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EDITORIAL

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

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

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

I also receive more than occasional complaints from readers who are outraged that a particular article appeared in the *JSE*'s pages. Sometimes they object to the topic of the paper, and sometimes they complain about the way the topic was handled. I'm frequently puzzled about the former sort of complaint. If the reader has such a strong reaction to a topic (s)he considers too disreputable to be covered in the *JSE*, this would seem to be someone who doesn't quite get what the journal is all about. The latter sort of complaint often displays a different kind of shortcoming—namely, a failure to understand the nature and function of peer review. For example, last year a reader was moved to write: "I can't believe a paper with such faulty logic could be published in a peer-reviewed scientific journal. Don't you think saying things like this based on their lame evidence is totally nonsensical?" Then, after quoting a remark which out of context looks much more questionable than when read in context, my correspondent asked: "Why doesn't this demonstrate that your guys' peer review is a joke? How in the world can you possibly justify publishing such a shoddy paper?"

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

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

this perspective, induction will disappoint only if we're engaged in a quixotic foundationalist quest for final or absolute justifications.

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

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

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

* * *

One last note, on a much different topic. As I hope all readers have noticed, some of our book reviews have been of works that are quite old. For example, our previous issue contained a review by Carlos Alvarado of Charles Richet's 1922 *Traité de Métapsychique.* That's not because Richet's publisher was slow to provide us with a review copy. On the contrary, since at least 2002 the *JSE* has occasionally published reviews of important older books, and this will continue to be a recurring (if not regular) feature of our book review section. David Moncrief (our book review editor) and I share Carlos's hope "that these reviews may bring the old material to the attention of current researchers and to the new generation" (personal communication). It's all too easy to dive

into the study of scientific anomalies with little or no awareness of the often sophisticated theoretical and empirical work that's already been done. I've seen this many times in parapsychology, where newcomers to the field think the only qualification for doing first-rate work is competence in some related area of mainstream science.

At the moment, the reviews of older books in our editorial pipeline all concern important theoretical and empirical research in parapsychology. But that's just a temporary and unplanned state of affairs. In the past, reviews of older material have been in areas other than parapsychology. In any case, I want readers to know that this is a general project we will continue to pursue. The *JSE* will continue to publish occasional reviews of older seminal works in various areas of science relating to the study of anomalies.

Stephen E. Braude

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RESEARCH

Psi in a Skeptic's Lab
A Successful Replication of Ertel's Ball Selection Test

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Abstract—In the Ball Selection Test for assessing psi, ping pong balls are drawn blindly from an opaque bag one at a time with replacement. Each ball has an integer from 1–5 and red or green dots marked on it, thereby producing 10 distinct alternatives. On each trial, a participant jumbles the balls, and attempts to guess both the number and the dot color on the ball prior to pulling it out of the bag. Because the 10 ball types are equally represented in the bag, the probability of correctly guessing both the number and the dot color by chance is 10%. In the full protocol, participants first test themselves at home without supervision. Those who score significantly above chance are then retested in the laboratory under an experimenter's supervision. In an experiment by the author with participants of the Georg-Elias-Müller Institute (GEMI), 47 participants achieved a hit rate of 11.6% in the at-home phase of the study, $p = 10^{-14}$ by a one-tailed binomial test; nine selected participants retested in the laboratory achieved a hit rate of 17.3% ($p = 10^{-50}$). A replication of the laboratory procedure was conducted by two graduating students working under the guidance of a skeptical professor at the Anomalistic Psychology Research Unit (APRU) at Goldsmiths College, University of London. Their 40 unselected APRU participants achieved a hit rate of 10.75, which was very significant by a binomial test ($p = .002$) and $p = .0003$ by summed Z^2 values. The lower hit rate of the APRU participants compared with GEMI participants was significant ($p = .02$) and predicted. It is argued that this low-tech testing procedure is less monotonous and more psi-conducive than conventional multiple choice procedures for testing psi.

Keywords: ESP—Ball Selection Test—skeptics

Introduction

Lack of replications of psi test results is a hotly debated problem of parapsychological research. Professor Chris French, head of the Anomalistic Psychology Research Unit (APRU), Goldsmiths College, University of London, offered me an opportunity to give a lecture to his team on the Ball Selection Test, a novel multiple choice procedure designed for screening psi abilities (Ertel, 2005a, 2005b, 2005c).[1] The participant's task in this test requires manual actions that are less monotonous than actions commonly required for conventional multiple choice procedures. Less tiresome conditions are generally regarded as more psi-conducive. In my lecture I claimed that the Ball Test results are generally

more replicable than those of other multiple choice procedures. Moreover, this test allows for considerably more trials per time unit; increased trial numbers improve statistical significance levels, if psi effects are real. I left a specimen of my test material at APRU, i.e. an ordinary opaque sports bag containing 50 ping pong balls as well as instruction and record sheets, in case members or students of French's team might want to give this test a try.

Shortly thereafter, two students at APRU, Johanna Körting and Luke Hagstrom, used this material for experiments that they conducted for a Final Year Project in fulfillment of BSc Psychology requirements (Hagstrom, 2002, Körting, 2002). After receiving their BSc certificates, they kindly provided me with copies of their theses and the data that they had collected. I wanted to know whether the students had replicated the results of the Ball Selection Test that I had obtained with participants at the Georg-Elias-Müller Institut für Psychologie (GEMI), Göttingen University, Germany. Two independent replications using this test had previously been made, one under my supervision by a student at GEMI who collected Ball Selection Test data for her diploma (Masuhr, 2000). Another study had been performed under the sole responsibility of researchers at IGPP (Institut für Grenzgebiete der Psychologie und Psychohygiene), Freiburg (published in Ertel, 2007). Both studies yielded very significant psi effects, even though the effect size was smaller than those that I, as experimenter, had obtained myself. The differences of results between my own study and the two replications might partly be due to "experimenter effects" (as investigated by Rhine & Pratt, 1957, Honorton, Ramsey, & Caribbo, 1975, Palmer, 1993, 1997, Schlitz & LaBerge, 1994, Watt & Ramakers, 2003, Smith, 2003). Psi manifestations are known to be affected by an experimenter's attitudes and personality traits. Open-mindedness toward claims of the paranormal is deemed a favorable condition for psi effects, whereas skeptical attitudes seem to obliterate them. I suggested to Professor French, editor of *The Skeptic,* to encourage research with my Ball Test in his Unit;[2] however, with some concern, because participants, probably influenced by their skeptical teacher, might perform this test with reservation. But I also presumed that the "skeptical look" by APRU would be mild enough and the Ball Test apt enough to let psi become manifest even under those suboptimal environmental conditions.

The following account of the APRU study is based on Körting's and Hagstrom's theses.[3] An independent re-analysis will be conducted first, and the results of the two analyses (mine and the students') will be compared.

Methods

The students followed the standard procedure of the Ball Selection Test, as introduced at GEMI, Göttingen University. They had obtained GEMI's standard instructions (see Appendix A and Procedure below). Yet, the APRU study

differed from GEMI studies in certain respects. The GEMI standard procedure has two stages. Stage 1: Participants, after receiving test material and instruction, complete the test at home, without supervision. After returning the filled-out record sheets and after analyzing results, participants with significant home test scores—a minority out of the total—are invited to complete, under supervision, additional tests at the Institute, using the same material and instruction (Stage 2). The students at APRU and their teacher, however, decided to forgo home tests of participants. All tests were conducted under their supervision with unselected participants.

Experimenters and Participants

The experimenters, Luke Hagstrom and Johanna Körting, were undergraduate psychology students, aged 26 and 23, respectively. They tested 20 participants each.

The tests were completed by 40 participants, 14 male and 26 female, their ages ranged from 19 to 56, mean age 28.6. The majority were undergraduate students at APRU, most of them fellow students of the experimenters. Older family members were also tested. The participation was voluntary.

Materials

A participant's test material consists of 50 table tennis balls in an opaque bag with a drawstring opening. The drawstring is adjusted to make the opening wide enough to fit a hand into the bag. On each ball, one of the numbers 1, 2, 3, 4, or 5 is written in black permanent marker. Each number is written on 10 balls. A number is repeated ten times around one ball's sphere so as to make the number immediately visible irrespective of the ball's position. In addition, either red or green dots are marked on each ball in the space between the numbers; 5 balls of each number have red dots and 5 have green dots. This is the standard material of test version II which was used by Körting. Hagstrom used "air-flow" balls instead; these were of the same size as table tennis balls but with small holes. Half of the balls were white and half orange; the colors of the balls replaced the two-color marks of the table tennis balls. The change of material was, according to Hagstrom, due to "availability and time constraints". No further explanation was given.

Procedure

A participant's ball selection task consists of 6 runs. For each run, 60 trials are made within 10–15 min, on average. The total of 360 trials, completed in 60–90 min, was generally distributed over 2 or 3 sessions on different days. Since the participants were acquaintances of the experimenters, runs took place

under familiar surroundings at the College or in student homes. The experimenter tried to create a relaxed atmosphere.

The participants were approached by oral invitation and were informed about the purpose of the experiment (a "test of extrasensory perception"). When they arrived for the test, individually, they were told that the data would be treated confidentially and that they were free to withdraw from the experiment at any time. They confirmed having received this information by signing a paper which gave informed consent. Only then were they asked to read the two pages of instructions (Appendix A) and to ask questions if they had any.

For each trial, participants place their left hand in the bag, left-handed individuals used their right hand, so as to have their writing hand free to record results immediately. The participant turns the bag over once in order to jumble the balls and to randomize their positions. Next, participants may proceed in one of two ways. They may single out a ball and, before taking it out, they guess which number is written on it and which color the dots have. Or they may first guess which number and color they will draw and then single out a ball. Participants are told that they may change the sequence of guessing and ball picking, but changes are only allowed between runs and are recorded by the experimenter.[4] After having taken a ball out of the bag, the experimenter records the selected number and color on the record sheet (Appendix B). Finally, the ball is put back into the bag, and the next trial begins.

The instructions for at-home testing of Phase 1 (Appendix A) direct that participants themselves record their guesses and the drawn numbers and colors. Hagstrom followed this same procedure for the laboratory tests at APRU, remaining inactive except for monitoring the participants to ensure that the procedures were correctly followed and that no cheating occurred. In contrast, Körting filled out the record sheets herself.

APRU participants also guessed, prior to testing, how they thought they would do and rated how they thought they had done after the testing. They also filled out personality and attitude questionnaires. But only the actual psi performance data are discussed in this article.

According to the instructions (Appendix A) (translated into English from GEMI's instructions for home test runs), participants jot down the guesses and selected numbers and colors. Hagstrom's participants complied with this: *"The experimenter was inactive throughout, except for watching the participants to ensure procedures were carried out correctly, and that cheating did not occur"* (p. 12). Körting as experimenter was in charge of filling out the record sheets. She recorded the guesses as well as the drawn numbers and colors.[5] Before reading the instructions, the participants had to rate, on a five-point scale (*"very likely"* to *"very unlikely"*), whether they thought that they "will correctly guess, above chance, the numbers and/or colors written on the balls". After test completion, they had to select an answer to the question whether they believed

they "correctly guessed the numbers/colors, above chance expectation. . . . " (*"I completely agree"* to *"I completely disagree"*). They then also completed an extraversion scale and a paranormal belief scale. The students' analyses also included analyses of correlations between hit scores in the ball test and personality data; Hagstrom was in charge of looking at extraversion, Körting of looking at possible "sheep and goat" effects (influence by belief). The student experimenters' pre- and post-experimental judgments and questionnaire data which are not part of the standard procedure had not been made available and are not considered in the present re-analysis. Hagstrom's original trial-by-trial data had not been provided either. He merely listed, for each participant, totals of number hits, color hits, and double hits for 360 trials (6 runs times 60 trials) (see Table 1). A trial-by-trial re-analysis can therefore be conducted only for Körting's participants.[6]

Results

The analyses as done by Hagstrom and Körting differed from what I would call appropriate. An account of an independent data analysis is provided first. The students' results are provided in detail below, for comparison.

1. Steps of analysis. The main variable of the ball test, version 2, which was used, is the double hit count, the count of hitting numbers and colors of single balls. Table 2 displays the results of the GEMI at-home tests on unselected participants, the GEMI laboratory tests on the selected subsample of participants successful at home, and the results of the APRU laboratory replication with unselected participants. Row 01 gives counts of participants, row 02 counts of trials which, divided by 60, yield the count of runs per participant. The sample's total trials are given in row 03. An analysis, using uni-directional tests of psi effects, yields Z_{bin}, which is based on scores summed across participants (row 04). The error probability p for this Z value is given in row 08, and the corresponding effect size ES_1 (for formula, see Equation (1) in the Table 2 legend) in row 09.

Since for the present data the observed hit rates per run (row 06) are all larger than expected (row 05), a one-tailed significance test (row 08) is considered appropriate. Negative deviations from chance (psi missing) were not hypothesized.

However, a more powerful way of analyzing psi test data of the multiple choice type has been provided by, among others, Timm (1983:222). Individual Z^2 values are used which, when summed across participants, yield Chi^2 values ($df = N$, number of participants, row 13). In contrast to using the summed hits procedure (rows 07–09), by using summed Z^2, participants with large psi-missing values contribute equally and positively to the psi indicator (Chi^2), the direction of deviations from MCE (mean chance expectancy) being irrelevant.

TABLE 1
Summary Data for Each APRU Participant in Rank Order

Rank	Hit Count				
	N	***C***	***NC***	Z_{Bin}	***p***
1	89	224	67	5.36	10^{-6}
2	87	189	51	2.55	.007
3	75	180	50	2.37	.01
4	87	187	48	2.02	.03
5	90	189	47	1.84	.04
6	83	176	47	1.84	.04
7	68	187	46	1.67	.05
8	70	199	45	1.49	—
9	82	181	43	1.14	—
10	83	207	42	0.97	—
11	61	190	42	0.97	—
12	78	199	41	0.79	—
13	65	179	41	0.79	—
14	82	186	40	0.61	—
15	77	168	40	0.61	—
16	74	179	40	0.61	—
17	79	190	40	0.61	—
18	80	187	40	0.61	—
19	67	189	40	0.61	—
20	73	196	39	0.45	—
21	74	192	38	0.26	—
22	78	171	38	0.26	—
23	73	172	37	0.09	—
24	64	175	37	0.09	—
25	68	185	37	0.09	—
26	78	188	37	0.09	—
27	79	180	36	0.00	—
28	77	171	36	0.00	—
29	79	187	35	0.00	—
30	83	180	34	−0.26	—
31	68	180	34	−0.26	—
32	61	171	33	−0.44	—
33	55	195	32	−0.44	—
34	68	158	31	−0.79	—
35	52	184	29	−1.14	—
36	64	183	28	−1.32	—
37	58	146	28	−1.32	—
38	59	146	27	−1.49	—
39	72	171	26	−1.67	—
40	58	175	26	−1.67	—

N, C, NC, counts of number, color, and double hits (number plus color hits for individual balls).
Z_{Bin}, binomial *Z* values. *p*, one-sided significance level. —, not significant.

