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Journal of Scientific Exploration, Vol. 25, No. 4, pp. 635–637, 2011 0892-3310/11

EDITORIAL

As subscribers to the hard copy version of the *JSE* have already noticed, this is a particularly hefty issue. I'm pleased that we've been able to wrap up 2011 with a gratifyingly substantive and larger than usual array of papers on a variety of interesting and important topics. This issue contains too many papers for me to comment on them individually. But I do want to direct your attention to the detailed exchange over the data from the Global Consciousness Project (GCP). This series of papers addresses not only the specific questions of how to interpret the GCP data and what it is that the GCP is actually tracking, but also the long-standing and more general debate among parapsychologists over the merits of Decision Augmentation Theory (DAT), hailed by some as a more viable ESP (or cognition)-based alternative to physicalistic explanations of much of the apparent evidence for psychokinesis. Ed May and James Spottiswoode argue first for the DAT point of view. They contend that the statistical deviations reported in the GCP reflect a cognitive form of experimenter psi rather than a force-like physical effect. Then Roger Nelson and Peter Bancel reply separately, and from quite different perspectives. May and Spottiswoode get the last word in this exchange. I'm personally pleased to see the details of the debate presented so thoroughly, and I hope readers will agree that the exchange significantly advances our understanding of the issues.

I also hope to feature additional dialogues on topics of interest to SSE members in future issues, and I encourage readers to let me know what key topics they would like to see debated. Of course, I can't promise to satisfy all (or any) suggestions. Already in my brief tenure as *JSE* Editor-in-Chief, I've learned that I can't always extract submissions of target articles (or replies) from relevant researchers, no matter how pathetically or aggressively I frame my requests. But I'll do what I can, and I'm genuinely interested in knowing which topics are of particular interest to our subscribers.

Since this is the holiday season and an appropriate time for reflecting on the year that's coming to a close, I'd like once again to acknowledge and thank my dedicated and hardworking—in fact, overworked—team of Associate Editors and the many reviewers on whom we all rely in vetting papers for inclusion in the *JSE*. As I've noted before, producing this *Journal* poses a distinctive challenge. Because the *JSE* deals with topics either shunned altogether or dealt with shabbily by more mainstream publications, the community of qualified readers for high-level peer review is quite small. Ideally, I'd prefer to have a larger team of Associate Editors, in order to lighten the editorial load for those who—perhaps inscrutably—continue to volunteer large chunks of time to

shepherding submissions through our system. However, adding members to that team inevitably subtracts members from the small pool of qualified referees. So I'm deeply grateful to my Associate Editors, who realize the need to maintain the high standard of scientific and scholarly excellence that's characterized the *JSE* since its inception, who recognize that there are only so many people on whom the *JSE* can rely, and who accordingly and generously donate their valuable time. I'm equally grateful to our many referees, many of whom we call upon over and over, simply because they have expertise in the relevant areas of research, and because the number of people who have both that expertise and the relevant degree of open-mindedness about new ideas remains too small for us to look elsewhere.

I must also express my deep appreciation for the breathtaking efficiency, technical panache, and thorough understanding of the publishing business of our Managing Editor, Kathleen Erickson. Kathleen does it all, and she does it brilliantly. I'm sure *JSE*'s Associate Editors and readers agree with me on this. We benefit, time and again, from Kathleen's assistance, patience, and good nature. In fact, I've never met anyone who can issue a reminder with such a winning combination of grace and coercion.

On a quite different matter, I've learned recently that some found my previous Editorial disturbingly pessimistic. That Editorial dealt with the problem of finding reliable and stable repositories for the book and journal collections, and other scholarly research materials, of those working on the frontiers of science. I commented on the financial struggles of some parapsychological institutions, and I ended my Editorial with what I thought were some appropriate and justifiable concerns about the long-term reliability of current benefactors. But I don't believe there was any more doom and gloom behind those remarks than I'd attribute to the average purchaser of an insurance policy, who doesn't expect the worst but who certainly wants to be protected in case it occurs.

Let's face it, shit happens, and unfortunately the world suffers its share of rogues and scoundrels. The story I told in my Editorial about finding a good home for the Eisenbud/Serios collection was, I thought, a cautionary tale about protecting what members of this diverse community have worked so hard and lovingly to build. But by no means do I feel that the problems are insurmountable. It's just that we need to be smart and careful about protecting our scholarly and research legacies, like any investment.

One reason the SSE is such a valuable community is that its members are united, not so much by shared specific research interests, but rather by a resistance to scientific complacency and a readiness to challenge various forms of received wisdom. It's a sensible empirical stance buttressed by a clear and extensive historical record of scientific development. I'd argue that my cautionary comments are likewise supported by a wealth of historical

examples, which can guide us as we look for ways to preserve our scholarly and research legacies. Fortunately, the SSE abounds in smart and resourceful members who, either individually or collectively, should be able to address this recurring problem. I'd hoped that my previous Editorial would have provoked some discussion and scheming on the matter, and I'm sorry if—for at least some—it elicited more discouragement than enthusiasm for the challenge. And I'm sorry too if I managed to obscure the positive message that, for the Eisenbud/Serios material at least, we were able to find a respectable, respectful, and indeed grateful repository, whose director very actively and energetically promotes the collection. While it's true (as I noted) that we must remain wary of possible changes in attitude along with changes in personnel, I hope that this story encourages *JSE* readers to explore their own mainstream connections for protecting other research archives.

Stephen E. Braude

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RESEARCH ARTICLE

Revisiting the Ganzfeld ESP Debate: A Basic Review and Assessment

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Abstract—This paper presents a brief review of the debate between parapsychologists and skeptics regarding the issue of replication in experimental tests of extrasensory perception (ESP) using a sensory reduction technique known as ganzfeld. The review is followed by a basic assessment of 59 ganzfeld ESP studies reported in the period following the publication of a stringent set of methodological guidelines and recommendations by R. Hyman and C. Honorton in 1986. The assessment indicates that these 59 studies have a combined hit rate of approximately 30%, which is significantly above the chance expected hit rate of 25%. A comparison of the hit rates across four ganzfeld meta-analyses, as well as across fifteen laboratories, seems to further indicate replication of the ganzfeld ESP effect by a broad group of independent researchers.

Keywords: extrasensory perception (ESP)—ganzfeld—meta-analysis—psi—parapsychology

Introduction

In attempting to make a case for the existence of ostensible psychic (psi) phenomena such as extrasensory perception (ESP), parapsychologists have been regularly faced with the challenge from skeptics of providing a body of notable evidence that can be reproduced under laboratory conditions. In rising to this challenge, many parapsychologists have focused in recent years on the data from a particular type of experiment often used to test for telepathy, and which makes use of a sensory reduction technique known as ganzfeld.

Within the context of parapsychology, the ganzfeld (German for "total field") is a technique intended to help improve the reception of ESP by briefly exposing a person to a static and uniform sensory field.[1] This is done by covering the person's eyes with translucent eye shields (usually halved ping-pong balls externally illuminated by a red light) and filling the person's ears with soft static noise played through headphones. While in this homogeneous ganzfeld "state,"

the person may report a dimming of the visual field and experience a diffuse background that has been described as a "cloudy fog" (Wackermann, Pütz, & Allefeld, 2008:1366). After several minutes, the person may begin to experience hallucinatory-like images and/or sounds, similar to those experienced during the hypnagogic state between wakefulness and sleep.[2] Presumably, if the ESP assumption is valid, some of the images and sounds may correspond to the ESP target.

A typical experimental test session for telepathy using the ganzfeld proceeds in the following manner: Two participants, one acting as the "sender" and the other as the "receiver,"[3] are isolated in separate, soundproofed rooms. In one room, the receiver is placed in the ganzfeld state and asked to describe any images, sounds, or impressions that come to mind while in that state. In the other room, the sender is shown a randomly selected visual target, such as a photograph or a video clip, and asked to concentrate on its details. After about thirty minutes, the receiver is taken out of the ganzfeld and shown a collection of four photos or video clips, one of which was the target that the sender was concentrating on (the other three are decoys). The receiver is then asked to rank the four photos/videos according to their degree of correspondence with the images, sounds, and impressions received while in the ganzfeld. If the photo/video that the sender was viewing is ranked as having the highest degree of correspondence, the test session is considered a success, or a "hit." With the probability of a hit being 1 in 4, the hit rate expected by chance is 25% (for further discussion of the ganzfeld and its use in ESP experiments, see, for example, Bem & Honorton, 1994, Honorton, Berger,Varvoglis, Quant, Derr, et al., 1990, Wackermann, Pütz, & Allefeld, 2008).[4]

For a period of approximately 28 years, there has been an ongoing debate between parapsychologists and skeptics over the issue of whether or not the ganzfeld experiment can provide the independently replicable evidence necessary to support the empirical case for psi. This paper seeks to address the issue in two ways. First, it provides a brief review of the substance of the debate as it has persisted from 1982 to the present. Second, it presents a basic assessment of a collection of 59 ganzfeld ESP studies reported in the years following the publication of a stringent set of methodological guidelines and recommendations for ganzfeld research developed by Ray Hyman, a cognitive psychologist and long-time critic of parapsychology, and the late Charles Honorton, a parapsychologist and contributor to the ganzfeld database (Hyman & Honorton, 1986).

The Ganzfeld Debate

ESP research using the ganzfeld was initiated in the early 1970s largely through the efforts of three independent researchers: Charles Honorton, William Braud,

and Adrian Parker (Braud, Wood, & Braud, 1975, Honorton & Harper, 1974, Parker, 1975). Between 1974 and 1981, a total of 42 ganzfeld studies had been reported by ten different laboratories. Of these early studies, 23 (55%) produced results that were statistically significant at the .05 level.

The debate commenced at the 25th Annual Convention of the Parapsychological Association (PA) in August of 1982, where two preliminary meta-analyses of the early ganzfeld database were presented.[5] On the basis of his analysis, Ray Hyman argued that the initial rate of successful replication may have been overestimated. Noting that some of the experiments contained slight variations on the standard ganzfeld procedure, Hyman suggested that each variation should be counted as a separate study. By his count, there were 80 ganzfeld studies in all, 25 of which (31%) were successful. Although this was still considered notable, Hyman further argued that the significance of the database could be further discounted through the effects of selective reporting.

In his review of the database, Hyman found a significant tendency for studies with a small number of test sessions to have a higher proportion of significant positive results, suggesting to him the possibility that some studies may have been stopped and reported early on because of their promising results (i.e. optional stopping on a hit). In addition, Hyman claimed that there was circumstantial evidence suggesting that some pilot or exploratory studies in the database were being retrospectively counted as formal ones solely on the basis of their significant outcomes. He further suggested that, in contrast to the significant ones, studies with nonsignificant results might not have been reported, contributing to a possible "file-drawer" effect. According to Hyman's argument, when the effects of selective reporting are taken into account, the success rate of the database comes much closer to chance. Lastly, Hyman cited a number of potential study flaws in the database relating to target randomization, adequate security, sensory cuing, statistical errors, and the use of multiple analyses.

Honorton responded to Hyman's critique with his own meta-analysis of the early ganzfeld database. To address the issue of varying conditions, Honorton proposed that researchers should examine each study and decide for themselves whether or not each varying ganzfeld condition should be classified as a separate study. To adjust the results for the effects of multiple analyses, Honorton applied a Bonferroni correction and showed that the initial success rate would only be reduced to 45%. To further counter the multiple-analysis argument, Honorton focused on the 28 studies that reported a hit rate, the most common analysis measure used in the database. Of these, 12 studies (43%) were significant at the .05 level. When combined, these 28 studies were shown to have a Stouffer's Z of 6.60 ($p = 2.1 \times 10^{-9}$). In addition to citing the PA's policy against selective reporting, Honorton used Rosenthal's (1979) "file

drawer" estimation statistic to show that approximately 423 studies would be needed to nullify the significance of the 28 studies, amounting to 15 unreported studies for every one that was reported. Lastly, to address the issue of flaws, Honorton's analysis showed that there was no significant correlation between rated study quality and experimental outcomes.

The two opposing meta-analyses were refined and published together in the *Journal of Parapsychology* three years later (Honorton, 1985, Hyman, 1985). Instead of remaining in opposing camps, Hyman and Honorton (1986) came together soon afterward to develop a "joint communiqué" that highlighted the issues on which they agreed. In summarizing their agreements, they wrote:

> We agree that there is an overall significant effect in this data base that cannot be reasonably explained by selective reporting or multiple analysis. We continue to differ over the degree to which the effect constitutes evidence for psi, but we agree that the final verdict awaits the outcome of future experiments conducted by a broader range of investigators and according to more stringent standards. (p. 351)

To supplement their agreement regarding future experimentation, Hyman and Honorton (1986) also provided in their communiqué the jointly developed set of methodological guidelines and recommendations.

At the same time, Honorton and his colleagues at Psychophysical Research Laboratories (PRL) in New Jersey had designed a series of automated ganzfeld studies in which target selection, presentation, and data recording were handled by computer (Honorton et al., 1990). These eleven "autoganzfeld" studies were conducted from 1983 to 1989, and were made to be compliant with the guidelines and recommendations of the joint communiqué. In a meta-analysis published in the prominent mainstream journal *Psychological Bulletin*, Daryl Bem and Charles Honorton (1994) evaluated ten of the PRL autoganzfeld studies and found that they had collectively produced 106 hits in 329 test sessions for a significant hit rate of 32.2% ($z = 2.89$, $p = .002$). A graphical summary of Bem and Honorton's results is shown in Figure 1.

In addition to PRL, seven other laboratories had made efforts to design and conduct ganzfeld studies that complied with the joint communiqué guidelines and recommendations. Five years after the analysis by Bem and Honorton, Julie Milton and Richard Wiseman (1999) published a meta-analysis in *Psychological Bulletin* of the 30 ganzfeld studies conducted by these other laboratories between 1987 and 1997. Their analysis seemed to indicate that this independent database had produced an overall result consistent with chance (Stouffer's $Z = 0.70$, $p = .242$). A graphical summary of Milton and Wiseman's results is shown in Figure 2.

Two years later, Lance Storm and Suitbert Ertel (2001) published a

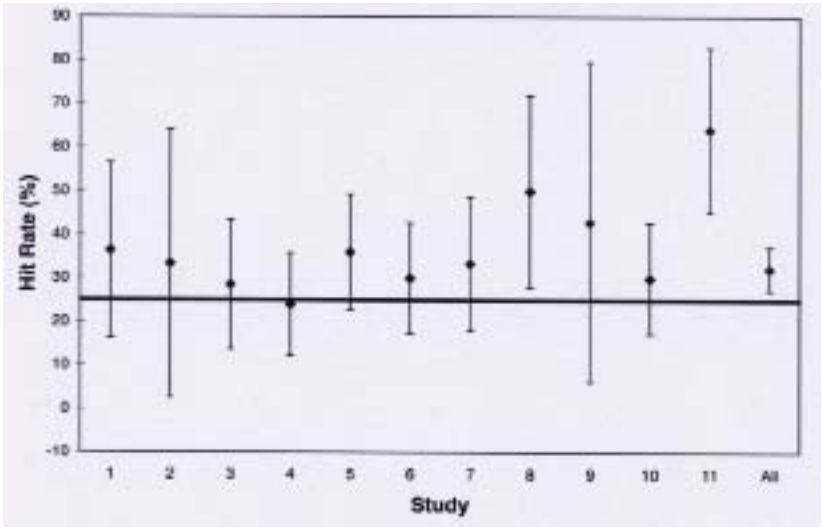

Figure 1. Results summary of the PRL autoganzfeld meta-analysis reported by Bem and Honorton (1994), in terms of hit rate and 95% confidence intervals.
The horizontal line at 25% indicates the hit rate expected by chance. The far right hit rate marked "All" represents the combined data of Studies 1–10.

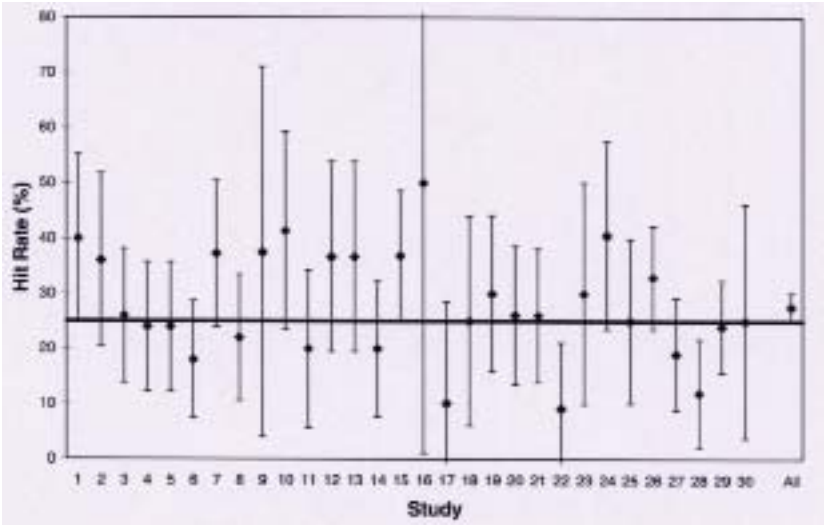

Figure 2. Results summary for the 30 ganzfeld ESP studies analyzed by Milton and Wiseman (1999), in terms of hit rate and 95% confidence intervals.
The horizontal line at 25% indicates the hit rate expected by chance.

commentary in *Psychological Bulletin* that raised several methodological issues with Milton and Wiseman's analysis, and that presented a meta-analysis of a larger database. They reasoned that, in order to reach a general conclusion on ganzfeld research, it was necessary to consider the results of all studies reported from 1974 to 1997. On this basis, they compiled a unified database of 79 studies that included the early ganzfeld and PRL studies, along with those contained in the Milton–Wiseman database. Their subsequent analysis found a significant overall effect (Stouffer's $Z = 5.66$, $p = 7.78 \times 10^{-9}$). In their reply, Milton and Wiseman (2001) claimed that Storm and Ertel's result was ambiguous because they had included the early ganzfeld database, which contained numerous flaws according to Hyman's (1985) analysis. Milton and Wiseman argued that this would make it impossible to determine what proportion of the significant effect was due to flaws.

Shortly after this exchange, Daryl Bem, John Palmer, and Richard Broughton (2001a) published a meta-analysis that seemed to shed light on a possible reason why Milton and Wiseman's results were null. They noticed that the database used by Milton and Wiseman comprised two types of study, labeled "standard" and "non-standard."

Standard studies were intended to be direct replications of the PRL autoganzfeld, and had therefore used methods and procedures very similar to (if not the same as) those used by PRL. In contrast, non-standard studies used methods and procedures that had been purposely modified from those commonly used in previous ganzfeld experiments in order to search for other psi-conducive conditions and begin exploring the processes involved in ESP. Some of the modifications made in non-standard studies include using auditory targets instead of visual ones (Willin, 1996a, 1996b), using more than one target during a session (Serial Ganzfeld section in Parker & Westerlund, 1998), exploring the effects of psychedelic drugs on receiver impressions (Series V & VI in Wezelman & Bierman, 1997), and exclusively using a clairvoyance design throughout the course of the study (Kanthamani & Broughton, 1996, Kanthamani & Khilji, 1990, Kanthamani, Khilji, & Rustomji-Kerns, 1989).

Several of the non-standard studies were noted by Bem et al. (2001a) to have shown negative or null results, consistent with their cautionary statement that ". . . such deviations from exact replication are at increased risk for failure" (p. 208). They hypothesized that, when combined with the standard studies, the results of the non-standard studies could have the effect of reducing the overall hit rate. To test this, Bem et al. separately grouped the two types of study based on ratings of how closely they adhered to the PRL autoganzfeld methods and procedures. The 29 standard studies were found to have a significant above-chance hit rate of 31.2% (Stouffer's $Z = 3.49$, $p = .0002$), whereas the nine non-standard studies had a nonsignificant below-chance hit rate of 24%

(Stouffer's $Z = -1.30$, $p = .903$). The difference between the two study types was significant ($U = 190.5$, $p = .020$). In addition, Bem, Palmer, & Broughton (2001a) found ten other ganzfeld studies that had been reported after Milton and Wiseman's analysis. When these studies were combined with the 30 studies in the Milton–Wiseman database, a significant hit rate of 30.1% was obtained (Stouffer's $Z = 2.59$, $p = .0048$). Graphical summaries of Bem et al.'s results are presented in Figure 3, Figure 4, and Figure 5.

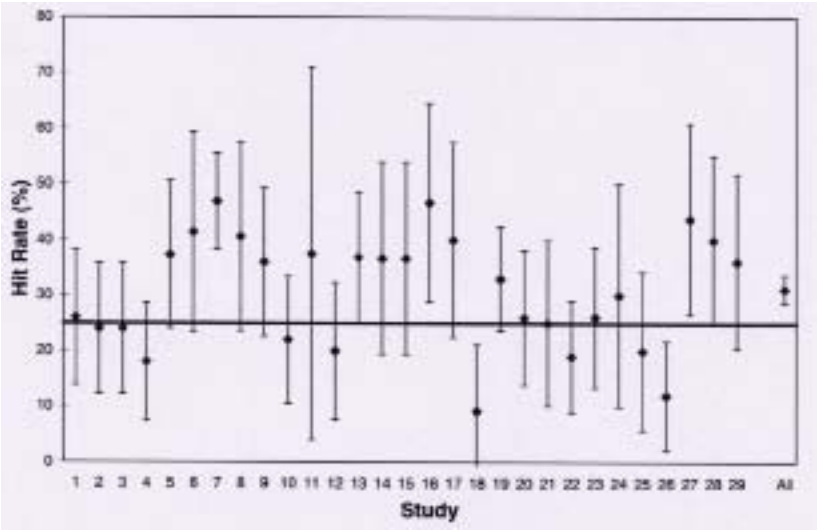

Figure 3. Results summary for the set of 29 "standard" ganzfeld studies contained in the database analyzed by Bem et al. (2001b), in terms of hit rate and 95% confidence intervals.
The horizontal line at 25% indicates the hit rate expected by chance.

Two other recent meta-analyses have reported overall hit rates that are significantly above the 25% expected by chance. In the first, Marilyn Schlitz and Dean Radin (2003) found that a unified database of all ganzfeld studies published between 1974 and 2001 had a hit rate of 32% (929 hits in 2,878 sessions, $z = 8.75$, $p << 10^{-15}$).[6]

Another unified database comprising 16 early ganzfeld studies, the eleven PRL autoganzfeld studies, and Bem et al.'s 29 "standard" ganzfeld studies was analyzed by Jessica Utts, Michelle Norris, Eric Suess, and Wesley Johnson (2010) in the second study. Their results indicated 709 hits in 2,124 sessions for a hit rate of 33.4% ($z = 8.92$, $p = 2.26 \times 10^{-18}$). In addition to this frequentist

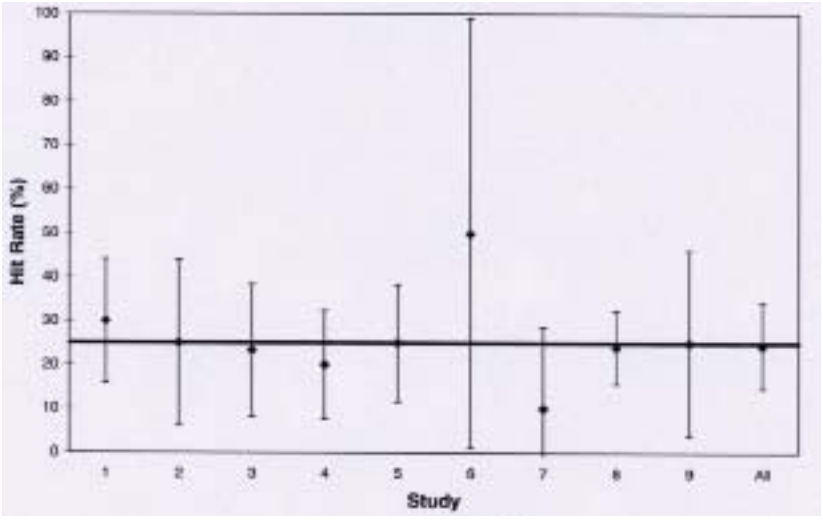

Figure 4. Results summary for the set of nine "non-standard" ganzfeld studies contained in the database analyzed by Bem, Palmer, & Broughton (2001b), in terms of hit rate and 95% confidence intervals.
The horizontal line at 25% indicates the hit rate expected by chance.

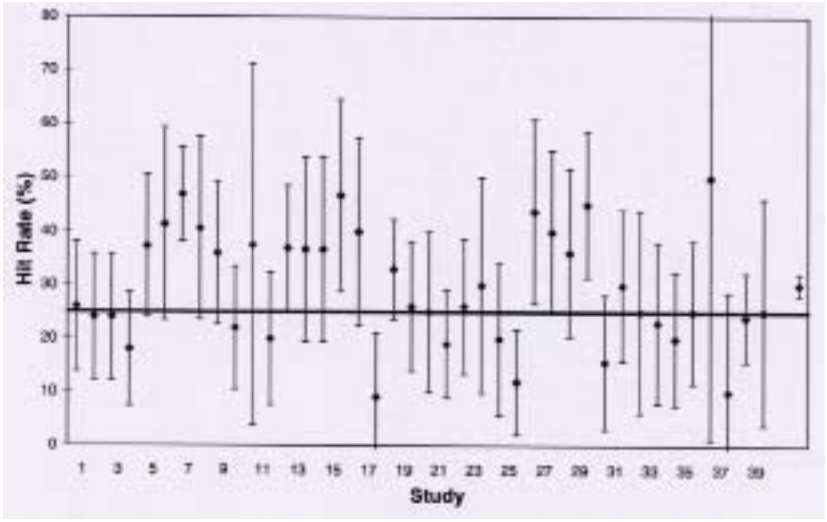

Figure 5. Results summary for the entire set of 40 "standard" & "non-standard" ganzfeld studies contained in the database analyzed by Bem, Palmer, & Broughton (2001b), in terms of hit rate and 95% confidence intervals.
The far right point interval represents the overall hit rate for all 40 studies combined. The horizontal line at 25% indicates the hit rate expected by chance.

analysis, Utts, Norris, Suess, & Johnson (2010) performed a Bayesian analysis on this database to illustrate how the differing levels of a priori belief regarding the probability for ganzfeld success (modeled in terms of a beta distribution) for three separate personal points of view (believer, skeptic, and open-minded) were each influenced by the experimental results of the database. While the probability distribution for the open-minded view was shifted more toward higher probabilities of success, the distribution for the skeptic view was not shifted much from probabilities close to chance level. The distribution for the believer view remained within the range of higher probabilities, but was found to have less variability.

Most recently, an analysis by Lance Storm, Patrizio Tressoldi, and Lorenzo Di Risio (2010a) of 30 ganzfeld studies reported from 1997 to 2008 found a hit rate of 32.2% (483 hits in 1,498 sessions, $z = 6.44$, $p < .001$).[7] A graphical summary of Storm et al.'s (2010a) results is shown in Figure 6.

More in-depth discussion of the ganzfeld debate and additional summaries of the meta-analyses reviewed here can be found in other published reviews by Dalkvist (2001), Palmer (2003), Radin (1997, Ch. 5, 2006, Ch. 6), Storm (2006), and Utts (1991, 1999b).

A Basic Assessment

An issue central to the ganzfeld debate as it appeared in *Psychological Bulletin* was the status of independent replication in the years following the publication of Hyman and Honorton's (1986) joint communiqué. At the end of their article on the PRL autoganzfeld meta-analysis, Bem and Honorton (1994) stated that, although they had produced significant overall results under stringent conditions,

> . . . the autoganzfeld studies by themselves cannot satisfy the requirement [in the joint communiqué] that replications be conducted by a "broader range of investigators." Accordingly, we hope the findings reported here will be sufficiently provocative to prompt others to try replicating the psi ganzfeld effect. (p. 13)

The analysis by Milton and Wiseman (1999) represented an initial attempt to determine whether this requirement had been met in the decade following the joint communiqué, and their null result suggested that the outlook for replication by others was not promising. In contrast, several subsequent analyses using a unified database of all ganzfeld studies seemed to suggest that a more positive outlook was warranted (Radin, 2006:120–121, Schlitz & Radin, 2003, Storm & Ertel, 2001, Storm et al., 2010a:477, Utts, Norris, Suess, & Johnson, 2010). Despite this, it might be argued that these analyses may be somewhat limited in

their ability to address the issue because of their inclusion of the early ganzfeld database. Assuming for the moment that the early database does indeed contain serious flaws, as argued by Hyman (1985), the argument can be made that inclusion of this database could potentially inflate or otherwise confound the overall results.[8] Meta-analyses that used a non-unified database also offer a positive outlook on the issue (e.g., Bem, Palmer, & Broughton, 2001a, Storm et al., 2010a:475), although assessing the broader, long-term trend in replication may be partially limited in these analyses by their confined periods of coverage. For these reasons, an attempt was made to basically assess the post-communiqué replication status of the current ganzfeld database, as well as to update and confirm some of the results of previous meta-analyses. However, it should be made clear that the assessment presented here was not meant to represent any kind of formal meta-analysis, and thus that may perhaps limit interpretation of its findings (addressed in the Discussion).

Method

To examine the current status of replication, a collection of ganzfeld studies was compiled from the databases listed in three previously published meta-analyses that addressed post-communiqué research (Bem, Palmer, & Broughton, 2001a, Milton & Wiseman, 1999, Storm et al., 2010a). This resulted in 59 studies reported in the period between 1987 and 2008 (see Appendix 1).

Formal meta-analyses of ganzfeld research have tended to use effect size as their primary measure of effect magnitude. However, to make the assessment results more accessible to a general interdisciplinary audience, the decision was made here to focus on hit rate, as this is a concept that is intuitively easier to grasp. Following the approach taken by Radin (1997), the hit rate was obtained by determining the proportion of hits achieved over the total number of test sessions in each study, and an associated 95% confidence interval was calculated based on the proportion of hits and its associated standard deviation (*SD*), derived from the equation (from Utts, 1999a:341):

$$SD = \sqrt{(h)(1 - h)/N}$$

where h is the proportion of hits and N is the number of sessions. To cover 95% of the values that fall within approximately two standard deviations of h, *SD* is multiplied by 1.96. Thus, the confidence interval is obtained using the equation $CI = h \pm 1.96(SD)$ (Howell, 1995:95–96, Utts, 1999a:341). To confirm and compare previously obtained results, the published data from four meta-analyses (Bem & Honorton, 1994, Bem et al., 2001a, Milton & Wiseman, 1999, Storm et al., 2010a) were reanalyzed in the same manner.

To test the statistical significance of the collection and the four databases,

the method of hypothesis testing described by Utts (1999a, Ch. 21) was used. In this basic four-step method, a test statistic is used to decide between two competing hypotheses: a null hypothesis and an alternative hypothesis. Under the null hypothesis of no ESP, the mean hit rate in ganzfeld experiments is expected to be around the chance rate of 25%. Under the alternative hypothesis of ostensible ESP, the mean ganzfeld hit rate will be significantly different from the expected chance rate of 25%. Here, the test statistic was a *z*-score of the form $z = (x - \mu)/SD$, where x is the proportion of hits observed in a given database of ganzfeld experiments, μ is the mean proportion of hits expected under the null hypothesis (.25), and *SD* is the expected standard deviation (determined by the *SD* equation above, using the chance-expected proportion of hits for *h*). Based on the resulting *z*-score, an associated probability value was obtained to determine the degree of significance. Because the prediction in many ESP tests (including the ganzfeld) is for an above-chance hit rate, the same prediction was maintained here and thus all reported probability values are one-tailed.

Results

Figure 1 shows a results summary of the PRL autoganzfeld meta-analysis by Bem and Honorton (1994), expressed in terms of hit rate and 95% confidence intervals. As in the Bem and Honorton analysis, Study 11 (No. 302–Experienced) was excluded from the overall hit rate because of its possible response bias. As noted in the previous section, Bem and Honorton found a total of 106 hits in 329 sessions, for a significant overall hit rate of 32.2%. Based on Utts' method of hypothesis testing, this results in a *z*-score of 3.02 ($p = .001$), which is slightly higher than, but still consistent with, the original reported finding.[9]

Figure 2 shows the results summary for the 30 post-communiqué ganzfeld studies analyzed by Milton and Wiseman (1999), expressed in terms of hit rate and 95% confidence intervals. It was noted in the previous section that Milton and Wiseman's analysis, as originally published, had produced a result consistent with chance. However, a few researchers (Radin, 2006:118, Schlitz & Radin, 2003:79, Utts cited in Storm et al., 2010a, Footnote 1) have pointed out that if their 30-study database is analyzed in terms of hit rate, a significant finding is obtained. An attempt was made to verify this by calculating the proportion of hits for each study in the database and then examining the combined hit rate.[10] This resulted in 331 hits in 1,198 sessions for a hit rate of 27.6% ($z = 2.08$, $p = .019$), consistent with the estimates made by these other researchers. It can be seen at the far right of Figure 2 that the lower bound of the confidence interval for the combined hit rate seems to include chance (by calculation, the lower bound is 25.07%, just marginally above chance expectation). This suggests that, even though the result is postive, caution is warranted in interpreting the combined result.

Results summaries for the meta-analysis by Bem, Palmer, & Broughton (2001a) are shown in Figure 3, Figure 4, and Figure 5. These summaries are based on data contained in a corrected table that was later published by the authors (Bem, Palmer, & Broughton, 2001b). Figure 3 shows the results for the set of 29 "standard" ganzfeld studies contained within their 40-study database. There were 402 hits out of 1,278 sessions in this set of standard studies, for an overall hit rate of 31.5%. By the Utts method, this is associated with a z-score of 5.37 ($p = 3.95 \times 10^{-8}$).

The results for the set of nine "non-standard" ganzfeld studies in the Bem et al. database is shown in Figure 4. As in the Bem et al. (2001a) analysis, the two studies in the database that fell on the boundary between standard and non-standard were excluded. Consistent with the point made by Bem, Palmer, & Broughton (2001a) that studies that deviate from standard ganzfeld methods and procedures are at greater risk for failure, the confidence intervals for all nine studies shown in Figure 4 include chance expectation, even when combined. This is further indicated by a nonsignificant hit rate of 24.3% for this set (73 hits in 300 sessions,[11] $z = -0.28$, $p = .610$).

When combined, the 40 standard and non-standard studies contained in the Bem, Palmer, & Broughton (2001b) database have an above-chance hit rate of 30.1% (503 hits in 1,661 sessions, $z = 4.80$, $p = 7.94 \times 10^{-7}$). However, there has been some debate about the effect of including a large and highly significant study by Kathy Dalton (1997), which apparently began over the considerable influence it had in affecting the overall significance of the ganzfeld database as it stood in March of 1999 (see Milton, 1999, Schmeidler & Edge, 1999, Storm, 2000). When this Dalton study is excluded from the Bem, Palmer, & Broughton (2001a) database, the overall hit rate decreases to 28.9%, which remains significant (443 hits in 1,533 sessions, $z = 3.53$, $p = .0002$). The results for all 40 studies are shown in Figure 5.

Figure 6 shows the results summary for 29 of the 30 ganzfeld studies analyzed by Storm et al. (2010a).[12] Ten studies contained within their database were also included in the database of Bem et al. (2001b). Here a small update is provided to the Storm et al. database by replacing the preliminary data from one conference-presented study (Ganzfeld Study 7 in Storm et al.'s Appendix A) with its more complete published data (Parker, 2010), and adjusting one study (Ganzfeld Study 11 in their Appendix A) for an extra hit that was later found and reported elsewhere (Parker, 2000). As noted in the previous section, Storm et al. reported a significant overall hit rate of 32.2% for their database, a finding that does not include the Dalton (1997) study. Recalculating based on the updated database gives 486 hits in 1,506 sessions for an overall hit rate of 32.3% ($z = 6.54$, $p = 3.09 \times 10^{-11}$), consistent with their finding.

A summary of the combined results for the collection of 59 post-

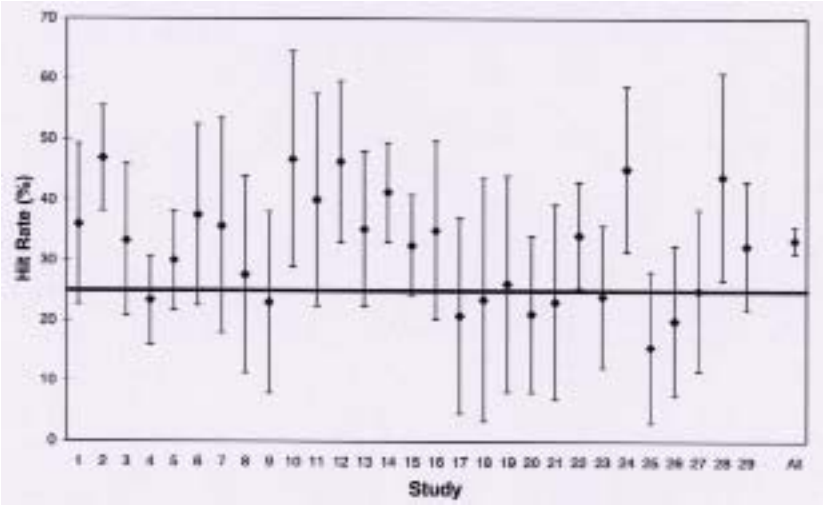

Figure 6. Results summary for 29 of the 30 ganzfeld studies included in the meta-analysis by Storm et al. (2010a), in terms of hit rate and 95% confidence intervals.
The horizontal line at 25% indicates the hit rate expected by chance.

communiqué studies compiled from the databases of Bem et al. (2001b), Milton and Wiseman (1999), and Storm et al. (2010a) is shown in Figure 7 in terms of a cumulative hit rate over time and associated 95% confidence intervals (modeled after the approach taken by Radin, 2006:120). It should be made clear that this does not include the PRL autoganzfeld results; it is based only on ganzfeld replication efforts independent of PRL. The graph indicates that the hit rate begins to average out over time at about 30%, significantly above the 25% expected by chance. Overall, there are 878 hits in 2,832 sessions for a hit rate of 31%, which has $z = 7.37$, $p = 8.59 \times 10^{-14}$ by the Utts method. If the Dalton (1997) study is excluded, there are 818 hits in 2,704 sessions for a hit rate of 30.3% ($z = 6.36$, $p = 1.01 \times 10^{-10}$). This suggests that, even if the early ganzfeld and PRL autoganzfeld databases are not considered, attempts to replicate the ganzfeld ESP effect by independent researchers are still collectively above what would be expected by chance, with the correct target being identified about one-third of the time on average.

The Issue of Replication: A Comparative Approach

Statistician Jessica Utts (1999b) suggests that, rather than being defined in terms of statistical significance, "[a] more appropriate definition of repeatability of an effect is that the estimated magnitude of the effect (odds ratio, hit rate, and so on)

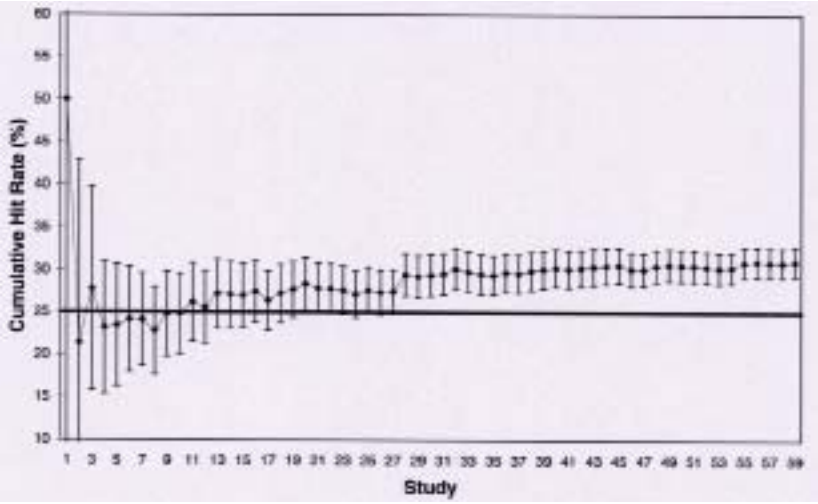

Figure 7. Results summary for the collection of 59 post-communiqué ganzfeld ESP studies reported from 1987 to 2008, in terms of cumulative hit rate over time and 95% confidence intervals.
The horizontal line at 25% indicates the hit rate expected by chance.

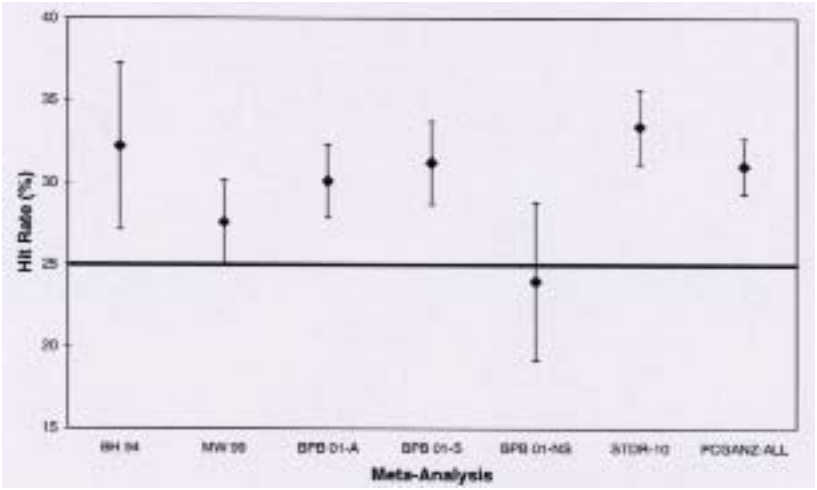

Figure 8. Comparison of the overall hit rates and 95% confidence intervals from four ganzfeld ESP meta-analyses.
The horizontal line at 25% indicates the hit rate expected by chance.
BH 94: Bem & Honorton, 1994; MW 99: Milton & Wiseman, 1999; BPB 01-A: combined result from all studies in the Bem et al. (2001b) database; BPB 01-S & BPB 01-NS: results from the "standard" and "non-standard" studies in Bem et al., 2001b, respectively; STDR-10: Storm et al., 2010a; PCGANZ-ALL: combination of MW 99, BPB 01-A, and STDR-10, representing all post-communiqué ganzfeld studies, excluding PRL.

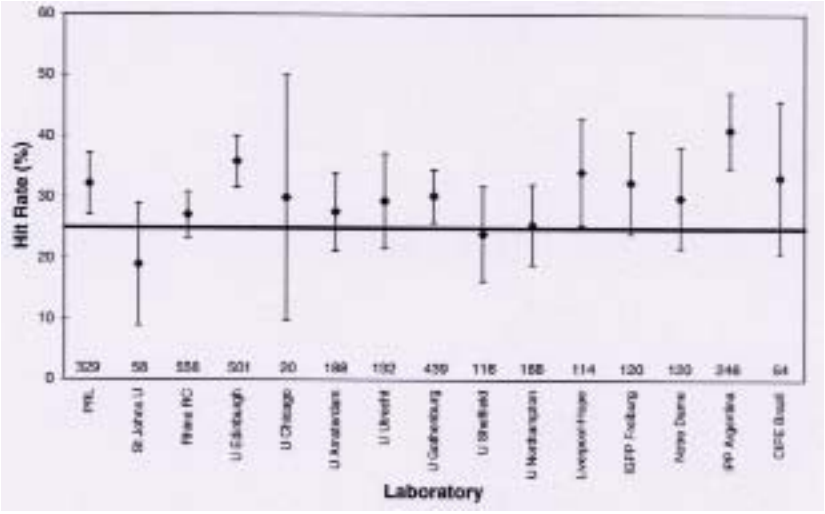

Figure 9. Comparison of the hit rates and 95% confidence intervals for 15 laboratories that have contributed to the ganzfeld ESP database. The horizontal line at 25% indicates the hit rate expected by chance. Above each lab is the total number of sessions contributed by that lab to the database.

falls within the same range from one repetition of an experiment to the next" (p. 631). This is something that can be assessed by looking at the confidence intervals. Figure 8 shows a comparison of the hit rate confidence interval from the Bem and Honorton (1994) PRL autoganzfeld database with the confidence intervals of the three meta-analyses used to compile the 59-study collection (Bem et al., 2001b, Milton & Wiseman, 1999, Storm et al., 2010a).

It can be seen that, when compared to the PRL autoganzfeld (BH 94 in Figure 8), the three other meta-analyses each have a mean hit rate that lies within the PRL 95% confidence interval. The same finding is evident when the results of the three analyses are combined. When split into standard and non-standard ganzfeld, the Bem et al. (2001b) database indicates that, as one might predict, the mean hit rate for standard studies replicates the PRL autoganzfeld finding, while the non-standard hit rate mean does not (although the confidence intervals do overlap).

Looking at replication from another perspective, Figure 9 compares the mean hit rates and confidence intervals across each of the fifteen laboratories that have contributed to the ganzfeld database.[13] While the intervals for most of the laboratories include chance, they also have mean hit rates above what is

expected by chance. Most importantly, ten of the fifteen laboratories (66.7%) produced mean hit rates that fall at or within the bounds of the 95% confidence interval for PRL, indicating a fair degree of replication of the obtained PRL hit rate.

Discussion

The results of the basic assessment presented here are consistent with those reported in previous meta-analyses of the ganzfeld ESP database, and seem to indicate three main things: First, there remains a significant overall hit rate in the series of ganzfeld studies conducted after Hyman and Honorton's (1986) joint communiqué.

Second, this significant finding remains apart from the results of the early ganzfeld and PRL databases. As noted, this goes toward addressing a criticism that could be leveled against meta-analyses that include the latter two databases as part of a larger unified database. In addition to his finding of flaws in the early ganzfeld database, Hyman (1994) claimed to have found subtle hints of artifacts present in the PRL autoganzfeld database that he argues may constrain interpretation of its results. Assuming his claims have merit, it can be argued that inclusion of the two databases in a unified database could potentially confound interpretation of any subsequent meta-analysis. However, in not being reliant on either of the databases, the collection of post-communiqué studies used in the present assessment circumvents this confound.

Third, this series of post-communiqué studies was contributed by fifteen different laboratories, more than half of which produced a hit rate statistically within the range of the hit rate obtained in the PRL autoganzfeld. Similarly, the combined series shows a comparable hit rate to PRL. If the replication issue is addressed in these terms, it would seem that, in answer to Hyman and Honorton's (1986:351) communiqué statement, the psi ganzfeld effect has indeed been replicated by "a broader range of investigators" under stringent standards.

Some consideration should be made of the potential limitations of the present assessment. Because it was not defined in advance to be a formal meta-analysis, the assessment had no well-defined criteria for studies to be included in the collection used here, and no formal check of the heterogeneity of the dataset was performed. However, it should be recognized that the studies included in the collection came from meta-analytic databases that did have defined inclusion criteria, and that the issue of how to properly handle a heterogeneity problem within the ganzfeld database is still under debate (e.g., see Schmeidler & Edge, 1999:340–349, Storm, 2000). Even so, at least one formal meta-analysis that included a test for heterogeneity has found a significant result with a trimmed, homogeneous database (Storm et al., 2010a:475).

As mentioned previously, the ganzfeld debate has persisted for nearly 30 years without adequate resolution. If the results of the present assessment and those of previous meta-analyses are carefully considered, they seem to stress the issue that there is a statistical anomaly within the ganzfeld database that is in need of a more sufficient explanation, if not ESP. Regardless of their interpretation, the results seem to offer reason that serious consideration should perhaps be put toward bringing final closure to the replication issue at the heart of the debate, rather than lingering endlessly on the proof-oriented questions of whether an anomaly exists and whether it is replicable.

Notes

[1] The idea of how the ganzfeld might improve ESP reception is based on the assumption that ESP is regularly overwhelmed or "drowned out" by incoming signals from the prime sensory channels of vision and hearing. However, if these sensory channels are reduced via the ganzfeld, then this might give the subtle ESP information a better chance of seeping into conscious awareness.

[2] The similarity between the ganzfeld state and the hypnagogic state has previously led some researchers to suspect that the two might be related. However, the findings reviewed by Wackermann et al. (2008) suggest that, rather than being a state of reduced awareness like the hypnagogic state, the ganzfeld may actually be a mildly active state, characterized in part by brain waves in the alpha range (8–12 Hz, usually associated with a state of relaxed awareness). Some studies offer evidence to suggest that ESP may be associated with alpha activity (see the reviews in Krippner & Friedman, 2010), perhaps suggesting a possible connection.

[3] Although the concept of telepathy traditionally assumes that the sender is "transferring" information about the ESP target to the receiver, there is currently little (if any) evidence to indicate that that is what is occurring. Thus, these terms are being used here for the convenience of distinguishing between the two participants, and should not be taken to imply that one necessarily transferred or "sent" something to the other.

[4] In addition to telepathy, a ganzfeld experiment may also be used to test for clairvoyance; this can be done by having no sender present to view the ESP target. Although a small number of the individual experiments in the ganzfeld database have tested clairvoyance, the majority of them have used a telepathy design.

[5] For simplicity, meta-analysis can be defined here as the statistical method of combining the data from many separate experiments in order to examine and weigh the evidence for a combined overall effect, rather than looking at the individual results of each experiment alone. This type of analysis is particularly useful when evaluating the experimental evidence for phenomena that are inherently weak or that tend to vary across conditions, such as psi and other forms of human behavior. For useful discussions on meta-analysis and its use in parapsychology, see Radin (1997, Chapter 4), Storm (2006), and Utts (1991, 1999b).

[6] See also two books by Radin (1997:86–89, 2006:120–121) for the results of two other unified ganzfeld meta-analyses.

[7] The ganzfeld was one of the three types of ESP experiment examined in Storm et

al.'s (2010a) meta-analysis. The other two, noise reduction induced through alleged psi-enhancing techniques (dreams, meditation, relaxation, and hypnosis) and standard waking free response, had also shown significant overall results.

[8] The issue of whether or not the early ganzfeld database does indeed contain serious flaws remains to be one of serious controversy; it was a major point of contention in the exchange between Milton and Wiseman (2001, 2002) and Storm and Ertel (2001, 2002) with regard to interpreting the latter's meta-analysis, and Hyman (2010:488) still apparently stands behind his argument of flaws (see Storm et al., 2010b:493, for a brief counterargument and supporting references). Although some analyses have found no significant correlation between rated study quality and effect size, one wonders whether this would satisfy the skeptics, given the persistent controversy. Rather than having to address it, the controversy was circumvented here by considering only the ganzfeld studies conducted after the joint communiqué (since the focus of this paper is on post-communiqué replication).

[9] The discrepancy between the *z*-score obtained by the Utts method and that reported by Bem and Honorton (1994) can be explained by the fact that the latter is associated with the exact binomial probability for the observed number of hits compared to chance expectation (p. 10). The same holds for the results of all the other meta-analyses reanalyzed here.

[10] In calculating the number of hits for two studies contained in the Milton–Wiseman database, it was necessary to make approximations. For Stanford and Frank (1991), Bem et al. (2001b:428) note that the hit rate was not reported and had to be estimated from a *z*-score. The number of hits was approximated based on this hit rate and the total number of sessions. For McDonough, Don, & Warren (1994), the approximated number of hits was based on a composite of the hit rates obtained both by receiver judging and by independent judging. The approximations for the two studies are noted in the table in Appendix 1.

[11] For the serial study by Parker and Westerlund (1998) and four studies summarized by Kanthamani and Broughton (1994), Bem et al. (2001b:428) note that the hit rate was not reported and had to be estimated from a *z*-score. The number of hits for these studies was again approximated based on the estimated hit rate and the total number of sessions. These approximations are noted in Appendix 1.

[12] One of the studies (Roe & Flint, 2007) in the Storm et al. (2010a) database was excluded because it had a hit probability of 12.5% (i.e. 1 in 8) rather than the usual 25% (1 in 4) of most other ganzfeld studies.

[13] To identify these contributing laboratories, the methods sections of the individually published studies cited in Appendix 1 were consulted in order to determine where the ganzfeld test sessions for each study had been conducted. In some cases, this information was also given in the study title (e.g., the Utrecht and Amsterdam series), and/or was available in the extended Parapsychological Association Convention abstracts for certain studies that were originally published in the annual anthology *Research in Parapsychology* and later in the *Journal of Parapsychology*. The numbers of sessions reported in the studies were tabulated and grouped according to the laboratory where each study was conducted. The session numbers for all studies conducted at a given laboratory were then summed to produce the totals for each laboratory listed in Figure 9.

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APPENDIX 1
The 59-Study Post-Communiqué Ganzfeld Collection

Study	Study Description	*N* Sessions	Hits	Hit Rate	SD*
1	Kanthamani et al. (1989)—FRNM Manual Series 5a	4	**2**	0.500	0.250
2	Kanthamani et al. (1989)—FRNM Manual Series 5b	10	**1**	0.100	0.095
3	Kanthamani & Khilji (1990)—FRNM Manual Series 6b	40	**12**	0.300	0.072
4	Stanford & Frank (1991)—Psych Verbal Indicators	58	**11**	0.190	0.052
5	Kanthamani & Broughton (1996)—FRNM Manual Series 6a	20	**5**	0.250	0.097
6	Bierman et al. (1993)—Utrecht Novice Series 1	50	13	0.260	0.062
7	Bierman et al. (1993)—Utrecht Novice Series 2	50	12	0.240	0.060
8	Kanthamani & Palmer (1993)—Subliminal Sending	22	2	0.091	0.061
9	Morris et al. (1993)—Cunningham Study	32	13	0.406	0.087
10	Morris et al. (1993)—McAlpine Study	32	8	0.250	0.077
11	Dalton (1994)—Sender–Receiver Sex Pairing	29	12	0.414	0.091
12	Kanthamani & Broughton (1994)—FRNM Manual Series 3	40	8	0.200	0.063
13	Kanthamani & Broughton (1994)—FRNM Manual Series 4	65	24	0.369	0.060
14	Kanthamani & Broughton (1994)—FRNM Manual Series 7	46	12	0.261	0.065
15	Kanthamani & Broughton (1994)—FRNM Manual Series 8	50	13	0.260	0.062
16	McDonough et al. (1994)—EEG Ganzfeld	20	**8**	0.300	0.102
17	Williams et al. (1994)—Senders/Geomagnetism	42	5	0.119	0.050
18	Bierman (1995)—Amsterdam Series III: Emotional Targets	40	16	0.400	0.077
19	Bierman (1995)—Amsterdam Series IV: Emotional Targets	36	13	0.361	0.080
20	Morris et al. (1995)—Sender/No Sender	97	32	0.330	0.048
21	Willin (1996a)—Musical Targets	100	24	0.240	0.043
22	Willin (1996b)—Musical Targets High Scorers	16	4	0.250	0.108
23	Broughton & Alexander (1997)—AG II: First Timers Series 1	50	12	0.240	0.060
24	Broughton & Alexander (1997)—AG II: First Timers Series 2	50	9	0.180	0.054
25	Broughton & Alexander (1997)—AG II: Emotionally Close	51	19	0.373	0.068
26	Broughton & Alexander (1997)—AG II: Clairvoyance Series	50	11	0.220	0.059
27	Broughton & Alexander (1997)—AG II: General Series	8	3	0.375	0.171
28	Dalton (1997)—Creativity and Psi	128	60	0.469	0.044
29	Parker et al. (1997)—Gothenburg Study 1	30	6	0.200	0.073
30	Parker et al. (1997)—Gothenburg Study 2	30	11	0.367	0.088
31	Parker et al. (1997)—Gothenburg Study 3	30	11	0.367	0.088
32	Symmons & Morris (1997)—7 Hz Drumming	51	23	0.451	0.070
33	Wezelman & Bierman (1997)—Amsterdam Series IVB: Emotional	32	5	0.156	0.064
34	Wezelman & Bierman (1997)—Amsterdam Series V: Altered States	40	8	0.200	0.063
35	Wezelman & Bierman (1997)—Amsterdam Series VI: Altered States	40	10	0.250	0.068
36	Wezelman et al. (1997)—Eigensender	32	14	0.438	0.088
37	Parker & Westerlund (1998)—Gothenburg Serial Study	30	**7**	0.230	0.077
38	Parker & Westerlund (1998)—Gothenburg Study 4	30	14	0.467	0.091
39	Parker & Westerlund (1998)—Gothenburg Study 5	30	**12**	0.400	0.089
40	Alexander & Broughton (1999)—CL1 Ganzfeld	50	18	0.360	0.068
41	Roe et al. (2001)—Sender–Receiver Creativity Scores	24	5	0.208	0.083
42	da Silva et al. (2003)—Ganzfeld vs. No-Ganzfeld	54	18	0.333	0.064
43	Morris et al. (2003)—Creative Population	40	15	0.375	0.077
44	Roe et al. (2003)—Sender as PK Agent 1	40	14	0.350	0.075
45	Wright & Parker (2003)—Real-Time Digital Ganzfeld	74	24	0.324	0.054
46	Goulding et al. (2004)—First Real-Time Digital Ganzfeld Judging	128	30	0.234	0.037
47	Lau (2004)—Bayesian Ganzfeld Approach	120	36	0.300	0.042
48	Parra & Villanueva (2004)—Picture Targets	54	25	0.463	0.068
49	Parra & Villanueva (2004)—Musical Targets	54	19	0.352	0.065
50	Roe et al. (2004)—Sender Role: No Sender	17	4	0.235	0.103
51	Roe et al. (2004)—Sender Role: Sender	23	6	0.261	0.092
52	Stevens (2004)—Feedback Reinforcement	50	12	0.240	0.060
53	Sherwood et al. (2005)—Experimenter Interpersonal Psi	38	8	0.211	0.066
54	Parker (2006/2010)—Identical Twins	**28**	**10**	0.357	0.091
55	Parra & Villanueva (2006)—Ganzfeld vs. Relaxation	138	57	0.413	0.042
56	Pütz et al. (2007)—Covert Ganzfeld Telepathy	120	39	0.325	0.043
57	Simmonds-Moore & Holt (2007)—Schizotypy Trait & State	26	6	0.231	0.083
58	Parker & Sjödén (2008)—Subliminal Priming	29	8	0.276	0.083
59	Smith & Savva (2008)—Ganzfeld Experimenter Effects	114	39	0.342	0.044
All		**2832**	**878**	**0.310**	**0.009**

* Based on the equation given by Utts (1999a:341); see Method subsection in text. FRNM: Foundation for Research on the Nature of Man. AG: autoganzfeld. Bold indicates hits and sessions adjusted or approximated (see text; Notes 10 & 11). Studies 1, 2, 3, & 5 are summarized in Kanthamani & Broughton (1994).

