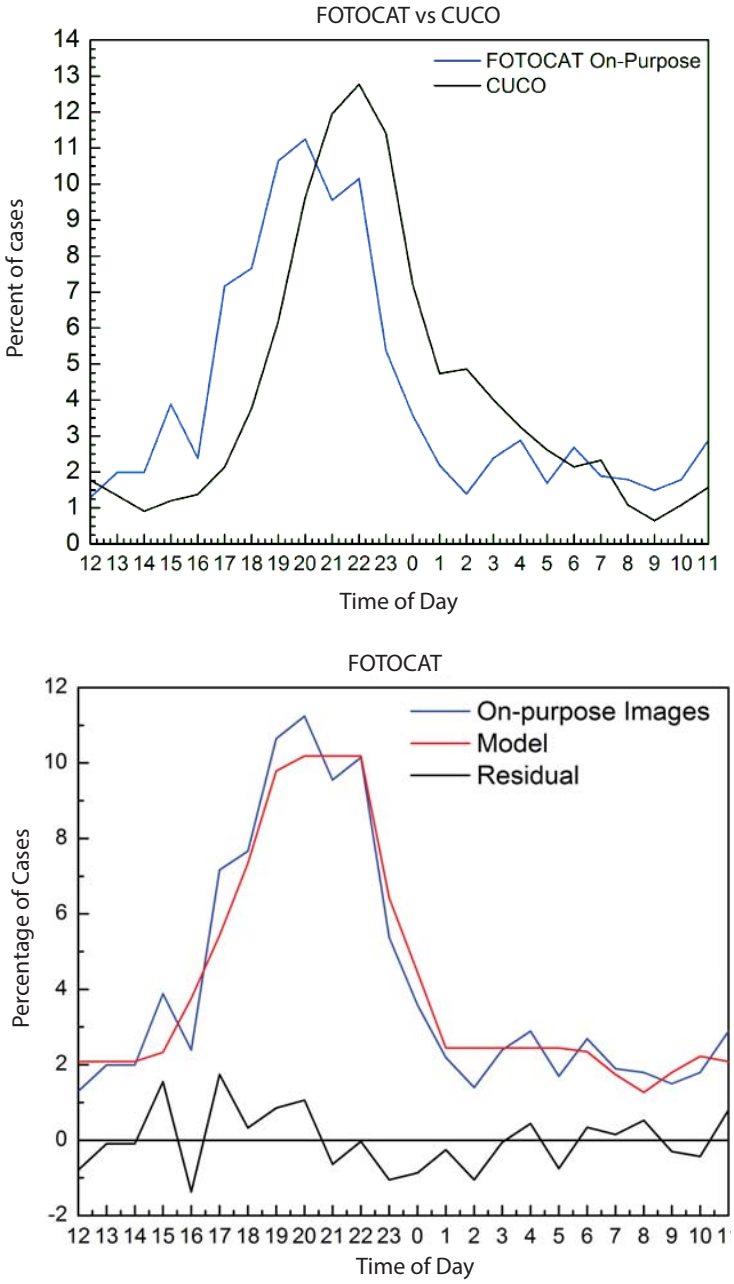

Figure 19. (Top) FOTOCAT On-Purpose Images and CUCO Law of Times, (Bottom) Reproduction of FOTOCAT On-Purpose images with the model.

TABLE 3

Parameters for P_w That Maximizes Correlation of Accidental Images and Witnessing Probability

Parameter	Value
Wake-Up time (h_a)	9:08 h
Wake-Up deviation (σ_a)	0.35 h
Go-to-Bed time (h_b)	23:03 h
Go-to-Bed deviation (σ_b)	1.47 h
Awake population at night (p_0)	24%
Correlation coefficient	0.94

FOTOCAT someone would create a fake image, it is reasonable to think that one would make a photograph or video clearly showing a fake object, and clear images are easily taken during daylight. On the other hand, in CUCO, if one wants to fool others, a stimulus has to be confused, not identified by observers, and this is more easily done at night time.

Our main focus, however, is images taken On-Purpose. This implies that the photographer saw an event and tried to take a picture of it. The origin of the sighting fits directly under the assumptions of the model, and we can see how that curve resembles the usual shape than the other categories, when compared to CUCO in Figure 19 Top. The slope of the rise of the main peak is more similar to CUCO, but it is remarkable to see that the secondary peak in the FOTOCAT On-Purpose Images distribution seems to be absent.

We can try to fix some parameters in a similar way as we did with CUCO. A close look at the three IFO categories reveals that the decrease of cases in FOTOCAT at night is at about the same time, clearly showing when people go to bed. We can take advantage of the apparent direct relationship between accidental images and human activity, and deduce starting values for Witnessing Probability. The best correlation between P_w and Accidental Images is shown in Table 3, and is as high as 0.94.

To fix the geographical parameters, a geographical distribution analysis shows that the mean position is at ϕ = 42.58º N, λ_Z = 7.31º W. That is 10.7º to the east of CUCO, which explains the fact that the FOTOCAT main peak rises earlier.

The only parameters free to be fitted into the model are those related to

TABLE 4
Model Parameters for CUCO and FOTOCAT

Parameters		CUCO	FOTOCAT
Fixed	Witnessing probability (P_w)		
	Wake-Up time	7:23 h	9:08
	Wake-Up deviation	0.99	0.35
	Go-to-Bed time	0:03	23:03
	Go-to-Bed deviation	1.39	1.47
	Awake population at night	3%	24%
Visibility (P_v)	Mean latitude	40.35° N	42.58° N
	Mean longitude z	17.98° W	7.31° W
Free	Mean event magnitude	2	−2.98
	Event magnitude deviation	4.1	1.26

the magnitude of events. The final fit yields a Coefficient of Determination $R^2 = 0.94$.

Discussion

Table 4 shows the model parameters for both CUCO and FOTOCAT. The next question to answer is whether those values are reasonable, taking into account what they represent.

For CUCO, we used the power demand to fix the parameters for Witnessing Probability. Thus, values are directly related to human activity. Results have to be interpreted as people going to bed at midnight as the mean hour, and 95% of people doing it between 21:15 and 2:45. This seems reasonable for a country like Spain. FOTOCAT, on the other hand, yields a mean Go-to-Bed time an hour earlier, and the time interval in which most of the population goes to bed is roughly the same as CUCO. These values for both catalogs seem reasonable at first sight, but should be validated with some related statistics, or social studies.

As for the Wake-Up time, we fixed a mean Wake-Up time of 7:23 h. This hour is typical for the beginning of commuter rush hours, and thus it seems reasonable. However, the number of cases is overestimated by the model. Should another factor be taken into account? In the morning people

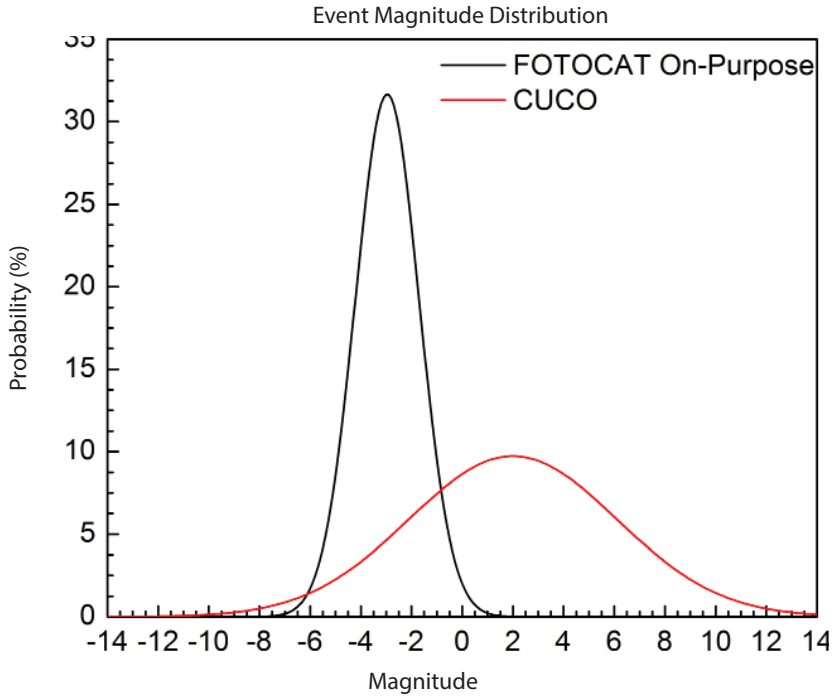

Figure 20. Event Magnitude distribution for CUCO and FOTOCAT.

are just beginning their day, and going to work. Rush hour requires attention to the traffic, and some people sleep while going to work on the bus or train. In the evenings, people tend to be more relaxed, and perhaps their attention can be more easily diverted to look at the sky. Should something like an "attention factor" be included in the model?

Something similar happens in FOTOCAT. Assuming that the time distribution of accidental images is directly related to the Witnessing Probability, we fixed a Wake-Up mean time at 9:08. It is obviously a very late hour. In any case, since the catalog is strongly influenced by technology, perhaps Witnessing Probability should be interpreted as the availability of cameras, or the availability of taking images. This, again, could be related to rush hours and the beginning of the day: A person driving to work would not stop to take an image.

The parameter for people awake at night is strangely high for FOTOCAT. We do not have any interpretation for this value.

Finally, the fits give an interesting difference with CUCO, since Events for FOTOCAT need to be brighter (Figure 20). Sensibility of devices is not

the same as that of the naked eye. Also, the Limiting Magnitude can be different for different cameras or devices.

Conclusions

To the author's best knowledge, this is the first time a quantitative model has been developed to explain the Law of Times. The model can successfully reproduce the main feature of the law, which is the peak at 21–22 h.

The model was derived using three factors: event activity, a geographical–astronomical factor, and a social factor. The second one accounts for the conditions of sightings, i.e. the light conditions are determined by the position of the Sun with respect to the location of the sighting. The third one accounts for the availability of someone to see the event.

Poher and Vallée, and Process Theory, assumed an ideal UFO activity to explain the origin of the Law of Times. We account for a similar factor in Event Activity that can be modeled in different ways. Experience shows us that among unidentified cases in catalogs, there are an undetermined number of potential IFO cases with natural or common causes. When considering all possible sources of events, a constant event activity hiding a real UFO activity can be considered, and thus the Law of Times becomes independent of event activity. Therefore, only the other two factors are enough to describe most time distributions.

With this approximation, some thoughts can be derived about the nature of events within our model. An event may be anything: lights, stars, planets, reflections on balloons or clouds . . . the only way those can be reported as UFO sightings is by misperception. The model is telling us that these misperceptions are happening continuously, and their time of sighting is solely related to the aforementioned factors.

That does not rule out the sighting of any strange or unusual phenomenon, but since UFO and IFO time distributions show basically the same curve, it is just straightforward to think that the vast majority of UFO reports may also be misperception cases that are still unexplained. However, the continuous investigation of individual UFO cases to become IFO cases should, eventually, reveal the actual UFO activity, if there is any.

We also saw that other factors can influence the time distribution, such as photographic cameras or video recorders: lens flares, flying-by birds or bugs, dust, development flaws. . . . These do not depend on astronomical factors, but on a technological one. Nowadays, it is easy and quick to take photos or videos at any time. But, was it as easy to have a camera ready at any time in the 1960s, 1970s, or 1980s? Technology evolves over time. Film sensibility, development processes, and optics have improved since the times of old manual cameras. And then, digital technology made film

cameras obsolete. Could this evolution have any influence on the time distribution of image catalogs in any way?

Hoax is another factor that is not a priori dependent on astronomy or a wake/sleep cycle, although it can be considered as a sort of social factor, and we saw that that can be indirectly influenced by technology. Furthermore, we can think about how digital image editing makes it extremely easy to create hoaxes at any time, and we can often see hoax videos going viral on YouTube. In a few decades, the time distribution of future photographic catalogs might be different from FOTOCAT.

On the topic of drawbacks, we have to mention that the secondary peak is not reproduced by the model. Its origin is totally uncertain, but on the other hand it appears in both UFO and IFO time distributions. This suggests that it cannot be attributed to real UFOs or strange-phenomena activity. Also, Witnessing Probability was thought up as a quantification for awake people. It seems to yield reasonable values to describe the main peak. But there is an overestimation for morning cases in CUCO, which can only be fitted assuming a later Wake-Up time. Also for FOTOCAT, the estimation of awake people at night seems incorrect. Therefore, a better interpretation of the Witnessing Probability is needed.

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RESEARCH ARTICLE

Can Solar Activity Influence the Occurrence of Economic Recessions?

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The views expressed in this paper are solely those of the author and do not represent IMF views or policy.

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Abstract—This paper revisits evidence of solar activity influence on the economy. We examine whether economic recessions occur more often in the years around and after solar maximums. This research strand dates back to late 19th century writings of the British economist William Stanley Jevons, who claimed that "commercial crises" occur with periodicity matching solar cycle length. Quite surprisingly, our results suggest that the hypothesis linking solar maximums and recessions is well anchored in data and cannot be easily rejected.

Keywords: business cycle—recession—solar cycle—unemployment—sunspot

Introduction

This paper reviews empirical evidence of the apparent link between cyclical maximums of solar activity and economic crises. An old theory outlined by British economist William Stanley Jevons in the 1870s claimed that "commercial crises" occur with periodicity broadly matching the solar cycle length of about 11 years. It is common knowledge that this "beautiful coincidence" claimed by Jevons and its theoretical explanation linking the "commercial crises" to bad harvests did not stand the test and were rejected by subsequent studies. We could not find any publications in reputed economic journals in the last 80 years testing the core proposition of Jevons' theory, namely that there is a link between cyclical maximums of solar activity and economic crises. Our results suggest that economic recessions in the U.S. and other advanced economies do occur more frequently in the years around and after the solar maximums. These results are broadly consistent with the hypothesis advanced by Jevons more than 100 years ago with regard to "commercial crises."

Since the beginning of the 20th century, for more than 100 years each

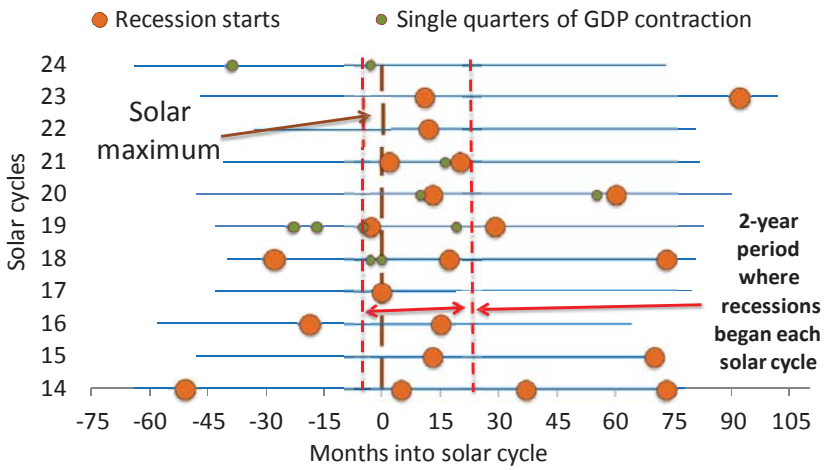
of the 10 cyclical maximums of solar activity overlapped closely with a recession in the U.S. economy. This relation became even more apparent from the mid-1930s on, when 8 out of 13 recessions identified by the National Bureau of Economic Research (NBER) began in the 2 years around and after the solar maximums. On a global scale, over the last 50 years (from 1965) when consistent recession dating is available for all G7 countries, nearly 3/5 of these recessions started during the 3 years around and after sunspot maximums. Looking back at the 19th century, out of 12 years that could be identified as episodes of "commercial crises," 6 were within 3 years of a sunspot maximum (Figure 1, Figure 2, and Figure 3). Was it a mere coincidence or a part of a broader pattern?

To verify robustness of these empirical observations, we ran statistical tests. We estimated the probability of so many U.S. recessions starting in the narrow period around sunspot maximums. And we checked the statistical significance of correlations of time series characterizing the occurrence of economic crises and the series of sunspot numbers reflecting intensity of solar activity. Our tests suggest that the hypothesis of the more frequent occurrence of economic crises around the periods of maximum solar activity cannot be rejected. At the same time, our results do not shed much light on the exact factors of solar influence that could trigger these events nor do they reveal the channels of their propagation.

The rest of the paper is organized as follows. What Are Solar Cycles and Sunspots? outlines the basic facts about solar cycles and their measurement. How Does Elevated Solar Activity Affect Earth? discusses how various types of solar radiation can affect Earth and outlines core propositions of the theories advanced by Jevons and other scientists, most notably Russian scientist Alexander Chizhevsky, about the possible impact of solar activity on the economy and society. Solar Activity and Economic Crises examines the links between solar maximums and recessions as well as with selected indicators of economic activity fluctuating with the business cycle. Building on these results, What Can We Project for the Next Solar Maximum? outlines practical implications of our findings for forecasting economic activity in the U.S. and other advanced economies. Conclusions and Prospects for Further Research concludes and sets an agenda for future research.

What Are Solar Cycles and Sunspots?

Sunspots are temporary phenomena on the Sun's surface that appear as visible dark spots compared to surrounding regions. They are caused by intense magnetic activity that inhibits convection and forms areas of reduced surface temperature. In 1610, Galileo Galilei and Thomas Harriot recorded the first European observations of sunspots. Daily observations began at



Sources: NBER; FRED; NASA; and author's calculations.

Figure 1. U.S. recession starts in 1901–2014 (solar cycles 14–24 centered on solar maximums).

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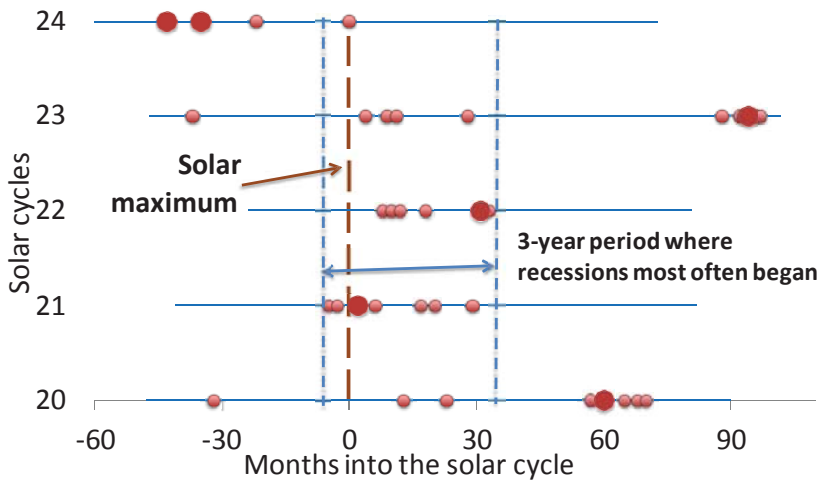
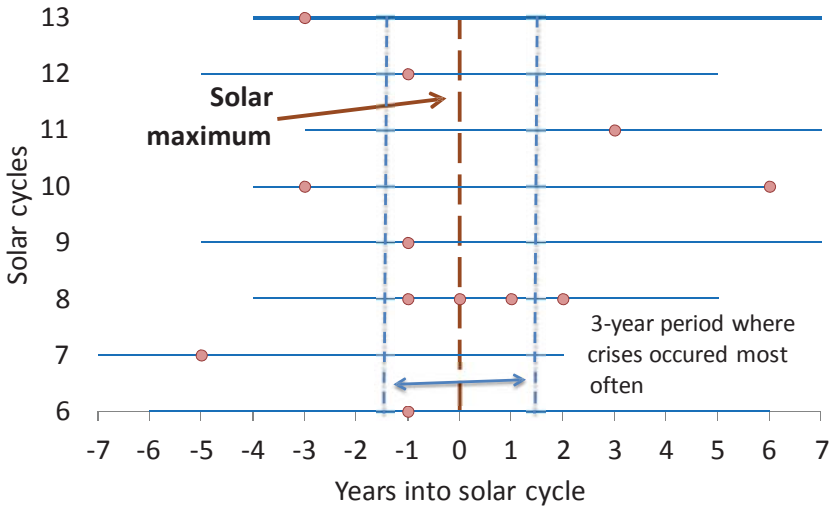


Source: NBER; ECRI; NASA; and author's calculations.

Figure 2. G7 recession starts in 1965–2014 (solar cycles 20–24 centered on solar maximums). Larger markers are used for months when recession began in two countries.



Sources: Hyndman (1892); NASA; and author's calculations.

Figure 3. Commercial crises in the 19th century (solar cycles 6–13 centered on the years of maximums).

the Zurich Observatory in 1749. The "international sunspot number"—also known as the Wolf number or Zurich number—is calculated by first counting the number of sunspot groups and then the number of individual sunspots. The sunspot number is obtained as the sum of the number of individual sunspots and ten times the number of groups. Since most sunspot groups have, on average, about ten spots, this formula for counting sunspots gives reliable numbers even when the observing conditions are less than ideal and small spots are hard to see.

Monthly averages of the sunspot numbers show that the quantity of sunspots visible on the Sun fluctuates with an approximate 11-year cycle known as the "solar cycle," which was first discovered in 1843 by Heinrich Schwabe. Sunspot populations quickly rise and more slowly fall on an irregular cycle of 11 years, though significant variations in the length of this cycle have been recorded over the centuries of observations. The cycles are numbered since 1750, with the first cycle running from the minimum in 1755 to the next cyclical minimum in 1766. Currently, the 24th cycle is unfolding from a minimum in December 2008 through the cyclical maximum estimated by NASA scientists to have occurred in April 2014 toward the next minimum possibly around 2020.

In addition to the sunspot number, which remains the primary index of

solar activity, many other indicators have been established and recorded, particularly in recent years. They include the indicators of radio activity, radiance, proton emission, solar wind, flares, and coronal mass ejections (CME). All these indicators broadly follow the solar cycle as measured by the sunspot index and reach their maximums around sunspot maximums (Kane 2002). The degree of variation from minimum to maximum differs widely among these indices, with some of them barely detectable. For example, the total solar irradiance (TSI) measured as the amount of solar radiative energy incident on the Earth's upper atmosphere varies with an amplitude of just about 0.1% (and maximum deviations of about 0.3%) around its average value of about 1,366 W/m^2 (named the "solar constant"). Variations of this magnitude were undetectable until satellite observations began in late 1978.

How Does Elevated Solar Activity Affect Earth?

Main Channels of "Physical" Impact

Events associated with elevated solar activity can produce a significant "physical" impact on Earth. Solar flares and CMEs can disrupt radio and telecommunications and cause satellite malfunction. In particular, CMEs can trigger fluctuations in the geomagnetic field known as "magnetic storms" and even induce electromagnetic impulses in power grids that damage electric equipment. In their visible manifestation, CMEs can cause particularly powerful polar lights (auroras), which become visible in much lower latitudes than normal. In addition, solar flares produce high-energy particles and radiation, such as high-energy protons and x-rays, which are dangerous to living organisms. However, Earth's magnetic field and atmosphere intercept these particles and radiation and prevents almost all of them from reaching the surface.

In 1859, British astronomer Richard Carrington observed a solar flare of enormous proportions. The CME associated with it reached Earth within a day, producing the strongest geomagnetic storm ever recorded. In a visible manifestation of it, the storm induced auroras as far from the poles as Cuba and Hawaii. Reportedly, the people who happened to be awake in the northeastern U.S. could read a newspaper by the aurora's light. The storm caused massive failures of telegraph systems all over Europe and North America. In certain cases, telegraph operators reportedly got electric shocks, and telegraph pylons threw sparks. More recently, in 1989, a much smaller solar event triggered a geomagnetic storm that caused the collapse of northeastern Canada's Hydro-Quebec power grid.

Luckily, the "Carrington event" occurred in the era before electricity.

Had it happened today, it would have likely caused up to $2 trillion of damage to electric grids and equipment all over the world, which would have taken years to rebuild (NAS 2008). A recent study estimated the probability of a comparable "extreme space weather event" impacting Earth at 10–12% over a 10-year period (Riley 2012). To give an idea of the risks associated with elevated solar activity, a CME of a magnitude comparable to that of the "Carrington event" occurred most recently in mid-2012. Luckily, it was not directed at Earth, but had it happened just a week earlier it would have hit our planet (Baker, Li, Pulkkinen, Ngwira, Mays, Galvin, & Simunac 2013).

Possible Impact on the Economy and Society

Perhaps the earliest recorded hypothesis about the relationship between solar and business activity appeared in 1801. In a paper presented that year, Sir William Herschel, an astronomer, called attention to an apparent relationship between sunspot activity and the price of wheat (Herschel 1801). Then in 1838 and again in 1847, Dr. Hyde Clarke noted an 11-year cycle in trade and speculation and advanced the idea of a physical cause for this regularity (Clarke 1847).

Building on these and other anecdotal observations, British economist and statistician William Stanley Jevons developed the theory explaining the period of the trade cycle with variations in solar activity. In Jevons' lifetime, the commercial crises had occurred at intervals of 10–11 years (1825, 1836–1839, 1847, 1857, and 1866), which broadly matched the average solar cycle length. In his papers, Jevons carried back this history of "commercial crises" at 10–11 year intervals almost to the beginning of the 18th century (Jevons 1875, 1878, 1879). This "beautiful coincidence," as he called it, produced in him a strong conviction of causal nexus, going from cyclical solar activity through crop-harvest fluctuations to commercial trade cycles. He linked the crises first to harvests in Europe, and subsequently to Indian harvests, which, he argued, transmitted prosperity to Europe through the greater margin of purchasing power available to the Indian peasants to buy imported goods (Keynes 1936).

However, there were several significant flaws in Jevons' theory and calculations. First, he assumed that the solar cycles were highly regular, while in fact their length varied considerably. When he later got hold of the actual sunspot number series, he discovered that the "commercial crises" identified by him landed in various phases of solar cycles, thus breaking the perception of the "beautiful coincidence" (Jevons 1882). Second, he devoted insufficient attention to the exact dating of deficient harvests in relation to the dating of commercial crises. As a result, some of the bad harvests identified by Jevons appeared to have happened after the "commercial

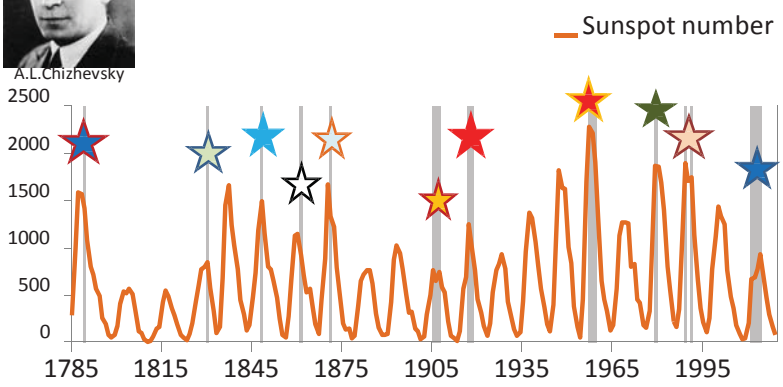
crises" that they were supposed to explain. These apparent flaws exposed Jevons' theory to strong criticism. Also, they diverted attention from his core proposition of the "commercial crises" relationship to the solar cycle to the question of solar influence on crops and agriculture.

Jevons' writings inspired other researchers to look for possible links between solar activity and events on Earth, which became a popular topic. In 1918, an article by an American "prodigy" became one of the first to extend the causal chain from solar cycles (and poor harvests) to revolutions (Sidis 1918). It hypothesized that revolutions occur in "warm countries" near the cyclical minimums of solar activity and in "cold countries" near the solar maximums.

Meanwhile, Russian scientist Alexander Chizhevsky advanced a theory suggesting that all of human history was influenced by the cycles in solar activity (Chizhevsky 1924). His thinking was probably influenced by the striking observation that two Russian revolutions of the early 20th century (in 1905–1907 and 1917) and several major European revolutions of the 19th century (in 1830, 1848, and 1871) occurred in the years of maximum solar activity (Figure 4). To justify his conviction, Chizhevsky scrutinized the available sunspot records and solar observations comparing them to riots, revolutions, battles, and wars in Russia and 71 other countries for the period from 500 B.C. to 1922. He found that a significant percent of revolutions and what he classified as "the most important historical events" involving "large numbers of people" occurred in the 3-year periods around sunspot maximums. Chizhevsky proposed to divide the 11-year solar cycle into four phases: (1) a 3-year period of minimum activity (around the solar minimum) characterized by passivity and "autocratic rule"; (2) a 2-year period during which people "begin to organize" under new leaders and "one theme"; (3) a 3-year period (around the solar maximum) of "maximum excitability," revolutions, and wars; (4) a 3-year period of gradual decrease in "excitability," until people become " apathetic."

Through his subsequent studies, Chizhevsky came to believe that correlations with the solar cycles could be found for a very diverse set of natural phenomena and human activities. In his book, he compiled a list of as many as 27 of them that supposedly fluctuated with the solar cycle, ranging from crop harvests to epidemic diseases to mortality rates (Chizhevsky 1938, 1976). Chizhevsky presented various quantitative and anecdotal evidence in support of his views. According to his studies, the periods of maximum solar activity were generally associated with negative effects such as lower harvests, intensification of diseases (including psychological ones), and higher mortality rates.

Subsequent studies generally did not confirm the strength and scope of



	YEARS	EVENTS
	1789	Great French Revolution
	1830	Revolutions in Europe (France, Poland, Germany, Italy, Greece)
	1848	Revolutions in Europe (Italy, France, Germany, Austria, etc.)
	1861	Secession of 13 southern U.S. states that formed C.S.A.
	1871	Uprising in Paris, "Paris Commune"
	1905-07	Revolution of 1905-07 in the Russian Empire
	1917 1918	February Revolution, Great October Socialist Revolution in Russia Revolution in Germany, collapse of the Austro-Hungarian Empire
	1957-59	Revolution in Cuba
	1979	Islamic Revolution in Iran
	1989 1991	Fall of Berlin Wall, collapse of communism in Eastern Europe Collapse of Soviet Union and Yugoslavia
	2010-13 2013-14	"Arab Spring," Revolutions in Egypt, Libya, Syria, Yemen, Tunisia, etc. Revolution in Ukraine

Sources: Chizhevsky (1924, 1976); NASA; history textbooks

Figure 4. Selected revolutions that overlapped with solar maximums, 1785–2014.

the links between solar activity and various physical and social processes claimed by Chizhevsky and before him by Jevons. Still, occasionally new papers appeared that claimed the existence of such strong links. In 1968, Edward Dewey reported that cycles of 43 activities fluctuated with the sun's 11-year cycle, including commodity and stock prices, banking and business activity, industrial production, and agricultural productivity (Dewey 1968). He also compiled a comprehensive review of the previous literature on the subject. In 1993, Bryan Walsh revisited Dewey's findings using the newly available data for the changes in geomagnetic field that broadly followed the solar cycle. He claimed that perturbations in the geomagnetic field preceded several common indicators of economic and financial performance (GNP, CPI, stock prices, etc.) by 6 to 12 months, with correlations as high as 65% (Walsh 1993).

And even as the link between solar activity and revolutions was not as strong as originally claimed by Chizhevsky, it appeared to be able to withstand a statistical test. Russian scientist Putilov analyzed large samples of historical events mentioned in the chronology sections of two of the largest Soviet historical encyclopedias (numbering nearly 13,000 events in one book and 4,600 in another). He classified the events into four groups on the dimensions of "tolerance" (e.g., riot–reform) and "polarity" (e.g., civil war–external war). Putilov found that frequency and "polarity" of historical events increased in the year of the maximum of the sunspot cycle and in the next year after it, particularly when compared with the year of the minimum and the year before the minimum. The probability of revolution (the most polar and intolerant of historical events) was the highest during the maximum and the lowest in the year before a minimum of solar activity, with very high statistical significance. The results suggested that solar activity does impact historic events, particularly in the years of sunspot maximums (Putilov 1992).

And from time to time, researchers come across striking correlations between solar activity and economic events. In 2010, an analytical memo observed that in the postwar period, maximums of solar activity were preceded by troughs in the U.S. unemployment rate, while its peaks followed about 3 years after the peaks in sunspot activity (McClellan 2010). In 2011, a paper by two Russian scientists reported that from 1968, the cyclical fluctuations of the banking interest rate ("prime-rate") closely followed the solar activity cycle. In their other paper, those scientists reported a close correlation of the U.S. and global GDP with solar cycles (Poluyakhtov & Belkin 2011a, 2011b). In 2012, another memo observed that recessions in the U.S. economy often occur after solar cycle peaks, corresponding to the peaks in geomagnetism that lag solar maximums (Hampson 2012).

It is subject to much debate—producing a growing body of literature—whether and how elevated solar activity affects human health. One apparent channel of impact is solar activity causing disturbances in the Earth's magnetosphere leading to "magnetic storms" that affect people with cardiovascular health conditions and those having particular sensitivity to it (Palmer, Rycroft, & Cermack 2006). Another possible channel is solar or geomagnetic activity affecting the human brain and thus exacerbating psychological and mental illnesses. For example, a recent study reported significant correlation between sunspot periodicity and brain (cervical) pathologies and selected human physiological functions (Hrushesky, Sothern, Du-Quiton, Quiton, Rietveld, & Boon 2011). These findings suggest that solar-induced magnetic storm periodicities are mirrored by cyclic rhythms of similar periods in the human psyche and in health.

Solar Activity and Economic Crises

Solar Cycles and "Commercial Crises" of the 19th Century

By their nature, "commercial crises" of the 19th century stand close to what we define as "recessions" now. In the late 19th century, the commercial crisis was defined as a "disturbance of the course of trade at a given time, arising from the necessity of re-adjusting its conditions to the common standard and measure of value" (Cyclopædia 1899). Compare this with a standard contemporary definition of recession as a period of temporary economic decline during which trade and industrial activity are reduced, generally identified by a fall in GDP in two successive quarters.

The "commercial crises" appear to be correlated with the solar cycle variation, as Jevons once noticed. If we go with the list of major commercial crises as identified by Hyndman (1892), the resulting annual series (with 1 for crisis years and 0 for no crisis) has a correlation of 0.24 with the annual series of sunspot numbers (Table 1). With 100 data pairs, this correlation appears to be highly significant, suggesting that the hypothesis of a link between the crises and the solar cycle cannot be easily rejected. Out of 12 crises years, 4 fall on the years just before the solar maximum, resulting in a high correlation with the sunspot series (Figure 3).[1] However, with only a dozen observations, the significance of this correlation becomes sensitive to the exact dating of the crises. For example, if we treat the event of 1836–1839 as one crisis in 1837, the correlation with the annual sunspot series drops to 0.08, which is not statistically significant.

And if we widen the definition of crises events to include bank crises and stock market panics, correlation with the solar cycles disappears. For example, the list of major banking and financial crises in Conant (1915)

TABLE 1
Sunspots and Commercial Crises of the 19th Century

Source	Time period	Crisis years	Correlation	Significance *
Hyndman (1892)	1801–1900	1815, 1825, 1836-39, 1847, 1857, 1866, 1873, 1882, 1890	0.24	Very high (P=0.02)
Hyndman (1892)	1801–1900	Crisis of 1836–1839 treated as one event in 1837	0.08	Not significant
Conant (1915): Banking and financial crises	1798–1912	1810, 1814–1819, 1825, 1837–1839, 1847, 1857, 1864–1866, 1873–1879, 1882–1884, 1890, 1893, 1907	0.05	Not significant

Source: Conant 1915, Hyndman 1892, and author's estimates.
* In this and other tables, significance is based on t-statistic probability distribution, with lower probability **P** standing for higher significance of the correlation. We describe the significance of correlations characterized by probabilities of up to **0.0005** as "Extremely high"; by probabilities of about **0.01–0.02** as "Very high"; by probabilities of **0.05–0.08** as "Satisfactory"; by probabilities of about **0.1** as "Low"; and by probabilities above it as "Not significant."

has a correlation of only 0.05 with the sunspot series for the period from 1798–1912. This low correlation has no statistical significance. The events covered by Conant stand close to what we describe as financial or banking crises today. This suggests that when searching for correlation with solar cycles, we need to focus on crisis events related to fundamental economic conditions rather than on purely financial or banking crises, which occur much more frequently.

Economic Recessions in the U.S.

During the entire 20th century and in the early 21st century, each cyclical maximum of solar activity overlapped closely with the start of a recession in the U.S. economy. There were ten solar cycles from 1901 to 2008 numbered 14 to 23 by astronomers. And each time the solar cycle reached its maximum, a recession in the U.S. economy broke out within a 2-year period counting from 3 months before the maximum to 21 months after it (Figure 1). Out of 22 recessions officially identified by NBER from 1901–2008, 11 recessions began in this 2-year period around and after a sunspot maximum. The share of recessions beginning around solar maximums got even higher after the Great Depression. Counting from solar cycle 17, which began in 1933, 8 out of 13 recessions during 1933–2008 began in the 2 years around and after

the solar maximum. However, this relationship did not occur in the 19th century. Eleven recessions identified by NBER for 1854–1900 spread rather evenly across solar cycle phases, and only one began in the same 2-year period around the solar maximums.