TABLE 2
Double-Hit Results of APRU Data Compared with Results from GEMI Studies with Unselected and Selected Participants

Double Hits Expected 10%	No.	Variables	APRU Unselected under Supervision	GEMI I Unselected without Supervision	GEMI II Selected under Supervision
Database essentials	01	$N_{Participants}$	40	47	9
	02	$Trials_{participant}$	360	480	480
	03	$Trials_{total}$	14,400	22,560	4,320
	04	Hit_{total}	1,548	2,620	748
	05	$Expected_{run}$	6.00	6.00	6.00
	06	$Observed_{run}$	6.45	6.97	10.39
Summed hits analysis	07	Z_{bin}	2.99	8.07	16.00
	08	p	**0.002	**10^{-14}	**$<10^{-50}$
	09	ES_1	0.025	0.054	0.243
Indicator bi-directionality	10	*Kurtosis*	5.95	5.93	2.10
	11	Z'	2.88	2.89	−0.27
	12	p	**0.002	**0.002	n. s.
Summed Z^2 analysis	13	Chi^2 $(df = N)$	76.3	288.0	279.4
	14	p	**0.0003	**$<10^{-50}$	**$<10^{-50}$
	15	Z''	3.43	>16.00	>16.00
	16	ES_2	0.029	0.067	>0.243

At GEMI, student assistants were in charge of experimenter control for seven participants, the author for two participants.
n.s., not significant.
**, very significant.

(1) $$ES_1 = \frac{Z_{bin}}{\sqrt{Trials_{total}}}$$ (2) $$ES_2 = \frac{Z''}{\sqrt{Trials_{total}}}$$ (3) $$Chi^2 = \sum_{n=1}^{N} Z^2$$

Z_{kurt} : Z of Kurtosis
Z_{Chi} : adapted from p of Chi^2 (D'Agostino, Belanger, & D'Agostino, 1990)

Chi^2 can be transformed into equivalent Z values for the sample (row 15) and an effect size ES_2 is obtainable (see Equation (2) in Table 2) which may be compared, e.g., with effect size ES_1 (see Equation (1) in Table 2). The main advantage of individual Z score summation, with Z values squared, is that decisions between one- and two-sidedness of p tests are obsolete.[7]

An indicator of the kurtosis (curvature) of hit score distributions was also obtained. A significant kurtosis deviation from the expected value of 3.0, as shown in rows 11 and 12 for APRU and GEMI I data, indicates a flat distribution, i.e. a non-normal spreading of hit scores in psi-hitting and psi-missing directions. A significant positive kurtosis would call for the Chi^2 analysis (summed Z^2). A Chi^2 analysis may not be needed, but it is applicable nonetheless, as a rule, with insignificant kurtosis deviations.

2. Summary of re-analyses of APRU data. Summed hit scores of APRU participants are very significantly larger than MCE (p = .002) by a one-sided test. The significance is more conspicuous (p = .0003) by summing Z^2 (Chi^2 test). The kurtosis of the data is flat (positive), which is noticeable by mere inspection of Table 1. Binomial Z values below zero (for participant ranks 30 to 40) increase rapidly in size, compared with the Z values above $Z = 0$. More indications of psi-missing tendencies of individuals in this sample are looked at below.

3. Comparing results of APRU and GEMI participants. Göttingen's GEMI I hit scores from 47 unselected participants, conducting the test under home conditions, are significantly larger than those from the 40 unselected APRU participants, who completed the test under supervision. Averages for one run are: 6.97 hits (GEMI) vs. 6.45 hits (APRU), respectively; expected are: 6 hits, see rows 05 and 06; the difference amounts to $Chi^2 = 6.55$, $df = 1$, $p = .01$.

GEMI II results of 9 participants, selected from the sample of 47 as good home test scorers and tested subsequently under control by one of two student assistants or by this author, obtained very large deviations from expectancy, compared with the total home test sample (averages 8.51 vs. 6.97 hits per run; expected: 6 hits). Not surprisingly, the hit score deviation of the selected N = 9 sample from the total of unselected N = 47 is very significant, $Chi^2 = 28.4$, $df = 1$, $p = 10^{-7}$.

The students' data analyses with commentaries. Both student experimenters subjected the scores of their N = 40 participants for number, color, and color-plus-number hits (= double hits), to one-sample t tests. The mean chance expectancy (MCE) of double hits of a participant was 36 (6 runs × 60 trials × 0.10 expected). However, t tests in this analysis are unsuitable because, to some extent, the size of deviations from expectancy (which is very important for assessing psi effects) is underrated while their variance (completely unimportant) bears upon the result. For example, with an MCE = 36 for each participant, a sample A of N = 6 participants obtaining, say, 37, 40, 43, 46, 49, 52 hits (total 266 hits) would obtain, with t test, a less significant p value for their successes ($t = 3.7$, $p = .007$) than an N = 6 sample B obtaining, say, 39, 40, 41, 42, 43, 44 hits (total 249 hits) ($t = 7.2$, $p = .0004$), while the actual deviation from chance probability of observed hit scores of sample A (266−216 = 50) is larger than that of sample B (249−216 = 33).

Even though a t test applied on the present data lacks power, both students obtained by two-sided tests significant or almost significant p's for double-hit scores, the main psi indicator. Körting reports an almost significant p = .052; Hagstrom, for the same data, a significant p = .038. Hagstrom's two-sided p is correct, Körting's deviating value which should equal Hagstrom's is apparently due to a calculation error. However, the students should have applied a one-

sided significance test since their "hypothesis one" has direction ("psi generates positive deviations from chance"). The correct one-sided error probability of observed APRU hit scores, obtained by a *t* test, is $p = .019$.

Hagstrom renders his *t* test insignificant by a Bonferroni correction since he claims that multiple indicators had been applied, i.e. aside from double hits (numbers plus colors) the significance also of number hits and color hits was obtained. But the Bonferroni correction is not admissible here since by instruction for participants the double-hit score is the ultimate success measure (see Instructions in Appendix A). A Bonferroni correction would have been admissible only for either number hits or color hits, if the double-hit score which the participants wanted to raise had not been significantly raised. Körting renders the (wrong) "almost significant" *p* insignificant as she concludes: *"None of the hit rates reached a level of statistical significance, implying that, from the overall results obtained in this study, one could not conclude that psi was operating for guessing of colors and numbers written on the balls"* (Körting, 2002:24). Her results of $p = .052$ which was not exactly significant should have been called "marginally significant".

Hagstrom, however, while taking a similar view of analysis, also did what should have been done in the first place. He calculated binomial *Z* values for participants individually and refers to the results briefly as follows: *"Although hypothesis 1 was not supported, the data offers support to the idea of psi existing with several participants reaching sig. hit levels ($p < .05$). This is particularly evident with the 'outlier'* [see participant rank #1]. *The probability of 224 or more color hits is* $p = .00000$, *of 89 or more number hits is* $p = .012056$, *and of 67 or more D [double] hits is* $p = .0000003$*"* (p. 16).

Hagstrom does not discuss the methodological difference between *t* test and binomial test, nor does he combine, which should have been done, the binomial *Z* values across participants. He concludes: *"Hypothesis one was offered no significant statistical support after data had been subjected to bonferroni adjustments"* (Hagstrom, 2002:17). Notwithstanding, his binomial *p* observations for individuals do not put it to rest: *"What proved to be the most interesting data came from participant no. 6 when binomial distribution was employed. 67 or more double hits had a probability of* $p = .0000003$ *(about 1 in 3 million). This although not in direct support of hypothesis one, does suggest that psi may exist."* The student does not simply verify a highly significant deviation from expectancy, he feels obliged to claim, at the same time, that this does not support hypothesis one ("psi exists"). Nevertheless, he continues: *"Psi may not be something we are all able to use or even possess. There may just be some gifted individuals.*[8] *This idea is supported by data that Ertel (2000) has collected, with some of his participants achieving hit rates over 100% above MCE. When we look at most human abilities it is clear that people vary in how good they*

are, so why not psi" (Hagstrom, 2002:18). Körting apparently did not look at individual differences of ball test scores; her final conclusion seems to be unambiguously consistent with possibly unsaid negative expectations: *"The results obtained by Ertel (2002) were not replicated"* (Körting, 2002:30).

Discussion

The APRU students' way of analyzing their data (*t* tests) lacked power. By using appropriate statistical procedures, hypothesis one of the thesis writers ("psi effects exist") found support. It should be added that the statement "psi effects exist" does not imply any commitment as to how many participants in the sample manifest psi effects individually and in which measure.

The APRU participants' hit scores, obtained under supervision, are significantly lower than results obtained, without supervision, from unselected GEMI participants. Apparently, in general, supervision is a psi-detrimental condition, probably due to increased emotional tension (for reviews, see White, 1976a, 1976b). In addition, differences between more optimistic (GEMI) vs. more skeptical social embeddings of the experiments (APRU) might have been effected.

The summed hit scores of 9 selected GEMI participants (GEMI II sample), tested under supervision after showing significant hit surplus under at-home conditions, were considerably larger than the hit scores of the 47 unselected GEMI participants who completed the test at home. On average, test participants, good at home, are still successful under supervision, but often with reduced effect size (Ertel, 2005b).

However, individual differences regarding hit scores under supervision are large. Under supervision, the double-hit scores for 7 out of the 9 participants of the GEMI II sample dropped more or less, compared with their home scores. But the scores of two participants, already high at home, increased noticeably under supervision—a surprising and as-yet-unexplained observation. At home the two high scorers obtained, with 480 trials, 80 and 89 double hits, respectively. Expected are 48 hits. Under supervision by the author they reached, again with 480 trials, 230 and 143 hits, respectively (230 is no typo!). These hit scores are unexpected and amazing.

An analysis of the Ball Selection Test data of version II (5 numbers and 2 colors as targets) considers double-hit scores as the crucial variable. Hit scores for numbers and colors alone may be of further interest, above all for looking at individual differences. One might want to know, e.g., whether participants differ regarding relative amounts of number vs. color hits. The correlations between number and color hits, regardless of occurring alone or in combination, are low (APRU: .40, GEMI: .30) (Table 3). Results for the two hit variables, numbers and colors separately, are given in Appendices C (Table 4) and D (Table 5) so as

TABLE 3
Binomial *Z* Values for Number, Color, and Double Hits

			APRU Data (*N* = 40)			GEMI Data (*N* = 47)		
			Z_n	Z_c	Z_{nc}	Z_n	Z_c	Z_{nc}
	01	*a.M*	0.05	0.19	0.37	1.04	0.71	1.16
	02	*SD*	1.28	1.49	1.33	1.81	1.59	2.21
	03	Chi^2	65.7	90.6	76.3	204.4	140.2	288.6
r correlations	04	Z_n	1.00	0.40	0.66	1.00	0.30	0.83
	05	Z_{col}	0.40	1.00	0.63	0.30	1.00	0.56
	06	Z_{nc}	0.66	0.63	1.00	0.83	0.56	1.00
p of differences	07	Z_n	0.00	n.s.	n.s.	0.00	n.s.	n.s.
	08	Z_{col}	n.s.	0.00	n.s.	n.s.	0.00	n.s.
	09	Z_{nc}	n.s.	n.s.	0.00	n.s.	n.s.	0.00

Z_n, binomial Z of number hits only.

Z_c, binomial *Z* of color hits only.

Z_{nc}, binomial Z of double hits.

Rows 07–09, *p* values (two-tailed) are gained from two-sample paired *t* tests, $df = N - 1$.

to enable researchers to make comparisons with future findings.

Skeptical observers of the ball test project tend to raise the objection that home test scores are worthless because they cannot be trusted. Their objection loses weight in view of the fact that most participants, successful at home, show psi effect, still above chance, also under supervision. Students serving honestly and cooperatively as participants for a scientist's research project hardly deserve to be looked upon with generalized suspicion. Bierman and Gerding (1991) should be mentioned as pioneers of studies with reduced experimenter control.

In view of the present results, more joint research of parapsychologists and their critics is called for. Professor French's cooperation was a rare exception. Die-hard skeptics want to make psi effects disappear altogether. They tend to do so by denaturalizing the test environment, i.e. by using dividing walls, blindfolds, and gloves; by letting participants pick each ball only once; by not giving feedback of hits and misses; by increasing numbers of skeptical onlookers and psychologically inhibiting techniques (as observed by Ertel, 2007). Experimental successes at keeping the null-hypothesis unrejected would, however, hardly add much to our knowledge except by confirming that psi manifestations may be obstructed through inhibiting test conditions. The "does-psi-exist" question cannot be tested intelligently in this manner.

The test conditions as given in Appendix A should be revised only if experimental evidence would reveal that hit success above chance, as obtained under

the Göttingen–London standard conditions, were due to sensory or memory leakage or some other non-psi factor. Several attempts to find evidence supporting a non-psi factor explanation for large hit rate deviations in the ball test had negative results (Ertel, 2004, 2005a, 2005b, 2005c). Success of further such attempts is deemed unlikely. Nevertheless, they should be continued.

Can fraud explain the results? If the answer is yes, then a considerable proportion of students in Göttingen must have been skilled conjurers. The student percentage with significant double-hit deviations from MCE was 32%, in London it was 15%. After subtracting a rate of chance expected, 5% from the successful Göttingen subsample of 27% (32%−5%) fraudulent students are obtained for Göttingen's campus, and 10% (15%−5%) for London's campus, if the observed hit rates were due to fraud. Such speculation disregards the reality of trust governing the majority of ordinary social interactions. Among students of science where curiosity usually gains the lead, fraud to raise hit scores in an experiment can only be expected to occur as an extremely rare exception.

Conclusion

Highly significant hit scores have been obtained by the author's Ball Selection Test applied under the supervision of an eminent British skeptic. The results, as analyzed by the student experimenters Hagstrom and Körting, who through inexperience were not fully conversant with the methods that should have been applied in the first place, nevertheless demonstrate that psi manifests itself even in a suboptimal environment.

This result should help to reduce a persistent reluctance to acknowledge the reality of the paranormal in skeptical circles. Moreover, the efficiency of this test for obtaining psi effects with a minimum of effort and expenditure might motivate researchers to try further replications. Parapsychological experiments are generally conducted, even today, without selecting psi-gifted participants. This might be the main reason why results in this discipline are often not replicable. Research is under way trying to further explore the validity of the ball test indicators. First correlations between ball test scores and other experimental psi manifestations have been obtained (convergent validity, Ertel, 2005a). The ball test might eventually serve as a valid tool for recruiting participants for studies in which a general psi ability is a desirable or even indispensable precondition.