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RESEARCH

The Global Consciousness Project: Identifying the Source of Psi

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Abstract—Analysis of the formal events listed on the Global Consciousness Project (GCP) website as of 9 November 2009 showed that the total Stouffer's Z computed across all events was 5.81—a strong statistical effect. The overwhelming evidence from laboratory-based random number generator studies demonstrates that there are no forces involved in creating the significant effects. Similarly with the GCP formal events, we found that the best fit line through the Z^2 versus number-of-RNGs scatter plot had a slope of $(-5.37 \pm 340) \times 10^{-5}$ ($p = 0.506$) indicating there is no evidence of an asymmetric force to explain the deviant GCP statistic; rather, we show that it is likely that experimenter psi can account for the effect. Dr. Nelson brought 234 events to the attention of the GCP for a Stouffer's Z for his contribution, alone, of 5.91, whereas the 66 other events yielded a Stouffer's Z of 1.26, and the Z of the difference was 3.29 ($p = 4.97 \times 10^{-4}$). This suggests that Dr. Nelson's psi-mediated decision capacity drives the GCP result, and it is unlikely that their primary hypothesis of a putative global consciousness connection to the RNG devices can account for the results.

Keywords: EGG—random number generator (RNG)—Global Consciousness Project (GCP)—Decision Augmentation Theory (DAT)

Introduction

The Global Consciousness Project was launched in 1998 in part in anticipation of the then upcoming Y2K (i.e. date transition from the 20th to the 21st century). It is beyond the scope of this paper to describe the historical development of this intriguing project. Much of it can be found on the website for the project, http://noosphere.princeton.edu; however, we will provide some of the fundamentals here.

The basic idea sprang from the random number generator (RNG)[1] research which may have had its beginning with Helmut Schmidt's seminal publication entitled "Precognition of a Quantum Process" (Schmidt, 1969). The RNG used by Schmidt in both studies that were reported had its base in the radioactive decay of the isotope Strontium-90 (i.e. often written in nuclear physics as Sr^{90}), which is an electron emitter mediated by the weak nuclear force. When an electron was detected by a Geiger-Müller tube, a repeating clock that cycled the integers 1, 2, 3, 4, 1, 2, 3, 4, . . ., etc., at one microsecond per integer was interrupted and the resulting integer between one and four, inclusive, energized an appropriate light on a display panel.

The participants in this study were asked to guess which of four lamps would light after they registered their choice with a button press. *After* the guess was recorded, the RNG chose which of the four lamps to light. Schmidt reported significant results in both studies: Study 1: $n = 63{,}066$, $z = 6.36$, $p = 1.01 \times 10^{-10}$, *effect size* = 0.0253. Study 2: $n = 20{,}000$, $z = 6.55$, $p = 1.91 \times 10^{-11}$, *effect size* = 0.0463.[2] We note that the z-scores are relatively constant with respect to the number of trials and the effect size scales as the square root of the ratio of the trials. We will return to this point in the discussion section below.

The RNGs associated with the Global Consciousness Project accumulate 200 binary bits each second and report back to a central server the number of binary ones accumulated within that second. Over time, the number of such RNGs has grown, and as reported on the website above as of August 2009 there are 65 of them located worldwide.

The RNG Network

May and Spottiswoode (2001) conducted a detailed analysis of the data produced by the network of RNGs. A downloadable PDF version can be found at http://www.lfr.org/LFR/csl/library/Sep1101.pdf

They used all of the 31 days in August and all of the 30 days in September 2001. Each day consists of 86,400 seconds with the number of binary ones (i.e. hits) associated with each RNG for each second. For each second, they only included RNGs that were active (i.e. non-zero hits) and whose hits were in the range [50,150]. That is, if the number of hits were less than 50 or greater than 150, which correspond to a z-score of ±7, they assumed that the RNG in question was faulty. For each second, they computed a Z and Z^2 for each RNG, a Stouffer's Z across the valid RNGs and χ^2 as:

$$\chi^2 = \sum_{i=1}^{n} Z_i^2 \, , df = n.$$

where n is the valid number of RNGs.[3]

For completeness, they examined the Stouffer's *Z* data for all 86,400 seconds of 11 September 2001 in Eastern Daylight Time (EDT). For each *Z*, there is an associated *p*-value, which is the integral of the normal distribution from *Z* to infinity. They computed the theoretical expectation for the *p*-values resulting from *Z*s in the range [−5.0, 5.0], and the observed values from the data of the *p*-value for each *Z* as:

$$P-Value=\frac{\#\ of\ Zs>Z_g}{Total\ \#\ of\ Zs}$$

where Z_g is the given value of *Z*.

The results of their extensive analysis confirms that the network of RNGs at that time and presumably now, satisfy the accepted criteria for randomness and show that:

- The distribution of *p*-values for Stouffer's *Z* meet mean chance expectation even in the rare event tails of that distribution.
- The number of high-value *z*-scores of 4.0, 4.5, and 5.0 for the months of August and September, 2001, individually meet mean chance expectation and do so for the combined months as well.

Finally, in spite of the terrible events of 11 September 2001, we conclude from these analyses, that the network of RNGs function as an excellent source of random numbers both individually and collectively.[4]

The Global Consciousness Project Hypothesis

The overall hypothesis of the Global Consciousness Project has been difficult to understand in that the project, until recently, appeared to have been in a continuing state of exploration—something which more psi researchers should do. We do not put as much attention in hypothesis formulation as we think we need.

The most succinct statement of the hypothesis to date can be found in Bancel & Nelson (2008):

> Periods of collective emotional or attentional behavior in widely distributed populations will correlate with deviations from expectation in a global network of RNGs.

Even in this paper, it remains ambiguous as to what is meant by this hypothesis: Who and more importantly when are people emotionally or attentionally engaged and to what strength and for how long? Is it at the time of

some large tragedy/joyful event? Or when most people become aware of said event? Are the correlative deviations of the RNGs constrained to be in real time with the events. If not, what time window is acceptable?

It is not the intent of this paper to provide an in-depth critique of the GCP in general nor specifically upon the details of the analytical approach; rather, it is to demonstrate a potential source of the psi in the project.

Assumptions

In order to develop the arguments presented in this paper, the following statements will be assumed to be true:

- The network of RNGs (a.k.a. EGGs) are sound and unbiased random number generators.
- The various methods of analyses to produce *z*-scores are sound.
- The hypothesis can change with regard to starting time and duration of the events that are counted as part of the formal set of trials.
- The summary results posted on the GCP website accurately represent a significant effect.

Source of the Psi

Before we can identify the source of the psi that results in the GCP's significant effect, we must examine the limited number of possibilities. Although the title of Schmidt's original paper (1969) referred to precognition, from that time onward the name accepted for the observation of deviations from mean chance expectation of the data stream from RNGs was micro-PK (μPK) or just PK. For example, Schmidt almost immediately began using the PK term (Schmidt, 1970). This, of course, implies, by definition, that these devices physically change in some way, as a result of some PK effort, so as to affect their outputs. Some of our colleagues have criticized May by saying that most people at the time never thought of μPK in terms of a force/bit in the device. In our opinion, this is an Orwellian-like attempt to rewrite history.

Dean Robert Jahn, head of the former Princeton Engineering Anomalies Laboratory, illustrates the point:

> Over this large a data base [PEAR's RNG data], there arises some quantitative statistical regularity in the PK process, epitomized by the mean slopes of the cumulative deviations in Figs. 14 and 15 and by the terminal values of the average deviations in Fig. 16. Traced back to the elemental binary samples, these values imply *directed inversions* from chance behavior of about one or one and a half bits in every one thousand or, alternatively, of 0.2 or 0.3 bits per trial. (Jahn, 1982, emphasis added)

Taking Jahn's estimated hit rate of 1.5 bit/thousand or 0.5015, we compute an effect size of 0.003, a value which is typical in the RNG literature. That is, this effect size is the estimate of the degree to which RNG hardware yields to human-mediated μPK. This supposition is testable using Decision Augmentation Theory (May, Utts, & Spottiswoode, 1995a). In a typical laboratory RNG study, a participant (or experimenter) presses a button that samples n bits from the generator. A z-score statistic is usually computed from the total number of binary ones in the observed sequence.

Decision Augmentation Review

May, Utts, and Spottiswoode (1995b) analyzed 128 RNG studies which constituted the published results up to 1989. In accordance with the DAT formalism, they constructed a scatter plot of n versus z^2, where n is the number of bits per button press and z^2 is the square of the z-scores that resulted. A simple weighted least squares regression was used to compute the intercept and slope of the best fit straight line through these data. DAT predicts zero for the slope and if $\varepsilon_{\mathrm{PK}}$ is the putative PK effect size, then the best fit line under the PK hypothesis will have a slope of: ε_{PK}^2 . See May, Utts, & Spottiswoode (1995b) for the derivation of these results.

Figure 1 shows the results of the DAT analysis of that historical RNG database.

For readability, Figure 1 displays only a portion of the problem space in that the minimums/maximums are [0.862, 3.86] and [16, 10000] for z^2 and n, respectively. The thick horizontal line at $z^2 = 1$ is the mean chance expectation under the null hypothesis of no psi at all, and the solid black line at $z^2 = 1.036$ is the best fit line through all the data:

$$y = 1.036 \pm 0.05 + (1.73 \pm 10.01) \times 10^{-6} (n - 1750).$$

The dashed lines surrounding this line display the one standard error of the slope. The sloping dot-dashed lines represent what the best fit line would be under two values of the PK effect size of 0.003 for the lower one and 0.01 for the upper one.

The elevated best fit line is significantly above the mean chance expectation for Z^2 of one ($z = 6.4$, $p = 7.77 \times 10^{-11}$), and the one standard error for the slope encompasses zero and is not significantly different from zero ($t(126) = 0.173$, $p = 0.432$).

Clearly, any asymmetric force/bit model must be rejected in that the standard error of the slope surrounds zero, the DAT prediction, and the lines representing values of the best fit under the PK hypothesis lie mostly outside the one standard error for the fitted slope.

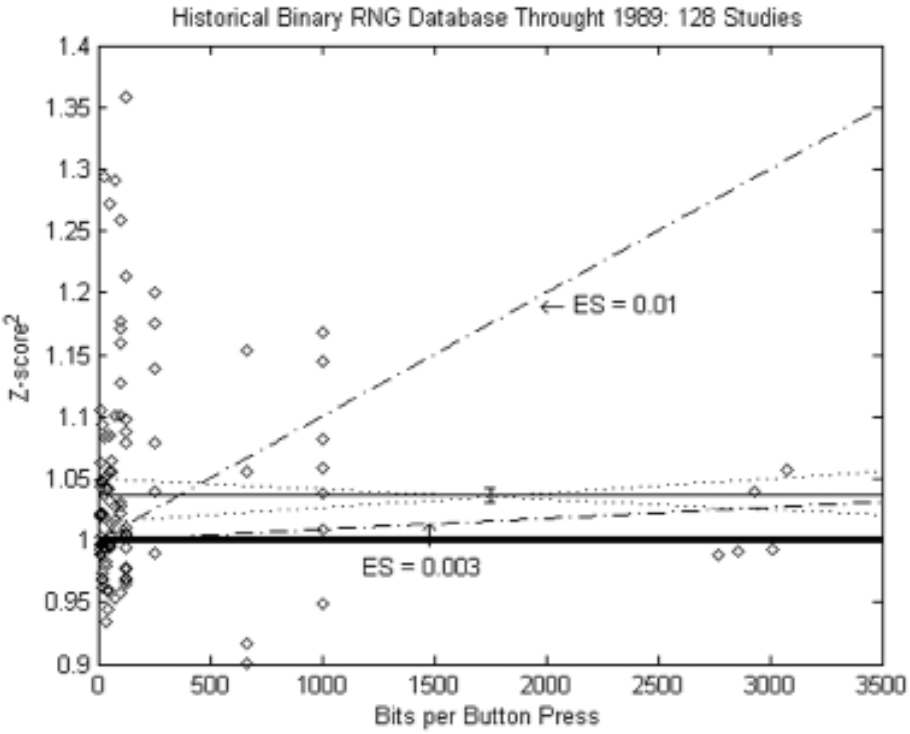

Figure 1. DAT Analysis of the Historical RNG Database.

It appears, then, that participants in these kinds of studies use their psi ability to select out locally deviant subsequences from otherwise unperturbed output sequences from the devices. There is a caveat to this assertion. If it turns out that participants' PK ability (i.e. effect size) across time and across participants dropped off as $\frac{1}{\sqrt{n}}$, then this DAT analysis could not distinguish between a force-like and informational mechanism. Similarly, as we point out above, this analysis would not be able to detect interactions that leave the mean of the parent distribution exactly the same as under the null hypothesis of no psi.

Returning to Schmidt's original study (1969), we find that the effect sizes reported in that paper scale as $\frac{1}{\sqrt{n}}$, just as DAT predicts.

With regard to the results from RNGs in the Global Consciousness Project network, there are a very limited number of possible explanations.

GCP Potential Explanations

The first, most obvious, and easiest to reject is the mean chance expectation (MCE) null hypothesis. The analysis shown on the GCP website clearly demon-

strates a sizeable effect and we have assumed their result to be correct.

The second and the most popular supposition is that the network of RNGs somehow responds to human and natural events. That is, the RNGs exhibit significant deviations from MCE during or temporally near these events. This supposition divides cleanly into two hypotheses:

1. Human and/or natural events exert PK-like forces upon the devices, which account for their significant deviations from MCE.
2. There is no PK-like force/bit; rather, somehow these devices are simply correlated with the human/natural events.

We might have to reject the first hypothesis for a number of reasons. First of all, a large portion of the laboratory-based RNG studies clearly show no force per bit. Second, many private communications from the GCP community also reject the force per bit hypothesis and even go so far as to criticize May for even suggesting it. So we are left with hypothesis two. The GCP data will be the final arbiter with regard to this point.

As we all have learned in our statistics courses, correlation does not necessarily imply a causal relationship between the variables. Hypothesis 2 above also bifurcates. Either human/natural events magically happen on average only during times of locally deviant, but expected, excursions of the RNGs, or vice versa. Even though there does not have to be a causal relation for this correlation to arise, we are obligated to search for a third (or more) variable(s) that gives rise to the correlation. In many cases, an external (to the primary correlative variables) variable is difficult or impossible to identify.

In the case of the GCP correlations, a third variable to consider is experimenter psi operating by means of Decision Augmentation Theory or DAT.

Decision Augmentation Theory and the GCP

There are two aspects to identify a possible third experimenter psi variable. The first is to determine if there is evidence for a force/bit in the GCP dataset. If there is no evidence, then the next step is to determine the degree to which the known experimenters may have contributed to the result. Thanks to the extensive, and quite laudable, reporting of the results on the GCP website, we can test both of these suppositions.

The Dataset. The table in Appendix 1, which has been taken directly from the GCP website and added here, shows the formal events that contribute to the stated results. In accordance with the preamble to this table on the website, there were a few events, indicated by a leading red asterisk, that we have removed from all analyses. These 13 events number 2, 10, 18, 19, 20, 30, 33, 34, 38, 44, 66, 81, and 116. For completeness, we have included all other events in the analyses that follow.

We computed the Stouffer's *Z*-score for the 300 remaining events to be 5.81 which is consistent with the value 5.78 that appears on the site. As was stated in the Assumptions section above, we accept these numbers to be robust and evidential of some non-chance phenomenon.

Formal DAT Analysis. To determine whether there is a force/bit effect in these data, we created a scatter plot of the stated *Z*-score squared against the number of RNGs that were used to compute the *Z*-score. Most of the number of RNGs (i.e. column number 3 in the table in Appendix 1) were integers. One (row number 25) however was listed as "Var" and was removed from the DAT analysis. A few others scattered about the dataset showed small ranges of the number of RNGs used. In these cases, we used the mean of the range for the single number for that individual event. Figure 2 shows the DAT analysis for the remaining 299 events.

The axes in Figure 2 have been expanded for clarity. The Z^2 range was $[3.5 \times 10^{-5}, 10.3]$, and the range of the number of RNGs was [3, 72].

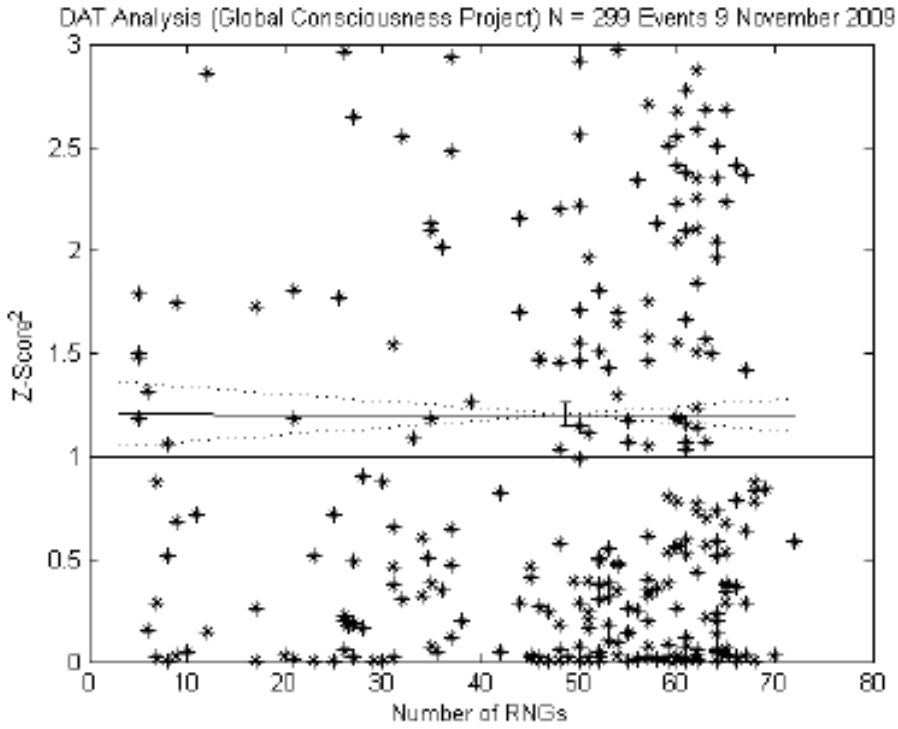

Figure 2. DAT Analysis of the Global Consciousness Project.

The horizontal black line at $Z^2 = 1$ is mean chance expectation, and the error bar at 48 RNGs is one standard error for the intercept at 48 RNGs for the best fit line through the data, which is shown as a nearly horizontal line at $Z^2 = 1.200 \pm 0.058$. The slope of this line is $(-5.37 \pm 340) \times 10^{-5}$. One standard error in the slope is shown in Figure 2 as sloping dotted lines. The one standard error bars for the intercept are shown as the weighted mean of the number of RNGs.

An asymmetric force/bit mechanism requires a non-zero slope and the observed slope is first of all negative and the slope's one standard error easily encompasses zero, which is required for a DAT interpretation.

A more general interpretation requires a deeper discussion. Rejecting the force argument on the basis of a zero slope of the regression line is valid only for forces that distort the parent distribution in asymmetrical ways; that is, all force-like interactions that leave the mean of the parent distribution exactly equal to zero will give rise to a zero slope in this analysis. Since it was common in the literature and in the psi research "culture" of the 1970s and for the next 20 years or more that RNG micro-PK involved a force per bit (Jahn, 1982), our analysis was focused on this point.

Thus we conclude that the effect from the current formal dataset for the Global Consciousness Project appears not to include a force/bit or other kind of asymmetric influence. That is, the network of RNGs associated with the GCP are not physically changed asymmetrically as a result of human and or natural events.

The original DAT formalism accounted only for direct linear forces. That is under the PK hypothesis, the parent distribution mean shifted proportionally to the PK effect size. This approach was reasonable in that the RNG community collectively thought in terms of micro-PK, or a force per bit interaction. A linear shift in the mean predicts a non-zero slope to the best-fit regression line in a number of studies with Z^2 versus number of bits resulting from a single button push. We have come to realize that a zero slope through such data is insufficient to reject more complex PK interactions. For example, any interaction that does not shift the mean but changes other moments of the parent distribution would not be detected with this analysis.

The putative interaction claimed by the GCP community arises only in the variance of the parent distributions and thus would not lend itself to a DAT analysis. But the RNGs in the GCP are conceptually similar to the ones used in RNG studies (including those conducted by PEAR) in the vast literature in which the interaction arose as a linear mean shift of the parent distribution. Why would the GCP data be any different? Thus we call into question the GCP's underlying assumption of variance interaction. In addition, for the DAT analysis to be invalid requires the mean shift to be nearly identically equal to zero—unlikely to be sure.

Our earlier work showed that in the non-GCP studies, there was no mean shift of the parent distribution; rather, the sampling was biased by the operators' precognitive ability. So we think that the DAT analysis stands for the GCP data.

Stouffer's Z Analysis. As indicated above, the Stouffer's *Z*-score for the total dataset was 5.81. The GCP website should be further commended for indicating which individual(s) brought the formal event to the GCP for analysis. This allows for an unprecedented opportunity to determine the degree to which any differences can be observed.

Of the 300 formal events, we found that Dr. Nelson, the founder and arguably the driving force of the GCP, either singularly or among others brought 234 events to the project; whereas, all others totaled 66. The Stouffers's *Z* for the "Nelson" events was 5.91 and for the others the Stouffers's *Z* was 1.26. The *Z*-score for the difference is 3.29 ($p = 4.97 \times 10^{-4}$). Thus, there appears to be something "special" about the events that were brought to the attention of the GCP.

Conclusion and Discussion

The Global Consciousness Project's array of RNGs is an impressive engineering feat. It is clear from the GCP's own analyses and ours that the "control" output of these devices, individually and collectively, meet the current standards for producing random bit streams. Furthermore, the raw data and the analyses are available to the public.

The DAT analysis of the formal events ($n = 300$), shows no evidence of any asymmetric interaction with the physical devices. Under the DAT hypothesis, the expected slope for a regression line through the scatter plot of Z^2 versus number-of-RNGs is zero. The observed slope was zero to three significant figures and the one-standard error of the slope surrounded zero (slope = $[-5.37 \pm 340] \times 10^{-5}$). Tested against a zero slope, the *p*-value is 0.506. Thus, these physical devices are not responding asymmetrically in any way to human or natural events. Even though the formal DAT analysis is insensitive to symmetric influences such as affecting the variance of the parent distribution, we think it is unlikely that such an interaction would leave the mean unchanged given that most all of the published RNG PK data suggest otherwise.

Yet, there is a strongly significant effect. As we indicated above, we must now rely on some correlation to account for these effects. It seems most unlikely since the RNG devices do not "know" about human or natural events, that these events somehow line up in such a way as to correlate with the unperturbed random fluctuations of the RNGs.

A possible third variable that may link the RNGs to the events is the experimenters. And among the experimenters (i.e. source), Nelson is nearly singularly

responsible for the effect. In private communication with Dr. Nelson, he suggested that the reason this is true is that he knows, by means other than psi, what events are best suited for the analysis. We find this argument to be spurious. To realize that, say earthquakes would be an effective event while sporting events would not, would require an independently supported model which predicted, and hopefully explained, why these classes of event would show differing GCP effects. No such model has been offered.

We are left then to conclude that Dr. Nelson's DAT-like decision capacity drives the GCP result, and it is unlikely that their statistically robust result is due to a variation of their primary hypothesis of some global consciousness connections to the RNG devices.

Unfortunately, this kind of psi-mediated experimenter effect is not limited to Nelson alone. May, Paulinyi, and Vassy (2005) demonstrated in their skin conductance study that the primary, and presumably cherished, hypothesis that their participants' skin conductance was reacting, in advance, to a future randomly chosen startle acoustic stimulus was not supported by the data. Instead, the results strongly suggested that the results arose because of a psi-mediated experimenter effect enabled by DAT. Spottiswoode and May (2003) published the protocol and pilot results of their pre-stimulus response study with acoustic stimuli. Their still-unpublished formal results of over 5σ can be attributed directly to DAT by the experimenters.

Clearly we are not the first to notice the potential of experimenter psi in studies. DAT just added a formal mathematical and testable method to allow for the possibility of determining whether force-like or informational processes better describe the observable. This kind of statistically robust experimenter effect represents a major challenge to researchers in parapsychology. If psi-enabled experimenters, such as Dr. Nelson and ourselves, can achieve significant results for their favored hypotheses by the DAT process, then discovering the mechanism of psi through classical hypothesis testing is problematic indeed.

Notes

[1] We will use this acronym for the devices rather than the popular term random event generator which seems to us to be contrived.

[2] We have recomputed the statistics and added the effect sizes based upon the reported raw results. The *z*-scores agree with those reported by Schmidt as Critical Ratios.

[3] It is important to emphasize that the GCP analysis uses the χ^2 approach derived from the summed Z^2 scores.

[4] We do not use these results to refute the significant data posted on the GCP website; rather, we use it to show that at least in 2001 the network of RNGs appeared to function according to mean chance expectation in the aggregate.

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APPENDIX 1

This table was taken direct from the Global Consciousness Project website (http://noosphere.princeton.edu) on November 9, 2009. The preamble to this table from the site is:

> Statistical evaluations use a "normalized" database, with normalization based on an empirical estimate of variance for each egg, calculated from its full database of trials. In addition we exclude all "bad data" identified by standardized rules (e.g., trial scores outside the range 55 to 145 are almost certainly errors). The formal database also excludes 13 poorly defined or partially redundant events marked in the results table with a red asterisk. Cases with no normalized calculation are marked with a double asterisk. Statistics for very recent events (marked with ~) will change slightly when normalized calculations are done.
>
> red = significant; light red = predicted direction;
> green = opposite and significant

In the hardcopy print version of this issue of the *Journal*:

bold = significant; gray = predicted direction;
bold italics = opposite and significant

The online *Journal* version retains the original red, light red, and green colors.

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
1. Embassy Bombings, 980807	Nelson	3	15-min	3.209	0.001
2. * Omagh Bombings, 980815	Nelson	4	15-min	-0.675	0.75
3. US Airstrikes, Afghanistan, 980820	Nelson	5	15-min	1.148	0.126
4. Swissair 111 crash, 981003	Nelson	5	15-min	1.338	0.091
5. McGwire, record homerun, 980908	Nelson	9	15-min	1.32	0.093
6. Clinton Affair Grand Jury, 980911	Nelson	5	15-min	0.387	0.349
7. Nicaragua, Casitas collapse, 981030	Nelson	5	15-min	-1.066	0.851
8. Nicaragua, Casitas flooding, 981030	Nelson	5	15-min	1.215	0.113
9. Global Peace Vigil, Taiwan, 981113	Nelson	5	15-min	1.223	0.111
10. * Iraq, 11th hour decision, 9811207	Nelson	4	15-min	-0.412	0.658
11. World Peace Prayer, 981210	Nelson	7	15-min	-0.135	0.554
12. Iraq, Bombing, 981217	Nelson	7	15-min	-0.535	0.704
13. House votes Impeachment, 981219	Nelson	7	15-min	-0.934	0.826
14. Christmas Eve, UTC, 981224	Elrod	6	15-min	-1.028	0.848
15. Christmas Eve, EST, 981224	Elrod	6	15-min	-0.078	0.531
16. Christmas Eve, PST, 981224	Elrod	6	15-min	-0.715	0.763
17. New Years, 24 hrs, 981231	Nelson	9	1-sec	-0.171	0.568
18. * New Years, Three Sevens, 981231	Nelson	9	1-sec	0.871	0.208
19. ** New Years, Euro vs US, 981231	Ekman	9	1-sec	0.845	0.199
20. ** New Years, Mind vs Mist, 981231	Broughton	9	1-sec (EM)	1.185	0.122
21. Columbia, Amanda Cooke, 980126	Nelson	9	15-min	0.823	0.205
22. Shuttle accuda Clinton, 980212	Nelson	10	15-min	0.217	0.414
23. Nida Bombs Yugoslavia, 990324	Nelson	12	15-min	1.691	0.045
24. Dow Closes Over 10000, 980329	Cohen, Nelson	12	15-min	0.384	0.35
25. Praying for Peace, 980403 to 0503	Taylor	Var	1-min	0.819	0.206
26. Lillehen School Tragedy, 980420	Polk	11	15-min	-0.844	0.801
27. Autocracy, Israel, PLO, 980505	Knaak	17	15-min	-0.057	0.523
28. "Peace at Last", Hamilton, 980510	Nelson	17	15-min	0.505	0.307
29. Yugo War Ends, Milosevic, 980610	Nelson	17	15-min	1.313	0.095
30. * JFK Jr Crash, 30-min, 980717	Nelson	21	15-min	0.283	0.388
31. JFK Jr Crash, 3 Hours, 980717	Nelson	21	15-min	1.344	0.089
32. India, Train Crash, 980801	Nelson	17	15-min	1.813	0.035
33. * Solar Eclipse, 980811	Westermann, Nelson	20	15-min	-0.008	0.502
34. * Turkey, Earthquake, 30-min, 980817	Nelson	20	15-min	-0.698	0.754
35. Turkey, Earthquake, 4 Hours, 980817	Nelson	20	15-min	-0.171	0.568
36. Japan, Nuclear Accident, 980930	Nelson	21	15-min	1.086	0.138
37. Million Person Meditation, 981006	Nelson	21	15-min	2.342	0.012
38. * Typhoon, India, 2 Hours, 981028	Nelson	22	15-min	-0.663	0.72
39. Typhoon, India, 24 Hours, 981028-30	Nelson	23	15-min	-0.038	0.515
40. Earthquake, Turkey II, 2 Hrs ±, 981112	Dunne	21	15-min	-0.105	0.542
41. Full Moon, Solstice, 4.8 Min, 981222	Founder	26	1-sec, epoch	-0.845	0.801
42. New Year, Y2K, 24 hrs, 19981231	Nelson	28	1-sec, epoch	-0.457	0.658

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
43. New Year, Y2K, Variance, 19991231	Radin	27	1-sec, epoch	0.157	0.438
44. ** Hi vs Low Population, Y2K, 991231	Nelson	27	1-sec (SDs)	-0.647	0.741
45. Just A Minute, 1 Min Epoch, 20000101	Srinivasan	27	1-sec, epoch	0.697	0.243
46. Papal Visit, Israel, 20000321-26	Reinhilde	27-29	1-min	2.426	0.008
47. Gaiamind Experiment II, 20000423	Nelson	28	1-sec	-0.23	0.591
48. World Earth Healing Day, 20000504	Nelson	26-27	1-sec	-0.413	0.66
49. Concorde Crash, Paris, 20000725	Reinhilde	23	1-sec	-0.715	0.763
50. Kursk Tragedy, 30-min, 20000812	Nelson	27	1-sec	1.625	0.052
51. Kursk, 10 days, 20000812-21	Nelson	24-27	1-min	1.33	0.092
52. US TV: Survivors Finale, 20000824	Lettieri	28	1-sec	-0.471	0.681
53. Peace Summit Relig Spirit, 20000828-31	Murali Dias	28	1-min	-0.448	0.672
54. Olympic Opening Ceremony, 20000915	Robin Taylor	26	1-sec	0.022	0.491
55. Group Mind Meditation, 20000924	Nelson	28	1-sec	1.72	0.043
56. Pierre Trudeau Funeral, 20001003	China Srinivasan	28	1-sec	1.982	0.024
57. Group Mind Meditation 2, 20001022	Matthew Webb	28	1-sec	-2.085	0.981
58. US Election 2000, 20001107, 8, 9 ...	Paul Bethke	30-32	1-sec	0.169	0.437
59. Group Mind Meditation 3, 20001112	Matthew Webb	30	1-sec	0.027	0.489
60. Group Mind Meditation 4, 20001126	Matthew Webb	28	1-sec	-0.028	0.511
61. Miss World 2000, 20001130	Mukesh Garg	31	1-sec	1.242	0.107
62. US Election 2000, 20001208	Bethke, Nelson	27	1-sec	0.436	0.331
63. US Election 2000, Finality, 20001212	Nelson, Bethke	30	1-sec	0.934	0.175
64. New Year 2000-2001 Mean, 20010101	Nelson	34-35	1-sec	-1.778	0.962
65. New Year 2000-2001 Var, 20010101	Nelson	34-35	1-sec	-0.708	0.761
66. ** Lovewave 010101, 20010101	Nelson, Lettieri	34-35	1-sec	2.688	0.004
67. Central America Quake, 20010113	Nelson	35	1-sec	1.445	0.074
68. Bush Inauguration, 20010120	Nelson	32	1-sec	-0.55	0.709
69. Kumbh Mela, India, 20010124	Panchal, Srinivasan	35	1-sec	1.457	0.073
70. Western India Quake, 20010126	Nelson	33	1-sec	-1.042	0.851
71. WorldPuja Webcast, 20010331	Joanne Karl, Nelson	38	1-sec	0.591	0.277
72. Jahrel Ceremonies, 20010401	Nelson, Ammar	38	1-sec, epoch	1.419	0.078
73. Earth Day, 2001, 20010422	Nelson	32	1-sec	1.595	0.056
74. Full Moon in Taurus, 20010507	Don Watson	28	1-sec	0.946	0.172
75. World Peace Meditation, 20010520	Masami Kondo	34	1-sec	0.773	0.22
76. World Earth Healing Day, 20010621	WEHD Organizer	31	1-sec	0.609	0.271
77. Roll Your Own Blackout, 20010621	Greg, Eileen, Roger	31	1-sec	-0.807	0.79
78. Beijing Gets 2008 Olympics, 20010713	Peter Bancel	31	1-sec	0.681	0.248
79. Buddhist Stupa Ceremony, 20010808-17	Peter Bancel	34-37	1-min	-0.213	0.584
80. Terrorist Disaster, Sept 11, 20010911	Nelson	37	1-sec	1.873	0.031
81. ** Terror, Variance, Sept 11, 20010911	Dean Radin, Nelson	37	1-sec	1.305	0.096
82. Silent Prayer, Sept 14, 20010914	Doug Mast	36	1-sec	1.087	0.138
83. Sea to Shining Sea, 20010922	Paul Bethke	37	1-sec	1.713	0.043

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
84. MUM Peace Meditation, 20010823-27	Orme-Johnson	37	1-sec	0.346	0.365
85. Basin Afghan Bombing, 20011007	Nelson	37	1-sec	0.684	0.247
86. Children Photon Attendance, 20011012	William Braud	34	1-sec	0.628	0.265
87. Binding Spell on Bin Laden, 20011016	Greg Nelson	36	1-sec	0.616	0.269
88. Assassination in Israel, 20011017	Joseph Menahem	37	1-sec	1.673	0.047
89. World Series, Yankees 3/4, 20011031	Lelieri, Nelson	36	1-sec	0.287	0.387
90. World-Wide Meditation, 20011111	Wolf, Watson	38	1-sec	-0.447	0.673
91. Crash of AA 587, 20011112	Nick Reiter, Nelson	38	1-sec	-1.123	0.869
92. Ramadan Muslim Prayer, 20011116	Pasternak, Nelson	37	1-sec	1.838	0.033
93. WorldPuja Meditation, 20011118	Ferguson, Nelson	37	1-sec	-0.802	0.789
94. George Harrison Tribute, 20011203	Watson, Nelson	42	1-sec	0.222	0.412
95. Sir Peter Blake Murdered, 20011206	Peter French	42	1-sec	-0.908	0.818
96. Astrological Moments, 20011209-14	Anonymous, Nelson	42-43	1-sec	2.207	0.014
97. New Year 2001-2002, Mean, 20020101	Nelson	44	1-sec	1.468	0.071
98. New Year 2002, Var, 20020101	Nelson	44	1-sec	0.53	0.298
99. Volcanic Eruption, Congo, 20020117	Outo, Nelson	46	1-sec	-0.034	0.514
100. Astrid Lindgren Dies, 20020128	Suurküla, Nelson	46	1-sec	0.513	0.304
101. Winter Olympic Opening, 20020208	Nelson	46	1-sec	1.216	0.112
102. Sri Lanka Peace Meditation, 20020215	Johnson-Lenz	47	1-sec	-0.086	0.534
103. Earthquake, Afghanistan, 20020325	Nelson	45	1-sec	-0.68	0.752
104. Passover Bombing, 20020327	Roger Nelson	47	1-sec	-0.064	0.526
105. Jenin Incursion, Israel, 20020403	Haim Azzam, Nelson	47	1-sec	0.487	0.313
106. Tiger Woods Triumph, 20020414	Ray Anderson	46	1-sec	-0.147	0.558
107. Indigo Peace Meditation, 20020420	Don Watson	44	1-sec	-1.303	0.904
108. Erfurt School Shooting, 20020426	Roger Nelson	46	1-sec	0.753	0.226
109. World Cup Soccer, 20020607	Tobias Bodine	48	1-sec	-0.082	0.533
110. Astrological Moments II, 20020620-23	Anonymous, Nelson	47-45	1-sec	1.206	0.114
111. Summer Solstice 2002, 20020621	Joe Giove	46	1-sec	-0.638	0.738
112. World Cup Soccer, 20020630	Pedro de Oliveira	45	1-sec	0.147	0.442
113. World Healing Day, 20020622	Tremblay, Giove	50	1-sec	0.986	0.16
114. 9/11 Anniversary, March Prod., 20020911	Eckhard Etzold	50	1-sec	1.307	0.096
115. 9/11 Anniversary, Mean, 20020911	Mitchell, Nelson	50	1-sec	-1.598	0.945
116. ** 9/11 Anniversary, Var, 20020911	Nelson	50	1-sec	-0.313	0.623
117. Kenny's Birthday, 20021009	Marla Buchholz	51	1-sec	-2.04	0.98
118. Bali Bombing, 20021012	Nelson	50	1-sec	-0.274	0.608
119. Terror in Bali, 20021012	Multiple Sources	50	1-sec	0.032	0.487
120. EarthDance 2002, 20021012	Pete Douza	50	1-sec	-0.056	0.522
121. Wellstone Crash, 20021025	Nelson	51	1-sec	0.489	0.312
122. Chechen Hostage Crisis, 20021026	Nelson	51	1-sec	0.401	0.344
123. Kenyan Terrorist Attacks, 20021128	Pasternak	52	1-sec	-0.7	0.758
124. Suicide Bombing, Grozny, 20021227	Nelson	51	1-sec	-0.036	0.514
125. New Year, 2003, Mean, 20030101	Roger Nelson	54	1-sec	-1.723	0.958

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
126. New Year, 2003, Var, 20030101	Roger Nelson	64	1-sec	-0.303	0.619
127. Tel Aviv Bombing, 20030105	Gadi Guy	62	1-sec	-0.008	0.503
128. Antiwar Protests, 20030118	Ashley Young	60	1-sec	1.207	0.114
129. Columbia Shuttle Disaster, 20030201	Various, Nelson	48	1-sec	-1.014	0.845
130. Peace Meditations, 20030208	Various, Nelson	48	1-sec	-0.244	0.596
131. Global Peace Demonstrations, 20030215	Nelson	48	1-sec	1.483	[illegible]
132. Korean Subway Fire, 20030218	Nelson	61	1-sec	0.625	[illegible]
133. Lysistrata and Women, 20030303	Nelson	60	1-sec	-1.707	[illegible]
134. Gather The Women, 20030308	Carol Hansen Grey	60	1-sec	-0.532	0.703
135. Serbian Assassination, 20030312	Nelson	60	1-sec	-1.243	0.893
136. Candlelight Vigil, 20030316	Various & Nelson	61	1-sec	1.783	0.037
137. War in Iraq, 20030320	Nelson	48	1-sec	-0.422	0.663
138. GE Prayer for Bush, 20030401	Nelson	61	1-sec	-0.155	0.562
139. Saddam Falls, War Ends, 20030409	Nelson	62	1-sec	1.341	0.09
140. Bombing in Riyadh, 20030512	Nelson	63	1-sec	0.55	[illegible]
141. Bombing in Morocco, 20030516	Nelson	60	1-sec	1.071	0.142
142. Earthquake in Algeria, 20030521	Nelson	62	1-sec	0.551	0.291
143. Aqaba Summit, Middle East, 20030604	Nelson	64	1-sec	1.136	0.128
144. Rainbow 4th of July, 20030704	Nelson	64	1-sec	-0.683	0.753
145. Love, Peace, Water, 20030725	Nelson	48	1-sec	0.154	0.439
146. Manila Coup Attempt, 20030727	Beth Macdonald	61	1-sec	1.054	0.146
147. Blackout 2003, 20030814	Nelson	48	1-sec	-1.206	0.886
148. Baghdad UN Bombing, 20030819	Nelson	45	1-sec	-0.175	0.569
149. Mars Close Approach, 20030827	Adrian Patrut	48-50	1-sec	0.623	0.267
150. World Healing 2003, 20030910	Various	66	1-sec	-1.062	0.86
151. 911 Anniversary, 20030911	Nelson	66	1-sec	-0.382	0.649
152. Typhoon in Korea, 20030912	Nelson	66	1-sec	0.365	0.358
153. Dalai Lama in NYC, 20030921	Nelson	62	1-sec	0.145	0.442
154. Harmonic Concordance, 20031109	[illegible]	62	1-sec	0.612	0.27
155. Turkish Synagogue Bomb, 20031115	Various	63	1-sec	0.425	0.335
156. Turkish Bank & Consulate, 20031120	Various	62	1-sec	0.091	0.464
157. Russian Bombing, 20031205	Nelson	60	1-sec	-1.488	0.932
158. Saddam Hussein Captured, 20031213	Nelson	61	1-sec	0.451	0.326
159. Oprah Winfrey in Africa, 20031216	Nelson	62	1-sec	0.741	0.229
160. Earthquake in Bam, Iran, 20031226	Nelson	63	1-sec	0.616	0.269
161. 60 Seconds for Peace, 20031227	Craig Hamilton	64	1-sec	1.303	0.096
162. New Year, 2004, Mean, 20040101	Nelson	64	1-sec	0.16	0.436
163. New Year, 2004, Var, 20040101	Nelson	64	1-sec	-0.684	0.753
164. Haiti [illegible], 20040201	Nelson	63	1-sec	0.742	0.229
165. Iraq Train Explosion, 20040218	Nelson	62	1-sec	1.226	0.11
166. Oscar Ceremonies, 20040229	Nelson	57	1-sec	0.159	0.437
167. Attacks on Shiites, 20040302	Nelson	58	1-sec	0.617	0.269

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
168. Terror Attacks in Madrid, 20040311	Nelson, Lassen, Bierman	58	1-sec	-1.971	0.976
169. Demonstrations in Spain, 20040312	Nelson, Lassen	58	1-sec	1.582	0.057
170. Global Day of Peace, 20040320	Carl Haug, Nelson	57	1-sec	-1.324	0.907
171. Cricket, India vs Pakistan, 20040324	Srinivasan, Nelson	60	1-sec	1.243	0.107
172. Rantisi Assassination, 20040417	Nelson	62	1-sec	-1.11	0.867
173. Korean Train Explosion, 20040422	Don Watson	60	1-sec	0.748	0.228
174. Floods in Haiti, 20040524	Nelson	66	1-sec	0.634	0.267
175. Iraq Insurgent Bombs, 20040624	Nelson	63	1-sec	0.761	0.228
176. Train Crash, Turkey, 20040722	Don Watson	64	1-sec	-0.854	0.803
177. Democratic Conv, Kerry, 20040729	Nelson	63	1-sec	1.381	0.09
178. Fire in Paraguay, 20040801	Nelson	64	1-sec	0.364	0.358
179. Bob Moris, 20040812	Berger, Nelson	63	1-sec	0.461	0.322
180. Olympic Opening Athens, 20040813	Antonatouca, Pikanis, Nelson	61	1-sec	-1.298	0.901
181. Day of Violence, 20040831	Nelson	64	1-sec	1.401	0.081
182. Republican Conv, Bush, 20040902	Nelson	57	1-sec	-0.445	0.672
183. Russian School Hostages, 20040903	Nelson	57	1-sec	2.256	0.012
184. Earthdance 2004, 20040918	Nelson and Others	60	1-sec	-1.48	0.932
185. Peace Vigil + Hurricane, 20040921	Nelson and Others	62	1-sec	-1.533	0.937
186. Bombing, Taba, Egypt, 20041007	Nelson	63	1-sec	1.252	0.105
187. US Elections 2004, 20041102	Nelson and Others	64, 63	1-sec	-1.223	0.889
188. Arafat Dead, 20041111	Nelson	64	1-sec	1.583	0.057
189. Tsunami Indian Ocean, 20041226	Nelson	62	1-sec	0.084	0.462
190. New Year, 2005, Mean, 20050101	Nelson	57	1-sec	-0.634	0.737
191. New Year, 2005, Var, 20050101	Nelson	57	1-sec	-1.636	0.967
192. Elections in Iraq, 20050130	Nelson	62	1-sec	1.608	0.054
193. Hariri Assassination, 20050214	Nelson	60	1-sec	0.169	0.433
194. Steam Deli York, 20050301	Spano	62	1-sec	-0.301	0.628
195. Quake, Indian Ocean, 20050328	Nelson	64	1-sec	0.244	0.404
196. Pope John Paul II Dies, 20050402	GCP/CNS Group, Paiul, Nelson	66	1-sec	0.057	0.477
197. Pope John's Funeral, 20050408	Nelson	61	1-sec	1.781	0.037
198. Prince Charles' Wedding, 20050409	Paiul	66	1-sec	1.485	0.067
199. Concord Resonance, 20050423	Danilo, Nelson	61	1-sec	1.888	0.031
200. Live 8 Concert, 20050702	Nelson and Others	64	1-sec	-2.003	0.977
201. London Bombings, 20050707	Nelson and Others	66	1-sec	0.181	0.428
202. Hurricane Katrina, 20050829	Raffay, Nelson	58	1-sec	-0.728	0.766
203. Stampede, Baghdad Bridge, 20050831	Nelson	58	1-sec	2.183	0.014
204. End the War Rally, 20050924	Nelson	63	1-sec	1.636	0.051
205. Bali Bombing 2, 20051001	Nelson	63	1-sec	1.844	0.033
206. Earthquake Pakistan, 20051008	Nelson	63	1-sec	1.739	0.041
207. India Train Crash, 20051029	Nelson	61	1-sec	0.347	0.364
208. India Delhi Bombs, 20051029	Nelson	61	1-sec	0.125	0.45

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
209. Jordan Bombs, 2005110[illegible]	Nelson	[illegible]	1-sec	1.[illegible]	[illegible]
210. New Year Dev-Nel Var, 20051231	Nelson, Bancel	[illegible]+	1-sec, Dev-Nel	-0.51	0.[illegible]
211. New Year Cover Mix, 20051231	Nelson, Bancel	[illegible]+	Cover, smooth	0.[illegible]	0.1[illegible]
212. Hadj Stoning Stampede, 20060112	Nelson	[illegible]	1-sec	0.8[illegible]	0.2[illegible]
213. Mudslide Philippines, 20060217	Nelson	[illegible]	1-sec	0.771	0.22
214. Golden Dome Bombing, 20060222	Nelson	[illegible]	1-sec	-0.[illegible]	0.[illegible]
215. Planetary Play, 20060401	Watson, Nelson	[illegible]	1-sec	-0.07	0.5[illegible]
216. Earth Day 2006, 20060422	Vera, Nelson	[illegible]	1-sec	1.227	0.11
217. Indonesian Earthquake, 20060527	Nelson	[illegible]	1-sec	2.[illegible]	0.002
218. [illegible] News Release, 200607[illegible]	Bancel	[illegible]	1-sec	-0.[illegible]	0.[illegible]
219. Bombay Train Bombings, 20060711	Nelson	[illegible]	1-sec	0.7[illegible]	0.2[illegible]
220. Avebury Meditations, 20060722	Koch, Kyborg, Russell, Nelson	[illegible]	1-sec	1.0[illegible]	0.151
221. Qana, Lebanon, 20060730	Nelson	[illegible]	1-sec	1.7[illegible]	0.041
222. Terror Plot Foiled, 200608[illegible]	Roffey, Nelson	[illegible]	1-sec	1.44[illegible]	0.074
223. TM Resonance, 20060729-0806	Nelson, Orme-Johnson	[illegible]	1-sec	-2.41[illegible]	0.[illegible]
224. [illegible] Reconciliation, 2006[illegible]	Danilo, Nelson	[illegible]	1-sec	-0.184	0.577
225. Native American Ceremony, 2006[illegible]	Nelson	[illegible]	1-sec	0.[illegible]	0.1[illegible]
226. US Election Results 2006, 20061108	Roffey, Nelson	[illegible]	1-sec	1.42[illegible]	0.07[illegible]
227. Gemayel Assassination, 20061121	Nelson	[illegible]	1-sec	-0.[illegible]	0.74[illegible]
228. Global [illegible], 2006[illegible]	Ardagh, Nelson	[illegible]	1-sec	0.0[illegible]	0.4[illegible]
229. Global Orgasm for Peace, 2006[illegible]	Sheehan, Nelson	[illegible]	1-sec	0.604	0.273
230. Saddam's Execution, 2006[illegible]	Bancel	[illegible]	1-sec	-0.[illegible]	0.5[illegible]
231. New Year Mean, 2007, 20070101	Nelson	[illegible]	1-sec	0.61	0.271
232. New Year Var, 2007, 20070101	Nelson	[illegible]	1-sec	0.56	0.2[illegible]
233. European Storms, 20070118	Wendl, Nelson	[illegible]	1-sec	1.0[illegible]	0.137
234. Iraq Worst Bombing, 2007020[illegible]	Nelson	[illegible]	1-sec	1.0[illegible]	0.143
235. World Sound Healing, 20070214	Goldman, Nelson	[illegible]	1-sec	0.0[illegible]	0.4[illegible]
236. India Train Fire, 2007021[illegible]	Wendl, Nelson	[illegible]	1-sec	-1.073	0.[illegible]
237. Earth Hour Sydney, 20070331	Wendl, Nelson	[illegible]	1-sec	-0.72[illegible]	0.7[illegible]
238. Solomon Islands Quake, 20070401	Nelson	[illegible]	1-sec	0.445	0.3[illegible]
239. Virginia Tech Massacre, 200704[illegible]	Wendl, Nelson	[illegible]	1-sec	1.015	0.155
240. Tai Chi & Chigong Day, 20070428	Douglas, Nelson	[illegible]	1-sec	0.8[illegible]	0.1[illegible]
241. Global Peace Day, 20070520	[illegible], Laszlo, Ferenc, Nelson	[illegible]	1-sec	-2.113	0.[illegible]
242. [illegible], 2007[illegible]	Nelson	57	1-sec	0.[illegible]	0.277
243. Palestine Split, 20070614	Nelson	[illegible]	1-sec	0.761	0.22[illegible]
244. Live Earth, 20070707	Nelson	[illegible]	1-sec	-0.[illegible]	0.6[illegible]
245. Fire the Grid, 20070717	Many people	[illegible]	1-sec	1.5[illegible]	0.0[illegible]
246. Bridge Collapse, 20070801	Wendl, Nelson	[illegible]	1-sec	0.10[illegible]	0.4[illegible]
247. Peru Earthquake, 20070815	Nelson	[illegible]	1-sec	0.[illegible]	0.3[illegible]
248. Burning Man 2007, 2007[illegible]	Nelson	[illegible]	1-sec	1.5[illegible]	0.0[illegible]
249. Global OM, 20070915	Nelson	[illegible]	1-sec	0.2[illegible]	0.3[illegible]

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
250. International Day Peace, 20070921	Nelson	58	1-sec	-0.887	0.815
251. Gore Wins Nobel, 20071012	Nelson	62	1-sec	1.601	0.055
252. Bomb Blasts Bekun, 20071018	Nelson	61	1-sec	-1.885	0.952
253. Bangladesh Hurricane, 20071115-8	Wendl, Nelson	61	1-sec	1.401	0.081
254. Algerian Bombs, 20071211	Nelson	58	1-sec	-0.15	0.58
255. Studio Assassination, 20071227	Ridley, Petrul, Nelson	58	1-sec	0.035	0.486
256. New Year Mean, 2008, 20080101	Nelson	60	1-sec	1.427	0.077
257. New Year Var. 2008, 20080101	Nelson	60	1-sec	1.086	0.138
258. Kandahar Bombs, 20080217	Nelson	58	1-sec	-0.602	0.862
259. Attacks in Gaza, 20080301	Nelson	57	1-sec	1.209	0.113
260. Tibet Demonstrations, 20080314	Nelson	58	1-sec	1.458	0.072
261. Earthday 2008, 20080422	Glove, Eskins, Nelson	58	1-sec	0.595	0.276
262. Cyclone in Myanmar, 20080503	Wendl, Hades, Nelson	62	1-sec	-0.126	0.55
263. World Laughter Day, 20080504	Schreiber, Nelson	62	1-sec	-0.242	0.596
264. Earthquake, China, 20080512	Nelson	60	1-sec	-1.587	0.945
265. 3 Min Silence, China, 20080618	Schreiber, Nelson	61	1-sec	0.725	0.235
266. Obama Wins Nomination, 20080603	Weiss, Nelson	58	1-sec	2.153	0.016
267. Kabul Car Bombing, 20080707	Nelson	62	1-sec	0.214	0.415
268. Olympic Opening Beijing, 20080808	Nelson	64	1-sec	1.281	0.1
269. Georgia War Ends, 20080812	Nelson	59	1-sec	-0.218	0.586
270. World Meditation, 20080815	Dongen, Nelson	59	1-sec	1.184	0.118
271. Obama Acceptance Denver, 20080828	Zelgneva, Nelson	59	1-sec	2.039	0.021
272. Palin Acceptance Speech, 20080903	Bancel, Nelson	62	1-sec	1.685	0.046
273. McCain Accepts, St Paul, 20080904	Nelson	57	1-sec	-0.273	0.608
274. Peace Intention Expt, 20080904	McTaggart, Bancel, Nelson	56-58	1-sec	-1.845	0.96
275. Islamabad Hotel Bombing, 20080920	Wendl, Nelson	57	1-sec	-0.558	0.716
276. International Day of Peace, 20080921	Many people	56	1-sec	-0.943	0.827
277. Bellrut Vale Fails, 20080929	Nelson	57	1-sec	1.255	0.105
278. Temple Stampede Jodhpur, 20080930	Wendl, Nelson	57	1-sec	-0.782	0.783
279. Phillies Win World Series, 20081029	Dames, Nelson	57	1-sec	1.025	0.153
280. US Elections 2008, Obama, 20081028	Bancel, Nelson	60	1-sec	1.635	0.051
281. Mumbai Terror Attacks, 20081126	Bancel, Nelson	64	1-sec	-0.58	0.722
282. Sri Annual Global-O, 20081221	Sheridan, Nelson	57	1-sec	0.59	0.277
283. New Year 2009, Mean, 20090101	Nelson	56	1-sec	1.033	0.151
284. New year 2009, Variance, 20090101	Nelson	56	1-sec	0.505	0.307
285. Gaza Incursion, 20090103	Nelson	58	1-sec	0.282	0.416
286. Miracle on the Hudson, 20090115	Nelson	63	1-sec	-1.764	0.963
287. Obama Inauguration, 20090120	Nelson and others	62	1-sec	-1.358	0.912
288. Amtrallen Bombings, 20090207	Nelson	61	1-sec	-1.032	0.849
289. Valentines Meditations, 20090214	Goldman, McTarsek, Nelson	61	1-sec	2.878	0.002
290. Wisconsin School Shooting, 20090311	Wendl, Nelson	56	1-sec	-0.821	0.794
291. G20 News Conference, 20090402	Nelson	58	1-sec	0.984	0.168

Event Description	Hypothesis Source	RNGs	Resolution	Z	P
282. Binghamton Killing Spree, 20080403	Wendl, Nelson	88	1-sec	-0.938	0.826
283. Earthquake L'Aquila Italy, 20080406	Bancel, Nelson	86	1-sec	-0.281	0.603
284. Earthday 2008, 20080422	Nelson	84	1-sec	0.052	0.479
285. Swine Flu Panic, 20080424	Galileo	70	1-sec	-0.18	0.571
286. India Elections 2009, 20080516	Nelson	72	1-sec	0.763	0.223
287. Air France Disappearance, 20080601	Wendl, Nelson	87	1-sec	0.788	0.213
288. Iran Election Protests, 20080619	Nelson	88	1-sec	1.662	0.04
289. Live H2O Celebration, 20080621	Pratschke, Nelson	88	1-sec	0.145	0.442
290. Michael Jackson Dies, 20080625	Wendl, Braidwood, Schneider, Nelson	88	1-sec	0.914	0.18
291. Michael Jackson Memorial, 20080707	Radin, Nelson	87	1-sec	-0.183	0.585
292. Wenatchee Path 2008, 20080726	Otte, Koch, Nelson	87	1-sec	1.537	0.062
293. Fire the Odd 8, 20080728	Fucciolo, Nelson	86	1-sec	0.619	0.268
294. Tesoro Hits Tahoe, 20080807	Nelson	87	1-sec	-0.53	0.702
295. Ted Kennedy Dies, 20080826	Nelson	88	1-sec	-0.03	0.512
296. Earthquake in Java, 20080902	Nelson	87	1-sec	1.191	0.117
297. Internet Day of Peace, 20080921	Nelson and many others	86	1-sec	1.637	0.051
298. Tsunami in Samoa, 20080928	John Todd, Nelson	82	1-sec	0.185	0.423
299. Earthquake in Padang, 20080930	Nelson	82	1-sec	1.449	0.074
-810. Obama Peace Nobel, 20081009	Weiss, Bancel, Nelson	84	1-sec	0.788	0.222
-811. Action 350 for Climate, 20081024	Nelson	84	1-sec	-0.22	0.587
-812. Baghdad Bombings, 20081026	Wendl, Nelson	84	1-sec	1.531	0.063
-813. Fort Hood Massacre, 20081025	Nelson	84	1-sec	0.491	0.316

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REPLY

Reply to May and Spottiswoode on Experimenter Effect as the Explanation for GCP Results

Roger Nelson

Global Consciousness Project

This paper is dedicated to the memory of Helmut H. W. Schmidt

I appreciate the opportunity to respond to the article by May and Spottiswoode (hereafter M&S) in which they attempt to identify the source of the anomalous correlations reported by the Global Consciousness Project (GCP). Their aim is to show that the GCP data, like laboratory micro-PK data, can be explained in terms of Decision Augmentation Theory (DAT), and in particular as an experimenter effect. The experimenter they have in mind is Roger Nelson, and while I suppose it is some sort of honor to be perceived as a powerful psi source, I consider it unlikely that the highly significant composite findings in the GCP experiment are attributable to me. In this paper, I will discuss why, and in the process show logical and factual errors that undermine and largely if not completely destroy the case for DAT and the experimenter effect. Before proceeding, however, I want to say that I appreciate the civility of expression and argument M&S bring to bear. I hope the discussion in their paper and mine will be helpful to readers who are interested in the GCP experiment, in the important questions of interpretation it raises, and in the substantial implications it may have for psi research.