Statistically, the chances of so many recessions occurring in a given 2-year interval within the 11-year solar cycle are very low. Solar cycles 17–23 corresponding to 1933–2008 run a total of 901 months. Out of this number, the 2-year period around and after solar maximums accounts for 168 months, which is 19% of the total. Thus, if we assume that recessions spread evenly over the solar cycle, the probability of a given recession occurring in that 2-year period is 0.19. Further assuming that the recessions occur as independent events, we can estimate that the probability of 8 or more out of 13 recessions occurring in the 2-year period around and after the solar maximum is less than **0.1%** (in other words, fewer than 1 out of 1,000). Extending the sample to 1901 (corresponding to solar cycles 14–23), we can estimate in a similar way that the probability of 11 or more out of 22 recessions occurring in the 2-year period remains less than **0.1%**. Even if we consider the entire scope of NBER-identified recessions from 1855 to 2008, including the period from 1855 to 1900 when only one recession occurred in that same 2-year period, the estimated probability of 13 or more out of 33 recessions occurring within it rises to about **1%**, which is still very low. This indicates that the hypothesis that U.S. recessions occur more often in the 2 years around and after solar maximums cannot be rejected.

Correlation analysis confirms the statistical significance of the link between U.S. recession starts and solar cycles. On a monthly frequency, a series of U.S. recession starts (with 1 for months when recession starts and 0 for all other months) has a correlation of nearly 0.09 with the sunspot series over the period 1933–2008. With 901 monthly pairs, this coefficient is highly significant. However, the value and significance of the correlation coefficient drops if we extend the sample to the beginning of the 20th century, and even more so if we include all recessions identified by NBER from 1855 on (Table 2).

Why would the correlation of U.S. recessions and solar cycles become so significant from the mid-1930s on? It might be because of changes in the frequency and nature of U.S. recessions after the Great Depression, in part because of shifts in the U.S. government policies induced by it. Changes in the economic structure also played a role. The diminishing role of agriculture reduced the impact of sporadic weather-related shocks, while rising globalization facilitated synchronization of business cycles across all advanced economies. Further on, after the mid-1940s the occurrence of military conflicts declined markedly, at least in the advanced economies.

TABLE 2
U.S. Recession Starts and Sunspots

Source	Time period	Correlation	Significance
NBER	Nov. 1993–Nov. 2008	**0.088**	Very high **(P=0.01)**
NBER	Sep. 1901–Nov. 2008	0.050	Satisfactory (P=0.07)
NBER	Jan. 1955–Nov. 2008	0.036	Low (P=0.12)

Source: NBER, FRED, NASA, and author's estimates.

Apparently, all these developments suppressed the impact of "random shocks" on the economy and increased the relative importance of recessions produced by fundamental factors that could be linked to elevated solar activity.

NBER provides precise monthly dating of U.S. recessions from 1855 on. With regard to the recession length and frequency, the entire period of 1855–2014 can be broadly divided into "before" and "after" the Great Depression of 1929–1933. In the time up to and including the Great Depression, recessions occurred more frequently and lasted about twice as long as in the period after it (Table 3). Consequently, the U.S. economy spent nearly 50% of the time in recessions during the period 1856–1933 (corresponding to solar cycles 10–16). This compares to only 15% of the time during 1933–2008 (solar cycles 17–23).

And why would the Great Depression lead to a "structural break" in recessions? Most notably, it exposed the dangers of deep and prolonged recessions, which prompted powerful shifts in government policies and regulation toward minimizing the chances of recessions and toward alleviating and shortening them. In practice, it meant that many random shocks, such as bank runs or stock market crushes, were either pre-empted (for example, through timely bank resolution) or no longer resulted in economy-wide recessions. And if a recession occurred after all, the government rushed to apply powerful economic stimulus to re-start growth as quickly as possible. This was not the case in the 19th or even early 20th century.

Switching attention from recession starts to recession length, between 1933 and 2008 the U.S. economy was in recession most often within about 3 years after the solar maximums. Over this period, from the month of the solar maximum to 36 months afterward, the U.S. economy spent more than 34% of its time in recession. In about 1 year in the middle of this range, from 12 to 24 months after the solar maximums, the "recession" indicator averaged 44%. Furthermore, in the very middle of this range, 1½ years after

TABLE 3
U.S. Recessions in 1856–2008
(before and after the Great Depression of 1929–1933)

Time period	Jan 1856 – Oct 1933	Nov 1933 – Nov 2008
Corresponding solar cycles	10 to 16	17 to 23
Months in recession	934	901
Recession starts	20	13
Total recession length, months	433	136
Average recession length, months	21.7	10.5
Time in recession, percent of total	46.4	15.1

Source: NBER; and author's calculations.

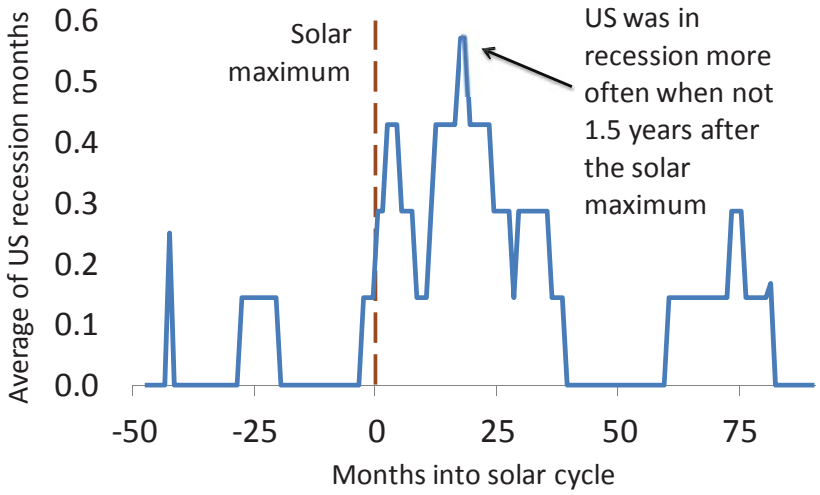
the solar maximums, there were two months when the "recession" indicator averaged 57%. In other words, the U.S. economy was in recession more often than not 1½ years after a solar maximum, which is much more often than in any other time across the solar cycle (Figure 5). Statistically validating this pattern, monthly series of U.S. recessions (with 1 for recession months and 0 for no recession) have a correlation of as high as 0.21 with the sunspot series (with the lag of 18 months) over 1933–2008 (Table 4).

However, this correlation loses significance once we extend the sample to 1901 and then to 1855, the same as for recession starts.

Economic Recessions in G7 Countries

The Economic Cycle Research Institute (ECRI) provides consistent dating of economic recessions for all G7 countries from 1965. This year corresponds neatly to the beginning of the 20th solar cycle. Using NBER recession dating for the U.S. and ECRI dating for the other 6 countries, we can expand to G7 the analysis of recession and solar cycle links done above for the U.S.

Our analysis indicates that during 1965–2008, recessions in G7 countries occurred much more often in the 3 years around and after solar maximums. Out of the total of 36 recessions that began in 1965–2008 in the G7 countries, 21 recessions started in the period from 5 months before the solar maximum to 33 months after it. Thus, about 3/5 of recessions started in



Source: NBER; FRED; NASA; and author's calculations.

Figure 5. Average of U.S. recession months in 1933–2008 (solar cycles 17–23, centered along solar maximums).

the 3 years around and after the solar maximums. This is remarkably close to the same proportion for the U.S. for 1933–2008 (8 out of 13 recessions), though in the case of the U.S. the time period was shorter (about 2 years around and after sunspot maximums). Correlation between the monthly G7 recession starts series (with 1 or 2 for months when recessions started in one or two countries and 0 for all other months) and sunspots is nearly 0.08 for 1965–2008, which is on the border of statistical significance (Table 5).

Turning from the recession starts to recession length, during 1965–2008

TABLE 4
U.S. Recession Periods and Sunspots (with lag of 18 months)

Source	Time period	Correlation	Significance
NBER	Nov. 1993–Nov. 2008	**0.21**	Extremely high **(P<0.0001)**
NBER	Sep. 1901–Nov. 2008	0.04	Not significant
NBER	Jan. 1955–Nov. 2008	–0.02	Not significant

Source: NBER, NASA, and author's estimates.

TABLE 5
G7 Recessions and Sunspots

	Source	Time period	Correlation	Significance
Recession starts	NBER/ ECRI	Dec. 1964– Nov. 2008	0.08	Satisfactory (P=0.08)
Recession length	NBER/ ECRI	Dec. 1964– Nov. 2008	**0.22**	Extremely high (**P<0.0001**)
With 18-month lag	NBER/ ECRI	Dec. 1964– Nov. 2008	**0.44**	Extremely high (**P<0.0001**)

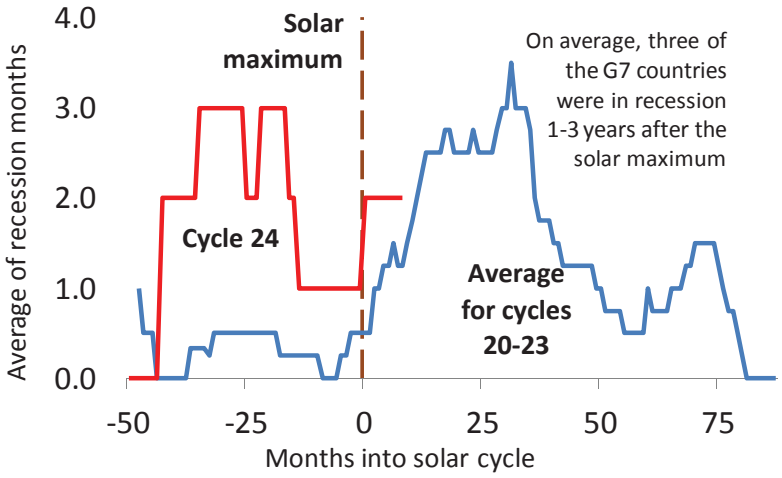
Source: NBER, FRED, ECRI, NASA, and author's estimates.

G7 countries found themselves in recession most often in the 1 to 3 years after the solar maximum. During this period, on average, about 3 out of 7 countries were in recession, which is much more often than in any other point of the solar cycle (Figure 6). Monthly series of the G7 recessions (that count the number of G7 countries being in recession each month) have a correlation of as high as 0.22 with the sunspot series, which is highly significant. Moreover, this correlation rises to as high as 0.44 if we take sunspot series with the lag of 18 months, to account for the fact that the G7 recessions peak 1–3 years after the sunspot maximum (Table 5).

Other Indicators of Changes in the Business Cycle

Once we established that recessions in the U.S. and G7 countries occurred more often around and after solar maximums, it is reasonable to expect that economic indicators fluctuating with the business cycle would deteriorate around the same period as well. In particular, we can expect it from aggregate measures of business activity such as composite leading indicators (CLIs) compiled by the Organisation for Economic Co-operation and Development (OECD). The OECD CLI system was developed, specifically, to predict cycles in a reference series and give early signals of turning points of economic activity (OECD 2012).

And indeed, CLIs for the U.S. and other G7 economies exhibit negative correlation with the solar cycle. In the U.S. over the period 1955–2008 (corresponding to solar cycles 19–23), the CLI took a dip (signaling deteriorating business conditions), on average, in the 3 years around and



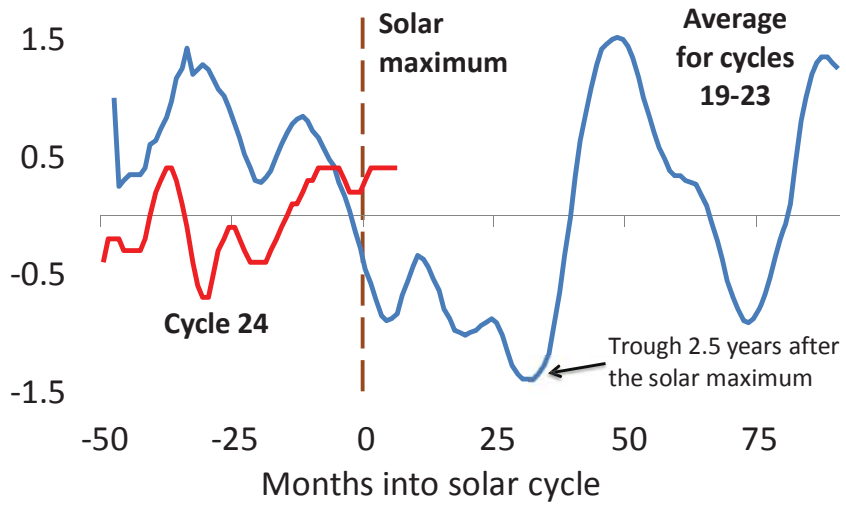
Source: NBER; FRED; ECRI; NASA; and author's estimates.

Figure 6. Average of G7 recession months in 1965–2014 (solar cycles 20–24 centered along solar maximums).

after the solar maximum, reaching its lowest point about 2½ years after it (Figure 7). Over the period 1955–2014, the CLI series had a statistically significant negative correlation of −0.15 with the sunspot series with a lag of 24 months (Table 6). A similar pattern is observed for all other G7 countries (Figure 8, Figure 9, Figure 10). The CLIs for these countries all have negative correlations with the sunspot series with a lag of 24 months, which is highly statistically significant for all countries but Japan. Moreover, the same pattern and statistically significant correlations are observed for aggregate CLIs for all G7 countries, for the entire OECD, and for OECD plus 6 "non-market economies" (Brazil, China, India, Indonesia, Russia, and South Africa).

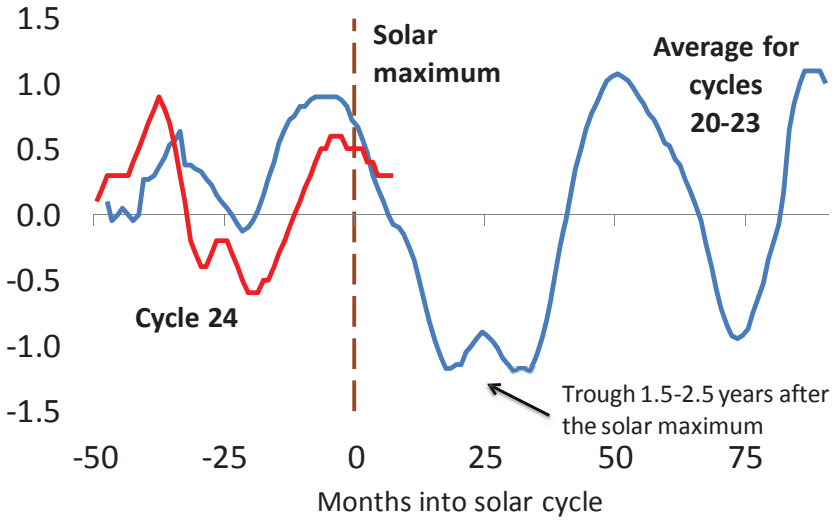
For the U.S., the results for the CLI are corroborated by similar findings for the Aruoba–Diebold–Scotti (ADS) business conditions index. The ADS index is designed to track real business conditions at high frequency. It blends high-frequency and low-frequency information and stock and flow data: jobless claims, payroll employment, industrial production, personal income, manufacturing and trade sales, and GDP (Aruoba, Diebold, & Scotti 2009). During 1964–2008, the ADS index had low negative values in the 3 years after the solar maximums, which indicated worse-than-average business conditions (Figure 11). And the index had a highly statistically significant negative correlation with the sunspot series (Table 7).

The U.S. unemployment rate exhibits even stronger correlation with



Source: OECD; NASA; and author's calculations.

Figure 7. U.S. CLI in 1955–2014 (solar cycles 19–24 centered along solar maximum).



Source: OECD; NASA; and author's calculations.

Figure 8. G7 average CLI in 1964–2014 (solar cycles 19–24 centered along solar maximum).

TABLE 6
OECD CLIs and Sunspots (with 24 months lag)

Countries	Time period	Correlation	Significance
US	Jan. 1955–Aug. 2014	**–0.15**	Extremely high (**P<0.0001**)
Canada	Jan. 1956–Aug. 2014	**–0.18**	Extremely high (**P<0.0001**)
France	Jan. 1970–Aug. 2014	**–0.16**	Extremely high (**P=0.0003**)
UK	Dec. 1957–Aug. 2014	**–0.11**	Very high (**P=0.01**)
Germany	Jan. 1961–Aug. 2014	**–0.10**	Very high (**P=0.01**)
Italy	Jan. 1962–Aug. 2014	**–0.10**	Very high (**P=0.01**)
Japan	Jan. 1959–Aug. 2014	–0.04	Not significant
G7	Jan. 1959–Aug. 2014	**–0.09**	Very high (**P=0.02**)
OECD	Jan. 1961–Aug. 2014	**–0.18**	Extremely high (**P<0.0001**)
OECD+6 *	Jan. 1970–Aug. 2014	**–0.20**	Extremely high (**P<0.0001**)

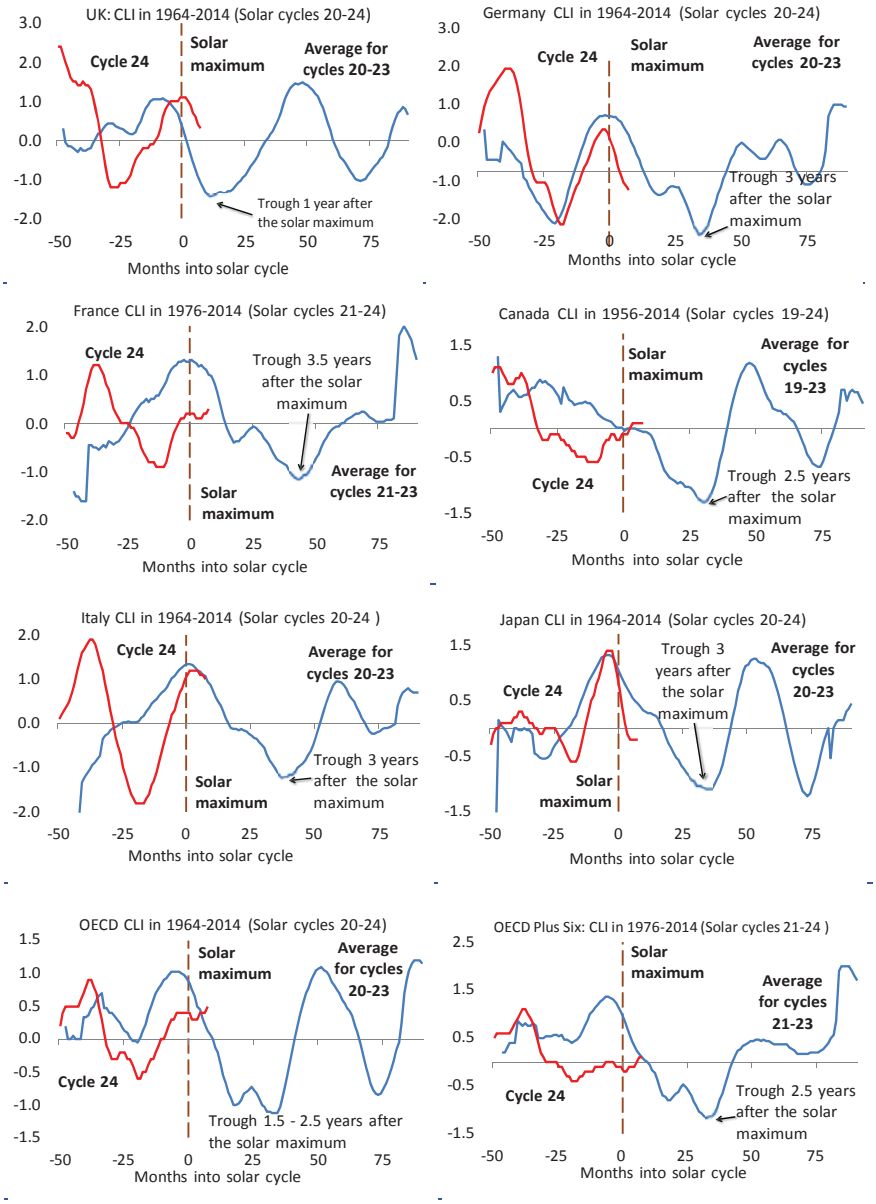
Source: NBER; FRED; ECRI; NASA; and author's estimates.
* OECD plus Brazil, China, India, Indonesia, Russia, and South Africa.

TABLE 7
US ADS, Unemployment, and Sunspots (with 24 months lag)

	Source	Time period	Correlation	Significance
ADS	US FED *	Mar. 1960–Oct. 2014	**–0.16**	Extremely high (**P<0.0001**)
Unemployment	Bureau of Labor Statistics	Jan. 1948–Oct. 2014	**0.12**	Very high (**P≤0.0006**)

Source: NBER; FRED; ECRI; NASA; and author's estimates.
* Published by Federal Reserve Bank of Philadelphia.

the solar cycle. Consistent monthly U.S. unemployment data is available from 1948 on. In the 66 years from 1948 to 2014, all 6 sunspot maximums overlapped closely with minimums of the U.S. unemployment rate. Moreover, each time the dynamics of unemployment changed from a



Source: OECD; NASA; and author's calculations.

Figure 9. G7 and OECD countries: sunspots and CLIs in 1956–2014 (solar cycles 19–24 centered along solar maximums).

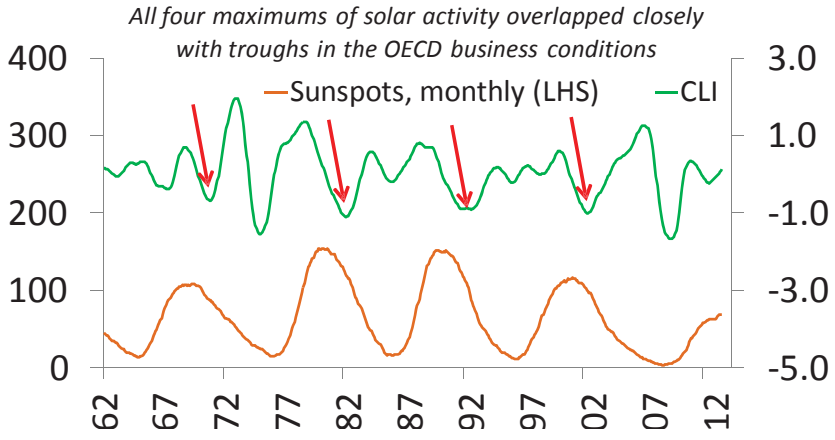
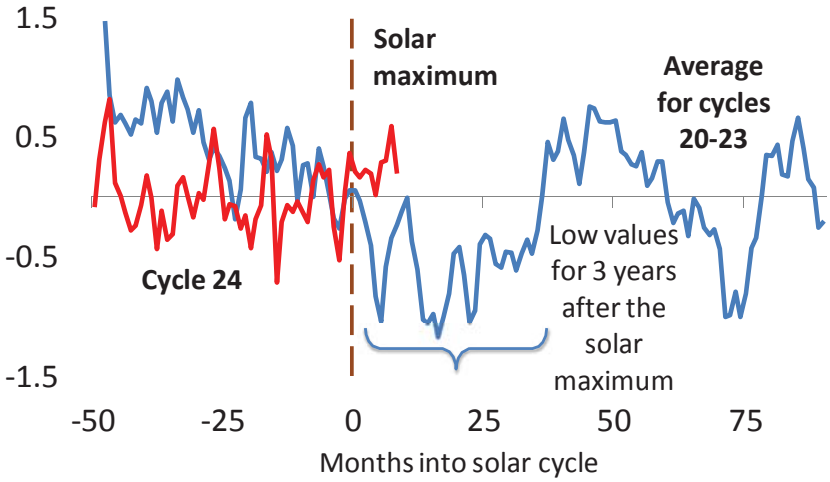
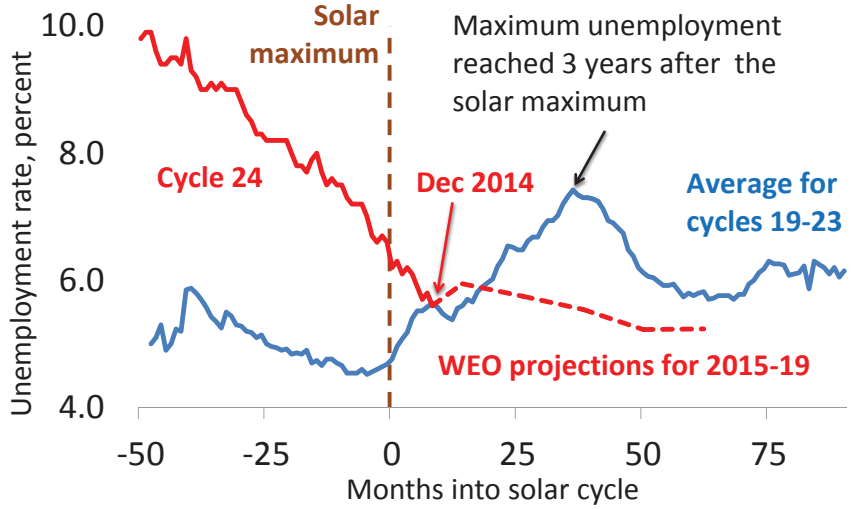


Figure 10. Solar cycle and OECD CLI, 1962–2013 (smoothed with 25-month moving average).



Source: US FED ; NASA; and author's calculations.

Figure 11. U.S. ADS in 1964–2014 (solar cycles 20–24 centered along solar maximum).



Source: FRED ; NASA; IMF *WEO (October 2014)*; and author's calculations.

Figure 12. U.S. unemployment in 1954–2019 (solar cycles 19–24 centered along solar maximum).

declining trend to a rapid increase, with the unemployment rate peaking 2–3 years after the sunspot maximums (Figure 12 and Figure 13). Consequently, the unemployment rate exhibited highly significant correlation with the sunspot series with a lag of 24 months. This apparent link with the solar cycle is particularly important in view of the role given to the unemployment rate in dating U.S. business cycles.

Furthermore, the G7 unemployment rate shows the same correlation with the solar cycle. During the period 1956–2014, all 5 sunspot maximums overlapped closely with minimums in the G7 unemployment rate, followed by its increase to a peak a few years later (Figure 14 and Figure 15). Moreover, the unemployment rate for the entire group of advanced economies[2] follows the same pattern.

However, the relation between unemployment and the solar cycle was not uniform across G7 economies. On the one hand, the data for Canada exhibit very much the same correlation as in the U.S. On the other hand, the long-term monthly data for Japan did not confirm the existence of such a strong link. In fact, volatility of unemployment in Japan was remarkably low for many years, and this began to change only in the last 30 years or so. And the available data for the European G7 economies indicate a relatively weak link between sunspots and unemployment. This can be explained in part by the lack of uniform unemployment data. For example, the available

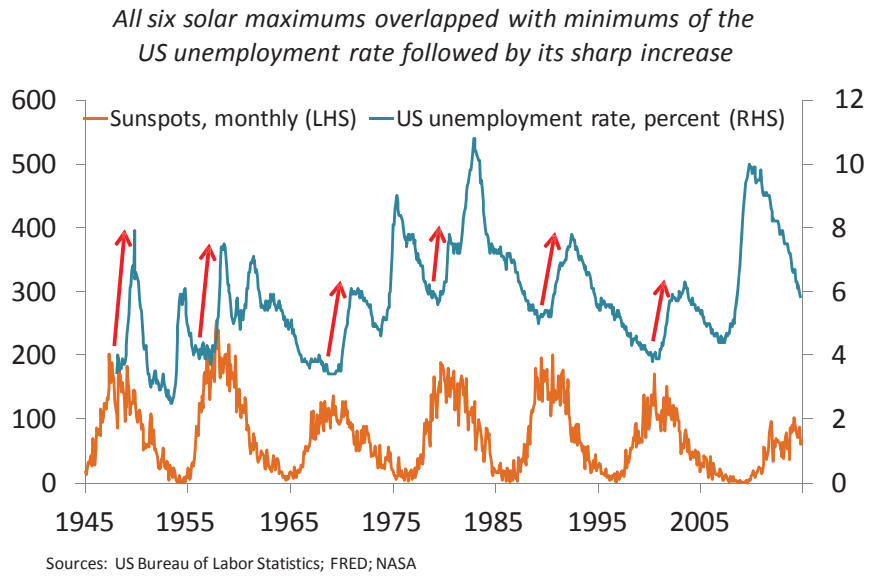
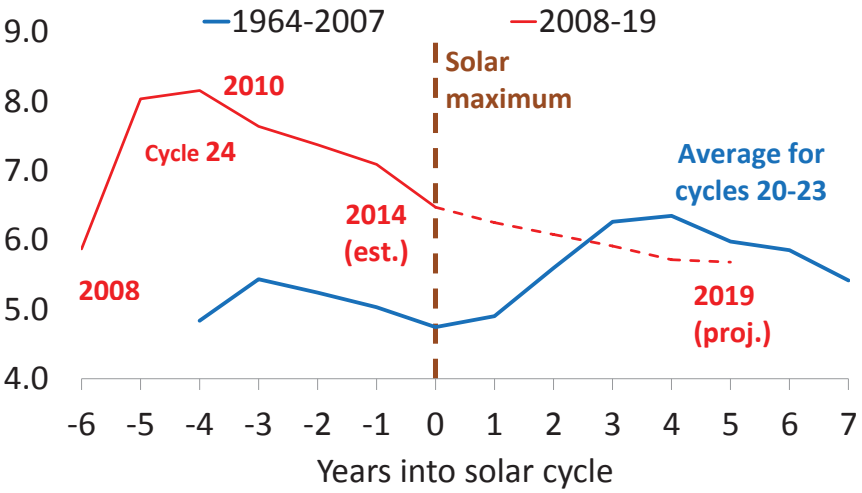


Figure 13. Solar cycle and U.S. unemployment, 1948–2014.



Source: IMF *WEO (October 2014);* OECD; NASA; and author's calculations.

Figure 14. Unemployment in G7 economies, 1964–2019 (solar cycles 20–24 centered along solar maximum).

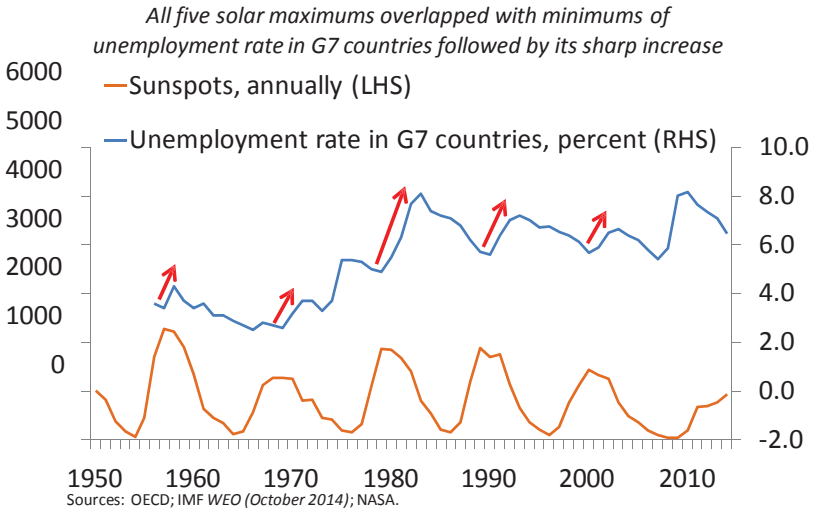


Figure 15. Solar cycle and G7 unemployment, 1964–2019.

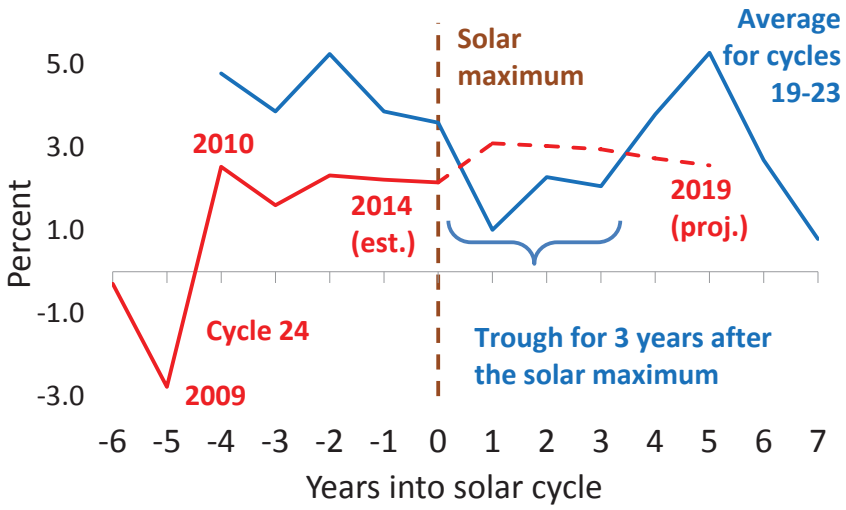
data for the UK suggested that the unemployment rate bottomed out before the sunspot maximum and increased sharply after it, but the relatively short data span covering only 2 solar cycles did not allow the claim that this was a reliable pattern.

Not surprisingly, the U.S. GDP also takes a dip in the years after solar maximums. During 1954–2008 (corresponding to solar cycles 19–23), on average low GDP growth rates were observed for 3 years after the solar maximum (Figure 16). The same pattern is observed for the aggregate growth rate of all G7 countries, of all advanced economies, and in the GDP growth series for the entire world (Figure 17).

What Can We Project for the Next Solar Maximum?

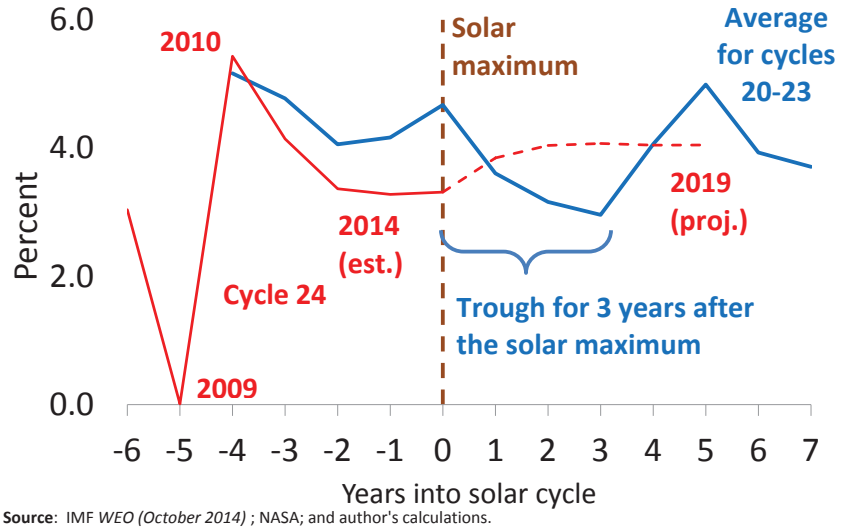
Our study documented that the cyclical maximums of solar activity have been associated with recessions in the U.S. and other G7 economies. For the past 100 years, each solar maximum overlapped closely with a U.S. recession. And from 1965 on, for which time consistent recession dating is available for all G7 countries, about 3/5 of recessions started in the 3 years around and after sunspot maximums. In view of this fascinating coincidence, can we expect that the next solar activity maximum will be followed closely by another U.S. recession and recessions in other G7 countries?

According to NASA estimates, the 24th solar cycle reached its



Source: Bureau of Economic Analysis; IMF *WEO (October 2014)* ; NASA; and author's calculations.

Figure 16. US GDP growth in 1954–2014 (solar cycles 19–24 centered along solar maximum).



Source: IMF *WEO (October 2014)* ; NASA; and author's calculations.

Figure 17. World GDP growth in 1964–2014 (solar cycles 20–24 centered along solar maximum).

maximum in April 2014. This estimate can be updated and subject to further developments (for example, in the summer of 2012 NASA projected the solar maximum to be in early to middle 2013). It appears that 2014 will see the highest number of sunspots unless something unexpected happens with solar activity and it increases further in 2015 instead of the currently expected slowdown. Using these current NASA projections, we can compare the actual and projected dynamics of economic indicators for the 24th solar cycle with the averages of the previous cycles.

For the U.S. economy, our analysis points to elevated risk of a recession starting in early 2014 to late 2015 (Figure 1). At the time this article was drafted, we know that there was no recession up until the end of 2014, and the consensus forecast for 2015 does not point to high recession risks. However, the U.S. economy contracted quite unexpectedly by 2.1% in annualized terms in the first quarter of 2014. Many professional forecasters downplayed this episode as a one-off glitch caused by extreme weather conditions in the winter of 2013/2014. However, let us note that this was one of the deepest single-quarter GDP declines outside of recession in the entire span of quarterly GDP statistics from 1947 on. Moreover, it was only the third such single-quarter contraction that did not trigger full-scale recession in the last 30 years. This suggests that the same forces that triggered U.S. recessions after previous solar maximums might have been at work in the first quarter of 2014. However, the U.S. Federal Reserve System's highly accommodative monetary policy (including its "quantitative easing" operations) and other stimulus measures deployed by the U.S. government in 2014 could have prevented these forces from triggering a full-scale recession.

