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## EDITORIAL

It's not often that I get to feel like a spokesperson for empirical conservatism. But that happened recently when I was invited to give a talk at the 50th Annual Conference on Anomalous Phenomena sponsored by the International Fortean Organization (INFO). The occasion provided several healthy illustrations about what I suppose we can call boggle relativity. The conference was stimulating, challenging, and professionally run, and I was happy to meet quite a few very smart and pleasant attendees—among them, the SSE's treasurer John Reed, MD. In fact, I very much look forward to attending future INFO conferences.

But one thing that struck me especially was the difference I frequently noticed between the phenomena I was (more or less) comfortable incorporating into my worldview and the phenomena others there were equally prepared to accept. Often enough, that difference felt to me like a gaping chasm. For example, I was chatting with one clearly bright and well-read man about the evidence for remote viewing. Initially, we seemed to be very much on the same page. We apparently agreed on what the evidence was, we agreed that the phenomenon was genuine, and initially at least I thought we also agreed on the implications of the data and what they suggested about the place of human beings in nature. But then, in what struck me as a dazzling and swift series of unfounded assumptions and apparently unjustified inferential leaps, my interlocutor started asserting—with the same degree of assurance he'd lavished on the experimental evidence for remote viewing—that remote viewers were having out-of-body experiences in which they traveled to distant parts of the universe and communicated with rocks and other apparently inert objects. And he interpreted OBEs literally, insisting that OBE-ers were in fact leaving their bodies—rather than, say, having imagery-rich clairvoyant episodes while remaining thoroughly embodied.

Perhaps some *JSE* readers will be more sympathetic to these claims than I was. Nevertheless, they were moves I was not ready to make. Still, I had to be careful not to fall instinctively into the sort of knee-jerk skepticism I frequently encounter and about which I've often complained in print. I had to remember that at one time I was equally ready to dismiss—no doubt with a disdainful flourish—any sympathetic claim regarding the evidence of parapsychology. I also had to recall that, even after coming to terms philosophically with the experimental evidence in parapsychology, I was still contemptuous of the non-experimental evidence—that is, until I studied that evidence carefully and eventually documented my conceptual evolution (Braude, 1997). In fact, I couldn't help but remember that, much more recently, I had to re-evaluate my dismissive attitude toward astrology in the face of my wife's astonishing virtuosity (see Braude, 2007: chap. 8).

Don't get me wrong. I still *have* my dismissive attitude toward what I considered to be the extreme positions of my interlocutor. Whether I like it or not,

that attitude is a fact about my current intellectual and emotional life, and I can't simply make it disappear with a cunning and quick bit of ratiocination. So for now at any rate, I'd be surprised (to say the least) if I later came to believe that we can communicate with alien (or terrestrial) sticks and stones. But I felt and continue to feel that it would have been inappropriate and unwarranted for me to have expressed my attitude and to have attached any great importance to it. I felt that if I'd done that, I'd have been every bit as contemptible as the glib and condescending skeptics whose attacks on parapsychology I've often tried to expose. After all, I couldn't pretend that my skepticism was rooted in a command of the relevant material. In fact, I hadn't even read the works to which my interlocutor was referring. So although I realized I wasn't a total ignoramus about the topics under discussion and was arguably entitled to at least some degree of skepticism, I knew also that I probably hadn't identified and thought through all the relevant issues. As far as I knew at that moment, my dismissive attitude was grounded mostly in my smugness about what I thought I knew. I also knew that if the history of science has taught us anything, it's shown that humankind is a very poor judge of the empirically possible. So the only thing I felt I could honestly and appropriately do at the time was to confess both my doubts and my ignorance, and not pretend that my judgments on the matter were delivered from a privileged post atop Mt. Olympus.

What continues to disturb me, though, is how easily I lapsed into a kind of superciliousness I've worked hard to combat both in myself and others. Maybe it's one of those demons in life that can never be fully vanquished and which will forever require a certain amount of vigilance. So it occurs to me that perhaps the time is right to remind *JSE* readers and others working in the area of anomalistics or frontier science of the need for humility and collegiality.

The *JSE* exists for the purpose of examining carefully empirical and theoretical claims about which many people, including regular readers of the *Journal*, have very strong opinions, both pro and con. The community of *JSE* subscribers is hardly uniform. It's a collection of individuals from different educational and scientific backgrounds, with different interests and assumptions, and of course with different boggle thresholds. I know that some readers of the *Journal* discount the interests of others, and I consider that state of affairs unfortunate. I'd like to think that *JSE* authors and readers have all been somewhat chastened about reacting quickly and negatively to empirical claims that strike them as beyond the pale. I would imagine that most of them have been stung at some time by others' negative and seemingly ignorant or hasty reactions to their own beliefs, and probably many have experienced changes in their own boggle thresholds similar to those I've mentioned from my own life.

I share the view of C. S. Peirce that of all earthly creatures we seem to have a distinctive knack for understanding the world around us. But that knack is merely what allows us to make scientific and intellectual progress and to frame increasingly successful theoretical frameworks. However, our faculty of understanding at no time provides a guarantee that we're making steady and unimpeded scientific

progress and have managed to avoid getting off track—much less that we've arrived at a kind of timeless truth beyond mere warranted assertibility.

So when I now reflect back on the INFO conference, what stands out for me is how refreshingly tolerant and warm the participants were. I know that open-mindedness comes in degrees and that it shades gradually and eventually imperceptibly into credulity. But that's no different from the way cognitive caution or skepticism shades into intellectual rigidity and close-mindedness. Despite the differences between my beliefs or theoretical orientation and those of some other conference attendees, in important ways I felt I was among kindred spirits. What I particularly admired about those I met was their respect for data, their recognition that data are always subject to varying interpretations, and their willingness to question not only received opinions but their own opinions as well.

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Alert readers will have noticed some changes in *JSE*'s stable of Associate Editors. York Dobyns has returned to his previous *JSE* role as trusty manuscript reviewer. So although York no longer needs to shoulder the responsibilities of Associate Editor, he's hardly off the hook as far as editorial assignments are concerned (demonstrating once again that no good deed goes unpunished). In any case, I extend to him my warmest and sincerest thanks for his years of conscientious and dedicated service in his former role. In the meantime, my former post as Associate Editor will now be filled by another philosopher, Michael Sudduth, who coincidentally makes his debut in this issue—and in my opinion, a very impressive one at that—as *JSE* author. I'm also pleased to announce two more distinguished additions to my team of Associate Editors, the noted psychologists Daryl Bem and Etzel Cardeña. Both are current members of the Parapsychological Association Board of Directors (Etzel is PA President). Daryl's sophisticated and comprehensive grasp of the evidence for psi phenomena will clearly be a valuable asset in his role as Associate Editor. And Etzel's expertise not only in parapsychology, but in dissociation and hypnosis, will be crucial as the *JSE* expands its coverage of anomalies of consciousness.

And since I neglected to point this out in my debut editorial last issue, let me emphasize how fortunate I am to have the loyal and scrupulous assistance of my entire team of Associate Editors. I rely heavily on their expertise in areas of research outside of my own, and I count on their experience and wisdom in evaluating submissions to the *Journal* and in maintaining the *JSE*'s scholarly integrity. There is absolutely no way I could do my job without their help. Similarly, I'm indebted to the *Journal*'s diligent and tenacious book review editor, David Moncrief, who somehow manages to keep abreast of things worth reviewing, while at the same time procuring review copies from (often inscrutably unresponsive) publishers and managing to extract thoughtful reviews from typically overworked volunteers.

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## RESEARCH

### Appraisal of Shawn Carlson's Renowned Astrology Tests

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**Abstract**—Shawn Carlson's 1985 study, published in *Nature*, which ended with a devastating verdict of astrology, is scrutinized. The design of Carlson's study violated the demands of fairness and its mode of analysis ignored common norms of statistics. The study's piecemeal analysis of sub-samples avoided testing the totals for astrological effects, as did the neglect of test power, effect size, and sample size. Nevertheless, a correct reanalysis of Carlson's two astrological tests reveals that astrologers matched profiles of the California Personality Inventory to natal charts better than expected by chance with marginal significance (three-way forced choice,  $p = .054$ ), and that a positive result was replicable by a different assessment method (10-point rating,  $p = .04$ ). The results are regarded as insufficient to deem astrology as empirically verified, but they are sufficient to regard Carlson's negative verdict on astrology as untenable.

*Keywords:* astrology—anomalous—medical flaws—fairness

#### Introduction and Overview

Among experimental studies of skeptical researchers who aim to test astrological claims, Shawn Carlson's (1985) study, published in *Nature*, the prominent science journal, ended with a devastating verdict on astrology. His study ranked first in subsequent discussions on astrological claims. Today, his paper is referred to on more than 400 Internet pages (Google search), more often than any other paper of its kind. Does Carlson's paper deserve first rank?<sup>1</sup>

Carlson tested two astrological predictions and conducted one study to test the usefulness of his main assessment tool, the California Personality Inventory (CPI).<sup>2</sup> An overview is given in Table 1. Carlson's report is somewhat disorderly; a new numbering of his three studies will be used henceforth.

#### Study 1

A sample of fewer than 28 astrologers<sup>3</sup> matched 116 birth charts with CPI profiles. CPI profiles are graphs showing 18 scale values, connected by a contour line, on the Y-axis (an interval scale). They represent 18 personality dimensions like dominance, sociability, self acceptance, tolerance, etc., which are aligned on

TABLE 1  
Overview of Carlson's Three Studies

Revised division:	Study 1	Study 2	Study 3
Carlson's division	Part 2	Part 1, 2nd section	Part 1, 1st section
Aim of study	Astrological	Astrological	Methodological
Test participants	Astrologers	Students et al.	Students et al.
Invited	N = 90	N = 128	N = ?
Participating	N = <28 <sup>a</sup> (116) <sup>b</sup>	N = 83	N = 56
Control participants	—	Students et al.	Students et al.
Invited		N = 128	N = ?
Participating		N = 94	N = 50
What was to be matched	CPI profiles <sup>c</sup> with birth charts	Personality descriptions <sup>d</sup> with Self	CPI profiles <sup>c</sup> with Self

Note: CPI = California Personality Inventory.

<sup>a</sup> "Some" astrologers initially willing to cooperate refused to continue after checking the task. The number of actually participating astrologers was not given.

<sup>b</sup> In parentheses: Number of CPI profiles that astrologers matched with birth charts.

<sup>c</sup> CPI profiles of students condensing the results of item-wise judgments by 18 scale values.

<sup>d</sup> Personality descriptions obtained from astrologers' interpretations of the students' birth charts.

the X-axis, a categorical scale. The scale values are based on a total of 480 CPI items. The birth charts were divided among the astrologers and three CPI profiles were provided for each chart. One profile was for the chart's owner and the other two were randomly added. The astrologers were asked to match the chart to the CPI profiles and to make first, second, and third choices. They were also asked to rate, on a 10-point scale, the fit of each CPI profile with the birth chart. One astrologer was given a larger number of charts to match them with CPI profiles and to rate their fit. The astrologers had "some background in psychology" and "were familiar with the CPI" (Carlson, 1985: 420). A copy of the CPI interpretation manual (15 pages) was made available in case additional information was needed.

### Study 2

Eighty-three volunteer participants, undergraduate or graduate students and others, were each given three different descriptions of personality, which were written by the astrologers who interpreted the persons' birth charts. One of the three descriptions was based on the participants' own birth charts and the other two were added at random from the other chart descriptions in the pool. Each participant was asked how well the descriptions pertained to themselves and to tell which was the first, second, and third best fit. They were also asked to rate the fit of each description on a 10-point scale. The descriptions had been worked out by the astrologers of Study 1. The astrologers' interpretations had been limited to 1000 words and covered the categories of "personality" (300 words), "relationships" (150 words), "career" (150 words), "education" (100 words), and

“current situation” (150 words).<sup>4</sup> Clues related to age, place, time, and astrological concepts were to be avoided. The same material was given to 94 control subjects, matched to the test group with regard to sex, age (“at least three years” difference), and birth date (same sun sign category). Controls were also asked to rank the three personality descriptions with regard to fit to themselves and to rate the fit on a 10-point scale. For the controls, none of the three descriptions was based on their own birth chart.

### *Study 3*

A sample of 56 undergraduate and graduate students and others, who in an earlier session completed the self-administered CPI, were each given three CPI profiles. One of the three profiles was obtained from their own earlier CPI responses, and the other two were randomly selected from the pool of  $56 - 1 = 55$  CPI profiles. The participants’ task was to choose which profile described their own personality. Unlike participants of Studies 1 and 2, the participants of Study 3 did not also rate the fit on a 10-point scale.

### **Carlson’s Results**

Study 1 (astrological aim): According to Carlson, the astrologers ( $N < 28$ ) failed to select, among three CPI profiles, the one that had been obtained from the natal chart owner. He alleged that their judgments on 10-point scales regarding fits of the three profiles to the birth chart did not reveal astrological abilities either. Study 2 (astrological aim): The students ( $N = 83$ ) were not able to select, among the three personality descriptions, the one that was based on their own birth chart, i.e., the astrologer’s authentic interpretation was not correctly identified. The selections of personality descriptions by matched controls ( $N = 94$ ) who were presented the same material, of which nothing was related to their own birth charts, were even somewhat better than the test group’s selections. Study 3 (methodological aim): The students ( $N = 56$ ) who were presented three CPI profiles, one of them based on their CPI responses, were not able to pick the authentic profile. This was concluded because the controls’ ( $N = 50$ ) selections of the same CPI profiles, none of them based on their own CPI responses, did not differ from the test group’s selections.

### **Criticism**

Criticism is raised regarding Carlson’s data generation (1), his method of data analysis (2), and his neglect of adequate test power (required number of independent observations) (3).

#### *1. Carlson’s Procedure of Data Generation*

Carlson’s personality data were generated by the California Personality Inventory (CPI), occasionally also called the California Psychological Inventory.

Eysenck (1986: 7) objected to Carlson's use of this measurement instrument whose scales he considers "arbitrary and subjective" because they were not chosen by ordinary psychometric techniques. The interpretation of such scales needs, if used at all, "a competent psychologist who has become familiar with this particular device" (quoted from the CPI manual by Hamilton, 1986: 11). None of the participants were psychologists; the experimenter was a graduating physicist. In addition, scale interpretations must take into account the gender of the tested persons, which Carlson did not consider (Eysenck, 1986: 8; Hamilton, 1986: 10). McGrew and McFall (1990: 76) objected, in addition, that "standard psychological tests, like the CPI, may not include the types of information that astrologers require to complete a matching task successfully."

Carlson's test participants were asked to choose one out of three units of personality information (birth chart interpretation in Study 2 or CPI profile in Studies 1 and 3, respectively). A three-choice format, however, is less suited for discrimination than a two-choice format (pair comparison). The advantage of pair comparison is that the complexity of the subject's task is minimized and the precision of the results is increased. Scientists testing a null hypothesis, which they prefer to come true a priori, should provide, following Karl Popper's demand, a fair chance for its refutation. Apparently, Carlson doubted the validity of astrological claims as he developed the protocol of his study. Hence he should have used pair comparison, the fairest existing test format on which Thurstone's "law of comparative judgment" has been based (Thurstone, 1927) and whose methodical aptness is considered as unparalleled in psychophysical research (Gridgeman, 2006). The pair comparison format had been used earlier in another much quoted study by Vernon Clark (1961) that Carlson disregarded.<sup>5</sup>

Another procedural weakness is Carlson's neglect to provide sufficient dissimilarity between the three personality descriptions or CPI profiles, respectively. Supplementing, for multiple choice, the authentic object with two other objects, chosen at random, will generate, by chance, sets of similar objects which are hard to discriminate in the first place. Astrologer Teresa Hamilton, who was willing to cooperate, preferred to resign after having been given three CPI profiles: "I was given some of these charts to match myself, and noticed immediately that the three profiles were often quite similar" (Hamilton, 1986: 12).

An additional flaw in Carlson's protocol is that he did not sufficiently consider the astrologers' methodological suggestions. Astrologer Hamilton, to whom Carlson referred as an advisor giving "valuable suggestions", complained later that "Carlson followed none of my suggestions. I was never satisfied that the experiment was a fair test of astrology" (Hamilton, 1986: 9). Carlson's claim that he followed the astrologers' methodical recommendations is hardly believable since he does not give an account of Hamilton's—and other astrologers'—objections. It is Carlson's misleading account of the role that astrologers had played in his project which earns criticism.

## 2. Carlson's Data Analysis

Carlson's analysis of his three-choice data was also flawed. He tended to analyze the data in a piecemeal fashion, i.e., first, second, and third choice data were treated separately, and total effects were disregarded.

Given the three-choice data, Carlson should have analyzed them, at least, by combining first and second choice frequencies. He himself had intended, "before the data had been analyzed," to combine them: "We had decided to test to see if the astrologers could select the correct CPI profile as either their first or second choice at a higher than expected rate . . ." (p. 425). He ignored his own protocol without giving reasons.

Correct analyses of Carlson's data are required:

*Study 1 data: astrologers matching CPI profiles with natal charts.* In Study 1, the astrologers matched the subjects' natal charts ( $N = 116$ ) with CPI profiles. They chose the one that they thought fit best and another one that fit second best from the three supplied profiles. Carlson's Table 2 (Carlson, 1985: 423) provides the following frequencies (Table 2); proportions of authentic choices are also given.

Two charts received a first choice only. The small difference (114 vs. 116) may be neglected. The present analysis is done as if 116 second choices had been made; the number of second choice hits might only have increased (not decreased) if the missing choices had been made.

By combining first and second choice hits, as initially intended by Carlson, we find that 86 hits occurred versus the expected  $(116)(2/3) = 77.3$  hits. The binomial Z-ratio for this result is 1.61,  $p = .054$ . Thus, the astrologers chose the correct CPI profile, either as first or second choice, more frequently than expected by chance, at a marginally significant level.

Comment: Carlson does not explain the statistical procedure of his analysis. He uses *standard deviation* as a term to denote both, standard deviations of original distributions and of normalized or Z-distributions, the latter with  $M = 0$  and  $SD = 1$ . Readers will be confused by an uncommented use of two different word meanings. For some analyses Carlson seems to calculate confidence intervals of proportions, which differ, however, from the confidence intervals obtained by ordinary procedures. A standard procedure for calculating the confidence interval of proportions dates back to Wilson (1927), of which an account can be found in Newcombe (1998), and in Lowry's VassarStats online (Lowry, 2008).

TABLE 2  
Data Obtained from Carlson's Study 1

	Total	Authentic	Proportion authentic
1st choice	116	40	.345
2nd choice	114	46	.404
3rd choice	114	28	.246
1st + 2nd choice	116	86	.741

Using Lowry's procedure, we find that for the expected first + second choice hit proportion ( $77/116 = .663$ ) the lower and upper limits of .95 confidence are .57 and .75. The observed proportion  $86/116 = .74$  is near the upper confidence limit (.747) and thus marginally significant. Carlson dealt with the proportion of errors or misses (third choices) instead of first and second choices. The proportion of errors is  $28/116 = .240$ ; the limits of .95 confidence for the expected proportion ( $39/116 = .336$ ) are .253 and .431; the observed proportion (.240) is thus lower than the lower confidence limit (.253) and is therefore significant.

However, the distribution of hits is binomial, not normal; the significance of hits should be obtained from the binomial test (see Lowry, 2008), as shown above. It turns out that in Carlson's Study 1 the binomial Z of hits = 1.61 is significant ( $p = .05$ ). Carlson incorrectly attributed his Study 1 result to chance, arguing, "Since the rate at which the astrologers chose the correct CPI as their third place choice was consistent with chance, we conclude that the astrologers were unable to chose [sic] the correct CPI as their first or second choices at a significant level" (Carlson, 1985: 425).<sup>6</sup>

*An independent analysis based on the astrologers' rating data.* In addition to ranking each birth chart into first, second, and third fit categories for their CPI cases, each astrologer "also rated each CPI on a 1–10 scale (10 being highest) as to how closely its description of the subject's personality matched the personality description derived from the natal chart" (Carlson, 1985: 420). Again, Carlson analyzed these ratings piecemeal for each of the three choice categories separately, and found his result "consistent with the scientific prediction of zero slope" (Carlson, 1985: 424). Yet the ratings should have been analyzed together, across choice categories, because they had been made independently of the three ranking choices.<sup>7</sup> Carlson's data, i.e., plot readings from his Figures 3 and 5, are listed in Table 3.<sup>8</sup>

TABLE 3

Frequencies of Astrologers' Ratings 1–10 for 1st, 2nd, and 3rd Choice CPI Profiles: Hits (Authentic Profiles) and Totals (= Authentic Plus Other CPI), Sums for All Choices, and Percentages of Hits

Rating	1st choice		2nd choice		3rd choice		All choices		
	Carlson Figure 3		Carlson Figure 5		Carlson Figure 5		Hits	Total	%
	Hits	Total	Hits	Total	Hits	Total			
1	0	0	0	0	3	18	3	18	16.6
2	0	0	3	6	7	30	10	36	27.7
3	0	1	2	11	7	27	9	39	23.0
4	2	5	4	13	1	9	7	27	25.9
5	3	7	9	23	4	12	16	42	38.1
6	5	11	12	26	0	1	17	38	44.7
7	7	22	5	14	1	1	13	37	35.1
8	16	43	3	7	0	0	19	50	38.0
9	4	14	0	1	0	0	4	15	26.7
10	1	4	0	0	0	0	1	4	25.0
Sum	38	107	38	101	23	98	99	306	

The percentages from the last column are plotted in Figure 1. A tau-correlation was obtained across  $N = 306$  CPI profiles between the rated fit values (variable  $X$ , 1, 2, 3, . . . , 10) and the actual hit values (variable  $Y$ , 1, 0). For example, from 18 ratings of rank 1, 3 were associated with choice 1 (correct choice) and 15 with choice -1 (incorrect choice); from 36 ratings of rank 2, 10 were associated with choice 1 (correct choice) and 26 with -1 (incorrect choice), etc. The nonparametric correlation across 306 paired rating values (1-10) and correctness values (1 and -1) was  $\tau = 0.088$ ,  $Z = 1.78$ ,  $p = .037$  (one-tailed),  $ES = .10$ . The correlation is significant. This result gives reason to take into account the probability that the astrologers were able, to some extent, to successfully match birth charts with CPI profiles.<sup>9</sup> The 10-point rating task (free ratings with allowance for ties) and the three-place matching task (forced choice without ties) are methodically different approaches for testing astrological effects. As such, they are replications, despite changing methods of assessment.

*Study 2 data: subjects choosing their own personality descriptions.* In Study 2, a test group of subjects ( $N = 83$ ) tried to choose from three personality descriptions the description that the astrologers had worked out, in Study 1, from their birth charts. They selected from three profiles the one that fitted best and the one that fitted second. The control group ( $N = 94$ ) was asked to match the same personality descriptions with themselves while none characterized their own personality. Carlson analyzed this data (Table 4) piecemeal, as always,

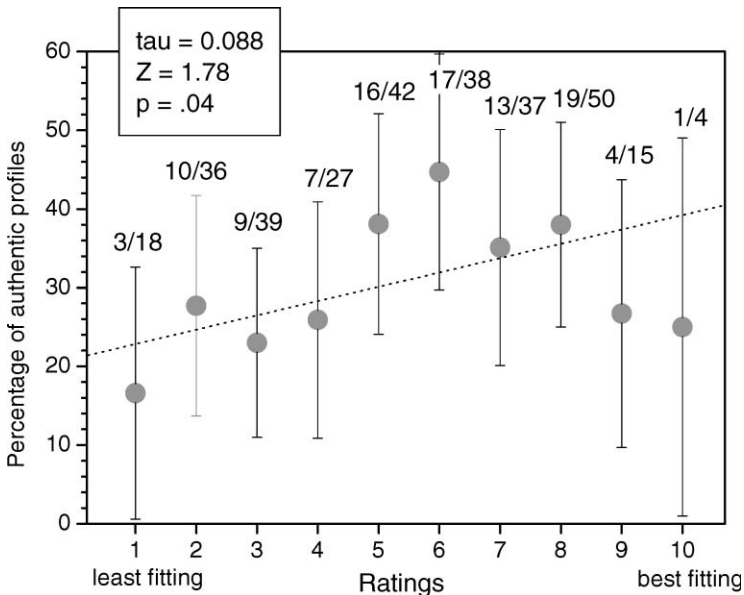


Fig. 1. Percentage of authentic profiles for 10 points of a rating scale obtained from astrologers who rated three CPI profiles as to how well each matched the natal chart. Limits of 95% confidence. Expected by astrologers, a significant positive correlation; by skeptics, no correlation.

TABLE 4  
 Numbers of Authentic Personality Descriptions Placed as Best (1), Less Good (2),  
 and Least Good (3) Fit; Data Obtained from Carlson's Study 2

	N	1st	2nd	3rd	1st + 2nd	Proportion	
						1st + 2nd	3rd
Test group	83	28	33	22	61	.73	.27
Expected					55	.67	.33
Control group	94	42	34	18	76	.81	.19
Expected					63	.67	.33

although first and second choice frequencies should have been combined. One straightforward method of analysis is to compare the difference of correct choice proportions (first + second choice) between the test and control subjects, as follows, in Table 4.

Surprisingly, the direction of the difference is contrary to expectation, yet not significant:  $Z = -1.17$ ,  $p = .24$ , two-tailed.

Comment: The result of Study 2 is odd. As stated previously, astrologers were successful, to some extent, when they matched CPI profiles with natal charts. Before matching the subjects' profiles, they had interpreted their natal charts (Study 1). In Study 2, the chart owners were asked to choose their authentic descriptions. It is reasonable to expect good quality chart interpretations from astrologers who have successfully matched the same charts with the correct CPI profiles. At least one should not expect the astrologers to produce misleading chart interpretations that would make the students tend to avoid (!) the astrologers' interpretations of their own charts.

Furthermore, personality descriptions with full text are easier to understand than a list of 18 verbal tags with scale values. But, according to Carlson's classification, the test group tended to avoid (!) the astrologers' correct interpretations and instead chose interpretations of the two randomly selected charts so that the control group was even better at this task than the test group, as if they, the controls, had been the genuine subjects.

The result of Study 2 might be due to statistical "chance fluctuation", which is Carlson's claim. A suspicion has been brought forward that Carlson might have mixed up test and control group data, perhaps inadvertently (Joseph Vidmar, personal communication), but this is unverifiable speculation.

Carlson's own analysis of Study 2 data is unacceptable. Instead of calculating the significance of the difference of proportions (first + second choices combined) between test and control groups, he calculated the significance of the deviation of the proportions from expectancy, separately for first, second, and third choices and separately for test and control groups. His procedure ignores the logic of control group designs: Test group frequencies must be compared with control group frequencies. The actual expectancy of the null hypothesis is "no difference between test and control data".

*Study 3 data: subjects picking their own CPI profiles.* The goal of Study 3 was to find out whether participants who completed the CPI questionnaire (480 items) and who were later presented 18 scale points (profiles) based on their responses to the items, can successfully pick their own profiles from three profiles, two of which, generated by other participants, had randomly been added.

Frequencies and proportions of Study 3 are displayed in Table 5.

The difference between test group and control group proportions has the expected direction, but it is not significant ( $Z = 0.589$ ,  $p = .28$ ).

Comment: By our reanalysis, as shown above, it was found that in Study 1 the astrologers matched CPI profiles to natal charts significantly better than chance. The seeming contradiction between success of Study 1 (astrologers matched CPI profiles to birth charts) and failure of Study 3 (students matched CPI profiles to Self) may be due to unequal amounts of effort which astrologers and students exerted on their tasks. Astrologers were aware of the importance of their test participation; their world views were put on stake. They might have spent much effort on those three CPI profiles of which one belonged to the chart owner while the students might have been less eager to obtain correct results by their matchings and ratings. They might also have been more liable than astrologers to response bias such as social desirability since they had to make judgments about themselves.

### 3. Test Power of Carlson's Studies

The small number of participants in Carlson's astrological studies is another weakness. For comparison, consider the renowned Gauquelin planetary effects, which showed effect sizes of merely .03 to .07 (Gauquelin, 1988).<sup>10</sup> The effects were generally significant only due to the large sample sizes which amounted to  $N = 500$  to 1000 or more birth data. Even though the effects were small, they nevertheless appeared real because they were statistically very significant. Otherwise, skeptical organizations such as the Comité Para, CSICOP, and CFEPP (Comité Français pour l'Étude des Phénomènes Paranormaux) (Ertel & Irving, 1996), and individuals such as Geoffrey Dean, a strong-minded critic of astrology (Dean, 2000),<sup>11</sup> and H. J. Eysenck, the cautious "metrologist" (Eysenck, 1988),<sup>12</sup> would not have cared about them. Astrological claims should be regarded as being refutable only if the number of independent observations is large enough to reveal

TABLE 5  
Numbers of Authentic CPI Profiles Placed as Best (1), Less Good (2),  
and Least Good (3) Fit to Self; Data Obtained from Carlson's Study 3

	N	1st	2nd	3rd	1st + 2nd	Proportion	
						1st + 2nd	3rd
Test group	56	25	16	15	41	.73	.27
Control group	50	21	13	16	34	.68	.32

small effects, if such effects exist. The Gauquelin results should be taken as an example for significant astrological correlations.

This is not to say that astrological effects would never exceed the low Gauquelin effect level; our result obtained from Carlson's Study 1 data do not suggest such restriction. The effect size of his result (matching) with  $p = .054$  is  $ES = .15$ ; the effect size of the complementary result (rating) with  $p = .037$  is  $ES = .10$ .<sup>13</sup>

### Final Conclusion

The design of Carlson's study was unfair. It was flawed because effect size and test power were not considered. The number of recruited subjects and astrologers was insufficient. Nevertheless, one of Carlson's two astrological studies (Study 1) showed replicable significant results by two methods of assessment.

Studies 2 and 3, for which volunteer students were recruited, seem to have suffered from the participants' lack of effort. Since half of their data (ratings) were so poor that they could not even be analyzed, there is reason to doubt that the analyzable other half of the data has been carefully completed.

In view of at least two significant test results (in Study 1) Carlson's conclusion is not justified: "We are now in a position to argue a surprisingly strong case against natal astrology as practiced by reputable astrologers" (Carlson, 1985: 425). Such conclusion could not be drawn even if Study 1 had shown an *insignificant* result. Not being able to reject a null hypothesis does not justify the claim that the alternate hypothesis is wrong.

As a personal remark: My impression is that research on extraordinary claims, conducted by prominent members of skeptical organizations, when they do such research at all, is likely to suffer from bias and fact-distorting procedures. I noted this repeatedly when I scrutinized studies done under the responsibility of members of the US, Belgian, Dutch, and French skeptical Committees (Ertel, 1995, 1998/1999, 2000, 2005; Ertel & Irving, 1996, 1997).

This is not to say that studies with astrology-favorable results would have an a priori chance to survive my scrutiny any better (Ertel, 1996, 1997, 1998; Ertel & Dean, 1996). Reports about negative results are numerous (Gauquelin, 1955, 1988; Kelly, 1997; Müller & Ertel, 1992; McGrew & McFall, 1990). Confirmations of classical astrological predictions are rare exceptions (Timm & Köberl, 1986; Smit, 1997; Ertel, 2004). Moreover, they do not verify astrological beliefs.<sup>14</sup> Correct astrological diagnoses, if they occur, might be due, e.g., to paranormal intuitions of psi-gifted astrologers (Ertel, 2004). Parapsychological phenomena are likely to eventually become reconcilable with the growing body of scientific knowledge (Radin, 2006); astrology is far from having the same chance. Yet disparaging attitudes against astrology are hardly recommendable as they are articulated in a paper titled "Top Scientists Must Fight Astrology or All of Us Will Face the Consequences" (Crease, 2006). H. J. Eysenck's occasional involvement in astrological research issues may be regarded as a model (Eysenck, 1990; Eysenck & Nias, 1982).

## Notes

- <sup>1</sup> Shawn Carlson, PhD in physics, is a science writer and chairman of the Society for Amateur Scientists (SAS) which he founded. He is also active as a member of the Committee of the Society of Claims of the Paranormal (CSICOP), and as a supporter of the James Randi Education Foundation (JREF).
- <sup>2</sup> The proper name of this test is California Psychological Inventory which is sometimes loosely replaced, as by Carlson, with California Personality Inventory which term will be used here henceforth.
- <sup>3</sup> See Note a of Table 1.
- <sup>4</sup> Unpublished information taken from Carlson's Guidelines for Preparation of Interpretation.
- <sup>5</sup> Vernon Clark's result was astrology-consistent; its evaluation, however, remained controversial (Eysenck, 1984).
- <sup>6</sup> For his study, Carlson diverged from statistical conventions by defining significance: "2.5 standard deviation [actually Z] [was] the level we had chosen to call 'significant' . . ." (Carlson, 1985: 423). The probability of  $Z = 2.5$  is  $p = .006$ , the conventional *first* degree of significance is  $Z = 1.64$ ,  $p = .05$ .
- <sup>7</sup> The astrologers had been instructed to do the ratings prior to making the first and second selections: "Compare the chart to each CPI profile. Rate each profile on a scale of 1–10 as to how well each matched the chart overall. Note this on the evaluation sheet. Then select which profile you feel is (1) most likely, (2) second most likely to be the one" (from Carlson's unpublished information sheet).
- <sup>8</sup> The numbers based on Carlson's plots of ratings (from his Figures 3 and 5, pp. 421 and 424) differ to a certain extent from his numbers of matchings (in his Table 2, p. 423). His tabulated totals for first, second, and third choice are 116, 114, and 114; his corresponding plotted totals are 107, 101, and 98. His tabulated "correct CPI chosen" are 40, 46, and 28; his corresponding plotted totals are 38, 38, and 23. Carlson does not explain the difference. It seems that some astrologers who completed the matchings did not complete the ratings.
- <sup>9</sup> The rather low hit percentages within the two "best fitting" categories might be due to extreme response sets of some astrologers. Because the original data are not available, response sets of the astrologers cannot be taken into account here.
- <sup>10</sup> Gauquelin's studies yielded results, of which the "Mars effect" has widely been discussed in public. By numerous replications, Gauquelin obtained significant correlations between birth frequencies of eminent professionals (writers, musicians, painters, physicians, scientists, etc.) and the positions of Mars, Jupiter, Saturn, and Moon at their birth hours. "Effect-sensitive" planetary positions were rise and culmination. Gauquelin's results were confirmed by independently scaling the eminence of professionals (Ertel, 1988) as well as by independent data collection (Müller & Ertel, 1994).
- <sup>11</sup> "Gauquelin has covered every possible non-astrological source of error so thoroughly that his results seem beyond doubt. . . . [His results] support some of the most fundamental astrological concepts of all . . ." (Dean & Mather, 1977: 394).
- <sup>12</sup> If the replication should be successful, Gauquelin would rank with the great scientists of the past who changed our conceptions of the Universe; if they should fail, he will be remembered as a brave and honest scientist who was misled by appearance" (Eysenck, 1988: 10).
- <sup>13</sup>  $ES = Z/\sqrt{N}$ ,  $N$  = number of independent observations.

<sup>14</sup> Claims of classical astrology (or astrologies) which lack replicable verifications must be sharply distinguished from Gauquelin claims which have been largely verified. The term neo-astrology for Gauquelin's field of studies came into use, which points at this distinction.

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## RESEARCH

### A Field-Theoretic View of Consciousness: Reply to Critics

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**Abstract**—This paper replies to a critique (Fales & Markovsky, 1997) of a study reporting that group practice of the Transcendental Meditation program had a measurable effect on objective measures of the quality of life in Israel and the war in Lebanon (Orme-Johnson et al., 1988). The critics proposed various cultural/political events as alternative explanations for the results. These events could not explain the results, as indicated by (1) simple inspection of the published data; (2) statistical analyses controlling for these events; (3) analyses of reduced data sets that completely eliminated the days of the events from the analyses; and (4) analyses of six random samples of 50% of the data. Although some of the cultural/political events suggested did have a significant effect on a composite index of crime, traffic accidents, fires, war intensity, stock market, and national mood, the effects of these events were independent of the effect of the meditators and could not explain it. We argue that Maharishi's theory of collective consciousness provides a unifying framework that explains these results through a logical structure of clearly defined, operationalized terms grounded in physiological and behavioral research, which makes specific quantifiable and socially important predictions that have been extensively replicated.