Let's begin with some simple mistakes. In their Abstract the authors imply that we propose an asymmetric force or force per bit to explain the deviant GCP statistic. We do not posit or speak of forces at all, so in terms of what the GCP does, this can be seen as a straw man. Of course this is the language M&S are accustomed to using, so we will accept that and deal with the actual issues ab initio. To begin, our primary measure as well as the independent measures we have developed all are correlations or correlation-based. We simply do not make claims about forces. That said, we have found better fits to the empirical data with field-like models than the classic selection models (DAT) that M&S believe should apply (Nelson & Bancel, 2009).

M&S say the "basic idea" of the GCP sprang from Helmut Schmidt's research with RNGs whose behavior was the target of participant guesses or

influence, though they don't explain how this leads to the GCP. Schmidt's test trials were typically decisions based on 1 bit (one binary decision) and typically the trials took a few seconds. Crucially, there was a participant with an intention to influence that bit. There is no such participant in the GCP, and to construe the experimenter in that role demands a convoluted argument. Nevertheless, we encourage data-based modeling to test such notions empirically.

We also do not claim, as they assert, that global consciousness is the source of the anomalous effects, rather, we use an operational definition of the object of study. The hypothesis we test simply says that we expect deviations in the data from our global network of RNGs during major world events. Formally:

> **Periods of collective attention or emotion in widely distributed populations will correlate with data deviations in a global network of physical random number generators.**

This general hypothesis is tested via a replication series of completely specified simple hypotheses of the form: *The GCP network variance statistic (or other specified measure) will be greater than expectation from time 1 to time 2 on a given date.* That is, we conduct a series of replications in which the exact data segment is identified along with the statistical test that will be applied. The composite across these replications constitutes a formal test of the general hypothesis (Bancel & Nelson, 2008, Nelson & Bancel, 2011). Over time, we expect to be able to discriminate between models, including one that is physical and might be given a name like "global consciousness" because of the link to collective human attention postulated in the general hypothesis. In the first instance, however, we seek evidence for a correlation, not for a theoretical entity.

It is a surprise to see M&S describe the inception of the GCP as "launched in 1998 in part in anticipation of the then up-coming Y2K." That, I am afraid, is made up. The project as conceived in 1997 was an evolution of FieldREG studies (Nelson, Bradish, Dobyns, Dunne, & Jahn, 1996, Nelson, Jahn, Dunne, Dobyns, & Bradish, 1998), and was concretely modeled on two prototype efforts to expand that concept to a larger scale by combining data from a dozen or more RNGs in Europe and the U.S. (Nelson, 1997, Nelson, Boesch, Boller, Dobyns, Houtkooper, et al., 1998). New Year's was an obvious candidate for a "global event" from the beginning, and the Y2K moment was of course included, but it was not a motivator for the project.

M&S say they have trouble understanding the GCP hypothesis, and knowing what the process is for selecting the events. The latter is a reasonable question, which we have addressed in some detail (Nelson & Bancel, 2011), but valid statistics are not dependent on these issues. It is sufficient that each event is selected and the hypothesis registered prior to examining the data, and that

the results are all reported. Whether we are looking at the time of the event or the time when people become aware of the event isn't relevant to the validity of the simple hypothesis tests in the replication series. Fortunately, that question can be empirically explored in the large GCP database, because the event definitions typically cover a time span that includes both aspects. Similarly, many other questions can in principle be asked of the data because we employ a two-stage hypothesis, with a general statement that is flexible to allow explorations, and formal testing via a series of rigorously defined simple hypotheses. In practice, the event definitions are standardized based on experience, with events in each of several categories specified using the same relative starting point and duration.

M&S make a point of disputing an "Orwellian rewrite of history" with regard to the use of PK or a force per bit model, but they are arguing with someone else. The GCP does not use this language; instead, we speak in terms of correlations. Indeed, the primary measure (the only one M&S address) is equivalent to an average pair-wise correlation in the RNG data. As the general hypothesis states, we are asking whether there is a deviation of this inter-node correlation that occurs during (is correlated with) the formally selected events. M&S wonder if the prediction is "constrained to be in real time with the events" and of course it is. To imagine otherwise is to confuse the defined event with the putative effect—an important distinction which, again, can be assessed in the GCP database. For more detail on the point, see Bancel and Nelson (2008) and Nelson and Bancel (2009).

An issue that is never discussed by M&S is the fact that all their calculations and theorizing about the sources of effects derive from a model that was designed to address intention experiments, that is experiments where someone is attempting to change the behavior of an RNG. However, while the GCP experiment uses RNG technology, it is not about intentions to affect the behavior of the devices. Its design is better regarded as an environmental monitor, where the environment of interest is variations in the coherence of consciousness and emotion across large populations. M&S might argue that the experimenter has an intention, but it would be of a categorically different sort. As the experimenter of interest, I would characterize my intention in the GCP as a desire to learn something—I'm not much interested in getting more ones or zeros. (Good thing, too. As a participant in the PEAR REG experiments, I produced a very small, non-significant positive effect over several years and thousands of trials.)

Moreover, the primary metric for evaluating the GCP hypothesis is not an increase or decrease of ones and zeros, but excess correlation among the RNGs. We predict and test for an increase in the composite spatially distributed network variance, which is equivalent to predicting an increase (from zero) in the average correlation between hundreds or thousands of RNG pairs. The

M&S model requires that the experimenter intuit or precognize the outcome of these tests. Of course the experimenter might guess well when looking for events that will be correlated with changes in the GCP network, but that seems an obvious, mundane talent, assuming there are correlated changes. It can most likely be learned, as well, with no experimenter psi required. This is a testable proposition, and is the subject of a program we are developing to define and teach consistent criteria for event selection which can be applied by independent observer/analysts, including skeptics.

In the "Formal DAT Analysis" section, the authors define their procedure: "To determine whether there is a force/bit effect in these data, we created a scatter plot of the stated *Z*-score squared against the number of RNGs that were used to compute the *Z*-score." This states that the N of RNGs is used, not the number of bits, as is usual in the DAT literature. It would have been useful for M&S to explain the switch and show its equivalence. The quoted statement also says they use GCP's stated *Z*-scores for individual events. Thus, their proposition appears to be that the *Z*-score, which represents a spatially distributed variance measure (or increased pairwise correlation), is dependent on the number of RNGs in the force per bit model, but independent of N for the DAT model. They do not further discuss the models or their assumptions, but let's accept that for the moment. Taking the alternative formulation for the GCP effect since it is easier to visualize, we can ask whether the significance of the correlation should depend on the number of RNGs. Since increasing the number of pairs should grow the number of correlations, leading to smaller error bars on the average correlation, the significance represented by the calculated *Z*-scores would be expected to increase. Thus, the discovery of a null relationship of *Z*-scores (representing the correlations) to the N of RNGs would be surprising, and inconsistent with a physical model. We believe the data do not support the M&S claim that the regression has zero slope.

Putting it explicitly, the bottom line drawn by M&S is premature at best: "We are left then to conclude that Dr. Nelson's DAT-like decision capacity drives the GCP result, and it is unlikely that their statistically robust result is due to a variation of their primary hypothesis of some global consciousness connections to the RNG devices." In a personal communication responding to an earlier version of this paper, Peter Bancel stated that "simulations show that it is not possible to distinguish between the models—there's not enough statistical power in the data." He goes on to say that while the data may be consistent with DAT, they are also consistent with a reasonable "force" model. York Dobyns has tested the DAT model against data from RNG experiments and finds it inadequate. He too points out the problem of small effects: "The selection model assumes that the operator somehow becomes aware of the actual run outcomes and assigns intentions to suit, but I also present an argument

showing that given the small overall effect size, a standard DAT model would produce the same statistics in the output data as the intention-selecting model . . . " (Dobyns, 1993, 1996, Dobyns & Nelson, 1997).

A little later in their paper, M&S recognize that their analysis doesn't really discriminate alternative models for the GCP data very well, but then say that after all since the GCP does use RNGs the analysis of laboratory intention experiments should still apply: "Why would the GCP data be any different? Thus we call into question the GCP's underlying assumption of variance interaction." This is a very weak argument. Perhaps M&S are confused by differing uses of the term "variance" and perhaps their comments are directed, inappropriately, to the variance of the individual RNGs. In any case they miss the point that, far from being an assumption, we *define* the network variance as our primary measure.

Moving to a different perspective, M&S attempt to compare the success of Nelson vs. other predictors. They state that Nelson "brought 234 events to the attention of the GCP," but their count is based on the assumption that whenever Nelson is included in the "source" column of the formal results table, he is the source. In fact, whenever names other than Nelson are included, they can legitimately be considered the source(s). When others suggest an event, there is frequently a need for collaboration to establish the analysis parameters. For example, an event will be suggested, but not the start and end times required for a formal event specification. Because of their faulty assumption, the counts made by M&S are wrong. In a recent categorization, May 28, 2011, I found that a little more than half of the events had been suggested by one or more others, sometimes including me (N = 188) and that Nelson alone had been the source for the rest of the predictions (N = 177). Looking at the two subsets separately, we see that Nelson's composite Z is 5.188, agreeing pretty well with that calculated by M&S on the smaller database they used, but the composite Z for the other predictors is 3.706, not even close to the M&S calculation. The difference in composite effect size attributable to Nelson vs. others is substantial, but not significant; the difference Z-score is 1.143. What is more important is an obvious logical flaw in the reasoning behind this comparison of outcomes for Nelson vs. others. Since Nelson is involved in registering, analysis, and writeup for every event, and would presumably always have a similar interest, it is clear ex hypothesi that this attempted comparison is artificial and invalid. It cannot tell us whether the GCP effect is due to experimenter psi.

Beyond that, the question whether Nelson is the primary source of the effects is far more complex than M&S apparently recognize. Even if the Nelson vs. other comparison were legitimate and the difference in composite Z were significant, such a comparison selectively ignores other factors. In particular, because the predictions for the GCP formal series are made a priori, they are

guesses. They are explicit attempts to specify, without any prior knowledge, a period of time when the data will be found to deviate from expectation in a certain statistic. There is a history of predictions and outcomes, that is a feedback loop that can be expected to educate the predictor as to what factors or features of events are associated with confirmations of the predictions. Does it not seem reasonable that Nelson, who pays more attention than anyone else to the sequence of successful and failed predictions, might learn something along the way about which are the "good bets" to make? That's pretty mundane compared with DAT or the psychic experimenter effect postulated by M&S, but it seems very likely to account for some considerable part of the (non-significant) advantage Nelson has over other predictors. When the details are considered, there are still other reasons why non-Nelson predictions may fail. They are often about local and relatively small items, and many of the ones accepted for registration and analysis are about meditations, peace prayers, earth days, and the like. I'm attuned to the ideas and ideals, and in order to learn about these events I accept many such suggestions, but our categorization studies have shown that they tend to have small effects.

In their discussion, M&S argue that the difference in success rate for Nelson compared with other predictors cannot be attributed to practice and experience. "To realize that, say earthquakes would be an effective event while sporting events would not, would require an independently supported model which predicted, and hopefully explained, why these classes of event would show differing GCP effects. No such model has been offered." In fact, though it doesn't have the status of a formal model, categorical analysis reveals characteristics which *do* help identify types of events that produce larger and smaller effects (Nelson, 2008). While these are general and descriptive findings, they are adequate to provide the sort of advantage Nelson's predictions show, simply as a matter of experiential learning.

I am pleased that May and Spottiswoode took the time to attempt an explanation of the GCP data deviations, though it seems to me they should have thought more deeply about various issues. They confuse or conflate various levels of description, and they make unexamined assumptions. I don't have a fundamental problem with an "experimenter effect" as a contributor to deviations from expectation in psi experiments, even in the GCP data. But there are no good reasons to think it is all or even most of the source. May and Spottiswoode make two separate attempts to persuade us otherwise. Their DAT explanation, even if applicable, fails because it is unable to discriminate between appropriate models. Their attribution to Nelson as experimenter fails because their assumptions about who is the source of predictions is faulty. It is, however, useful to think through these issues. They help us understand the experiment, and stimulate efforts to make it a better research vehicle.

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REPLY

Reply to May and Spottiswoode's "The Global Consciousness Project: Identifying the Source of Psi"

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I am pleased that May and Spottiswoode have initiated a discussion about the Global Consciousness Project (GCP), and I would like to thank the *JSE* editors for this opportunity to respond to their paper. May and Spottiswoode (M&S) suggest that the source of the statistical deviations reported by the GCP can be attributed to an experimenter effect and that Decision Augmentation Theory (DAT) can adequately model the GCP results. While I disagree with the analysis, their contribution is particularly welcome since they address an essential question that needs to be resolved by any model: Does the GCP measure a real, physical effect?

It is easy to see why the question is pertinent if we recall the experimental methodology. The GCP hypothesizes that data from a network of random number generators (RNGs) will deviate during events of global significance. Testing the hypothesis is a two-step procedure: 1) From time to time, data from the continuously accumulating RNG database are selected according to a blind procedure in which an event is identified from news or other sources and a data segment corresponding to the event is specified. A pre-designated test statistic is then calculated for the selected data. 2) The test statistic is converted to a standard normal *Z*-score and added to a table of *Z*-scores for all events. The formal experimental result is the mean of these *Z*-scores. As of late 2011, the GCP obtains a mean *Z*-score that exceeds zero by 6 standard deviations. This is the hugely significant result that M&S seek to explain.

The explanation proposed by the GCP is that, given the blind selection procedure, the change in the network statistics during events is due to a change in the physical behavior of the RNG devices themselves. This proposal can be tested by developing suitable models of the data. The GCP maintains that models which posit a physical mechanism that acts on the RNGs represent, at the least, a plausible avenue of investigation. What M&S correctly point out is that one cannot exclude, a priori, that a psi-mediated intuition, which informs the experimenter's designation of events, might compromise the blind

selection procedure. In such a case, one cannot rely on the formal result to make inferences about the RNG behavior. According to M&S, the freedom of choice in selecting the events and their start/end times, in conjunction with psi-mediated information about the resulting test statistics, sets up a satisfactory explanation of the experiment. For M&S, the experimental result is merely the consequence of fortuitous selection of naturally occurring data deviations.

M&S go a step further by using a DAT model to test their idea against the GCP data. DAT derives from the well-known principle whereby the ratio of signal-to-noise in a sample (i.e. the *Z*-score of a measurement) increases as $DF^{1/2}$ where DF ("degrees of freedom") is the sample size. M&S distinguish between degrees of freedom which are relevant for DAT—those which designate an elemental instance of decision concerning what data to include in a measurement—and irrelevant "internal" degrees of freedom which have no inherent relevance for the decision process. In the GCP event experiment, the elemental DAT degree of freedom is the selection of a data block representing an event. In the DAT picture, a constant effect size, Z_{DAT}, is attributed to each instance of event selection. Z_{DAT} is independent of all internal degrees of freedom, such as the number of seconds or RNGs in the data block. It is thus evident that any physical model which *does* depend on the internal degrees of freedom can be distinguished from DAT models by testing for an association between $DF_{Internal}$ and *Z*. If the DAT model holds, no association will be found, whereas a physical model will yield a positive association between $DF_{Internal}$ and measured values of *Z*. A standard way to test for association is by ordinary least squares regression (OLS). M&S chose to do an OLS for Z^2 versus N, the number of RNGs in the network during the event. Their OLS yields a regression slope within a standard error of zero, and they conclude that this supports a DAT interpretation of the GCP.

This conclusion might have some weight if their regression analysis were done correctly. Unfortunately, M&S make several errors which are fatal to their argument. Here, I briefly sketch their errors and show that a proper test leads to the opposite conclusion from M&S: There is a clear association between Z^2 and $DF_{Internal}$ and reasonably strong grounds for rejecting DAT in favor of a physical model.

I discuss four separate errors, in order of increasing consequence. The first two have negligible impact, but the others invalidate M&S's calculations and reverse the conclusions one must draw from the DAT analysis.

1. Incorrect values of explanatory and response variables

M&S use values for the explanatory variables, N, that are listed on the GCP website. The website values are only approximate and should be replaced

with exact values which account for null data trials. In addition, it would be preferable to calculate exact *Z*-scores for events 310–313, rather than use the estimates M&S list in their table.

2. Incorrect determination of fit parameter standard errors

M&S take the Z^2 as response variables. This choice yields non-normal fit residuals which impact the reliability of the usual OLS estimators for the fit parameter standard errors. Reliable errors for the regression slope and intercept parameters need to be determined by simulation. A 20,000-iteration Monte Carlo calculation (in which I use the correct values of N and *Z*) yields standard errors of the slope and intercept of 0.0061 and 0.303, respectively. These are substantially larger than the M&S values of 0.0034 and 0.058.

3. Failure to control for influence points

M&S neglect to perform regression diagnostics. It is well-known that OLS regression is sensitive to outlier and leverage points which may unduly influence the estimation of fit parameters. A common diagnostic is the Cook distance, *d*, which measures a point's relative influence on parameter estimation. Typically, a cutoff value sets an acceptable level of influence. In the representation I use here, *d* has a cutoff of 1 and points with $d > \approx 3$ may be considered substantially influential. Data points exceeding the cutoff need to be assessed carefully for experimental errors or other irregularities which might invalidate their inclusion in the regression dataset. For the GCP regression data, five data points have *d*-values greater than 3 and Event 1 has an exceedingly high value of $d = 42.3$.

A recent paper published in *JSE* (Bancel & Nelson, 2008) assessed the GCP Event Experiment in detail (the paper is cited by M&S in their article). The paper clearly states that, due to network instabilities during the first months of operation, *Z*-scores for the first 10 events are not reliable and should be excluded from analyses (footnote 21 in the paper). With these *Z*-scores excluded, a re-calculation of the OLS regression yields a positive slope parameter, increasing from −0.00064 to 0.0063. Correspondingly, the one-tailed Monte Carlo *P*-value for a test of the DAT hypothesis decreases from 0.51 to 0.13, indicating a much weaker agreement with the DAT model than M&S claim. More importantly, the re-calculation shows that OLS is an ill-suited choice for testing association between Z^2 and N. M&S would do better to use a modern technique of robust regression estimation. Robust methods are far less sensitive to outliers, influential data points, non-normality, and heteroskedasticity, and they frequently provide a power advantage over OLS. A slope estimate using one such robust technique, the Theil-Sen estimator (TSe), is discussed below.

4. Incorrect assignment of the regressor variable

The most serious error M&S make is in their choice of regressor. In Bancel and Nelson (2008), we show in considerable detail that the measured effect can be traced to correlations between pairs of RNGs. If the RNG output is written as *z(i,t)* where *i* labels the RNGs and *t* is the time in seconds, then the average correlation, ξ, is simply

$$\xi = \mathrm{DF}^{-1} \sum z(i,t)\, z(j,t) \ .$$

The sum is over all unique RNG pairs for each second so that $\mathrm{DF}_{\mathrm{GCP}} = \mathrm{T}(\mathrm{N}^2 - \mathrm{N})/2$, where T is the number of seconds during the event. The correlations ξ distribute normally (to high approximation, under the central limit theorem), and the event *Z*-scores are given as $Z = \xi \sqrt{\mathrm{DF}_{\mathrm{GCP}}}$. The appropriate regressor is thus $\mathrm{DF}_{\mathrm{GCP}}$ and not N as M&S propose. Inappropriately selecting a DF of N introduces a large dispersion in the regressor variable, $\mathrm{DF}_{\mathrm{GCP}}$. This leads to a partial randomization of the regressor (see Figure 1) and all but guarantees that the regression test will accept the DAT hypothesis.

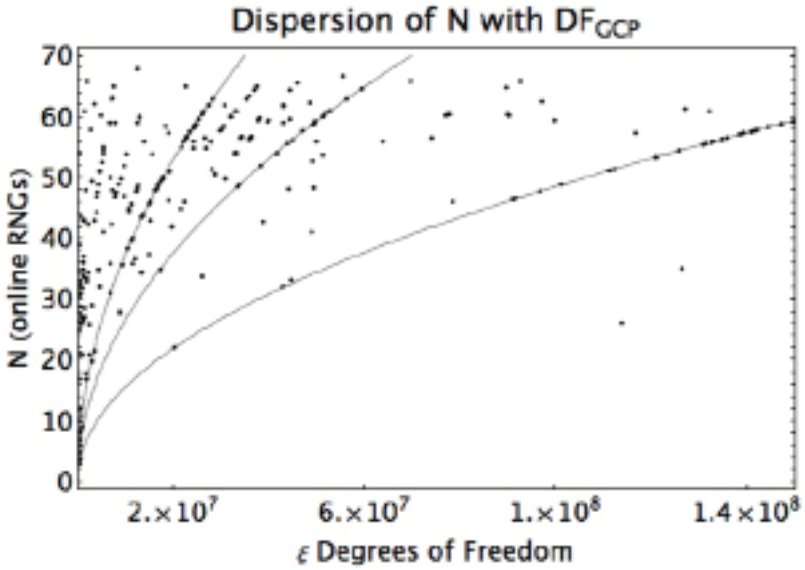

Figure 1. Dispersion of N with $\mathrm{DF}_{\mathrm{GCP}}$.
The plot shows the correspondence between DF = N and the correct $\mathrm{DF}_{\mathrm{GCP}}$ as identified by Bancel & Nelson for the 299 events cited by M&S. At fixed N, there is a broad dispersion in the values of $\mathrm{DF}_{\mathrm{GCP}}$. The dispersion greatly reduces the power of Z vs. N regressions. The gray curves are lines of constant event duration (4, 8, and 24 hours, left to right).

To conclude, I show that the DAT model is rejected when a robust estimator and the appropriate regressor are used. I employ the Theil-Sen estimator which has a considerably higher power than OLS for the Z^2 vs. DF regression. The TSe slope estimate is taken as the median slope of all pairs of data points. Confidence intervals can be determined by bootstrap analysis, but a hypothesis test of the DAT model requires empirical determination of the TSe distribution by Monte Carlo simulation. A one-tailed Monte Carlo test of DAT yields a P-value of 0.024. Using the recommended dataset which excludes early events, the *P*-value falls to 0.0053. These correspond to *Z*-scores of 1.98 and 2.56, and indicate that the GCP data reject the DAT model with moderately high confidence. Although it is beyond the scope of this Reply, one can show that a similar procedure which tests the alternate hypothesis of a physical effect accepts that hypothesis as being consistent with the data (in preparation by Bancel).

In summary, M&S highlight a fundamental interpretational issue of the GCP: whether the measured effect has its source in a physical perturbation of the network RNGs. The issue can be addressed by testing for such structure in the event data as would be predicted by a physical effect. The association of Z and DF_{GCP} is one example of this approach, and the analysis presented here supports the GCP proposal. However, the issue is sufficiently important that further, independent tests are needed before a convincing conclusion can be drawn (Nelson & Bancel, 2011). A number of independent tests have been identified, and a report is currently in preparation by Bancel.

Acknowledgements

My thanks to Roger Nelson for many useful discussions. This work is supported in part by a grant from the Parapsychological Association. I would like to dedicate this Reply to the memory of Helmut Schmidt and his pioneering contributions to psi research.

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RESPONSE

The Global Consciousness Project, Identifying the Source of the Psi: A Response to Nelson and Bancel

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First of all we want to express our thanks to the Journal for allowing us to have the "last" word. We put last in quotes because the very good news about this exchange is that the discussion will undoubtedly continue. While it might be possible to respond point by point to Nelson and Bancel's remarks, we prefer to focus on serious points of disagreement. We will comment upon their responses separately. Before we begin, it is important to recognize that while our interpretations differ, there are large areas of overlap in our thinking.

Response to Roger Nelson

Nelson has documented that the GCP arose from PEAR's field-RNG studies; yet the GCP represents a major conceptual change from the field-RNG studies. One of the differences is that the dependent variable in field-REG studies is a mean shift in the sampling distributions and the effect sizes are consistent with much of the laboratory-based RNG database of that time (Nelson, Bradish, Dobyns, Dunne, & Jahn, 1996). While it is true that field-RNG studies differ from laboratory studies because of the lack of volitional involvement, to generalize that argument to somehow isolate the GCP results as something fundamentally different from laboratory studies is logically incorrect in that it makes psychological assumptions of the process which, at best, are premature. There are examples of studies with "hidden" RNGs (i.e. the participant is unaware of a second RNG in the equipment) that produced significant changes in the mean of the hidden RNG (Varvoglis & McCarthy, 1986). Thus, Nelson's point that the lack of volition means removing forces from the discussion does not follow.

One lesson we have learned from our exchange is that our early thinking on Decision Augmentation Theory (DAT) was very limited mainly because of the RNG zeitgeist of the day of a force per bit model, as we document in our article in this issue of this *Journal*. The DAT analysis shown is completely insensitive to any forces that may be symmetrical about zero or in cases where there is no force at all. However, the main idea behind DAT is equivalent to experimenter psi; that is, experimenters (or agents acting like experimenters) make psi-mediated choices to affect the outcome. Of course, this is not a new idea (Stanford, Zenhausern, Taylor, & Dwyer, 1975).

We believe Nelson is splitting hairs with regard to their published hypothesis with regard to the GCP when he claims "We also do not claim, as they [May & Spottiswoode] assert, that global consciousness is the source of the anomalous effects, rather, we use an operational definition of the object of study." In our rebuttal, we offer two points taken from Bancel and Nelson (2008) and the GCP website, respectively:

> Periods of collective emotional or attentional behavior in widely distributed populations will correlate with deviations from expectation in a global network of RNGs.

> When human consciousness becomes coherent and synchronized, the behavior of random systems may change. Quantum event based random number generators (RNGs) produce completely unpredictable sequences of zeroes and ones. But when a great event synchronizes the feelings of millions of people, our network of RNGs becomes subtly structured. The probability is less than one in a billion that the effect is due to chance. The evidence suggests an emerging noosphere, or the unifying field of consciousness described by sages in all cultures.

To us, the paragraph from the GCP website seems to be the quintessential definition of global consciousness.

Nelson implies that we fail to understand the GCP hypothesis (to which we plead guilty) because we are concerned that the "time" of the event is vague. From our perspective this project has been in a continuing state of exploration and we think this is the case, at least in part, because addressing whether the people who are actually involved in, say a natural disaster, or the people worldwide who later see the disaster on television are somehow related to the variance of the networks RNGs is the goal of the GCP project. Or even peaks in the variance that occur *before* the event which requires a precognitive description. It seems to us these considerations are not trivial.

This brings us to our major criticism of Nelson's defense. Claiming that the GCP is only about correlations does not absolve him of a responsibility to

untangle the pressing correlation issues we raised in our original paper. Without belaboring the point, we quote from our paper:

> As we all have learned in our statistics courses, correlation does not necessarily imply a causal relationship between the variables. Hypothesis two above [correlation hypothesis] also bifurcates. Either human/natural events magically happen on the average only during times of locally deviant, but expected, excursions of the RNGs or vice versa. Even though there does not have to be a causal relation for this correlation to arise, we are obligated to search for a third (or more) variable(s) that give rise to the correlation. In many cases, an external (to the primary correlative variables) variable is difficult or impossible to identify.

Correlations are not magical. Somewhere in the variable chain there is a causal relationship. We find both of the correlation cases we outlined above as substantially implausible especially given the often-observed psi-mediated experimenter effects, which might be sufficient to explain the observables.

Nelson appears to have changed the definition of the "Hypothesis Source"—the GCP's words, not ours. Part of his argument against Nelson being the source of the psi rests upon his self-proclaimed modest abilities in laboratory RNG studies. We find that argument to be not apropos. Our claim is that Nelson's psi is not involved in the RNGs at all; rather it arises in the selection of what events get counted in the GCP formal database and which do not.

Finally, if Nelson is correct in his redefinition of the column heading (i.e. Hypothesis Source) used to define the chooser of GCP events, then he opens the door for further theoretical musing and experimenter-effect analyses. We arrive at the notion that the GCP has nothing to do with the people on our planet in general not only because of our analysis of Nelson versus the rest of the contributors but also because of the questionable correlational arguments we illustrate above.

Response to Peter Bancel

The vast majority of Bancel's response is a mathematical and technical refutation of our DAT analysis. Both in our primary paper and in our comments above, we stipulate that DAT does not enter into the RNGs at all if the effects, as claimed by the GCP, are correlational or if the effects arise because of an exactly symmetric force. Rather, thinking of DAT as equivalent to a psi-mediated experimenter effect as does Robert Rosenthal[1], then as we illustrated extensively above the psi-mediated experimenter effect may enter into the system at the event-selection level, which renders arguments with regard to variables for any z^2 versus number of eggs moot. Bancel notes that the events

are selected by a blind procedure and thus, by implication, are not subjected to DAT. This is simply factually incorrect and flies in the face of most all of the psi literature in which psi happens under double blind and sometimes even more layers of blindness. The event selector could be a major psi contributor to the successful outcome of the GCP.

Conclusion

If Nelson is accurate in his description of the meaning of the "Hypothesis Source," then clearly there is more work to be done to definitively identify the source of the psi in the GCP. However, it is clear to us that global consciousness however defined is not a contributor to the observables. We look forward to working with Bancel with regard to extending our DAT analysis by incorporating his correlational variable insight.

There is, however, one simple thing that can be done from this point forward that would go a long way to answer the questions raised in our debate. Rather than posting the GCP data for every second, post only, say, the even-numbered seconds. Then data snoopers and data-mining programs can be unleashed with impunity to isolate events that show significant correlations. Among the things we have stipulated is the excellence of the RNG hardware, which means the autocorrelation of non-zero lags is statistically zero. In simple language this means that the data from second to second are independent of each other under the null hypothesis of no effects. When the data mining produces a significant effect on the even-numbered seconds, it must also be seen nearly exactly on the odd-numbered seconds which now act as a formal "within session" control.[2] Any causal or correlational effects should replicate on a second-by-second basis.

Notes

[1] Private communication.

[2] We thank Professor Richard Broughton for first suggesting this to us.

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RESEARCH

Alien Visitation, Extra-Terrestrial Life, and Paranormal Beliefs

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Abstract—The present paper investigated the nature and structure of extra-terrestrial beliefs. Respondents completed a booklet containing items measuring belief in extra-terrestrial life, alien visitation, and paranormal belief (Revised Paranormal Belief Scale, R-PBS; and the Australian Sheep Goat Scale, ASGS). Responses were analyzed using principal component analysis (PCA), and a three-factor structure emerged: alien visitation, belief in extra-terrestrial life, and the search for extra-terrestrials. Further analysis revealed that males scored higher than females on belief in extra-terrestrial life and the search for extra-terrestrial life. No difference was observed for alien visitation. Correlational analysis found significant positive associations between each of the extra-terrestrial life factors and alien visitation. In addition to this, the extra-terrestrial life factors and alien visitation were found to correlate with overall paranormal belief (ASGS), and the two factors of the R-PBS (Traditional Paranormal Belief and New Age Philosophy). Alien Visitation was more strongly correlated with the paranormal belief measures than the extra-terrestrial belief factors; these were found to be negatively correlated with the paranormal belief measures when Alien Visitation was controlled for. These findings indicate that only more extreme Alien Visitation beliefs were associated with belief in the paranormal.

Keywords: alien visitation—belief in extra-terrestrial life—search for extra-terrestrial life—paranormal belief

Introduction

The origin, nature, and prevalence of extra-terrestrial beliefs have been relatively under-researched within psychology (Swami, Furnham, Haubner, Stieger, & Voracek, 2009, Swami, Pietschnig, Stieger, & Voracek, 2010b). This is surprising because such beliefs have been recorded throughout human history (Crowe,

1986) and continue to prevail within modern society (Clarke, 1991, Zullino, Verdu, Khazaal, & Borgeat, 2006). Indeed, recent work indicates that a significant proportion of the population believe that extra-terrestrial life exists and that UFOs are evidence of alien life (Biasco & Nunn, 2000, Chequers, Joseph, & Diduca, 1997, Gallup, 1997, Patry & Pelletier, 2001, Swami et al., 2009).

Pertinently, since the 1960s, the development of contemporary alien research has coincided with an observed growth in extra-terrestrial beliefs (Gallup & Newport, 1991, Genta, 2007), which has been accompanied by an increase in reported accounts of UFO and alien-related experiences (cf., French, 2001, French, Santomauro, Hamilton, Fox, & Thalbourne, 2008, Swami et al., 2009). French et al. (2008) estimate that worldwide, the number of people claiming to have conscious memories of alien abduction is likely to run into at least several thousands. Further studies have suggested this figure may be higher, between 2% (Appelle, 1995/1996, Hopkins, Jacobs, & Westrum, 1992) and 5%–6% of the population (Jacobs, 1992). While these figures seem unusually high, even more conservative measures, such as consideration of reported cases, have identified significant numbers of incidents. Notably, Bullard's (1994) survey of 13 investigators yielded 1,700 reports (Appelle, 1995/1996). Collectively, these findings indicate that extra-terrestrial beliefs and experiences represent important phenomena, which merit further study and clarification.

Historically, the study of extra-terrestrial beliefs has been hindered by the fact that such beliefs have been frequently subsumed within measures of general paranormal belief (e.g., the Revised Paranormal Belief Scale [R-PBS], Tobacyk, 1988). Swami et al. (2009) contend that this has occurred because extra-terrestrial beliefs are premised upon notions and theories, which transcend the explanatory power of mainstream science (Gray, 1991). Certainly, the scientific community has yet to accept evidence of extra-terrestrial life as authentic and has failed to agree on a definitive position on the existence of other life forms. It is worth noting that the merits of including extra-terrestrial beliefs within paranormal measures is much debated, and hence not all paranormal scales make reference to extra-terrestrial beliefs (e.g., Australian Sheep Goat Scale, Thalbourne, 1995a, Thalbourne & Delin, 1993).

Where extra-terrestrial beliefs have been incorporated into paranormal belief measures, they have received a partial, limited treatment. For instance, the R-PBS (Tobacyk, 1988) contains a three-item Extraordinary Life Form subscale, which assesses the existence of extra-terrestrial life alongside other extraordinary life forms (i.e. the abominable snowman of Tibet and the Loch Ness Monster). The use of a single item to measure extra-terrestrial beliefs is problematic because there is evidence to suggest that extra-terrestrial beliefs are multidimensional (Dagnall, Munley, Parker, & Drinkwater, 2010a, Dagnall, Parker, Munley, & Drinkwater, 2010, Swami et al., 2009).

This issue is exemplified by the R-PBS item, "There is life on other planets." The item is a general statement, which is difficult to refute because life could take many shapes and forms (e.g., bacteria) (Lawrence, 1995a). Hence, the question of interest should not be whether life exists on other planets but whether life from other planets is visiting the Earth. The latter UFO-related belief(s) are contentious, and there is evidence to suggest that only these radical notions (visitation, abduction, medical examination, etc.) are related to paranormal belief (Dagnall, Parker, Munley, & Drinkwater, 2010, Dagnall, Munley, Parker, & Drinkwater, 2010a).

Limited consideration of extra-terrestrial and alien-related beliefs is not peculiar to the R-PBS, but is typical of several paranormal belief measures: The Belief in the Paranormal Scale (Jones, Russell, & Nickel, 1977) assesses UFO sightings via a single item; the Supernaturalism Scale (Randall & Desrosiers, 1980) assesses belief in extra-terrestrial life and UFOs via three items; the Anomalous/Paranormal Belief Subscale (Kumar, Pekala, & Gallagher, 1994, Kumar & Pekala, 2001) assesses belief in intelligent life on other planets; the Paranormal Short Inventory (Randall, 1997) assesses belief in UFOs via one item; and the Exeter Superstitions Questionnaire (Preece & Baxter, 2000) assesses belief in alien visitation via one item. Examining these measures, it is clear that they fail to provide an adequate measure of extra-terrestrial–related beliefs because they are unable to discriminate between extra-terrestrial life and UFO-related beliefs and contain insufficient items to measure the alien constructs (Dagnall, Munley, Parker, & Drinkwater, 2010a).

The distinction between belief in extra-terrestrial life and UFO-related beliefs has previously been found to be important (Chequers at al., 1997). Notably, Chequers at al. (1997) designed eight items to measure extra-terrestrial life/alien beliefs alongside schizotypy. Following a review of their paper, they subdivided their items into two measures dealing with extra-terrestrial life and UFO related-beliefs. Endorsement rates were lower for items related to UFO-related beliefs (e.g., 1% agreed that they had been taken on board a spaceship and 32% agreed that the government refuses to tell the truth about flying saucers) than items related to belief in extra-terrestrial life (e.g., 85% agreed that there is good evidence that life exists on other planets). These findings suggest that the subscales measure different constructs.

Despite these differences, Chequers et al. (1997) observed some overlap between UFO-related beliefs and extra-terrestrial life; 96% of respondents disagreed that people who say they have been abducted by aliens are mentally ill. Interestingly, Chequers et al. (1997) found that the two subscale measures were differently related to scores on the schizotypal traits questionnaire (Rawlings & MacFarlane, 1994); only UFO-related beliefs were found to positively correlate with level of schizotypy. Chequers et al. (1997) hypothesize that this is because

extra-terrestrial life beliefs are more plausible than UFO-related beliefs. Clearly, their distinction between belief in extra-terrestrial life and UFO-related beliefs is a useful theoretical dichotomy that requires further explanation.

Noting this, Dagnall, Munley, Parker, and Drinkwater (2010a) explored whether alien-related beliefs (life on other planets and alien visitations) were related to extraordinary life forms, as suggested by the R-PBS, and if such beliefs could be considered to represent facets of paranormal belief per se (Diaz-Vilela & Alvarez-Gonzalez, 2004). This was achieved by identifying commonly used measures of paranormal belief (R-PBS, Paranormal Short Inventory, etc.) and related phenomena (e.g., scales assessing belief in extra-terrestrial life and UFO-related beliefs, Chequers et al., 1997; and Poltergeists and Hauntings, Kumar & Pekala, 2001). The latter scales provided only a limited range of related beliefs and consequently the authors generated additional items by exploring reports of alien-related and haunting experiences.

The new and original scale items were combined to produce a 124-item composite measure. This was completed by 1,481 respondents and the data were then analyzed using exploratory factor analysis (principal components analysis), and a nine-factor structure emerged: Hauntings, Other Life, Superstition, Religious Belief, Alien Visitation, Extrasensory Perception (ESP), Psychokinesis (PK), Astrology, and Witchcraft. Consistent with Chequers et al. (1997), other life (extra-terrestrial life) and Alien Visitation (UFO-related beliefs) were identified as separate factors; both were coherent, possessed face validity, and demonstrated excellent internal reliability.

Dagnall, Munley, Parker, and Drinkwater (2010a) in a followup study further explored the relationships among extra-terrestrial life, UFO-related beliefs, and paranormal belief. They found that despite being positively correlated with each other UFO-related beliefs were more highly correlated with paranormal belief, as measured by the R-PBS and ASGS, than belief in extra-terrestrial life; partial correlation, controlling for the overlap between belief in extra-terrestrial life and UFO-related beliefs, found only the more extreme UFO-related beliefs to be associated with paranormal belief. These findings suggest that belief in extra-terrestrial life is multifactorial and that further research is required to identify the structure of such beliefs.

Swami et al. (2009) conducted a study examining the structure of beliefs about extra-terrestrial life. In order to do this they recruited 577 respondents (320 participants from Austria and 257 participants from Britain) and asked them to complete their Extra-terrestrial Beliefs Scale (EBS). The EBS is a 37-item scale measuring belief in evidence of extra-terrestrial life, governmental knowledge of the existence of extra-terrestrial life, scientific search for extra-terrestrial life, and the existence of UFOs. In addition to this, participants provided information on sex, age, ethnicity, religion, marital status, highest educational qualification,

religious belief, and political orientation. Exploratory factor analysis suggested three primary factors: belief that extra-terrestrial life has visited Earth and that governmental agencies have knowledge of this fact (Factor 1), scientific search for extra-terrestrial life (Factor 2), and general beliefs about the existence of extra-terrestrial life (Factor 3). Separate factor analysis for the Austrian and British participants revealed similar factor structures.

Examination of responses to the three factors indicated that respondents made a clear distinction between paranormal-related beliefs (Factor 1) and more science-based beliefs (Factor 3) (Swami et al., 2009). Typically, respondents endorsed the notion that other life exists elsewhere in the universe but were skeptical of the idea that extra-terrestrial life has visited Earth. Thus only Factor 1 was found to be meaningfully related to the categorization of extra-terrestrial beliefs as paranormal. On this basis, Swami et al. (2009) made a distinction between UFO-related beliefs (paranormal-related beliefs) and general belief in the possibility that extra-terrestrial life exists. Interestingly, although participants believed that extra-terrestrial life may exist, they expressed only moderate support for exploration of such life. With regard to demographic variables, Swami et al. (2009) found that: stronger general extra-terrestrial beliefs were associated with higher levels of education; higher religiosity and more right-wing political orientation were associated with decreased belief in extra-terrestrial life; and there were no gender differences across factor scores.

Looking at the previous research makes it evident that further work in the area of extra-terrestrial–related beliefs is required for a number of reasons. First, while the dichotomy between extra-terrestrial life and UFO-related beliefs has been established, the factorial structure of extra-terrestrial beliefs has yet to be fully assessed; Chequers et al. (1997) simply subdivided their items on the basis of content, while Dagnall, Parker, Munley, and Drinkwater (2010), Dagnall, Munley, Parker, and Drinkwater (2010a), and Swami et al. (2009) employed exploratory factor analysis. Hence the present study was designed to extract common factors from the two existing measurement scales.

Dagnall, Parker, Munley, and Drinkwater (2010) and Dagnall, Munley, Parker, & Drinkwater (2010a) identified two factors (life on other planets, Factor 1; and alien visitations, Factor 2), while Swami et al. (2009) outline three factors: belief that extra-terrestrial life has visited Earth and that governmental agencies have knowledge of this fact (Factor 1); scientific search for extra-terrestrial life (Factor 2); and general beliefs about the existence of extra-terrestrial life (Factor 3). In order to do this, the two-item sets from the respective studies were combined to form a composite measure of extra-terrestrial–related beliefs. Second, it was hoped that this approach would lead to an enhanced, psychometrically validated measure of the facets of extra-terrestrial belief, which would be of use within the current area of research

as well as to related areas such as false memory (Clancy, McNally, Schacter, Lenzenweger, & Pitman, 2002, French et al., 2008).

Methods

Materials and Procedure

Participants were asked to complete: items assessing belief in extra-terrestrial life and alien visitation (Dagnall, Munley, Parker, & Drinkwater, 2010a, Swami et al., 2009), the Revised Paranormal Belief Scale (R-PBS, Tobacyk, 1988, Tobacyk, 2004, Tobacyk & Milford, 1983), and the Australian Sheep–Goat Scale (ASGS, Thalbourne, 1995a, Thalbourne & Delin, 1993). Presentation order across questionnaires was counterbalanced to prevent order effects. The current questionnaire measures have been previously psychometrically validated:

Belief in extra-terrestrial life and alien visitation (Dagnall, Parker, Munley, & Drinkwater, 2010, Dagnall, Munley, Parker, & Drinkwater, 2010a, Swami et al., 2009). Belief in extra-terrestrial life and alien visitation was assessed via a 37-composite-item measure; 23 items from Swami et al. (2009) and 14 items from Dagnall, Munley, Parker, & Drinkwater (2010a). The Swami et al. (2009) items represented three factors: alien visitation and coverup (e.g., "The government of this country is covering up the existence of extra-terrestrial life"), 11 questions; scientific search (e.g., "The search for extra-terrestrial life is a serious and important scientific endeavour"), 6 questions; and general beliefs (e.g., "Just because we have no evidence of extra-terrestrial life does not mean that such life does not exist"), 6 questions. The Dagnall, Munley, Parker, & Drinkwater (2010a) items represent two factors: extra-terrestrial life (e.g., "Somewhere in the universe there are other forms of life"), 6 questions; and UFO-related beliefs (e.g., "Aliens are abducting human beings)", 8 questions. In order to facilitate direct comparison with Swami et al. (2009), all questions employed a 7-point Likert-type scale (where 1 was disagree, 4 was neither agree nor disagree, and 7 was agree). Previously, both item sets have been found to be conceptually coherent and possess good to excellent internal reliability (Cronbach's alpha, α). Swami et al. (2009): alien visitation and coverup, $\alpha = .90$; scientific search, $\alpha = .82$; and general beliefs, $\alpha = .75$. Dagnall, Munley, Parker, & Drinkwater (2010a): belief in extra-terrestrial-life, $\alpha = .91$; and UFO-related beliefs, $\alpha = .95$.

Revised Paranormal Belief Scale (R-PBS) (Tobacyk, 1988, Tobacyk, 2004, Tobacyk & Milford, 1983). The R-PBS is an amended form of the Paranormal Belief Scale developed by Tobacyk and Milford (1983) and is the most frequently used self-report measure of paranormal belief (Irwin, 2004). It contains 26 items assessing seven facets of paranormal belief: traditional religious belief, psi, witchcraft, superstition, spiritualism, extraordinary life forms, and precognition. The R-PBS can be totalled to produce overall scores,

or subscores can be calculated for each of the facets. However, recent attempts to refine/purify the scale to eliminate differential item functioning (arising from age and gender bias) and subsequent factor analysis has identified an alternative two-factor solution (Lange, Irwin, & Houran, 2000). This is composed of factors assessing New Age Philosophy (NAP) and Traditional Paranormal Belief (TPB): NAP measures belief in psi, reincarnation, altered states, and astrology (11 items), while TPB assesses belief in concepts such as the devil, witchcraft, and heaven and hell (5 items) (Irwin, 2004). R-PBS items are presented as statements (e.g., "I believe in God"), and respondents record answers on a Likert scale ranging from 1 (strongly disagree) to 7 (strongly agree). Higher scores on the scale/sub-scale indicate higher paranormal belief.

The current paper, in line with Irwin (2004) and recent convention will employ the two-factor solution suggested by Lange, Irwin, & Houran (2000). This is achieved by recoding the scores; 1–7 is converted to 0–6, and the rasch scaling procedure is used (Andrich, 1988). Rasch scaling produces scores ranging from 6.85 to 47.72 on NAP and 11.16 to 43.24 on TPB. While the factorial structure of the R-PBS has frequently been debated (Lawrence, 1995a, 1995b, Lawrence, Roe, & Williams, 1997, Tobacyk, 1995a, 1995b, Tobacyk & Thomas, 1997), the scale overall has been found to be a conceptually and psychometrically satisfactory measure (Tobacyk, 2004). Particularly, the R-PBS has been found to possess adequate validity (Tobacyk, 1995a, 1995b, 2004) and good test–retest reliability (Tobayck, 2004).

Australian Sheep–Goat Scale (Thalbourne, 1995b, Thalbourne & Delin, 1993). The ASGS measures belief in, and alleged experience of, three core concepts of parapsychology (life after death, psychokinesis, and extrasensory perception). These concepts, while independent, have been found to be highly correlated. Hence, the ASGS is generally considered to measure belief in psychic ability (Thalbourne, 1995a, 1995b, Thalbourne & Delin, 1993, Thalbourne, Dunbar, & Delin, 1995, Wiseman & Watt, 2006). The ASGS is composed of 18 items, and the response options are False (scored as zero), "?" (Don't know: scored as 1 point), and True (scored as 2 points). The scale has a range from 0 to 36, higher scores indicating higher levels of belief and experience. Recent attempts to rasch scale the ASGS (Lange & Thalbourne, 2002) suggest that scoring should be limited to 16 rather than 18 items. Across a range of studies the ASGS has demonstrated good reliability and validity (Thalbourne, 1995a, Thalbourne & Delin, 1993).

Respondents

527 respondents completed the questionnaire. Ages ranged 16–75 years, with a mean of 25.27, a standard deviation of 10.96, and a median of 20 (lower quartile 19 and upper quartile 27); 73% female and 27% male. Respondents

were recruited through a range of sources: via undergraduate and postgraduate psychology and healthcare courses, through contacts at local colleges, via emails to staff and students across the University, and to external contacts. An opportunistic snowball-sampling technique was employed. Participation was voluntary and respondents could terminate participation at any point. All surveys were completed under anonymous and confidential conditions.

Procedure

All participants were informed that the questionnaire measure was concerned with the measurement of belief. They were told that their responses would be anonymous and that they should ensure that all items were completed. Participants were instructed that there was no time limit and that they should work through the questions at their own pace.

Results

Exploration of the Empirical Structure of the Questionnaire

In line with Swami et al. (2009) participants' questionnaire responses were subjected to principal components analysis (PCA) with orthogonal (varimax) rotation. Prior to conducting PCA, the correctness of the data for factor analysis was assessed. The data was found to be suitable for PCA: The Kaiser–Mayer–Oklin value (.956) exceeded the recommended value of .6 (Kaiser, 1970, 1974); Bartlett's Test of Sphericity (Bartlett, 1954) was significant (χ^2 = 12797.746, df = 561, $p < .001$), and the correlation matrix contained many coefficients of .3 or above. An item-loading cut-off value of .45 was selected; Comrey and Lee (1992) suggest that item loadings above this value provide a good measure of a factor.

The initial PCA resulted in a solution comprising six factors with eigenvalues of greater than 1, accounting for 64% of the total variance. Parallel analysis was conducted using the MonteCarlo PCA (Watkins, 2000). This indicated that Factors 4, 5, and 6 should not be retained in the final analysis. Reliability analysis, in the form of Cronbach's alpha (α), revealed that Factor 1 (α = .95) and Factor 2 (α = .92) possessed excellent internal reliability and Factor 3 (α = .80) good internal reliability.

Following the initial PCA and the subsequent reliability analysis, a second PCA was undertaken. This included the three factors which produced an acceptable internal reliability coefficient ($\alpha \geq .7$) and used a reduced set of 25 items (see Table 1).[1]

TABLE 1
Scale Items with Mean Scores and Standard Deviations (SD) for UK Participants

		GENDER				COMBINED TOTAL	
		WOMEN		MEN			
Item		Mean	SD	Mean	SD	Mean	SD
1	Given the size and age of the universe, it is very likely that extra-terrestrial life must exist.	4.80	1.64	5.50	1.60	4.99	1.66
6	If Earth-like planets exist in the universe, then it is likely that Earth-like organisms will have evolved on those planets.	4.82	1.41	4.94	1.71	4.85	1.50
8	Intelligent extra-terrestrial life has visited Earth.	3.16	1.58	3.26	1.73	3.19	1.62
11	The search for extra-terrestrial life is a serious and important endeavour.	4.41	1.55	4.81	1.75	4.52	1.61
15	Just because we have no evidence of extraterrestrial life does not mean that such life does not exist.	5.51	1.55	5.70	1.58	5.56	1.56
18	Governments should direct more funding to the scientific search for extra-terrestrial life.	2.98	1.66	3.36	1.69	3.08	1.67
21R	The search for extra-terrestrial life is a waste of time and money.	4.18	1.77	4.61	1.83	4.30	1.79
23R	Earth is the only planet in the universe that harbours life.	4.84	1.62	5.36	1.63	4.98	1.64
26R	The search for extra-terrestrial life is a pseudoscience (not proper science).	4.38	1.58	4.52	1.80	4.42	1.64
28	Extra-terrestrial creatures visited Earth in the distant past or at the dawn of civilization.	3.38	1.39	3.21	1.66	3.33	1.47
38	Somewhere in the universe there are other forms of life.	4.88	1.55	5.30	1.68	4.99	1.60
39	People have been taken on board alien spaceships.	2.65	1.50	2.72	1.64	2.67	1.54
40R	The Earth is the only planet in the universe that supports life.	4.78	1.67	5.21	1.67	4.90	1.68
41	Aliens are abducting human beings.	2.35	1.42	2.36	1.48	2.35	1.44
42R	The only intelligent life exists on earth.	4.46	1.67	4.80	1.73	4.55	1.69
43	Aliens have implanted objects into people.	2.28	1.38	2.31	1.41	2.29	1.39
44	There is life on other planets.	4.79	1.49	5.31	1.58	4.93	1.53
45	Alien spaceships regularly visit Earth.	2.57	1.52	2.57	1.56	2.57	1.53
46R	There is no such thing as extra-terrestrial life.	4.68	1.74	5.22	1.75	4.83	1.76
47	Alien spaceships have crash-landed on Earth.	2.64	1.53	2.86	1.71	2.70	1.58
48	Intelligent life exists beyond our universe.	4.49	1.57	4.87	1.66	4.59	1.60
49	Alien intelligence is responsible for some UFO sightings.	3.20	1.59	3.14	1.68	3.18	1.62
50	Extra-terrestrials have visited Earth throughout history.	3.25	1.69	3.22	1.75	3.24	1.71
51	Unidentified Flying Objects suggest that some kind of extra-terrestrial life form has approached the surface of the Earth.	3.28	1.63	3.18	1.67	3.25	1.64

TABLE 2
Principal Component Loadings for Scale Items

Factor	Item/Number	Communalities	Component		
			1	2	3
Alien Visitation (Factor 1)					
45	Alien spaceships regularly visit Earth.	.76	**.85**	.09	.15
39	People have been taken on board alien spaceships.	.75	**.85**	.11	.10
41	Aliens are abducting human beings.	.70	**.83**	.03	.10
47	Alien spaceships have crash-landed on earth..	.74	**.83**	.27	.19
50	Extra-terrestrials have visited Earth throughout history.	.79	**.83**	.19	.12
43	Aliens have injected objects into people.	.68	**.81**	.01	.10
49	Alien intelligence is responsible for some UFO sightings.	.73	**.79**	.24	.18
51	Unidentified Flying Objects suggest that some kind of extra-terrestrial life form has approached the surface of the Earth.	.71	**.79**	.25	.17
8	Intelligent extra-terrestrial life has visited Earth.	.64	**.74**	.27	.12
28	Extra-terrestrial creatures visited Earth in the distant past or at the dawn of human civilization.	.58	**.71**	.20	.20
Existence of Extra-Terrestrial (Factor 2)					
40R	The Earth is the only planet in the universe that supports life.	.71	.05	**.83**	.14
38	Somewhere in the universe there are other forms of life.	.71	.14	**.80**	.19
23R	Earth is the only planet in the universe that harbours life.	.64	.07	**.77**	.23
1	Given the size and age of the universe, it is very likely that extra-terrestrial life must exist.	.63	.16	**.76**	.14
44	There is life on other planets.	.62	.08	**.76**	.18
48	Intelligent life exists beyond our universe.	.65	.27	**.75**	.06
46R	There is no such thing as extra-terrestrial life.	.64	.14	**.73**	.31
42R	The only intelligent life exists on Earth.	.57	.18	**.72**	.12
15	Just because we have no evidence of extra-terrestrial life does not mean that such life does not exist.	.43	.12	**.61**	.22
6	If Earth-like planets exist in the universe, then it is likely that Earth-like organisms will have evolved on those planets.	.40	.22	**.74**	.01
Search for Extra-Terrestrial Life (Factor 3)					
21R	The search for extra-terrestrial life is a waste of time and money.	.70	.08	.33	**.73**
26R	The search for extra-terrestrial life is a pseudoscience (not proper science).	.67	.29	.25	**.72**
18	Governments should direct more funding to the scientific search for extra-terrestrial life.	.64	.45	.15	**.67**
11	The search for extraterrestrial life is a serious and important scientific endeavor.	.57	.24	.37	**.61**

Bolded numbers represent values loaded on to particular factors.

The second PCA, also using varimax rotation, provided a solution comprising three factors with eigenvalues greater than 1, accounting for 64% of the total variance. Inspection of the pattern matrix revealed that all emergent factors demonstrated good levels of internal consistency, and were conceptually distinct. One of the 25 items was omitted because it failed to meet the loading cut-off value of .45:

Factor 1 (Alien Visitation), eigenvalue of 10.59, accounted for 42.38% of the variance. This factor demonstrated excellent internal reliability (α = .95) and comprised ten items.

Factor 2 (Belief in the Existence of Extra-Terrestrial Life), eigenvalue of 4.00, accounted for 15.99% of the variance. This factor demonstrated excellent internal reliability (α = .92) and comprised ten items.

Factor 3 (Search for Extra-Terrestrial Life), eigenvalue of 1.30, accounted for 5.20% of the variance. This factor demonstrated good internal reliability (α =.80) and comprised four items (see Table 2).

Gender Differences

In order to allow comparisons between the three factor scores, the mean was calculated for each factor. In order to test for gender differences on factor scores, a multivariate analysis of covariance was conducted (MANCOVA). A significant main effect was found for gender, $F(3, 523) = 5.53$, $p = .001$, Wilk's Lambda =.97, partial eta-squared = .031.[2] Analysis of each alien/extra-terrestrial belief factor revealed that: males ($M = 5.22$, $SD = 1.18$) scored higher than females ($M = 4.80$, $SD = 1.24$) on Belief in the Existence of Extra-Terrestrial Life, $F(1, 525) = 12.08$, $p = .001$, partial eta-squared = .022; and males ($M = 4.33$, $SD = 1.39$) scored higher than females ($M = 3.99$, $SD = 1.30$) on Search for Extra-Terrestrial Life, $F(1, 525) = 6.89$, $p = .009$, partial eta-squared = .013. No difference was found between males and females on Alien Visitation, $F(1, 525) = .005$, $p > .05$, partial eta-squared = .00 (see Table 3).