G7 countries as a group entered the 3-year period of elevated recession risk at the end of 2013 (Figure 2 and Figure 6). Averaging historical data across previous solar cycles suggests that 3 or even 4 out of 7 countries could fall into recession within 1 to 3 years after the solar maximum. In the second quarter of 2014, two of the G7 countries—Italy and Japan—were in recession. Remarkably, Japan fell into recession in 2014, quite unexpectedly for most professional forecasters. Moreover, two other G7 countries, France and Germany, appear to be at high risk of tipping into recession.

In particular, the dynamics of CLI for Germany appears to resemble most closely the average pattern of this indicator in previous solar cycles (Figure 9). This is broadly consistent with the analysis of the World Economic Outlook (WEO 2014) published by the IMF in October 2014, which estimated the chances of the entire Euro area falling into recession in 2015 at close to 40%.

Even as our analysis points to elevated risks of recession ahead, the

available medium-term economic forecasts for 2015 and subsequent years do not seem to factor in such risks. For example, we can see that the IMF WEO published in October 2014 projected further reduction in world unemployment during 2015–2019, while the average of historical observation across previous solar cycles suggests that it can increase after the solar maximum (Figure 14). In the same vein, the IMF WEO projects increasing world economic growth in 2015–2019, while the experience of the previous solar cycles points to elevated risks of slowdown (Figure 17). As the actual developments unfold, it will be interesting to see if the calculations based on the solar cycle pattern could be helpful in forecasting the economic trends.

In any case, the U.S. experience suggests that concerted policy actions could shape the dynamics of economic variables against the unfavorable odds driven by the solar cycle. In particular, the "quantitative easing" monetary policy was very effective in engineering the persistent decline in unemployment rate from late 2009 to the end of 2014. Unemployment kept declining in 2014 even as our previous analysis based on the solar cycle pattern pointed to risks of its rising in this period (Gorbanev 2012). Moreover, the accommodative policy could have averted a new recession in early 2014, by limiting GDP contraction to the first quarter of the year. However, by the end of 2014 U.S. authorities wound down the "quantitative easing" operations, consistent with the brisk economic expansion in 2014 and generally upbeat forecasts for 2015. Over the next year we will see whether the downside risks associated with elevated solar activity are relevant for the U.S. economy in the absence of powerful stimulus measures.

Conclusions and Prospects for Further Research

Our results imply that we can project recessions, at least some of them. The solar cycles follow a more or less regular 11-year pattern. Solar cycle projections—including projections for the solar maximums—are available from several reliable sources. The results reported above indicate that we can use these projections to forecast periods of elevated recession risks in the U.S. and other economies.

Because of space and time constraints, in this paper we focused on solar cycle links with only a few selected economic time series. Beyond them, there are other series for the U.S. and other countries that seem to follow the patterns of solar cycles. The research scope could be widened to cover consumer confidence, labor productivity, capacity utilization, purchasing manager's indices (PMI), and other indicators that broadly follow the business cycle pattern.

Another implication of this research is the possibility of classifying

recessions as those which overlap with solar maximums and those falling between them. Are there fundamental differences between these two groups of recessions? Can we say that the recessions closely following solar maximums are triggered by factors related to solar activity, while those occurring during other phases of the solar cycle are caused by shocks of earthly nature such as banking and financial panics? What are the properties of recessions that overlap with solar *minimums*, including the Great Recession of 2007–2009? Does it imply that the counter-cyclical economic policies should be designed eyeing the solar cycle phase?

In addition to sunspot numbers, it would be interesting to study correlations with economic data for other series related to solar activity for which long-term data is available. One such series is the 10.7-cm radio emission flux denoted as F10 and recorded since 1947. Another series is the disturbance in geomagnetic field measured by Aa, Ap, and Kp indices, with data available from the 1890s and even earlier.

Above all, a closer look at a broader range of indicators of solar activity could help identify the exact channels of its influence on the economy and society. Correlation of certain economic time series with the solar cycle documented in this paper and other studies tells us little about the nature of the relation between them, leaving it open to criticism that the link is purely coincidental. But what if a strong correlation with the sunspot number series could be confirmed by an even stronger correlation with another indicator of the solar activity directly affecting Earth, such as the intensity of solar flares or CMEs? This would point to the possible channel of solar impact propagation and pave the way for further research on verifying and documenting the exact nature of the impact.

Research in the nexus of solar activity, recessions, and revolutions looks particularly promising. Even as it might be difficult to believe that solar maximums increase the risk of economic recessions, what about Chizhevsky's claim that solar maximums increase the chances of *revolutions*? Can we prove that major revolutions overlapping with peaks of solar activity—such as the revolution of 1917 in Russia that brought communists to power and a chain of revolutions in 1989–1991 that led to the collapse of the USSR and the Soviet Bloc—was not a coincidence? As with recessions, we have obtained results confirming that revolutions do occur more frequently in the years around and after solar maximums. Further research in this area can lead to remarkable discoveries about solar activity influence on human life and behavior.

Notes

[1] To compare data series across solar cycles, we "stack" the data corresponding to particular cycles by aligning the years (or months) of the solar maximum and then calculate averages (or sums) of the observed variable for particular years (or months) of the solar cycle. For the annual data, we define the year of solar maximum simply as the year with the maximum sunspot number across the cycle. For the monthly data, due to the high volatility of observations, we follow the NOAA definition that relies on a moving average (http://www.ngdc.noaa.gov/stp/space-weather/solar-data/solar-indices/sunspot-numbers/miscellaneous_in-process/docs/maxmin.new). In the resulting charts, the years of solar maximums are denoted as 0 on the horizontal axis, the years immediately preceding the maximum are denoted as −1, and so on. And on the vertical axis, we show observations for particular years of the solar cycle (or their averages or sums) across all cycles in the selected time interval under consideration.

[2] Countries classified as "advanced economies" in the IMF World Economic Outlook (WEO).

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RESEARCH ARTICLE

A Correlation Study between Human Intention and the Output of a Binary Random Event Generator

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Abstract—This paper reports on a correlation study between human intention and the output of a binary random number generator. The study comprises a total of 288 million bits from 40 equal sessions, each on a different human participant. Each participant spent 2 hours of time attempting to "influence" the outcome of the random number generator according to a pre-selected intention. During this time the participant was provided feedback on his/her performance by an analog mechanical display, with the needle of a galvanometric instrument moving to the left- or right-hand side of its current position, according to the instantaneous output of the random number generator. The data analysis procedure was defined before looking at the data. Out of four pre-defined analyses, one was found to be significant with a probability $p = 0.0366$ that this result occurred by chance under a null hypothesis. The combined analysis of the four individual analyses is found to be not significant, with $p = 0.2655$ to have occurred by chance under a null hypothesis.

Introduction

The debate on the existence or non-existence of mind–matter interaction (MMI) is a topic at the fringes of mainstream science, with sometimes strong opinions held by individual researchers defending either view. While for some researchers in the field of anomalous psychology the existence of mind–matter interaction seems not to be in doubt—see for example Radin and Nelson (1989, 2003) and Jahn and Dunne (1986)—this is not the case at all for the majority of the scientific audience (Odling-Smee 2007, Bösch, Steinkamp, & Boller 2006). Experimental evidence is often a matter of interpretation of the research results, which makes it difficult for new researchers to form an opinion on the research performed to date, as visibly exemplified in the dispute on the interpretation and validity of meta-analysis of existing mind–matter experiments (Bösch, Steinkamp, & Boller 2006,

Radin, Nelson, Dobyns, & Houtkooper 2006). See also the references in Bösch, Steinkamp, and Boller (2006) for an overview of existing research.

Also, the more cautious label of mind–matter correlation (that is correlation between human intention and the output of a physical system), which may not postulate direct causality, seems largely neglected by most scientists, even though attempts at explanation of a putative correlation effect, such as the interpretation as entanglement correlations in a Generalized Quantum Theory (Atmanspacher, Roemer, & Walach 2002, Filk & Römer 2011), do exist (von Lucadou, Römer, & Walach 2007, Walach, von Lucadou, & Römer 2014).

For these reasons, it seems of value to the field if new mind–matter experiments are performed from time to time, in particular if new researchers conduct such experiments and possibly introduce new aspects to the experimental approach. They should also serve to avoid strict replications of earlier MMI-like experiments, which may suffer from a possible decline of a putative effect, found by a number of replication studies in this field, and discussed in Kennedy (2003), von Lucadou, Römer, and Walach (2007), and Walach, von Lucadou, and Römer (2014) and references therein.

The primary intent of the study described in this paper was not that of investigating a specific aspect of putative mind–matter correlation, but rather to contribute with an original new experiment to this field of research. However, beyond the standard analysis of looking for correlations between the output of the binary random number generator in the direction of the participants' intention (see **Analysis 1**), more complex types of data analysis were performed in this study, which were partially inspired by the correlation matrix technique that has been used by von Lucadou and others (von Lucadou 2006, von Lucadou, Römer, & Walach 2007).

One of the basic ideas of this technique is to not make predictions about deviations of any particular statistical test of the data, but rather to look at the number of deviations of a total ensemble of statistical texts. For this purpose, a combined figure of merit of a number of statistical tests is defined, and compared with its corresponding expectation value. This is further detailed in the section **The Data Analysis Procedure**. The analysis of data was defined before any of the data was actually analyzed, and it was decided to publish the result of this study, regardless of the outcome of the analysis, in order to not contribute to publication bias.

In the section **The Experiment**, the experimental setup is described, followed by **The Data Analysis Procedure** on the predefined data analysis plan. The results of the analysis are presented in the **Results** section. Finally, the last section, **Discussion**, contains a brief discussion of the analysis and results in the context of existing research and terminology in the field.

The Experiment

The experiment described in this paper was designed and conducted by the author. Participants were 40 people (including the author) with different relationships to the author (friends, friends of friends, work colleagues, etc.) who were interested in the topic, and willing to spend two hours each in actual experimentation time. With one exception, none of the participants had ever taken part in any similar experiment of this kind. The participants' ages spanned from 15 to 73 years old, and participants included both genders.

Each participant had agreed to carry out 120 runs, with each run lasting 60 seconds. A single run would always begin with the participant selecting whether he/she would try to influence the motion of the needle of a galvanometer display to the left-hand side or to the right-hand side during that run. Then the participant would press the start button to begin the 60-s run. While the run was active, a red light was lit in the background of the display needle, to signal the participant that the run was going on.

During each 60-s–long run, random binary events would be generated at a rate of 1,000 per second. The draw of a logical 0 would result in the step of the display needle to the left-hand side of its current position, while a logical 1 would result in a step of the needle to the right-hand side of its current position. In this way, 60,000 binary random draws were accumulated during each 60-s run, resulting in a random walk of the needle.

Figure 1 shows an image of the experimental device in active display.

Figure 1. The experimental device, photographed in a state with active display, simulating a real data-taking run. The needle (upper right) has moved to the right-hand side during this run, as a result of the random walk, accumulating the binary random generator output. The alphanumeric display on the upper left side of the image shows the name of the participant (here Test), the chosen intention (here in German *Rechts* for right), the actual accumulated random generator bits as deviation from equal distribution (here 97), and the remaining seconds for the actual run (30).

The participants operated the device (almost exclusively) at their homes and at times convenient to them, according to their own choice. They were instructed to preferably be alone in the room when operating the device, and to finish the assigned 120 runs within one to two weeks, if possible.

An individual run of 60 s could not be interrupted by any means, but the participants were free to distribute the time to perform the runs at their choice of time. The participants could choose for any run between left or right intention, but had to respect the constraint that out of the 120 runs left and right intention had to be picked the same number of times, 60, respectively. For example, it would have been possible to do all 60 left-intention runs first, followed by the 60 right-intention runs, but the device would not allow for either intention to be chosen more than 60 times, to assure the balancing of intentions. Therefore, each participant conducted 60 runs with left intention and 60 runs with right intention, accumulating 2 hours of data in total. Each participant committed to collecting these 2 hours of experimental data, and each participant fulfilled this goal. The total timespan used by the participants to complete the 120 runs varied from less than 1 day to about 2 months. The experimental data-taking started in the summer of 2009 and concluded late in 2012, when the number of 40 participants had been reached. Up to 4 participants could share the device (e.g., members of a family) by freely distributing experimentation time among them. Each participant simply had to choose his/her name on the display ahead of a run, in order to allow the data to be associated with the correct participant.

The experiment data was stored in two different formats in the device, to be safe against errors in the storage. No such error occurred. Data before storage was reduced to 4,000 cumulated bits each, corresponding to 4 s of data. This reduced dataset was used for the analysis of the experiment. The data was transmitted to a personal computer after 1–4 participants had completed their runs, and the device was prepared for the next participant(s). The data transmission to the personal computer used checksums to be safe against transmission errors, and no such errors occurred. In addition to the participants' actual data, 2 different sets of control data were taken, which were not explicitly subject to any interaction with the intention of any participant:

Control set a): Whenever a participant decided to end a series of runs (but at the latest after 30 consecutive runs), the device automatically collected data from the random number generator without feedback to the display. During these times, the message ***Kontrolllauf*** (German for control run) was displayed in the alphanumeric display of the device, and no particular instruction was given to the participants during these times. This way, reference data of the same length for left and right intentions was taken, that is 1 hour of data for each participant.

Control set b): Between participants (that is when the device was in the hands of the conductor of the study for transferring data and preparing the device for new participants), a number of complete datasets for dummy participants was automatically generated. For this purpose, dummy persons with names 01 to 40 were generated by the conductor, and when the device recognized a dummy participant name it would automatically start an individual run after a random time interval of order 1 minute length. The intention for each such run was chosen randomly but satisfied the required equal total number of left and right intentions as for the real runs. This way a complete set of 40 dummy participants was created and spread throughout the years of acquisition of participants' data, which will be taken as a complete control dataset for the study.

As a particular feature of this study, the participants carried the experimental device to their homes, where they could work on the experiment at times and in environments of their choice. While this may appear as giving up control over the conductance of the experiment compared with a laboratory setting, it has the advantage that the participants might feel more at ease in environments of their choice, and thus might be more involved in their effort to influence the needle. Ultimately, even in the laboratory, the conductor of the experiment has no control over whether the participant asserts influence on the device according to the prestated intention or not. Although no fraud on the participants' side was to be expected whatsoever, principal measures to detect physical manipulation or malfunctioning of the binary random number generator were taken, as detailed below.

The author preferred to choose a real physical system (the needle of a galvanometer display) over a computer screen, which is often used in other experiments of this kind. Computer screens are so common in our modern life that a mechanical display also carries an element of being different.

The Binary Random Number Generator

The random number generator (RNG) is a hardware RNG combined with a software RNG. **Figure 2** shows a simplified schematic of the RNG components. The hardware RNG is based on the differential thermal noise of two resistors. The difference in the resistors' thermal noise voltage is amplified and fed into the input of a comparator, comparing the noise voltage to its time average. This yields a random sequence of logic high and low levels at the output of the comparator with close to equal distribution, but which is still sensitive, for example, to offset voltage drifts of the involved amplifiers, etc. The data gets better equalized in distribution by feeding it into a frequency divider, which toggles its logical output on the transitions from high to low of the comparator output. This corresponds to a frequency divi-

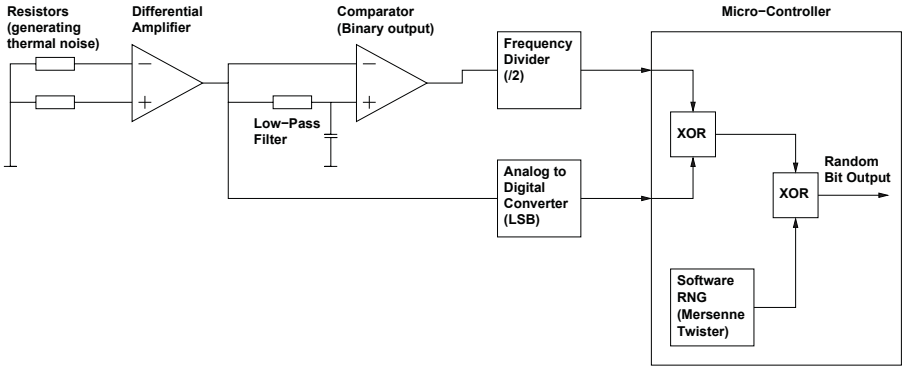

Figure 2. Schematic of the binary random number generator. See text for description.

sion by a factor of two, and is a technique to equalize in time the high-level to low-level ratio of a binary signal. One random bit is then generated by reading the logical output state of the frequency divider. On average, the divider registers 65 high-to-low transitions of the comparator per millisecond, corresponding to an average count frequency of 65 kHz. The quality of randomness of this bit is further increased by a logical exclusive–or operation with the least significant bit (LSB) of one sample of an analog-to-digital converter, which samples the actual noise voltage at the time the random bit is requested. This resulting bit is the output of the hardware random number generator.

In order to further improve the quality of randomness, and to get a very high level of security against potential (albeit unexpected) misbehavior of the RNG hardware as well as principal fraud attempts by the participants, it was planned to combine the output of the hardware RNG with the output of a software RNG. If the outputs of two randon number generators are combined, (e.g., with an exclusive–or operation in the case of single bits), the resulting output is of a higher quality of randomness, as long as the two generators are uncorrelated. There seems to be no reasonable doubt that the latter is the case when a hardware generator is combined with a software generator.

While some researchers may assume that this design (the combination of a hardware RNG with a software RNG) will make any sort of influence of the RNG harder or impossible, there may be evidence in the literature that this is not the case, and that significant results may be obtained with substantially different sources of randomness: See for example Schmidt

(1987) for a discussion on the use of different types of random number generators in MMI experiments.

For the software RNG here, an algorithm called Mersenne twister (Matsumoto & Nishimura 1998) was chosen, with a simplified implementation named TT800. This algorithm has a period length of $2^{800} - 1$ and was seeded with numbers from the hardware RNG prior to the start of a participant's contribution.

The combined random number generator used in the experiment uses the exclusive–or operation to combine the subsequent outputs of both generators: For each bit obtained from the hardware RNG, a bit from the software RNG is generated and X–or'd with the hardware RNG bit. The resulting output is used as a random bit for the main experiment (see also **Figure 2**).

As noted above, for the purpose of the main experiment, random bits are generated with a rate of 1 kHz, thus producting 1,000 bits per second. In the following, the bits 1 will be referred to as the high bits whereas the 0 bits will be referred to as the low bits.

A test run of this combined RNG comprising N = 9508571000 (9.5 billion) bits yielded n_h = 4.754282524 billion high bits, corresponding to 49.9999687% of the cases. The corresponding z value for the null hypothesis (no bias) is

$$z := \frac{n_h - E[n_h]}{\sigma[n_h]} = -0.611 \qquad (1)$$

where $E[n_h]$ and $\sigma[n_h]$ are the expected value and the standard deviation for n_h, respectively, in the absence of bias. $E[n_h] = Np_{nb}$ and $\sigma^2[n_h] = Np_{nb}(1-p_{nb})$ with $P_{nb} = 0.5$ being the hit probability of a single trial. The corresponding cumulative chance probability (similar as defined in **Equation 4**) is 47.6%.

A test run of the hardware RNG comprising N = 9508571000 (9.5 billion) bits yielded high bits in 49.99866% of the trials. The corresponding z value for the null hypothesis (no bias) is $z = -2.6$. This excess of low bits has a chance probability to occur in a realization of this same experiment that is less than 5 per thousand (cumulative chance probability of 0.45%), which indicates that the hardware RNG is not free from bias. This small bias is not relevant (but reported for completeness), since it is subsequently removed by the combination with the software RNG.[1]

A test run with 9,508,571,000 (9.5 billion) bits of this software RNG yielded high bits for 50.00000371% of all bits generated, corresponding to a z value of $z = 0.072$ and a cumulative chance probability of 50.2%.

While the combination of the hardware RNG with a software RNG

already serves as a safeguard against a possible (in principle) malfunctioning of the hardware RNG or fraud attempt, the functioning of the hardware RNG was also automatically monitored throughout the experiment. This monitoring was done by counting the number of high to low transitions of the random noise generator for each second and requiring that a threshold number of transitions be passed. No error on the hardware RNG occurred during regular experimental times of the participants.[2]

Personal Statements of Participants

To illustrate involvement and subjective experience, three of the participants have been asked to describe their perception of participating in the experiment. Here are their statements (translated from German to English by the author).

Participant S.R.: My approach to the experiment felt ambiguous. On the one hand, "This is not possible. This cannot work," which probably resembles the mainstream view around me. Also it is somewhat important to me to cling to (putative) logical reasoning, after all I am also culturally imprinted by my scientific study (of medicine), etc. On the other hand, there is the fun of resistance against all this, against this kind of all-too-fixed worldview. On performing the experiment, this kind of resistance attitude kept me going. Secondly, I was in a kind of aroused state, to enforce my will against this stupid machine. Such boosts of motivation were interrupted by phases of frustration and feelings of uselessness, in particular when I had the impression to have had a lot of failures. Altogether though—and against my expectation—I felt relatively motivated during this long experimental time.

Participant D.U.: I approached the experiment in a kind of unbiased, playful way. And like every good player, I want not only fun, I also want to score! Anyway, the experiment developed a certain dynamic: I tried different techniques, for example extremely relaxed, almost indifferent, leaving the needle almost without my intention. Then at other times I imposed pressure, or tried to make the "way back" for the needle harder if it was moving in the right direction. Of course there were also phases of resignation, but altogether I can say that I took up the fight.

Participant A.B.: The execution of the experiment was interesting. In principle I had expected it would be boring to concentrate on this little metal needle. However, after a short time I realized that I reacted strongly to the action of the needle. If it performed according to my wish, I not only had fun but also perceived it as my accomplishment, and this even against my rational conviction. This feeling of accomplishment got stronger and had quite an impression on me. Conversely, if I was not successful, I did not interpret failure to move the needle in the intended direction as my personal

fault, but rather I perceived the machine as a stubborn opponent. However, I felt motivated then to work for new success with more effort.

The Data Analysis Procedure

In order to avoid any bias, the data analysis procedure was defined before any of the data was actually looked at. Four different investigations (Analyses 1–4) were carried out, as described in the following subsections. The principal outcome of each of the four analyses is a number describing the probability that the obtained result would have occurred by chance under the null hypothesis, that is assuming no correlation between the data and the experimenters' intention.[3] The chance probability for the combined results of the four investigations is also given, taking into account possible overlaps in the four individual analyses.

Besides the comparatively simple and fully analytical **Analysis 1** (as defined below), there are two principles to be used for Analyses 2, 3, and 4.

The first principle is to estimate likelihoods of statistical test results by comparison with a large number of simulated data. This is, in essence, a Monte Carlo procedure used to estimate a background stochastic process. It is a standard technique when the background cannot be easily modeled analytically and in low signal-to-noise experiments. The null hypothesis distributions against which the measured scores are evaluated are generated using software random number generators, simulating trials like the ones that the participants in the experiment undertake. However, there is actually no participant providing an intention and so we take the results from these fake trials as realizations of the statistical scores under the null hypothesis.[4] The simulated (Monte Carlo) data consists of 10,000 complete sets of data, each resembling data of a full study comprising 40 "participants."

The second principle of the data analysis procedure is to not make predictions about the outcome of individual statistical tests, but to combine the results of a number of tests in one figure of merit (FOM). This FOM can, for example, be the product of the estimated likelihoods of the applied statistical test results. The second principle was inspired by the correlation matrix technique used by von Lucadou and others, as mentioned in the **Introduction**. In the form used here, it mainly consists in a method to perform multiple analyses, as will be discussed in the subsection ***The Choice of Data Analysis***.

As detailed in the sections below, both principles are combined in the defined data analysis. The data are either combined over all participants, or separately analyzed for each participant. The statistical tests on the data will either be a single test (the integrated binomial distribution with respect to

TABLE 1
A Simple Overview of the Four Types of Analysis

	All Data Combined	Data Split by Participants
Single Binomial Distribution Test	Analysis 1	Analysis 2
Multiple Statistical Tests	Analysis 3	Analysis 4

participant intention), or multiple statistical tests of different kinds. **Table 1** gives an overview of the four types of analysis as defined in the subsections below. The control data set b), as defined in the section The Experiment will be subject to the same Analyses (1–4) as the main dataset. The analysis of control dataset b) is expected to show a high (that is nonsignificant) probability to have occurred by chance when compared with the reference data. Thus it is expected to corroborate the assumption that the reference dataset is sufficiently randomly distributed, as well as the control dataset b).[5]

Finally, we point out that the description of the experiment, the definition of the preplanned data analysis, as well as the analysis code and the complete experimental data was uploaded to the website opensciencefamework https://osf.io/ prior to the actual analysis of the data. Also prior to the actual analysis, the data on said website was marked as a read-only representation of the project (it cannot be modified) and can be made accessible to interested readers upon request.

Analysis 1

We define a hit to be a high bit when the participant's intention was to move the needle to the right, and to be a low bit when the participant's intention was to move the needle to the left. Conversely, we define a miss to be a low bit when the participant's intention was to move the needle to the right, and to be a high bit when the participant's intention was to move the needle to the left. The total number of hits n_{hits} is the sum of hits scored under right intention plus the hits acquired under left intention. From **Equation 1** it is straightforward to see that the z value for n_{hits} over a total number of trials N is:

$$z = \frac{n_r - n_l}{\sqrt{N/2}} \tag{2}$$

where n_r are the high bits scored under right intention and n_l the high bits scored under left intention. These quantities will be determined by considering together the scores ($n_{r,p}$ and $n_{l,p}$) from all participants:

$$n_r = \sum_{p=1}^{40} n_{r,p} \qquad n_l = \sum_{p=1}^{40} n_{l,p} \tag{3}$$

The *z*-score is a useful quantity because it immediately provides a sense of the deviation of the results from the expectations. However, for the estimation of the actual chance probabilities associated with each result, it will be more convenient to refer back to the original binomial distributions.

The cumulative chance probability (null hypothesis) for the obtained results will be determined analytically here. The cumulative chance probability, $P_0(n_{hits})$, that is the probability of obtaining the measured number of hits, or greater, by chance is simply the integrated binomial probability:

$$P_0(n_{hits}) = \sum_{n'=n_{hits}}^{N} \left(\frac{N}{n'} \right) p_{nb}^{n'} (1 - p_{nb})^{N-n'} \tag{4}$$

Notes on Analysis 1. This is the classical way of analyzing this type of experiment. This analysis tests for a (positive) correlation between the participant's intention and the given task, that is to influence the display in the given direction and thus to increase the number of hits for each direction above chance expectation. The probability is defined as a one-sided probability. Note, however, that this analysis is still balanced between trials acquired under left intention and right intention.

Analysis 2

This analysis analyzes the data as detailed in the previous section (calculating *z*-scores for the number of obtained hits) but for each of the 40 participants separately, such that 40 *z*-scores are generated. These 40 *z*-scores are then sorted and (frequentist) *p* values are generated for the highest ranking, second-highest ranking, third-highest ranking, and so forth down to the lowest ranking, by comparison with the distribution of the same ranking values determined from a reference (null hypothesis) dataset. These *p* values are two-sided, with $p = 1$ if a data point is exactly in the middle of the distribution being compared to. The resulting 40 *p* values are combined (by summing over the inverse squares of *p* values) and the result is the FOM for this test. The chance probability for the value of this FOM is measured on the distribution for the same FOM derived from the Monte Carlo dataset. A one-sided probability will result in the FOM of the test data (or a lower one)

occurring by chance. This is the result of Analysis 2.

Notes on Analysis 2. This analysis is sensitive in particular to the distribution of results among the participants. It is also sensitive to deviations from randomness in directions opposite to a participant's intention.

Analysis 3

This analysis comprises a number of statistical tests for randomness (as listed below) of the acquired data of all participants combined. It is not predicted which of the pre-specified statistical tests would show a significant deviation from the expected distribution under a null hypothesis, but each of the test results (which are scalar numbers) is compared to the equivalent test results of a large number of reference data (again by ranking). By this comparison, a two-sided (frequentist) probability is estimated for each test, that the acquired result (or a lower/higher one) would have occurred by chance. In a second step, all of these probabilities (one for each statistical test) are multiplied to yield a single figure of merit (FOM) of the acquired data. Finally, this FOM is compared with the distribution of the same FOMs of the reference data, and a one-sided (frequentist) likelihood results, that the actual FOM (or a lower one) of the data being tested would have occurred by chance. This likelihood is the result of Analysis 3.

Notes on Analysis 3. As mentioned above, the analysis chosen here has some similarity with the correlation matrix technique as described for example in von Lucadou (2006) and von Lucadou, Römer, and Walach (2007). A correlation matrix (as used in these references) shows the number (and strength) of correlations between several physical and psychological variables of the experiment as a whole. In terms of the Analysis 3 defined here, different physical variables correspond to different statistical tests of the data. The psychological variables in the experiment under report in this paper are just the left or right intention to influence in the direction of the needle display. In this case, the corresponding correlation matrix would consist of only 2 rows (left and right intentions), and n columns, if n is the number of statistical tests applied.[6]

This matrix could be given as a table in principle, but as defined above a figure of merit will be used instead to combine the obtained probability levels of all tests numerically (second principle). In the last step, the resulting figure of merit is compared to the set of computer-generated reference data (first principle). The statistical tests to be applied to the data are the following (tests that do not include a combination of right and left intention explicitly are performed on both intentions individually, as two separate tests, as the numbers in brackets denote):

* successful runs of 60 s length (2)
* sum of bits (2)
* standard deviation (2)
* skewness (2)
* kurtosis (2)
* chi square goodness of fit to expected binomial distribution (2)
* Ansari Bradley test if variance between right and left intention differs (1)
* distribution of sign permutations in 5-Tuples of data (2)
* correlation between left and right intention data (1)
* runs test for expected number of runs with same sign (2)
* runs test for expected number of runs with same slope (2)
* Fourier transform (Welch method) of the time series (2)
* sum of absolute difference between all consecutive values (2)
* chi square test of uniformity on 2,400 stretches of 60 s length (2)
* chi square test of uniformity on 4 stretches of 10 h length (2)

If a test result is not obviously a scalar, an algorithm is to be defined to calculate a scalar out of the test result.[7]

Analysis 4

This analysis is one step more complex than Analysis 2 and Analysis 3, and is a combination of the two: The data is first split according to the 40 participants. Then a number n of statistical tests (as listed below) is applied to each participant's data. Then for each out of the n tests and for each of the participants the following is performed.

Each test result is compared to (400,000) reference datasets and the resulting (two-sided, frequentist) probability p_{ki} is calcuated, describing the probability that this result (or a lower/higher one) occurred by chance. Here $k = 1..40$ is the number of the participant and $i = 1..n$ is the number of the applied statistical test.

From these p_{ki} values, an FOM is computed for each participant by multiplying all the p_{ki} values with $i = 1..n$ of the respective participant $k = 1..40$. The resulting 40 FOMs of the participants are then sorted and compared to reference data in a way that the highest of the 40 participants' results is ranked against the highest of all the reference data results, where the highest (of the reference data results) refers to all the highest of the (40) participants' each, of the reference dataset. In the same way, all the second-highest of the (40) participants' results are ranked against the second-highest (out of 40 each) of the reference data results. And so forth for the remaining 38 results.[8]

A final FOM is then computed by combining the participants' FOMs (by summing over the inverse squares of the p values). This final FOM is

then compared to the same FOMs of the reference data and a final one-sided likelihood will result, describing the likelihood that this result (or a lower one) occurred by chance.

Notes on Analysis 4. This analysis should be particularly sensitive to variations between individual participants (with respect to the statistical test applied) which might (if existent) be averaged out in the other analysis.

The statistical tests to be applied to the data are the following (as for Analysis 3, tests that do not include a combination of right and left intention explicitly are performed on both intentions individually as two separate tests, indicated by the number in brackets):

* successful runs of 60 s length (2)
* sum of bits (2)
* sum of bits of first half of data (2)
* standard deviation (2)
* skewness (2)
* kurtosis (2)
* single largest (/smallest) value (2)
* chi square goodness of fit to expected binomial distribution (2)
* Ansari Bradley test if variance between right and left intention differs (1)
* distribution of sign permutations in 5-Tuples of data (2)
* correlation between left and right intention data (1)
* correlation between first and second half of data (2)
* runs test for expected number of runs with same sign (2)
* runs test for expected number of runs with same slope (2)
* Fourier transform (Welch method) of the time series (2)
* sum of absolute difference between all consecutive values
* chi square test of uniformity on 60 stretches of 60 s length (2)
* chi square test of uniformity on 4 stretches of 900 s length (2)

Results

Analysis 1 Results

Figure 3 shows the full dataset obtained by the 40 participants. The total number of hits (from left and right intention) is $n_{hits} = N/2 + 2{,}018$, that is an excess of 2,018 hits over the expected number of hits $N/2 = 144{,}000{,}000$. The probability for the result of the participants' data to have occurred by chance (under the null hypothesis) is $p = 0.406$, and thus not significant.

Figure 4 shows the control dataset b), which was obtained with the experimental device running unattended for 40 dummy "participants," as described in the section **The Experiment**. The probability for the result of the control dataset b) to have occurred by chance (null hypothesis) is $p = 0.599$, which is not significant.

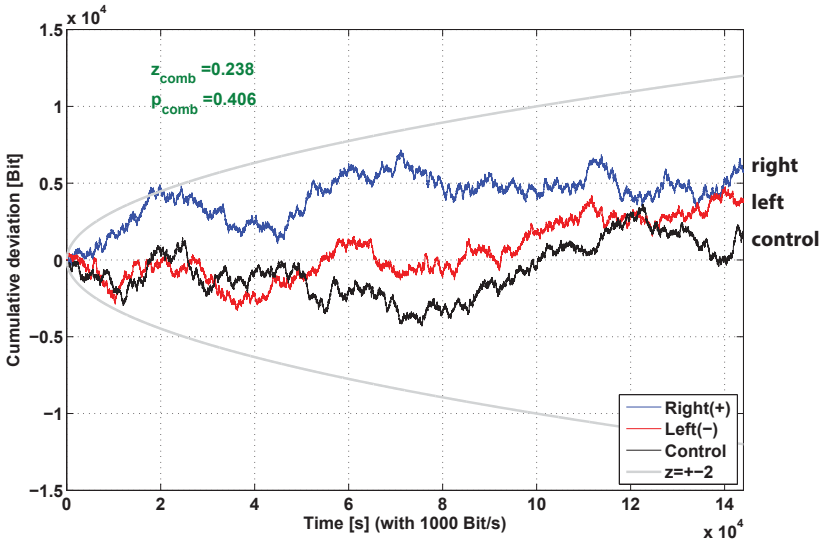

Figure 3. The full dataset obtained by the 40 participants. The horizontal axis denotes the time over which data was acquired, equivalent to the accumulated number of bits generated. The vertical axis shows the cumulated deviation from the expectation value, separated for bits obtained under right and left intention. Also shown is the control dataset a), as defined in the section **Analysis 2**, which is, however, not subject to any analysis. The grey (smooth) line denotes the level of two standard deviations. The combined probability p_{comb} of the data under right and left intention is given as defined in the section **Analysis 1**.

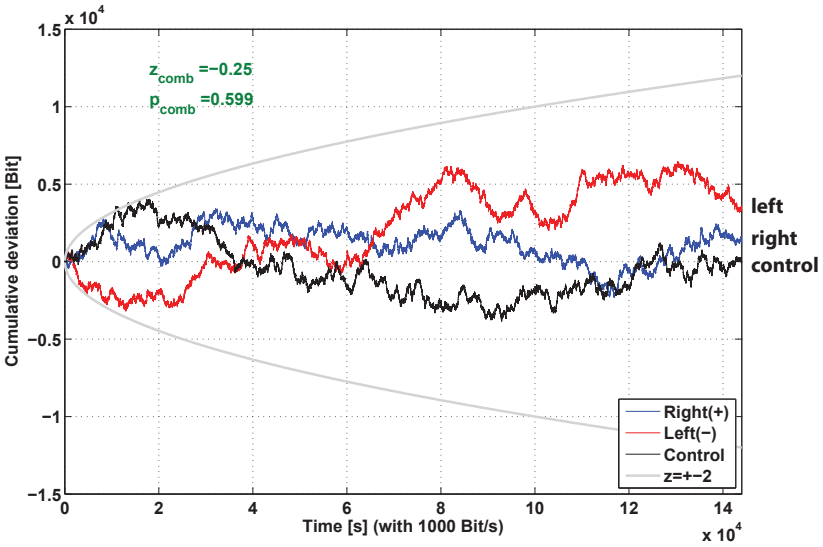

Figure 4. Control dataset b), in identical representation as the data in Figure 3.

