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EDITORIAL

In 2010, I wrote a pair of editorials dealing with issues concerning peer review and the quality of papers appearing in the *JSE*. While I'm not so naïve as to think that my editorials exert any great influence (or even that *JSE* subscribers actually read them), I'm nevertheless a bit surprised to find—five years later—that I still receive a fairly steady stream of complaints about our peer review process. Those complaints fall primarily into two broad categories: (1) charges of rigidity, bias, or tyrannical censorship from authors whose papers were rejected, and (2) complaints from readers who believe that papers appearing in the *JSE* should never have survived peer review.

So I'm thinking it's time to review the issues again. And since I don't believe I can substantially improve on what I wrote five years ago, I offer below, in a spirit of unjustified optimism, my two earlier editorials for your (re)consideration.

From *Journal of Scientific Exploration*, *24*(3):393–396, 2010:

There's been a lot of chatter lately on Internet discussion groups to which I subscribe about the virtues (but mostly about the vices) of journal peer review. In substance, the commentary adds little to the ever-growing number of published or online discussions of that subject. And not surprisingly, it resembles the correspondence I receive from authors whose papers have been rejected by the *JSE*. Typically, the negative comments are predictable and familiar complaints about how editors and reviewers tyrannically impose their prejudices on authors who express dissenting or minority opinions, or—even worse—who argue for novel (if not radical) points of view. In this way, we're told, journals reinforce the status quo and keep worthwhile scientific or conceptual innovation at bay.

Of course, censorship of this kind undoubtedly occurs, and some of the incidents recounted in listserves and published articles are horrific and infuriating. But these practices are also nothing new, and I wonder whether it wouldn't help to step back a bit, strive for some perspective, and in particular see if we can find some helpful analogies to the situation regarding peer review. It seems to me that peer review doesn't deserve the battering it often receives.

Ever since Plato's *Republic*, a standard criticism of democracy has been that at best it's inefficient, and at worst it puts important decisions in the hands of people who lack the competence to make those judgments. However, an equally standard rejoinder is, first, that there's no such thing as absolute competence to rule; even equally intelligent and informed people can reasonably disagree. Moreover, the alternatives to democracy are worse in crucial respects. G. B. Shaw once remarked, "Democracy substitutes election by the incompetent many for appointment by the corrupt few." What many want to say about democracy is that non-democratic

systems are inherently brittle, in the sense that a challenge to the ruling authority is (in effect) a challenge to the political system itself, and thus it can undermine the whole political structure. By contrast, democracies are inherently (if inefficiently) self-correcting. Leaders and their policies can be challenged and replaced without having to question or overturn the very system in which they have a place.

Perhaps an analogous series of arguments and counter-arguments can be made about peer review. Is it fallible and vulnerable to abuse? Of course. Can editors and reviewers behave badly or merely exercise poor judgment? Of course. In fact, nothing can be used exclusively for the good, and humans seem to have an inexhaustible supply of disappointing behaviors.

But, as in a democracy, peer review allows for checks and balances, and avenues for appeal. The review process is flexible and potentially self-correcting, and so hasty judgment or instances of outright abuse don't undermine the process itself. Naturally—in fact, clearly—some journals are more editorially myopic, unscrupulous, or cowardly than others. But I can assure our readers that *JSE* editors and reviewers take their responsibility and their commitment to openmindedness very seriously. That's never a guarantee that our biases don't sometimes cloud our judgment, and in fact it's impossible to assess a submitted paper from no point of view whatever. But I can tell you that at the *JSE*, we're particularly alert to this, and in fact rejected papers *are* sometimes reappraised (usually by different readers) and then accepted. Indeed, we recognize that this sort of flexibility is essential in a journal devoted to controversial topics outside the mainstream. But let's not stop with examples from political theory.

In my noble quest for analogies, the following episode from the history of philosophy also occurred to me. In his *Principles of Nature and Grace*, Leibniz famously (though some say, insincerely) claimed that this is the best of all possible worlds. Now as students of modern philosophy know, that claim isn't as optimistic as it sounds. It's rather like saying: If you think this world is bad, you should consider the alternatives. For the case at hand, it's like saying, if you think a world with Steve Braude as *JSE* Editor-in-Chief is bad, imagine it instead with [and then fill in the blank with your favorite tyrant—unless, of course, that would be me].

In fact, Leibniz seemed to think that in the best possible world, some evil is actually inevitable. For Leibniz, the best possible world was one that contained the greatest *surplus* of good over evil. Perhaps a world with no evil is not even a possible world. But even if it is possible, Leibniz wouldn't have considered it as good as the actual world, because it wouldn't contain the greatest *surplus* of good over evil. And that's because, according to Leibniz, some of the greatest goods, such as free will, can't even exist in the absence of certain evils; those goods and evils are necessarily connected. (The necessity here would be stronger than mere empirical necessity: It would be metaphysical or logical necessity.)

For a somewhat down-to-earth example of the sort of relationship Leibniz had in mind, consider the good of satisfying one's hunger. Clearly, the hungrier one is, the greater the good of satisfying that hunger. So the great good of feeding the starving can't occur without the evil of their having suffered great privation. Of course, in the case of free will, the issue is that the great good of human freedom must allow both for the freedom to do good as well as evil, or to act reasonably as well as rashly.

Although this might be stretching it, perhaps there's an analogy here with the

journal peer review process. Perhaps the best possible journal would *not* be one in which editorial prejudice never exists or in which editorial misjudgments never occur. In fact, so long as fallible humans have anything to do with the editorial process, it's plausible that an error- or prejudice-free editorial board and journal are *not* possible (at least not empirically possible). So perhaps the best possible journal, editorially speaking, will be one containing the greatest surplus of fair and reasonable editorial decisions. And perhaps the existence of prejudice and poor judgment is a necessary correlate of having humans do the work. If so, complaining about peer review because the process can be unreasonable or unfair would be analogous to complaining about the existence of free will because it allows for evil. Interestingly (and more or less as an aside), Leibniz seemed to think (or at least he claimed) that his position solved the notorious *problem of evil*: the alleged incompatibility of evil with God's existence. (Roughly, the idea behind the problem is that if God is omnipotent, omniscient, and benevolent, He would anticipate and prevent evil from occurring. Hence, since evil exists, it follows that there is not an omnipotent, omniscient, and benevolent God.)

However, according to the Leibnizian view sketched above, the existence of evil did not count against the existence of God. Quite the contrary; from Leibniz's standpoint it was an indication of God's greatness. Evil would simply be an unavoidable side-effect of God's actualizing the best of all possible worlds. However, as Bertrand Russell once observed, Leibniz's reasoning here is less than compelling. One could just as well claim that this is the worst of all possible worlds, created by an evil demon, and that good things exist only to heighten the evils. So one could argue that the evil demon created us with free will in order to ensure the existence of an excess of sin, and that the demon created good people so that there could be the great evil of their suffering.

Now this might really be stretching it, but I suppose that one could argue that some particular journal is the worst of all possible journals (not the *JSE*, of course), in the sense that it maximizes the amount of editorial abuses over editorial good. *JSE* readers will probably be ready with some likely candidates for that honor. And perhaps the existence of such a journal could even be cited as evidence for the existence of an evil publisher or managing editor who created or uses the journal precisely to suppress or deny certain points of view. One obvious nominee comes immediately to *my* mind (and I'll wager to those of many readers).

Ironically, however, when it comes to the journal I have in mind, defenders of its editorial policies and practices actually follow Leibniz's lead and claim that what others consider editorial error or abuse is actually a manifestation of editorial greatness. That is, they would say that it's exactly what journal editors heroically must do in order to protect and promote what they consider (or "know" to be) the truth, and strive to shield unwary readers from the subversive and dangerous influence of irrational or stupid ideas.

So let me be clear; I don't endorse that cynical assessment of editorial rigidity and censorship. Granted, the *JSE* does have an agenda—namely, to give a proper airing to scientific data and theory which more mainstream publications ignore or treat shabbily. But the journal doesn't exist to advance or exclude any particular point of view or set of data. What matters to the *JSE* are *conclusion-independent criteria* of scholarly and scientific integrity. In fact, that's why we often publish papers with which my Associate Editors or I disagree. Still, the next time an irate or disappointed

author complains to me about the negative judgment rendered over a submitted paper, perhaps I shouldn't be averse to giving the more cynical position a try.

From *Journal of Scientific Exploration*, *24*(4):577–580, 2010:

I think I now understand why Gene Fowler once said, "An editor should have a pimp for a brother, so he'd have someone to look up to." That unflattering sentiment about editors isn't nearly as uncommon as I'd thought before taking on the job of *JSE* Editor-in-Chief. And I can see why; people in my position have many opportunities for making others unhappy. In fact, because the *JSE* is such an unusual, cutting-edge publication, those opportunities may be especially plentiful. So although I don't want this to become a recurring theme of my editorials, I feel that a few more remarks on editorial business and peer review wouldn't be out of place.

I mentioned in the last issue that my Associate Editors and I occasionally reappraise papers that were previously rejected. That can happen for various reasons. For example, in the case of complex, technical, or less than ideally clear submissions, reviewers can misinterpret what they've read, and authors are quick to point that out. But sometimes it's because the submission's initial review may have been hasty, superficial, or even prejudicial. Now make no mistake: I trust the folks on my editorial team and I don't believe these infrequent cases reveal anything sinister about them or about the review process. As any teacher knows from grading essays, no matter how scrupulous and fair you try to be, sometimes things just rub you the wrong way, and sometimes (probably more often than we'd like to admit) our critical faculties aren't as sharp as we'd like. These lapses can happen to the best of people, and we try to be alert for them and honest about our fallibility. In fact (as I've mentioned before), we are especially alert for the kinds of negative reactions that can all too easily be elicited by works in areas of frontier science.

However, a number of disappointed authors have proposed to me that we make it a policy to re-evaluate submissions, always allowing the author the opportunity for appeal. I haven't yet decided if I oppose that idea in principle, but I must certainly oppose it for practical reasons. It's simply not something we can afford to do as a matter of course. The main problem is that the *JSE* is a very specialized publication, and relatively few people are both technically competent and sufficiently open-minded to referee papers for it. So our pool of potential reviewers is quite limited, and we often have great trouble finding people qualified and available to evaluate submissions. In fact, the JSE's valiant (and unpaid) Associate Editors and reviewers are overloaded as it is. To routinely re-assess papers we reject just because the authors disagree with the judgment would strain our system (and my team) to the breaking point.

I also receive more than occasional complaints from readers who are outraged that a particular article appeared in the *JSE*'s pages. Sometimes they object to the topic of the paper, and sometimes they complain about the way the topic was handled. I'm frequently puzzled about the former sort of complaint. If the reader has such a strong reaction to a topic (s)he considers too disreputable to be covered in the *JSE,* this would seem to be someone who doesn't quite get what the journal is all about. The latter sort of complaint often displays a different kind of shortcoming—namely,

a failure to understand the nature and function of peer review. For example, last year a reader was moved to write: "I can't believe a paper with such faulty logic could be published in a peer-reviewed scientific journal. Don't you think saying things like this based on their lame evidence is totally nonsensical?" Then, after quoting a remark which out of context looks much more questionable than when read in context, my correspondent asked: "Why doesn't this demonstrate that your guys' peer review is a joke? How in the world can you possibly justify publishing such a shoddy paper?"

Let's ignore for now whether the criticism of the article is justified. In fact, let's suppose it is justified. Even so, the complaint about peer review misses the point by several miles. I don't know o f any journal for which the peer review process is flawless. But more important, peer review never guarantees that only worthy papers and books are published. If that were the case, we'd see far fewer publications across the board. Many journals would go out of business, publishers would probably remainder far fewer books, and many Ph.D. or academic tenure candidates would find their futures jeopardized by painfully skimpy publication lists. And as I mentioned in the previous journal issue, although I don't always concur with the decisions of my Associate Editors and their readers, I'm strongly committed to the view that reasonable and informed people can always disagree. Moreover, the JSE doesn't exist merely to promulgate the views of the Editor-in-Chief or some oligarchic body behind the scenes. Among other things, peer review is supposed to guard against editorial tyranny; but it's never been conceived as a guarantee of quality.

In fact, there's a parallel here with what some have said about inductive reasoning. Unlike deductive reasoning, induction doesn't guarantee true conclusions from true premises, no matter how massive our body of evidence may be. But we needn't lapse into Humean skepticism and insist that induction is rationally indefensible. As Herbert Feigl and Hans Reichenbach noted years ago, even if we agree with Hume that induction can't be rationally justified (as providing guaranteed good results), we can at least vindicate induction. Their general idea was that inductive reasoning is better than—or at least as good as—any alternative method of a posteriori reasoning. So if empirical truth is to be attained at all, induction is as likely as any method to get it for us. From this perspective, induction will disappoint only if we're engaged in a quixotic foundationalist quest for final or absolute justifications.

Analogously, and I think plausibly, one could argue for the vindication of peer review. Given the breathtaking varieties of human fallibility, peer review will never guarantee that only the best works, or even just decent works, get accepted for publication. However, if the evaluation process aims to filter out for publication works that deserve attention, peer review is probably better than—or at least as good as—any alternative method of achieving that result. We'll find it unsatisfactory only if we naively look for a surefire reliable method of assessment.

I must emphasize, however, that I'm confident in my superb and hardworking team of Associate Editors, and I believe we have a very loyal, responsible, and thoughtful stable of referees on whom we can rely. In fact, I'm personally pleased and satisfied with the way the Journal maintains a high standard in accepting papers for publication, even in cases when my opinion differs from that of my Associate Editors or reviewers. No doubt the quality of *JSE* articles is not uniform. I know of no publication for which that's the case, and in fact I think it would be miraculous if it occurred. What matters is that *JSE* articles are regularly (not uniformly) of high quality.

One more observation on this general topic. Because of the *JSE*'s commitment to providing a forum for speculation and data that more mainstream publications tend reflexively to shun, our editorial team often finds itself in a quandary. For instance, we want to be open-minded about airing novel scientific proposals, but quite a few such submissions nevertheless still lack a reasonable amount of theoretical development, empirical grounding, or engagement with competing points of view. Understandably, the less egregious of these sometimes teeter on the border of acceptability, and editorial decisions in such cases are always tough calls to make. That's why in these borderline cases we may invite the authors to resubmit after substantial revision.

—Stephen E. Braude

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RESEARCH ARTICLE

Can Death-Related Dreams Predict Future Deaths? Evidence from a Dream Journal Comprising Nearly 12,000 Dreams

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Abstract—Dreams that appear to predict future events that could not have been anticipated through any known inferential processes have been reported for centuries, and dreams that appear to anticipate the death of an acquaintance or loved one are particularly common. Such reports become more suggestive of genuine precognition if there are no natural cues (such as an illness) to an impending death and if the time interval between the dream and the subsequent death is brief. Most reports are difficult to evaluate because we dream many times each night but typically remember and report only a salient subset of our dreams. Thus we cannot assess whether the time interval between a death-related dream and the death of the dream character is brief or lengthy because we have no control set of non-death–related dreams to which its time interval can be compared. The study reported here provides just such a control set by comparing death-related and non-death–related dreams featuring the same set of dream characters who died after the dreams occurred. These were drawn from the author's own dream journal in which he has recorded his nightly dreams for nearly twenty-five years. The mean time interval between death-related dreams and the person's subsequent death was significantly shorter than the time interval between non-death-related dreams and his or her death, $t(11) = 3.30$, $p = .004$, one-tailed. Cases in which death-related dreams occurred after the characters had died are also considered. Seven of the cases are discussed in detail.

Keywords: death—dreams—psi—precognition

Introduction

Precognitive dreams—dreams that appear to predict future events that could not have been anticipated through any known inferential processes—have been reported cross-culturally and historically for centuries (Myers 1920). Among such dreams, those that appear to anticipate the death of an

acquaintance or loved one are particularly common (Stevenson 1970). Such dreams provide stronger evidence for a psi-mediated or paranormal process of precognition if there are no clues to the impending death—such as an illness, threats of suicide, or participation in a life-threatening activity—and if the time interval between the dream and the subsequent death is relatively brief.

A major problem with most reports of such dreams, however, is that people are most likely to recall and report dreams that are salient precisely because subsequent events make them appear predictive and to forget or fail to report dreams that have no subsequent related events. This is a real-life variant of the "file-drawer" problem in experimental work, in which studies with positive results are more likely to be published than studies with null results, thereby biasing the known database toward false positive conclusions (Scargle 2000). Thus we cannot assess whether the time interval between a death-related dream and the death of the character in the dream is brief or lengthy because we have no control set of non-death–related dreams to which its time interval can be compared. In the absence of such a control set, we are left with the reductio ad absurdum that all death-related dreams about living persons will eventually be predictive of their deaths: All we have to do is wait long enough!

The study reported here provides just such a control set by retrospectively analyzing nearly 12,000 nightly dream scenes from my own journal, in which I have recorded my nightly dreams nearly continuously for twenty-five years (Paquette 2011). For this study, I first used a word search to locate all dreams containing death-related content and to identify the characters in the dream. Next, I located all non-death–related dreams for that same set of dream characters. Finally, I attempted to determine whether the character in each dream was still living or had died. If the character had died, I noted the date of death. For many of the characters, I had already recorded this information in the journal itself, but in several cases I had to check other sources to determine whether the dream character was living or deceased. For this report, I computed for each dream character the mean time interval between death-related dreams and the date of death with the mean time interval between non-death–related dreams. A death-related dream is considered to provide stronger evidence for precognitive anticipation of a subsequent death to the extent that it occurs closer in time to the death than do "control" dreams that are not death-related.

In the psi literature, there are also reports of *post-death* communications, apparitions that occur after the featured character has already died but before the percipient had been made aware of the death through normal non-psi channels (Stevenson 1995). Some of these post-death communications take

the form of what are called "leave-taking" dreams, in which the spirit of a deceased person purports to communicate to the dreamer from within a dream (Barrett 1991/1992). Although I will discuss some of my post-death dreams, most of them were discarded as evidence for a psi-related process because I was aware of the character's death at the time the dream occurred.

Method

The Dream Journal

The dream journal from which the dreams in this study are drawn was begun on September 15, 1989. Ironically, I started it to demonstrate to my wife that I was not having precognitive dreams but that memory errors and faulty reasoning could easily produce the apparent correspondences between dream content and subsequent events. Instead, several persuasive correspondences accumulated (Paquette 2012).

The cutoff date for the study reported here was February 2, 2014. On that date the journal contained reports of 11,779 individual dreams, an average of 3.1 dreams per night. For purposes of this study, all dreams from a single night are combined into a single dream record, yielding 3,732 dated dream records to be searched for death-related content. Each record in the journal had been entered by hand immediately after waking, and notes of events that appeared to confirm or disconfirm points in the dream were added and dated only after the journal entry itself was committed to writing. These were recorded in the margin of the journals on the date of the verification attempt or any followup attempts. Beginning on October 12, 2012, all dreams were recorded on an iPad and emailed to myself to create an independent time-stamped record at a cloud storage network.

Although dream recordkeeping was fairly constant across the 25 years covered by the journal, there is a block of 1,980 days, from February 9, 1993, to July 13, 1998, during which there were only 32 entries. This was due to my life/work situation at the time and was not due to differences in the number or types of dreams I was having during this period. These 32 entries were included in the search for death-related content.

The level of detail in each entry varies widely depending on dream recall and waking circumstances. If I do not recall much, then the entry is brief. On the opposite end of the spectrum, I may remember far more than can be recorded in the time I have before I must go to work or deal with other obligations. When that happens, I will write as much of what I consider to be the most salient information before I run out of time. Some entries run to more than 20 handwritten pages and took as much as three hours to write. The average record word count is 201, with a maximum

value of 3,018. Many entries are accompanied by illustrations, sometimes multiple pages of them. These have not been counted in full but to date 1,019 of these pages have been scanned. In some records, the word count is close to zero because illustrations arranged as a storyboard were used instead of written descriptions.

Identifying Death-Related Dreams

The database of dream records was searched for death-related content using the following 10 search terms: *Dead, Death, Ghost, Spirit, Deceased, Dying, Die, Murder, Kill, Suicide*. Dreams containing any of the search words were defined as death-related even if the words were used symbolically and did not appear to refer to an actual death. For example, one dream contained the following sentence "*I had twenty-four hours before a wrongly decided death sentence was carried out.*" Later that day, the person I identified in my journal had his position terminated at my office (to my genuine surprise): The 'death' in the dream was termination of employment, not an actual death. On the other hand, dream reports were excluded if (a) they included a keyword in an innocuous context, such as in the sentence *"an extremely small chameleon . . . has something like a death grip on my finger*"; (b) appeared in the context of a threat to cause harm "*[Colonel] Klink is convinced that Hogan is out to kill him . . .* "; or (c) the dream characters involved were fictional—as in the example given in (b).

Of the 11,667 individual dreams recorded in the journals, 545 (4.7%) were identified as containing death-related content. To be retained as a death-related dream for the study itself, it also had to pass the following tests:

1. The dream character must be identifiable.
2. The dream either mentions the death of the character, the possibility that the character is dead, or contains details characteristic of deceased characters.
3. It was possible to verify whether the character is living or, if deceased, the date of death.
4. In the case of post-death dreams, I must not have been aware of the character's death when the dream occurred.

After applying these criteria, 87 dreams featuring 50 unique identifiable dream characters remained for analysis. They include both living and deceased dream characters, but this report concerns only the dream characters who are deceased.

Results

Pre-Death Dreams

Of the 50 dream characters identified in the search, 12 of them had died after I had experienced one or more death-related dreams about them. Table 1 displays for each character the mean number of days between death-related dreams and the date of death; the mean number of days between control (non-death–related) dreams and the date of death; and the percentage of total days contributed by the death-dream intervals. For example, for Noah, there was a mean interval of 165 days between my death-related dreams and his death and a mean interval of 6,612 days between my non-death–related dreams and his death. So the percentage of total days contributed by the former is 165/(165 + 6612) = .024, or 2.4%.

The cases are listed in order of increasing percentages, with percentages less than 50% indicating that the death-related dreams occurred, on average, closer to the date of death than did control dreams. As shown in Table 1, this was true for 9 of the 12 characters.

Because there were no control dreams featuring Hannah or Ken, two data analyses were conducted. The first analysis substituted the mean number of control days for the remaining 9 characters as an estimate for the 2 missing control-dream intervals—displayed in parentheses. With this substitution, the death-dream percentages are significantly less than 50% using a 1-sample, one-tailed t test across the 12 cases: $t(11) = 3.30$, $p = .004$. In the second analysis, the records for Hannah and Ken were simply omitted. When this is done, the death-dream percentage is still significantly lower than 50%; $t(9) = 2.45$, $p = .018$.

Case Descriptions for Pre-Death Dreams

In this section I provide details of my dreams and the circumstances of the subsequent deaths for the first 3 characters listed in Table 1: Harmony, Noah, and Hannah. These are the cases in which the relative intervals between the dream and the subsequent death are the shortest.

Harmony. Harmony is a relative of mine whose case is one of the most remarkable in the dream journal. As can be seen in the first row of Table 1, I had a death-related dream about her on the same day that her death occurred, July 2, 2003. There had been 24 earlier dreams about her, but this was the only one that mentioned her possible death. When I awoke from the dream, I was certain she had died, telling this to both my wife and daughter the next morning. The day after I had entered the dream in the journal, I received an email telling me that she had, indeed, died.

TABLE 1
Pre-Death Dreams: Mean Time Intervals (Days) between Dreams and Subsequent Death

Pseudonym [a]	Relationship	# death dreams	# control dreams	Mean # days between death dreams and death	Mean # days between control dreams and death	% days contributed by death-dream to death days [b]
1 Harmony	Relative	1	24	0	2479	0.0
2 Noah	Relative	4	14	165	6612	2.4
3 Hannah	Friend	1	0	120	(4297) [c]	2.7
4 Ken	Stranger	1	0	491	(4297) [c]	10.3
5 Isador	Friend	2	17	2255	5973	27.4
6 Lim	Relative	1	17	1389	3335	29.4
7 Chris	Relative	1	9	2466	5037	32.9
8 Yi	Relative	5	28	2614	4033	39.3
9 Jonathan	Celebrity	2	6	3723	5515	40.3
110 Astrid	Relative	1	1	2836	2831	50.0
111 Dominik	Colleague	2	5	4695	3702	55.9
112 Mostafa	Celebrity	3	6	5742	3456	62.4
Total		**24**	**127**	-	-	-
Mean		-	-	**2207.87**	**4297.28**	**0.559**
(SD)		-	-	**(1873.75)**	**(1268.77)**	**(0.528)**

[a] Names of dream characters are pseudonyms generated by the online Random Name Generator: http://www.behindthename.com/random/
[b] If <50%, then death-related dreams occurred closer to the date of death than did control dreams.
[c] No control dreams for these 2 dream characters. Mean value from the other 9 cases used as an estimate.

The death-related dream about Harmony is a classic leave-taking dream, a dream in which a spirit of the deceased appears as an announcement of his or her death. In the earlier control dreams, Harmony appeared thin and frail, but she looked very different in my final dream. Here, in full, is my written record of the dream from my journal:

> [Harmony] is here briefly. She is literally shining, like sunlight through diamonds. She is very beautiful, radiant. Has she died?

I had not been not in contact with Harmony or other members of this branch of my family for more than a year and I did not know Harmony well. Based on emails I received several years before she died, I knew that she had Alzheimer's Disease and had been in a nursing home. But I did not know at the time that the disease shortened one's lifespan (Burns & Iliffe 2009), so my knowledge of her illness did not serve as a cue that her death was imminent—and certainly not on the exact day of her death. I believe this dream provides good evidence for a psi-mediated process of information retrieval.

Noah. Noah is the first of my three stepfathers. My mother divorced him in 1971, but remained in contact with him until at least 1980, when I saw him with my mother in Santa Barbara. That was the last time I saw him.

On September 29, 2010, I dreamed of Noah. Here is the relevant portion of that dream:

> Mom, Karina, Tomoyo, and my first stepfather Noah. It feels like I'm in Los Angeles, walking around with the above-named group. It is puzzling that Noah is present because I haven't seen or heard from him in so long. It is nice to see him again regardless because I've always liked him. . . . After what seems like several hours with Noah, he is gone. I tell my mom that I wished I had said more while he was here, but she said not to worry, that he'd just wanted to see me "One more time." Does this mean he's died? Must check with mom.

After this dream I asked my mother about Noah, but she didn't know where he was living or how to contact him. A little over a month later, on October 19, 2010, I dreamed of Noah again:

> . . . I had been with Peter and Karina, all of us talking about Peter's printing press, when Noah appears to say hello. . . . This is [the] second 'Noah' appearance in these pages, making me wonder if he is alive or not. This dream may not be interesting in any other respect, but two appearances in two weeks (? Or so) is a lot from someone I haven't seen in over 20 years and have no reason to be thinking about.

On January 8, 2011, I had a lucid dream that Noah has given me a wristwatch that has something to do with the future. On February 23, 2011, I dreamed I was with Noah again in the context of a party on Memorial Day.

Checking on Noah's status proved difficult because we had both moved multiple times and had not made any attempt to remain in communication. Nor did Noah have a presence on the Internet. My mother was eventually able to turn up an obituary listing Noah's death date as May 19, 2011. Dreams about Noah appeared in the journal 18 times prior to his death. As

shown in Table 1, these four death-related dreams occurred much closer to the date of his death than the control dreams.

The first dream of September 29, 2010, sounds very much like a leave-taking dream. Noah wants to see me "one last time," as if he is about to leave forever. The second dream of October 19 is only interesting because of the first, but the idea of his suddenly wanting to say hello, a theme that does not appear until the "one last time" dream, is interesting and suggestive of a second attempt. The dream of January 8, 2011, appears to be symbolic. Noah gives me a wristwatch that is somehow important to the future, to something that will happen soon. In the dream closest to the date of his death, I dream of him in the context of Memorial Day, a day to remember the dead. Collectively, the set of dreams can be interpreted as a last-goodbye and a message to me that Noah will die soon. If this interpretation is correct, it would relate to Noah's actual circumstances as he prepared for death. In that sense he is not saying good-bye because he has already died, but in preparation for death.

These are the only four dreams of Noah to appear within eight years, no dreams outside of this group suggest his death in any way, and two of the dreams explicitly suggest his death while one implies it (Memorial Day)—an interesting coincidence in itself because Noah died nine days before Memorial Day.

Hannah. Hannah was the mother of my best boyhood friend, Donovan. In 1976, my family moved from San Jose, California, to Las Vegas, Nevada, the last time I would see him until decades later. Sometime after I moved, I learned that his family had also moved. I made occasional attempts to find them over the next few years, but had given up by 1981, when my family moved to Santa Barbara, California.

On 24 September 1999 I reported the following dream in my journal:

> . . . next, I realize that Donovan is standing directly in front of me. I am quite happy to see him again. Donovan doesn't seem to know where he is or what is going on. After realizing who he is, I grab him, pull him over, and give him a big hug. I then give him a business card so that he can get in touch. After that he faded out and disappeared.

A little over a week later, on October 3, 1999, I wrote the following:

> [My Uncle Luke] tells me that Donovan [see dream a few nights ago] . . . died in 1995 after getting sick. My impression was he died of asthma.[1] Luke also tells me that several others have had dreams of Donovan recently and that each had a little piece of the story. I think we found out about his death when we were thinking about him and his mom [Hannah] just appeared.

I did not write it in the journal, but that morning I told my wife that when Hannah appeared, she said something that was different from what Luke said, but I didn't remember how it was different, just that she was correcting Luke because he had made a mistake in what he told me. Four months later, on February 20, 2000, I had the next dream in this series. In it, I observe an older man and woman described in the journal as "devout Christians." The focus in the dream is on the wife, who stands near the man as he sits in a chair in a darkened living room, looking bereft. I write *"Is this man a ghost?"* in reference to the man. His wife talks to me in a friendly way, while the man seems completely oblivious to our presence. *"Her aura is on plain view here, and it's a good one,"* I write. Although not written in the journal, this dream character reminded me of Hannah and made me think of Donovan and their house.