TABLE 3
Means, Standard Deviations, & Reliability Statistics for Factor Scores

		GENDER						COMBINED TOTAL		
		WOMEN			MEN					
	Factor	*M*	*SD*	*α*	*M*	*SD*	*α*	*M*	*SD*	*α*
1	Alien Visitation	2.87	1.27	.95	2.88	1.38	.96	2.88	1.30	.95
2	Existence of Extra-Terrestrial Life	4.80	1.24	.93	5.22	1.18	.90	4.92	1.24	.92
3	Search for Extra-Terrestrial Life	3.99	1.30	.80	4.33	1.39	.79	4.08	1.33	.80

Relationship between Extra-Terrestrial and Paranormal Beliefs

Prior to conducting correlation analysis, the reliability of the paranormal measures was assessed. This revealed that the Revised Paranormal Belief Scale (R-PBS) (Tobacyk, 1988, Tobacyk, 2004, Tobacyk & Milford, 1983) and the Australian Sheep-Goat Scale (ASGS) (Thalbourne, 1995a, Thalbourne & Delin, 1993**)** possessed good interval reliability. In addition to this, the two-factor solution for the R-PBS (Lange, Irwin, & Houran, 2000), which comprised factors assessing New Age Philosophy (NAP) and Traditional Paranormal Belief (TPB), demonstrated good internal reliability (see Table 4).

TABLE 4
Means, Standard Deviations, and Reliability Statistics for Paranormal Belief Measures

Paranormal Belief Measures	*M*	*SD*	*α*
R-PBS	57.01	27.75	.88
TPB	22.16	4.70	.89
NAP	22.84	5.07	.79
ASGS	9.60	7.05	.93

A series of Pearson's Product-Moment correlations were conducted between the three alien/extra-terrestrial belief factors and the measures of paranormal belief (NAP, TPB, and ASGS). Significant positive correlations were found between the alien/extra-terrestrial belief factors. Positive correlations were also found between: NAP and the alien/extra-terrestrial belief factors; TPB and Alien Visitation and Search for Extra-Terrestrial Life; and ASGS and the alien/extra-terrestrial belief factors (see Table 5).

TABLE 5
Correlations: Alien Visitation, Extra-Terrestrial, and Paranormal Belief

		1	2	3	4	5	6
1	Alien Visitation						
2	Belief in ET	.41**					
3	Search for ET	.54**	.58**				
4	NAP	.56**	.15**	.25**			
5	TPB	.48**	.07	.15**	.68**		
6	ASGS	.49**	.24**	.25**	.65**	.53**	

* $p < .05$; ** $p < .01$ (all probabilities one-tailed).

Looking at the pattern of correlations, it is clear that there is a stronger relationship between Alien Visitation and the measures of paranormal belief than there is between the two extra-terrestrial-belief–related factors (Belief in the Existence of Extra-Terrestrial Life and the Search for Extra-Terrestrial Life) (see Table 6).

TABLE 6
Differences in Correlation Magnitude between Belief in Alien/Extra-Terrestrial Life Factors and Measures of Paranormal Belief

	Alien/Extra-Terrestrial Belief Factors		Z	Sig
	Alien Visitation	**Belief in ET**		
NAP	.56	.15	9.72	$\leq$.001
TPB	.48	.07	9.48	$\leq$.001
ASGS	.49	.24	6.03	$\leq$.001
	Alien Visitation	**Search for ET**		
NAP	.56	.25	8.57	$\leq$.001
TPB	.48	.15	8.55	$\leq$.001
ASGS	.49	.25	6.32	$\leq$.001
	Belief in ET	**Search for ET**		
NAP	.15	.25	2.33	$\leq$.001
TPB	.07	.15	2.11	$\leq$.001
ASGS	.24	.25	0.47	$\leq$.05

A series of first-order partial correlations was conducted to determine the degree to which the significant positive correlations between the alien/extra-terrestrial belief factors and paranormal belief were explained by belief in Alien Visitation. Controlling for Alien Visitation beliefs revealed weak negative correlations between Belief in the Existence of Extra-Terrestrial Life, Search for Extra-Terrestrial Life, and the two factors of the R-PBS (NAP and TPB) (see Table 7). Contrastingly, controlling for Belief in the Existence of Extra-Terrestrial Life revealed positive correlations between Alien Visitation, Search for ET, and the two factors of the R-PBS (NAP and TPB) (see Table 8). Finally, controlling for Search for ET produced positive correlations between Alien Visitation and the two factors of the R-PBS (NAP and TPB), and no correlation was found between Belief in ET and the R-PBS (see Table 9). These findings indicate that the relationship between the alien/extra-terrestrial belief factors and paranormal belief is best explained by belief in Alien Visitation.

TABLE 7
Partial Correlations: Belief in the Existence of ET, Search for ET, and Paranormal Belief Controlling for Alien Visitation

		1	2	3	4
1	Belief in ET				
2	Search for ET	.47**			
3	NAP	–.10*	–.08*		
4	TPB	–.16**	–.15**	.56**	

* $p < .05$; **; $p < 0.1$ (all probabilities one-tailed).

TABLE 8
Partial Correlations: Alien Visitation, Search for ET, and Paranormal Belief Controlling for Belief in the Existence of ET

		1	2	3	4
1	Alien Visitation				
2	Search for ET	.40**			
3	NAP	.55**	.19**		
4	TPB	.50**	.14**	.68**	

* $p < .05$; ** $p < 0.1$ (all probabilities one-tailed).

TABLE 9
Partial Correlations: Alien Visitation, Belief in the Existence of ET, and Paranormal Belief Controlling for Search for ET

		1	2	3	4
1	Alien Visitation				
2	Belief in ET	.15**			
3	NAP	.52**	.02		
4	TPB	.48**	–.03	.67**	

* $p < .05$; ** $p < 0.1$ (all probabilities one-tailed).

Discussion

The nature and structure of alien/extra-terrestrial beliefs has been relatively under-researched within psychology (Swami et al., 2009, Swami, Pietschnig, Stieger, & Voracek, 2010b). Hence, the current paper was designed to extend research by examining and reconciling theoretical differences between important recent work (cf., Dagnall, Parker, Munley, & Drinkwater, 2010, Dagnall, Munley, Parker, & Drinkwater, 2010a, and Swami et al., 2009). Particularly, debate has arisen around the nature and number of alien/extra-terrestrial belief factors: Dagnall, Parker, Munley, and Drinkwater (2010) and Dagnall, Munley, Parker, and Drinkwater (2010a) forwarded two factors (Factor 1, belief in extra-terrestrial life; and Factor 2, UFO-related beliefs), while Swami et al. (2009) outlined three factors (Factor 1, belief that extra-terrestrial life has visited Earth and that governmental agencies have knowledge of this fact; Factor 2; scientific search for extra-terrestrial life; and Factor 3, general beliefs about the existence of extra-terrestrial life). Although these studies successfully proposed alien/extra-terrestrial factors and produced valid and reliable measures, the lack of agreement over the number and nature of factors suggests that there is currently no theoretical consensus, and that greater conceptual clarity is required. In this context the current study's intention was to disambiguate these differences and in so doing further elucidate understanding of alien/extra-terrestrial beliefs.

To achieve this, items from the Dagnall, Parker, Munley, & Drinkwater (2010) and Dagnall, Munley, Parker, & Drinkwater (2010a) and Swami et al. (2009) papers were combined to produce a composite measure of alien/extra-terrestrial belief(s). Using Principal Components Analysis (PCA), this measure was found to be reducible to three related, but conceptually different factors: Factor 1, Alien Visitation (e.g., "Alien spaceships regularly visit Earth"); Factor 2, Belief in the Existence of Extra-Terrestrial Life (e.g., "Somewhere in the universe there are other forms of life"); and Factor 3, the Search for Extra-Terrestrial Life (e.g., "Governments should direct more funding to the scientific search for extra-terrestrial life"). The existence of three factors supports the notion that alien/extra-terrestrial life beliefs are multifactorial (Chequers et al., 1997, Dagnall, Parker, Munley, & Drinkwater, 2010, Dagnall, Munley, Parker, & Drinkwater, 2010a, Swami et al., 2009) and implies that simple measures, such as those contained within general measures of paranormal belief (e.g., R-PBS) are insufficient and unable to account for the complex nature of alien/extra-terrestrial beliefs.

Importantly, support is provided for the dichotomy between alien visitation/UFO-related beliefs and extra-terrestrial belief (Dagnall, Parker, Munley, & Drinkwater, 2010, Dagnall, Munley, Parker, & Drinkwater, 2010a, Swami et al., 2009, Chequers et al., 1997). Differences, however, were observed between

the factors identified in the present study and those proposed by Dagnall, Parker, Munley, & Drinkwater (2010), Dagnall, Munley, Parker, & Drinkwater (2010a), and Swami et al. (2009). Particularly, the Dagnall, Parker, Munley, & Drinkwater (2010) measure was extended by the identification of an additional factor (Search for Extra-Terrestrial Life), while the government component of the Swami et al.'s measure (2009) (Factor 1) was found to be redundant.

Within the present study, males scored higher than females on Belief in the Existence of Extra-Terrestrial Life (Factor 2) and Search for Extra-Terrestrial Life (Factor 3), and no difference was found for Alien Visitation. While these findings contradict Swami et al. (2009, 2010b), who found no sex difference, they are consistent with previous studies, which found men more likely to believe in extra-terrestrial life (Goode, 2000, Patry & Pelletier, 2001, Rice, 2003). Although gender differences were observed, the accompanying effect sizes were small (Cohen, 1988), and this may explain the previously reported inconsistent findings.

The present paper was also designed to examine the relationship between alien/extra-terrestrial belief and paranormal belief. Previous work reports that participants endorse the existence of life elsewhere in the universe but are skeptical that such life has visited Earth (Dagnall, Parker, Munley, & Drinkwater, 2010, Dagnall, Munley, Parker, & Drinkwater, 2010a, Swami et al., 2009). Noting this distinction, Swami et al. (2009) proposed a dichotomy between paranormal-related alien/extra-terrestrial beliefs (Factor 1, belief that extra terrestrial life has visited Earth and that government agencies have knowledge of this fact) and more science-based beliefs (Factor 3, general beliefs about the existence of extra terrestrial life). Dagnall, Munley, Parker, & Drinkwater (2010a) found only the more radical UFO-related beliefs to be positively associated with paranormal belief; alien visitation was found to correlate more strongly with paranormal belief than the extra-terrestrial belief factors. Partial correlations controlling for alien visitation revealed no association between the extra-terrestrial belief factors and paranormal belief. These findings suggest that belief in alien visitation can be viewed alongside facets of paranormal belief, while belief in extra-terrestrial life cannot. Perhaps one interpretation of this is that both sets of belief are dependent upon impaired/faulty reasoning and critical evaluation (e.g., Blackmore, 1997, Bressan, 2002, Dagnall, Parker, & Munley, 2007, Rogers, Fisk, & Wiltshire, 2010). However, in spite of this, it is clear that belief in alien visitation or an alien presence is not always considered to be unfounded and without evidential base (e.g., Carlotto, 1995, 1997, 2002, DiPietro, Molenaar, & Brandenburg, 1988, Friedman, 2008, Greer, 2006, Leir, 2005, Maccabee, 2000, Sitchin, 1976, 2004, 2010).

Indeed, many researchers hold particular ideas concerning extraterrestrials predicated upon reasoned argument and the evaluation of evidence.

Consequently, what remains for further study is how the nature of alien visitation beliefs might differ between groups of individuals depending upon how those beliefs are derived. In some instances at least, beliefs in alien visitation or an alien presence is the result of analytical processes that form the basis of empirical and critical evaluation. Thus, the relationship between paranormal beliefs and alien visitation beliefs may not be found when the latter beliefs are based upon reasoned argument. This requires further study.

In this context one variable of particular interest is reality testing (Irwin, 2003). Reality testing has been defined as the inclination to test critically the logical plausibility of beliefs. The importance of reality testing is derived from the notion that pathological beliefs and delusions arise in part from the failure to subject hypothetical explanations of sensory experience to critical testing (Irwin, 2004, Langdon & Coltheart, 2000); problems arise because experiences require interpretation (casual attributions) and are subject to bias (Kahneman & Tversky, 1972, Weiner, 1986). Thus, pathological belief generation is characterized by the failure to test the plausibility of generated explanations/hypotheses.

Reality testing deficits have been used to explain the development and maintenance of paranormal beliefs (Dagnall, Drinkwater, Parker, & Munley, 2010, Irwin, 2004, 2003, Zusne & Jones, 1982). The acceptance and maintenance of paranormal explanations over conventional alternatives arises from the intuitive–experiential interpretation of stimuli and an absence of analytical–rational processing (reality testing) (Irwin, 2009). Irwin and Young (2002) argue that people with an intuitive–experiential processing style will be predisposed toward accepting paranormal explanations because they find them appealing and therefore do not subject them to reality testing. This notion could be extended to include alien visitation/UFO-related beliefs.

Recent research has found that scores on cognitive–perceptual measures (schizotypy and transliminality) affect level of paranormal belief (Dagnall, Munley, Parker, & Drinkwater, 2010b). Particularly, participants scoring above the median have demonstrated higher levels of endorsement across a range of paranormal belief subscales (Hauntings, Aliens, Superstition, Other Life, Religion, PK, ESP, Astrology, and Witchcraft) than those below the median. In line with Swami et al. (2010b), this suggests that the individual-differences approach may be usefully employed to clarify the underlying processes that give rise to extra-terrestrial and alien-related beliefs.

Indeed, extra-terrestrial and alien-related beliefs have been found to be predicted by different variables. Swami et al. (2010b) reported that extra-terrestrial beliefs were predicted by paranormal beliefs, the unusual factor of schizotypy, openness to experience, and education. While Swami, Chamorro-Premuzic, and Shafi (2010a) noted that Agreeableness, Neuroticism, and

Extraversion were positively correlated with beliefs in alien visitation and government coverups, when additional variables were controlled for (e.g., conformity and sensation-seeking) only Conscientiousness significantly predicted such beliefs. Extending this work, it would be interesting to see which individual cognitive–perceptual factors (reality testing, schizoptypy, dissociation, etc.) best predict belief in extra-terrestrial and alien-related beliefs (Swami et al., 2010b), and whether, as is the case with paranormal beliefs, this relationship may be largely explained by reality-testing deficits (Dagnall, Drinkwater, Parker, & Munley, 2010, Irwin, 2004, 2009). Again, more work is required in this area.

In conclusion, this paper provides further support for the dichotomy between extra-terrestrial–related beliefs and alien visitation/UFO-related beliefs. The former are likely to be considered more conventional by members of the mainstream scientific community, especially those involved or affiliated with SETI (e.g., Shostak, 2009, Vakoch, 2011). The latter views could be construed as somewhat less conventional by these same researchers. However, it is perhaps more likely that the endorsement of belief in alien visitation and its relationship to paranormal belief is not fixed but variable, depending upon how such beliefs are derived and the manner in which such beliefs are woven into the individual's scientific worldview.

Notes

[1] Within Table 1 and Table 2, reversed (item numbers with an R) denotes items that are reversed-scored.

[2] A partial eta-squared between .01 and .06 reflects a small effect size, within the .06–.13 range a medium effect size, and .14 or higher a large effect (Cohen, 1988).

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RESEARCH

Anomalous Switching of the Bi-Stable Percept of a Necker Cube: A Preliminary Study

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Abstract—Psychophysiological research has shown anomalous correlations between unconscious states reflected by physiological fluctuations and random future conditions. Where the future conditions concerned emotional and neutral events, this anomalous effect has been called presentiment. In the present research, the domain of interest regarding apparent retrocausal effects is further extended to the visual experience of a so-called "transparent" Necker cube. When a picture of this cube is presented to subjects, their experience switches spontaneously between two viewpoints. In one perspective the cube is experienced as observed from "above," in the other it is experienced as observed from "below." We measured switching times from "the above" to "the below" experience. Once the subject had indicated by pressing a button that this shift had taken place, the picture of the transparent cube changed into an opaque presentation of one of the two possible viewpoints. The choice of which perspective was presented, "from above" or "from below", was random. When the opaque view was "from above" this corresponded to the view for which the duration was measured (congruent), the opaque view "from below" was the incongruent condition. Arguing that in the incongruent condition the opaque view would "retrocausally" interfere with the "top view" for which the duration was measured, we predicted that in that condition the duration would be shorter. The switching-time effects found in the pilot and two confirmatory studies were in the same predicted direction. The pooled results showed a mean difference in switching time of 126 msec. These results seem to fit into a growing database of anomalous correlations between conscious and unconscious behavior and random future conditions. It extends the domain of these anomalous correlations to other non-emotional events. Alternative possibilities, such as procedural errors, are discussed.

Keywords: time symmetry—retroactive interference—anomaly—retroactive priming—Necker cube

Introduction

Theory

Psi phenomena can be formally defined as correlations that seem to transcend space or time or both. For instance, there may be correlations between what subjects choose from a set of four potential targets and the actual target about which the subject has no information. This information may be distant in space

so that access by normal sensory channels is prohibited, or the information may be distant in time. When the target is known only in the future, the anomaly is quite obvious because any correlation in that case seems to contradict causality.

One of the general issues in this field of anomalous correlations is the role of emotions. Do these anomalous correlations arise especially when the events are very emotional (Broughton, 2006)? That idea originates from case studies such as crisis telepathy. The vast majority of reported cases from the field deal with highly emotional events such as the passing away of relatives. However, one could argue that this is not an intrinsic aspect of the anomalous correlations but rather an intrinsic aspect of the reporting bias (or that emotion tends to focus attention, or provide a motivational factor that non-emotional targets cannot). One cannot exclude that trivial cases just aren't reported but happen nonetheless. In presentiment studies the role of emotions is quite explicit (Radin, 2004). But are correlations between physiological behavior and a random future stimulus restricted to paradigms where we compare neutral and *emotional* events?

In a recent theoretical approach, it has been argued that these correlations should occur in non-emotional events as well. According to this approach, these anomalous correlations arise from time-symmetry restoration (Bierman, 2010). Time symmetry is quite basic in most physical formalisms. For instance, electromagnetic systems theory predicts that there are two consequences of a specific initial state. These two solutions are called the retarded solution (with time running forward) and the time-symmetric advanced solution (where time can be interpreted as running backward). The advanced solution is generally considered a meaningless oddity due to the mathematics used, because in physical systems this symmetry hasn't been observed empirically. However, an alternative view is that the boundary conditions in most simple physical systems are such that the advanced solutions are prohibited. Introducing the brain, while it is sustaining consciousness, into the otherwise material system is assumed to restore time symmetry to some degree. The restoration is assumed to be more complete when the brain system is in a more coherent state. This theoretical approach does not rely on emotions and would predict "retroactive" effects for all events that interact with consciousness, not only emotional ones.

Another issue relates to the idea that it is easier to find anomalous correlations when measuring non-conscious dependent variables such as physiological variables rather than when measuring consciously produced variables (such as explicit oral predictions). In several studies, anomalous correlations were established between non-conscious physiological states and random external (actual or future) conditions while the subjects appeared to be unable to "use" this information in order to improve conscious guessing of the external random condition (i.e. Lobach, 2010). The dependent variable in the current study (switching) can be interpreted as somewhere between conscious and non-conscious behavior.

Finally there is a theoretical issue related to the stability of the system under consideration. It has been proposed that it is easier to find anomalous correlations in unstable or labile systems (Stanford, 1990).

These three issues together suggest the use of the experience of a bi-stable percept as a dependent variable. There is an ongoing discussion in the field of bi-stable conscious states about where in the brain bi-stability is handled. Globally there are two points of view. Either there is a top-down (attentional) process originating in higher parts of the brain that is the origin of the switching between the two experiences, or the bi-stability is processed and resolved in the very early stages of information processing by the brain (Tong, Meng, & Blake, 2006, Blake & Logothetis, 2002).

Earlier Experiments

The roots of the current research can probably be traced back to the middle of the last century, when instruments to measure physiological processes became more commonly available. A. J. Good reportedly suggested measuring brain potentials on the surface of someone's skull (EEG) while he sits in a dark room and a light is flashed at random moments, to discover whether "the EEG shows any tendency to forecast the flashes of light" (Good, 1961, cited in Radin, 2006:163). In the 1970s, there was in fact a study conducted to explore whether the EEG showed any tendency to forecast, not flashes of light, but the gender of faces in pictures (Hartwell, 1978). The results showed no significant differences in EEG for different genders, however, despite laborious (especially at the time) and extensive analyses. At about the same time, Vassy (1978) did report highly significant results in an experiment that was set up to measure telepathy. That study is worth mentioning because its design was rather similar to that of later presentiment studies. Vassy measured the electrical activity of the skin (EDA) preceding an electrical shock for which the participant either was or wasn't warned telepathically by someone in another room. As with Hartwell's EEG study (Hartwell, 1978), judging and analyzing physiological measures was cumbersome and prone to error in those days. This is perhaps why it took a rather long time before more studies were undertaken in this direction. By the end of the last century, Radin picked up the trail and used modern, automated equipment in the first of a series of presentiment studies (Radin, 1997). Radin got interesting, statistically significant, results corroborating his hypothesis. These results were soon replicated by Bierman, and together they published a summary of five different presentiment studies in a "mainstream" psychological journal (Bierman & Radin, 1997). As in Vassy's study, these early experiments used mainly EDA as the dependent physiological measure of presentiment; this measure seemed to produce the most reliable results.

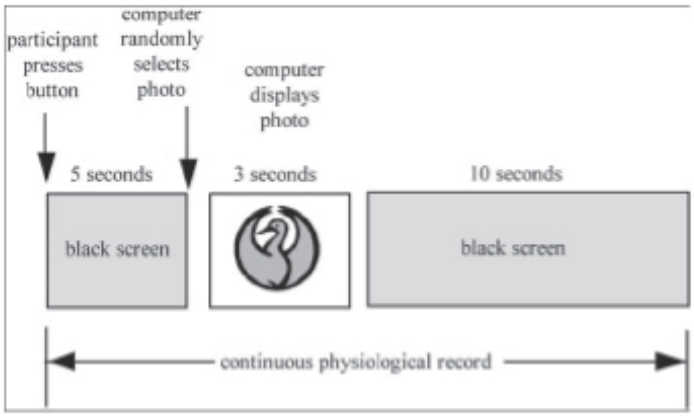

Figure 1. Illustration of a trial in a presentiment experiment (from Radin, 2004).

In a typical presentiment study as reported by Bierman and Radin (1997), a participant is hooked up to an instrument to measure EDA. Changes in the electrical activity of the skin are measured continuously during the whole session. The participant is seated in a comfortable chair in front of a computer screen and remains passive, except when a signal indicates that he or she can start the next trial by pressing a button. The number of trials may vary, but is typically about 30. After the button press, there is a delay of a few seconds until a stimulus is presented, after which there is a cool-down period until the next signal (Figure 1). Stimuli are drawn randomly from two picture pools. One pool contains calm pictures, such as photos of landscapes. The other pool contains arousing pictures with violent (e.g., a bloody car crash) or erotic content. Experiments in mainstream psychology have shown clear EDA responses after arousing stimuli. In presentiment studies the focus is on the time interval directly preceding the stimulus, from the button press until the picture is presented on the screen.

EDA measures can be analyzed in a number of different ways. The studies conducted by Radin and Bierman looked at the average skin conductance level response (SCL). The statistical analysis compares the SCL averaged across all emotional trials with the SCL averaged across all neutral trials. Although in most studies the future condition was either emotional or non-emotional using pictures from the International Affective Picture system (Lang, Bradley, & Cuthbert, 1999), there have been a few studies that used a pleasant or an unpleasant tone (Spottiswoode & May, 2003, May, Paulinyi, & Vassy, 2005). These studies yielded comparable results.

Since then, several other physiological measures have been used as dependent variables with a similar design as in Figure 1 or with a design where the stimuli are unexpected loud sounds or light flashes to induce strong responses like in the study by May and Spottiswoode. Variables that have been used include Evoked Potentials (Radin & Lobach, 2007), CNV (Bierman, 2006), Bold (Bierman & Scholte, 2002, Bierman, 2007), Eye Movement (Radin & Borges, 2009), Pupil Dilation (Radin & Borges, 2009), Blinking (Radin & Borges, 2009), and HR (heart rate) (Tressoldi, Martinelli, Massaccesi, & Sartori, 2005, McCraty, Atkinson, & Bradley, 2004a, 2004b). The results of all these studies suggest anomalous correlations, though the interpretation is far from clear and the results are not very robust. Also a number of experiments have used behavioral measures such as preference scores in a mere-exposure experiment where the preference score was given before the mere exposure (Bem, 2011). Preferences that increase as in mere exposure or decrease as in habituation can still be seen in the framework of emotion research. Apparent retrocausal effects were also observed in a priming task where the prime was presented after the response was given (de Boer & Bierman, 2006). This retroactive priming study showed a clear effect of a faster response in a gender-discrimination task when the target was followed by a congruent "prime" (which actually should be called "post"). In this case, "emotions" apparently were not involved explicitly or implicitly.

Research Question

In the present research, the domain of interest regarding apparent retrocausal effects is further extended to the visual experience of a so-called "transparent" Necker cube. When a picture of this cube is presented to subjects, their experience switches spontaneously between two viewpoints. In one perspective the cube is experienced as observed from above, in the other it is experienced as observed from below. We measured switching times from the "above" to the "below" experience. Once the subject had indicated by pressing a button that this shift had taken place, the picture of the transparent cube changed into an opaque presentation of one of the two possible viewpoints. The choice of which perspective was presented, "from above" or "from below", was random. This created two conditions. When the opaque view was "from above", this corresponded to the view for which the duration was measured (congruent), the opaque view "from below" was the incongruent condition. Arguing that in the incongruent condition the opaque view would "retrocausally" *interfere* with the "top view" for which the duration was measured, we predicted that in that incongruent condition the duration would be shorter. Alternatively one could argue that presenting a future congruent opaque view would stabilize retroactively the experienced "top view", thereby enhancing the switching time. The

direction of the differential effect between congruent and incongruent condition would be the same for both arguments. Deciding between the two "models" is possible only when a baseline condition with no future opaque view of the cube is presented. We have considered using a balanced design where the duration of the bottom view would also be measured in a congruent and incongruent condition. However, most subjects find it easier to experience the "top view" and also have difficulty performing the task. For this preliminary study, we therefore opted to keep the task as simple as possible (see also in the Discussion section the recommendations for future research).

Method

Subjects

Subjects for the pilot and confirmatory studies were recruited from the Dutch University of Groningen student population. The second confirmatory study used voluntary subjects from the Amsterdam area. About half of those subjects in the Amsterdam study practiced yoga while the other half consisted of matched control subjects. See Table 1 for gender counts and age information.

TABLE 1
Mean Age and Standard Deviations Split for Study and Gender

	FEMALE			MALE		
Study	**N**	**Mean**	**sd**	**N**	**Mean**	**sd**
Pilot	3*			3*		
Amsterdam	13	41	17.13	16	36.25	13.4
Groningen	41	22.88	3.84	93	21.62	1.42
TOTALS	***57***	***26.91***	***11.41***	***112***	***23.75***	***7.30***

* Data from the pilot study were unavailable.

Procedure

The experimental software was developed at the University of Amsterdam, and then the same program was mailed to the University of Groningen where the experiment was conducted by a Ph.D. student unaware of the hypotheses.

Parallel to that study, a Master's student in Amsterdam ran the same experiment in Amsterdam, also blind to the hypotheses.

Subjects were exposed to a picture of a Necker cube with a fixation point embedded "inside" the cube (see Figure 2).

They were asked to gaze at the fixation point and wait until they expe-

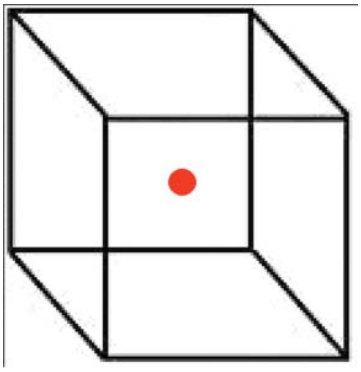

Figure 2. Transparent view of Necker cube.

rienced the cube with a "bottom view" perspective and then press the spacebar (response 1) at the moment that this subjective experience spontaneously changed to a "top view." As soon as the "top view" returned to the bottom view they pressed the spacebar again (response 2). The trial then ended by the software changing the picture into an opaque view of the cube randomly in either "top view" or "bottom view" (see Figure 3).

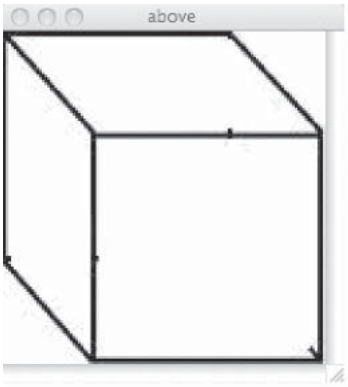

Figure 3. Example of opaque "top view" of Necker cube used for feedback.

This opaque-cube feedback remained for one second on the screen. The time between the two responses was the duration that the "top view" was experienced. The subjects were unaware of the psi hypothesis. In the Groningen study also the experimenter was unaware of the psi hypothesis. The experiment was framed as a study in the category of consciousness research.

From the pilot experiment we learned that this task is not an easy one and that subjects sometimes missed a switch or did not follow the gaze instruction. We therefore adjusted the subject's instructions and also added instructions for naïve experimenters. Further, in the confirmatory studies, the subjects were asked to indicate if they succeeded in giving the response according to instructions, otherwise the trial was not included in the analyses. For each subject, a session consisted of 32 valid trials and lasted about six minutes.

Analysis

Data from the Groningen confirmatory experiment were mailed to the University of Amsterdam to be analyzed. The Amsterdam experiment was analyzed separately and later all data were pooled. Outlier trials with a perspective switch time value larger than three standard deviations or with a switch time smaller than 800 msec were removed. This procedure, based upon trying out several outlier removal algorithms on the pilot data, was repeated until no further outliers were present. Then the mean durations of the "top view" percept were calculated for each subject and for each of the two (future) conditions. The means were compared using SPSS 16 (Mac-version) with a two-sample *t*-test.

Results

Pilot

Six subjects were tested. Outliers were removed as described above. The mean difference of 58 msec between the two (future) conditions was in the theoretically expected direction but not significant. Inspection of the preprocessed data showed that one subject had more than nine outliers. When this subject was removed from the analysis, the two conditions differed by 384 msec and the *t*-test was marginally significant ($t = 2.5$, df = 4, $p = 0.065$ two-tailed). Two-tailed tests were used here because we decided that in spite of the theoretical argument that gave us an expectation for the direction of the effect, we would use the pilot data to specify a direction to be used in the confirmatory phase of the study. Although the *p*-values of the pilot study are marginal, the fact that the effect was in the predicted direction gave us confidence to also predict the same direction for the confirmatory studies and thereby justified the use of one-tailed testing in those studies. It can be argued that two-tailed testing is always required when effects on both directions are of interest, and we certainly

feel that this might be the case for the study of anomalous phenomena. On the other hand, we feel that this field is in need of theory-driven research generally predicting a direction of an effect, rather than that any anomaly be published.

It should be noted that the preprocessing procedures dealing with outliers introduce some extra degrees of freedom. A subject was removed from the study if he or she produced nine or more outliers. This value is rather arbitrary and was chosen on the basis of optimization of the end result of the pilot study outcome. Once these parameters were set, they were not changed again when analyzing the confirmatory experiments.

Removal of the pilot study's one subject who obtained a large number of outliers also considerably improved the correlation between the two conditions. The correlation between conditions for all six subjects was 0.77, while after removal of the suspect subject the correlation became 0.96, adding to the impression that this removed subject really was an outlier.

The results of the pilot experiment were used as a predictor for the confirmatory experiment. This allowed us to predict a direction for the effect. The predicted direction was in line with the idea that the duration of the "top view" perspective was disturbed by showing an opaque bottom view afterward. We called this retroactive interference. We also expected on the basis of the pilot experiment that subjects in the confirmatory experiments with nine or more outliers would not contribute to a switching time effect.

Confirmatory Studies

The overall difference between the two conditions pooled for all 169 subjects from the three studies was in the same direction and of the same magnitude (88 msec) as in the pilot study, but as in the pilot this effect was non-significant ($t = 1.41$, df = 168, $p = 0.08$ one-tailed). After removal of the 16 subjects (9.5%) who had nine or more outliers, the mean differential effect was 129 msec ($t = 1.97$, df = 152, $p = 0.026$ one-tailed). In Table 2 the results are given for each of the three studies separately.

The mean number of outliers per subject over all studies (including the pilot) was 3.00 from the 32 trials (sd = 1.78). The mean time that each participant spent was 12:20 (sd = 11:53).

Discussion and Conclusion

The statistical strength of the differential effect does not allow for a strong conclusion, rather it suggests further experimentation along these lines. The established scientific worldview appears to conflict with the effects reported here because these effects would imply a violation of traditional causality where cause precedes effect. And extraordinary claims need extraordinary evidence, which

TABLE 2

Review of Results for the Two Conditions, Top View Feedback and Bottom View Feedback, of the Opaque Final Picture

Study	N	Top View	Bottom View	Diff. effect	st error	t	P*
PILOT	5	3669	3306	+363	142.7	2.5	0.065 2-t
AMS	26	4959	4765	+184	104.6	1.76	0.045
GRONINGEN	122	5027	4959	+103	78.2	1.36	0.090
TOTAL	***153***	***5004***	***4875***	***+129***	***78.2***	***1.97***	***0.026***

* One-tailed p-values in confirmatory experiment.

in the current experimental results is lacking. The results, therefore, should be considered only as suggestive and should be replicated widely before drawing stronger conclusions.

Recommendations for further studies are:

1. a balanced design with regard to measurement of top-view duration and bottom-view duration, but between subjects because mixing of these two conditions is too confusing for the subject.

2. An extra baseline condition without an opaque view after measurement of the top- or bottom-view duration. This would allow for discrimination between retroactive interference and retroactive facilitation.

It should be emphasized that the study outcome is sensitive for the choice of parameters that determine how to handle outliers and individual subjects who generate many outliers. Other parameters often result in smaller effect sizes although the direction of the effect is unaffected by change of any of these parameters. For example, the effect becomes statistically non-significant if the 16 subjects who had many outliers are included in the overall analysis. One could argue that many outliers might be an indication of subjects not performing according to task instruction, but this was not explicitly assessed in the exit interview.

Furthermore, one can object to using parametric testing on response-time differences because the data are in principle non-normally distributed. We therefore repeated the analysis using a random permutation test. This test yielded a p-value of 0.023, slightly smaller than the p-value obtained by the analytical approach.

If we assume the effects to be real, it can be concluded that future random feedback correlates with the earlier response times, a controversial effect that hitherto was mostly associated with emotional events. However, the Necker cube switching is a non-emotional phenomenon and the one-second feedback

is hardly noticed by the subjects, and above all there is no reason to assume that this feedback induces any emotional response in the subject. Therefore it might be concluded that this finding supports the idea that apparent retrocausal effects do occur in all events, neutral or emotional. Also, the percepts from above and from below are conscious percepts rather than non-conscious physiological states. Thus we might conclude that these anomalous effects might also induce correlations between future conditions and a conscious state.

We did not formally compare stable with unstable systems, because the Necker cube switch is by definition a phenomenon due to instability. One could argue that when the mean switching time is small the (brain) system is even more labile than when the mean switching times are larger. We therefore correlated the relative effect size for each subject with the mean switching time of that subject. This correlation was very small and far from significant. Thus this study does not lend support to, nor contradict, the idea that more labile systems are more sensitive for these apparent retrocausal effects.

An alternative paranormal explanation is that the study results are an example of an analyzer psi effect: Choosing the analysis criteria precisely in such a way that a significant outcome arises; although a counterargument is that this freedom of choice was constrained by adhering to the parameters that gave the best result for the pilot series in the confirmatory studies. The analyzer effect "explanation" has been put forward in a number of anomaly research studies. The idea is that the analysis is also a future condition, although it is further in the future than the feedback per trial, and a more complex task.

Another explanation that does not resort to an anomaly is that the code that was executed for time measurement is in some way different for the two future pictures. We tested this by simulating key-presses using an independent timer. The mean response times for the two future pictures thus obtained did differ by 2.4 msec (not significant). This difference is a factor of 50, smaller than the differences obtained in this experiment.

Finally there could be a problem with data integrity. However, a copy of the raw data stayed at Groningen University and can be compared by independent researchers to the data that finally entered into the formal analysis.

The studies reported here are generally classified as para*psychological*. That is a misnomer. There is nothing in Psychology that prohibits these anomalous effects from occurring. The term *anomaly* solely refers to the accepted interpretation of physics. If anything, these studies should be classified as para*physical*. However, as was argued in the Introduction, current physical frameworks do not really prohibit advanced phenomena. The nature and the arrow of time is still a very open issue in physics. Therefore, the anomaly refers to the fact that in physics advanced phenomena haven't been observed (yet).

It is important to develop the time-symmetry model further in order to

produce testable hypotheses. Most notably, future work might focus on individual differences and correlate these with the coherence aspect of states of consciousness.

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RESEARCH

Color Distribution of Light Balls in Hessdalen Lights Phenomenon

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Abstract—Hessdalen lights (HL) are unexplained light balls usually seen in the valley of Hessdalen, Norway. In this work, we present a model to explain the spatial color distribution of luminous balls commonly observed in HL phenomenon. According to our model, these light balls are produced by electrons accelerated by electric fields during rapid fracture of piezoelectric rocks under the ground. Semi-relativistic light balls in the HL phenomenon are produced by ionic acoustic waves (IAW) interacting with a central white-colored light ball of HL phenomenon.

Keywords: Hessdalen Lights—Rock piezoelectricity—electron avalanche—Fractoemission

Introduction

Several rare and unexplained light phenomena can be seen in the atmosphere. For example, ball lightning (Paiva, Pavão, Vasconcelos, Mendes, & Silva, 2007), blue jets (Pasko & George, 2002), red sprites (Pasko, Inan, & Bell, 2000), and terrestrial gamma ray flashes (TGFs) (Paiva, Pavão, & Bastos, 2009, Paiva, 2009). Hessdalen Lights (HL) are unexplained lights usually seen in the valley of Hessdalen, Norway (Teodorani, 2004). They have the appearance of a free-floating light ball with dimensions ranging from decimeters up to 30 m. HL often show strong pulsating magnetic perturbation of about 5 Hz. They are often accompanied by small, short-duration pulsating "spikes" in the HF and

VLF radio ranges, sometimes showing Doppler features. HL explicitly shows visually some kind of "satellite spheres" around a central luminous core. The absolute luminosity of this cluster of light balls has been estimated to be about 19 kW. The empirical evidence is that the small balls which can be ejected to a large distance (on the order of 50–100 m) from the large white-colored nucleus tend to be green-colored, while the small balls that appear to be very close (distance on the order of 2–5 m) to a cluster nucleus tend to be white- (high intensity) or red- (high intensity) and blue- (low-intensity) colored.

The reason for the different colors, which are apparently related to distance from the nuclear region, remains unknown (Teodorani, 2004). According to Teodorani (2004), the production of balls of distinctly different color, recorded at Hessdalen, differs from standard ball lightning behavior. If so, then the color of the light balls might be produced by quantum dots from mold spores on just one side of the plasma or by natural aerosols whose nature varies with locality. Quantum dots are nanoparticles made from a semiconducting material and range in diameter from 2–10 nm. Spontaneous production of almost mono-disperse quantum dots might come from mold spores, as the main semi-conducting elements, decomposed by the central plasma of the light ball. However, this theory does not explain the color intensity of satellite light balls.

No existing theory or model can account for all (and sometimes contradictory) observations of HL. One explanation attributes the phenomenon to an incompletely understood combustion process in air involving clouds of dust from the valley floor containing scandium (Bjorn, 2007). Some sightings, though, have been identified as misperceptions of astronomical bodies, aircraft, car headlights, and mirages (Leone, 2003).

A theory that has attracted great attention was proposed by Takaki and Ikeya (1998). It involves piezoelectricity generated under a rock strain. Change in seismic stress releases piezo-compensating, bound charges due to changes in the piezoelectric polarization of quartz grains in granitic rocks, which produces an intense electric field at the fault zone. In the specific Hessdalen area, where light phenomena are seen very often some meters over the ground, an electric triggering mechanism above might be produced by the existing high abundance of quartz, copper, and iron underground. When quartz is subjected to tectonic stress, it generates piezoelectricity (Lockner, Johnston, & Byerlee, 1983), while copper is an ideal electricity conductor and consequently might be an electrical amplifier of the HL phenomenon.

One recent hypothesis suggests that the lights are formed by a cluster of macroscopic Coulomb crystals in a plasma produced by the ionization of air and dust by alpha particles during radon decay in the dusty atmosphere (Paiva

& Taft, 2010). Coulomb crystal is a regular structure (cubic, triangular, etc.) formed by microparticles (dust) in the plasma of electrons and ions under certain conditions. Several physical properties (oscillation, geometric structure, and light spectrum) observed in Hessdalen Lights phenomenon can be explained through the dust plasma model.

Enomoto and Hashimoto (1990) have detected the emission of charged particles from indentation fracture of rocks. The charge generated by hornblende andesite indentation fracture is about 1.2×10^{-11} C/s. The volume of the fractured zone, estimated from the size of the indent, was 0.02×10^{-9} m^3. Thus, the net production rate would be ~0.6 $C/m^3/s$. If a massive fracture occurs during one second at ground level, over an area extending some meters, the charge generated may be compared to the total electric charge produced by one bolt of lightning (1 Coulomb). Charge separation on fractured surfaces produces high electric fields on the order of 10^6–10^7 V cm^{-1}, causing the field emission of electrons in the atmosphere. The energy of emitted electrons may be in the keV range. This may be sufficient to cause geoelectromagnetic disturbances.

Ogawa, Oike, and Miura (1985) showed in laboratory experiments that rocks radiated wide-band EM waves (10 Hz–100 kHz) when they were struck by a hammer and fractured. Very low frequency EM emission (0.01–10 Hz) was also observed from earth rocks before and during earthquakes according to Park, Johnson, Madden, Morgan, and Morrison (1993). Satellites showed intense EM radiation at frequencies below 450 Hz (Serebryakova, Bilichenko, Chmyrev, Parrot, Rauch, Lefeuvre, & Pokhotelov, 1992). These data are in accord with the EM signals recorded by the spectrum analyzer and the magnetometer at Hessdalen covering the band of 0.5–80 MHz (Strand, 1990). So it appears that EM waves could have been emitted by rocks in the region where strange lights were observed.

In this work, we present a model to explain the spatial color distribution of luminous balls commonly observed in HL phenomenon. According to our model, different colors of light balls in HL phenomenon are produced by accelerated electrons due to electric fields formed by rapid fractures of piezoelectric rocks under the ground during water freezing (i.e. during the winter). Semi-relativistic green light balls in the HL phenomenon are produced by the light emission of ionic oxygen transported by ionic acoustic waves (IAW) interacting with a large white-colored light ball of HL phenomenon.

The Model

Let us consider the model of the HL cluster shown in Figure 1.

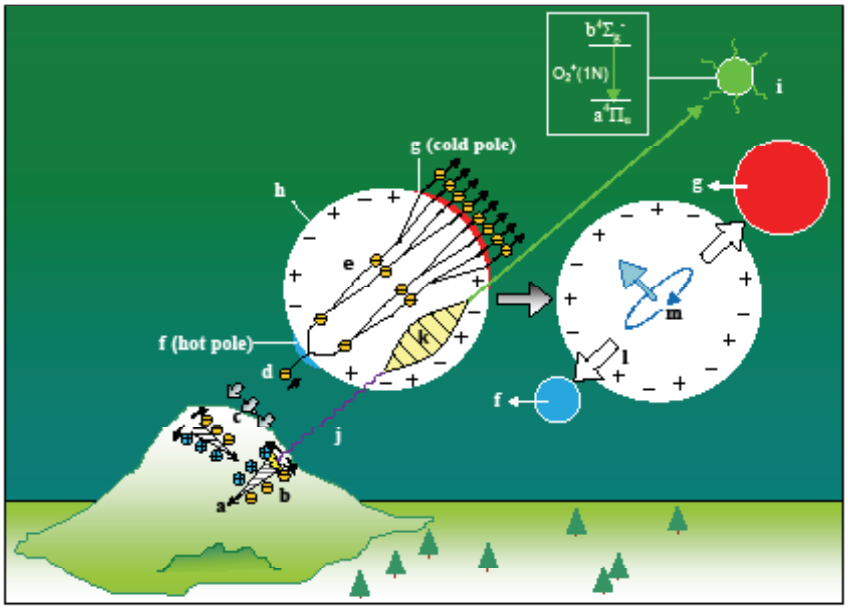

Figure 1. Light balls in HL cluster: Rock fractures under the hill ground (a), charge separation (b), high electric fields (c), acceleration of these electrons in the atmosphere (d), electron-avalanche (e), low-intensity blue-colored light ball (f), high-intensity red-colored light ball (g), central white-colored light ball (h), small green light ball (i), very low frequency electromagnetic waves (j), ion-acoustic waves (k). Rotation (l) of the central white ball (m) ejecting its hot and cold edges, forming, respectively, blue (f) and red light balls (g).

According to Figure 1, the abrupt rock fractures under the hill ground (a) (probably produced by water expansion during freezing in the lithosphere) results, temporarily, in charge separation (b). One possibility is that charge separation on fractured surfaces produces high electric fields on the order of 10^6–10^7 V cm^{-1} (c). Free electrons with the density of 4×10^6 to 1×10^7 electrons m^{-3} s^{-1} are generated by cosmic rays and natural radiation due to atmospheric radioactivity. The electric field generated induces charges on the ground causing the acceleration of these electrons in the atmosphere (d). When these electrons collide with atmospheric atoms, knocking off electrons, they will form an electron-avalanche (e). Low-flux of high-energy (temperature) electrons will produce a low-intensity, blue-colored light ball (f), and high-flux of low-energy (temperature) electrons will produce a high-luminosity, red-colored

light ball (g) in the opposite direction of the central white ball (h). Ejection of a small green light ball (i) from a white ball is due to radiation pressure produced by the interaction between very low frequency electromagnetic waves (j) and atmospheric ions in the central white-colored ball through ion-acoustic waves (IAW) (k). Centrifugal forces (l) caused by the rotation of the central white ball (m) can eject its hot and cold poles, forming, respectively, blue and red light balls.

IAW is a longitudinal oscillation of the ions (and the electrons) much like acoustic waves traveling in neutral gas. The IAW velocity will be (Alexeff & Neidigh, 1961):

$$V_{IAW} = \sqrt{\frac{\gamma_e Z_i k_B T_e + \gamma_i k_B T_i}{\langle m_i \rangle}} \tag{1}$$

where k_B is Boltzmann's constant, $\langle m_i \rangle$ is the mean mass of the ion, Z_i is its charge, T_e is the temperature of the electrons, and T_i is the temperature of the ions. Normally γ_e is taken to be unity, on the grounds that the thermal conductivity of electrons is large enough to keep them isothermal on the time scale of ionic acoustic waves, and γ_i is taken to be 3, corresponding to one-dimensional motion. In the plasma the electrons are often much hotter than the ions, in which case the second term in the numerator can be ignored. Thus, we have:

$$V_{IAW} \sim \sqrt{\frac{\gamma_e Z_i k_B T_e}{\langle m_i \rangle}} \tag{2}$$

According to Teodorani (2004), the HL spectrum gives a gas (ion) temperature of about T_i = 5,000 K. Generally, the radiant species present in atmospheric plasma are N_2, N_2^+, O_2, and O_2^+, NO^+ (in dry air) and OH (in humid air). At higher temperatures, atomic emission lines of N and O, and (in the presence of water) H, are present (Laux, Spence, Kruger, & Zare, 2003). Thus, considering $T_e = 10 \times T_i$ = 50,000 K, and mean ion mass as being:

$$\langle m_i \rangle = \frac{m_i(O^+) + m_i(N^+)}{2} \tag{3}$$

where $m_i(O^+) = 2.3 \times 10^{-16}$ kg is the ionic oxygen mass, and $m_i(N^+) = 2.6 \times 10^{-16}$ kg is the ionic nitrogen mass, we have $V_{IAW} \sim 10^4$ ms^{-1}. This is the velocity of the energetic wave packet of an ion acoustic wave in a dusty plasma. This

value is close to the observed velocity of some ejected light balls from HL, which is estimated as being 2×10^4 ms^{-1} (Teodorani, 2004).

Night vision systems revealed that the HL phenomenon produces a very strong infrared signature even when it is very faint or invisible in the optical range (Teodorani & Nobili, 2002). The dissociative recombination of N_2^+ in excited neutral atoms can explain the infrared emission by HL phenomenon even when it is very faint or invisible in the optical range. When VLF eject ions in high velocity from a white ball by IAW, dissociative recombination of N_2^+ [i.e. destroying molecular ions to produce excited neutral atomic species] can occur through the reaction $N_2^+ + e^- \rightarrow N + N$ + kinetic energy (Kasner, 1967). Dissociative recombination (DR) exhibits a high exothermicity, which makes DR the only source of kinetically energetic atoms (>1eV) (Peterson, Le Padellec, Danared, Dunn, Larsson, et al., 1998). These species (i.e. excited nitrogen atoms) are disconnected by IAW because they have zero charge. Few experimental studies of recombination have been carried out under conditions where dissociative recombination is the predominant process (Fowler & Atkinson, 1959). It will be the predominant electron loss process in the plasma only in regions where the concentration ratio of atomic to molecular ions is $>l0^4$ (Biondi, 1969).

Why is the ejected ball always green-colored? Ejection of a small green light ball from HL is due to radiation pressure produced by the interaction between very low frequency electromagnetic waves (VLF) and atmospheric ions (present in the central white-colored ball) through ion-acoustic waves (IAW) (See Figure 1). Probably only O_2^+ ions (electronic transition ($b^4\Sigma_g^- \rightarrow a^4\Pi_u$)), with green emission lines, are predominantly transported by IAW. Electronic bands of O_2^+ ions occur in auroral spectra (Chamberlain, 1961, Nicolet & Dogniaux, 1950). Electron–molecular-ion dissociative recombination coefficient rate α as functions of electron temperature T_e and cross sections σ as a function of electron energy E have been measured by Mehr and Biondi (1969) for N_2^+ and O_2^+ over the electron temperature interval 0.007 to 10 eV. The estimated temperature of HL is about 5,000 K (Teodorani, 2004). At this temperature, the rate coefficient of dissociative recombination will be, respectively, $\alpha(T_e)_{O2+} \sim 10^{-8}$ cm^3 s^{-1}, and $\alpha(T_e)_{N2+} \sim 10^{-7}$ cm^3 s^{-1}. Thus, the nitrogen ions will be decomposed in $N_2^+ + e^- \rightarrow N + N^*$ more rapidly than oxygen ions in the HL plasma. Only ionic-species are transported by IAW. Therefore, only oxygen ions will be predominantly ejected by IAW from a central white ball in HL phenomenon forming high-velocity green-light balls presenting a negative band of O_2^+ with electronic transition $b^4\Sigma_g^- \rightarrow a^4\Pi_u$. Additionally, the first positive bands of N_2($1PN_2$, electronic transition $B^3\Pi_g \rightarrow A^3\Sigma_u^+$) make a distinct contribution to the source spectrum of red balls, while the second positive band of N_2($2PN_2$, electronic transition $C^3\Pi_u \rightarrow B^3\Pi_g$) and

the first negative band of N_2^+ ($1NN_2^+$, electronic transition $B^2\Sigma_u^+ \rightarrow X^2\Sigma_g^+$) play a minor role since they are caused by high-energy electrons—both bands are responsible for blue-colored balls around the central white ball in HL (Wescott, Sentman, Heavner, Hallinan, Hampton, & Osborne, 1996).

Relativistic runaway electron avalanche (RREA) is an avalanche growth of a population of relativistic electrons driven through a material (typically air) by an electric field. RREA has been hypothesized to be related to lightning initiation (Gurevich & Zybin, 2005), terrestrial gamma-ray flashes (Dwyer & Smith, 2005), and red sprites (Lehtinen, Bell, & Inan, 1999), and it is unique that it can occur at electric fields an order of magnitude lower than the dielectric strength of the material. When an electric field is applied to a material, free electrons will drift slowly through the material as described by electron mobility. For low-energy electrons, faster drift implies higher friction, so the drift speed tends to stabilize. For electrons with energy above about 1 keV, however, higher speeds imply lower friction. An electron with a sufficiently high energy, therefore, may be accelerated by an electric field to even higher and higher energies, encountering less and less friction as it accelerates. Such an electron is described as a "runaway." Free electrons with the density of 4×10^6 to 1×10^7 electrons m^{-3} s^{-1} are generated by cosmic rays and natural radiation due to atmospheric radioactivity (Paiva & Taft, 2011). The electric field generated by the rock fracture or stress induces charges on the ground that accelerate these electrons which ionize or excite N_2 and O_2 molecules in the air, forming the electron avalanche by RREA. Indentation fracture of moist andesite (under wet conditions) can produce a net negative charge density of about 0.6 C m^{-3} s^{-1}. Typical occurrence altitude of the HL phenomenon is generally very low (a few tens of meters over the treetops), and the vast majority of the lights were reported to be below the tops of mountains (Bjorn, 2007). Mountainous soil has a mean dielectric constant (permittivity) $\varepsilon_s \sim 5$ (Saveskie, 2000). Thus, the electric potential in the air on the failure will be $\Delta V = q/4\pi\ \varepsilon_s\varepsilon_0 = 2.2$ GV. Let us calculate the electron number produced by the runaway electron avalanche (RREA) mechanism in HL phenomenon. The runaway electron avalanche multiplication factor is given by Dwyer (2003, 2007):

$$N_{RE} = \exp\left(\frac{\Delta V - 2.13\times10^6 I}{7.3\times10^6}\right) \tag{4}$$

where ΔV is the potential difference of the avalanche region in volts and I is the column depth of the avalanche region in g/cm^2. In the case of HL altitude occurrence, atmospheric depth will be $I = 10^3$ g/cm^2 (Bacioiu, 2011). The number of electrons in the final path of the RREA avalanche will be N_{RE} = exp (9.5) =

10^4 electrons. This value is 10^{12} times lower than that responsible for terrestrial gamma ray flashes in high altitudes on thunderclouds (Paiva, Pavão, & Bastos, 2009). Finally, high luminosity white balls around a central white ball are formed by a fragmentation process of Hessdalen lights–like dusty plasmas (Paiva & Taft, 2011). According to Teodorani (2004), sudden appearance of satellite light balls around a common nucleus can be related to the re-minimization of the effective surface energy (with the formation of new condensation nuclei) predicted by Turner's ball lightning model (Turner, 2003). However, several chemical species from rocks, such as scandium and silicon ions, were detected in the HL spectrum, suggesting dust from the valley (Bjorn, 2007). Thus, HL can sometimes assume the dusty plasma structure (Paiva & Taft, 2010). In this case, fragmentation of HL in a cluster of light balls can be produced by the interaction of low-frequency electromagnetic waves through the dust-acoustic waves. It is known that laboratory dusty plasmas are longitudinally fragmented by dust-acoustic waves (Barkan, Merlino, & D'Angelo, 1995). Similarly, video images show linear fragmentation of atmospheric light balls (UFO Hessdalen Norway, 2010, Amazing REAL looking UFO Sightings in INDIA, 2008). Dusty acoustic waves (DAW) is a complete analog to the common ionic-acoustic wave, where the dust particles take the role of the ions and ions and electrons take the role of the electrons (Thompson, Barkan, D'Angelo, & Merlino, 1997), and is an extremely low-velocity normal mode of a three-component dusty plasma comprising electrons, ions, and massive micrometer-size charged dust grains.

Conclusion

The appearance of "satellite spheres," presenting different colors and intensities, composing a cluster around a main nuclear region, is produced by the interaction between high-energy electrons and very low frequency electromagnetic waves and atmospheric steady plasmas. A very strong infrared signature even when HL is very faint or invisible in the optical range is produced by the dissociative recombination of N_2^+ in excited neutral atoms (i.e. with high kinetic energy) in regions where the concentration ratio of atomic to molecular ions in the plasma is $>l0^4$ (Biondi, 1969). Probably other related characteristics of HL can be explained by the high-energy electrons (accelerated by the RREA avalanche mechanism) accelerated in the atmosphere by electric fields from fractured rocks, or by the interaction between very low frequencies (from ground) and ions or charged dust particles in the atmospheric plasmas. Several chemical species from rocks, such as scandium and silicon ions, were detected in the HL spectrum (Bjorn, 2007) suggesting dust from the valley. Thus, HL can sometimes assume the dusty plasma structure (Paiva & Taft, 2010). The spectrum of the Hessdalen light phenomenon appears to be a continuum with

no resolved lines (Teodorani, 2004). In the three-dimensional analysis of the intensity distribution of the lights, it appeared that the radiant power is due to a heated substance. Nevertheless, the light phenomenon, in both a photometric and spectroscopic sense, does not have the characteristics typical of a classic plasma of free electrons and ions (Teodorani & Nobili, 2002). When the atmospheric transparency was low, which was most of the time, and when the orbs were low over the horizon, the intensity distribution (ID) profile was very similar to that of an image of a heated, glowing plasma, i.e. a Gaussian shape with exponential wings. When the atmosphere was clear, with no fog, the ID profile of the image was nearly flat on top with steep sides such as when luminous point-like objects (e.g., stars) are observed through thick atmospheric layers. Probably this is due to the effect of optical thickness on the bremsstrahlung spectrum which is produced by electrons. At low frequencies, self-absorption modifies the spectrum to follow the Raleigh-Jeans part of the blackbody curve. This spectrum is typical of dense ionized gas. Additionally, the spectrum produced in the thermal bremsstrahlung process is flat up to a cutoff frequency, and falls off exponentially at higher frequencies. This sequence of events forms the typical spectrum of HL phenomenon when the atmosphere is clear, with no fog. One other possibility is that this typical spectrum is an effect due to solid particles (dust grains) immersed in a hot plasma. Unfortunately, the spectrum of laboratory dusty plasma still has not been obtained.