*Keywords:* Transcendental Meditation—collective consciousness—Maharishi Effect—social indicators—conflict resolution—global consciousness—distant intentionality—consciousness

#### Introduction

Evidence has accumulated from a number of sources over the past three decades supporting a field-theoretic view of consciousness (e.g., Dillbeck, 1990; Hagelin et al., 1999; Jahn et al., 1997; Jonas & Crawford, 2003; Nelson et al., 2002; Orme-Johnson et al., 1988, 1990, 2003; Radin, 2006; Schlitz et al., 2004; Schlitz & Braud, 1997). Research has demonstrated that both individuals and groups focusing their attention on a common event may produce small but statistically significant effects on inanimate detectors, such as random generators (Jahn et al., 1997; Nelson et al., 2002; Radin, 1997, 2002, 2006). Well-controlled electroencephalogram (EEG) studies have shown that evoked potentials in one person's brain may produce changes in the brain of another person to whom they feel close personal connections, but who was isolated from them in an

electromagnetically shielded room (Radin, 2004; Wackermann et al., 2003). Studies using functional magnetic resonance imaging (fMRI) also suggest that the brains of individuals separated from each other may become significantly correlated (Standish et al., 2003). Healers focusing positive energy on patients to whom they feel a special connection appear to produce fMRI changes in the brains of the patients, even though the patients were isolated and the periods of focusing were on a random schedule (Achterberg et al., 2005). Nineteen studies indicate that when a person sends anxiety-provoking thoughts to another person isolated from him, they may produce significant electrodermal stress reactions in that person (Schlitz & Braud, 1997). Reviews have found over 2200 reports of distant intentionality, including distant healing (Dossey, 1999; Jonas & Crawford, 2003; Schlitz, 1996; Schlitz et al., 2004).

Since 1975, 33 studies have been published in peer-reviewed journals and another 18 in research anthologies reporting that the Transcendental Meditation (TM) program and its advanced variant, the TM-Sidhi program, may have beneficial effects on the general population (Orme-Johnson, 2009). In these studies there is no obvious physical connection between the meditators and the affected population, nor is there any intention from the meditators to have any effect. The phenomenon, which has been named the Maharishi Effect, appears to be a by-product of increased coherence in the collective consciousness (Dillbeck et al., 1987). These effects are measured by changes in objective social indicators, such as crime rate, economic indicators, and war-related variables (e.g., Cavanaugh & King, 1988; Davies & Alexander, 2005; Dillbeck, 1990; Dillbeck et al., 1987, 1988; Gelderloos et al., 1990; Hagelin et al., 1999; Orme-Johnson et al., 1988, 1990).

The majority of these studies on social indicators have used Box and Jenkins (1976) autoregressive integrated moving averages (ARIMA) time series analyses. The main threat to internal validity in time series experiments is "history," that is, some other variable more or less simultaneous with the experimental variable, which, in fact, may have caused the effect. The credence of the experimental results depends on ruling out plausible and even implausible other variables or arbitrary events that may possibly have caused the observed change (Campbell & Stanley, 1963). The ARIMA methodology controls for a major class of confounds from "history," which are cycles and trends originating from known or unknown sources (such as weekends, seasonal effects, long-term changes in economic trends, and random walks). It also addresses the statistical issue inherent in most time series, which is that they are auto-correlated and are not statistically independent data points, which is a necessary condition for virtually all statistical tests.

The ARIMA methodology allows the researcher to identify, model, remove, and diagnose the effects of time dependencies in the data before applying inferential statistics. It also permits the study of multiple input variables, so that alternative explanatory or moderator variables can be modeled and their independent effects and interactions with the experimental variable can be directly quantified,

assessed and controlled. Such controls for history have been used in research on the Transcendental Meditation technique to control for monetary growth and supply shocks on an index of inflation and unemployment (Cavanaugh & King, 1988), to control for a new police vigilance system on crime rate (Dillbeck et al., 1987), to control for changes in police staffing on crime rate (Hagelin et al., 1999), and to control for the effects of holidays on a quality-of-life index (Orme-Johnson et al., 1988).

Another major research issue is external validity, which in this research means whether the results generalize to different target populations, treatment (meditator) groups, and outcome variables (Campbell & Stanley, 1963). Studies have demonstrated the Maharishi Effect in many different nationalities and cultures, e.g., Canada (Assimakis & Dillbeck, 1995; Cavanaugh, 1987); India, Philippines, and Puerto Rico (Dillbeck et al., 1987); Israel and Lebanon (Davies & Alexander, 2005; Orme-Johnson et al., 1988); and the U.S. (Cavanaugh & King, 1988; Dillbeck, 1990; Dillbeck et al., 1987, 1988; Hagelin et al., 1999). The effects have been demonstrated on the levels of cities (Dillbeck et al., 1987; Hagelin et al., 1999), states (Dillbeck et al., 1987), nations (Davies & Alexander, 2005; Orme-Johnson et al., 1988), and the world (Gelderloos et al., 1990; Orme-Johnson et al., 2003). The different groups practicing the TM technique have been composed of individuals from many nationalities, so external validity is strong for this technique.

Many of the scholarly exchanges about this research have focused on one particular study published in the *Journal of Conflict Resolution* (Orme-Johnson et al., 1988). These exchanges illustrate how criticism of the research has been addressed (Brown, 2005; Duval, 1988; Orme-Johnson et al., 1988, 1990; Russett, 1988; Schrod, 1990). We will briefly review this published debate and then turn to replying to a critique of the study (Fales & Markovsky, 1997), which has not previously been addressed in the published literature.

The study in question, entitled "The International Peace Project in the Middle East," was a prospective experiment, supported by a grant from the Fund for Higher Education. During a 2-month period (August and September 1983), a group of Israeli meditators was assembled in Jerusalem in a hotel to practice the TM and TM-Sidhi program together as a group (Orme-Johnson et al., 1988). The outcome variables were all the relevant social indicators available and included crime, auto accidents, fires, war deaths, war intensity, stock market, and national mood. All were obtained from governmental or other public sources, and specific predictions were lodged in advance of the study with an independent project review board. Given that these were public data and predicted in advance, there was no possibility of opportunistic data mining (Campbell & Stanley, 1963). Moreover, assessor blinding is built into research on such measures because the data are routinely collected by governmental and other public agencies without any knowledge of the experiment. In addition, there were no known changes in how the data were defined or collected over the experiment, which could potentially be another confound (Campbell & Stanley, 1963).

The independent variable was the daily number of meditators in the group, which met twice a day. The size of the group, which ranged from 65 to 241, was precisely known because the meditators were counted every session. Three times during the experiment before the data were analyzed the daily totals were sent to members of the project review board (Orme-Johnson et al., 1988).

ARIMA transfer function and impact assessment analysis of daily data over the 2-month period of the study showed significant covariation in the predicted direction of the social indicators with the size of the meditator group, controlling for time-correlated history variables and religious holidays. The effect was particularly strong for composite indices of the dependent variables. This would be expected if there were a common influence on all of the variables, which was enhanced by signal averaging (Orme-Johnson et al., 1988: 806).

The study went through twice as many reviewers as usual before it was published (Russett, 1988). One reviewer suggested that the results might be due to reverse causation. He suggested that news of the war increasing could have motivated the meditators to come from various parts of Israel to join the group (Duval, 1988). But he dropped this suggestion when it was pointed out that (1) such a scenario would have resulted in an increased number of meditators in the group being correlated with increased war, which was the opposite of what was observed, and (2) changes in the meditator group size led changes in the war and other variables (Orme-Johnson et al., 1988).

Another reviewer argued that the modeling of time series is flexible, and therefore vulnerable to false positives (Schrodt, 1990). The study authors demonstrated that using the objective Akaike criteria of model efficiency the “best” model strongly supported the hypothesis (Orme-Johnson et al., 1990). The study authors also showed that the “best” model according to Liu’s linear transfer function approach, as well as the simplest possible adequate model, also strongly supported the hypothesis.

The reviewer also argued that time series analysis gives many possible lags at which a significant effect could show up, which increases the chances of a Type I error (Schrodt, 1990). However, all the major variables showed significant effects at lags 0 or 1, indicating an immediate or next day effect, as predicted by the theory. Moreover, the conclusions were not changed if one ignored the few longer lagged effects that were statistically significant (Orme-Johnson et al., 1990). At the suggestion of the reviewer (Schrodt, 1990), the study authors also conducted robustness checks of the analysis by using random “pseudointerventions” and did not find any evidence of false positives (Orme-Johnson et al., 1990).

However, Fales and Markovsky (1997) maintain that previous reviewers overlooked several events occurring at the time of the experiment, which could potentially provide mundane explanations for the results. The remainder of this paper is a detailed reply to these critics, and all references to “the reviewers” or “the critics” will be to their paper, unless otherwise indicated. References to “the original study” and “the study” refer to Orme-Johnson et al. (1988), unless

otherwise indicated. In the first section of the paper we address the methodological issues raised by the reviewers, and then we take up the theoretical issues in the second section.

### Methodological Issues

The reviewers maintain that several potentially confounding variables were not controlled: (1) other religious holidays; (2) August holidays; (3) summer heat; (4) the announcement and eventual withdrawal of Israeli troops from the Shouf mountains and its effects on the war; (5) the announcement and eventual resignation of Prime Minister Menachem Begin; and (6) a successful sweep of Beirut by the Lebanese army. (These events are described in detail in the *New York Times* Index “Middle East” for this period.) However, most of these events do not satisfy even the most minimal requirements of an alternative explanation, because they do not account for the observed significant correlations between the size of the meditator group and changes in the dependent variables in the predicted directions over the entire experiment.

#### *Holidays*

The original study did control for Rosh Hashanah, September 8, 1983, and Yom Kippur, September 17, 1983, and found that they did not diminish the meditator effect, nor did they have any effect on the war in Lebanon, although they did have a significant independent effect on a composite index for Israel, as shown in Tables 2a–e of the original study (p. 799). However, the reviewers contend that three other religious holidays, Succoth, Shemini Azeret, and Simhat Torah, could influence the results. In 1983, Succoth was celebrated in Israel over an 8-day period, from September 22 through September 29. Shemini Azeret and Simhat Torah were celebrated together in Israel on the eighth day of Succoth, so the 8 days of Succoth include all three holidays. It is obvious from simple visual inspection of figures published in the original paper that these 8 days cannot explain the co-variation between the size of the meditator group and the social indicators seen in the preceding 51 days for War Intensity (see Figure 2 of the original study, p. 794) or the Overall composite index (see Figure 3 of the study, p. 795). On 5 of the 8 days during Succoth, the war intensity in Lebanon was above the mean, disproving these reviewers’ contention that these holidays might have been correlated with reduced hostilities.

#### *August*

The reviewers maintain that many people leave Israel in August because it is a vacation month and they propose that this may explain the reduction in automobile accidents. However, simple inspection of the published plots of the raw data, which were given to the reviewers, indicates that there was no special reduction in auto accidents during August (Orme-Johnson et al., 1988: Appendix A).

### *Summer Heat*

The reviewers speculated that there may be a tendency to cook less during the hot weather of August, which might account for fewer fires in August. Again, inspection of the data plots shows that there were actually more fires in August than September, indicating more, not fewer, fires on hotter days. The reviewers also suggested that war hostilities may decrease during the desert heat of August, and inspection of Figure 2 of the original paper shows that war intensity does appear to be generally less in August than September. However, this does not explain the detailed correlation between meditation attendance and reduced warfare in both August and September.

### *Military/Political Events*

The reviewers do not provide any rationale, specific predictions or even *post hoc* speculations for how the resignation of Prime Minister Begin, the withdrawal of Israeli troops from Lebanon, or the sweep of Beirut might be expected to influence the social indicators for Israel and Lebanon. Neither do they offer an explanation for how these events could account for the observed correlation between the rise and fall of the size of the meditator group and rise and fall of the quality-of-life indices. The quantitative analyses presented below show that although some of these events did have an effect on the quality-of-life index, they could not explain away the correlation between the meditator group size and the quality of life.

## **Methods**

### *Control Variables*

The first step in quantitatively assessing the reviewers' proposed alternative hypotheses was to create control variables that could be used in statistical analyses. This was done by coding the control days as "1's" and all other days as "0's" (see Table 1).

It can be seen in Table 1 that in addition to the controls suggested by the reviewers, other control variables were also coded: Allhol = all religious holidays in one variable; Aughol = August plus all religious holidays. Also coded were weekends vs. weekdays, vacation vs. work days, and hot vs. cool days. The weekend in Israel is considered to be Friday and Saturday. In addition to this definition, weekends were also defined as Saturday and Sunday, as well as Fridays, Saturdays, and Sundays taken together and also taken individually.

### *Dependent Variable*

The Overall composite index of six individual quality-of-life indices shown in Table 1 was analyzed because it was the major dependent variable used in the

TABLE 1  
Definitions of Variables

Variable	Definition
Overall	$z$ -Transformed daily arithmetic mean of six standardized variables: Lebanon war intensity scale, automobile accidents, fires, stock market, total Israeli crime, and national mood. The index was $z$ -transformed (mean = 0, SD = 1), and the negative variables were inverted so that positive values indicate improved quality of life.
OverallAR1	The residuals of Overall for an autoregressive 1 (AR1) ARIMA noise model.
ME Quartile	A categorical variable in which the four different quartiles of TM group size were coded as 1, 2, 3, or 4, respectively. The range of numbers of meditators in the ME group for the four quartiles were Q1 (61–124), Q2 (125–157), Q3 (158–179), and Q4 (180–241).
Succoth	The 8 days of Succoth (September 22–29, 1983) = 1's; the other 53 days = 0's.
Allhol	The 10 days of Succoth plus Rosh Hashanah (September 8, 1983) and Yom Kippur, September 17) = 1's; the other 51 days = 0's.
August	August 1–31 = 1's; September 1–30 = 0's.
Aughol	The 41 days of August 1–31 and Allhol = 1's; the other 20 days = 0's.
Weekends	The 17 days of all Fridays and Saturdays in September and August, 1983 = 1's; the 44 weekdays = 0's.
Vacations	The 26 days when the stock market was closed (all Fridays and Saturdays, August 10 and September 7, 8, 21, 22, and 26–29) = 1's; the 35 workdays = 0's.
Begin	The 19 days from when Begin announced he would resign (August 28) until he resigned (September 15) = 1's; the other 42 days = 0's.
Pullout	The 28 days from when Israeli troops withdrew from the Shouf mountains to their own border (September 3) until the end of the study (September 30) = 1's; the 33 days from August 1 to September 2 = 0's.
Temperature	The 31 days over the median maximum daily temperature for August and September = 1's; the 30 days below the median temperature = 0's.

original study. It captures a global influence, which the theory predicts would be most clearly seen in a composite index due to signal averaging amplifying common variance (Orme-Johnson et al., 1988: 806).

In one of the analyses we used the Overall index without pre-whitening it, because a previous reviewer has suggested that the pre-whitening process may have created statistical artifacts that caused spurious significant outcomes (Schrodt, 1990). In addition, to meet the statistical assumption of independence of data points (Box & Jenkins, 1976), the prewhitened version of the Overall index was also analyzed. This variable, called "OverallAR1," has the first-order autoregression term removed. This is the simplest adequate noise model and is the same model that was used in the original paper (p. 789). Diagnostic tests showed that the AR1 model was adequate, with no significant individual autocorrelations or partial autocorrelations in the residuals, which were also not jointly significant, as indicated by the Ljung-Box  $Q$  statistic at 15 lags ( $Q_{15} = 6.93$ ,  $p < .96$ ). Consequently, the AR1 model rendered the data points statistically independent, effectively controlling for the possible confounding influences of cycles, trends, and drifts in the Overall index, such as from weekend effects or any other known or unknown time dependencies (Box & Jenkins, 1976).

*Independent Variable*

The independent variable was the quartile size of the TM group in Jerusalem (ME Quartile), in which “1’s” represented days of the smallest quartile of group size and “2’s,” “3’s,” and “4’s” represent successively larger quartiles, as defined in the original paper (see Table 1).

Figure 1 shows the temporal distribution of the four quartiles of meditator-group size over the course of the 61-day experiments, as well as the distribution of key controls: Aughol, Hot Days, the Begin Period, and Vacations.

*Statistical Analyses*

*Two-way analyses of variance (ANOVAs).* The effects of each control variable were first tested one at a time. Controls and ME Quartile were entered together as independent variables in two-way ANOVAs to predict the dependent variables Overall or OverallAR1. These ANOVAs provided independent main effects for ME Quartile and for each of the Controls, as well as their interactions. The ANOVA model was:

$$\text{Overall or OverallAR1} = \text{ME Quartile} + \text{Control} + \text{ME Quartile} \times \text{Control}.$$

*Reduced data sets.* To further test if any of the controls could account for the results, one-way ANOVAs of the effects of ME Quartile on Overall or OverallAR1 were conducted on reduced data sets *in which the control days were completely eliminated from the analyses.* For example, the effects of ME Quartile were assessed for only the 35 workdays or the 31 hottest days.

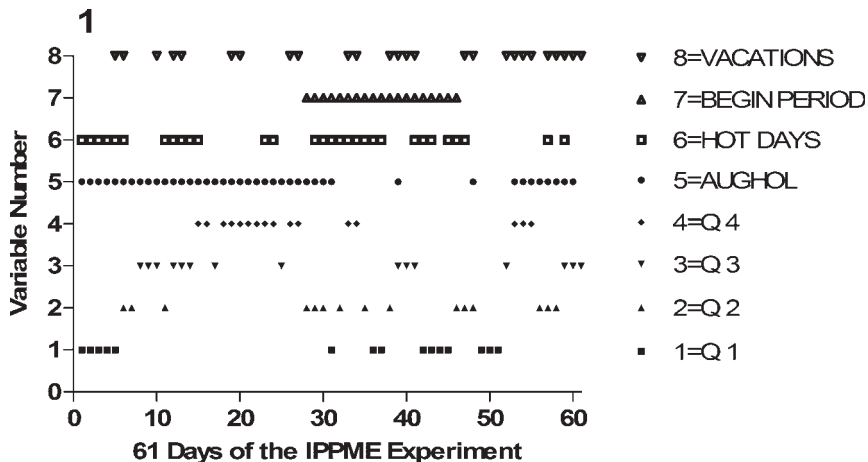


Fig. 1. The temporal distribution over the 61-day period of the International Peace Project in the Middle East (IPPME) of the independent variable, which was the quartiles of meditator-group size, Q1–Q4 (labeled 1–4), and four key control variables (labeled 5–8). It can be seen that the four quartiles of group size were irregularly distributed over the experiment, and autocorrelations indicated they were virtually randomly distributed.

*Random samples.* As a general test of whether the correlation between the size of the meditator group and change in social indicators was due to any particular days or events, random samples of data were also analyzed. The sequence of the 61 days of data was randomized three times, and both the first and second halves of the randomized series were used, giving six 50% random samples. One-way ANOVAs of the effects of ME Quartile on Overall and OverallAR1 were then performed on each random sample.

*Analyses of covariance (ANCOVAs).* To test whether any combination of control variables could account for the results, different sets of control variables were systematically entered as covariates in ANCOVAs, using the model:

$$\text{Overall or OverallAR1} = \text{ME Quartile} + \text{Covariates.}$$

## Results

Overall and OverallAR1 met the ANOVA assumptions of homogeneity of variance, normal distribution, interval scale or better, and for OverallAR1, independence of cases.

*Results of two-way ANOVAs.* The simplest ANOVA model of ME Quartile without any control variables in the equation, as well as all models that included control variables, showed that ME Quartile had a significant effect in the predicted direction (see Figure 2A and B for plots of the means; Tables 2A and B for the significance levels). Some control variables had independent significant effects (Figure 3A and B; and Tables 2A and B, column 3). However, none of the controls eliminated the significance of ME Quartile on either Overall or OverallAR1 ( $p$  values ranged from  $< .003$  to  $< .0001$ , see Tables 2A and B, column 2).

The other variables studied (i.e., weekends defined as Saturdays and Sundays; Fridays, Saturdays, and Sundays taken together or separately; the specific days of Begin's announcement and resignation; the specific day of the pullout of the Israeli army) also had no effect on the significance of ME Quartile.

*Interactions.* Column 4 of Tables 2A and B shows that none of the computable interactions between ME Quartile and Controls (August, Pullout, and Temperature) were significant. The other interactions could not be calculated because of too few observations in the interaction cells. However, additional analyses, in which adjacent ME Quartiles were collapsed to create cells with large enough  $n$  values to compute the interactions, found that none of the other interactions were significant (Weekends,  $p < .34$ ; Vacations,  $p < .1$ ; Begin,  $p < .28$ ). This indicates that as the size of the meditator group increased, the Overall and OverallAR1 indices increased the same way during control days as during non-control days.

### *Effect Sizes*

The effect size of the meditator group on the Overall and OverallAR1 quality-of-life indices was here defined as the difference between the means for

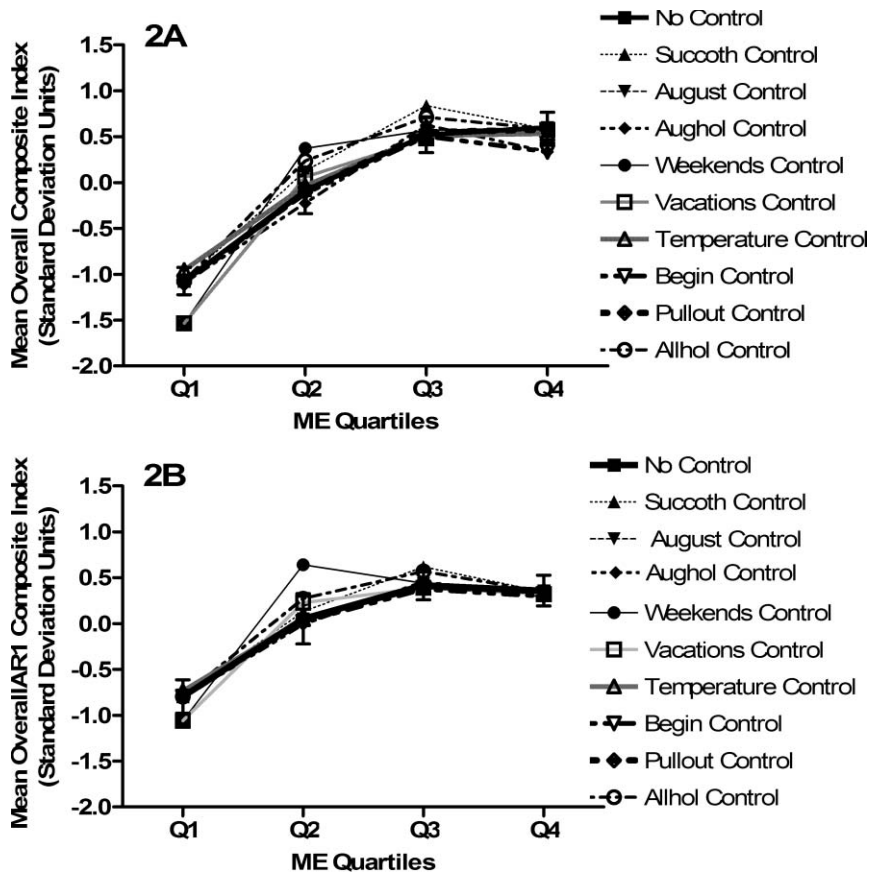


Fig. 2. A. The means from ANOVAs on the Overall composite index for each of the four quartiles of group size of Transcendental Meditation and TM-Sidhi program participants (ME Quartile). In all cases, increases in the group size were significantly associated with increases in quality of life (see *p* values in Table 2A). The darkest line shows the results with no control variable present in the analysis, and the other lines show the results with the nine various controls included. The error bars are standard errors of the mean for the analysis with no control variable. B. The results for the OverallAR1 index (the Overall index prewhitened with an ARIMA AR1 term). These results are parallel to those for the un-prewhitened index shown in A—a predicted increase in the quality of life as a function of increasing the group size of meditators, irrespective of the influences of the control variables. The error bars are standard errors of the mean for the analysis with no control variable present, indicated by the darkest line (see *p* values in Table 2B).

the smallest and largest quartiles. The effect sizes are in standard deviation (SD) units because these indices are z-transformed data, already expressed in SD units (Cohen, 1988; Glass et al., 1981).

ME Quartile had a larger effect size than any of the control variables, which can be graphically seen by comparing Figure 2A and B with Figure 3A and B.

TABLE 2A  
Overall Index: *p* Values from the Two-Way Analyses of Variance

Dependent variable = Overall			
Control variable	<i>p</i> Value, main effect, ME quartile	<i>p</i> Value, main effect, control	<i>p</i> Value, interaction, ME × control
Succoth	.0001	.005	NA
Allhol	.0001	.0002	NA
August	.0001	.24	.24
Aughol	.0001	.34	NA
Weekends	.0001	.3	NA
Vacations	.003	.24	NA
Begin	.0001	.03	NA
Pullout	.0001	.09	.49
Temperature	.001	.1	.98

Note: NA = not applicable because of insufficient data in one of the ANOVA cells to compute the interaction.

TABLE 2B  
OverallAR1 Index: *p* Values from the Two-Way Analyses of Variance

Dependent variable = OverallAR1			
Control variable	<i>p</i> Value, main effect, ME quartile	<i>p</i> Value, main effect, control	<i>p</i> Value, interaction, ME × control
Succoth	.0005	.1	NA
Allhol	.0001	.01	NA
August	.0003	.29	.75
Aughol	.001	.82	NA
Weekends	.001	.04	NA
Vacations	.003	.04	NA
Begin	.001	.18	NA
Pullout	.0003	.22	.9
Temperature	.003	.66	.98

Note: NA = not applicable because of insufficient data in one of the ANOVA cells to compute the interaction.

The mean effect size of the 10 two-way ANOVAs for ME Quartile on the Overall index was 1.67 SD. By comparison, the difference between the control days and non-control days was only 0.97 and 0.84 SD for Allhol and Succoth, respectively. During the Begin period, the quality-of-life index decreased by 0.5 SD compared with all other days, and during hot days it decreased by 0.36 SD compared with cool days. After the withdrawal of the Israeli army from Lebanon (Pullout, which was basically September compared with August), the quality-of-life index decreased by 0.34 SD. These data indicate that the effect size of ME Quartile was 1.7 times as large as Allhol, twice as large as Succoth, 3.3 times larger than the Begin variable, 4.6 times larger than Temperature, and 4.9 times larger than the effects of the Pullout of the Israeli army.

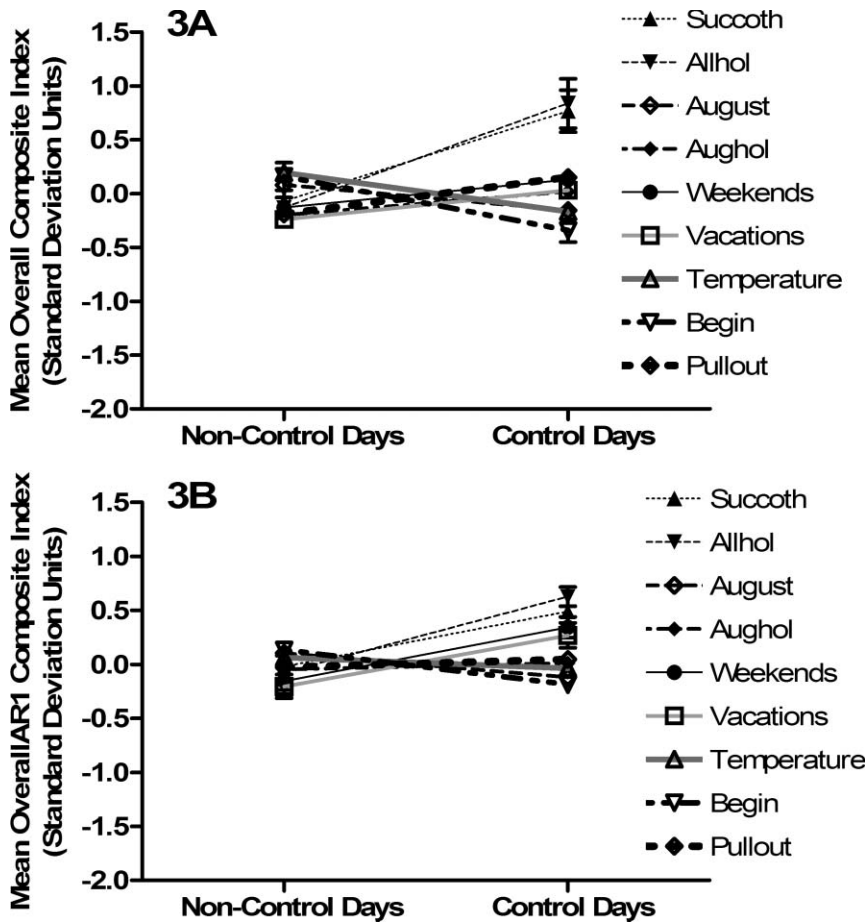


Fig. 3. A. The main effects of control days contrasted with non-control days for the Overall composite index. Standard error bars are shown for the variables that changed significantly on the control days (higher quality of life on Succoth and Allhol, lower quality of life during the Begin resignation) (see *p* values in Table 2A). B. The main effects of control days contrasted with non-control days for the prewhitened OverallAR1. The index was significantly higher during Weekends and Vacations and all religious holidays (Allhol) compared to other days, but these effects could not explain the meditator effect (see *p* values in Table 2B).

*Results for reduced data sets.* The results for the reduced data sets in which control days are eliminated are shown in Table 3. The *n* values in Table 3 show the number of days used in each analysis (also refer to definitions of variables in Table 1). In all cases, the ME Quartile had a significant effect, even during the days when the various control days were completely eliminated from the analysis. This conclusively shows that none of the control days could account for the effects of the co-variation between the meditator group size on the quality-of-life indices.

TABLE 3  
 Results (*p* Values) for Reduced Data Sets: One-Way ANOVAs for ME Quartile on the Overall and OverallAR1 Indices of Quality of Life for Data Sets in which the Control Days Were Eliminated from the Analyses

	Non-Succoth ( <i>n</i> = 53)	Non-Allhol ( <i>n</i> = 51)	Septem-ber only ( <i>n</i> = 30)	Non-Aughol ( <i>n</i> = 20)	Week-days only ( <i>n</i> = 44)	Workdays only ( <i>n</i> = 35)	Non-Begin ( <i>n</i> = 42)	Non-pullout ( <i>n</i> = 33)	Hot days ( <i>n</i> = 31)
Overall	.000001	.000001	.0006	.0009	.000002	.000001	.0003	.0001	.00006
Overall-AR1	.0007	.0003	.02	.04	.01	.002	.009	.01	.003

Note: *n* = number of days included in the analysis.

*Results for random samples.* The additional ANOVAs on the six random samples of 50% of the data showed that in all cases ME Quartile was significant for both Overall and OverallAR1 (*p* values ranged from <.02 to .00001, see Figure 4).

*Results of ANCOVAs.* ANCOVA showed that no combination of control variables eliminated the significance of ME Quartiles on Overall or OverallAR1. The significant controls for Overall from the two-way ANOVAs, as shown in Table 2A, were Succoth, Allhol, Begin and, marginally, Pullout and Temperature. Since Allhol included Succoth, Allhol was initially used as the covariate entered along with Begin, Pullout, and Temperature as covariates. In this combination, only Begin and Pullout were significant, and when just these two were entered together, they were both significant predictors (Begin, *p* < .003; Pullout,

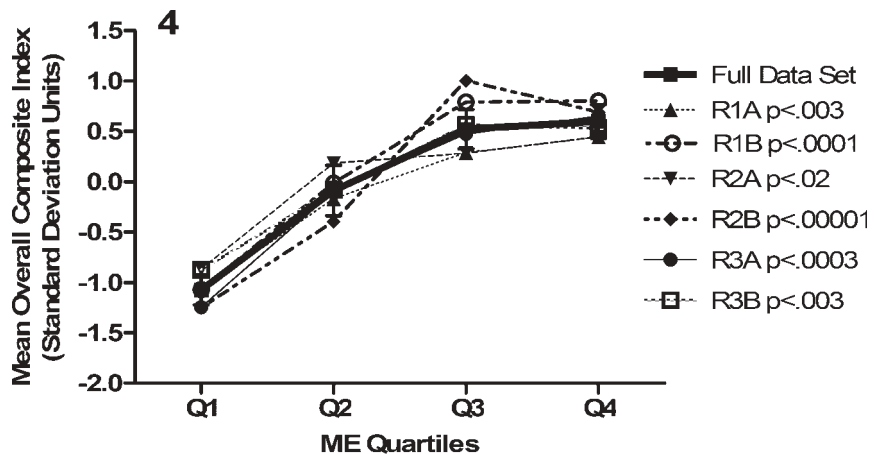


Fig. 4. The effects of ME Quartile on the Overall composite index for six random samples of 50% of the Data (R1A to R3B) as well as for the full data set (darkest line). It can be seen that the effect of the mediator group was evident for each random sample of data. The *p* values are indicated in the legend. Parallel results were found for OverallAR1.

$p < .009$ ), and they were correlated with each other  $r = .3$ . The multiple  $R$  was 0.45,  $p < .002$ . However, with Begin and Pullout added jointly as covariates, ME Quartile was, if anything, even a more significant predictor of Overall than before ( $p < .000001$ ).

For OverallAR1, the significant controls from the two-way ANOVAs were Allhol, Weekends, and Vacations (Table 2B). These variables were highly redundant, because Vacations is a composite of the other two. We saw earlier that Vacations alone could not explain the results, because the results were significant when these days were completely eliminated from the analysis (see Table 3, Workdays,  $p < .002$ ). Therefore, we entered Allhol and Weekends together as covariates. They were both significant ( $p < .05$  and  $p < .006$ , respectively;  $R = 0.42$ ,  $p < .004$ ) and were only correlated by  $r = 0.02$ . This indicates that religious holidays and weekends made independent contributions to predicting the quality of life as represented by OverallAR1. However, with these covariates jointly included, the effect of ME Quartile on OverallAR1 was still strong ( $p < .009$ ).

Other combinations of covariates also did not alter the basic conclusions. For the Overall index, the best combination of covariates, defined as the largest  $R$ , was Begin ( $p < .001$ ), the high holidays (Yom Kippur and Rash Hashanah,  $p < .03$ ), Pullout ( $p < .03$ ), and Weekends ( $p < .08$ ); multiple  $R = 0.56$  ( $p < .0004$ ). With these covariates in the model, ME Quartile was significant ( $p < .000004$ ). The best set of covariates for OverallAR1 was Weekends ( $p < .01$ ), high holidays ( $p < .02$ ), and Begin ( $p < .05$ ); multiple  $R = 0.49$  ( $p < .002$ ). With these covariates in the model, ME Quartile had a significant effect on OverallAR1 ( $p < .009$ ).

## Discussion

Formal statistical analyses verified the impression given by simple inspection of published graphs of the data that none of Fales and Markovsky's (1997) proposed alternative explanations could account for the results of the International Peace Project in the Middle East (Orme-Johnson et al., 1988). The finding that the meditator effect became more significant when controls were added as covariates indicates that the controls had an independent effect. In consequence, the inclusion of controls reduced the error variance, thus increasing the statistical significance of the meditator effect.

The positive results for the un-prewhitened "raw" Overall index demonstrate that the outcomes were not due to distortion of the data by the prewhitening process, as a previous critic has suggested (Schrodt, 1990).

The positive outcomes using the prewhitened index (OverallAR1) demonstrate that the results are not due to autocorrelations in the data, as was previously demonstrated using the ARIMA methodology in the original paper (Orme-Johnson et al., 1988) and in a reply to a critic (Orme-Johnson et al., 1990). Nor can the results be attributed to possible autocorrelations in the independent variable (ME Quartile), because an independent variable can take any form as long as the dependent variable is prewhitened (Box & Jenkins, 1976).

The results of the two-way ANOVAs demonstrate that none of the control variables *taken one at a time* could account for the effects of the meditator group of the Overall index of social indicators. The results of the ANCOVAs further showed that *no combination* of the control variables could account for the results. The results of the ANOVAs when the control days were completely removed from the data sets conclusively establish that the control variables could not account for the results. Finally, the analyses of six random samples of 50% of the data are a generalized demonstration that the meditation effect is a general feature of the data, which does not depend on any special events or days.

The effect size of the meditator group was strong in comparison to the effects of holidays and the major military/political events at the time. The fact that vacations, the Begin resignation, and the pullout of the Israeli army from Lebanon did have significant effects on the composite index of quality of life in Israel and Lebanon demonstrates construct validity for the index. It shows that the index was sensitive to key social and political events at the time. Construct validity is further supported by the apparent face validity of these effects, e.g., increased quality of life during vacations and decreased quality of life during the Begin resignation.