Because of these three dreams, I was concerned that Donovan may have died. I made several searches on the Internet, looking for Donovan's obituary, but found nothing. A year later, I was re-reading the dreams when I noticed the detail about Hannah appearing in the dream with Luke and that she had disagreed with what he had said. I then noticed the February 20th dream and realized that the mistake Luke made was identifying Donovan rather than Hannah as the person who had died. Pursuing this hypothesis, I conducted a new Internet search looking for Hannah's obituary. I learned that she had died of cancer at the age of 64 on January 31, 2000—only 120 days after my first dream about her. I also had a dream about Hannah 20 days *after* her death, well before I had learned of it (See Table 2, below).

Post-Death Dreams

The dream journal contained 68 death-related dreams and 25 control dreams that occurred after the death of the death character. When I eliminate all dreams in which I knew of the character's death at the time of the dream, only 8 death-dream characters remain, each one appearing in a single death-related dream. These are shown in Table 2 and include both Hannah and Lim, who appeared also in Table 1. There were no control dreams for any of these 8 dream characters, so it is not possible to conduct an analysis parallel to the one for pre-death dreams. Here I describe the cases of Lim, Robert, Wynona, and Isabel.

Lim. As in the case of Harmony, the case of Lim is persuasive as an example of a psi-mediated dream because there is such a short interval between my dream and her death; in this case, the dream occurs one day *after* the death, but before I knew about it. On the morning of June 24, 2003, I wrote the following about Lim in my journal:

TABLE 2
Post-Death Dreams: Mean Time Intervals (Days) between a Death and a Subsequent Dream

Pseudonym	Relationship	# Death dreams	# of days between death and death dreams
1 Lim [a]	Relative	1	1
2 Robert	Acquaintance	1	2
3 Rikard	Stranger	1	4
4 Therese	Acquaintance	1	18
5 Hannah [a]	Friend	1	20
6 Isabel	Stranger	1	26
7 Leo	Relative	1	44
8 Wynona	Acquaintance	1	340
Mean			**56.9**

[a] Lim and Hannah also appear in Table 1.

> At Leontine's, she complains that I never follow directions properly. As an example she describes a recent wake where she asked people to sit by the "lapidary bench" (what is that?).

A few hours later, Leontine called to pass on the information that Lim had died the previous night. The purpose behind the call was to inquire whether my wife and I would attend the wake, which she was organizing. I didn't know if it was relevant, but asked my wife if she knew of any connection between something called a "lapidary bench" and either Leontine or Lim. She told me that Leontine was an amateur gemmologist, that a lapidary bench has something to do with making jewelery, and that Leontine owned a lapidary bench.

Although Lim does not appear explicitly in this dream, the dream appears to refer to her wake and thus is counted as a death-related dream with her as the dream character. The only death-related dream about Lim that appears in the journal occurred about 4 years earlier.

Robert. As with the case with Lim, my post-death dream about Robert is notable for its temporal proximity to his death. On the morning of February 2, 2014, I wrote the following in my journal:

> Someone comes to tell me that "Bobby" has died. I ask for more clarification, get the idea it is Robert W. Green.

Thirteen hours after emailing the dream journal entry to myself, I received an email from someone named "Bobbie" to whom I had spoken on perhaps three other occasions in my life. The email referred to a mutual acquaintance named "Robert H." or "Bob," who had died the night before. Like Bobbie, I did not know Bob well. The last occasion I had spoken to him was about 2012, and before that, 2011 and 2010. In total, we had conversed on between three and five occasions, usually for about an hour or two on each occasion. When I met him for the first and only time in person in 2011, he had just had a cancerous growth removed from his nose and he appeared to be quite ill. As with Harmony, this information about his health could hardly have been the basis for an accurate prediction of the date of his death three years later. This is the first and only mention of this character in the journal. It is also interesting that the name "Bobby" or "Bobbie" in the dream journal and my email history occurs only in reference to Robert H.

Wynona. The case of Wynona provides an interesting contrast to that of Harmony, whose death occurred the same day as my dream. In Wynona's case, the time interval between her death and my subsequent dream is 340 days, the longest in Table 2.

Wynona was a friend of my mother's in the late 1960s through about 1977. The last time I saw her was in 1977, when I was living in San Jose, California. My family moved away and both my mother and I lost contact with her. On December 9, 2010, more than thirty years after I had last seen her, I dreamed of Wynona. Here is the relevant part of my dream report:

> A little later, I hear that Wynona has a gift for me also. I see her go by to see Karina. She says hello as she passes, and then is gone. I notice that a woman has been drawing in Karina's sketchbook . . . I retrieve it for Karina and then go to a booth at a Wolfgang Puck's restaurant to find her. She is in a booth with Wynona, who looks better than I've ever known her to look. Is she dead?

Checking on Wynona's death proved to be difficult. A Google search yielded nothing, and my mother had no idea whether Wynona was alive or dead. A year later when I started work on this study, I asked my mother to make another attempt to determine Wynona's status. She discovered

that Wynona had died in 2010, the same year as my dream. The only other dream in the journal that mentioned Wynona was a pre-death, control dream from 19 years earlier. In other words, the only dream recorded in the journal to mention Wynona's death over a 19-year span occurred in the same year as her death.

Unlike the dream of Harmony, the dream of Wynona does not depict her at what appears to be the moment of death. Instead, it leaves the impression of a spirit that is already dead, coming by to say hello and bestow a gift of some kind. In this sense, the impression is accurate. She did not die on the date of the dream, but was deceased at the time of the dream. It seems unlikely that somehow I had learned about Wynona's death through non-psi channels but then suppressed that knowledge. We had never lived in the same city again. She was not a well-known person and had no presence on the Internet. The possibility that I could have somehow learned her status normally and then suppressed the information seems quite remote.

Isabel. Isabel was a stranger to me at the time she died. The dream occurred on August 12, 2003. Here is the relevant section of my dream report:

> . . . I am talking with someone . . . when I am told that one of the employees of this place may be about to die. I think it was Matthew from Arizona Flooring.[2] Someone in his family was very sick or had died, so he started taking drugs, drinking, not taking care of himself. He was going to die, or had died also. I am very worried about him here. There is a pervasive feeling of the unnecessary here, that he doesn't have to die at this time but he will if he doesn't shape up.

After waking from the dream, I wrote down the details and then discussed it with my wife. The person I was talking to in the dream was a youngish-looking woman, and she had urged me to contact Matthew to get him to stop drinking and taking drugs. She said that if he didn't, he would die. My wife wanted me to follow through and tell Matthew about the dream. I was less happy about the idea because I barely knew Matthew, having spoken to him on only a handful of occasions when I bought supplies at the store he worked at. My wife persuaded me, however, so I made the forty-five minute drive from North Phoenix to Tempe and entered the store.

After introducing the fact that I had dreamed of him, I asked Matthew if a close relative had recently died and if he had started abusing alcohol because of it. He affirmed that this was true. His sister-in-law, Isabel, had died three weeks earlier, on July 27, 2003. A police cruiser collided with her car from behind while she was stopped at a stoplight. I assumed that this meant that the young woman in my dream was his sister-in-law and told him as she had told me in a dream that he had to stop abusing alcohol

or he would die. Matthew admitted to being self-abusive and to drinking heavily after Isabel's death, and then told me that he had had a dream of Isabel earlier that same week. In it, she gave him exactly the same message I had received in my dream. This dream is the only one in the journal that mentions Matthew and the only one that plausibly contains a character that can be identified as Isabel. What is remarkable is that we both got "the same message" from Isabel.

Conclusion

This article has presented 9 cases of dreams that appear to anticipate the deaths of the dream characters through a paranormal or psi-mediated process. A file-drawer control was provided by comparing the time intervals between the death-related dreams and the subsequent deaths against a set of control (non-death–related) dreams. Some post-death dreams also appeared to provide evidence suggestive of psi-mediated information retrieval.

The 20 test cases presented were culled from a database containing 3,732 nightly dream records that were compiled over a nearly 25-year span. This reveals how difficult it would be to conduct a replication study that similarly controls for a file-drawer effect.

Notes

[1] This is a subjective thought on my part, probably a deduction based on my knowledge that Donovan had asthma.

[2] This company name is a pseudonym.

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RESEARCH ARTICLE

A Review on the Relation between Population Density and UFO Sightings

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Abstract—In the search for patterns of the UFO phenomenon, the relationship of sightings to population density has historically shown contradictory results. After more than 40 years of studies, there is not yet a clear conclusion on whether the relationship between the number of reports and population density is direct or inverse. We have reviewed some of these works and found out how to reconcile all of them. We found that there is a direct relationship between the number of sightings and population density; however, the increase in number of reports is not proportional but sublineal with respect to the increase in population.

Introduction

The study of Unidentified Flying Objects (UFOs) or UFO phenomena has been an activity focused on finding patterns that may help us understand whether the phenomenon is real and what its nature is. One of these patterns has been the relationship between the number of UFO reports and population density. Since UFO reports are dependent on the presence of witnesses, common sense dictates that more populated areas should produce more UFO reports than unpopulated areas. However, the first person to study this relationship was Jacques Vallée (1966), who reached the conclusion that there was an inverse correlation, that is unpopulated areas produced more reports than populated areas. This conclusion led him to think that there was some kind of intelligence behind the phenomenon that made it avoid populated places and be more active in isolated places.

After this initial study, the relationship has been addressed on many occasions by different authors (see references throughout). However, the results in these works seem to lead to contradictory conclusions. Some show the inverse relationship found by Vallée, but other studies show a direct relationship, in which populated areas provide more UFO reports than less populated areas.

These studies, however, have important differences, especially regarding the variables taken into account: Number of reports (N), Population (P), Number of reports per capita (N/P), Population density (P/S, or δ), Reports per square kilometer (N/S), and some other subtleties such as the kind of cases taken into account, which makes it harder to find a direct comparison and to understand the origin of the different conclusions. A summary of these works, spanning a time period of 48 years between 1966 and 2014 can be seen in Table 1.

TABLE 1
Summary of Studies Regarding Population Density and UFO Sightings

Authors / reference	Variables	Correlation	Comments
Vallée 1966	N vs δ	Inverse	Landings in France
Vallée 1968	N/P vs δ	Inverse	All kinds of sightings
Condon 1968	N vs δ	Direct	Non-urban areas
Bonabot 1971	N vs δ	Inverse	Landings
Saunders 1975	N vs P	Direct	All kinds of sightings
Poher & Vallée 1975	N vs δ	Inverse	Landings
Poher 1976	N vs δ	Direct	All kinds of sightings
Ballester Olmos 1976	N/P vs δ	Inverse	Landings
López et al. 1978	N/S vs δ	Direct	All kinds of sightings
Fernández & Manuel 1980	N/S vs δ	Direct	Landings
Fernández & Manuel 1980	N vs δ	Direct	Sightings in the Comunidad Valenciana
Weiller 1980	N vs P	Direct	All kinds of sightings
Verga 1981	N/δ vs δ	Inverse	Landings in Italy
Ballester & Fernández 1987	N vs δ	None	Landings. UFO and IFO cases
Breysse 1993	N vs P	Direct	All kinds of sightings
Ballester Olmos 2014	N vs δ	Direct	Photo and video images
Rospars 2014	N/P vs δ	Inverse	UFO and IFO cases

N = Number of reports. P = Population. S = Area. N/P = Reports per inhabitant. N/S = Reports per unit area. δ = Population density.

In this work we are going to review those studies, and we will show that it is possible to reconcile their results. The methods used in the above-mentioned works as well as in this one are explained in basic terms in Appendices A and B.

As we will see, the use of different variables in each study is the origin of the different conclusions reached by different authors. In particular, it is important to keep in mind that an inverse relationship in the number of reports per capita with population density (N/P vs δ) does not necessarily imply an inverse relationship of the total number of reports with population density (N vs δ).

Historical Review

Vallée and the First Negative Law

The interest in the geographical distribution of UFO reports goes back to 1966, when Vallée (1966) argued against the hypothesis of a psychological origin of UFO sightings, put forward by Georges Heuyer, to explain the 1954 wave in France. The development of a *psychosis* should follow strict rules, it is not a random phenomenon, and populated areas like Paris, Lille, Marseille, or Bordeaux would have had to have the right conditions for the propagation of rumors.

Vallée analyzed 200 landing reports, most of them occurring in France during 1954. After plotting these landings on a map (Figure 1 Left), Vallée argued that these did not concentrate in populated areas, and finally stated what he called the *First Negative Law*:

> The geographic repartition of the landing sites in 1954 is inversely correlated with population density.

Later, in 1968, Vallée published another paper (Vallée 1968), analyzing more than 8,000 sightings in the U.S. He reported that the number of reports per capita was higher in low-population areas, confirming the rural character previously noted in 1966. A plot of the number of reports per capita versus population density, grouped by states of the U.S., showed an inverse relationship (Figure 1 Right). Note that the variables are not the number of reports (N), but the number of reports per capita (N/P). Here we find one of the first misunderstandings with respect to the variables, since the statement of the first negative law referred to the total number of reports.

Also in 1968, the Condon Report (Condon 1968) had a paragraph pointing out that:

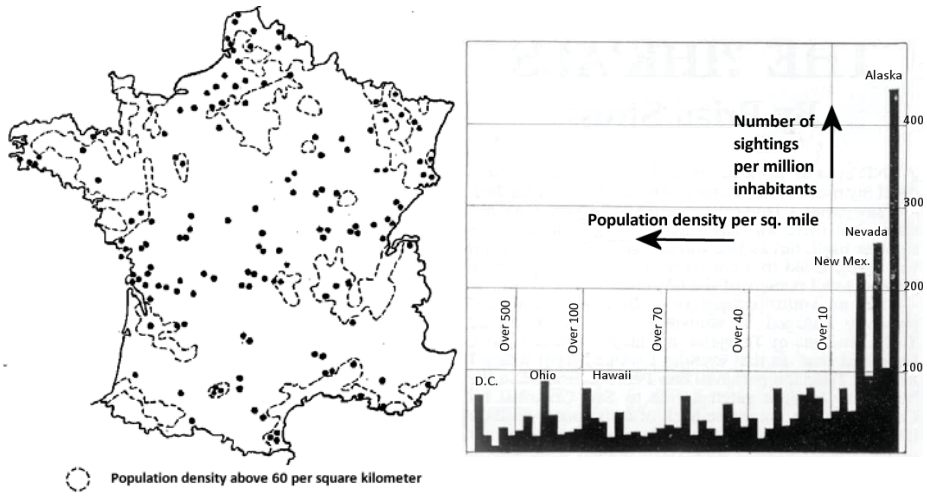

Figure 1. Left: UFO Landing sites in France, 1954. Reproduced from Vallée (1966). Right: Distribution of UFO sightings in U.S. states (Vallee 1968).

> The geographical distribution of reports correlates roughly with population density of non urban population. Very few reports come from the densely populated urban areas. Whether this is due to urban sophistication or to the scattering of city light is not known, but it is more probably the latter.

This partially contradicts Vallée, but agrees in that urban areas do not significantly contribute to the total of UFO reports. However, an interesting reason is sketched to account for that: City lights would eclipse lights from UFOs.

Studies During the 1970s

Using a similar graphical analysis as Vallée, Jacques Bonabot (Bonabot 1971) made a simple analysis of the distribution of UFO events in Belgium. Landings and close encounters seemed to follow Vallée's first negative law. But he also noted that flying or distant objects seemed to be more frequent near populated places like Brussels and Liege.

David Saunders (Saunders 1975) did excellent work on a huge database of U.S. cases, UFOCAT, seeking multiple correlations with up to 14 different variables, population and geographical area among them. He took into account about 18,000 reports scattered in more than 3,000 counties. In January 1975 he presented his results at the 13th Aerospace Sciences Meeting, showing that the main correlation was between number of reports and population, and it was a direct one.

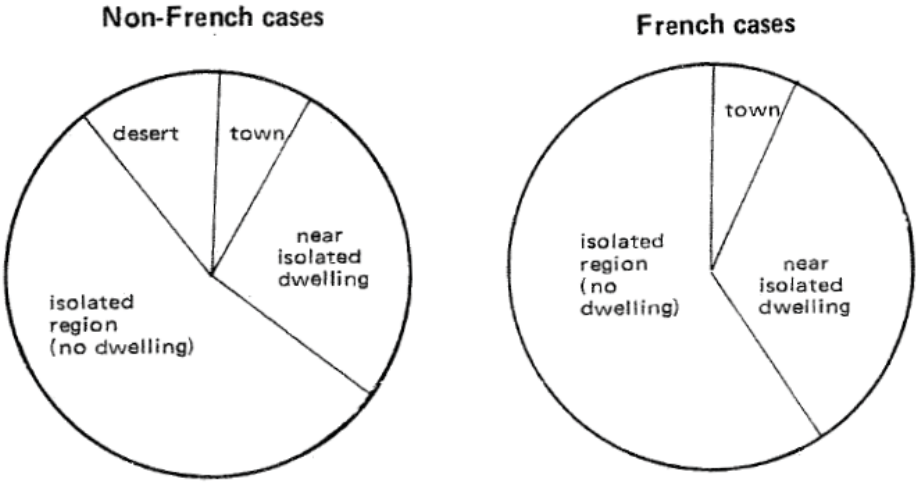

Figure 2. Distribution of Type I cases as a function of population density. Reproduced from Poher and Vallée (1975).

Also at this meeting, Poher and Vallée (1975) presented a work on basic patterns in UFO observations. The distribution of reports as a function of the location of the sightings is reproduced in Figure 2. More than 70–80% of the reports occurred in or near isolated places.

These results are only part of a larger statistical study done by Poher between 1971 and 1976 (Poher 1976). In that work, he also analyzed the distribution of 220 sightings and 40 landings, summarized in Table 2. The landing reports are too few to draw conclusions. As for the sightings, it looks like a direct correlation between number of reports and population density could be possible. However, the division of population density into only three bins of different sizes for the population density seems poor and inadequate for drawing conclusions.

TABLE 2
Number of Reports vs. Population Density (taken from Poher 1976)

Density [Pers/km^2]	Total (220)	Landings (40)	Landing with occupants (16)
$\delta > 80$	110	48%	31%
$50 < \delta < 80$	64	20%	25%
$\delta < 50$	46	32%	44%

This possible direct relationship was also mentioned by Poher (1975), albeit the main argument was to show a dependence on visibility and atmospheric conditions. Poher compared two groups of French departments: a first group of 1,200 hours of sunlight on average, and a second group with about 2,500 hours of sunlight on average. This difference is caused by atmospheric conditions. Both series showed a direct correlation with population density, but the second one also showed a higher number of reports.

A review of French cases published in the magazine *Lumières dans la Nuit* was carried out by Bettini et al. (1977). They noted that departments such as Nord and Pas de Calais, which have high population density, contained the majority of reports.

Ballester Olmos (1976) used a database of 200 landings in Spain in 1976. He grouped 48 provinces into 8 groups of 6 areas. The relationship between reports per inhabitant and population density had a negative correlation, although with a low statistically significant level. It is worth remarking that Ballester Olmos took into account an effect that Saunders had already warned against, the *investigator effect*: Local UFO investigative groups can generate more reports than would otherwise be predicted for that area.

Finally, in 1978, a long work was presented by López et al. (1978) at the First National Congress of Ufology in Spain. The main focus was the development of a model to predict when and where a UFO would be seen. The model supposed that UFO phenomena occurred randomly, and the number of cases was determined by parameters like area, population, climate, and orography. The theoretical number of reports was compared to real values that were also corrected to take into account the *investigator effect*. López et al. finally found a direct relationship between population density and UFO sightings for all types of reports, and also specifically for landing reports. Theoretical and real values correlated quite well, meaning that UFOs basically had a random pattern.

Later Studies

Following the methodology of López et al., Fernández Peris and Manuel Garijo (Fernández & Manuel 1980) produced another excellent work on UFO patterns. They focused on landing events in the Spanish wave of 1974. Even though their results showed a direct relationship (i.e. number of reports per unit area increasing with population density), they reasoned that when comparing the theoretical values (given by the López et al. model) against the real ones, the number of reports was higher than predicted by the model for low-population areas, and lower than predicted in high-population areas.

Thus, their final conclusion was that a negative law was correct, since low-population areas produced more reports than expected.

However, this kind of "inverse relationship" is different from the one originally stated by Vallée. Vallée is referring directly to the behavior of the variables (N vs δ, or N/P vs δ). If Fernández and Manuel analyzed the same variables in the same way, the relationship is direct for both the model and the real data, and thus contrary to Vallée's negative law. However, the comparison made by Fernández and Manuel is between the *proportionality* of the model and their data, and thus they are comparing something completely different from Vallée. Any conclusion on such a comparison is not valid with respect to Vallée's negative law.

Fernández and Manuel also made a second interesting study (Fernández & Manuel 1980b). Instead of analyzing large areas like countries, they studied the distribution of sightings in the regions of Valencia, in Spain. A total of 208 cases were distributed in smaller regions, and although the number of reports was low, analysis showed high values of the correlation coefficient for population and population density. On the other hand, the correlation between reports and surface area was low, and the final conclusion was that it had no effect on the sightings. They went into further detail with a *finer structure*, and the distribution was studied as a function of the population of the village or town where the sighting occurred. Once again, there was a remarkably high correlation.

In 1980, Weiller (1980) used the correlation between reports and population to hypothesize about the existence of an unknown meteorological effect associated with UFO sightings. He used a database of nearly 2,000 reports in France scattered across departments. The correlation using the whole database is weak, but positive.

Weiller then introduced a luminosity index for each department, based on meteorological data. It is based on the fraction of sky covered by clouds in each department, and thus is very similar to the definition of sunlight hours used by Poher (1975). With this new index, he selected the 22 most luminous ("least cloudy") and the 22 least luminous ("most cloudy") departments. He found that the most luminous group contained more sightings than the least luminous. The correlation coefficients improved remarkably. But also, when taking into account a multiple correlation with population and luminosity index as variables, he deduced that it is also a factor contributing to UFO reports.

This dependence on luminosity agrees with Poher (1975) using sunlight hours as a variable. The definitions of luminosity and sunlight hours are based on meteorological conditions that affect visibility, or the distance at which objects may be seen. Both Weiller (1980) and Poher (1975) show

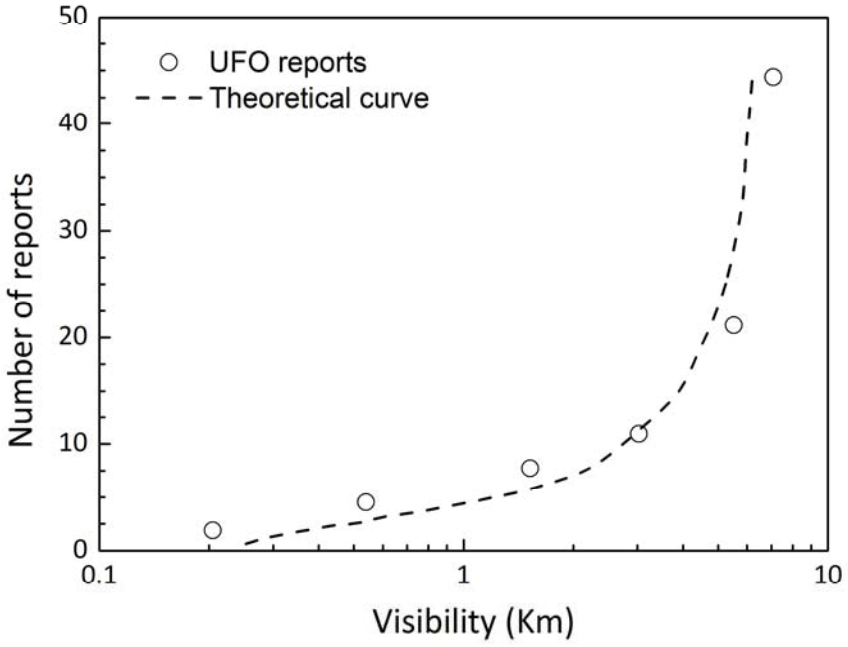

Figure 3. UFO reports versus visibility and atmospheric transparency. Reproduced from Poher and Vallee 1975.

coherent results with those obtained by Poher and Vallée (1975), where they analyzed the relationship between the number of reports and visibility (Figure 3). The results are totally consistent with sightings produced by misperceptions of stars, planets, airplanes, etc. Cloud cover or low visibility conditions prevent these stimuli from being seen; even though they do not account for all sightings, they represent an important fraction of UFO reports.

One year later, Maurizio Verga made a review on the subject, and also conducted a study on a database of 326 landings in Italy (Verga 1981). He noted the contradictory results, and assumed it was due to the different methodologies used, including the definitions of *populated* and *unpopulated* areas. He also noted that many different factors could affect the data count. Therefore, in his final remarks, Verga stated that the majority of studies were useless to debate about. However, he also noted that about 90% of the reports came from places with *a high probability of witness presence*.

In 1987, Ballester Olmos and Fernández Peris published a book on landing reports (Ballester & Fernández 1987). They updated Ballester's landings database used in previous studies, and selected only cases with a high strangeness, as defined in Ballester and Guasp (1981). With the help of

collaborators throughout Spain and Portugal, a rigorous study was done in each case and finally two catalogs were produced:

- **LANIB** (new catalogue of LANding reports in the IBerian peninsula): Landing cases with a high strangeness factor as defined in Ballester and Guasp (1981) (230 cases).
- **NELIB** (catalogue of NEgative Landings in the IBerian peninsula): Landing cases for which an explanation was found (310 cases).

NELIB was thought of as a *control database*. If the UFO phenomenon is real, some kind of difference has to be found when compared to databases containing real UFO cases. Ballester and Fernández found a correlation coefficient of 0.62 for NELIB, meaning a direct relationship between number of reports and population density, with a moderate correlation. For LANIB they found a correlation coefficient of 0.21, which was interpreted as a lack of correlation.

Breysse (1993) tested two different hypotheses: whether the number of reports was a function of surface area, or dependent on population; and also whether this dependence was proportional ($N \sim S$, $N \sim P$) or quadratic ($N \sim S^2$, $N \sim P^2$). After analyzing two databases from the U.S. (643 reports in 51 states or territories) and France (346 reports in 95 departments), he came to the conclusion that surface area was not correlated to the number of reports. On the other hand, the best correlations were between number of reports and population, and they were direct correlations (Table 3). However, the data did not allow differentiating between the two models, proportional or quadratic.

Just recently, Ballester Olmos published some basic statistics on FOTOCAT (Ballester 2014), showing the distribution of 11,060 UFO images (photos and footage) by continent. The result was a positive correlation between the number of reports and population density.

Finally, the last study to date was presented at the CAIPAN International

TABLE 3
Correlations for Two Databases (reproduced from Breysse 1993)

	N vs S	N vs P	N vs δ
US (N = 643)	0.063	0.887	0.386
France (N = 346)	0.145	0.391	0.126

Workshop by J. P. Rospars (Rospars 2014). His analysis on reports per unit area and reports per capita versus population density in France showed that probability of reporting an observation increases with population in large areas, whereas the probability for an inhabitant to report an observation decreases when the density increases. As we will see, this last study offers the best description for the real dependence of the number of reports on population density.

Review and Recalculation of Results

So far, we have reviewed several studies and analysis on the geographic distribution of reports, showing different and apparently contradictory results. Intuition tells us that the more people, the more probability somebody can witness a UFO event. However, this seems to be true when all kind of reports are considered, but the relationship seems the opposite when only landings are studied.

As Verga pointed out, there are multiple factors that can affect the number of reports. Population changes over the years, and cities grow in size absorbing towns that become suburbs. We have already mentioned the *investigator effect* as another factor that can affect the results, and some of the studies effectively show an increase in number of reports in regions where these groups existed.

We can even extend the argument to a *hot spot effect*: In FOTOCAT, the state of New Mexico is the sixth least populated per square kilometer of the 50 states in the U.S. However, it is the sixth contributor in number of reports, most likely due to the fame of the Roswell incident. A similar effect can be presumed for Nevada (9th least populated, 16th contributor) and the famous Area 51.

To be able to compare different areas, the time interval of the databases should be the same for every region accounted for, and the efficiency in collecting reports should be the same. Using FOTOCAT again as an extreme example, countries like Bosnia-Herzegovina have existed for a short time compared with countries like France and Spain. Also, it does not seem reasonable to have only one entry for a country like Bangladesh with 160 million inhabitants.

Poher and Weiller showed that even visibility and different meteorological conditions throughout the year have an effect on reports from different geographical areas.

All these effects add noise to the results and make a direct quantitative comparison between different databases difficult. Thus, only trends can be compared. However, the main issue in past studies is that they used

different variables to describe the relationship between number of reports and population. Saunders already noted that taking Vallée's first negative law as stated in 1966 literally was different with respect to the relationship N/P vs. δ that he plotted in 1968. Other investigators have used more exotic variables like the Number of reports per unit area (N/S), or the Number of reports per inhabitant and unit area. Therefore, it is not surprising that different results have come up during these years, since the variables explored have been different each time.

For that reason, we have calculated the same set of correlation coefficients for most of the studies we have reviewed, and also added new data using databases not taken into account before. Three correlation coefficients were calculated for each database:

- Number of reports vs Population (N vs P)
- Number of reports vs Population Density (N vs δ)
- Number of reports per inhabitant vs Population Density (N/P vs δ).

The results are shown in Table 4, which also shows values and results for some of the studies for which we could not calculate the correlation coefficient. The reported values are shown instead.

We can see a very clear and unmistakable trend:

- The Number of reports correlates **directly** with Population.
- The Number of reports correlates **directly** with Population Density.
- The correlation with Population Density is weaker than with Population.
- The Number of reports per capita correlates **inversely** with Population Density. Even if not all p-values are statistically significant, and most correlations are weak, they all show a negative sign, except for López et al. (1978).

Looking at Table 4, we can now see that all reported data fit coherently when compared with the proper variables of the other studies. It is also worth remarking that there is no difference in correlation trends when analyzing all kind of reports, landings, or images. Past studies did not show contradictory results. The issue was that **different variables were being compared**. There is only one exception: The original 1966 claim of an inverse correlation between N and δ is the only contradiction with the data presented. We will look further into it in the next section. Also, we will look into the fact that N/P vs δ is an inverse correlation and what it means with respect to the N vs δ direct correlation.