Finally, two stationary particles of the same electrical charge will repel each other, but two particles of the same electrical charge moving in parallel will develop a force of attraction. This can be the key to plasma confinement in nuclear fusion reactors. If atomic nuclei can be squeezed together by the positive charged mass flow, without the need for random collisions in superhot plasmas, then fusion engines could be designed to produce electricity directly by pulsing the fuel into the mass flow that compresses itself until fusion is reached. The extra energy from fusion will cause the mass flow to accelerate, and bind itself even tighter, releasing its energy as electromagnetic fields or energetic electrons until the fuel pulse is exhausted. In this way, the fusion ignition temperature could possibly be attained. Furthermore, helium lines have been detected in the HL spectrum (Bjorn, 2007). This can be strong evidence for cold nuclear fusion in these atmospheric plasmas since this chemical element is a product of nuclear fusion between deuterium atoms in nuclear fusion reactors. The currently accepted theory of special relativity (SRT) doesn't suggest that atomic nuclei can be squeezed together by positive charged mass flow. In SRT, mass velocities do not exceed light speed, so magnetic forces bringing nuclei together do not exceed Coulomb forces keeping them apart. If it nevertheless happens, that is exciting evidence that SRT is not entirely right. A number of other mechanisms can be suggested, without violating the

SRT principle. For example, quantum tunneling between deuterium nuclei can occur in HL phenomenon. In this process a particle passes through a potential barrier that it classically could not surmount. Winter is the season of Hessdalen Lights, when water is abundant. About one in every 6,000 water molecules contains deuterium atoms. On the other side, experiments on board the MIR orbital station (1991), the ISS (International Space Station) (2002), and the Kolibri-2000 satellite (2002) at an altitude of 400 km detected neutron bursts (one signature of nuclear fusion) in the equator regions connected with lightning discharges (Paiva, 2009). Whether these neutrons are thermonuclear in origin or are generated by photonuclear processes, this remains to be experimentally determined. In the case of HL, the mechanism responsible for helium emissions needs to be elucidated. Another possibility is that the helium comes from inside rocks. Fractured rocks can liberate helium ions which emit light when they recapture electrons in the atmosphere. In fact, there are rocks (for example, uraninite) that release helium from the natural decay of uranium.

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COMMENTARY

On Elephants and Matters Epistemological: Reply to Etzel Cardeña's Guest Editorial "On Wolverines and Epistemological Totalitarianism"

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The Guest Editorial *On Wolverines and Epistemological Totalitarianism* by Etzel Cardeña (*JSE 24*(3), Fall 2011) is little more than a rant, in which invective, ridicule, and mockery take the place of reasoned argumentation. Mind you, there's nothing wrong with a good rant, especially when one agrees with the overall perspective, and I actually found myself in agreement with much of what the author had to say. Most of Cardeña's anger is directed at those Materialist philosophers and psychologists who happily pontificate against the possibility of psi while remaining studiously ignorant of the data that parapsychological research has uncovered. I think everyone who comes to parapsychology with an open mind at some point experiences the same frustration that Cardeña expresses toward Materialist idealogues, whose conclusions and opinions have been formed a priori and appear to be impervious to empirical data. But he seems equally upset with those who are "pro-psi," lamenting

> the epistemological absolutism that pervades both the strident anti-psi and pro-psi proponents from what I consider a healthy abeyance from fully committing to a closed position in science or in other aspects of life. (Cardeña, p. 539)

Cardeña singles me out as a pro-psi proponent (a charge which I proudly acknowledge), then proceeds to misrepresent my views, quote me out of context, and hold up what he has quoted for ridicule and contempt. In thinking about whether and how I should respond, I believe I have come across, not a wolverine, but rather an elephant, an epistemological elephant, in our parapsychological living room. The main purpose of this Reply is to bring attention to this elephant, in the hope that it will lead to fruitful discussion across differing epistemic perspectives.

In what follows, I shall use the expression "the data" to refer to empirical data collected by parapsychologists and survival researchers over the past 130 years. The term "evidence" is a relational term, and is used in conjunction with a specific hypothesis for which the data are alleged to be evidence. The specific hypothesis for which the data are taken to be evidence in this case can be and has been formulated in many different ways: (i) Materialism is false, (ii) consciousness is not produced by the brain, (iii) the mind can acquire information that is not mediated by the body's sensory channels, (iv) the consciousness that constitutes our self continues after the death of the body. Although we may quibble over this, I take these to be roughly equivalent formulations of the same underlying hypothesis. Perhaps we can agree to use William James' formulation: The brain is a transmitter, not a producer, of consciousness.

The epistemological question here is how strong is the data as evidence for our hypothesis, or, to shorten it, how good is the evidence? From a logical perspective, there are three possible points of view, corresponding to the logical quantifiers: (1) no, (2) some, and (3) all.

(1) The data does not constitute any evidence (against Materialism) at all. This is the perspective of Materialist ideologues, who usually reach this conclusion without examining the data, then project their conclusion onto whatever data, if any, they examine. This is what frustrates Cardeña the most, and I agree.

(2) The collective data constitute some evidence against Materialism, but it is hardly conclusive, and much more research is needed. I believe that this is the epistemological perspective of most practicing parapsychologists, including Cardeña.

But some scientists and philosophers, who have studied the data, have concluded that (3) the data as they now stands is sufficiently strong to conclude that James' hypothesis is correct. More data are of course always welcome, but the data already obtained is evidentially sufficiently strong to assert that Materialism is false.

Now for decades our efforts have been rightly directed against the deniers (1) . . . those who deny that the data constitute any evidence against Materialism. That is, those who belong to the (2)nd and (3)rd epistemological perspectives have been so united in our efforts against the Materialist ideologues, that we have perhaps failed to notice the major epistemological differences among ourselves. This is the elephant in our living room.

Cardeña's off-the-wall ridicule of me began to make some sense to me when I tried to see how things look to someone who is committed to the (2)nd epistemological perspective. In the next two paragraphs I will try to examine how each perspective looks from the vantage point of the other perspective.

So let us suppose, as Cardeña does, that we believe the (2)nd perspective

to be the correct one. We believe that the evidence is sufficiently strong to justify further research, but not sufficiently strong to assert that Materialism has already been falsified by science. What must we now say about someone who belongs to the (3)rd perspective, who believes that the evidence as it now stands is sufficiently strong to declare, as a finding of science, that James is correct in his belief that consciousness is not produced by the brain. Well, according to this epistemological position the data are only "suggestive," but by no means conclusive. So anyone who has concluded that Materialism is false, that the brain does not produce consciousness, etc., cannot have reached this conclusion on the basis of scientific data and reasoning alone. Something must be added to the data to reach this conclusion. What is this something? What else but the usual suspects: wishful thinking, sloppy reasoning, dogmatic suppression of alternative theories, and so forth. Cardeña compares those who believe that the evidence is conclusive with the "person in a New Age fair trading in everything from magical rocks to mysterious odors" (p. 539). And this is how it must seem from within this epistemological perspective. Cardeña cannot even acknowledge that this (3)rd perspective exists, and lumps those of us who have concluded that the evidence warrants our strong conclusion together with starry-eyed crystal gazers. Speaking of epistemological totalitarianism. Wow!

Now, fair is fair, and it is about time someone tried to describe how the (2)nd epistemological perspective looks from the vantage point of the (3)rd perspective. We have perhaps a psychological advantage, in that most of us . . . those of us in the (3)rd perspective . . . have come to that perspective by way of the (2)nd perspective, so we know what that perspective feels like. But from the point of view of this (3)rd perspective, it seems that those in the (2)nd perspective are just sitting on the fence, are excessively fond of hair-splitting, can't see the forest for the trees, are not familiar with all the relevant data, or have emotional issues (such as fear of ridicule from dogmatists in the first two epistemological perspectives).

In referring to Cardeña as a dogmatist, I am in a way accusing him of espousing the very epistemological totalitarianism that he rails against. For he takes the (2)nd epistemological perspective, his own, to be absolute. In the passage quoted above he advocates "a healthy abeyance from fully committing to a closed position in science." This appears to be an open-minded statement about always being open to alternative hypotheses and new ideas in science. But this, as an epistemological rule, precludes that science could ever reach a conclusion about anything. Science has, as a matter of fact, arrived at a "closed position" about many things that at one time were open questions: Does Cardeña recommend a "healthy abeyance" from "fully committing" to such things as (i) global warming, (ii) cigarette smoke causing cancer, (iii) the heliocentric theory, and (iv) the age of the Earth. I can readily imagine a fundamentalist agreeing with

Cardeña, that we should maintain a "healthy abeyance from fully committing to a closed position in science," and that Creationist theories should be taught in our schools along with geology. The truth of the matter is that sometimes science does reach a conclusion, in which case it is unscientific to keep sitting on the fence, always demanding more and more evidence, and then, just like our Materialist friends, moving the goalpost whenever such evidence seems to be forthcoming.

I would now like to examine a specific passage in which Cardeña quotes me out of context, then seriously distorts and misrepresents my actual views. Here is the passage:

> On the other side, we have the milder contempt of Grossman stating that whoever holds a Materialist perspective is not "a responsible investigator" and is dogmatic and "irrational." He also stated that those who succeed academically do so not on the grounds of "talent, but mostly on competition, self-promotion, and so forth." He also implies that anyone disagreeing with his conclusion has not accepted the primacy of love. (Cardeña, p. 544)

Cardeña's last sentence here is so outrageous that I will not dignify it with a reply. But let's take a look at the first sentence. All of Cardeña's quotes from my work are taken from a Foreword I wrote to Chris Carter's book, *Science and the Near-Death Experience*. In the Foreword, I had quoted the following passage from Kelly, Kelly, Crabtree, Gauld, Grosso, & Greyson (2007:421):

> . . . the central challenge of NDEs (Near-Death Experiences) lies in asking how these complex states of consciousness, including vivid mentation, sensory perception, and memory, can occur under conditions in which current neurophysiologic models of the production of mind by brain deem such states impossible. This conflict between current neuroscientific orthodoxy and the occurrence of NDEs under conditions of general anesthesia and/or cardiac arrest is head-on, profound, and inescapable. In our opinion, no future scientific or philosophic discussion of the mind–brain problem can be fully responsible, intellectually, without taking these challenging data into account. (Grossman, 2010)

The relevant word in this quote is "responsible." In my Foreword, I expressed agreement with Kelly et al. that it is not responsible for a philosopher or psychologist to discuss the mind/brain problem while being studiously ignorant of the data from parapsychology, especially the near-death experience. Here is what I wrote:

> Given that there is a large body of empirical data that (i) is highly relevant to this question and (ii) has convinced virtually everyone that has taken the time to examine it that Materialism cannot explain it, I find myself agreeing with

> Kelly, Grosso, and Greyson that it is intellectually irresponsible for a philosopher or psychologist to be ignorant of this data. (Grossman, 2010:xii)

Those who have read Cardeña's Guest Editorial will know that this sentence is in complete agreement with everything Cardeña has to say regarding our Materialist colleagues who refuse to look at the data. Yet when I say it, I am expressing "contempt."

Perhaps I crossed a line here by using the word *love*, and perhaps it was the use of this word that, in Cardeña's mind, triggered an association with New Age Fluff. But my Foreword was to a book on the near-death experience, and the concept of unconditional love plays an indispensable role to everyone who has had an NDE. It is well-documented that one of the main difficulties, perhaps "the" main difficulty that NDErs have, is in returning to a world that is not organized around the principles of unconditional love that they experience in their NDE. This "unconditional love" business is something that those of us in the (3)rd epistemological category are obliged to take very seriously. If we are convinced that the NDE is real (this does not apply to the inhabitants of the (2)nd epistemological category), and if we are concerned to understand the nature of this consciousness that we now know is a fundamental existent, and if we wish to remain empirical in our undertakings, then it is incumbent upon us to seriously examine the testimony of those who have experienced consciousness in itself, independent of the body: mystics and NDErs. They all speak to the issue of Love, and validate Ken Ring's suggestion that the Golden Rule is how we are supposed to live our lives.

> This forces one to think about the meaning of the Golden Rule in an entirely new way. Most of us are accustomed to regard it mainly as a precept for moral action. . . . But in the light of these life review commentaries, the Golden Rule is much more than that—*it is actually the way it works.* Familiar exhortations, such as "Love your Brother as Yourself" from this point of view are understood to mean that in the life review, you *are* your brother you have been urged to love. And this is no mere intellectual conviction or even a religious credo—*it is an undeniable fact of your lived experience*. (Ring, 1998:161–162)

And in a passage cited approvingly by two famous parapsychologists writing 110 years apart, psychiatrist Richard Bucke, describing his mystical experience, states

> I did not merely come to believe, but I saw that the universe is not composed of dead matter, but is, on the contrary, a living Presence; I became conscious in myself of eternal life. . . . I saw that all men are immortal; that the cosmic order is such that without any peradventure all things work together for the good of each and all; that the foundation principle of the world, of all the worlds, is what we call love. . . . (James, 1994:435; also Tart, 2010:330)

So I think it is incumbent on those of us who are in what I called the (3)rd epistemological category to follow the argument where it leads, to take Love seriously, and to apply these "Lessons from the Light" to both our personal lives and the institutions of our culture. If, as Bucke states, Love is the foundation principle of the world, then there can be no theoretical understanding of the nature of consciousness that does not involve the concept of love. My suggestion here is that the social and cultural forces that make it difficult for an NDEr to return are the same cultural forces that make it difficult to do this research in a university setting. I understand that it is not proper form to use the four-letter word *love* in an academic context. But in times of major paradigm change, and this is such a time, everything should be open for question. Why is it the case that talking about love in an academic context is taboo? What might be the vested interests that are threatened by such talk? This is a conversation that we must have, at some point.

Let me close with a little story. Perhaps the most egregious example of what Cardeña calls "epistemological totalitarianism" was committed by philosopher Robert Almeder. In the first chapter of his book *Death and Personal Survival*, he spends sixty pages or so examining some of the stronger cases of the reincarnational type (CORT), including every alternative hypothesis that has been put forth to explain the data. He concludes by saying that, given the evidence, "it is unreasonable to reject belief in reincarnation." This is very blunt language. Almeder is saying that anyone who examines the evidence and denies that reincarnation is the case is behaving unreasonably. Even I thought that this was a bit over the top when I first read it. And it must be very offensive to those in the (2)nd epistemological perspective, and perhaps even to those in the (3)rd perspective who do not believe that reincarnation is the case. But instead of ranting about "epistemological totalitarianism," one could do what some of my bright undergraduates did in a seminar I taught recently. They took Almeder's statement not as criticism but as a challenge. They went deeply into the cases, read books and articles by Tucker and Stevenson, and went over Almeder's argumentation in detail. In the end, they could find no fault with Almeder's argumentation. And when one cannot find anything wrong with an argument . . . the premises are true and the reasoning is sound . . . then it is not reasonable to reject the conclusion. My students saw this. And they also saw that when one has an argument that one cannot refute, but one is still not able or willing to accept the conclusion, this is the point where nonrational factors (such as ridicule, intimidation, name-calling, denial) enter the discussion.

Thus, to consider a few more examples and mention a few more names, the scientists van Lommel (*Consciousness beyond Life*), Tart (*The End of Materialism*), Kelly (*Irreducible Mind*), Radin (*The Conscious Universe*), and Tucker (*Life before Life*) announce the conclusions they have come to through

the titles of their books. Their conclusions are, respectively, that consciousness continues after the death of the body, that Materialism has been falsified by science, that the human mind is not reducible to the brain, that the Universe is conscious, and that consciousness exists prior to birth. These scientists did not arrive at their conclusions by attending "New Age Fairs" or overdosing on "*The Secret* type of New Age theories" (Cardeña, p. 548). No. Their conclusions were arrived at only after a meticulous and exhaustive examination of all the relevant empirical data, together with a detailed analysis and refutation of all alternative hypotheses. If Cardeña does not like their conclusions, then I invite him, and others who feel as he does, to accept the challenge, as my students did, to go deeply into the subject matter, and to examine their arguments, and tell us where they are mistaken in their reasoning. And if you cannot find any errors in their argumentation, as I could not, then is it not incumbent on you, as a scientist and rational human being, to embrace their conclusion that "consciousness can exist independent of the brain and that Materialism is therefore empirically false" (Grossman, cited disapprovingly in Cardeña, p. 541)?

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RESPONSE

Response to Neal Grossman's Reply "On Elephants and Matters Epistemological"

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In his long Reply, Professor Grossman opines that my Guest Editorial is "little more than a rant." Although rant can be interpreted as a "wild revel," even without telepathic means I strongly doubt that that is what he had in mind. Contrary to what the reader might conclude from his statements, my essay was not at all an attack against the probable reality of psi phenomena, something that I have supported repeatedly, nor a defense of Materialism. Instead, it criticized epistemological totalitarianisms that endorse absolute and simplistic certainties regarding psi or other issues and consider any disagreement with their positions or interpretations, no matter how large or small, as lacking in reasonableness, love, or whatever. As to the charge that I misrepresented Professor Grossman, I provided quotations along with their source, so the reader can judge whether I was fair or not. But more telling, I believe, is that Professor Grossman's letter exemplifies the problem I was describing better than I could in a limited space. Therefore I will cede the last word to, in my view, the brightest mind we have had in parapsychology, William James. He held that psi phenomena were real, but instead of assuming that he completely or finally understood this issue (or any other topic), he challenged us to develop "the habit of always seeing an alternative" (James, 1896:4).

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HISTORICAL PERSPECTIVE

Ernesto Bozzano: An Italian Spiritualist and Psychical Researcher

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Abstract—Ernesto Bozzano (1862–1943) was a luminary in psychical and spiritualistic studies in Italy, to which he contributed numerous publications that were also distributed abroad, and for more than forty years he tirelessly defended "human survival after death" against its critics. This article, after a brief look at the studies dedicated to him, furnishes a profile of the life and thinking of Bozzano, paying particular attention to the events that brought him to the eyes of the international community of psychical researchers.

Keywords: Ernesto Bozzano—Italian psychical research—spiritualism—survival research

Introduction

Ernesto Bozzano (1862–1943) was probably the most important Italian representative of psychical and spiritualistic studies before the 1940s, as well as one of the few to emerge on the international scene, thanks to his numerous publications which gained him the esteem of scientists, philosophers, and psychical researchers. He was at the center of an intense network of correspondence with Italian, European, and American intellectuals, receiving an average of 200 letters a month, and was furthermore one of the few Italian scholars to have been named an honorary member of the Society for Psychical Research (SPR), the American Society for Psychical Research (ASPR), and the *Institut Métapsychique International* (IMI). Despite the fact that the academic historian Bruno Di Porto (1933) wrote a description of him for the *Dizionario Biografico degli Italiani* (Di Porto),[1] an element which testifies to his relevance, Bozzano is completely unknown in Italy to those who do not deal with the history of psychical research. However, this did not prevent Italian scholars from continuing to remember him (e.g., Biondi, 1988, Inardi & Ianuzzo, 1981, Macaluso, 1972, Orlandi, 1971) or his works from continuing to be reprinted, even recently, in journals or by specialized publishers (e.g., Bozzano, 1948a,

1957a, 1967a, 1972, 1975, 1982, 1998a, 2001, 2008). Still cited in France (e.g., Clauzure, 1983, Dumas, 1973), Bozzano does not appear in some important historical accounts of Anglo-Saxon psychical research (e.g., Beloff, 1993, Inglis, 1977) but does appear in the bibliographies of various authors of the same linguistic group (e.g., Stevenson, 1977, Van de Castle, 1977). Only beginning in 1982–1983, coinciding with the fortieth anniversary of his death, did some historians of psychical research begin to study him in more depth (Biondi, 1984, Iannuzzo, 1982, 1983a, 1983b, Ravaldini, 1983). From this renewed interest, there were only two critical volumes regarding Bozzano taken as a whole (Iannuzzo, 1983b, Ravaldini, 1993a), and some more recent articles that examine specific aspects of his biography or works (Alvarado, 1986, 1989, 2003, 2005, 2006, 2008, Biondi, 2010, Caratelli, 1998, Cellina, 1993, Cugnaschi, 2002).

In addition, there are the first biographical articles written by his disciple Gastone De Boni (1908–1986) (the main ones being De Boni, 1941, 1946, 1947). There are also some autobiographical articles written by Bozzano in his old age, which are usable with reservations (Bozzano, 1924c, 1930a, 1938a, 1939). For a more in-depth study, it is however indispensable to refer to unpublished primary sources, among which the correspondence between Bozzano and De Boni is indispensable for reconstructing his life.[2]

The present article has, as its objective, a brief presentation of the life, works, and thinking of Ernesto Bozzano.

Biographical Profile

Ernesto Bozzano was born in Genoa on January 9, 1862, the fourth son of a lower-middle-class Genoese family, but we know very little of his childhood, in reality of his entire life up to 1928; by his own admission, that part of his life was without relevant biographical events, so much so that the only information conserved regards his intellectual life. He had an early vocation for study for which he received no support since, when he was fourteen years old, he was taken out of school in order to begin a commercial career. Other than this brief experience of work in his youth and a similar brief journalistic collaboration with the Genoese daily newspaper *Il Secolo XIX* around 1893, of which there is no trace, he never needed to work. Since he lived with his brother Vittorio (1860–?), and was in part economically supported by his well-to-do brother Adolfo (1859–?) and probably had a small income sufficient to maintain a secluded lifestyle, Bozzano was able to dedicate all his time to studying and writing. In fact, he managed to study by himself, dedicating himself first to poetry and literature, then to the sciences, and finally to philosophy, his great passion. He became a supporter of positivism and a fervent follower of the British philosopher Herbert Spencer (1820–1903) who, in the 1880s, was

his habitual correspondent together with William James (1842–1910), Alfred Russel Wallace (1823–1913), Théodule Ribot (1839–1916), Jean-Marie Guyau (1854–1888), and Henri Bergson (1859–1941) (Letter from Savona, 7 May 1941, unpublished, in Bozzano & De Boni, 1930–1943).

The biographical and autobiographical articles present Bozzano's interest in spiritualism and psychical research as a philosophical and rational conversion from materialism (a total negation of phenomena) to demolition of his misoneism and, finally, to spiritualism (e.g., De Boni, 1941:14 and on), but numerous other data do not correspond to these facts. On the basis of Iannuzzo's studies (1983b) and mine, it is much more probable that Bozzano was already inclined toward spiritualism in 1890 and that his presumed conversion, completed in 1893, was, in reality, reached after three years of psychical and spiritualistic readings, but, above all, after three years of going to a mediumistic Genoese club whose culminating event had been the apparition of his dead mother in a seance in July 1893.

Independently of the problem of this conversion, what is certain is that, until 1943, Bozzano never stopped dedicating himself to psychical research, or *metapsichica* (*métapsychique* or metapsychical research) as it was called in continental Europe, understood as a single science capable of demonstrating the existence of the spirit and its survival after bodily death as opposed to religion, which was too dogmatic, and official science, which was too materialistic. Bozzano's methodological approach was specific and comparable to that of an eighteenth-century naturalist; he did not carry out experiments or directly gather testimony because he did not feel the necessity of proving the existence of the psychic, but he summarized the accounts of the phenomena present in the literature (which he patiently classified during his entire life) as immediately valid natural phenomena, and he inserted them into an inductive process based on comparative analysis and convergence of proofs; in other words, he pointed out all the likenesses of a certain class of phenomena and reached specific conclusions. Therefore, his monographs were created in order to

> collect an adequate number of events . . ., carefully selected from the point of view of their authenticity as facts, in order to then classify, analyze and compare them, and deduce the laws which govern them." (Bozzano, 1972:228)

The monograph *Dei Fenomeni di "Bilocazione"* (*The Phenomena of "Bilocation"*) (Bozzano, 1934) can furnish a concrete cross-section of his procedural method. In this monograph, Bozzano held that the phenomena of bilocation "assume decisive importance for the experimental demonstration of the existence and survival of the human spirit" (Bozzano, 1934:7) since it would prove that, in the somatic body, an etheric body exists, capable of making oneself autonomous, often carrying along with itself consciousness,

memory, identity, and its own other supernormal faculties, thus ending up with independence of the spirit from the body and subordination of the brain to the mind. He reached this conclusion after having analyzed four categories of phenomena. The first included the cases of sensitivity in amputees and hemiplegics; discarding the neurological explanations, Bozzano believed he was dealing with the initial levels of bilocation. In the second, we find the cases in which a subject would see his own double; although accepting the possible pathological explanation with reserve, for Bozzano these cases represented, as a general rule, the second level of incipient bilocation, the transition between being both inside and outside one's own body. In the third group, he placed all the cases in which consciousness would be completely transferred to the etheric double which verified the sensation of seeing reality from a position external to one's own physical body. Finally, in the fourth group, there were the cases in which the etheric double was seen by one or more people. Considering all the categories cumulatively and taking the cases of this last group as crucial proof of their intersubjectivity, Bozzano thought he had scientifically deduced some conclusions from some facts that were evident for him but which were often not "self-evident facts" but rather an "interpretation of the cases he considered" (Alvarado, 2005:228). In any case, in order to understand his methods, a quotation from his work can be useful:

> As soon as the processes of comparative analysis are applied to hundreds of similar episodes in which all the gradations which employ this phenomenology are represented, [there can no longer exist any doubts regarding the] objectivity of the phenomenon itself; in the sense that the "dreamlike" and "hallucinatory" hypotheses must be excluded and they are also the only ones which are opposed to the phenomena. (Bozzano, 1934:124)

If, for Bozzano, the phenomena of bilocation represented the passage from animism to spiritism, the phenomena of transcendental music dealt with in the monograph *Musica Trascendentale* (*Transcendental Music*) (Bozzano, 1982) were instead of clear spiritual genesis. In this monograph, he subdivided the phenomena into six classes, even if, dealing with animistic phenomena, he dedicated less space to the first two (musical mediumship and transcendental music with telepathic externalization). The classes of transcendental music of haunting origin, of music perceived without a relationship to events dealing with death, music at the deathbed, and music which is manifested after an event dealing with death would instead be, cumulatively taken, reliable testimony of spiritual intervention. In fact, from the moment that musical phenomenology was often externalized together with apparitions of the dead person at the deathbed, in such a way as to prove the spiritual identification of a dear deceased person who comes to assist the dying person, because of the numerous cases in which music

Ernesto Bozzano, at about forty years old.

was heard by everyone present and, sometimes, by all of them except the dying person, and as a result of the cases in which the music was heard on fixed dates after the death of someone, Bozzano was certain that, thanks to his demonstrative process, the hallucinatory and psychometric explanations as well as suggestion and telepathy among the living could be discarded in favor of the spiritual explanation of the phenomenon. Also in the case of transcendental music, the gradual exposition of the phenomenology permitted Bozzano to exclude the antagonistic hypotheses step by step and to guide the reader, by means of the convergence of the proofs, to understand how "the numerous branches of metaphysics (. . .) all converge as a center toward the experimental demonstration of the existence and survival of the human spirit" (Bozzano, 1982:156–157).

Bozzano, therefore, understood science as a process of researching truth, carried out rigorously starting from the facts, and capable of rationally demonstrating conclusions using comparative analysis and the convergence of proofs. It is understood that, on the basis of this assumption, *metapsichica* also, at least as he saw it, was to all effects a science, and that Bozzano the spiritualist and Bozzano the psychical researcher were one and the same.

In 1899, in Genoa, Bozzano and the writer Luigi Arnaldo Vassallo (1852–1906) founded the Circolo Scientifico Minerva (Minerva Science Club), which had as its aim the scientific study of mediumistic phenomena and the promotion of debates and publications regarding them (Minerva, 1899). Many people of the Genoese middle class joined, including without a doubt Francesco Porro (1861–1937) the astronomer and Enrico Morselli (1852–1929), the celebrated psychiatrist who, for his honest and impartial attitude regarding research, compared this Club to a small SPR (Morselli, 1908:vol. I, 174). The principal activity of this Club in 1901–1902 was to study the medium Eusapia Palladino (1854–1918), which led to various publications by its members (the most important were Bozzano, 1903, 1904, Morselli, 1908, Vassallo, 1992). In 1904, the Club was dissolved as a result of disagreement among the members.

Since, from 1890 on, Bozzano followed the developments of his discipline

with attention (he dedicated notable energy to constant reading, writing, analysis, and critiques of numerous publications), many of his writings took the form of critical reviews of his colleagues' texts, many of whom were also friends, thus generating small disputes, always carried out with logical strictness and politeness. Their common characteristic was that of wanting to nip in the bud the theories contrary to the hypothesis of human survival. Among the most relevant debates, we recall that which took place with Morselli and went on from 1899 to 1917; since Morselli denied the spiritual hypothesis, Bozzano opposed Morselli regarding the phenomena of the etheric body, of apport, of identification of spiritual personalities and reincarnation, challenging him many times to deny that, from these facts, the existence of a soul surviving a body and the intervention of the deceased was able to be deduced (Gasperini, 2010). Nobel Prize winner and psychical researcher Charles Richet (1850–1935), who, by upholding the hypothesis of the *cryptesthésie* (cryptesthesia), a faculty of superior cognition but at the human level, was not, according to Bozzano, able to explain many undoubtedly spiritual facts, such as the phenomena of telekinesis at the deathbed, those of haunting, those of transcendental music or the cases of identification of deceased people unknown to the medium and those present at the seances (Bozzano, 1922a, 1922b, 1922c, Richet, 1922a, 1922b, 1922c)[3]. There was also biologist and psychical researcher William Mackenzie (1877–1970), advocate of the polypsychical hypothesis used to explain the mediumistic personalities without resorting to spiritualism; for Bozzano, the fact that, notwithstanding the constant changing of participants at the seances, the mediumistic personalities maintained their own identity was more than sufficient to hold that the opposing thesis was destroyed (Bozzano, 1923a, 1923b, Mackenzie, 1923a, 1923b). And psychical researcher René Sudre (1880–1968) explained the phenomena of intelligent mediumship with the *prosopopèse-métagnomie*, that is attributing them to the latent subconscious personality in the mind of the medium, capable of producing anomalous facts. Elaborating on the theories of Richet, Bozzano disproved the hypothesis of the *prosopopèse* using the same reasoning advanced against *cryptesthésie* (Bozzano, 1926, Sudre, 1926).

The majority of the debates were carried out in the journal *Luce e Ombra* (*Lo*), which from 1900 on became the principal publication of Italian spiritualism (Alvarado, Biondi, & Kramer, 2006). From 1906 to 1939 when *Lo* was made to close by the fascist regime, Bozzano was the principal contributor to the journal with his contribution of almost 4,000 pages, thanks to which he acquired considerable notoriety, above all abroad; starting in 1920, he also regularly published articles in the most important English, French, American, and South American spiritualistic and psychical journals. In Italy, he published approximately 90 volumes that, starting from 1920, were translated into nine

different languages, such as English, French, German, and Turkish (Alvarado, 1986, De Boni, 1941, Ravaldini, 2000) and reviewed critically in the most important psychical and spiritualistic journals. Although agreeing that Bozzano was one of the greatest scholars in the field of psychical research, the reviewers often criticized his methodology, e.g., Troubridge (1919) accused him of accepting too quickly the reality of the phenomenon whose existence he had to demonstrate; Wilson (1933) repeated the same argument, while Saltmarsh (1938) judged his reasoning with regard to the biological evolution and the independence of the spirit from the body to be completely erroneous. Collins (1939) criticized him for assuming as proven some facts that could be defined as arbitrary, and, in *Nature*, the spiritistic hypothesis of Bozzano was defined to be of scarce interest for the skeptics but merited in-depth study for the enthusiasts of psychical studies (Review of *Discarnate Influence in Human Life*, 1938). In France, Quartier (1927a, 1927b) described Bozzano as a sage of the pre-scientific era because of his non-use of the experimental method, and even Count Cesar Baudi de Vesme (1862–1938) criticized his esteemed colleague, so certain of the reality of psychical phenomena and the explanatory value of the spiritistic hypothesis that he never doubted it, for omitting, among other things, in an unfair manner, topics that were contrary (de Vesme, 1934, 1936).

From 1927 to 1929, Bozzano was called on to cover the role of expert in noted mediumistic experiences, such as the seances of Millesimo; these were a series of seances held in the ancient castle of Millesimo, a picturesque town not far from Savona (Italy), and presided over by Marquis Carlo Centurione Scotto (1862–1937), Senator and medium, during which numerous direct voice and apport phenomena were supposedly verified, culminating in the presumed dematerialization and successive materialization of the Marquis himself, occurring on July 29, 1928 (Ferraro, 1989). Thanks to accounts published in *Lo* by Bozzano (Bozzano, 1927b, 1927c, 1928a, 1928b, 1928c, 1928d), which were then collected into one volume (Bozzano, 1929b) and disseminated abroad also thanks to his collaborators (e.g., Bozzano, 1928e, 1929d, 1929e, 1930e, Hack, 1930), the seances obtained vast visibility, throwing the international research community into turmoil. Officially, he upheld the veracity of the happenings and the unquestionable intervention of the disembodied entities,[4] drawing upon himself first the criticism of psychical researcher Rudolph Lambert (1866–1964) (Bozzano, 1929a, 1929c, 1930b, 1930d, Lambert, 1929, 1930) and then that of the exponent of the SPR, Theodore Besterman (1904–1976) (Besterman, 1930). The answer to the English scholar was given for him by an indignant Sir Arthur Conan Doyle (1859–1930), who dismayed by the attacks that his Italian friend was undergoing resigned from his role as honorary member of the SPR (Doyle, 1930), taking with him 77 members (Mauskopf & McVaugh, 1980:28).

The seances at Millesimo, together with those with Palladino at the *Circolo*

Scientifico Minerva about thirty years earlier (Bozzano, 1903), were the only two experiments worth noting in the course of his research, which was otherwise carried out almost exclusively in written analyses of books and articles. Space does not permit the chronicling of the eloquent and detailed accounts of the seances that Bozzano published; however, according to those who have written about it, it seems that the scholar went to the seances, on both occasions, already profoundly convinced of the reality of the facts to which he would have attested, and of the authenticity of the mediums. This can be deduced by his lack of doubts and from his critical tone against the skeptics (e.g., Bozzano, 1903:362–363) as well as from the clearly demonstrative presentation of the events narrated, not simple events to explain to readers and to assess critically, but compelling results of experimentation demonstrating the intervention of disincarnate intelligence. Our affirmation is also supported by the weight Bozzano gave to the evidence of the facts and, in particular, to psychological control: To whoever contested the lack of verification of the mediums at the seances of Millesimo, he responded that their psychological profile, namely being aristocrats, cultured, and rich, meant therefore automatically that they were not interested in committing fraud, as well as the clear evidence for paranormal phenomenology could not but render clearly truthful all his accounts and exempt him from subjecting the mediums to humiliating anti-fraud verification (e.g., Bozzano, 1929a). Let us be clear, we do not want to insinuate anything nefarious regarding the Genoese psychical researcher and his honesty, but simply to demonstrate how his behavior could seem suspect, or at least naïve, to many of his contemporaries.

Thanks to the wide dissemination of his writings, Bozzano managed to begin a correspondence and friendship not only with Conan Doyle but also with many other psychical researchers, scientists, and philosophers of that era, such as William Crookes (1832–1919), Henry Sidgwick (1838–1900), Oliver Lodge (1851–1940), Camille Flammarion (1842–1925), and James Hyslop (1854–1920), with the Italian philosopher and psychologist Angelo Brofferio (1846–1894), with Italian psychiatrists Enrico Morselli and Cesare Lombroso (1835–1909), and also with many others (Letter from Genoa, 6 October 1942, unpublished, in Bozzano & De Boni, 1930–1943). But he also received a lot of letters from non-scholars who were greatly consoled by him and the doctrines he proposed. Unfortunately, almost all his correspondence has been lost (Ravaldini, 1993a:73–76).

But, as the Gospel says: *nemo propheta in patria* [never a prophet in his own country]. In fact, in Italy, the works of Bozzano were not very well-known outside the circle of readers of *Lo* or its staff; of these, positive comments on his works came, other than from De Boni (e.g., De Boni, 1946, 1947), from two important Italian psychical researchers, Emilio Servadio (1904–1995), who

extolled the argumentative force of Bozzano (Servadio 1931, 1934), and from Antonio Bruers (1880–1954), who was certainly more cautious regarding the Genoese scholar in accepting the evidence of some alleged spiritualistic facts (Bruers, 1929, 1930). The situation greatly improved when De Boni made an agreement with the publisher L'Albero of Verona (Italy) and began publishing the *opera omnia* of Bozzano[5]; the first volume *Popoli Primitivi e Manifestazioni Supernormali* (*Primitive Cultures and Supernormal Manifestations*) in 1941 (Bozzano, 1941)[6] was a big success and attracted the attention of a goodly number of Italian intellectuals, above all anthropologists and religious and oriental historians such as Giuseppe Tucci (1894–1984), Raffaele Pettazzoni (1883–1959), and Ernesto de Martino (1908–1965), who critiqued it in newpapers and specialized journals (e.g., de Martino, 1941, Gasperini, 2011) and began a brief correspondence with Bozzano (unpublished correspondence). Outside of Italy, an enthusiastic reader of *Popoli Primitivi* was Carl Gustav Jung (1875–1961) (De Boni, 1949).

Bozzano died on June 24, 1943, from circulatory complications, and his death was mentioned in Italian and French spiritualistic and psychical journals (De Boni, 1947, Necrologie, 1946, Weissenbach, 1949).

Outline of the Metapsychical Philosophy

Between 1922 and 1943, Bozzano produced and updated his most important monographs which, together with his articles, permitted the reconstruction of the central points of his thinking and the ordering of them into a scheme, something which he had never formally done.[7] As Iannuzzo (1982) also noted, that which seemed to emerge when studying Bozzano's works, was the attempt that he made, probably based on the body of Spencerian philosophy, to create a "metapsychical philosophy" capable of interpreting and coherently connecting paranormal phenomena, above all considering the demonstration of human survival, the topic which interested him primarily, but also secondarily deriving some notions of metaphysical and cosmological order of the general guiding hypotheses with which to return and compare the psychic phenomena in order to justify and organize them in a wider perspective, namely that of the spiritual evolution of the universe (Cugnaschi, 2002). Indeed, the latter subject rarely emerges from his writings and certainly does not distinguish itself for originality from a conceptual point of view, but it is equally an unpublished mixture of Spencerian philosophical tenets and paranormal phenomena assumed to be empirical data.

At the root of his metapsychical philosophy, Bozzano posed his "Spiritistic hypothesis" which was not immediately synonomous with spiritualism, but rather a criterium of the interpretation of paranormal phenomena which only later on pointed to the veracity of spiritualism. With this hypothesis, he maintained

that paranormal, physical, and intellectual phenomena were products and proof of the existence of a spirit as well as an active and immaterial principle independent of the body, and which due to these characteristics cannot help but survive them inasmuch as the spirit is incarnate by life to certain phenomena (telepathy, clairvoyance, telekinesis, etc.) which Bozzano called "Animistic" (or psychobiodynamic) and since the spirit is disembodied from life in the other categories of phenomena (communication with the dead, apparitions at the deathbed, transcendental music, etc.) which he called "Spiritistic" (or transcendental). As Bozzano wrote in a work expressly dedicated to this subject,

> supernormal phenomena (. . .) are the effects of a single cause and it is the human spirit which, when it is manifested fleetingly during the incarnate existence, determines Animistic phenomena, and when manifested under conditions of disembodiment in the world of the living it determines Spiritistic phenomena. (Bozzano, 1967a:295)[8]

All in all, the spiritic hypothesis is quite simple; the vast amount of the writings and subjects that he produced to support it is, if anything, magnificent and is Bozzano's real contribution to psychical research. Above all, the discourse on the autonomy of "subconscious supernormal faculties," namely the faculties of the incarnate spirit which is positioned in the subconscious, is, to all effects, the reservoir for psychic phenomena. For example, the fact that telepathy and clairvoyance emerged on rare occasions of severe physical and psychical weakening, as the mediums and people in the state of mesmeric sleep demonstrated, for Bozzano meant that they were completely useless in this life since the potential senses of the incarnate spirit would become real only when this spirit would have passed through the crisis of death (Bozzano, 1899, 1924d). For Bozzano, another strong proof in favor of the autonomy of the bodily spirit was the evident independence of supernormal faculties from the laws of natural selection, based on their uselessness in the struggle for life (Bozzano, 1923c). Bozzano resolved the mind–body problem utilizing the concept of the etheric body which envelops the incarnate spirit linking it to the body but is also capable of breaking off, bringing with it the individual consciousness and the integral subconscious memory contained in the etheric brain, the true seat of thinking for which the somatic brain only serves as an interpreter of physical sensations (Bozzano, 1930c, 1931).[9]

From the preceding considerations, together with the conviction that the spirit is also present in animals, with them having the same subconscious faculties as man (Bozzano, 1975), and the belief in the faculty of thought and willingness to mould the subject which, according to Bozzano, appeared to emerge from the phenomena of ideoplasty and ectoplasmy (Bozzano, 1967b, 1967c), Bozzano deduced a cosmological theory reconstructable from some

writings that went beyond pure psychical research. In summary, he believed that the universe was in continuous evolution and that the true evolutive motor was the spirit which had to pass through all the inferior animal forms until reaching man in order to finally reach a perfect state of existence in the spiritual sphere (Bozzano, 1967b); the evolution of the individual spirits falls within the end of the evolution of the great universal spirit, God or Absolute or Unknowable, which is both intelligence tinged with material reality and the material itself, constructed and supported, thanks to its most fundamental material expressions: Force, Motion, Energy, and Ether (Bozzano, 1924b). For Bozzano, this form of idealistic and evolutionist pantheism was not only, as was said, a working hypothesis used as a theoretical framework but also, together with all that happened after the birth of spiritualism, a new form of rational religious revelation not antagonistic to Christian thinking (Bozzano, 1927a).

Although implied in all his writings, the theme of survival after bodily death is the specific object of the controversial monograph *La Crisi della Morte (The Crisis of Death*) in which Bozzano attempted to demonstrate human survival, speaking about the environment and the conditions of spiritual life using transcendental communications, namely all that copious information received directly from the dead by direct writing or mediumistic dictation right from the beginning of the spiritualistic movement (Bozzano, 1998b). Bozzano believed that communication is an instrument indispensible for proving the reality of disembodied entities since these can furnish information regarding their own identity and, vice versa, information regarding their identity proves that communication was taking place with a dead person. We are dealing with very slippery ground and even Bozzano (1996) understood this; exactly for this reason, he stated a list of proofs in favor of the spiritualistic hypothesis he held to be unattackable by countertheories: the existence of subconscious supernormal faculties free of biological evolution, time, and space; bilocation; apparition of the dead at the deathbed; premonitions of accidental death; cross-correspondence between mediumistic communications received by mediums who are not together and cannot communicate; apparitions of the dead.

Implicitly, this list was an invitation to globally consider paranormal phenomena since only globally would they have furnished an incontestable demonstration of the spiritualistic hypothesis. Bozzano was loyal to his own intention since, with his monographs, he collected all paranormal phenomenology, thus constructing the empiric base and the construction of indirect proof (psychic phenomena) and direct proof (spiritualistic phenomena) he required. For example, among the most important monographs dedicated to psychic phenomena, we find the one dealing with the phenomena of bilocation (Bozzano, 1934),[10] that dealing with clairvoyance (Bozzano, 1947a), that dealing with telepathy (Bozzano, 1946a), and the triptych dealing

with premonition in which he also espoused his solution to the ancient determinism–free will dilemma (Bozzano, 1947b, 1947c, 1948a). Regarding the spiritualistic phenomena, he dealt with telekinesis phenomena in relation to the crisis of death which he attributed to the participation of the dead (Bozzano, 1948b),[11] experiences of an auditory nature at the deathbed as well as cases of trancendental music (Bozzano, 1982), the apparition of the dead at the deathbed (Bozzano, 1947b), haunting (Bozzano, 1936), and also polyglot mediumistic phenomena (Bozzano, 1946b). At the end of each monograph, he stated with certainty that had reached logical and necessary conclusions regarding the existence of supernormal faculties independent of the strict dictates of time, space, and natural selection with regard to the existence of disembodied spirits capable of interfering in the daily events of the living.

The thirst for proof brought Bozzano to also devote himself to phenomena which took place at other times and places; he wrote to reaffirm the veracity of the events of some precursors of the spiritualistic movement (Bozzano, 1957a, 1957b, 2001), to demonstrate the hand of the spirits in the composition of the supposed works dictated psychographically or produced by direct writing (Bozzano, 1998a), and he concluded with the grandiose attempt of complete recognition of the paranormal on the part of primitive cultures (Bozzano, 1941).

Conclusion

Bozzano was deeply convinced of his Spiritistic hypothesis and therefore spent 50 years of his life collecting his immense paranormal record of cases in order to demonstrate them scientifically, so that no one could any longer voice doubts about them. He built a solid reputation as a psychical researcher, but it is evident that the image which has survived is that of a spiritualist (Fodor, 1933:36). If we keep this in mind, together with the fact that, since the 1930s, parapsychology has moved on different tracks from those of Bozzano and has, above all, become a discipline conducted in the English language (e.g., Mauskopf & McVaugh, 1980), and that in Italy this area of study is languishing, we can form a rough idea of why the conclusions of Bozzano have not recently been taken into consideration.

In effect, the methods of Bozzano, which Iannuzzo (1983a) defined as observational and naturalistic and which we can also call bibliographic, must have seemed rather simplistic to the parapsychologists of the experimental school. In fact, he not only made exclusive use of qualitative sources but refused to adopt the experimental method, believing that it was not worthwhile (Ravaldini, 1993b:129), and, as was contested so many times (e.g., Di Porto, no year, Inardi & Iannuzzo, 1981), by taking the facts reported in the literature as immediately valid, he ended up assuming an uncritical attitude toward his own sources.

Nevertheless, from a historical point of view, he symbolically epitomized the interest of his time and place for spiritualism and psychical phenomena, and to study him permits, if nothing else, a more in-depth reconstruction of the Italian situation, in general suffering from a historiographic void on this subject. From a more straightforward parapsychological point of view, some attempts have been made to recover the case records and conclusions set forth by Bozzano in his monographs, which could still be rich in suggestions and ideas for research (e.g., Alvarado, 2005, 2008, Biondi, 2010).

Notes

[1] The *Dizionario Biografico degli Italiani* (acronym: DBI) is a work of highly respected scientific value edited by the Institute of the Italian Encyclopedia, begun in 1925 and still not completed; its aim is to gather approximately 40,000 biographies having rich bibliographies and edited by scholars, of as many illustrious Italians. Paper publication has been interrupted, and it is now possible to consult it online at http://www.treccani.it/Portale/ricerche/searchBiografie.html

[2] His collection of letters is conserved at the Bozzano–De Boni Library Foundation in Bologna (Italy) and includes 450 letters, of which 275 are unpublished. Those from 1928 to 1936 have been published (De Boni, 1974, 1975, 1976, 1977, 1978), and while those from 1930 to 1936 have been published, with, however, some letters missing, the bulk of the unpublished correspondence is from 1937 to 1943. In addition to the published writings of Bozzano, approximately 150 binders of additional unpublished material are conserved at the Foundation among which much other correspondence and various collections of notes, citations, drafts of articles, and monographs never published and even manuscripts of some of his most important publications are found. The Foundation, which also publishes the journal *Luce e Ombra* and is one of the most important Italian libraries of the history of spiritualism and psychical research, is the only association which is involved in keeping the memory of Bozzano alive. For further information, see http://www.bibliotecabozzanodeboni.it

[3] Richet and Bozzano shared a professional friendship as is seen by their letters which have been conserved. Thanks to these letters, we know that the French scholar frequently sent his own publications dedicated to Bozzano, and many times he proposed publishing a collection of all Bozzano's works at his own expense; furthermore, initially antispiritualistic, Richet attributed his attraction to the spiritualistic hypothesis to reading Bozzano's works. See Bozzano (1924a) and unpublished letters: Richet–Bozzano, Paris, 14 May 1935, Paris, 31 May 1935, Paris, 28 June 1935.

[4] Utilizing the unpublished material of the Bozzano–De Boni collection of letters, Biondi (2009) strongly questioned the honesty of Bozzano and De Boni regarding the events at Millesimo, quoting, in particular, their involvement in keeping secret the Centurione Scotto fraud in the seance on July 29, 1928, and that of George Valiantine in successive seances with the group of Millesimo, whose accounts have never been published.

[5] Although De Boni (1941), with regard to the complete edition of Bozzano's writings, indicated 15,000 pages in more than 50 volumes, in the end he chose 17 writings,

those more theoretically important which had been updated with new material by Bozzano between 1939 and 1943 (De Boni, 1946). Until the 1970s, De Boni did his best to enable everybody to see the light, relying on various Italian publishing houses, also including *Editrice Luce e Ombra* which was refounded by him in 1967.

[6] The majority of Bozzano's books are monographs, specific studies on a category of metapsychical phenomena, but *Popoli Primitivi* is one of the few writings to veer away from his usual theme; with this, he attempted to shed light on the entire paranormal phenomena of the cultures which the anthropology of that era defined as primitive, principally African peoples, but also including Indians, Aborigines, and Maori, in order to find the occidental case records (generated not by mediums but by yogin, shamans, and medicine men) and the solution to the question of the origin of religions (the observation of spiritualistic facts).

[7] The only vaguely systematic work, written in 1938 (the definitive and quite different edition was published in 1967), and for this reason definable as the clear synthesis of his 50 years of work, is *Animismo o Spiritismo? Quale dei Due Spiega il Complesso dei Fatti?* (Bozzano, 1967a). The 1938 work was also translated into English as *Discarnate Influence in Human Life* (Bozzano, 1938b).

[8] For the formulation of this hypothesis, Bozzano was greatly inspired by the work of Aksakof (1890) which he read in French in 1895 (Aksakof, 1895). There, in fact, we find (I consulted the Italian translation: Aksakof, 1912) the complete formulation of many of the points of Bozzano's metapsychical philosophy, such as the distinction between animistic and spiritistic phenomena, the theory according to which animistic phenomena reside in the subconscious and are proof of the existence of an immortal spirit free of the body, and also the importance of spiritualistic communication with cases of identification of the deceased as the main proof in favor of survival.

[9] For a more in-depth study of Bozzano (1931), also in relation to the most recent reports of a *life review* in parapsychology events, see Biondi, 2010.

[10] In the category of the phenomena of bilocation, Bozzano inserted a class of facts corresponding to today's out-of-body experiences, which, although not being classified as such and explanations of which resorted to hypotheses of the ethereal body and survival, had been studied for a long time. For a close examination of this history which also deals with Bozzano from the nineteenth century until the 1980s, see Alvarado, 1989. For a specific study of Bozzano and the phenomena of bilocation, see Alvarado, 2005.

[11] For a historical summary of these and other phenomena linked to near-death experiences, see Alvarado, 2006. For a specific study of this monograph of Bozzano, see Alvarado, 2008.

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OBITUARY

In Memory of William Corliss

William R. Corliss, regarded by many as the world's greatest contemporary anomalist, passed away at his home in Glen Arm, Maryland, on July 8, 2011, at the age of 84. During a span of some 40 years, the physicist turned stalker of paradoxical data brought to light a mind-boggling collection of unexplained observations, embarrassing deviations, and paradigm-shattering discoveries that orthodox science had largely swept under the carpet of consensus. In recognition of these contributions, he received, in 1994, the Tim Dinsdale Award presented by the Society for Scientific Exploration.

Corliss was born on August 28, 1926, in Stamford, Connecticut, and served in the Navy during World War II. After receiving degrees in physics from Rensselaer Polytechnic Institute (BS) and the University of Colorado (MS), he worked for more than a decade as a physicist in industry, first with Pratt and Whitney Aircraft, then with General Electric Company, and finally with the Martin Company where he was Director of Advanced Programs in their Nuclear Division. In 1963 he began another career, in technical writing, and produced works for NASA and the National Science Foundation on such topics as electric power generation, computers, space radiation, robotics, and telecommunications.

With an interest in "outlaw science" that had been sparked by the reading of a controversial book on geology in 1951, Corliss turned to writing about scientific anomalies in 1974, an endeavor he christened The Sourcebook Project. In the decades that followed, he conducted a massive amount of library research, poring through many thousands of scientific journals and gleaning from them a wide assortment of neglected data in the fields of geology, biology, archeology, astronomy, psychology, and geophysics. He first reprinted the accounts he found in a series of six ring-bound volumes, followed by six massive hardback volumes he called "handbooks." But by 1982 he had switched to a hardback catalog format that not only presented examples of various anomalies and their sources, but also gave an evaluation of the quality of data—and an evaluation of an anomaly's possible impact on science, from being a mere curiosity to being "revolutionary," by which he meant that the anomaly could not even be explained by a modification of present scientific laws.

The evaluations were necessarily subjective. He admitted that it was difficult to categorize and organize the unknown, and always pointed out that

the material he chose to include in his anomaly catalogs reflected what—in his opinion—was not well-explained, as "anomalousness is often in the eyes of the beholder." Not all the anomalies he highlighted presented a threat to mainstream science. Some are mere blemishes. Others are leaks, cracks, and fissures in the foundations and facades of the various sciences. But there are potholes as well, the potential game changers. "Instead of simply accepting nice, slick theories like evolution, relativity, and continental drift," he said in 1980, "I think we should occasionally reexamine them to be sure they are not accepted just because they are so slick. And based upon the material I've collected, what I'm saying is: I'm not so sure." Among the major paradigms widely considered to be fact that his catalogs of anomalies put at risk are: the expanding universe; the Big Bang origin of the universe; Neo-Darwinism, specifically evolution via random mutation and natural selection; plate tectonics and continental drift; Special and General Relativity; and the assumption that genomes are the complete blueprint for life forms.

Corliss made no claims of completeness. Indeed he would constantly point out that he had covered just a fraction of the literature on a subject. In 2005, he wrote that his 40 published volumes detailing more than 2,000 scientific anomalies and "provocative" phenomena represented just 50% of his database. And even after decades of work, only a handful of English-language journals had received his serious attention. "The journals in other languages, government reports, conference papers, publications of research facilities, proceedings of state academies of science, and an immense reservoir of pertinent books," he noted, "remain almost untapped." The task he faced was daunting: "The anomalies residing in the world's literature seem infinite in number."

But he never lost his enthusiasm, and one has to admire his courage in single-handedly attempting a project of such enormous scope. His catalogs are unique in the annals of science, in that he cataloged not what is known but what is not known. "It seems to me that any organized activity like science would have done this a long time ago," he said. "It is at least as important to realize what is not known as it is to recognize the well-explained."

Though Corliss has often been compared to a modern-day Charles Fort, their differences are considerable. Unlike Fort, he avoided using newspapers as the source of his data whenever possible, preferring instead to depend on academically accredited journals that described anomalies that were the product of scientific observation, research, and exploration. Furthermore, Corliss, unlike Fort, was not anti-science and he did not editorialize. He thought the data were damning enough on their own. "In the Catalog of Anomalies," he wrote, "the data rule; all theories and hypotheses are held to be tentative. The history of science proves that this is a wise policy." Corliss saw anomalies as a way to renew, to reinvigorate, science.

Though his first volume of anomalies, entitled *Strange Phenomena*, was actually recommended by both *Nature* and *Science*, quite often the publication of his catalogs met with disbelief, even disdain. The critics claimed that the data must be in error, that the data is anecdotal, that it was too old, that a supposed anomaly was explained long ago. His reply? "The baseline of well-established theories, against which anomalousness is measured, is always shifting and some data, indeed, are bad. But for every anomaly or example that can be legitimately demolished, ten more take its place. Nature is very anomalous or, equivalently, Nature is not yet well-understood by science." Such words did not endear him to the scientific mainstream, which largely ignored much of his later work.

Corliss did not have any illusions about the impact The Sourcebook Project would have on science. Would it revolutionize science? "Probably not—at least not immediately," he wrote. The late sociologist Marcello Truzzi called Corliss "an unsung hero of science."

I was introduced to The Sourcebook Project in the late 1970s, when I received my very first published volume of anomalies from the mail-order service he operated with his wife, Virginia. (Most volumes are still available from The Sourcebook Project, P.O. Box 107, Glen Arm MD 21057. See also: http://www.science-frontiers.com.) Shortly afterward, I met and interviewed him for an article I was writing on his work for *Science Digest.* We kept in touch over the years, and I would occasionally send him a newsclipping for the newsletter he published called *Science Frontiers*. After being involved in producing a couple of science exhibits for museums, I began to think that his work should have a wider audience, that there should be a William Corliss Museum of Anomalies or at least an exhibit for museums based on his work, called What Science Doesn't Know. I can't imagine anything more stimulating to the minds of young people than to discover areas of science that are up for grabs, puzzling topics they could explore, wide open fields of research where they could make a difference, instead of being presented with science as a closed book of knowledge, as at most science museums. The work of William Corliss is an inspiration, a wonder-filled refutation that we have not come to the end of science. Quite the contrary. As he would often say, "Much remains to be done."

Patrick Huyghe

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LETTER TO THE EDITOR

Pipefish or Pipe Dream?

This Letter is a reply to the research article "A Baby Sea-Serpent No More: Reinterpreting Hagelund's Juvenile 'Cadborosaur' Report," in *JSE* 25:3, Fall 2011 (Woodley, Naish, & McCormick, 2011). Naish and colleagues indulge in the common home-quarterbacking habit of insisting that anything described as different must be an erroneous description of something found in a book that vaguely looks like it. A comparison of Hagelund's "baby-Caddy" with a pipefish (Figure 1) shows significant differences in the latter:

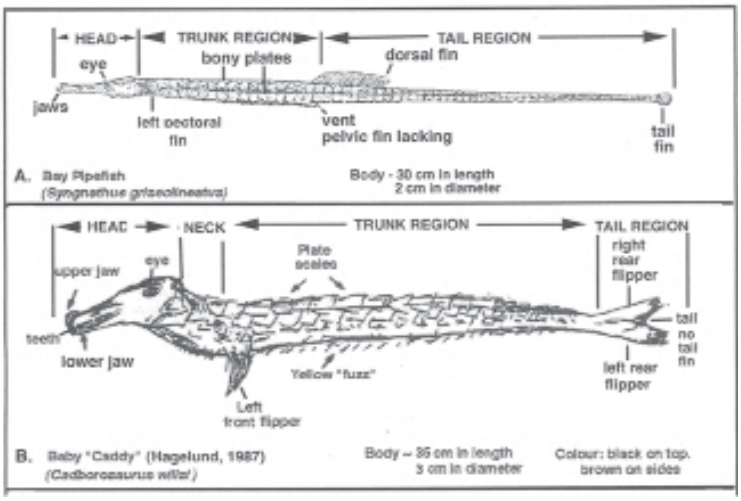

Figure 1. A comparison of the pipefish (*Syngnathus griseolineatus*) (A) with Hagelund's "baby Caddy" (B). (Hagelund, 1987; Leblond & Bousfield, 1995)

1 – no neck;
2 – nearly fused jaws;
3 – no posterior paired appendages (pelvic fins);
4 – pronounced dorsal fin;
5 – very elongate post-vent tail region (> 1/2 body length);
6 – lateral plates encircling the body and tail region (vs. only dorso laterally on trunk region in Hagelund's creature);
7 – presence of a small and vertically oriented fin.