However, the effect sizes of the control variables, which ranged from 0.34 to 0.97, were not as strong as the effect sizes for the meditator group, which averaged 1.67. Given that an effect size of over 0.8 is considered large in the social sciences (Cohen, 1988), the effect size of 1.67 for ME Quartile is impressive. Moreover, the finding that it was 1.7 to 4.9 times larger than major events at the time supports the suggestion that the Maharishi Effect operates from the fundamental level of the unified field (Hagelin, 1987, 1989).

The lack of significant interactions between the ME Quartile and the control variables indicates that the effect operates the same way during control days as during other days. For example, the effect was apparent during both August and September, during both hot days and cool days, during both vacation days and workdays, etc. This shows that the effect is robust across different cultural, military/political, and climatic conditions, as has been demonstrated by previous research (Assimakis & Dillbeck, 1995; Cavanaugh, 1987; Cavanaugh & King, 1988; Davies & Alexander, 2005; Dillbeck, 1990; Dillbeck et al., 1987, 1988; Hagelin et al., 1999).

Since the various potentially confounding events suggested by Fales and Markovsky (1997) do not provide a mundane explanation for the results, consideration of their criticism of the theory of the collective consciousness is warranted.

### Theoretical Issues

Fales and Markovsky (1997) build their critique of the theory around five main points, which they quote from the original paper (Orme-Johnson et al., 1988):

- (1) Collective consciousness is the wholeness of consciousness of the group, that is more than the sum of the consciousnesses of all individuals composing that group. (p. 778)

- (2) [The theory] posits a unified field of “pure consciousness” as the basis of the diverse activities of all individual minds. All processes of thought and perception are viewed as fluctuations or qualified expressions of this underlying, unqualified, least-excited state of consciousness. Maharishi likens the individual mind transcending its more active levels and experiencing its basis in pure consciousness to a localized wave settling to become the silent, unbounded surface of the ocean. Such experiences are said to create nonlocal, field-like effects of order and coherence in the environment. (pp. 778–779)
- (3) Nonlocal effects could be mediated through the agency of the unified quantum field due to the intrinsically nonlocal structure of space-time at this scale. (p. 784)
- (4) A potential explanation for the apparent propagation of such coherent effects may lie, however, at the ultimate scale of superunification, the Planck scale of  $10^{-33}$  cm and  $10^{-44}$  sec, where the fundamental force and matter fields are said to become fully unified. (p. 784)
- (5) The localized conscious awareness of the individual becomes experientially connected back to pure consciousness, the unified source of order and intelligence at its base, thus increasing coherence, reducing stress, and accelerating development in the larger society. (p. 785)

The reviewers then cite an equation from the paper, which predicts how many people will be affected by the meditators living in the population and by a special advanced group. This equation was explained in the original paper as follows:

With the introduction of the more advanced TM-Sidhi program in 1976, Maharishi anticipated an even more marked influence of coherence in collective consciousness. He predicted that when the TM-Sidhi program was practiced in a group by as few as the square root of one percent ( $\sqrt{1\%}$ ) of a population, there would be a measurable effect on standard indices of quality of life. This prediction is based on a field theoretic model describing the coherent superposition of amplitudes, in which the intensity of the effect generated is proportional to the square of the number of participants (Hagelin, 1987). For example, in coherent systems such as lasers, the coherent elements in the system have an influence that is proportional to their number squared, whereas incoherent elements generally have an influence that is proportional only to their number. Thus the predicted population size influenced by a given number of TM and TM-Sidhi program participants would be tentatively modeled by the polynomial:

$$ME = aN_1 + bN_2^2 \quad (1)$$

where ME (Maharishi Effect) is defined as the size of the population that is positively influenced by the number of independent meditators distributed throughout the population ( $N_1$ ), and the number of individuals practicing the more advanced TM-Sidhi program collectively in one place ( $N_2$ ). The quadratic term reflects the proposed coherent influence resulting from constructive interference of the group of  $N_2$  subjects. Coefficients  $a$  and  $b$  are empirically defined constants, with data suggesting that both have an estimated value of approximately  $10^2$  (for values of  $N$  over 100). The absence of a constant term follows from the assumption that the effect vanishes (and does not diverge) as  $N$  tends to zero. (Cubic and higher-order terms are neglected because they have no clear theoretical motivation). The apparent necessity for having a single group meet at one time and place to produce this  $\sqrt{1\%}$  effect may again be understood with reference to coherent physical systems such as lasers. In these systems, close proximity of elements is required to ensure that they have sufficient opportunity to stimulate coherent behavior in other members of the group. (p. 782)

In a research paper, such a short summary of theoretical issues is the norm, since the dominant interest is assumed to lie in the methodology and the resulting evidence that will tend to confirm or disconfirm the theory. Such short summaries have been a feature of all 33 published studies on the Maharishi Effect, indicating that a large number of peer reviewers have found the theory elucidated with a level of meaningfulness typical of other published research in the social sciences (Duval, 1988; Russett, 1988).

The authors of the critique, however, argue that nearly every term used in the theory section is “ambiguous,” “vague,” or outright “meaningless.” The strategy behind their critique is revealed in the following quote: “Vagueness about a theory’s empirical referents or how they interact with previously understood parts of our world will undermine any attempt to claim that certain phenomena provide evidence for (or against) that theory” (p. 2). More plainly, the reviewers argue that any such failing of the theory means that it can no longer “benefit from the test results.”

To begin with, this assertion that “ambiguity” and “vagueness” undermine objective evidence is simply wrong as a matter of scientific practice. Ambiguity of terms does not prohibit a clear decisive experimental decision about the hypothesis in question, which is, simply, did the war and other social indicators co-vary with the meditation groups? That is a clear and simple question that can be answered whether or not one understands the theoretical model. The real issue is whether the theoretical hypothesis being tested is unambiguous enough to permit a decisive empirical test.

Take a simple case from quantum mechanics as an example of how a precise empirical science can develop even though key theoretical terms may be ambiguous. The primitive elements are called quanta, which famously display wave-particle ambiguity—despite the fact that a particle must by definition be highly localized and a wave must be widely extended in space. This essential ambiguity is only increased by the vagueness inherent in the Uncertainty Principle, which states that no quanta can be attributed a precise position and a precise velocity at the same time. This ambiguity and vagueness of fundamental terms and concepts helps account for the following quotation from Richard Feynman: “I think I can safely say that no one understands quantum mechanics” (Van Flandern, 1999: 118). Despite such profound theoretical failings, quantum electrodynamics is widely considered the most successful theory in the history of science—due to unprecedented precise experimental evidence that supports the mathematical predictions.

Second, the reviewers’ argument that vagueness undermines objective evidence inaccurately states a scientific principle. “Vagueness” and “ambiguity” are in themselves vague and ambiguous terms. Since there is no standard against which they can be objectively evaluated, the use of these labels in any given case is necessarily a subjective decision. Thus, their position resolves to the proposition that a subjective judgment (about, in this case, theoretical clarity) has precedence over empirical data, which is a close approximation to the opposite of what the scientific endeavor means to most scholars.

Third, the terms adjudged by the reviewers as “vague” and “ambiguous” have, to the original authors, clear and understandable meanings—though the theoretical concepts are new to most, and admittedly require sustained, good-faith attention.

A similar assertion by the reviewers makes the case that a theory, to be meaningful, must articulate a precise mechanism for its predicted effects. As an apposite example, they state: “To be credible, [a theory of the Maharishi Effect] must explain (1) how group meditation affects the unified field, (2) how these effects in turn cause changes in the actions of individual human beings, and (3) how those individual actions have their claimed social impact.”

The notion that experimental evidence can be undermined by a lack of theoretical mechanism is at odds with accepted scientific practice. To take a quintessential counterexample from the history of science, Sir Isaac Newton was never able to supply a mechanism that explained why massive bodies were attracted to each other by the law of gravity. His theory of gravity is still so mathematically accurate that it is used to send rockets to Saturn, but as he frankly said, “the cause of gravity is what I do not pretend to know” (Kohanski, 1984: 290). Once again, experimental evidence answers the question about whether the theory predicts observable outcomes, and in a social science article, does not and need not attempt to give a fully developed theory. That is the place of a theoretical paper (Hagelin, 1987).

### *Bayes' Theorem, Prior Knowledge, and Theory Confirmation*

The reviewers use a version of Bayes' Theorem, usually a mathematical tool for dealing with particular types of probability issues, in a nonstandard English language formulation, as a means to address the issue of “theory confirmation.” In their definition of Bayes' Theorem, a theory's confirmation is greater to the extent that (1) it is compelling in view of prior knowledge; (2) the evidence to be assessed is made probable by theory and prior knowledge; and (3) the [evidence] is not probable relative to alternative hypotheses and prior knowledge.

In this “Bayesian” framework, which becomes the logical heart of their critique, “prior knowledge” is the main criterion (1), and is essential in (2) and (3). By “prior knowledge” they mean the current state of orthodox theory and evidence in a field, what they elsewhere call a “well-entrenched position.” Their intent to use “prior knowledge” as a criterion for disqualifying “heterodox” theories would eliminate any new theory from sociology. Their point is that new theories must be consistent with and coherent with previous knowledge, and by connecting pure consciousness with the unified field and the quantum level of nature, we have done that precisely. The point is that unified field theory suggests that there is a field pervading nature, that all phenomena come out of it, and that it is causally basic in nature. This is the field that, according to hypothesis, bears the responsibility for bringing about change on a large social scale. It is not necessary to establish theoretically that the unified field is a field of consciousness, for

that is what the empirical outcome of the experiment is intended to establish. As the reviewers frankly admit, sociology has only a “rudimentary understanding” of the “phenomenon we study.” A similar situation holds in the rest of the social sciences; there is no generally agreed-upon theoretical basis to psychology, for instance, or political science.

Moreover, the Maharishi Effect is a consciousness-based phenomenon, and there is no systematic theory of consciousness available in any academic discipline. This complete lack of fundamental principles that can give order and meaning to studies of social phenomena in general—what to say of avowedly consciousness-based social phenomena—should be kept in mind as the reviewers emphasize that the theory behind the Maharishi Effect is improbable to the extent that it runs counter to “prior knowledge.” Indeed, there are many contemporary streams of thought that are congruent with a field-theoretic view of consciousness (e.g., Dossey, 1999; Jahn et al., 1997; Jonas & Crawford, 2003; Nelson et al., 2002; Radin, 1997, 2002, 2004, 2006; Schlitz, 1996; Schlitz & Braud, 1997; Schlitz et al., 2004; Wackermann et al., 2003). Moreover, the theory does not run *counter* to prior scientific knowledge, namely of the unified field, but it adds to unified field theory a hypothetical link to consciousness that makes it accessible to human uses and purposes that benefit all mankind. This leads to practical application and testing of theory (Hagelin, 1987, 1989; Hagelin et al., 1999).

With “prior knowledge” ruled out as a meaningful criterion for a new theory in the social sciences, the reviewers’ version of Bayes’ Theorem relapses to two criteria: “the [evidence] to be assessed is made probable by theory,” and “the [evidence] is not probable relative to alternative hypotheses.” The use of “probable” in both criteria is hardly optimal; there is no objective input into their version of Bayes’ Theorem, and no mathematical analysis, and thus there is no means to assess what is considered “probable” other than, once again, subjective judgment.

Why do the reviewers emphasize arguments based on subjective judgment? The logic is the same as above: “Whatever evidence we have for the improbability of a heterodox theory is, *a fortiori*, evidence for the illegitimacy of data purportedly favoring that theory” (p. 3). In this striking assertion that objective evidence can be made illegitimate, the authors use the term “evidence,” but as we have seen, their version of Bayes’ Theorem is free of all objective input. What is presented is not fact, but opinion.

Two other points—first, in the reviewers’ version of the Bayesian framework, the last criterion assumes that the critic, who wishes to disconfirm a theory, will develop one or more “alternate hypotheses.” They agree with this, as they must from adopting the Bayesian framework: “The case against the heterodox is strongest when a competing hypothesis can be established” (p. 3). Or, as peer reviewer Robert Duval (1988: 817) pointed out in the conclusion to his commentary on the original paper printed in the *Journal of Conflict Resolution*, “It is incumbent upon the critic to provide a better model.” They have failed to provide a better

model to explain the observed data, whereas the model we present is supported by the data.

Nevertheless, the attempt to escape this (ambiguous) requirement, which is both self-imposed and logically necessary, becomes one of the central strategies of this critique. As we have seen in the research section, the reviewers do not offer a competing hypothesis to explain the observed data of the study—the fact that changes in the attendance at the meditation group covary with the changes in social indicators. In fact, they offer no hypothesis at all, just a few *post hoc* events and the speculation that, “In fact, it is sufficient that a well-entrenched position offer plausible alternative explanations for the challenging data.” A “well-entrenched” academic or scientific position, however, would require just the type of “prior knowledge”—especially a widely accepted theoretical framework—that is totally lacking. Without the protection of any theoretical position, the critics must do better than offer an assortment of unrelated *post hoc* explanations that are plausible to them but perhaps no one else. To many scientists who have studied the theoretical model of our paradigm, this is by far the most plausible hypothesis to explain the observed data. The critics must offer an alternate hypothesis that better fits the evidence, but this, as we have seen, they never do.

Second, in the case of a theory as potentially theoretically significant as the present one, there are types of prior knowledge not considered by the reviewers that deserve mention. For example, they do not acknowledge that the original paper and other papers on the Maharishi Effect have documented that many of the founders of the social sciences, such as Emile Durkheim, Carl Jung, and William James, have proposed field-theoretic views of consciousness (Dillbeck, 1990; Dillbeck et al., 1987; Orme-Johnson et al., 1988).

### *Critique of the Theory of Collective Consciousness*

The reviewers state that collective consciousness is non-material, omnidirectional, and in violation of the inverse-square law of signal strength (p. 5). The original paper considered and rejected the four fundamental forces—electromagnetism, weak interaction, gravity, and strong interaction—as potential candidates for mediating the Maharishi Effect because they all function at inappropriate scales (p. 784). Hagelin’s (1987) suggestion that such coherent effects may be propagated at the ultimate scale of super unification is based on evidence from physics, indicating that Planck scale phenomena are described by a totally new structure of natural law, which defies characterization in the normal spatial, temporal, and causal sense. The reviewers argue that “Detailed experimental evidence is lacking for Planck scale phenomena . . . the soundness of the ME theory’s quantum field connections is an open question at best” (p. 5). However, while unified field theory and its precise mathematical formulation remain lively areas of research and debate, there is little dissension among theorists as to the validity of the overall picture that natural law is unified at the Planck scale (Hagelin, 1987, 1989).

### *Critique of the $N^2$ Formula*

The reviewers say that the paper ignored the specific hypothesis deriving from the  $N^2$  term in the Maharishi Effect equation above. In fact, the original paper addressed this issue by noting the shape of the curves relating the effect size to meditator group size (Orme-Johnson et al., 1988: 802). The authors use the meditators group size (not squared) in the calculation, and they looked for an upturning function as evidence of a quadratic effect. Such an effect was found for the war in Lebanon, but not for the composite index. (This leveling off of the Overall index at the largest quartile of group size can be seen in Figure 2A and B above.) However, as discussed in a previous paper, testing the quadratic effect is complicated by the fact that populations are not closed systems (Orme-Johnson et al., 1990). Perhaps if the effect could be contained within a single population, say a country, then as the size of the meditator group in that country increased there would be a predicted quadratic increase in the effect. But the effect is not contained, but spreads out to other populations as the group gets bigger. What the formula predicts is how many more people are affected, not how much the effect will increase in populations in close proximity to the meditator group. A related question concerns the measurement units of the formula.

### *Measurement Units*

The reviewers question why the measurement unit is the number of people rather than, say, physical distance (p. 5). The following quotation shows how this issue has previously been addressed.

First, in the forty studies in this area, the  $\sqrt{1\%}$  formula has been consistently applied based on quantification of the surrounding population in terms of political units—cities, states, nations—rather than purely on geographical distance, which ignores these community boundaries (e.g., Dillbeck et al., 1987; Dillbeck et al., 1988). These political units reflect greater homogeneity, closer personal ties, more frequent interactions, and stronger internal lines of influence (cultural, emotional, and economic, as well as political) than those across boundaries and hence cannot be ignored in calculating the pattern of “spread” of predicted coherent effects on collective consciousness and behavior. . . . Our common experience with such everyday field effects as transmission of radio or television waves tells us that local conditions (including weather, the terrain, and other electromagnetic sources, such as power lines) affect patterns of transmission across large areas. The proposed intimate connection between consciousness and the unified field would support similarly uneven patterns of influence due to local boundaries in collective consciousness. (Orme-Johnson et al., 1990: 759)

### *Critique of Lagged Effects and Thresholds*

The reviewers also maintain that some lagged effects reported in the paper were not specified prior to the research. The authors have also previously replied to this issue: “Even if one were to dismiss the results for longer lags, the consistent and even stronger results found for lags 0 and 1 cannot be overlooked” (op. cit.:

764–765). In that reply we speculated that the longer lags may reflect effects on policy makers (a key element of the theory), which take some time to manifest down the chain of command.

The critics assert that there is no rationale for the threshold effects in the Maharishi Effect theory, which holds that the effect will suddenly manifest in the system as a whole after 1% of the population is practicing the TM technique, or a group of the square root of 1% is practicing the TM-Sidhi program. Such sudden sharp changes from relatively disordered to more ordered states are “phase transitions” which are common throughout nature. Examples are the sudden transitions between gaseous, liquid, and solid states of matter at specific temperatures, and numerous instances in which the concentration of a certain substance of the body rises slowly without effect and then suddenly, as a discrete threshold is reached, target tissues respond. To give other examples, the entire functioning of our nervous system relies upon threshold principles and a small change in one or a few component species in an ecosystem can rapidly alter the entire environmental picture. In social systems, the sudden outbreak of war, riots, and other forms of violence are evidence of threshold effects. In the present case, there is considerable empirical evidence for a sudden emergence of coherence in society once a sufficient threshold of meditators in the population is exceeded (Assimakis & Dillbeck, 1995; Cavanaugh, 1987; Gelderloos et al., 1990).

#### *Effects on Inanimate Detectors*

The reviewers point out that if the material world is presumed to be influenced by collective meditation (as it must be if the unified field is affected), then more direct measures using inanimate detectors should be possible (p. 7). In fact, research has shown that persons practicing the TM technique can directly influence quantum mechanical processes (Farwell & Farwell, 1995) and that group meditation influences random generators placed near the group (Mason et al., 2007). Moreover, independent research has shown in two replications that large groups practicing the TM and TM-Sidhi programs affect a network of 50 random generators distributed throughout the world (Nelson, 2001, 2006). Furthermore, there are research programs indicating that human consciousness may interact in a measurable way with inanimate detectors (Jahn et al., 1997; Nelson et al., 2002; Radin, 1997, 2002, 2006).

#### *The Issue of Brainwave Coherence*

Another issue raised by the reviewers is how the TM technique might increase brain wave coherence and synchrony (p. 6). The short answer is that it is through the experience of pure consciousness, which is the traditional subjective term for an experience that has been shown to be real based on its consistent physiological correlates (Jevning et al., 1992; Wallace, 1970). Research has indicated that the experience of pure consciousness is associated with a state of “restful alertness,”

which is distinct from waking, dreaming, and sleeping (Jevning et al., 1992; Wallace, 1970). Evidence shows that the experience produces long-range spatial ordering in the brain (EEG coherence and synchrony) (Badawi et al., 1984; Hebert et al., 2005; Travis & Wallace, 1997). Neuroimaging studies of subjects practicing the TM technique suggest that the pure consciousness experience is associated with reduced sensory and cognitive processing at the thalamic, and hippocampal levels as well as global integration by the frontal cortex (Newberg et al., 2006), corresponding to the reported subjective experience described as the mind settling inward to a silent state of unbounded awareness. Sympathetic markers show greater reductions during the experience than during ordinary eyes-closed rest (Dillbeck & Orme-Johnson, 1987; Jevning et al., 1978).

A fundamental principle of neuroscience is that experience shapes the brain's development. Cumulative effects of regular practice of the TM technique are evidenced by greater "coherence" of the individual, operationally defined by numerous measures, such as improved balance and sensitivity of hormonal control (Werner et al., 1986), greater stability and flexibility of perception (Dillbeck, 1982), improved general cognitive ability (So & Orme-Johnson, 2001), improved cardiovascular reactivity to stress (Barnes et al., 2001), improved general health (Herron et al., 1996; Orme-Johnson, 1987), and decreased mortality (Schneider et al., 2005). Given the assumption of Maharishi's theory that individuals are the units of collective consciousness, increased coherence at the individual level could be expected to have a positive effect on the population level.

#### *Why Only Positive Socio-Cultural Effects?*

In answer to the reviewer's question of why coherence only causes phenomena that happen to be defined socio-culturally as "positive" (Fales & Markovsky, 1997), we suggest the following explanation. Through its long evolutionary history and the process of natural selection, the brain and physiology have become "hardwired" with intrinsic pleasure/pain circuits and homeostatic feedback mechanisms, which have the obvious adaptive advantage of directing life away from physical damage and towards life-promoting influences. Maslow (1968) and others have extended this line of reasoning to say that the positive qualities of self-actualization are as fundamental to our biological makeup as the motivations for self-preservation and reproduction. Consider the potential adaptive advantages of the qualities of self-actualization: being "present" oriented rather than being distracted by non-useful thoughts and feelings; having broad comprehension and increased ability to focus sharply; increased ability to integrate dichotomies into greater wholes; heightened perception; spontaneous skilled behavior; increased empathy and ability to function cooperatively with others (Maslow, 1968).

All of these qualities have obvious adaptive advantages, for modern human life as well as for hunting mastodons. An implication of Darwin and Maslow's work

is that such self-actualizing qualities are inherent in the human genome, arising as the species evolved through its interaction with various environments. A further implication is that self-actualizing qualities are inherent in the very structure of natural law itself and are not arbitrary human conceptions. A fundamental tenet of science is that all the actors and the entire field of action are entirely comprised of the same elementary particles, forces, etc. and are governed by the same laws of nature. Furthermore, if the unified field is the basis of existence, then all that exists must in some way be inherent in it, even in seed form.

Maslow speculated that self-actualizing qualities inherent in every individual become expressed through experiences that he called “peak experiences” and “Being cognition,” which are experiences pertaining to pure consciousness (Alexander et al., 1990, 1991). A meta-analysis of 42 treatment outcomes found that regular practice of the TM technique produced a threefold greater increase in self-actualizing qualities than other meditation and relaxation techniques (Alexander et al., 1991). This result supports the hypothesis that systematic transcending is the key factor contributing to the development of self-actualization. We propose that the physiological state associated with pure consciousness sets an ideal internal milieu for the expression of self-actualizing qualities inherent in the human genome. From this perspective, an influence of pure consciousness (coherence) in collective consciousness would augment the expression of these qualities in the general population as well, resulting in behavioral changes in the larger society, which would universally be regarded as more “ideal.”

### *Replication*

The association of TM practice with positive influences on social indicators has been replicated 50 times (Orme-Johnson, 2009), including seven times specifically on the Lebanon war (Davies & Alexander, 2005). It is beyond the scope of this paper to evaluate the strength of all these studies, but only to say that many of them are at least as well controlled as the present one (e.g., Cavanaugh, 1987; Cavanaugh & King, 1988; Davies & Alexander, 2005; Dillbeck, 1990; Dillbeck et al., 1987, 1988; Gelderloos et al., 1990; Hagelin et al., 1999).

### *Conclusion*

Fales and Markovsky’s (1997) criticism of the International Peace Project in the Middle East (Orme-Johnson et al., 1988) is not supported by either the empirical data nor by a logical analysis of the theory. Their conjecture that social, political, and military events at the time could explain the results does not hold up with simple inspection of the published data nor with decisive statistical tests. Their position that direct interactions between humans via a field of consciousness is a “heterodox” hypothesis not worthy of publication in social science journals is challenged by 33 published studies on the Maharishi Effect (Orme-Johnson, 2009) as well as by several other independent research programs (e.g., Dossey, 1999; Jonas & Crawford, 2003; Nelson et al., 2002; Radin, 1997,

2002, 2004, 2006; Schlitz, 1996; Schlitz & Braud, 1997; Schlitz et al., 2004; Wackermann et al., 2003).

We provide evidence that the theory of the Maharishi Effect has clearly defined, operationalizable terms and that it makes specific predictions, which have been repeatedly tested using objective, public data sources, with hypotheses being lodged in advance with independent observers. This theory has been tested on a larger scale than any other theory of conflict resolution (Orme-Johnson et al., 1990). More than any other approach in the social sciences, it has demonstrated outcomes that are directly relevant to resolving national and international conflicts and improving the quality of life in society (Assimakis & Dillbeck, 1995; Cavanaugh & King, 1988; Davies & Alexander, 2005; Dillbeck, 1990; Dillbeck et al., 1987, 1988; Gelderloos et al., 1990; Hagelin et al., 1999; Orme-Johnson, 2009; Orme-Johnson et al., 1988, 1990).

What the critics are asking for ultimately is a theory of science capable of integrating consciousness with all aspects of the physical, biological, and social sciences. This is still a ways off, simply because not enough is yet known about those fields, much less about consciousness itself. As science rapidly expands in all areas of knowledge through its objective methodologies, what is needed now is a theoretical framework and subjective methodology to explore the fundamental domains of consciousness. Arguably, the theoretical knowledge and the subjective technologies provided by Maharishi have done that (Hagelin, 1987, 1989).

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ESSAY

## Super-Psi and the Survivalist Interpretation of Mediumship

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**Abstract**—According to the survivalist interpretation of mediumship, the existence of discarnate persons provides the best explanation for the data associated with physical and mental mediumship. Others—advocates of what is often called the “super-psi hypothesis”—maintain that the data of mediumship may be at least equally explained in terms of living agent psi (ESP and psychokinesis). Many defenders of the survivalist interpretation of mediumship attempt to deflate the alleged explanatory virtues of the super-psi hypothesis by arguing that the hypothesis is unfalsifiable and lacks independent evidential support. My central contention in this paper is that these frequently encountered survivalist criticisms of the super-psi hypothesis are ultimately self-defeating to the case for survival from mediumship. To show this I first argue in some detail that the survivalist interpretation of mediumship is committed to a kind or degree of psi that is indistinguishable from what is required by the super-psi hypothesis. From this vantage point it can be shown that any attempt to impugn the explanatory virtues of the super-psi hypothesis on account of the kind or degree of psi it requires undercuts the argument for survival itself.

*Keywords:* super-psi—survival—mediumship—telepathy—clairvoyance—postmortem

### Introduction

Data collected from physical and mental mediumship constitute an important strand of ostensible evidence for postmortem survival. I will refer to such data as *M* evidence. *M* evidence includes significant veridical information about the life of some deceased person(s) possessed by the medium and communicated to sitters, as well as the medium’s knowledge of various postmortem facts ostensibly originating from the deceased personality, especially knowledge of events or activities in the lives of friends and family of the deceased. *M* evidence also includes data about how this veridical information is communicated to the sitter by the medium. The communications often reflect the linguistic features and personality traits of the deceased. Lastly, *M* evidence includes various physical phenomena frequently alleged to occur in séance settings (for example, materializations, levitations, apports).

Arguments from *M* evidence to postmortem survival have traditionally relied heavily on eliminating alternative competing explanations of *M* evidence. This is a reasonable strategy since an argument for survival from *M* evidence is best interpreted as an inference to the best explanation. As such its strength crucially depends on considering and sufficiently ruling out other relevant explanations of the evidence as having at least equal explanatory value. If the hypothesis of survival of death is the *best* explanation of *M* evidence, then the survival hypothesis must have a comparative explanatory superiority over various competitors with respect to this evidence. Competing explanations of *M* evidence include ordinary culprits such as chance coincidence and fraud, but the most exotic and perhaps most challenging alternative explanation has been the so-called “super-psi” hypothesis.<sup>1</sup>

Roughly stated, the super-psi hypothesis attempts to explain *M* evidence in terms of psychic functioning among living persons, that is, in terms of living agent ESP (telepathy, clairvoyance, retrocognition) and psychokinesis (PK). It is widely held that the survivalist interpretation of mediumship must attribute some degree of psychic functioning to living agents, but, whereas the survivalist interpretation of mediumship also posits the psychic influence of discarnate spirits, the super-psi hypothesis attempts to explain *M* evidence solely in terms of psychological and paranormal resources of living agents. The hypothesis is designated *super-psi* since it is commonly held that the exclusive appeal to living agent psi to explain *M* evidences would require a higher degree or more refined form of psi than living persons have demonstrated in other contexts, especially in controlled laboratory experiments. This point may be challenged of course, and arguably the term “super-psi” is laden with unwanted and misleading connotations.<sup>2</sup> Although I will follow the traditional terminology for much of the paper, I will attempt in the course of the paper to separate the conceptual wheat from the terminological chaff.

Advocates of the super-psi hypothesis contend that *M* evidence may be at least equally explicable in terms of living agent psi as by personal survival. The *prima facie* force of the appeal to living agent psi lies in (i) the independent evidence for living agent ESP and PK and (ii) central features of *M* evidence appearing explicable in terms of either ESP or PK.<sup>3</sup> For example, the data extracted from mental mediumship are suggestive of survival because some living agent has highly specific veridical information about a deceased person’s life. This information is the sort that the deceased person would be ideally situated to possess, and it is unlikely that the living person could have acquired the information by any ordinary means. However, veridical information may be acquired by ESP, so we might suppose that in these cases the information was acquired by some exotic mode of cognitive functioning among living persons. This would eliminate the need to postulate survival. Explanations in terms of living agent psi get an additional boost once we recognize that, even on the survival hypothesis, some of the data of mediumship requires a degree of living agent psi. For example, mediums must exercise telepathy to engage in communications with discarnate spirits.

Moreover, in some prominent cases of mediumship there is evidence that indicates that the medium is at times in telepathic rapport with the sitter or with others at a distance.<sup>4</sup> This naturally raises the possibility that the medium acquires *all* her veridical information about the deceased by means of telepathic interactions with the minds of living persons. And even where telepathy with other living persons can be ruled out, there remains the possibility that veridical information was acquired by means of a broader repertoire of psi functioning, e.g., clairvoyance or retrocognition.

Not surprisingly, many survivalists have attempted to deflate the purported explanatory virtues of the super-psi hypothesis as part of their argument for or defense of the survivalist interpretation of *M* evidence. Since most survivalists affirm the reality of living agent ESP and PK, they typically target (ii) above, the idea that we can adequately explain *M* evidence by restricting ourselves to psi functioning among the living. There are different reasons why the super-psi hypothesis might be inadequate as an explanation of *M* evidence, even if we concede some degree of living agent psi. One common strategy is to argue that living agent psi can explain *M* evidence only if we are prepared to countenance a degree or form of psi for which there is no independent evidence and which seems invulnerable to any sort of counterevidence. If the super-psi hypothesis is evidentially impoverished and unfalsifiable, then it has little going for it as an explanatory competitor to the survival hypothesis.

In the present paper I will show that these frequently encountered criticisms of the super-psi hypothesis are both misguided and self-defeating for empirical arguments for survival from the data of mediumship. In the first three parts of the paper I explore the logical relationship between the survivalist interpretation of mediumship and psi functioning. I argue that if *M* evidences are good evidences for discarnate survival, then there are at least some discarnate persons and some living agents who exhibit a powerful and highly refined form of psi. I will argue that this *survival psi* is indistinguishable from what is required by the super-psi hypothesis in its attempt to explain *M* evidence exclusively in terms of psi functioning among living persons. From this vantage point, it can be shown that the charges of evidential deficiency and unfalsifiability are equally applicable to the survivalist interpretation of mediumship, so these objections cannot effectively be used to give the survival hypothesis an explanatory advantage.

## I. Mediumship and Discarnate Interactionism

### A. *The Survival Hypothesis*

We should begin by clarifying the survival hypothesis. As a first approximation, the survival hypothesis asserts the continued postmortem existence of a formerly living agent, typically as a discarnate or disembodied spirit.<sup>5</sup> I will be concerned with this specific and widespread notion of *discarnate* survival. The survival hypothesis attempts to explain some range of observational data in terms

of the intentions, memories, beliefs, and desires of a discarnate person, where these persisting mental states have continuity with the psychological life associated with some formerly living person. Of course, empirical arguments for survival do more than argue for the *existence* of surviving persons. After all, surviving persons might exist and yet have no causal connections to the world of embodied persons. Survival arguments postulate surviving persons to explain observable phenomena. So they presuppose that surviving persons causally interact with living persons and the physical world at some level, communicating with us, acquiring empirical knowledge of happenings in our world, and in some cases bringing about physical effects in our world. I will refer to this as *discarnate interactionism*. A survival hypothesis would seem to entail discarnate interactionism.

To be clear, the claim that a survival hypothesis entails discarnate interactionism is distinct from the claim that discarnate *survival* entails discarnate interactionism. It is logically possible that discarnate interactionism is false, even if human persons survive death as discarnate persons. Persons who survive death as discarnate entities might occupy a world constructed solely from their stock of memories and desires.<sup>6</sup> This idea is intelligible I think even if such persons never have knowledge of what is happening in the physical world, never communicate with the living or other discarnate persons, and never bring about effects in the physical world. Interactionism is nonetheless a requirement for the *hypothesis* of discarnate survival. The survival hypothesis relies on a range of observational data that are taken to be the effects of discarnate persons, but this can only be so if discarnate interactionism is true.<sup>7</sup>

### *B. The Data of Mediumship and Interactionism*

A survivalist interpretation of mediumship involves postulating the postmortem survival of the person as the best explanation of the data taken from cases of ostensible mediumship. One of the reasons why mediumship is fascinating is that it provides a rich illustration of how the survivalist hypothesis is committed to discarnate interactionism, and it is arguably the high degree of interactionism involved in mediumship that makes its data provocative and some would say compelling as evidence for survival of death.

One tier of this interaction is cognitive in nature. According to mental mediumship, discarnate persons possess the ability to communicate with living persons by directly sending information to the mind of the medium or otherwise causally influencing the mind of the medium. The content of such communications often involves ante-mortem facts from the life of the deceased and so entails that deceased persons retain memories from their embodied state.<sup>8</sup>

- (m1) In the Rev. and Mrs. Sutton's sittings with Boston medium Leonora Piper in December 1893, Mrs. Piper's control Dr. Phinuit communicated detailed information about the Sutton's recently deceased daughter Katherine (nicknamed Kakie)—information ostensibly originating from

Kakie herself. She provided details concerning her fatal ailment and physical condition shortly before death, her nicknames for her brother, sister, and favorite rag doll, her love for riding horses, daily routines, and the book from which her mother read during Kakie's illness (Hodgson, 1897–1898: 484–494).

- (m2) Beginning in March 1892, a personality claiming to be the recently deceased George Pelham (G.P.) manifested at sittings with Mrs. Piper. The G.P. communicator regularly disclosed the names of many of his personal friends and intimate details of his associations with them throughout his life (Hodgson, 1897–1898: 295–335, 413–441). For example, G.P. recalled going to college with a particular sitter's son, and he provided a correct description of the sitter's summer home where G.P. had once visited (Hodgson, 1897–1898: 300, 329, 457–458). On another occasion, G.P. recalled a very specific conversation with the daughter of a particular sitter (Hodgson, 1897–1898: 297–298, 329).
- (m3) In January 1939 a "drop-in" communicator named Runolfur Runolfsson (Runki) appeared in a sitting with Icelandic medium Hafstein Bjornsson. Although Runki had appeared in sittings with Bjornsson before, on this occasion Runki provided various details of his life: his full name, age and place of residence at the time of his death, some of the details of conversations with friends the night of his death, and the circumstances of his death, which involved falling asleep near the sea and being carried away by the tide (Haraldsson & Stevenson, 1975).

Other mediumistic communications have postmortem content and so presuppose—on the survivalist interpretation of the data—that deceased persons, while in their discarnate state, acquire information about persons and events in our world.<sup>9</sup> For example, they exhibit knowledge of persons present at the sittings, what sitters say during the sittings, current events, and events that have taken place on earth since their death. In some cases, they even exhibit apparent knowledge of future events.<sup>10</sup>

- (m4) In Mrs. Piper's sittings with friends of G.P., G.P. regularly identified friends who came as sitters, and he stated the activities of family members and friends on particular occasions away from the sittings (Hodgson, 1897–1898: 305–307, 413–418). For example, G.P. correctly reported that on a particular day his father took a picture of G.P. to a photographer to have it copied, and on another occasion that his father took a book of poems to the printer to be copied (Hodgson, 1897–1898: 304, 414). G.P. also claimed to have seen his mother remove a pair of studs from his clothes and give them to his father. He was also aware that his father had sent the studs to G.P.'s friend John Hart (Hodgson, 1897–1898: 297). In a sitting on December 5, 1892, G.P. reported that he had seen his friend Jim Howard visit a man named Fenton and engage in a conversation about G.P. In the same session, G.P. said that Jim Howard was reading at the time of the sitting (Hodgson, 1897–1898: 422–423).