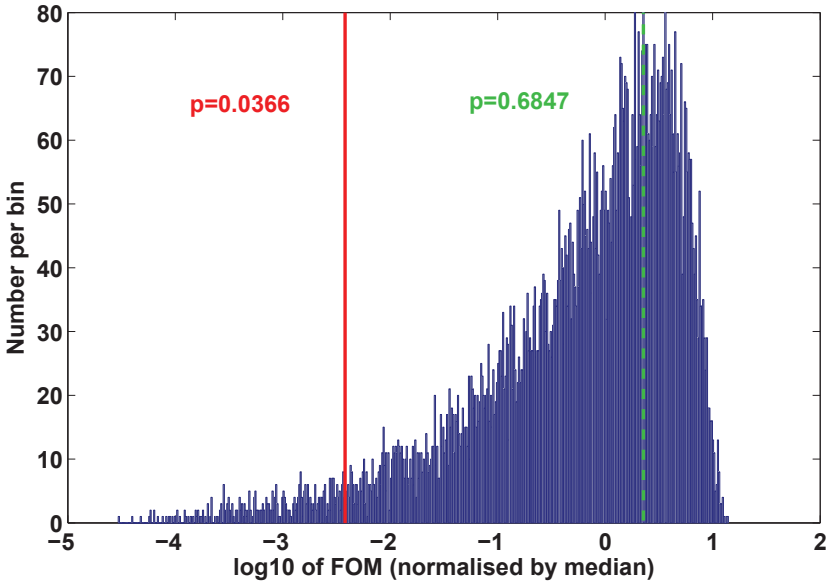

Figure 5. Result of Analysis 2 for the participants' dataset and the control dataset b) compared to Monte Carlo data. The horizontal axis denotes a logarithmic representation of the figure of merit (FOM) as described in the section **Analysis 2**. The vertical axis denotes the counts per bin of the Monte Carlo dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the reference dataset b) (green/dashed).

Analysis 2 Results

Figure 5 shows the result of Analysis 2. The probability of the participants' results to have occurred by chance (null hypothesis) is $p = 0.0366$, which is significant with respect to a 5% significance level. This probability is obtained by the fraction of more extreme results (more negative FOM) divided by the number of all results of the Monte Carlo data. As implicit in the description of this analysis in the section **Analysis 2**, this result means that the distribution of the 40 participants' results deviates significantly ($p = 0.0366$) from the expected distribution.

The probability for the result of the control dataset b) to have occurred by chance (null hypothesis) is $p = 0.6847$, and thus not significant.

For further illustration of this potentially interesting result, **Figure 6** shows the distribution of the individual participant's results, from which the FOM is calculated. The largest deviation of the participants' data from the expected distribution can be seen around the highest rank numbers (those

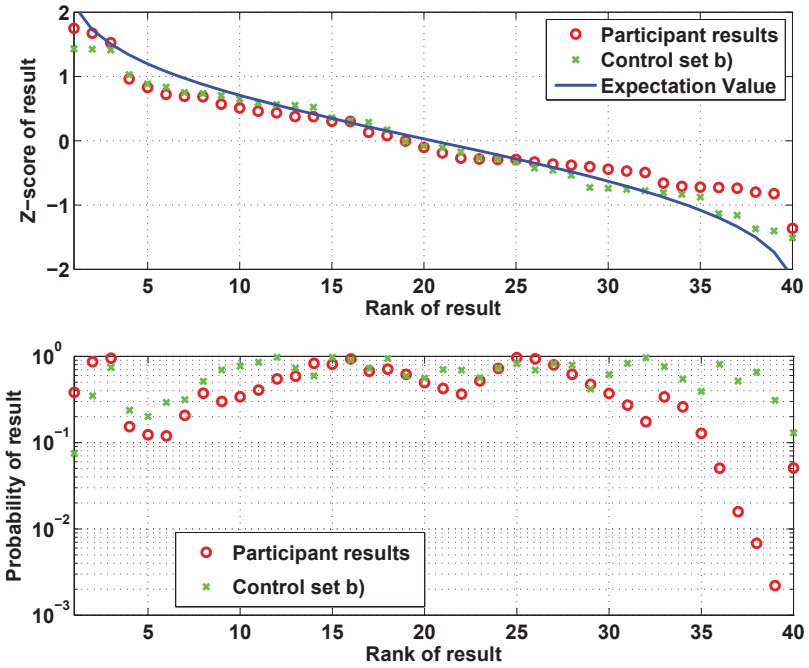

Figure 6. Individual results of Analysis 2 for the participants' dataset and the control dataset b). The horizontal axis denotes the rank (1–40) of each of 40 individual results. The vertical axis of the upper graph denotes the *z*-score of each individual result. The participants' data points are shown as (red) circles and the control data points are shown as (green) crosses. In the upper graph, the distribution of the expected *z*-scores is given as well, as the (blue) solid line, obtained from the Monte Carlo data. The lower graph shows the individual p values of the results of Analysis 2, as obtained from the Monte Carlo data. The lowest p values correspond to the largest deviations from the expected *z* values in the upper graph.

with the lowest *z*-scores in the upper graph): The ensemble of all results is slightly short of results of more negative *z*-scores. Corresponding to these deviations seen in the upper graph, the lower graph shows the *p* values of the individual results with respect to the individual expectation value of their rank.

As can been seen in the lower graph, there are 5 participants with individual results of probabilities smaller than $p \approx 0.05$. However, it would seem not quite right to isolate these individuals as extraordinary performers, since in fact no single individual in this analysis has performed significantly on his/her own (all absolute *z*-scores in the upper graph are smaller than 2). Rather it is the performance of all the participants that has to be taken into

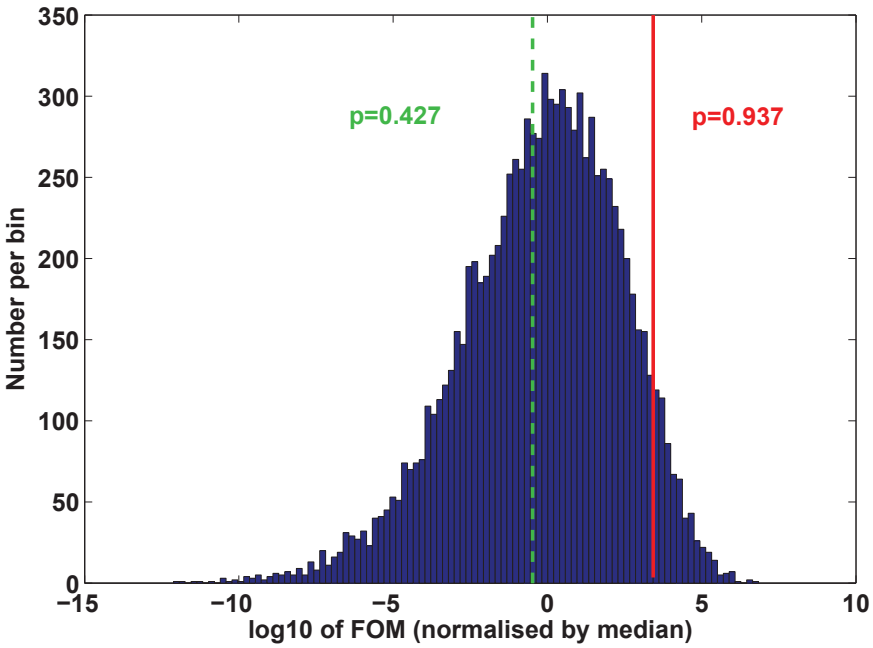

Figure 7. Result of Analysis 3 for the participants' dataset and the control dataset b) compared to Monte Carlo data. The horizontal axis denotes a normalized logarithmic representation of the figure of merit (FOM) as described in the section **Analysis 3**. The vertical axis denotes the counts per bin of the Monte Carlo dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the reference dataset b) (green/dashed).

account for the composition of this distribution. Therefore, all participants have contributed to this result.

Analysis 3 Results

Figure 7 shows the result of Analysis 3. The probability of the participants' results to have occurred by chance (null hypothesis) is $p = 0.9411$ (one-sided, as was defined for this analysis), and thus not significant. The probability of the result of the control dataset b) to have occurred by chance (null hypothesis) is $p = 0.4468$, also not significant.

Analysis 4 Results

Figure 8 shows the result of Analysis 4. The probability of the participants' result to have occurred by chance (null hypothesis) is $p = 0.517$ and thus

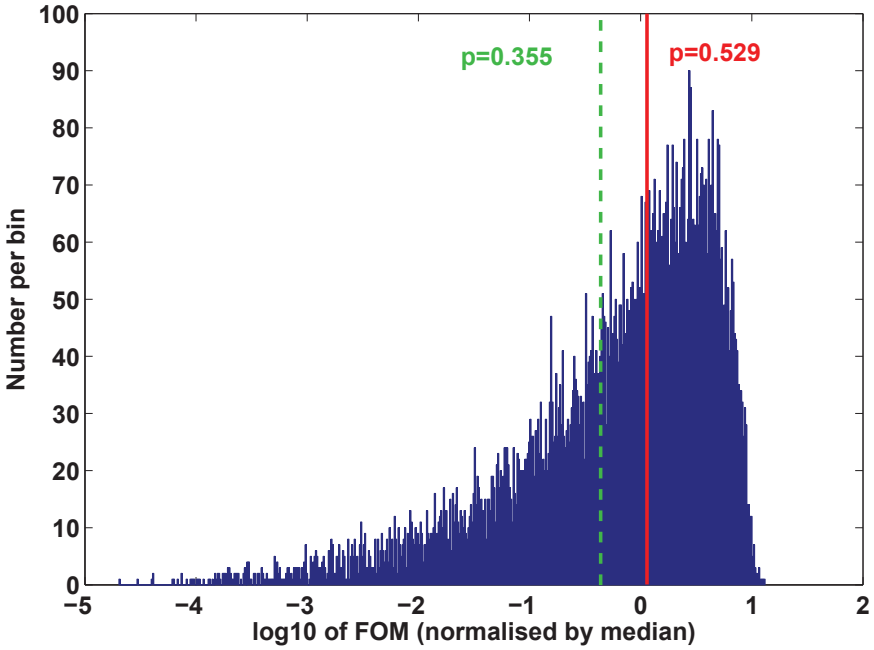

Figure 8. Result of Analysis 4 for the participants' dataset and the control dataset b) compared to Monte Carlo data. The horizontal axis denotes a normalized logarithmic representation of the figure of merit (FOM) as described in the section **Analysis 4**. The vertical axis denotes the counts per bin of the Monte Carlo dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the reference dataset b) (green/dashed).

not significant. The probability for the result of the control data to have occurred by chance (null hypothesis) is $p = 0.374$ and thus also not significant.

Combined Analysis

The combined analysis was not predefined, but it was planned to execute a significance evaluation of Analyses 1–4 combined, in case at least one of them would be significant, or at least two would be nearly significant. The most straightforward way is chosen here, which is the calculation of a figure of merit combining the 4 results from Analyses 1–4 (by calculating the product of the 4 probability results noted in the subsections above), and comparing this FOM to the same FOM from the Monte Carlo dataset. The final result is obtained as a one-sided ranking. Because this final p value is obtained from simulated data, it includes the adjustment for possible

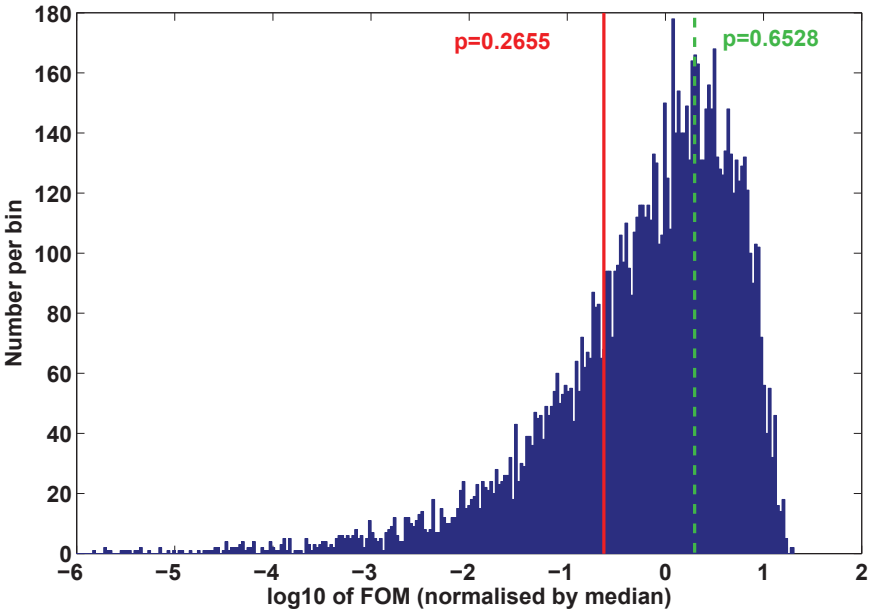

Figure 9. Result of the combined analysis for the participants' dataset and the control dataset b) compared to Monte Carlo data. The horizontal axis denotes a normalized logarithmic representation of the figure of merit (FOM). The vertical axis denotes the counts per bin of the Monte Carlo dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the reference dataset b) (green/dashed).

multiple analyses across the 4 predefined Analyses 1–4. **Figure 9** shows this FOM for the participants' data and the control dataset b), in comparison with the Monte Carlo data. The probability of the participants' data in the combined analysis to have occurred by chance (null hypothesis) is $p = 0.2655$, and thus not significant. The probability for the control data to have occurred by chance (null hypothesis) is $p = 0.6528$, also not significant.

Discussion

The reader may judge on the chosen analysis methods, study design, and results on his/her own; however, some discussion in the context of existing research concepts might be useful.

The Choice of Data Analysis

The basic structural description of the analysis has been given in the section **The Data Analysis Procedure**, but I will expand a bit on this here.

The correlation matrix technique used by von Lucadou and a few others is based on the idea that mind–matter correlations might be interpreted as entanglement correlations (von Lucadou 2006, von Lucadou, Römer, & Walach 2007, Walach, von Lucadou, & Römer 2014). Along this idea, and given the postulate that entanglement correlations cannot be used for signal transmission, one consequence is that strict replication studies tend to fail, and no prediction can be made in what part of a system a mind–matter correlation may show up. According to von Lucadou and co-workers, one should give a system many degrees of freedom to increase the likelihood of such correlations appearing more often than would be expected by chance. The correlation matrix as has been used by von Lucadou et al. consists of two main ingredients:

(1) A number of physical and psychological variables are arranged in a table or matrix form, and for each intersection between a physical and psychological variable the corresponding correlation between these 2 variables is calculated (and can be entered in the corresponding matrix position).

(2) In the final analysis, the number of significant correlations in this matrix is counted and compared with the number of correlations that would have been expected just by chance.

With ingredient (2), it is possible to estimate a likelihood that the combined result (the number of observed correlations) has occurred by chance (under a null hypothesis).

It is worth pointing out that ingredient (1) can be seen as the process of defining and using a number of tests (correlations between variables in this case), whereas ingredient (2) resembles a sort of multiple analysis: A combined statistical measure is derived from a multitude of individual tests. As stated above, no prediction is made of which of the individual tests would be significant, but the combined statistics of all tests can finally be judged.

It is mainly this ingredient (2) that formed the basis of the analysis as defined for the study in this paper, in the sense that a number of different methods/tests are used. In particular, Analysis 1 is the classical analysis looking for mean shifts in the intended direction. Analysis 2 is different from this in that it looks at the distribution of the mean shift results from the 40 participants. It is possible that Analysis 2 would be highly significant (that is the distribution would deviate from the expected distribution under a null hypothesis), while at the same time the combined mean shift of all participants (which is precisely Analysis 1) would not be significant. Note that Analysis 1 is constituted by only a single statistical test, whereas Analysis 2 consists of 40 test results. No prediction is made on how the distribution of results in Analysis 2 may deviate from expectation. It was this principle that was inspired by the matrix method of von Lucadou et al.: To make no pre-

diction of precisely where a statistical deviation would occur, but rather to leave the system many degrees of freedom for deviations to show up. In the correlation matrix method, the number of significant correlations is counted and compared to the expectation value of a control dataset. In Analysis 2, a figure of merit is defined that describes numerically the deviation of the distribution from the expected distribution. This is just a more general form of how to combine the results of a multitude of tests. To make this point clear: This is not a replication of the correlation matrix method, in particular since the matrix elements in von Lucadou et al.'s experiments resemble correlations between psychological and physical variables (ingredient (1)). However, it resembles the idea of many degrees of freedom and applies it to a different kind of analysis (ingredient (2)).

One may argue that Analysis 1 would correspond to a classical analysis where a signal may be isolated from the data, whereas Analysis 2 would be more reasonable under the assumption of entanglement correlations as a putative explanation for significant effects. In this sense one may find it confusing to mix these two kinds of analysis. However, I would note two points with respect to this: First, Analysis 1 is still balanced between left and right intention, and the sequence of those intentions has been freely chosen by the participants. Therefore, if the chosen sequence would not be known, it would be impossible to derive a signal from the data (under the alternative hypothesis that the data has been influenced). In other words: Without knowing under which intention a stretch of data was generated, the computation of the result of Analysis 1 would not be possible. Second, one may view the combination of different types of analysis (like Analysis 1 and Analysis 2 here) just as an application of ingredient (2) of the matrix method as explained above: a case where multiple tests are done without predicting which one would be significant. It is also along this line that even more tests have been added, as for the cases of Analysis 3 and Analysis 4.

For Analysis 3 and Analysis 4, the number of stratistical tests (viewed as physical variables) was expanded from one to many. However, other than in the correlation matrix technique used by von Lucadou et al., the only correlation of the physical variables pertain to the left or right intention. This is a very reduced form of correlation and has only a loose connection to ingredient (1) of the correlation matrix method. However, the idea behind this was just to see if something surprising might happen, in that more correlations (or significant results) than expected might show up.

Pilot Study

No dedicated pilot study was conducted for this experiment, basically because the hardware and procedural design of this study seemed sufficiently

straightforward to make problems seem unlikely to occur throughout the data collection period. For the data analysis, the choice was to fully specify the analysis (as described in the sections above), and to put forward different kinds of analysis, though in a statistically sound way. Since one guiding idea was to make no prediction on which individual test or analysis would be significant, it was also not deemed necessary to conduct a pilot study with respect to the data analysis. Naturally, the study described in this paper might be regarded as a pilot study with respect to the design of new experiments but clearly not in the sense that in a typical pilot study the analysis might not be prespecified and subject to adjustment after the data had been looked at.

Exploratory vs. Confirmatory Analysis

Similar to the case for a pilot study, one may want to categorize the experiment at hand in terms of exploratory vs. confirmatory analysis. The label *exploratory analysis* seems often used to describe a process where a number of statistical tests is used on existing data, to find out which type of analysis might yield an interesting or unexpected result. A finding of interest may then be used as a hypothesis to test on new data, a process that then may be described as confirmatory analysis. Obviously it would be improper to report significant results of an exploratory analysis of this kind, without setting this into the context of all types of analysis that have been tried on the given database. Rightly so, this kind of practice might be the one most criticized.

The analysis done in this work is not exploratory in this sense, since the analysis has been prespecified before the data was looked at. It may be called exploratory only in the sense that a number of different analyses have been conducted, without predicting which one would yield a significant result. In this sense, the work is exploratory if the results would be used to generate new hypotheses to investigate in further studies. However, it should be pointed out that thinking along this line would imply that one has in mind to isolate one type of analysis, which then may show significant results on all future experiments of of this kind. According to von Lucadou, Römer, and Walach (2007) and Walach, von Lucadou, and Römer (2014), a confirmatory study that uses a single analysis that has been put forward from a former exploratory study may well fail. This is (according to those authors) due to the signal non-transmission theorem, and the decline effect that may be derived from it. It is in this sense that the study at hand has been designed to have many degrees of freedom in which correlations may show up.

Watt and Kennedy (2015) give a nice overview of exploratory vs. con-

firmatory analysis, and add the term *prespecified exploratory analysis*. Perhaps this might be an acceptable label for the study presented here.

Results Summary

To repeat the main results: Out of 4 predefined analyses, one was found to be significant with a probability $p = 0.0366$ that this result occurred by chance under a null hypothesis. The combined analysis of the 4 individual ones is found to be not significant, with $p = 0.2608$ to have occurred by chance under a null hypothesis.

A skeptical observer may say that the fact that the combined analysis is not significant means that no further discussion is necessary. If one's prior inclination is more to the end that psi may exist and may show up in experiments like this, then one may find the result of Analysis 2 at least interesting. The significant result for the distribution of the participants' individual results in Analysis 2 may yield a hypothesis for further study. To the knowledge of the author, no such investigation has been performed by other investigators. As stated above: Looking for the distribution of individual results rather than the significance of a combined result of many individuals has some similarity with the correlation studies where a statistical analysis is performed on the total number of significant correlations, without predicting which individual one would be significant. This direction of research may be supported by entanglement correlations in a generalized quantum theory (von Lucadou, Römer, & Walach 2007, Walach, von Lucadou, & Römer 2014, Atmanspacher, Roemer, & Walach 2002, and Filk & Römer 2011).

However, a followup experiment that would use only Analysis 2 might open up discussion about whether to view such an experiment as too strict a replication such that it might fail, or whether it may have sufficient internal degrees of freedom to allow for further significant results.

Acknowledgments

The author is grateful to M.A.P. for thoroughly reading the manuscript and for useful discussion. The author further thanks Walter von Lucadou and Eberhard Bauer for fruitful discussion and comments. Finally, I also thank the *Journal* reviewer for constructive criticism. This study was performed as private research by the author.

Notes

[1] Even if bias were not removed by the software RNG, this is a level of bias that would not be significant in the main experiment, because the main

experiment comprises 33 times fewer trials than this test run (288 million of the main experiment versus ~9,509 million of the test run). The bias detected with 9,509 million trials, in an experiment with 288 million trials would result in a z value of –0.46 and an insignificant cumulative chance probability of 32.5%.

[2] In three cases an error occurred on the hardware number generator during generation of control data of set a). This was caused by a minor bug in the program, which led to a low battery state and thus a low count rate on the number of zero crossings of the voltage comparator. The control data of the participants where this occurred was regenerated. It should be noted as well, however, that control data of set a) is not foreseen for analysis anyway.

[3] The author is aware of possible criticism of p values for some domains of research and hypothesis testing. However, p values as used in classical (frequentist) statistical analysis still have their merits and reasonable domains of applications, as pointed out in a recent overview article on Bayesian and classical hypothesis testing (Kennedy 2014).

[4] Of course, in principle it would be possible to calculate the likelihood of the employed statistical tests analytically; however, a Monte Carlo approach was chosen here for simplicity and for better transparency of the data analysis. Furthermore, the Monte Carlo method makes it straightforward to combine different statistical tests and analyses that may be overlapping. The analytic approach would be exceedingly complex in this case. However, care has to be taken to assure that the random number generator used for the background distribution suffices for the intended usage. For the case here, different algorithms have been compared with no significant differences found in the resulting distributions relevant for this analysis. A better approach in principle can be to use the existing dataset with random incursion points to generate the background distribution. However, in this case a problem might be the limited amount of available data.

[5] If the control set b) shows a significant deviation from randomness, it would be possible to subsequently generate more control datasets of type b) and/or more reference data to test whether the deviation would be systematic, or was a deviation by chance. If the deviation were systematic, the whole study would face an unforeseen problem, and probably no conclusions on the main experimental data could be drawn in this case.

[6] As can be seen from the statistical tests defined, some of the tests are performed on a combination of data under left and right intention, such that there would only be a single field for this column in a corresponding correlation matrix. However, this does not matter for the purpose here,

where all test data are combined by their individual probability rankings.

[7] Normally this would be a number describing the deviation of the test result from an assumed reference distribution. Since a ranking is applied subsequently, the reference distribution does not necessarily have to describe the exact expectation distribution of the test.

[8] The resulting 40 participant probabilities that each result occurred by chance in their corresponding class is an intermediary result here, which can be used to identify individual participants as deviating from the expectation value.

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COMMENTARY

Commentary on "Does a Cosmic Ether Exist? Evidence from Dayton Miller and Others"

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As a former researcher in relativistic rotation (see, for examples, Klauber 2002 and Klauber 2007), I was greatly intrigued by James DeMeo's (2014) well-written and highly informative *JSE* Historical Perspective article on the Miller and other experiments that effectively repeated, and refined, the Michelson–Morley experiment. I had been aware of Miller's controversial findings, as well as those of Múnera, Deckers, Arenas, and Alfonso (2006), which seemed to show non-isotropic light speed, but I had not studied them extensively and had essentially been seduced by the arguments of Shankland and others. I was not aware of the other similar research mentioned in the article, nor of the interactions between Miller, Einstein, Shankland, and others on the subject, and I thank Dr. DeMeo for recounting them.

Having read a large number of articles by those purporting to have found holes in Einstein's relativity theory, I, frankly, have found the vast majority to be cases of simple misunderstanding of fundamental concepts within that theory. My own position has long been that relativity theory is correct, but that it has been misinterpreted and misapplied for the particular case of relativistic rotation (see my above-cited articles for details).

But this case is decidedly different. Here, we are not talking about interpretations of theory, but about experiments carried out by highly competent, well-credentialed, meticulous researchers. As Einstein himself said, ". . . a single experiment can prove me wrong . . . " Well, here we have several such experiments.

As much as I appreciated DeMeo's article, I was, however, disappointed that it did not mention any of a substantial number (approaching twenty) of other experiments, performed from the early 20th century up to the past decade, which effectively repeated the Michelson–Morley experiment to far greater accuracy and found no cosmic light speed anisotropy. Up to 2004, these are listed in Klauber (2004); from then through 2007, in Klauber (2007). I mention a select few below.

Brillet and Hall (1979) (the latter a Nobel laureate), found no cosmic

anisotropy up to the order of 3×10^{-8} of the speed of light, i.e. about .01 km/sec, a far cry from the 20 km/sec Miller and others found. Granted, the Brillet and Hall experiment was enclosed in materials and a building, plus was near ground level, which via Miller's reasoning would reduce the signal significantly. However, Miller's results suggested a reduction from such causes to yield an anisotropy on the order of 1 km/sec, not one hundredth of that.

In this regard, the Wolf and Petit (1997) results are particularly noteworthy as the light signals tested in their experiment traveled from the global positioning system satellites and so passed primarily through empty space, with virtually no possible "ether drag." Such a signal, under the ether hypothesis, would be on the order of at least 200 km/sec, but they found no anisotropy to an accuracy of .002 km/sec.

More recent ground-based tests with accuracy comparable to, or greater than, that of Brillet and Hall include Braxmaier, Müller, Pradl, Mlynek, and Peters (2002), Antonini, Okhapkin, Göklü, and Schiller (2005), Herrmann, Senger, Kovalchuk, Müller, and Peters (2005), and Stanwix, Tobar, Wolf, Susli, Locke, Ivanov, Winterflood, and van Kann (2005). No anisotropy found there either.

So what are we to believe? I must admit to being as perplexed as anyone. I have no answer.

On one hand, we have top researchers, carrying out experiments diligently and carefully, who find results that conflict with extant theory, and for which, try as some might, no reasonable, non-paradigm–rupturing explanation seems sufficient. On the other hand, we have other top researchers, no less diligent and careful, with quite opposite, theory-consonant, results.

One hint Miller's experiment may give us is the very close alignment of the anisotropy he found with the perpendicular direction to the plane of the ecliptic of Earth's orbit (see figure 13 in DeMeo 2014). Could there be some new physics hidden somewhere therein that could alter the detected signal in some types of experiments, but not that in other types? For example, as one "shot in the dark" (pun intended), could dark matter orbiting the sun near the Earth somehow subtly affect certain measurements, but not others? If so, one might not be too surprised by an anisotropy alignment like that Miller found, i.e. either roughly perpendicular to, or roughly parallel with, the plane of the orbits of the planets.

This is an anomaly begging for exploration.

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COMMENTARY

The Ether and Psychic Phenomena: Some Old Speculations

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DeMeo's (2014) article about the idea of the ether in the *JSE* brings to mind the historical relationship of this concept to psychic phenomena.

Much more than a simple physical theory of force, the ether was one of those powerful and overreaching concepts that captured the imagination of both scientists and the general public during parts of the Nineteenth and the Twentieth Centuries (on the concept see Cantor & Hodge 1981). As argued by Asprem (2014:222):

> Ether metaphysics provided a worldview that emphasized the immanence of the divine, through the all-encompassing, interprenetating, but invisible ether. This medium functioned as a kind of "world-soul"; it was the seat of animation in general, the source of life, and also the plane on which much mental functioning was thought to take place.

The metaphysical dimensions of the ether connected the topic to philosophical and religious views, as well as to ideas of the unity of nature, the transcendence of the spirit, and of human faculties. Such issues were discussed by Balfour Stewart and Peter G. Tait (1875) and Oliver Lodge (1925) (on the various dimensions of the ether see Asprem 2014, Noakes 2005, and Wynne 1979), among others.

Several interesting speculations about the ether and psychic phenomena appeared during the Nineteenth Century. For example, magazine editor James T. Knowles wrote in *The Spectator* that assuming the existence of brain waves, these could spread via the ether in such a way that each "brain then would become a centre of undulations transmitted from it in all directions through space . . . with the varying nature and force of brain actions . . ." (Knowles 1869:136). Another writer in the *Melbourne Review* speculated along the same line about the possibility of propagation of thought from one person to another via brain waves. This, it was proposed, made sense supposing "the existence of a medium, analogous to that of the luminiferous ether" (Andrew 1876:109).

Several others had similar ideas. French physician Prosper Despine wrote about the possibility of a distant influence from a neuropathic person to another through the "intermediary of an ether fluid which fills all space, which penetrates all our organs, and which puts us in communication with all the beings of the universe" (Despine 1880:132).

American electrical engineer Edwin J. Houston speculated that:

> Cerebral energy . . . is dissipated by imparting wave motions to the surrounding ether, and such waves are sent out in all directions from the brain, possibly in greater amount, or of greater amplitude from some of the brain openings, as, for example, those of the eyes. (Houston 1892:490)

Similarly, the idea of vibrations propagating thought throughout the ether was speculated on by William Crookes (1897).

There are also examples of the use of the concept of the ether in nineteenth century Spiritualist publications. Some examples are as seen in Robert Hare's *Experimental Investigation of the Spirit Manifestations* (1855:160, 163) and Hudson Tuttle's *Arcana of Spiritualism* (1871:163). Tuttle expressed in a later book that there was a psychic ether "related to thought, as the luminifereous ether is to light." This ether, he thought, "may be regarded as the thought atmosphere of the universe. A thinking being in this atmosphere is a pulsating center of thought-waves, as a luminous body is of light." Furthermore: "All the so-called occult phenomena of mesmerism, trance, clairvoyance, mind reading, dreams, visions, thought transference, etc., are correlated to and explained by means of this psychic ether" (Tuttle 1889:5).

Another writer stated:

> We have seen that the ethereal medium interpenetrates all transparent bodies, and probably opaque bodies too. . . . The Spiritual body is said to consist of a subtle form of ethereal matter interpenetrating the material body and bearing a definite correspondence to it. (Bryce 1871:126)

Referring to "ethereal" beings, presumably spirits, Alfred Russel Wallace (1875) speculated on the possibility that they could produce physical and visible effects "drawn perhaps from the boundless ether, perhaps from the vital energies of human beings" (p. 45).

The topic of the ether has also appeared in mediumistic communications. One example is discussions of the cosmic ether (as discussed in physics), and another is spiritual discussions about the attributes of God. The cosmic ether always stayed physical, while the spiritual one could continue to evolve, becoming more refined with time. The interplay between both

ethers, as directed by spirits and by God, "have produced the universe, visible and invisible" (Anonymous 1887).

Ideas of this sort come into the twentieth century, as seen in the writings of individuals such as Oliver Lodge (1919), and were also associated with various branches of occultism, such as Theosophy. My purpose here is not completeness, but a brief reminder of the fact that the ether was more than just a physical concept providing the media for the propagation in space of principles such as magnetism and light. For many thinkers it was a construct with wider implications about the nature of reality and of human beings, a meeting ground for matter and mind, a topic ably discussed recently by Asprem (2014).

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COMMENTARY

The Importance of Retractions and the Need to Correct the Downstream Literature

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The past three to four years has seen—as far as can be perceived—what appears to be an increase in retractions (Fanelli 2013), possibly due to an increase in awareness. This awareness relates to the issues underlying science publishing, whether these involve authorship issues, publisher-related ethics, or what appears to be an explosion in open access journals (Butler 2013), which is making more science more visible to a wider audience. This aspect in itself is an extremely positive development, and we have only to thank the freedom of the Internet and the existence of increasingly global databases, some of which are publisher-controlled, for creating this wider perspective on science and science publishing. However, like anything in life, or society, with such openness comes a darker side. The issue of revelations and anonymous whistle-blowing (Yong, Ledford, & Van Noorden 2013) are all aspects of science publishing that may have been poorly discussed even as little as 5 or 10 years ago. Now, with tools such as Retraction Watch (http://retractionwatch.com/) and other blogs that allow for greater awareness and interaction about publishing, and sites like PubPeer (https://pubpeer.com/) or PubMed Commons (http://www.ncbi.nlm.nih.gov/pubmedcommons/), which allow for a more frank and open discussion of the issues surrounding individual papers or topics, science publishing has, without a doubt, entered a new era of debate, and scrutiny. Those who do not observe this change, who find it insipid, or who wish to ignore it, ultimately risk becoming its victims. This increase in awareness has also drawn the attention and focus to research misconduct, including duplications, plagiarism, and even the issue of fake peer reviews (Ferguson, Marcus, & Oransky 2014), and pseudo-scientific journals or paid-for authorship (Seife 2014). These are issues that affect all scientists and that have now become the centerpiece of science publishing. Thus, greater

awareness, and acceptance, of these issues is required.

One of the greatest emergent issues that has not yet been explored or resolved is the need to correct the downstream literature. When a manuscript is retracted—for whatever reason—it effectively ceases to exist in the literature. Even if the data within that manuscript is valid—to any extent—unless that paper or portions thereof is republished one can claim that a retracted paper represents a case of null and void science literature, and should thus not be referenced for one simple reason: Theoretically, it no longer exists. Although the retraction notice and the retracted paper should remain as part of the public, open access record, to serve as a historical beacon to highlight each and every case, a retracted paper should—save for rare exceptions—not be referenced, nor should its use as a citation serve for the purpose of increasing a journal's citation metrics, such as an impact factor.

Here I focus on a case from the medical literature. In this case, Shigeaki Kato, formerly a researcher from The Institute of Molecular and Cellular Biosciences at the University of Tokyo, now has 33 retractions that have been cited almost 700 times (Oransky 2014). The Japanese media (*Mainichi Weekly*) indicated, following an investigation by the University of Tokyo, that 43 papers should be retracted. This indicates that there are still potentially 10 of Kato's papers that will be removed from the scientific literature. In that sense, the scientific literature has been partially corrected, but not fully. What about those papers that cite the 33 already retracted papers? That literature (hereafter, secondary paper) is now citing de facto nonexistent literature. Thus, any scientist who then references a secondary paper is also indirectly propagating the error. This infinite cascading effect will undoubtedly influence the metrics of a journal, such as its impact factor, even if to a small extent. What then should happen, and who should be held responsible for correcting the downstream literature, i.e. the secondary paper(s)?

At the outset, it is the corresponding author of the retracted paper who should be responsible for contacting the authors, editor, and/or publisher of the journal that cites the retracted paper, and requesting an erratum. The erratum should indicate clearly that that reference has now been retracted, and ideally should provide the web link and as much detail as possible, to alert readers to this important background. Should the author not assume this responsibility, then it is incumbent upon the author's institute and/or co-authors to then assume that responsibility, and if that responsibility is not assumed then the authors, editor, and/or publisher of the journal that cites the retracted paper are ultimately responsible for correcting the literature—with a *corrigendum*—even if, in the latter case, it is not their fault that the

literature has become erroneous. In the case of Shigeaki Kato, there are at least 677 citations from 33 retracted papers. Those numbers in themselves are not worth much to scientists and science itself, but should a random number of retractions be assumed from the wider science literature—a ballpark number of let's say 5,000—and factor in the multiplicative downstream effect of downstream referencing, then the issue becomes not only critical to the integrity of science publishing, it becomes alarming. Simply because we are then dealing with potentially tens of thousands, if not hundreds of thousands of errors, caused exclusively by the existence of retractions, in the downstream literature. This has profound implications not only on the science literature, but also on science education, which relies on an accurate scientific literature for didactic purposes. Should the basal literature be flawed, then there is also the very real risk that the education system (via incorrect teaching materials) can start to become corrupted. The other issue that exists is the potential economic fallout from retractions and an erroneous literature, least of which is wasted taxpayers' money (Resnick 2014).

Very rarely can one observe errata that correct the literature by acknowledging the existence of a retraction within the reference list, but this is an urgent and important aspect that has to be increasingly adopted and accepted not only by the wider scientific community of researchers, but also by editors and publishers. Simply because, within the context of post-publication peer review (Teixeira da Silva 2014), correcting the downstream literature constitutes not only an integral element of accountability in science, but also one core responsibility of authors, editors, peers, and publishers (Teixeira da Silva 2013a). Moreover, as publicly questioned papers raise greater awareness and thus increase the risk of retractions (Van Noorden 2014), so, too, should increased public awareness allow for better correction of the downstream literature related to a retraction. Only through a holistic approach can integrity in science publishing be achieved (Teixeira da Silva 2013b).