Observed behavioral differences are equally great. The pipefish doesn't swim with its head continuously out of the water for more than a few seconds, not the estimated 5–10 minutes observed originally by Hagelund, and the pipefish cannot open its tiny jaws (*Syn-gnathus*!), whereas the baby Caddy opened its large jaws and displayed a mouthful of conspicuous teeth.

So much for the pipefish idea. As to whether Caddy eyewitness accounts relate to one or more unknown creatures: "*entia non sunt multiplicanda praeter necessitatem"* to quote William of Occam; and while there may be a remote chance that more than one single unconfirmed animal species may be hiding under the appellation *Cadborosaurus,* there is not enough evidence to introduce specific distinctions at this time.

We appreciate the interest expressed in Cadborosaurus and encourage commentators to look closely at the published evidence before jumping to conclusions.

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ESSAY REVIEW

Miracles and Modern Spiritualism: A Re-Review

On Miracles and Modern Spiritualism. Three Essays by Alfred Russel Wallace. London: James Burns, 1875. 236 pp. Free PDF at http://books.google.com/books?id=7ZU0AAAAMAAJ&pg=PR1#v=onepage&q=&f=false [Reprinted by General Books, 2010, $21.09, ISBN 9780217265287]

About 1875, Modern Spiritualism in Britain attained a peak of cultural influence. In January 1874 William Crookes published "Notes of an Enquiry into the Phenomena called Spiritual during the Years 1870–1873" in his *Quarterly Journal of Science,* reaching positive conclusions and postulating the existence of a psychic force.

Another scientist was also reporting on his investigations. Alfred Russel Wallace, co-discoverer of evolution with Darwin, contributed "A Defence of Modern Spiritualism" to *The Fortnightly Review* in May 1874.

James Burns, a Spiritualist editor, publisher, and bookseller, gathered together Crookes's writings on Spiritualism, to form a volume *Research into the Phenomena of Spiritualism* (1874) which was to be very influential. In March 1875, Burns enjoyed another coup when he issued *On Miracles and Modern Spiritualism. Three Essays* by Alfred Russel Wallace (*OMMS*). Although this was a much more comprehensive work than Crookes', it has had less impact, perhaps because it did not include the kind of laboratory experiments which Crookes reported with the medium D. D. Home.

Nevertheless, *OMMS* was often reprinted, and has merits which still commend it to us today, which outweigh its disjointed origin. Wallace wrote clearly, and had a command of the literature, a background in science, and personal experience of the phenomena.

The first essay, "An Answer to the Arguments of Hume, Lecky and others against Miracles" (28 pages), had been read before the Dialectical Society in 1871. This London organization has largely vanished from history, except for its resolution on January 26, 1869, "to investigate the phenomena alleged to be Spiritual Manifestations, and to report thereon."[1]

The second essay, "The Scientific Aspect of the Supernatural" (102 pages), had a much longer history. It had first appeared serially in a journal, *The Leader,*

in 1866, and then promptly became as now a scarce pamphlet. Sections had been reused in *The Spiritualist* newspaper in 1871. It is worth noting that Wallace not only habitually reissued material, he also revised it. To the 1875 version of the essay, Wallace added "Notes of Personal Evidence." The 1866 essay may be seen as an argument for taking Spiritualism seriously.

Finally the paper "A Defence of Modern Spiritualism" (92 pages, including an appendix of the author's replies) had appeared in *The Fortnightly Review* in 1874 and been reprinted as far afield as Boston (USA) and Dunedin (New Zealand) before being revised for this book.

The content of the essays varied. The first essay was mainly philosophical and logical. The second was the nearest to a general survey of Spiritualism, including a section on its teaching. The third was chiefly a literature review.

In the first essay, Wallace discusses the philosopher David Hume whose definitions of Miracles had been very influential, and might be said to rule out the study of paranormal phenomena in advance.[2] Hume had argued that a uniform experience amounted to a proof that miracles did not happen, but Wallace gives many examples of testimony to miraculous events, by no means limited to Modern Spiritualism. Wallace also exposes the limitations to scientific rejection a priori, without investigation, of psychic evidence, with many examples of scientific error (e.g., "Sir Humphry Davy laughed at the idea of London ever being lighted with gas").

Wallace also takes issue with his contemporary, the historian William Lecky, who had attacked the belief in miracles, and had suggested that belief in the supernatural existed only when men were destitute of the critical spirit and when the notion of uniform law was as yet unborn. Wallace pointed to Joseph Glanvil as a critical mind who defended the supernatural.[3]

A third thinker whom Wallace challenges is the anthropologist Edward Tylor who had asserted that psychic beliefs were an example of the survival of savage thought.[4] Many modern people, Wallace noted, can testify to the phenomena which cause such beliefs.

Wallace concludes this first essay by repeating that he seeks only to clear the ground of arguments supposed to disprove miracles and Spiritualism without examination. This he has done effectively, and in a clear way that the general reader can follow.

In the second essay, "The Scientific Aspect of the Supernatural," Wallace again makes some general points in defense of the miraculous, such as our limited knowledge of the laws of nature, and the possibility of ethereal intelligences of whose existence we are generally unaware. He calls attention to the good quality of the witnesses who testify to modern miraculous phenomena, and he claims:

> during the eighteen years which have passed since the revival of a belief in the supernatural in America, not one single individual has carefully investigated the subject without accepting the reality of the phenomena, and while thousands have been converted to the belief not one adherent has been converted back from it. (p. 49)

This may be doubted, although uncertainty about how much investigation would be called "careful" offers a loophole. Certainly Dr. Carpenter, with whom Wallace had a long combat, was less than careful in his investigations, as Wallace was able to show in a number of rejoinders.[5]

This may be a suitable moment to recommend to all students of Wallace and of the history of psychical investigation the website conducted by Dr. Charles Smith known as the Alfred Russel Wallace page, which contains a vast amount of information about Wallace, including original writings: http://people.wku.edu/charles.smith/index1.htm

Others would question how much care Wallace himself took in his seance work. Moreover, it was one thing to accept the reality of the phenomena and another to be convinced of human spirit return, as Spiritualists believed. Preeminently, William Crookes was not so convinced at that time.

Wallace devotes a number of sections in the essay to citing eminent witnesses to various phenomena, including od-force, animal magnetism, clairvoyance, apparitions, and poltergeists, making particular use of Robert Dale Owen's *Footfalls*.[6]

He calls attention to the Cideville, France, disturbances of 1850–1851,[7] and to "the remarkable resemblance of the phenomena to those which had occurred a short time previously in America [i.e. Hydesville rappings], but had not in 1850 become much known in Europe." He also compares them with the Epworth Parsonage case in the Wesley family, which was often recalled in the early days of Modern Spiritualism.

Wallace then moves on to quote witnesses to Spiritualist phenomena, choosing Augustus De Morgan, Robert Hare, and Judge Edmonds from science, though the first was primarily a mathematician and the last was a lawyer.

For literary and professional men, he selects T. Adophus Trollope, James Gully, M.D., Col. Wilbraham, S. C. Hall, Nassau William Senior, Rev. William Kerr, Thackeray, Lord Lyndhurst, Archbishop Whateley, Dr. Elliotson, Captain Burton, and Professor Challis, who as an astronomer ought really to be in the previous chapter. But it may be said of nearly all these witnesses that neither their powers of observation nor their recordkeeping would meet the standards which would soon be sought by the Society for Psychical Research, founded in 1882.

Wallace then turns to a general discussion of the theory of Spiritualism, and the hypothesis of the existence of spirits. He discusses the possibility of continuity after death, and "the agency of beings of a like mental nature to ourselves—who are, in fact, ourselves—but one step advanced on the long journey through eternity" (p. 103). This continuity was a central theme in Wallace's outlook for the rest of his life.

In the next section, "The Moral Teachings of Spiritualism," he summarizes in a sympathetic manner the philosophy of Spiritualism: "There are no bad spirits but the spirits of bad men, and even the worst are surely if slowly progressing," he notes (p. 109). Wallace over four pages quotes from an address by the medium Mrs. Emma Hardinge [who had actually married and added the surname "Britten" in 1870] who warns of "The effects of vice and ungoverned passions" (p. 112). It seems possible that Wallace had been significantly affected by Emma's lectures; they may even have been a trigger for his 1866 essay, and caused him to consider the relationship between Evolution and Spiritualism.[8]

After some remarks on the nature of God, Wallace defends Spiritualism as having inspired thousands to devote their lives to good works.

In the 1875 edition of this essay, Wallace added "Notes of Personal Evidence." These include his early experiences in mesmerism, his participation in a home circle, his sittings with a professional medium, Mrs. Marshall, and further experiments in the home circle, at some of which Miss Nichol (later a professional medium, better known as Mrs. Guppy) was the focus of phenomena. These personal experiences had now caused him to accept both the phenomena and the philosophy.

Whatever criticism we may make of Wallace as either witness or recorder of these events, his practical experience was much in excess of some of his critics.[9]

In the third essay, "A Defence of Modern Spiritualism," Wallace first criticizes some recent treatments of the subject by popular and scientific writers, who have strong negative feelings but limited practical experience. He then gives a historical sketch of the origins of Spiritualism centered on the Misses Fox in Hydesville, New York, in 1848. Wallace here used such books as Robert Dale Owen's *Footfalls* and Emma Hardinge's *Modern American Spiritualism*. We have only recently realized that these accounts, like some other sacred narratives, had evolved away from the actual historical record.[10]

It has now been noted, for example, that the connection of the Fox sisters with the first phenomena was more limited than later reported, that the age of the sisters was uncertain, that the name of a supposed peddler who communicated was not in the original narrative, and so on. We may criticize Wallace for accepting the story as it had come down to him (Owen and Hardinge had both known the Fox sisters), but must recognize that although there were many in his

Alfred Russel Wallace

lifetime who accused the sisters of fraud, no one properly analyzed the history of the Fox story. The Spiritualists themselves did not enquire too closely.[11]

In support of the facts of Spiritualism, Wallace then gives summary accounts of several mediums and researchers. Most of these remain neither exposed nor vindicated. Even Kate Fox (p. 156 ff.) who lived in England for a time, produced some phenomena that are hard to explain. One might assume that her materializations for Livermore, the New York banker (p. 157), were exploded long ago, but they have simply been forgotten. In contrast, the literature on the medium D. D. Home continues to expand steadily without a definitive blow to either medium or critics.[12]

On the investigator side, Wallace was able to cite William Crookes' lately published work mentioned above. Here, too, a voluminous literature has left uncertainty. William Brock, in a magisterial biography, acquitted Crookes of fraud, but suggested he might have suffered from an eyesight problem.[13]

Another witness to phenomena, George Sexton, once a rationalist orator then a Spiritualist, was to become a Christian preacher for the rest of his life, though he does not appear to have repudiated his belief in mediumship.[14]

Wallace devotes a section to "Spirit-Photographs" (his use of a hyphen has not generally found favor), the supposed appearance of human spirits on photographs. Because of its physical nature, he was impressed by the evidential value of photographs, but, in retrospect, it was perhaps the most vulnerable part of his case. The most famous European photographer, Buguet, was arrested in Paris and convicted of fraud just after Wallace's book appeared.[15]

Fortunately, Wallace had used British photographic cases to make his case. But Mrs. Sidgwick exposed the limitations of these in 1891.[16]

Since then, although photographic images, most recently orbs, have continued to cause occasional puzzlement, the value of spirit photographs as survival evidence has been accepted by few.

In a section on "Historical Teachings of Spiritualism," Wallace briefly identifies mediumship in ancient Greece, the Bible, the lives of saints, and the witchcraft trails. He offers an explanation of the testimony to intercessory prayer given in *A Narrative of Some of the Lord's Dealings with George Müller, Written by Himself* (1860):

> The perfect simplicity, faith, boundless charity, and goodness of George Müller, have enlisted in his cause beings of a like nature; and his mediumistic powers have enabled them to work for him by influencing others to send him money, food, clothes, &c., all arriving, as we should say, just in the nick of time. The numerous letters he received with these gifts, describing the sudden and uncontrollable impulse the donors felt to send him a certain definite sum at a certain fixed time, such being the exact sum he was in want of, and had prayed for, strikingly illustrates the nature of the power at work. (pp. 210–211)

As in the second essay, Wallace gives a sympathetic account of the "Moral Teachings of Spiritualism." He draws attention to the consistencies of spirit teachings received in many places which are radically different from those of orthodox religion held by the medium or by the supposed communicators in life.

Finally in an appendix, Wallace crosses swords with Carpenter once more, adds a little more evidence, and responds to critics of his third paper. This ongoing discussion was characteristic of Wallace. In the *OMMS* third edition of 1901, he included, for example, chapters on apparitions and phantasms, and a new preface.

……..

This book was one of Wallace's most durable. But its main readership has been Spiritualists, rather than psychical researchers or scientists. He wrote at a time when the psychic field was still, to a large extent, organizationally and ideologically undivided, except between those who accepted the phenomena and those who did not. Within months of the book appearing, The Theosophical Society had been formed in New York, which would develop a more critical view of Spiritualism in the light of a revived occult tradition. The rise of psychical research gradually emphasized the importance of the laboratory approach, championed by Crookes, rather than the natural history of Wallace.

Notes

[1] The ensuing *Report on Spiritualism* appeared as a book—London: Longmans, Green, Reader & Dyer, 1871; reprinted (slightly shortened) London: J. Burns, 1873; reprinted, London: Arno Press, 1976.

[2] "A miracle is a violation of the laws of nature" and also "A miracle is a transgression of a law of nature by a particular volition of the deity, or by the interposition of some invisible agent." Wallace shows that both definitions are defective.

[3] Henry Sidgwick, first president of the Society for Psychical Research, read Lecky, but was then led to take more seriously the evidence for medieval marvels. Interestingly, Lecky seems to have facilitated the inclusion of a dream case in an early SPR survey *Phantasms of the Living* (1886), being acquainted with the dreamer.

[4] It is now known that Tylor was privately investigating the subject: Stocking, J., &

George, W. (1971), Animism in theory and practice: E. B. Tylor's unpublished Notes on 'Spiritualism', *Man, New Series 6*(1), 88–104.

[5] For example, Dr. Carpenter and Psychic Force, *The Spiritualist,* 15 February 1872. http://www.wku.edu/~smithch/wallace/S206.htm

[6] Robert Dale Owen, *Footfalls on the Boundary of Another World,* London: Trubner, 1860.

[7] A. Lang, The poltergeist at Cideville, *Proceedings of the Society for Psychical Research, 41*(xviii), 454–463. Cideville and Epworth remain in the case files of psychical research, still discussed but not generally accepted outside the research community.

[8] Personal communication, Charles Smith.

[9] Charles Smith comments in a personal communication to me: "I don't think it is true that Wallace was an observer only, as on some occasions he added constraints to the séance settings that were designed to eliminate fraud. This included both careful investigation of the physical layout of the settings, and adding slips of paper and such to detect forms of fraud-initiated movements of furniture, etc."

[10] The re-evaluation is first apparent in Barbara Weisberg's *Talking to the Dead, Kate and Maggie Fox and the Rise of Spiritualism,* San Francisco: Harper, 2004, and in many articles in the online journal *Psypioneer* (archived at http://www.woodlandway.org) founded in 2004.

[11] Podmore (*Modern Spiritualism,* 1902) was aware of an 1848 report by E. E. Lewis (Mysterious Noises), who had interviewed witnesses at Hydesville, but did not obtain it himself. The Lewis report was reprinted by *Psypioneer* in April 2005.

[12] A good entry point is Peter Lamont's *The First Psychic* (2005). Dr. Lamont suggests that Home's phenomena are not yet fully understood, though he is confident that they were not paranormal.

[13] *William Crookes (1832–1919) and the Commercialization of Science,* Aldershot, Hampshire, UK: Ashgate, 2008.

[14] See Timothy Larsen's *Crisis of Doubt. Honest Faith in Nineteenth-Century England,* New York: Oxford University Press, 2006, Chapter 8, George Sexton.

[15] In his recent study *Laboratories of Faith, Mesmerism, Spiritism, and Occultism in Modern France* (Ithaca/London: Cornell University Press, 2008) John Warne Monroe gives a detailed account of the Buguet case, using police records.

[16] On spirit photographs: A reply to Mr. A. R. Wallace, *Proceedings of the Society for Psychical Research,* 1891, 268–289. After the First World War, it looked for a time as if spirit photography might make a comeback, but one of its main enthusiasts, Fred Barlow, lost faith. See Fred Barlow and W. Rampling Rose, Report on an investigation into spirit-photography, *Proceedings of the Society for Psychical Research,* 1933, 121–138.

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ESSAY REVIEW

Ian Stevenson's *Twenty Cases Suggestive of Reincarnation*: An Historical Review and Assessment

Twenty Cases Suggestive of Reincarnation by Ian Stevenson. University Press of Virginia, 1980 (second edition). 396 pp. $25.93, ISBN 9780813908724.

Introduction

Twenty Cases Suggestive of Reincarnation (first published in 1966) is a classic of 20th-century parapsychology that can still be read with profit.[1] Along with *Children Who Remember Previous Lives* (2001),[2] it is an ideal introduction to Stevenson. The latter work, intended for the educated general reader, provides an overview of 40 years of research and includes capsule summaries of several cases, but *Twenty Cases* contains detailed reports that illustrate reincarnation-type cases much more fully.

The cases reported in *Twenty Cases* come from India, Ceylon (now Sri Lanka), Lebanon, Brazil, and the United States (the Tlingit Indians of Alaska). They were selected from about 200 personally investigated by Stevenson in order to show the variety of features this type of case presents. The subjects of all were young children at the time they claimed to have lived before. Collectively these twenty cases help define "cases of the reincarnation type," as Stevenson came to call them, though they vary substantially in detail.

The book includes both evidentially strong and weak cases, cases among strangers and in the same family, cases with strong behavioral features, cases with birthmarks and congenital deformities related to the previous person,[3] a case with a change of sex between the previous person and the subject, and a case in which the previous person died after the birth of the subject. The last type is extremely rare. Stevenson worked for years on a volume that was to include "anomalous date" cases, but it remained incomplete at his death in 2007 and has not been published. He also did not live to complete a planned volume on non-tribal American cases, although he analyzed a series of 79 of them in an article published in the *Journal of Nervous and Mental Disease* in 1983 (Stevenson, 1983b).

The Canadian-born Stevenson was already a tenured professor and Chair of the Department of Psychiatry at the University of Virginia Medical Center when he turned his attention to reincarnation-type cases. From 1960

on, he enjoyed the financial support of Chester Carlson, inventor of the Xerox process. Carlson endowed a Chair and Stevenson became Carlson Professor of Psychiatry in 1964. In 1967, he resigned as chairman of the Department of Psychiatry and established a Division of Parapsychology (later renamed the Division of Personality Studies)[4] within it. From then on, he devoted all of his efforts to psychical research. Carlson continued to give annual donations, and on his death in 1968 left a $1,000,000 bequest to the University of Virginia in support of the work (Stevenson, 2006).

Twenty Cases was first published in 1966 in the *Proceedings of the American Society for Psychical Research,* and reprinted with additional material that included followup information on the subjects by the University Press of Virginia in 1974 (Stevenson, 1974b). In this historical review, I describe reincarnation studies in Anglo–American psychical research before *Twenty Cases* appeared, the reception that book received, and the influence it has had. I also assess it from the vantage of current research. If nothing else, *Twenty Cases* brought a new type of spontaneous case[5] to the attention of parapsychologists and the world, although some scholars, like Almeder (1996), believe that it (together with the works that succeeded it) accomplished much more and that it would now be "irrational" to deny that reincarnation occurs.

Reincarnation in Psychical Research Before 1960

Phenomena related to survival of death were a core subject matter of parapsychology from the outset. Indeed, the Society for Psychical Research (SPR) was founded in 1882 partly to look into the claims of Spiritualism (Gauld, 1968). The earliest work centered on mediumship, apparitions, and other spontaneous cases. Investigations in these areas furnished the main empirical support for and against survival, and were debated back and forth for decades, stalemated by questions about the limits of ESP and the possibility that some form of "super-ESP"[6] could dispose of the evidence (Gauld, 1961, Hart, 1959).

In his landmark *Human Personality and Its Survival of Bodily Death*, F. W. H. Myers carefully considered two cases often discussed in the context of reincarnation. One was that of Lurancy Vennum (Stevens, 1887), who took on the personality of a dead girl, Mary Roff, for about four months, during which time she recognized people from Mary's life, but not her own (Myers, 1903, Vol. 1:360–368). Lurancy returned to herself, however, and this case is better regarded as one of possession than of reincarnation. The other case was that of Hélène Smith, the pseudonym of a trance medium who claimed to have had several previous lives, among other places in India and on Mars (Flournoy, 1900). This case was persuasive to many in French spiritualist circles, but psychologist Theodore Flournoy demonstrated how the "past-life" personas were produced by the medium's subconscious (Myers, 1903, Vol. 2:130–144).

Myers agreed and understandably concluded that "for reincarnation there is as yet no valid evidence" (1903, Vol. 2:134).[7]

Reincarnation was not a tenet of Anglo–American Spiritualism,[8] but Theosophy embraced it and promoted it heavily (Besant, 1897, Cooper, 1920, Walker, 1888).[9] Psychical research took little interest in it,[10] though a few workers did comment on it. Sir Oliver Lodge believed that individual spirits emanated from a common "larger self" and accepted pre-existence but not reincarnation in the ordinary sense (1907:85–87). James Hyslop of the American Society for Psychical Research (ASPR) was skeptical, in the absence of good evidence that previous lives could be recalled (1906, 1919). Sir William Barrett, who was not troubled by the memory problem, found the prospect attractive (1917:287–291). Hereward Carrington thought that its plausibility rested on survival of death in general being proved (1930:57).

The official Spiritualist position notwithstanding, mediumistic communicators not infrequently spoke about reincarnation, and at times asserted links to the mediums in past lives they said they had shared. Frederick Bligh Bond (1924) employed an automatist (an automatic writer) in his psychic archaeology at Glastonbury Abbey and she transmitted communications from a monk who claimed to have known both Bond and her in previous lives there. J. Arthur Hill (1929) reported on a series of automatic scripts in which communicators claimed to be successive reincarnations of a man in love with a previous incarnation of the automatist. Lady Nona, the communicator in the Rosemary case of apparent Egyptian xenoglossy[11] (Hulme & Wood, 1936, Wood, 1935), claimed to have known Rosemary, the medium, in an earlier life three thousand years before.

These cases and others like them are more properly ones of mediumship than of reincarnation, in that the mediums do not themselves claim to remember previous lives (unless we want to take the position that the communicators, rather than being independent entities, are parts of the mediums' personalities). The story of Nyria (*Soul of Nyria,* Praed,[12] 1931) is different. It was initiated in an hypnotic session with the hypnotist suggesting a return to a life in ancient Rome and continued in trances of which the subject, a young English woman, had no conscious awareness. Nyria purported to be a slave-girl and gave an account full of verified names and other period detail well beyond the normal knowledge of the subject, although the existence of Nyria herself was never confirmed.

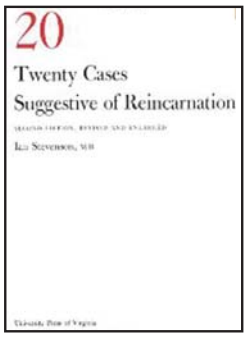
Soul of Nyria is not the only example of supposed past-life memory cast as fiction. Beginning in 1937 with the best-selling *Winged Pharaoh*, Joan Grant published a series of historical novels told in the first person which in 1956 she said were based on memories of previous lives. From an early age, Grant experienced dreams with what she believed were fragmentary memories. In

her 20s, she trained herself to access this "far memory" and began dictating, in trance, her seven novels, set in ancient Egypt, classical Greece, and contemporary Rome, Renaissance Italy, and among American Indians in pre-contact times (Grant, 1956, Kelsey & Grant, 1967).

Some have credited the impressive output of Pearl Curran writing as Patience Worth to past-life memory, although Patience herself repeatedly denied reincarnation. She identified herself as a 17th-century English woman who had emigrated to the American colonies and been killed by Indians there. She dictated through a Ouija board and then through automatic writing six long novels and an array of other literary works. Most of the novels are set in 17th-century England, but one is set in Victorian England (two hundred years after Patience is supposed to have lived) and another in Biblical-era Palestine. Like *Soul of Nyria* and the far-memory novels of Joan Grant, these works are replete with recondite period detail and language that Curran did not know or use normally (Prince, 1927, Yost, 1916), and their source remains a mystery (Braude, 2003).

Thanks to the Spiritism of Alan Kardec (1875), which like Theosophy endorsed the idea, reincarnation was taken more seriously in continental Europe than in Great Britain and the United States, and European psychical researchers took more interest in it than did their American and British counterparts. Albert de Rochas (1911) is well-known for his pioneering exploration of hypnotic age regression to previous lives, but perhaps because his book has not been translated into English it is not generally realized that the regressions compose only a small part of the presentation. It is a wide-ranging discussion of reincarnation that includes a review of spontaneous cases culled from a variety of sources, including Fielding (1898). Even less well-known is a book by de Rochas's countryman and colleague Charles Lancelin (1922), who described the important Italian child case of Alessandrina (Alexandrina) Samona, one of the earliest European cases on record. This case has the interesting feature that the rebirth was heralded in multiple ways (in dreams, séance communications, and poltergeist raps). There are striking physical and behavioral similarities with the previous person, and Alessandrina apparently recalled an incident from that person's previous life.

In 1924, R. B. S. Sunderlal reported four Indian cases in the *Revue Métapsychique*.[13] That same year, Gabriel Delanne (1924), a follower of Allan Kardec, released another general study of reincarnation. In addition to surveying spontaneous child cases, child prodigies, and déjà vu experiences, Delanne examined cases of retrocognition and reviewed cases in which rebirths had been announced in mediumistic communications. Again, because his book has not been translated into English, it has had little influence on Anglo–American psychical research. Other French Spiritists, such as Geléy (1920, 1930) and



Flammarion (1923), whose works have been translated, are better-known, but unfortunately, although they endorse reincarnation, they do not mention the many spontaneous cases documented by Rochas, Lancelin, and Delanne.

The publisher Ralph Shirley (1936) produced the first well-rounded analysis of the evidence for reincarnation in English. He discussed the strengths and weaknesses of automatic writing, hypnotic age regression, and spontaneous memories, including many of those assembled by the French writers. He briefly mentioned Shanti Devi, based on an Indian newspaper story.[14] Yeats-Brown (1936) summarized many of the same cases. The following year, Arthur Osborn (1937) treated reincarnation as an aspect of the human experience in *The Superphysical*. This book contains descriptions of several British cases, gathered in response to Osborn's own surveys. Osborn's cases are less evidential than those of Shirley and Yeats-Brown, but they give a sense of the quotidian ground from which the better cases spring.

Age regression to previous lives under hypnosis was largely a parlor game of mesmerists and amateur hypnotists before the researches of de Rochas (1911), and after him there are no significant reports until Bernstein's *The Search for Bridey Murphy* exploded into public awareness as a newspaper serial in 1954 and a best-selling book in 1956 (Bernstein, 1956). Bridey Murphy purported to be a 19th-century Irish woman, and Bernstein's book convinced many people that reincarnation had occurred. Although her account of herself included obscure details that were verified, the case was attacked on various grounds, and the public lost faith in Bridey as quickly as it had fallen for her (Ducasse, 1960). Another veridical hypnotic regression case, that of Naomi Henry (Blythe, 1956), passed almost unnoticed in the wake of the Bridey Murphy controversy, and after Zolik (1958) showed how easy it was to construct fantasies under hypnosis, psychical research all but abandoned age regression as a reliable doorway to previous lives.

The "life readings" of the psychic Edgar Cayce began as an inadvertent offshoot of his health or "physical readings" (Cerminara, 1950, Sugrue, 1942), but quickly became part of a post–world-war cult craze that continues to this day. Many of Cayce's physical readings and prescriptions were uncannily accurate, so there was a presumption of authenticity for the life readings as well. He attributed not one but a series of lives to each petitioner, with links between lives explained through various types of karma. Many readings had the same sequence of settings for the lives, such as "Atlantis, Egypt, Rome, the Crusades period, and the Early [American] Colonial period," explained

on the theory that period-cohorts tended to reincarnate together (Cerminara, 1950:43). The entranced Cayce said that he drew information partly from the subconscious of petitioners and partly from the "Akashic Records" (Cerminara, 1950:45). Although it was possible to verify information given for the more recent lives in a few instances, earlier lives were not amenable to checking, and psychical researchers discounted Cayce's readings further by pointing out that they came from a sensitive rather than from the subjects themselves.

In a well-received book published in 1953, the Australian physicist Raynor Johnson tied together psi, survival, and mystical experience in a grand portrait of human nature reminiscent of Myers. He argued for reincarnation and karma, which like other authors he considered to go hand in hand, and pointed to child prodigies, déjà vu experiences, and occasional memory claims (citing Shirley, 1936) as evidence of pre-existence and reincarnation.

Among philosophers, J. M. E. McTaggart (1906) advanced a reasoned argument for what he called the "plurality of lives," avoiding the word reincarnation, perhaps because of its occult associations. He found support in love at first sight (the lovers had known each other in earlier lives) and innate character traits, arguing at length that the self might persist through a series of lives while having memories only of the present one. James Ward (1911), in his Gifford Lectures of 1907–1910, considered reincarnation to be consonant with the economy of nature and superior to the Christian concept of bodily resurrection as a theory of immortality.

C. J. Ducasse (1948, 1951), who was well-acquainted with psychical research and served for years on the ASPR Board of Trustees, wrote at length on reincarnation or, as he termed it, transmigration. Ducasse (1951) cited the Japanese case of Katsugoro (Hearn, 1897), in which a seven-year-old boy made verified statements about a child who had died in another place several years before, as an example of a memory claim, and considered various objections to the idea of reincarnation. C. D. Broad (1958), who like Ducasse was well-acquainted with psychical research, conceptualized reincarnation in terms of his theory of Ψ-components. He regarded reincarnation as the most likely form survival might take, but offered no evidence of it.

Paul Siwek (1953) contributed what appears to be the first thoroughgoing skeptical treatment of reincarnation,[15] although it was directed not to psychical research but to Theosophy, which continued to be the most prominent promoter of the idea in England and America. Siwek addressed déjà vu and apparent memories arising in dreams and under hypnosis, as well as the claims of children. He was skeptical of the last because of children's tendency to fantasize and suggested that Indian cases might be prompted by cultural expectations.

Enter Ian Stevenson

In 1960, Stevenson published a literature review that ushered in a new chapter in the study of reincarnation. He reported having found 44 apparently credible accounts of persons who claimed to remember having lived before. In 28 of these cases, the subjects had made at least six statements relating to the previous life and the two families were unknown to each other before the previous person was identified and the statements were confirmed. The majority of the subjects were young children, like Katsugoro, Alessandrina Samona, and Shanti Devi. The cases came from 13 countries, including India, Burma, Italy, England, Belgium, Greece, Cuba, Mauritius, Japan, France, Syria, Canada, and the United States (Stevenson, 1960).

The previous lives in these cases all occurred close by the subjects and not long in the past, very different from what Western occult traditions, cases like Rosemary and Nyria, the far-memory novels of Joan Grant, and the life readings of Edgar Cayce, had led one to expect.[16] Moreover, the memories were veridical, long the gold standard of spontaneous cases in parapsychology. No one since Shirley (1936), Yeats-Brown (1936), and Osborn (1937) had brought cases like these together and they were little-known. Most writers on reincarnation appealed to logical argument and quoted cultural luminaries who believed in it. Their evidence consisted largely of child prodigies and déjà vu experiences, and many took pains to explain why previous lives were not normally recalled (e.g., Johnson, 1953:385–388). No one seems to have realized there were so many spontaneous cases on record, or that they shared such a class similarity.

In their last major works, Ducasse (1961) and Broad (1962) referenced Stevenson's paper, although it appeared too late for them to consider at length. Ducasse, who had been in contact with Stevenson and received from him complete reports of several cases, analyzed some of the more impressive (1961:241–247) and concluded that they provided "the best conceivable kind of evidence" for reincarnation (1961:306). Broad concurred that the best of them were "strongly suggestive" of reincarnation (1962:411). A. J. Ayer also may have had Stevenson's paper in mind when he wrote: "I think it would be open to us to admit the logical possibility of reincarnation merely by laying down the rule that if a person who is physically identified as living at a later time does have the ostensible memories and character of a person who is physically identified as living at an earlier time, they are to be counted as one person and not two" (1963:127).[17]

A dissonant note was sounded by C. T. K. Chari, a professor at the Madras Christian College in South India. He pointed to similarities among mediumship, possession, reincarnation, and multiple personality, arguing that what seem to be past-life memories are fantasies produced in altered states of

consciousness (Chari, 1961–1962a, 1961–1962b, cf. Stevenson, 1961–1962). These papers were quickly followed by others that offered explanations in terms of cryptomnesia (Chari, 1962a), paramnesia (Chari, 1962b), and psychometry allied with GESP (general extra-sensory perception) (Chari, 1962c).

Stevenson's paper caught the attention of two other people who were to have a profound influence on his life. One was Chester Carlson, whose importance has already been noted. The other was Eileen Garrett of the Parapsychology Foundation. At the beginning of 1961, she told Stevenson that she had heard of a child case in India and offered him funds to investigate it. By the time he left for India and Ceylon later that year, he knew of a few other cases, but he went expecting to find children who only spoke about having lived before. He should have been prepared by the accounts he had reviewed for behaviors, physical traits, and birthmarks related to the previous persons as well, but these latter features of the cases caught him by surprise, and he was slow to appreciate their significance. He was surprised also by the large number of additional cases he learned about. Later, with Carlson's support, he went to Lebanon, Brazil, and Alaska. He returned to several of these places more than once. The result was *Twenty Cases* (Stevenson, 2006).

Twenty Cases and the Cultural Conformance Theory

Twenty Cases introduced terminology that has been adopted by other researchers, and it set the standard for investigating and reporting reincarnation-type cases. Stevenson's methods were modeled on the investigations of spontaneous cases by the early SPR and emphasized the careful recording and consideration of facts, aimed at establishing paranormality. The great majority of cases were some years old and the two families had met by the time Stevenson arrived, so his research centered on interviews with first-hand witnesses and the scrutiny of what written documents were available. Only rarely did he reach a case before it was "solved," allowing him to make a record of the subject's statements and behaviors before attempting to verify them, and to observe the initial meeting of the subject and the family of the previous person, if the latter could be identified. Solved cases with records made before verification are ideal because the investigator can reduce the chance of informants misremembering or forgetting key details, but they are rare (Keil & Tucker, 2005).

The twenty case studies are grouped by country or culture with each section prefaced by a resume of the reincarnation beliefs of that region or people. Most reports include tabulations of statements and behaviors along with brief comments that are expanded upon as appropriate in the general discussion. Each report describes how the case was investigated and assesses possibilities such as fraud, malobservation, tricks of memory, and so forth, as well as sundry

paranormal explanations, before deciding that the case is best interpreted as one of reincarnation. The arguments are summarized and reconsidered in a chapter at the end of the volume.

Neither in that chapter nor elsewhere does Stevenson assert that the cases prove that reincarnation occurs—only that the best of them are highly suggestive of it. Of the book's reviewers, Beloff (1966) and McHarg (1969) accepted this conclusion, with McHarg pointing out that what reincarnated appeared to be something less than a full personality. Chari (1967) proposed various alternative explanations, including ESP. Louisa Rhine (1966) suggested that the cases might be the result of parents unconsciously shaping the behavior of their children to conform to cultural expectations about reincarnation, a position anticipated by Siwek (1953) and assumed by many later critics.

The fullest and most oft-cited expression of the psycho-cultural (or socio-psychological) theory was made by Brody (1979) in a review of a later book by Stevenson. Pasricha (1992), however, found that parental guidance could not account for cases in North India, and Schouten and Stevenson (1998) tested the possibility by comparing cases with and without written records made before verification. On the psycho-cultural theory, cases with written records would be expected to have many fewer verified statements than cases without them. The test did not support this theory. Children in the group with written records made more statements, an average of 25.5 as against an average of 18.5, a statistically significant difference ($p < 0.01$), while the percentage of correct statements was roughly the same in both groups—76.7% in the cases with written records and 78.4% in the cases without them.

Mills (1990a, 1990b) studied several Indian cases with differences of religion (Hinduism and Buddhism) between the previous person and the subject and wondered why religious parents would choose to impose another religious identity on their children. We could ask a similar question about the large number of Indian cases with differences of caste. Also, many parents attempt to stop their children from talking about their memories, believing that they will suffer from them in some way.[18] Suppression attempts are seldom successful (Stevenson & Chadha, 1990), but if the parents are responsible for the cases, why do they seek to quash them once they have brought them into being? Is it because they have taken on lives of their own, so to speak? Many children insist they have other families and demand to be taken to their previous homes, and this must not be pleasant for their parents to hear.

The psychological and interpersonal conflicts necessary to produce reincarnation-type cases in the psycho-cultural theory led Rhine (1966) and Brody (1979) to call for explorations of the children's psychologies. This has now been done by Haraldsson and his colleagues in Sri Lanka (Haraldsson, 1995, 1997, Haraldsson, Fowler, & Periyannanpillai, 2000) and Lebanon

(Haraldsson, 2003) and Mills (2003) in India. Children with past-life memories are viewed by their parents as being more highly strung, more tense, more argumentative, and more anxious and fearful than children in matched control groups (though teachers do not report behavioral problems, and in fact the children perform better in school than their peers). They score higher on dissociation scales but are no more suggestible than their peers. In Lebanon, where 80% recalled violent deaths, they seemed to suffer from a mild PTSD. On the whole, differences between the groups appear to be attributable to effects of the memories and do not explain them. Braude (2003), however, wants more information on what psychological needs the memories fulfill in a particular case. He regards the statements of Stevenson and others on this point as superficial (as they often are), but it is not at all clear that further probing would turn up anything of consequence.

Cultural conformance theories (of which psycho-cultural theories are a variety) are challenged by the veridical aspects of the cases, the birthmarks and other physical features of many, and the strong emotions and personations exhibited by the children, forcing critics to include super-psi (as super-ESP is now called) in their explanatory paradigms. Braude (1989, 1992, 2003) believes that super-psi has not been properly appreciated and suggests that the children may be accessing it in psi-conducive dissociated states (2003:24). There is little doubt that, given sufficient ingenuity, super-psi can be stretched to cover any eventuality, and therefore in a strict sense it cannot be ruled out, no matter how crippling its complexity becomes. However, not everyone finds it as plausible as Braude does. Griffin (1997) introduces what he calls retroprehensive inclusion, essentially a new type of psi, to account for survival cases in general, but regards even that as failing to explain the better reincarnation-type cases.

In any event, super-psi would operate within cultural confines and be dependent upon the cultural conformance theory, so any evidence against the latter would count also against the former.[19] With this in mind, let us examine some of the cases in the book.

Seven Cases

A Lebanese Druse boy, Imad Elawar, expressed over and over again his joy at being able to walk. He gave many names and other details that pertained to a man from another town who had been bedridden, probably with tuberculosis of the spine, for two months before he died. Imad also had a pronounced fear of large motor vehicles. The man whose life he recalled had been involved in a bus accident and had had a cousin who had died following a truck accident.[20] He had spoken French well and Imad learned that language quickly, although no one else in his family could speak or understand it. This case was unsolved when Stevenson reached it and he was able to record much of what Imad said

and how he behaved before searching for and identifying the previous person, and thus it cannot easily be attributed to cultural construction (which may be why Angel, 1994a, 1994b, attacks Stevenson's research methods instead, cf. Stevenson, 1995).

An Indian boy named Ravi Shankar was born with a long, linear mark, closely resembling the scar of a knife wound, across his neck. He spoke about having been murdered, named the killers, and gave other details of the crime, in which the head had been severed from the body. Ravi's birthmark is typical of birthmarks in reincarnation-type cases. Few are of the common types but rather are congenital marks matching wounds and other marks on the bodies of the deceased persons whose lives the children recall (Stevenson, 1997a). The men Ravi named as the murderers lived in his town, and he was afraid of them whenever he saw them. His fear remained with him as he grew older, even as his imaged memories faded.

The case of Mallika, an Indian girl, is strikingly different. Imad was between one and a half and two and Ravi between two and three years old when they made their first statements, apparently spontaneously. Mallika was four when her family moved to a new city, where they rented the ground floor of a house. The first time she visited her landlord's apartment, upstairs from her own, she noticed some embroidered cushions, and said that she had made them. She later commented on several other things in the apartment that identified her with the landlord's wife's deceased sister. She exhibited some striking behavioral similarities to this woman, but she made no statements that were not recognitions.

Younger children, many of whom make their first statements as soon as they begin to speak, are more likely to make them spontaneously, while subjects of Mallika's age or older are more likely to make them in response to something they have seen (Matlock, 1988, 1989). It is as if the images have increasing difficulty breaking into consciousness as the children age.[21] Pratomwan Inthanu, a Thai woman, was 20 when veridical memories of two different lives came to her while meditating (Stevenson, 1983b). Uttara Huddar was 32 when she met a man she believed to be the reincarnation of her past-life husband and began to enter periodic fugue states, in which she behaved and spoke like an early 19th-century Bangali woman called Sharada (Akolkar, 1992, Stevenson, 1984).[22]

Not only was Mallika relatively old (for a child subject) when she made her recognitions, her case is a rare South Indian case, the only one among seven Indian cases in *Twenty Cases*. The population of South India is largely Dravidian or descended from Dravidian tribes, the most prominent of India's indigenous peoples. The Indo-European speaking Aryans arrived in North India around 1500 BC and their religion, Hinduism, adopted the belief in reincarnation from the peoples they encountered there (Fürer-Haimendorf, 1953, Obeyesekere,

1980). Belief in reincarnation is as strong today in South India as it is in the rest of the country. If beliefs sufficed to produce the cases, we would expect to find as many in the south as in the north, yet there are few cases in South India (Chari, 1967, Pasricha, 2001).

The comparative lack of cases in South India again shows the cultural conformance theory to be too facile, issues of veridicality aside. Moreover, reincarnation is not a single monolithic belief but a general term that designates a set of kindred beliefs and sub-beliefs. The exact beliefs and sub-beliefs vary from tradition to tradition (Neufeld, 1986, O'Flaherty, 1980). If the cultural conformance theory were correct, cases should reflect not only the general belief in reincarnation, but the local beliefs also (or in particular). However, at least in North India, this is not so. People unacquainted with actual cases expect them to develop in ways they do not (Pasricha, 1990).

Still, we can find traces of cultural influences on reincarnation-type cases, if we step back a little. Let us take the example of intermission length, the duration of the interval between lives. Intermission length varies across cultures more or less in line with cultural expectations, though it does not match them closely (Matlock, 1990b:225–226). For instance, the Druse expect the deceased to reincarnate in a newborn immediately upon death. The median intermission length for 79 of Stevenson's Druse cases was eight months, the second shortest of all the cultures in which he had studied cases (Stevenson, 1986:212),[23] but longer than the expectation.

If the cases are produced in conformance with cultural demands, it is hard to understand why the Druse would create this awkward situation for themselves. They hypothesize brief intermediate lives to make up the extra time (Stevenson, 2001:176), thereby bringing their belief into conformance with the cases, but would it not be easier to imagine fully culturally compliant cases from the start? From a reincarnation perspective, the tendency for intermission length to vary by culture can be explained if a person's beliefs can influence the circumstances of his or her rebirth (Matlock, 1990b:238). This may seem improbable, but I will give other examples of its possible occurrence later.[24]

The North Indian case of Swarnlata is instructive for very different reasons. Swarnlata was three and a half when she drove with her family through a town unknown to her and said they were near her old house. Thereafter she described what were evidently spontaneous memories of an earlier life. Her father and an outside investigator made notes of her statements, which the investigator then matched to a woman from the designated town who had died several years before Swarnlata's birth. When Swarnlata was taken to meet this woman's family, she recognized numerous people and places from her life, even passing tests intended to mislead her.

Swarnlata also performed dances and songs she said were from another

life, intermediate between the life she recalled best and her own. She sang the songs in Bengali, the language of East Pakistan (now Bangladesh). This intermediate life was never confirmed, but in the 1974 second edition of *Twenty Cases*, Stevenson reported that the songs had been identified as traditional ones from the Bengal region. Although Swarnlata was unable to converse in Bengali, as Uttara Huddar did, her singing is a form of recitative xenoglossy. If the intermediate life was real, Swarnlata's main memories may have been stimulated by her environment, even though the life to which they related was not the most recent one.

Another noteworthy Indian case in the collection is that of Jasbir, who at the age of five suffered a serious illness and seemed to die, but revived and after he had recuperated claimed to be a different person, named Sobha Ram. This case is similar to the case of Lurancy Vennum, except that Lurancy's possession by Mary Roff lasted less than four months, whereas Sobha Ram came to occupy Jasbir's body permanently. In this respect, it is like the Sumitra case (Stevenson, Pasricha, & McLean-Rice, 1989), also one of permanent possession or "replacement reincarnation," to coin a term.[25] It is different from the Uttara Huddar case in that Sharada was not an invading entity but was connected to Uttara as a past-life personality whose initial emergence was triggered by a highly emotional encounter. The revitalized Jasbir gradually came to accept his new circumstances and developed normally in them, even accepting the name Jasbir, which he at first resisted.

The Ceylonese case of Wijeratne is the only case in *Twenty Cases* to feature a birth defect related to the previous person. Wijeratne claimed to recall the life of a man who had killed his arranged bride when she refused to move from her parents' house after their civil wedding but before this was publicly celebrated in a marriage feast. He had been tried for the crime, convicted, and hung. Wijeratne was born with a shrunken left arm, the same arm the previous person had used to wield the murder weapon.

Wijeratne attributed his deformity to karma, but reincarnation-type cases provide scant evidence of karma as a moral system of rewards and punishments, much less as conceived in this coercive, cause-and-effect way. Although there are many Asian cases in which the social and economic situation of the subject varies greatly from that of the previous person, there is no discernable correlation with what is known about the previous person. Because the karmic reason for the shifts in social status is not apparent, the assumption is that there must be something that is not known, perhaps something from an anterior or intermediate life, that has resulted in the present circumstances (Stevenson, 2001). Again, beliefs are adjusted to fit the cases, not the other way around, as advocates of the cultural conformance theory would have it.

Karma is considered moral because it is derived from one's actions (and

sometimes one's thoughts, intentions, etc.), good and bad. It has been taken by many writers as a corollary of reincarnation and advanced as an ethically appealing aspect of the belief (e.g., by Osborn, 1937, and by Johnson, 1953). But there is nothing about reincarnation that logically entails karma, and in small-scale or tribal societies like the Dravidian and Tlingit, reincarnation beliefs do not include it (Matlock, 1993, Obeyesekere, 2002).[26] *Karma* is a Sanskrit word that originally denoted ritual action but came to be linked to reincarnation and took on its moral coloring in Hinduism and Buddhism after they had acquired the belief in rebirth from Indian tribal peoples (Fürer-Haimendorf, 1953, Obeyesekere, 1980, 2002). I return to this issue in my discussion of Stevenson's Tlingit cases, below. If not evidence of karma, Wijeratne's case can be explained as due to the previous person's belief that his conduct would have this result, and thus is another example of a person's beliefs influencing the reincarnation process.[27]

The case of Paulo Lorenz, one of two Brazilian cases[28] in the book, has several interesting features, not least of which is that Paulo recalled the life of his deceased sister, Emilia, who had poisoned and killed herself, saying that she wished to be reborn a boy. Paulo did not start speaking until he was three and a half. The first thing he said was to tell another child, who was about to put something in his mouth, that it was dangerous to do that. He identified with Emilia, and until he was four or five refused to wear boy's clothes. He only accepted trousers when an old skirt of Emilia's was used to make him a pair. Neither Paulo nor Emilia showed any interest in cooking and both disliked milk and had the habit of breaking corners off bread. Most importantly, both were skilled at sewing, the only members of a family of fifteen who showed any aptitude for it. Paulo recognized and demonstrated how to thread and use Emilia's sewing machine when he was younger than four years old.

Sex change is one of the features of reincarnation-type cases that varies according to cultural expectation, being found most often in places with traditions that allow for it, and seldom or not at all where it is believed not to happen (Matlock, 1990b:226–227). This sometimes is held to be a telling point in favor of the cases as cultural constructions, but it could just as well be that in places that sex change is thought impossible, people do not consider it an option for themselves, and so return as members of the same sex.

In the majority of sex-change cases, the subjects adjust to their new anatomical sex, but in some cases, gender confusion persists for years (Mills, 2004, Stevenson, 1977b, Tucker & Keil, 2001). Paulo began to lose his intense feminine traits when he was about six but was effeminate even in adulthood. He never married and, Stevenson tells us in the second edition of *Twenty Cases,* he killed himself when he was 43. Most of the other subjects in *Twenty Cases* moved beyond their childhood memories and developed normally, but

Wijeratne also experienced difficulties. He suffered a series of psychotic breaks triggered by imagined rejections by women he liked and was several years late in qualifying for university studies. Such severe adjustment problems are rare, but less extreme ones have been reported on occasion. Mills (2006) describes memories that impeded arranged marriages in two Indian cases because the subjects, then in their twenties, still had strong attachments to the persons they considered their past-life spouses. Shanti Devi provides another example of the same (Lönnerstrand, 1998:84–85).

Animistic Reincarnation

We turn now to the Tlingit cases. These are divergent from the other cases in some respects, although they share many features with them. The Tlingit are an Alaskan Indian tribe whose reincarnation beliefs are rooted in a long Amerindian (Jefferson, 2009, Mills & Slobodin, 1994) and global (Matlock, 1993) tradition that includes the Dravidians and others. This is the tradition called animism, a collection of beliefs about souls and spirits and their operation in the natural and supernatural worlds (Tylor, 1871).[29] Belief in postmortem survival is universal in animistic societies and reincarnation beliefs are more common than might be thought. Half of the world's tribal peoples in cross-cultural samples have or at one time had them (Matlock, 1993, 1995).[30]

In discussions of reincarnation beliefs, a contrast is often made between Hindu and Buddhist ideas. This is an important distinction, because Buddhists do not recognize an eternal soul but imagine rebirth propelled by attachments to the material world (with karma playing a central role), whereas Hindus conceive of a personal soul that continues to evolve through a succession of lives (O'Flaherty, 1980). However, there is another contrast to be made, and that is between reincarnation beliefs that incorporate karma and those that do not. I believe this latter distinction to be the more basic and so group Hindu and Buddhist beliefs together in opposition to animistic ones (Matlock, 1996). Because reincarnation-type cases provide little evidence of karma, regardless of the culture from which they are reported (Stevenson, 2001:251–253), they fall under the heading of Animistic reincarnation.

Without the concept of karma,[31] the Tlingit and other tribal peoples are free to believe that they may exercise some control over the reincarnation process, and in two of the Tlingit cases in *Twenty Cases* (Corliss Chortkin, Jr., and William George, Jr.), the previous persons stated their intentions to be reborn to the mothers of the subjects. Planned returns were expressed in the two Brazilian cases in *Twenty Cases*, but they are unusual under Indic belief systems (including Jainism and Sikhism along with Hinduism and Buddhism),[32] where karma is thought to govern the rebirth process.[33] Planned returns are distinctly animistic in implying a discarnate agency, and Stevenson (2001:39) regarded

them as the only variety of reincarnation belief for which there is empirical evidence.

However, the Tlingit cases are weaker evidentially than those of Southeast Asia, and Stevenson evinced relatively little interest in them. He published papers on the Tlingit (1966a) and neighboring Haida (1975a), but never produced a volume of case reports about them, as he did for other cultures (1975b, 1977a, 1980, 1983b). The weaknesses stem in part from the planned returns, which set up the expectation of the rebirth, thereby opening the cases to charges of parental and societal shaping in accordance to the expectation. Another reason is that in most Tlingit cases (including all seven in *Twenty Cases*), the previous person and the subject are related, so that the subject in theory could have learned about the previous person normally or paranormally from relatives. These cases are also often less well-developed, with fewer statements and recognitions attributed to the subjects, than are the better Southeast Asian cases (though rich cases may occur also among tribal peoples; see Mills, 2010).

A striking feature of the Tlingit cases in *Twenty Cases* is the birthmarks. Six of the seven have birthmarks, but elsewhere in the collection they appear only in the case of Ravi Shankar (Wijeratne's birth defect is of a different order). Along with planned returns, announcing dreams,[34] and physical and behavioral similarities, birthmarks are signs that allow the Tlingit to identify a child with a particular deceased person even before he or she begins to talk (de Laguna, 1972, Matlock, 1990a). These signs, which have become well-known as recurrent features of reincarnation-type cases, have been reported by ethnographers and other observers in relation to animistic reincarnation beliefs for many years. They were noted by Tylor (1871:3–5), who assumed that they had occurred from time immemorial and suggested that they were the foundation of the belief in reincarnation, as seems very possible.

In small-scale tribal societies, a premium typically is placed on returning in the same lineage or kin grouping, allowing for an almost literal "social reproduction." When signs suggest a child is a returning relative, he or she may be given that person's name and grow up to take on or to qualify to take on that person's rights and responsibilities and even to inherit his tangible and intangible property (Matlock, 1990a, 1993, Mills, 1988). However, when signs suggest that the child is the reincarnation of someone outside the kin grouping, this also is accepted, and reincarnation thereby promotes social cohesion (Mills & Champion, 1996). In the much more populous Indic societies, the pattern is reversed, with the majority of cases occurring between non-relatives, often strangers. This is because, as Obeyesekere puts it, "karma theory produces dislocation and the dispersal of kin" in rebirth (2002:344). It does so by de-emphasizing personal relationships and ascribing rebirth to an impersonal moral force.

I suggest that personal agency always plays a role, but different sets of expectations (on the part of the previous persons), generated by different cultural ideals (discarnate agency vs. karma), produce the different outcomes (same-family cases in animistic societies vs. non-relative and stranger cases in Indic societies). Beliefs about the reincarnation process influence the cases from the inside, as it were, carried by dying persons into their postmortem state. Discarnate actors are involved in selecting their new births, unconsciously if not consciously.[35] This accords with animistic thinking and is what I call Animistic reincarnation.

Keil (1996) called cases in which the child makes no or very few statements "silent" and "near-silent" cases. His data suggest that these account for a relatively small percentage of cases among the Turkish Alevi, whereas the impression one receives from the ethnographic literature is that silent and near-silent cases are in the majority in tribal societies. The less well-developed tribal cases are like cases from Europe and non-tribal North America (Harrison & Harrison, 1991, Jacobson, 1974, Osborn, 1937, Rivas, 2003, 2004, Stemman, 2005, Stevenson, 1960, 2003, Tucker, 2005) in being notably underdeveloped and often unsolved. Moreover, solved cases similar to those from other places, have been reported from Europe and non-tribal North America (Cockell, 1994, Leininger & Leininger, 2009, Stevenson, 1960, 1983a, 1983b, 2003, Tucker, 2005). There is no correlation between the strength of reincarnation beliefs and the strength or even the appearance of cases. Both strong and weak cases may occur where the belief is weak or absent as well as where it is strongly present, and cases where the belief is strongly present are not always strong as we saw in the instance of South India[36] and see again with the cases from tribal societies. We must therefore seek explanations for these cases in terms of something other than the belief in reincarnation.[37]

The same year the revised edition of *Twenty Cases* appeared, Stevenson introduced the idea of a psychophore to explain how reincarnation might operate. He described the psychophore as an "intermediate 'non-physical' body which acts as the carrier of . . . attributes between one life and another" (1974a:406). He returned to this idea in *Children Who Remember Previous Lives*, where he speculated at greater length about processes (2001:233–254). The psychophore would not simply convey characteristics, but would have will and intention at its disposal. It might exercise some initiative, such as waiting for a body of the desired sex to be available, if it did not operate on the developing zygote directly. The psychophore sounds very much like an astral body—perhaps a "minded" astral body, to borrow a term from Wheatley (1979)—and it harmonizes well with my concept of Animistic reincarnation.[38]

If reincarnation happens for some, does it happen for all? Griffin assumes that the only people who have reincarnated are those who remember having

lived before (1997:186), but silent cases suggest that reincarnation may occur without imaged memories, and it is possible that a person might reincarnate without having any indication of it whatsoever. And if we all reincarnate, we may all have past-life memories accessible to us. They may enter consciousness on occasion, and perhaps be retrievable under hypnosis, trance, and other dissociative states, as we find with adults such as Pratomwan Inthanu and Uttara Huddar. They may also lie in the background of fictional productions such as those of Pearl Curran and Joan Grant. I believe the assumption that we all reincarnate handles the data better than the assumption that only some of us do, but as Stevenson observed (2001:216), we may never be certain on this point.

Stevenson's Legacy

Twenty Cases depicted reincarnation very differently than had been imagined before Stevenson's 1960 paper, and it more than confirmed the findings and fulfilled the promise of that paper. All the major recurrent features of reincarnation-type cases appear in the book.[39] Indeed, it is remarkable how well it lays out the parameters of this type of case. Subsequent work has added details but has not changed our understanding in any substantial way. Moreover, as I have shown, the twenty cases collectively provide the basis of a theoretical conception of reincarnation in terms of an Animistic as opposed to an Indic model, a way of thinking about reincarnation radically different from that assumed before 1960.

Twenty Cases did much to pull reincarnation studies out of the realm of speculation and to provide it a scientific footing. However, Stevenson's fellow parapsychologists were slow to recognize its significance, and the larger world of science, to which Stevenson appealed constantly, has yet to come to grips with it (Edelstein, 2008). Attitudes may be changing, however. Astronomer and science writer Carl Sagan, a skeptic about most things parapsychological, wrote in his last book that he regarded reincarnation-type cases to be one of the few promising areas of research in the field (Sagan, 1997:302).