- (m5) Kakie Sutton identified her father and mother at the sittings, noticed her father's absence at one sitting, was aware of the objects presented during the sittings, and knew the type of flowers laid on her casket (Hodgson, 1897–1898: 485–486, 489).
- (m6) When the Richard Hodgson communicator emerged at sittings with Mrs. Piper, he often revealed knowledge of events and the activities of friends that had taken place since Hodgson's death. For example, at sittings where Hodgson's friend Miss Margaret Bancroft was present, the Hodgson communicator indicated that he had visited Bancroft at her home and attempted to communicate with her after his death. He provided details of what Bancroft was doing at particular times on particular dates, the company she was keeping, and what words were exchanged (James, 1909: 51–52). At subsequent sittings, the Hodgson communicator exhibited awareness of various changes that had taken place at Bancroft's summer home since Hodgson's death (e.g., new wallpaper, a new bathhouse, and new pier and platform) (James, 1909: 59).
- (m7) On September 24, 1931, in a sitting with Rev. W. S. Irving, Gladys Osborne Leonard's control Feda relayed information ostensibly originating from Mrs. Dora Irving (the deceased wife of Mr. Irving). The Dora communicator had been asked at a prior sitting to visit the house of Mr. and Mrs. Stansfield—Mr. Irving's cousins—so that a book test could be conducted.<sup>11</sup> She provided an accurate current description of the house and some of its contents. She was also aware that a conversation involving reference to “rollers” had recently been held in one of the rooms of the house. And, of course, in accordance with the book test the Dora communicator selected a particular book from a bookshelf and said that a picture or portrait of significance to Mr. Irving and which would remind him of a portrait he has at home would be found on a specific page. She had identified a portrait of Mr. Irving's grandfather. A different portrait of his grandfather was at the residence of his sister where he had been staying (Besterman, 1932).
- (m8) Rev. David Kennedy documented with considerable detail a large number of ostensible communications from his deceased wife Ann during a 6-month period after her death (Kennedy, 1973). The communications came through a dozen different mediums, including Albert Best and Mrs. Lexie Findlater, sometimes at sittings and at other times during impromptu telephone calls. On one occasion Mrs. Findlater telephoned Rev. Kennedy and informed him that Ann had impressed her to call Kennedy at that very moment and to tell him, “Get out now and use the old notes” (Kennedy, 1973: 45). Rev. Kennedy had fallen asleep and would have been late for his evening church service had he not been awakened by the telephone call at that moment. On another occasion, Albert Best telephoned Kennedy and provided him with information, ostensibly from Ann, about the content of specific chapters of a book

Kennedy was writing. In another phone conversation Best told Kennedy that Ann said that she was with her husband 10 minutes prior and knew that he was reading about a particular person, Rev. Drayton Thomas, at the time (Kennedy, 1973: 105). In another phone conversation, the message from Ann concerned the whereabouts of clean clerical collars, which Kennedy was looking for at the time of the phone call. Ann not only knew where the collars were located in the house but she knew that Kennedy was going to be giving a memorial service for a specific person later that evening (Kennedy, 1973: 105–106). On a particular day Best phoned Kennedy and said that Ann had just been watching her husband doing laundry and that he had put too much soap in the wash, resulting in an overflow of suds. She also knew the specific items of clothing that were in the wash (Kennedy, 1973: 105–106). On another occasion Ann claimed to have watched Kennedy cook fish earlier in the day. She knew the kind of fish and added that the pot, which Kennedy had placed in the cupboard, wasn't properly cleaned (Kennedy, 1973: 69–70). In other communications through Best, Ann revealed that her husband had withdrawn £45 from his bank account earlier in the day (Kennedy, 1973: 115) and that the lights on Kennedy's car needed to be replaced (Kennedy, 1973: 125–126).

It is worth emphasizing that most survivalists include postmortem knowledge in the data of mediumship and typically as having significant evidential value. There is a very strong presumption in favor of doing so. First, postmortem knowledge is often conveyed together with veridical claims about the ostensible communicator's ante-mortem existence. The data in such instances mutually reinforce postulating an actual discarnate agent with the memories and interests of the deceased. Secondly, postmortem knowledge is at least implicit in traditional mediumistic communications, for ostensible communicators not only convey veridical information about their ante-mortem existence but they are *responsive* to the statements, questions, and gestures of the sitters, as well as aware of objects presented at the sitting. This presupposes that discarnate persons have some knowledge of what is occurring in the physical world. Hence, even if we exclude postmortem knowledge of perceptually remote facts, the responsive and interactive nature of mediumistic communications during actual sittings makes postmortem knowledge an essential feature of the data of mediumship.

But many paradigmatic cases of mediumistic communication involve more than a series of cognitive transactions. In cases of trance mediumship, we must suppose that discarnate persons are not merely knowers but also agents who bring about physical effects, for instance temporarily controlling the vocal cords of the medium or animating her body. So there must be a causal chain originating from the discarnate person and terminating in some physical event. In Mrs. Piper's mediumship, communicators (for example, Pelham and Hodgson) often communicated by way of automatic writing, trancelike states during which Mrs. Piper was capable of writing messages from alleged spirits. Phinuit controlled Mrs.

Piper's voice and gestures, as did other communicators on occasion (Hodgson, 1897–1898: 331). Physical effects are even more pronounced in cases of physical mediumship where discarnate spirits allegedly bring about raps and knocks on tables and walls, as well as more large-scale physical effects like levitating tables, materializations of objects, apports, and disembodied voices. The physical phenomena associated with the mediumship of D. D. Home and Eusapia Palladino are impressive examples of such physical effects.<sup>12</sup>

If taken as evidence of survival of death, the above data from mediumship imply the following theses:

- ( $\alpha$ ) Discarnate persons send veridical information to the mind of living agents.
- ( $\beta$ ) Discarnate persons have highly specific knowledge of living agents and the physical world acquired after their death.
- ( $\gamma$ ) Discarnate persons operate causally on the physical world.

I will refer to the conjunction of these three theses as *discarnate interactionism*. We can now state in a preliminary way a conditional claim that expresses the link between discarnate interactionism and the data of mediumship interpreted as evidence for survival.

- (1) If  $M$  phenomena are evidences for postmortem survival, then discarnate interactionism is true.

### C. Mediumship and Other Survival Evidences

Classical empirical evidences of postmortem survival make up a broad evidential territory. In addition to physical and mental mediumship, this territory includes apparitional experiences, near death experiences, and claims to past life memories. It should be relatively clear that while the survivalist interpretation of all the data of mediumship entails the conjunction of ( $\alpha$ ), ( $\beta$ ), and ( $\gamma$ ), not all alleged evidences of survival have this entailment. In fact, one might plausibly argue that some putative survival evidences do not entail even the disjunction of ( $\alpha$ ), ( $\beta$ ), and ( $\gamma$ ). For example, apparent “past life memories” common in spontaneous reincarnation-type cases do not entail either ( $\alpha$ ), ( $\beta$ ), or ( $\gamma$ ). While there are different models of reincarnation that may accommodate the transmission of memories, at least some of these are consistent with either there being no intermediate state of discarnate existence or there being an intermediate state of discarnate survival but during which discarnate persons do not communicate with the living, acquire knowledge of living agents or physical events, or bring about physical effects, other than perhaps the physically datable event of the re-embodiment of their consciousness. So we are not driven to conclude that just any putative survival evidence entails discarnate interactionism.

Nonetheless, three things seem relatively clear. First, the data of mediumship are important to the empirical case for survival, and the initial pull of  $M$  evidence

in the direction of survival may be attributed at least in part to features of mediumship suggestive of a high degree of interaction between discarnate persons and the living. Secondly, discarnate interactionism is present to some degree in other ostensible evidences of survival (for example, apparitional experiences, near death experiences). Mediumship arguably exhibits a very high degree of interactionism, but interactionism is not unique to mediumship. Third, the implications of interactionism in mediumship will apply to the case for survival in general if *M* evidence is part of the case for survival. Although there is some dispute among survivalists about the strength of *M* evidence, there is considerably less dispute that a strong case for survival will rely on several strands of survival evidence, including *M* evidence (Almeder, 1992: 255–256; Braude, 2003: 301–306; Griffin, 1997: 263–268). Discarnate interactionism seems unavoidable in the empirical case for postmortem survival.

## II. Discarnate Interactionism and Psi Functioning

Discarnate interactionism, though, raises an important question about how exactly discarnate persons are able to interact with living agents. How would they send messages to mediums, acquire knowledge about events taking place in our world, and bring about effects in our world?

Several prominent philosophers and parapsychologists have argued that discarnate persons, if they exist, would have to rely on exotic modes of cognition and causation in their communications with the living. These include Frederic Myers, Anthony Flew, H. H. Price, C. D. Broad, Terence Penelhum, and more recently Alan Gauld and Stephen Braude (Braude, 2003: 20–22; Broad, 1962: 409; Flew, 1953: 69; Gauld, 1982: 139, 145, 159, 231–232, 236, 241, 248–250; Myers, 1903: chap. 9; Penelhum, 1970: 30–36, 39–43; Price, 1953, 1957). The widely held intuition here seems correct. After all, discarnate persons lack the sensory perceptual systems used by living agents to acquire knowledge about their environment. They do not have bodies and so cannot affect the world by moving their bodies in some particular way. Their means of acquiring information, sending it, and causally influencing the physical world would have to be through powers or capacities that operate independently of our sensory perceptual system and body. And this is at least approximately how psychic functioning is understood. However, we should consider this in greater detail.

### A. *Discarnate Knowledge: Telepathy and Clairvoyance*

As explained above, the survivalist interpretation of mediumship indicates that discarnate spirits not only have knowledge of persons, places, and events from their earthly lives, but they also exhibit a range of knowledge of postmortem facts, usually involving people or places that were significant to them during their earthly life. The possession of knowledge of postmortem facts means that we cannot sufficiently explain the content of the deceased person's knowledge by appealing to knowledge that was naturally acquired during the deceased person's

life. Some of this postmortem knowledge reflects facts related to the séance sessions, as exemplified in the ability of communicators to identify sitters and objects presented at the sitting. A considerable amount, though, extends beyond the sitting to details about particular places, the location of certain objects, and what certain people were doing at particular times. So, even if we supposed that a deceased person relied solely on the sensory perceptual faculties of the medium to know the identity of the sitters and what they say during the sitting, no such appeal would be available to explain knowledge of perceptually remote facts. We would have to appeal to psi functioning.

There remains the interesting question, of course, as to what kind of psi is utilized to acquire postmortem knowledge, whether clairvoyance, telepathy, or both.<sup>13</sup> In the Ann Kennedy communications, telepathy would arguably be insufficient to explain some of the communications since neither Rev. Kennedy nor any other living person was aware of the facts at the time. For example, the Ann communicator correctly identified a dangerous defect in the electrical system in Rev. Kennedy's car, though it was unknown to Rev. Kennedy at the time (Kennedy, 1973: 125–127). She also disclosed the location of a very specific pair of earrings Rev. Kennedy had never seen before (Kennedy, 1973: 147–148). The “book tests” with Mrs. Leonard also suggest the exercise of discarnate clairvoyance. Other evidence, though, suggests telepathy with living agents. For example, the G.P. communicator claimed to have watched his father compose a letter to G.P.'s brother Frank on a particular day. Although G.P.'s father did not write such a letter, he had intended to do so on the day in question (Hodgson, 1897–1898: 315, cf. 307). On another occasion, sitters asked G.P. to provide a description of the activities of his friend Mrs. Howard at the time of the sitting. G.P. correctly described Mrs. Howard's activities from the previous day (Hodgson, 1897–1898: 305–307). Hodgson argued that errors of this sort suggest that rather than clairvoyantly acquiring information about the world, communicators retrieve information about the world indirectly from the minds of living agents, a process that sometimes results in distortions of facts.

### *B. Discarnate Communication: Telepathy and PK*

But discarnate psi functioning is needed not only to account for how the deceased acquires knowledge of postmortem facts, but also in accounting for how a discarnate person can *communicate* this information to the living. In discarnate cognition the causal chain operates *from* the world or human minds *to* the discarnate mind, but communications between the deceased and the living would seem to require causal chains that move in the other direction, from the mind of the discarnate person to the mind or world of the living. Even where discarnate knowledge concerns ante-mortem facts naturally acquired in life and stored in memory, communicating this knowledge to or through a medium would require psi functioning. A discarnate person by definition lacks the ordinary means of communicating by her own voice, writing, bodily gestures, or facial expressions.

Such a person must either utilize telepathy to directly send information to the mind of the medium or utilize PK to either (a) gain control of the medium's physical body and use it as a surrogate for speaking, writing, bodily gestures, or facial expressions<sup>14</sup> or (b) bring about some other physical effect that can serve as a means of communication. (a) and (b) of course highlight the importance of discarnate agency, which requires a causal chain that moves from a discarnate mind to the physical world. While living agents bring about physical effects by using their bodies, the discarnate agent's mental states would have to directly produce changes in the physical world. We find (b) in physical mediumship where the effects attributed to discarnate spirits include levitating tables, the materialization of objects, the movement of objects from one location to another, playing of instruments, and so forth. The automatic writing involved in trance mediumship is an example of (a). So discarnate psi is required for both the acquisition of post-mortem knowledge (by ESP) and the communication of anything a discarnate person knows (by ESP or PK).

While I have been arguing that the discarnate interactionism involved in the survivalist interpretation of mediumship entails discarnate psi (ESP and PK), I think we must also hold that this interactionism entails living agent psi. First, mediumistic communications generally suggest a telepathic link between the medium and the discarnate person. We might suppose that the medium acquires her knowledge of discarnate minds by telepathically scanning their minds or that the discarnate person is telepathically sending information to a medium's mind. In either case, living agent telepathy is operative. Secondly, the medium often initiates contact with the spirit world by way of spirit controls. The spirit controls are either discarnate persons or secondary personalities of the medium. If the former, the medium would have to rely on telepathy to initiate contact with them. If the latter, the medium must telepathically interact with communicators, for "the control" is supposed to be a discarnate spirit who telepathically interacts with other discarnate personalities. Third, as pointed out earlier, there are numerous instances where it is apparent that the medium has telepathically mined information from the minds of the sitters or other living persons. If we suppose that the medium has telepathic abilities that allow her to interact with discarnate persons, we just might expect that the medium would periodically pick up on information in the minds of sitters or other living persons. But again, this reinforces the contention that some mediums exhibit a significant degree of psi functioning.

So we can now state a conditional claim that links discarnate interactionism and psychic functioning in discarnate and living persons.

- (2) If discarnate interactionism is true, then discarnate persons and some living persons exhibit psi functioning.<sup>15</sup>

The conjunction of (1) and (2) entails

- (3) If *M* phenomena are evidences for postmortem survival, then discarnate persons and some living persons exhibit psi functioning.

### III. Survival Psi and the Super-Psi Hypothesis

So the survivalist interpretation of mediumship requires psi functioning in both (deceased) discarnate persons and living agents. Suppose we designate the psi required by the survivalist interpretation of mediumship *survival psi*. A fairly important question arises at this point. Is survival psi something in the neighborhood of the psi required by the so-called “super-psi hypothesis,” that is, the psi needed to explain putative *M* evidence if we limited ourselves to psi functioning among the living?

#### A. Physical and Mental Mediumship

In the case of phenomena manifested in physical mediumship, I think it is difficult to argue that survival psi is less potent or less refined than super-psi. The psi in each case would be PK, and the effect being measured is the same. It is only the postulated *source* that is different, whether it is a living or deceased person. If we suppose that a discarnate spirit is levitating a 25-lb table, we must minimally attribute to him PK powers sufficient for bringing about this effect. But we would have to postulate no more than this to account for a living agent levitating the same table. If D. D. Home utilized PK to play songs on the caged accordion, it would be a no less refined exhibition of PK for a discarnate spirit to do so. In fact, the discarnate person’s exercise of PK in this context is arguably a more refined form of psi functioning. When discarnate spirits produce physical effects as a means of communicating with the living, these effects are often *responses* to the actions or words of living agents. In that case, the discarnate person would have to rely on knowledge of what living persons are doing or saying at the time and then produce the appropriate effect, be it knocks or raps on tables or playing some particular song on an instrument (see Penelhum, 1970: 53). This would have to be done consistently to produce an intelligible series of responses. But, as already argued, if a discarnate person knows what is happening at any moment in the physical world, he must utilize ESP to acquire this knowledge. So a discarnate person’s PK effects (if responses to the words or actions of living persons) would have to be coordinated and calibrated with an efficacious ESP, whether telepathy or clairvoyance.

Mental mediumship presents a more complex set of issues. In (m4)–(m8) above, the communicators have detailed knowledge of a wide range of facts about objects in different physical locations and events or activities taking place at different locations and times. For example, Ann Kennedy (whose ostensible communications were cited in [m8]) had knowledge of events taking place in different places on earth. The content of the communications involves highly specific information about the location of objects and activities of people, some of which were not even known to Rev. Kennedy at the time of the communications. Ann describes herself as being “there” at particular locations or “with David” at specific times. Her *presence* must have been quite regular because she exhibits

knowledge of facts not as isolated events but nested within a larger chronologically accurate narrative. She not only knows *that* certain events happened but she knows the temporal *order* of many events. The deceased person has what we might call “narrative knowledge” of various postmortem facts. This is also suggested by prolonged conversations between discarnate persons and sitters in which the former are capable of keeping track of the conversation and sometimes recalling the content of earlier sittings.

The first thing to see here is that if a living agent acquires knowledge of some target fact *f* by way of ESP, it will plausibly take the same degree of ESP for a discarnate person to acquire knowledge of *f*. If Mrs. Piper accesses information about Miss Bancroft’s summer home (m6) or the activities of George Pelham’s father or friends on a particular day (m4), *prima facie* it seems that a discarnate person must have ESP powers of the same degree to acquire knowledge of the same facts, that is, assuming—as seems reasonable—that discarnate persons operate in the same causal nexus as living persons. Secondly, as illustrated in the mediumship of Mrs. Piper, Mrs. Leonard, and the mediums consulted by Rev. Kennedy, the clairvoyance (or telepathy) needed for acquiring knowledge of specific postmortem facts is extraordinary, and this seems true whether it is the product of psi functioning in discarnate persons or psi operating solely in living persons. In either case, psi must be utilized to acquire knowledge of the same set of facts, including their systematic and chronological relations. Psi would have to be extremely powerful and sufficiently refined to consistently produce such results.

### *B. The Problem of Crippling Complexity and Goldilocks Psi*

The above points can be reinforced and developed in the light of what Stephen Braude has designated the problem of “crippling complexity” (Braude, 2003: 86–95). The efficacious exercise of our abilities or capacities is conditioned, limited, and even prevented by a variety of causal influences. Like other kinds of abilities or capacities, there are constraints on psi functioning imposed by the larger network of causal interactions in which psi is embedded. More specifically, like normal abilities, psi functioning would be subject to various kinds of interference from other causal chains in the world. As Braude has argued, psi “would be embedded within an enormously complex web of interactions, psi and nonpsi, overt and covert, local and global, and it would be vulnerable to equally potent interferences or checks and balances (including psychic defenses) within that network” (Braude, 2003: 87, cf. 89–90). The more complex the causal nexus is, the greater the number of obstacles that must be circumvented for the efficacious exercise of psi on particular occasions. To accomplish this, psi would have to be quite powerful, and given the frequency of the kind of mediumistic data illustrated above, psi would have to be extremely powerful on more than isolated occasions. But the efficacious exercise of psi, whatever its degree of potency, would be included in the network of causal interactions that potentially undercut psi functioning elsewhere. If psi functioning is widespread, there will be a vast array of

psychic factors that will regularly inhibit or impede psi functioning, or otherwise thwart the production of psi effects. So in addition to insulating itself from ordinary forms of interference, psi would have to be powerful enough to insulate itself from undercutting psychic influences. However, once we grant that psi has *that* degree of potency, we significantly increase the likelihood that it will interfere with various individual psychic efforts. Extremely potent psi runs the risk of being self-defeating.

Braude initially introduces the problem of crippling complexity to highlight a particular difficulty with the super-psi hypothesis, namely that the degree of psychic functioning among the living needed to explain ostensible survival evidences, especially the data of mediumship, is likely to be so strong that it runs the risk of defeating itself. Of course, Braude thinks the survival hypothesis faces a similar difficulty (Braude, 2003: 92). But this indicates a fundamental parity in the psi that must be postulated by these competing explanations of *M* evidence. If we suppose that there *is* consistent efficacious psi functioning, which procures the kinds of knowledge outlined above, this psi must be both extremely powerful and extremely refined. It must be powerful enough to overcome a vast array of potentially contravening conditions (including other psychic efforts); yet it must be refined enough not to disrupt psychic activities in the causal neighborhood. In other words, psi must not be too weak, and it must not be too strong. What is needed is *goldilocks psi*,<sup>16</sup> an efficacious, highly calibrated psi that can handle the problem of crippling complexity. Here is the crucial point: goldilocks psi is needed whether we are dealing with living agent psi or discarnate psi. As Braude says, “If deceased communicators exist and interact with the living, then presumably both they and the living contribute to the total underlying causal nexus. In that case, one would expect the deceased to confront the same sort of interferences that frustrate psychic activities among the living” (Braude, 2003: 94). This underscores why discarnate psi would have to be at least as powerful and refined as living agent psi.

Take the medium Albert Best’s knowledge of the specific portion and content of a book that Rev. Kennedy was reading 10 minutes before Best called Rev. Kennedy (Kennedy, 1973: 105). On the survival hypothesis, we must suppose that Kennedy’s deceased wife acquired the information using ESP and communicated the information to Best using ESP. On the super-psi hypothesis, we must suppose that Best acquired the information by ESP. But many of the obstacles or interferences that Best’s psi functioning would have had to overcome are obstacles or interferences that Ann Kennedy’s psi functioning would have also had to overcome. This reason for this is that (a) it is the same fact that is ostensibly known by Mrs. Kennedy and Albert Best and (b) some of the important factors that prevent an efficacious exercise of psi are relative to the fact(s) known by ESP, for example, the nature of the event, its duration, location, and specific time(s) of occurrence. This can be contrasted with defeating factors related to the person exercising ESP, for example, simple weakness of ability. I might fail to hear what someone says in a crowded room because my hearing itself is not very good, but it might be that at

the time the person speaks or at some point during speaking, there is increased chatter in the room which prevents me from hearing all or some of what is said. This illustrates how some kinds of interference are relative to the time and duration of an event. The psychic parallel should be clear. While psi may fail to be efficacious on occasion because of factors relative to the person's psi functioning, it might also fail because at or during the time of the event there is something like "increased chatter"—an array of causal influences operative at the time—that prevents an efficacious exercise of psi or otherwise interferes with psi functioning. So what we see here is that not only are there *general* obstacles to efficacious discarnate psi, but it is quite plausible to suppose that discarnate psi would face the same *specific* obstacles in particular cases that living agent psi would face. So however powerful or refined living agent psi must be to overcome these kinds of interferences, discarnate psi must be as powerful and refined.

### C. *Magnitude of Psi and Multiple Source Issues*

The survivalist is likely to point out, though, that restricting ourselves to living agent psi involves a more *extensive* use of psi and that this is a significant discontinuity between survival psi and super-psi I have neglected to consider. We can develop the point here by considering four kinds of knowledge involved in mental mediumship:

- (i) knowledge of a significant range of highly specific ante-mortem facts about the deceased person's life.
- (ii) knowledge of the temporal order and larger context of a multiplicity of ante-mortem facts.
- (iii) knowledge of highly specific postmortem facts about the lives of friends and family of the deceased.
- (iv) knowledge of the temporal order and larger context of a multiplicity of postmortem facts.

The mere survival of a person in any psychologically robust sense logically entails (i) and (ii), so a discarnate person would only need to utilize psi to acquire (iii) and (iv). However, if the veridical data produced in mediumship is explained solely by the operation of psi among living persons, then the medium would have to use psi also to acquire (i) and (ii), as well as (iii) and (iv). So we might conclude that, although discarnate psi is no less powerful than living agent psi with respect to (iii) and (iv), the super-psi hypothesis requires a more extensive use of psi by a living agent to account for the entire range of veridical data involved in mediumship. Psi functioning would have to accomplish more than its survival psi counterpart. We might suppose that this increased task complexity requires a more powerful or refined form of psi since it is harder to carry out a larger number of tasks than a smaller number. Of course, this assumes that psi would be something akin to a set of refined tasks, and that it is more difficult to carry out several tasks than a few. But psi might operate more like a magic wand, requiring little more

than an efficacious wish, in which case psi effects could be causally streamlined (see Braude, 2003: 11–12, 89–90). So there may be no task complexity required to acquire the kinds of knowledge in question. But we can restructure the argument here in terms of crippling complexity. If living agent psi must be utilized to acquire a broader scope of information, then—owing to the complexity of the causal nexus in which psi operates—it looks like there will be a greater number of obstacles for psi to circumvent. This will be a problem even if psi works like a magic wand, for the problem of crippling complexity is concerned with conditions that undercut psi functioning, whether psi functioning is simple or complex. If psi must be employed to acquire a robust body of information, which is in turn used to construct a convincing trance persona of the deceased, there are far more ways this can be frustrated. So it looks like the psi involved in the super-psi hypothesis is after all more extraordinary than survival psi given the problem of crippling complexity.

The problem is that the survival hypothesis is not merely committed to claiming that discarnate agents possess the kinds of knowledge in (i)–(iv) but—like the super-psi hypothesis—that the medium herself either possesses this knowledge or is an instrument for transmitting it to living persons. Discarnate interactionism entails that discarnate persons send (i)–(iv) through the medium or directly to her mind, but as already argued above a discarnate person would have to rely on psi for this kind of communication. Also, the medium herself would need significant telepathic abilities, either to receive information sent from discarnate persons or to send information to the mind of the discarnate person. So the survival hypothesis cannot avoid attributing a significant degree of telepathy to the medium. On the super-psi hypothesis, psi is required to account for the medium's possessing (i)–(iv), but psi is not required to explain the communication of this knowledge to others. For the survivalist, psi is required to explain how the discarnate person acquired (iii) and (iv), *as well as how the discarnate person communicates (i)–(iv) to others*. Discarnate persons may get away with a less extensive use of psi to account for what they know, but they will need a considerably more extensive psi repertoire to communicate this knowledge to the medium. The problem of crippling complexity suggests that the greater the magnitude of psi functioning the greater the need for something like goldilocks psi to avoid the Scylla of psychic impotence and the Charybdis of omnipotent self-defeat.

Of course, defenders of the survival hypothesis are likely to point out that other features of mediumship reveal that the super-psi hypothesis faces its own set of complexities, complexities that demand a form of psi that is more refined and potent than survival psi. A widely advertised problem for the super-psi hypothesis is accommodating mediumistic data that is drawn from multiple sources. For example, Alan Gauld notes that in some cases the medium's stock of knowledge would have to rely on the telepathic retrieval of information from more than one living mind or clairvoyance directed toward multiple sources of information, as opposed to a single causal chain between the discarnate person and the medium (Gauld, 1982: 55–56, 59–61, 70, 130–131, 139–140). We can accept Gauld's

point, though arguably an expanded repertoire of psi functioning, which includes retrocognition, might be able to avoid the problem of multiple sources. Following Braude, I would suggest that the challenge Gauld's "multiple sources" scenario poses is not increased task complexity but a more precarious navigation through the causal nexus. If the medium must rely on multiple sources of information, there are more opportunities for interference arising from the underlying causal nexus. Mediumistic success would require a very potent and refined form of psi functioning to overcome the increased likelihood of failure that is engendered by having to mine information from multiple sources. By contrast, if the medium's veridical information about the deceased originates from the deceased personality, all that is required is a single stable psychic link between the mind of the discarnate person and the deceased. It seems that the psi required by the super-psi hypothesis is more *super* than what is needed for the survival hypothesis.

The survivalist, though, faces a parallel difficulty. To account for the discarnate person's knowledge in (iii) and (iv) the survivalist will have to say that discarnate persons draw information telepathically and/or clairvoyantly from multiple sources. For example, Ann Kennedy's postmortem knowledge (m8) could not have all been telepathically acquired from the mind of her husband, as he was ignorant of some of the postmortem facts at the time they were communicated to him by the medium. Ann must have either drawn this information telepathically from other minds or clairvoyantly from multiple physical locations. This is also illustrated in several of the G.P. sittings with Mrs. Piper (m4) in which G.P. provided reports of what other people were doing at specific locations while the sitting was underway. Moreover, if we suppose that communicators telepathically acquire their knowledge of the sitters' questions and answers, communicators must be capable of telepathically drawing information from multiple minds at about the same time. In all these cases, communicators must integrate information from multiple sources. Why should the medium's act of mining information from multiple sources require a more potent or refined form of psi? "Narrative knowledge" requires both a consistent psychic link with the world and a potent process of acquiring and integrating information from multiple sources.

What Gauld in effect argues is that for the survivalist the causal chain involved in the *communication* of knowledge from the deceased to the living is less complex than what the super-psi hypothesis requires for the causal chain involved in the medium's *acquisition* of knowledge about the deceased. Or, to put this in terms of the problem of crippling complexity, the communication of knowledge from the deceased to the living (medium) involves a less precarious navigation of the causal nexus than the medium's acquisition of knowledge about the deceased. The problem, of course, is that the deceased person's acquisition of knowledge of postmortem facts will require a more precarious navigation of the causal nexus than the medium's communication of knowledge about the deceased to the living. And the discarnate person faces no fewer kinds of obstacles or obstructions in psychically acquiring knowledge of postmortem facts than the medium faces in psychically acquiring knowledge of ante-mortem facts about the deceased. On

the survivalist interpretation of mediumship, the discarnate person must use ESP to acquire postmortem knowledge about different locations, different people, and different events (sometimes concurrently), each of which is represented as parts of a coherent and temporally ordered narrative. On the super-psi hypothesis, the medium must do the same with reference to ante-mortem facts about the deceased person's life. In each case psi will have to be powerful enough to be efficacious, despite so many potentially undercutting causal factors at work in the world, but it will have to be refined enough to avoid becoming the prime undercutter of psychic activity itself. Each case requires goldilocks psi.

We see here that we can easily select features of survival psi, compare these features with features of the super-psi hypothesis, and as a result the living agent psi required by the super-psi hypothesis looks more extraordinary than survival psi. But if we switch our focus to other features of survival psi, such as the acquisition of knowledge of postmortem facts, survival psi begins to look as impressive as the psi required by the super-psi hypothesis, if not more impressive. Here is the crux of the issue. There is really no way to non-arbitrarily privilege any of the comparative features of survival psi and super-psi. It is not as if the psi involved in the medium's acquisition of ante-mortem knowledge about the deceased is more important than the psi involved in the deceased person's acquisition of various bits of postmortem knowledge. Both make up equally significant portions of the data of mediumship. Consequently, we cannot say that the psi required by the super-psi hypothesis must be more potent or more refined than survival psi. To be sure, I have suggested at points why survival psi might be more extraordinary than super-psi, but clearly we must exercise as much caution here as I have urged in connection with comparative judgments about the power and sophistication of super-psi. The main difficulty is weighing the relevant features of comparison. What we can say is that survival psi is a highly refined and efficacious sort of psi-functioning (what I have designated "goldilocks psi"), which is indistinguishable from the degree or kind of psi required by the super-psi hypothesis.<sup>17</sup>

#### IV. Deflating Deflationary Strategies

Having argued for a fundamental parity in the strength and sophistication of survival psi and the psi required by the super-psi hypothesis, we are now in a good position to consider and evaluate survivalist strategies for deflating the apparent explanatory virtues of the super-psi hypothesis. I will consider two of the more ubiquitous strategies encountered in the literature.

##### A. *Survivalist Deflationary Strategies*

According to one deflationary strategy, the problem with the super-psi hypothesis is that, independent of its alleged explanatory power with respect to *M* evidence, there is no evidence in support of such a hypothesis. C. J. Ducasse (1961: 191–199) raised this objection in the early 1960s in his response to E. R. Dodds's preference for explaining the veridical component of mediumistic

communications in terms of a complex form of telepathy among living persons. It is a standard objection in more recent works on survival (Almeder, 1992: 46, 52–53, 118–120, 226–227; Fontana, 2005: 104–107, 245, 339; Gauld, 1982: 15, 127–128, 248; Lester, 2005: 213–214; Paterson, 1995: 52–53, 159–160, 173–174, 182). For example, Alan Gauld (1982: 248, cf. pp. 130–131, 237, 239–240) has contended that the super-psi hypothesis postulates psi of “an extent and complexity for which there is no other warrant.” According to Robert Almeder, appealing to living agent super-psi to explain survival evidences is like proposing that a particular person robbed a bank even though we have no antecedent or independent evidence that the person in question exists (Almeder, 1992: 52). While some authors focus on the alleged lack of experimental evidence for super-psi, others speak generally of the lack of either experimental or non-experimental evidence.

The authors who raise this objection to the super-psi hypothesis minimally intend to argue that on account of this evidential deficiency the super-psi hypothesis is inferior to the survival hypothesis as a putative explanation of *M* evidence. Almeder (1992: 52) says, “before one can appeal legitimately to super-psi as an alternative way of explaining anything, one should have some empirical evidence that in fact super-psi exists.” It is unclear what “legitimately” means here, though one is left with the impression from the rest of Almeder’s discussion that a hypothesis cannot do *any* explanatory work unless it is antecedently supported by independent evidence. This seems unnecessarily stringent.<sup>18</sup> It would be more sensible to suppose that when we have two competing explanations for observational evidence, if the hypotheses are equal in all other respects, the superior hypothesis is one that already has something going for it epistemically in the way of independent evidential support.

The other common deflationary strategy contends that nothing could in principle count as evidence *against* the super-psi hypothesis, so it is unfalsifiable (Almeder, 1992: 52–53; Fontana, 2005: 110–111; Lester, 2005: 213–214). It is fairly clear that critics intend specifically to claim that the degree and kind of psi required by the super-psi hypothesis is such that nothing could count against its existence. The argument is developed in different ways. Almeder (1992: 53), for example, claims that a hypothesis that cannot be falsified is empirically meaningless and so cannot do *any* explanatory work. On the other hand, it is frequently held that a theory is *scientific* only if it is falsifiable, falsifiability being a common though problematic demarcation principle. In that case, this particular deflationary objection would be that the super-psi hypothesis is not a genuine scientific hypothesis, not necessarily that it is empirically meaningless. Of course, we might also cast the objection in more general terms. Falsifiability is *a* criterion to be employed in comparing rival hypotheses with each other. All other things being equal, a hypothesis that is falsifiable is to be preferred to one that is not. This allows, at least in principle, a deflationary argument against the super-psi hypothesis as an explanation *simpliciter*; not merely an argument against the scientific status of the super-psi hypothesis.

The crucial point to see here is the general structure of deflationary strategies. They posit some condition, *c*, that is necessary for a hypothesis *h* to do certain explanatory work, deny that the super-psi hypothesis satisfies *c* because of the degree or kind of psi it requires, and then argue that the survival hypothesis satisfies *c*. Consequently, the survival hypothesis has an explanatory edge. Some deflationary strategists seem to think the satisfaction of *c* is necessary for *h* to have *any* explanatory power. That seems unnecessarily strong, so I will formulate the deflationary argument in a more modest form. For this purpose, we can use the generic notion of “significant explanatory efficacy.” I will assume that in any strong inductive argument from evidence *e* to hypothesis *h*, where *e* renders *h* more probable than not, *h* will have significant explanatory efficacy, though arguably the notion applies also in cases where *e* merely confirms *h* by adding to the probability of *h*. If my subsequent argumentation about deflationary arguments is cogent, it will apply equally to stronger versions of the deflationary argument that contend that the super-psi hypothesis has no explanatory power at all.

The deflationary argument may be formally stated as follows:

- (4) A hypothesis *h* has significant explanatory efficacy only if (a) *h* is independently supported and (b) *h* is falsifiable.
- (5) The super-psi hypothesis postulates a degree or kind of psi that is neither independently supported nor falsifiable.

So

- (6) The super-psi hypothesis does not have significant explanatory efficacy.

### *B. Two Initial Problems*

There are two closely related initial problems with the deflationary argument. Both issues affect the soundness of the argument.