There is also, of course, a segment of the scientific community that believes that the downstream literature does not need to be corrected, and hence the need for greater discussion. In the brief period of time that this paper was in re-review, retractions by two more high-profile scientists in their fields of study, Jacob H. Hanna of the Weizmann Institute of Science in Israel (Marcus 2015) and Robert A. Weinberg at the Massachusetts Institute of Technology in the USA (Ferguson 2015), whose papers are also cited several hundred or thousands of times, fortify the need to address this issue as urgently as possible.

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OBITUARY

In Memoriam: Larisa Emilia Cheran (1962 –2015)

This April, SSE Council Member Larisa Emilia Cheran transitioned to a world still unbeknownst to us. Dr. Cheran left strong and lasting scientific contributions to the world of bioelectrochemistry upon which much remains to be built after her passing.

Larisa's professional endeavors began by obtaining her doctorate in Microelectronics at the University of Bucharest, and meeting The Romanian Academy's most respected teachers in her fields of study. She immediately flourished as a prominent Romanian scientist as Chief of The Biomedical Applications Laboratory, at the Institute of Microtechnology in Bucharest, Romania.

Upon the invitation by Physics Nobel Laureate Ivar Giaever, who was credited for the discovery of tunneling phenomena in superconductors, Dr. Cheran came to the USA and continued her work on electrical properties of cells and tissues. From there, she was invited to the University of Toronto, and made it her home for the next two decades. While working there, she was credited with the invention of the Scanning Kelvin nano-probe, the first of its kind to detect non-invasively, and with high resolution, single base changes in DNA.

As a senior research scientist at the University of Toronto, Department of Bio-sensors and Bio-analytical Chemistry, Larisa promoted her Scanning Kelvin Nano-probe instrument for pioneering applications in the label-free detection of DNA, proteins, and living cells, particularly neurons. Her interest was focused on developing biosensors and new detection methods for neuroscience research, and on using vibrational fields and quantum physics in nano-neuromedicine and regenerative medicine, biomolecular electronic interfaces, bio-photonics, and organic semiconductor materials.

Larisa passionately and unrelentingly probed all aspects of the bioelectrochemistry of living cells. Her passing leaves a void in the quest for fundamental understanding in critical areas of consciousness research. She had the innate ability to amalgamate disparate concepts and was well-acquainted with the techniques and personnel in the fields of consciousness and parapsychology research. She applied her technological expertise to

many of the difficult and complex questions of mind–matter interactions and their direct applications to the fields of neuroscience.

Before passing, Dr. Cheran was performing research on the alteration of acoustic properties of mouse neurons as influenced by a healer's directed attention. The effect was so strong that it merits replication. Unfortunately, this experimental program was interrupted by her untimely death just prior to carrying out initial control tests. If anyone has information and interest in continuing Larisa's experiments, please contact George Hathaway, a collaborator in this effort. He can be contacted at Hathaway Consulting Services, Toronto, at ghathaway@ieee.org

C. M. Chantal Toporow
SSE Education Officer

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ESSAY

Essay Review of *The Survival Hypothesis*

Alan Gauld

The Survival Hypothesis: Essays on Mediumship edited by Adam J. Rock. Jefferson, North Carolina: McFarland & Company, 2013. viii+310 pp. $49.95 (paperback). ISBN 978-0-7864-7220-8.

In his Introduction, the Editor of this book, Adam J. Rock, manages to present us in the space of twelve pages—no mean feat—with some key definitions, a "necessarily brief and incomplete history of mediumship," an introduction to the 'source of psi' problem with respect to mediumship, an outline of the structure and contents of the present volume, and his concluding wish that survival-related topics, such as mediumship will receive more attention than hitherto within mainstream parapsychology.

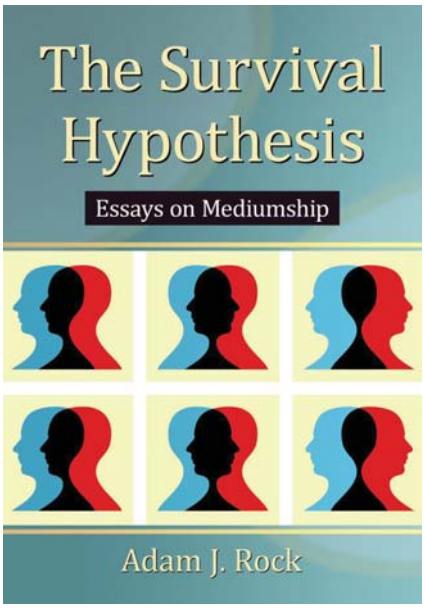
Part I of the book is entitled Explanation and Belief, and opens with two closely linked essays by Stephen E. Braude on The Possibility of Mediumship: Philosophical Considerations, and by Michael Sudduth on Is Postmortem Survival the Best Explanation of the Data of Mediumship?

Stephen Braude is one of a rather limited number of writers in this field whose clarity and brio one can enjoy regardless of whether or not one agrees with his opinions. In the present article he tackles, with special reference to mediumship, the question of whether there can be a scientific or other rational basis for belief in the survival of bodily death. He notes at the outset that this question is hedged about with a variety of troublesome philosophical issues in which the unwary may become ensnared. One such philosophical tar-pit is the problem of personal identity. Many philosophers insist (p. 22) that "our personhood and personal identity are intimately and essentially tied to our physical embodiment," so that "one might wonder whether *anything deserving to be called Stephen Braude* could survive my bodily death." He does not, however, himself agree with this (more or less) received philosophical position. Most people, he says (p. 23), including scientists, "have only a very fuzzy notion of what identity is, or what a *person* is," but nonetheless ordinarily "we have little if any trouble deciding who's who." And this is generally true even if we cannot see the person concerned, but can only (say) interact with him by telephone, or can see

him but observe no psychologically significant behavior. In fact, what we value most about persons are their psychological rather than their physical characteristics (I have met individuals whose practice suggests otherwise), and that is why we are often content to make identity judgments solely on the basis of psychological continuity, and to do so despite our conceptual difficulties as to what constitutes identity, our scientific difficulties as to the basis of bodily continuity, and our ignorance as to how psychological continuity is achieved. These facts Braude feels (and I am inclined to agree) "should be enough to undercut the claim that we can't acceptably make identity judgments in cases of ostensible post-mortem survival" from which bodily continuity is necessarily absent. He proceeds, therefore, to consider possible explanatory options for such cases.

He quickly disposes of such obvious counter-explanations to post-mortem survival as fraud, misreporting, malobservation, and cryptomnesia (he calls them "The Usual Suspects") as radically inapplicable to the best cases. He is better disposed toward a more exotic group of skeptical explanations ("The Unusual Suspects") that he has himself promoted. One subgroup of these involves comparing certain mediumistic performances (also skills exhibited by some individuals who claim memories of previous lives) to "abnormal or rare processes, such as dissociative pathologies, rare mnemonic gifts, extreme or unprecedented forms of savantism,[1] or equally rare latent creative capacities," all of which may manifest quite suddenly and without any obvious period of practice. Braude thinks (p. 25) that such cases "must be considered when evaluating a medium suddenly manifesting an ability associated with an ostensibly deceased person." I have reservations as to the range of applicability of these ideas, but since he says the very best cases are still immune to them, let us press on to his second subgroup of more exotic explanations. These together constitute "The Living Agent Psi hypothesis" or "LAP," a more restrained term which, following Sudduth, he prefers to the customary "super-psi hypothesis."

The LAP counter-hypothesis rests on the point that if a medium produces putative information about some deceased person, and that information, though not available to her by ordinary means, turns out to be correct, there must, for it to have been verified, exist sources for that verification (the memories of still living persons, public records, archives, etc.), sources that would in principle be accessible to the medium through telepathy with the living or through clairvoyance. The LAP theory would, Braude thinks, be difficult, perhaps impossible, for a survivalist to rule out on the basis of mediumistic phenomena. For if it be replied, as it generally is, that this theory would in not a few cases require the medium to exercise psi of a degree never yet reliably reported, the answer would be (p. 27)



that survivalists must themselves "posit comparably impressive feats of ESP, simply to explain how mediums interact with deceased communicators and how deceased communicators are aware of current physical states of affairs."

This sort of answer is not new for Braude. It was, for instance, propounded by Antony Flew (1953:69–70) as part of a lucid and highly critical analysis of the survivalist position. In fact, one thing that has always been clear about survivalist hypotheses—so clear that it is often taken for granted and not spelled out or discussed—is that any hypothesis that permits discarnate persons to communicate coherently with the living and with each other (and this is the only form of the hypothesis we can investigate or for which we can have evidence) necessarily requires that they possess psi abilities of a very high order. They are, after all, bodiless, or without bodies of the sort biologists could examine, and furthermore, according to some, it is the very release from the impediment of the flesh that frees the 'spiritual' faculties. So if the survival hypothesis is correct, there clearly has to be 'super-psi' somewhere in our universe.

What is not so clear is that, in order to pick up and pass on messages from the discarnate, mediums would in all cases have to perform "impressive feats of ESP." Consider: Most such messages, assuming them to be 'evidential,' are supposedly conveyed 'by telepathy,' meaning in this context that information (in an everyday rather than a technical, mathematical sense) is, in some mysterious manner, obtained by one mind (the medium's) from another mind (incarnate or discarnate) by means other than the ordinary channels of sense. But it is not hard to find cases in which, *prima facie*, the message has been in some sense transmitted by the medium to its presumed target (a sitter), and yet the medium has apparently not at any prior stage possessed the knowledge that she is supposed to have thus conveyed. Such, for instance, are cases of automatic writing in which an automatist, in a state of dissociation or of trance, comes out with information that (a) was not previously known to her and (b) only became known to her (if at all) in

consequence of what she herself had written. *Mutatis mutandis*, this could be true in some cases of table-tipping or of ouija board or planchette writing, or even of psychic raps centering round the organism of the medium. The upshot is that in such cases it seems incorrect to ascribe the messages to telepathy (as commonly understood) between the medium and either an incarnate or a discarnate person, for the medium did not at the relevant time possess the information transmitted. The alternative survivalist explanation would be that in these cases a discarnate communicator, possessed of that information, was somehow able to transmit it through the medium as instrument and unregistered by her, she being presumed to possess a suitably pliant psychophysiological constitution.

This kind of view of ostensible mediumistic communication with the discarnate was probably quite widely held or presumed in the early days of the Spiritualist movement but at the present time would be generally rejected on the supposition that the medium must have had the information, however acquired, tucked away in her subconscious mind and ready to emerge in the right circumstances—a *de facto* untestable idea that brings its own additional quota of complexity to an already complicated situation.

But to return to the conventional accounts of the living agent and survivalist hypotheses: These two hypotheses are commonly (and to my mind wrongly) presented as rivals, and there follows from the protagonists of each a series of well-worn arguments and counter-arguments as to their respective merits. However, Braude sensibly steers clear of these routine issues, and instead discusses various illustrative cases or kinds of case, some real, some made up, with a view to demonstrating that, survivalists to the contrary, the discarnate person hypothesis has no advantages in terms of the degree of psychic functioning required over the living agent hypothesis. Taking a 'real' case picked on by Braude as an example, let us consider the curious instance of the supposed chess-playing communicator, the deceased Hungarian Grandmaster Géza Maróczy (1870–1951), between whom and the celebrated living Russian–born Grandmaster Viktor Korchnoi a match was arranged, on the lines of postal chess (Eisenbeiss & Hassler 2006, Neppe 2007). The German-based automatist Robert Rollans (who had initially no knowledge of chess) acted as medium and put up the moves as it were on Maróczy's behalf on a traveling chessboard. The match lasted for some seven years. 'Maróczy's' play was deemed appropriately old-fashioned but of high quality. He resigned after the 47th move.

Braude argues that for the deceased Maróczy to have been responsible for the 47 moves, he would have needed (p. 28) "repeated and accurate ESP . . . to know what the state of play is, and then ongoing and effective ESP . . . to convey the desired next move." And (disregarding here any

advantage that Maróczy's known prowess as a blindfolded chess player might have given him) this would amount to "virtually the same degree of psychic functioning" as the living agent hypothesis would require, presumably for the medium to pick the brains—if one may so speak—of an unawares advisory living grandmaster to whose subconscious he would first have needed telepathically to convey the state of the board and the excitement of the game. It would surely be simpler to suppose that the brains thus subconsciously picked might have been those of Korchnoi, who already had the state of play at his disposal and would have been thinking about 'Maróczy's' likely moves as well as his own. Korchnoi (whom the medium did not meet until near the end of the match) would thus have been unknowingly involved in playing both sides of the game. If this is the simplest account of the matter that the LAP theorist can provide, it might certainly encourage one to reassess the complexities that confront the discarnate person hypothesis, which at least does not require one to engage in so many unverifiable speculations about events in and interactions between the subconscious minds of the medium and assorted living individuals.

Braude goes on (pp. 28–29) to consider a further class of cases that might be thought to pose difficulties for the LAP hypothesis, namely cases which, on that hypothesis, would require the medium to access one or more obscure sources of information not already known to her. He points out that since we know so little about the nature and workings of ESP we are "in no position to insist that normally obscure information is also psychically obscure," or that "psychically accessing multiple sources of normally obscure information" is "more imposing than accessing one." Indeed, he queries whether we currently have grounds for assuming that psychic functioning has any limits at all.

Taking the last suggestion first, though the limits of ESP are certainly vague, we have no grounds for the extraordinary suggestion that it may have none. One might note here (for instance) the rather numerous and, so far as I know, totally unsuccessful attempts by ostensible psychics, from the days of animal magnetism to the early twentieth century—the celebrated Mrs. Piper among them—to give us new information about the other planets of the solar system and their inhabitants. More interesting is the question of how or whether the LAP hypothesis could handle cases that might require it to assume the psychic tapping of one or more sources of normally obscure information.

Let us consider the LAP hypothesis in its two most common forms, namely (1) that the medium acquires information through clairvoyance, i.e. through extrasensory awareness of certain (often distant) physical states of affairs, and (2) that she acquires it through telepathy with the living, i.e.

through extrasensory awareness of what is going on in another person's mind.

(1) The clairvoyant version. This might cover such putative phenomena as clairvoyant awareness of newspaper obituaries, wills, inscriptions on gravestones and monuments, private correspondence, biographical works, and so forth (let us for simplicity refer to all these as 'documents'). Now if a medium is to gather her information clairvoyantly from such sources, her task would have two phases or aspects. Firstly, she would need clairvoyantly to track down (from a doubtless very large range of irrelevant possibilities) a 'document' or 'documents'—which could be anywhere from the storerooms of a large library to a remote country graveyard—relating to the individual she has been assigned, or has taken, as her 'target.' And in many, perhaps most, cases, this could only be done not from the document's physical properties (e.g., a heading involving a certain series of letters of the alphabet), but from its sense (meaning) or reference (what or whom it is about), which in turn might be determinable only from its overall context.

So each candidate document would have to an extent to be cognised and understood and considered in its setting. Secondly, and following on from the last, each chosen document would need to be read in greater detail to extract from it such information as might constitute the sort of 'evidence' being sought.

Now while there have indeed been examples of supposedly clairvoyant 'living agent' psychics who have succeeded in 'reading' a few words (not known to the witnesses) from a piece of paper in a thick sealed envelope (usually placed before them), I don't think anything of this kind has ever been convincingly achieved with an archival document the length, say, of an ordinary newspaper obituary or a will. Without further strong evidence there can be little justification for claiming that such feats can be achieved through the clairvoyance of a medium.

(2) The telepathic version. In common usage 'telepathy' is generally supposed to involve one person coming to know (other than via the normal channels of sense) what is currently going on in another person's mind. A problem with the telepathic version, right from the early days of attempts at the scientific investigation of mediumistic phenomena, has been that the most likely candidates for a telepathic source of the information concerned (sitters at the sittings) have often firmly denied that any such thoughts had been passing through their minds,[2] indeed have sometimes claimed that they were not even aware of the facts concerned. The next move for the LAP hypothesis would be to suggest that some outsider must have been churning over those very thoughts at or soon before the relevant time and floating them on the psychic ether for an attuned sensitive to pick up. It

would be hard if not impossible to prove the negative here, but I don't think that positive evidence has very often if ever been found. A final move by the LAP theorist would be to invoke the subconscious minds or subliminal selves of source and medium (telepathy was widely supposed to operate below the level of consciousness and then suddenly emerge), a stratagem that would leave the occurrence or non-occurrence of the telepathy effectively unverifiable, but open to speculation.

The upshot of this would be that if someone, somewhere, can be supposed to possess the relevant information, i.e. to have it, even though not currently activated, within the potential reach of his or her memory, at least on a good day, the medium can be fancifully supposed, without possibility of contradiction, capable of gaining telepathic access to it. This in turn seems to lead inexorably toward the notion that each of us stores away 'memory traces' which a prying medium might telepathically flick through person by person and item by item until she hits pay dirt. We are getting close again to the idea that psi may have no limits. However, the notion of memory as a sort of filing cabinet, in whatever terms it may be couched, has been repeatedly criticized by philosophers and others as incoherent, with Braude (e.g., 2014) himself among them. It is perhaps not surprising therefore that after appearing, for much of the first half of his essay, to be inclining toward the LAP hypothesis, he starts to swing somewhat away from it. But I still find the exact reasons for his apparent change of heart, or rather mind, a mite obscure.

The change begins on pp. 31–32 where he lists ten desirable features of a case that might seriously strain the living agent psi hypothesis. Some of these relate to Braude's especial concern with psychopathology, psychology, and the motivations of medium and sitters (mediums should not be sufferers from dissociative identity disorder, the manifestations should make better sense in terms of the interests and motivations of the deceased rather than of living persons); some to the need to guard against the possibility of relevant information reaching the medium through normal channels; some to the need for careful and contemporary recording and the communication of verifiable facts; and some to the kinds of personal characteristics of and facts about the alleged communicator that the communications might most desirably bring out. Taken together the items constitute a compact and perceptive yardstick against which to measure the merits of any case that might come before one.

Although no cases ever fulfill all of Braude's requirements, he concedes (p. 33) that "the very best cases are rich enough to give us pause—at least if we don't have a metaphysical axe to grind." We may still not be able to say anything interesting about how survival could occur following bodily

death, and may be at a loss philosophically and scientifically, but (p. 34) "that's hardly unique to post-mortem survival . . . practical considerations trump abstract philosophy every time." After this proem, with which I entirely agree, he passes on to consider the bearing of all this on "the lure of physicalism," and how it should be responded to or resisted. But I have to confess that I found his last few pages—with the exception of some well-put points, and his discussion of the views of the Cambridge philosopher J. M. E. McTaggart (1866–1925), a tad disappointing. It is as though he had not left himself the time or space to go into these issues in sufficient detail or quite with the enthusiasm he showed in dealing with some of them elsewhere. There is, however, no arguing with his concluding remarks (p. 38):

> It's clear . . . that philosophical issues . . . greet us at every turn in evaluating survival cases. These may be precisely the matters about which we need to be most aware, and to which we should also be the least attached as we interpret the evidence. Once we grant that sufficiently powerful cases *could* persuade us—despite our philosophical predispositions or cherished theories—that personal consciousness can survive bodily death and dissolution, the only relevant question then is whether the actual evidence pulls us sufficiently in that direction.

The ensuing chapter, by Michael Sudduth, is a systematic examination of the claim, often heard in one form or another, that post-mortem survival is the best explanation of the data of mediumship. Although I do not always find myself in agreement with his arguments, much work and much thought have gone into this paper, and anyone who takes the trouble to study it carefully and think about it at length will benefit.

The paper is in effect divided into two parts. In the first part, Sudduth attempts to undermine the survival hypothesis by showing that its predictive power is seriously defective and could well be outshone by that of the rival living agent psi hypothesis. In the second part, he tries to strengthen the LAP hypothesis and to defend it against some standard criticisms by developing what he terms a "robust" form of it. As a prelude he briefly discusses the philosophical problem of 'inference to the best explanation' (also known as 'abduction').

He says (p. 42) that the best explanation "is typically an antecedently credible hypothesis that leads us to expect observational data that are otherwise improbable . . . and where the hypothesis exhibits other explanatory virtues (e.g., simplicity, consilience, conservatism, and coherence)." As an example of inference to the best explanation he chooses Halley's 1705 prediction, based on Newtonian celestial mechanics, that the comet, last seen in 1682, would return "about the end of the year 1758, or the beginning of the next." The fulfilment of this prediction both confirmed the Newtonian

hypothesis and extended its scope (comets were little understood at the time, and Halley's has a retrograde orbit).

The case of Halley's comet becomes for Sudduth a kind of benchmark against which he measures the claims of the survival hypothesis to be the 'best explanation' of certain mediumistic phenomena. He begins (p. 47) with the observation that postulating a consciousness that survives death leads *per se* to no predictions whatever as to the kind of evidence for survival that such a consciousness might provide. For that (pp. 47–48) we would have to supplement the simple survival hypothesis with auxiliary hypotheses, e.g., that discarnate persons are intelligent agents with causal powers (ESP, PK), have beliefs, desires, and intentions, much as before, and can be aware of what is going on in our world. These further postulates might lead us to expect that there should be various kinds of evidence for survival. But we are still a long way from measuring up to the Halley benchmark.

To achieve that, the survival hypothesis would have to yield highly specific and preferably novel predictions comparable to the Halley one, for instance predicting who among the available deceased persons will communicate and when, what they will communicate, and whether through the organism of a medium, or in some other way. But the hypotheses we currently have do not yield predictions that even approximate this degree of precision. Of course (p. 49) given various arbitrary further assumptions, "the survival hypothesis can easily *ex post facto* accommodate just about any phenomenon that reveals veridical information about a deceased person." Such assumptions, however, are of no value unless they are not just stories but themselves lead to predictions that can be tested against further facts, which so far they have not been. All this, Sudduth claims (p. 51), deflates the explanatory power of the survival hypothesis.

A survivalist might reply (p. 51) "that but for the survivalist hypothesis the data would be inexplicable." For sometimes the mediumistic data seem to relate uniquely and in detail to some particular deceased person. But this is where the LAP hypothesis comes in. For the LAP hypothesis purports to offer an alternative account of the very data—uniquely indicating as they do a particular deceased person—which survivalists have claimed only they can adequately explain. And this brings us immediately to the question touched on above: What are the potential scope and the *de facto* limits of living-agent psi?

Here Sudduth deploys three lines of attack on the survival hypothesis.

Firstly, he raises the obvious point, touched on above, that if a medium's statements of fact can be verified from extant sources, those sources would be available to LAP, thereby undermining the claims of the survival hypotheses to uniqueness.

Secondly, he tries to show (pp. 52–53) that data from "paradigmatic cases of what we might call ordinary LAP" can help to further undermine these claims. He looks in turn at forced-choice ESP tests, PK experiments involving participants altering the output of random number generators, at free response ESP experiments, and at what he calls "spontaneous case data." With regard to the free response experiments, he briefly mentions (p. 53) "telepathic, clairvoyant, and perhaps even precognitive acquisition of veridical information corresponding to complex and dynamic targets . . . often mediated by detailed mental imagery. As for the spontaneous cases, his examples somewhat surprisingly consist mainly of "large-scale PK effects," many of which could hardly be described as "spontaneous."

It is impossible to go into all this in detail here. The results of forced-choice ESP experiments, particularly the more recent ones, are in general too variable in success rate, too low in significance, and too susceptible of varied explanations to be of much help to the LAP hypothesis. Free response experiments, which have often been combined with experiments on altered states of consciousness, seem *prima facie* more relevant, and some gifted subjects have 'hit' distant and quite complex 'targets' with unmistakable accuracy. It is, however, hard to know along what dimensions to evaluate such cases against the better mediumistic ones, and there will doubtless be differences of opinion as to the likely upshot of such an exercise. My own opinion is that the ESP displayed in such cases, if ESP it be, is still a long way from matching the achievements of the best mediums in terms of the scope of the items of specific and correct information delivered in connection with particular discarnate persons, the sequential delivery of such items in a relatively short space of time, and sometimes in a manner appropriate for the alleged communicator (a possibility, of course, not open in free response experiments).

As for the "large-scale PK effects" (poltergeists, the startling phenomena produced by such "physical" mediums as D. D. Home and Eusapia Palladino), these, where genuine, could certainly be called instances of 'super-psi,' but that does not *per se* make them directly relevant to either the survival or the LAP hypothesis. They could only (with perhaps some rare exceptions) have such relevance if verified information were transmitted through them, and in that event the survival versus living agent issue would be the same as in ordinary cases of 'mental' mediumship.

Thirdly (and this in effect brings us to the second part of his article), he advances his own version of the LAP theory, which he calls, or describes as, A Psychologically Robust Living-Agent Psi Hypothesis. He develops it to deal with what he regards as two remaining problems for the LAP hypothesis: Why should a medium who gains her knowledge by telepathy

present that knowledge (as sometimes happens) by assuming a *persona* with characteristics unique to some deceased person, i.e. in a manner that appears to support the survival hypothesis? And how would the LAP hypothesis lead us to expect (predict) the actual detailed knowledge of that discarnate person's life, concerns, friends, relatives, characteristics, verbal and behavioral mannerisms, etc., which the medium may so strikingly manifest?

His answer (pp. 54–55) is what Braude refers to as a "motivated psi hypothesis," the gist of which seems to amount to something like this: Sitters typically have a powerful desire (usually but not necessarily consciously formulated) for reassuring contact with their departed loved ones. Many mediums have an overriding interest in providing comfort for their sitters. They may sense (by normal or extrasensory means) a sitter's need and pick up something about the discarnate person in question. We may then predict that they will follow this trail, pick up more detail about that person and his or her concerns, and organize the knowledge thus acquired into a sufficient semblance of the original personality to gladden the sitter. Mediums' abilities to perform such feats may be heightened by the dissociative states into which many of them fall, and which are widely believed to be "psi-conducive."

It is once again difficult to know what to say about this hypothesis. There is a shortage of systematic data, though plenty of plausible-sounding off-the-cuff speculation, as to how far and in what ways sitters' emotions may influence the medium, and how far the urgency of a medium's desire to help a sitter may promote or hinder its fulfilment. And not a few verified cases are on record in which neither medium nor sitter(s) had any antecedent knowledge of, and corresponding emotional attitude toward, the ostensible communicator.[3] With regard to the presumed psi-conducive properties of dissociative states—a long-established tradition, and tradition has undoubtedly some influence on the surface form of the phenomena—it is certainly possible to assemble plenty of cases in which this has apparently been so, but one needs also to bear in mind examples of highly successful mediums who did not habitually pass into 'trance.'

A matter in which the motivated psi hypothesis might well have the advantage is the perennial problem of how some mediums are seemingly able to locate wished-for discarnate persons wherever in the universe they might be and forthwith summon them to an ongoing sitting. It might be easier to suppose that no-one is really summoned and no-one really arrives, and that the whole business is got up within the séance room by LAP.

If I had to pick the kind of evidence (other than neurophysiological) that is most awkward for the survival hypothesis and most encouraging

for the LAP hypothesis it would be none of those mentioned by Sudduth, but one that considerably impressed E. R. Dodds in his powerful paper Why I Do Not Believe in Survival (Dodds 1934:147–172). It relates to the performances of certain individuals whom one might loosely describe as 'psychics,' 'sensitives,' or 'clairvoyants,' and who commonly operated by 'psychometry,' i.e. they would be given, and hold in their hands, small objects about the history of which, or the past or present owners of which, information was desired. Dodds particularly mentions studies of such persons by E. Osty (1923), Director of the Institut Metapsychique in Paris, but there were other serious studies of them by other apparently careful persons.[4] Dodds remarks (p. 157) of one of Osty's subjects that she "has obtained numerous veridical communications both about the living and the dead, comparable in range and accuracy with those of the best mediums," and the same appears to have been widely true of others. However, Osty's work has been subjected to criticisms (Schiller 1924) that I cannot go into here, and I have expressed some reservations about it myself (Gauld 1982:133–137), though Prince's short study of Pagenstecher's comparable subject might be immune to them (Prince 1921).

Dodds points out that the same lady has no 'controls' and no 'communicators' and does not regard the dead as the source of her knowledge. He thinks that if she had had spiritualist convictions they would certainly have emerged in her sittings. And indeed little of a spiritualist kind of explanation appears in Osty's published records, though one can of course not be sure what the private convictions of his sensitives may have been.[5] Many celebrated mediums have had 'guides' who at times act very much like the clairvoyants we are discussing, and indeed liked to grasp 'psychometric' or 'token' objects while holding forth, though it is not clear that these were really of help to them. Dividing lines between psychometrists, clairvoyant mediums, and trance mediums are often unclear.

It will be noted that these cases go back to the period between the wars, and such 'sensitives' seem latterly to have been squeezed out of the parapsychological scene by laboratory experimentation on the one hand, and a renewed interest in mediumship and shamanism on the other. This is a pity, because some of the longer cases, difficult though they are to assess, become quite striking when read at length and in detail.

Sudduth raises a number of other interesting issues, but considerations of space preclude my broaching them. I will conclude instead by reverting to what might be called his opening gambit. He began his inquiry, as he well might, by asking what one is aiming at when one seeks to infer the 'best explanation' of the data. As a focus for his discussion he takes (as noted above) the example of the reappearance of Halley's comet, which

was predicted from and therefore explained by the principles of Newtonian celestial mechanics. Now there is a fair-sized philosophical literature (not without controversies) on inference to the best explanation, and I certainly don't want to plunge into it, but it seems to me that for present purposes the Halley's comet example is not a very good one, and leads Sudduth into a necessarily fruitless fixation on exact prediction. What makes the Newtonian explanation such a good one is not just the accuracy of the prediction but also the fact (merely hinted at by Sudduth) that the same theoretical schema can also embrace such diverse other phenomena as the fall of an apple, the movements of celestial bodies in general, the mass of the earth, tides, the effect of changes of latitude on the workings of pendulum clocks, and the precession of the equinoxes. And it is upon this sort of feature (we might call it 'multiple subsumption') that the 'best explanation' status of many scientific theories largely rests. A favorite example has been the Darwinian theory of evolution, especially in its earlier stages. Exact prediction of the course of evolution was never possible, but the theory's strength lay in its having plausibly (and generally retrospectively) fitted various categories of observed facts into an overall explanatory framework. A somewhat simpler example might be the progress of the theory of continental drift as it led into plate tectonics. Of course what could be meant here by "plausibly fitting" is a question indeed.

I don't wish to go into questions of plausibility of fit or multiple subsumption, but only to suggest that it is here rather than with a search for precise prediction that the issue between the survival theory and the LAP theory largely lies (a point clearly appreciated by F. W. H. Myers in his celebrated magnum opus of 1903).

The remaining three papers in Part I are largely concerned with what the authors of the first one, Chris Roe and Elizabeth Roxburgh, term Non-Parapsychological Explanations of Ostensible Mediumship. Roe and Roxburgh begin by remarking, with supportive data, that the general public tends to be quite favorable to "the claim that mediumistic communication has some evidential or practical value." The mainstream scientific view, however, is by and large quite different, and Roe and Roxburgh set out to explore some of the "stock sceptical responses." They start with 'cold reading,' the set of tactics by which a *soi-disant* medium can mislead clients, especially new ones, into believing that he or she knows more about their affairs than could possibly have been learned by ordinary means. Of these tactics Roe and Roxburgh's account, though brief, should be sufficient to inspire sitters and potential sitters with caution as to how they should assess a medium's statements and how they should respond during such occasions, and indeed dress for them.

The second part of the article is headed Accounting for Physical Phenomena and is so exiguous in relation to the scale of the topic that I shall pass it by. The third concerns the reported experiences of mediums themselves. Here the authors remark (pp. 1–2) that mainstream accounts of the mediumistic experience "have characterized it as a dissociative-type one that involves hallucinations, feelings of being controlled by an external power, personality shifts, and alleged post-trance amnesia." This rather readily leads to comparison with certain psychiatric disorders. Pursuing this idea, Roe and Roxburgh review various studies, including their own, which have assessed medium's scores on measures of dissociation, absorption, depersonalisation, 'boundary thinness,' temporal lobe symptoms, and general mental health. Overall there was a tendency for mediums and sensitives to score more highly than controls (though not inordinately so) on several of these measures. However, their scores on general mental health were, if anything, better than those of controls. This finding might perhaps be accounted for in terms of the supportive socio-cultural context from which many of them emerge.

Roe and Roxburgh express the view that in all three of the areas in which they have examined mainstream explanations for mediumistic phenomena, though the proffered accounts might seem plausible, very little empirical evidence has been amassed to provide a persuasive case. They call for further investigations, a suggestion with which there will surely be wide agreement.

The next article, by Krissy Wilson, will no doubt be of considerable value to those who wish to sample the flavor, and assess the standards, of contemporary hard-core scepticism. Some of her concluding remarks (p. 87), together with her list of references, rather strongly suggest that she may not have read many of the original detailed reports on some of the more remarkable mediums, and has relied perhaps (as often happens) on second-hand or third-hand accounts by individuals of orientation kindred to her own. She says that the evidence for survival is anecdotal, often inaccurate, and based mostly on wishful thinking, and is furthermore littered with fraud and questionable methodologies. So it is—if you look only at certain parts of it, including (as Wilson does) various popular television shows.

The succeeding article, The Psychology of Belief in Discarnate Communication, by Tony Jinks, leaves me somewhat uncertain as to its aim. He notes near the beginning (pp. 93–94) how much fraud has gone on in connection with mediumship, but then observes (p. 94) that "much of the contemporary psychological literature . . . is more comfortable associating mediumship experience with a sincere underlying experience, albeit one generated by psychological dysfunction." The body of his article is devoted

to a review of the kinds of dysfunctional experience that may be involved.

The most obvious of these is dissociation, which, he says (p. 94) "exists as a continuum, from absorption to more intense depersonalisation, dissociative amnesia, and ultimately identity alterations." It is, as he remarks, easy to understand how explanations of mediumistic phenomena might be offered in these terms. Although the more extreme forms of dissociation are likely to be regarded as pathological, not a few authorities regard the dissociative manifestations of mediumship as non-pathological and in some cultural circumstances even beneficial. Jinks further notes that the dissociative trait also interacts with other measurable psychological constructs, for instance hypnotizability, suggestibility, fantasy proneness, and transliminality (ready permeability of the threshold between ordinary consciousness and material from a putative 'subconscious').

It all adds up to what he felicitously calls a "medicalization of mediumship," with which psychologists can be relatively happy. These constructs can also be used to explain (in some sense of the term) why mediums have unusual experiences, which may in turn—particularly against a background of Spiritualist beliefs and practices—convince them, and through them others, of their own psychic gifts. As for table rapping, automatic writing, and ouija board practices, these (p. 97) are "unremarkable events experienced as anomalous by susceptible individuals who subsequently develop mediumistic belief," and who, in the case of the ouija board, may even lose sight of the fact that their own subtle behaviors arise from themselves and may in consequence develop "the psychiatric delusion of alien control."

When it comes to the question of why members of the general public, both those who have attended mediumistic demonstrations and those who have not (the latter being a good deal more numerous than the former), should (some of them!) come to believe that mediums may possess psychic gifts, Jinks has a good deal to say, but much of it is obvious and little of it enlightening. He throws in the resistance of belief systems to change, the lack of scepticism in audiences, the influence of social environment, biases in information processing, cognitive dysfunction, and the lower intelligence of believers as compared to non-believers, but such a list is never going to add up in a way that will comprehensively explain all cases.

At the end (p. 102) he makes the following curious statement: "From this perspective, and if an assumption is made that mediumship practice is not scientifically legitimate, then standard psychological theories are valuable in explaining how [mediumistic] practice experience generates belief." In other words, even those persons whose belief that there may be a truly paranormal element in some mediumistic communications is

based on a careful consideration of the best available evidence (and there are not a few such) are to be kept out of court by *fiat* or lumped in with the psychologically dysfunctional, whose views don't count. This may seem like staging *Hamlet* without the Prince of Denmark, but it exemplifies a rather common but largely misguided kind of procedure, which we may describe as dismissing awkward phenomena by impugning the mental stability of those who occasion or witness them. But in the current context this strategy (should Jinks seriously want to utilize it) for setting aside apparently well-evidenced paranormal phenomena (of which there are quite a few), and *a fortiori* any belief in them, simply will not work.

The reason for this is that a medium's mental state (whether normal, abnormal, or positively weird, and whatever her own opinions about it) is largely irrelevant to the question of whether the information ostensibly transmitted by or through her is correct and beyond anything she could have found out by ordinary means. That question—the question of the status of the evidence, good or bad, acceptable or not—can only be settled by competent investigators on the basis of detailed records and inquiries. The medium could be out of her mind and still produce remarkable evidence that she possesses knowledge of matters not accessible to her by ordinary means. *Mutatis mutandis*, the same goes for the table tippers, ouija board operators, and automatic writers on whose mental aberrations, real or supposed, Jinks also dwells.

As for the ordinary folk whom Jinks envisages attending, and uncritically admiring, public mediumistic performances, such individuals may indeed be regularly present, but that hardly affects the validity of evidence collected under more favorable conditions or (in rather rare circumstances) at the meetings in question.