TABLE 4
Summary of Results of the Works Analyzed in this Paper

Source	Reports (N)	r (N vs P)	p-value	r (N vs δ)	p-value	r (N/P vs δ)	p-value	Comments
Vallée 1966	151	0.307	0.007	0.245	0.026	−0.307	$7{\cdot}10^{-4}$	Landings in departments of France (population from Weiller 1980)
		0.286	0.11	−0.007	0.49	−0.429	0.033	Landings in regions of France (population taken from Weiller 1980)
Vallée 1968	8,260	-	-	-	-	(Inverse)	-	Deduced from graph
Saunders 1975	18,122	(0.723)	-	-	-	-	-	Data from simple correlation calculations
Poher 1975	-	-	-	(Direct)	-	-	-	Deduced from graph
Ballester 1976	200	-	-	-	-	−0.40	0.0033	Landings in provinces of Spain
Bettini et al. 1977	299	0.35	0.0025	0.21	0.048	−0.37	0.001	Departments of France
López et al. 1978	1,721	0.79	$2.1{\cdot}10^{-11}$	0.71	$9.1{\cdot}10^{-9}$	0.009	0.52	All kind of sightings. Provinces of Spain
	237	0.49	$4.0{\cdot}10^{-4}$	0.40	0.0040	−0.23	0.069	Landings in provinces of Spain
Fernández & Manuel 1980a	49	-	-	-	-	−0.53	0.004	Landings in provinces of Spain
Fernández & Manuel 1980b	208	0.74	$1.1{\cdot}10^{-5}$	0.67	$1.1{\cdot}10^{-4}$	−0.53	0.0027	Regions of the Comunidad Valenciana
		(0.91)	-	-	-	-	-	Villages in the Comunidad Valenciana
Weiller 1980	1,919	0.54	$1.4{\cdot}10^{-8}$	0.15	$5.3{\cdot}10^{-6}$	−0.31	0.0014	All sightings in France except for Paris
	483	0.81	$1.9{\cdot}10^{-6}$	0.77	$1.0{\cdot}10^{-5}$	−0.09	0.34	Most luminous departments in France (22 departments)
	398	0.79	$5.4{\cdot}10^{-6}$	0.77	$1.5{\cdot}10^{-5}$	−0.12	0.30	Least luminous departments in France (22 departments)
Verga 1981	326	0.67	0.0012	0.45	0.023	−0.31	0.087	Landings. Population by regions of Italy
Ballester & Fernández 1987	205	0.37	0.009	0.14	0.17	−0.57	$1.5{\cdot}10^{-6}$	LANIB, high-strangeness landings in Spain
	310	0.60	$1.9{\cdot}10^{-6}$	0.47	$4.0{\cdot}10^{-4}$	−0.29	0.0026	NELIB, negative landing cases (IFO) in Spain
Breysse 1993	643	(0.887)	-	(0.386)	-	-	-	Cases in states of the U.S.
	310	(0.392)	-	(0.126)	-	-	-	Cases in departments of France
Ballester 2014	11,060	0.70	0.040	0.73	0.059	−0.91	$5.4{\cdot}10^{-4}$	Photo & video. Sorted by continents
CUCO	8,298	0.79	$3.7{\cdot}10^{-12}$	0.71	$4.3{\cdot}10^{-9}$	−0.06	0.34	All kind of sightings. Provinces of Spain
ALLCAT	953	0.60	$1.7{\cdot}10^{-6}$	0.46	$4.2{\cdot}10^{-4}$	−0.45	$1.6{\cdot}10^{-4}$	Landings in Spain
FOTOCAT	2,785	0.81	$2.9{\cdot}10^{-13}$	0.32	0.039	−0.45	0.024	Photo & video in states of the U.S.

Correlation coefficients (r) were recalculated when possible, taking logarithms of the variables. Values in brackets are those reported or deduced from the references, for which r could not be calculated.

First Negative Law

> The geographic repartition of the landing sites in 1954 is inversely correlated with population density.

The only support for this claim is a map of France showing the landing sites (reproduced in Figure 1 Left) that seems to *qualitatively* support the statement. Let us have a closer look at Figure 1. Enclosed by dashed lines are areas with a population density greater than 60 inhabitants per km^2. We can count 159 landings, 46 of them inside the *high-density* areas, and 113 outside them. That is, 28.9% of landings occurred in high-density areas. On the other hand, in a rough calculation counting the number of pixels in each zone, we can estimate that high-density areas represent only 26% of France.[1]

Let us suppose that UFO landings occur uniformly throughout the country. Let us also assume for the moment that population density has no effect at all. That is, every landing has the same probability of being witnessed regardless of the population of the area. Under those assumptions that we can label as the *null hypothesis*, the number of reported landings must be proportional to the area taken into account. If we take an area representing 26% of the total, then an average of 26% of the landings (41 landings) should occur in that area. If for any reason UFOs tried to avoid those areas, then this value would decrease accordingly, leading to a negative law. With a lower probability of a UFO visiting the area, the number of UFO landings should be lower than the null hypothesis prediction. However, what we find is that the actual value (46 landings, 28.9%) is greater than expected by our null hypothesis, meaning that some factor is favoring their sighting. Since we are considering only two regions that differ in population density, this factor could be the one increasing the probability of a UFO being witnessed in the >60% inhabitants per km^2 areas, and thus we should have a direct relationship.

We have to remark that this difference between the null hypothesis and the actual number of landings is *not statistically significant* (p-value = 0.4). This means that this data does not support either a direct or inverse relationship. On the other hand, the same data does not support Heuyer's *psychosis hypothesis* either, which was the main point being made by Vallée at that time.

This analysis was based only on Vallée's graph. It suggests that there is not any inverse relationship between sightings and population density, but it is not especially accurate, and yields a not statistically significant result. Therefore, we have made a second analysis looking for a more accurate quantification of the relationship.

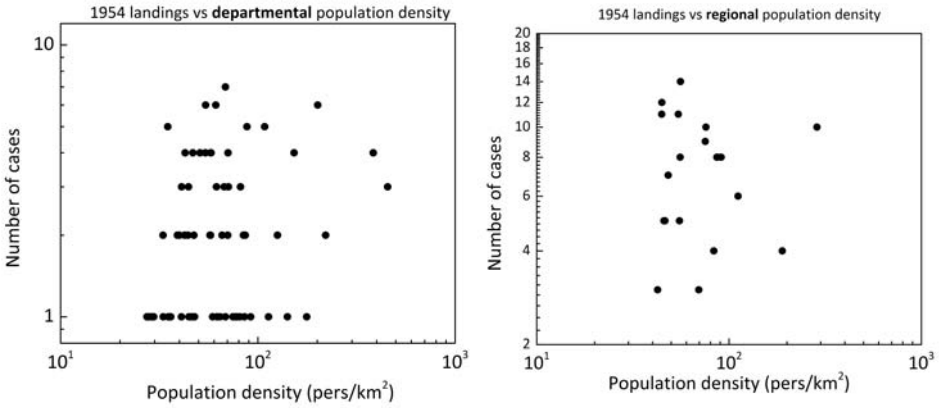

Figure 4. Left: Number of landings versus population density in French departments. Right: Number of landings versus population density in French regions. Landings data from Vallée (1966), population data from Weiller (1980).

We were able to unambiguously locate 151 of the landings. Since the 1966 paper did not contain data about population and regions, we used that of Weiller, circa 1973 (Weiller 1980). 151 landings are very few data points, and divided into 90 departments means many of the areas have 1, 2, or no datapoints at all. Looking for a different distribution, we also tried to group the data by regions, where each region includes several departments. Plots of number of landings versus population density can be seen in Figure 4.

The correlation coefficients were calculated for N vs P, N vs. δ, and N/P vs. δ, and are shown in Table 4, along with the p-values assuming as an alternative hypothesis the correlations being greater than (i.e. direct) or lower than (i.e. inverse) zero. Results are summarized in Table 5.

TABLE 5
Correlations for Two Different Geographical Distributions of 151 Landings in France, Taken from Vallée (1966)

	r	p-value
Correlations by regions		
N vs P	0.286	0.11
N vs δ	–0.007	0.49
N/P vs δ	**0.429**	**0.033**
Correlations by departments		
N vs P	**0.307**	**0.007**
N vs δ	**0.245**	**0.026**
N/P vs δ	**–0.307**	**0.0007**

Bold p-values are statistically significant at 0.05 level.

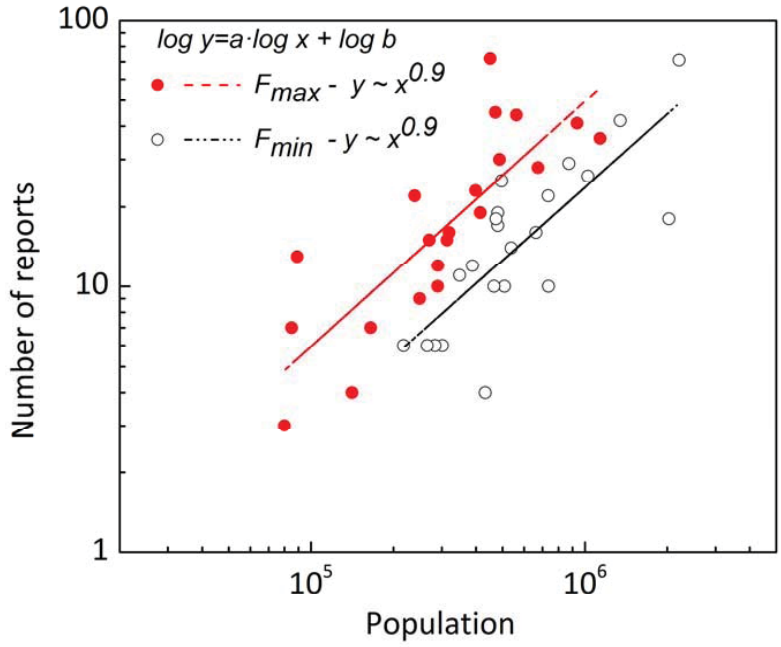

Figure 5. Number of reports versus population in French departments, re-elaborated from Weiller (1980). Open circles represent the 22 least luminous (F_{min}) departments. Closed circles represent the 22 most luminous (F_{max}) departments.

Table 5 shows that the results in a regional distribution are not statistically significant, except for the N/P vs δ inverse correlation. The departmental distribution shows that all correlations are statistically significant at the p = 0.05 level, even if they are by a small margin.

We must conclude that the negative law, as it was stated, had no basis since an inverse correlation between Number of Cases and Population Density could not be found in any case.

Sub-Lineal Relationship between Number of Reports and Population

Let us look now at the study by Weiller (1980). Figure 5 reproduces his results after the separation of 2,000 reports into two series. F_{min} corresponds to the 22 departments with fewer hours of light per year. F_{max} corresponds to the 22 most luminous departments. In both series, not only the light available because of geographical latitude, but also meteorological data relative to cloud cover were taken into account. Both series show a clear direct relationship between number of reports and population.

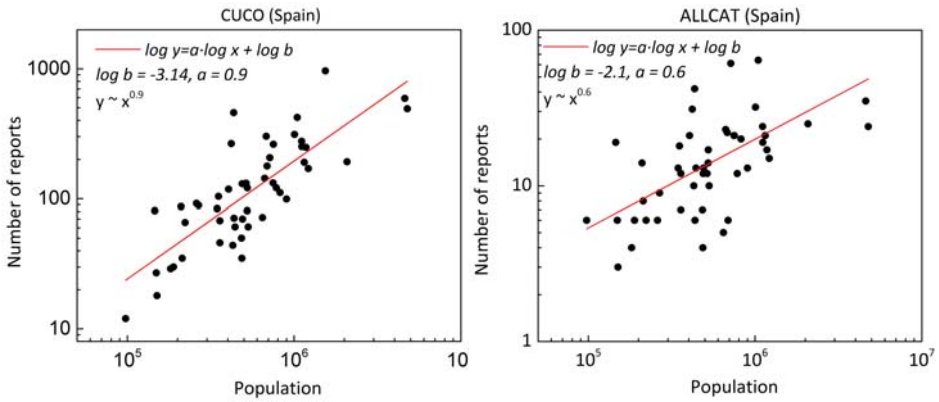

Figure 6. Left: Number of reports versus population in Spanish provinces for CUCO. Right: Number of reports versus population in Spanish provinces in ALLCAT (2013).

Using the logarithmic scale, Weiller also calculated the slope of the linear regression, whose value is lower than 1. As discussed in Appendix B, this value corresponds to a direct function with *sub-lineal growth*, i.e. the proportion in which the number of reports grows is lower than the proportion in which population increases.

We have looked for this sub-lineal growth in other databases, to check for the reproducibility of the results. First, we used the data from (CUCO), a database including all types of reports, divided by Spanish provinces (more than 8,000 reports in 50 provinces). The cases are dated over several decades (60s, 70s, 80s, and 90s). We used population data from 1986, from the Spanish National Institute of Statistics (INE). After plotting the data in logarithmic scale, the linear regression again shows a sub-lineal growth (Figure 6 Left). ALLCAT, a database of 953 landings in Spain, was also examined using the same population data as in CUCO. Figure 6 Right shows the sub-lineal growth.

We did the same with data from U.S. states in FOTOCAT (2,785 cases in 50 states and Washington, D.C.). Population data have been obtained from Wikipedia. Again, a *sub-lineal growth* is observed (Figure 7 Left). Next, we checked the data from Fernández and Manuel on the regional database centered on Comunidad Valenciana (Fernández & Manuel 1980b), and the sub-lineal relationship showed up again.

Finally, NELIB was also explored as the *control group* (as originally thought by Ballester and Fernández). The sub-lineal growth, once again, is clearly seen (Figure 7 Right). Table 6 summarizes the values of correlation coefficient, p-values, and lineality coefficient *a*, given by

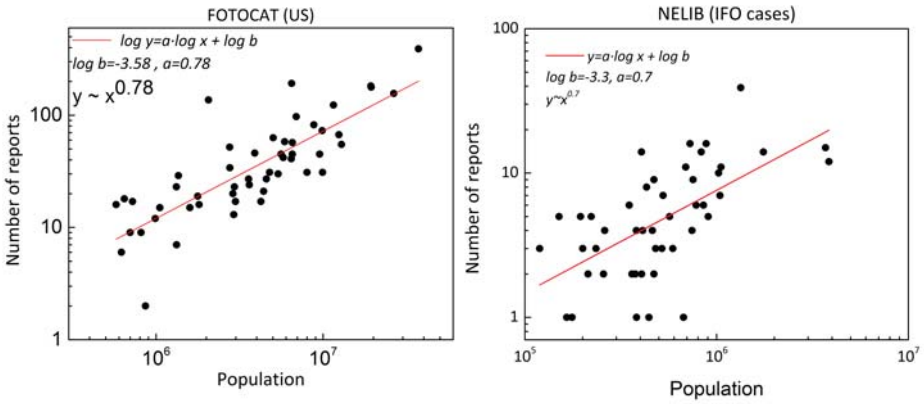

Figure 7. Left: Number of reports versus population in U.S. states for FOTOCAT (catalog of images and video footage). Right: Number of reports versus population in NELIB (catalog of IFO cases).

$$Y = b \cdot X^a$$

for all these databases.

It is worth remarking on the different sorts of databases used: Weiller's and CUCO are databases of all kinds of reports. Weiller's data was divided into two regions of high and low luminosity (hours of day per year). ALLCAT is a landings database, and FOTOCAT is a photo and video/film footage database. We have regional data covering much smaller areas compared to the other databases (Fernández and Manuel 1980b). And we also explored NELIB, a negative (IFO) landings database.[2]

Despite all their differences, all of them behave in the same way. This result allows us to understand now why the number of reports per inhabitant is inversely correlated with population density. Since we have established a relationship of the type $N \sim P^a$, with $a < 1$, if we calculate the quantity N/P we get:

$$N/P \sim P^{a-1}$$

Since $a - 1 < 0$, the relationship becomes inverse. Also, as population density and population are proportional ($P = \delta \cdot S$), this inverse relationship is inherited by population density. Thus, all the results shown in Table 4 agree with a sub-lineal growth, whose main feature is that even if the number of reports grows with population (or population density) it grows in a lower proportion. This is exactly what Rospars (2014) is showing in his results: an increase of total reports, as well as a decrease of reports per capita with population density, due to sublineality.

TABLE 6
Summary of Correlation Coefficients and Lineality for Different Data and Catalogs

Source of data	r (log N vs log P)	p-value	Lineality (*a*)
Weiller 1980 (all data)	0.54	$1.4 \cdot 10^{-8}$	0.6 ± 0.1
Weiller 1980 (22 most luminous departments)	0.81	$1.9 \cdot 10^{-6}$	0.9 ± 0.2
Weiller 1980 (22 least luminous departments)	0.79	$5.4 \cdot 10^{-6}$	0.9 ± 0.2
Fernández & Manuel 1980b (Comunidad Valenciana)	0.74	$1.1 \cdot 10^{-5}$	0.6 ± 0.2
NELIB (Ballester & Fernández 1987)	0.60	$1.9 \cdot 10^{-6}$	0.7 ± 0.2
CUCO	0.79	$3.7 \cdot 10^{-12}$	0.9 ± 0.1
ALLCAT	0.60	$1.7 \cdot 10^{-6}$	0.6 ± 0.1
FOTOCAT—U.S. states	0.81	$2.9 \cdot 10^{-13}$	0.78 ± 0.08

Isolated Places, Population Density, and Homogeneity

Poher and Vallée showed in 1975 that more than 75% of sightings took place in isolated places (Figure 2). How does that reconcile with the results from previous sections?

When we speak about population density we are making a homogenization in the area of a department, province, or state. We assign a homogeneous population density to each of them, but the population is actually concentrated in villages, towns, and cities, whereas the space between them (roads, countryside) is in fact empty, uninhabited, or isolated. We are considering that in a region, every square kilometer is occupied by the same number of inhabitants, but actually most of it is uninhabited.

Even inside a city we can find isolated places. The city of Madrid has a population density of about 5,000 inhabitants per square kilometer. But in a park like Parque del Retiro with an area of 1.18 km^2, at 21:00 h on a winter night, it is difficult to find 5,000 people walking around the place. We should understand population density in the way Verga wrote about it:

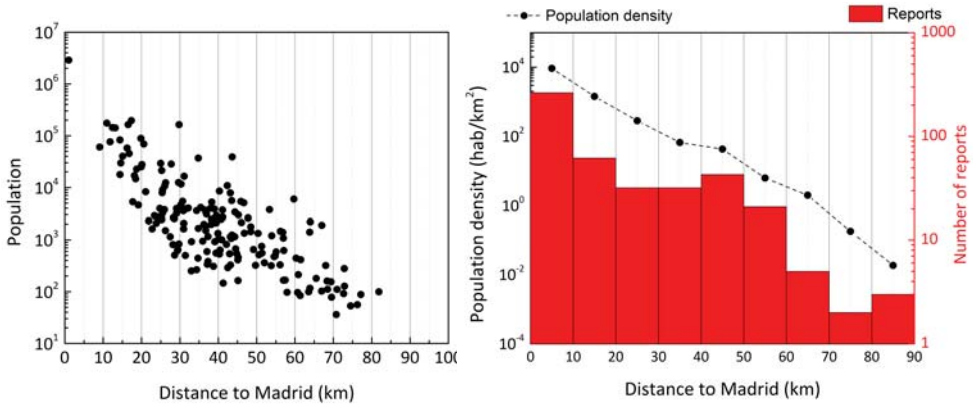

Figure 8. Left: Distribution of population as a function of the distance of villages of Madrid province to the city center, 1996 population (source: INE). Right: Distribution of UFO reports in the province of Madrid as a function of distance to city center.

an area with high population density is an area with *"high probability of witness presence."*

Let us consider the province of Madrid, with the data available in CUCO. Figure 8 Left shows the distribution of population in the villages and towns of the province, according to 1996 demography, as a function of the distance to Madrid city center. There is a clear and simple trend, and the farther a village is, the lower its population. Some towns near the capital (<10 km) were absorbed years ago, and they do not appear anymore as independent towns, but are considered suburbs of Madrid, and their population is included in that of Madrid.

The greater the distance from Madrid, the lower the population of the villages, and the easier it is to find isolated places. According to Poher and Vallée, these places should generate a higher number of UFO reports. We grouped the reports depending on their distance from Madrid in bins of 10 km: from 0 to 10 km, 10 to 20 km, etc., and the trend we find again is to have more reports in places closer to Madrid, which have a *"higher probability of witness presence"* (Figure 8 Right), including places close to the urban areas of Madrid.

Let us remember that such a *fine structure* of the population distribution was also studied by Fernández and Manuel (1980b) for Valencia. They also showed a higher concentration of reports in the vicinity of the three main cities: Alicante (22 cases), Valencia (35 cases), and Castellón (10 cases).

Discussion and Conclusions

We have reviewed several works that studied the patterns in the geographical distribution of UFO reports. These studies had different and apparently contradictory results because they compared different variables. Correlations were recalculated to compare the same variables, and we have shown that all the results are coherent, consistent, and show the same trends.

We have to remark that these trends are also followed by NELIB, a database of negative landings (i.e. IFO cases). Following the idea behind the compilation of this database, we can see that there is no difference between UFO and IFO cases.

There are at least two possible interpretations:

- UFO databases contain a high number of cases that can be solved due to misperceptions, fakes, hoaxes, or any other mundane causes. If there is a real UFO phenomenon, its pattern is hidden within its noise.
- UFO cases are indistinguishable from IFO cases. It is just straightforward to think that all UFO reports can be explained in terms of mundane causes, even if there is not enough information to find that cause.

Regarding the first interpretation, we have to remember that the LANIB database gathered cases with a high strangeness factor, but does not show any difference from other databases.

The so-called First Negative Law stated that the geographical distribution of landings in 1954 was inversely correlated with population density. However, the data available did not allow for making that statement. After checking the data, we found that the correlation of number of reports and population density was direct, although at a low statistically significant value.

A basic and more accurate description of the geographical pattern of UFO reports would be that more reports come from more populated areas, but reports grow in a slower proportion than population does.

Historically, the inverse relationship was interpreted as some kind of intelligence in the UFO phenomenon that avoided populated areas. However, the direct relationship is only a consequence of a higher probability of witness presence in an area, and compatible with a uniformly random distribution of UFO events.

We have checked that this direct relationship is valid for large distributions like continents, counties, or provinces, and also for micro-distributions like villages in a single province. One of the points to take into account is that population density represents a homogenization of an area, and it does not mean that in every square kilometer there are a constant number of people. Therefore, we must understand that in populated places, the probability that any person may witness a UFO event is higher than in low-populated places.

We have been comparing trends, but we have not tried to compare values directly, since as explained in previous sections, there may be many factors contributing to the number of reports in different areas and most likely adding noise. In this regard, Rospars did directly compare values for UFO and IFO distributions belonging to the same catalog, and found no differences when considering departments, but did find highly significant differences when considering the density of French communes.

Is it possible to go into a *finer* structure to find or reject differences? Some analyses have been done with villages, the smallest geographical entities that are usually defined. Density (whatever the field of study) is a variable defined to average over a representative portion, and so reducing the area of averaging would make the value of density meaningless at some point.

Another of the findings of this paper is that even if the total number of sightings increases as population increases, this growth is not lineal, but sub-lineal. That is, doubling the population means that the probability of any person witnessing a UFO should double, too. But the actual number of sightings increases to less than double. What is the origin in this reduction in the proportion of witnessed events as population grows? Is it because fewer people can see UFOs in more populated areas? Is it more difficult to witness those events because of any factor not yet taken into account? Poher, Vallée, and Weiller noted that a lower visibility due to meteorological conditions reduces the probability of a UFO being witnessed, and Condon also suggested the possibility that city lights are reducing the visibility. Are city lights reducing the probability of witnessing events that can be reported as UFOs?

We think that the features of the relationship between UFO sightings and population density are finally well-known, and it should be possible to test different models to reproduce them. López et al. produced a semi-empirical model based on different probabilities: area, population, climate, and orography. Their model assumed a uniformly random UFO phenomenon, and differences in number of reports come from differences in parameters not related to UFOs themselves: It can be regarded as a *null-hypothesis model*.

Breysse studied the relationships using proportional and quadratic (super-lineal) models, but those models are not supported by our results showing a sub-lineal growth. Toulet tried a *contagion* model (Toulet 1974) with interesting results, but it did not take into account population density.

López et al.'s model based on probabilities seems to be the right way to go. An improvement accounting for a lower visibility due to an increase in public lights in more populated areas could be interesting to introduce. Also, adding Toulet's contagion model could be a good basis for Monte Carlo simulations, and the study of UFO waves.

Acknowledgements

The author thanks Vicente-Juan Ballester Olmos for all the information, discussions, comments, and references.

Notes

[1] A rough calculation of the number of pixels in a scanned image is not difficult to produce with image editing software. Higher-resolution images may yield more accurate values. We encourage other researchers to perform their own estimation.

[2] We have to point out that NELIB is included in ALLCAT, and therefore it is not surprising that they yield similar values.

Appendix A: Correlation Coefficient

The mathematical tool most used in the works reviewed here is Pearson's correlation coefficient (Pearson Correlation). This tool allows the study of correlations between the data to be able to establish with some certainty whether a variable is dependent on another.

The correlation coefficient takes a value between −1 and 1. A negative correlation means that the relation is inverse: The dependent variable decreases when the independent variable is increased. A positive value means that the relation is direct: The dependent variable increases when the independent variable is also increased. The correlation (or anti-correlation in the negative case) is stronger when it is closer to 1 (or −1). On the other hand the closer to 0 the weaker the correlation.

From the correlation coefficient, however, the exact mathematical relationship between both variables cannot be deduced. But it can be shown that when the relationship between two variables X and Y is proportional (or lineal)

$$Y = a \cdot X + b \tag{1}$$

the correlation coefficient is closer to 1 (or −1, if $a < 0$). Since there is no way to a priori determine whether a relationship is lineal, a more general relationship can be supposed, such as

$$Y = b \cdot X^a \tag{2}$$

Taking logarithms at both sides, and applying their properties, we arrive at:

$$\log Y = a \cdot \log X + \log b \tag{3}$$

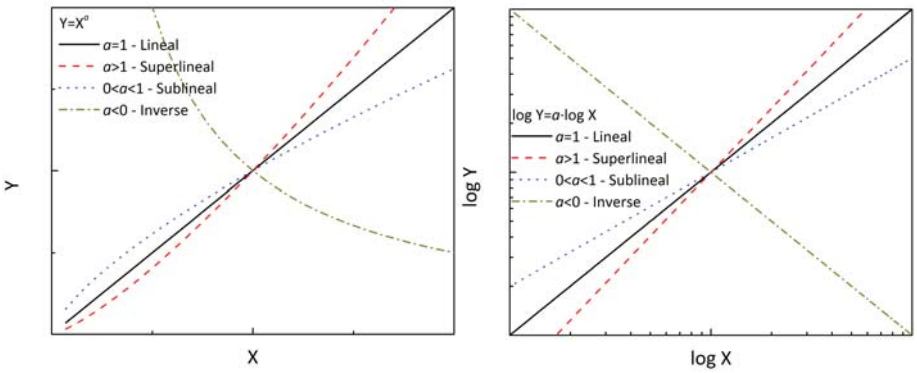

Figure 9. Left: Direct relationships (lineal, sub-lineal, and super-lineal), and inverse relationship between *X* and *Y*. Right: The same relationships between log *X* and log *Y*.

If there is a lineal relationship between log X and log Y, the correlation coefficient would tend to its maximum value (or minimum if $a < 0$), and the correlation is stronger than that without taking logarithms.

Appendix B: Lineality and Sub-Lineality

Equation 1 represents a lineal relationship between X and Y. Equation 2 is a more general equation, which includes a particular case Equation 1 when $a = 1$. When the relationship is lineal, it means that Y increases in the same proportion in which X is increased. That is, if X is doubled, Y doubles.

When $a > 1$, Y increases in a proportion higher than the increase in X. It is a super-lineal relationship. And when $a < 1$, but $a > 0$, the proportion in which Y increases is lower than the proportion in which X is increased, and it is a sub-lineal relationship. In these three cases, the correlation coefficient between log X and log Y is positive, showing a direct relationship.

In the last case, when $a < 0$, we find an inverse relationship, and thus, the correlation coefficient between log X and log Y is negative. Figure 9 shows these four cases graphically.

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RESEARCH ARTICLE

Multivariate Entropy Analysis of Oxidative Stress Biomarkers Following Mobile Phone Exposure of Human Volunteers: A Pilot Study

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Abstract—A multidisciplinary project was conducted to study the possible biological impact of mobile phone emissions. As part of that project, we conducted a pilot study on 18 human volunteers, with the treatment being GSM mobile phone exposure. The volunteers were randomized and the study was a double-blind, crossover design. Two categories of oxidative stress biomarkers were followed and measured in blood and exhaled air: those assessing oxidative attacks of cell membrane lipids (malondialdehyde, exhaled alkanes, aldehydes, and isoprene) and those accounting for the

organism's antioxidant defense systems (superoxyde dismutase, glutathion peroxydase, and exhaled halogenated alkanes). The overall entropy of the system with and without GSM exposure was then calculated for each volunteer, using a statistical approach based on the global entropic difference of raw data. A significant modulation of organization of the biomarkers after 30 minutes of mobile phone exposure was found, as evidenced by a decreased entropy of the dataset associated to the emitting mobile phone condition. While these results illustrate neither deleterious effects nor the innocuity of mobile phone use, they nonetheless constitute evidence of actual interactions of these wavelengths with complex biological systems. These results will need to be confirmed in larger, future studies.

Introduction

Despite much contrary evidence, there is continued concern that electromagnetic fields (EMF) might affect human health (Kheifets, Swanson, Kandel, & Malloy 2010, Repacholi, Saunders, van Deventer, & Kheifets 2005). In addition to the extremely low frequency EMF (ELF) emitted by domestic electrical apparatus and supply lines, mobile phones are the most common source of daily exposure to EMF. The GSM ("Global System for Mobile") network and its more recent evolutions, emitting fields from 900 to about 2,000 MHz, are the most widespread form of mobile communications (http://www.gsm.org). The rapid expansion of these devices and dedicated infrastructures very legitimately raised questions about the effects of their associated radiofrequencies (RF) on human health, making this topic the subject of much public health debate. As of today, research has been very active but failed to reach consensus, as data and conclusions remain anything but clearcut (Genuis 2008, Carpenter, & Sage 2008, Otto & von Muhlendahl 2007).