First, even if there is no independent evidence for the existence of super-psi, it hardly follows that there is no independent evidence for the super-psi *hypothesis*, but it is the latter claim that is needed for the deflationary argument to be formally valid. Critics of the super-psi hypothesis move too quickly from the claim that there is no independent support for the existence of a certain degree or magnitude of psi (never precisely specified) to the conclusion that there is no independent support for the super-psi hypothesis. The so-called “super-psi hypothesis” is fundamentally the exclusive appeal to efficacious living agent psi to explain ostensible evidences for survival or—in this paper—specifically the data of mediumship. It is not the claim that living agents are omnipotent or omniscient, nor is it the supposition of efficacious living agent psi with a tacked on auxiliary assumption that psi is unlimited. The designation “super-psi” hypothesis is objectionable for precisely this reason. It imposes on the hypothesis a claim (this psi is super) that it does not make, which in turn presupposes a distinction (super-psi vs. ordinary psi) that the hypothesis does not make. Granted, if living agent psi provides a

good explanation for *M* evidence, then living agent psi must be potent and refined enough to produce *M* evidence. But it is far from obvious that the experimental and non-experimental data does not provide some evidence for this (see Braude, 2003: 14–15). Moreover, independent evidence for living agent psi does provide a confirmation of the special powers or capacities postulated by the super-psi hypothesis, even if there is no direct evidence that such powers or capacities have previously produced the specific results in *M* evidence. Suppose a rare Egyptian artifact is stolen from a particular museum that has the most sophisticated security system in the world. We may have no independent evidence that Joe Blogs has ever used his security hacking and heist abilities to pull off such a feat. However, Joe Blogs's track record of having pulled off several similar feats in the past adds to the probability of the supposition that he is the culprit in the present case. How *much* it adds to the probability of the hypothesis depends on the degree of similarity between the robberies. But the bald assertion that there is no independent support at all absent direct evidence for the identical effect is without warrant. So, in addition to the validity problem, we may be unwarranted in accepting (5).

Secondly, and parallel to the point raised above, even if the claim that super-psi exists cannot be falsified, it would not follow that the super-psi *hypothesis* cannot be falsified. Again, there is reason to doubt the validity of the deflationary argument as it stands. One of the problems here is that otherwise falsifiable theories may have unfalsifiable components or entailments (for example, absolute time in Newtonian mechanics). It may be quite difficult to siphon off the metaphysical components of empirical theories.<sup>19</sup> What falsifiability requires is that theories have *some* testable consequences, and the more the better. The super-psi hypothesis claims that human persons have efficacious psi powers. If these claims are falsifiable, then there is a fairly important aspect of the super-psi hypothesis that *is* falsifiable. Of course, even the falsifiable aspects of scientific theories depend on what auxiliary hypotheses are adopted. Since auxiliary assumptions are often unfalsifiable, other criteria must be used to evaluate them. So it may matter less whether claims about *super-psi* are falsifiable and more whether there are plausible auxiliary assumptions that permit efficacious and refined living agent psi to have testable consequences. This would permit a core element of the super-psi hypothesis to be falsifiable. Also, as Stephen Braude (2003: 17–19) has argued, even if a super-psi hypothesis (with various auxiliary assumptions) is compatible with all observation-statements, it does not follow that nothing at all could count against the hypothesis. The super-psi hypothesis may be unfalsifiable in the former sense, but falsifiable in the latter sense.

### C. A More Serious Problem

So there are at least two initial reasons to be concerned with the plausibility of the deflationary argument. But there is something more profoundly wrong with

the deflationary argument. The argument, designed to give the survivalist interpretation of mediumship an explanatory edge against its main exotic competitor, actually undermines the case for survival from mediumship. The deflationary argument generates a kind of logical or explanatory “blowback” against the survival hypothesis itself. Once we view the deflationary argument in the light of what I have argued about survival psi in the earlier parts of the paper, it should be clear that it *cannot* both be the case that (a) the survival hypothesis has significant explanatory power and (b) the super-psi hypothesis does not have significant explanatory power on account of the degree or kind of psi it requires. Consequently, the deflationary strategist faces a dilemma: either there is no good empirical case for survival from mediumship or the deflationary argument is unsound.

To set this up, recall that above I argued that the survivalist interpretation of mediumship entails discarnate interactionism and that discarnate interactionism entails survival psi, that is, psi functioning among discarnate persons and at least some living agents. I have avoided making the claim that survival psi is super-psi. In fact, I have tried to avoid speaking of *super*-psi altogether. The phrase “super-psi hypothesis,” as I have used it in this paper, simply designates the hypothesis that attempts to explain *M* evidence exclusively in terms of living agent psi. I have argued that *this* hypothesis requires goldilocks psi, a very refined and efficacious form of psi functioning among the deceased and the living that is capable of overcoming the problem of crippling complexity. I have also argued that survival psi entails goldilocks psi, and for this reason there is a fundamental parity between the psi required by the survival hypothesis and the psi required by the so-called super-psi hypothesis.

One of the difficulties facing the deflationary argument should be immediately apparent. If the survivalist demands independent support for the degree or kind of living agent psi required by the super-psi hypothesis before any such hypothesis may be invoked to explain *M* evidence, epistemic parity requires imposing a similar explanatory constraint on the survival hypothesis itself. And therein is the difficulty. The survival hypothesis is committed to survival psi, so the survivalist must either concede that survival psi is without parallel in the literature on spontaneous and experimental ESP (Gauld, 1982: 145) or he must claim that—unlike the psi required by the super-psi hypothesis—there is independent, empirical support for survival psi. The first admission prevents the survival hypothesis from having superior explanatory power over *M* evidences. The second admission prevents the survivalist from sustaining his deflationary objection against the super-psi hypothesis. Any attempt to argue that there is independent evidence in support of survival psi will be evidence for goldilocks psi. This undercuts the contention that there is no warrant for belief in the psi required by the super-psi hypothesis, for a fundamental way of characterizing this psi is as goldilocks psi. Even if the psi required by the super-psi hypothesis is greater than survival psi, surely evidence for the latter would be inductive evidence for the former existing, even if there is no direct evidence for it. The survivalist simply cannot plausibly conclude

that there is *no* evidence for super-psi and yet maintain that his own hypothesis involves a degree or kind of psi that is independently confirmed.

But the survival hypothesis actually compounds the difficulty. The survival hypothesis appeals to discarnate persons (significantly continuous with formerly living persons) to explain *M* evidence. However, if as Robert Almeder says, it is necessary to have “some evidence of the causes cited in offering an explanation” (before employing such explanations), then we cannot appeal to discarnate persons to explain observational data unless we have independent, empirical reasons to suppose that such entities exist. Almeder (1992: 52) says that there must be independent, empirical evidence that Jones exists before we can postulate that Jones is the robber of Rabun Gap Bank. By parity of reasoning, though, there must be independent empirical evidence that a certain entity exists before we appeal to that entity to explain observational data. So it is not just that the survival hypothesis postulates capacities and powers the existence of which stands in need of independent, empirical support, but we would need independent, empirical support for the existence of the kinds of entities who ostensibly have these capacities and powers. But independent of the purported explanatory virtues of the survival hypothesis, it would seem that the empirical evidence for discarnate persons is fairly thin.

Now whereas the super-psi and survival hypotheses each postulate the existence of goldilocks psi (which, let us suppose, is without independent evidence), the super-psi hypothesis would seem to at least have the virtue of not postulating *additional* entities for which there is little if any independent evidence. If the absence of independent evidence for super-psi is a problem, surely the problem is compounded by the lack of independent evidence for the kinds of entities that ostensibly exhibit survival psi. The thrust of the deflationary demand for independent evidence suggests a kind of explanatory conservatism: only appeal to antecedently known entities and processes, until there is sufficient evidence for expanding our ontological inventory. The survival hypothesis seems to doubly violate this epistemological disposition.

Something similar will hold for the charge that the super-psi hypothesis is unfalsifiable. Even if this is true it is difficult to see how the objection can be sustained in a way that does not apply to the survivalist interpretation of mediumship. If the super-psi hypothesis is unfalsifiable on account of postulating goldilocks psi, so is the survivalist interpretation of the data of mediumship. Indeed, given that the survival hypothesis appeals to discarnate persons, it is in double trouble, for it is not obvious what sort of observation-statement can falsify the supposition that there are discarnate persons. To be clear, I am not arguing that the survivalist hypothesis cannot be falsified. Indeed, in the weak sense of falsifiability suggested earlier, the survivalist interpretation of mediumship looks falsifiable (as does the super-psi hypothesis). The challenge the survivalist deflationary strategist faces is to find a formulation of falsifiability that includes the survivalist interpretation of mediumship but excludes the super-psi hypothesis.<sup>20</sup>

## V. Conclusion

In this paper I have examined the relation between the super-psi hypothesis and the survivalist interpretation of the data associated with mental and physical mediumship. In Part I of the paper I argued that if the data of mediumship are evidence of postmortem survival, then a very strong interactionist thesis about discarnate persons and their relation to the world must be true. In Part II I argued that this interactionist thesis requires psi functioning in discarnate persons and living persons. The heart of the argument was developed in Part III, where I showed that the survivalist interpretation of mediumship is committed to the same kind or degree of psi functioning as postulated by the super-psi hypothesis. If this is true, then certain attempts at deflating the explanatory virtues of the super-psi hypothesis are self-defeating. In Part IV I showed that deflationary strategies undercut the case for survival from mediumship if they attempt to impugn or discredit the super-psi hypothesis on the grounds that super-psi is unfalsifiable or without independent support. To the extent that other ostensible evidences for postmortem survival are also committed to discarnate interactionism, a similar conclusion will follow. The sensible course of action I believe is for the survivalist to reject these particular deflationary strategies and attempt to defend the survival hypothesis on other grounds.

## Notes

- <sup>1</sup> The terminology “super-psi” may be traced to Hornell Hart’s designation “Super-ESP” in Hart (1959), but the idea of explaining mediumistic communications in terms of extraordinary living agent psi originated in the late 19th century among members of the British and American societies of psychological research.
- <sup>2</sup> Stephen Braude nicely addresses this point. See Braude (2003: 12).
- <sup>3</sup> Advocates of the survival hypothesis typically concede that living agents exhibit some degree of telepathy or clairvoyance. They have had to concede this on the basis of their own appeal to mediumistic communications, which seem to require telepathic abilities on the part of the medium. See Price (1966).
- <sup>4</sup> We find this, for example, in the mediumship of Leonora Piper. One kind of case here is where the medium’s information about the deceased is incorrect, but the incorrect beliefs correspond to incorrect beliefs held by the sitters (see Myers, 1889–1890: 568–571, 581–583; Podmore, 1910/1975: 165–166). In other cases, obviously fictitious communicators or controls appear at séances, but their identities happen to correspond to what sitters were thinking prior to the séance (see Sidgwick, 1915: 85, 297ff, 437–448). It seems implausible that these kinds of specific correlations would be merely fortuitous.
- <sup>5</sup> While a discarnate person may be a wholly immaterial substance or mind persisting without any physical substrate or physical properties, it may also be understood as possessing some physical properties (e.g., spatial location), or even an ethereal or astral body. On the range of possible physical properties of the soul, see Zimmerman and Van Inwagen (2007: 23–28). Hence, a “discarnate person” on my understanding lacks a conventional body.
- <sup>6</sup> For a development of this conception of the environment of disembodied survival, see Price (1953, 1957).

- <sup>7</sup> This shows that a well-formulated survival hypothesis needs as its content not merely a claim about a person surviving death as a discarnate spirit, but something more specific that will lead us to expect discarnate interactionism. If we postulate the persistence of persons with roughly the same sort of desires and intentions as they had in life, then we should expect the deceased to attempt to communicate and interact with our world.
- <sup>8</sup> For a good overview of such evidences, see Broad (1962: 253–383); Gauld (1982: chaps. 2–8); and Braude (2003: chaps. 2, 3).
- <sup>9</sup> H. F. Saltmarsh analyzed 142 sittings with medium Mrs. Warren Elliott and found that postmortem statements were just as frequent as ante-mortem statements. They were also slightly more accurate than ante-mortem statements, 74% to 66.3%. See Saltmarsh (1930–1931: 91–92); and Broad (1962: 317–320).
- <sup>10</sup> The mediumship of Mrs. Leonard provides examples of this. See Thomas (1939); Salter (1926); and Smith (1964: chap. 7).
- <sup>11</sup> The “book test” involved a communicator disclosing the contents of a particular page in a particular book at a specific location (residence and location on a particular bookshelf), where these details were unknown to the medium and sitters.
- <sup>12</sup> See Braude (1997: 53–129) for an excellent summary of the phenomena encountered in the physical mediumship of Home and Palladino.
- <sup>13</sup> For an analysis of clairvoyantly acquired postmortem knowledge, see Stephen Braude (2009: 195–210).
- <sup>14</sup> Frederic Myers designated this “telergy” and distinguished it from a discarnate person’s telepathic influence on the medium. See Broad (1962: 298–299).
- <sup>15</sup> It might be argued that discarnate persons are only discarnate in a weak sense. They lack a conventional body, but they are not wholly immaterial persons. Discarnate spirits have bodies of a different sort, astral or ethereal bodies (Fontana, 2005: 422–425, 445–450). The communicators and controls of mediums sometimes state that they possess something analogous to a human body, and they use their own sense organs to acquire knowledge of our world and other deceased persons (see Broad, 1962: 280–285). It is plausible to suppose that it seems to these communicators and controls that they are embodied even if they are not, in much the same way that it seems to us that we are embodied in our dream states. However, even if we suppose that discarnate spirits have astral bodies, their interaction with the physical world would still have to be considered paranormal. Psi functioning is typically regarded as a mode of cognition or causation that does not depend on the physical body or its sensory system. This would be no less true for cognitive and causal powers grounded in an astral body.
- <sup>16</sup> In the children’s story “Goldilocks and the Three Bears,” a young girl named Goldilocks secretly enters the home of three bears and tastes their three bowls of porridge. The first is too hot, the second is too cold, but the third is “just right.” The term “goldilocks” is often used to signify unique or special conditions. I use the term here to designate psi that is just right, given the problem of crippling complexity.
- <sup>17</sup> Gauld concedes that survival theorists “must postulate what is in effect, super-ESP” (Gauld, 1982: 250, cf. pp. 139–140). Braude (2003: 20–21, 92–94) has developed this point.
- <sup>18</sup> Almeder’s position prevents us from ever acquiring knowledge of the existence of something first by way of inference to best explanation. This is simply unacceptable in both science and ordinary experience. See Braude (2003: 14–15).
- <sup>19</sup> For a discussion of how scientific or empirical theories have embedded ontologies that resist observation-refutation, see Wisdom (1972).

<sup>20</sup> Fontana (2005: 110–111) attempts this, but the effort is unsuccessful. Fontana claims that the super-psi hypothesis is not falsifiable, whereas the survival hypothesis is falsifiable. According to Fontana, a hypothesis *h* is said to be falsified on one of two conditions, *h* does not effectively fit with known facts or there is some other hypothesis *h\** that fits the known facts better. Fontana then claims that the survival hypothesis fits the known facts better than the super-psi hypothesis. However, if what Fontana says is true, the super-psi hypothesis is falsified, from which it self-evidently follows that the super-psi hypothesis is falsifiable.

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ESSAY

## Perspectival Awareness and Postmortem Survival

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**Abstract**—Critics of survival research often claim that the survival hypothesis is conceptually problematic at best, and literally incoherent at worst. The guiding intuition behind their skepticism is that there’s an essential link between the concept of a person (or personality or experience) and physical embodiment. Thus (they argue), since *by hypothesis* postmortem individuals such as ostensible mediumistic communicators have no physical body, there’s something wrong with the very idea of a postmortem person, personality or experience. However, critics can’t simply beg the question and assert that physical embodiment is essential to personhood, personality, or experience, because the evidence suggesting survival is a *prima facie* challenge to the contrary. On the other hand, defenders of ostensible mediumistic communication need to explain how post-mortem awareness and knowledge of the current physical world can occur without a physical body that experiences the world and represents it accurately enough to ground veridical postmortem reports. This paper will first consider why survivalists face potentially serious problems in trying to make sense of apparent postmortem perception. Then it will consider a plausible—and arguably the only—way to deal with the issues. However, that solution turns out to be a double-edged sword. Ironically, the best way to deal with the problem of perspectival postmortem awareness may render the survival hypothesis gratuitous.

*Keywords:* postmortem survival—mediumship—ESP

### 1. Introduction

Critics of survival research often claim that the survival hypothesis is conceptually problematic at best, and literally incoherent at worst. The guiding intuition behind their skepticism is that there’s an essential link between the concept of a person (or personality or experience) and physical embodiment. Thus (they argue), since *by hypothesis* postmortem individuals such as ostensible mediumistic communicators have no physical body, there’s something wrong with the very idea of a postmortem person, personality or experience. Consider, for example, the following representative passage from A. R. Miller.

I find the very notion of *disembodied personality* logically inconceivable. A “person” is, essentially, a being which, among other things, perceives, acts, and thinks. Normally,

perception requires sense organs, action requires limbs, and thinking (in the broadest, Cartesian sense) requires a brain; I cannot *see* and read the billboard unless my eyes are open, I cannot *kick* the football without a leg, I cannot *imagine* Santa Claus without a cerebral cortex, and so on. In the total absence of such physical accoutrements, I cannot see how any of the sorts of activities constitutive of personhood are or could be possible. (Miller, 1998)

Although this line of thought is hardly outlandish, some versions of it are admittedly disappointingly glib. For example, Antony Flew's variant turns on the methodologically naive assumption that something is logically impossible if we can't form a mental image of it (see Flew, 1976, 1987, and the discussion in Braude, 1993, 2003; also, see Almeder, 1992). However, other accounts are more serious and raise genuine puzzles about postmortem existence—specifically, concerning the possibility and apparently *perspectival* nature of postmortem awareness of the physical world. In fact (and to his credit), Miller is one of those who recognizes that both advocates and critics of survival must address some interesting and complex issues here. Critics can't simply beg the question and assert that physical embodiment is essential to personhood, personality, or experience, because the evidence suggesting survival is a *prima facie* challenge to the contrary. On the other hand, proponents of survival (hereafter, *survivalists*) need to grapple with puzzles arising especially from cases of apparent mediumship. That's because mediumistic communicators often respond appropriately to and describe correctly—and, in fact, claim to experience—what's currently going on in the physical world. Of course, survivalists must endorse at least some of these occurrences in order to legitimate mediumistic communication as a source of evidence for their position. So they need to explain how postmortem awareness and knowledge of the current physical world can occur without a physical body that experiences the world and represents it accurately enough to ground veridical postmortem reports.

It may be surprising, then, that proponents of survival often have little to say about the relevance of physical embodiment to the manifestations of personhood and experience, apart from their efforts to deflect the more superficial and question-begging versions of the anti-survivalist critique. For example, readers will look in vain for a discussion or even acknowledgment of the issues in Fontana's recent book, widely (but incorrectly) regarded as a respectable defense of the survival hypothesis (Fontana, 2005; see Kelly, 2005, for a critical review). And in another—and much more conceptually sophisticated—work (Almeder, 1992), the instructive problems about perspective discussed below are missed entirely, although Almeder correctly targets some related issues (including the superficiality of Flew's position).

In this paper, I want first to consider carefully why survivalists face potentially serious, vexing, and largely unheralded problems in trying to make sense of apparent postmortem perception. Next, I want to consider a plausible—and, arguably, the only—way to deal with the issues. And finally, I want to show why that explanatory strategy is a double-edged sword. Ironically, the best way to deal

with the problem of perspectival postmortem awareness may render the survival hypothesis gratuitous.

## 2. The Perplexing Problem of Perspectival Postmortem Perception<sup>1</sup>

Generally speaking, the substantive problem at issue here is the following (see Penelhum, 1970, and Sorabji, 2006, for presentations of the relevant arguments). Our everyday visual and auditory sense perceptions are perspectival—that is, they present themselves to us relative to and from the specific perspective of our location in space. That’s why our experiences of seeing and hearing are always from a point of view. We see and hear things to the right, left, or straight ahead, and at a certain distance. Of course, we explain the perspectival nature of these experiences with reference to the fact that our sensory receptors occupy specific positions in space.

Now suppose it’s true, as survivalists maintain, that after death we may continue having such perspectival experiences in the absence of a body. And suppose further that some of those experiences are veridical—that is, that they provide accurate information about states of the physical world. How are survivalists supposed to make sense of that? Since in disembodied survival nothing is literally *at* (i.e., extended at) any relevant location in space, there is apparently no basis for the alleged reports (transmitted through mediums) of a postmortem individual’s perspectival awareness of what the living are doing or saying (i.e., things that normally can only be observed or experienced from certain points of view in space). Presumably, when a person’s body has decomposed (or at least ceased all organic functioning), nothing in space can anchor and provide the spatial orientation of a sensory experience.

Initially, it might seem as if survivalists have a way to avoid this apparent problem. Perhaps they need only say that postmortem individuals experience physical states of affairs *as if* they are perceived from a spatial position. After all, by hypothesis these individuals no longer have (functioning) sensory organs to mediate sensory experiences. So perhaps survivalists should say that perspectival postmortem experiences are at best only *ostensibly* sensory, not genuinely sensory. But what does that mean? Under one reasonable interpretation, it even seems to undercut the survivalist position. For without sensory information arriving at spatially oriented sensory organs, why should we say that with these experiences postmortem individuals are actually *gaining information* about a certain location? They seem, rather, merely to be *imagining* what’s going on at a location.

Some of the more astute writers on mediumship have, in fact, taken this position, or at least come very close to it. For instance, Una Lady Troubridge offered the following in connection with Mrs. Leonard’s mediumship.

... Feda employs a vocabulary of very limited extent wherein erudite psychological terms have no place. Beyond the occasional emergence of such non-committal spiritualistic terms as “I sense” or “I get an impression of,” Feda is content to tell the sitter that she “sees,” “hears,” “feels,” or “smells,” as the case may be, though the medium’s eyes are invariably closed and neither the sights, sounds, sensations nor smells described are perceptible to the sitter. (Troubridge, 1922: 369)

... there are certain aspects of the Fedra phenomena which leave me very doubtful as to whether these simple sensory terms convey any accurate analogy with the processes really involved. (*ibid*)

For example, Lady Troubridge reports that on one occasion Fedra described “to Miss Radclyffe-Hall with accuracy and in great detail a portrait of Miss Radclyffe-Hall herself.” Fedra correctly noted the coloring of the picture, the pose of the figure and hands, and the seriousness of the figure’s expression. According to Fedra’s own statements, she “*sees* this picture and is able to describe it at such length, [but] never apparently for a moment grasps the fact that the picture being described by her is a portrait, and a striking resemblance at that, of the very familiar sitter to whom she is speaking” (pp. 370–371).

Similarly, Lady Troubridge writes:

It is surely incredible that Fedra or anyone else should *see* a person minus their most striking peculiarity of features or colouring, and yet this must frequently be presumed to be the case if Fedra’s seeing is to be accepted at face value. I have myself known her purport to see clearly a communicator whose appearance she minutely described, giving a perfectly accurate account of his features, complexion, expression, including the fact that he was remarkably handsome and struck her as having what she most evidentially described as “a clear look,” but she remained to all appearance in ignorance that the most distinguishing features of his appearance were prematurely snow-white hair of remarkable abundance, and eyes of a peculiarly vivid blue. (pp. 371–372)

Parenthetically, I have to note that I’m unsure just how revealing this incident really is. Don’t many or most people attend selectively to those things that matter to them, and don’t the things that matter vary widely from one person to the next? I know many people, myself included, who routinely miss the color of someone’s eyes in favor of other traits that to them are more outstanding or meaningful.

At any rate, Lady Troubridge concludes that Fedra’s alleged “sensory impressions could only be hallucinatory . . .” (p. 369). Of course, there’s a venerable (if not exactly noble) tradition within psi research of speaking about veridical hallucinations—for example, in connection with apparitional experiences (see Braude, 1997, for a discussion of this). However, even if that locution is defensible and not an oxymoron—which is certainly debatable—Lady Troubridge seems to be using the term “hallucination” in its more customary sense, according to which any correspondence between the content of hallucinatory experiences and actual states of affairs is fortuitous.

But survivalists won’t want to treat all mediumistic perception reports as nonveridical in that sense. That is, they can’t treat communicators’ ostensible perceptions *generally* as corresponding only fortuitously to the states of affairs in question, because those experiences are supposed to undergird some of the true claims communicators make about the physical world. And those true claims comprise most if not all of the empirical support from mediumship for the survival hypothesis.

Let’s look at this more closely. Mediumistic communicators’ claims are often regarded as evidence of survival precisely because they suggest the postmortem,

disembodied, existence of ante-mortem personality and *its continuing awareness of and interaction with the physical world*. So when communicators respond appropriately to spoken sentences, or correctly describe what's currently going on either with the medium, sitters, or with more remote states of affairs, survivalists interpret that as evidence that a deceased person somehow survives bodily death and continues to be in touch with what's happening in the world of the living. In fact, the deceased's awareness of and interaction with the living is a *necessary condition* for mediumistic communication, at least of the sort documented since the early days of the Society for Psychical Research (SPR).

But this means that survivalists must interpret *in causal terms* the ability of communicators to respond appropriately to interlocutors and to make true claims about the current physical world. For example, they would say that when deceased communicator "Uncle Harry" correctly describes the current location of an object in a sitter's house, what enables him to make that claim is his awareness of the actual state of the sitter's house. In that respect, at least, survivalists understand some ostensible postmortem cognitive states to be analogous to ante-mortem perception. Ordinarily we would say that I perceive—rather than merely imagine or hallucinate—the table before me because my experience results in part from my interaction with the object I perceive. And not only that. Ordinarily we suppose that my ability to *correctly describe* the objects I perceive is not random or accidental. In fact, we suppose it needs to be explained in terms of lawlike *causal regularities* having to do both with properties of the objects perceived and the physical properties of my sensory system. For example, it's in virtue of those regularities that I'm generally able to describe green objects as green or rectangular objects as rectangular. Granted, if I instead hallucinate or imagine the table, my inner episode might be qualitatively identical to a genuine perception of the table. But if the experience isn't caused by the table before me, it's not a postmortem analogue to perception. Indeed, in the absence of relevant causal regularities between the object's properties and my own, it would seem to be a matter of sheer serendipity that I manage to describe the object correctly. So if postmortem communicators merely imagine or hallucinate things in the world, their alleged experiences would—at best—correspond only fortuitously to the states of the world they ostensibly represent. But that undercuts the principal basis for taking mediumship seriously.

So if survivalists know what's good for them, they *must* claim that mediumistic communicators can interact causally with states of the physical world in a way that results in their having non-hallucinatory (or non-imaginary), nonbodily, and perhaps quasi-sensory awareness of those states. And then we're back where we started; the question remains: In the absence of physical sensory receptors, how would a disembodied individual be able to correctly describe current physical states of affairs? What enables that individual to detect the causally relevant features of the object(s) correctly described? And what supplies the perspective from which the information is apparently received and from which veridical mediumistic claims seem to be made?

Several potential survivalist maneuvers clearly won't work here. For example, survivalists can't claim that the medium's body temporarily supplies the physical basis for a communicator's sensory perspective, and that this enables communicators to perceive what's happening in the physical world. For one thing, communicators report that they're still aware of events in the physical world even when they aren't interacting with a medium. And for another, communicators often report physical states of affairs at locations perceptually remote from the medium. Moreover, survivalists can't maintain that a secondary or astral body supplies the needed perspective, because in some survival cases information is provided about matters that can't be perceived from *any* position in space—for example, the contents of a page in a closed book. Must we conclude, then, that survivalists are committed to a process (postmortem awareness, with perspectival features analogous to those in ordinary sight and hearing) that, given the hypothesis of disembodied survival, seems to be incomprehensible or impossible?

One proposed strategy for preserving both logical coherence and veridicality is to posit telepathic causal chains between sitters (or remote others) and mediumistic communicators. For example, Lady Troubridge says she suspects

that in many instances where Feda describes persons and objects, she uses the term “seeing” merely as a habit of speech, and that the process involved may be more likely a series of impressions received by her telepathically one at a time, or collected by her telepathically one by one from some mind incarnate or discarnate, as the case may be. (p. 371) (see also Salter, 1921: 87ff)

Although this strategy seems intelligible, it, too, can't be generalized to cover all communicators' reports of apparent sensory experiences. That's because communicators sometimes accurately report physical states of affairs unknown at the time to any living person and which are subsequently verified. Mrs. Leonard's book tests offer prime examples.

### 3. Philosophers Weigh In

Because only a handful of philosophers have taken both a serious and well-informed interest in the conceptual problems of survival research,<sup>2</sup> it would be odd to speak of a philosophical consensus about the issues. Nevertheless, it's interesting that two sophisticated philosophers have tried to make sense of ostensible perspectival postmortem experiences in purely subjective terms.

In his well-known essay, “Survival and the Idea of ‘Another World’,” H. H. Price argued—contrary to the usual skeptical dismissals of survivalist claims—that the concept of a disembodied life subjectively similar to our own is at least intelligible (Price, 1953). He described how a dreamlike world of images could provide a postmortem individual with a first-person analogue to our subjective ante-mortem existence. And he suggested, further, that telepathic interactions between the deceased (including the telepathic production of apparitions) might furnish an analogue to objective relations and interactions between individuals in this world.

Now whether or not Price successfully demonstrates the intelligibility of a disembodied life in a next world, his conjectures are of no help to the survivalist in the present context. That's because Price doesn't explain how postmortem individuals manage to acquire veridical and apparently perspectival awareness of *this* world. In fact, Price makes no effort to explain how the deceased, locked into their own exclusively postmortem nexus of paranormal causality, interact with the living to produce *evidence* of their survival. Evidence of survival within a Pricean next world requires empirically discernable manifestations of postmortem existence—in particular, the deceased's continuing psychology (intentions, concerns, etc.). But that, in turn, requires some chain of causality *running in both directions* between the living and the deceased, allowing for mutual awareness and communication. But that's precisely what Price fails to posit, and without it, anti-survivalist interpretations of survival cases (including those positing nothing but living-agent or so-called "super" psi) seem to have a clear explanatory edge.

Price even appears to grant as much in another paper, "The Problem of Life After Death" (Price, 1968). In this paper he suggests that mediums might engage in a kind of "dreaming aloud," in which (as in normal dreaming) they imaginatively supply their own apparently perceptual perspective, and in which they occasionally acquire veridical information about this world by ESP. Of course, that's simply an appeal to the sort of refined living-agent ESP that many survivalists mistakenly argue is antecedently implausible. Later we'll consider more closely why that survivalist position is mistaken.

But let's return now to the issue of apparently perspectival postmortem experience, and in particular to a proposal advanced by Terence Penelhum (Penelhum, 1970). At first, Penelhum seems to agree with H. H. Price that disembodied communicators enjoy an inner life of dreamlike images. But then he suggests, further, that we can construe these merely *seeing as if* (i.e., only apparently sensory) experiences as cases of *genuine* seeing. Unfortunately, however, that approach seems to suffer from problems analogous to those afflicting the claim (provisionally attributed to Lady Troubridge) that all communicators' apparently sensory experiences are hallucinatory.

Penelhum writes:

... there seems no difficulty in saying of a disembodied person that it might look to him as though there were objects before him which looked to him as they would to a normal observer under optimal circumstances from a certain position in space. I feel obliged to start from some such account as this because I can attach no sense to the notion of seeing from no point of view, or seeing non-perspectivally. Given the intelligibility of this story, and given that there *are* objects in space arranged as stated, it seems quite pedantic to deny ... that our disembodied person *sees* them[.] So let us say he does. (p. 25)

There are several issues here. First, Penelhum may be right that the notion of *seeing* from no point of view is unintelligible or empty. However, he may also have overlooked a viable option. The evidence from relatively humdrum clairvoyance indicates that subjects can be *aware* in some sense of physical states of affairs (e.g., targets sealed in envelopes) whose sensory perception ordinarily

requires being suitably situated in space, but which at the time could not be perceived from any position in space. Thus, the evidence from clairvoyance can be taken to show that veridical awareness of physical states of affairs is possible even when there is no actual point of view from which the states of affairs can be accessed by sensory means. So even if non-perspectival *perception* is unintelligible, non-perspectival *awareness* seems to be a genuine option in both logical and empirical space. I'll return to this point later.

Moreover, it seems easy to demonstrate the implausibility of Penelhum's suggestion that a disembodied person really sees objects under the conditions he describes—i.e., conditions we might have described instead as being merely of the *seeing as if* variety. Consider the following situation. Suppose an embodied person *S* hallucinates an object *X* as being before him. Furthermore, since every hallucination (even the most fantastic or seemingly arbitrary) has some cause or other, let's suppose that *S*'s experience occurs as the result of a hallucinogen mischievously added to his breakfast cereal. But suppose further that *X* is really before *S*, so that *S* would have had a qualitatively identical visual experience had he *seen* rather than hallucinated *X*. Now, because *S*'s experience of *X* is caused by his spiked cereal rather than by *X*, its phenomenal content corresponds only fortuitously to what is actually in *S*'s perceptual field. That's why we wouldn't say that *S* saw or perceived *X* in this case. But then why attribute genuine seeing to a disembodied person *S<sub>d</sub>* whose visual experience merely happens to be that which an embodied person would have from a certain position in space? It *doesn't* seem at all pedantic to say that *S<sub>d</sub>* fails to really see the object.

Indeed, as I noted earlier, whether or not *S* sees *X* is something that needs to be cashed out in terms of an appropriate causal story. In particular, the existence, veridicality, and perhaps also the phenomenal (perspectival) quality of *S*'s experience must be explainable, in part at least, as the *result* of lawlike causal relations obtaining between *X* and *S*. But on Penelhum's proposal, a sufficient condition for *S*'s (say) genuinely seeing a person wearing a pink shirt is the mere fact that the person is wearing a pink shirt. Incredibly, it wouldn't matter whether the content of *S*'s experience is causally related to the state of the world it ostensibly represents. Hence, for Penelhum, genuine seeing (or sensing) gets robbed of its essential nomological character. Interestingly, Penelhum seems to recognize this. At one point he considers whether to assign the disembodied observer a location in space—that is, a position from which *X* would look to a normal observer the way it does to the disembodied *S*. And he writes: “we have to say that the disembodied person is at the place from which, when a normal observer sees the objects which our survivor now sees, they look to that observer the way they look to our survivor. Roughly, he has to be at the centre of his visual field” (p. 25).

But then Penelhum notes, “the first thing that seems to follow is that his seeing things the way he does cannot be construed as a [causal] *consequence* of his being where he is, for his being where he is *consists in* his seeing things the way he does” (pp. 25–26). And once again, the example above about hallucinating *X* shows why this won't work. We must still be able to differentiate hallucinating or imagining *X* from seeing (or otherwise being genuinely aware of) *X*, whether

or not *S* is embodied. But we can't do that unless we can tell some causal story about how the existence and nature of *S*'s experience results (in part at least) from the presence of *X* in the world and also lawlike regularities obtaining between *S* and *X*.

Now there's a notorious philosophical position, *phenomenalism*, according to which physical objects—although real—are nothing more than logical constructs out of more primitive sense-data (i.e., raw ingredients of perception, such as patches of color, shapes, textures, odors, etc.). So, for example, phenomenalist would say that the table before me is not really a lump of mind-independent matter affecting my equally material, mind-independent, and lumpy sense organs. Rather, the table is nothing more than a construct out of the sense-data I do in fact currently experience and also the sense-data I and others *would* experience under an indefinitely large array of possible (i.e., counterfactual) circumstances. And those possible circumstances would likewise be analyzed in purely subjective sensory terms—for example, having the experience of seeing the table through tinted glasses, or having the experience of lying beneath the table, or of seeing the table from a great distance.

So in the spirit of phenomenalism, some might think we can salvage the hallucination/perception distinction by claiming that only in the case of perception can we tell an appropriately robust counterfactual story. What we'd need to say would be something like the following: When a person genuinely perceives an object *X*, others, also having the experience of being suitably situated with respect to *X*, would also have experiences of *X* from corresponding points of view. However, if *S* merely hallucinated *X*, there would be no such correlations between what *S* experiences and what others do or *would* experience. For example, if I hallucinate (rather than perceive) a hippo in the corner, we wouldn't expect others having the experience of looking in that direction also to have visual hippo-in-the-corner inner episodes.