Notes

[1] The original term "Ball Drawing Test" is ambiguous and may be replaced, as in this paper, with "Ball Selection Test".

[2] From Goldsmiths College Psychology Dept. web page for Chris French: "*The challenge to those who adopt the working hypothesis that paranormal forces do not exist [Professor French's working hypothesis] is to provide plausible non-paranormal*

accounts, supported by strong empirical evidence wherever possible, of the ways in which psychological and physical factors might combine to give the impression that a paranormal event had occurred when, in fact, it had not." From the *The Skeptic* magazine's homepage: "*UK's only regular magazine to take a skeptical look at pseudoscience and claims of the paranormal."*

[3] I had preferred to publish this article with Professor French and the two (now) graduates as coauthors. Professor French declined coauthorship, but allowed use of the undergraduate experimenters' names, Körting and Hagstrom, who cannot be asked for consent as the APRU administration is unable to procure their addresses. Caroline Watt (2006) has shown that using undergraduate theses for re-analysis or surveys may be a profitable undertaking. Körting's and Hagstrom's theses provided on request.

[4] The participants' freedom of choosing between the two procedures is expected to increase their confidence. Their actual performance is deemed independent of procedural preferences, since sensory cues of the written numbers on the balls are lacking in both cases.

[5] This option is preferable since the experimenter's role, from the participants' perspective, is more meaningful and apparently not intrusive. It is actually also used in GEMI experiments in the lab, for second-stage experiments under supervision.

[6] Hagstrom provided summed individual hits for number, color, and double hits for 40 participants, in an Appendix of his thesis. Körting provided trial-by-trial data for her share of the sample, i.e. for 17 of her 20 participants only. The reason why the data of three participants were missing remains unknown.

[7] The reason is that Chi^2 tests are two-sided anyway. A one-sided p test for Chi^2 results would only be relevant if one predicted that the hit scores would cluster above chance close to MCE (yielding a negative kurtosis). But such prediction does not make sense.

[8] Hagstrom adds that the participant with extreme hit rates was his mother. It seems unreasonable to surmise that the young experimenter was deceived by his mother.

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Appendix A

Instructions for the Ball Selection Test

(Home Test Instructions)

The Ball Selection Test has been developed to test participants' paramental abilities. Paramental abilities are sometimes referred to with terms such as *psi sensitivity* or *sixth sense.* They may manifest themselves as sensory or motor accomplishments that cannot be explained by psychophysical mechanisms. In theory, all people might possess psi abilities. Their expression, however, often seems to be inhibited subconsciously. Inhibited psi effects remain untraceable, and this test cannot reveal conspicuous successes in this task despite general inhibition of pertinent subconscious dispositions.

The test material consists of 50 table tennis balls in an opaque bag. On each ball, one of the numbers 1, 2, 3, 4, or 5 is written. Ten balls carry number 1, 10 balls number 2, etc. Aside from the numbers, each ball carries either red or green dots. Among 10 balls with number 1, five balls have green dots, and five balls have red dots. The same distribution of colored dots applies to the numbers 2, 3, 4, and 5.

You are provided with a record sheet [Appendix B] on which you will, please, note the results of this test. One sheet gives space for two runs.

For each run, when you begin, put down on the record sheet the actual date and clock time. Blanks for name and telephone number (and email address, if available) should also be filled out on the first record sheet.

Start off with a ball selection trial by putting your left arm into the bag (or your right arm if you are left-handed). The bag's opening should be adjusted so that you can move your arm in and out without much friction.

After putting your arm into the bag, you should jumble the balls so that their position becomes random. Turning the bag over on the table once, like a pancake on a pan, is an efficient jumbling technique. This should be done for each trial.

Next, single out a ball and draw it out of the bag. Before doing so, however, you guess and jot down on the record sheet the number and color that you may think you will draw on the next trial (see first row: "number/color"). For example, "3r" for trial (a) means "I expect/hope to draw number 3 with red dots on the ball". Use "r" for red and "g" for green.

After selecting the ball, see which number and color you have drawn, and jot down the number and color in the row underneath ("number/color drawn"). You might fail for both, number and color. You might hit the color but fail the number, or vice versa. Such partial hits are good, but of course hitting both number and color is the best result. Put an "H" in the third row for such double hits.

It is up to you how you guess the numbers and colors and how you select the balls. It might be helpful to close your eyes and visualize balls and numbers with colored dots. But no rule needs to be considered. You may open your mind for intuitions or concentrate mentally on your goal, you may grasp the balls in a straightforward way or surf over the balls trying to "feel" the numbers. You may try one ball first and then exchange it with another ball later, if you want, as long as you keep your hand in the bag. Peeping into the bag is not allowed. Raising your head as if you would look up to the ceiling, perhaps even with your eyes closed, would be the best way to avoid taking advantage, subconsciously, of visual cues.

Some people prefer first to grasp the ball and then make their guess. If you prefer this way of guessing, please make a note on the record sheet and maintain this procedure at least for the rest of that run.

After recording the number and color of the ball just drawn, please throw it back into the bag. It should bounce down, so do not leave it in your hand when you put it back for a next trial.

Sixty trials make one run. One run is divided on the record sheet into four rows of 15 trials. At the end of each row, please write down the number of double hits in that row. For data analysis, all hits will be considered, not only double hits.

While taking this test, you might have interesting experiences. Put down on the back of your sheets any observations that you think might be interesting to the experimenter.

If you would like to do some dry trials before filling out the record sheets, you may do four such trials without recording.

One run takes 10–15 min on average, first runs perhaps a little more. Your personal speed might deviate from the average; there is no time pressure.

Hit rates not only vary among participants, they also vary across sessions of individual participants. Considerable changes of hit rates of individuals across sessions cannot fully be explained, but participants tend to believe that low scores are due to bad mood, tenseness, overdrawn expectations, tiredness, and boredom. Please make a note on your record sheet if you suffer from any of the above conditions.

As soon as the analyses of your data is submitted, you will receive written feedback, if you wish. Thank you very much for your participation.

Appendix B

Record Sheet for the **BALL SELECTION TEST**

On the 1st sheet fill in your Name:__________________________________ *Tel:*_________________________

For each run jot down its ***number*** *and* ***date*** *and* ***time*** Email:_____________________________________

Number of run:______ **Date**: _______________ **Time**: ____________

	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	
Number/color **guessed**																
Number/color **drawn**:																Score
Mark double hit:																
	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	
Number/color **guessed**																
Number/color **drawn**:																Score
Mark double hit:																
	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	
Number/color **guessed**																
Number/color **drawn**:																Score
Mark double hit:																
	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	
Number/color **guessed**																
Number/color **drawn**:																Score
Mark double hit:																

Appendix C

TABLE 4
Number Hit Results of APRU Participants Compared with Results from GEMI Studies

Number Hits Expected 20%	Number	Variable	APRU Unselected under Supervision	GEMI I Unselected without Supervision	GEMI II Selected under Supervision
Database	01	$N_{Participants}$	40	47	9
	02	$Trials_{participant}$	360	480	480
	03	$Trials_{total}$	14,400	22,560	4,320
	04	Hit_{total}	2,918	4,942	1,190
	05	$Expected_{run}$	12.00	12.00	12.00
	06	$Observed_{run}$	12.16	13.14	16.52
Summed hits analysis	07	Z_{bin}	0.78	7.15	12.38
	08	p	n.s.	$^{**}10^{-12}$	$^{**}10^{-35}$
	09	ES_1	0.007	0.048	0.188
Indicator bi-directionality	10	*Kurtosis*	2.22	4.45	1.62
	11	Z'	−1.16	2.01	−1.23
	12	p	n.s.	*.02	n. s.
Summed Z^2 analysis	13	Chi^2 $(df = N)$	65.71	187.2	216.11
	14	p	**0.006	$^{**}10^{-13}$	$^{**}<10^{-35}$
	15	Z''	2.49	7.40	>12.38
	16	ES_2	0.021	0.049	>0.188

At GEMI, student assistants were in charge of experimenter control for seven participants, the author for two participants.
n.s., not significant.
**, very significant.

(1) $ES_1 = \dfrac{Z_{bin}}{\sqrt{Trials_{total}}}$ (2) $ES_2 = \dfrac{Z''}{\sqrt{Trials_{total}}}$ (3) $Chi^2 = \sum_{n=1}^{N} Z^2$

Z_{kurt} : Z of Kurtosis

Z_{Chi} : adapted from p of Chi^2 (D'Agostino, Belanger, & D'Agostino, 1990)

Appendix D

TABLE 5
Color Hit Results of APRU Participants Compared with Results from GEMI Studies

Color Hits Expected 50%	Number	Variable	APRU Unselected under Supervision	GEMI I Unselected without Supervision	GEMI II Selected under Supervision
Database	01	$N_{Participants}$	40	47	9
	02	$Trials_{participant}$	360	480	480
	03	$Trials_{total}$	14,400	22,560	4,320
	04	Hit_{total}	7,292	11,647	2,342
	05	$Expected_{run}$	30.00	30.0	30.00
	06	$Observed_{run}$	30.38	30.97	32.52
Summed hits analysis	07	Z_{bin}	1.53	4.88	5.52
	08	p	n.s.	$^{**}10^{-6}$	$^{**}10^{-7}$
	09	ES_1	0.013	0.032	0.084
Indicator bi-directionality	10	*Kurtosis*	4.76	3.62	3.38
	11	Z'	2.17	1.26	1.31
	12	p	0.01	n.s.	n. s.
Summed Z^2 analysis	13	$Chi^2\ (df = N)$	90.58	138.4	101.8
	14	p	$^{**}10^{-5}$	$^{**}10^{-10}$	$^{**}<10^{-17}$
	15	Z''	4.3	6.4	8.5
	16	ES_2	0.036	0.043	0.129

At GEMI, student assistants were in charge of experimenter control for seven participants, the author for two participants.
n.s., not significant.
**, very significant.

(1) $ES_1 = \dfrac{Z_{bin}}{\sqrt{Trials_{total}}}$ (2) $ES_2 = \dfrac{Z''}{\sqrt{Trials_{total}}}$ (3) $Chi^2 = \sum_{n=1}^{N} Z^2$

Z_{kurt} : Z of Kurtosis

Z_{chi} : adapted from p of Chi^2 (D'Agostino, Belanger, & D'Agostino, 1990)

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RESEARCH

Anticipatory Alarm Behavior in Bengalese Finches

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Abstract—The ability for short-term alarm precognition was explored in Bengalese finches. During the experimental trials, subjects were put individually for 20 minutes in a testing cage and in the last 5 minutes a 15-second video clip of a slowly crawling snake was shown to them in a TFT screen. The video clip was presented at random starting out from 20 possible randomly predetermined options. During the control trials, no snake video clip was shown to the birds. Subjects were filmed, and, in a double-blind fashion, the frequency of their display of alarm was registered 0–3, 3–6, and 6–9 seconds immediately before stimulus presentation and before the same moment of the control trial for each bird. As a second control, behavior frequency was also registered immediately before the 10-minute point after the initiation of the experimental trial. The results showed that the birds reacted to the snake video clip at least 9 seconds before presentation, the frequency of the alarm display during that period being higher than that during both controls. Females and males did not differ significantly in any of the measures.

Keywords: alarm display—Bengalese finches—birds—precognition

Introduction

There has been an upsurge of interest in human, unconscious, short-term anticipatory response, or precognition, especially since the first paper by Dean Radin on electrodermal activity and presentiments (Radin, 1997a).

Anomalous anticipatory effects using galvanic skin response as a conditioned-dependent variable were first explored with success by Vassy in the late 1960s, but reported later (Vassy, 1978). In this study, all data were obtained by human observation. In later and more exact investigations, electrodermal activity (Skin Conductance Level (SCL), which measures fast and slower-moving changes), was the unconscious response variable in experiments in which a series of randomly selected photographs (so that the incoming stimuli could not be inferred) was shown to the subjects. SCL increased more if the future

picture (next 2–3 seconds) was going to be emotional (shocking photos, such as erotic scenes and autopsies), than if it was going to be a calm one (pleasant and neutral photos of nature, landscapes, and cheerful people). Anticipation was also manifested, although at lower intensity, by an increase in heart rate and a reduction in fingertip blood volume (Radin, 1997a, 2004). Electrodermal activity was also significantly higher before presentation of the emotional pictures (as compared to that before the calm pictures) when Skin Conductance Response (SCR, which measures only fast-moving changes in electrodermal activity) was the responding variable (Bierman & Radin, 1997, 1999). The fact that participants anticipate much better the presentation of positive emotional pictures (generally erotic) than negative emotional pictures (violent and injurious) (Radin, 1997b) suggests that the emotional meaning of the future event may be important.

Another study (Globish, Hamm, Esteves, & Öhman, 1999) concerned with skin conductance took advantage of a previously published conventional study (i.e. with completely different goals) about the speed with which fear arises in animal-phobic subjects, and with a long enough period of measurement before the stimulus. Re-analysis of the raw data searching for a precognition effect (Bierman, 2000) showed a larger anticipation to occur before the erotic stimuli as compared to neutral (marginally significant) and animal (snakes or spiders) (non significant) stimuli.

To avoid the problem of idiosyncratic responses to pictorial stimuli, random audio startle stimuli and control moments of silence have been used. In this case, the effect of anticipation was revealed by a higher increase in relative skin conductance before the stimulus than before the control (Spottiswoode & May, 2003, May, Paulinyi, & Vassy, 2005).

Heart Rate (HR) was registered while subjects were asked to choose one out of four calm (non-emotional) pictures, the randomly selected picture being selected before they made their choice or after making their choice but not knowing about it. The experiment yielded very significant changes in HR before correct choices as compared to failed ones in the first situation, and still significant in the second experimental situation (Sartori, Massaccesi, Martinelli, & Tressoldi, 2004, Tressoldi, Martinelli, Massaccesi, & Sartori, 2005). The use of Heart Rate Variability (HRV, which refers to beat-to-beat alterations in HR, and is usually regarded as an indicator of cardiovascular autonomic control) during presentation of calm and emotionally arousing pictures produced still higher effects. This experiment resulted in a significantly higher heart rate deceleration occurring prior to future emotional stimuli compared to calm stimuli, starting about 4.75 seconds before stimulus presentation and showing some gender differences. Comparison of these results with those in the medical literature in relation to cardiac decelerations/accelerations suggests that the psychophysi-

ologic system processes the prestimulus information in the same way as conventional sensory input (McCraty, Atkinson, & Bradley, 2004a).