While numerous studies conclude that there is a lack of a biological effect or association with pathology, other results from both in vitro or in vivo animal studies point to possible effects (Lerchl, Kruger, Niehaus, Streckert, Bitz, et al. 2008, Panagopoulos, Chavdoula, Nezis, & Margaritis 2007, Ammari, Lecomte, Sakly, Abdelmelek, & De Sèze 2008, Del Vecchio, Giuliani, Fernandez, Mesirca, Bersani, et al. 2009, Ragbetli, Aydinlioglu, Koyun, Ragbetli, Bektas, et al. 2010). Physiological biomarkers–based studies on human volunteers exposed in vivo are not as widespread (Soderqvist, Carlberg, Hansson Mild, & Hardell 2009), in contrast to studies centered on behavioral and cognitive endpoints (Panda, Jain, Bakshi, & Munjal 2010, van Rongen, Croft, Juutilainen, Lagroye, Miyakoshi, et al. 2009), studies describing ex vivo exposure of human samples (Belyaev, Markova, Hillert, Malmgren, & Persson 2009), or classical epidemiology/meta analysis studies (Makker, Varghese, Desai, Mouradi, & Agarwal

2009). Importantly, RF and sham exposures of humans require strong multidisciplinary and interdisciplinary control and knowhow, ranging from the exposure setup itself to the biomarkers and the statistical analysis. Based on the putative effects of EMF on cellular components, some of these studies, alongside numerous animal studies, have focused on the potential influence on oxidative stress (Moustafa, Moustafa, Belacy, Abou-El-Ela, & Ali 2001, De Iuliis, Newey, King, & Aitken 2009, Agarwal, Desai, Makker, Varghese, Mouradi, et al. 2009, Tomruk, Guler, & Dincel 2010, Dasdag, Akdag, Ulukaya, Uzunlar , & Ocak 2009). Indeed, the equilibrium between oxidant and reductive species is precisely regulated, making this balance a suitable and important target for the investigation of possible effects of EMF. The usual ways to investigate this phenomenon rely on either monitoring the activities of the antioxidant defense systems per se or, alternatively, examining the presence of the free radicals directly or that of byproducts of their interactions with biological macromolecules.

In the present study, the activity of superoxide dismutase (SOD), which specifically removes superoxide anion and that of glutathion peroxidase (GPx), scavenging various peroxides via concomitant oxidation of glutathione, were monitored as prominent actors in the enzymatic antioxidant system (Gelain, Dalmolin, Belau, Moreira, Klamt, et al. 2009, Giustarini, Dalle-Donne, Tsikas, & Rossi 2009). On the other hand, volatile organic compounds (VOCs) such as alkanes (in native or halogenated states, referred here to as, respectively, BAA and BHA), aldehydes and isoprene found in the breath, and malondialdehyde (MDA) in erythrocytes were assayed as products of degraded lipids, downstream of the initial oxidizing attack of the free radicals (Phillips 1997, Phillips, Herrera, Krishnan, Zain, Greenberg, et al. 1999, Phillips, Cataneo, Greenberg, Grodman, Gunawardena, et al. 2003, Phillips, Cataneo, Ditkoff, Fisher, Greenberg, et al. 2003).

We evaluated the biomarkers' modifications in the course of the study with standard ANOVA tests, but more importantly using an innovative statistical analysis strategy consisting of entropy calculation to measure the degree of organization of the whole system (Durbin, Eddy, Krogh, & Mitchison 1998). The portions of the global dataset (comprising every single measurement of the study) corresponding to either the treated or the untreated conditions could then be compared to highlighted changes in their respective entropy. Our global effect analysis is expected to remain robust and effective in addressing questions such as the present one where wide inter-individual and even intra-individual variations are described, which might otherwise hide finer effects if any are present. The entropy calculation method is thus central to the present study. Noteworthy, the biomarkers were collected for

24 human volunteers before and after a single exposure to a 30-minute GSM emission (900 MHz, SAR [specific absorption rate] 0.3 W/kg). The phone was positioned with respect to each volunteer's head as it would be during regular operation, albeit through the use of a hands-free attachment system eliminates the need to actually grasp and hold the device.

Importantly, our results indicate a significant change in the organization of the biomarker panel constituted by the totality of each sample and individual systemic redox-related species analyzed. As such and under carefully controlled conditions, this work illustrates biological modulations that can only be attributed to a non-thermal effect of GSM exposure. Naturally, these studies need to be confirmed with a greater number of volunteers and integrated in a wider research project on the effect of mobile phone emissions on human biomarkers using lifelike (typical) exposure.

Materials and Methods

Ethics Statement

Each detail of the investigation was approved by the Comité de Protection des Personnes (CPP), the French commission in charge of ethical questions and agreements related to studies on human volunteers. The latter were made fully aware of the experimental procedure using a walkthrough document prior to giving their written consent.

Subjects

Healthy adult individuals (12 women and 12 men, age between 20 and 35 years) volunteered to participate in this study. 3 volunteers out of 24 were excluded from this study because they displayed high rates of C-reactive protein, a biomarker of inflammation (Elkind 2010, Genest 2010, Kaysen 2009), and three others did not participate integrally, not allowing full collection of the required samples. Eventually, 18 volunteers fully completed the study. All subjects were non-smoking, were not taking any medication, and did not regularly engage in intensive physical activities. There were no detectable caries in their mouths nor were there any signs of inflammation before exposure. On the day of the experiment, volunteers were kept 1 hour at rest before installation of a collection catheter, experimental samplings, and mobile phone exposure, "On" or "Sham".

Mobile Phone Exposure

Each volunteer participated in 2 one-day sessions, separated by a week, one including an actual EMF exposure (test) and the other a simulated (SHAM)

pseudo-exposure without any kind of emission; the day of actual exposure was randomly distributed among volunteers. A Motorola dual-band mobile phone, M3688 (900–1800 MHz), was used and located near the right ear using a PVC head holder, as previously used in the COMOBIO study of Maby, Le Bouqin, Faucon, Liegeois-Chauvel, and De Sèze (2005). This system guaranteed good reproducibility of phone positioning. During the actual exposure session, participants underwent a 900 MHz radiofrequency field for 30 min, pulsed with a repetition rate of 217 Hz with a pulse width of 0.576 ms at 250 mW mean full power. The SAR over 10 g of tissue, calculated and measured as specified on the IEC 85-214 standard, was 0.3 W/kg for the actual emission. The EMF exposure was carried out under double-blind conditions: Phones were activated through a test card controlled by codes on the keyboard by one experimenter, but on half of the phones the RF signal was routed to an internal resistance. A second investigator handled and installed the phone on each volunteer, ensuring the local experimenter and volunteers were not aware of the actual status of emission.

Exposure Control

Continuous monitoring of all exposures was performed through 2 PMM 8053 recorders during the experiments. The recorders were hidden so that volunteers could not see them nor be troubled by unknown devices. The electric field was measured every 10 seconds during the 4 hours of one single session for 4 volunteers. Each day, recordings were downloaded on a PC and sent to external collaborators who checked the correct course of the study. Samples from blood and expired air were collected in the morning and in the afternoon (three time-points before exposure and three time-points after exposure). Three markers were measured in blood: GPX, SOD, MDA. Four markers were measured in expired air: BAA, BHA, Aldehyde, Isoprene. The original design was evenly randomized in order to separate exposure and day effects. The exposed or non-exposed status was disclosed to the experimenters only after the data was statistically analyzed.

Sampling

A first batch of blood and exhaled air samples was collected during a 1-hour window prior to exposure. After the 30 minutes of mobile phone exposure, three additional samplings were performed, three exhaled air samples were collected during the first hour following exposure and three blood samples collected within two hours following exposure. Logistics and apparatus requirements made simultaneous cohort-wide sampling impossible, as such

samplings were individually scheduled to ensure volunteers had comparable time points on both experimental days.

Peripheral blood was drawn from an intravenous catheter (Jelco 18G, Rossendale, UK) with subjects in a sitting position. 5 mL of whole blood was collected in a lithium heparin tube (BD Vacutainer LH 119IU, Plymouth, UK), inverted 3 times, and spun immediately in a centrifuge (2,000×g, 15 minutes at room temperature). Supernatant was discarded and red blood cells were washed 2 times with a cold isotonic NaCl 0.9% solution by inversion and centrifugation (2,000×g, 5 minutes at room temperature). After each centrifugation, the supernatant and white blood cells interface were removed. Erythrocytes were stored in 500 µL aliquots at –20 °C prior to analysis.

Collection and gas chromatography–mass spectrometry (GC–MS) of the volatile organic compounds in human alveolar breath were performed using a dedicated, transportable apparatus (Exp'Air, AR2i, France) that specifically sampled alveolar breath. Briefly, the technique carried out in this study is a concentration method using a pump and active carbon-containing cartridges. Subjects are breathing room air through the apparatus via a set of valves, but upon reaching the end of each breath a procedure is carried out to isolate the final exhalation corresponding to the alveolar breath. This enabled collection of a concentrated alveolar breath sample through the Carbotrap 200 glass tube (Supelco, Sigma-Aldrich, France) across 2 minutes of effective pump work. After collection using this transportable system, the adsorptive trap was removed and sealed in a screwtop glass storage container prior to GC–MS analysis for volatile compounds.

Biomarkers Analysis

SOD: Superoxyde dismutase activity (SOD Cu/Zn) was measured with a commercial kit (RANSOD, Randox Laboratories, Ardmore, UK) based on the method developed by McCord and Fridovich (1969).

GPx: Glutathion peroxidase GPx activity was determined with a commercial kit (RANSEL, Randox Laboratories, Ardmore, UK) using a method based on that developed by Paglia and Valentine (1967).

MDA: A fluorimetric method, developed by Conti, Morand, Levillain, and Lemonnier (1991), was used to determine total MDA in erythrocytes. Briefly, 50 µL of erythrocytes solution was added to 1 mL of 10 mmol/L diethylthiobarbituric acid (DETBA) in phosphate buffer (0.1 mol/L, pH 3). The mixture was mixed for 5 s and incubated for 60 min at 95 °C. Samples were placed in ice for 5 min and then 5 mL of butanol was added. The DETBA-MDA adduct was extracted by shaking for 1 min, then centrifuged at 1500×g for 10 min at 4 °C. The supernatant was collected

and the fluorescence intensity measured at 553 nm. Results were quantified by comparison with the standard curve obtained with standard solutions prepared in the same conditions.

Expired alkanes, halogenated alkanes, aldehydes, and isoprene: The samples were thermally desorbed, and alkanes and halogenated alkanes were analyzed using a gas chromatograph GC 8000 TOP coupled with the mass-spectrometric detector device Automass II (electron ionization) led by Xcalibur software (Finnigan Corporation, Quad Services, Poissy, France). Compounds were identified by reference to a computer-based library of mass spectra.

Statistical Analysis

A multiple testing strategy (multivariate and univariate) was planned at the outset of the conception of this study, based on observations derived from a similar but unrelated in-house study. This resulted in one single multivariate p-value associated with entropy calculation along with marker-specific univariate p-values. As a consequence of that configuration, the single multivariate p-value was multiplied by two to adjust for test multiplicity, while univariate p-values were corrected for multiplicity using Bonferroni correction and then multiplied by two as well.

ANOVA test: For each volunteer on each day, there are 6 values for each biomarker, 3 samples collected before exposure (T01–T03) and 3 samples collected after (T1–T3). For each day and biomarker, the values T01, T02, and T03 were averaged, lending a single value. 3 ratios were then computed by dividing each of the 3 post-exposure measurements (T1, T2, and T3) by this average. As a consequence, the final dataset for each of the latter comprise 6 entries: 3 ratios of sham exposure and 3 ratios of actual GSM exposure (at this point the type of exposure is still undisclosed; only exposure days are known). A global mean effect was eventually calculated based on these 6 entries and subtracted from each individual ratio, leading to the final dataset suited for ANOVA analysis. A regular two-way ANOVA was computed, including correction multiplicity targeting "group" and "volunteer" effects on the ratio of afternoon measurements over the average of morning measurements. There are 7 biomarkers under consideration and a multiple testing strategy (multivariate and univariate), so a Bonferroni factor of 14 was used for multiplicity correction. Information about the nature of exposure for all volunteers on each day is then disclosed for final presentation of results.

Multivariate analysis: To perform multivariate entropy analysis, a table was set up with 126 columns (18 volunteers × 7 biomarkers) by 12 rows (6 samples × 2 days). The first six rows of non-exposure (sham)

were labeled "A" and referred to as *dataset OFF*, while the last six rows of EMF treatments were noted "B" and referred to as *dataset ON*. Note that exposure was randomized so as to distribute the actual or sham exposure on different days for the different volunteers.

Entropy calculation: Entropy is a statistical measure of randomness that can be used to characterize the texture of the input image. Entropy is defined as: $-\Sigma(p*\log(p))$ where p contains the histogram of the gray levels used in the image. Each column of the dataset ON or OFF was scaled (divided by its average) to put columns on a comparable scale. The entropy calculation depends on resolution, the number of bins in the histogram. An insufficient resolution results in a sharp decrease in the entropy, merely indicating that substantial information has been lost. The entropy achieved with three or four decimal points is still close to the maximum reached when the number of decimal points exceeds five, so we decided to run the permutation test with three and four decimal points and considered the mean p-value.

Results and Discussion

18 of the recruited 24 volunteers completed the study. The 30-minute EMF duration was chosen since it represented a significant exposure while remaining plausibly close to normal usage. Figure 1 illustrates the whole sampling and exposure strategy, described further here. Following their inclusion in the study, volunteers committed to following strict guidelines at least for one whole day (or more, whenever possible and if desired) before participation in the experiments. The latter included avoiding use of personal mobile phones, alcohol and pharmaceutical drugs intake, intense physical activity, and any possibly stressful situation. Upon their arrival by means of non physically exerting transportation means on the day of the experiments, volunteers were first invited to rest for one hour in a relaxed sitting position to reduce extraneous stress and to allow for gentle adaptation to the conditions of the study. As Figure 1 indicates, the first collection of both breath and blood samples took place after this resting period: Over the span of another hour, 3 breath and 3 blood samples were collected for each volunteer as described in Materials and Methods. These samples thus constituted the resting, pre-exposure status for all biological parameters explored, referred to as "T0". Volunteers then underwent either the sham or the actual GSM exposure depending on the group they were assigned to. Indeed, the two different modes of exposure took place for each participant on two different days separated by a whole week and distributed in a double-blind, randomized fashion. Thus, one half of the volunteers received a sham exposure while the other half underwent actual exposure

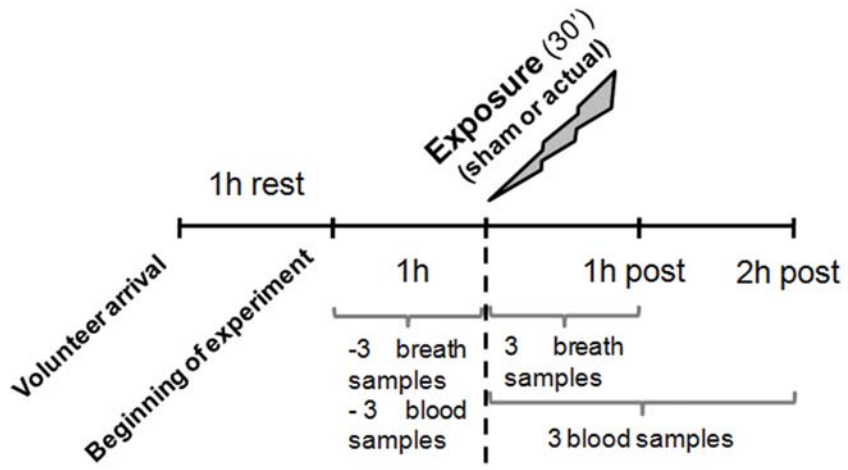

Figure 1. Sampling protocol implemented during each experimental day. Two similar experimental days took place across two consecutive weeks, with each volunteer randomly ascribed either to the sham or actual GSM exposure group for any given day. Each volunteer experienced the two types of exposures at the end of the experimental campaign.

on day 1 and vice-versa on day 2, enabling us to cancel out the day effect. In between the two experimental days, separated by exactly one week, the participants were asked to abide by the aforementioned daily-life behaviors as thoroughly as possible to reduce possible impact of these activities on any subsequent measurements. The experimenters made sure to carefully monitor that over the duration of the study no volunteer developed skin redness, rashes, or expressed discomfort or illness of any kind at any time. Local heating due to the exposure device (mainly the phone itself but also to the bands and supports used to reproductively position it against the volunteers' heads) was virtually non-existent regardless of the type of exposure, as were the local thermal effects expected from the actual GSM emission. Hence at the end of the study, each participant had undergone the two types of exposures, sham and GSM, without themselves or the operators being aware of which exposure was applied at any given moment. Disclosure of the exposure pattern and data assignation was carried out only after all statistical analyses were performed. The study was triple-blind; neither volunteer, experimenter, nor the statistician knew the treatment until after the analysis was entirely computed and results gathered. As again illustrated in Figure 1, 3 breath samples were then collected in the span of the first hour immediately following either treatment, while 3 blood samples were drawn in parallel, but on a slightly longer time interval of two

TABLE 1
Mean Blood Antioxidant Enzymatic Activities and Breath Oxidative Metabolism Markers Before (T0) and After (T) Sham or Actual EMF Exposure as Measured in the Entire Volunteer Panel

		Erythrocytes			Breath			
		GPx (U/g Hb*)	SOD (U/g Hb*)	MDA (nmol/g Hb*)	BAA (index)	BHA (index)	Isoprene (ng)	Aldehyde (ng)
Sham	T0	57.5 ± 3.0	1198 ± 15	25.5 ± 1.0	6.3 ± 0.6	6.3 ± 2.0	1302 ± 161	439 ± 36
	T	57.2 ± 3.2	1186 ± 15	24.9 ± 0.9	6.1 ± 0.5	4.0 ± 1.1	1421 ± 180	417 ± 35
EMF	T0	58.1 ± 3.1	1200 ± 12	25.4 ± 1.0	5.2 ± 0.4	3.3 ± 0.7	1093 ± 131	424 ± 33
	T	56.9 ± 3.1	1188 ± 14	24.9 ± 1.0	6.3 ± 0.4	3.7 ± 0.6	1512 ± 140	486 ± 38

Values are means ± S.E.M.
Hb = Hemoglobin, GPx = Glutathion Peroxidase, SOD = Superoxide Dismutase, MDA = Malondialdehyde, BAA = alkanes, BHA = Halogenated alkanes.

hours. These samples were denoted T1–T3, and refer to the post-exposure status of all parameters followed. This slight offset in sampling time reflects the respective expected onsets of the various biomarkers monitored: Breath markers of oxidative stress are known to appear relatively early while blood antioxidant enzymatic activities or peroxylipids were expected somewhat later, although they also are known to be rather quick-responding systems (Larstad, Toren, Bake, & Olin 2007, Corradi, Alinovi, Goldoni, Vettori, Folesani, et al. 2002). Table 1 lists the results of the measurements of blood antioxidant enzymatic activities and breath oxidative metabolism markers thus obtained, as described in more detail in Materials and Methods. All values obtained for the 3 samples of either matrix, both before and after exposure and for every volunteer, were averaged in the present set. Intuitively, the results therein do not seem to point to an obvious influence of the exposure on whatever biomarker was considered. However and importantly, it should be noted that these calculations are, in the present form, not intended nor suited for actual statistical analysis, as they lack crucial computations such as subject effect and confidence interval among other things. Table 1 is given here for reference and serves to ascertain that the measured levels of each parameter are in line with those expected (simultaneously shedding light on sampling and measurement quality).

That said, application of the entropy analysis itself was done on the global dataset comprising every single measurement / experimental point taken individually, as described in Materials and Methods. This process resulted in two datasets: The ON dataset corresponded to the matrix of 126 columns (7 markers × 18 individuals) by 6 rows (3 T0 + 3 T1 values measured during exposure day), whereas the OFF dataset corresponded to the similar matrix of 126 × 6 values measured during non-exposure day. In order to make a quantitative assessment of the level of contrast in the ON dataset, we calculated its entropy. Intuitively, the most organized dataset has the lowest entropy. Now, the question of how low the entropy should be to prove significantly low can be answered by applying a resampling-based testing strategy (Westfall & Young 1993): For this we compared the entropy of the dataset ON with the entropies of thousands of permuted datasets obtained through random selection of eight individuals arbitrarily exposed to GSM emission on day 1, whereas the ten remaining individuals were arbitrarily exposed on day 2. Specifically, each permuted dataset was obtained by: (i) picking up at random 8 individuals among the 18 individuals, (ii) labeling them as exposed to GSM emissions on day 1 (independently of the actual day of exposure for each individual), (iii) labeling the 10 other individuals as exposed to GSM on day 2 (again independently of the actual day of exposure for each individual). The number 8 was chosen to reflect the fact that due to the initial randomization of exposure sequences on the 24 starting volunteers and the completion of the study by only 18 of them, only 8 volunteers were exposed to the actual GSM emission on day 1. In these conditions, there were 43,758 possible permutations, one of them resulting in the particular dataset ON. We then counted the number of the random datasets generated, which entropies prove lower than the tested ON dataset. The ratio of this number over the total number of random datasets constitutes the significance (p-value) of the entropy of the tested dataset.

Another important point to be noted is that null values for BHA and Isoprene levels were naturally found in numerous samples because of the known detection limit of the technology used and with regard to common physiological levels. However, these measurement values could not be simply set arbitrarily at the detection limit in the original dataset because of the important number of identical values it would have generated. Indeed, random permutations that would assign values that are all equal to the detection limit into the same dataset would artificially present much lower entropy, and background noise of such datasets would be grossly underestimated. Thus, in order to prevent a bias in our multivariate test, we imputed random values for samples measured below the detection limit using a uniform distribution (in order to minimize assumptions on the error

TABLE 2
Significance of the Entropy Represented by the p-Value of the Tested Sub-datasets ON and OFF

	Sub-dataset ON	Sub-dataset OFF
Raw data	0.0202 *	ns
Raw data without outliers [a]	0.0169 *	ns
Raw data without outliers lines [b]	0.0205 *	ns

* significant ($p < 0.05$).
ns: not significant.
[a] 7 outliers out of 864 values.
[b] 28 outliers out of 864 values.

model). Since ON and OFF state values could fall below the measurement threshold, both types of measurements could result in missing values. Consequently, our test statistic is not biased by imputation.

Nevertheless, the entropy reduction might turn out to be unduly sensitive to the model used to fill in these missing values. Therefore, to ensure that our multivariate test stood robust to this imputation, a spectrum of 30 different datasets was created, each with imputed values below detection limit, prior to application of the multivariate test on each of these datasets. Eventually, for 23 out of the 30 tests, entropy of dataset ON was found to be significantly lower than the usual level of 0.05. Specifically, p-values ranged from as low as 0.005 up to 0.074, with median p-value equal to 0.0202. Since we have a multiple testing strategy (multivariate and univariate), we applied a simple multiplicity adjustment by multiplying the p-value by two. Even then, the median p-value was still lower than 0.05, showing that knowledge of exposure status while constructing the dataset resulted in a significant decrease in the entropy of dataset B. The "cleaner," more homogenous organization of dataset ON could only be attributed to an event taking place between sample collection in the morning and samples collected in the afternoon, hence most probably the exposure to the radiofrequencies. Indeed and importantly, note that if this greater organization were just a time-of-day effect, we would have seen more organization on the sham treatment day as well (dataset OFF). On the contrary, the p-value for dataset OFF always proved insignificant for any of the created datasets (p-value > 0.5).

TABLE 3
ANOVA Calculation and Biomarkers Expression Comparison between Sham or Actual EMF Exposure as Measured in the Entire Volunteer Panel

	Erythrocytes			Breath			
	GPx	SOD	MDA	BAA	BHA	Isoprene	Aldehyde
Sham exposure [a]	0.98	0.99	1.00	1.02	1.36	2.13	1.03
EMF exposure [a]	0.99	0.99	1.00	1.50	2.81	2.44	1.24
% Change [a]	1%	0	0	+47%	+107%	+15%	+24%
p-value	0.8462	0.9037	0.9179	0.0013 [b]	0.0333 [b]	0.6516	0.0932
Corrected p-Value [d]	1	1	1	0.0182 [c]	0.4662	1	1

[a] Values are least square means, percent change calculated as increase of EMF mean vs. Sham mean.
[b] Significantly different from sham exposure using ANOVA test ($p < 0.05$).
[c] Significantly different from sham exposure after adjustment for test multiplicity ($p < 0.05$).
[d] Bonferroni correction using a factor of 14 (7 biomarkers and both multi- and univariate testing strategies).

Additionally, we applied the multivariate testing strategy to the dataset cleared from outliers after implementation of a dedicated statistical analysis. Results became even more significant: Depending on whether we either mathematically removed only the seven found outliers or even more stringently the entirety of measurements datamined from breath-collection cartridges displaying such outliers, median p-values were found to be equal to 0.0169 and 0.0205, respectively. Regardless, all p-values based on different imputations remained below 0.05. The integrality of these results is summarized in Table 2.

We also applied more classical statistical tools such as least square means and ANOVA calculation to the measurements obtained. As shown in Table 3, notable increases were only evidenced in all expired air markers, albeit only the BAA increase eventually proved significant, with a p-value of 0.0013, and remained so even after stringent multiplicity correction, with a final p-value of 0.0182. These observations are paramount in the sense that they vouch for the superior relevance of the entropy calculation strategy in the context of extended, wildly varying datasets. These may indeed present minute variations across the board (something which is expected for the complex networks and equilibriums of physiological systems), albeit with no one parameter individually shifting enough to register positive via classical approaches, even though this was not the case here as illustrated by the BAA. Of note, all the permutations and corrections implemented

during entropy assessment ensured that this single result alone could not "weigh in" enough to explain the observed modulations, which is owed to the global evolution of the entire dataset versus the impact of only a handful of measurements.

Conclusion

The results of the present study, at least in our experimental conditions, support the hypothesis of non-thermal effects of mobile phone-emitted electromagnetic fields on biological tissues and/or functions. As such, the data illustrate modulations of chosen biomarkers associated with or directly implicated in oxidative stress response/onset in human organisms. This is in agreement with observations reported in other studies, although in very different experimental conditions, which in the end tends to enforce the initial statement (Xu, Zhou, Zhang, Yu, Zhang, et al. 2009, Ozgur, Guler, & Seyhan 2010, Kovavic & Somanathan 2010).

The manifestations of oxidative stress-related events were chiefly evidenced by a significant increase of alkanes in the breath of volunteers (at the exclusion of all other pro-oxidant actors monitored) who participated in the study. In parallel, the activities of the antioxidant enzymes SOD and GPX did not seem to be modified in erythrocytes. However, it should be noted here that the aforementioned results were obtained through a classical ANOVA testing that, in essence, singles out each biomarker and the various measurements associated with them. Interestingly, the implementation of the multivariate, entropy-based analysis of the dataset taken as a whole (as opposed to segmented into each individual biomarker and value) painted a much different picture. The latter revealed global modulations across the board, modulations that appeared individually, with the exception of BAA, too minute to score significantly for a given parameter but in the end confirming a global interaction of the applied electromagnetic fields upon the entirety of the data gathered.

Although it is likely, still, that the overall oxidative balance was not dramatically offset by the mobile phone exposure in the end, it is important to remember that all biomarkers chosen were systemic and monitored in the complex compartments and interfaces that blood and breath constitute. Hence, the potential relevance in terms of biological effects, while impossible to ascertain based solely on these observations, should not be too hastily ignored.

In conclusion, these data illustrate interactions of EMF emitted by mobile phones with whole biological systems, in a context that still presents no clear consensus (Verschaeve, Juutilainen, Lagroye, Miyakoshi, Saunders, et al. 2010, Gaestel 2010, Ziemann, Brockmeyer, Reddy, Vijayalaxmi, Prihoda, et

al. 2009, Kundi & Hutter 2009) regarding that particular issue. Results must naturally be confirmed with more subjects in a similar experimental setup; nevertheless, the design proposed here undoubtedly allows for appropriate statistical robustness and relevance of exposure and readouts in its present form. It would surely prove very enlightening to extend this investigation to chronic EMF exposures in an identical context in order to explore potential, longer-term effects of the use of cellular phones in human organisms.

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HISTORICAL PERSPECTIVE

Telepathic Emissions: Edwin J. Houston on "Cerebral Radiation"

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Abstract—Interest in telepathy during the Nineteenth Century developed in the context of ideas of magnetic, nervous, and psychic forces said to project from the physical body to cause various phenomena, as seen in the literatures of mesmerism, Spiritualism, and psychical research. An article about cerebral radiations authored by American electrical engineer Edwin J. Houston in 1892 is reprinted here and commented upon. Houston speculated that cerebral waves were projected to other brains via the ether, a process involving resonance with a similarly disposed brain. These ideas were affected by concepts from physics dealing with such forces as magnetism and constructs like the ether. In fact, the phenomena of thought-transference stimulated many speculations involving ideas of brain waves and radiations that were part of a tendency to reduce unexplained phenomena to physical and physiological principles, or in this case biophysical ones. Houston's paper is one of the most detailed presentation of ideas of this sort published in the Nineteenth Ccentury. Nonetheless, Houston showed no familiarity with the literature of his time about telepathy. While his article did not originate ideas of this sort, it is representative of speculations of the period about what today we refer to as physical transmission models of ESP.

Keywords: Edwin J. Houston—telepathy—thought-transference—history of telepathy—brain waves—cerebral radiations

Let it be granted that whensoever any action takes place in the brain . . . an atomic movement occurs . . . Let it also be granted that there is . . . [an] "Ether" . . . But if these two assumptions be granted . . . should it not follow that no brain action can take place without creating a wave or undulation . . . in the ether . . . Each acting, thinking 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 . . .