Of course, ordinary folk would explain this difference between hallucination and perception with respect to actual or possible relationships between observers and mind-independent physical objects. They'd say that in the case of genuine perception there really *is* some lump of matter that affects *S*'s sense organs in accordance with various causal laws, and which does or would likewise affect the sense organs of suitably situated others. But that avenue is not open to someone who construes physical objects as *constructs* out of actual and possible sense experiences, or as J. S. Mill put it, "permanent possibilities of sensation." So, unfortunately for the phenomenalist, there seems to be no comparable causal story, since on that view there are no mind-independent lumps of matter to interact causally with a perceiver's sense organs. And that renders the difference between hallucination and perception completely mysterious. Unless one is prepared to abandon strict phenomenalism and posit a deity behind the scenes either arranging things in advance (à la Leibniz) or holding models (archetypes) of objects in mind (à la Berkeley), phenomenalist have no explanation of why suitably situated possible observers *would* have the experience of perceiving a physical object. Of course, if survivalists were to adopt this phenomenalist strategy,

that might be the least of their problems. They would also inherit *all* the famous problems afflicting the phenomenalist program, including having to defend themselves against the charge of solipsism and having to explain how—on their idiosyncratically empiricist grounds—they can justify reference to other minds. But that’s another story (see Aune, 1985, for a nice summary).

#### 4. A Philosophical Digression

At this point, it might be useful to take a slight detour from the topic of postmortem survival. That’s because key features of the debate we’re considering about perspectival experiences may have a counterpart in traditional epistemology. If so, there’s reason to think that errors in that arena may prove instructive for the topic at hand.

A well-known position in the philosophy of perception is *causal realism*. Roughly, the causal realist maintains that what we perceive directly are private internal states or mental images (e.g., Humean impressions and ideas), and that in at least many cases these subjective inner episodes are caused in us by mind-independent external objects and events. (A subset of causal realists maintain—arguably incoherently or at least gratuitously—that these inner states tend to resemble or represent their external causes. That view is usually called *representational realism*.) Following Hume, some have argued for causal realism (CR) along the following lines.

CR:

- (1) When we look at a table and then back away from it, what we perceive gets smaller.
- (2) But the table presumably does not change size.
- (3) *Ergo*, what we perceive is not the table, but a private mental image caused in us by the table.

So causal realists maintain that we’re not in direct perceptual contact with the physical world. Rather, a veil of ideas (or mental images) stands between us and the external things that produce many of those inner states. In effect, these mental images are *intermediate objects of perception*. Moreover, since we don’t directly perceive tables and other external objects, the best we can do epistemologically is to infer that these things exist. Thus, the causal realist position very quickly raises the spectre of solipsism. If what we know immediately are only private, inner episodes, and if we have to infer the existence of their causes in an external physical world, how do we know there’s anything out there at all?

Beginning in the mid-twentieth century, many philosophers launched what they considered to be a kind of common-sense reaction against causal realism and (they hoped) its egocentric descent into the black hole of solipsism. Common sense seems to tell us that we are, in fact, directly in touch with the things around us—a view often disparagingly called *naive realism*. But the twentieth-century resurgence of that position (which I like to call *born-again naive realism*) offered an interesting argument strategy in its defense.

Born-again naive realists argued that traditional causal realism was misled (or seduced) by a too-easy adoption of perception language when describing the relation we have to our impressions and ideas. They claimed that by carefully re-describing situations like backing away from a perceived table, we can undermine the temptation to posit intermediate objects of perception and thereby restore to respectability the view that we're in direct contact with the world around us.

The proposed re-description would go something like this. When we back away from the table, we continue to perceive the table all the while, but the table merely *seems* or *looks* smaller. In this way, we avoid positing an epistemological gap between the table and ourselves; the only object of perception is the table. The point of this re-description is not to deny the existence of inner experiences (sense impressions) such as mental images. It's merely to deny that perception of external things is *mediated* by a more basic perception of internal things, so that we're inevitably separated from a world of things-in-themselves by a veil of ideas or sensuous curtain.<sup>4</sup>

A related strategy works even for hallucinations and dreams. For example, when I hallucinate a hippo in the corner, typical causal realists might say I perceive the hallucinated hippo (a collection of sense data), and that this intermediate and inner object of perception has no external counterpart as its cause. The born-again naive realist would counter that even here there's no need to say that I'm seeing a special, private mental object. Rather, in this case I perceive nothing at all. I merely *seem* to perceive a hippo. Again, no need to posit a perceptual relationship between me and a mental image.

Now, we needn't worry about whether or not this strategy succeeds in undermining Humean skepticism about the physical world. In fact, some would argue that so long as perceptual error is possible, skepticism can successfully drive a wedge between us and knowledge of the external world. Although I would dispute that dialectical strategy, we can nevertheless treat the debate between causal and born-again naive realists as a kind of philosophical cautionary tale. Even if skepticism about the physical world is a legitimate philosophical problem, it's far from obvious that Hume (or causal realists generally) have set up the problem correctly. At least one form of that problem may result from an inappropriate adoption of perception language and an unjustified positing of special, private objects of perception. Similarly, in the debate about perspectival postmortem awareness, both survivalists and anti-survivalists may have hastily adopted perception talk when describing how discarnate communicators respond to physical states of affairs.

## 5. The Solution That Isn't

With the foregoing in mind, we're now in a position to consider how survivalists might best respond to the puzzles about perspectival postmortem awareness. In my view, a promising strategy—arguably the only one—is to focus on the point noted earlier about humdrum clairvoyance—namely, that the evidence demonstrates how living persons can have a kind of non-perceptual awareness of remote

physical states (e.g., targets in sealed envelopes, pages of a closed book) whose perception ordinarily requires being suitably situated at a location, but which at the time could not be perceived from any position in space. Since that form of awareness apparently doesn't rely on ordinary (or, quite possibly, any) spatial cues, survivalists might therefore argue that postmortem awareness of the physical world is "merely" clairvoyance, and that the only difference between ante-mortem and postmortem clairvoyance is the ontological status of the subject. Survivalists can thereby deflect concerns about perspectival perception; they would be positing no form of perception at all. On this view, mediumistic communicators (like successful clairvoyant subjects) can enjoy either perspectival or non-perspectival awareness (not perception) of physical states in the absence of suitably positioned sensory organs.

This strategy has several virtues. First, it connects the survival hypothesis to a large body of both experimental and anecdotal evidence for clairvoyance. So even though some aspects of the survival hypothesis strike many as wildly conjectural, this way of interpreting the hypothesis at least gives it a kind of empirical footing, albeit partial and still somewhat controversial. Second, it preserves the pre-theoretically useful distinction between (on the one hand) hallucinating, imagining, or dreaming of an object and (on the other hand) having a veridical and non-fortuitous awareness of it causally mediated by that object. After all, however unusual it may be in other respects, clairvoyance is still a fundamentally causal concept. To posit clairvoyant awareness is to posit a causal link between the subject and the remote state of affairs of which the subject is aware. Granted, the mechanisms of clairvoyance, if there are any,<sup>5</sup> may be mysterious. But if clairvoyance really occurs (as various converging strands of evidence indicate strongly), and especially if it ranges over objects not currently perceivable from any position in space, then we may need to regard it as a form of veridical awareness that differs profoundly from paradigmatically emanative or transmissive forms of perception such as seeing and hearing (see also Broad, 1953).

And if we're willing to take that step, we might find it tempting to make a further bold move. We could entertain seriously the exciting possibility that ordinary sensory (i.e., embodied) perception is merely a special, biologically-mediated case of a more primitive form of clairvoyant awareness operating outside a strictly biological domain.<sup>6</sup> And for those not yet willing to drive a survival-sympathetic wedge between empirical awareness and biological functioning, we could retreat a step and substitute "organismically-mediated" for "biologically-mediated." That is, we could regard ESP (both telepathy and clairvoyance) as basic—and a typically unconscious or subconscious—way by which at least some complex organisms acquire information about mental and physical states of the world, and then we could take ordinary conscious and discursive forms of awareness to be considerably less frequent subsets of those interactions not only mediated by, but also constrained by, the organism's needs and limitations.

This would not be a new point. In fact, H. H. Price once cautiously advanced a similar suggestion and linked it to Leibniz's monadic theory (Price, 1940). For

Leibniz, each monad (mental unit) represents or expresses the entire universe from a point of view, and that process of representing or expressing the universe is what Leibniz termed “perception.” Price controversially interprets that claim as meaning that perception for Leibniz is always both telepathic and clairvoyant. I doubt whether that reading of Leibniz is justified, but in any case there are more serious obstacles to resolving the present problems in terms of Leibniz’s monadology. For one thing, I’m not sure it’s a good idea to dissolve the distinction between perceptual and non-perceptual awareness. As I hope the preceding discussion has illustrated, that distinction has considerable utility. Second, Leibniz’s metaphysics works only through the grace of a benevolent deity arranging all perception according to a principle of pre-established harmony. And third, even if we manage to purge this view of its theological trappings, we would still need to explain “why there *seems* to be so little clairvoyance, and why the vast bulk of our perceptions or representations remain unconscious” (Price, 1940: 57). At any rate, important as this thread may be, it’s an avenue of speculation that must be reserved for another time. For now, we must return to the matter at hand.<sup>7</sup>

A third virtue of the approach I’ve been suggesting concerns the fact that much of the evidence for clairvoyance points to a form of awareness not necessarily accompanied by rich mental imagery, or in fact any imagery at all. For example, in classic card-guessing experiments, many anecdotal reports, and even some successful remote-viewing trials, subjects may report nothing outstanding in the way of internal imagery, although they often have hunches and impulses to act. In that respect, clairvoyance would resemble subliminal perception, which also occurs in the absence of reportable phenomenal correlates. However, it differs from subliminal perception in that the latter relies on familiar causal links to objects in one’s vicinity, the same kind that account also for the perspectival nature of ordinary, non-subliminal perception. By contrast, in clairvoyance the spatial location of a person’s sensory receptors presumably plays no causal role. The reason all this is important is that it offers a precedent for those survivalists willing to claim that postmortem communicators can have veridical awareness of physical states in the absence of mental imagery caused by those states. Granted, communicators often use perception terms to describe their states of awareness, but as Una Lady Troubridge suggested, that may indicate nothing more than our limited linguistic options for reporting those states. We needn’t suppose that the awareness is actually accompanied by vivid, ordinary, or any mental imagery—the kind that has traditionally generated the puzzles we’ve been considering. So by modeling postmortem awareness after “ordinary” clairvoyance, survivalists can posit a process distinguished from both hallucination and subliminal perception by either the existence or nature of their causal links to the physical world, but which (like subliminal perception) lacks the familiar phenomenal features associated with ordinary sensory perception.

So I think it would be prudent for survivalists to adopt a threefold strategy: first, to claim that postmortem communicators can be clairvoyantly (not perceptually) aware of physical states; second, to claim that this type of awareness may or may not be accompanied by internal imagery; and third, to claim that when there

is imagery it's explainable either in terms of causal properties of the objects of which the subject is aware or else by the subject's own creative and idiosyncratic tendencies to generate internal imagery—just as seems to be the case with living subjects in successful clairvoyance experiments.<sup>8</sup>

Of course, many survivalists will probably be reluctant to pursue this strategy, because then they would clearly need to abandon an argument they unwisely use against the rival “super-psi” hypothesis—namely, that the nagging alternative of living-agent psi posits a kind and degree of psychic functioning that is antecedently implausible, or at least far in excess of any that has been demonstrated experimentally. Whether they like it or not, nonbodily postmortem awareness of the physical world would be a paradigm instance of clairvoyance. It would be an awareness of physical states unmediated by the physical and sensory mechanisms leading to ordinary perception. Moreover, in scope, consistency, or refinement it wouldn't differ significantly from the clairvoyance super-psi proponents attribute to mediums or sitters instead. On the contrary, every exchange of information between a communicator and the mind of a living person, and every apprehension by a communicator of a physical state of affairs, would be an instance of ESP. And of course, the mediumistic evidence for survival consists of a great many of these purported events, many of them quite startling in the specificity and obscurity of the information they provide.

So, ironically, the best defense against the arguments noted earlier might be one that undercuts a standard attack survivalists use against their chief parapsychological rival. It would require an explicit and serious concession to psi-sympathetic anti-survivalists: an endorsement of the view that the survival hypothesis presupposes the operation of refined or frequent clairvoyance and telepathy between the deceased and the physical world.

It seems to me, then, that survivalists are faced with the following challenge. First, they must learn to embrace the possibility of refined psi if they plan to count mediumship as a source of evidence for survival and if they hope to counter the puzzles we've considered about perspectival postmortem experiences. That's the only way survivalists can satisfactorily explain postmortem awareness of both physical states of affairs and also thoughts of the living. So not only is the appeal to postmortem ESP mandated by the survival hypothesis, it also offers significant explanatory benefits. But in that case, if survivalists hope to argue effectively against living-agent psi as a general alternative to the survival hypothesis, they must rely on some strategy other than asserting the implausibility of so-called “super psi.” My suspicion is that survivalists can escape this dilemma only by claiming—without any clear justification—that the anti-survivalist appeal to living-agent psi posits not simply the unfortunately labeled super-psi, but something much grander and considerably more implausible. Let's call it (with tongue firmly and appropriately in cheek) *supercalifragilisticexpialidocious psi*.

I should emphasize that I'm not arguing against the survival hypothesis, even though I've appealed to clairvoyance to solve the more thoughtful puzzles raised about ostensible postmortem awareness. If my approach has any merit, it merely

demonstrates again, and from another angle, why survivalists should abandon their insistence on the implausibility of anti-survivalist appeals to psi among the living. Granted, it also reinforces a conclusion I've defended elsewhere at length: that it's exceedingly difficult (if not impossible) to defend the survival hypothesis against the hypothesis of living-agent psi (see Braude, 2003). Nevertheless, it demonstrates how survivalists can deflect the usual concerns about the intelligibility of the survival hypothesis without severing needed causal links between the worlds of the living and the deceased.

### Notes

<sup>1</sup> Sorry, but the temptation for excessive alliteration was irresistible.

<sup>2</sup> On the other hand, philosophers have had plenty to say—both pro and con—about the intelligibility and possibility of disembodied existence. Typically, though, these works proceed without any consideration of the evidence suggesting survival, and sometimes they're adorned with the usual ignorant disparaging references to that material. For example, Blose (1981) says that the survival hypothesis "is supported by meager evidence and that from the most disreputable of sources," none of which he bothers to cite. At any rate, the philosophical literature on disembodied existence, while extensive, is singularly unhelpful in the present context. Nevertheless, intrepid readers might wish to investigate, e.g., Everitt, 2000; Gillett, 1986; Hocutt, 1974; Long, 1977; Smart, 1971; Steinberg and Steinberg, 2007; Taliaferro, 1997; and Tye, 1983.

<sup>3</sup> "Super psi" is a very unfortunate term which by now is probably too well-entrenched to abandon routinely in favor of "psi among the living" or something equally cumbersome. For a discussion of the problems with that clearly loaded expression, see Braude (2003). I prefer to follow Michael Sudduth in replacing the term "super psi" with the less prejudicial and clearer "living-agent psi" (Sudduth, 2009: 167–193).

<sup>4</sup> The debate here also concerns other related issues—for example, whether inner experiences can be known with certainty, or whether they can be described in a way not parasitic on a more fundamental level of description applicable to outer things. But those issues needn't concern us here.

<sup>5</sup> For comments on the possibly irreducible nature of paranormal causal connections, see Braude (1997, 2003).

<sup>6</sup> I'm grateful to Andreas Sommer for reminding me of this.

<sup>7</sup> Ed Kelly has pointed out to me another intriguing possibility considered by some psychologists—namely, that "ordinary perception is oneiric activity constrained by sensory input" (personal communication, July 9, 2008). This, too, is a topic which, although clearly relevant—and perhaps also happily compatible with Price's suggestion (noted earlier) that mediums dream aloud—goes beyond the scope of the present paper.

<sup>8</sup> For more on the idiosyncratic and highly variable experiences of clairvoyant subjects, see Braude (2003: chap. 8).

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*HISTORICAL PERSPECTIVE*

**Psychical Research in the *Psychological Review*,  
1894–1900: A Bibliographical Note**

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**Abstract**—While there was much conflict during the 19th century between psychology and psychical research, the latter was occasionally discussed in psychology journals. The purpose of this paper is to provide a guide to existing discussions of psychical research and related topics in the American journal *Psychological Review*. Many of the discussions were authored by individuals favorably disposed to psychical research, such as William James and James H. Hyslop, but also by such skeptics as James McKeen Cattell and Joseph Jastrow. With a few exceptions, the majority of the authors were critical of psychical research. This reflected the hostility on the topic shown by many psychologists at the time.

**Keywords:** *Psychological Review*—history of psychology—history of psychical research—criticism—William James

Many works discuss the interactions between 19th-century psychology and psychical research (e.g., Alvarado, 2002; Coon, 1992; Oppenheim, 1985; Plas, 2000). Some of the primary literature on the subject appeared in a variety of intellectual reviews and psychology journals, such as the *Psychological Review*. While some general information about the historical context of the material is presented, the purpose of this note is to provide bibliographical guidance for those interested on the topic by identifying papers and book reviews about psychical research published in the *Review*.

Psychical research during the 19th century was on the rise, as can be seen in the work of members of the Society for Psychical Research (SPR) in England (Gauld, 1968), and the work of individuals in other countries.<sup>1</sup> Several prominent figures in psychology, such as William James (1896d), Julian Ochorowicz (1887), and Charles Richet (1884), defended the validity of some of the claims of psychical research. In addition, psychical researchers contributed to the study of the subconscious mind, of dissociation, and of hallucinations. The work of members of the SPR (e.g., Myers, 1884; Sidgwick et al., 1894) provides examples of this.

While aspects of the work of the SPR were controversial and rejected by many psychologists, several of these individuals were acquainted with it (e.g., Janet, 1889; Jastrow, 1889). One indication that the SPR work was well-known was the fact that educator, psychical researcher, and SPR member Eleanor Mildred Sidgwick (1902) contributed a summary of the work of the SPR for the well-known reference work *Dictionary of Philosophy and Psychology*. Furthermore, members of the SPR, and psychical research itself, appeared often at 19th-century congresses of psychology (Alvarado, 2006; Nutting, 1992). Psychical research was also discussed frequently in the journal *Science* and in prominent newspapers (e.g., Mason, 1893; "Psychical Research," 1884; "The Society for Psychical Research," 1884).

In the United States William James was the most prestigious defender and popularizer of psychical research during the 19th century.<sup>2</sup> James wrote in the *Psychological Review* that psychical research "has . . . many enemies, fair and foul, to elude before she gets her scientific position recognized . . ." (James, 1896d: 649). Many of those enemies were psychologists who reduced telepathy and mediumship to known natural processes. Mediumship was reinterpreted by Pierre Janet (1889) as a dissociative phenomenon, while others argued that telepathy could be accounted for by unconsciously perceived sensory cues (e.g., Hansen & Lehmann, 1895; Rualt, 1886). There was also a literature in which psychical researchers were considered to lack the necessary scientific training to conduct their studies, a situation that was believed to lead to the use of improper methodology (e.g., Jastrow, 1889; Scripture, 1897; see also Coon, 1992).

In addition to complaints about methodology and theoretical interpretation of unexplained phenomena, professional psychologists rejected the anti-materialistic ideas some psychical researchers used to explain their phenomena (on the latter see Gurney et al., 1886). The new empirical psychology, in an attempt to separate itself from metaphysical ideas about the soul, embraced a materialistic view in which the mind was the product of the nervous system.<sup>3</sup> Working under this assumption, action at a distance (such as telepathy), and the idea that the mind could be independent of the body, was unlikely to be accepted as part of psychology.

Several of these topics, as well as the phenomena of psychical research, were occasionally discussed in leading psychology journals, among them the American journal *Psychological Review*.

### ***The Psychological Review***

The *Psychological Review* was founded in 1894 by James Mark Baldwin and James McKeen Cattell, who were both the owners and the editors (in alternate years) of the journal between 1894 and 1903 (Calatayud et al., 1987; Kintsch & Cacioppo, 1994; Sokal, 1997). From the beginning, the *Review* became a prominent journal that represented the new empirical psychology then prevalent in American universities (O'Donnell, 1985). The first volume, published in 1894,

included contributions by such authors as James R. Angell, Mary Whitton Calkins, John Dewey, Christine Ladd Franklin, William James, Joseph Jastrow, George H. Mead, Hugo Münsterberg, Edward W. Scripture, and Lightner Witmer, among others. In addition to articles, the book review section of the journal informed the American public of the wide range of available literature from psychology, as well as from areas such as philosophy and sociology. The journal, still in print today, currently publishes theoretical papers.

### **Psychical Research in the *Psychological Review***

#### *Articles*

One of William James' (1896d) papers was a reply to James McKeen Cattell's (1896) critique of James' (1896a) presidential address to the SPR.<sup>4</sup> To counter James' view that groups of apparitions cases were more impressive than single cases, Cattell argued:

When we have an enormous number of cases, and cannot find among them all a single one that is conclusive, the very number of cases may be interpreted as an index of the weakness of the evidence. The discovery of a great many gray crows would not prove that any crows are white, rather the more crows we examine and find to be black or gray, the less expectation have we of finding one that is white.<sup>5</sup> (p. 582)

Cattell was not impressed by James' assessment of Mrs. Piper as his "white crow." In a critical tone, Cattell (1896: 582–583) stated: "The ablest of men have followed alchemy and astrology, have worshiped strange gods, have consulted witches and burned them. Geese have before now been mistaken for swans, and often to the honor of those who made the mistake. One white crow is enough, but its skin should be deposited in a museum."

James (1896d) disagreed with Cattell's argument of the weakness of accumulated cases, arguing that he did not have any evidence of "erroneousness," but instead was working on the assumption common to some scientists that the evidence against psychic phenomena was too strong to be questioned. In other words, he implied that Cattell was talking from prejudice and not from actual evidence or conventional explanations. James further wrote:

The presumption has remained presumption merely, the scientist saying, "I can't believe you're right," whilst at the same time he has been unable to show how or where we were wrong, or even except in one or two cases to point out what the error most probably may have been. . . . Professor Cattell says: Can the exhibition of any number of gray crows prove that any crows are white? But our reports are not of gray crows; at the very worst they are of white crows without the skins brought home . . .; and where there are such obvious reasons why it must be easier to see a wild beast than to capture him, who can seriously maintain that continued reports of merely seeing him tend positively to decrease the probability that he exists? . . . Continued reports, far from strengthening the presumption that such things cannot exist, can only detract from its force. (p. 650)

In a long paper, philosopher and psychical researcher James H. Hyslop (1898) discussed "psychical research and coincidence." He argued that the coincidental

phenomena studied by the SPR “has done much to strengthen the interest and belief in the possible meaning of such phenomena, especially when they take a certain form” (Hyslop, 1898: 362). Hyslop, aware of veridical cases and cases with supporting testimony, was not convinced that chance coincidence and illusions explained all the cases reviewed. However, he believed he found in some cases “evidence of an extraordinary combination of emotional interests and a predisposition to automatism to simulate supernormal phenomena” (Hyslop, 1898: 386).

Hyslop (1899) also criticized Hugo Münsterberg’s critiques of the evidence for psychic phenomena, which generated a counter reply (Münsterberg, 1899a).<sup>6</sup> In a short note complaining that the newspapers had distorted his statements about the immortality of the soul, Hyslop (1900b: 65) stated that the time was past for ridiculing the study of psychic phenomena: “Scientific method must control the study of these phenomena or lose its prestige and authority where it has the supreme right. . . .”

In a fascinating paper, University of Iowa’s psychologist G. T. W. Patrick (1898) discussed secondary personalities. He criticized the assumption of discarnate agency presented by Hodgson (1898: 556) as an illegitimate hypothesis that failed to “connect the phenomena in question with any other known facts or laws.” Similarly, he criticized the lack of attention experimental psychologists had dedicated to the topic, and to automatism in general, perhaps designed to “maintain the dignity of experimental psychology” (1898: 556).

Patrick believed these phenomena could throw much light on the workings of the mind, and called for the investigation of simple cases to further understand the more complicated ones. Expressing the typical skepticism of psychologists to survival of bodily death and the ideas of Frederic W. H. Myers, Patrick wrote that the former were similar to Descartes’ concept of “animal spirits,” while the latter’s concepts about the mind, and its telepathic dimensions, were essentially a “metaphysical, not a psychological hypothesis” (1898: 562). However, Patrick touched on many topics previously discussed by Myers without any acknowledgement. Some examples were the “remarkable activity of the constructive imagination” (1898: 573), the suggestibility of secondary personalities, and the low intellectuality of some of them.

Patrick referred to the veridical statements of mediumistic personalities as a “happy intuition.” This did not imply the action of either spirits or telepathy because he believed other explanations could be forthcoming when the “characteristics of the secondary personality become subject to accurate scientific description” (1898: 576).

Following on the topic of conventional explanations, E. E. Slosson (1899), from the University of Wyoming, discussed the power of suggestion to produce hallucinations. He reported that during a lecture to students he was able to induce an olfactory hallucination in three-fourths of the audience. Hallucinations of sensations of temperature and pain, he said, “are easily induced by suggestion in susceptible individuals by the use of magnets. . . . It is of course, necessary that

the subject should have hazy ideas about magnetism. . . . Sensations of heat may be produced by the north pole of the magnet, and cold by the south, or one pole may be made to give a tingling or smarting pain in the right hand and side of the body, and the south pole on the left . . ." (1899: 407–408).<sup>7</sup>

### *Book and Article Reviews*

The rest of the discussions consist of book and article reviews, such as those of William James. In some of these writings James criticized criticisms of work with veridical hallucinations (James, 1895a, 1897c), and commented on SPR work on the subject (James, 1895c,d). In addition, James (1896b) summarized the paper in which Hansen and Lehmann (1895) attempted to redefine experimental thought-transference as an exercise in involuntary whispering.<sup>8</sup>

Other reviews were about discussions of telepathy (James, 1895b, 1896c, 1897b), possession (James, 1895e), the subliminal self (James, 1896e), involuntary whispering and thought-transference (James, 1897a), and Mrs. Piper (James, 1898b). James also brought attention to publications in languages such as French (James, 1894), German (James, 1895a), and Italian (James, 1897b). All of these writings contributed to the popularization of psychical research topics among American psychologists.

John Grier Hibben (1897), from Princeton University, commented on William Crookes' SPR Presidential Address. Referring to Crookes' (1897) hypothesis that ether waves affecting the brain could account for telepathy, Hibben commented that the concept was not even a hypothesis: "At best he establishes merely the possibility of his speculation, for he presents no facts to indicate its probability or to save it from being relegated to the sphere of bare conjecture" (1897: 536).

In his discussion of two books about stage magic and illusions, Cattell (1899) continued his critical attitude, arguing that suggestibility and the psychology of the crowd are topics for psychological study. The books in question "should certainly be read by those interested in 'psychical research'" (Cattell, 1899: 554).

The psychology and physiology of the dying, including reports of panoramic memory, were reviewed as well. H. N. Gardiner (1896a,b, 1897, 1898), from Smith College, commented on several articles on the topic published in France. The reviewer was skeptical of ideas in which a psychological self was held to react to the experience of being near-death by producing past memories.

Finally, Joseph Jastrow (1900), from the University of Wisconsin, a well-known critic of psychical research (e.g., Jastrow, 1889), reviewed the French edition of Théodore Flournoy's psychological study of medium Hélène Smith, which was translated into English (Flournoy, 1900). The medium produced communications about past lives in India, descriptions of planet Mars, and a Martian language.<sup>9</sup> The main value of the book, Jastrow stated, was the "success with which the various phases of these 'mediumistic' phenomena have been described and traced to natural and tangible points" (1900: 406). Flournoy's tracing of the

medium's Martian language, Jastrow commented, was a "truly classical instance of the psychological comprehensiveness of the automatic self in exceptional cases" (1900: 411). He wrote further:

The dangers of a false interpretation of this "psychic" tale are many, and are certain to mislead many who's interest in and powers of comprehension of cases of this kind are not equally developed. . . . It seems probable that when a sufficient number of these cases have been collected, that their general nature and law-abiding character will be understood, that a rational group of associations may be clustered about the term 'mediumistic,' (sic) and that the temptation to magnify the doings of some of this class into a bundle of miracles will gradually pass away—all of which would be welcome consummations for the progress of Psychology. (p. 411)

### Concluding Remarks

The contents of the *Psychological Review* reflect the hostile position many psychologists took towards psychical research during the 19th century. As argued elsewhere (e.g., Alvarado, 2006; Coon, 1992), psychical research was generally rejected by psychologists of the time for a variety of reasons, one of which involved psychology's own struggle to be recognized as a science. Coon (1992: 150) has stated: "Psychologists were stationed at the periphery of science, and therefore they were the most threatened by challenges to the boundary and the most susceptible to cultural anxieties about what it meant to be 'scientific.'" Psychic phenomena represented one of these threats, both in terms of expertise and the non-materialistic explanations of telepathy and other manifestations.

Some of those openly critical of psychical research in the *Review* were Cattell, Jastrow, Münsterberg, and Patrick. These authors redefined psychic phenomena as the operation of sensory cues, hallucinations, and a variety of subconscious phenomena that included secondary personalities and automatic writing, among others.

Nonetheless, there were a few exceptions. The most notable was the work of James. But Hyslop also defended the field.<sup>10</sup>

It is my hope that the above comments will guide interested readers to the psychical research content of the *Psychological Review*. However, the study of this literature needs to be extended to contributions appearing in several other journals. Interested readers should also consider publications such as the *American Journal of Psychology*, *Mind*, and the *Revue Philosophique de la France et de l'Étranger*.

### Notes

<sup>1</sup> For reviews of developments in different countries see Biondi (1988), Moore (1977), Oppenheim (1985), Plas (2000), and Wolfram (2005).

<sup>2</sup> This is clear in the material referenced below, as well as in critiques (James, 1886) and reviews of specific works (James, 1887). Recent discussions of James and psychical research include the work of Blum (2006) and Knapp (2003). For a collection of his psychical research writings see James (1986).

- <sup>3</sup> This process, intimately related to the study of localization of functions in the nervous system (Clarke & Jacyna, 1987), has been discussed in general overviews of the history of psychology (e.g., Boring, 1950), as well as in histories devoted to specific countries (e.g., Nicolas, 2002; O'Donnell, 1985).
- <sup>4</sup> There were other controversies between James and Cattell. In response to criticisms about research with medium Leonora E. Piper (Cattell, 1898a) in the pages of *Science*, James (1898a) wrote a letter to the journal, which was answered by Cattell (1898b).
- <sup>5</sup> This was a reference to James' famous comment about Mrs. Piper: "If you wish to upset the law that all crows are black, you musn't seek to show that no crows are; it is enough if you prove one single crow to be white. My own white crow is Mrs. Piper" (James, 1896a: 884).
- <sup>6</sup> Münsterberg (1899b) was a well-known critic of psychical research. He was criticized by Hyslop (1908) and Schiller (1908).
- <sup>7</sup> This should be seen in the context of 19th-century interest in the influence of magnets to induce a variety of phenomena on human beings (e.g., Binet & Féré, 1885; Durville, 1895).
- <sup>8</sup> Hansen and Lehmann's (1895) paper was very influential. It was cited by many psychologists either implying that the study took care of the evidence for telepathy (e.g., Titchener, 1898: 897), or directly saying so (Scripture, 1897: 63–68, 259–260). As another psychologist wrote a few years later, this study showed "that much of the alleged transfer of thoughts might be accounted for by hints and suggestions given . . ." (Stratton, 1903: 206). See also the important critical comments of Sidgwick (1897), and the controversy between William James and Edward B. Titchener in the pages of *Science* (James, 1899a,b; Titchener, 1899a,b).
- <sup>9</sup> The medium's real name was Catherine Élise Müeller. Her performances, and Flournoy's work, have been discussed by Shamdasani (1994).
- <sup>10</sup> In later years Hyslop became even better known than James in the United States as a defender of psychic phenomena from its numerous critics (e.g., Hyslop, 1909). His work as a popularizer of psychical research is evident in his books (e.g., Hyslop, 1905), and in ideas discussed in newspapers ("Mrs. Piper, Trance Medium," 1900), and magazines (Hyslop, 1900a).

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## LETTER TO THE EDITOR

**Editor:** In the Spring 2009 issue of *JSE*, Douglas Stokes commented on a paper I coauthored with Lund, Emoto and Kizu that was published in the Winter 2008 issue. That paper described an experiment involving the effects of distant intention on the formation of frozen water crystals. Stokes' critique was that multiple drops of water were drawn from the same bottles, and thus those samples could not be considered statistically independent. From this, he suggested that the bottle rather than the water sample should be treated as the unit of analysis, and because there were only six bottles used in the experiment, the best possible significance level attainable would be 0.167, whereas we had reported  $p = 0.03$ .

The question of independence, in this context, is primarily concerned with assumptions about differences in water samples taken from different bottles. If each bottle came from a different source, say Evian, Perrier, Fiji, etc., then variations in mineral concentrations from one brand to the next might well result in some bottles inherently producing more attractive crystals than others. And thus statistical comparisons based on the assumption that the underlying water samples were the same would be invalid.

We were aware of this potential problem, so in the experiment we used six bottles of water from the same brand (Fiji). Those bottles were bound together in a single package, and they were all part of the same bottling run. This means the six bottles we used were filled with water from the same source, all within a matter of seconds, and subjected to the same environmental conditions from the moment of bottling to arriving at our lab. Once in the lab, we took care to handle each bottle the same way, and after "exposure" to distant intentions we repackaged the bottles again, where they remained bound together until they reached the lab in Tokyo. In this way, rather than using three hundred 10-ml bottles filled from a single source of water, for convenience we used six 500-ml bottles filled from the same source.

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## BOOK REVIEWS

**Is God a Mathematician?** by Mario Livio. New York: Simon & Schuster, 2009. 308 pp. \$26.00 (hardcover). ISBN 074329405X.

Is God a mathematician? (Or is he a computer programmer or a software engineer?) Is mathematics invented or discovered? Why is mathematics so useful in physics? And why isn't it at all useful in biology? These are stimulating fundamental questions, and Mario Livio obviously had fun collecting the material for this book. Livio gives no definite answers, but the book is entertaining and uplifting, with many beautiful quotations from distinguished thinkers and many photographs of the title pages of books that are inspiring masterpieces of the scientific literature of previous centuries.

Clearly, as Bertrand Russell says at the end of the book, there are no definite answers; everyone must try to answer these questions to their own satisfaction. Here are some of the answers I've come up with. And I'll now give some of my own favorite quotations, not ones that are in Livio.

On why mathematics works so well in physics, I have two answers. First of all, math and physics co-evolved. Secondly, if you believe in the multiverse, there would not be any life in this universe marveling at the mathematical nature of reality if our universe did not have enough structure for life to evolve within it. Chaotic universes have no observers.

On whether mathematics is invented or discovered, I have an ambiguous position. I believe in the Platonic world of ideas, so you might guess that I will answer "discovered." However, our knowledge of this perfect, unchanging, eternal Platonic world is far from perfect, unchanging, and eternal. I believe that our theories are necessarily invented, that we do not have direct perception of the Platonic world of ideas. And here are quotations from Bertrand Russell, Kurt Gödel, and Albert Einstein which also suggest that the answer to whether mathematics is invented or discovered is "invented":

My object in this paper is to explain in what sense a comparatively obscure and difficult proposition may be said to be a premise for a comparatively obvious proposition, to consider how premises in this sense may be discovered, and to emphasize the close analogy between the methods of pure mathematics and the methods of the sciences of observation. [Russell (1907)]

The analogy between mathematics and a natural science is enlarged upon by Russell also in another respect (in one of his earlier writings). He compares the axioms of logic and mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced; which of course would not exclude that they also have a kind of intrinsic plausibility similar to that in physics. I think that (provided "evidence" is understood in a

sufficiently strict sense) this view has been largely justified by subsequent developments, and it is to be expected that it will be still more so in the future. It has turned out that (under the assumption that modern mathematics is consistent) the solution of certain arithmetical problems requires the use of assumptions essentially transcending arithmetic, i.e., the domain of the kind of elementary indisputable evidence that may be most fittingly compared with sense perception. Furthermore it seems likely that for deciding certain questions of abstract set theory and even for certain related questions of the theory of real numbers new axioms based on some hitherto unknown idea will be necessary. Perhaps also the apparently unsurmountable difficulties which some other mathematical problems have been presenting for many years are due to the fact that the necessary axioms have not yet been found. Of course, under these circumstances mathematics may lose a good deal of its “absolute certainty”; but, under the influence of the modern criticism of the foundations, this has already happened to a large extent. [Gödel (1944)]

[T]he concepts which arise in our thought and in our linguistic expressions are all—when viewed logically—the free creations of thought which can not inductively be gained from sense-experiences. . . . Thus, for example, the series of integers is obviously an invention of the human mind, a self-created tool which simplifies the ordering of certain sensory experiences. But there is no way in which this concept could be made to grow, as it were, directly out of sense experiences. [Einstein (1944)]

Kant, thoroughly convinced of the indispensability of certain concepts, took them—just as they are selected—to be the necessary premises of any kind of thinking and distinguished them from concepts of empirical origin. I am convinced, however, that this distinction is erroneous or, at any rate, that it does not do justice to the problem in a natural way. All concepts, even those closest to experience, are from the point of view of logic freely chosen posits. . . . [Einstein (1949)]

These are all quotations that I believe support what Imre Lakatos termed a *quasi-empirical* view of mathematics, which is the idea that although mathematics and physics are different, perhaps they are not as different as most people think. In particular, mathematical quasi-empiricism denies the primacy of the axiomatic method and maintains that although the axiomatic method may be of expository value, mathematicians actually discover new concepts and principles by trying to unify and organize their mathematical experiences, much as empirical scientists do. For more on this, see any of my books or Tymoczko (1998) and Borwein et al. (2003, 2004, 2007).