Several important results were also obtained in a similar experimental design where data were also collected from the electrocardiogram (ECG) and the electroencephalogram (EEG). Information was obtained from the latter on cortical Event-Related Potentials (ERP, the changes of the ongoing EEG due to stimulation, especially clear as a response to unpredictable stimuli), Heartbeat-Evoked brain Potentials (HEP, in which characteristic brain waves are seen to accompany rhythmic activity of the heart), and cardiac decelerations/accelerations (derived from the ECG). The results showed that: 1) There were significant differences in prestimulus ERPs and HEPs for calm versus emotional stimuli. 2) Both the heart and the brain are involved in stimulus anticipation, the heart apparently receiving and responding to prestimulus information before the brain. 3) The frontal cortex, temporal, occipital, and parietal areas appear to be involved in the process. 4) An interaction between HEPs and ERPs is especially apparent in females (McCraty, Atkinson, & Bradley, 2004b).

The use of functional Magnetic Resonance Imaging system (fMRI) to obtain information on Blood Oxygenation Level Dependence (BOLD, magnetic signal variation related to oxygen intake and neuron activity) enables the observation of fast-changing events in the brain. This technique was used while the participants were presented erotic, violent, and calm images, and showed that the above-quoted anomalous anticipatory effects are replicated with BOLD: Anticipatory activation of the visual cortex preceding emotional stimuli was larger than the anticipatory activation preceding neutral stimuli (before the erotic stimuli in males and before the erotic and violent stimuli in females) (Bierman & Scholte, 2002).

Although there is no reason to suppose that humans are better-equipped than other animals with respect to their ability to foresee the future, the literature on precognition by non-humans is very scant. In any case, the results of the few studies are quite promising, and if the effect is definitely demonstrated in them, the biological mechanisms involved in precognition would be open to research.

In an experiment performed with earthworms, in which the response (small changes in skin impedance) to mechanical vibration was recorded, the nearly significant results were more positive the more trials were performed, and the time window in this case would extend one second before stimulation (Wildey, 2001).

The results of experiments with dogs can be interpreted as evidence for precognition. In these studies the animals showed an anticipatory behavior long before their owners returned home, the owners returning at randomly selected times and travelling in unfamiliar vehicles (Sheldrake & Smart, 1998, 2000).

Analysis of environmental variables suggests that the dogs' anticipatory behavior is significantly affected by changes in global geomagnetic flux (Radin, 2002).

Therefore, the cumulative evidence strongly suggests that short-term precognition occurs in humans and other animals. The subjects apparently can respond physiologically to an emotionally arousing stimulus seconds before it is actually experienced, as if the body's perceptual apparatus were continually scanning the future.

Since predation is a major selective force in the evolution of morphological, physiological, and behavioral characteristics of animals, the alarm response to predation is probably a good candidate among different activities when trying to detect the capacity for an anticipatory response in them. In addition, the behavior of alarm is unfailingly produced by naïve animals in the presence of danger, and is sufficiently conspicuous for reliable data collection (Alvarez, Braza, & Norzagaray, 1976, Alvarez, Sánchez, & Angulo, 2006, Alvarez, 1993, Alvarez & Sánchez, 2003). The capacity for anticipatory perception of alarm as well as potential gender differences will be explored in Bengalese finches.

Methods

All subjects (23 adult male and 24 adult female Bengalese finches *Lonchura striata* var. *domestica*) lived in unisexual adult groups of conspecifics in 1.5 × 3 × 2 m aviaria near Seville, Spain, and were fed a varied diet of seeds and vegetables. The experiments were carried out between December 1, 2008, and April 30, 2009. Sex of Bengalese finches was determined from blood samples (Griffiths, Double, Orr, & Dawson, 1998).

Estrildine finches, among them the Bengalese finch, are potentially good subjects for this kind of study, since they are easily maintained in captivity (Vriends & Heming-Vriends, 2002) and show a very distinct behavior of alarm in the presence of potential predators (mainly tail-flicking, wing-flapping, and alarm calls) (Goodwin, 1982, Lombardi & Curio, 1985a, 1985b, Zann, 1996, Marler, 2004).

Starting between 7:30 and 10:30 UT, the subjects (one at a time) were taken from their groups and put in the 70 × 35 × 35 cm testing cage with transparent glass at both ends (Figure 1), where they were out of sight from other birds. Through a wide-open window the subjects could see the area with vegetation outside the aviarium, since being visually enclosed apparently reduces their expression of alarm (Lombardi & Curio, 1985a). During the experimental trials, after a period of time of 15 minutes, a video clip of 15 seconds duration of a slowly crawling horseshoe whip snake *Coluber hippocrepis* was shown in a TFT screen located at one end of the testing cage. The video clip was shown at random by a true random number generator (Orion Electronics), starting with

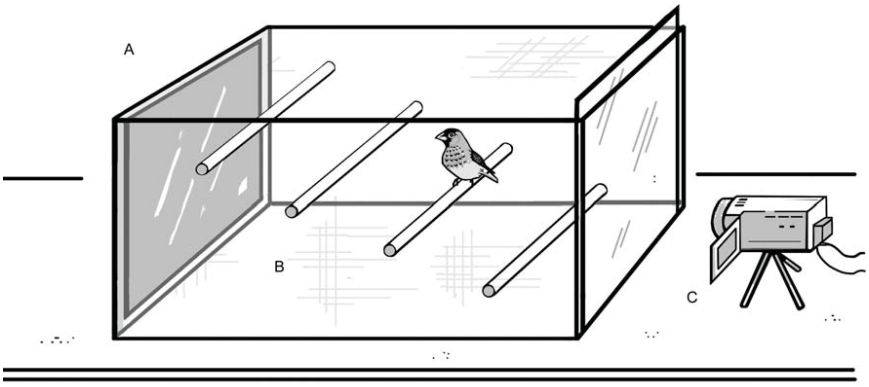

Figure 1. Testing situation. A) TFT screen, B) testing cage, C) video camera.

one of 20 possible options (unknown to the observer during the tests, and also randomly determined, Table 1) in the five minutes following the 15-minute period. The computer was located in an adjacent room and was connected to the TFT screen by a long cable.

TABLE 1
Experimental Trials

Time of Initiation of the 15-Sec Snake Video Clip after the 15-Min Accustoming Period, of the 20 Randomly Determined Options	
1	0 min 4 sec
2	0 min 11 sec
3	0 min 19 sec
4	0 min 46 sec
5	0 min 59 sec
6	1 min 5 sec
7	1 min 7 sec
8	1 min 13 sec
9	1 min 24 sec
10	2 min 2 sec
11	2 min 20 sec
12	2 min 44 sec
13	2 min 57 sec
14	3 min 11 sec
15	3 min 13 sec
16	3 min 16 sec
17	3 min 26 sec
18	3 min 36 sec
19	3 min 37 sec
20	4 min 41 sec

Although still images have been used to elicit behavior in the Bengalese finch (Watanabe & Jian, 1993), the use of video playback is usually more effective in birds (Dawkins & Woodington, 1997, D'Eath, 1998, Fleishman, McClintock, D'Eath, Brainard, & Endler, 1998, Cuthill, Hart, Partridge, Bennett, Hunt, & Church, 2000). Then, while cathode ray tube (CRT) screens are not appropriate to present stimuli to songbirds, thin film transistor (TFT) screens have been used with great success to obtain responses toward presented video clips from Bengalese and zebra finches (Ikebuchi & Okanoya, 1999, Galloch & Bischof, 2006, 2007).

During the control trial (separated 33–49 days from the experimental trial, their order being determined at random by using the true random number generator), only the same video clip background used during the experimental trials was shown to the same birds, also with a duration of 20 minutes. For 26 subjects the sequence was experimental Þ control, and for 21 subjects control Þ experimental.

During the whole experimental and control trial, each subject was filmed with a 25-frames-per-second video camera (a Sony DCR-SR72E), located outside the testing cage at the opposite end of the TFT screen (Figure 1), and controlled by the experimenter from a hidden location in an adjacent room.

Video analysis allows quantification of the intensity of alarm, which in the Bengalese finch is especially conspicuous. The alarm display lasts 0.1–0.2 seconds and consists first of very rapid flapping of both wings and then an abrupt sideways thrust of the whole body, and especially of the tail (Figure 2). The frequency of this behavior in 3-, 6-, and 9-second blocks immediately before the presentation of the snake stimulus was compared to that immediately before the same moment of the trials when no snake stimulus was shown, but only the same video clip background (control 1). As an additional control (control 2), the pre-stimulation frequency was compared to that occurring immediately before the 10-minute time point after the initiation of the same trial (when only the video clip background was shown). During frame-by-frame film analysis (accuracy of 0.04 seconds), I counted the frequency per 3-, 6-, and 9-second blocks in the above-indicated periods of the subjects' alarm behavior, using the VideoDubMod 1.5.10.2 computer program. To prevent subjective bias, 9-second video clips segments immediately before the stimulus and during the two controls were extracted from the recordings by another person, and the key that identified the segments was kept by a third party (the computer department of the institute). Once those video clip segments were analyzed by the author, without knowing whether each segment would correspond to a particular subject or to the stimulus or the controls, that key was provided.

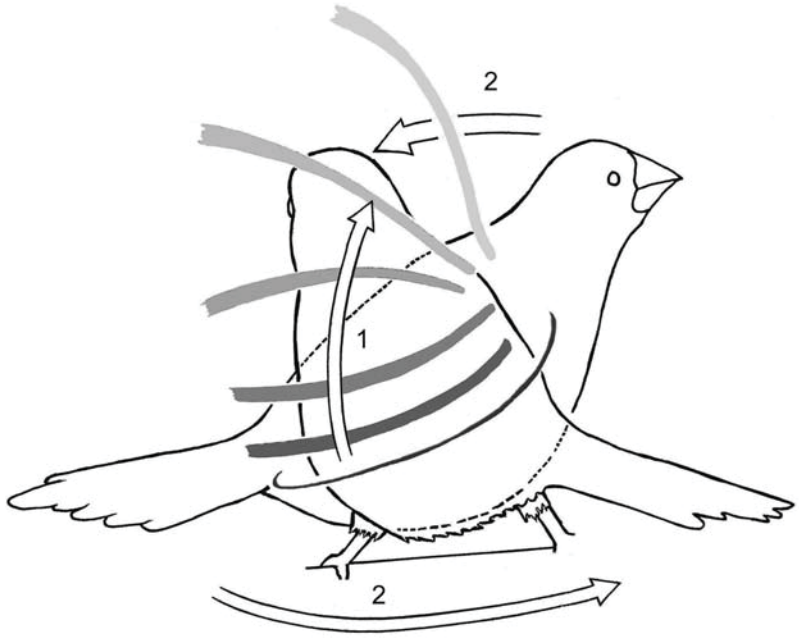

Figure 2. Acts of the alarm display of the Bengalese finch. 1) wings flapping, 2) body and tail sideways thrust.

Statistical Analysis

When separate analyses were done for males and females, nonparametric tests (Friedman ANOVA, Wilcoxon matched pairs test, Mann-Whitney U test, Siegel & Castellan, 1988, Zar, 1996) were used. When applying parametric tests, a $\log_{10}(x+1)$ transformation of behavior frequencies was carried out, and the resulting distributions did not depart from normality (p = 0.10–0.20, Kolmogorov-Smirnov test). The data were analysed using a repeated-measures ANOVA, in which control 1, control 2, and pre-stimulus measurements of the same experimental bird composed the replicate data. The t-test for dependent samples was used to compare pairs of distributions (Sokal & Rohlf, 1995, Zar, 1996). Data analysis was done using the STATISTICA 6.0 computer program. All reported p are two-tailed.

Results

When the frequencies of the behavior of alarm for the three periods of 0–3, 3–6, and 6–9 seconds before stimulus presentation were compared, they were found to belong to the same population for both females ($c^2 = 0.24$, N = 24, df = 2, $p = 0.886$; Friedman ANOVA) and males ($c^2 = 1.73$, N = 23, df = 2, $p = 0.421$). Consequently, I lumped together the three periods and considered for the rest of the analysis for males and females the 0–9 seconds pre-stimulus period.

Comparisons of behavior frequencies among the pre-stimulus period and the two control periods yielded significant results for both sexes (females: $c^2 = 9.06$, N = 24, df = 2, $p < 0.018$; males: $c^2 = 12.31$, N = 23, df = 2, $p < 0.0012$; Friedman ANOVA). Post-hoc two-variable comparisons showed, also for both sexes, that behavior frequencies for the two control periods were statistically similar (females: N = 24, T = 106, $p = 0.682$; males: N = 23, T = 112.5, $p = 0.650$; Wilcoxon matched pairs test). Furthermore, and also for both sexes, the pre-stimulus period showed significantly higher behavior frequencies than for control 1 (females: N = 24, T = 62, $p < 0.036$; males: N = 23, T = 29.5, $p < 0.008$) and control 2 (females: N = 24, T = 27.5, $p < 0.002$; males: N = 23, T = 35, $p < 0.005$).

The ratio of the frequency of the alarm display during the 9 seconds preceding stimulation to 1 plus the average frequency during the two 9-second controls was considered an adequate index of individual precognition ability (IPA). When the values for the two sexes were compared, no significant difference was obtained (IPA: mean ± SE; females: 1.59 ± 0.26, N = 24; males: 1.77 ± 0.27, N = 23; U = 247, $p = 0.537$, Mann-Whitney U test).

The frequencies of the alarm display for females and males in the 9-second blocks during the experimental and control trials are presented in Table 2.

TABLE 2
Frequency (Mean ± SE) of Bengalese Finches' Alarm Display during the 9-Second Blocks of the Different Periods

Period	Females (N=24)	Males (N=23)
Control 1	4.88 ± 0.82	4.57 ± 1.01
Control 2	5.79 ± 0.79	5.04 ± 0.95
Pre-stimulation	9.38 ± 0.84	8.61 ± 1.39

The absence of male–female significance differences enables the analysis of combined information for both sexes. In this case, the normalized pre-

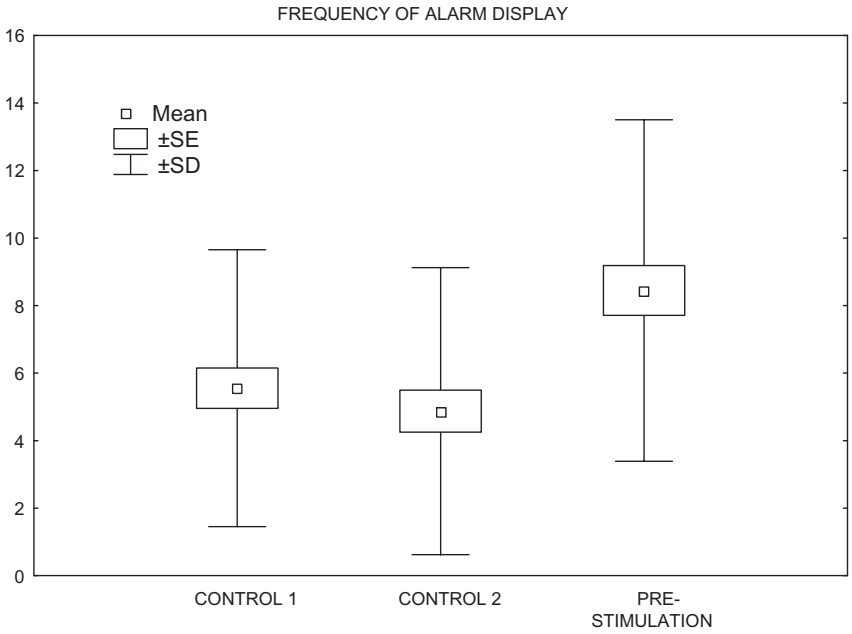

Figure 3. Frequency of the alarm display of Bengalese finches during the 9-second blocks in the two control periods and in the period immediately before stimulus presentation.

stimulation values during the three periods of 0–3, 3–6, and 6–9 seconds were also found to belong to the same population ($F_{2, 92} = 0.60$, $p = 0.552$; repeated-measures ANOVA). Consequently the 0–9 seconds pre-stimulus period was considered for the rest of this study.