Part 2 of this book is headed Culture, Psychopathology, and Psychotherapy, and consists of five chapters. The first is a densely packed—sometimes rather too densely packed, not to say opaquely expressed—piece by Joan H. Hageman and Stanley Krippner on Cultural Aspects of Personality, Beliefs, and Attentional Strategies in Mediumship. After summary sections (surprisingly detailed) on theory and measurement of personality and theory and measurement of mediumship and mediumistic practices, the authors settle in to describe (p. 112) their own multicultural studies of "personality temperament and the personality traits of absorption among mediums and mediumistic-like practitioners with their cultural counterparts." Following the typology of Keirsey, they divide their subjects into 16 personality types, which they assemble into four larger groups of "temperaments," labeled Artisans, Idealists, Rationals, and Guardians, the latter group being absent from their sample. It takes the authors three pages

to give us word-pictures of the characteristics of these groups, and I would not care to attempt a further digest. However, from the point of view of assessing the frequency and influence of experiences of dissociation and absorption across the various groups, this perhaps does not much matter. Differences between groups (as measured by various standard scales and questionnaires) appeared, at least in part, to reflect the cognitive and attentional strategies embedded in each group's traditional mediumistic and meditative practices, and could bring different benefits. For instance, (p. 117) "the capacity to become fully absorbed and suspend belief may help mediums to deal, in their cultural role, with people under stress and it may also help in meditative practices." Likewise, dissociative capacity "may help mediums to differentiate and to dissociate their ordinary identity during their hypnotic-like ritual procedures." But even when these mediumistic or mediumistic-like episodes involve experiences as of encountering spirits, ancestors, or deities, or undergoing spiritual or ecstatic experiences, it does not follow that the practitioners cannot function normally in their jobs and personal lives. The authors suggest in conclusion (p. 118) that rather than focusing upon whether or not these practitioners are deluded in their beliefs and experiences, scientists should try to clarify "*how* these individuals use these abilities, and in particular *how* and *when* these practices become adaptive and life affirming," or the reverse.

The next paper, Shamanism and Mediumship: Confluence and Difference by Rafael G. Locke, Adam J. Rock, and Roger N. Walsh, tackles the often controversial matter of the relation (overlap, similarities, and differences) between shamanism and mediumship. As an initial working definition of shamanism they take (p. 124) one by co-author Walsh. Shamanism is "a family of traditions whose practitioners focus on voluntarily entering altered states of consciousness in which they experience themselves, or their spirit(s), traveling to other realms at will and interacting with other entities in order to serve their community." Shamanic journeying imagery is held (pp. 124–125) "to be consistent with the shaman's cosmology," which they say "typically consists of a multi-layered universe featuring various NPWs [non-physical worlds]," each of which may contain different categories of inhabitant. Within this context they pass on to problems in distinguishing between shamanism and mediumship.

Their solution is the 'visionary practice' model of shamanism recently developed by Locke. This notion is aimed (p. 127) "at identifying the core features of both experience and action in shamanism and related phenomena (e.g., mediumship, healing, mysticism, meditation) by adopting an existential phenomenological attitude." We shortly find (p. 127) that "the notion of SoCs [states of consciousness] becomes redefined as states of

being (SOB; in and from worlds); that is, being is always in some world and those worlds are both emergent and intentional from the actions of shamans." After this, I fear, I largely lost track of the visionary practice model, which avowedly owes a lot to the early work of the French philosopher Maurice Merleau-Ponty (1907–1961). I was sorry for my non-comprehension, because at times it seemed that something interesting might lie behind the opaque exposition, even something awfully important:

> Our view is that a useful starting point is the virtual space of embodied consciousness and the intentional arcs through which worlds are engaged and realized . . . SOBs revolve about these axes of embodiment and intention in such a way that agency, and its associated identities and actions, is defined in a comprehensive ontology which would then inform neuroscience, psychology, and anthropology. (p. 131)

The most interesting part of this article is its itemized analysis (pp. 128–130) of the "intersection of mediumistic and shamanic vocational expressions and phenomenology." If we set aside the predominance of contact with departed persons in a medium's job specification, the difference between shamanism and mediumship seems to consist mainly (though not exclusively) in the latter being a somewhat or even considerably watered down version of the former. Thus shamans are more likely to gain control of spirits, and western mediums to be more passive-receptive with regard to them; shamans have "a much greater repertoire of roles than mediums (e.g., prophets, magicians, sorcerers, and mediums for minor deities and the spirits of totem animals); shamans have a greater range of expedients for changing state than mediums (most notably the use of drugs and of sensory deprivation); shamans tend to be much more physically active in the expression of their vocation, as in dancing, singing, and feats of strength and endurance; shamans' contact with other worlds "might be used to divine new ways of collective governance, dealing with environmental challenges, warfare, and disease"; a shaman's transformations are often aided or accompanied by group rituals.

These features may be considered in connection with the next article by Christopher C. Cott on Communications with Gods and Spirits in East and Southeast Asia, notably China, Taiwan, Japan, South Korea, Malaysia, and Indonesia. It is a scholarly, interesting, and (to me) surprising piece of work. It seems that practices unmistakably from the shamanism–mediumship tradition are quite widespread in these regions, may involve ostensible contacts not only with ancestors and other discarnate humans, but also with superior and inferior gods, and with undesirable evil spirits. Mediums may come to their vocations by various different routes, and have

affiliations to one or other of the leading religions of those regions. What it would be interesting to know is whether from all these activities there sometimes emerges any such 'evidential' material, as is claimed for Western mediumship.

The next two articles move into the terrain of psychopathology and psychotherapy. In a wide-ranging and informative essay, Jacob Kaminker homes in, after raising some introductory general issues about mediumship,[6] on the tendency, pervasive until recently among mental health professionals, to pathologize extraordinary human experiences, mediumship among them. He enquires—with due recognition of cross-cultural differences—whether, how, and to what extent one can differentiate mediumship from psychopathology, questions which have in the past, though now fortunately rather less so, evoked rather strong feelings.

Epilepsy, historically often advanced as an explanation of supposed possession, Kaminker regards (p. 153) as highly unlikely to explain mediumship completely. Epilepsy often involves sudden and unintentional onset, which is not often the case with mediumship, and in any case the presence of brain disorder, as in epilepsy, "does not discount the possibility of contact with the deceased" (p. 153). Turning to schizophrenia, Kaminker notes that fewer studies have drawn a direct correlation between schizophrenia and mediumship, a possible relation between schizophrenia and shamanism being more often mooted. But while it is the case that a modest percentage of shamans seem to undergo psychotic episodes during their (often stressful) initiations, there appears to be little evidence that established shamans are commonly thus afflicted.

That leaves us with the favorite candidate for a psychopathological interpretation of mediumship, namely that it arises from dissociative tendencies (including trances and identity disorders with subsequent amnesia), often taken to be a defense against the recollection and effects of earlier traumatic episodes. Reviewing relevant studies, Kaminker's view seems to be that the evidence on the connection between mediumship and dissociation is probably linked to cultural differences and variations in training. Some studies (p. 154) found no significant relation between mediumship and dissociative symptoms, others that people with histories of spirit possession "had significantly more severe dissociation than healthy controls." The same went for reports of traumatic experiences, but much depended on whether the mediums were compared with 'normal' controls or with groups of those diagnosed as suffering from dissociative identity disorder (DID). Kaminker concludes (p. 156) that ". . . it is clear that the post-positivist dismissal of mediumship, and the indigenous explanation of all psychopathology as spirit possession, are both oversimplifications." In

a field where almost all professed statements of fact and proposed theories are most probably oversimplifications, this appears highly likely to be true.

Rafael G. Locke writes under the heading of Mediumship and Psychotherapy, but his topic is not what this title might immediately suggest, to wit that psychotherapy might cure mediums of their strange idiosyncrasy. His idea is the rather more interesting one that in certain cases, which might (whatever their origins) be called cases of latent, or potential, or suppressed mediumship or shamanism, with somewhat unnerving or distressing symptoms, the most effective direction of 'treatment' might be to release what has hitherto been latent or suppressed and (p. 166) "engage in consolidating the challenging experiences, providing a new and meaningful framework for them, and settling them into a social environment which understands and supports the person and their otherwise unusual experiences." Locke suggests at the beginning that a partial analogy is provided by the way in which, during the nineteenth century, mediumship in its various forms (p. 160) "allowed many women to challenge the traditional roles and correlated career pathways which Victorian society imposed with its associated maladies of body and mind." But there are certainly instances in the literature of mediumship and of shamanism in which individuals have suffered from bizarre and worrying symptoms that disappeared when they were induced to 'go with' the symptoms (subsequently attributed to promptings from the world of spirits) and admit the psychic side of their natures.[7] At the end of his article Locke describes his own successful treatment of two (not uninteresting) cases of this general kind.

As in a previous chapter, Locke's favored theoretical foundation for his ideas and practice is a phenomenological one founded on the earlier views of Merleau-Ponty and leavened this time by input from transpersonal psychology. There are certainly interesting thoughts and suggestions in his exposition, and my own failure to grasp quite how they fit into a coherent whole or constitute a better framework for thinking about and dealing with such matters than any other that patients might find persuasive may be ascribed to my own shortcomings or to the limitations of the space available to Locke, or to both.

Part 3 is headed Empirical Approaches and consists of a further five articles of which the first, by Julie Beischel, is on Advances in Quantitative Mediumship Research. Quantitative approaches to mediumship research are generally undertaken in the context of a methodology that aims at demonstrating statistically or otherwise (a) that a medium's correct statements (if any) are not simply due to her coming out with commonplace statements likely to be true of a good many persons in her possible client base; (b) that such statements are not prompted by cues inadvertently provided by

interactions with the sitter (even responses confined to "yes" or "no" can be developed into a subtly conducted game of 'Twenty Questions'), or by the sitter's appearance, hands, dress, jewellery, mannerisms, tone of voice, accent, etc.; (c) that nothing is or can be given away by any experimenter involved; (d) that the medium's apparent success rate is not artificially boosted by over-generous assessment by sitters scoring their own sittings; (e) that a medium's successes cannot be due to prior acquaintance with the sitter.

Beischel's article is not greatly concerned with quantitative (i.e. statistical/mathematical) methods but rather with issues of experimental design and control in cases where the principal aim is to demonstrate the occurrence of 'Anomalous Information Reception (AIR) by the medium. She deals only with research carried out in the late 1990s or after (she reviews this pretty incisively), and her principal focus is on the issue of the 'blinding' of participants in mediumistic sittings. The normal condition in ordinary sittings is 'unblinded,' that is medium and sitter (questions of 'trance' apart) both see and hear each other during the reading. In single-blinded sittings the medium is kept from the sitter before and during the reading, and may or may not receive feedback through a proxy. The sitter afterward scores the reading and knows it is his or hers. In double-blinded sittings sitters score the record without knowing whether it is their sitting or someone else's. In triple-blinded readings the experimenters running the sittings are each kept in ignorance of what the others know so that no single individual could give correct information to the medium, or to sitters acting as scorers.

Beischel greatly favors triple-blinding, which she thinks should be mandatory for studies aimed at demonstrating AIR in mediums. One might well suspect that the progressively less personalized conditions imposed by the successive grades of blinding would inhibit a medium's paranormal gifts, but Beischel and her collaborators have obtained significant results even with triple blinding. It would be interesting from several points of view to institute carefully controlled comparisons between the various degrees of blinding.

She also briefly discusses the use of quantitative methods in studies of the phenomenology of mediumship and of the personality and psychological well-being of mediums.

The complementary chapter on qualitative mediumship research is by Kylie Harris and Carlos S. Alvarado. An initial problem for these authors is which of the numerous and varied possible meanings of the word 'qualitative' they should adopt. They proceed (p. 196) by taking over an earlier definition of rather wide scope, according to which qualitative

research explores a social or human problem, and "The researcher builds a complex, holistic picture, analyzes words, reports the detailed views of informants, and conducts the study in a natural setting" (Cresswell 1998:15). This has only a rather limited overlap with dictionary definitions of "qualitative," but is used by the authors to licence general overviews of the early history of mediumship research, of psychological studies of the ways in which mediums receive putative information from discarnates, of mediumship as a social role, of the once widespread view that mediumship is a form or symptom of psychopathology, of mediums' own views of their experiences and how to handle them, of anthropological studies of mediumship and kindred matters in other cultures, and of sociological studies of the interaction between mediums and their sitters in Western or other cultures. The article is perhaps best regarded as a valuable source of information and references on a diversity of interesting mediumship-related issues.

Elizabeth C. Roxburgh and Chris A. Roe follow with a chapter advocating that mediumship research should preferably be approached by a mixture or combination of qualitative and quantitative methods. This is surely indisputably true, and in fact the authors spend much of their article discussing mixed methods designs with examples of their application and advantages from their own work.

The succeeding article by Graham A. Jamieson and Adam J. Rock is entitled A Systems Level Neuroscience Approach to Mediumship and the Source-of-Psi Problem. Its ultimate, and very laudable, aim would appear (p. 248) to be "to integrate descriptions of the conscious psychological processes identified in the medium during readings with the analysis of neural network dynamics recorded in those same readings." I must confess, however, to becoming seriously bogged down in parts of their exposition. After some scene-setting general remarks about the 'source-of-psi problem' in mediumship, the authors take up the question of knowledge of other minds, not from a philosophical but from a practical point of view. They point out how important it is for social mammals, particularly primates, to be able to identify and respond to "three core features of another's mental states: what are they attending to, what are they feeling, and what are their goals" (p. 239). Their 'primary awareness' of these factors is underpinned by discrete neural systems, and is evident in many features of the interactions of mother and child.

Reverting to mediumship, they remark (p. 239) that a medium's "encounter with a discarnate mind (if veridical) is fundamentally the encounter of an embodied human agent with another mind." They go on to propose (p. 241) that when a medium 'communicates' by speech or writing

there must be an extensive chain of complex neural activity ultimately generated by the source of information and by the causal mechanism linking that source to the medium. Now if this chain of neural activity originates from the medium's encounter at some level with another mind we might expect it to reveal features that indicate activation of our deeply ingrained systems for detecting the presence of an external agency. Of course (given that the communications in question are veridical), we still would not know whether the external mind in question was that of a living or a discarnate individual. The authors ingeniously suggest (p. 250) that this problem might be overcome by experimentally manipulating the beliefs, expectations, and motives of the putative living agents, and observing (or not) "corresponding changes in the structure of the mental states disclosed in the communication sequences received by the medium."

A considerable part of the article is devoted to an overview of neurophysiological and psychological techniques by which the activities of the suppositious systems in mediums' brains might (just possibly) be detected and identified and their relationships to known neural networks established. It reads rather like, and probably is, the basis for a research proposal. If so, I wish the authors success—and long lives.

Concluding Part 3 is a particularly interesting article by Julio F. P. Peres, Alexander Moreira-Almeida, and Leonardo Caixeta on Neuroscience of Trance and Mediumship. Near the beginning there is a numbered list of very sensible recommendations for avoiding theoretical and methodological pitfalls in this area of work, and near the end a further such list of hints (based on the author's own experiences) for making volunteer mediums feel comfortable during laboratory procedures. Between these two are a useful survey of previous neurophysiological studies of mediumistic and possibly related spiritual and religious phenomena (which, are said [p. 259] to have "reignited old debates over mind-body dualism and the soul") and an account of the authors' own neuroimaging studies of the brains of mediums during the production of dissociative writing in a trance state.

The subjects of these experiments were 10 mediums, 6 female and 4 male, specializing in trance writing (psychography), all well-educated or relatively well-educated, five classified as experienced and five as less experienced. With one exception, all were psychologically well-adjusted, and none were paid for their participation. The method of brain imaging used was SPECT (single-photon computed tomography), which assesses activity in different parts of the brain through changes in regional cerebral blood flow (rCBF). The results were not as predicted, but of course all the more interesting for that reason. During 25 minutes of trance writing, all subject groups showed overall reduced activity in important areas of both brain

hemispheres compared to their activity during a control period of non-trance writing. Furthermore, the more experienced group showed a significantly greater reduction than did the less experienced one, and overall there was a trend in both samples for the (independently assessed) complexity of the trance scripts to be greater than that of the control ones, i.e. for greater complexity to be associated with lower brain activity. The content of the trance writing was original in the sense that it had not been previously produced. It seems not to have contained veridical communications but to have "involved ethical principles, the importance of spirituality, or joining science with spirituality" (p. 266).

The authors are cautious as to the interpretation of their findings. They note (p. 268) that "the lower level of activity in the temporal cortex and precentral gyrus, as well as the hippocampus and anterior cingulate in experienced mediums lends support to their subjective reports of being unaware of content written during psychography." They also note (p. 269) that the fact that experienced mediums showed reduced rCBF changes during psychography is consistent with the notion that an outer source was planning the written content. And they go on to point out that brain regions involved in planning writing "were activated less, even though the content was more elaborate than their non-trance writing. These findings are not consistent with faking or role-playing." However, they rightly conclude (p. 269): "As the first step toward understanding the neural mechanisms involved in non-pathological dissociation, we emphasize that these findings deserve further investigation both in terms of replication and explanatory hypotheses."

Part 4 consists of two items, the first being an essay by Julie Beischel, Mark Boccuzzi, and Edwin C. May on Mediumship and Its Place within Parapsychology. It adopts a position of what might be called qualified optimism with regard to the future and possibly central place of mediumship research within parapsychology, and a considerably more guarded optimism as to its possible applications in the wider world. The second item, a collection of short pieces on The Future of the Field of Mediumship, gives the various contributors a chance to express their views on this topic and to offer any concluding reflections.

Looking back on the contents of this volume, it would be easy enough to pick on shortcomings. It contains, for instance, rather an excess of acronyms, all too fashionable these days, which may look impressive and save expenses, but can be irritating for the reader, and are not always clarified in the (rather inadequate) Index. It is curious, too, that in a volume devoted to the problems and ramifications of mediumship there is so little in the way of accounts (even condensed ones) of mediums (particularly

the more remarkable ones) in action, to give readers some feeling for what, basically, it is all about. It is as if someone were to write a treatise on autistic savants that did not give detailed examples of the gifts that they may actually possess.

Again with regard to the various possible interpretations of mediumistic phenomena, notably the tangled issue of survivalist versus living agent approaches—and (as Flew [1953:68–69] remarks) tentative and cautious terms such as 'interpretations' and 'approaches' are far more appropriate here than the more ambitious 'theories' or 'explanations'—these questions can only be properly considered within a much broader context than that of just the phenomena of 'mediumship'. One might mention here such obvious matters as ostensible cases of possession, reincarnation, OBEs, NDEs, and certain cases of veridical apparitions, dreams, and visions. These topics could not be effectively dealt with in anything smaller than a considerable tome, but it should be possible to indicate, in one chapter (with references), that such cases do occur and would have a bearing upon the matter of inference to the best explanation.

Despite all this, it would be fair to say that this book fills a niche that needed filling and that it conveniently opens up a good selection of interesting and important issues within its subject area. It was also encouraging to note how much more work is going on in most of these interesting areas now than there was a few years ago. Even though that amount of work is still quite limited, and still seriously underfunded, the main tenor of the book is toward the future rather than being, as so often happens in these areas, simply a review of what pioneers, however worthy, achieved in decades now receding from view.

Notes

[1] One might well wonder if there could be such a thing as 'psychic' savants. D. A. Treffert, a leading authority on autistic savants, mentions (2010:23 and cf., 46–47) that he has been criticized in print for even mentioning in a review article that some parents have referred to 'extrasensory perception abilities' in their autistic children.

[2] Examples can readily be found in the early papers on Mrs. Piper, e.g., Hodgson (1892, 1898).

[3] I have in mind here 'proxy' sittings, in which a proxy sitter substitutes for the person who wishes for communications, and 'drop in' cases, in which a 'communicator' unknown to medium and sitters 'drops in' and delivers subsequently verified information about himself or herself.

[4] For example, Osty (1926), Pagenstecher (1922), Prince (1921), and cf. Barrington, Stevenson, and Weaver (2005).

[5] Pagenstecher's subject believed that she was under the influence of 'higher spirits'. And Osty's subject, Pascal Forthuny (Osty 1926), began his career by attempting (without much success) to receive automatic writing from his late brother.

[6] I was mildly taken aback to find Kaminker attributing to me (p. 146) views on the survival issue that I have never expressed, and certainly not in the reference he gives.

[7] There are some interesting examples from Iceland in Dempsey (2013).

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ESSAY REVIEW

Three Routledge Reissues in Philosophy and Parapsychology

Lectures on Psychical Research: Incorporating the Perrott Lectures Given in Cambridge University in 1959 and 1960 by C. D. Broad. Routledge, 1962/2012. 450 pp. + xi. $150 (hardcover), $46.95 (paperback). ISBN 978-0-415-61072-8 (hardcover), ISBN 978-0-415-61086-5 (paperback), ISBN 978-0-203-83187-8 (e-book).

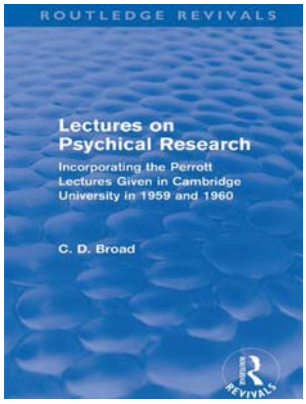
Matter, Mind and Meaning by Whately Carington. Routledge, 1949/2014. 258 pp. $120.00 (hardcover). ISBN: 978-1-13-882491-1.

Brain and Mind: Modern Concepts of the Nature of Mind edited by J. R. Smythies. Routledge, 1965/2014. 272 pp. + x. $120.00 (hardcover). ISBN 978-1-13-882494-2.

With very little fanfare (as far as I've seen), Routledge has republished three books in the relatively recent history of psychical research. All are available in quite expensive hardback versions, and Broad's book is also mercifully available as a less expensive (but hardly bargain-priced) paperback. Moreover, all three can be purchased as e-books, but don't expect bargains there either. As of this writing, the best Kindle price I saw for Smythies' and Carington's book is $92, although Broad's can be had for about $35.

Broad's book (included in Routledge's "Revival" series) is especially valuable for its detailed and probing discussions of OBEs (out of body experiences), apparitions, and mediumistic evidence for postmortem survival. The book was intended originally to serve as a state-of-the-debate presentation of the evidence for psi generally (excluding PK). But unfortunately, the discussion of experimental evidence was limited to S. G. Soal's subsequently discredited card-guessing tests. However, that still leaves more than 300 pages of superb presentation and analyses of various strands of spontaneous case material and mediumistic investigations.

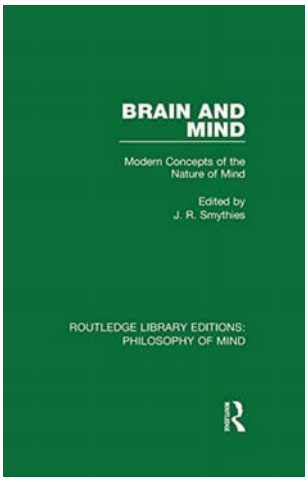
Make no mistake, Broad's book still warrants close attention today. Newcomers to the field (as I was when I first read it) will receive an absolutely first-rate education about the early work of the Society for Psychical Research by reading Broad's discussions of the SPR's "Census of Hallucinations," dreams and out-of-body experiences, which are



rich in case-detail, breathtaking taxonomic flourishes, and probing theoretical analysis. The same may be said about his discussion of trance mediumship, which focuses on the cases of Mrs. Leonard, Mrs. Willett, and Mrs. Warren Elliott. And in his Epilogue, "Human Personality, and the Question of the Possibility of Its Survival of Bodily Death," Broad also offers a sophisticated analysis of the implications of survival evidence for our understanding of the mind.

The Epilogue (indeed, the entire book) is too subtle and rich in detail to be adequately summarized in this three-part Essay Review. For now I'll just note that readers will find much to savor and also much to challenge. As an example of the former, in discussing what it is to be a person, Broad helpfully distinguishes three different kinds or levels of "streams of experience": the personal, the animal, and the biotic (p. 391). And as an example of his controversial assertions, Broad argues that "*apart from and prior to all theory, it is a known fact* that a human being is a psychophysical unit, having two mutually irreducible but most intimately interrelated aspects, viz. the bodily and the mental" (p. 287) (italics added). Although I happen to agree with Broad's claim about the mutual irreducibility of these two components, I doubt that Broad was justified in claiming either that this is a known fact or that this is a pre-theoretical commitment in this or in any domain of discourse. Indeed, although that metaphysical claim may be presupposed by some theories, it nevertheless seems to be paradigmatically theoretical itself. After all, those taking the evidence for postmortem survival seriously represent a wide spectrum of philosophical positions, including various flavors of dualism, panpsychism, and physicalism. Some approaches to survival, therefore, reject the mutual irreducibility of the mental and physical.

Smythies' anthology is structured as a dialogue on the relation between mind and brain between several prominent theoreticians of the day—four philosophers (H. H. Price, C. J. Ducasse, Antony Flew, and Anthony Quinton), one neuroanatomist (Hartwig Kuhlenbeck), one neurologist (Lord Russel Brain), one psychiatrist (Smythies), one psychologist (John Beloff), and a cyberneticist (Donald M. MacKay). It begins by reprinting part of Price's oft-cited (but I'd say overrated) paper "Survival and the Idea of 'Another World'" (Price 1953), whose merits Smythies and philosopher Antony Flew then debate (with responses by Price). *JSE*



readers may already know that this is the paper in which Price argues that the concept of a disembodied life subjectively similar to our own is at least intelligible, contrary to what many skeptics critical of survivalist claims have alleged. Price claimed that a dreamlike world of images, supplemented by telepathic interactions between the deceased, could provide a surviving mind with a first-person analogue to our subjective ante-mortem existence. However, as I've noted elsewhere (Braude 2009), Price in fact offers no help to the survivalist. That's because he doesn't explain "how postmortem individuals manage to acquire veridical and apparently perspectival awareness of *this* world. In fact, Price makes no effort to explain how the deceased, locked into their own exclusively postmortem nexus of paranormal causality, interact with the living to produce *evidence* of their survival" (Braude 2009:201).

The remaining papers in the volume are as follows: The Identity Hypothesis: A Critique (Beloff); Some Aspects of the Brain–Mind Relationship (Lord Brain); Minds, Matter and Bodies (Ducasse); A Rational Animal (Flew); The Concept of Consciousness in Neurological Epistemology (Kuhlenbeck); Mechanism and Mind (McKay); Mind and Matter (Quinton); and The Representative Theory of Perception (Smythies). Contributors comment on the papers and also reply to the comments. Ideally, one would like to think that this kind of dialogue leads to some sort of progress, but as one reviewer of the original version of the book noted, "Although this adds to the interest of the book, and gives it a unity it would otherwise have lacked, one is not left with the impression of philosophers and scientists making much progress with one another" (Vesey 1966:382). Regrettably, that situation has changed very little in the nearly half-century since this book first appeared. In any case, although the philosophy of mind and empirical studies of consciousness have both advanced considerably in the interim, many of the core underlying issues remain the same, and so the various papers in this volume still have something to offer.

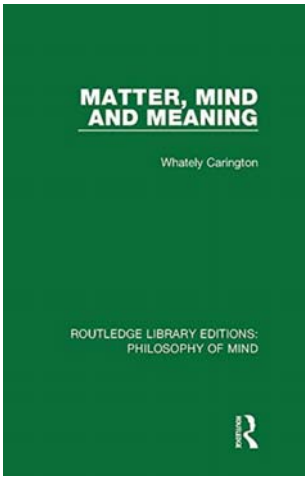
The weakest book in this trio is Carington's. It's actually an uncompleted monograph on philosophy, only a small part of which traces the consequences of Carington's epistemology for psi research (actually, primarily for our thinking about telepathy). The book's first five chapters were more or less finished at the time of Carington's death. The work was

then put into publishable form by H. H. Price, who added a few footnotes and cross-references to those chapters, contributed a brief Preface, and did what he could to complete Chapter 6 on Mind and Matter from fragmentary pencil notes written during Carington's final illness. Carington had intended that chapter to be the philosophical core of the book, but unfortunately that chapter is only a few pages long. So Price also added three appendices, which I gather were unpublished manuscripts. The first, Don't Shoot the Philosophers—Yet, is a more popular and accessible version of Chapter 2, The Failure of Metaphysics. Appendix 2, Life after Death, complements the material in Chapter 4, Mind. And Appendix 3, Does To-Morrow Exist?, presents some musings about precognition, parts of which complement Carington's theory of normal perception in Chapter 4 (and which suffer from the same defects as those noted below).

One gets the impression from reading this opus that it might have been written during a period when Carington realized his life was drawing to a close, and that he was seizing the opportunity to get various matters off his chest. There's a clear vein of anger running through the book, expressed through a steady stream of disdainful remarks—rants, actually—about the state of philosophy generally and metaphysics in particular. I have no problem with that, but overall I found Carington's book somewhat annoying, and annoying in the same way as many advanced student essays. Carington had obviously read enough philosophy to have detailed opinions about it, but his grasp of the relevant issues nevertheless remained rather rudimentary.

Before explaining why I say that, I must also note that, to his credit, Carington offers some delightfully cynical barbs and other choice comments. My favorite: "Spinoza . . . , though an archetypal metaphysician, can hardly be read without profit and a certain uplifting of the soul. But Spinoza was a very great man, whose thoughts about what he called 'God' were so far in advance of his age (and for the most part of ours also) that he was promptly denounced as an atheist; and *he probably could not have written a treatise on sewage analysis without infusing it with his own austere nobility*" (italics added) (p. 13).

At any rate, despite the occasional well-deserved chuckle and felicitous turn of phrase, the substance of Carington's criticisms is quite thin. He proudly aligns himself with the views of the logical positivists and complains repeatedly about the writings of philosophers (apart from logicians). His primary philosophical target is a certain style of rationalistic metaphysical inquiry (classic examples of which would be the works of Leibniz and Spinoza) that was already becoming passé in his day, and which at the time he wrote his book had already been effectively criticized



by the American pragmatists (among others), who were shrewd enough to realize that not all approaches to metaphysics deserved to be scuttled. In fact, they realized that metaphysical commitments are ultimately *unavoidable* in trying to understand the empirical world. What the pragmatists realized, and what Carington apparently failed to understand, was that every branch of science rests on untested and individually untestable philosophical assumptions, methodological, logical, and metaphysical (see Braude 2014). But in that case, all science rests upon the kinds of philosophical claims to which Carington objects. Moreover and somewhat curiously, although Carington seems to have read quite a lot of philosophy, he apparently didn't read the brilliant works of one of his contemporaries, R. G. Collingwood, whose *An Essay on Metaphysics* might have clarified a great deal for him about the nature of both science and philosophy—perhaps especially Collingwood's emphasis on the role of what he called science and philosophy's "absolute presuppositions," and also his well-known analysis of three senses of the word *cause* (Collingwood 1940/1998).

Moreover, Carington's positive epistemological views rest on a notoriously shaky foundation. First, he maintains that we can and should fall back on "hard and 'atomic' statements of fact [about] the irreducible constituents of what we are actually aware of, or do immediately know (cognize)" (p. 15). Indeed, he believes that rational empirical inquiry ought to begin with such observational claims. But empirical statements, at best, are always conditionally, rather than absolutely or categorically, acceptable. That is, there are no empirical statements that are inherently irreducible or simple and that themselves are not undergirded by, or inextricably linked to, an extensive network of assumptions or commitments, the totality of which can only be evaluated pragmatically.

Furthermore, when he explains what his candidates for hard and atomic statements are, Carington embraces a rather naïve sense-datum theory of perception, a form of causal realism positing a Humean "veil of ideas" between us and the objects, the impressions of which (or the properties of which) we report. For example, Carington writes, "we must examine the situation known as perceiving a material object. When we do so, we find that the only entities of [sic] the existence of which we can be absolutely sure are certain 'sensations' (e.g., visual) or 'sensa'" (p. 20). Ironically, that

view is the source of some of the bad philosophizing to which Carington objects, and Carington doesn't see that it leads very quickly to solipsism and relies on the kind of metaphysical commitment against which he'd been ranting—in this case, a commitment to the existence of other minds. (For an account of how that works, see Aune 1970, 1985). Moreover, it's a view that Wittgenstein, for example, attacked successfully in his later philosophy, showing that our use of terms referring to subjective impressions and ideas is actually parasitic on inter-subjective agreements about language-use applied to public objects. See, e.g., the famous beetle-in-the-box example from Wittgenstein's *Philosophical Investigations* (Wittgenstein 1955, para. 293ff).

For those who might be interested—and especially because many still think our most basic and unimpeachable knowledge claims concerns our first-person inner episodes—let me briefly explain. Wittgenstein didn't make the point very clearly (he was struggling to formulate some important points for the first time), but in a nutshell his view was this. Wittgenstein writes:

> Suppose that everyone had a box with something in it which we call a "beetle." No one can ever look into anyone else's box, and everyone says he knows what a beetle is only by looking at his beetle.—Here it would be quite possible for everyone to have something different in his box.

So consider: In such a situation, how is it possible for people to communicate about what they have in their respective boxes? In fact, how is it possible for people to *know* what's in their private boxes? The answer is: only by agreeing to use the term *beetle* with respect to some ostensively identified public object. We still won't know what the other person has in his/her box (i.e. what the person is experiencing privately, what the person's qualia might be), but this is the only way the term can have a real use. Now replace the term *beetle* by the word *pain* or *red*. The meaning of these terms—hence our knowledge of what's in the box or what we're experiencing—can't be fixed solely by connecting it to what's in our private boxes, as many sense-datum epistemologies claim, because there's no way to compare what we're referring to and determine whether the terms are being used correctly or not. That's possible only when the meanings of those terms had been linked to a public object of some kind, such as a beetle one could point to, or something language-users could agree is a red object or an example of pain behavior. That's not to say that Wittgenstein is offering any positive theory of meaning to replace the (still lamentably fashionable) subjectivist theory he's criticizing. In fact, the later Wittgenstein opposed the idea that meanings can be given merely by making a connection (private or public)

between a word and a thing. Rather, he's simply noting that the terms we use to pick out inner states need to be anchored in intersubjective practices to have any use at all. In that respect, knowledge can be said to move from outer to inner, not the reverse.

But, back to the books themselves. The clear winner in this trio is Broad's classic text. The other two books are now rather quaint, although Smythies' volume still offers rewards. Carington's book, I regret to say, is primarily of historical interest.

Stephen E. Braude

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ESSAY REVIEW

Psychiatry Declares Consciousness an Illusion

Manufacturing Depression: The Secret History of a Modern Disease by Gary Greenberg. New York: Simon & Schuster, 2010. 433 pp. (out of print). e-Book $11.66, ISBN 9781416570080.

Of course psychiatry is not asserting explicitly that consciousness is just an illusion; but Gary Greenberg demonstrates that this assertion underlies implicitly what has become standard psychiatric practice: the dispensing of pills to treat purported mental illness. So the title of this book does not do justice to the depth and breadth of its contents. Still, "depression" is the book's explicit focus throughout.

History illustrates that the task of defining mental illness in general is impossible: What are the criteria for distinguishing frank "illness" from "normal" eccentricity and the huge range of human behavior under different social and environmental circumstances? The sociologist David Rosenhan showed—through an undoubtedly unethical experiment—that diagnosing schizophrenia (for example) is highly fallible, and that normal behavior is not recognized as non-pathological once such a diagnosis has been rendered (pp. 41–42). The obvious inference is catastrophic for the profession: "What kind of doctor doesn't know the difference between sickness and health?" (p. 237). Homosexuality was officially said to be a mental disorder until 1973; since then it is not. Even as the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM*) offers elaborately detailed guidance, psychiatrists often disagree over the diagnosis to be assigned in any given instance (e.g., pp. 234–236). Greenberg illustrates the profession's attempts to cope with these circumstances by recounting the history of the several revisions of the *DSM*.

Somatic illness can be recognized through fever, or rank dysfunction of an organ, or the presence of bacteria or viruses; no equivalent diagnostic markers are available with mental illness. Instead, the *DSM* defines illnesses in some such way as: at least some number of items on a list of symptoms, for a period of more than some specified duration, to an extent that is clinically significant. Every part of such a definition is imprecise or arbitrary and is assessed subjectively. Contrast this with "real" medicine. Sore throat and fever do not necessarily mean that there's an infection,

nor does an infection consist only of fever and sore throat: There's a clear distinction between the disease (and its cause) and certain accompanying symptoms. But in *DSM*'s psychiatry, "the symptoms constitute the disease and the disease comprises the symptoms" (p. 64). For example, extended grief or sadness is regarded not as a state of mind but as a disease to be treated by medication. This illustrates Greenberg's main theme, that current psychiatric practice amounts to biological determinism: "We" are taken to be what the neurotransmitters in and between our brain cells determine that we are. This is fundamentalist materialism: Not only is there no free will, there is not even "will". The relation between mind and brain remains not understood, and the contemporary fashion of ascribing mental illness to physical causes—"chemical imbalances" in the brain—is based on presumption, not evidence or proof. Greenberg keeps reminding the reader that one cannot separate the philosophical issue of mind–body relations from any consideration of mental illness in general and depression in particular.

Greenberg is doubly an insider, a practicing psychotherapist as well as having personal experience of periodic bouts of depression. A pervasive theme of the book is his contention that treating depression as an illness denies that sadness, grief, pessimism, cynicism, or melancholia, might be rationally justifiable reactions to particular events or to the general state of the world.