Enough of my own opinions. Returning to Livio, what his book shows well is that even though metaphysics is currently out of fashion in academic philosophy, it is alive and well in the scientific community. Just look at Leonard Susskind’s and Max Tegmark’s idea that the physical laws of this universe are not particularly interesting, because they are just, as it were, our “street address” in the multiverse/landscape of all possible physical universes with all possible laws of nature. You cannot get more metaphysical than that, but Tegmark is a cosmologist and astrophysicist, not a professor of philosophy, and Susskind is a string theorist.

Mario Livio is to be congratulated for keeping such high level intellectual ambitions alive in spite of a hostile contemporary zeitgeist and for packaging

his book in such a fashion that it is acceptable to commercial publishers while still being of interest to philosophers and theologians.

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**La Lévitiation** [Levitation] by Joachim Boufflet. Paris: Jardin de Livres, 2006. 202 pp. ISBN 2-914569-27-0.

Over the years there have been individuals concerned with documenting psychic phenomena—such as bilocation, levitation, luminosity of the body, stigmata—reported to have taken place around mystics and saints, particularly of the Christian tradition. Examples coming from the 19th century to more recent times are Albert Farges, Johann Joseph Görres, Olivier Leroy, Jerome Ribet, Herbert Thurston, and Joseph de Tonquédec. More recently, others have followed in this tradition, such as the author of the book reviewed here, Joachim Boufflet.

Boufflet, who holds a history Ph.D. from la Sorbonne, has worked with the Congregation for the Causes of Saints in Rome regarding beatification processes. He is well known for his writings about the phenomena of mystics and saints, an example being his three-volume work *Encyclopédie des phénomènes extraordinaires dans la vie mystique* (Boufflet, 2001–2003). In the book reviewed here, Boufflet focuses on levitation, with emphasis on Christian mystics and saints. As we will see, his study is heavily influenced by Olivier Leroy's *Levitation: An Examination of the Evidence and Explanations* (1928), a unique study of saintly levitation. Furthermore, and following Leroy and others, Boufflet is concerned with establishing differences between natural and supernatural phenomena.

From the beginning of the book, the author affirms his belief in the reality of the phenomena, and comments that, from the spiritual point of view, unusual manifestations are by-products of spirituality that have caused many controversies. Boufflet starts discussing the levitations of Copertino, comparing them to those of medium D. D. Home (such comparisons were also presented by Leroy). Copertino levitated suddenly, unexpectedly, and in plain light, while Home's levitations seemed to be arduous, took place slowly, and only rarely in plain light. There are other differences. Home, he states, never levitated over 5 feet from the floor, while Copertino went much higher. In summary, he stated that mediums "produce but a pale copy of the spectacular manifestations" (p. 47) shown by some saints, which is also in agreement with Leroy.

Boufflet reproduces a table (p. 68), based on Leroy's study, in which he contrasts features of the levitations of mystics and saints compared with those of mediums. Some of the items, presenting the mystics before the mediums, include phenomena independent of the will vs. willfully induced; spontaneous ecstasy vs. induced trance; bright luminous phenomena vs. weak and rare luminosities; revulsion for publicity vs. looking for publicity; lack of any interest vs. lucrative activities; and place does not matter vs. a specific place. Furthermore, and following Leroy, Boufflet argues that saints and mystics show ecstasy during levitation, while mediums show trance. The first are seen to be highly spiritual, something that is not the case with the second group. Furthermore, and following a French psychiatric tradition barely acknowledged in the book (on this issue see Le Maléfan, 1999), mediums are seen as pathological individuals. In contrast, saints are assumed to be well balanced. In addition, it is said that they look for union with God, for perfection, something that manifests as virtues, particularly as charity.

All these differences led Boufflet to ask if there were two types of levitation: one with a "religious connotation and significance, and a profane one . . ." (p. 66). He also refers to a possible third type, which he calls "natural levitation," taking place in normal people.

Boufflet also documents the fact that levitation is not only reported in Christian contexts. In addition to mediumship, he reviews beliefs in the phenomenon from antiquity and from different places. This includes sections about ancient Greeks and Romans, fakirs in India, witches from Africa, and shamans in America and Oceania. The discussion has examples of "emblematic figures" or important religious figures such as Buddha.

Levitation, Boufflet writes, is sometimes accompanied by other phenomena in mystics and saints. These include luminosity of the body and stigmata. More rarely, there are fragrances around the person's body and, "during some exceptional cases, the apport of flowers . . ." (p. 186).

Boufflet does not enter into speculations about the "physics" of levitation. Instead he follows on the ideas of those, such as Albert Farges, that have stated the phenomenon represents an "outward and visible sign of what happens inwardly, when the soul is raised by God to those heights which approach so closely to

the beatific vision” (Farges, 1926: 169). According to Bouflet: “All extraordinary phenomena . . . signal a high degree of union with God . . . . They are the sign and manifestation of ecstatic unity . . .” (p. 186). Levitation represents the “degree of perfection and union with God of the person that is such favored” (p. 200), regardless if the person was a Christian, a Buddhist, or a Muslim. The phenomenon, Bouflet believes, may be interpreted as the objectification of the spirit’s internal state into the physical realm through a temporary exemption of physical laws that mimic or dramatize the spiritual liberation of the soul acquired by the grace of God.

The strength of the book lies in the presentation of a variety of levitation cases from the literature about mystics and saints. Many sources in various languages are used to illustrate the phenomenon. In doing this, Bouflet has reminded us that, because of its long history and recorded observations, this phenomenon needs to be remembered as an important aspect of the history of religion and mysticism. In addition, the book also restates the important fact that many of the testimonies for levitation are better than some critics in the past have assumed them to be.

Another good thing about the book is that it reminds us that saints and mystics exhibit more dramatic levitations than mediums. This also seems to be the case with other manifestations, as I have noticed in the case of luminous phenomena (Alvarado, 1987). Such an observation deserves further study, even if this consists only on new analyses of old cases. In fact, the study of accounts of past phenomena such as levitation is in need of systematic quantitative analyses of case features and antecedents, analyses that could support and give better and more precise empirical support to the observations presented in this book. What I have in mind is something such as the coding of cases for a variety of features such as Alan Gauld did in his analysis of hauntings and poltergeists (Gauld & Cornell, 1979).

On the weak side, Bouflet’s analyses are not very detailed. I do not believe he has examined the “profane” levitation literature in much detail, particularly the mediumistic one. Contrary to what he states, D. D. Home levitated beyond a height of 5 feet. I would also question the assertion that mediums produce levitation willfully.

The author does not consider levitation in poltergeist cases, a topic examined by Owen (1964). In addition, his contrasts of the levitations of mystics and saints and mediums do not tell us the proportion of features in question. Lacking this information we cannot be sure if Bouflet is referring to small or to marked differences.

I also feel that Bouflet is too quick to classify mediums as individuals with pathology, and saints and mystics as well adjusted. First of all the author does not review the subject in great detail. He focuses on old descriptions of the presumed pathology of mediums but, conveniently, he glosses over a literature that has classified saints as hysterics and the like (Mazzoni, 1996). While I am not arguing for the latter, and I feel both literatures are problematic in different ways, I feel more balance is needed before one classifies both groups as sharply as the author does.

It is also important to realize that Boufflet fails to consider other possibilities. The “reduced” manifestations of levitation in the séance room are used to show they come from a lower source, while the levitation of saints and mystics comes from the highest possible agency, that of God. But no allowance is made of the psychological context. Assuming both groups of individuals have similar psychic potential—admittedly a concept far from being understood—perhaps the difference can be found in training, expectations, and the like. Certainly mystics and saints work in a tradition in which certain phenomena are expected, while mediums also have a tradition in which they have developed. Higher magnitude manifestations may be the province of mystics and saints because their ascetic way of life leads them to focus their mind in more efficient psi-conductive ways than do mediums. Mystics and saints may develop more piety and selflessness than mediums, aspects that may be associated to the manifestation of large effects. The point here is that the observed differences may not be a question of different sources. One can only speculate what type of phenomena (or what magnitude) D. D. Home would have shown if he had lived the same life as Copertino. Contrary to Boufflet, I do not present my speculations as facts. I am not even convinced of their validity. But authors such as Boufflet should consider these issues in more detail, if only to be fair to other perspectives.

It is unfortunate that Boufflet’s treatment of the subject is so one sided. He basically promotes a Christian worldview that does not consider alternate arguments. In any case, one hopes that more studies about the past literature of levitation will continue to appear in the future so as to learn more about these manifestations.

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**My Stroke of Insight: A Brain Scientist's Personal Journey** by Jill Bolte Taylor. New York: Viking, 2009. 183 pp. ISBN 978-0-670-02074-4.

In 1996, at the age of 37, Harvard-trained neuroanatomist Jill Bolte Taylor suffered a major stroke on the left side of her brain. The stroke produced a life-transforming experience, which is the subject of her memoir. The feature of interest to this journal lies in a special combination of facts. The first was the character of the author's hemorrhage, which affected her motor and sensory cortex, her ability to speak (Broca's area) and to understand speech (Wernicke's area), and the part of the cortex that mediates the subject's orientation in space and time (p. 55). The second was that the subject was a neuroscientist able to observe, remember, and describe (brilliantly) the stages of her neuro-functional disintegration as well as her experience. The third fact concerns the nature of the experience, which had all the earmarks of profound mysticism. By the time Taylor realized she was having a stroke, finding the phone number of her colleague, dialing it, and pleading for help had become a task of immense difficulties; the parts of her brain that enabled her to negotiate the external world were rapidly falling apart. In the midst of her struggle and growing fatigue, however, she also noticed a remarkable change taking place: "... I was consistently distracted by an enveloping sense of being at *one* with the universe ..." (p. 54). She could no longer distinguish writing as writing or symbols as symbols; memories of her empirical self were washed away, the sense of her physical boundaries vanished, along with her internal clock; she ceased feeling like a solid being but perceived herself as something fluid and diffuse. Engulfed by a growing bliss, she still clung to the vestiges of her left-brain idea of who she was. As the left-brain chatter involuntarily died down, fear and pain retired to the background of her consciousness.

Once she could discriminate between her traumatized left-brain self and the vast right-brain consciousness that was unfolding, she felt despair at having survived her stroke, and yearned to cut loose from her shattered body. (This reaction is reminiscent of near-death experiencers.) "I felt like a genie liberated from its bottle," she writes. "The energy of my spirit seemed to flow like a great whale gliding through a sea of silent euphoria. ... As my consciousness dwelled in a flow of sweet tranquility, it was obvious to me that I would never be able to squeeze the enormosity of my spirit back inside this tiny cellular matrix" (p. 67).

Dr. Taylor's cerebral accident (due to a genetic arteriovenous malformation), achieved what mystics the world over try to achieve by means of fasting, sensory and conceptual reduction, and countless other techniques practiced from time immemorial to induce higher states of consciousness.

Her insight? This is how she put it: "My stroke of insight is that at the core of my right hemisphere consciousness is a character that is directly connected to my feeling of deep inner peace. It is completely committed to the expression of peace, love, joy, and compassion in the world" (p. 133). She describes various practical consequences of her experience, and sketches a new worldview, based on her personal discovery of the hemispheric duality of the brain.

Taylor, before her stroke, was an advocate for people diagnosed as mentally ill. This concern took on new meaning in light of her experience. She speaks to a certain mindset, predominantly left-brain in character, that can be more toxic than therapeutic for traumatized or mentally disturbed patients. Although a wreck in her stricken condition to the outward eye, her receptive mechanisms had sprung into high gear. During her near vegetative state she experienced heightened empathy, which sharpened her insight into the value of therapeutic kindness and compassion. Dr. Taylor argues for a more holistic education of medical professionals. Caregivers should train their right-brain circuits and free up their capacity for love while moderating the more abstract and less sensitive left-brain functions.

As testimony to the power of this stroke-induced experience, Dr. Taylor thinks the right brain should be the basis of a *general re-education of humanity*. Her premise for this spectacular claim: “For me, hell existed inside the pain of this wounded body as it failed miserably in any attempt to communicate with the external world, while heaven existed in a consciousness that soared in eternal bliss” (p. 73). Her idea of how to bring this blissful form of awareness into the center of our lives would entail a paradigm change in the conduct of daily life.

In order to grasp from within the values and qualities of right-brain enlightenment, she recommends that we shift from over-reliance on rationalistic “mental chatter” to more esthetic and contemplative modes of thought. Everyday life is the great field of experiment; we need merely to pay attention to the flow of the now to wean ourselves from the debilitating excesses of the left brain. The more we are present to the world, the greater the influx from the right hemisphere of consciousness. The arts, moreover, are tools toward this end, and the great spiritual teachings of the world are there for us to draw upon.

Dr. Taylor’s call for the re-education of humanity around the premise of right-brain consciousness is visionary, with a touch of the messianic. But if her conception is sound—her experience is one piece of testimony for it—we should listen carefully. The idea of a science of enlightenment may seem visionary; but for all we know it may be the wave of the future.

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**Intermediate States: The Anomalist 13** edited by Patrick Huyghe and Dennis Stacy. San Antonio and New York: Anomalist Books, 2007. 188 pp. \$12.95 (paperback). ISBN 978-1-93366-526-9.

*The Anomalist*, which shows up every year or so, is not a magazine, a journal, or a book, but something of all of them. Its various articles are usually

intelligently written and researched, but not scholarly in any formal sense. As the title indicates, its focus—broadly speaking—is on the sorts of mysteries that engage the curiosity of *JSE* readers, though *Anomalist* writers are as concerned with the folklore of anomalies and the paranormal as with their actual manifestations and mechanics. Thus, while certainly no pinch-mouthed debunking exercise, the current issue—11 contributions long—typically is as devoted to things that have been claimed to happen but did not, or at least not quite in the manner alleged.

The proceedings open with John Reppion's strangely fascinating "Suspension of Disbelief: The Great Yarmouth Bridge Disaster of 1845," whose subject could hardly be more arcane: did a clown floating in a tub propelled by geese cause hundreds of spectators to drown when an overloaded suspension bridge collapsed in that English city? Nothing extraordinary—at least in the otherworldly sense—is implied in any of this, but Reppion's investigation is driven by his curiosity about whether a clown known simply as "Nelson" in fact existed and whether a stunt such as alleged could have occurred. If you're curious, you can read the article, which eventually provides an answer, and an amusingly counterintuitive one. It's the kind of item that makes *The Anomalist* so likably unpredictable, as willing to probe esoteric oddities as profound enigmas.

At the other extreme, literally (it's the last piece) and figuratively, is Mark Macy's "In Touch With Other Worlds," which recounts the writer's research into electronic and computer communications with believed-to-be discarnates. It's riveting stuff, but at a considerable distance past my boggle threshold. Consequently, I leave it to others less reflexively resistant to its more than usually fantastic claims to judge its credibility.

Dutch Fortean Theo Peijmans contributes an excellent summation of an obscure Massachusetts legend, "The Black Flash of Cape Cod: True Heir of Spring Heeled Jack." The subtitle refers to a British leaping-ghost tradition that sometimes figures (if dubiously) in UFO-age aliens-in-our-midst ruminations. The Black Flash was, in the words of one account, "an elusive superman, a super-human leaping lizard . . . dressed all in black," which scared the bejesus out of Provincetown from 1938 to 1945. Local rumor held the depredations to be an on-going prank perpetrated by several prominent citizens (presumably with more time on their hands than was good for either them or their community), though the truth—as with the original Spring Heeled Jack tales of the mid-19th century—remains hazy and probably by now unrecoverable. Peijmans offers up speculations about how seemingly impossible feats of jumping could have been accomplished, but one wonders if the story isn't simply too nebulous to merit the effort.

In "The Flying Saucer That Never Was," Nick Redfern links a phony Hollywood-generated story of a UFO filmed in Alaska in 1948 with some remarkable real sightings in the same general time and place. The latter, he writes, accounts for the otherwise-inexplicable official interest in the former, concocted as a promotional scheme for huckster Mikel Conrad's Grade-Z 1950 science-fiction flick *The Flying Saucer*.

Other notable contributions include Victoria Alexander's extended inquiry into "Medieval Mysticism and Its Empirical Kinship to Ayahuasca." Alexander's charmingly good-humored tone, I presume, is intended to sooth the reader's stomach. Some of the appallingly unappetizing practices described—vomiting figures prominently, and it's almost the least of it—may generate distress in the more susceptible digestive system. The closest thing to a straight popular-science article is Sharon Hill's "Whispers from the Earth: Can Science Grasp the Hints Before an Earthquake?" Ulrich Magin answers the question posed in the title "Sargon's Sea Serpent: The First Sighting in Cryptozoology?" in the negative, but interestingly so.

Any issue of *The Anomalist* is welcome, but this one is even more entertaining than most. Here's to the next issue and—let us hope—more to come.

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**The Body Electric: Electromagnetism and the Foundation of Life** by Robert O. Becker and Gary Seldon. William Morrow & Co., 1985. 364 pp. (paper). ISBN 978-0-68800-123-0.

Robert Becker passed away May 14, 2008 at the age of 84. His most famous book is entitled *The Body Electric* (1985) and although a heavily reviewed book, it seemed appropriate to write another review on the occasion of his passing.

*The Body Electric* is one of those classic books that can be read, and then read again, with each reading opening up new wonders. Breakthrough discoveries can be gleaned from its pages as the book takes the reader into hidden and diverse areas, some of which continue to remain unexplored.

Despite its two authors, the book is written in the first person, and is substantially chronological. When it deviates slightly, the chapters are arranged by subject matter, so the reader is able to follow along both chronologically and substantially. This makes the book especially easy to follow. The story, however, is not so much about the life of Robert Becker as it is about the scientific life of a serious researcher.

*The Body Electric* begins with a lamentable introduction to penicillin. Lamenting not the saving power it offered to those who were sick, but the impact it had on the philosophy of medicine. Becker reminisces that medicine became very mechanistic, too biochemical, and sorely lacking in innovation once penicillin hit the medical scene. Becker comments that most doctors who have graduated since 1950 have never even seen pneumococcal pneumonia in crisis. As I write this review, this would include almost all doctors today.

This difficult past helped to shape Becker, the medical student, as well as reshape the direction his research would eventually lead. In an effort to move away from a purely chemical way of treating patients, he wandered into the field of bioelectromagnetics, the study of electromagnetic fields on life. This scientific field was not as developed as it is today, so Becker is credited with being one of its pioneers. He used bioelectromagnetics in order to study bone healing, and did so with wildly successful results. In addition to bone, he would eventually open up to the more generalized field of regeneration, and investigated the head of the hydra, the body of the flatworm, the legs of the salamander, and finally skin regeneration in man. As a scientific field, regeneration is relatively new. The first description of it came in 1712 from a French scientist, René Antoine Ferchault de Réaumur, and it would be remiss of this entomological reviewer not to mention that this Frenchman studied insects and other invertebrates during his productive scientific career.

Although Robert Becker earned an M.D. and not a Ph.D., he possessed a penchant for research. This desire to conduct research led him to find the truth, which unfortunately, offered no guarantee of popularity among his peers. Forced out of research in 1980, Becker was unable to continue his groundbreaking research in bioelectromagnetics. The shortening of Becker's scientific career did not adversely affect the quality of the work he completed, nor the book that so wonderfully came as a result of it.

Because Becker had his medical degree, and because he was not a full-time researcher, he did see patients occasionally. He was a devoted researcher and this research philosophy permeated his practice. He was often trying to improve someone's life utilizing the scientific method, rather than writing a drug prescription, shaking the patient's hand while shuffling him out the door, and then sending him a bill. He writes, "I've spent far too much time on a few incurable patients whom no one else wanted, trying to find out how our ignorance failed them." One memorable case for this reviewer was of a man who suffered from a broken leg that had not properly healed despite bed rest for over a year. When Becker was introduced to him, he let the patient know in advance that he wanted to try something new, but could not guarantee the outcome. Becker wanted to implant some electrodes and apply a tiny electric current directly to the bone. Becker's results needed no apology as the man improved immediately; the bone began to grow, and he gained the use of his leg within a few short months. This type of success is not common, but it speaks well for maverick researchers who are willing to move out of their comfort zone and challenge prevailing dogma.

As a researcher, Becker discovered the existence of electrical currents in parts of the nervous system. These are not the usual neuronal pulses familiar to many scientists, but direct currents (DC) in the glial cells. Prior to his work, the glial cells had been considered "blankets" for the nerve cells, their function beyond protection and insulation being unknown. It is unfortunate that universities still teach that glial cells are mere insulators for the neuron. Becker writes in regards to generally accepted theories:

Science is a bit like the ancient Egyptian religion, which never threw old gods away but only tacked them onto newer deities until a bizarre hodgepodge developed. For some strange reason, science is equally reluctant to discard worn-out theories, and, even though there was absolutely no evidence to support it . . . (p. 45)

and he continues

After all, you can disagree with a theory, but you should respect the data enough to check them. If you can't duplicate them, you're entitled to rest easy with your own concepts, but if you get the same results, you're obligated to agree or propose an alternate theory. (p. 84)

The small amount of electricity in the glial cells is important for many life functions, but most notably regeneration. For example, Becker was able to dedifferentiate certain somatic cells with a very small charge of electricity (200–700 picoamps). These dedifferentiated cells could then develop into any one of a number of alternate cell types. This regenerating ability has parallels in modern research, which has shown that adult stem cells can cure or alleviate almost 100 different ailments.

He also contributed to the field of anesthesia. Becker discovered he could anesthetize salamanders with currents and then have them regain consciousness in a matter of seconds. This method contrasts sharply with other forms of anesthesia. I have personally heard of this type of anesthesia being used today, but not in this country.

Once while attempting to treat a patient, Becker decided to switch the more common stainless steel electrode to a silver electrode. The desired effects were to use a less reactive metal as well as to more efficiently transmit the electrical current. But the side effect led to the re-discovery of a beautiful antibiotic (silver) and the dedifferentiation of many cells that subsequently rearranged to form clean, healthy tissue. This in turn led to the discovery of utilizing a silver nylon mesh to treat osteomyelitis (bone infection), which in turn led to the accidental discovery that this treatment healed bones quite nicely. Becker commented:

Whatever its precise mode of action may be, the electrically generated silver ion can produce enough cells for human blastemas; it has restored my belief that full regeneration of limbs, and perhaps other body parts, can be accomplished in humans. (p. 175)

More amazingly, Becker observed complete regeneration in a newt's heart so long as a minimum of 30% of the heart was removed or severely traumatized. Less than 30% damage produced less than impressive results. Becker learned that massive trauma was not just sufficient, but necessary. Becker calls this the Polezhaev principle, which states, the greater the damage, the better the regrowth (named after Lev Polezhaev who spent his career investigating this seeming contradiction). To Becker's surprise, regeneration was found to be well under way 15 minutes after removing most of the heart and then completely healed, without scar tissue, in about a day. Amazing discoveries like these are sprinkled throughout the book with a lively scientific discussion as to how he arrived at these unsuspecting discoveries. Sometimes he got lucky, and he admitted that. Sometimes

tremendous results were achieved through common sense. All of these scientific discoveries were described in such a way that anyone with a background in high school biology would be able to follow along. No previous background in regeneration nor in bioelectromagnetics is necessary to read *The Body Electric*. This book is truly written for the public and helps to explain its popularity.

*The Body Electric* is, in my opinion, a scientific odyssey. A good scientific story is a page-turner, and this book will not disappoint. Becker and co-author Gary Seldon weave a scientific drama that includes his own research, his own thoughts, but at the same time builds these on the research of others, who are duly named. Becker has thoughts about morphogenetic fields, acupuncture, the connection between brainwaves and atmospheric waves, biological semiconductors, extrasensory perception, Kirlian photography, and the Shroud of Turin. I could unreservedly recommend it to any fellow scientist, no matter their field. To the non-scientist, there is enough in *The Body Electric* to entertain since politics and personalities are woven into his stories and these will hit home to many of us in the SSE (also read *The Emperor of Scint* [2002] by Chandler Burr, reviewed by Henry Bauer in *JSE* 17[3] for a similar but more political story).

*The Body Electric* ends with a change of pace. Moving away from the personal stories of research inquiry in the first three-quarters of the book, it then switches to recent issues surrounding electromagnetic pollution. The book takes a noticeable change and the tone is not as enjoyable to read. True, he makes good points in regards to electropollution, but this reviewer found the tone dry and a laborious read; the tone of discovery morphed into a tone of speculation. Now instead of talking about his own research, exciting as it was, many of the studies he quotes later in the book are not his own, and there are many he quotes. The end of the book starts to read like a long list. The list covers individual research projects, their results, and maybe a brief commentary by Becker. This part of the book should have probably been removed, or at least saved for another book. I understood better why this ending was chosen when I realized that another book dealing with electropollution had been penned by Becker, *Cross Currents: The Perils of Electropollution, the Promise of Electromedicine*, although nowhere as near as popular as *The Body Electric*. *The Body Electric* ends essentially as an introduction to *Cross Currents* so I cannot fault Becker if this was his intended goal.

Becker made an important point, however, in regards to experiments in which cells or organisms are exposed to a single unmodulated frequency, as opposed to more complex modulated frequencies. Although these experiments may provide some useful answers in a laboratory setting, they are irrelevant outside the laboratory. They are most often conducted by researchers whose only goal is to be able to say, "See, there's no cause for alarm." While attending a Bioelectromagnetics Society meeting in 1998, I noticed with dismay that almost all the research on single, unmodulated frequencies had no effect on the test organism, whereas studies involving modulated frequencies almost always had observable effects.

Becker's now prophetic observation is still valid today, and I consider it a shame that we have not corrected this seemingly blatant error.

I found my interest returning while reading the final chapter entitled "Post-script: Political Science." While *The Body Electric* weaves a truly fascinating story on the excitement of discovery, there is also the element of politics. I refer not only to governmental politics, but also scientific politics, which is not so familiar to many in this country, but has been the main subject in other important books (i.e., *Profscam* by Charles J. Sykes [1988]). There are plenty of examples of scientific dishonesty littering the book, so many of us in the SSE will nod our heads at some of Becker's experiences with the implicit understanding of having "been there and done that." Our respective scientific field might be different than Becker's, but the politics are identical.

Since Becker worked at a university and applied for grants, he was continually subjected to the domineering spirit of peer review that seeks to squelch true advancements in science. The excuse used by the scientific establishment is that they only wish to fund reputable science, but in so doing, fund "safe" science that is largely boring, uncited, and irrelevant. Becker's experiences help to bring some of that excitement back into research. *The Body Electric*, although penned in 1985, should be brought back as required reading for scientists today.

Thank you for presenting this research synopsis and congratulations on your reward, Dr. Becker. May you rest in peace.

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**Forbidden Science: Volume Two: Journals 1970–1979 The Belmont Years** by Jacques Vallee. Documatica Research, LLC, November 2008. 547 pp. \$44.42 (hardcover). ISBN 978-0-615-24974-2

A decade and a half after publishing *Forbidden Science: Journals 1957–1969*, his groundbreaking book about the early days of Ufology, Jacques Vallee has presented the public with another insightful work. *Forbidden Science: Volume One* detailed the events of the late 1950s and 1960s. *Volume Two* addresses the next decade in fascinating detail.

The format is that of a diary, with entries sequentially spaced every few days for the entire decade. The information is not limited to UFOs, but also includes other significant events in Vallee's life, as well as some of his family members. Those who know Vallee are well aware of his involvement in the development of the field of computer science, and events leading to what is commonly accepted today as the Internet. Therefore, he has included substantial background about

advances in information technology. As a diary, the reader learns quite a bit about Vallee's personal life. That even includes commentary on French cuisine and selected restaurants in which he dines. Also included are a few historic markers of major world events that provide both temporal context and background pertaining to the environment in which his research projects were conducted.

Members of the Society for Scientific Exploration (SSE) are treated to an extensive *Who's Who* of the formative membership of our organization. Vallee informs us of the pre-conception inquisitive activities of many of the key personnel of the SSE. Among those in the Stanford area with whom he worked extensively are Peter Sturrock, Hal Puthoff, and Russell Targ. In Part Six of this book, "Psychic Underground," Vallee describes how closely the historical study of UFOs and other psychic phenomena are intertwined. In particular, readers are provided a little known perspective about the development of remote viewing at SRI, the experimentation with Uri Geller, and intellectual curiosity of lunar astronaut Edgar Mitchell.

Worth noting is an experiment conducted with Uri Geller at Lawrence Livermore National Laboratory. While the desired effect of bending metal without touching the material was achieved, it was another observation that may have been equally significant. The infrared cameras that were placed to ensure Geller had no physical contact with the metal object captured unexplained coherent light beams of high intensity bathing him during the experiment. The source of the illumination could not be determined, despite repeated attempts to identify them. As Vallee notes, although both effects were scientifically significant, there was no published report of this unique experiment. This lesson will not be lost on SSE members.

Even those well versed in these topics are likely to be amazed at the intricate interpersonal relationships that formed and influenced the evolution of phenomenological research. We would not be where we are today without the architectural pressure of these pioneers. The list of names is extensive and well worth exploration by young researchers who are interested in the formative factors in the study of various phenomena.

Of course the UFO discussion includes extensive material about the U.S. Air Force chief scientific consultant, Dr. J. Allen Hynek, with whom Vallee was at times closely associated. This research period includes both case studies and the eventual publicity that attended Hynek following the publication of his books, *The UFO Experience: A Scientific Enquiry* (1972) and *The Hynek UFO Report* (1977). There is also mention of Vallee's meetings with Steven Spielberg during the development of his blockbuster movie, *Close Encounters of the Third Kind*, in which the character of the French scientist, Claude Lacombe, played by François Truffaut, is tailored after Vallee.

There are physics and physicists of all ilk. These range from the most rigid, like John Archibald Wheeler, who acted aggressively to evict consciousness studies from the halls of science, to the very flaky, some of whom he does name. Then there are mysterious deaths, such as SRI remote viewer Pat Price, and UFO

investigator Jim McDonald, plus intrigue and murder in the exploits of Ira Einhorn who later became an international fugitive. Conspiracy theories abound as Vallee had encountered many of them both in the U.S. and abroad. His exploration covered the nutty, such as The Two (Bo and Peep) who emerged very publicly as contactees, to the tragic Marshall Applewhite, who later led his Heaven's Gate cult to mass suicide. He cites the Philadelphia Experiment as a "ludicrous myth," delves into the occult world of Aleister Crowley, and tackles the UMMO hoax that gained global notoriety.

Vallee's experience with members of the Intelligence Community (IC) evokes a sense of both fascination and caution. His extensive relationship with Dr. Christopher "Kit" Green, then a CIA Life Sciences officer, is central to his ventures into the IC. While many UFO buffs might know of Kit, few are likely to be aware of other important, but lesser-known figures, such as Art Lundahl, the director of the National Photographic Interpretation Center, or NPIC as it was called by those who even knew of its existence. When Vallee interacted with these various agencies, he was surprised to learn that he knew more than they did about the UFO topic. More importantly, he came to understand that each agency was more interested in what the others were doing about the field than in the UFOs themselves. All seemed to believe that the other agencies, including friends, were holding information back. In reality, this may have actually foreshadowed the findings of the 9-11 Commission, which determined that interagency rivalry was a key factor in lack of sharing of critical data which prevented them from identifying the Al Qaeda attack.

The perspective of projects is as perceived by Vallee, and includes personal observations pertaining to the status of relationships between people. These are not always smooth and harmonious. Not infrequently he elucidates his own emotional state as it relates his wife, Janine. One criticism is the need to have a *program to tell the players*. Vallee often uses only first names, and those not totally familiar with the participants may have some trouble keeping track of whom he is speaking. Considering the breadth of scientific fields covered, it is easy to get lost in those topics with which the reader is not familiar. There are a few characters, usually personal friends not involved in research, who are only identified by first name. There is an index that will assist the reader, but the accuracy is not 100%.

The conclusions in this book are consistent with Vallee's other publication. Some of these include

- The phenomenon is real but it offers multiple levels
- No simple extraterrestrial explanation fits the facts
- Some governments have keen interests, but no scientific research projects seem to exist
- No solution will be found by mediocre, amateurish research

For truth in advertising, I should mention that I have known Jacques Vallee for several decades and consider him a personal friend. In general, I agree with

his conclusions, especially that the UFO phenomena are terribly complex and no single solution is likely to provide an explanation that fits all of the facts. However, we differ slightly on the role of governments in studying UFOs. My position is that while there has been periodic official scrutiny, by far the most significant reports can be attributed to intense personal interests of individuals who happen to serve in those agencies. One thing that comes to light is the episodic recurrence of UFO studies within U.S. Government agencies, often at the instigation of an internal champion supported by likeminded players. Readers are invited to examine the material and make up their own mind concerning the depth of involvement any agency might have.

*Forbidden Science: Volume Two* is essential reading for serious UFO and phenomenology aficionados. To quote Hal Puthoff, "I'm sure glad somebody was taking notes." Now that Vallee has brought us up through 1979, let us hope he will quickly fill in the next decade.

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**Flying Saucers and Science: A Scientist Investigates the Mysteries of UFOs: Interstellar Travel, Crashes and Government Cover-ups** by Stanton T. Friedman. Franklin Lakes, NJ: The Career Press, 2008. 317 pp. \$16.99 (paper). ISBN 978-1-60163-011-7.

Stan Friedman has been talking openly about flying saucers and UFOs for 42 years, presenting his own book review of Frank Edward's *Flying Saucers—Serious Business* in 1967. I was privileged to meet him two years later, and have followed his remarkably determined career sharing his views with everyone who cares about the UFO problem. This book is a great summary of what he has been saying about the topic over the years, and it displays his natural tendency towards very logical thought. His goal has been to try to shed light where there is ignorance and to back it up with impressive evidence of research, with credible references to meticulous hours spent in archival libraries all supporting his arguments.

The scope of the book includes discussion of most of the questions that both ordinary people and scientists would typically raise. Most UFO researchers encounter these questions right away: where do they come from, why are they here, why the cover-up, why don't they land on the White House lawn, why aren't our scientists excited, and are there documents that prove there has been a covert program? Stan deals with the logical answers to all of these. The book is organized generally along the lines of the answers to these questions in the first 10 chapters, finishing with Chapter 11, "The Operation Majestic 12 Documents."

Like many of the topics that *JSE* deals with, this one is controversial, and Stan and the book are at their best dealing with the skeptics who have not done their “homework.” He identifies the flawed logic and incomplete research not only with the classical skeptics such as Phil Klass, but also with the scientific skeptics like Robert L. Park (former Executive Director of the American Physical Society), Seth Shostak, Jill Tarter (current head of SETI—the Search for Extraterrestrial Intelligence), and Carl Sagan, with whom he was a contemporary at the University of Chicago. No one is safe from Stan’s incisive logic with his fascinating encounters with renowned science fiction writers Isaac Asimov, Ben Bova and Arthur C. Clarke, each of whom is the loser of the hypothetical argument with Stan.

Scientists who study anomalies are sometimes accused of representing a “cult.” Because ad hominem attacks are not infrequent, especially in UFO research, he takes the trouble to define the word in a logical way and then goes on to show that some of the skeptics are more cult-like than the UFO “believers” by applying it to the SETI program. Cults have charismatic leadership, strong dogma, tend to ignore or repress testimony opposed to their beliefs, and have an enlarged view of their importance. Stan’s summary: “Case closed.” This is discussed in credible detail in Chapter 5, “The Cult of SETI.”

Stan’s healthy skepticism is displayed in many examples: checking the claimed academic background of questionable witnesses to reverse engineering; basing many of his points on research he has personally accomplished at Government Archives; and being prepared to go to any level of detail in connection with claims of authenticity of documents, e.g., document control numbers, military rank confusion, typewriter fonts, date format, and apparent emulation suggesting fraudulence.