Joint analysis of the values for the pre-stimulation and the two controls periods yielded a highly significant result ($F_{2, 92} = 10.12$, $p < 0.001$). Post-hoc two-variable comparisons showed the values for the pre-stimulation period to be significantly higher than those for control 1 and control 2 ($t = 3.56$, $P < 0.001$, $t = 4.49$, $p < 0.001$, respectively; df = 46; *t*-test for dependent samples). Values for both controls were not significantly different ($t = 0.93$, df = 46, $p =$ 0. 357) (see Figure 3).

Discussion

The higher frequency of the alarm display immediately before stimulation, as compared to that during the two controls, supports the idea that Bengalese finches may be able to anticipate short-term future events, also suggesting that

transtemporal perception is not limited to humans. Then, and contrary to findings in humans (Bierman & Scholte, 2002, McCraty, Atkinson, & Bradley, 2004b), no sex difference was found in Bengalese finches.

If human unconscious precognition is actually an old evolutionary mechanism in which the brain and the autonomic nervous system play a role (Bierman & Scholte, 2002, McCraty, Atkinson, & Bradley, 2004a, 2004b, Sartori, Massaccesi, Martinelli, & Tressoldi, 2004, Tressoldi, Martinelli, Massaccesi, & Sartori, 2005), and this ability has also been detected in other animals (Sheldrake & Smart, 1998, 2000, Wildey, 2001, present study), we may ask ourselves whether the different nervous systems may perform more ample functions than the classical elaboration of the information received through the ordinary sense organs (Laszlo, 2009).

Although the existence of precognition has been settled a long time back (Rhine, Smith, & Woodruff, 1938, Schmidt, 1969), the question still remains as to how it works. On the one hand, no explanation has been found so far in the deterministic framework of classical physics, as precognition seems to violate the causality principle. On the other hand, it does not fit in the unpredictable scheme of quantum physics either, suggesting that quantum theory, when applied to humans and other living systems, may not always be correct (Schmidt, 1993).

According to Bierman's (2008) psychophysical theory of Consciousness Induced Restoration of Time-Symmetry (CIRTS), the consciousness-sustaining brain can partially restore the break in time-symmetry, thus allowing for time-reversed processes like precognition. In a broad sense, the data for these Bengalese finches accommodate that hypothesis.

The ability to anticipate the presence of a predator by the Bengalese finches and other prey species would obviously increase their chances of surviving. If there would be a definite relationship between the precognitive response and the intensity or quality of the emotional event, and since the latter are expected to differ between both parties (potential predator and prey), the ongoing evolutionary arms race (Vermeij, 1987) would be asymmetrical, and perhaps to the advantage of the potential prey.

Several areas of research are more open when subjects are animals, such as selective breeding for enhanced hereditary precognition ability, exploration of physiological mechanisms involved in precognition, relationship with sex and age classes, and with social organization and personality profiles.

Although most psi studies need to be done in the controlled conditions of the laboratory, observation and experiment of animals in natural conditions should not be discarded, as they could provide hints about the natural selection and adaptive value of short-term alarm precognition.

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RESEARCH

The Daniel Experiment: Sitter Group Contributions with Field RNG and MESA Environmental Recordings

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Abstract—In an effort to further explore ostensible macroscopic psychokinesis (macro-PK) effects like those previously reported by Batcheldor (1966), Bourgeois (1994), Owen and Sparrow (1976), and Ullman (2001) in a sitter group setting, the first author designed and conducted a series of fifteen experimental sessions in which sitters claiming exceptional abilities attempted to generate a pseudo-spirit named "Daniel," to whom physical phenomena were attributed. To explore possible physical correlates of macro-PK, two approaches to measurement were utilized. In the first, sample data from a local random number generator (RNG) continuously running as part of the Global Consciousness Project were extracted and examined for statistical deviations from randomness occurring in conjunction with the sitter group sessions. In the second, the Multi-Energy Sensor Array (MESA) was used to collect on-site environmental data during one of the sessions. While displays of ostensible macro-PK were observed during several of the sitter group sessions, the RNG data did not show a significant overall correlation with the sessions. The MESA data indicated notable drops in infrared and visual light, as well as changes in DC magnetic field strength, that are comparable to those observed in field studies of haunts.

Keywords: sitter groups—macroscopic psychokinesis (macro-PK)—random number generator (RNG)—Multi-Energy Sensor Array (MESA)

Introduction

Spontaneous case reports within the parapsychological and psychical research literature suggest that instances of ostensible macroscopic psycho-

kinesis (macro-PK)—that is, overt displays of apparent mind–matter interaction manifesting on the level of human perception, without the need for statistical evaluation—tend to be rather scarce. Most accounts of ostensible macro-PK by psychical researchers were presented within the context of their studies of physical mediums who claimed the ability to move physical objects and produce materializations in group situations.[1] While the psychical researchers found many physical mediums to be fraudulent, they also found a certain few, such as D. D. Home, Eusapia Palladino, and Rudi Schneider, who were apparently able to produce macro-PK effects under (semi-)controlled conditions (for reviews, see Fielding, 1963, Gauld, 1968, Ch. 9 & 10, Gregory, 1985, Roll, 1982, Sect. 6.2).

Accounts of macro-PK are equally rare in modern parapsychological studies. In surveying her extensive case collection of spontaneous psi experiences, L. E. Rhine (1963) found only 178 cases of macro-PK, contrasted against the more than 10,000 cases she had of extrasensory perception. Similarly, cases of recurrent spontaneous psychokinesis (RSPK, or "poltergeist"), which involve displays of macro-PK that tend to recur sporadically around a certain individual over a brief period of time, are few in number. A survey by Roll (1977) of the parapsychological and psychical research literature found only 116 reported RSPK cases in the period between 1612 and 1974.

The apparent rarity of macro-PK has often made it, and the conditions under which it occurs, difficult to observe and document. As a way to possibly overcome this difficulty, some researchers have formed a sitter group, which may be described as a small group of individuals who meet on a regular basis under semi-informal social conditions with the intent of producing brief, spontaneous displays of macro-PK. Sitter groups that were apparently able to produce ostensible macro-PK effects with varying degrees of success have been described previously by Batcheldor (1966), Bourgeois (1994), Brookes-Smith (1973), Brookes-Smith and Hunt (1970), Campbell and Murray (2007), Owen and Sparrow (1976), Storm and Mitchell (2003), and Ullman (2001). In this paper, we report an independent attempt to observe and explore macro-PK effects similar to those described by the previous researchers in a sitter group setting. In addition, we report an initial attempt to explore the possible physical correlates of macro-PK using data collected from a local field random number generator (RNG) and a system of physical instruments known as the Multi-Energy Sensor Array (MESA).

Background

The conceptual framework for a sitter group finds its basis in the traditional séance setting that was commonplace during the height of the Spiritualism movement of the mid to late 19th century. This setting was often characterized

by a group of Spiritualist enthusiasts gathered in a dark or dimly lit space to witness various kinds of apparent physical phenomena, such as table and object movements, percussive sounds (e.g., knocks or raps), cold breezes, and materializations. These phenomena were usually attributed to the claimed abilities of a physical medium present among the group, or to discarnate spirits who were believed to communicate with the group through percussive sounds or through an entranced mental medium.

There are generally two types of sitter group. The first type attempts to closely emulate the traditional séance setting, which may include adopting some of its spiritualistic assumptions (e.g., Bourgeois, 1994, Storm & Mitchell, 2003, Ullman, 2001). Perhaps the most recognized example is the Philip group, formed and led by Iris Owen in 1973. According to Owen and Sparrow (1976), the group, composed of members of the Toronto Society for Psychical Research, had observed knockings, rappings, and table movements that were attributed to "Philip," a fictitious spirit invented by the group as a way to emulate the practice of spirit communication during séances, as well as to reduce the possible inhibiting effects of ownership resistance (discussed below). The second type may seek to develop conditions resembling the séance setting, but does not adopt spiritualistic assumptions, instead recognizing that any apparent displays of macro-PK could be due to the latent PK abilities of the group members. The groups formed by Batcheldor (1966), Brookes-Smith (1973), and Brookes-Smith and Hunt (1970) serve as examples of this second type of sitter group.

Batcheldor (1984) determined that there are two possible factors that may inhibit the production of macro-PK in a sitter group setting: ownership resistance and witness inhibition. Ownership resistance refers to the reluctance of individuals to acknowledge that they may possess the ability to produce macro-PK. Witness inhibition refers to the initial response of surprise, shock, or fear expressed by individuals that tends to accompany the first display of apparent macro-PK. Batcheldor suggests that the two factors may relate to the fear of unknown and uncontrollable phenomena, and that this may lead to emotional resistance to the phenomena by group members.

In order to minimize the possible effects of these two factors, some sitter groups may attempt to circumvent any feelings of fear or personal responsibility among the group members by inventing an imaginary "pseudo-spirit" to whom they could attribute any ostensible macro-PK occurrences (e.g., Bourgeois, 1994, Campbell & Murray, 2007, Owen & Sparrow, 1976, Storm & Mitchell, 2003). Alternatively, Batcheldor (1984) suggested that a social atmosphere characterized by plenty of noise, joking, singing, and casual conversation may provide a mental diversion from inhibiting feelings and thoughts among the group members, in addition to promoting social cohesion. Brookes-

Smith (1973) further suggested that resistance to macro-PK effects may be reduced through a method of designated cheating. This involves anonymously designating a member of the group to initially create spurious effects at random until seemingly genuine effects began occurring. This is meant to serve in providing some degree of relief and reassurance to the group members, in addition to promoting expectation and momentary belief in the occurrence of genuine effects.

On the basis of the previous studies, the sitter group described in the present study was intended to be a group of the first type, modeled primarily after the Philip group (Owen & Sparrow, 1976) and guided by the suggestions of Batcheldor (1984) and Brookes-Smith (1973) in producing conditions that may be PK-facilitative. However, unlike in the previous studies, the present sitter group largely comprised individuals who claimed varying levels of psychic, mediumship, or healing ability.

Macro-PK Effects and Field RNG

Sitter group settings may be akin to the kind of group situations that are seemingly conducive to possible PK-related "field consciousness" effects (e.g., Nelson, Bradish, Dobyns, Dunne, & Jahn, 1996, Nelson, Jahn, Dunne, Dobyns, & Bradish, 1998), in that they may promote a sense of social group "resonance" or "coherence" through unified group activities (e.g., group meditation, focused group concentration) and shared group experiences (e.g., laughter and excitement) that are reported as possibly being conducive to macro-PK effects in the sitter group setting (e.g., Batcheldor, 1984, Bourgeois, 1994). In addition, the results of two RNG-based PK studies (Dunne, 1991, Honorton & Barksdale, 1972) suggest that focused intention by two or more individuals can produce randomness deviations in a target RNG, which at times may be of a slightly higher magnitude than individual intentions (Dunne, 1991). On this basis, we might hypothesize that if a form of collective intention or concentration is developed among group members in order to elicit physical effects, then this collective intention or concentration might affect a local RNG to a subtle degree, even if no overt physical effects are witnessed.

A small amount of evidence suggests that some PK effects are not limited by the spatial distance between the PK subject and the target RNG, with randomness deviations in the RNG output occurring even when subject and RNG are separated by distances of one to nearly 9,000 miles (Dunne & Jahn, 1992). Some field RNG studies of both local (Hagel & Tschapke, 2004) and global events (Nelson, 2001) also seem to suggest this possibility. The findings of a natural study of group "field consciousness" effects (Nelson, 1997) further suggest that such effects may extend to the surrounding physical and atmospheric environment to some degree. It was thought on the basis of these

findings that it might be feasible to explore possible sitter group PK effects even on a distant, but local, RNG.

Currently, only four studies have explored possible field RNG-related effects in relation to macro-PK and spirit-related phenomena. During a field investigation of an allegedly haunted Manhattan high-rise building, Maher and Hansen (1992) had collected random event data using a portable RNG system. They found a weakly suggestive ($p = .086$) difference in variance between data from reported haunt sites and data from control sites. To explore the physical correlates of apparent RSPK disturbances occurring in the home of a family in the Dutch village of Druten (Gerding, Wezelman, & Bierman, 1997), Bierman (1996) installed a continuously running field RNG in the home. Analysis of data taken in conjunction with RSPK events reported by the family indicated a significant deviation from standard randomness in the negative (psi-missing) direction, suggesting that the occurrences were associated with an increase in random noise. As part of a series of field RNG explorations, Nelson, Jahn, Dunne, Dobyns, & Bradish (1998:439, 442) collected RNG data during four channeling sessions in which group members purportedly attempted to channel an intelligent spirit entity named "Samuel." The session data were at chance overall, although at least one session produced an individually significant result ($p = .002$). Radin and Rebman (1996) utilized an RNG as part of a study of possible physical correlates of apparitional experiences. They found a weakly suggestive result ($p = .07$) in the RNG's output in relation to subject sessions. These explorations offer preliminary suggestion that RNG correlates of macro-PK may be worthy of further study, as this may help to indicate whether macro-PK and microscopic psychokinesis (micro-PK) share any similar characteristics, or whether they might simply be two different facets of the same phenomenon. The study of RNG correlates of macro-PK may also be helpful in determining whether RNG studies of macro-PK in relation to apparitional and haunting phenomena might be worthy of effort.

On the basis of these findings, the present study explored the potential of the sitter group sessions correlating with the activity of a distant, yet local field RNG operating as part of the Global Consciousness Project (Bancel & Nelson, 2008, Nelson, 2001).