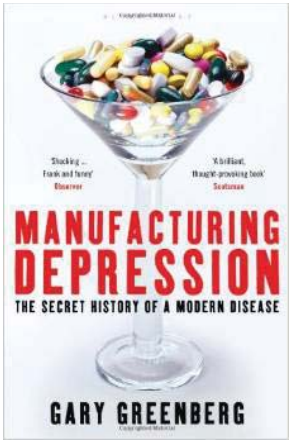
While the issues are deep and serious ones, and at times perhaps a bit technical, the book is easy to read: Greenberg is wonderfully witty and the book is chock-full of bon mots and zingers—for instance, that the chief architect of *DSM-III* "had destroyed the profession in order to save it"; "his denial was, as any psychoanalyst would suspect, an unconscious affirmation of his wishes, his protestations of peaceful intent really a warning of impending hostility" (p. 239). That illustrates why I say "zingers". Greenberg is a very angry man, and passion is rarely far from the surface. This book is a polemic, but it is no loose rant. The language is measured, making its points with irony and wit, eschewing exclamation marks or their adjectival equivalents. I was reminded of the political satirist Mort Sahl, who has been credited for being the first stand-up comedian to skewer politicians simply by recounting their words and deeds in ways that underscored their sheer absurdity, hypocrisy, and lack of truth.[1] And in describing worldly events and circumstances to which depression would seem to be a quite realistic reaction, this book reminded me of Allen Ginsberg's very angry poem, *Howl*,[2] which came, like Mort Sahl in the 1950s, as harbinger of the notorious "Sixties". Greenberg may self-describe as "a hippie–libertarian at heart",[3] but he is also erudite and adept at harnessing his passion to produce writing that is insightful and logically compelling.

The "depression" whose manufacture gives the book its title is the depression that is said to affect perhaps 30% or more of the American population, according to the diagnostic mode enshrined in the *DSM*. Greenberg recounts a long history of views about depression-like states and their treatment. Sigmund Freud's influence is credited with changing psychiatry in a fundamental way, from dealing only with in-patients, people who could not function safely outside asylums, to dealing with out-patients—potentially the whole population. Asylums were for those with clinical depression (or schizophrenia, or other extreme morbidities); whereas the "depression" that supposedly affects that 30% is something very different, neurotic and not psychotic. That difference is demonstrable; for example, electric-shock treatment (or electroconvulsive therapy) appears to be effective with about 80% of *clinically* depressed individuals, but it is not effective with the (30% or so) *neurotically* depressed (pp. 146–147). Current practice blurs that distinction by regarding anti-depressant drugs as appropriate treatment in both cases.

In several places, Greenberg recounts interesting histories of some medications and how they originated in chance observations; for instance, the finding that methylene blue stains nerve cells preferentially led eventually to phenothiazine tranquilizers and to the first really effective anti-psychotic, chlorpromazine (p. 179 ff.). Also of historical interest and contemporary pertinence is that it took the thalidomide tragedy to convince Congress to require (in 1962) that new drugs be approved only if proven safe and effective (pp. 213–215). However, the subsequent reliance on Random Clinical Trials as gold-standard proof of efficacy is flawed: Although such trials can conclusively *dis*prove claims of efficacy, they can only give *probabilistic* evidence of potential efficacy (p. 219); indeed, abuse of clinical trials has had damaging consequences (Bauer 2014). Greenberg gives a useful discussion of the pitfalls of the usual statistical approach (pp. 220–224), insights woefully missing from most of the medical literature.

Psychiatry cannot acknowledge its implicit adherence to materialist ideology since the latter is so blatantly unsupportable: If chemicals determine thought, then nothing anyone (including psychiatrists) says has what human beings call "meaning"—all statements are just outputs of chemical reactions. This dilemma of materialist ideology is less debilitating for physical science most of the time, though it pops up there too when fundamental issues of interpretation and meaning come into play, as say in cosmology and issues of ultimate origins; as Steven Weinberg remarked, "The more the universe seems comprehensible, the more it also seems pointless."[4]

Psychiatry evades this dilemma by ignoring it. The placebo effect is the clearest demonstrable proof that the mind's software can sometimes control



or dominate the brain's chemical/electrical hardware, yet when Greenberg tried to engage Donald Klein, a psychopharmacologist at Columbia University, in a discussion of placebo, Klein declined "for the same reason that I don't debate creationists" (p. 336). Nevertheless, placebo is central to any discussion of psychiatric drugs: Only about half of all clinical trials show the drugs as superior to placebo, and then only by about 20% (Greenberg takes pains to emphasize, however, that this 20% may nevertheless be crucially important for people with really severe clinical depression (pp. 203–204)).

Anti-depressant and other psychiatric drugs are mind-altering. So are "recreational" drugs like Ecstasy or LSD. What is the difference? Greenberg himself experienced relief from a bout of depression with the aid of Ecstasy. The essential difference is in our attitudes to the drugs and not in the drugs themselves: There's something "wrong" with taking "recreational" drugs just to change our moods, but it's perfectly OK to take *prescription* drugs to treat an illness (pp. 23, 192–193). So classifying our feelings as diseases allows us to use drugs guilt-free in order to change our moods.

Psychiatry turned to drugging in part as a way of demonstrating its place within medicine, by contrast to psychoanalysis or psychotherapy in general. Legitimation of mood-altering helped make that move widely acceptable. Huge profits give the pharmaceutical industry reason to pull out all stops to boost sales of psychiatric drugs. By and large, the wholesale peddling of "prescription" drugs, including those prescribed for mental "illness", is causing incalculable damage to incalculable numbers of people.[5] But change seems impossible, and not only because of the influence of Big Pharma; the whole social climate needs to change, since "The captains of the pharmaceutical industry are merely doing what they get paid big bucks to do—to sail their corporate ships eagerly on the winds and currents of the times" (p. 22).

Needed are changes to beliefs so deeply embedded as to be subconscious; primarily, the materialist ideology underlying drug-based treatment must be recognized as such and thereupon jettisoned. Does the *DSM* accurately describe an actual disease, "depression"? "Every approval of an antidepressant also ratifies the claim that the disease it treats really exists" (p. 40). That depressed people get better after taking imipramine entrenches the notion that they had really been sick. Under this reasoning,

GlaxoSmithKline invented "restless leg syndrome" to market a medication for Parkinson's disease that had been selling only poorly (pp. 40–41); and Big Pharma invents and markets diseases galore as a way to sell drugs.[5] Yet the evidence is clear that the theories underlying use of psychiatric drugs are wrong: Depression is not an imbalance [deficiency?] of serotonin, for example, since both increasing and decreasing it can (sometimes only) relieve symptoms. In any case, despite such names as "selective serotonin uptake inhibitor" (SSRI), no psychiatric drug actually affects selectively only one specific neurotransmitter.

This book is highly recommended reading for everyone. It is informative about psychiatric diagnosis and psychiatric treatment, scrupulously sourced, and delightful reading for anyone who can appreciate the use of wit and sarcasm to puncture hypocrisy by a writer who does not shy away from pointing to the Emperor's actual nudity. But enjoyable reading aside, the issues grappled with are far-reaching, of great importance to anyone suffering emotional or "mental" "illness", and thereby also of pervasive social importance.

Notes

[1] About Mort Sahl, http://www.mortsahlofficial.com/biography.html; Mort Sahl, http://en.wikipedia.org/wiki/Mort_Sahl

[2] Allen Ginsberg, "Howl", 1955–1956; http://www.poetryfoundation.org/poem/179381

[3] Greenberg, G. (2014). Scotland: The Epilogue, 26 September 2014 (find online by Googling "Gary Greenberg Scotland blog").

[4] Rigden, J. S. (1994). A reductionist in search of beauty. Review of *Dreams of a Final Theory* by Steven Weinberg, *American Scientist*, *82*(January–February), 69.

[5] For many volumes documenting these assertions, see "What's Wrong with Present-Day Medicine"; https://dl.dropboxusercontent.com/u/56983081/What%27sWrongWithMedicine.pdf

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ESSAY REVIEW

Strange Beliefs and Why They Are Believed

The Unpersuadables: Adventures with the Enemies of Science by Will Storr. New York: Overlook Press, 2014. 355 pp. $27.95 (hardcover), $16.95 (paperback). ISBN 978-1-4683-0818-1.

Scientific Explorers might interpret this title[1] as just another Pseudo-Skeptical[2] debunking of anomalistics. It is not that at all, though it begins like that with a rather jocular treatment of a creationist.

I found interesting descriptions of some truly extraordinary beliefs and practices, and enjoyed much of what is said about Skeptics (and Randi in particular); on the other hand, many sections are quite naïve or misinformed about science and human behavior, and the book concludes without pointing to any significant lesson learned.

The continuing theme seems to be: How and why do people hold strange beliefs, or false beliefs? The trouble is that Storr never defines what makes a belief strange or what makes it false; though implicitly he seems to regard as strange any belief that seems strange to him, and as false any belief that contemporary science does not propound. Nor does the book ever suggest an answer to that large and ill-defined question. There is a great deal about humans being governed by emotion and not thought, by the unconscious and not the conscious mind, which might seem to be at least a partial answer—except that these lengthy disquisitions on emotion and the unconscious have the same effect as extreme relativism from philosophers and sociologists: If everything we "think" is determined by genes, emotions, nerve impulses, and neurotransmitters, then why should we pay any attention to anything anyone says, including Storr?

Perhaps we shouldn't: Storr himself at various places says that he knows that he might be wrong. Unfortunately he never specifies in which way or over what. He reveals much about himself, including his inability to remain friends with someone who believed the USA should invade Iran, or with a Jewish (former) friend after the latter confessed herself unwilling to share a taxi with an Arab (p. 9); thereby he certainly exemplifies the triumph of unfettered emotion over judicious thought. Storr is also disarmingly honest in admitting that he listens to Richard Dawkins because of his scientific credentials and because he shares the same beliefs (p. 10).

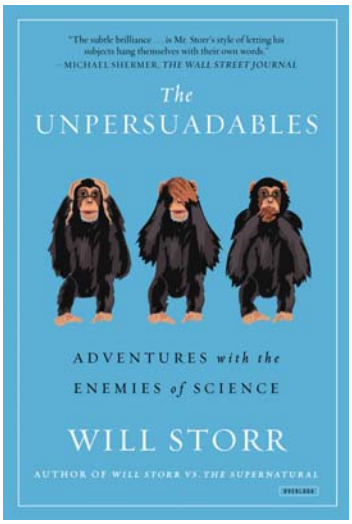
All this would make Storr a doubtfully reliable guide. Moreover, he disclaims any knowledge of science, and seems to illustrate that in appearing not to have known that humans did not evolve from anything like present-day chimps but from mutual ancestors (p. 19); or in asserting that science is "predicated" on materialism (p. 256), apparently unaware of the hordes of religious believers who also do science. One wonders whether he is serious in writing, "We are agents of reason. Everything we know about people tells us this is so" (p. 67). Everything I have learned about people tells me it is *not* so, and large sections of this book assert that it is not so. It is also difficult to believe that Storr was ever as naïve about love and marriage as he reports having been in his mid-twenties (pp. 133–134).

Nevertheless, this book has something of interest for anyone and some things of interest particularly to anomalists. For instance, Storr illustrates the common tendency for people who change beliefs to do so from one extreme to the other (Leiter 2002): the creationist had been raised in an anti-Christian house, the evolutionist by fundamentalist Christians (p. 19). Storr himself rebelled against a Catholic upbringing (p. 21).

Chapter 2 begins with a couple of believers in the more extreme notions about UFOs, but then has a not-unsympathetic section about John Mack and his despicable treatment by his colleagues and by Harvard University. Chapter 3 debunks an Indian guru who claims that breathing correctly—pranayama—can cure all ills. That some individuals attest to having been cured leads to a brief consideration of placebo, a phenomenon that apparently came as a surprise to Storr. He describes Beecher's classic 1955 study (Beecher 1955) as "at best, highly careless," but "it would go on to affect the practice of medicine forever" (p. 41). Really?

Storr experiences past-life regression in Sydney, spends a miserable 10 days at the Vipassana Meditation Centre, and then describes a notorious hoax in which phone calls lead to humiliation of a female employee (pp. 68–69), leading into mention of the Stanford Prison Experiment and Stanley Milgram's study at Yale, which supposedly demonstrate that ordinary people can behave very badly when ordered to do so (p. 69 ff.). A lengthy Chapter 6 then again asserts that we cannot control our own cognition, that our brains are structured so that we *construct* reality and we cannot glimpse the real thing.

Chapter 7 begins with the story of a woman apparently cured of brain tumors by homeopathic pills. Then the book shifts from recounting strange and presumably false beliefs to an unflattering description of the Manchester "Skeptics in the Pub," whose "main hobby seems to be not believing in things. Psychics, homeopathy, chiropractic, God" (p. 96). An editor of *Skeptic* sees the responsibility of Skeptics as "safe-guarders of



the truth" who "are never wrong" (p. 98). Randi's Million Dollar Challenge is cited as failed or evaded by "high-profile Greek homeopath . . . George Vithoulkas" (p. 99); but toward the end of the book (p. 277 ff.) Storr reveals that it might actually have been Randi and not Vithoulkas who reneged from the trial of homeopathy. Skeptics further surprise Storr as "so few of these disciples of empirical evidence seem to be familiar with the scientific literature on the subject that impassions them so" (p. 104). Amen!

Chapter 8 describes a medical self-delusion I had not heard of before; and reminds us that doctors cannot always help patients with unwanted physical sensations. Chapter 9 follows fairly naturally, about people who hear voices, and whether or not this constitutes certifiable illness; the dubious validity of psychiatric diagnosis is mentioned.[3] In connection with a woman's imaginary friend, Storr fails to mention that this is a quite common experience among youngsters—"Many young children (about 65 percent) develop imaginary friends between the ages of 3 and 5" (Gurian no date). A psychiatrist's comment (p. 156) that "about twenty to thirty per cent of what we think are real memories are probably false"[4] leads into Chapter 10, largely a horror story about a young woman who may have died through "recovery" of false memories; but an optimistic note is struck when official psychiatry in 2000 described as "important" an approach to hearing voices that in 1994 it had described as "dangerous" (p. 162).

If the book's title is taken seriously, then among the unpersuadable enemies of science must be counted the one-time "Head of Ethics, Science, and Information for the British Medical Association" (p. 162) and psychiatrists such as Valerie Sinason (p. 171 ff.) who treat multiple personality disorder in which recovery of (false) memories can play a part and who take seriously their patients' accounts of Satanism, child abuse, and cannibalism. Sinason's rationale for believing the truthfulness of these accounts is like that offered by some investigators of UFO abductions: So many people give "ludicrously similar testimony" (p. 179).

Chapter 11 once more harps on the fallibility of the human mind, confirmation bias, cognitive dissonance, and so on. This is marred by gross overstating such as "We can't question ourselves"; most thinking is

emotional, and we are not aware of it (p. 188). I beg to differ. Introspection, not to speak of psychotherapy or psychoanalysis, does enable us to become aware of our emotions and prejudices. Jack Good may have been unique in managing his thinking along Bayesian lines of probability, but large swaths of at least some societies have managed to harness reason and evidence over the centuries to discard laws and practices based purely on ignorance and bias—say about slavery, miscegenation, homosexuality.

Chapter 12 lampoons "the famous climate-change sceptic Lord Monckton" (p. 200) and his "explosively heretical defiance of the scientific establishment's now inarguable case for the dangers and reality of man-made climate change" (p. 203). Here Storr exemplifies what he criticized about Skeptics: He is being dogmatic on a topic about which he is personally ignorant. There is nothing inarguable about the case for human-caused climate change (Bauer 2012: 18 ff. and *passim*). Here Storr also cites the claim that political attitudes are heavily influenced (one-third to one half) by genetic heredity (p. 205), following not quite accurately Haidt's *The Righteous Mind*.[5] Monckton's take on Bill Gates will likely appeal to some who think Gates holds forth pedantically about some matters of which he knows little, say education (p. 212).

Chapter 13 harpoons and lampoons David Irving, "Hitler's ambassador . . . [and] notorious right wing historian" (p. 219) and a number of Storr's companions on an Irving-led tour of WWII sites including concentration camps. But if the book's long discourses are correct, about how our thoughts and beliefs are not ours to choose, should Storr not be pitying or empathizing with these and other victims of false and strange beliefs instead of lampooning and harpooning? After all, "intelligence simply *does not work* in the service of truth" (p. 244, emphasis in original). That extraordinary claim cites a study of views on "socio-political issues," which are not issues of truth or untruth but of values and preferences. Storr's castigating of Irving is such that it almost made me sympathize with this befuddled Holocaust-minimizer.

Chapter 14 deals badly also with Rupert Sheldrake and parapsychology. But then Chapter 15 deals very unkindly with James Randi, whose "boosters are known for their cautious and critical evidence-based thinking" (p. 271); one wishes Storr's tongue was firmly in cheek when he wrote that. In citing Randi's record of self-contradictions and ethical lapses, Storr fails to mention the infamous occasion when Randi trained a couple of lads to deliberately sabotage parapsychological research ("Project Alpha").[6]

Cohorts of Randi also are mentioned here, for instance "Skeptic celebrity Dr. Steven Novella . . . [asserts that] skepticism is incompatible with dogma and ideology . . . [because] it's very anti-dogmatic and anti-

ideological at its core" (p. 272)—to which Robbie Burns (1786) might respond, "O wad some Pow'r the giftie gie us, To see oursels as ithers see us." Bartenders become excellent judges of personality, like the one cited here by Storr: "Skeptics. They're like conspiracy theorists" (p. 291).

So this book offers points of interest as well as some disappointments. Storr disappoints by not grappling consistently or thoughtfully with his major question, about the origin of strange beliefs. He confesses fallibility, but doesn't proceed to think about how it is that humans collectively have managed to attain quite a lot of empirically and demonstrably non-false understanding. He regards as counterintuitive the fact that intelligence is no protection against strange beliefs, something I imagine most Scientific Explorers learned early in their ventures into anomalistics; and he imagined that "simple facts and basic logic" should suffice to disabuse creationist belief (p. 26).

It seems to me fairly obvious that the question to ask is not how some people can succumb to strange beliefs, but how some people manage to align their beliefs with sound evidence (Bauer 1984:185). If sociobiology has any insights to offer, surely one of them is that all living things, at least from birds and mammals on, learn unequivocally from their parents and other nurturers. Birds show their offspring how to fly and how to get food; over many summers at Loch Ness, I watched young ducks being taken on what were obviously training runs. Humans are no different: We rely on our parents for everything, for years, because we have no choice about it; so naturally we acquire our parents' beliefs, and changing them later appears not to be easy and may even be traumatic, as when belief then swings to the opposite extreme. The wonder is not that people have strange beliefs or false beliefs, the wonder is that some people some of the time manage to align some of their beliefs with evidence.

Notes

[1] First published in the UK by Picador, 2013, as *The Heretics: Adventures with the Enemies of Science*.

[2] Marcello Truzzi pointed out that self-styled "Skeptics" are actually not at all skeptical about mainstream science and should therefore be described as pseudo-skeptical.
http://www.anomalist.com/commentaries/pseudo.html

[3] For much more about lack of validity of psychiatric diagnosis, see my reviews of *Saving Normal* and *The Book of Woe* (*Journal of Scientific Exploration*, *29*(1) [2015] 142–148) and *Manufacturing Depression* (*Journal of Scientific Exploration*, *29*(2), 354–361 [this issue]).

[4] One of my most gratifying moments as an author came when a colleague

told me he remembered exactly the people and events in a story in my Dean's memoirs (Bauer 1988)—a story I had created from stereotypes and not from actual people or events. My colleague illustrated how false memories can be even when we experience them as genuine.

[5] See review in *Journal of Scientific Exploration*, *26* [2012], 719–720.

[6] http://www.nytimes.com/1983/02/15/science/magician-s-effort-to-debunk-scientists-raises-ethical-issues.html

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ESSAY REVIEW

Abusing Probabilities, and Other Pseudo-Skeptics' Misdeeds

Reality Check: How Science Deniers Threaten Our Future by Donald R. Prothero. Bloomington/Indianapolis: Indiana University Press, 2013. 369 pp. $35 (hardcover), $29.99 (e-book). ISBN 978-0-2530-1029-2.

A common ploy by pseudo-skeptics[1] is to make a correct statement warning against a general sort of error, followed by committing that error in some minimal sort of disguise. For instance, warn against taking correlations as reflecting causation but do that very thing concerning, say, carbon dioxide and global temperature; or about cancer and smoking: "the link between cancer and smoking is about 99%" (p. 32).

No source is given for this claim, reprehensible in a book that professes to be evidence-based. But what does this even mean? Is a link a cause, as the context implies but as the book explicitly warns against presuming?

Does it mean that 99 out of 100 smokers will get cancer? Or that 99 out of 100 researchers say so? Or that only 1 study out of 100 did not support the connection? Or that there is a 1% probability that smoking does not cause cancer?

Whatever the meaning, "Based on statistical analysis, we can show that if something has a 99% likelihood of occurring, or being true, then this level of confidence is so overwhelming that it would be foolish to ignore it" (p. 32).

This is nonsense. There is no statistical analysis that determines whether or not something is foolish. Foolishness is a human characteristic diagnosed subjectively and statistical analysis has nothing to say about it.

The asserted foolishness is then "illustrated" by the high likelihood of injury or death if one jumps off a building, an entirely inappropriate, unwarranted analogy. The evidence about the consequences of jumping off buildings is quite directly observable, no inferences needed; by contrast, the link between cancer and smoking is based on inferences from data that are probabilistic: analyzing records from people who have smoked varying amounts for varying lengths of time and applying statistical tests of significance.

The most subtly misleading or deceitful aspect of that "99%" assertion is the implication that smokers will inevitably get lung cancer, and this

illustrates a highly important point about probabilities and their (mis) interpretation, a point that crops up in a number of quite different matters.

If a smoker dies of lung cancer, there is a high likelihood that smoking was a causative factor; but that is not at all the same as saying that smoking is highly likely to cause death by lung cancer. In actual fact: "Smoking accounts for 30 percent of all cancer deaths and 87 percent of lung cancer deaths" *but* "fewer than 10 percent of lifelong smokers will get lung cancer" (Wanjek 2008).

The same point applies to the risk of false positives in medical tests, for example positive mammograms in a woman with no known risk factors is highly likely to be a false positive—whereas of course a woman with breast cancer will very likely have a positive mammogram (Strogatz 2010). All sorts of inferences can be quite unsound if one does not understand that probabilities cannot be turned around in this sort of way. O. J. Simpson benefited from a statistic cited by Alan Dershowitz that only about 0.1% of wife-batterers go on to actually kill their wives. But this was misleading. Although the probability that a wife-batterer will actually kill his wife is indeed very small, the turned-around or commutated probability that the murdered wife of a battering husband was killed by the husband is high. As I. J. Good pointed out, that latter probability is greater than 1 in 3 (Good 1995) and perhaps as high as 90% (Good 1996).

At any rate, *Reality Check* is guilty of ignorance about probabilities and also misleading about how smoking was proven to be a cause of lung cancer: not by statistics but because dogs forced to inhale tobacco smoke did develop lung cancer at an appreciable rate. The book is ignorant about science as a whole by claiming that the way not to get fooled is to use "the scientific method"; as David Goodstein (1992) pointed out, "I would strongly recommend this book [Bauer 1992] to anyone who hasn't yet heard that the scientific method is a myth. Apparently there are still lots of those folks around." Including among scientists and pseudo-skeptics like Prothero, more than two decades on.

The errors and flaws in this book are so numerous that it would be wearisome as well as impractical to list even most of them. Just for the flavor:

- As with smoking, so with many other things. Prothero believes that everyone should accept whatever mainstream science happens to say at the moment, and that those who don't are foolish or worse: those who question whether HIV causes AIDS, or who resort to chiropractic instead of always trusting mainstream medicine. At times the book is more than a little self-contradictory since it rants throughout against greedy corporations even though the latter includes the greedy pharmaceutical

industry that is in cahoots with supposedly to-be-trusted mainstream medicine.

- "Vioxx . . . remains an isolated case of a drug that was not pulled off the market as soon as the test data became available" (p. 148) displays colossal ignorance about the reality of drugs improperly approved and withdrawn only after too many have suffered harm and death; see numerous documented instances in many of the books listed in the bibliography *What's Wrong with Present-Day Medicine*.[2]
- Regarding the paper by Wakefield et al. reporting 12 cases of autism apparently related to multiple simultaneous vaccinations, Prothero asserts that "it is customary *not* to publish such preliminary results" (p 150). Nonsense. Medical journals publish "case reports" about as few as a single patient. Such reporting is invaluable for working physicians who are thereby able to realize that something inexplicable that they themselves come across is not unique. For example, it was a succession of case reports that brought recognition of "AIDS". *Every* discovery of a previously unknown condition must inevitably begin with a case report, a "mere" anecdote.
- While claiming to be evidence-based, Prothero just takes as "science" whatever the current mainstream consensus is. However, the history of science shows quite clearly that science progresses as the mainstream consensus is modified or overturned. That most published articles support the consensus is cited as evidence for its validity (p. 91). However, Scientific Explorers, independent thinkers, and researchers who differ from the mainstream view know that this is not owing to any validity of the mainstream view, it is because the mainstream successfully, for instance through peer review, keeps dissenting claims from being published—read for example about the emasculation of *Medical Hypotheses* for daring to publish evidence against HIV/AIDS theory (Bauer 2012: Chapter 3).
- Technology is conflated with science, whereas historians have published innumerable volumes showing that those two enterprises, while of course related, are not a matter of technology being applied science: Instead, scientific understanding has often followed after technological invention, as thermodynamics followed from the invention of steam engines.
- Cold fusion, according to Prothero, was proved impossible within a month of the announcement by Pons and Fleischmann (p. 18). However, hundreds of researchers have continued to report and publish positive results of excess heat or energy in systems similar to those of Pons and Fleischmann (Bailey & Borwein 2015).
- In several places, Prothero makes the typical pseudo-skeptical claim that only those who have worked in a given field are truly qualified to

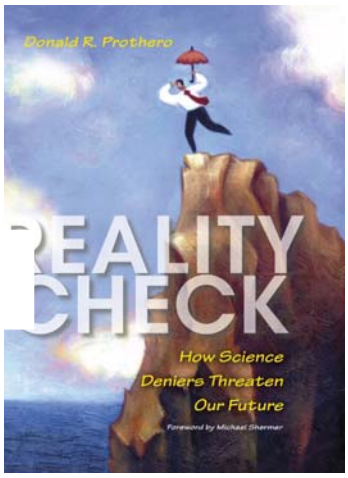
evaluate work in it. Yet Prothero himself displays considerable ignorance about the history of science, philosophy of science, and sociology of science, but does not hesitate to pronounce dogmatically about matters in the purview of those disciplines. Moreover, he has not himself worked on most (evolution is the exception) of the technical issues he purports to evaluate in this book—he just parrots mainstream sources, mostly secondary ones at that.

- Prothero describes scientists, using himself as an example, as idealists making sacrifices instead of going into business or law with their "huge salaries" (pp. 62–63). Scientists in recent times have become wealthy, and even celebrities, through patents and the like (say, inventing a new anti-HIV drug).
- "[W]e [scientists] do not get away with biases for long" because "the rest of the scientific community will jump in and criticize it" (p. 63). I know of no such instance, and none is cited.
- Guilt by association is routinely invoked; thus "the membership lists of creationists and climate change deniers have a great deal of overlap, and both causes are promoted equally by right-wing political candidates, news media (especially Fox News), and religious organizations such as the Discovery Institute" (p. 3); "Fox News, Glenn Beck, and Rush Limbaugh" (p. 98). Moreover, both "denier movements" are "heavily funded by wealthy entities with vested interests"—Howard Ahmanson, Coors family, McClellan Stewardship Foundation, ExxonMobil, Koch Industries. This book is an unrestrained rant, without nuance or distinction, against 9/11 Truthers, conservatives, Flat-Earthers, fundamentalists, Holocaust deniers, snake handlers, anti-vaxxers, right-wingers, corporate greed, and cults and cultists.
- The degree of *ad hominem* labeling is extreme. Anyone who has conservative political views is denounced as a corporate or right-wing shill, including among others (p. 53) eminent physicists William Nierenberg, Edward Teller, and Frederick Seitz (president of Rockefeller University and of the National Academy of Sciences).
- "The evidence for climate change has been accumulating since the 1950s, and was a minor political topic in the 1970s and 1980s." Yes, indeed. There was marked climate change in the form of *cooling* in mid-20th Century and public media were reporting in the 1970s scientific fears of a new Ice Age,[3] whereas global *warming* hysteria took off in the 1990s; and the latter was re-named "climate change" because the carbon-dioxide-warming hysteria could not be justified as temperatures have failed to increase appreciably in the last fifteen years or so while CO_2 has continued to increase.[4]

- "[T]he fact that AIDS was caused by the HIV virus was as well established and uncontroversial as gravity or the idea that the earth is round and goes around the sun" (p. 164). This is typical pseudo-skeptical sleight of words, and is simply not true. There is a vast literature debunking HIV/AIDS theory, and its flaws are evident in the mainstream literature itself.[5]
- Elementary errors:
- * Ozone is "made of three oxygen molecules bonded together" (p. 56); no, 3 *atoms*.
- * Thimerosal in vaccines was mistakenly blamed for causing autism: "Intuitively, having heard that mercury in its raw elemental form is toxic, some people naturally jump to the conclusion that any mercury compound is also dangerous" (p. 153). Nonsense. Elemental mercury is harmless, it can't get absorbed. Some inorganic mercury compounds are unhealthy, but the most dangerously toxic ones are the *organic* mercury compounds—of which thimerosal is one. Prothero compounds this blunder by asserting that there was no difference in side effects from vaccines after thimerosal had been withdrawn from use—not realizing, apparently, that withdrawal from use is acknowledgment that it is potentially harmful. That it was present in only tiny amounts and that global statistics showed no correlation with autism is also misleading. Human beings are not all the same, and some number might be specifically sensitive to a given material; if that number is not large, it will not show up in global statistics. Vaccination guidelines stipulate that the multiple vaccine not be given to babies under one year of age. So at 364 days it is risky but at 366 days completely safe? For every child everywhere? Moreover, no one claimed that the multiple vaccine was the only cause of autism, only that it could be one such stimulus for some babies at some times. When it comes to environmental matters, Prothero adopts the precautionary principle: When not entirely sure, err on the side of being overly careful. Vaccination is a challenge to the immune system. Babies have only partly developed immune systems. Does it not make perfect sense to administer vaccines singly over a period of time rather than all at once? And should not parents be allowed to choose a later rather than earlier age for their baby to have its immune system challenged?

So this is a really, really bad book.

JSE former Book Review Editor David Moncrief and I discussed at various times whether bad books should be reviewed, more particularly reviewed in the *Journal of Scientific Exploration*, and we usually concluded that they should not be, kinder to authors and publishers just to ignore them



and kinder to readers of the *Journal* not to clutter up space and waste their time even glancing at negative reviews. Yet here, I believe, is an exception; in part because it allows illustration of that important general point about probabilities, but also because the book has received such plaudits: published by a university press, it has close to 5-star rating at amazon.com, and it gained a Foreword Silver Award for Science.

If anyone still pays attention to amazon.com rankings or ratings, here is an opportunity to be disillusioned. But what about a Foreword Silver Award for Science? Apparently some putatively qualified group found this an outstanding example of good science writing?

The for-profit corporation Foreword Reviews[6] publishes 150 reviews every quarter in their magazine *Foreword Reviews*. But if your book fails to make the cutoff for inclusion there, the services of the same professional reviewers can be retained to have published "objective, 450-word reviews (including a star rating) by *Clarion Reviews*, Foreword's fee-for-review service"—at just $499 per review.

This adds another arrow to the quiver of self-publishing: Pay not only for the actual costs of publishing but also for any and every way to get publicity, including having "objective" and star-rated reviews "published" for books that were not regarded as good enough to be among the 150 most noteworthy ones published in the same quarter of a given year.

The chances of receiving an award are greatly increased because there are 62 theme categories handing out "IndieFab" awards in each, and it costs only $99 to nominate your book for an award[7]—you are not paying for the award itself, of course. And after the "award" is made, you can purchase "foil seals" to stick on each book as evidence of the award, only 20¢ per seal when ordering 500. I was not able to negotiate the Internet well enough to discover whether the IndieFab awards included Foreword's Book of the Year award, which may or may not include its *Gold* Book of the Year award. Google does have images of a Gold Seal for Foreword's Book of the Year *Finalist* as well as for Foreword's Book of the Year *Winner*.

At any rate, how *Reality Check* qualified for a Foreword Reviews Silver (but not Gold) Award for Science is no longer a mystery to me. Why the book's Foreword is laudatory was never mysterious since it is written

by leading pseudo-skeptic Michael Shermer. Nevertheless, I am more than a little sad about all this praise, for it requires no recognition of Prothero's numerous gaffes to recognize the pervasive *ad hominem* comments and the over-generalizations unsupported by any cited evidence, which ought to be obvious to any halfway observant reader.

I would nominate the book for a Wolfgang Pauli (WP) award. Pauli is often cited for dismissing some writings as so uninteresting and badly done as to be "not even wrong." Consequently, the WP award is for books so bad that they are nothing but wrong.

Notes

[1] Those who are skeptical only about the views of others, not their own; in particular self-styled "Skeptics" who demand allegiance to every contemporary scientific consensus.

[2] https://dl.dropboxusercontent.com/u/56983081/What%27sWrongWithMedicine.pdf

[3] https://stevengoddard.wordpress.com/1970s-ice-age-scare

[4] http://www.bbc.com/news/science-environment-28870988; http://www.nature.com/ngeo/focus/slowdown-global-warm/index.html

[5] The Case Against HIV; http://thecaseagainsthiv.net

[6] https://publishers.forewordreviews.com/reviews

[7] https://publishers.forewordreviews.com/awards

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BOOK REVIEW

Secular Spirituality: The Next Step Towards Enlightenment (Studies in Neuroscience, Consciousness, and Spirituality) by Harald Walach. New York: Springer, 2015. ISBN 978-3-319-09344-4.

Harald Walach's *Secular Spirituality: The Next Step Towards Enlightenment* is an ambitious book that touches on a variety of abstruse materials, ranging from medieval mysticism to quantum mechanics. The author is a clinical psychologist who in this book takes on the role of physician of worldviews. The title *Secular Spirituality* may confuse the reader because the word *secular* connotes non-religious and non-spiritual. But by secular he actually means "not part of any institutionalized dogma or belief-system." Although the distinction between spiritual and religious, a popular trope nowadays, can be overdone, it is central to Walach's project.

He begins by describing himself as having had important spiritual experiences, but treats them critically and with nuance, a stance essential to his notion of secular spirituality. In this book, he comments on many topics, distinctions, and presuppositions, and not always with clarity, which can retard the flow of the narrative; therefore my comments are highly selective.

Walach's project seems to be twofold. He wants to disentangle the gold of spiritual experience from the dogmatic, variously tainted, residues. The first step is to rescue spirituality from religion and its biases. But the author also takes on the complementary task of rescuing the spiritual from the hubris of science. To succeed in freeing the spiritual from the fetters of religion *and* from the ignorance of dogmatic science: that happy conjunction of fates would constitute 'the next step towards enlightenment'—our book's subtitle.

Walach stipulates that his idea of enlightenment is meant in the 18th Century European sense where science throws off the shackles of superstition and tyrannical religion; but his usage also includes mystical enlightenment, as described in the ecstatic poetry of a Rumi or the meditations of a Meister Eckhart. So much for the main idea of the book: saving spirituality from science that has fallen under the spell of physicalism, and creating a new science informed and transformed by spirituality.

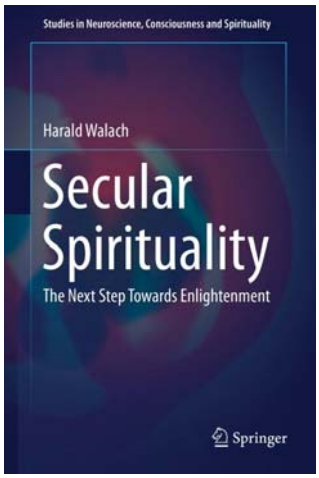
Walach begins by clarifying his terms and making explicit the important presuppositions of his thought. Experience is central and immediately self-verifying; conflict arises only at the level of doctrine (p. 5). The crucial

thing for those who yearn to take the next step is to preserve the freedom to hold whatever "doctrines" (or non-doctrines) we choose to name or describe our original experience. As experience lives in tension with doctrine, so does spirituality live in tension with religion. Walach refers to religion as the "vessel" of spirituality. The vessel can rust, crack, break—and is therefore replaceable; the spiritual is self-renewing because it is anchored in something "absolute." Walach is not happy with postmodern relativities, and invokes William James, Robert Forman, and others as having documented the universal aspect of spiritual, especially mystical, experience.