One reason for Stevenson's early difficulty in parapsychology was his adherence to the old SPR style of field investigation at a time when the gravitational center of the discipline had moved from spontaneous cases and survival questions to laboratory studies of ESP under the influence of J. B. Rhine (Alvarado & Zingrone, 2008). Stevenson was out of step with the majority of his colleagues, especially in the United States. It was widely believed that he was the only researcher turning up reincarnation-type cases, something easily disproved (Matlock, 1990b). Similar cases had been reported by many people over many years, and other investigators, such as Brazilian parapsychologist Hernani Andrade, were finding them also. However, because Andrade published

in Portuguese (e.g., 1988), he was and still is not as well-known in the English-speaking world as he should be (but see Playfair, 1975, 2006, and Andrade, Rossi, Playfair, & Lima, 2010, for English-language summaries of his cases.)

Of those Stevenson trained to assist him in the field, Satwant Pasricha has made the most significant contributions, but she is not the only one. K. S. Rawat has recently co-authored an article and a book with newcomer Titus Rivas (Rawat & Rivas, 2005, 2007). Stevenson's research has attracted others as well. Anthropologist Antonia Mills was acquainted with the reincarnation beliefs of the Indians of British Columbia when she met Stevenson in 1984 and became interested in cases (Mills, 1994). Stevenson underwrote her work with the Gitksan and Beaver, reported in 1988, as well as her first field trips to India, in 1987 and 1988, in an attempt to "replicate" his findings (Mills, 1989). Later, she joined Erlendur Haraldsson and Jürgen Keil in a larger replication of Stevenson's research (Mills, Haraldsson, & Keil, 1994).[40]

As readers of this *Journal* know, Pasricha, Mills, Haraldsson, and Keil have continued to report reincarnation-type cases and have carried the research in new directions. Haraldsson (1995, 2000, 2003) has studied the psychology of the child subjects, Mills (2001, 2006, 2010) is demonstrating the power of combining anthropological and parapsychological approaches, and Keil (1996, 2010) is drawing attention to unusual and problematical cases. Meanwhile, Tucker (2005) has been working with non-tribal American cases, Rivas (2000, 2003, 2004) has reported cases from The Netherlands, and an increasing number of original accounts are appearing in popular publications (Bowman, 1997, 2001, Cockell, 1994, Harrison & Harrison, 1991, Leininger & Leininger, 2009). Important new voices are also emerging on the side of commentary and critique (Edelman & Bernet, 2007, Moura Visoni, 2010, Nahm & Hassler, 2011). Stevenson's passing has not brought an end to serious reincarnation studies, as some may have expected.

I have focused on the professional reception of *Twenty Cases* because that is where Stevenson placed his emphasis, but I cannot altogether ignore the popular sphere, for it is there that the book has had its greatest impact. By 1990, *Twenty Cases* had been translated into seven languages and sold some 50,000 copies (Stevenson, 1990), astonishing for a university press offering. It is difficult to find a book on reincarnation published after 1966 that does not refer to it. Many popular books summarize its cases and conclusions, making them more accessible to a wide audience. Angel identifies it as "one of the most influential sources of empirical evidence for reincarnation" (1994a:481) and says that his students in the philosophy of religion routinely cite authors who have been persuaded by it.

As of 1990, Stevenson's research had received little attention from academia (see Matlock, 1990b). Since that date, it has been addressed by several

philosophers, who have raised the level of discourse on the subject considerably. Angel (1994b) limits himself to the case of Imad Elawar, but Almeder (1992), Becker (1993), Paterson (1995), Edwards (1996), Griffin (1997), Braude (2003), Grosso (2004), and Lund (2009) treat Stevenson's work generally. Edwards, a humanist, ridicules much of the data. Almeder, as already noted, is convinced. The others fall somewhere in between. Most contrast reincarnation with super-psi and psycho-cultural explanations. Griffin, Becker, Lund, Paterson, and Grosso lean toward reincarnation, but Braude favors super-psi.

Stevenson had high hopes for *Reincarnation and Biology*, his massive two-volume examination of birthmarks and birth defects (1997a, 1997b). This "medical monograph" includes detailed reports of 225 cases, together with supporting photographs and citations from autopsy reports, and he was very disheartened when it was met with silence. Edelstein (2008) suggests that this non-reaction was due in part to Stevenson having done little to show how reincarnation could be integrated with biology. His psychophore concept was not well-enough articulated to serve the purpose. I agree with this, but I think there may be other large obstacles also. One is Stevenson's steadfastly parapsychological presentation. He was very much a psychical researcher of the old school and was not good at communicating with scientists of other disciplines, despite his many publications in mainstream journals. Another part of the problem may lie with the word *reincarnation*.

Do the cases Stevenson studied suggest or support reincarnation? Not if we define it in the Indic sense, as involving karma. If we want to say that these cases suggest reincarnation, we must be clear that we mean Animistic reincarnation, and we would do well to point out that the evidence we have suggests that it occurs most often in the same community or region and that there typically are very few years between lives.[41] There is no hint in the spontaneous cases of past lives centuries before in distant foreign lands, as was commonly envisaged before 1960. Nor is there much evidence of past lives spent as animals, as is allowed under Hinduism and Buddhism (and in some societies with animistic beliefs). In other words, we must distinguish an empirically based, scientific understanding of reincarnation from a religious or occult one. We may also want to follow McTaggart's lead and come up with a new name for the process.

Stevenson's most important legacy arguably lies in making reincarnation a problem for science, not merely religion and philosophy, but we must now take the next steps. It is good to show that reincarnation is logically coherent and that it makes better sense of the data than other theories do, but until we can demonstrate its relation to established concepts in biology and psychology, we will not have advanced much beyond where we were in 1960, as far as the majority of scientists are concerned.[42] Moreover, although I think that current data point in the direction of reincarnation, we must be cautious in our

conclusions, since it may turn out that our present ideas are not quite right and that another solution, which we cannot yet see, is the correct one. Regardless, reincarnation-type cases without a doubt present a problem for science, one that we will always be indebted to Stevenson and *Twenty Cases* for having brought to our attention.

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Notes

[1] *Twenty Cases* remains in press in its 1980 paperback edition.

[2] The 2001 publication is a updated edition of the book of the same title published in 1987.

[3] Stevenson referred to the person of the previous life as the "previous personality," but persons and personalities are not at all the same thing. The individuals concerned were more than personalities, and I prefer the term "previous person" (Matlock, 1990b).

[4] The current name is Division of Perceptual Studies.

[5] I use the term "spontaneous case" as it used in parapsychology, to denote paranormal experiences. Spontaneous past-life memories resemble what psychologists call involuntary memories and flashbulb memories, except that many have behavioral and physical components.

[6] Super-ESP is ESP of a nature and range not otherwise observed.

[7] There was, however, more evidence than Myers realized. The earliest recorded reincarnation-type cases presently known are Chinese cases dating from the 3rd to the 10th centuries AD (De Groot, 1901:143–145, and Paton, 1921:26–27, per Gauld, 2008:33, Note 7). Nineteenth-century cases are described by Wortabet (1860:308–309n; retold by Oliphant, 1880:322–323), Hearn (1897:267–290), and Fielding (1898:335–353). Signs of the sort now recognized to be recurrent features of cases appear in the reports of travelers, missionaries, and government functionaries from the 17th through the 19th centuries (Tylor, 1871:3–5). Besterman (1930) cites 19th-century examples from sub-Saharan Africa, and Matlock and Mills (1994) have several 19th-century references for North American native societies.

[8] Many Spiritualists were opposed to reincarnation because they thought that it was contradicted by mediumistic communication and because mediums were said not to hear about it from deceased communicators. This was not the case in continental Europe, where Kardec's Spiritism and, later, Steiner's Anthroposophy were popular (see Note 9), leading to tensions between adherents of the different spiritualist schools (see Alvarado, 2003:83–84, for examples of the Victorian Spiritualist attitude toward Spiritism).

[9] Other occult systems, including Spiritism (Kardec, 1875), Anthroposophy (Steiner, 1914), and Rosicrucianism (Heindel, 1909), also taught reincarnation, but Theosophy was by far the most prominent in the earlier part of the 20th century in the United States and Great Britain. On reincarnation and 19th-century Spiritism in France, see a recent book by Sharp (2006).

[10] Rogo (1985:17–18) suggested that the SPR's lack of interest in reincarnation may have stemmed from their dislike of the Theosophist Helena Blavatsky, whom they had investigated and found a fraud (Hodgson, Netherclift, & Sidgwick, 1885). Another factor may have been doubts raised by the different positions taken by Spiritualism and Spiritism (Sudre, 1930).

[11] Xenoglossy is the correct use of an unlearned language. It may be either recitative (use of words or expressions only without the ability to converse) or responsive (use of language in an interactive way, showing the ability to understand as well as speak).

[12] This author has generally been cited in psychical research as Campbell-Praed (e.g., by Rogo, 1985, and Stevenson, 1960), probably because Shirley (1936) hyphenated the name. However, the name is not hyphenated on the title pages of her books and appears as Praed in the catalog of the Library of Congress, the authority followed by most libraries.

[13] Sunderlal submitted this paper first to the ASPR *Journal,* but the editors thought it possibly a hoax because similar cases were unknown to them (documents in the ASPR archives).

[14] The case of Shanti Devi began to develop in the early 1930s and led to a formal investigation in the middle of that decade (Gupta, Sharma, & Mathur, 1936). Shanti retained her memories into adulthood, which permitted other investigators to interview her as well (Bose, 1952, Lönnerstrand, 1998, Rawat, 1997). Today it is one of the best-known Indian child cases but this was not so in the period before 1960. Ducasse (1961) appears to have been the first after Shirley (1936) to comment on it in English. Tenhaeff (1958) dealt with it, but in Dutch, and an English translation of his book was not published until 1972.

[15] Objections were raised earlier by Pringle-Pattison (1922), whose concept of reincarnation was informed mainly by Hindu and Buddhist teachings. Siwek's treatment is, so far as I know, the first skeptical one to deal extensively with memory claims.

[16] Indeed, Ducasse doubted the regressions of de Rochas (1911) not only because they were not veridical but because the lives ostensibly recalled were located in France rather than in distant locales (Ducasse, 1961:274).

[17] However, Ayer made the same point a few years earlier (1956:193–194), so this passage may be no more than a coincidence of timing.

[18] Stevenson has found this idea in various parts of the world, among the Alaskan Tlingit and Nigerian Igbo as well as throughout South Asia (2001:96).

[19] I do not have space to consider other ESP models, such as Murphy's application of Carington's psychon theory (Murphy, 1973), Roll's long body (1982), or Keil's thought bundles (2010), but as none transcend culture, the same general considerations apply to them also. See Matlock (1990b) on the earlier theories and Nahm and Hassler (2011) on Keil.

[20] Stevenson learned about another man who as a child claimed to remember the life of the cousin who had been killed in the truck accident. Because both he and Imad spoke about the same accident, albeit from different points of view, this case has been por-

trayed as one of "merged and divided" rebirth by Roll (1977, 1982) and Rogo (1985). There would seem little basis for this reading (Matlock, 1992). Apart from the truck accident, the memories of the two subjects were entirely distinct.

[21] Adult spontaneous past-life memory cases have received relatively little attention. In adults, apparent memories (much less-developed and less often veridical than in young children) tend to arise in dreams, trances, and other altered states (Jacobson, 1974, Lenz, 1979, Osborn, 1937, Pasricha, 1990:109–112, Rogo, 1985, 1991).

[22] This case is commonly known by the name of the previous person, Sharada. It has been analyzed as one of possession rather than reincarnation by Griffin (1997) and Braude (2003), but both Stevenson (1984) and Akolkar (1992), who investigated it, treat it as one of reincarnation. The key is appreciating that Uttara was relatively old at case onset. She was practiced in meditation and this may have played an important role in the way her memories presented (Matlock, 1988).

[23] The median intermission was four months among the Haida of Alaska. The longest median intermission of ten societies compared was 141 months in a series of 25 non-tribal U.S. cases (Stevenson, 1986:212).

[24] I am suggesting something more than the idea that thoughts at the point of death have an influence on the new birth, as one finds in Tibetan and other forms of Buddhism. I mean long-held, firmly established beliefs that may be largely unconscious and may persist after death in the "mind" of a discarnate actor who brings about his or her own reincarnation.

[25] Only a few other cases of this sort have been reported (Barrington, Mulacz, & Rivas, 2005, Pasricha, 1990:104–109, Stevenson, 1983b:171–190). However, although rare, the phenomenon is common enough in India to be recognized by a special name in Hindu religious thought—*parakaya pravesh*, which refers to the entry of a wandering soul into a physical body, replacing the soul with which the body was born (Nivedita Nadkarni, personal communication). *Parakaya pravesh* generally is glossed as "possession" in English, though this obscures the fact that it covers both temporary and permanent forms of possession.

[26] Karma is also missing from ancient Greek and other reincarnation concepts, as Obeyesekere (2002) shows in some detail. It is found only in the Indic religions and occult systems such as Theosophy that are derived from them.

[27] It is interesting that Wijeratne did not have a birthmark related to the hanging. I suggest that he did not because the previous person was more concerned (perhaps preoccupied) with the murder he had committed. I do not think that birthmarks are automatically produced but rather that they are conditioned by the previous person's focus of attention and emotional attachments. This hypothesis allows us to explain why not all death wounds produce birthmarks and why some represent marks made to the body after death, as well as why some represent wounds or marks from earlier in life (Stevenson, 1997a, 2001).

[28] Despite opposition from the Catholic Church, reincarnation is widely accepted in Brazil, with beliefs derived from the West African culture of former slaves, reinforced by the Spiritism of Allan Kardec.

[29] The animistic worldview is fundamentally empirical. Tylor (1871), who introduced animism, argued that concepts of the soul and its survival of death were suggested by observations and experiences such as paranormal dreams, apparitions, and what we now call out-of-body and near-death experiences. Tylor believed that the soul was

then generalized to lower animals and in some situations to plants and to natural force and even to words and names, although these last are by no means universal features of animism as it appears in the ethnographic record (Matlock, 1993, 1995).

[30] Animistic peoples do not necessarily see a conflict between ancestral spirits and reincarnation. In a cross-cultural study (Matlock, 1995), I found a statistically significant relationship (p =.003) between beliefs in reincarnation and active ancestral spirits, those thought to interact with the living in some way. In another study, I found a significant relationship (p =.035) between beliefs in reincarnation and the fragmentation of the soul upon death (Matlock, 1993:128–129). Typically, the soul is thought to split three ways upon death, one part staying with the corpse, another part going on to the land of the dead, and a third part reincarnating.

[31] In the introduction to the Tlingit cases in *Twenty Cases,* Stevenson described what he thought were indications of karma in Tlingit beliefs and speculated that these were influenced by contact with Buddhism. However, the examples he gives of karma are of no more than a belief in the continuity of identity from one life to another and have no reference to the moral qualities of actions with which karma is concerned. He does not repeat this assertion on other occasions.

[32] This classification of Hindu with Buddhist, Jain, and Sikh beliefs as Indic is problematical because in Hinduism, unlike in the other religions, there has long been debate about whether karma is the result of individual action alone or whether it is implemented or adjusted by God (O'Flaherty, 1980). In modern Hinduism, God is involved in the mediation of karma to such an extent that modern Hinduism is better assigned to a Theistic category (Nivedita Nadkarni, personal communication). In Theistic reincarnation, God determines how a new birth is assigned. Other examples of Theistic reincarnation beliefs are the Druse and Alevi (see Stevenson, 2001:38).

[33] Obeyesekere (2002) compares planned returns in animistic societies to the "rebirth wishes" that often are a part of Buddhist merit-making rituals. The latter may include desires to be reborn to certain people, especially in the same family, but Obeyesekere—I think correctly—reads these as survivals of earlier animistic beliefs because they are fundamentally at odds with karma (2002:344).

[34] Announcing dreams are dreams in which a deceased person appears and "announces" his or her intention to be reborn, usually to a certain woman.

[35] This brings us to the "selection problem" (the problem of how the new parents are selected), but I do not have space here to go further into the issue. For a longer discussion, see Stevenson (2001:236–244), and see also the literature on intermission memories (Rawat & Rivas, 2005, Sharma & Tucker, 2005, Story, 1975:191–199).

[36] Chari (1967) observed that there are few cases not only in South India, but in most of North India also. It appears that reincarnation-type cases occur more frequently in some places than others. The reasons for this variation would seem to have little to do with belief but are not yet understood.

[37] This conclusion would be stronger if it were more than impressionistic. Tucker's (2000) Strength of Case Scale could be used to rate the strength of cases, which could then be compared to the strength of reincarnation beliefs in different cultures, if this could be assessed in some way.

[38] Many religious traditions and occult systems have similar concepts, but Stevenson introduced his neologism, which means "soul bearing," to avoid their connotations (2001:309). Elsewhere he suggested that the psychophore might be composed of mor-

phogenetic fields (1997b:2086–2088). However, he left the idea undeveloped, and as Gauld points out, it "seems to be simply a dummy concept filling (pending further information) a vital gap in an explanatory system" (2008:31).

[39] This includes intermission memories (see Note 35). The second Jasbir said that after his death as Sobha Ram he met a holy man who told him to take refuge in the first Jasbir's body. This is an example of what we may call "assisted reincarnation," an exception to the rule that discarnate actors take the initiative in Animistic reincarnation.

[40] These replication studies were aimed at seeing if different persons, working with the same methods, would find similar cases and come to similar conclusions regarding them. All three found it easy to discover similar cases. Mills and Haraldsson agreed with Stevenson's conclusions, but Keil preferred an ESP interpretation (Mills, Haraldsson, & Keil, 1994).

[41] According to Stevenson, all but a few cases have intermissions of less than three years (2001:120). There are solved spontaneous cases with longer intermissions, but not on the order of centuries, and long-distance or international cases are also rare. Even when these do occur, we cannot be sure that there have not been intervening lives intermediate in distance as well as time. This issue relates to the selection problem and a fuller discussion will have to await another occasion.

[42] One way would be to link the psychophore or minded astral body concepts to Sheldrake's (1981, 2009) morphogenetic fields and morphic resonance (Matlock, 1988, 1990b). Stevenson (1977b:2086–2088) saw this, but, as Edelstein (2008:98) points out, Sheldrake has had little success in getting his own ideas accepted.

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BOOK REVIEWS

Consciousness and the Source of Reality: The PEAR Odyssey by Robert G. Jahn and Brenda J. Dunne. Princeton: ICRL Press, 2011. 398 pp. $19.95. ISBN 9781936033034. Hardcover deluxe limited color edition, $59.95, info@icrl.org.

A few years ago I attended a lecture by a well-known Ivy-League physicist who is quite skilled at presenting basic scientific principles to a lay audience. At the end of his talk, which was intended to communicate the essentials of the way that modern physical theory conceptualizes the world, he was asked a simple and direct question: Have you any opinion of experiments that suggest that consciousness can influence random physical processes?

I was impressed by his reaction to the question. Without being dismissive at all, he leaned on the podium for what seemed an extended time before carefully crafting his answer. It was obvious that he took the question seriously. Speaking quite slowly and deliberately, he unequivocally said that if consciousness could influence random physical events, then everything that he thinks he knows is wrong. Everything. After another long pause, he continued by recalling that one of his respected colleagues told him that "someone" had worked on this problem at he thought perhaps Princeton, but that nothing significant ever came of it.

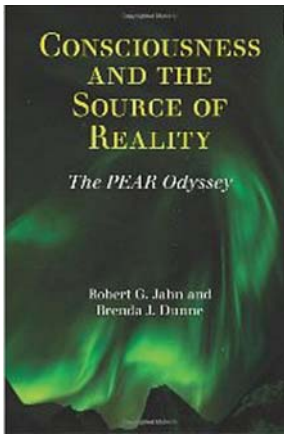
I walked over to the line at the microphone to make a suggestion, but before it was my turn the allotted time was up and the speaker left the stage. What I wanted to suggest was this: If, in your own words, everything you think you know would be wrong if consciousness could influence random physical events, then I think it might be worth a few hours of your time poring over some of the PEAR data. But be careful, I also wanted to say, once you look at the data closely there's no academically safe place to hide. The PEAR data are game changers.

Most readers of this *Journal* will be familiar with at least the outline of the PEAR lab work, as its accomplishments and output have reached almost mythical status. In the late 1970s, Robert Jahn, then Dean of the School of Engineering and Applied Science at Princeton, hired Brenda Dunne to be his laboratory manager, and the rest, as they say, is history. So began an ambitious collaborative program to investigate 1) whether human operators could consciously or unconsciously *influence* the output of random physical systems of various stripes; 2) whether human operators could consciously *extract*

information from the physical environment in ways which would be considered anomalous; and 3) how to construct useful theoretical models which make sense of the experimental data.

These three areas of inquiry roughly translate into the five sections of the book. Section I, *Venues, Vistas, and Vectors,* contains six short chapters which serve as an introduction to the major themes and questions that are discussed at greater length in later sections. There is some history, sociology, and philosophy of science about some grand questions usually discussed only in rarified specialty texts. How does the mind/body problem illustrate the Western science traditional division between the "objective" physical world and the softer "subjective" experience of people? Isn't all "objective" knowledge "subjectively" experienced? In Jahn and Dunne's words, "Mind without matter leaves us with a world of ephemeral abstraction; matter without mind eliminates the essence of life itself."

Section II, *Human/Machine Connections: Thinking Inside the Box*, is the longest section of the book, comprising fifteen chapters, the last of which is entitled "Inconclusive Conclusions." I highlight this last chapter title as an indicator of how careful, thorough, and humble Jahn and Dunne are with their presentation. There is never any overreaching, and when speculative thoughts arise, they are identified as such. These are careful researchers indeed. This section almost overwhelms the reader in its recounting of the scope and depth of inquiry by the PEAR lab. By the end of the section I was intellectually exhausted by exposure to so much data, even as I was titillated and exhilarated by them. The early work with random event generators looked for statistical shifts in the output based on the pre-stated intentions of the operators. Later, so-called field-REGs (portable machines) were taken into a wide variety of locations that were thought to be emotionally "coherent," such as sporting events or musical concerts. Jahn and Dunne give us a statistical primer on interpreting deviations from expected chance that should be comprehensible to the intelligent layperson, so that when they intersperse a selected few graphs and tables it really augments the discussion. Did the results depend on whether the operator was male or female? Do multiple operators add to the effect size? If one operator intends "high" deviations and another "low," do they cancel each other out? Does immediate feedback enhance performance? Does practice improve performance? Does it matter whether the generated random events are "true" or "pseudo"? Does it matter whether the randomness is generated electronically, mechanically, through fluid dynamics? Does distance matter? Time? You get the idea. I don't want to give away the story line, but I do guarantee that you will be swept along with their intellectual playfulness, and you really will care about the results, even as nature keeps hurling surprises at our fledgling attempts to make sense of the world.



Section III, *Remote Perception: Information and Uncertainty*, contains seven chapters on the PEAR "remote perception" work. As in all of their work, Jahn and Dunne decided early on to use "ordinary" volunteers in their experiments rather than specially trained people who claimed a history of producing extraordinary phenomena. Their "operators" were instructed to use whatever subjective techniques that they wanted to either affect the REGs or to gather information from volunteer percipients who were elsewhere, and often not time-synchronized. Some meditated, some closed their eyes, some left them open, some performed a ritual, but all gave the task their own personal stamp. As in the previous section, think of all of the interesting questions that can be addressed: Does distance matter? Does time matter? Does practice improve performance? When the target is correctly perceived, what is the nature of the signal? Again, I don't want to give away the empirical results, because this work reads like a mystery, which in fact it is. To titillate: When their analytical techniques became more sophisticated, the effects weakened. Whew. Most researchers would ignore this as an annoyance or possibly an artifact. Jahn and Dunne unabashedly throw this in the pile of surprises to be thought about.

Section IV, *Thinking Outside the Box*, deals with the mother lode scientific question: How do we make sense of these daunting data? Once again, they face the problem head on. Any scientific model, they write, must deal with a hierarchy of extraordinary features: tiny informational increments riding on random statistical backgrounds; correlations of objective physical evidence with subjective psychological parameters, most notably intention, attitude, meaning, resonance, and uncertainty; time and space independence; oscillatory sequential patterns of anomalous performance; data distribution structures consistent with alterations in the prevailing elemental probabilities; complex and irregular replicability. Whew, again. Their "out of the box" response is to begin with what they call a "Science of the Subjective." In their words:

> . . . any neo-subjective science, while retaining the logical rigor, empirical/theoretical dialogue, and cultural purpose of its rigidly objective predecessor, would have the following requirement: acknowledgment of a proactive role for human consciousness; more explicit and profound use of interdisciplinary metaphors; more generous interpretations of measurability, replicability, and resonance; a reduction of ontological aspirations; and an overarching teleological causality. More importantly, the subjective and objective aspects of

> this holistic science would have to stand in mutually respectful and constructive complementarity to one another if the composite discipline were to fulfill itself and its role in society.

Dare I give one more "whew"? In this section they explore whether quantum metaphors have sufficient power to help us understand their data; whether it is more productive to think of the apparent correlations between the conscious mind and tangible output in a more circuitous route involving unconscious processes (their M^5 model); the place of filters in the communication between consciousness and its Source. Finally, they anticipate the intellectual pushback in the reader reacting to their paradigm-busting presentation. Are the data wrong? Are they real but not important? Should we consider this outside of scientific inquiry? Should we keep working to get back to our safe deterministic models? Should we change the rules of science? Jahn and Dunne, in a masterly essay, recommend the latter. Let's "Change the Rules!"

The final section, *Consolidation and Closure*, is as promised, and presents itself as the most speculative of the sections. As they pose the question of how to distill both their empirical data and theoretical propositions, Jahn and Dunne really let out all of the stops. Again, in their own words:

> . . . these efforts must struggle through the entangling undergrowth of philosophical and functional dogma that has accumulated over eons of endemic human greed, self-serving rationalization, and malicious and inadvertent attentional neglect, to constrain, and often to enslave, our minds, hearts, and souls, and that has brought our species to a precipice of spiritual stagnation that cannot much longer support its survival. Our contributions here cannot be more than puny on the grand scale of such an impending catastrophe . . .

Again, not to give away the punch line, they suggest that traditional science has been focused on the famous equivalence of matter and energy, but they have left information out of their equations. To them, the most facile conceptual language to describe their results is information: in the case of REGs, insertion into the random binary strings; in the case of remote perception, extraction from a global array of possible targets. And returning to the science of the subjective, they implore us to somehow balance the more objective measurements of information quantification with the more subjective sense of personal meaning. Indeed, more attention to such subjective states as "intention," "resonance," "unconscious processing," and more are called for. Imagine "a functionally proactive subjective consciousness . . . added to the arsenal of scientific concepts and tools . . ." Game changer.

This is a beautiful book. I recommend reading it slowly, thoroughly, and reflectively. The prose is rich and is actually aesthetically pleasing. I found

myself reading a chapter, putting it down, reflecting, and then re-reading to find even more nuance. Even consistent readers of the PEAR Lab's more than 150 articles and technical reports (many of which are to be found in the *Journal of Scientific Exploration*) will gain a new perspective as you take in the entire "odyssey" of their work in one publication. The book can also serve as a model of humble, yet relentless, scientific thinking. To dream: Imagine the next generation of scientists reading works like this to balance out the stale textbooks that present knowledge as "finished." Imagine a book that fills you with awe and wonder as it relentlessly presents an incredible challenge to our way of making sense of the world. Imagine the experience of actually having a skeptical, open mind, and coming upon this book. What a gift.

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Aping Mankind: Neuromania, Darwinitis and the Misrepresentation of Humanity by Raymond Tallis. Acumen, 2011. 416 pp. $29.95 (hardcover). ISBN 9781844652723.

What might physician and professor of geriatric medicine Raymond Tallis and actor John Rhys-Davies have in common? In Peter Jackson's epic film *The Lord of the Rings,* Rhys-Davies (as Gimli the dwarf) wields an axe with such consummate skill as to challenge, intimidate, and lend a hand in the defeat of the evil orcs of Mordor; while in *Aping Mankind* Tallis (as philosopher and scientist) with a finely-honed axe of logic takes on perhaps equally formidable foes: those Cognitive Scientists possessed by Neuromania (p. 26) and Evolutionary Biologists obsessed by Darwinitis (p. 40) (called respectively Neuromaniacs and Darwinitics).

An inapt comparison? Orcs are degenerate mutations from a once-benign race, who would destroy or enslave all humankind, while Evolutionary Biologists and Cognitive Scientists are, certainly, benign professionals enriching the store of knowledge for the benefit of all. Yet as Tallis makes abundantly clear, many Cognitive Scientists believe that the mind is the brain, the brain is a computer, and since a computer has no self and does not exist in a world of intentionality, human beings have no selves and do not exist in a world of intentionality (p. 101).[1] Some biologists and psychologists, influenced by the twin premises that the brain is a product of evolution and that the mind is a computer–brain,

reduce mind to a single-purpose biological mechanism programmed only to ensure the survival of the gene pool. The wedding of the mind–brain–computer theory to the Darwinian impulse produces a "grand synthesis of Darwinitis and Neuromania" against which Tallis mounts his argument (p. 145). He is convinced that these views, riddled as he believes they are with faulty logic and bad science, are in fact dangerous.

> The distinctive features of human beings—self-hood, free will, that collective space called the human world, the sense that we lead our lives rather than simply live them as organisms do—are being discarded as illusions by many, even by philosophers. . . . Such views may have consequences that are not merely intellectually derelict but dangerous. (p. 8)

In this densely packed, well-researched, and carefully argued volume of more than 400 pages, Tallis offers penetrating critiques of assumptions prominent in Neuroscience and Evolutionary Biology and lays out what he considers to be their potential negative consequences. The book moves on to a "Defense of the Humanities" and a stimulating though inconclusive effort to provide a solution to the chief problem raised by his analysis of the mind–body relation.

What Is the Danger?

Tallis argues that the conclusions of Neuromaniacs (NMs) and Darwinitics (DTs)[2] have added weight to traditional determinism with its corollary that there is no such thing as personal responsibility (pp. 49–50). Although it is not new to debate the existence of free will, Tallis holds that the incursion of neuroscience into our sense of ourselves as conscious agents is "more up close and personal . . . [and] the personal gives way before the impersonal" (p. 51). Thus he cites the view of neurophysiologist Colin Blakemore:

> The human brain is a machine which alone accounts for all our actions, our most private thoughts, our beliefs. . . . All our actions are products of the activity of our brains. (p. 50)

In referring to private thoughts and beliefs, this does not go far enough; a significant theme in neuroscientific circles is eliminative materialism, which argues that thoughts are merely the flow of physical energies within the computer–brain, and beliefs are illusions of a "folk psychology" eventually to be replaced by a new conceptual framework provided by neuroscience.[3] And since the self is an illusion, the idea of any thought being private is also in error, since there is nothing for a thought to be private *to*. The brain is a machine which has no thoughts and no beliefs. And "you" are "your" brain (but there is no you).

Surely, though, the vast majority of humanity will just go right on "thinking" that "they" have "beliefs" (even NMs and DTs seem unable to avoid this illusion), so why worry? Tallis's first concern is a perceived potential for fostering human self-hatred. His book begins with a keynote citation from *Straw Dogs* by John Gray, Professor of European Thought at the London School of Economics, in which Gray is reported as saying that the lives of humans, who are rapacious, destructive, predatory animals, are obviously not worth preserving, and have no more meaning than that of a slime mould (p. 1). Thus, Tallis's first concern has to do with psychological consequences: encouragement of despair and inactivity (p. 64).

A second "even more frightening" concern rests on a proposed NM solution to the destructive aspect of the human animal–machine: Legal and governmental decision-making should be determined by neuroscientific understanding of the brain's "system of justice" and of how the brain reacts to conflicts. Although this may appear extreme and even irrational (if the brain is that of a rapacious predatory animal, why should anyone trust its system of justice?), such ideas are in fact being considered (p. 65, citing Zeki & Goodenough, 2006.)

A third concern, characterized by Tallis as sinister, is what may follow from the notion that there is more of the animal in some people than in others. Tallis here cites views from which it would follow that we should treat mentally handicapped human beings as we would animals (p. 68). Tallis does not go so far here as to suggest whether the determination of who, or of what group or population, is handicapped, should be placed in the hands of NMs and DTs, but it is a reasonable question to ask.[4]

One might think such views are of little consequence because they are supported only within relatively small areas of scientific study and academic commentary. Not so: Tallis notes that the idea of neuroscience having dominion over territory that once belonged to the human sciences is fostered not only incessantly in the popular press but in the burgeoning growth of disciplines such as neuro- or evolutionary- jurisprudence, economics, aesthetics, theology, architecture, archaeology, and ethics (p. 58).

Of particular interest to this reader is Tallis's commentary on the incursion of NM/DT assumptions in aesthetics. It would seem reasonable to hold that the existence of the arts testifies most strongly against the notion that human beings are computers driven by neuro-biological programming. What has a machine after all to do with ballet, opera, string quartets, or the Night Café? But Tallis reports a different view.

> The aficionados of neuroaesthetics explain the impact of different kinds of art by referring to what is seen on fMRI scans. . . . The creation of art itself is a neurally mediated activity by which the artist, unknown to himself, behaves in such a way as to promote the replication of his genetic material. (p. 58)

If Vincent van Gogh had understood this explanation of his artistic endeavors, one could readily understand why he sliced off his ear: His entire life's work had no more value than the satisfaction of lusts in copulating animals. (But since he had no knowledge of neuroaesthetics, there must have been something else wrong with his brain.)

A key term here is value. In the purposeless world of material science (and of eliminative materialism), there can be no values, since value rests on beliefs, purposes, goals, satisfactions, and disappointments. Divesting the world of intentionality is divesting it of meaning, and the psychological condition of living in a meaningless world is Nihilism (p. 66). Over a hundred years ago, Nietzsche came to the following conclusion upon considering the rise of science, the desire for supernaturally sanctioned truth, and the relation between value and purpose:

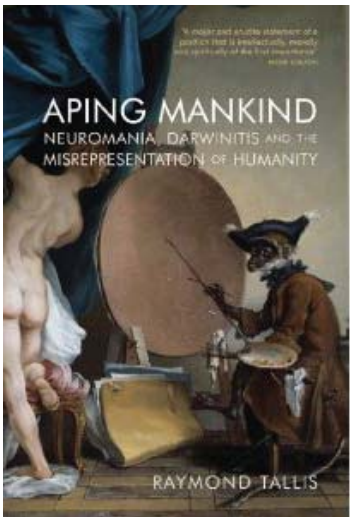
> What I am now going to relate is the history of the next two centuries. I shall describe what will happen, what must necessarily happen: the triumph of Nihilism. . . . What does Nihilism mean? That the highest values are losing their value. There is no bourne. There is no answer to the question: To what purpose? . . . Thorough Nihilism is the conviction that life is absurd. (Nietzsche, 1910, Preface and p. 8)

Seen from the perspective provided by Tallis, one may wonder whether this prediction is well on its way toward fulfillment. Surely it is cavalier, with the presumed authority of science, to divest the world of meaning by intentionally denying the existence of intentionality. But whether the dire result feared by Tallis is avoidable or inevitable would seem to depend on the question of whether the identification of humanity with animality and of the mind with a computer–brain is founded on truth or scientistic confusion.[5]

The Two Towers of Scientism

Of course Tallis is speaking not of Neuroscience or Evolutionary Biology in general, but only of the allied edifices of NM and DT. Tallis devotes Chapters 3 and 4 to a scathing critique of each in turn. There are four primary lines of argument which he raises against NM: argument from methodology and technology, from causality, from the phenomenological description of consciousness, and from logic.

Regarding the first, Tallis describes limitations of fMRI brain scans (p. 74), oversimplified experimental designs (pp. 74–77), evidence for non-modular distribution of brain activity such as memory (p. 80), and other technical and procedural limitations. However, since it can always be argued that such limitations may, with better technology, be overcome, Tallis must take into account other dimensions of the problem.



The first of these is the attribution of the causes of conscious states to specific areas of the brain. Giving a causal status to putative functional modules in the brain raises an acute problem of conceptual confusion among three quite different relations: correlation, causation, and identity. Mere correlation of a particular area of brain activity with some specific mental activity cannot serve as proof that a specific locus of brain activity is the sole cause, or even identical with, the associated mental activity (p. 83). Against the assumption of discrete modules, Tallis points out that when a particular area of the brain becomes active in the presence of some stimulus, much more of the brain is already active (p. 75). Tallis shares this point with other critics who hold that the brain is a necessary but not sufficient condition for consciousness, and that one cannot separate the brain from the nervous system as a whole (e.g., Rockwell 2007, Chemero 2009).

What Tallis adds, however, is a depth of detail and a broadened perspective not usually encountered in similar critiques. Tallis opens up the greater sphere of experience that he posits is systematically neglected in the discussion: the human world. "Even those who locate the roots of consciousness in the brain should still recognize that brains together create a space that cannot be stuffed back into the brain" (p. 235). For example, speaking of studies claiming to have found the location of unconditional love in the brain by recording brain activity while the experimental subjects look at photographs of those with whom they are deeply in love, he says "anyone who is not a Martian" knows that

> Love is not like a response to a simple stimulus such as a picture. It is not even a single enduring state, like being cold. It encompasses many things, including: not feeling in love at that moment; hunger; indifference; delight, wanting to be kind; wanting to impress; worrying over the logistics of meetings; lust; awe; surprise; joy; guilt; anger; jealousy; imagining conversations or events; speculating what the loved one is doing when one is not there; and so on. . . . The more you think about the idea that human life can be parcelled out into discrete functions that are allocated to their own bits of the brain, the more absurd it seems. (pp. 75–80)

In many ways, Tallis's book is an extended, impassioned evocation of this greater world. This is his third argument, that from the phenomenological description of human experience, the explanation of which he says is not even

remotely approached by NMs or DTs. By limiting the concept of experience to an artificially narrow range, simplistic experimental designs and broadly brushed conclusions are made to seem reasonable. Tallis, we might say, is inviting NMs and DTs, like the denizens of Plato's Cave, to come out into the vastly wider realm of truth, fact, belief, error, beauty, love, community, and, in short, intentionality; putting into proper perspective the endeavors and findings of science as these may enrich, rather than impoverish, that world (p. 91 passim).[6]

Logic and Language: A Failed Attempt at Conceptual Judo

Here however we are brought to the fourth of the criticisms mentioned above: logic. The claim that intentionality is an illusion, and that such things as beliefs do not exist, appears to plunge NMs into a morass of self-contradiction: They believe that their beliefs do not exist; that they themselves do not exist. But as Tallis puts it, it is not possible to deny viewpoint (pp. 112, 336–338). Against this, neuroscientists have argued that such seeming contradictions emerge only because we (temporarily) must use the defective language of folk psychology; but just as soon as the neuroscientists provide a linguistic framework reflecting the true reality, such contradictions will simply go away (cf. Churchland, 1986).

This defense is like a move in martial arts: Use your opponent's strength against him. But that move can easily be reversed. The language the NM calls for in fact already exists. It is the language of the physical sciences. Only the purely physical description of the universe is real, and whatever does not fit that framework is an illusion.[7] And that is an example of what philosopher John Dewey long ago termed *the* philosophical fallacy: taking objects of selective preference and converting them into antecedent existence, i.e. into the fundament of reality (Dewey, 1958:25–30). The NM is not asserting a scientific truth but is instead promulgating a metaphysical doctrine.

Although Tallis never cites Dewey (he is not even listed in the 16 small-type pages of references), Tallis's argument from the quality of human experience is strongly reminiscent of Dewey's views. Advocating substitution of the language of the Physical Sciences for the language of intentionality creates a schism between the specialized sciences, with their plethora of abstract theoretical entities, and the world of experience and common sense out of which those sciences grew and to which they are irretrievably related. The allegedly stable, certain, and unchanging law-like character of the physical sciences, in contrast to the unwieldy face of the experienced world, promotes the refined objects of science to a level of selective preference as the pure reality. Then

> The stable ideal meanings which are the fruit of nature are forbidden . . . from dropping seeds in nature to its further fructification. (Dewey, 1958:58)

As I read him, this is the central point of Tallis's deeply felt concern: Instead of running away from the world of common sense and calling it pejorative names such as folk psychology, science should always, no matter how abstract its theory becomes, return to that world and give back with interest what it has taken. The startling degree to which the neuro- and evolutionary-pseudosciences manage to impoverish the world is starkly highlighted in a summary Tallis gives in Chapter 9 (p. 337).

Misplaced Anti-Animality

Tallis to this point has presented a richly detailed and convincingly argued position, and he does this in a highly readable style. But now we come to a less satisfactory discussion. Tallis still must put Evolutionary Biology in its place. Here he commits a large-scale blunder. In order to reject the idea that all human behavior is explained by reference to animal instinct driven exclusively by the mechanism of natural selection, he finds himself having to advocate a yawning gulf between animal life and human experience. The form his argument takes is to mount a wholesale denigration of animal existence. Human behavior is *fundamentally* different (p. 233). He comes close to concatenating animals with insentient matter (p. 232). Animal vision is "programmed response" while that of humans is "the gaze which looks out and sees" (p. 171). Animal life, in contrast to the human shared world, is rather a world of "bumped-into objects and forces," seeming to suggest that animals are little different from billiard balls. Animal emotions are exhausted by the "rapid heart rate and increased respiratory rate of a beast being prepared for fighting, fleeing, feeding, or copulating" (p. 233). It is, he says, bad biology to assimilate animal emotions to human feelings. He does not appear to realize that it is also bad biology to assimilate animal behavior to that of billiard balls.

Animals as he sees them also do not live in any dimension of time. While human experience has temporal depth (p. 250), the behavior of animals such as crows caching food for future use does not indicate any sense of future need but is rather a mechanical hard-wired activity with no relation to a felt need or the existence of a future (p. 134). In such a view, an animal chasing a prey, for example, is not pursuing a goal within a temporal dimension, but is merely reacting mechanically from one instant to the next, with all the composite instances being separate billiard-ball reactions to separate stimuli. There is no telic quality (possibly rudimentary intentionality) to animal behavior, this despite the testimony of biologists such as Edmund Sinnott, who speaks of the "persistent directiveness or goal-seeking that is the essential feature of behavior and thus finally the basis of all mental activity" (Sinnott, 1955:52).

Sinnott is not a DT. He does not reduce human behavior to that of animals. But he posits a continuity in the development of consciousness from animal to

human life. Such continuity works both ways: There is something of the animal in the human, but there is also something of the human in the animal. Tallis is exaggeratedly wary of admitting continuity as it might apply to consciousness because he mistakenly feels that to admit any incremental or significant developmental flow from animal to human is to give in to the DTs. We shall see however that although Tallis repeatedly gives the (sometimes excessively brutal) impression that nonhuman animals have nothing like a conscious existence, he cannot hold firmly to this position, and it causes him trouble when he arrives at his concluding attempts at a remedial theory.

The Wrap-Up: A Stimulating Theory That Stumbles

His first step is to highlight the difficulty which his own argument has created: the acute difference between the human world and its biological and material predecessors. He has concluded that natural selection is a "mindless, pointless process" that has no goal and is thereby in stark contrast to the human world. He believes this, he says, because he is an atheist humanist. In other words, in his view, to believe that evolution has a goal is to believe in a supernatural designing deity (p. 209). It is here that Tallis's penetration begins to weaken. For one thing, the possibility that there might be some degree of directionality, i.e. a telic property within the sweep of evolution but not necessitating a designing deity, does not seem to occur to him (or perhaps he does not think it worthy of consideration). He does not distinguish between the having of goals and the having of one overall *Goal* (my capitalization). Evolution has no *Goal* but humans are able to consciously aim at stated goals, which, he concludes, means that "humans are not a part of nature or not entirely so" (p. 210).

The rather peculiar syllogistic reasoning appears to be this: Evolution is part of nature; evolution has no *Goal*; humans have goals; therefore humans are not part of nature (or not entirely so). The caveat "not entirely so" renders this less fallacious; but in any case the difference between *Goal* and goal is overlooked. The argument might be recovered by the following: Animals have no goals (*sans* capitalization); humans have goals; therefore humans are not animals (or not entirely so?). In order to make this work, however, he must deny that a hawk in search of prey, a titmouse building a nest, or a beaver chewing a branch off a tree for use in constructing a dam, have goals; or if we must say they have goals, we must distinguish between animal goals and human goals. Human goals are conscious, anticipated, explicit. Animal "goals" are so only by misplaced analogy. The animal is without consciousness. It has no existence in time, no past or future, no anticipation. For the animal, nothing is or can be "explicit."

This appears to me to be what Tallis wishes to say when he is attacking Darwinitics. But now a different story arises. If, as he has argued, humans are not a part of nature or not entirely so, the absolutely necessary requirement for

a coherent view is to answer, without appealing to supernatural intervention or alien devices hidden in monoliths, the question: How did humans get to be so different? (p. 210). This Tallis attempts, although tentatively, to answer.

Let us review once again the immensity of the difference as understood by Tallis. It is not the difference between, for example, ordinary chimpanzees and exceptionally gifted chimpanzees (p. 212). It is not a minute incremental step on a ladder of progress responding to some teleological impulse inherent in the processes of life. It is not simply the advent of a larger frontal cortex (p. 213). No. It is a shock, a jolt, the advent of something stunning. And that something, Tallis has it, is the human hand with its opposable thumb, its ability to be used for grasping and pointing, and in particular its placement upon an upright bipedal body that allows it to be seen at a distance from the head but at the same time to be felt as a part of the whole: "The thumb . . . taken in conjunction with the upright position, transformed the primate hand into a proto-tool" (p. 213).

Yet the question remains: What has a better paw got to do with bridging his carefully, painstakingly constructed, immense gulf between unconscious animal life and the world of consciousness inhabited by humans? Tallis engages in an elaborate account of what he considers the reasons that "something so small as the hand . . . should have had such momentous, indeed massive, consequences." It is a clever, stimulating, and interesting demonstration of the functional relationship between the hand, upright posture, the opposable thumb, the extending of the arm, and the visibility to the eye of the hand's actions. And it has a convincing ring to it. But none of it answers the fundamental question as to how something totally unconscious and without any sense of self or existence in time, can come to experience temporal depth, become conscious, and become conscious of itself *as* a self. It is here, at the crucial moment, where Tallis's scenario collapses.

> The hand . . . made the human animal, our hominid ancestor, uniquely aware of its own actively engaged body. This awoke the dim intuition that I *am* this body. (p. 212)

So the key, the turning point, is the "awakening" of a *dim intuition*. But how can an intuitionless being have an intuition? "Intuition" belongs to the language of mind; an intuition can occur only to a self—even if it is "dim" and is occurring to a limited kind of self. Otherwise "dim intuition" is just a couple of words explaining nothing. And calling this intuition *dim* admits of degrees. Something was there, some kind of self-consciousness, as a necessary condition for the having of any intuition whatsoever. And if we grant the possibility of a "dim intuition" there is no way to avoid the possibility of a *dim* anticipation or a *dim* sense of having a goal: a dim, but nevertheless extant, temporal depth.

What he has stumbled upon, driven by the force of his own reasoning, is a theoretical position similar to that of thinkers such as Pierre Teilhard (de Chardin), another profoundly relevant and much-neglected philosopher who is also not within the sphere of Tallis's references.

> Properly observed, even if only in one spot, a phenomenon necessarily has an omnipresent value and roots *by reason of the fundamental unity of the world*. . . . Consciousness is completely evident only in man we are tempted to say, therefore it is an isolated instance of no interest to science. . . . Consciousness is evident in man, we must continue, correcting ourselves, therefore, half-seen in this one flash of light, it . . . is surrounded by an aura of indefinite spatial and temporal extension. In the world, nothing could ever burst forth as final across the different thresholds successively traversed by evolution . . . which has not already existed in an obscure and primordial way. (Teilhard 1961, my italics)

My point here is not to argue for the validity of Teilhard's view, which is nonetheless vastly more accommodating than Tallis's attempt to slip consciousness in where he has previously fought to deny it. The point is that Tallis cannot get out of his dilemma without admitting a prior development of degrees of consciousness within the evolutionary process, thereby arriving at a position close to that of Teilhard.

The real contrast, then, seems to me to be between the closed world empty of consciousness and deprived of selfhood as envisioned by the NMs and DTs sitting huddled with the others in Plato's cave, or an open world of continuity within which human consciousness is a part of nature simply because in one degree or another, the spawning of consciousness is an entirely natural phenomenon and extends somehow to the roots of matter.[8] If the latter is one's choice, and if that choice means a revolution in our understanding of matter and of a healthier relation between science and humanity, so be it. It should be a conclusion with which, however reluctantly, Tallis must agree.

Notes

[1] Intentionality refers essentially to the sphere of meaning, as evidenced in what are called propositional attitudes such as hopes, desires, fears, and, more broadly, beliefs, which are directed at objects . . . or clusters of possibilities that are felt to be other than the subject (p. 101).

[2] These capitalized abbreviations are my own. Since the terms in full have a rhetorical purpose but nevertheless do refer to specific theoretical attitudes, the more or less neutral abbreviations are preferable.

[3] For a description and criticism of these views, see Will Wilkenson, *Churchland Debunked, Commonsense Psychology Vindicated.* http://enlightenment.supersaturated.com/essays/text/willwilkinson/churchlanddebunked.html

[4] Tallis gives a more incisive discussion of this concern in his small volume *Why the*

Mind Is Not a Computer, pointing out how scientistic ideas contributed to oppression of the Jews in Germany and the Kulaks in Soviet Russia (Tallis, 2004:26).

[5] Scientism is "the mistaken belief that the natural sciences . . . can or will give a complete description and explanation of everything" (p. 13).

[6] Tallis' view of a *World* here is reminiscent of John Dewey's concept of "Experience" as a fundamental category of being and the obligation of science to enrich, rather than diminish, it. Dewey's philosophical work has emerged from undeserved obscurity recently as a major influence in critiques of the mind–brain identity theory (Noë, 2009, Chemero, 2009, Rockwell, 2007).

[7] An example of this fallacy outside the realm of neuroscience: "From a quantum world view, we and the things around us are mostly empty space. The way we experience ourselves . . . is really just a 'figment of our imaginations shaped by our senses.'" This from ASU Regents' Professor David Ferry reported at http://www.physorg.com/news197266420.html, July 2, 2010.

[8] Considering matter as an expression of energy, Teilhard proposed a revision of the concept of energy. This involved his hypothesis of the existence of a radial energy, which is the energy leading to organization, specifically the functional organization of matter around a center which he called the within of things (Teilhard, 1961:63ff.). The question of the relation of this concept to standard physics has been discussed at length in O'Manique (1969).

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Consciousness Explained Better: Towards an Integral Understanding of the Multifaceted Nature of Consciousness by Allan Combs. Paragon House, 2009. 196 pp. $19.95 (paperback). ISBN 9781557788832.

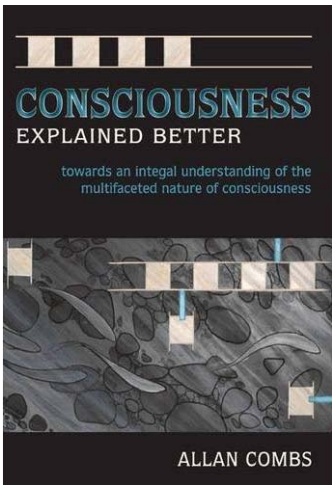
In this accessible book, Allan Combs takes on the daunting task of addressing the subject matter of consciousness. The title, *Consciousness Explained Better*, alludes to the title of Daniel Dennett's book *Consciousness Explained*, in which Dennett gives a reductionist account of consciousness. But the title of Combs's book is misleading in that his book consists of a *description* of the structures of experience and not an *explanation* of consciousness, nor of experience or its structures. However, for those who are unfamiliar with a developmental approach to the structures of experience, this book is a good introduction.

Combs starts by taking the reader back to William James's "world of pure experience," conceptually prior to the splitting of experience into subjective and objective distinctions. His main thesis is that the actual events that occur for us depend upon the structures through which our experience is lived. These structures form a developmental sequence from Jean Piaget's sensorimotor period through his formal operational thinking and then, linking with Ken Wilber's cosmology, to various levels of transpersonal mentation culminating in "nondual awareness" (p. 84), which Combs characterizes as the "*Ever-present ordinary mind; the direct experience of the nondual ground*" (p. 100). For Combs, these stages not only take place within the individual development of people, but can also be seen as giving rise to historical time periods in which they were first expressed. For this, Combs brings in the work of Jean Gebser and provides examples from science and art to illustrate that contention. With reference to the writing of Sri Aurobindo, Combs pays particular attention to the first developmental stage after formal operational thinking, that of "Integral or Vision Logic" (p. 100), where "multiple perspectives" (p. 144) can be held simultaneously. Combs wants this to be more than just "a laundry list of characteristics" and considers the relationship of integral consciousness to "enlightenment" (p. 144).

The main strength of this book, to my mind, is the adoption of such a broad approach to its subject matter that it forces us to reconsider the usual framework within which consciousness is discussed. In particular, according to Combs,

> the distinction we commonly make between our "inner" perspective of thoughts . . . and our "outer" perspective of the external world . . . has not always been with us. (p. 111)

Combs identifies the cleaving of experience into subjective and objective aspects with the cogitations of René Decartes. But such splitting might not



actually tell us anything about the nature of experience, just about the way it manifests for us. I think it is clear that we usually tend to choose some variation on these two aspects for taking an ontological stand, so that we end up with mental monism, materialism, or some form of dualism. In particular, we sometimes try to explain away mental events using neuroscience. Initially, Combs refrains from taking any metaphysical step. He goes on to make distinctions, including distinctions between the inner and the outer, but these are simply reflections of the heuristics humanity has used for parsing experience. It is not until the second half of the book, when he has laid out a multiplicity of ways that experience can be structured, that he revisits metaphysical questions by asking whether

> we create these realms of experience . . . through our own modes of thinking [or whether they are] already part of the Kosmos waiting for us to refine our mental instrument sufficiently to detect and experience them. (p. 91)

Combs leans toward a "perennialist view," and, in the end, asserts that there are "universal dimensions of consciousness that mark us as human beings" (p. 144).

Although Combs's account is largely descriptive, I think that he has discerned the crux of the solution to the problem with consciousness when he elucidates the nature of experience before its evolutionary split into subjective and objective aspects and after its recombination into the nondual state of being for those for whom such transcendent events have occurred. In doing so, Combs's work can reset the course of consciousness studies onto a more productive track. And I would very much like to see him, along with other researchers, develop these ideas in such a way as to explicate the nature of experience and the reasons for its manifestations.

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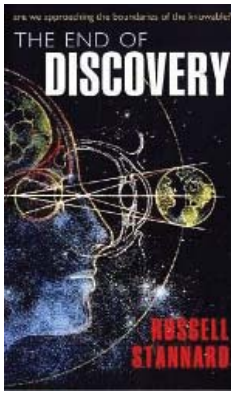
The End of Discovery: Are We Approaching the Boundaries of the Knowable? by Russell Stannard. Oxford/New York: Oxford University Press, 2010. 228 pp. $24.95. ISBN 978-0199585243.

In 1872 the physiologist Emil du Bois-Reymond surveyed "The Limits of Science" in a keynote address to an annual meeting of his colleagues. Eight years later he returned to the subject in a lecture to the Prussian Academy of Sciences that discussed "Seven Shortcomings" in our understanding of the world. Reprints of both speeches did well: "The Limits of Science" went through eleven editions in German, not counting sales in English, French, Italian, Romanian, Serbian, and Russian. As du Bois-Reymond admitted, he was far from the first to delimit the boundaries of knowledge: Philosophers from Locke to Kant had referred to unanswerable questions, and scientists such as John Tyndall and Thomas Henry Huxley had indicated the failures in their mechanical models of nature. But du Bois-Reymond had a knack for rhetoric, and his audience had expected him to defend the efficacy of reason in overcoming ignorance and superstition. Contemporaries reported that his speech hit them "like the unexpected explosion of a mine," coming as it did from "the center of the center of science," his chair at the University of Berlin, the leading university in the world at the time.



Since then authorities on science have imitated du Bois-Reymond's example. Some, such as John Horgan or John Barrow, have developed the argument of his first lecture, pointing to the essence of matter, the nature of consciousness, and other riddles impervious to the investigations of cosmology and neuroscience. Others, like Roger Penrose and Stephen Hawking, have taken up the theme of his second lecture in reviewing the outstanding problems of their field. Russell Stannard belongs to this second category of scientific popularizers. After a nod to the philosophy of mind, he devotes eleven chapters to the current state of astronomy and physics. His book has the merit of clear exposition and easy style. In terms of originality, however, it is no match for the theses of Penrose and Hawking, and in terms of insight, it pales beside the analyses of Horgan and Barrow. Anyone with any abiding interest in whether science has limits would do better to look at the work of Stannard's peers, if not his predecessors.

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The Kensington Runestone: Approaching a Research Question Holistically by Alice Beck Kehoe. Long Grove, IL: Waveland Press, 2005. 102 pp. (figures, bibliography, index). $14.50 (paperback). ISBN 9781577663713.

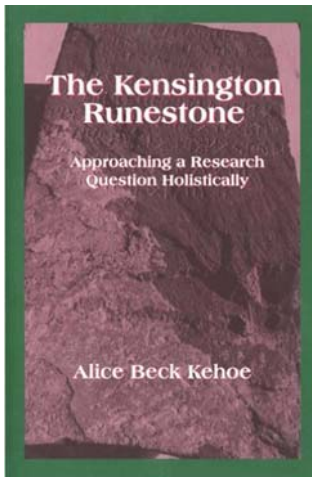
Veteran anthropologist Alice Kehoe has written a small volume using the Kensington Runestone controversy as a cautionary case study illustrating the need for a multidisciplinary approach in assessing antiquities, their contexts, and their implications. In the process, she reveals her own positive conclusions regarding one particular object, which inadequately informed professionals have routinely relegated to the official dustbin for infamous fakes and hoaxes.

The Kensington stone is a yard-long, shaped slab of greywacke on whose surface appears an incised inscription in Scandinavian runic characters and language. The inscription includes a declared carving date of 1362. In her first chapter, Kehoe discusses the initial reports of the finding of the stone, clasped in the roots of a modest-sized aspen tree that was being winched out of the ground on the farm of one Olof Ohman, near Kensington, Minnesota, in 1898. The stone and copies of its text came to be circulated to regional universities as well as in Scandinavia, with inconclusive results. In 1907, avocational historian Hjalmar Holand examined the object, became convinced of its authenticity, and promoted its study, involving, among others, the Minnesota Historical Society.