Macro-PK Effects and Environmental Variables

An issue that remains relatively unexplored is whether some of the phenomena reported in sitter group settings (e.g., object movements, percussive sounds, and localized changes in temperature) are purely subjective, or whether they could have an actual physical correlate. To date, one of the only preliminary efforts to explore this issue was made by Campbell and Murray (2007), who collected DC magnetic field, temperature, and humidity data during a series of

17 sitter group sessions with an imaginary pseudo-spirit. During two of their sessions, a decrease in temperature of about 0.4 degrees was noted, as well as "large peaks" in magnetic field strength (Campbell & Murray, 2007:190). Both changes reportedly occurred in conjunction with questions addressed to the pseudo-spirit, and were unable to be artificially reproduced by the group through breathing or movement. We made a further preliminary effort to explore this issue by utilizing the Multi-Energy Sensory Array (MESA) during one of the sitter group sessions reported here.

MESA is an eight-channel computer system that was designed to gather and record data relating to six physical variables in the local environment (infrared light, ultraviolet light, visible light, seismic vibration, AC magnetic fields, and DC magnetic fields) (Harte, Black, & Hollinshead, 1999). It has been previously used in field applications to explore the possible physical correlates of the phenomena reported at allegedly haunted sites (e.g., Harte, Black, Hollinshead, & Mitchell, 2001).

Hypotheses

Three main hypotheses were explored in this study:

H1. Guided by the methods used and the suggestions offered by previous researchers, it was predicted that the present sitter group would be successful in reproducing at least some of the phenomena reported in previous sitter group research.

H2. It was predicted that the activity during the sitter group sessions would show a positive correlation with the activity of a local field RNG, as indicated by a significantly positive (i.e. upward) deviation from standard randomness in the RNG data collected in conjunction with the sessions.

H3. It was predicted that, for the single session in which the local environment was monitored using MESA, any physical phenomena observed during the session would correlate with one or more of the eight physical variables measured by MESA, as indicated by a notable change recorded from one or more of the variable measures.

Method

Experimenters

M.W., who was the principal experimenter in the study, designed and conducted all of the sitter group sessions, kept a log of each individual session, and communicated information regarding the session dates and activities to

coauthors B.J.W. and T.M.H. to coordinate periods of physical measurement.

B.J.W. gathered and analyzed the field RNG data for each sitter group session. He was not present during any of the sessions.

T.M.H. transported, deployed, and operated MESA during the single sitter group session in which it was used (Session #13), and he conducted the analysis of the resulting data. He was present only for the single session in which MESA was utilized.

W.G.R. served as the supervising parapsychologist for the present study, and acted as a consultant. He was not present during any of the sitter group sessions.

Sitter Group Participants

Participants in the sitter group were recruited through flyers distributed in a New Age bookstore, advertisements in a local newszine, and word of mouth in two local Spiritualist churches. With the exception of two individuals who were recruited through the bookstore, all of the participants were acquainted with each other prior to the study, either through shared classes or church services. In order to encourage regular participation and discourage attempts at cheating (see the Sitter Group Procedure section), participants who attended ten or more of the fifteen total sessions were each paid a modest sum for their participation following the completion of the final session.

In addition to author M.W., there were nine other regular participants in the sitter group (participant initials are pseudonyms):

T.G. is a 65-year-old male who is an ordained Spiritualist minister.

E.P. is a 45-year-old female who is also an ordained Spiritualist minister.

M.C., B.L., and R.H. are a 54-year-old female, a 25-year-old male, and a 38-year-old female, respectively, all of whom are actively studying for the Spiritualist ministry. M.C. is the wife of V.C.

V.C. is a 58-year-old male who is a Reiki Master.

A.F. is a 34-year-old female who is the manager of the New Age bookstore in which the first two sitter group sessions were held.

P.L. and L.L. are both females, 58 and 28 years of age, respectively, who are mother and daughter, and who are both Spiritualist church members.

Each of these nine participants claimed to have one or more exceptional abilities, including ESP (telepathy, precognition), mediumship, empathy, and spiritual or physical healing. Given the venues from which these participants were recruited, the fact that nearly all of them claimed exceptional abilities was the result of coincidence rather than of study design.

Two other participants, both individuals who did not claim exceptional abilities, attended the initial sitter group session but dropped out afterward due to an apparent lack of interest.

Apparatus

Sitter Group Materials. Standard card tables with plastic tops and metal folding legs were used for each of the sessions. Print copies of the Daniel character biography (Appendix 1) were produced for distribution to the participants.

Local Field RNG. The local field RNG from which data were gathered for use in this study is one of approximately 60 nodes in continuous operation as part of the global-spanning network of RNGs established by the Global Consciousness Project (GCP). We provide here only a brief description of this particular RNG node as it is relevant to the study; detailed information regarding the GCP and the RNGs that make up its global network may be found in Bancel and Nelson (2008) and in Nelson (2001).[2]

The field RNG comprises a hardware circuit manufactured by Mindsong, Inc.[3], which uses thermal Johnson noise in resistors as its source of randomness. Identified in the GCP network as Node #2222, the RNG continuously operates on a standard PC located in Ann Arbor, Michigan, approximately 50 miles west of the two sites in Royal Oak, Michigan, and Ferndale, Michigan, where the sitter group sessions were held. The RNG is continuously sampled by the PC at a rate of 200 bits per second and the resulting data are stored on the PC's hard drive. These second-by-second data are compressed by the PC into individual packets, each containing five minutes of data. These packets are later transmitted over the Internet to a central server located in Princeton, New Jersey, for archiving. Node #2222 in the GCP network is classified as a "dial-and-drop" node, meaning that its operating PC only connects to the Internet at a specific time of day to transmit its data packets to the central server. After being received and archived by the central server, the data from the individual nodes in the GCP network are made available for public access via an Internet-based data-extraction interface available on the GCP's website (http://noosphere.global-mind.org/extract.html).

MESA. The MESA system's components and specifications have previously been described in detail by Harte, Black, and Hollinshead (1999). We provide here only a basic description of the system and its use in the study.

The MESA system consists of an electronic data acquisition board containing eight separate channels to which individual physical instrument sensors are attached. Channel 1 monitors a CdS photoresistor sensitive to infrared light (Radio Shack No. 276-1657). Channel 2 monitors a second CdS photoresistor sensitive to visual light (Radio Shack No. 276-1657). Channel 3 monitors a custom-built gamma radiation sensor provided by an anonymous donor. Channel 4 monitors a Tri-Field Meter (Alphalab, Inc., Salt Lake City, UT; http://www.trifield.com) sensitive to 60 Hz AC magnetic fields. Channel 5 monitors a piezo-electric transducer sensitive to seismic vibrations (Radio

Shack No. 273-091). Channels 6, 7, and 8 monitor three static DC magnetic field sensors made by Speake & Company, Ltd. (Murietta, California), each oriented along one of the three standard axes (x = West, y = Up, z = North). A sampling rate of 40 times per second was used for each channel. The individual data streams collected during the monitored sitter group session (Session #13) each span a total period of 66 minutes.

The input from each of the eight sensor channels is fed into an analog-to-digital converter and stored on the hard drive of a portable laptop computer for analysis.

During the monitored session, the MESA system was supplemented by an infrared videocamera made by Clover Electronics (http:///www.cloverelectronics.com) and a pressure zone microphone (Radio Shack No. 33-1090B) connected to a VHS video recorder, which documented the session.

Sitter Group Procedure

A total of fifteen sitter group sessions were conducted from June 5 to December 11, 2005. Sessions 1 and 2 were held in the back room of a New Age bookstore located in Royal Oak, Michigan. Sessions 3 through 15 were held in the basement of a Spiritualist church located in Ferndale, Michigan.

Prior to the first session, author M.W. formed the background for a fictional character named Daniel O'Dugan, who served as the "pseudo-spirit" for the sitter group (Appendix 1). From the outset of the study, group participants were made fully aware of the fictional nature of the character, and each participant acknowledged his or her understanding that the character's fictional biography contained several historical inaccuracies.

During the first two sessions, the group participants were asked to read and familiarize themselves with the Daniel character biography. As a way to help them relate to the character, the participants were asked to point out certain key characteristics of the character's life and personality with which they identified, and to empathize with the difficulties he had faced in life.[4] To help them gain a better understanding of sitter group dynamics, the participants were given an introductory background to the Philip group (Owen & Sparrow, 1976) and the elements that seemed to be conducive to macro-PK in a sitter group setting. Instructions concerning the ways in which witness inhibition and ownership resistance may be reduced were also given.

Throughout each of the subsequent sessions, the participants sat around card tables with their hands lightly placed on top of the table. The initial sessions were conducted under conditions of full room lighting, and the lighting intensity was gradually diminished over the subsequent sessions.

For each session, the participants were allowed maximum discretion in determining the session's direction and format. In general, each session opened

with a short period of quiet meditation or prayer, in which participants focused on the Daniel character and the success of the session. The participants also discussed the progress of the Philip group and how a similar goal of producing macro-PK might be met among the present group. The aim of continually keeping thoughts and communication positive, as well as creating a lighthearted and relaxed atmosphere, was emphasized.

Following this opening period, the participants generally engaged in activities that would encourage group cohesion, as well as possibly facilitate macro-PK events. These activities included meditation, invocations, singing, joking, and calling out encouragement for the Daniel character to reveal himself. To aid in the facilitation, as well as to set the atmosphere, some participants occasionally elected to present Irish cultural items to the group that they felt would have been familiar to the Daniel character, such as Irish whisky, blarney stones, and shamrocks.

As in previous sitter group research, a concern for the present experiment was the potential for cheating during the sessions. As an added means of possibly facilitating macro-PK events early on in the sessions, the group participants made use of the designated cheating method. Prior to each session in which this method was used, individual playing cards were distributed at random to each of the participants, with the participant receiving the "joker" card being the designated cheater. This participant was asked to artificially mimic a macro-PK event at randomly chosen times using any means available to him or her while sitting at a table, without indicating to others that he or she was producing the event.[5] The designated cheater was instructed to stop whenever an event occurred that was not of his or her doing. At the end of the session, the designated cheater was revealed to the other participants, along with the means he or she used to produce an event.

Designated cheating was acceptable within these defined limits, but intentional (i.e. non-designated) cheating was strictly forbidden. To help reduce the potential for intentional cheating, each of the group participants was asked during the initial session to sign an affidavit that defined the limits of designated cheating and stated that intentional cheating was unacceptable. In addition, the affidavit required that the sitters immediately and verbally accept responsibility for any artifactual event they unintentionally produced (e.g., by accidentally kicking a table leg) that could be misinterpreted as genuine by others in the group. In his role as principal experimenter, M.W. continually observed the group members for intentional cheating during the sessions, and participants not acting as the designated cheater were asked to watch for others who might be tempted to intentionally cheat.

To further discourage intentional cheating, participants were given a motivation against doing so, other than their own inward ethics to comply with

the group rules, by being offered a modest monetary sum that was to be paid at the end of the study. Participants were warned that any participant caught engaging in intentional cheating would be immediately removed and banned from the sitter group, and that he or she would forfeit receipt of the offered sum. In addition to this motivation, the offered sum was also intended to help motivate the participants to regularly attend the sessions.

Field RNG Procedure and Analysis

Following each sitter group session, experimenter M.W. provided coauthor B.J.W. with the exact time and duration of the session. On the basis of this information, B.J.W. used the GCP's Internet-based data extraction interface (http://noosphere.global-mind.org/extract.html) to download the data file containing the individual second-by-second samples for each RNG node in the GCP network for the corresponding time and duration. The samples for Node #2222 were then manually extracted from the file for analysis. The individual GCP RNG data files are logged in Universal Coordinate Time (UTC), and to ensure that the correct data were being extracted, proper time conversions from U.S. Eastern Time to UTC were applied to the times supplied by M.W. prior to data extraction and checked for accuracy using a standard Internet-based time zone converter (http://www.timezoneconverter.com). The individual nodes in the GCP RNG network regularly fluctuate in their operation times, with some occasionally going off-line for brief periods due to local power failures, Internet connection and server problems, maintenance, or the required use of the host computer for other tasks. Given this, the possibility existed that data from Node #2222 for each individual sitter group session either might not be immediately available, or might not be reported at all. In the case of the former, three checks were made by B.J.W. for a given session's data over the course of three days following the session by downloading the given session's data file and rechecking the column corresponding to Node #2222 to see if the blank entries were filled. In the case of the latter, there was only one session (Session 14) in which RNG data from Node #2222 were apparently unavailable, and thus data from 14 of the 15 total sitter group sessions are reported here.

Analysis of the RNG data was done following the method employed by Nelson (2001) for GCP data analyses, which is based on conventional statistical approaches (e.g., Aron & Aron, 1997, Snedecor & Cochran, 1980): The raw data were first normalized as z scores using the equation $z = (x - 100)/\sqrt{50}$, where x is the individual RNG sample for a given second, 100 is the theoretical mean for a binomial distribution, and 50 is the trial sum variance. These z scores were then squared to create a mean deviation value with one degree of freedom (df) that is Chi-square distributed. Since Chi-square values are additive, a cumulative summation of all of the scores was taken across time (with df = number of

values summed) to represent the overall measure of mean deviation, and an associated probability value was obtained based on the resulting cumulative Chi-Square and *df* values.

MESA Procedure and Analysis

During Session 13, the MESA system was deployed on a three-foot-by-six-foot table placed along the wall of the sitting room in the church basement. This table was located approximately fifteen feet from the position of the card table at which the participants sat. The system was activated using the controlling laptop computer and allowed to continuously sample each of the eight channels for 66 minutes at the standard sampling rate of 40 times per second.

The resulting data were analyzed using a fast Fourier transformation written into customized Linux software to produce graphical displays of 1-second averaged data from each of the eight channels.

Due to schedule changes to accommodate the participants, as well as limited opportunity for travel and transport of the MESA system, only one session was monitored.

Results

Sitter Group Results

The following is a concise summary of the ostensible phenomena that were noted to occur across the fifteen sitter group sessions. A slightly expanded account of the sessions, extracted from the log of the session proceedings, can be found in Appendix 2.

Sessions 1–5. Following a few initial sessions with no apparent phenomena, rapping sounds and intermittent knocking were heard by the participants, which were reported to come from various areas in the ceiling and walls of the sitting room. During alternate sessions, loud crashing sounds, "as if a pile of heavy boxes had collapsed," were heard. However, no boxes or other objects were found to be displaced or fallen during searches made after the session had ended.

During group attempts to communicate with the Daniel character in Sessions 3–5, some of the participants claiming mediumship abilities were observed to enter apparent trance states, bringing forth pseudo-communicators who claimed to be either relatives/acquaintances of the Daniel character, or a secondary Daniel who was out-of-character as described in Appendix 1. Information allegedly provided by the communicators was noted to be inconsistent with the Daniel character biography, and differing views among some of the group participants regarding the nature of this information and its

alleged sources seemed to lead to a social difference among the participants that seemed detrimental to the intended positive social atmosphere of the group. As a result, three participants opted out of the study. An attempt was made to remedy the situation and to restore the intended atmosphere in subsequent sessions.