This is a very good book for academics to read because one of Stan’s breakthroughs was to determine very credibly that astronomer Don Menzel of Harvard did indeed have connections into a highly classified world of UFO study while maintaining a debunking position in publications and books. The procedures of classified projects and the ability of the various agencies to keep them secret from the general public is something that many academics are unable to grasp unless they have been directly involved in a top secret project.

The last chapter on the Majestic Documents is particularly interesting because it deals with aspects of more current research about the authenticity of some of the documents. He presents convincing evidence for the authenticity of several documents that reveal a comprehensive Government evaluation project and claims that some others have been faked. This reviewer believes that because of the large number of documents of this genre, it is necessary to apply a variety of tests to the leaked or faked documents individually, and that there is a lot of work still to be done before we can confidently describe the genuine history reflected by some of the Majestic Project documents.

Notwithstanding the many excellent attributes of the book, the title is a bit misleading. One might logically expect that if flying saucers are real and have the

remarkable attributes reported, then we would wish to understand how they work. Never does Stan try to deal with any of the scientific aspects of the genuine debate about how our laws of science would have to be modified, other than to refer briefly to the work of Puthoff, Haisch, Maccabee, or Deardorff, all SSE members. Chapter 2, “You *Can* Get Here from There,” is limited to essentially established scientific principles, albeit with fascinating detail on our pre-1960s aircraft nuclear propulsion work with which he is intimately familiar.

The well-written and highly applicable forewords by Edgar Mitchell and Bruce Maccabee offer helpful perspective. A comprehensive bibliography and a solid index reinforce the quality of presentation offered by this book, an excellent summary of the thought processes of one well-known flying saucer researcher, Stan Friedman.

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**Body Snatchers in the Desert: The Horrible Truth at the Heart of the Roswell Story** by Nick Redfern. Paraview-Pocket Books, a division of Simon & Schuster, 2005. 256 pp. \$14.00 (paper). ISBN 0-7434-9753-8.

It's obvious that films based on comic books are among the most successful in Hollywood's current output. They feature everything that the prime moviegoing demographic finds most appealing: violent action, gaudy special effects, gadgets, mutants and aliens, and especially stories of secret identities, government conspiracies, conflicted motives, and the apocalyptic struggle of good versus evil. Often, too, there are Nazis.

Modern writers and producers, aware that even the most popular characters can become overly familiar and stale over time, have come up with the concept of “rebooting” the hero and his “origin story”: rewriting his universe from the ground up, updating locales, and incorporating modern cultural references to make the story relevant to the sensibilities of young audiences. The fundamental characters may be essentially the same, but the stories grow more elaborate. Entire plotlines can be modified or eliminated completely. Inconvenient inconsistencies can be voided. Historical figures and events can be intertwined with fictional ones. Elaborate “backstories” can be added to flesh out unexplained aspects of the original world.

Another term for this technique is “retconning,” short for the phrase “retroactive continuity.” First recognized by comic book connoisseurs, retconning has become integral to the modern entertainment industry—a commercial necessity to ensure maintenance of market share and franchise viability.

British author Nick Redfern's book *Body Snatchers in the Desert* represents an ambitious retconning of the well-known Roswell story. Constructed as a journalistic reexamination of the events in New Mexico in 1947, *Body Snatchers* tells, in Redfern's words, a "distinctly darker" version of the facts—one based on his claim that what actually occurred was not the crash of an extraterrestrial spacecraft, but a coverup of a bizarre program of top secret weapons experiments and their effects on hapless human victims. This retcon allows for preservation of the "real world" aspects of the case—the 1947 media stories, most of the eyewitness testimony—while providing a completely alternative deeper explanation for them, one that incorporates a panoply of comic book stock villains and contemporary cultural bogeymen, plus a lot of cool technology, mutants, and Nazis. This reboot of the mythos is more appealing to modern tastes, since it carries none of the negative associations of "little green men" but presents a superficially more believable alternative scenario involving Pentagon "black programs" and US politico-military perfidy.

Arguably, the "classical Roswell" story drew much of its power from 1980s hostilities to governmental secrecy stimulated by Watergate and the intelligence scandals of the 1970s. Redfern's Bush-era reboot is far more sophisticated than Stanton Friedman's graying meme that the official UFO coverup is a "Cosmic Watergate"—much more in line with ultra-jaded post-9/11, post-anthrax, post-Iraq conspiracy theories about U.S. "false flag operations" on massive scales.

What Redfern presents in *Body Snatchers* is a grand synthesis of several genuine WW II and post-war experimental projects, including the following:

- Project Paperclip, the importation of German scientists to the US
- Horrific Japanese biological warfare experiments by "Unit 731," and similar atrocities perpetrated by the Nazi researchers on human victims
- The German Horten flying wing projects
- The Japanese Fu-Go balloon-bomber program
- The U.S. Skyhook high-altitude plastic balloon program
- Radiation experiments on human subjects in the United States during the Cold War
- The Nuclear Energy for the Propulsion of Aircraft program (NEPA)

He provides considerable background detail on each of these threads, establishing an air of documentary verisimilitude, and includes an appendix featuring actual papers related to them. But the weakness of the story is that the "horrible truth" about Roswell referred to in the book's title only comes to light thanks to a trope familiar to all *X-Files* fans: the Anonymous Whistleblower.

As Redfern tells it, he was launched on his investigation after he was contacted by several of these dubious characters, designated "Levine," "the Black Widow," and "the Colonel," who he encountered in 1996, 2001, and 2003, respectively. Levine, a British Home Office employee, told Redfern that in 1989 he had been

shown, by representatives of British and U.S. intelligence agencies, a movie that sounds a lot like the infamous *Alien Autopsy* film. The Ministry of Defence (MoD) intelligence officer informed Levine that this was in fact an autopsy of an alien body. Levine says the British agent later reversed his story and claimed that the film was part of an elaborate multinational disinformation project (a joint effort of the U.S. National Security Agency [NSA], the U.S. Air Force Office of Special Investigations [OSI], and the Royal Air Force's Provost and Security Services) designed to contain the "real truth" behind Roswell! According to Levine, the MoD official claimed that since UFO investigators were verging on discovering the sinister reality at the core of the Roswell story, the intelligence agencies had conspired to fabricate documents and information that represented a "limited hang-out" of factual information mixed with complex and fanciful fabrications, all designed to confuse and distract future civilian investigators. The MoD source, for whatever reason, was leaking this to Levine, who was in turn passing it to Redfern.

As a professional investigative reporter, and far more sophisticated than many researchers, Redfern doubted that his unsubtle and talkative source was genuine—that is, until he encountered the Black Widow, who claimed to have worked at the NEPA project at Oak Ridge, Tennessee, and said she had seen several "Oriental" bodies with "devastating injuries" that were brought to the site from New Mexico in 1947. She told Redfern that these dead bodies were no aliens at all—they were Japanese people. Victims of a horrible conspiracy, these individuals were deformed survivors of unspeakable Second World War experiments who had been collected by the U.S. and strapped into an experimental, balloon-launched vehicle that was part of the nuclear-powered aircraft program. This bizarre contraption, the story went, was an amalgam of German flying wing gliders, Japanese balloons, and U.S. prototype atomic airplane engines, intended to test concepts for actual NEPA aircraft. When it crashed, traumatically injuring the mutant crewmembers, it was only natural that the U.S. authorities were determined to cover the whole thing up because of the tremendous secrecy of the nuclear energy projects and the horrendous scientific atrocities (both Japanese and American) represented by the human victims. The alien UFO story, just as Levine had said, was only the cover story for the horrible truth of human nuclear and biowarfare experiments.

Topping off the Whistleblowers was the Colonel, who approached Redfern with an expansion on the previous accounts. This alleged former Defense Intelligence Agency officer claimed to have seen voluminous documentation dating from the 1960s on a long-term "psyops" disinformation project aimed at hiding the Japanese-mutant-victims-launched-in-experimental-NEPA-balloons story. It was all designed as far back as 1951, according to the Colonel, to be released to the Soviets to "flood their intelligence channels with disinformation" to prevent their discovery of the sinister and illegal biowarfare and nuclear experiments. The secondary victims of the "blowback" from this operation, said the Colonel, would

be naïve ufologists and the U.S. public, who would be conned into believing in nonexistent aliens and UFO crashes.

The conspiracy, then, extends back to the Truman era itself, and involves intelligence agencies on both sides of the Atlantic—if the tales of the Whistleblowers are true.

What can be made of this?

The issue is not whether the experimental programs listed above existed. They did. Assuming Redfern's accounts of the Whistleblowers' stories are accurate, the issue is whether the fantastic story of the Japanese victims is true. My opinion is that it is utterly absurd. I have a personal interest in the NEPA project and have collected a considerable amount of documentation from official sources on the actual hardware that was developed by the program. If these reports make anything clear, it's that as of 1947, NEPA envisioned any future operational nuclear aircraft as being enormous and tremendously heavy. NEPA based its studies on the Convair B-36 bomber, which had a wingspan of over 200 feet and weighed almost half a million pounds. The conceptual nuclear engine and its radiation shielding weighed on the order of fifty tons. The idea that any sort of meaningful experimentation could have been conducted on a vehicle lifted by balloons is ludicrous. The documents also make it abundantly clear that as of 1947, the NEPA project had barely started its work. There are countless other technical arguments that contradict the nonsensical "cobbled-together Nazi flying wing hanging from balloons" idea. Why would malformed mutants be used for the experiments? Wouldn't healthy subjects, which would more closely resemble actual crewmen, be more scientifically useful? If the experiments were to test radiation shielding technology with simulated nuclear engines, why couldn't they be conducted on the ground? (In reality, they were.) Why would they be conducted with such a jury-rigged assembly of illogical, unreliable components?

But to even discuss these facts is to dignify the comic book yarns of Redfern's sources with a level of seriousness they in no way deserve. The new story is a clever retcon of Roswell that can gain some media attention, but it makes no sense, adds no clarity, and is fundamentally no more believable than the versions it tries to replace.

The central question about *Body Snatchers In The Desert* is, what are these sources trying to accomplish? Is there in fact any reality in this wilderness of mirrors? Is Redfern's account of his sources' stories itself a product of journalistic license? Is this retconned version of Roswell merely an attempt to milk another few years out of the tired UFO crash story, or is it yet another official attempt to confuse the issue with yet another complex layer of deception? In order to believe that, we would have to accept that the sources are accurate about one thing: that an elaborate deception project has focused on Roswell for decades. The *Alien Autopsy* film and the MJ-12 documents are then all lies concocted by military intelligence forces. The Roswell eyewitness accounts have been completely misinterpreted. But logically, the *Body Snatchers* tales are bogus too. The reader is

left to decide which story is more plausible: the coverup of the crash of an alien vehicle, or the coverup of heinous war crimes involving Japanese mutants dangling from balloons. Neither is very attractive. The disinformation project may, after all, have been a success. It will be interesting to see how the next permutation of the Roswell Retcon takes shape, and what new layers will be added to the story the next time it is revived.

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**Body Snatchers in the Desert: The Horrible Truth at the Heart of the Roswell Story** by Nick Redfern. Paraview-Pocket Books, a division of Simon & Schuster, 2005. 256 pp. \$14.00 (paper). ISBN 0-7434-9753-8.

Nick Redfern is a British journalist whose career has focused primarily on ufology and Fortean interests. Having previously authored several best-selling books on government intelligence and UFOs, Redfern was uniquely qualified to investigate what is arguably the watershed event behind modern UFO interest: the crash of an unknown object near Roswell, New Mexico, in 1947.

For Redfern, events at Roswell had nothing to do with intelligent extraterrestrial life. Rather, the incident involves the intersection of highly classified activities on the part of Imperial Japan, Nazi Germany, and the United States during WW II and the years immediately following. Briefly, these programs were as follows:

1. **The Japanese Fugo Balloon Program**—This program involved the use of experimental high-altitude balloons as weapons. The goal was to launch these balloons from Japan in such a way as to ensure that they would be carried by the winds over the western United States. The intended payload on these balloons were lethal biotoxins.
2. **Japan's Unit 731**—Unit 731 was an officially sanctioned and funded bioweapons facility headquartered in Harbin, Manchuria. Headed by Shiro Ishii, Unit 731 has become synonymous with human experimentation for those who know of its existence. This Unit was the potential source of the bioweapons to be used for the Fugo balloon project.
3. **Operation PAPERCLIP**—The now well-known program began under the Truman administration to bring Nazi scientists to U.S. soil for their knowledge and expertise. As Redfern demonstrates, the program eventually included Japanese scientists.

4. **Nazi Advanced Wingless Aircraft/the Horten Brothers “UFO” Development**—This refers to the work of Reimer and Walter Horten, mainly toward the end of the war, to produce a flight-worthy wingless disk aircraft. Although models were tested and commissioned by the Luftwaffe in 1944–1945, the war ended before the Nazis could more perfectly develop and mass produce the model. The Horten craft was discovered by the British, after which the Horten brothers were invited to Britain to continue their work.
5. **NEPA (Nuclear Energy for Propulsion Aircraft)**—This program focused on developing nuclear energy for the propulsion of aircraft. One of the significant obstacles with achieving the project goal was shielding pilots from radiation. Redfern argues that, in the wake of the 1947 close of the Nuremberg trials—which called for the end of human experimentation—those involved in our testing program felt rushed to gain official permission to use human subjects. Redfern demonstrates that attempts were made to procure legislation allowing human experimentation and that certain “specimens” (possibly mongoloid children or progeria victims) were procured from Formosa, home of Unit 731.

The intersection of all these elements produces a simple but complex answer as to what crashed near Roswell in 1947. The disc-shaped craft referred to by witnesses and various de-classified and unprovenanced government documents published prior to Redfern’s work was in reality a wingless craft launched from a U.S. base in the southwest via a high-altitude Fugo balloon. Two or more of these crafts crashed at Roswell and other nearby locations, explaining the multiple site and date problem referenced by debunkers. The bodies reported over the years by Roswell Army Air Base personnel and a few civilians were human, but physically unusual to those who discovered them. Redfern speculates that the victims were small and perhaps Asian, or even children who suffered from progeria or Turner’s syndrome (which involved baldness, enlarged head, and polydactylism). The UFO explanation was deliberately leaked to the public to deflect attention away from the fact that Nazis and other Japanese war criminals were on the U.S. payroll. After the late 1940s, the UFO scenario proved useful for misdirecting the Soviets, and so the myth was continued. Roswell was, in effect, a PAPERCLIP debacle.

Redfern supports this reconstruction in a twofold way: de-classified documents and unidentified clandestine sources. The former allows Redfern to make a compelling case that secret programs in all these areas were being conducted simultaneously and that personnel in these programs had occasion to know each other and work at more than one of the project sites. Definite links between the projects with the specific goals outlined by Redfern are provided by interviews with project insiders whose knowledge was not unexpectedly compartmentalized. The most important of these insiders did not wish to be identified.

What does all this add up to? On one hand, there is strong documentary evidence for the data points of Redfern's thesis. Redfern has presented a compelling case that potentially has comprehensive explanatory value. Nevertheless, the most important relationships between the data points are ultimately dependent on anonymous testimony. Without unassailable documentary evidence for the connections articulated by his witnesses, Redfern's work is not completely successful in burying the extraterrestrial hypothesis. Those who are not already committed to that view will welcome Redfern's work and the explanatory power it provides. Those on the other side will not be persuaded. But by presenting a thesis that *can* account for all of the "alien" features of Roswell lore, Redfern has advanced the discussion in an important way.

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*(A version of this review appears at the Web site of the reviewer at  
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**The Tujunga Canyon Contacts** by Ann Druffel and D. Scott Rogo. San Antonio and New York: Anomalist Books, 2008. 302 pp. \$16.95 (paper). ISBN 978-1-93-366533-7.

When an old UFO book is reprinted, we revisit two things: whatever data the book might present and whatever thoughts the writer had about that data. When I read this reissue of a book first published in 1980 (and then updated in 1989), or rather reread it since I'd bought it when it first came out, it's the thoughts that struck me the most.

That's not to say that the data isn't worth having. Druffel and Rogo tell us about some apparent abduction cases, linked because they involved a group of women who were friends and in some cases lovers, and who all lived at various times in Tujunga Canyon, northeast of Los Angeles. That the women were gay is something the writers treat discreetly for most of the book, though as we'll see, it jumps up to take center stage at the end.

The cases themselves are standard abduction stuff—unsettling lights, beings by the women's beds, the women floating away to UFOs for what seems to be medical examinations. Some of the abductions (or supposed abductions) were shared. Some of the women remember a lot of what they think happened to them, some remember very little. Some want to examine their memories, some resist. (Since the women involved in shared abductions fall on both ends of these spectra, the shared abductions don't tell us as much as they might.) Hypnosis is used, and the hypnotists don't seem impartial. Sometimes they ask leading questions. Hypnotist: "What's happening now?" One of the women: "Nothing." Hypnotist: "Make something happen."

There's no scientific corroboration of any of this, and sometimes the writers seem credulous. They endorse alleged photos of aliens, taken by Harrison E. Bailey, which have since been debunked. They get excited over similarities with other abduction cases, but they ignore differences. When they compare the beings these women say they've seen with the entities that Betty Andreasson reported (she's a famous abductee from that time), Druffel and Rogo just about gush. "Both types of creatures had extremely thin arms, legs, and body, and had *three-fingered hands!*" [Their emphasis.] But when one of the women says her abductors wore black or charcoal gray clothes, not at all a standard abduction detail, the writers skip right over it.

I can smile at all of this, because the faults are transparently obvious, because I know that abduction research hasn't been done by qualified scientists, and because I myself have encountered abduction stories first-hand and know how gripping they are and how breathtaking their similarities can seem.

But I can't accept Druffel and Rogo's speculations. At the end of the book, both writers (with great respect for each other) present their own conclusions. Druffel thinks (among other things) that the women were abducted in part because they were gay: "Those UFO entities concerned with the reproduction (and evolution) of the human race were possibly investigating to obtain details of this non-procreative life-style [sic]." Rogo thinks one of the abductions was "*a rape fantasy drawn from Sara's mind and objectified into physical reality in the form of a genuine UFO sighting and abduction.*" [His overwrought italics.]

Which makes me sigh. Druffel—to state what ought to be obvious—should first have asked whether gays are abducted more often than heterosexuals in the overall abduction picture (or less often, which might be equally suggestive). Rogo's idea totters on top of two assumptions, which Rogo doesn't (and couldn't) substantiate. Did Sara really have a rape fantasy? He doesn't know. Can fantasies really manifest themselves physically? He couldn't know.

But still I'm touched by this book. Both authors and subjects seem a little lost, as people often do when they're face to face with the abduction phenomenon (whatever it might turn out to be). One of the women (Sara again) thought the aliens taught her a cure for cancer. She tried to verify that cure, and most of all bring it to the rest of us, throwing herself into these efforts with great hope and passion. I feel for her. If she really thought she was abducted, and really thought the aliens showed her the cure, why wouldn't she want to share it with the world?

In fact, I feel for everyone who's had these experiences, and also for people who say they've had close-up sightings of UFOs. Anecdotal accounts, I know, are hardly scientific proof. But when a dozen different people (some of whom have never heard of abductions) independently tell me they've awakened with beings around their beds, and when half a dozen more tell me they've seen unexplainable craft floating in utter silence over their homes, or their cars, or over a farmer's field, what am I supposed to think? What if the stories are true?

And what if the stories in this book are true? Then the lack of real investigation and analysis would truly be something to sigh about.

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**The Invasion from Mars: A Study in the Psychology of Panic** by Hadley Cantril (with a new introduction by Albert H. Cantril). New Brunswick & London: Transaction Publishers, 2005. xxxii + 224 pp. \$24.95 (paper). ISBN 0-915554-45-3.<sup>1</sup>

Everyone knows where he or she was on 11 September 2001. I was sitting in my car early in the afternoon (CET) on that date. Just a short drive, the return trip from a quick visit to the bank. A melody like thousands of others on the car radio. Suddenly, an urgent announcement interrupts the music. A breathless voice reports that an aircraft flew into one of the twin towers of the World Trade Center in Manhattan just a few minutes before. The initial reports would seem to indicate that only a small light aircraft is involved. However, the person in the broadcast, on the verge of losing control of his voice, says that he can clearly see the WTC from his vantage point just a few city blocks away and that the damage must be enormous. There had been a huge explosion. An inferno of flames. Several stories had been destroyed according to eye witnesses. People trapped in the upper stories were desperately waving at those below. Everybody was in complete panic. A studio presenter's voice assures the listeners that there will be an update as soon as exact information is available. The music starts up again. The whole event has lasted little more than a minute.

"Orson Welles", is my first thought. "More than 60 years after he and the actors of his Mercury Theatre had created utter panic among thousands of people, mostly in New York and New Jersey, who were listening to a CBS radio broadcast on 30 October 1938, the evening of Halloween, with Howard Koch's 1-hour adaptation of H. G. Wells' classic *The War of the Worlds*, he's trying it again with another of his dramatic productions." Just like back then, everything sounds both realistic and at the same time unreal. The only thing missing now is the announcement, which was repeated four times in those memorable 60 minutes all those years ago, that this story is fiction.

"But", my second thought, "I'm sure that that great genius Orson Welles died 16 years ago. And I also know that the American broadcasting networks agreed at

the time they would never again transmit fictional news reports on the ether. So why would German radio stations, and ones governed by public law at that, now act differently? This couldn't be a radio play—or could it?—so early in the afternoon? No, very unlikely”. I get home in time to switch on the television and see live footage of a second aircraft—no, not a light sports plane—burying itself in the second of the WTC towers. I don't feel any panic. And how could I? I am not in Manhattan. But I am certainly horrified.

Not even Orson Welles would have dared to create a multi-media spectacle of such monstrosity. But his panic-sowing CBS radio play about a blood-thirsty Martian invasion is at least as legendary as its literary predecessor (Wells, 1898), with barely concealed political intentions that have been almost completely forgotten today. We can refrain from a more detailed description of the original story or its dramatic adaptation here. It is sufficient to point out that the radio programme, with a combination of relaxing dance music and increasingly frequent interruptions and ever more threatening sounding reports of an invasion of fearsome, malicious and uncivilized Martians in Groves Mill, New Jersey, apparently drove many listeners to flee for their lives or caused other panic-stricken reactions. Many people, according to newspaper articles at the time, wandered through the streets and in the parks wrapped in blankets (!) as protection against the anticipated gas and death-ray attacks. On the following day, the newspapers were full of reports on injuries and some deaths caused by heart attacks, miscarriages and even suicides. Parts of the population of the United States in the late 1930s, who, having just survived the Great Depression, were perhaps more fearful and expectant of a German rather than a Martian invasion, appeared to have lost all sense of reality and reacted hysterically to the broadcast.

As a brilliant dramatic touch, Welles, Koch and Houseman, the producer, used a recently introduced technique that had been tried and tested in the same year during the Munich conference and other events, in order to interject direct eyewitness reports. In fact, only a few short weeks before the broadcast, millions of listeners had kept their radios tuned for the latest news from a Europe apparently about to go to war. They had learned to expect that musical programs, dramas, basically broadcasts of all kinds would be cut off in a serious emergency to inform or warn an eager and apprehensive public. However, shortly before the broadcast took place, Orson Welles, who was only 23 years old at the time, and his ensemble considered cancelling the radio play altogether and broadcasting a substitute programme—not because the subject matter seemed too risky, but quite to the contrary, because as producer John Houseman was to say 10 years later, they doubted whether the piece could “be made interesting or in any way credible to modern American ears” (Houseman, 1948: 76).

On the other hand, rumor has it that the broadcast was a psychological warfare experiment in fear conducted by the Princeton Radio Project that was funded by the Rockefeller Foundation from the fall of 1937. An Office of Radio Research had been set up at Princeton University with sociologist Paul F. Lazarsfeld as

director, and Frank Stanton (who also was a CBS executive) and Hadley Cantril, a young Professor of social psychology, as associate directors (Garfinkel, 1987). A mere week after the *War of the Worlds* radio play was broadcasted, Hadley Cantril, who in spite of his youth was an experienced researcher into the social influence of radio (see Cantril & Allport, 1935), seized the opportunity and used a special grant from the General Education Board to study the effects of the broadcast. He recorded interviews with 135 selected listeners whose reaction to the broadcast had been predominantly one of panic, resulting in absurd actions and odd perceptions in some cases. In addition to these extensive interviews, he also performed multiple questionnaire-based surveys and analyzed approximately 12,000 newspaper clippings, which were published in the period immediately after the radio broadcast, and telephone records kept by the police and the radio station. Cantril published his study in book form in 1940 as part of a series of studies sponsored by the Federal Radio Education Committee. New editions were to follow in 1942, 1947, 1966 and 1985. Today, this study is widely regarded as *the* classical, authoritative work on the psychology of panic reactions. After many years of being out of print, the new edition of 2005, which includes an extra, highly detailed and instructive introduction (pp. vii–xx) by Albert H. Cantril, the son of the author who died in 1969, is most welcome. Like the first edition of 1940, the current version contains the literal and complete text of the radio play that Orson Welles directed and played in (pp. 3–44) and the author’s forewords from the 1940 and 1966 editions.

Cantril’s research is of particular significance because—contrary to earlier, partly theoretical studies (e.g., Gudden, 1908; Le Bon, 1896)—it is probably the first study to use unique empirical material resulting from a presumed mass hysteria that had just occurred. The primary goal of the study was to discover why so many people were under the impression that they were listening to eye-witness accounts of an actual, real-time invasion from Mars, while many others, even though they had tuned in later on and missed the first of the four announcements that these events were not in fact happening, had no difficulty in identifying the broadcast as a fictional radio play. Many were apparently too stricken by fear to realise that the time-line of the described events was much too short for them to have actually taken place (pp. 89–102). What Cantril and his team of researchers found most surprising was that people of a low educational level in particular were not made to feel insecure by the broadcast (pp. 120–124), while an astonishing number of well-educated listeners were obviously less able to see through the suggestive power and, for the time, extraordinary technical brilliance of the programme.

The educational level of the listeners and the situational circumstances in which they respectively listened to the broadcast were obviously insufficient to adequately explain the different reactions. So what was the reason why many listeners demonstrated a greater “susceptibility to suggestion” than others? Why did they believe “what they heard without making sufficient checks to prove to

themselves that the broadcast was only a story” (p. 190)? Cantril is certain that the greater suggestibility evidenced by many listeners can be attributed to a lack of self assurance, extreme fatalism, exaggerated concern, control issues or deep religious beliefs. In his analysis, while none of these personal characteristics is responsible for exaggerated, partly panic-stricken reactions on its own, they are certainly capable of making certain individuals more susceptible to panic reactions when they occur in combination. Cantril describes this form of hysterical “susceptibility syndrome” in the following way: “We must infer that some predisposition has operated as a selective force so that some persons are consistently impressed by experiences which leave others unaffected. This particular pattern of sensitivity and the characteristic behavior it determines is a general personality trait” (p. 137).

Probably nobody would subscribe to this surprisingly simple explanation in this form today. The methodology and the statistical analyses of the research, which in 1940 may have been in accordance with normal standard, would almost certainly not be accepted today. And drawing far-reaching conclusions about a general disposition to panic from interviews with a sample population of 135 people, of whom at least 100 were only selected because they had already previously admitted to experiencing panic reactions as a result of the radio play broadcast, would probably not even properly satisfy the methodological requirements that were applicable at the time. As one might expect then, Cantril’s research and his conclusions were challenged to some degree, particularly in the 1980s and 1990s. Based on various surveys and estimates based on those surveys, Cantril believed that of the reported 1.7 million listeners of *War of the Worlds*, approximately 1.2 million could be described as “excited” (p. 58) to a lesser or greater degree. Contrary to this, the number of the few actually documented cases of unusual or “panic-stricken” behavior (collecting together belongings, the over-hasty flight away from the places that had apparently been attacked, arming oneself with firearms) is significantly less than one would expect by chance of more than 1 million people on any other night (Bainbridge, 1987). Miller (1985: 106), for example, concludes that it would also not be permissible to assume a case of mass hysteria based on the limited number of interviewees who had been specifically selected on the basis of their relevance. Furthermore, a 40% increase in telephone calls during and after the radio play broadcast is hardly meaningful, if these phone conversations cannot be broken down and evaluated in terms of their content (Goode, 1992: 315; Miller, 1985: 107). In contradiction to Cantril’s findings, Forman (1963), in his own study, which he performed 25 years later, even diagnoses a general “attitude of resignation” among the American people in reaction to uncertain threat scenarios. The latter finding appears to be supported, at least to some extent, by the results of an (unpublished) study that, again a few years later, Marcello Truzzi conducted on organizational rather than individual responses to the threat of destruction for the Society Under Stress Project for the Office of Civil Defense (Truzzi, 1970).

Something however that is not disputed is that the listeners' reactions, whatever their strengths and whatever the individual variances, were triggered immediately by the broadcast and were therefore directly media-induced. This arguably illustrates the influence that the media can have on the population, particularly in times of general uncertainty. As if that were not enough, the media in this particular case also had a remarkable effect at a secondary level: the newspaper reports about deaths, miscarriages and suicides resulting from the broadcast of the radio play have all subsequently been shown to be false or remained highly controversial (Harrison & Elms, 1990: 214). Furthermore, Cantril's study also provides no evidence that lends any plausibility to the original descriptions of these events. In spite of this, the media (newspapers, radio and television and more recently Internet) have also perpetuated this myth of mass (and in some cases fatal) hysteria and made it part of accepted American folklore. The lesson we learn here is that the media determine reality. Reportedly far more dramatic than the factual effects of Orson Welles' radio play broadcast in 1938 were the consequences of panic reactions to Spanish adaptations of this same radio play, which were broadcast by radio stations in Santiago, Chile (12 November 1944) and Quito, Ecuador (12 February 1949). There were documented fatalities in both cases; for example, 15 people died when an agitated crowd burned the radio station in Quito to the ground (Bulgatz, 1992: 130–137).

The legendary radio play event of 1938 itself, which—even if in distorted and mythologized versions—has become part of our collective memory, the reactions of the listeners and Cantril's study also have a further facet, which has regained particular relevance for the sociological study of anomalies in recent years: About a quarter-century ago, Jan H. Mejer (1983) ventured to suggest exo-sociology as a new sociological sub-discipline, and he asked how “alienity” was socially constructed and what conclusions might be drawn for our understanding of the concept of the extraterrestrial alien. While Mejer's approach was largely ignored at the time, it has found several late followers in recent years (e.g., see Harrison, 1997; Michaud, 2007; Schetsche, 2008; Schetsche & Engelbrecht, 2008; Wendt & Duvall, 2008), and the topic has entered even such an unlikely place as the Vatican (see Valiante & Funes, 2008). In the context of these investigations and discussions, Cantril's study of presumed panic reactions in the wake of the *War of the Worlds* broadcast has re-acquired the status of a standard reference.

These scenario analyses are mainly using sociological, psychological and futurological methods to project and evaluate conceivable social consequences in case a SETI project would actually be successful or mankind would be confronted in any other way with the existence of an advanced extraterrestrial civilization. Potential consequences of contact with an extraterrestrial civilization for life and culture on earth in fact can be considered on the basis of our actual knowledge of terrestrial circumstances such as the psychological constitution of mankind and its forms of social and political organization. The question is not so much whether we will actually find extraterrestrial intelligence, but whether we should want to.

Encounters with extraterrestrials can conceivably come in a variety of ways and with a variety of consequences. They are likely to have dramatic impact on life on earth and its social and cultural organization. The results of the current projective scenario analyses, such as the ones referred to above, remind us that a meeting with intelligent aliens, if it ever takes place, will likely be the stuff of nightmares and possibly even a disastrous experience. If make-believe and suggestively evoked aliens are capable of leaving behind such deep-rooted psychic, social and cultural marks as the ones sketched out by Hadley Cantril, what extremes should we be prepared for when they do actually come knocking on the door, either in peace or in an aggressive attack?

In spite of, but also mindful of, the stated inadequacies of the contemporary research that took place immediately after the radio play broadcast, the primary scientific and historical significance of Cantril's research with regard to the effect of the media is absolutely indisputable. This research continues to be regarded as one of the definitive works on the psychology of individual and collective panic reactions and as such is still worthy of being read today, either again or for the first time. Whoever is interested in the power and subtle (and sometimes not-so-subtle) influence of the media will find copious and informative material in both Cantril's book and in the discussion that it sparked in later years. It is highly recommended reading in particular for starry-eyed SETI enthusiasts who cannot wait for the arrival of extraterrestrial visitors.

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### Note

<sup>1</sup> This book review is an expanded and up-dated version of an earlier review that was published, in German, in the *Zeitschrift für Anomalistik*, 6, 252–257, 2006.

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## ARTICLES OF INTEREST

"Au delà de Paris et Nancy, 'l'École de Charles Richet' selon Pierre Janet: Son impact et ses réseaux, ses membres et son hétérodoxie de l'appel à un congrès international de psychologie (1881) à la fondation d'un Institut Psychique (1900)" ["Beyond Paris and Nancy, 'The School of Charles Richet' according to Pierre Janet: Its impact and its network, its members and their heterodoxy, from the call to an international congress of psychology (1881) to the foundation of a Psychical Institute (1900)"] by Frédéric Carbonel. *Janetian Studies*, 5, 2008. Available online: <http://pierre-janet.com/JSarticles/2008/fc08a.pdf>

This paper was published in the online journal *Janetian Studies* (<http://pierre-janet.com/JanetianStudiesHome.htm>) supported by the Institut Pierre Janet, and devoted to topics related to the life, work, and influence of Pierre Janet (1859–1947). Its author, Frédéric Carbonel, focuses on a suggestion presented by Janet to the effect that, in addition to the Salpêtrière and Nancy rival schools that shaped views about hypnosis and hysteria in late 19th-century France, there was a third group Janet called the "School of Charles Richet." Carbonel includes in his discussion aspects relevant to the history of psychical research.

According to Janet there were nine members of this "hypothetical" school, as Carbonel refers to it. From France there were Henri Beaunis (1830–1921), Alfred Binet (1857–1911), and Charles Féré (1852–1907). The group also included American G. Stanley Hall (1844–1924), Swiss Auguste Forel (1848–1931), German Paul Möbius (1853–1907), Polish Julian Ochorowicz (1850–1917), and English Frederic W. H. Myers (1843–1901) and

Edmund Gurney (1847–1888). In addition to being interested in hypnosis and hysteria, these individuals were characterized by representing various countries, by their contributions to the psychology and physiology of the idea of the unconscious mind, and by their openness to many areas of research, one of which was psychical research. In fact, some of these individuals were members of the London-based Society for Psychical Research (SPR) founded in 1882.

The author starts with a discussion of Charles Richet (1850–1935), a remarkable figure by any account, who contributed to multiple areas of knowledge, including physiology, medical research, literature, the humanities, and psychical research. Six of the members of the school were involved in the Société de Psychologie Physiologique, founded in 1885. A meeting of this organization was what Carbonel refers to as a “major event” for the members of the school: Janet’s presentation of his tests of induction of trance at a distance with the celebrated Léonie. Other important events that involved members of the school were the 1889 International Congress of Physiological Psychology, later known as the International Congress of Experimental Psychology, and the founding of the Institut Générale Psychologique, which in its beginnings, included psychical research. Carbonel also points out that some of the individuals in question were SPR members.

Focusing on psychical research, there are some short but interesting discussions about Ochorowicz, Gurney, and Myers. Carbonel recognizes Myers’ pioneering work regarding the subconscious mind and credits him with popularizing the work of Janet. However, it is important to remember that there are indications that Myers influenced Janet. The latter cited Myers several times in his classic 1889 work, *L’automatisme psychologique*, and stated that Myers had done more than other authors for the scientific study of the phenomena of spiritism.

Unfortunately, I do not think Carbonel makes a good case for the existence of a Richet school. The discussion of each of the figures is not specific enough to support commonalities or generalizations. In fact, it can be argued that their ideas about the subconscious mind were dissimilar. The author is clear in stating that part of this group’s interest was to “reintegrate to ‘general psychology’ the scientific study of paranormal phenomena otherwise called ‘psychic researches’ or *Psychical Research*.” While this statement may apply to Richet, Gurney, and Myers, and perhaps to Ochorowicz, it is doubtful that it applied to Binet and Hall, who had expressed skepticism about such phenomena.

On the positive side, the author’s recognition that psychical research interacted in significant ways with psychology during the 19th-century is not only part of recent developments in the historiography of psychology, but also a contribution that informs psychologists, psychiatrists, and other current professionals of the complex nature of the past of these disciplines.

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