Chapter 4 defines the concept of spirituality as a form or mode of consciousness, and then poses the question of the "reality" of consciousness. The shortcomings of idealism and materialism are reviewed, the author preferring a kind of dual aspect monism, in which the mental and the physical are conceived as irreducibly real but complementary, each rooted in a oneness and wholeness of being that mystics and few others can intuitively grasp. Walach underscores that experiences of secular spirituality may occur via scientific or artistic inspiration and in other surprising ways. Science and secularized spirituality are compatible; there is duality but not dualism; oneness without reductionism.

Chapter 5, Neurobiology and Physiology of Concentration and Relaxation, should be useful to the spiritual psychologist. Combining secular science and consciousness studies, it shows how certain traditional spiritual practices like meditation generate health benefits as well as heightened cognitive and creative powers. And, I would add, beyond the lab science of recent decades, in the history of religions, anthropology, medicine, mediumship, shamanism, and psychical research, we find a plethora of reportage on altered states of consciousness and extraordinary phenomena. All this material offers the reader a way to practice "daily psychological hygiene." This phrase of Walach's is apt for our secular world, which needs to hear talk of spiritual life translated into talk of psychological health and earthly well-being.

From here we move on to a discussion of some of the dangerous ways spirituality may be distorted. Of this there is abundant material to observe and analyze in the world around us today, but Walach instead raises the question of how Nazi psychology was driven by a sick spirituality. Narcissism is also a danger that can sneak into the sanctuary of spiritual life. The old spiritual masters called it pride, and reckoned it the worst of sins; but it helps to find equivalents that speak to the 21st century. Also in line with the author's project, the spiritual dimension he wants to retrieve from oblivion in no way entails that we disparage reason; it does, however, broaden our concept of rationality along with our concept of what is possible in human experience.



In the course of discussion, the spiritual therapist becomes a prophet and a reformer. For in order to take the next step towards enlightenment—the untrammeled flowering of our spiritual consciousness—there are many problems we have to deal with. There are, we are reminded, daunting issues of meaning, energy, climate, fundamentalism, peace, poverty, misery, knowledge, values, commitment, unity, and scientific progress. The contention is that spirituality, wed to science and an ethos of human solidarity, is our best bet for coping with all problems and crises, our best hope for a more enlightened society. For the author, systematic spiritual education offers the only reliable, long-range remedy for the ills of modern society.

We cannot cover all his views on so many monumental matters, but some of them are quite sensible.

> This culture will only take root when we have top politicians and political administrators who are fearless and work from a place of inner conviction . . . . (p. 190)

This is clearly a matter of consciousness and spirituality, generally lacking in most politicians and plutocrats, and would point the way to an enlightened society. Moreover, peace between religions can only proceed from nurturing the depths of spiritual experience.

Creating "a culture of experience of connectedness" is the way to cultivate peace between religions. The term *connectedness* is a bit threadbare here; something deeper and more profound is at stake. I need to feel more than "connected" to the people around me before I can care deeply about their fate. In any case, the idea of 'connectedness' has a dark side, and can be used to serve the interests of domination. If we hope to advance toward enlightenment, we need to celebrate pluralism as well as 'connectedness.'

One thing keeps the world in continual *dis*-connectedness: the disproportion of wealth and power of the entrenched few and the poverty and oppression of the many. Apart from the gross discomforts of deprivation, there is the more racking pain of injustice. Walach believes that secular spirituality has the power to speak to the evils of poverty and oppression. The impoverished and the oppressor both suffer from a deficit of consciousness;

a failure to realize the ultimate, all-uniting state of consciousness. "The experience of a basic unity," Walach writes, "would certainly prohibit maximizing profit as a guiding principle of society and politics" (p. 197). The dominant market-driven mentality isolates and separates us, feeding our narcissism and turning everything into a competition.

> But if the critical mass of people has understood in their depth that individual welfare is impossible without common welfare . . . then structures [political and economic, he means] will change. (p. 198)

If we could train people to have such experiences, it might be possible to create an enlightened nation, city, or at least significant groups of people. But in the objective world we inhabit, the 'profit motive' is not an abstract idea we can banish by fiat. It incarnates a ruthless system of global capitalism, which undergirds and controls our politics, the wars we constantly prosecute, and the things we consume, think, love, and crave—in short, our world. These are not promising circumstances for promoting a revolution of spiritual consciousness that could kickstart a new age of enlightenment—an outcome that Walach holds is empirically conceivable and necessary for our survival.

How to create a science of secular spirituality potent enough to neutralize the evils wrought from our material sciences and ideologies? Walach's entire project favors one thing: education. He wonders why have we not invested billions of dollars in a national department of peace instead of the trillions we lavish on 'defense', 'security', and military technology? The only realistic prospect is to train and educate human beings to undergo transformative, spiritual experiences—but without coercion or dogma.

There is also this big but unfortunately utopian idea. Science, according to Walach, ought not to operate in a value vacuum. We need a paradigm of science that is oriented toward serving the common good—not the militaristic needs of empire or the profit-driven goals of corporate capitalism. Evidence suggests that we have yet to realize our potential as a species; but to bring it about we cannot rely on the dream of some messiah saving us but on a willed collective process of human self-education. The whole idea of salvation needs to be scrapped and in its place erected the ideal of self-knowledge and self-mastery. I call this idea utopian but at least possible and worthy of admiration.

Is there another way this miracle of enlightenment might come about? The literature of so-called "crisis apparitions", the "third man" phenomenon, deathbed visions, and near-death experiences offer hints. It is conceivable that as we disintegrate from climate change, economic collapse, revolution,

crime, war, and lawlessness, a spontaneous transformation might occur: a kind of global near-death experience. It may appear as a collective response to the collective cry of humanity in extremis. A nature-spawned apocalypse may await us: A moment may arrive when it dawns on human consciousness that the game is up, the old options are off the table, and something completely different is necessary.

The book ends on a note of paradox. The scientific quest has taken us far but not far enough. A more well-rounded enlightenment requires that we free ourselves "from the monopoly of a certain kind of rationality and doctrine, even from the monopoly of the rationality that has arisen out of the enlightenment itself" (p. 206). The paradox applies also to getting rid of the monopoly of religious dogmatism by returning to the "fountain" of immediate spiritual consciousness that is the root of all religions.

Michael Grosso

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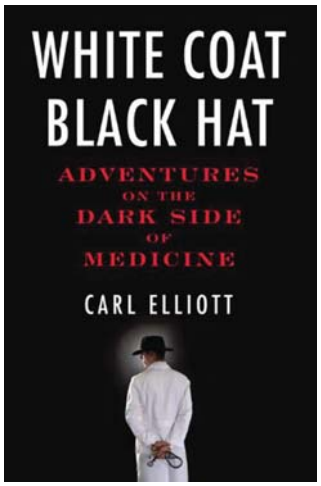
BOOK REVIEW

White Coat, Black Hat: Adventures on the Dark Side of Medicine by Carl Elliott. Boston: Beacon Press, 2010. xvi + 211 pp. $24.95 (hardcover), $16 (paperback). ISBN 978-0807061442.

A host of books describes and analyzes what's wrong with current medical practices (Bauer 2014). This one mentions a few important points I had not seen emphasized in other places.

The author is a bioethicist, M.D. and Ph.D., professor of pediatrics and philosophy at the Center for Bioethics, University of Minnesota. Three chapters of the book deal with matters much written about elsewhere: Chapter 2, The Ghosts—drug-company–written material published as though coming from independent and authoritative sources; Chapter 3, The Detail Men, the drug salespeople who visit doctors and hospitals dispensing goodies and biased information; Chapter 4, Thought Leaders, the doctors and researchers co-opted (and handsomely remunerated) by drug companies to advertise their wares as though they were independent authorities.

Chapter 5, The Flacks, is about the medical insiders recruited to help design strategies for stealth marketing: selling while appearing not to be selling. I had not come across this informative distinction between advertising and public relations (PR) elsewhere. Advertising is plain to the eye, under the imprint of the company selling the product. PR, by contrast, seeks to create an environment in which the company's drug seems eminently desirable. Many volumes have described how the pharmaceutical industry invents and sells *diseases* as a way to sell drugs (e.g., Moynihan & Cassels 2005). Rare conditions are named and made to appear common but also serious; thus heartburn is replaced by gastroesophageal reflux *disease*; urge incontinence is not common, but as "overactive bladder" almost anyone could imagine that they might have it. The diseases are stealth-sold through "Public Service Announcements" (PSAs) provided by drug companies to television and radio, which air them without charge under the illusion that this actually is a public service. Similar but more elaborate are the video news releases (VNRs) that look like news clips but bear messages that serve industry interest; for example, a conversation among academic experts about the benefits of giving up smoking—funded by GlaxoSmithKline who sells the smoking-cessation drug Zyban (pp. 112–113). Again, many TV stations air VNRs without charge.



Chapter 1, The Guinea Pigs, reveals that some people have made a profession of participating in Stage 1 clinical trials, which test for safety of new drugs. Large fees for each person enrolled are paid to those who conduct these trials, enabling them to hire professional guinea pigs, who may also be given free room and board during a trial. This profession is particularly attractive to the homeless and to undocumented immigrants. When I mentioned this recently in a graduate seminar, it turned out that one of the students had actually worked for a corporation that conducts such trials, and she mentioned some of the corollaries, for instance that the professionals may try to enroll in more than one trial at the same time, interfering with the ascribing of possible side effects to a particular drug. Elliott mentions also that the professional guinea pigs are likely to be much healthier than the patients who will later be administered the drug, so that adverse "side" effects are less likely to show up in the safety trials. Moreover, those who conduct the trials are naturally eager to enroll people who rarely suffer adverse reactions, bringing into being a class of elite trial subjects who can attract higher payments. I had earlier been unaware of this profession, which adds to the numerous other ways in which clinical trials are routinely biased in favor of a drug.

Chapter 6, The Ethicists, is also fresh information as insider Elliott reveals conflicts of interest that beset his own profession. For example, he cites one bioethicist who consulted with Pfizer over how to market Viagra without appearing to market sex—the consultant was making money but hardly practicing bioethics.

When human beings are subjects in clinical trials, federal regulations require that the protocols be vetted by Institutional Review Boards (IRBs). I found surprising as well as reprehensible that there exist for-profit IRBs, and that such institutions as Johns Hopkins and the National Cancer Institute actually outsource their reviews of protocols to such outfits. The conflicts of interest are quite clear. A commercial IRB gets clients when it becomes known for finding ways to approve trials; and the client sheds responsibility and gets plausible deniability: If anything later goes wrong, the blame can be laid on the IRB. At the recent seminar, an actual example was given: An IRB turned down a proposed protocol, whereupon a different IRB was hired that managed to give the protocol its approval.

Warning: Reading this book may make you sick to your stomach.

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BOOK REVIEW

Experimentelle Parapsychologie: Eine Einführung [Experimental Parapsychology: An Introduction] by Stefan Schmidt. Würzburg: Ergon, 2014. 159 pp. € 24.00 ISBN 978-3-95650-079-4.

It might be a little bit unusual for a reviewer to have (almost) nothing to criticize about a book, and not to list, at least, minor errors, because a lot of critics see their main function in doing so (the more so as this might point to the 'real expert' in the field).

It has been a long time since the last German overview of the actual state of parapsychological research has been published with the intention of informing a broader audience, beyond specific scientific and academic communities. The first one of this kind was written by biologist and philosopher Hans Driesch (*Parapsychologie*, published in 1932), followed by Hans Bender's *Unser sechster Sinn* (1971), and Walter von Lucadou's *Psyche und Chaos. Theorien der Parapsychologie* (1995). Hence, the book under review, *Experimentelle Parapsychologie: Eine Einführung* by Stefan Schmidt, continues a small, but long and important, line of tradition.

The author might not quite be unknown to the readership of the *JSE* because he has been doing experimental parapsychological research since the mid-1990s, and has published several articles and books relevant to parapsychology (see http://prof-stefan-schmidt.info/publikationen). He is a member of the Parapsychological Association, and the Society for Psychical Research, among others. In 2001, Schmidt received the Gertrud Schmeidler Award for Outstanding Student Contribution to Parapsychology, and in 2004 he chaired the 47th Annual Convention of the Parapsychological Association in Vienna. Since 2010, he has been assistant professor at the Institute for Transcultural Health Studies of the European University Viadrina in Frankfurt a.O. (Germany), and, since 2012, head of the Academic Section for the Evaluation of Complementary Medicine, as well as of the Center for Meditation, Mindfulness, and Neuroscience Research at the University Medical Center Freiburg (Germany).

The book *Experimentelle Parapsychologie* is based on his Ph.D. dissertation (Schmidt 2002), as well as several articles the author wrote for journals and edited volumes during the last few years (e.g., Schmidt 2015a,b). It appeared as Volume 11 of the monograph series *Grenzüberschreitungen: Beiträge zur wissenschaftlichen Erforschung außergewöhnlicher Erfahr-*

ungen und Phänomene [Crossing Frontiers: Contributions to the Scientific Exploration of Exceptional Experiences and Phenomena], edited by Eberhard Bauer and Michael Schetsche on behalf of the Institute for Frontier Areas of Psychology and Mental Health (IGPP) in Freiburg, Germany, and consists of 14 chapters. To begin with an overview of the contents: The first four chapters include a short Introduction, an assessment of the scientific classification of parapsychology (Chapter 2), a brief history of experimental parapsychology (Chapter 3), and methodological basics and particularities (Chapter 4). Chapters 5 through 11 are dedicated to the description of relevant research paradigms, including the respective state of research (results, controversies, etc.). Chapter 12 provides a summary and a short discussion of some specific problems of psi research in general. Chapter 13 describes some issues of process-oriented parapsychological research. The last chapter introduces the most important theoretical models for the explanation of psi effects.

Schmidt interprets his role as a scientist in the sense of an explorer who is keen to explore unknown territory at the frontiers of knowledge, and recommends—as an apt attitude for doing so—a skeptical mind as well as a willingness to tolerate inconsistencies and contradictions (p. 10). He points out that this represents a major challenge for many scientists which leads to a situation in which a researcher in the field of parapsychology runs the risk of disqualification for practicing pseudoscience. Although parapsychological research has existed for 120 years, the situation has not changed much in this regard. This is a well-known fact to most readers of the *JSE*. The crucial point is on the one hand that the subject matter of this research consists of anomalies that seem to contradict our current scientific understanding of the world, and, on the other, that the field of parapsychology lacks a clear definition of the phenomena as well as a generally accepted theoretical model. However, these obstacles didn't prevent parapsychological researchers from doing their homework; they addressed relevant methodological issues in a particularly creative and sophisticated way. Schmidt mentions this point in his historical and methodological chapters. He emphasizes the role of meta-analyses (and the closely related publication bias) for the assessment of the probability and effect size of psi effects, in order to deal with the well-known replication problem.

Even though the nature of the phenomena remains puzzling, it is possible to categorize them. In most cases, such categorizations include theoretical presuppositions and reflect the individual approach of the person who structures the field. It is therefore not surprising that the subcategories of the field can vary quite considerably in different books on parapsychology.

A main problem lies in the often existing possibility that one and the same phenomenon can be interpreted as clairvoyance, psychokinesis, or precognition. Schmidt circumvents this difficulty by structuring the field with regard to established experimental paradigms, although he titled some of his chapters with commonly used psi terminology. In Chapter 5, Schmidt describes the *Ganzfeld* technique, presents the results, and discusses five meta-analyses that have been carried out so far. He points toward the problem that the method of meta-analysis, contrary to the initial hope, does not provide unambiguous results because many small decisions have to be made during the procedure. Chapter 6 is dedicated to the *remote viewing* research paradigm which has some similarities to the Ganzfeld technique but relies almost exclusively on gifted subjects. The following chapter introduces experiments that are commonly subsumed under the label *precognition experiments* (forced-choice, free-response). Schmidt focusses on three important versions: the card-guessing studies (with Zener cards) invented by J. B. Rhine, the presentiment studies introduced by Dean Radin, and the retroactive experiments developed by Daryl Bem. In this context, he refers to the controversy that has been provoked by the results, and mainly by the fact that they have been published in the highly respected *Journal of Personality and Social Psychology*. Chapter 8 addresses experiments with *dream telepathy*, and especially the so-called Maimonides experiments (conducted at the Maimonides Medical Center in Brooklyn during the years 1966–1972). In contrast to this rather short overview, the two following chapters on *remote staring detection* and *DMILS* (*direct mental interaction of living systems*) experiments are relatively comprehensive. This is hardly surprising, since Schmidt and his research group themselves have conducted studies with these techniques. However, the author uses his own experience with the staring detection paradigm to discuss exemplary particular methodological aspects as well as the permanent development and improvement of experimental designs. In the DMILS chapter, he outlines the short history of this research paradigm, which is characterized by the fact that paranormal interaction is measured by physiological and behavior-related data. He presents three forms of experiments: EDA-DMILS, Remote Staring, and Attention Focusing Facilitation Experiment (AFFE), and discusses the results of three meta-analyses in this field. The last chapter on experimental paradigms is dedicated to *psychokinesis*, starting with the early tossing dice experiment series at Duke University, followed by two paragraphs on micro-PK using random event generators (REGs). The author then presents the results of meta-analyses, and points finally to the *Global Consciousness Project*.

Chapter 12 provides a synopsis of the (impressive) results of the

reviewed meta-analyses in different fields of experimental parapsychological research, and considers the implications of these small effect sizes as well as the replication problem.

While Chapters 5–12 are mainly proof-orientated (does psi exist?), Chapter 13 deals with issues of process-orientated parapsychological research considering moderating variables. Schmidt selects two psychological (personality traits, experimenter effect) and two physical (sidereal time, fluctuations of the geomagnetic field) variables that can be offered as possible candidates. Finally, he discusses different attempts to explain the well-known decline effect.

The last chapter of the book provides a useful supplement to the empirical part: an overview of theories of parapsychology. Although many such theories exist (cf Stokes 1987, 1997), they can be categorized into several groups. The author focuses on three threads of theories: (1) theories of unconscious psi perception (PMIR, DAT, FST), (2) theories analagous to quantum mechanical processes (MPI, GQT), and (3) a theory of the restoration of time symmetry (CIRTS). He succeeds in summarizing these theories in an understandable manner in a few pages.

This method of presentation applies to the whole book. Schmidt writes about complicated issues in a clear and didactically sophisticated way. The book is well-balanced and gives an excellent overview of the current state of experimental parapsychological research. The author does not ignore weak points and problems, and he remains cautious in his conclusions. However, he points decidedly to the undeniable anomalies: "In the data of parapsychological experiments irregularities can be found which could not be explained by chance; little is known about the nature of these irregularities" (p. 103, translation by G. M.).

The only thing I have to complain about is a small bias in weighting the individual chapters. The chapter on dream telepathy, for example, only consists of three pages compared with the 16-page chapter on staring detection. Some of the given references are not the most up-to-date (however, this does not apply to the presentation of results). In short, I would have wished for another 20 pages, and it would still have remained a short and easily readable book. And moreover, I would wish for an English translation because this well-written, well-informed, and concise introduction to the field of experimental parapsychology deserves a larger readership.

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BOOK REVIEW

Memoir of a Trance Therapist: Hypnosis and the Evocation of Human Potentials by Adam Crabtree. Friesen Press, 2014. 189 pp. $24.99. ISBN 978-1-4602-5515-5 (hardcover), 978-1-4602-5516-2 (paperback), 978-1-4602-5516-2 (eBook).

Dr. Crabtree's *Memoir of a Trance Therapist* is not a memoir per se, but an explication of his theoretical explorations over the years. It begins with his theory of hypnosis, how it evolved and its implications. He argues that an understanding of hypnosis is yet to be achieved by present-day researchers and theorists. His story is personal and human, letting us know how and why his thinking has developed as it has. He concludes that trance is the experiential foundation for all experience. This conclusion seeds his exploration of other human phenomena—how humans evolve and develop extraordinary abilities, how groups and culture influence individuals, and how individuals can intuit non-sensory knowledge and display paranormal abilities. He provides a historical, philosophical analysis of the human potential movement. To my surprise it began with Schopenhauer. Finally, relying on the philosophy of Charles Sanders Pierce, he argues that we are all immersed in evolutionary love, a universal process that leads us, our actions, and our world closer to perfection or God. I found it engaging for the most part, thought-provoking in the main, but lacking rigor. Some of what he proposes takes courage to state professionally and publicly, for example discussing clairvoyance, knowing outside the senses, and influencing events.

Dr. Crabtree was a Benedictine monk and Catholic priest, studied philosophy at the University of Toronto, went on to earn a doctorate, and has been a practicing clinician ever since. Over the years he has become a well-known scholar and written books about dissociation, hypnosis, and the history of hypnosis. He has been a participant and leader in the human potential movement, centered at Esalen, Big Sur, California. It is clear that his background as a monk, psychotherapist, philosopher, hypnotherapist, and historian and scholar all come to bear in this interesting and thought-provoking book.

Yet, in spite of his impressive credentials, I question some of his conclusions. Even though I frequently disagree with him, he engages me and stimulates my thinking and creativity. And, despite these questions,

they have led me to seriously consider the issues he raises. In this review, I hope to detail some of those questions in the hopes that they will stimulate a revision.

The first and most critical issue has to do with "trance," the theoretical term he uses to designate the state of hypnosis. What is most interesting about his choice of this term is his bypassing the mind–body problem. The solution in psychological and behavioral science is to define a mental phenomenon in terms of its measures. Dr. Crabtree never addresses this issue—an issue of which he must be well aware. From a philosophical perspective, he is resting his whole theory on a specific mental state called "trance." The elegance of this term is its historical and current use. "We" all seem to know what that is and this makes communicating about it easy. As well, given its historical usage, its meaning should have some important role in understanding hypnosis.

What seems problematic to me in Dr. Crabtree's approach is how he defines "trance" and then asserts that it is the basic experiential structure for all human experience. This is particularly important for his theory since it is the foundation on which he builds all of his later conclusions. If this seminal idea has no foundation, then it undermines what follows.

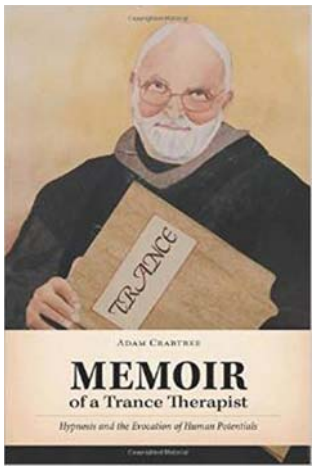
Dr. Crabtree's explanation rests on what he considers to happen during hypnosis: The subject focuses on an object (inside or outside) and the fringe disappears. The more intense the focus, the deeper the trance. Whenever there is focus, there is trance. Since all experience, even the everyday, involves focus, we are, according to the author, always in a trance, though he emphasizes not necessarily a hypnotic trance.

As I have tried to understand why he theorized in this way, it seems to me that he is attempting to show that hypnosis is not discrete from normal experience. Since that is the case, he might argue, there must be a reasonable connection between trance and everyday experience. In fact, he writes that all trance phenomena can happen when a person is not in trance—a fact with which I concur. As a result, he puts trance on a continuum as a function of the intensity of focus on an object—the more exclusive, the more hypnotic. As a result, he now can explain why extraordinary hypnotic experiences (amnesia, hallucinations, self-healing) can also occur during everyday experiences. His theory, therefore, considers all hypnotic phenomena while also explaining how those occur in normal "waking" states.

I would point out, however, that human experience is not always continuous. Sleep and dreaming, for example, are discrete states that are different from everyday, waking experience. I make this observation to point out that hypnosis, as a state of mind, could reasonably be considered discrete from everyday experience, and, as a result, does not need to be

placed on a continuum with it. To continue this line of thought, Dr. Crabtree even refers to hypnosis as a kind of sleep. On the other hand, to critique what I just wrote, such an observation does not consider light "trance", which is close to everyday experience. Clearly, the author is struggling with a complex and confounding mass of facts about hypnosis and attempting to integrate them under a single conceptual umbrella. This is a worthy endeavor, but I think his solution fails.

I detailed elsewhere (Beere 2012) some of my rationale for why "trance," as defined by Crabtree, is not a viable construct to explain hypnotic phenomena. From my perspective, his *Memoir* clarifies in large measure why he has done this and how he applies it conceptually. Many readers might find his thinking useful, instructive, and thought-provoking. On the other hand, there seem to be some additional complications. And if those complications find support, then they undermine the validity of the theory. According to Dr. Crabtree, trance is the result of intense focus. In my clinical experience, individuals who are very logical and focused intently on the hypnotic procedure are difficult to hypnotize. In other words, their intense focus interferes with trance. From a different point of view, my experience of working with a hypnotized client does not involve their having an intense focus. Rather, the client needs to be able to follow my lead, my suggestions, which shift their focus—listening to my voice, noticing their breath, discovering relaxation slowly beginning somewhere, awareness of thinking . . . and so on. The client's awareness is not tightly focused but being led by me and shifting from "object to object." My understanding of what happens with a client does not entail an intense focus but rather an easily led focus that is open to suggestion. This is not an intense but an absorbed focus. Adding yet another difficulty with intense focus on an object, my clinical experience has demonstrated that problem-solving, or, using Dr. Crabtree's terminology, the elicitation of subliminal resources, occurs in states of inner receptivity or openness, not in intense focus. I would argue as well that intense focus interferes with the client's ability to allow, to know, or to activate these subliminal resources. My final difficulty with intense focus has to do with the inherent limitations most individuals have in their everyday lives. If, as Dr. Crabtree asserts, intense focus automatically elicits subliminal resources linked to the object of focus, everyone should be spontaneously accessing what they need in order to change, and, as a result, changing or getting better. They do not. In fact, this is exactly why individuals go to therapy. Furthermore, if individuals responded as Dr. Crabtree suggests, hypnosis should never be needed. Simply focusing intently on any problem should automatically evoke the subliminal resources required. And individuals should spontaneously change all the



time. I believe Dr. Crabtree's theory cannot explain these situations and, thus, needs to be revised.

Continuing my critique, Dr. Crabtree ignores two other spheres of human experience in which hypnosis-like phenomena can occur: meditation and dissociation. What is unique about these two different experiences is the circumstances evoking them. Meditation is generated solely by the individual. Dissociation occurs spontaneously, often in terrorizing circumstances. Hypnosis results from the actions of a hypnotist. What, one may ask, is the commonality cutting across all these experiences? I have no answer to this question but I would assert it is not an intense focus on an object. In this regard, consider meditation.

There is a spectrum of meditative practices ranging from intense focus on a single object to remaining aware of how attention shifts, moment to moment, from object to object. There is, as well, analytical meditation, which requires the meditator to continually analyze, in the same fashion, what arises in mind or what arises as the result of the prior analysis. The variety of these practices does not match the intense focus on an object, though it might involve maintaining a particular kind of attention or awareness.

I developed a theory of dissociation (Beere 1995) based on what happens perceptually during dissociation, namely that the perceptual background is blocked out. The background comprises perceptual constants: "I," mind, body, world, and time. The specific dissociative experience is linked to what aspect of the background is blocked out. A summary of the empirical research to support the theory can be found in Beere (2009). Clearly, my term "background" is almost identical to what Crabtree calls "the fringe." As well, my theory posits that the background is blocked out when perception focuses narrowly and exclusively on something. I have conjectured, as well, that there is a not-yet-determined connection between dissociation and hypnosis. My theory of dissociation uses some of the same concepts that Dr. Crabtree uses in his theory of hypnosis. Why then do I disagree with what he proposes?

From my perspective, loss of background is an extreme and unusual occurrence. The object of focus must be of determining significance, for example life-threatening. It is only under these circumstances that dissociation occurs. To rephrase this observation, loss of background does

not occur during the everyday focusing of attention, even if that is intense and undistracted such as during the creation of a wooden bed. In doing the wood work, thinking about the project and so on, the background remains intact, framing, in a larger context, the construction of the bed. To actually block out the background (or, using Dr. Crabtree's term, fringe) requires a situation of powerful significance that "pulls" attention to focus exclusively on it.

I would like now to address "subliminal resources," which, according to Crabtree, lead to and explain all hypnotic phenomena.

> . . . . Trances automatically evoke in the entranced person hidden resources appropriate to the object of focus of the trance. This evocation is, in my view, infallible—it always occurs when we engage with the world. (p. 111)

From my point of view, Dr. Crabtree is correct that trance makes previously unavailable inner resources available. However, the clinical evidence I have seen does not convince me those resources are "automatically evoked." More pointedly, during hypnotherapy, once a life problem is brought to the client's attention, spontaneous solutions and change do not occur. It is only after suggestions are made by the hypnotherapist that change begins to occur. What is notable here is the necessity of the hypnotherapist to help evoke those resources—resources, I would note, always available to the client but not previously accessed. As a consequence of these observations, I find problematic Dr. Crabtree's thinking about how subliminal resources are elicited.

There is an intriguing chapter on "Asking" that addresses the difficulties I have detailed in the previous paragraph. He discusses the Hawaiian Huna system via an interview with someone who uses this approach. The system explains how to access not only inner resources but also to engage a response from the universe. This is something I have attempted to understand in my own life, given that I have had numerous such experiences. I explored this in another publication (Beere 1997). Dr. Crabtree does not directly connect this chapter on "Asking" with his overriding project of explaining hypnosis. Yet the Huna system provides a method allowing an individual to access information from and have influence beyond the boundaries of the body. From another perspective, however, given Dr. Crabtree's assertions that subliminal resources are automatically evoked when one focuses on an object, there should be no need for the Huna system. I had hoped that Dr. Crabtree would juxtapose his earlier assertions about the automatic evocation of subliminal resources with the process of asking, which yields a result only after certain conditions have been met. This is an unaddressed contradiction in his book.

Dr. Crabtree has a fascinating explanation for why suggestibility is enhanced in trance. He theorizes that the distinction between inside and outside merges in trance—or to put it differently the sense of self is no longer so contained. The hypnotist is incorporated into the sense of self, and thus suggestions are readily experienced as coming from oneself. Even though I am not sure about this, the explanation intrigues me.

I found logical problems in Dr. Crabtree's discussion of group-mind. Here are some quotations.

> . . . people and groups can be considered living persons . . . (p. 71)

> . . . the group-mind influence is also conveyed along another, less obvious, pathway, with a more effective access to the member's inner life: the pathway of unconscious communication. Here the group's inner mind operates directly on the inner mind of the member. (p. 77)

Dr. Crabtree seems to suggest that a group exists as if a living person and has an inner mind. Furthermore, that inner mind communicates with the inner mind of group members. I have taught group dynamics and led many groups so I know what Dr. Crabtree is referring to in terms of group pressure and group-defined patterns of thought, belief, and behavior. I do not discount the phenomena: I question the mechanism he attributes to those group phenomena. To make that phenomenon into an entity equivalent to a human founders logically. How many people does it take to develop a group-mind? Does the group-mind continue to exist when individuals leave the group? Does it communicate to individuals who are physically distant from that group and uninvolved in what is occurring at the time? Does the group-mind persist once the group has dissolved? Where are both the inner mind and the group-mind located? Since group-mind is also "historical," does it extend into the past? Does a past group-mind persist in its existence into the present? Dr. Crabtree does not address these questions. From my point of view, the chapter on group-mind was not supportable. And, now, despite my questions, I need to acknowledge that Dr. Crabtree supported his position about group-mind and culture with his discussion of Charles Sanders Pierce and other philosophers who consider groups and culture to have minds.

Having been a participant in the human potential movement, I found his historical perspective fascinating. I had no awareness that the genesis of these ideas linked back to Schopenhauer, Myers, and others. In the larger context of hypnosis, the belief that we all have untapped and undeveloped potentials fits precisely with his theory. Hypnosis, or "trance," from Dr. Crabtree's point of view, is one such avenue to develop human potential.

He argues further that given his belief that everyday experience is trance, human beings are always having their untapped potentials elicited. This leads to the natural and positive evolution of the human race.

In various sections of the book, Dr. Crabtree overtly states that we can know the thing-in-itself, a reference to Kant, who states that we never have access to it. Also in various places in the book, Dr. Crabtree explores the thinking of post-Kantian philosophers who disagreed with Kant. This supports his assertions of such direct knowing. And, more interestingly, he argues that this knowing does not use the senses. There can be direct intuitive knowing.

Despite my critiques, there is something sweet and optimistic at the heart of this book. Dr. Crabtree deeply believes in the goodness and potential of human beings. He sees the universe as inherently evolving in better and better, ever more loving ways.

This is a book I recommend.

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FURTHER BOOK OF NOTE

The Placebo: A Reader edited by Franklin G. Miller, Luana Colloca, Robert A. Crouch, and Ted J. Kaptchuk. Baltimore: Johns Hopkins University Press, 2013. 327 pp. $49.95 (paperback). ISBN 978-1-4214-0866-8.

This volume illustrates the serious interest that mainstream medicine has begun to take in the placebo effect; two of the editors are at the National Institutes of Health and one of the others heads a Program in Placebo Studies and the Therapeutic Encounter (at Harvard Medical School).

Placebo should also be of high interest for Scientific Explorers, because it is so directly involved with questions of consciousness and because it is also highly mysterious, perhaps as mysterious as some "psychic" phenomena.

This book is a *reader*, a collection of salient articles with minimal introductory material.

Part I, on the concept and significance of placebo, includes the seminal paper of 1955 by Henry Beecher in *JAMA* that is credited with making placebo "a central feature of general medical knowledge." But it was an earlier (1950) paper by S. Wolf that made the remarkable claim that placebo could "include objective changes at the end organ which may exceed those attributable to potent pharmacological action."

Part II surveys experimental studies. It remains an open question, how universally placebo is experienced. Some observations show that conditioning in animals can simulate placebo effects. With humans, mere suggestion and expectancy work—and placebo can modify (add to or subtract from) drug effects.

Placebo can be effective even when patients are told that placebo is being administered—perhaps because they don't believe it? In some studies, placebo produces similar brain changes as do opioids; perhaps placebo involves release of endogenous opioids. Other studies have reported activation of the hormone cholecystokinin and the neurotransmitter (and hormone) dopamine.

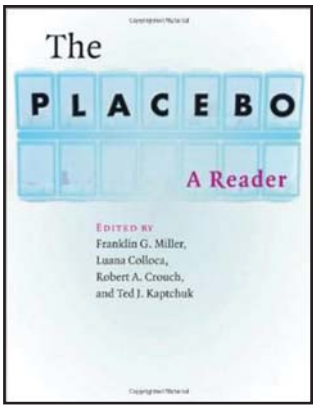
One knotty problem in experimental studies: how to compare placebo with "no treatment"? How to enroll and monitor in a study a control group that doesn't know it's a control group?

Part III considers ethical issues related to placebo. Since placebo is

often at least as effective (statistically) as other treatments (drugs or psychotherapy), is it ethical for doctors to use placebo and in doing so deceive their patients? Is it ethical to conduct clinical trials with placebo controls?

Research on placebo frequently mandates some degree of deception and transgression of the usual requirement for informed consent.

Trying to understand placebo offers challenges akin in some ways to the challenges of trying to make sense of scientific anomalies, most directly psychic phenomena.



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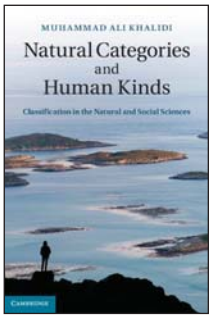
FURTHER BOOK OF NOTE

Natural Categories and Human Kinds: Classification in the Natural and Social Sciences by Muhammad Ali Khalidi. Cambridge: Cambridge University Press, 2013. 250 pp. + xvi. $94.99 (hardcover). ISBN 978-1-10701-274-5.



How do—or how *should*—we parse the world into *kinds* of things? Going back at least to Plato, most philosophers have done so with respect to some notion or other of *natural kinds*. And many analyses of natural kinds have been essentialistic—that is defining those kinds with respect to universals, or some set of intrinsic properties, or necessary and sufficient conditions. And there's a long-standing dispute between thinkers who regard scientific categories as natural kinds with essential properties fixed by nature—those that "cut nature at its joints"—and thinkers who maintain that our classifications and categories have no essence and instead merely reflect human interests and values. A typical example of the former would be "having a mass of $1.7 \times 10{-}27$," and examples of the latter would be the categories of "ADHD," "race," or "child abuse."

Khalidi aims for an epistemic, naturalistic, non-essentialist account of natural kinds, one which comfortably embraces not only the usual candidates favored by essentialists (e.g., elementary particles, chemical elements, biological species), but also categories in the social and behavioral sciences. Drawing on cases from many scientific fields, from fluid mechanics and polymer science to virology and psychiatry, Khalidi argues that "natural kinds *are* investigative or epistemic kinds, in the sense that they are the categories revealed by our systematic attempts to gain knowledge of nature" (p. 43). Moreover, he claims that natural kinds can be "fuzzy" (i.e. have indefinite boundaries), satisfy epistemic virtues to varying degrees, and be mind-dependent in a way that doesn't detract from their reality or objectivity.

Although the book is pitched for a sophisticated and philosophically informed audience (and, needless to say, too complex to be adequately summarized in a brief notice such as this), it's clearly written, nuanced, compellingly argued, and worth the effort for *JSE* readers curious about the unavoidable metaphysical dimensions of doing science of any kind.

Stephen E. Braude

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