Sessions 6–10. Rapping sounds from the table were commonly reported, as were localized decreases in temperature and the sudden appearance of cool breezes. Tactile sensations were reported by two participants: One claimed that he had felt an unseen hand grab his thigh, while the other subsequently claimed to have had her shoulder grasped by a firm unseen hand. In later sessions, group attempts at communication with the Daniel character were reportedly met by a wider variety of auditory events, including raps, loud clock ticking, electrical buzzing, and loud, abrupt crashes.

Sessions 11–15. Session 11 was noted to be the most pronounced with regard to the occurrence of ostensible phenomena. Brief instances of table tipping were reported during the session, with one end of the table rising up on one or two legs, and the table apparently "walking" on two occasions. During the second occasion, the table legs reportedly struck the floor with heavy force, and would rise on one leg when shifting direction.

In addition to occasional instances of table tipping and "walking," several other forms of auditory phenomena were reported during Sessions 12–15. Multiple footsteps were heard to move across the floor of the church sanctuary situated above the sitting room, despite the church being locked at the time of the session. Chimes similar to those of a doorbell were heard on a separate occasion, although the church is not equipped with a doorbell. Low, tinny voices similar to those heard on an old radio were reported, although the voices were too low to be intelligible.

RNG Results

Table 1 shows the RNG data results from 14 of the 15 sitter group sessions.

The results indicate that, overall, the RNG data hovered very close to chance expectation, and thus the general prediction of an overall significant positive deviation from standard randomness in the RNG data was not supported. It may be of some note that the RNG data from a few of the individual sessions produced weakly suggestive (e.g., Session 4: $p = .103$; Session 7: $p = .093$) to significant (Session 11: $p = .008$) results, including one that was significant (Session 1: $p = .986$) in the negative direction. These results are similar to those found by Nelson, Jahn, Dunne, Dobyns, & Bradish (1998) in relation to the channeling sessions, where notable individual sessions were also found.

TABLE 1
"Daniel" Sitter Group Session Field RNG Results

Session	Date	Time (ET)	Location	Chi-Square	*df*	*p*-Value
1	5 Jun 2005	12:00 – 14:00	New Age bookstore – Royal Oak	1829.34	1965	0.986
2	26 Jun 2005	13:00 – 15:00	New Age bookstore – Royal Oak	7081.44	7200	0.838
3	10 Jul 2005	13:00 – 15:00	Church basement – Ferndale	7072.54	7200	0.856
4	24 Jul 2005	13:00 – 15:00	Church basement – Ferndale	7351.88	7200	0.103
5	10 Aug 2005	18:00 – 20:00	Church basement – Ferndale	7264.36	7200	0.294
6	21 Aug 2005	15:00 – 17:00	Church basement – Ferndale	7206.30	7200	0.476
7	10 Sep 2005	14:00 – 16:00	Church basement – Ferndale	7359.18	7200	0.093
8	24 Sep 2005	20:00 – 21:30	Church basement – Ferndale	5278.26	5400	0.879
9	7 Oct 2005	20:15 – 21:30	Church basement – Ferndale	4391.54	4500	0.874
10	20 Oct 2005	20:15 – 21:40	Church basement – Ferndale	5088.48	5100	0.542
11	4 Nov 2005	20:00 – 22:00	Church basement – Ferndale	7490.78	7200	0.008
12	14 Nov 2005	20:15 – 21:20	Church basement – Ferndale	3759.48	3807	0.705
13	28 Nov 2005	20:15 – 21:50	Church basement – Ferndale	5670.70	5700	0.605
14	4 Dec 2005	15:00 – 17:00	Church basement – Ferndale	0	0	0
15	11 Dec 2005	20:15 – 21:30	Church basement – Ferndale	4467.16	4500	0.630
Total				81311.44	81372	0.559

Note: No RNG data were available from Node #2222 during Session 14. All times are in U. S. Eastern Time (ET).

MESA Results

Notable changes were observed on five of the eight channels of the MESA system during Session #13:

Channel 1 indicated three separate transient decreases in infrared light in the recording period from 42 to 47 minutes into the session, ranging in magnitude from five to seven footcandles.

Channel 2 indicated five separate transient decreases in visual light, occurring at 16, 36, 40, 49, and 52 minutes into the session. Four of the changes were from 70 to near 50 footcandles, while the fifth was from 70 to 40 footcandles.

Channel 3 registered no notable changes in the ambient background radiation of the room.

Channel 4 registered no notable changes in 60-Hz AC magnetic field strength, with the ambient background fluctuating between 0.75 and 4 milliGauss. The fluctuation was likely due to electrical wiring present in the wall of the room.

Channel 5 registered two subtle vibrations in the table on which the

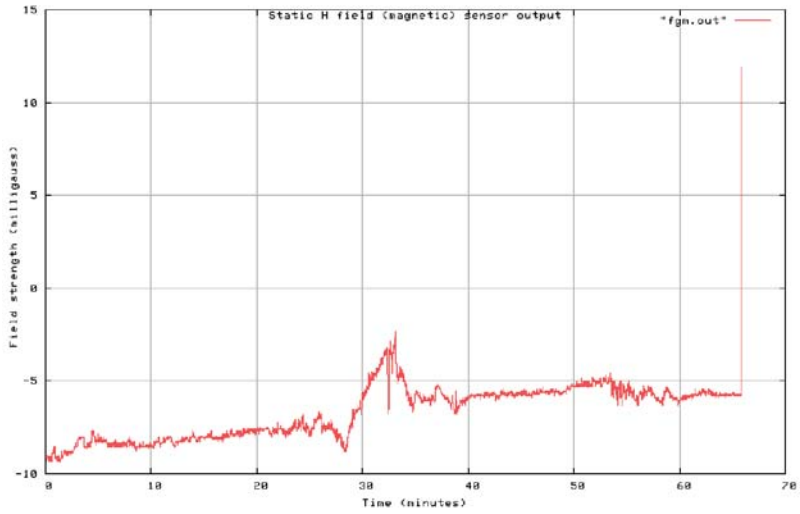

Figure 1. Graphical representation of the piezo-electric voltage recorded by Channel 5 (seismic sensor) of the MESA system during Session 13, with the two attenuated vibration events indicated.

MESA system was deployed (Figure 1). The first vibration was recorded at 24 minutes into the session, and the second at 45 minutes, lasting until the end of the recording period. The card table at which the participants sat was noted to bounce up and down several times as the session progressed, and it is thought that these events may reflect attenuated vibrations in the concrete floor produced by the card table movements.

Channel 6 indicated an increase in DC magnetic field strength along the x-axis (West), from −7 to −2 milliGauss (Figure 2). The onset of the increase was at 28 minutes into the session, and had an approximately seven-minute duration.

Channel 7 indicated a modest increase in DC magnetic field strength along the y-axis (Up), from −7 to −4 milliGauss. This increase occurred within the first five minutes of the session, and remained steady for approximately 45 minutes. At approximately 46 minutes, the magnetic field slightly decreased from −3.5 to −6.5 milliGauss, and then remained steady at about −5 milliGauss until the end of the session.

Channel 8 indicated a steady increase in DC magnetic field strength along the z-axis (North) throughout most of the session (Figure 3), with an increase from −2.5 milliGauss to approximately 5 milliGauss occurring within the first 19 minutes of the session. The field strength peaked at about 8 milliGauss

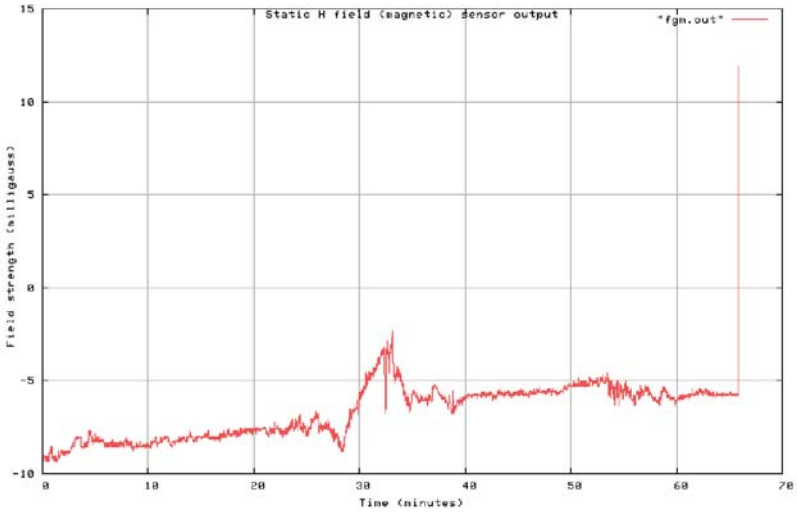

Figure 2. Graphical representation of the DC magnetic field strength recorded along the West axis by Channel 6 (x-axis DC magnetic sensor) of the MESA system during Session 13.

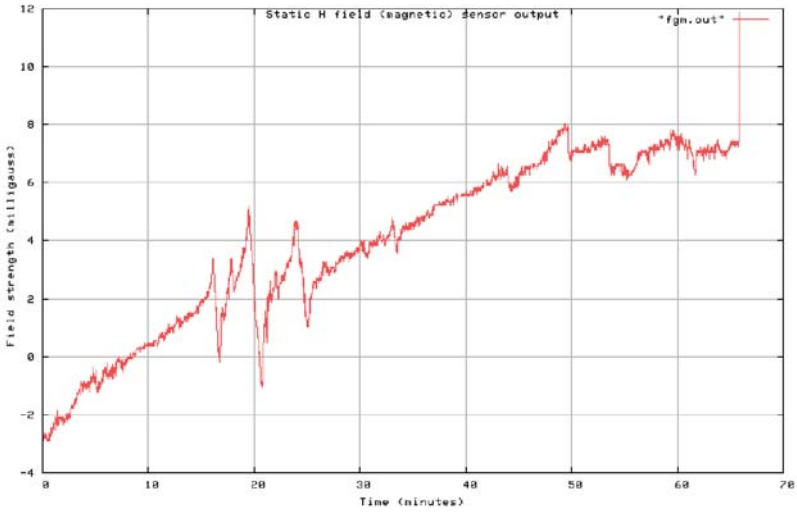

Figure 3. Graphical representation of the DC magnetic field strength recorded along the North axis by Channel 8 (z-axis DC magnetic sensor) of the MESA system during Session 13.

approximately 49 minutes into the session before steadily fluctuating around 7 milliGauss until the end of the session.

In addition, several instances of floating spherical lights ("orbs") that appeared to have no clear source were noted on the infrared video recording of the sitter group session, and several percussive sounds (thumps and cracks) were recorded by the pressure zone microphone both before and during the session.

Discussion

Sitter Group

In general, the sitter group sessions described here displayed both successful and non-successful elements. One successful element related to session attendance. Some previous sitter groups, such as the Philip group (Owen & Sparrow, 1976), had some difficulty in maintaining a regular group of participants over time, and this problem was circumvented in part by offering a financial motivation to the participants in the present group. In addition, it was felt that prior acquaintance among most of the participants was helpful in maintaining the stability of the group, as it may have allowed the participants to be more comfortable around familiar people and thereby more rapidly facilitate a sense of group rapport than if the participants had initially been strangers. The group participants were further noted to have shown respect to one another and applauded whenever another participant had offered insightful ways to improve on the group situation such that macro-PK might be encouraged, another possible sign of group rapport that may have aided group stability. If group rapport is one of things that is crucial to the facilitation of macro-PK in the sitter group setting, then this element may be particularly important for future experimental groups.

While allowing discretion to the participants with regard to the situation, direction, and format of each session seemed to be beneficial in that it may have allowed participants to speak their mind and have a sense of involvement in the experiment (within the confines of the rules), it may have also been something of a non-successful element in that it may have contributed to the social difference that arose between the participants claiming mediumship abilities and those not so claiming, where it seems that differing viewpoints may have given rise to negative opinions of character among these participants, a situation that seemed detrimental to the intended goal of the sessions. While an attempt was made to carefully remedy the situation through positive refocusing, it did result in three participants opting out of the study and seemed to hinder the sense of group rapport that was forming at the time. Future sitter groups should perhaps take

heed of this example and carefully monitor group interactions to help prevent it, particularly if some or all of the participants claim exceptional abilities, as was the case here.

In addition, following the suggestion by Batcheldor (1984), a continual effort was made to keep group thoughts and communication positive in order to encourage a group rapport, as well as the facilitation of macro-PK through momentary belief. Campbell and Murray (2007) found a significant tendency for phenomena to occur when their sitter group participants felt positive, suggesting that this element may also be particularly important.

From a subjective perspective, the success of the present sitter group in reproducing the phenomena reported by other groups was mixed. Several phenomena, including percussive sounds, table tilting, and tactile sensations, were observed, but these seemed to be relatively mild compared to the pronounced phenomena reported by seemingly more successful groups (Batcheldor, 1966, Owen & Sparrow, 1976, Ullman, 2001). For instance, rapping regularly occurred under the hands of the participants of the Philip group. In contrast, the rappings and knockings heard by the present group did not always occur on demand, and never under the participants' hands. Brief levitation of the table was reported by the Philip group and by Batcheldor's group, while the present group observed brief table tipping. The Philip group had comprised participants who claimed no exceptional abilities, while nearly all of the participants of the present group made such a claim. Even with the potential benefit of the participants' claimed exceptional abilities, the inadvertent creation of a second Daniel character by the participants claiming mediumship abilities again led to a social situation that seemed to hinder the group's progress. In this sense, it seems that the difficulties that the Philip group had in keeping regular participants was conceptually matched in the present group by participants who held opposing views on how the Daniel character should be perceived.

In general, phenomena reported by other sitter groups were observed by the present group, although they did not seem to approach the levels initially anticipated by the participants, which included table and object levitation, communication by raps, apportation/deportation, and materializations.

Field RNG

Overall, the attempt to explore possible field RNG correlations with the sitter group sessions was largely unsuccessful. However, a few individual sessions did appear to produce findings that may be of some note, comparable to other field RNG-related explorations of macro-PK and spirit phenomena (e.g., Nelson, Jahn, Dunne, Dobyns, Bradish, 1998:439, 442). Perhaps the most notable of these was the significant positive RNG result observed in

conjunction with the highly productive Session 11. While we recognize that this result was highly significant ($p = .008$), we also approach it with a high degree of caution, as it appears to be the only result of its kind within the session data, and therefore could have likely been an expected chance occurrence.