The Historical Society submitted the text to Scandinavian linguists, all of whom pronounced the inscription to be a modern fake. This conclusion was based partly on the perceived improbability of fourteenth-century Scandinavians being in Minnesota, partly on the flimsily based suspicion that Ohman had faked the stone to promote Swedish pride, and partly on the linguistic and paleographic bases of words, runes, and usages seeming to be anachronistic, without attestation in other fourteenth-century documents.

In 1909, Minnesota's most eminent geologist examined the stone, noted significant patination in the runic incisions, and declared that in his opinion the object was genuinely old. However, in view of the linguistic objections, the Historical Society declined to conclude for or against authenticity.

Over the decades, the debate went on. Various Scandinavian-language specialists—notably UCLA's Erik Wahlgren in 1958—studied the text and the runes rather cursorily and affirmed fakery, and this conclusion became mainline belief. A pro-Runestone book by Romance-languages philologist Robert A. Hall, Jr., published in 1982, was rather technical, and it impressed some linguists but few, if, any, professional archaeologists or historians. The same may be said for the work of American avocational runologist Richard Nielsen, who speaks Danish as well as English. He has demonstrated that essentially all of the



perceived anachronisms and other anomalies in the stone's text are now attested by examples from fourteenth-century Scandinavia—most recognized by scholars only after the discovery of the runestone—and that the language could be specifically assigned to the spoken Bohuslän dialect; a nineteenth-century forger could not have known of forms not yet attested.

In 2000, a Minnesota professional petrographer, Scott Wolter, began an examination of the Kensington stone, and in 2004 issued his report. There were, indeed, root marks on the back that corresponded to Ohman's and others' declarations and drawings. Those runes that had not been subsequently retouched had the same patination as the worked side of the stone, including minute pits where pyrite had weathered away over time. Comparing the degree of weathering of the pyrite in the characters on a runestone known to have been faked in 1985 and which had remained fully exposed to the elements, Wolter concluded that the Kensington stone must have been buried in the ground long before Ohman came on the scene. This view was reinforced by examination of three tombstones in Maine dating from 1805–1815, which showed less weathering than did the Kensington object.

Kehoe notes that although tuberculosis, a disease of Old World origin, is recognized as also having existed here and there in the Americas since quite early times, an epidemic appears to have occurred in the Midwest around A.D. 1000—centuries earlier than the stated date of the Kensington stone but compatible with a Greenlander Norse introduction in or near Newfoundland, with Indians spreading it inland via trade routes (pp. 54–55). She doubts, however, that the Mandan of the Great Plains carried pre-trader Norse or Welsh genes as some have proposed on the basis of historic accounts of some Mandans' manifesting light skin and reddish-to-blond hair (pp. 55–58).

The author makes the point that within Europe, for centuries medieval Scandinavians traveled the rivers as traders, into Russia and to as far from Norden as Turkey. She asks, if this is so why couldn't Norsemen have managed to do likewise once they reached North America, which they are known to have done (in Newfoundland, where the Norse l'Anse aux Meadows site of around A.D. 1000 was discovered in 1960). In fact, might not North America have attracted peripatetic Scandinavians after the Hanse cut off their fur-trade access to Russia around A.D. 1360 (pp. 67–68)?

Concludes Kehoe, the question of the authenticity of the stone is less

important as testimony of a particular historical event than it is in terms of its possible implications for larger questions such as the late-fourteenth-century changes visible in Midwestern archaeology and the fifteenth-century expansion of European voyages of exploration. There is, especially among Americans, psychological resistance to the idea of overseas-originating activity in America before 1492, owing to the myth that America remained in edenic isolation until Columbus discovered that "new world," a discovery that sparked a migrationary response that rapidly led to European settlement and development of this newfound paradise, a paradise that would already have been conquered and populated by Europeans had people of the Old World possessed access to it previously. Thus, "It is a real paradigm shift to believe that the Americas have never been isolated" (p. 83).

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Born on a Blue Day by Daniel Tammet. London: Holder & Stoughton, 2007. 284 pp. £6.99. ISBN 9780340899755.

Embracing the Wide Sky by Daniel Tammet. London: Holder & Stoughton, 2007. 392 pp. £7.99. ISBN 9780340961339.

Readers will be familiar with Asperger's syndrome and autism spectrum disorders due to the success of the film *Rain Man* giving a picture of a so-called autistic savant based mostly on the real life of Kim Peek. It should be emphasized that only about 10 percent of autistic individuals show marked savant talents. Daniel Tammet, the author of the reviewed books, eventually gained a diagnosis of Asperger's syndrome, which is the part of the autism spectrum disorders characterized by usually high functioning individuals with poor motor ability. Daniel Tammet became something of a celebrity when he appeared in a BBC documentary and on the CBS News program *60 Minutes* because of his extraordinary mathematical and linguistic abilities.

Daniel is quite extraordinary and unique. He is not only highly intelligent (with an estimated IQ of 150) but also socially gifted. What is almost unique among savants is Tammet's ability to introspect and share whatever important information he can glean about the nature of his abilities. Allan Snyder, a leading researcher in this area, writes:

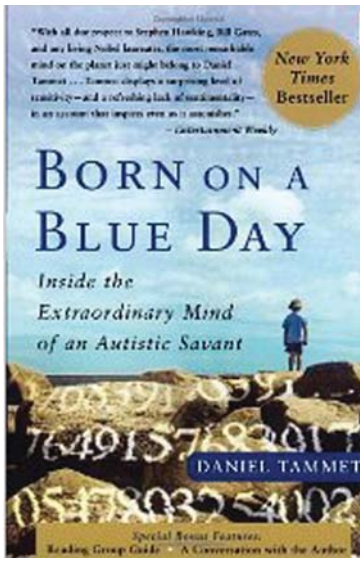
> Savants cannot normally give insight into how they perform their skill and are uncontaminated by learned algorithms. It just comes to them. They just see it. With maturity, the occasionally offered insights are suspect, possibly contaminated by the acquisition of concepts concerning their particular skill. Yet, I have labelled one savant, Daniel Tammet, a Rosetta stone. (Snyder, 2011:3400)

The Introduction to *Born on Blue Day* is written appropriately by Darold Treffert, probably the world's leading authority on Savant syndrome and the advisor for *Rain Ma*n. He has described Daniel Tammet as "articulate, soft-spoken, pleasant, gentle, and modest." I would also add benevolent and caring. Given that Introduction, Daniel can indeed ask "How could I—an otherwise healthy young man with a partner, job, and friends—be considered a 'rain man'?" Simplistic psychiatric diagnoses are at best describing a final common pathway and at worst a means of hiding ignorance and which can become a dangerous means of creating negative expectancies.

Undoubtedly, Daniel did fulfill many of the diagnostic criteria for Asperger's syndrome, and therefore it becomes enlightening and inspiring to follow his biography in this book describing his road toward becoming a socially skilled individual and his achievement in turning his predispositions into positive effects rather than being handicapped by them. Nevertheless, it should be emphasized that Daniel's biological potential may have made him more amenable to the influence of psychological and family factors than would be the case for children with a lower potential. All this is detailed in his first book: *Born on a Blue Day*, which I recommend reading before continuing with the subsequent followup, *Embracing the Sky*.

The latter book is an insightful review of the functioning of Daniel's abilities against the background of what is known as the psychology of cognition and intelligence, and it asks what might savants teach us all about our own minds. But first the book tries to answer the question which we all might want to ask about savants: How do they do it?

What is first evident from Daniel's childhood as related in *Born on a Blue Day*, is that he is not just a case of misdiagnosis or that he merely suffered from a very mild form of Asperger's syndrome. His difficulties appear to have been rather pervasive and typical of Asperger's, including as they did, not only perceptual–motor obsessions, asocial behavior, difficulties in spatial relationships, but also temporal lobe epilepsy. Much of this is a success story, and if we give credit to his story, and I think we should, the above difficulties were ameliorated by the warmth, guidance, and unconditional love of his parents. The parents wanted to avoid the stigma of diagnosis and its self-fulfilling prophecies. His epilepsy clearly did require medication, but this was phased out as soon as possible. Apparently Daniel's seizures originated in the



left hemispheres which fits the known finding of damage to the left hemisphere being a common occurrence in savants. One theory is that since the mathematical and language skills of savants are often associated with the right hemisphere (rather than the left as in normal individuals), then a transfer of function from the left to the right hemisphere may have involved some form of overcompensation (Treffert, 2000).

Rather than as we might expect, excelling as a child in literacy skills, Daniel records how he was actually late in acquiring reading and writing proficiency. His synesthetic ability seems to have first come into being primarily as a basis for his numeracy but later it may also have facilitated his imagery associations concerning words. Eventually this capacity and his own particular drive and interest in words gave fruit so that his language skills became his forte. The access to fantasy meant that in leading a solitary childhood, numbers and letters became his substitute friends. Although some authorities might dispute the generality of this, Daniel writes of this isolation (p. 98): "People with Asperger's syndrome do want to make friends but find it difficult to do so." During his childhood, Daniel was a loner often teased by his peers, and like many such individuals he developed an imaginary playmate. In Daniel's case, the playmate seems to have gained a greater degree of independence and served to give him support and reassurance. Being the first in a line of what were eventually nine children—with parents who were neither rich nor Catholic but simply liked children—actually in Daniel's case became an advantage since his siblings provided him with models and opportunities for finally developing his own social skills. Nevertheless, his parents seem to have treated his peculiar obsessions with the optimal combination of both tolerance and the gentle setting of limits. One obsession even reached a physical limit when his fixation on collecting piles of horse chestnuts finally threatened to cave in the floor of the house and damage the ceiling of those living below.

Born on a Blue Day gives a very insightful account of the demands on childhood development which most of us easily overcome but which for many children with Asperger's become persistent and dire problems—such as learning to ride a bicycle, understanding the importance of personal space, distinguishing left from right, developing a sense of direction, understanding metaphors, and

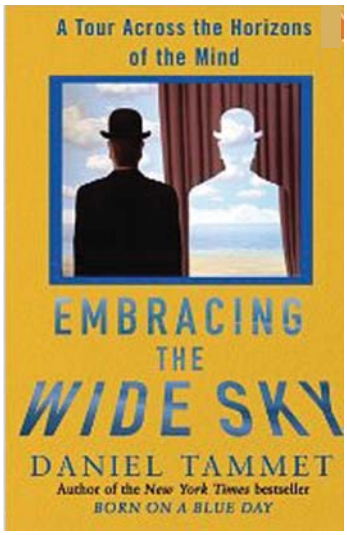
following complex instructions. Yet with the crucial help of friends, Daniel was even able to master what even ordinary people can find daunting: traveling on the London underground. Also vital seems to have been the support of parents and others in enabling him to spend a year doing voluntary service in Lithuania. This experience was evidently crucial to the building of his self-confidence and to the furthering of his fascination for languages. Shortly afterward, the advent of computers and the Internet enabled his world of social relationship to be greatly extended and eventually led to a lasting love relationship. The final confirmation of the consistent parental support given to Daniel, came with his parents' acceptance of his declaration of being gay (which he had realized from the age of eleven).

The book gives some insight into the feat which first brought Daniel to the attention of the mass media: his success at winning the British and European record for reciting pi. Daniel recited 22,514 digits in the course of more than five hours. This was not as spontaneous as might appear from the videorecording but required three months of practice. What is most distinctive in his various numerical performances is the recall of digits in the form of touchable landscapes. It is this aspect of synesthetic experience which is the truly remarkable. The synesthetic ability is augmented by his extraordinary memory span which has been established at 10–12 digits (compared to the normal range of 5–7 digits).

The attention given to his success at winning this record led to the BBC film, a documentary that included a journey to meet the real "rain man," the recently deceased Kim Peek. As part of the program, Daniel was willing to take part in research to which he clearly has a positive attitude. While not all researchers might prove worthy of this confidence, BBC's choice of Vilayanur Ramachandran and his assistants at the Center for Brain and Cognition proved to be an appropriate one. It was with interest concerning the fruits of this cooperation that I turned to the sequel book.

The title of this, *Embracing the Wide Sky*, is inspired from an Emily Dickinson poem and serves to emphasize one of Daniel's personal insights in life. Despite his intellectual prowess, he is able to reach a state of consciousness transcending this, and this transcendence means for him that he has a Christian religious belief. The book begins as a primer of contemporary findings in cognitive and neuropsychology before focusing on what Daniel Tammet has learned from his own experience and from his now considerable knowledge of the neuroscience literature.

One of the ready explanations for savant ability is what he calls "the drudge theory," by which he means that the spectacular performances are due to repetitious and even obsessional hard work in the use of rote memory. Clearly in most cases hard work is a prerequisite. Daniel Tammet succeeded



at speaking Icelandic after less than a week's effort but it was a week of intensive learning. Obviously this explanation is not enough. What is revealing is how Daniel makes use of his extraordinary gifts of seeing connections between words in different languages and between sounds and words. The account of Daniel's meeting with Kim Peeks reinforces this conclusion. He writes: "Kim remembers his vast repository of factual information by weaving the facts he learns together with a mental network of many thousands of different associations and interconnections" (p. 79). Daniel thereby agrees with what most psychologists conclude: that photographic memory is also a myth as an explanation and savants learn mainly what they have a biological predilection for and what they have the motivation to develop. In the case of Daniel, a study of him by Simon Baron-Cohen of the Cambridge Autism Research Center suggested the ability concerning numbers and his fascination with them might be due to the area for facial recognition having been taken over by digit recognition.

But there is much more to Daniel Tammet's abilities. The truly defining feature giving insight into his ability may be Daniel's synesthesia. In chapter 5, The Number Instinct, Daniel describes how synesthesia enables him to see numbers as meaningful relationships with recognizable visual and tactile shapes. But this is still no worthy explanation since why do we not all do this? His own theory, and it is one supported by Snyder and Ramachandran, concerns *disinhibition*. Disinhibition is the principle in neurophysiology that the functioning of the more primitive areas are held in check by areas expressing the higher cortical functions. When the latter areas are disinhibited, automatisms beyond normal consciousness awareness come in to play. Well-known examples of this are alien hand syndrome, but somnambulism and partial complex epilepsy, and even the mediumistic automatisms, may also be due to forms of disinhibition.

In the case of his numerical ability, Daniel notes that normally it is the left temporal lobe which inhibits the expression of synesthesia located as it usually is in the right temporal lobe. In his own case, the left hemisphere may have been damaged and this damage led to the disinhibition or activation of the right temporal lobe. That Kim Peek lacked a corpus callosum to coordinate hemispherical functions, he sees as consistent with this hypothesis. The

hypothesis is developed even further by maintaining that this disinhibition can occur even in schizophrenia and is a source of creativity in savants. Whereas certainly argument can be made for a link between psychotic states and creativity, it is nevertheless disputed whether most high-functioning Asperger savants are more creative than other individuals (Treffert, 2000:300–301).

Regrettably, in the final analysis, not even Daniel Tammet can give much deeper insight into the nature of his own calculating abilities. A rather similar limit is reached with the insight given into his language abilities (Chapter 4, A World of Words). He shares with the reader his many tips and suggestions for the learning of foreign languages, and it obvious that he is fascinated by words and enjoys playing with them. However, even applying these tips, it is still clearly beyond the ability of nearly every non-Scandinavian to learn Icelandic in a week in order to give a television interview in the language.

Because of this lack of insight, research on savants has rather reached an impasse. What it has shown is that sometimes they use memory and sometimes even elaborate numerical or language rules but most often without any form of awareness of them and without import into other areas of functioning. Yet there are some experimental findings that may give some support for the above theory. Neuropsychiatrist Darold Treffert describes in his book (Treffert, 2000:77–78) how a graduate student, Benj Langdon, tried day and night to practice the calendar-calculating skills of two savant brothers. After spending an enormous length of time at it, he still failed to match their performance. Then when apparently he had given up, it suddenly came, and he no longer consciously had to go through the operations. Like for many savants, the answers came intuitively and non-consciously.

But what does this actually mean? Allen Snyder, director of the Centre for the Mind in Australia, argued that "savants have privileged access to lower-level, less-processed information, before it is packaged into holistic concepts and labels—savants tap into or read off information that exists in all our brains, but this information is normally beyond conscious awareness owing to top-down inhibition" (Snyder, 2009:1399). Accordingly, Snyder and his co-workers have developed an experimental means of disinhibiting the left temporal lobe (Snyder, 2009, 2011). The method involves giving repetitive transcranial magnetic stimulation to the lateral anterior temporal lobe of right-handed normal individuals. Having done this, it was found compared to those receiving a sham stimulation, there appeared to occur changes in drawing, in accuracy of proofreading skills, and in numerosity—and also a reduction in false memories. Snyder believes that some cortical areas may be responsible for the top-down processes that create our holistic and meaningful interpretations. To do so may require an inhibition of those areas, presumably in the right hemisphere, concerned with perceiving details.

Furthermore, this theory is promising because it will work as a general theory of autism since an autistic person, being stuck in the perception of details, thereby lacks the ability to build whole concepts such as a "theory of mind" and a workable theory of how to form relationships.

The explanation is simple and yet elegant. Treffert and Snyder along with Daniel Tammet see it as having important implications for the existence of an enormous reserve of untapped human potential in so-called normal individuals.

Even so there are some incongruities. Daniel Tammet's major message is actually contrary to the above, since he explains that the key to his abilities is seeking meaningful and contextual relationships, and it is this ability to see the whole myriad of relationships that he believes enables his success at languages and calculating. Moreover, there is as yet no firm evidence that individuals with synesthetic ability are poorer at personal relationships and are more creative. But what surely remains enigmatic concerns the acquisition of complex savant skills of speaking languages or playing music. These would seem to be more than access to mechanical rote learning or associations, but some form of integrative ability for understanding the rules governing what goes together. Indeed the leading authority in the area, Darold Treffert, after reviewing the 35 years of research, doubts that brain damage or practice alone could account for the access to the rules of music, mathematics, and art that is innate in these individuals. He argues that we may be born with this "soft ware" already encoded as genetic transmission (Treffert, 2000).

Yet if we critically analyze the reason for proposing such a nearly all-encompassing mechanism, then the real enigma of some savants becomes undeniably apparent. As Keith Chandler (2004) pointed out in his provocative paper, while we might explain the performances of Daniel Tammet and Kim Peeks as a mixture of motivation, mindless learning, and synesthetic associations, the explanation is just not all-encompassing enough. He asks how does this explain the cases of prodigy savants (Treffert & Wallace, 2002) where complex skills are required and there is no apparent possibility of acquiring these skills? If the information we are told about their backgrounds is correct, then neither chromosomes nor software could not contain the necessary *specific cultural information for these skills to suddenly appear.* For instance, in the case of Leslie Lemke (Chandler, 2004) the prodigious talent for playing music appeared in its completeness literally in the middle of the night after hearing on television Tchaikovsky's Piano Concerto No 1. Chandler writes: "It is something he clearly remembers how to do but it is a remembered skill that he was never taught and being blind one that he could never have seen anyone else perform."

There is now an extensive literature on savants, and it should be of interest to make a critical research review of what actual opportunities prodigy savants

have had for learning such complex skills. The above books are now a very important contribution to this literature. Given that Daniel Tammet has access to this reservoir of semantic relationships that we are not normally conscious of and seldom use, it is just possible that he is far more unique than we have realized. We can be thankful that he has such a positive and generous attitude to research.

There is only one quip which I am almost reluctant to mention given Daniel's confessed obsessive attention to detail. The books show how well read he is, but *Born on a Blue Day* lacks references and *Embracing the Wide Sky* lacks many of those quoted in the text.

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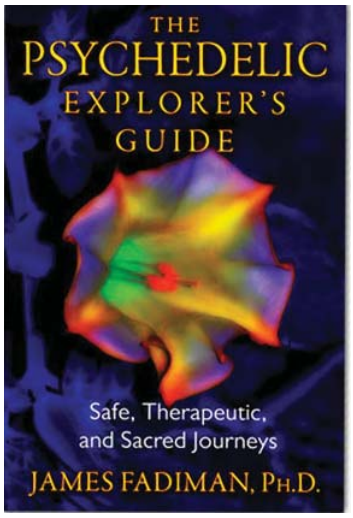
The Psychedelic Explorer's Guide: Safe, Therapeutic, and Sacred Journeys by James Fadiman. Rochester, VT: Park Street Press, 2011. 352 pp. $18.95. ISBN 978-1594774027.

Drawing upon more than four decades of professional and personal experience in the arena, James Fadiman has written a practical, informed, and entertaining handbook for people who desire to embark on an encounter with LSD, mescaline, peyote, psilocybin mushrooms, or the dozens of other natural and synthetic substances that fall under the psychedelic umbrella. Similar guidebooks have been written over the years, but none of them approach the authority, credibility, or utility of *The Psychedelic Explorer's Guide.*

Fadiman is a faculty member of the Institute of Transpersonal Psychology, a graduate school that he helped to found in 1975. He possesses considerable *gravitas* on this topic, having conducted research with psychedelics when



the substances were still legal. His book is extremely timely now that investigations are undergoing a Renaissance, and that several articles have been published in high-level psychological, psychiatric, and medical journals. These investigators need to read this book because it will provide important considerations with regard to the six factors that are described by Fadiman: set, setting, substance, sitter, session, and situation.

However, there are other potential readers who are conducting what is euphemistically referred to as "self-experimentation." These are seekers for what Fadiman calls their "birthright" to become "more aware." Fadiman presents data indicating that these intrepid explorers refer to their session as "the greatest experience of my life." The book's first two chapters provide guidelines for one's "trip." The next two chapters review the contributions of several major figures in this field who review what they have learned from their sessions, ranging from Albert Hofmann and Aldous Huxley to Alan Watts and Stanislav Grof. Letters to and from Humphry Osmond, Timothy Leary, and others are cited, as are passages from books by such icons as Alexander Shulgin, Ralph Metzner, Huston Smith, Rabbi Zalman Schachter-Shalomi, Richard Alpert (Ram Dass), and Frances Vaughan (the lone female in this collection of white males).

The next four chapters focus on self-exploration; one of them was written by Neal Goldsmith, a psychologist and psychotherapist, and is aptly titled, "Things Can Go Wrong." Neither Fadiman nor Goldsmith guarantee their readers a risk-free psychedelic journey, and Goldsmith offers 18 helpful suggestions ranging from regulated breathing to having a warm blanket on hand for emergencies. The next chapter brings up the topic of adulterants such as methamphetamine. However, the U.S. Drug Enforcement Administration's 1991 claim that strychnine is a common adulterant is examined and found wanting. The case can be made that adulterants have become less problematic in recent years, certainly since I published a short research article on this topic in the journal *Science* in 1970. Nonetheless, Fadiman merely tells his readers to obtain their supplies "from a trusted source" (p. 262). Perhaps this is all he can say without running afoul of the law, but an expansion of this guideline would have been useful.

The following six chapters provide a detailed description of Fadiman's pioneering investigations at San Francisco State University's Institute for

Psychedelic Research, a project abruptly terminated in 1966 by the U.S. Food and Drug Administration, along with similar projects across the country. Improvement of cognitive enhancement, especially creativity, was observed among these two dozen participants, and Fadiman provides both test data and subjective reports that illustrate these results. A few extensive case studies provide valuable accounts of how the psychedelic experience was put into practice once the session was over.

These accounts lead to four chapters outlining "new horizons" for psychedelic research; including the use of: "micro-doses" for specific purposes, much as indigenous shamans have used small amounts of mind-altering morning glory seeds, mushrooms, and the like to assist their service to their communities. Future medical and psychotherapeutic uses include expanding the pilot studies indicating psychedelics' value in treating cluster headaches and PTSD and for spiritual enhancement. This latter direction has led to what I consider the overuse of the term "entheogen" as a synonym for "psychedelic"; not every session is designed to find "the God within," and the term "potential entheogen" would be a more appropriate descriptor. "Psychedelic or "mind-manifesting" is far better than the negatively toned "psychotomimetic" and "hallucinogenic" descriptors. I have long favored the term "phantasticant," but it never caught on.

The final four chapters bring the reader up to date on the Amazonian brew ayahuasca. They consist of three well-written first person reports, but omit the research studies conducted by the psychiatrist Charles Grob and his team whose data found several long-lasting beneficial effects of ayahuasca imbibed in a religious setting, and no long-term negative effects. However, Fadiman provides an excellent account of the behavior changes that followed LSD and mescaline sessions with 67 participants at the International Foundation for Advanced Study in Menlo Park, California, in 1962 and 1963. The effects upon marriage, sexual performance, job satisfaction, and nighttime dreams have never been presented in such detail. Questionnaire data from 113 participants at the same institute are also included. It is a tribute to Fadiman's writing skills that these data do not bog down the book or drain the reader's attention, but add texture and richness to what could have been a superficial cookbook on how to run an LSD session.

Fadiman's autobiographical material is an important part of this book; he calls himself an "inadvertent pioneer" and describes how he fell into his role due to serendipitous meetings with Willis Harman, Myron Stolaroff, Charles Savage, and—of course—Timothy Leary and Richard Alpert, the future Ram Dass. When Harvard University terminated the services of Leary and Alpert, Fadiman knew that his own career was at risk. As he put it, "I really stepped back at that point," and his segue into the new field of transpersonal psychology was a wise

career move. He helped develop the *Journal of Transpersonal Psychology* and the Association for Transpersonal Psychology, which Fadiman misnames the "Transpersonal Psychology Association" (p. 235). He also employs the "genie out of the bottle" metaphor too often; that genie should be content to emerge from the proverbial bottle once and be on his way. Some seminal books remain unmentioned, most notably *The Varieties of Psychedelic Experience* (Masters & Houston, 1966), which I consider the best phenomenological account of these internal voyages.

But these are minor flaws in a magnificent tapestry. Fadiman does not proselytize, he simply provides useful information. He concludes by stating, "If you're going to use psychedelics, do it with someone you love, and hopefully someone who has been there before you, and be aware that the world is better than you have ever thought" (p. 238). I would rephrase the beginning of this advice to ". . . do it with someone who has been there before you, and hopefully someone you love." But the last phrase makes sense not only for those who want to embark on a psychedelic excursion but to those who prefer meditation, prayer, Nature walks, falling in love, or any of countless other ways to secure their birthright of becoming more aware.

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Wonders in the Sky: Unexplained Aerial Objects from Antiquity to Modern Times and Their Impact on Human Culture, History, and Beliefs by Jacques Vallee and Chris Aubeck. New York: Jeremy P. Tarcher/Penguin, 2009. 508 pp. $22.95. ISBN 978-1585428205.

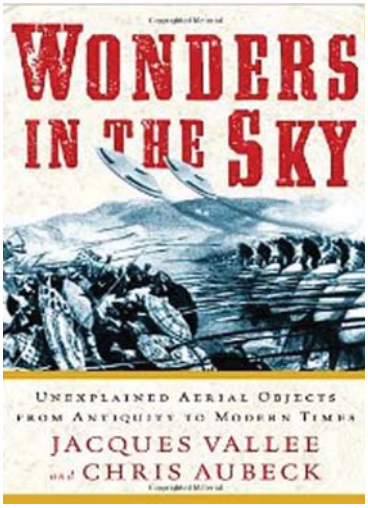
Ufology is a historical enterprise deep down in its heart. For all their scientific aspirations, ufologists spend most of their time collecting and collating cases, looking for connections over time, searching for patterns of meaning amid a welter of details—in short, the very sort of thing historians do to chronicle the lives of Roman emperors or events of the Civil War. One key date is June 24, 1947, and anyone familiar with UFOs recognizes it as the day when the

"first" flying saucers appeared; but no sooner had Kenneth Arnold reported his sighting than reports of antecedents called the uniqueness of this event into question. The Arnold incident holds benchmark importance, a beginning of the modern era when saucers began to fly thick and fast as they quickly established themselves as a cultural fixture and a mystery to reckon with, but historical awareness sets this event in a wider perspective: Arnold did not see the first UFO, just one more in an unbroken line that stretches back to the 19th century in some opinions; in others, to antiquity or even remote prehistory.

Early cases entered into UFO discourse almost from the start. In a sense this prior history was ready and waiting for the flying saucers to arrive through the writings of American author Charles Fort, who spent much of his life combing newspapers and scientific publications of the 19th and early 20th centuries for reports of strange phenomena damned to exclusion by official science. His four books and the still-active Fortean Society provided the new phenomenon with a robust lineage of suggestive observations. A segment on old sightings became standard in most UFO books of the 1950s. Ufologists have embraced old reports as stout supports for the extraterrestrial hypothesis, since skeptics' arguments that airplanes or satellites are responsible for many UFO reports hardly apply in ages when no man-made flying machine had yet left the ground. Proponents took a vested interest in finding modern UFOs flying over Roman legions and crusader castles, because if such reports existed, they had to describe alien vehicles. By simple elimination, no other explanation would work.

If ufologists have good reason to value history, they have not necessarily shown a very discriminating knowledge of it. The examples in the 1950s literature were haphazard and, when derived from sources other than Fort, often inaccurate, distorted, even rewritten to confirm a proponent's wishes. By the 1960s and 1970s historical research divided between two contrasting pathways: One led to systematic explorations of the phantom airship waves of the 1890s and early 1900s, and to Jacques Vallee's seminal *Passport to Magonia* (1969), which related UFO experiences to fairy and demonic lore with the suggestion that some broader mystery might underlie a multitude of anomalous phenomena. Along the other path, "ancient astronauts" theories ran rampant as every ancient myth and monument became evidence for alien visitation and intervention in human history. Hoaxes and wild speculations multiplied to the discredit of ufology and to the confusion of whatever message the historic reports carried for the UFO mystery as a whole.

Now at last a book has come along that intends to set the study of historical UFOs on a sound footing. *Wonders in the Sky* is authored by Jacques Vallee, one of the most respected names in UFO research, and Chris Aubeck, who has made a distinguished name for himself among serious historical researchers over the past eight years. In 2003 Aubeck organized the Magoniax project to link a



select few active UFO historians through the Internet, for the purpose of exchanging and compiling reliable accounts of anomalous phenomena prior to 1947. This book represents the first study to grow out of the Magoniax project.

The heart of the book, more than 300 pages long, consists of a chronological collection of 500 cases of "wonders" starting in 1460 B.C. and ending in 1879 A.D. Most of the accounts describe aerial lights or objects; some treat UFO-related phenomena like abductions, entities, and mysterious communications. Each entry provides a substantial summary of events, along with date, place, and source, and sometimes concludes with a brief commentary. The authors have screened an extensive body of records to eliminate the obvious comets, meteors, sundogs, hoaxes, and other identifiable causes, leaving readers with a genuinely puzzling residuum of aerial events from across the centuries. A decision to close this chronicle with 1879 was both practical and theoretical. The running understanding of aerial wonders from earliest times through the 17th century assumed a supernatural source, such as gods, angels, demons, or witches. Most educated writers from the 18th century onward favored naturalistic origins for strange sights in the sky, like meteors or weather phenomena. By the latter quarter of the 19th century expectations of man-made aircraft spilled over into the observational record as people began to describe phantom balloons and airships, with the reports becoming epidemic by the 1890s. Faced with this shift of concept and escalating numbers, the authors wisely chose to call a halt just before technological wonders began to dominate.

Among the more interesting cases is a Roman account from 91 B.C. of a fireball that descended to earth, then rose from the ground and was large enough to blot out the sun. A Chinese record from about 1059 tells of an object like a giant pearl that frequented a lake for some ten years. The object emitted such intense light that the shadows of trees miles away became visible. Solitary English travelers sometimes had to contend with a fiery wheel- or barrel-shaped object that would follow them at nights during the winter of 1394. In 1520 a beam of fire descended from the sky and burned many things on the ground before it ascended again and changed its shape into a circle of fire. A report to a London newspaper in 1794 describes a meteor over India that made frequent pauses before it descended behind some hills. Two minutes later the light

rose above the hills, illuminating them before it sank again and repeating this behavior twice more before vanishing. My favorite case is the Robozero Marvel of 1663, when residents of a Russian village watched a ball of fire with the diameter of a 14-story building pass back and forth overhead three times before hovering close over the surface of a lake for 45 minutes then finally flying away. If the nature of the objects in this chapter remains open to contention, their puzzling and uncharacteristic qualities are undeniable.

A second extensive section, titled Myths, Legends, and Chariots of the Gods, provides the authors with an opportunity to discuss some questionable stories taken as evidence for extraterrestrial visitation in the UFO literature. The entries include hoaxes like the "Dropa Stones," supposedly artifacts left by a group of aliens marooned on earth thousands of years ago, or the "silver shields" that flew over Alexander the Great's army though only in the imagination of some modern writer. Other instances exemplify an abundance of speculation applied to texts, so that the vimanas of ancient Indian epics become spaceships or the Star of Bethlehem acquires an extraterrestrial identity. Meteors, auroras, halos, and other conventional phenomena clearly explain some appearances that amazed witnesses in the past. Common folk beliefs contribute to stories of ships in the sky and to accounts that associate lights and entities with fairies, though traditional beliefs do not necessarily exhaust the strangeness of some reports. By treating these reports in a separate section, the authors can satisfy readers' curiosity about claims often associated with UFOs and at the same time distinguish the doubtful evidence from the sound.

The book consists mainly of cases, but it also provides orientation, connecting material, and some basic analysis. Considerable care goes into explaining the sources used and the criteria for selecting cases. A preference for the most original sources available—the medieval chronicles or scientific journals or the literature of prodigies and wonders like the 1557 *Prodigiorum ac Ostentorum Chronicon* of Conrad Lycosthenes—sets this collection apart from the usual derivative materials found in the UFO literature and on the Internet. The inherent strangeness of the case rather than interpretations imposed on it or suggested by current agendas qualifies an account for inclusion, so that even as tempting a report as a flying shield does not automatically mean a flying saucer, but only that an ancient historian drew a conventional comparison to describe a meteor. Scattered through the extensive chronicle of cases are pauses to update readers on the historical context of the reports, such as changes in religious beliefs, social conditions, and technological developments that contributed to the shape, dissemination, and interest in prodigious occurrences at a given time.

Issues of interpretation do not arise in this book. The authors are satisfied to establish the existence of unknown aerial appearances and not jump to the conclusion that alien visitation or any other particular cause was responsible. At

the same time they do not leave readers entirely adrift. In a Foreword by David Hufford, this noted scholar of anomalies reminds readers that beliefs often originate in experience, and a study of claims about strange phenomena stands to profit from an experience-centered approach. The authors' Introduction cites four conclusions—that unknown phenomena have appeared throughout history, that interpretations change from epoch to epoch, that these phenomena have had an influence on human civilization, and that historical cases teach lessons applicable to modern aerial manifestations. A final chapter returns to these conclusions and summarizes the case files to argue that similar phenomena have recurred down the centuries all over the world, while the phenomenology of past events anticipates the reported experiences of UFO witnesses today. Human interests and explanations vary according to time and place but the underlying appearances show a consistency worthy of further study. These measured and cautious proposals grow out of the historical materials as fully justified and free of the jarring leaps of faith so common in relating UFOs to their supposed antecedents.

This admirable study fulfills its goals and leaves little cause for complaint. Some of the case summaries would benefit from more complete information. Historical records of aerial anomalies are often frustrating in their brevity, while equally frustrating copyright restrictions hinder direct quotations and raise barriers that are especially onerous for such a wide-ranging project as this one. Still, a case like no. 473 appears almost devoid of details to indicate why it was included at all, even though it is familiar from Charles Fort and intriguing only because of the omitted descriptions. Other cases almost certainly have conventional explanations, like no. 482, wherein an astronomer watched the slow progression of a red "bolide" over Marseille in 1871. While the object certainly was not meteoric in its actions, its characteristics well suit the behavior of a fire balloon. While high standards govern the selection of cases and secondary sources are usually reputable, the authors draw on the UFO literature now and then. These lapses allow inclusion of Japanese and Chinese reports not otherwise available, but in rare instances the sources are doubtful, at least in details, yet included anyway (e.g., case no. 49 and case no. 188).

The authors are well aware that the written record tells a cultural truth that is not always the same thing as historical truth. Prodigies became tools of propaganda in the Reformation era, with some entries in the *Mirabilis Annus* collection so slanted toward the Puritan cause in 1660s England that royalist authorities sought to apprehend the author. Newspapers in the 19th century shamelessly resorted to bogus stories of extraordinary events to provide a form of journalistic entertainment known as "nature faking." These pitfalls are familiar enough, but the literature of signs and wonders twists and turns in a labyrinth of motives and customs wherein no amount of caution is ever quite enough. The

garrulous monk William of Newburgh seems never to have met a strange story he didn't like, but he simply represents one of the most visible examples of a credulous medieval writer. Others have idiosyncrasies of their own and even the most disciplined sometimes waive their sound judgment where anomalies are concerned. The written accounts cannot be taken entirely at face value and as a result cases cited in *Wonders in the Sky* provide an "enriched" sample of unknowns rather than a "pure" sample.

Another possible source of error is the stereotypical elements of some accounts. A "saints' lives" literature stands by itself but also infiltrates many mainstream medieval chronicles with recurrent motifs of luminous phenomena accompanying the birth, death, and miraculous activities of saints. These obligatory elements serve more to validate the sanctity of a historical personage in a biographical genre than to record literal history. Folkloric motifs intrude in some accounts, one example perhaps being the fiery object that followed travelers in 1394. This account bears similarities to the will-o'the-wisp or the fairy lantern that leads wayfarers astray, and even if the experience was real the description may have taken its shape from popular belief.

The brevity of the book's forays into the historical and intellectual contexts of its subject matter calls for expansion, as does the database itself and analysis of the findings. But these jobs are work for another day. What we have in hand is a book worthy of celebration in itself. The authors replace the faith and phonies too often characteristic of historical UFO research with a solid basis, both an extensive collection of genuinely interesting anomalies from original or creditable international sources, and the provision of a framework for understanding these reports and for building on this foundation in the future. Whatever opinion the reader may hold about the nature of these accounts, the factual matter of the case, the fundamental cause for wonder, stands out with unprecedented clarity and sets bounds on the speculative impulse. We can thank the authors for this important step toward the truth, whatever form it ultimately takes, and look forward to further scholarship at the same high standards.

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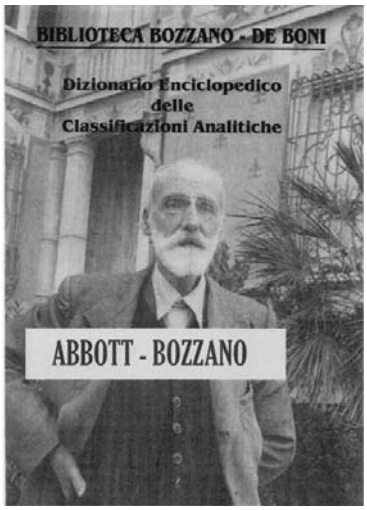
Dizionario Enciclopedico delle Classificazioni Analitiche della Biblioteca Bozzano–De Boni [Encyclopedic Dictionary of the Analytical Classification of the Bozzano–De Boni Library] edited by Silvio Ravaldini and Giulio Caratelli. Bologna, Italy, 2011. Eight volumes.

More than 2,200 *entries*—excluding those referring to terms and expressions—most of them containing bibliographic references, 2,800 pages in eight volumes. In the last volume, there are more than 50 pages of *Contents*, describing which entries and themes are present and where they are dealt with. These are the main features of a new "tool" of the Bozzano–De Boni Library, a *Cumulative Index* published at the very beginning of 2011 as the result of a work started a long time ago.

Ernesto Bozzano was the first one who had the idea of creating a reference list, an *Index* of publications concerning mediumship and spiritism. At the beginning of the 20th century he was committed to reading extensively about spiritism and psychical research, becoming a "supporter and promoter" of spiritism. He was a tireless reader of everything that was published—and everything he could obtain—on metapsychics, mediumship, spiritism, and related issues. While thoroughly examining the texts, he would annotate what he found interesting, from the most general and evident elements (i.e. theories, argumentations, original experience reports) to minor details that a superficial reader may fail to notice, such as private stories, referrals to persons and experts in the field, anecdotes, observations, and so on. Perhaps without him being aware of it, Bozzano's work led to the creation of *analysis sheets* for each book and review he read, containing detailed references on various themes (page numbers and referrals to other publications), which allow the reader to easily get to the materials. Those sheets were essential to him, since his monographs were composed of the systematic and logical combination of examples leading the reader to his own "inescapable" conclusions. Each example was specifically discussed, and the more numerous the cases reported, the more his thesis was strengthened and supported (Biondi, 1984, Ravaldini, 2011). This is the reason why he considered working with indexes extremely useful, and perseveringly continued his task for more than forty years.

After Bozzano's death, all his papers and documents were inherited by Gastone De Boni, who added other book analyses by following the same criteria. Some decades after, the entire work reached the hands of Silvio Ravaldini, who also gathered publications, personal documents, and materials of Bozzano, De Boni, and other prominent scholars of spiritism and psychical research. Once books, reviews, and other materials were ordered in a purpose-specific *Library*, he had to decide how to use the hundreds of *analysis sheets* that had



been collected, some of them dating back to the previous century but which still had remarkable information potential. Ravaldini continued that analysis work, at first on his own and then supported by Silvana Pagnotta. While he was ordering, verifying, and computer typing the old classifications of Bozzano and De Boni, a small work team, composed of Claudia and Cecilia Magnanensi, and Giulio Caratelli, carried on other parts of the work (Caratelli, 2011). Their commitment turned into a *project* aimed at indexing by 2000 all the most prominent journals and books kept in the Bozzano–De Boni Library at Bologna. The *Index* was concluded by the deadline, but it took another ten years to organize and structure the written texts, which have today the above-described characteristics.

The *Index* is essential for anyone who looks for solid documentary bases for supporting historical, experimental, or theoretical works. Publications on spiritism and parapsychology constitute a minor and marginal part of the contemporary culture, but are extremely numerous and it is difficult to find direction through them. Furthermore, the modern approach often ignores the historical perspective, and the wide variety of texts on the Web makes it impossible to have adequate control of the countless available "rough" materials. This is why a guide indicating if and where references can be found, not to mention discussions of specific issues, and the way an author dealt with a theme, is a very valuable tool for all who want to study this field, fill in some knowledge gaps, or simply satisfy their curiosity.

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Ravaldini, S. (2011). Preface. *Dizionario Enciclopedico delle Classificazioni Analitiche.* Bologna. Volume 1, III–V.

War and Shadows: The Haunting of Vietnam by Mai Lan Gustafsson. Ithaca and London: Cornell University Press, 2009. 206 pp. $19.95, ISBN 9780801475016.

This book is an anthropological study of spirit possession in postwar Vietnam. The author knows that the notion of spirit possession is widely rejected by most modern scholars. She has, however, chosen to record her observations from the viewpoint of her informants, trying not to impose any reductive assumptions on her narrative. Her object is to describe a complex human experience, not "explain" away any part of it that seems dubious according to mainstream bias. The big idea behind this book forces us to think about new dimensions of suffering caused by war, especially by modern war in which the physical obliteration of victims is more likely, thanks to more destructive technologies.

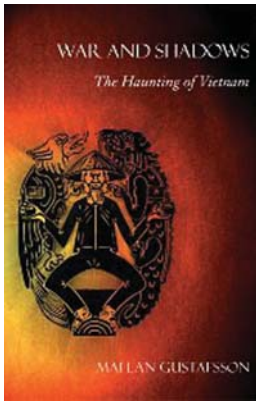
Dr. Gustafsson has collected 190 case histories of people mainly from Hanoi and nearby: individuals who have been possessed, assaulted, and haunted by "angry ghosts" (*con ma*). These are victims still living in the vast psychic wake of a war that officially ended in 1975 and began with the French colonialists returning to Saigon in 1946 after the Second World War. The encounter with these angry ghosts runs to epidemic proportions; so much so that the Communist (spirit-disbelieving) government a) has been forced to acknowledge the devastating economic reality of the problem, pronouncing it a "public health menace," and b) permits, despite ideological reluctance, the practice of spiritistic folk medicine; for it is the local mediums, sensitives, and diviners who seem to know what's going on and, often at great financial cost to the victim, can perform salutary rites and provide useful counsel for palliating the rage of the spirits. This may consist of something as simple as putting a nice frame around a dead uncle's photo. Something strange is undoubtedly happening in the Vietnam that America tried famously "to bomb back to the stone age." To understand what this is, at least two premises from the author's narrative must be underscored.

The first has to do with the all-important Confucian element in Vietnamese burial practices, and their significance for the psychic integrity of the entire community. The picture one gets from Gustafsson's account of these practices, which are taken very seriously, points to something probably pretty strange for most Western individualists. According to the Confucian *li*—the rites, duties, performances—proper burial is absolutely essential to the well-being of the deceased. The rites are offerings, acts of respect, love, honor, and recognition directed toward the dead; when done properly, the departed become guardian spirits (*lo tien*), sources of guidance and well-being for the living family. The Confucian roots of this points to the fact that the family is the basic religious

institution (Smith, 1994). The stability of society seems here to depend on ritual rapport with the dead. Anything that prevents that ritual rapport must corrode basic social bonds.

This leads to the second premise essential for understanding. What in fact was deeply antithetical to traditional Vietnam burial customs? Answer: The Vietnam War. For proper ritual burial you have to have a body. Without an intact body—a head or a limb won't do—the *li* cannot be performed. The Vietnamese believe that rite-deprived souls cannot handle their fate on their own in the next world. Facing what is perceived as insurmountable frustration of their support, they become angry ghosts. But before we sketch a picture of the angry ghost and the kind of havoc it is alleged to inflict on Gustafsson's informants, we need to comprehend the enormity of the war's cost, part of which seems to have involved denizens of the "other world" as well as living survivors. On page 125 we find that the American losers of this war still managed to inflict the following on the Vietnamese people: more than 5 million dead; 300,000 missing; 300,000 orphans; 64,000 injured, and 40,000 killed by landmines and unexploded ordnance, since the end of the war in 1975; nor should we omit the 250,000 boat people who died during attempts to escape nor the 10 million refugees, said to be a conservative number. The number of key importance to the angry ghost question is the 300,000 missing but no-doubt-dead bodies. According to the belief system, that means 300,000 angry ghosts are out there wandering about looking for ways to vent their fury. Suppose the invisible afterworld corridors do indeed swarm with such agents of ill will—truly horrifying is the belief that there is no hope of relief for them, no prospect of ever escaping from this anarchic psychic inferno. The author keeps hammering home this picture of hopelessness; it's a hellishly narrow place to be trapped in, dependent on the kindness and remembrance of the living, forever trying to be noticed, if necessary by means of cruel and spiteful actions.

The author provides an Appendix (I), titled "Table of Suffering" (pp. 147–167), summarizing what she learned from her 190 victims of otherworldly aggression. They cover all types, genders, ages. Besides basic facts about the informants, we learn of their symptoms, their diagnoses, and their treatments. The book also covers individual cases in greater detail. The possession experience presents a roster of symptoms. The drift of them suggest the displacement of the normal personality and something else forcing its way in, and in no gentle or kindly manner. Some symptoms are mainly physical and may indicate the resistance and discomfort in being displaced; for example, pains, tremors, shaking, convulsions, skin disorders, listlessness, and unexplained illnesses. (The author repeatedly underscores the failure of physicians to account for most of these symptoms.) Other symptoms show the outline of the invading personality itself: voices, obsessive thoughts, inability to concentrate, amnesia,



nightmares, violent behavior, sleepwalking, and the often-mentioned out-of-character behaviors.

The third item is *diagnosis*. What to make of the these unexplained symptoms and who provides the diagnosis? First of all, the diagnoses are intuitive, not definitive or rigorous in a quantitative way. The responses show a handful of popular sources of diagnosis, beginning with the victim's own self-diagnosis. Besides oneself, mediums, fortunetellers, and family members may confirm that one is possessed and by whom: often family members (from greatgrandmothers to sons), friends, strangers, and lots of ghouls. (The only kind of ghoul that fits the role here is the Arabian desert ghoul said to prey on travelers.)

Finally, we are given information about the treatment in each case and its success or failure. For example, in one case, "Symptoms stop after installing dead comrade's memory in pagoda, becomes 'no problem' after funeral service in Cambodia (per medium's advice)" (p. 149). But then in another case, "Symptoms persist, even after victim confesses to family that he stole offerings of food and money meant for these spirits" (p. 151). Generally, the symptoms stop or lessen when the possessed person follows the recommendations of the medium or other advisors. In the cases where the symptoms persist, despite the victim following instructions, the failure is attributed to lack of sincerity or responsibility. And then there are cases like the man diagnosed as possessed by his dead wife. Effects of the treatment were unclear: "Victim must renounce sex with prostitutes, per medium's instructions: status of symptoms unknown" (p. 159).

Often partly or with great difficulty, the angry ghosts can be laid to rest, or at least pacified. What does it take? These wretched spirits need to be recognized, remembered, memorialized, celebrated, and honored. In this thought-world, when the living adopt the right attitude and behavior toward the dead, the dead become gods, guides, guardians to the living. This is the basis of Confucian ancestor worship. When the living ungenerously isolate themselves from the dead, and fail for whatever reason to pay their respects, there is war between the living and the dead. By creating 300,000 possible angry ghosts, modern body-annihilating military technology vastly multiplies pain and suffering for possible afterlife survivors, and certainly for the haunted victims. The war has made Hell Day a popular holiday in Vietnam when people go out of their festive way to honor and make offerings and hope to placate the swarms of angry ghosts out there.

Are we afforded any evidence that there really are such conscious angry ghosts? Dr. Gustafsson abstains from making any explicit claims, and was not

aiming to produce proof in the manner of a parapsychologist, but I felt she was quietly persuaded that the touted ghosts were objectively real. I first heard her on public radio describe the case of an American who upon returning to Vietnam for a visit had symptoms of possession; the American, normally very even-tempered, began to have nightmares and shouted in his sleep furious outbursts in perfectly grammatical Vietnamese. His girlfriend was witness to these displays, and vouched for their grammatic excellence. This story would pack a wallop if the American knew no Vietnamese; but he had moved to Vietnam and did have a working knowledge of the language. What was impressive to witnesses was the fluency and idiomatic style of his execrations.

There is a broad argument meant to support, or at least suggest, the hypothesis that ghostly survival is the best explanation of the symptoms experienced by the author's informants. If the ritual recognition of the angry ghosts is effective, the symptoms do ease off or completely vanish; in short, it looks as if the ghosts are responding to the ritual treatment. The trouble is that the links in the chain of the argument are too fuzzy. We have at best a very sketchy medical knowledge of the symptoms. There is another crucial question. How did the angry ghost get identified as the culprit? Here again we're in what looks like a cloudy realm to the outside observer. And finally, the doubter might think: Couldn't all the beneficial effects from the treatments be explained by a powerful placebo effect and a highly active and culturally primed imagination? Perhaps the angry ghosts are really the guilty unconscious of the survivors punishing themselves and trying to make amends. In short, counterexplanations could be advanced to explain the angry ghost phenomenon; but they're not likely to persuade victims.

In my opinion, this very well-written and courageous book merits our attention for at least two reasons. First, it points to an area of research that may be of interest to investigators of postmortem survival. As it turns out, much survival evidence is found to relate to violent situations and mortal crisis: most obviously, near-death experiences; also many reincarnation memories, behaviors, and bodily marks; and many hauntings that involve violent death and violent emotion. The hauntings and possessions of angry, aggressive ghosts reported by Dr. Gustafsson may be included here, exacerbated by body-vaporizing warfare, and the special problems that result from lack of proper burial. If there is a transition to a next world, the how of the transition must make a difference. The ideal Vietnamese death is peaceful and harmonious with the surviving family. The purpose of this harmonious death is to establish a link with the invisible world and be led by the wisdom and virtual godlikeness of benevolent ancestors. On the other hand, being instantly blown to smithereens by a bomb might indeed, as the Vietnamese believe, transform a human soul into a permanent agent of festering ill will.

The second valuable point is that *War and Shadows* enlarges our understanding of the scope of human suffering. On any interpretation of the material recounted, war is costly in ways most of us can barely conceive. In the undoubtedly profitable business of war, the profound hell of hatred and misery that we create, not just for survivors but for possible afterdeath victims, is something we need to reckon with as part of the collateral damage.

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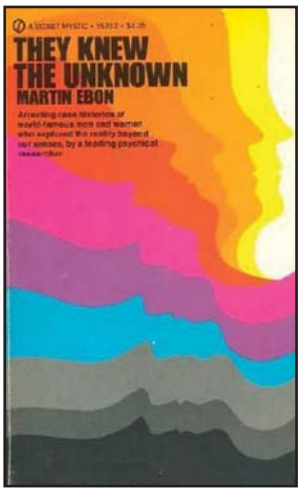
Further Book of Note

They Knew The Unknown by Martin Ebon. The World Publishing Company, 1972. 253 pp. $2.41 (used). ASIN: B00371827C.

Thanks to Amazon and Google, old books don't die. They're available online in near perpetuity. Some end up like those lonely paperbacks left behind on the bookshelf of a summer beach rental—worth but a bored afternoon when it's raining outside. Others are worth saving and savoring.

Martin Ebon's book, written in 1972, is the latter. It's a keeper. A professional writer, Ebon served as managing editor of Eileen Garrett's *International Journal of Parapsychology* and as a consultant to ESP studies pioneer Dr. J. B. Rhine, deeply exposing him to their body of work. But he honed his wordsmithing as a book editor with the New American Library and Playboy Press. Ebon deploys erudite writing: intelligent, sophisticated, with historical references, and touches of humor to drive home his key point—biographers of famous persons routinely sanitize their subjects' lives, glossing over or removing entirely any references to their psychic experiences or beliefs.

Ebon's richly woven tapestry of illustrious scientists, authors, politicians, and philosophers who wrestled with the unknown is extensive and delicious:



Socrates, Swedenborg, Kant, Schopenhauer, and America's own intellectual explorer of the survival of consciousness C. J. Ducasse; Shakespeare, Shelley, Hugo, Twain, Doyle, Browning, Dickens, Yeats, Thomas Mann, Aldous Huxley, and Upton Sinclair; President Lincoln and his publicly known participation in Spiritualist séances contrasted with Canadian Prime Minister W. L. Mackenzie King (1874–1950) and his concealed lifetime of consulting mediums; Strindberg's telepathic delusions; Thomas Edison's musings on how to contact the dead; Alfred Russel Wallace, co-founder with Darwin of the theory of evolution, sharing with a San Francisco audience in 1887 the séance experience which confirmed for Wallace the existence of an afterlife.

And then there's that iconic trinity of psychiatrists: James, Jung, and Freud. Ebon recounts the struggle between Freud and Jung over the nature, meaning, and interpretation of psychic experiences which sealed the fate of these quirky phenomena in psychology's infancy. Had Jung and his intellectual ally William James won, parapsychology today would simply be psychology. Unfortunately, a timid, intellectually dishonest Freud punted; consequently, a class of experiences persistently recorded throughout human history were stigmatized and marginalized to the ultimate detriment of humanity.

You can pick up a used paperback copy of Ebon's hidden history of these psychic explorers for 75 cents. In terms of education and entertainment, that's one heck of a bargain.

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Articles of Interest

The Alchemical Revolution by Sara Reardon, *Science, 332* (20 May 2011), 914–915.

Some historians of science are trying to replicate experiments done by alchemists, and they are finding clues to why the alchemists might have reasonably concluded, for example from color changes, that it might indeed be feasible to transmute less noble metals into gold. This article exemplifies how culturally ingrained is the view of alchemy as magic, superstition, religion, in contrast to science, pragmatism, objectivity.

Thirty years ago I had been taken aback to hear Isaac Newton's biographer, the well-known historian Richard Westfall (http://en.wikipedia.org/wiki/Richard_S._Westfall), say that his problem had been to reconcile the Newton of scientific genius with the Newton who spent more time on biblical exegesis and alchemical experiments than on his mathematics and science. Westfall was committing a fallacy that some other historians have long recognized, taking what they call a Whiggish view: presuming that there has been steady progress from ignorance to knowledge, and judging the past by the standards of later understanding. Surely Newton was, like other humans, an individual who will have felt no puzzling clash between his various interests, in Newton's case no incongruity in his pursuit of understanding by looking into alchemical claims, biblical claims, observational data, and better ways to calculate. Whigs of future centuries will find plenty of reasons to wonder how they could reconcile various aspects of some of the most prominent achievers of our times, say those present-day religious believers who are also scientists. Journalist Reardon doesn't fully understand this when she reports that "alchemy is certainly a thorn in the side of historians: an unwelcome reminder of science's foray into magic." A few years ago, my *Sydney Alumni Magazine* (University of Sydney) had a piece about an historian studying the doings of alchemists, and several later Letters to the Editor expressed dismay that people would spend time delving into superstition.

Mistaken views and approaches can take a long time to die off, among the media and the general public even more than among academics.

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Is "Alien Abduction" Extraterrestrial Visitation? Developing Prospective Study Designs to Gather Physical Evidence of Alleged "Alien Abduction" by Martin Hensher. *Journal of the British Interplanetary Society, 63* (2010), 307–315.

The alien abduction phenomenon remains a mystery, one primarily investigated either by UFO researchers or, in the academy, by psychologists. The former group has mainly recorded witness testimony, often with hypnosis; the latter crowd has proposed various mechanisms that might cause a person to report abduction experiences although no such thing had really occurred. Very few investigators have attempted to penetrate to the heart of the mystery and gather physical evidence that might conclusively demonstrate that something unusual is occurring, or alternatively reveal that nothing out-of-the-ordinary happens during a purported abduction.

Such a study is at least conceivable, if barely feasible, because abductions are said to happen routinely to some individuals, often in their homes or in other familiar settings. In this paper, Hensher brings to bear his expertise in epidemiological methods to investigate the key design and statistical issues of a potential cohort study that would use physical instrumentation for continual monitoring of abductees. He lays out the various factors, including abductees with and without memories of their experience, various event frequencies, study periods, drop-out rates, and so forth, and then conducts simulation studies to calculate sample sizes at various levels of statistical power. What soon becomes evident is that, if some, but not all, abductions are real, hundreds of abductees would need to be included in a study to obtain reasonable power (and thereby avoid a Type II error of concluding there is no physical evidence).

These calculations illustrate the difficulty faced by any concerted attempt to study abductees systematically and obtain objective evidence, especially given the lack of current funding in ufology for anything but small-scale efforts.

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Center for UFO Studies

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