—James T. Knowles (1869:136)

Introduction

In his book *The Kingdom of Man*, English zoologist and biologist E. Ray Lankester (1847–1929) wrote that wireless telegraphy did not offer support to the idea of thought-transference because:

> the important factors in such human intercourse—namely, a signalling-instrument and a code of signals—have not been discovered, as yet in the structure of the human body . . . (Lankester 1907:88)

The idea of such signals was inspired by the fast popularization during the Nineteenth Century of communication technology and the establishment of various physical processes, such as the various forms of radiation (L'Annunziata 2007). This affected conceptions of psychic phenomena, producing a corpus and conceptual tradition of physical and biophysical ideas that, in turn, were part of attempts to naturalize the unusual during the Nineteenth Century (e.g., Gonçalves & Ortega 2013, Méheust 1999). Such concepts ranged from accepted principles and forces—such as electricity and magnetism—to a variety of unorthodox ideas of human magnetism, nervous and psychic forces, kind of a psychical physics offering explanations for the mental and physical phenomena of psychical research.[1] These ideas were part of modernism, in the sense that various physical and psychical forces were important for technology, for beliefs, as well as for social and cultural developments, including the arts (Enns & Trower 2013).

Such physical transmission ideas are still around, as seen in modern speculations about ESP (e.g., Marciak-Kozlowska & Kozlowski 2013, Vasilescu & Vasilescu 1996). However, speculations involving hypothetical physical, or biophysical processes, were prevalent during the late Nineteenth Century (and even later), which is the topic of this paper, in which I focus on physical explanations of telepathy.

My purpose is to present a reprint of an article relevant to the history of these ideas. It is a paper published in 1892 by American engineer and electrician Edwin J. Houston about speculations of cerebral radiations to explain telepathy. The excerpt presented here is a good example of early examples of physical transmission models of ESP. The article is in fact one of the most detailed discussions of such ideas of the time.

In addition to being a good representative of old transmission models, the paper is of interest as well for other reasons. First, it was written by a prominent individual who was not identified with psychic phenomena, but with other disciplines. Second, the paper, while not widely cited in psychical research circles, was published in several non-psychic American publications, thus helping to popularize psychic phenomena. Third, Houston's article reminds us that, in addition to ideas of nonphysicality and mind–body independence emphasized by some parapsychologists,[2] and certainly prominent both in the past as in the present, the Nineteenth Century also had its share of physicalistic assumptions.

The Context: Forces and Psychic Phenomena

Psychic Forces

Nineteenth-Century physics brought us much information about forces of nature, among them electricity, light, electromagnetism, and concepts such as the ether and the conservation of energy.[3] In his work *When Physics Became King*, Morus (2005) documents how such work transcended scientific circles and influenced society in many ways. I argue, as others have done before (Luckhurst 2002), that this was the case as well with physical concepts and psychic phenomena, in this case telepathy.

The so-called imponderables, principles such as magnetism and electricity that had a subtle physical nature (Heilbron 1993), inspired many speculations about psychic phenomena. An important early one involved the concept of animal magnetism, a universal force interacting with physical and biological matter, discussed by Franz Anton Mesmer (1734–1815; 1779) and by his followers, which formed the movement of mesmerism.[4]

The construct of animal magnetism suggested new ways in which human beings were related to nature, a more intimate connection with the surrounding forces, the forces of existence. This magnetism, Segala (2001) has suggested, connected humans more with the cosmos and was a symbol of universal harmony, while pointing to the idea of the unity of forces. Animal magnetism (Mesmer 1779), which some believed was part of human physiology, and the principle of life itself, was described sometimes as a fluid:

> The magnetic fluid continuously escapes us: It forms an atmosphere around our body . . . which . . . does not act noticeably on individuals around us; but when our will pushes and directs it it moves with all the strength that we impart: It moves like light rays sent out by bodies ablaze. (Deleuze 1813:89; this, and other translations, are mine)

Another mesmerist stated: "Man is a congeries of forces all under the regulation of magneto–electric laws. He is, in fact, a magnet . . ." (Ashburner 1867:193). It was this force, or radiation, or fluid, that was believed to act from the mesmerist to the mesmerized person (or somnambule) and cause healings and other phenomena such as the transmission of the thoughts of the mesmerizer (for a pictorial representation of animal magnetism, see Figure 1).

Many mesmerists believed that magnetized individuals could perceive the thoughts of others. In Italy it was stated that after the magnetizer imparted his vital fluid to his subject, both shared a common vitality that allowed the latter to feel "different sensations experienced by the magnetizer" (Guidi

Figure 1. Illustration of the concept of animal magnetism (Lafontaine 1852).

1854:228). Such transmissions were believed by a mesmerist in the United States to be due to the fact that "the magnetic fluid passes from the brain and eye . . . of the *magnetizer* to the eye . . . and brain of the magnetic somnambulist . . ." (Durant 1837:79). In addition to this literature, and to later neo-mesmeric developments that included the work of Alexandre Baréty (1844–1918) and Julian Ochorowicz (1850–1917) (e.g., Baréty 1887, Ochorowicz 1891), over the years there were many discussions about human radiations to explain various phenomena.

These ideas of nervous and vital forces flourished in the context of Spiritualism and, later, in psychical research. The medium, instead of the somnambule, was the dynamo generating forces to produce all sorts of phenomena, including physical ones such as movement of objects and materializations.[5] As stated in an early classic of table turning:

> If my brain, active as a Leyden jar, emits and directs a fluidic current through my nerves, if the other members of the [mediumistic] chain follow similarly, it is evident that it would not be long for us to form sort of an electric battery, by which the influence will be felt according to our thoughts; we will produce a rotation, we will produce, also at a distance, vigorous liftings. (De Gasparin 1854:Vol. 1:514)

Various physical forces were adopted by Spiritualists to explain mediumistic phenomena. One was electricity, which soon was being discussed in terms of the transmission similarities between telegraphy and mediumship. In one case, mediums were seen as electrical conductors in the production of physical phenomena, acting "on principles strictly analogous to the magnetic telegraph" (Davis 1855:66; see also the discussions of Carroll 1997 and Thurschwell 2001). In turn, these speculations were conceptually related to older concepts (Amadou 1953), and to ideas of electrophysiology (Clarke & Jacyna 1987), and animal magnetism.

Later in the century other forms of radiation were highly influential. In fact the 1890s brought important developments, such as the discovery of X-rays. From the beginning X-rays captured the popular imagination (Knight 1986, Pamboukian 2001), and they were soon connected by many to psychic phenomena such as telepathy (Crookes 1897, Sibley 1898; see also Henderson 1988, Natale 2011). "The discovery of the famous X-rays that pass through opaque materials," wrote a commentator, "could well also put

us on the path to a rational explanation of clairvoyance" (Bois 1896:355). In the same year, another author who was discussing X-rays saw these new radiations as analogous to phenomena such as telepathy and clairvoyance and asked if the latter should also be generally accepted (Bixby 1896:880).

But the 1890s, the decade in which Houston was writing, also brought other ideas of body radiations from psychical researchers. These included: Cesare Lombroso's (1836–1909) belief that cerebral forces could be exteriorized from the body to cause physical phenomena due to the inhibition of centers where that agent was normally used; Hippolyte Baraduc's (1850–1902) studies of instrumental detection of a vital force emanating from the body; and Albert de Rochas' (1837–1914) studies of exteriorization of sensibility (or the projection of a neural principle which had sensation properties) (Baraduc 1893, De Rochas, 1895, Lombroso 1892b). The period also presented the use of photography to show the existence of invisible psychic vibrations and fluids, as seen in the work of Baraduc (1896) and others (e.g., Luys & David 1897).[6]

Thought Transference in the Nineteenth Century

Houston's ideas in the excerpt reprinted below appeared in the context of Nineteenth Century interest in thought-transference (Luckhurst 2002, Wiley 2012). An important development was the experimental study of the subject, initially called thought-reading, mind-reading, thought-transference, and eventually telepathy. Following on the observations of mesmerists and others (e.g., Brittan 1854:Chapter 24), and in the considerable interest brought in by the public performances of mind-readers such as Washington Irving Bishop (1856–1889; Romanes 1881), many tests were conducted by members of the Society for Psychical Research (SPR) in England. Among them were those reported with the Creery Sisters by Barrett, Gurney, and Myers (1882), and several others published through the end of the Nineteenth Century (e.g., Gurney, Myers, & Barrett 1884, Guthrie & Birchall 1883, Lodge 1884). But there was also work conducted in other countries, among them, Italy (Lombroso 1891), France (Richet 1884), and Germany (Schrenck-Notzing 1891).[7]

The results of this research, it is important to say, were criticized by many who argued that the claims for the reality of the transference of thoughts could have alternate explanations, among them fraud, muscle reading, and other sensory perceptions (e.g., Hall 1887, Jastrow 1895). Expressing skepticism, psychologist Edward B. Titchener (1867–1927) stated in *Science*: "No scientifically-minded psychologist believes in telepathy" (Titchener 1898:897).

As seen in the epigraph at the beginning of this paper, an author writing in 1869 in *The Spectator*, published in London, mentioned transmissions

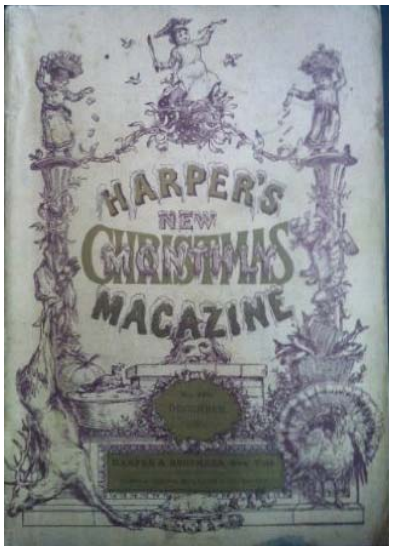

Figure 2. Cover of December 1891 *Harper's New Monthly Magazine* with Mark Twain's "Mental Telegraphy: A Manuscript with a History."

of thought at a distance caused by brain waves moving through the ether (Knowles 1869, see also Andrew 1876 and Despine 1880). Others mentioned the possibility of induction (W. F. Barrett 1876, McGraw 1875). Physicist William F. Barrett (1844–1925) speculated that "just as a vibrating tuning fork or string spends its energy most swiftly when it is exciting another similar fork or string in unison with itself, so the activity of the brain may be more speedily exhausted by the presence of other brains capable of sympathetic vibration with itself" (W. F. Barrett 1882:62).

Many had similar ideas which were published before the paper by Houston discussed here (e.g., C. 1887, Coues 1885, Haig 1874, Mancini 1891). "A candle burning in the night 200 meters away," wrote French physiologist Charles Richet (1850–1935), "produces a very clear light, it seems absurd that at three or four meters of distance cerebral activity shows no action on close-by objects. . . . If we consider this emissive power absurd, it is only because we do not see it" (Richet 1884:668).

In a prominent example about what he referred to as "mental telegraphy," Mark Twain (1835–1910) stated in *Harper's New Monthly Magazine* (Figure 2): "Doubtless the something which conveys our thoughts through the air from brain to brain is a finer and subtler form of electricity . . ." (Twain 1891:101). But such analogies about thought-transference and the telegraph were not new with Twain, having been used or implied before by others (e.g., Brittan 1864, Mitchell 1872). An earlier example was the ideas of a Spanish author who stated that

> [the soul] has in the surrounding electricity a telegraph that is always in immediate contact with our organism which uses it to send the expression of its thoughts to the person it wishes to regardless of distance. (Vergés 1857:14)

Similarly, many others continued discussing such concepts during the 1890s (e.g., Astére 1895, Wagstaff 1892). Lombroso (1892a) wrote about the projection of cortical activity carrying thoughts through the ether. Eng-

lish chemist and physicist William Crookes (1832–1919) speculated on the possibility that the brain may transmit and receive "electrical rays of wavelengths hitherto undetected by instrumental means" (Crookes 1892:95), which may account for thought-transference.

But it should also be stated that not everyone followed physical transmission models. In later writings about telepathy, some of the early SPR researchers were skeptical of explanations such as physical waves or radiations and saw the process as a psychical one, even though this idea did not involve a clear explanation (e.g., Gurney, Myers, & Podmore 1886). "If there really is a physical counterpart to the *fact* of transmission," wrote the authors of the SPR's Third Report of the Literary Committee, "that counterpart remains wholly unknown to us. The physical analogies hitherto suggested for telepathic impulses are aids to imagination and nothing more" (Barrett, Massey, Moses, Podmore, Gurney, & Myers 1884:135). Others, such as Italian physicist Giovanni Battista Ermacora (1869–1898), offered more specific critiques of physical transmission ideas of telepathy (Ermacora 1892). In his view such process was not proven, and the waves, if present, should weaken considerably with distance. He also pointed out that such ideas did not explain the intellectual content and selective nature of telepathy, asking why other people did not receive the transmission as well.

Edwin J. Houston

Edwin J. Houston

Edwin James Houston (1847–1914) was an educator, an electrical engineer, and an inventor.[8] He was born in Alexandria, Virginia, and taught most of his professional life at the Central High School of Philadelphia, where he had earlier obtained a master's degree. He later taught civil engineering there and was given a chair of natural philosophy, which he held until 1914, when he died. It has been said that Houston brought "to his lectures an extensive knowledge of electricity and regularly illustrated them with elaborate apparatus" (Carlson 1991:33).

Houston held many honors. He was a member of the electrical commission appointed by the U.S. government in 1884, President of the American Institute of Electrical Engineers (1893–1895), President of the electrical section of the Franklin Institute, and President of Section C of the Electrical Congress held in Chicago in 1893. In addition he taught physics at the Franklin Institute and the Medico-Chirurgical College, both in Philadelphia. He obtained an honorary Ph.D. from Princeton University in 1894.

Working with English engineer and inventor Elihu Thomson (1853–

1937), Houston developed various projects such as the Thomson–Houston system of arc lightning, and was co-founder of the Thomson–Houston Electric Company in 1883. After many developments, this eventually led to the creation of General Electric in 1892 (Carlson 1991).

Houston authored many books. Some examples are *A Dictionary of Electrical Words, Terms and Phrases* (Houston 1889), *Electricity and Magnetism* (Houston 1893b), and *The Electric Transmission of Intelligence* (Houston 1893a). With electrical engineer Arthur E. Kennelly (1861–1939) as co-author, Houston published various treatises such as *Magnetism* (Houston & Kennelly 1896), *Electric Telegraphy* (Houston & Kennelly 1897), *Electricity in Electro–Therapeutics* (Houston & Kennelly 1898), and *Electric Arc Lighting* (Houston & Kennelly 1906). In addition, Houston was known for his many lectures. In an announcement of a presentation about electricity, he was described as a "most interesting and instructive lecturer" (Anonymous 1892b:540). Another example was a lecture Houston presented at a meeting of the New York Electrical Society in December of 1892, where he covered topics such as early knowledge about magnetism, magnetic effluvia, Faraday's ideas, magnetic induction, methods of magnetization, and causes of the earth's magnetism (Anonymous 1892e).

Like others involved with technology and science in the late Nineteenth Century, Houston had a progressive view. In his Inaugural Address before the Congress of the American Institute of Electrical Engineers, held in New York in 1893, Houston commented on how common electrical technology had become among the public (Houston 1893c). But even though this was taken for granted by many, there was still the possibility of important advancements. This was the case, Houston, wrote, about developments that took place between the Philadelphia International Exhibition (1884) and the World's Fair of Chicago (1893).[9] Houston lived in an era of, and was an active participant in, the rapid technological development of the Nineteenth Century in the United States and elsewhere, as seen particularly in electrical developments, including the internationally known work of Thomas Alva Edison (1847–1931) and Nikolas Tesla (1856–1943) (Carlson 2013, Freeberg 2013).[10]

But Houston's interests went beyond electricity, magnetism, and their associated technologies. This was evident in books he wrote about chemistry (Houston 1883), natural philosophy (Houston 1884), forestry (Houston 1893d), and geography (Houston 1901), as well as in the paper discussed here. In fact his paper about telepathy may be seen as a connection between his general interests and his work with electricity and magnetism, particularly that which related to communication (Houston 1893a, Houston & Kennelly 1897).

Houston does not seem to have published anything about thought-transference before or after he published this paper. The article reprinted here was first presented at a meeting of the Electrical Section of the Franklin Institute in March of 1892 (Anonymous 1892d). Entitled "Cerebral Radiations," it was published in various journals. These include the *Journal of the Franklin Institute* (Houston 1892b), from which I copied the article, as well as the *American Gas Light Journal* (Houston 1892a), *Psychical Review* (Houston 1892c), *La Science Française* (Houston 1892g), *Scientific American Supplement* (Houston 1892d), and *Western Electrician* (Houston 1892e). The author explained thought-transference: "Cerebral energy . . . is dissipated by imparting wave motions to the surrounding ether, and such waves are sent out in all directions from the brain" (Houston 1892b:490).

The Houston Excerpt

Here is most of Houston's (1892b) article.

I have thought it possible that it might interest you to consider some rather wild speculations in which I have indulged for a number of years past, but which I have heretofore refrained from publishing. . . . Although the suggestions I have to offer as a basis for a hypothesis of the mechanism of cerebration, are confessedly incomplete, and, perhaps improbable, yet I have concluded to place them on record as of possible interest to the scientific world . . .

Postulating the existence of the universal or luminiferous ether, which is now generally accepted in scientific circles, and bearing in mind the fact that this ether passes through even the densest matter, as easily as water through a sieve, it follows that the brain atoms or molecules that are here assumed as the cause of cerebration, are completely surrounded by the ether.[11] Now, since the ether is a highly elastic, easily movable medium, it would follow that thought or cerebration, if attended by vibrations, must necessarily develop in the ether wave-motions, which have the brain atoms or molecules for their centres . . .

The exact nature of the motions that are assumed to attend an active condition of the brain must necessarily remain unknown as long as we are ignorant of the exact nature of the mechanism that is moved . . .

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.

Let us assume, then, that cerebral radiations or waves are given off from every sentient or active brain, and that these waves pass into the space around the brain something like the waves that are imparted to the air around a sounding tuning-fork . . .

If such waves, which I would call thought-waves or cerebral-waves, be present in ether that fills all space, it will be interesting to inquire what phenomena they might be expected to produce . . .

An active brain may . . . be regarded as moulding the ether around it into thought-waves, that are spreading outwards from it in all directions. In this respect, it is not unlike a conductor through which an oscillatory discharge is passing, pro-

ducing those waves which Hertz has so beautifully demonstrated as resembling the vibrations that produce light.

Assume, then, that the cerebral radiations partake of the nature of thermal, luminous, electric, or magnetic radiations, and the following explanation of telepathy, or thought transference, is, to say the least, not improbable.

I would explain the possibility of the transference of specific cerebral vibrations from an active brain, to a passive or receptive brain, by the simple action of what is known in science as sympathetic vibrations.

Take the case of a vibrating tuning-fork sending off its waves across the space which separates it from a second tuning-fork, not as yet in motion, but tuned so as to be able to vibrate in exact unison with it. As is well known, the exact correspondence between the period of the active or transmitting-fork, and the passive or receptive-fork, is such that the vibrations of one fork are gradually taken up by the other fork, so that the energy of the motion of the one is transferred or carried across the space existing between them, by means of pulses or waves, set up in the air which surrounds them . . .

Or, similarly, take the case of the sympathetic vibrations excited by waves of light. Solar energy is radiated or transferred across the space existing between the sun and the earth by waves or oscillations in the luminiferous ether . . .

Or, take the still more interesting case of what Hertz calls electric resonance. As already mentioned, it is now generally recognized by electricians that a conductor, which is the seat of an oscillatory electric discharge, is sending into the space around it electric waves or oscillations which travel with the velocity of light, and which are in fact of exactly the same nature as light itself. If these electric waves meet a circuit so tuned as regards the period of oscillation of the circuit in which they originally occurred, as to be in consonance with them, electric oscillations will be set up in this circuit, of exactly the same nature as those exciting it.

In view of these facts it does not seem improbable to me, that a brain engaged in intense thought should act as a centre of cerebral radiations, nor that these radiations proceeding outwards in all directions from such brain should affect other brains on which they fall, provided, of course, that such brains are tuned to vibrate in unison with them. In such cases the absorption of energy by the recipient brain may be either a species of selective absorption, in which its train of thought is only modified, or it may be absolute, in which case the recipient brain has excited in it an exact reproduction of the thoughts of the exciting brain . . .

If such a hypothesis be true, then these cerebreal [sic] vibrations or radiations must travel through space with exactly the velocity of light. This is of course on the assumption that the vibrating or oscillating brain molecules or atoms set up vibrations similar to those of light. Of course, this equality between the velocity of cerebral wave propagation and that of light is true only for free ether. In the ether which fills the interatomic or intermolecular spaces of gross matter, or, as it is technically called, combined ether, the velocity of wave propagation varies according to the particular character of the matter with which it is associated. A retardation or decrease in the velocity of the assumed cerebral waves would doubtless be experienced while passing through the materials of the skull and head.

If thought travels along waves in the ether similar to waves of light, it would be able to travel along any path by which rays of lights can pass. It can therefore travel along rays of light, i.e. along paths in the ether through which rays of light are moving.

There is a well-known experiment in hypnotism, in which the patient, placed in a state of semi- or complete unconsciousness, has his brain called into a more or less

active condition by the suggestions of the hypnotizer, which might seem to somewhat favor the hypothesis of cerebral radiation.

It might be interesting, in view of the above suggestions, to see whether a hypnotizer placed in such a position as regards the hypnotized that flashes of light falling on his eyes shall afterwards fall on the eyes of the patient, to observe whether or not acts of suggestion are more readily perceived by the brain of the hypnotized along such rays of light than without the aid of such rays.

If the preceding speculations be regarded as wild, and that this is true I have already granted, what may not be said as to the following?

If thought radiations partake of the nature of ether-waves, then there should presumably exist in the thought radiations or waves, phenomena corresponding to the various phenomena of thermal, luminous, electric, or magnetic radiation; even the phenomena of reflection, refraction, and possibly even of dispersion of such waves, would appear to be a possibility. In this connection, it is of interest to imagine the analysis or separation of a complex wave of thought into its component or elementary waves, corresponding to the separation of a beam of light, by means of a prism . . .[12]

If thought radiations or waves partake of the nature of light, then it would seem among the remote possibilities of science—to obtain, say by means of a lens—a photographic impression of such thought-waves on a suitably sensitized plate, somewhat after the manner of the ordinary photo graphic picture.[13] Such a thought-record, suitably employed, might be able to awaken at any subsequent time in the brain of a person submitting himself to its influences, thoughts identical to those recorded.

Of course, I am aware of the improbability of such a record being obtained in the near future, and of the exceeding difficulties that would seem to stand in the way of ever obtaining it.

Until we know something more definite concerning the nature of these assumed cerebral vibrations and of their lengths, we must necessarily be seriously handicapped as how to best permanently fix them on a suitable record-surface, and how afterwards to cause such record-surface to interpret its peculiarities to the brain. I merely throw it out as a possibility of what science may have in store for those who come after us. I would suggest, in this connection, that 100 years ago it would probably have been regarded as quite impossible that the telephone or the phonograph could have been produced.

Such a thought-record, however, if obtained would not be an image of the thought itself, or of the particular groupings of the particles, whose to-and-fro movements attend or produce thought, any more than the tracings of the phonograph record form an image of the spoken words. They would merely represent the to-and-fro motions of the ether set in motion by the thought or cerebrations.

An eye looking at such an image would not be influenced thereby. If, however, ether waves similar to those recorded could be reproduced by causing light to pass through such photographic images of the to-and-fro motions of thought-waves, such waves might be caused to influence the brain and thereby awaken thoughts similar or identical to those recorded . . .

Probably one of the most serious objections to the hypothesis is to be found in the fact that the phenomena of telepathy and thought transference are not of more frequent occurrence. This I must frankly acknowledge to be a most serious objection. I would suggest, however, that the comparative infrequency of the phenomena may possibly be explained by the presence in the human body of a shield which protects

the brain or the nerve centres from the effects of cerebral radiations. It is not impossible the sheathes of the nerves act as screens to prevent the reception by the brain of these thought radiations . . .

If there be any truth in the hypothesis I have briefly outlined, there should be what I would term a kind of vital radiation going on and passing outwards from the body of a healthy person, which may not improbably excite by sympathetic vibrations in the bodies of weaker persons around them, vibrations of a normal or more healthy type than those present in the sick person.

If this be true, the old belief of the efficacy of the laying on of hands, or of magnetic healing, may find some foundation, in fact, apart from what is most probably the general explanation of such causes, viz., hysteria.

I have thrown out the above hypothesis of cerebral radiations with considerable doubt and hesitation as a suggestion only to those working in the field of telepathy or thought transference, in the hope that I may thereby call the attention of such investigators to some phenomena in this very obscure field of research.

Comments About Houston's Paper

Houston's discussion was considered of interest to students of psychic phenomena to the point of being reprinted in the *Psychical Review* (Houston 1892c)[14] and in one of the books of Albert de Rochas (1895:206–216) in which he referred to the vibrations of the "fluid" of a person affecting another. It was also briefly mentioned by Angelo Brofferio (1893:140–141). However, I have not found mention of it in issues of the *Annales des Sciences Psychiques*, the *Journal* and *Proceedings of the Society for Psychical Research*, nor the *Psychische Studien* published between 1892 and 1899. However, some later writers on psychic topics mentioned him briefly (Carrington no date:60–61, Stocker 1906:18, 26).

A commentator in the *English Mechanic and World of Science* agreed with Houston that his ideas were wildly speculative, but stated that "they are based upon proper scientific analogies, and hence must be taken seriously" (Anonymous 1892c:371). Commenting about this in the theosophical journal *Lucifer*, an anonymous author wrote:

> Theosophical students will find them [Houston's ideas] familiar and even elementary, but, as they have often been derided for saying the same thing, they may be amused to see their own teachings welcomed with respect when they fall from lips scientific. (Anonymous 1892f:353)

Being unaware of the serious study of the topic, the commentator in *Electrical Engineer* praised Houston because he believed that the study of psychic phenomena had been left to visionaries. In his view, Houston's ideas "opened an enormous field for labor upon lines already familiar to men skeptical as to spooks, but intensely alive to the importance of all that bears upon the questions of everyday life." He also hoped that Houston

"will not be content to stop at suggestions" (Anonymous 1892a:604, for both quotes).

Houston's article was summarized in French newspapers (Bataillard 1892, Lucet 1892), in the *Revue Scientifique* (Anonymous 1892g), and in the Spiritualist publication *Light*, where the writer merely endorsed Houston's hope that his idea would be considered by researchers in the area (Anonymous 1893). A few years later Reverend T. E. Allen (1895) agreed with Houston's basic idea. Furthermore, the article was listed in bibliographies published during the second decade of the Twentieth Century under the headings of "Theories of the Ether" (Anonymous 1912) and "Thought-Transference" (Anonymous 1913).

Concluding Remarks

Simple transmission models based on radiations and waves such as Houston's have been criticized in the past as failing to explain the evidence for ESP (e.g., Braude 2002:Chapter 4:Section D). Nonetheless, historically it is important to pay attention to ideas such as Houston's because the study of past developments in parapsychology, and of any other scientific field, should not be limited to what is believed today to be correct. To understand the factors that shaped the development of the discipline, and its state in particular periods, it is essential to consider what we label today as superseded or rejected knowledge, and to avoid focusing only on aspects of the past that resemble our current interests and beliefs.[15]

While I do not claim that Houston's article had an important impact on the study of ESP (the reverse seems to be the case), a discussion of its content contributes to our understanding of the assumptions surrounding past ideas on the subject. In this case it is clear that Houston was influenced by the then current concepts of physics (or by extensions of these concepts), which included constructs such as brain-generated radiations and the ether. The influence of physics on parapsychology, it is interesting to see, continues to recent times (e.g., Radin 2006).

Regardless of the few above-mentioned citations of Houston's work in writings about psychic phenomena, including the reprint of the article in *Psychical Review* (Houston 1892c), there is no evidence to say that Houston's ideas were influential in the concepts of others, or that they generated research. While his ideas seem to me more detailed than previous ones, his main postulate, that of brain radiations transferred via the ether, was hardly original. Perhaps Houston would have been more influential if he had addressed his ideas directly to the psychical research community and if he had conducted research himself.

Regarding psychical research, the paper has no reference at all to

Figure 3. Magnetic radiations, (Durville 1921).

previous studies on the topic. Houston's work is, in fact, the work of someone who showed no evidence that he was familiar with the phenomenon he was trying to explain. Nowhere in his paper did he mention the ideas and studies of telepathy published before his article. Knowledge of this literature would have allowed him to discuss the actual things that were believed to be transmitted from mind to mind, including images, thoughts, emotions, and physical sensations. Interestingly, some of this literature, and particularly the work of the SPR, was discussed in many American publications that Houston must have had access to (e.g., Courtenay 1891, Hovey 1885, Prince 1887), not to mention the publications of the SPR.

Many ideas of physical transmission continued to be discussed after the appearance of Houston's 1892 paper in the Nineteenth Century (e.g., Astére 1895, Crookes 1899). Examples of Twentieth-Century discussions include magnetist Hector Durville's (1849–1923) graphic representation of the aura showing the constant reception and projection of "thoughts in the form of rays or waves" (see Figure 3; Durville 1921:82). In addition, there was the research and theories of Naum Kotik (1908) on psychophysical radiant energy and of Italian neuropathologist Ferdinando Cazzamalli (1887–1958) on cerebral waves (Cazzamalli 1925/1926). The radio analogy was discussed by many, perhaps more famously by American author and social activist Upton Sinclair (1878–1968) in his *Mental Radio* (1930).

The well-known French psychical researcher René Warcollier (1881–1962) speculated on resonance and vibrations from the nervous system, and on wireless telegraphy (Warcollier 1938).[16] Ideas of physical transmission exist in more recent times (e.g., Marciak-Kozlowska & Kozlowski 2012, Vasilescu & Vasilescu 1996), but these are outside the range of this paper.[17]

As pointed out before, there were some who were not convinced of the applicability of physical models to explain telepathy (e.g., Gurney, Myers, & Podmore 1886). In fact such thinking characterized most of the early SPR researchers. Initially open to consider physical models, Barrett changed his mind. After writing about the brain wave idea to explain telepathy, he stated that "supernormal phenomena . . . do not belong to the material plane, and therefore the laws of the physical universe are inapplicable to them" (W. F. Barrett 1911:109). Over the years many others opposed wave and radiation models (e.g., Rhine 1934, Tischner 1920/1925). As I have pointed out before (Alvarado 2006), physical transmission models declined in parapsychological circles with the rise of the Rhinean paradigm and ideas from modern physics.[18]

To conclude, Houston's article allows us to see in detail a type of theory that has been prevalent throughout the history of interest in telepathy. In fact his paper is probably the most detailed discussion of the topic in the later Nineteenth Century. Houston's ideas were part of a long conceptual tradition that has been influential both in parapsychological concepts, as well as in popular conceptions of telepathy.

Notes

[1] I have discussed this conceptual tradition in various articles (e.g., Alvarado 2006, Alvarado & Nahm 2011). On physics and psychic phenomena in the British context, see Noakes (2004, 2008). In addition to ideas coming from physics, the concept of telepathy was also part of the late Nineteenth-Century interest in the subconscious mind, as discussed by Luckhurst (2002) and Plas (2000).

[2] Regarding the nonphysicality tradition of previous times, see Alvarado (2009, 2012b). The position is represented today by individuals such as Dossey (2013), Kelly, Kelly, Crabtree, Gauld, Grosso, and Greyson (2007), and Tart (2009).

[3] For overviews, see Buchwald and Fox (2013), Harman (1982), and Purrington (1997). More specific studies include those of Buchwald (1985, 1989) and Smith (1998). Much has also been written about relevant topics such as the ether (Cantor & Hodge 1981), and about cultural aspects of the impact of physics and its associated ideas, practices, and technology (Marvin 1988, Morus 2005, Simon 2005).

[4] On the concept of animal magnetism, and mesmerism in general, see Crabtree (1993), Gauld (1992), and Méheust (1999). A similar concept was Karl von Reichenbach's (1788–1869) Od (Reichenbach 1851), which was considered by some to be the vehicle of telepathic phenomena. A late example of this was German philosopher Carl du Prel (1839–1899) in his *Die Magie als Naturwissenschaft* (du Prel 1899). Relevant to this paper is his discussion of wireless telegraphy and telepathy (Vol. 1:20–34).

[5] There are many examples of the use of the concept of nervous and vital forces to explain physical mediumship (Alvarado 2006). This includes authors writing in France (Chevillard 1869), England (Cox 1872), Germany (von Hartmann 1885), and the United States (Rogers 1853), among others. Years later German physician Albert von Schrenck-Notzing (1862–1929) discussed mediums and referred to "an emanation or projection [of] vital energies beyond the limits of the human organism" (Schrenck-Notzing 1920:180).

[6] This, in turn, was part of the wide use of photography in Nineteenth-Century science to make visible the invisible (Canguilhem 2004, Keller

2008). In fact the topic of "photography of the invisible"—both in conventional science and in psychic studies—has an interesting scientific and cultural history (Nowotny & Weiss 2000). These developments also follow on early attempts to photograph spirits, thoughts, and "fluids" (Chéroux, Fischer, Apraxine, Canguilhem, & Schmit 2005).

[7] Ermacora (1898) and Podmore (1894) present overviews of this work. They cover observations in the normal and in the hypnotic state, as well as the transference of images, feelings, sounds, and tastes, and the induction of trance at a distance. Some of the early SPR work with the Creery Sisters was qualified later when evidence for fraud was obtained (Gurney 1888:269).

[8] For biographical information, see Anonymous (1906, 1911, 2013).

[9] The World's Columbian Exposition—also known as the Chicago's World Fair—had remarkable exhibits of electrical instruments, showing the progress in that area (J. P. Barrett 1894). Some of the exhibits, and the then current technology, included incandescent and arc lighting, dynamos, motors, switchboards, telegraphy, telephony, electro-therapeutics, batteries, and clocks. Connected to the Exposition was the International Electrical Congress. Houston was a member of the Electrical Congress' Advisory Council, and Chairman of its Section C (Pure Practice) (Anonymous 1894:v, 338).

[10] On late Nineteenth-Century electrical developments, and particularly in the United States, see Carlson (1991), Freeberg (2013), Klein (2008), and Marvin (1988).

[11] As mentioned before, the ether was considered before by others in relation to thought-transference (Andrew 1876, Despine 1880, Knowles 1869, Lombroso 1891). The idea of the ether was not only important for the transmission of physical signals, but was also involved with political and religious issues (Noakes 2005).

[12] According to Mesmer (1779), animal magnetism was "augmented & reflected by mirrors" (p. 78). See also Barety's (1887) studies of animal magnetism, or neural force, in relation to a prism (pp. 14, 16–17) and to refraction on things such as mirrors (pp. 12, 110, 302–303).

[13] Referring to the brain waves mentioned in the epigraph, Knowles (1869) speculated on their photographic detection and on the possibility of obtaining a "dim portrait of thought" (p. 136). On the use of photography in Nineteenth-Century science to detect invisible processes and various forms of psychic photography, see Note 6. See also Reichenbach's (1862) early attempts to photograph Od. Houston (1892f, 1892h) showed interest in the use of photography, as seen in his discussions of the configuration of magnetic fields using photography of iron fillings. Consistent with his

interest in technology (e.g., Houston 1888, 1893b, Houston & Kennelly 1897), Houston continued to speculate about technological ways to record thoughts. Discussing future developments related to electricity, he mentioned that he saw "dimly outlined through the clouds, an apparatus for the automatic registration of unwritten, unspoken thought" (Houston 1894:199). Interestingly, there were other Nineteenth-Century discussions of the instrumental recording of thoughts (Anonymous 1896, 1897). Somewhat related were Alexander Graham Bell's (1847–1922) speculations about the transference of thoughts via electricity in two persons wearing helmets connected with wires (Moffett 1893:41–42).

[14] The *Psychical Review* was published by the American Psychical Society (not to be confused with the American Society for Psychical Research). Its officers for 1892 were writer Hamlin Garland (1860–1940) (President), journalist Benjamin O. Flower (1858–1918) (Vice-President), and Reverend Thomas E. Allen (1858–1930) (Secretary and Treasurer).

[15] On this topic, see Alvarado (2012a:619–621).

[16] The historically minded reader may remember many other early Twentieth-Century speculations (Alvarado 2008). One was French physiologist Charles Richet's (1850–1935) vague idea of vibrations to explain the sixth sense:

> The sixth sense is that one which gives us knowledge of a vibration of reality, a vibration which our normal senses are unable to perceive. (Richet no date/1928:224).

For the ideas of Hans Berger (1873–1941), see Millett (2001). More interesting, but less well-known, was French physician's Charles Binet-Sanglé's (1868–1941) belief in brain waves:

> Each thought corresponds to a particular system of ethereal waves. . . . In this way, by means of brain waves, every thought of the radiant brain awakens the same thought in some people. . . . In my view, the direct transmission of visual images is but a particular case of telephotography, the direct transmission of auditory images, a special case of wireless telephony. (Binet-Sanglé no date:25)

[17] It has not been possible to discuss in detail the vast early Twentieth-Century popular literature discussing telepathic vibrations, waves, and the like. This includes little-known works such as *Practical Mind Reading* (Atkinson 1908), *Telepathy, Mental Telegraphic Communication* (Stocker 1906), and *The Call of the Soul* (Freyer 1926). There were also several articles in magazines, among them one in the Catholic review *America*, where one author stated:

> Since the brain, however, is the seat of thought and consists of cells, there is no obvious reason why the activity of those cells could not cause a subtle wave and eject it into the outside world. (Murphy 1915:88)

[18] On ideas of nonphysicality of the Rhinean paradigm, see Rhine and Pratt (1957) and Zingrone (1985). It is not possible to discuss here the influence of physics on modern parapsychology, but see Oteri (1975).

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LETTER TO THE EDITOR

Quality in Parapsychological Meta-Analyses

Skeptical wisdom holds that parapsychological studies that produce significant results must have a low methodological quality. In most meta-analyses of parapsychological paradigms such as 'Ganzfeld telepathy' and so-called 'presentiment', the meta-analyzers, who are generally proponents of the psi hypothesis, also present a quality-effect size relation. These relations generally are at odds with the 'skeptical wisdom', i.e. they produce a positive or nonsignificant relation.

One can and should wonder what the quality is of the assessment of these relations. I became aware of this question when I found that a so-called PK–RNG (or Mind-over-Matter) study of mine got a low quality rating in one of the first large-scale meta-analyses (Radin & Nelson 2000). With my pride hurt, I delved a bit deeper into this and soon found that quality is assessed on the basis of the written reports. These reports generally have the standard structure of scientific writings, but often there is a special paragraph dealing with 'alternative explanations' where the authors go to some length to discuss sensory leakage, randomization problems, and other potential alternative explanations of their anomalous results. Never will one find here remarks such as: the sensory shielding was inadequate or the randomization was done by hand-shuffling. For the vast majority, these paragraphs carry information to persuade the reader that there are no alternative explanations.

I had no paragraph on alternative explanations simply because I didn't need one, because the results were nonsignificant. So my nonsignificant study got a low quality rating even though of course I did extensive randomization tests before even starting the study.

One doesn't need to be a mathematical genius to infer what this way of scoring quality (on the basis of a report rather than asking the author "did you check your RNG") is doing to the assessment of quality-effect size relations. Most if not all nonsignificant outcomes will get a low quality rating while they actually had a good quality. Even if in reality there is no relation, this approach will result in a positive relation between quality and effect size. And of course run against 'skeptical wisdom.'

I communicated this misuse of quality-effect size relations to the parapsychological community (in their discussion list) several times asking the culprits to stop using these relations as a 'proof' that results were more

significant with better quality. Nothing happened. Actually the practice just continued, most notably by some of the researchers with academic affiliations. That was the most shocking aspect of this. Could these "psi proponents" really not understand this obvious error?

The incorrect use of the report-based study quality ratings does of course not prove the skeptic wisdom. Therefore, real quality should be assessed by independent assessors inspecting the actual experiments, preferably onsite and preferably by raters blinded to the study outcome. This probably will never happen because it is a lot of work, and diehard skeptics don't need this because they have their wisdom unshakeably set already.

For the moment the best thing for the meta-analyzers of psi studies is to at least mention the caveat of using the current method of quality-study size asessment and for interested readers to consider the results published so far with a grain of salt. This effect introduces a bias into the quality–effect size relationship reported by the meta-analysis.

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ESSAY REVIEW

Crusading for Evidence-Based Actions

Galileo's Middle Finger: Heretics, Activists, and the Search for Justice in Science by Alice Dreger. New York: Penguin Press, 2015. 337 pp. $27.95 (hardcover). ISBN 978-1-59420-608-5.

Progress, wrote George Bernard Shaw, depends on the unreasonable person,[1] one who transgresses society's dogmas and taboos.

Alice Dreger is such an unreasonable one, and she has contributed mightily to tangible progress toward treating human beings as individuals, medically and socially; in particular those human beings who do not fall readily, physically or emotionally, into distinct categories of "male" or "female."

This book is Dreger's personal, passionate, colloquial account of three crusades. Few readers will fail to learn a great deal about the varieties of human sexual identity, and few will fail to be engrossed by these true tales of good deeds and bad deeds, of admirable actors and not-so-admirable ones.

Dreger—studying history and philosophy of science and believing herself to be a feminist—wanted a Ph.D. dissertation topic relating to issues of gender.[2] Mentors suggested hermaphroditism. That led to contact and collaboration with activists in the "intersex" community, individuals born with mixtures of the organs and tissues that physically define "male" and "female." Standard medical practice—into the late 1990s—was for pediatric surgeons to decide whether a given baby should be male or female and to perform "corrective" surgery as a matter of course, typically without consulting the parents. Dreger and her collaborators achieved a great deal toward changing medical practices so that parents make the early decisions, preferably (p. 49) to do nothing until the affected individuals are of an age to make their own individual decisions.

This crusade brought considerable publicity, which led to Dreger's being urged to look into the vicious attacks that had been made on Michael Bailey, an academic psychologist who expounded the views of Ray Blanchard, that there are two distinct categories of men who seek male-to-female sex change. One group comprises gay men, erotically attracted to men; the other are "autogynephiliac" men who experience erotic attraction

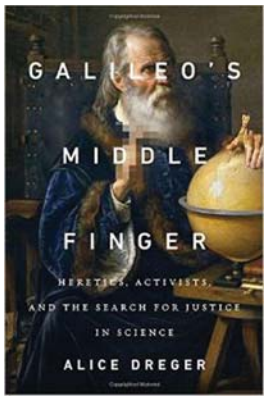
to the idea of being female without necessarily being attracted erotically to men. The concept of autogynephilia had offended several trans-women to the degree that they waged a campaign to blacken Bailey's reputation, using means that included public attacks on his family.

The intellectual passion driving Dreger's work is that justice must be evidence-based: Evidence and not ideology must be decisive. Good intentions don't guarantee good actions or outcomes, while those whose actions are damaging are not necessarily evil people (p. 275). The lessons of history may be clear, but historians are not listened to (p. 276). Dreger's analysis of what happened to Michael Bailey (Dreger 2008) illustrates the scrupulous seeking and collation of evidence that characterizes first-rate historical work.[3] Dreger was shocked to find that the anti-Bailey activists were wrong on salient facts, even as they claimed moral authority based on personal experience and feelings—in other words, they were politically correct. As *Galileo's Middle Finger* insists over and again, views of what is ethical and moral must accord with the facts; political correctness, in other words, is simply wrong and often irrational.[4]

Dreger's analysis of the willful destruction of Bailey's academic career on false grounds brought Bailey at least some after-the-fact comfort. It also brought Dreger the invitation to look into a similar scandal. Once more she contributed to a belated recognition that the anthropologist Napoleon Chagnon had been vilified and hounded in a campaign of ideologically based and factually false accusations (Dreger 2011).

Dreger's third crusade over human sexual identity concerned the disorder of congenital adrenal hyperplasia (CAH), which involves excessive production of androgens that can lead to ambiguous genitalia and quite serious risks to health in genetic females. CAH is genetically recessive, affecting female babies who inherit the pertinent mutation from both parents: There is 1 chance in 4 that a girl will suffer the disorder if both parents carry this genetic marker.

The activists who enlisted Dreger's help regarded as malpractice what one leading physician, Dr. Maria New, was doing and proselytizing: If both parents carried the recessive gene, the pregnant woman would be given dexamethasone (DEX), which dampens the effect of the excess androgen. However, this does not cure CAH and surviving babies are not protected from its considerable health risks. DEX seems to decrease the chance that babies will be born with ambiguous genitalia, but the evidence is purely anecdotal; the only controlled clinical trial (in Sweden) had turned up serious side-effects including damage to brain function. DEX has never been approved for such treatment, but physicians are allowed to prescribe it "off-label".[5]



The crusade against Dr. New's practices was unsuccessful. Dreger and her colleagues pointed out that New was effectively carrying out uncontrolled research without the required safeguards, citing differences between what New tells patients and what she writes in grant applications, but the authorities decided that New had not been shown to be doing anything actually illegal.

This episode illustrates that official agencies are bureaucracies whose actions are determined by legalities and traditions and not by evidence let alone common sense, and that professional associations have little or no leverage over what individual physicians do (p. 204). One sees how someone like Dr. New, long-established as a leading Establishment figure, can get away with maybe not murder but certainly practices that seem improper from any reasonable viewpoint. Professional specialists close ranks and circle the wagons (e.g., p. 216). The media fail to pursue such stories even when they are brought to their attention; there is a lack of enthusiasm for investigative journalism (pp. 211–222, 257). Thus Robert Gallo has continued to command status and prestige despite clear findings of scientific and personal misbehavior.[6]

Perhaps the only genuine solution would be that clinical trials be mandatory whenever there is widespread off-label prescribing of a specific drug for a specific medical condition, but such a solution is impossible under current circumstances in the USA where policy is determined by drug-industry lobbying. In the meantime, occasionally something sparks media attention: After Dreger and cohorts had been criticizing surgical practices on intersex babies for 15 years, eventually wide attention came to the fact that "culturally motivated female genital cutting wasn't just something that happened in Africa, it was going on in the United States" (p. 222).

The title of this book implies, rather misleadingly, something beyond a largely personal story. Dreger did come to realize that Bailey's and Chagnon's were not exceptional cases, that scholars in other fields suffer similarly, but the beleaguered individuals do not realize that others are in the same boat (p. 108 ff.). Dreger met anthropologist Craig Palmer, targeted for believing that rape has something to do with sex and lust and is not simply about power relations in a patriarchy, as self-styled feminist activist-scholars were insisting. Psychologist Ken Sher transgressed by not rejecting for publication an article reporting that not all those sexually molested as children are damaged to the same extent.[7] Psychologist Dave Geary dared

to study sex differences in mathematical talent. Mark Flinn was tarred through association with Napoleon Chagnon. All these were at a single institution, the University of Missouri at Columbia. (Later [p. 182] Dreger also mentions a neuroscientist at the Oregon Health & Science University who was hounded by LGBT activists and by PETA [People for the Ethical Treatment of Animals] for studying male sheep who prefer sex with other rams).

Although *Galileo's Middle Finger* offers no further examples, it does note that the experiences of Bailey, Chagnon, *et alia* are "typical" for "those who challenge conventional wisdom"; and it recognizes that these "Galileos" are often not their own best defenders, believing naively that all they need to do is keep drawing attention to the evidence (e.g., pp. 180–181). However, the reader does not learn just how widespread, indeed endemic, has become the persecution of non-mainstream scholarship (Bauer 2012). Peter Duesberg has been damaged at least as much as Chagnon or Bailey for pointing out that the Emperor of HIV/AIDS theory has no clothes. Such eminent physicists as Frederick Seitz and environmental scientists as Fred Singer are denounced as right-wing shills because they point to the lack of evidence for carbon-dioxide–caused climate-change. Physics Nobel Laureate Luis Alvarez threatened to destroy the career of paleontologist Dewey McLean if the latter kept opposing the asteroid theory of dinosaur extinction.[8] Etc., etc., etc.

No one, of course, can look in detail into every issue. On most matters, we make intuitive judgments about which mainstream views to accept and which to be skeptical about. So, unfortunately, Dreger slips up by citing as properly evidence-based the beliefs in human-caused climate-change and HIV-caused AIDS (e.g., pp. 137, 186, 257) when in fact there is overwhelming evidence against the latter (Bauer n.d., 2007, 2012) and no proof of the former (Bauer 2012). I have no doubt at all that if Dreger were to look into those issues, she would no longer consider the mainstream position to be evidence-based; in particular she would be horrified at the "treating" of "HIV-positive" pregnant women with antiretroviral drugs (Farber 2006).

Dreger was also mistaken in regarding the Food and Drug Administration (FDA) and the Office of Human Research Protections (OHRP) as "impartial and well-informed investigators— . . . accountants on white horses" (p. 228), crediting the FDA with "extensive" review of new drugs (p. 187): In actual fact, drugs have had to be withdrawn at increasing rates after ever-shorter times on the market because of inadequate review (Bauer 2012: 240). Dreger did learn that the FDA may simply ignore it when warned of an ongoing illegal practice (p. 250), just as the Centers for Disease Control

and Prevention and the Army Research Office ignored my queries about the epidemiology of HIV (Bauer 2009). And Dreger also came to realize that OHRP "simply is no longer doing its job" (p. 274).

Dreger is right when she describes "the ideal of peer review" as "the genius of science" (p. 133); but omits the important caveat that peer review *in practice* all too often entrenches mainstream doctrines and suppresses minority views.

At various places in her story, Dreger offers insights and principles of considerable generality. As the world is increasingly experiencing ideologically determined argument and action, "the pursuit of evidence is probably the most pressing moral imperative of our time" (p. 11). Dreger recognizes that activists and scholars, including scholars in Science Studies, all too often fail this imperative, and the book has many examples of disgraceful attacks not grounded in any evidence, including attacks on Dreger herself by both putative scholars and by activists (e.g., p. 127 ff.). Some postmodernists (e.g., in cultural anthropology, p. 141) even insist that scholarship should entail activism, spawning such oxymorons as the concept of "crisis disciplines."[9]

Dreger is clear about the difficulty of getting it right about contemporary issues, that bad deeds may be committed for honest reasons, that no one has a monopoly on virtue (pp. 18, 48, 275). It is "a rare trait: a belief in evidence even when it challenged our political goals" (p. 27); "most of our putative academic political allies . . . wanted to just spew cute slogans and academic postmodernist horseshit" (pp. 43–44). When Dreger herself is attacked, she bemoans that "people wouldn't look up the details. They never look up the details" (p. 106). Indeed. The Internet has made it very easy to blacken reputations with baseless falsehoods,[10] and once that has happened, it is irreversible in the world at large since most people don't bother to look beyond the original charges. Moreover, "the Internet has gutted the Fourth Estate" (p. 189). I would add, not only the Internet but also the cutthroat competition in the mass media for audience and circulation, whereby quality and investigative journalism are left without patrons. Still, on the other hand, as Dreger also mentions, the Internet makes it possible for individuals to find others in similar straits and thus enables activism by and for minorities.

Those who practice scholarship may discover that their work can be distorted for non-scholarly self-interested purposes; thus the finding that *not all* sexually molested children *are equally harmed* was distorted by NAMbLA, the North American Man/Boy Love Association, as justifying their claim that *no* child is harmed by early sexual activity (p. 111 ff.). Other activists may try to evade substantive issues in hopes of social or political

gain, for instance by playing down the influence of eroticism and lust in issues of gender identity (e.g., p. 63).

Being an activist can become one's identity, more important than actually achieving the purported goal of the activism (p. 45); and some activism can be grossly, absurdly dogmatic, as when a self-proclaimed feminist asserts that "All transsexuals rape women's bodies by reducing the real female form to an artifact, appropriating this body for themselves" (p. 64) or when feminist groups admit only "womyn born womyn" (p. 65).

Dreger also learned that relationships forged in a campaign may not remain the same after the campaign's aim has been largely attained (p. 51). It's a rather general phenomenon that those who excel at building are not best able to do the subsequent long-term housekeeping.

Stories can be more influential than facts, literature may do what nonfiction cannot (p. 46).

A pervasive theme is that medical practice ought to recall Hippocrates and aim first of all to do no harm. American practice tends rather to do something in preference to doing nothing, thereby wishing for the best and doing "what has always been done" if the pertinent evidence happens to be lacking (e.g., p. 39). This accords with the overall American ethos (Payer 1996). Dreger does point out that the dangerous use of DEX illustrates that "American clinicians . . . had learned nothing from history," in particular the disastrous result of administering thalidomide and diethylstilbestrol (DES) to pregnant women; nor were they conversant with the contemporary scientific literature (p. 201). That is a very general problem. Physicians cannot find time to keep up with the literature in all specialties, they have to rely on official pronouncements, and their main source of information is the self-interested propaganda from drug companies propagated by the drug representatives who besiege doctors and hospitals. Much standard medical practice nowadays is counterproductive (Bauer 2014). Dreger came to realize that the horror story of DEX is "something of an ethics canary in the modern medical mine" (p. 236). Sweden sometimes does better (p. 250).

Dreger's husband is credited with the insight that "for the sake of progressive change, people should sometimes be left in a state of productive naiveté." Dreger is quite clear about the host of systemic factors that resist evidence-based discussion, policies, and actions, including the commercialization of academe[11] (e.g., pp. 134, 257), yet she remains willing to battle against them.[12] May she long remain productively naïve, as indeed she intends (e.g., p. 253).

In the meantime, this book allows us to experience her battles vicariously and to learn a great deal about many things.

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Notes

[1] "Maxims for Revolutionists: Reason"; published for instance at pp. 281–282 in *Man and Superman*, Penguin Books 1946 edition. Shaw wrote not "person" but "man," which was generic for "human" in English usage in the good old days before political correctness; see, e.g., *The Concise Oxford Dictionary of Current English*, 1951 edition.

[2] This use of "gender" in place of "sex" is another child of political correctness: "gender . . . is a grammatical term only. [To use it in place of "sex"] . . . is either a jocularity (permissible or not according to context) or a blunder"—H. W. Fowler, *A Dictionary of Modern English Usage* (Oxford: Clarendon Press, 1961 printing).

[3] Would that all interviewers should practice as Dreger does, giving the interviewed person control over what finally gets cited (p. 146).

[4] A nice description of political correctness is "the dangerous intellectual rot occurring within certain branches of academe—the privileging of politics over evidence" (p. 139). I was astonished to be called courageous and politically incorrect—by Deans of Liberal Arts Colleges!—for saying that people should be treated as individuals rather than as members of a group (Bauer 1993). My university lost the services of a nationally renowned teacher of huge classes in economics because of a misguided campaign by self-styled feminists (Bauer 1992, 1992–1993); see obituary at Allan Beryle Mandelstamm, http://www.legacy.com/guestbooks/roanoke/guestbook.aspx?n=allan-mandelstamm&pid=167009170&eid=sp_gbupdate#sthash.fg8MAPgE.dpbs

[5] Drug companies capitalize on physicians' ability to prescribe off-label, finding ways to encourage off-label uses even though the companies are legally proscribed from advertising or recommending such uses; see, e.g., Bauer (2014).

[6] For full documentation of Gallo's misdeeds, see Crewdson (2002).

[7] That article has the distinction (so far) of being the only scholarly work condemned by an Act of Congress, instituted by the "not so honorable Tom DeLay" (p. 112 ff.). Read here also about the improper behavior of the American Psychological Association as against the proper behavior of the American Association for the Advancement of Science.

[8] Personal communication from Dewey McLean. I had been Dean of

McLean's college when Alvarez sought to block McLean's promotion.

[9] For example, "Conservation biology and environmental anthropology are disciplines that are both concerned with the identification and preservation of diversity. . . . Conservation biology has often been called a crisis discipline" (Drew & Henne 2006). "Identification" clearly qualifies as scholarly work, "preservation" just as clearly does not. The term "crisis discipline" seeks to award activism the undeserved intellectual status of discipline.

[10] As in Wikipedia, where individuals are powerless to correct entries about themselves; see "HIV skepticism, Nessies, homophobia, and racism," http://wp.me/p8Qhq-gl; "Beware the Internet: Amazon.com 'reviews', Wikipedia, and other sources of misinformation," http://wp.me/p8Qhq-a1

[11] Purportedly professional academic journals may be riddled with conflicts of interest (p. 272 ff.), and a spate of newly founded publications are in it purely for the money; see for instance Beall's list of what he calls predatory journals and publishers, http://scholarlyoa.com. Established commercial publishers like Elsevier also proliferate new journals for the same reason, solely to make money. Authors pay "production fees" that greatly exceed the actual costs of publishing these online journals.

[12] Dreger is of the ilk of those who work in nonprofits like the Innocence Project that uses DNA evidence to exonerate wrongly convicted people (www.innocenceproject.org) or the Office of Medical and Scientific Justice (www.omsj.org) which has rescued from potentially severe penalties dozens of individuals charged with passing on "HIV"; OMSJ forces experts under cross-examination to admit that the risk of "transmitting" the condition of "HIV-positive" is no more than 1 in 500 with unprotected intercourse.

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BOOK REVIEW

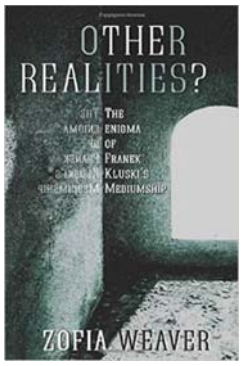
Other Realities? The Enigma of Franek Kluski's Mediumship by Zofia Weaver. Hove, UK: White Crow Books, 2015. 152pp + xxi. $16.99 (paperback). $8.99 (Kindle). ISBN 978-1-910121-39-9.

Scholarly studies of physical mediumship typically list D. D. Home and Eusapia Palladino as the most convincingly documented mediums of all time, and most also rate Home's case as among the most spectacular. Although many consider other cases of physical mediumship to be as dramatic as that of Home (e.g., those of Carlos Mirabelli, and Indridi Indridason), and while other less dramatic cases are often ranked as highly significant (e.g., Kathleen Goligher, Rudi Schneider, Eva C.), the prevailing view is that, despite their different virtues, these cases lie within the shadows of Home and Palladino. My own survey of physical mediumship (Braude 1997) represents this received opinion, an opinion which the book currently under review has forced me to reconsider.

I've known all along about the apparently spectacular case of the Polish medium Franek Kluski (1873–1943).[1] For example, I knew that (under conditions that seemed to rule out fraud) he reportedly produced lifelike materialized forms of both humans and animals, and that he frequently used warm paraffin to produce moulds of ostensibly materialized hands, some obtained under apparently quite tight conditions at the Institut Métapsychique International (IMI) in France, under the supervision of Gustave Geley and Charles Richet. The problem, however, was that the most important and extensive documentation of Kluski's phenomena had never been translated from Polish into English, and so there was very little on which I and other Polish-challenged commentators could rely. (But see Barrington 1994a, 1994b, Coleman 1994, Pawłowski 1925, Varvoglis 2002).

Zofia Weaver's new book has quite simply changed the landscape. In addition to summarizing the case as a whole, she has translated large chunks of the most important eyewitness accounts. So it's now possible for students of physical mediumship to form a competent assessment of Kluski's place in parapsychological history. My own take on the matter, after reading this book, is that Kulski probably deserves to be mentioned in the same breath as Home and Palladino.

I realize that many recoil at the prospect of relying on eyewitness accounts from cases of physical mediumship, but as I noted in my Editorial



in this *Journal* in Volume *28*(2), Summer 2014, the usual concerns result simply from sloppy thinking (see also the discussions in Braude 1997, 2007). And in the Kluski case, the conditions of observation were often quite good and unfavorable both to malobservation and trickery of sufficient magnitude, and the observers were often wily veterans of sittings with physical mediums or at least obviously alert and quite sharp. In fact, I agree with Weaver's comment that in the Kluski case "The quality of the sitters is reflected in the quality of the reporting" (p. 7).

In one important respect, Kluski's mediumship differs from that of the other major players in this arena, including Home and Palladino. As Weaver notes,

> He never performed publicly as a medium, never made any public references to his mediumship, never profited financially from it, and gave it up after a fairly brief period of intense experimentation. (p. 9)

Indeed, Kluski had a day job as a banking professional and creative writer. Physical mediumship was a passionate side interest, but Kluski had no interest in promoting his expertise. In fact, his mediumship had such an adverse effect on his health that after several years of mediumistic activity he either stopped it altogether or at least cut back severely. In fact, Weaver observes

> he never wrote about his mediumistic experiences, fearing the kind of sensationalism they would attract, and although he allowed researchers to investigate the phenomena he produced, he never tried to obtain any material benefit from his psychic abilities. He was offered large sums of money to undertake tours of America but, as he confided to a contemporary, he believed that he could not convince the whole world that he was not cheating, and therefore would rather leave his children a name unsullied by controversy. (p. 10)

Kluski could also have used his mediumship as a platform to promote his literary works, but he declined that opportunity as well, along with refusing the many invitations to speak publicly about his séances.

As for the phenomena themselves, they ranged from poltergeist-like knocks, raps, apports of objects from other rooms in the apartment, and movement of objects in the room, to dramatic light phenomena, some of which coalesced from nebula-like shapes, at close range and under either

red light or the light of nearby luminescent screens, into solid human or animal forms that interacted with sitters, and which could often be seen at the same time as the medium (and which in any case didn't resemble the medium). Sometimes the materialized figures (called "phantoms") even illuminated themselves, apparently from within. And then, of course, there were the moulds of hands, and the hands usually looked nothing like Kluski's (often they seemed to be those of a child or a woman). Moreover, the hand-moulds were often produced on the spot in accordance with the expressed or silent wishes of the sitters.

I should also note that the materialized phantoms exhibited certain intriguing regularities. Weaver writes:

> a significant difference began to be apparent between phantoms showing themselves in the light of a screen, and those which illuminated themselves with their own light. The latter were less numerous but incomparably more perfect in their appearance. While about half of the phantoms which illuminated themselves with a screen were recognised as those of deceased persons known to the participants during their lifetime, those emitting their own strong light were of a different nature. The dignified appearance of some of them sometimes resembled famous historical figures or types characteristic of ancient epochs, both Eastern and European. (pp. 31–32)

However, as in other cases of physical mediumship, Kluski's phantoms provided little if anything in the way of evidence for postmortem survival. Weaver is careful to note how the behavior and development of the materialized figures seemed to correspond to the spoken or unspoken wishes of the sitters. So for example, depending on what sitters wanted to experience, a materialized human figure of a male might gradually morph into that of a female, or the figure of a uniformed military officer might morph into that of a civilian. Weaver correctly notes that this seems to connect to the famous Toronto "Philip" experiments in which a group of sitters invented a character with whom they seemed to interact during a séance, and whose responses to questions never exceeded what the group had agreed upon in advance (see Owen & Sparrow 1976, 1974).

As for controls, Kluski was often held while phenomena were produced at a distance, and many reported either that he was immobile during those times or that his body seemed to react to or twitch in accordance with the object or light movements. Sometimes, bruises would appear on Kluski's body after phenomena occurred at a distance. Moreover, although Kluski was seldom undressed or searched, the medical examinations conducted after the sittings allowed for discreet opportunities to look for anything suspicious. On at least one occasion, though (April 24, 1922), Kluski sat

> totally naked for Geley and Richet in Warsaw . . . , but this did not stop the phenomena from appearing. Eventually, the researchers settled on controllers on either side of the medium holding his hands and touching his legs with theirs. (p. 39)

Furthermore,

> in the early stages of Kluski's mediumship the most elaborate controls were employed, and often repeated in different variants as new researchers flowed in. These controls were applied both in his apartment and in those of his friends. They had the medium tied up and put in a net and used seals, but this did not stop the phenomena, only acting as an irritant by forcing Kluski to adopt unnatural positions. Once it was established that regardless of the controls the phenomena were the same, the more intrusive methods were avoided so as not to exhaust and hurt the medium. (pp. 38–39)

Kluski's moulds merit a few more comments. The idea behind them was to produce a permanent paranormal object (PPO), something whose existence after the séance would provide continuing evidence of paranormality. Weaver provides a good survey of the inadequacy of skeptical efforts to dismiss the moulds. For example, it's implausible that the moulds were produced prior to the séance and smuggled in by Kluski. Some experimenters secretly added a blue dye to the paraffin just before the séance began, and others secretly added cholesterol. In those cases, the moulds appearing during the séance contained the added substances. Moreover, the

> splashing of the wax would be heard (and sometimes felt), and within a minute or so warm, soft gloves, different in size, character, and arrangement, would drop on sitters's hands or in their laps, so thin (1 mm) and fragile that most of them got damaged and did not survive. However, most of them did survive long enough for people to feel them when warm and soft, hang on to them, and examine them when cooled. (p. 108)

There's much more I could say here about this fascinating case. But why read me? Go to the source and read this book instead. It's a major addition to the parapsychological literature.

Stephen E. Braude

Note

[1] This is a pseudonym. Kluski's real name was Teofil Modrzejewski.

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BOOK REVIEW

Other Realities? The Enigma of Franek Kluski's Mediumship by Zofia Weaver. Hove, UK: White Crow Books, 2015. xxi + 152 pp. $16.99. ISBN 978-1910121399.

Within psychical research, Dr. Zofia Weaver is best known for her research on mediums in Poland (Weaver 2002). One of the more well-known among them was Teofil Modrzejewski (1873–1943), better known as Franek Kluski. Weaver previously wrote an article about him (Weaver 1992), but felt that she had barely scratched the surface. Nevertheless, she left Kluski alone for many years: "The reason was simple: putting a great deal of time and effort into translating and researching the enormous store of material, which looked totally unbelievable, seemed like a waste of time" (p. xi).

As Dr. Alan Gauld notes in his brief Foreword, the English literature on Kluski is limited. One of the more important sources of information is Dr. Gustave Geley's *L'Ectoplasmie et la Clairvoyance* from 1924, which was translated a few years later (Geley 1927). F. W. Pawlowski, who attended several of Kluski's séances, also published an account (Pawlowski 1925). As a native speaker of Polish, Weaver has been able to utilize much more material. The most important source for her was a thick book, published in 1926, written by Kluski's best friend Norbert Okolowicz. This book was also used by Dr. Roman Bugaj during his research, and his essay about Kluski has been translated (Bugaj 1995).

During his childhood, in addition to having a few premonitions, Kluski also saw and talked to apparitions and seemed to have had several out-of-body experiences. At times he also saw auras around people's heads and shoulders. As an adult he was however unwilling to talk about his paranormal experiences. About 1900 he fought in a duel and was shot in the heart area, but to the surgeon's surprise he woke up. It is not known if he had any memories of a near-death experience. What is known is that Kluski henceforth experienced sporadic violent heart palpitations. The bullet was not removed. Not mentioned in the book is that Albert Budden who has connected alien abductions and electromagnetic effects has apparently suggested that the bullet may have functioned as a source of endogenous magnetism (Weaver 2002:63).

In her book, Weaver mentions that Kluski could cause compass needles to spin. In addition, when he held a large magnet there were purple flashes from the poles. She notes:

> There are also reports of how Kluski reacted to an impending storm and during it. Okolowicz describes him becoming very agitated, his limbs hot, something like an electrical discharge going through his body. It manifested itself by the tingling and stiffening of toes and particularly fingers, with bluish flames escaping at times from the ends of his fingers, something which could be seen in a darkened room. A storm seemed to exhaust him in a way similar to a séance, and he would be adamant about not holding a séance at such time. (p. 21)

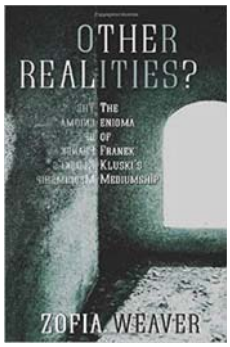
Weaver also describes phenomena that suggest that Kluski at times functioned like a poltergeist agent. Objects in his presence could move on their own, knockings and other noises without evident causes could also often be heard, and electrical lights would frequently turn on and off. These phenomena are nothing compared with the phenomena that took place around him during séances—all the traditional events and more! They bring to mind Dr. John Beloff's (1989) words:

> From time to time one comes across a claim about which one can say only that it makes one gasp. It goes so far beyond anything in one's experience, it makes such a mockery of all one's presumptions about what sort of a world it is that one is living in, one is at a loss for words. At the same time one can see no easy way of dismissing it as mere fantasy. (Beloff 1989:328)

Beloff (1996) did not seem to know what to make of Kluski, nor does this reviewer.

Kluski was identified as a physical medium in 1918 and held more than 350 séances, but the materialization séances took their toll, and he basically ceased in 1925 for health reasons. His health became better when he instead focused on conveying messages from the dead through automatic writing, but the physical phenomena were less impressive. Weaver shows that a development definitely occurred. During his first years as a medium, Kluski's trance was light, but it gradually became deeper. The séance phenomena also changed over time, for example partial materializations were common in his first years and later self-illuminating full materializations of humans appeared during his séances.

Most of the séances were held in his own apartment, but 14 were held at the Institut Métapsychique International. Dr. Gustave Geley (1927) has provided an account about the latter séances, during which thin fragile paraffin moulds were produced, allegedly by materialized hands. Oddly, the hand moulds had the size of childrens' hands, yet looked more like undersized adult hands. These moulds were much discussed in the Journal of the Society for Psychical Research in the early 1990s, and again after two skeptics published an account about how they had produced paraffin



moulds (Polidoro & Garlaschelli 1997). Paul Gaunt (2012), who provided us with a useful Summary, has nevertheless concluded that although paraffin moulds can be made in a number of different ways so far it does not seem possible to reproduce the paper-thin paraffin moulds that were made during some of Kluski's séances.

Since the moulds were so fragile, plaster casts were made and preserved. Sir Oliver Lodge (1924) noted:

> The paraffin gloves from which these casts were made, considered in conjunction with the conditions under which they were produced and the crucial tests made to ensure their genuineness, are a standing demonstration of something inexplicable by normal science. (p. 208)

Many years later, Dr. Mario Vargolis (2002) agreed.

Yet, as veteran psychical researcher Eric J. Dingwall (1926) noted, some doubt can be cast on Geley's account due to some errors in his account about the experiments with Eva C. (Marthe Béraud): "What becomes of the oft-repeated stories of Kluski? Had he one hand free as the photographs show was the case with Eva C. ..." (p. 389)? This argument was dusted off during the debate in the 1990s, but no one dared to explicitly suggest experimenter fraud or that the control was not as good as claimed. Needless to say, Geley was however not the sole witness.

Kluski's séances contained a number of phenomena. For example, there were a variety of light phenomena that sometimes transformed and various types of apparitions of both humans and animals. Many readers will find the apparitions difficult to accept as genuine. For example, Weaver notes: ". . . when close by, the apparitions seemed to have breath, heartbeat, and even tummy rumblings" (p. 45). In addition: ". . . there were many partial materialisations. There were unfinished busts, hands with missing fingers or fingers hanging on, as well as apparitions which looked as if they had been made from cardboard and rags" (p. 53). These quotes are admittedly cherry-picked. Weaver has, in contrast to Geley (1927) and Imich (1995), not included any pictures of the apparitions, which look artificial. Nevertheless, some observations about how the apparitions transform are difficult to explain.

Kluski basically ceased to give séances in 1925, but he gave a few later on. Dr. Eugene Osty participated in a séance in 1928 (p. 32), and, a year later a Swedish psychical researcher, Eira Hellberg, also sat in on a séance (Wallenkampf 1929)—both were impressed. Yet, in 1933, the veteran

psychical researcher Everard Feilding wrote a brief letter. He and his wife had been at a séance ". . . which seemed to us as so ridiculously fraudulent that we found it extremely difficult to believe in the earlier reports on the man" (Carrington 1957:35). Feilding does not however seem to have published any more detailed account. When the Danish psychical researcher Poul Thorsen visited Kluski in 1939, he had definitively ceased work—his doctor had forbidden him to hold materialization séances (Thorsen 1950). He was however still able to influence compass needles.

Weaver makes some comparisons between Kluski and other physical mediums (Daniel Dunglas Home, Eusapia Palladino, and Indridi Indridason), and a poltergeist agent, but arguably one should also compare accounts of séances with him with accounts of séances with known fraudulent mediums. Regardless, say what one will about Kluski, Weaver has produced a somewhat brief but thought-provoking book. Kluski may remain an enigma.

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BOOK REVIEW

Pharmageddon by David Healy. Berkeley & Los Angeles: University of California Press, 2012. xii + 302 pp. $41.95 (hardcover), $27.95 (paperback), $15.49 (Kindle). ISBN 978-0-52027576-8.

This book is a fully documented exposé of the considerable damage being done to health by modern "scientific" drug-based medical practice.

The author is a psychiatrist whose earlier books include *Mania: A Short History of Bipolar Disorder*, *Shock Therapy: The History of Electroconvulsive Treatment in Mental Illness*, *The Creation of Psychopharmacology: The Discovery and Development of Antipsychotic Medication*, and *The Antidepressant Era: The First Complete Account of the Phenomenon of Antidepressants*. These described how psychiatry had gone badly wrong by fixating on drugs and misinterpreting what they do, and how the pharmaceutical industry (Big Pharma) fought against acknowledging the harm done by these drugs, notably increased rates of suicide and significant decreases in expected lifespan. In this new book, Healy extends the critique to drug-based medicine across the board.

Increasingly in recent years, many insiders and observers have delineated the damage done by present-day drug-based medical practice (Bauer 2014). Prescription drugs are the 3rd or 4th leading cause of death in the developed world. Drug-company conglomerates are concerned with profit first and foremost and above all. They break laws against off-label marketing and pay fines that are tiny compared to the profits from the law-breaking. They hide data about harmful "side" effects. They mislead doctors and media and the public, and use financial incentives to buy favor from medical journals, researchers, practicing physicians, universities, politicians. They invent and market illnesses to match their drugs (e.g., seasonal affective disorder, attention deficit hyperactivity disorder, restless leg syndrome . . .), endlessly converting natural corollaries of aging into diseases (Bauer 2012) and extrapolating normal conditions of living (feeling low, feeling anxious, variations in laboratory-test numbers) into ailments calling for treatment.

What seemed like good ideas at the time had entirely unintended consequences. The trouble began with "scientific" approaches: quantitative measures like blood pressure, blood sugar, etc., etc. (Chapter 6, "The Mismeasurement of Medicine"; see also Greene [2007]); even the widespread use of weight machines had unforeseen, unintended, and

deleterious consequences (p. 166 ff.). Measuring bone density led to invention of the disease of osteopenia (p. 172 ff.).

Physicians no longer listen to and examine and think about their patients, instead they are essentially automatons taking instructions from lab tests and official guidelines and drug-company propaganda, as though all patients suffered from "some drug deficiency disorder" (e.g., pp. 5, 14, 186, 235). All of medicine has been infected by an absence of clinical diagnosis informed by physician's experience and patient's personal knowledge of and insight into what seems to be wrong: "Doctors . . . are treating diseases rather than treating us. There are no guidelines for treating us. There are only guidelines for the treatment of cholesterol levels or diabetes or depression" (p. 158).

The obsession with measurement extends even to emotional or mental matters. "Rating scales" can "diagnose" anxiety, depression, bipolar illness, etc., even in individuals who function overall quite well despite the normal human episodes of feeling low or exuberant or worried (p. 177 ff.); thus 15–25% of expectant mothers can be diagnosed as "depressed" and antidepressants become among the most commonly prescribed medications for pregnant women (p. 182).

Routine measuring amounts to mass screening without informed consent (p. 170), even though it can lead to damaging consequences from unnecessary treatment and apparent epidemics of newly created diseases (p. 175).

Healy blames primarily these factors:

1. Changes in patenting of medicines in the 1950s that have enabled monopolies (pp. 256–258). Markups on and profits from drugs are enormous. Big Pharma has bribed and corrupted medical journals, professional associations, official agencies, has co-opted if not bribed (e.g., pp. 136, 222 ff.) prominent physicians and researchers, and has assisted in the money-based corruption of politics. Journals and professional associations are complicit with Big Pharma in hyping claimed benefits by repetitive publication and suppressing risks of harm from drugs (pp. 122 ff., 245–246); journals should—but do not—insist that articles make available all supporting data (p. 245). Academe has been corrupted as an unintended consequence of the Bayh–Dole 1980 law that encouraged academics to partner with industry (p. 34). [The obsession with economic growth, Healy points out, may lower quality of life (p. 168). The explosion of healthcare costs, directly harmful to the general public, makes the associated increase in Gross Domestic (or National) Product look good to economists.] Drugs come to market not because they offer novel health benefits but

because the patent on an earlier drug had run out (p. 30 ff.). Thus Big Pharma marketed SSRIs (selective serotonin re-uptake inhibitors) to become the standard antidepressants even though they are not as effective as tranquilizers or older antidepressants (pp. 33–34, 87, 144). The earliest hypertensive drugs are still better than the newer, increasingly expensive ones (p. 87). The era of "blockbuster" drugs began with Zantac in 1990. Ten years later, nearly half of all drug sales were from blockbusters like SSRIs and statins which entail lifelong consumption. Drugs like antibiotics that actually cure an illness are used for only short periods and are not lucrative money-makers.

2. Making drugs available by prescription only, also in the 1950s. The unintended result has been that doctors, trained to use apparently accepted information and unfamiliar with marketing techniques, are subjected to "the most sophisticated marketing machine on the planet"—as are policymakers and the general public. The very terms SSRI, statin, ACE inhibitor, mood stabilizer, and more, are creations by marketing departments intended to distinguish new and "scientific" products from older ones (p. 34 ff.). Since doctors prescribe drugs only for medical conditions, Big Pharma's goal became to create as many medical conditions—illnesses—as possible (Moynihan & Cassels 2005). "Manic-depressive illness had been a rare and serious condition affecting ten people per million . . . . Bipolar disorder, in contrast, supposedly affects up to 50,000 . . . per million" (pp. 37–38, 152). To illustrate the unintended negative consequences of prescription-only drugs, Healy compares expensive prescription-only "SSRIs" with dangerous "side" effects to inexpensive over-the-counter (OTC) anti-histamines that also have a serotonin-uptake-inhibiting effect (see Hellbom 2006) but far less dangerous side effects (p. 249). Pregnant women have largely learned to avoid OTC substances, even coffee, yet they are prescribed SSRIs that double the rate of birth defects and miscarriages (p. 250).

3. No disinterested independent testing of drugs, since the time up to the 1950s when the American Medical Association (AMA) tested new drugs in its own laboratories. It does so no longer, and the AMA and its journals obtain most of their funding from Big Pharma (pp. 40, 247–278). *Clinical trials are carried out dishonestly and their conclusions are mis-used*. The fundamental flaw is that drugs are tested on people unlike those to whom the drug will later be prescribed—unlike in degree or even nature of illness as well as in age, race, sex, medical history, concurrent other medications, or conditions. Up to about the 1950s, drugs came into use because physicians saw tangible changes in actual patients (p. 150), not because some small average difference from placebo among large numbers of people could attain "statistical significance" (p. 211 ff.). Nowadays "guidelines" based on

dishonestly conducted clinical trials and marketed assiduously convince physicians to prescribe drugs when there is no tangible illness and when the drugs produce no improvement noticeable to physician or patient, only changes in biomarkers that may not reflect morbidity or mortality (p. 156). Outright fraud is now prevalent through the deliberate biasing of clinical trials and the hiding of unfavorable data (e.g., pp. 214, 252–253). Equating clinical trials with "evidence-based medicine" is a Trojan horse (pp. 12–13). "[C]ontrolled trials [are turned] inside out, neutering their potential to show that some currently fashionable drugs don't work and transforming them into a means to sell worthless remedies" (p. 65). Despite clear evidence that lifestyle is the chief risk for heart disease, drug marketing highlights the "much less" important association with cholesterol level (p. 169). One of the staggering, hard-to-believe, but fully documented circumstances is that Pharma marketing maintains prescribing of drugs even after they have been shown to be harmful, for example, SSRI antidepressants in pregnancy even though they cause birth defects (p. 44–45, 63) or beta-agonist inhalers that increase mortality (p. 161). Glaxo ignored the bacterial cause of ulcers because they were selling the hugely profitable anti-acid H-2 blocker Zantac (p. 50). With ulcers cured by antibiotics, H-2 blockers have since been marketed instead for GERD (gastro-esophageal reflux disease), previously a rare condition that has now been extended to include infant colic. Colic was never fatal, but the new anti-colic drug Prepulsid did kill some infants (pp. 53–54). *Propaganda misleads by enshrining "statistical significance" from clinical trials as demonstrating value for everyone*, when in reality each individual case may be unique (p. 211 ff.): That drug treatment of blood pressure of 250/120 may be a good thing does not entail that everyone with blood pressure over 140/90 should be administered drugs, but that is the current illusion. *Trials tend to use high doses*, to give the best chance that a given biomarker will show an effect. No data are gathered on what the lowest useful and least poisonous dose might be (p. 88).

Averaging together patients who responded favorably, those who responded very little or not at all, and those who became worse still allowed antipsychotic drugs to seem better than placebo and thereby gain approval for marketing as a general treatment for schizophrenia; yet between one-third and two-thirds of patients are not helped, and all of them suffer the "side" effects of increased rates of heart attacks, strokes, diabetes, and suicide—with an overall decrease of life expectancy by decades (pp. 88–89). Big Pharma is not interested in discovering which patients might be helped and which might not, so this knowledge remains hidden from doctors and patients.

The official pronouncement that "the drugs work" means that physicians seeing no improvement or even deterioration are likely to increase dosages instead of reporting inefficacy or possible harm from the medication (p. 253). Deterioration is blamed on the underlying illness and not on the treatment. So harmful "side" effects may not be reported for a long time; yet systematic, routine data-gathering, which has become so easy in the Internet age, could readily serve to evaluate the efficacy and safety of drugs once they are in general use (pp. 253–254). *Doctors are not free to exercise clinical judgment based on experience and individual circumstances* because of official guidelines sanctioned by all healthcare institutions. Purely statistical data outweigh the uniqueness and idiosyncrasies of the patient (p. 211 ff.).

Like Gøtzsche (2013), Healy makes a thought-provoking reference to the tobacco industry. Tobacco is helpful for ulcerative colitis and probably as good an antidepressant as Prozac or SSRIs. Had tobacco been available by prescription only, with the associated vested interests, how much longer would it have taken before the industry had to acknowledge its harmful "side" effects? (pp. 48–49, 251). Like the tobacco industry, Big Pharma recognized that "doubt is our product" to turn "scientific doubt inside out, transforming it from a means to detect truth into a means to conceal the truth" (pp. 118, 224, 260).

How do drug-company employees—from the top CEOs, VIPs, and Board members down to researchers and salespeople—manage to feel that they are serving the public good even as clinical trials are biased and results disseminated misleadingly? In the same sort of way as people in the tobacco industry did (and do). How do they—and doctors and the general public—square the long list of side effects reported in advertisements with continued marketing and belief in a beneficial value of the drugs? Healy offers an explanation: The multitude of "side" effects are just reports, *anecdotes*, not "scientific evidence." That comes from clinical trials, and these are carefully designed to be too short or too small to allow "side" effects to become "statistically significant" (p. 243). Perhaps 30,000 heart attacks occurred before Merck acknowledged that risk associated with Vioxx (p. 244).

Healy points out that free markets have co-opted and distorted science, in what he calls "industrial postmodernism" that has suppressed the ability to say "that an increase in mortality is an increase in mortality and blockbuster drugs cause adverse events" (p. 261).

Similarly radical and provocative remarks pervade this book, but they can hardly be dismissed as excessive since every assertion is fully documented. That medicine has gone badly wrong is demonstrated by the

failure of the drug-based "scientific" "evidence-based" approach to fulfill its promise of better health and lower healthcare costs (p. 184 ff.): To the contrary, in the most "advanced" society, the United States, healthcare costs have risen about 4 times faster than general inflation while overall health lags other developed countries by every measure including life expectancy and infant mortality (pp. 192, 260).

Every literate person should read this book. It ought to be required reading for every policymaker and every aide to a policymaker.

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BOOK REVIEW

Conversations with Ghosts by Alex Tanous with Callum E. Cooper. Hove, United Kingdom: White Crow Books, 2013. 138 pp. $15.99 (paperback). ISBN 978-1-908733-55-9.

I was invited to review this book because of my association with Alex Tanous. For more than a decade, beginning in 1975, I worked as a researcher at the American Society for Psychical Research (ASPR) alongside Karlis Osis, the ASPR's Research Director and Chester F. Carlson Research Fellow. During those years, Osis and I conducted a variety of experimental studies and field investigations; Alex Tanous participated as an unpaid subject in much of that research. Sadly, I am the sole survivor of our little team[1] and, therefore, the last insider who could provide firsthand insight about the people and research activities of the ASPR during that era, and who could critically examine this book's treatment of the same.

The authorship of this book is credited to the psychic Alex Tanous, who died in 1990, approximately 23 years prior to the book's publication. Using the designation of "with" (often employed by "ghostwriters,"[2] no pun intended), co-authorship was credited to Callum Cooper who, at the time the book was written, was a doctoral candidate at the University of Northampton's Centre for the Study of Anomalous Psychological Processes, Department of Psychology.

On p. xi of the 16-page Introduction, Cooper describes this book as "a collection of investigation accounts written by the late Dr Alex Tanous." He goes on to explain that "it was an unpublished manuscript consisting of only three chapters.[3] It was intended to be completed as a collection of case reports by Dr Tanous and Dr Karlis Osis of the American Society for Psychical Research."[4,5] Cooper then states: "However, Dr Tanous never finished this insightful and valuable addition to parapsychological literature. Therefore, I felt it was time for the book to be completed by bringing together a number of Dr Tanous' unpublished notes and personal thoughts on hauntings and apparitions, to fill in the missing gaps in the manuscript" (p. xii). In addition to Tanous' "notes and personal thoughts," it seems apparent that other resources were used to help provide the "filler" for the book (which, in addition to the original three chapters attributed to Tanous, contains three more chapters and four appendices); those sources appear to include previously published accounts, interviews, and the recollections of those

acquainted with Tanous, audio recordings made by Tanous while working on investigations, and other material from the archives of the Alex Tanous Foundation for Scientific Research. Although Cooper is too young to have known Tanous during his lifetime, judging from his tone he nevertheless appears to be an enthusiastic supporter of Tanous and his work.

On p. xvi of the Introduction, Cooper describes Tanous' participation in out-of-body experiments at the ASPR. Certain statements need to be addressed. Cooper's interpretation of the purpose of those studies is a bit off the mark; he states "Its aim was not only to see if Dr Tanous could project his mind beyond the body and view target images at a distance, but also to measure whatever was supposedly leaving the body during this process (i.e., human consciousness, the mind, the spirit, etc.)" (p. xvi). While the working hypothesis included the prediction that the subject would use a combination of extrasensory perception (ESP) and psychokinesis (PK) to correctly identify the optical target while simultaneously affecting the strain gauges in the location from which the optical target could be perceived, there was no possible way through that experiment "to measure whatever was supposedly leaving the body during this process." Moreover, I was surprised to read that Tanous produced "on average, a ninety per cent hit rate" (p. xvi) during those trials! I am puzzled by that error since, on p. 93 of *Conversations*, Cooper himself cites the published results of the experiment (Osis & McCormick 1980): "In all, 197 trials taken, of which 114 were hits and 83 were misses" on the optical targets; that works out to a hit rate of 57.87% not 90%.

On a more affirmative note, an incident Cooper described on p. xvii does ring a bell. Although I cannot recall all of the details, I can confirm that during an OBE session Tanous claimed he could not see the target image, and upon investigation I discovered that one of the bulbs in the optical apparatus had burned out.

In the nine pages of the first chapter, Ghosts and the Hunters, there is a description of Tanous' association with Osis and the ASPR. Tanous supposedly credits himself as being "one of the foremost ghost hunters in the country" (p. 1). There is a brief, rather subjective discourse on varieties of apparitional phenomena and how "true manifestations" (p. 2) differ from such other phenomena as poltergeist activity, along with a description of the typical protocol Tanous witnessed when investigating a haunting[6] with Osis.[7] While the description of the investigative protocol is mostly accurate, the description of the equipment, its use in field investigations, and the analysis of data that were collected is less than perfect; this is not surprising since Tanous' role in such investigations was to provide his impressions as a psychic, he was not involved in the other aspects of the investigative

process. I was also rather surprised to read the claim about Tanous having "solved" ASPR cases and that "The majority of cases in the ASPR files are closed" (p. 5); I can recall no member of the research team ever having indicated that they construed an investigation that way and I remember no cases that were definitively "solved" by Tanous or any other psychic.

Throughout the book, discussions about the phenomena and related theories are often presented in an assertive tone that is usually reserved for that which has been scientifically substantiated, rather than merely reflecting the author's own observations and opinions. While this may be understandable given the nature of the book, I find it somewhat disturbing as it could have a misleading effect on laypersons.

In Chapter 1, several pages are spent listing reasons—which seem based on conjecture or belief—why ghosts exist, e.g., "the entity can create an apparition (or related phenomena: movements, bangs, and raps, etc.) whose aim it is to restore the universe to harmony, including his or her own individuality within the wholeness of the universe" (p. 6). That explanation seems to fall under the category of "unfinished business," that is, the popular belief that the deceased personality persists in order to seek closure regarding unresolved life matters; with that, the chapter is concluded by saying that Tanous' job as "ghost psychologist" is to help the entity do just that (p. 9). I cannot speak about Tanous' skill as a therapist to the deceased, but I can say that I was repeatedly impressed by the calming influence Tanous' charm and compassion had on the living experients of the phenomena.

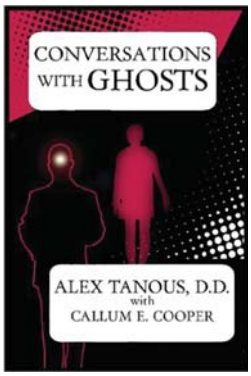
Chapter 2, Houses with Multiple Manifestations, is a 25-page treatment that begins with an explanation (attributed to Tanous) about why certain houses are haunted: "Spiralling effects occur when multiple manifestations converge on a site, drawing to the house people with similar character weaknesses as the original inhabitants who had begun the cycle with a specific psychic event to which they are eternally attached" (p. 11). Two investigations (conducted by Osis and in which Tanous served as a psychic) are presented in an effort to support this hypothesis. The first case, Cedar Rapids, is described as an investigation that was done remotely, that is neither Tanous nor Osis visited the location; the investigation is described as having been handled via phone interviews, as well as sessions in the ASPR laboratory where Tanous gave his impressions about the case. The second example, Hawk Mountain, was an onsite investigation.

While it is not stated as such, given the amount of quoted dialog that appears in this (and other) sections of the book, I am inclined to believe that this chapter largely consists of transcribed audio recordings made by Tanous, who frequently made his own tapes of field investigations and

experimental sessions. Interspersed with the quoted text is background material, which is presented as the facts of the case (e.g., information about the location, the experients, and their circumstances), as well as descriptions of the behavior of individuals heard in the recordings. Throughout the book, the case "facts" and claims are usually sketchy, which may reflect (1) the minimal amount of information Tanous was given about a case prior to his participation in it, (2) that the manuscript was in draft form, that is at a stage prior to Tanous having obtained all the information necessary to present the cases objectively and in full detail, and/or (3) that errors crept in during the posthumous edit. Regardless of the reason, it is difficult to evaluate the accuracy of Tanous' impressions from the information presented.

I also suspect that a degree of narrative license may have been taken with the dialog tags and descriptive material which, most likely, were added, when preparing the original three-chapter draft (or perhaps when preparing the manuscript for this book). I question the validity of some of this material as it often deviates from my own knowledge and recollections, e.g., "Dr Osis nodded and made notes on the pad in front of him in his small, precise handwriting" (p. 22; Osis' handwriting was neither particularly small nor precise especially when he was note-taking); "The three of us sat in the silence of the dining-room. Donna started to shiver" (p. 23; I'm not inclined to shiver, moreover I suggest that the person identified as me in parts of the transcript was actually a female volunteer who accompanied us on this investigation). Such descriptions sometimes include references to the output of physiological monitoring and other equipment during investigations, e.g., "'What is it?' Dr Osis was staring at the instruments. . . . They were giving out unusual readings, at increasing rates" (p. 16). While Tanous did make many audio recordings, he was not an incessant note-taker, especially not while he was giving his case impressions during field investigations, so it strikes me that details about behavior or comments about data were probably added later purely for the sake of readability.

Chapter 2 also includes a description of Tanous' participation in out-of-body research at the ASPR. One thing that puzzles me is the reference to a "black box" (p. 13) in which Tanous was situated during experiments. While I don't recall anyone ever calling it a "black box" (although Tanous did refer to it as his "Cape Kennedy," Osis and I referred to it as a "sound lock," and others called it "the meat locker" because of its resemblance to a walk-in refrigerator), the chamber in which experimental subjects were located was actually an electrically shielded, sound-attenuating booth; to my knowledge, it was not "built" or acquired specifically for Tanous as is claimed on p. 13. Subjects were placed in the booth during experimental sessions in order to isolate them from the researchers and equipment, as



well as from environmental influences and stimuli. For their comfort, subjects were either seated on a reclining chair or rested on cushions. Given that Tanous spent a great deal of time over the years in that booth, it is odd that the location would be described as "in the next room" (p. 13), that is next door to the monitoring lab, when, during all my years at the ASPR, it was actually situated at the opposite end of the building, three rooms and a long corridor away; the statement about the location is even more perplexing because it is inconsistent with the more accurate description on p. xvii of the Introduction. Perhaps I should also point out that, in the footnote on page 13, Cooper states that "The black box allowed Tanous to go out of his body and travel anywhere he desired"; for the record, the booth itself did not contribute to the psychic ability of Tanous (or any of the other subjects) except insofar as it provided a comfortable location where sensory input was minimized, thus allowing subjects to focus on the task at hand.

Continued Bonds is the title of Chapter 3. In these 14 pages, two investigations are described; they are grouped together because of the assertion that they both involved romantic relationships that persisted after death. Overall, the cases read like stories, which indeed they are: stories created largely from Tanous' impressions of the history of the house and its deceased inhabitants; those impressions were not substantiated in the book. The second case contains excerpts transcribed from what I assume were Tanous' audio recordings while on site; those excerpts include anecdotal accounts of the experients, as well as Tanous' own impressions. Unlike the cases in the previous chapter, there is no reference as to whether Tanous investigated these cases on his own: no mention was made of Osis or the ASPR in the first case; the second case just makes a passing reference to the residents calling the ASPR (pp. 40–41).[8] However, in the reports throughout the book, there was little or no mention made of efforts to investigate and, by so doing, to attempt to substantiate Tanous' impressions; such efforts were a routine part of ASPR investigations.

Chapter 3 is followed by five pages of photographs of Tanous and one full-page photo of Osis.

If I am correct that the first three chapters comprise Tanous' unfinished manuscript, then I can only assume the next three chapters were those written by Cooper and based on Tanous' notes, transcripts, etc. Each of Chapters 4 through 6 is prefaced by Cooper. The content ranges from reports of Tanous' impressions while on two additional haunting investigations,

i.e. Dandy House and Frolic House; to Ghosts, Souls and Spirits?, which contains excerpts from a December 11, 1981, interview with Tanous (interviewer unknown); to The Search and Research of Survival, which consists of multiple subsections relating to survival issues (e.g., Tanous' distinction between "spirit" and "soul"; an account of his first out-of-body experience and a later one resulting from an operation), including additional mention of Tanous' participation in the ASPR's research on the out-of-body experience.[9] None of these chapters are very long (8, 20, and 12 pages, respectively), so there is little room for more than cursory treatment of the subject matter. However, in their entirety, they do help provide a glimpse into Tanous, his work, and his beliefs.

In addition to a four-page index, the remainder of the book consists of appendices.

Appendix 1 contains another interview: Loyd Auerbach Interviews Dr. Alex Tanous, which has been reprinted (with permission) from Auerbach's 1986 book, *ESP, Hauntings and Poltergeists*.[10] The discussion focuses largely on Tanous' beliefs about apparitions and haunting phenomena.

Appendix 2, The Truth Behind Amityville, is a nine-page section that presumably was written by Cooper about the house that was the subject of the book and later the film, *The Amityville Horror*. Essentially, Cooper describes how a number of parapsychologists, including Osis (and Tanous), were invited by members of the Lutz family and their associates to investigate the Amityville house, but those researchers quickly became suspicious of the family's intent and abandoned any thought of pursuing an investigation. Cooper was correct in his presentation of the reactions of Osis and Tanous, both of whom told me they disengaged from the Amityville case as soon as they realized that those involved were seeking publicity and requested that investigators sign a blanket release document. Cooper also states that Tanous refuted media stories that falsely described his involvement; I assume that this Appendix was included in the book to carry on Tanous' efforts to discredit false claims made by the media and others about his involvement as a psychic investigator and as an experient of phenomena at the Amityville house.

In Appendix 3, Jennifer Allen's Memories, someone whom Cooper identifies as "a great friend of Dr Tanous for around twenty years" (p. 113) provides several personal anecdotes involving Tanous.

Finally, Appendix 4, Alex Tanous—Ghostbuster, is a "summarised and edited" (p. 124) version of an article in the *Casco Bay Weekly*, "Alex Tanous, Ghostbuster," written by Thomas Verde in 1988.

I can only imagine how difficult the task of writing a book with a deceased co-author must be and so I acknowledge Cooper's efforts in

Conversations. One of the difficulties he faced was the need to rely on Tanous' "unpublished notes and personal thoughts" (p. xii) as filler throughout the book. Another difficulty was the apparent need to edit the chapters credited to Tanous, chapters that were, in all likelihood, in a draft stage that had not been subjected to elaboration or scrupulous review by the senior author himself (i.e. Tanous), which might explain some of the inaccuracies (described above) and why the material that was necessary to substantiate Tanous' case impressions was lacking. Because of this, readers do need to be mindful that this is not a critical, scientific treatise on ghosts, hauntings, or other paranormal phenomena, the investigation thereof, or scientifically derived theories of the same. However, *Conversations with Ghosts* can be regarded as a resource that reflects the personality behind the pen and sheds some light on the work of Alex Tanous, a gifted individual who had a passionate desire to help achieve a scientific understanding of the paranormal.

Notes

[1] It should be noted that this book mostly concerns cases and experiments that took place during my era; my predecessor, Dr. Janet Lee Mitchell, worked with Osis and Tanous prior to that time.

[2] It was not unusual for Tanous to use professional writers, and to credit them as co-authors, when he intended a book for publication.

[3] We are not told which three chapters out of the six constitute the unpublished manuscript; I assume they are the first three, given that those chapters have no prefacing remarks by Cooper.

[4] While some have interpreted this line to mean that Tanous and Osis were co-authoring a book together, given what I know of both men I suggest that a more likely scenario is that Tanous intended to himself author a book that would recount, from his own perspective, the investigations which Osis invited him to participate in as a psychic.

[5] On p. xi of the Introduction, Cooper states that the ASPR was founded in 1884 by William James; it should be noted that, according to most accounts, including the historical information presented on the About the Society page of the ASPR's website, 1885 is the year attributed to the founding of the ASPR with William James listed as one among several founding fathers.

[6] Please note that my use of the (sometimes popular) jargon of the field is for convenience only; I am not imposing judgment as to whether the phenomena referred to exist or have been scientifically substantiated since questioning such matters is not within the purview of this book.

[7] While Tanous investigated cases independently, his work as a regular (psychic) subject in formal research under the auspices of a scientific psychical research institute was, to the best of my knowledge, done almost exclusively with staff researchers at the ASPR.

[8] I personally have no recollection of these cases, nor do I recall callers and their cases being turned over to Tanous without ASPR researchers being directly involved in the investigations.

[9] The descriptions of the equipment used in the experiments are largely accurate. However, no information is given to justify the statistical results claimed on p. 92: "A series of three (two with the Optical Image and one with the colour wheel) gives these results: The average score of the three tests calculated on the law of probability is one out of a hundred. In the third series, independently, the law of probability would be one of a thousand." The validity of the claims should be questioned if for no other reason than because the two sets of experimental series (one that involved the color wheel and the other set of two series involving the OID and strain gauges) are not directly comparable.

[10] Loyd Auerbach did indeed work in the Education Department of the ASPR for several years during which time he got to know Tanous.

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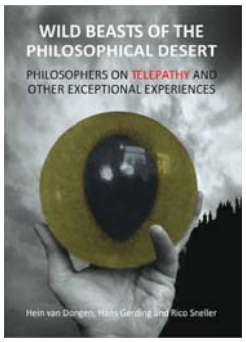
BOOK REVIEW

Wild Beasts of the Philosophical Desert: Philosophers on Telepathy and Other Exceptional Experiences by Hein van Dongen, Hans Gerding, and Rico Sneller. Newcastle upon Tyne: Cambridge Scholars, 2014. 176 pp. + xii. $59.59 (hardcover). ISBN 978-1443854535.

This slender and interesting volume by three Dutch philosophers examines the manner in which eight prominent philosophers dealt with ostensibly paranormal experiences arising both spontaneously and also as the result of hypnosis. Hans Gerding covers both Immanuel Kant and Arthur Schopenhauer; Rico Sneller discusses Friedrich Joseph Schelling, Hans Driesch, and Gabriel Marcel; and Hein van Dongen considers William James, Henri Bergson, and Jacques Derrida.

My guess is that *JSE* readers might already know about Kant's apparent ambivalence (or perhaps just change of heart) about Swedenborg's vision of a Stockholm fire (and his other reported experiences), as well as William James's investigations of mental mediumship (Mrs. Piper in particular) and his experiments with altered states. Nevertheless, I expect they will find much that they didn't already know in those chapters, as well as in the other chapters.

Interestingly (and perhaps surprisingly), the authors decline to take any stand on whether psi phenomena are genuine (although the philosophers they survey are often quite clear and positive on that score),[1] and they also refrain from judging the adequacy of the theoretical claims made by the philosophers they portray. Their avowed interest is in describing the philosophers' distinctive relationships with exceptional human experiences and the impact that had on their respective philosophies. That's fine, but I'm less happy with the authors' stated justification for withholding judgment on whether they believe any psi phenomena are genuine. In their view, it's important to be open to the "complexity and equivocality of our existence" (p. 8), which from their various other comments I understand to be a call to tolerate more humility and uncertainty in empirical knowledge claims. That's fine as well. But they claim that an impediment to achieving that openness would be to focus "on an artificial contrast between the ruling worldview and the anomalies that do not seem to fit" (p. 8). Now first, I don't see that this contrast is at all artificial in any way that justifies omitting it from consideration. Granted, the prevailing scientific worldview is a human



construct, but it's undeniable that there is a widely prevalent, politically potent, and psi-unfriendly received vision about how the world works. Ignoring that worldview, presumably because it's a human invention, seems as pointless as ignoring the man-made skyscraper your car is about to plow into. And second, I suppose it should surprise no one that as *JSE* Editor-in-Chief I see great value in focusing precisely on the contrast between received scientific theories and their corresponding anomalies, both in encouraging the open-mindedness for which the authors of this book strive, and for gaining a deeper understanding of the psychology and sociology of science and philosophy.

But my main regret about this book is that it gives little attention to Anglo–American philosophy apart from James. For example, C. D. Broad dealt in great detail (and considerable sophistication) over several decades with parapsychological research (see, e.g., Broad 1953, 1962, 1967 and my review of Broad 1962 in the previous issue of *JSE*).[2] The same could be said about Henry Sidgwick, C. J. Ducasse, H. H. Price, and arguably even Antony Flew. Perhaps the authors considered none of these figures to be in the same league philosophically as those they considered (if so, I'd disagree). But that surely can't be said about C. S. Peirce, James's colleague and the originator of American Pragmatism, who had at least as much engagement with parapsychological phenomena and research as some of the philosophers covered in this book (for more on Peirce's engagement with the paranormal, see Braude 1998).[3]

One very curious feature of the book is that all 554 of its footnotes are collected together at the end of the book. Presumably someone thought that was a good idea, but I don't see why. Personally, I would have at least liked to see the footnotes separated into groups corresponding to the chapters in which they appeared, or (even better) collected at the end of each chapter.

In any case—and although I doubt anyone can make Schopenhauer's philosophy intelligible[4]—the authors do an admirable job of summarizing complex philosophical positions, not to mention the sometimes equally complicated evolution of those positions. And I commend the authors for digging so sensitively, boldly, and thoroughly into what many would regard as the darkest corners of their subjects' thinking. So although the book is probably targeted for a rather specialized audience (even more so than that of the *JSE*), it's informative and interesting, and can be warmly recommended.

Notes

[1] For example, Schopenhauer bluntly states: "Whoever at the present time doubts the facts of animal magnetism and its clairvoyance should be called not a sceptic but an ignoramus" (Schopenhauer, 2000:229).

[2] In fact, he also wrote about Kant and Swedenborg (in Broad 1953).

[3] Although James claimed that his pragmatic view was that of Peirce, it actually modified Peirce's view in significant ways.

[4] However, Hans Gerding heroically and quite successfully lays out the various strands of his thinking.

Stephen E. Braude

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BOOK REVIEW

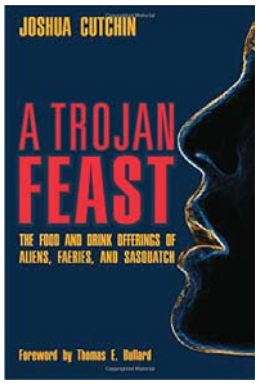
A Trojan Feast: The Food and Drink Offerings of Aliens, Faeries, and Sasquatch by Joshua Cutchin. Anomalist Books, 2015. 270 pp. ISBN 978-1938398353.

The bottom line is that if you find yourself abducted, invited, or otherwise in the company of sentient nonhuman entities, don't ingest any of their items of sustenance that they might offer. Cutchin reveals many tales of people who have reported such encounters and provides details about the different characteristics of the provisions. Chapters delineate breads, liquids, fruits, pills, and on rare occasions meats that are offered for a variety of reasons that range from drugging the victim, preventing them from leaving the altered realm and returning to their real world, to providing cures for current illnesses. Prohibitions aside, those presented with things to eat or drink often are not provided the option of whether or not to partake.

All of the book's research is based on an extensive literature search. Heavily annotated, the book with 186 pages containing script has 417 specific references and an additional 24 pages of bibliography. From a critical perspective it is the farrago of source material that is problematic. The veracity of citations varies widely. There are established credible sources such as Jacques Vallee, Thomas Bullard, Dennis McKenna, and others with known qualities of research. Then there are sources listed with unknown authors and taken from newsletters, tabloids, and the Internet with highly questionable provenance. Global in nature, the reports fluctuate from firsthand accounts of encounters with strange beings to reiteration of popular folklore.

Cutchin does have a point in accepting this wide range of citations. That is the comparison of common factors that permeate the stories and transcend diverse cultures. An interesting chapter explores the possible relationship between the offerings of entities and the Sattvic diet. The concept, he notes, sometimes coincides with ayurvedic philosophy that certain foods have innate characteristics that impact, or are displayed in, human behaviors. The Sattvic group, including sweet fruits, dairy products, honey, most nuts and grains, are believed to be the purest and "associated with balance and order." While these foods are not provided exclusively, they are found in a large number of the reports.

Another segment of his analysis addresses dimethyltryptamine (DMT),



sometimes referred to in popular literature as "The Spirit Molecule." Here Cutchin relies heavily on the works of ethno-anthropologists and medical researchers such as Rick Strassman who have studied the effects of ayahuasca in both laboratory and natural settings. The description pertaining to legalities is only partially correct. While U.S. laws are not specified in the text, it is correct to note that DMT is generally illegal for recreational use in North America. However, that is not true in the Southern Hemisphere. In fact, in Peru, not only is ayahuasca legal, it is officially considered part of the national heritage. Shamans from Ecuador, Colombia, and Brazil all legally incorporate ayahuasca in their ceremonies, and it is an integral part of the rapidly growing religion of Santo Daime. It is suggested that some of the abduction experiences with ingestion have a DMT component that may be responsible, at least in part, for the descriptions of unusual experiences.

Later Cutchin provides his own theoretical basis for these strange experiences. On page 184 he specifically states, "my hypothesis [is] these beings—as well as any craft they are piloting, clothes they are wearing, or food they are offering—are made manifest via DMT." That is a very big leap in logic as it could only apply to incidents in which victims/subjects have ingested some material or had it somehow implanted. The obvious problem comes from the multitude of cases in which no such interaction took place. In addition, there are cases in which physical evidence is acquired thus proving the incident was not a mental aberration or altered state of consciousness. What is attractive is the notion that these unusual encounters are presented in a manner that best fits the ability of the recipient to comprehend at least a modicum of what they are experiencing.

In fairness, Cutchin does admit there are "flaws in my hypothesis" and acknowledges that paradoxes abound. He does consider the problem that in studying phenomena, the pieces never quite fit together. It is the outliers, he notes, that are "both a blessing and a curse." Thus it is endemic that most hypotheses about them are incomplete at best. Any effort to combine such diverse anomalies as faeries, extraterrestrial aliens, and Sasquatch into a single study is fraught with problems. Most researchers who have examined any one of the topics are soon faced with massive complexities. Cutchin has taken a different approach by examining one of the parameters that crosscuts all of the phenomena, and has basis in all living things as well. It is an interesting attempt and at least provides guideposts for others who want to explore the wealth of literature he has uncovered. —**John B. Alexander**

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BOOK REVIEW

Evidence for Psi: Thirteen Empirical Research Reports edited by Damien Broderick and Ben Goertzel. McFarland, 2014. 332 pp. $49.95. ISBN 978-0786478286.

One reason I love parapsychology is the colorful collection of scientists it brings together. I think few fields can boast the shared research efforts of engineers, physicists, anthropologists, psychologists, sociologists, biologists, neuroscientists, philosophers, statisticians, and many other professionals. New, exciting ideas are often born at the crossroads of different fields.

The downside of so many different backgrounds is that each has its own culture and scientific jargon. This is obvious from the various chapters that are brought together in *Evidence for Psi: Thirteen Empirical Research Reports* introduced and edited by Damien Broderick and Ben Goertzel. Many contributors introduce their own words to describe very similar if not the same concepts. The unsuspecting reader is at risk of getting lost in a jungle of concepts and acronyms. In addition, using different words for the same thing, or the same words for different things, stands in the way of fruitful discussions among psi researchers, even more so between psi researchers and scientists in other fields, let alone discussions with skeptics. Although discussions with skeptics may be another story.

Researchers in parapsychology are sadly familiar with the following scenario: When you ask an outspoken skeptic what exactly in the scientific literature on psi causes such displeasure, it becomes clear that their knowledge is rudimentary at best. They justify their lack of investigative effort with the opinion that 'the laws of physics are not compatible with psychic phenomena.'

An example of this type of exchange is included in the book. Wishing to give a balanced view of the state of research on psi, Broderick and Goertzel invited skeptic physicist Sean Carroll to study the evidence and contribute his take on the evidence. He refused on the grounds that "our knowledge of the laws of physics rules [the abilities of purported psychics] out," and he was "not going to waste his time looking into claims to the contrary" (p. 26).

Some skeptics will never be convinced. But the more books become available that lay out the experimental evidence for psi, the harder it will

be to justify ignorance. Solely from this point of view, *Evidence for Psi* is already a welcome addition to what is out there. The book aims to seduce *rational and open-minded readers* with no more than a basic knowledge of statistics to learn more of some of the more important scientific endeavors in parapsychological research. The body of this book is taken up by the Thirteen Empirical Research Reports, of which quite a few are slightly adapted versions of previously published papers, although these were often not readily accessible to the general public. The Editors put these reports in context by means of an introductory chapter and a concluding chapter that look to the future of psi research.

I consider myself to be part of the intended audience. After all, who wouldn't want to be rational and open-minded? On the other hand, I'm also *not* part of the intended audience because I am no blank slate regarding psi. I do research in psi myself, and am familiar with many of the contributors, their research, and their points of view. My coming of age, parapsychologically speaking, was by means of the book *Psychic Discoveries Behind the Iron Curtain* by Sheila Ostrander and Lynn Schroeder, when I was in my late teens. I forgot most of the contents, but I do remember my awe of all these strange and wonderful things, apparently subjected to more or less thorough investigations. It would be great if *Evidence for Psi* would instill this same awe in contemporary young and curious people.

The Introduction to *Evidence for Psi* gives a well-structured overview of types of phenomena, the general experimental approaches to study those phenomena, and how the evidence might be evaluated and interpreted. This Introduction should give any reader at least a reasonably accurate impression of the present state of psi research in the U.S. and Europe. Below I will discuss a few of the contributions that I found especially remarkable for one reason or another.

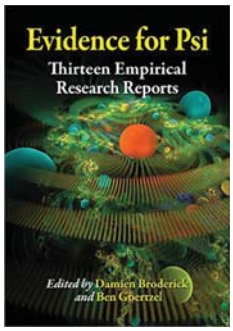
Jessica Utts contributes a slightly updated reprint of her now classic paper on The Significance of Statistics in Mind–Matter Research. It is clearly written and essential reading for anyone who wishes to understand the essence of widely used statistical techniques, and the basics of how to perform a meta-analysis, a technique used to combine the results of similar studies. Meta-analyses allow a more informed evaluation of whatever phenomenon is being studied. All techniques discussed by Utts are standard practice in many scientific fields. To some this subject matter might seem dull, but it is amply illustrated with real-life examples of psi research, contrasting the results to those of a conventional, medical study. After this, the reader should be suitably prepared to learn more about meta-analyses in later chapters.

I enjoyed Julia Mossbridge's chapter, written together with Jessica Utts

and Patrizio Tressoldi, on what she calls Anomalous Anticipatory Activity, AAA for short. AAA is about physiological processes that are associated with future events. These processes could be, for instance, changes in heart rate and skin conductance in anticipation of positive or negative feedback. These physiological processes are being measured while the participant carries out a computer task. In one such an experiment, participants were instructed to choose which of two neutral pictures is the 'correct' one. The participants had no ordinary way of establishing the correct choice; only after they entered their guess would the computer determine randomly which of the two pictures is the target. The results of a meta-analysis of 26 of this and similar studies showed that the anticipatory physiological processes differed significantly for the two different types of feedback (positive vs. negative). This means that, prior to feedback, the participant's physiology already reflected whether or not the choice was correct. What makes Mossbridge's contribution even more engaging, however, is that she lets you witness her scientific work as it progresses. You get to feel her ideas and dilemmas, almost inviting you to join her on her quest. Because she is primarily a neuroscientist, she has easy access to the work and data of her mainstream colleagues in neuroscience. It turned out that her colleagues' data showed possible AAA in mice. This is an exciting finding for two reasons. First, it shows that AAA can happen even if the experimenter is not looking for it; this practically rules out that the experimenter, consciously or subconsciously, somehow "produced" these results. Second, it is one of the relatively rare instances where psi is demonstrated with animals in a controlled laboratory setting. If psi is demonstrably not limited to humans, this has consequences for the theories that fit the existing data, e.g., it means that language processing or a high level of consciousness is not required.

Through Time and Space, The Evidence for Remote Viewing, a contribution by Stephan Schwartz, contains insights on the history of a number of labs that studied remote viewing. Remote viewing entails that people describe what happens at some distant loosely specified location, when sometimes only coordinates of longitude and latitude are given. In Schwartz's terminology, remote viewing is a form of *nonlocal perception*. The results range from interesting to amazing. For me, the added value of this chapter was learning more about the origin and development of the different research labs, the varied academic backgrounds of the investigators, how these different labs inspired and encouraged each other, and how criticisms by skeptics were embraced and used to further improve experiments. Schwartz's fluent writing style made this piece of history read like a psychological thriller. I'm happy that this history has been documented.

Speaking of history, especially the chapter by Bryan Williams, Empirical



Examinations of the Reported Abilities of a Psychic Claimant, made me realize how cumbersome and time-consuming research used to be. Nowadays the computer automates the process of randomizing and presenting stimuli, and registering and compiling responses. The psychic claimant in the title of Williams' chapter is Sean Harribance, whose patience must have been truly mind-blowing. He participated in a seemingly endless stream of experiments, many of which were conducted by the late William Roll. In a so-called ESP(Extra-Sensory-Perception)-task, Harribance was shown 12 opaque envelopes, hiding portraits of six men and six women. Harribance's task was to guess the gender of each. This task was performed a total of 5,890 times! A PK(PsychoKinesis)-task required Harribance to mentally influence six dice to turn face-up with a predetermined target number. Harribance dutifully performed this task almost 2,500 times. Williams describes many details, such as the color and material of the cloth hiding the envelopes, in what way cards were shuffled and by whom, and in what particular way an experiment was adapted to avoid possible cheating through 'sensory leakage.' Any of these details may turn out to be relevant, as long as we don't have a satisfying explanation for psi. Even though this painstaking reporting demonstrates the dedication and honesty of all those involved, the reader would probably have been better served if this chapter had been condensed. For a book aimed at a general public, this amount of detail tends to drown out the interesting results.

A chapter that presents an even bigger challenge to the reader, although of a different kind, is Peter Bancel's, An Analysis of the Global Consciousness Project. The Global Consciousness Project (GCP), clearly described in the preceding chapter by Roger Nelson, starts from the idea that mental activity of humans is globally connected. The GCP shows that global, emotional events are associated with decreased randomness in random number generators (RNGs). In other words, RNGs' output becomes more structured, or coherent. Previous research, e.g., the studies performed by York Dobyns, also described in this book, showed that human conscious intentions are associated with changes in random material processes such as the output of RNGs. This correlation is seen as a type of PK, often called microPK. The GCP is a natural extension of this kind of result. It's microPK gone global.

Bancel analyzes the massive amount of data of the GCP and uses it to answer a number of theoretical questions about possible mechanisms explaining the surprising correlations between 'global mind' and matter. To

answer these questions, Peter Bancel first explains a number of theoretical models that should or shouldn't fit the GCP-data, and consequently looks at how well each model fits. This chapter is very technical. I found it hard to follow the details of the arguments, despite Bancel's very careful and precise style of writing. Luckily the conclusions resulting from this admirable amount of work are not too hard to follow. The most important are that 1) a field model is compatible with the data. In a field model, an effect decreases with distance, just like the intensity of sound waves. This gives an indication of the type of theories that might explain the global consciousness data; 2) an experimenter effect cannot be ruled out. In this case, the experimenter Roger Nelson may theoretically by means of his subjective choices of global events, somehow unconsciously produce the association between the events and RNG output. To safeguard against this subjective part of the process, Bancel recommends that an algorithm should be developed that selects the global events of interest without human input.

All this attention to technical detail may make us lose sight of the broader context: How to evaluate the endeavors and discoveries in psi as part of our general efforts to make sense of the world. We can therefore be grateful for the clearly written contribution of Ted Goertzel and Ben Goertzel, in Skeptical Responses to Psi Research. The authors explore the types of criticisms that have been leveled at parapsychology, and compare them to similar criticisms in other academic fields. This insightful chapter concludes that parapsychologists would best avoid fights with vocal but ill-informed skeptics, and instead keep calm and study psi.

While the contributing authors are a varied bunch, the book is limited to research in the U.S. and Europe. It would have been nice if the editors had at least recognized that a country such as Russia also has a long history of psi research.

The writing style of Broderick and Goertzel reflects an admirable effort to be nuanced and precise, sometimes married with slightly humorous pomp and bombast. This may result in long-winded sentences such as:

> We argue that the total corpus of evidence for psi, while complex and vexing in various regards, contains sufficiently compelling positive evidence to: (1) cause a rational, open-minded observer to conclude that it is reasonably likely that one or more real physical processes underlie observed psi phenomena; (2) merit focus of significant resources of further scientific research into psi.

This is hard to read. Why not something like: "The evidence lets us conclude that psi is real, and we need more money to find out how it works."

Preferences for writing styles differ, of course, but if you deliver a

controversial message, it seems best to leave out the baroque. Luckily, many of the contributors write clearly and concisely, without compromising precision. Even though content should trump presentation, an uncluttered writing style can only help to keep the readers' focus on the message. And it should be obvious that this message deserves to be heard loud and clear and distributed widely.

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