There may be three considerable alternatives in attempting to account for the overall null result. The first alternative, as stated by the null hypothesis, is that there is simply no effect. However, the participants of the present sitter group did report several ostensible macro-PK events and physical phenomena during the sessions, which argues against this. It is therefore argued that one of the other two alternatives may be more likely.

The second alternative may be the effect of distance between the field RNG and the sitter group sessions. Although there is some data to suggest that PK effects on RNGs are not limited by distance (Dunne & Jahn, 1992), there may be some limit on PK-related "field consciousness" effects, in that the effects may be attenuated to some degree with increased distance from the source. Some events explored by the GCP in relation to local RNGs at a distance seem to suggest this possibility (Nelson, 2001).

In addition, it may be important to consider the distance factor in ostensible macro-PK. In reported cases of RSPK (Roll, 1972, 1975, 1977, 2003), the object displacement events are often localized to a certain area where phenomena repeatedly occur. This localized space is often very small in diameter, and this suggests that RSPK effects, when they occur, may often be limited to such space. This may seem sensible if one considers that RSPK may involve highly concentrated forms of energy as proposed by Roll and Persinger (1998), and that some macro-PK effects may only involve localized negentropic effects in the surrounding environment, as suggested by the tests of Puthoff and Targ (1975). The distance of Node #2222 from the locations of the sitter group sessions, while relatively short (50 miles), may have been considerably large in terms of the scale of possible macro-PK boundaries where the phenomena are focused. In other words, if macro-PK events are highly focused, then they may be limited to only small diameters of space, and the location of Node #2222 was much too far outside the boundary of this diameter to be sensitive to macro-PK effects.

The third alternative is one that follows from the second: that macro-PK and micro-PK, though related in form, are different facets of the same phenomenon. There has been a suggestion that micro-PK effects (which have been the focus of RNG studies), rather than involving a kind of "mental force" acting upon a target object to make it move, may instead involve a kind of "information exchange" that is statistical in nature, and which may act only on the quantum level of subatomic particles, thus allowing for the possibility of nonlocal effects (e.g., Jahn & Dunne, 2001, Jahn, Dobyns, & Dunne, 1991,

Schmidt, 1987). Macro-PK effects, on the other hand, may be more akin to the "mental force" concept, in that they may display characteristics more similar to known physical fields (e.g., attenuation with distance from the central person, object displacement trajectory patterns similar to magnetic vector fields, and correlates with electromagnetic fields; see Roll, 1972, 1977, 2003). While it may be possible that macro-PK effects to some degree involve negentropic effects like micro-PK, it seems that these may operate on a much wider scale than the purely statistical (e.g., the idea that macro-PK effects are negentropic by "acting" on the surrounding environment and somehow modulating surrounding energies in the environment). Because macro-PK effects are more overt, they may be more local rather than nonlocal. These ideas currently remain speculative, but if they have any degree of validity, then they may shed light on the possible difference between macro-PK and micro-PK. Following this argument, although there were local macro-PK effects reported by the present sitter group in the immediate vicinity of the test room, Node #2222, being a noise-based RNG perhaps more sensitive to nonlocal micro-PK effects, may not have been very sensitive to them.

We feel that these latter two alternatives may factor considerably in the null field RNG results observed here, and should be considered further in future studies of ostensible macro-PK using RNGs (particularly the factor related to distance).

MESA

During the session in which it was used, the MESA system registered notable changes in visible and infrared light, as well as changes in DC magnetic field strength. The changes observed in these variables are comparable to those observed in previous field investigations of allegedly haunted sites (Braithwaite, Perez-Aquino, & Townsend, 2004, Harte, Black, Hollinshead, & Mitchell, 2001, Persinger & Koren, 2001, Roll & Persinger, 2001). Changes in DC magnetic field strength were also noted in the sitter group sessions conducted by Campbell and Murray (2007), suggesting that monitoring of DC magnetic fields should be a part of future sitter group studies. The specific interactions within the surrounding environment that may have led to the observed changes currently remain unclear, and any possible insight can only be guided by further research.

Notes

[1] A physical medium should be distinguished from the more familiar *mental* medium, who claims the ability to communicate with deceased individuals through extrasensory perception.

[2] In addition to the publications cited here, detailed information may be found on the

GCP's Internet website: http://noosphere.global-mind.org/.

[3] The Mindsong RNG is no longer manufactured; additional details on the device are available at http://noosphere.global-mind.org/gcpdata.html#normalizing.

[4] As an additional way to help them relate to the character, the participants were provided with a 19th century black-and-white photograph showing the portrait of a man, which the participants were asked to imagine was the Daniel character as he looked in life. This was meant to help the participants view the character as a focus of attribution for any ostensible macro-PK occurrences. Since it was obtained from the Internet and we do not own the copyright for it, we are not reproducing the photograph here.

[5] These means included slightly tipping the table by pressing down on the edge of it, pushing against a table leg to displace the table, and quickly slipping a hand under the table and rapping on the underside.

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Appendix 1

Background of the Sitter Group "Pseudo-Spirit"

Below is the biography for Daniel O'Dugan, the character representing the "pseudo-spirit" for the sitter group described in the present study. We note that the biography is entirely fictional and was derived from M.W.'s imagination. It was modeled after the fictional biography created by the Philip sitter group (Owen & Sparrow, 1976). Some inaccuracies contained within the biography include misinformation about the Irish Potato Famine, which lasted from 1847 to 1852. The Daniel character left his native Ireland because of the famine in 1886, several decades after the famine had ended. The port town of Dubhgail, New Jersey, where Daniel lived, has never existed, and D. D. Home had died two years before Daniel was born.

The Daniel character history included elements that were intended to elicit empathy for the character from the group participants. These elements included the character becoming a geographical orphan for a short time, owning pets, doing charity work for the homeless, and having an ambiguous sexuality.

Life of Daniel O'Dugan

Daniel O'Dugan was born in 1880 in a small farming borough in Cork, Ireland, to Bridget and Patrick O'Dugan. His parents were poor farmers. When he was six they saved their money and sent him to New York to escape the Potato Famine. His aunt was

supposed to meet him at the dock. She was often drunk, had gotten her days mixed up, and was several days late. Being six years old, Daniel did not know what to do when she did not show up. He wandered the streets for several days in the cold October weather, begging for food and shelter. A kindly woman—a gentle person and keen Spiritualist—found him, took him in, and raised him as her own in Dubhgail, a nearby small port town on the New Jersey coast. Daniel found work at age 12 scrubbing the decks of the ships coming in to dock. He listened to the sailors talk about news events, their adventures at sea, and mediums, which were all the rage.

When Daniel was 14, his aunt recognized him by chance at a market and took custody. In his Aunt's home, Daniel's interest in the Spiritualist movement clashed with his Aunt Martha's Christian sensibilities. In a drunken rage she kicked him out after only a week, leaving him to go back to his former household. His foster parents were delighted, and welcomed him home by throwing a party for him.

Throughout his life, Daniel loved to read. As a youth he would read to his mangy dog, Randy. Randy wasn't very smart or amazing, but Daniel insisted that even the most pathetic of animals and humans deserve compassion. Later he owned a dog named Hansen, a dog that, despite its intelligence, would snarl and snap at anyone within biting distance for no discernible reason. He had a pattern of taking in unwanted stray animals and taking care of them.

As an adult, Daniel earned a respectable position working as a boilermaster for incoming ships. Others noted his preference for being friends with men, although he had many women friends. He never married, saying that marriage would prevent him from helping others as much and that he was not the marrying type. He became active in progressive causes such as women's rights, and education for all. He used much of his money to buy gloves and blankets for the homeless. Twice he took in orphaned children off the street, one boy and one girl, and provided a stable home life so they could learn their letters. The boy, Joseph, later became elected to city council for Dubhgail and presented Daniel with a humanitarian award. He was very proud of that.

Daniel also tried to learn what he could about the afterlife by attending séances. Twice he was able to sit with D. D. Home, the famous psychic of the day. Near the time of his death from pneumonia in 1945, Daniel promised that he would give seven years of Irish luck to anyone who could contact him after he died, and that he would do everything he could to let them know he was there. On December 21, 1945, Daniel passed on, surrounded by his many friends.

Appendix 2

Daniel Sitter Session Accounts

Sessions 1–5

The initial sessions did not produce any raps when sitters called out to Daniel. Eventually raps did occur, gradually becoming intermittent knocking. The sitters established a method of communicating: One knock meant no and two meant yes. The knocks appeared to come from the ceiling and walls. When the knock was a yes, it seemed to come from two different areas of the room—on a wall or on two different sides of the ceiling. Every other session or so, there were loud crashing sounds as if a pile of heavy boxes had collapsed. Nothing, however, was found to have been moved or

shifted when the place was inspected after the session was over.

There were apparent indications that some of the participants who were claimed mediums were attempting to preside over other mediums, as inferred by their taking an active part in answering questions aloud when other mediums asked questions of Daniel. In addition, several of the mediums seemed to go into trance during Sessions 3 and 4. They allegedly reached characters who claimed to be relatives, friends, and fellow church members who knew Daniel. The mediums were reminded after these sessions that the intention was not to approximate Daniel by reaching pseudo-characters he may have known, but to reach the devised Daniel.

During Session 5, A.F. brought through Daniel, but the alleged Daniel who addressed the participants did not reflect the personality outlined in the fictional character biography. The alleged Daniel seemed to express contempt toward the participants for trying to get him to respond. Through A.F., the alleged Daniel reminded the participants that he would come through in his own time, and that he (Daniel) attended séance sessions with D. D. Home, and therefore knew what was expected.

W.G.R. suggested that the participants be reminded that the sessions were to be upbeat, positive, and emphasize the caring aspects of Daniel. Future sessions had this added to the procedure before each session.

Sessions 6–10

Following Session 5, the instructions changed so that participants were not to speak for Daniel, but to let Daniel speak for himself. This in effect would hinder attempts by the mediums to answer questions they were not asked when other mediums asked questions of Daniel. Some of the mediums appeared at odds with the suggestion that they could not represent Daniel directly by speaking for him when not in trance. The intent was to get Daniel to produce anomalous effects without having the participants speak for him.

There was an ongoing attempt through singing and discussion to keep the attitude consistently positive. Throughout the sessions, when negative comments or doubts were raised, participants were reminded to keep a positive attitude. This seemed to diminish what momentary momentum the experiment had as it directed attention away from the phenomena.

The group became smaller as some of the participants opted out of the experiment for various reasons. As a result, the group size ranged from five to ten people during this period. Cold spots and breezes became common, along with the sense of being touched. In one case, a male participant claimed that an unseen hand grabbed his thigh. Upon hearing this, a female participant admitted that she had felt a firm hand grasp on her shoulder. Table raps became more common as the group began to sing and distanced themselves from the chiding, allegedly from Daniel, that was received through A.F. in Session 5. Daniel would at times answer questions on demand, seeming to confirm his presence, and started providing a wider range of auditory responses, such as tapping, making clocks tick louder, creating electrical buzzing noises, and sudden, loud crashing.

As the group decreased in size and distance from Session 5 grew, the cautious optimism of the group was replaced by confidence and an expectation that Daniel would show up physically. A level of rapport also seemed to grow among the remaining participants. This was demonstrated by participants repeating other participants' requests

of Daniel, cheering when the other participants showed up to the session, and asking Daniel to "c'mon down [the stairs]" to join the group.

Sessions 11–15

During Session 11, the three participants who had previously attempted to assert their way over the group did not show up (L.L., P.L., and A.F.). There were five regular participants who did show up: M.C., V.C., R.H., B.L., and P.S. E.P., also a regular, had notified us ahead of time that she was not available because of ministerial duties.

It was during this session that the macro-PK became the most pronounced of the entire fifteen session series. The group told jokes, and made feeble and awkward attempts at singing songs that Daniel might have known, which further steered the participants into a more hilarious mood as B.L. warbled *The Star Spangled Banner* in falsetto. The table, which at times had previously raised up on one or two legs, now "walked" on two legs into the lap of V.C. The table had backed V.C. into a pole, at which point it stopped walking and the sitters broke into spontaneous applause.

Later, the table "walked" on the cement floor with the legs making a hammering action, striking the floor with force. At times, as it changed direction, the table would occasionally move onto one leg. The anticipation was exhilarating, with the sitter group all shouting "Up! Up! Up!" in unison, wanting the card table to levitate.

During the jokes and singing, V.C. had ceased laughing at the jokes and rocked back and forth. Some members noticed but paid no mind. Suddenly he started laughing, but differently than we had heard him previously. His wife, M.C., exclaimed, "That's not (V.C.)'s laugh!" The experimenter asked, "Daniel, is that you?" V.C. started laughing harder as B.L., M.C., and R.H. continued their mock singing.

V.C. stopped laughing, recovered, and joined the rest of the group. R.H. then stopped singing and closed her eyes. R.H. began laughing and tried to talk, but couldn't stop as she was laughing too hard. It was apparent that the sitters had reached the "correct" Daniel as had been described to them, who was amused at our joking and singing.

The confidence level after Session 11 had reached its zenith of the sessions. M.W. felt certain that Daniel would show up physically, and perhaps even achieve levitations and apports. As a precaution to protect the progress made, the group was closed to anyone else who had not shown up, with the exception of E.P., a regular trusted medium.

Sessions 12–15 were attempts to use Session 11 as a jumping-off point for even more pronounced phenomena. B.L. was asked to repeat a guided visualization that ended up focusing on the middle of the table where Daniel was hoped to appear. While this did not occur, it had the effect of bringing a focused concentration on the Daniel Experiment.

Aside from the table rocking and walking at times, there were some additional previously unheard noises such as footsteps of many people walking on the floor above (the church was locked), chimes (the church does not have a doorbell), and low, tinny voices as if from an old radio. The voices were too quiet to be understood.

Also for Sessions 12–15, there was a heightened expectation that an apparition or a physical presence other than the sensation of being touched would be experienced. Many of the sitters watched the stairs as the sound of many people walking on the floor was heard from above (which was the church sanctuary), with the sitters fully expecting to hear the footsteps start descending the stairs.

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RESEARCH

Field RNG Data Analysis, Based on Viewing the Japanese Movie *Departures (Okuribito)*

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Abstract—This study examined the possibility that a random number generator (RNG) could detect field consciousness while people watched a film in a movie theater. We conducted 10 measurements at movie theaters showing *Departures (Okuribito)*. One hardware and two software random number generators simultaneously generated random numbers at 512 bits per second. The results showed significant *Z*-scores derived from *chi*-squares during the film viewing. But MANOVA using two variables of *Z*-scores (Stouffer's *Z* and the *Z* from *chi*-squares) showed no significant differences between these conditions because the differences of averages were small in the Stouffer's *Z*-scores. MANOVA using RNG and pseudo random sequences revealed time-related period effects during film viewing. The biases demonstrated by the three random variables were similar to each other. The similarities between hardware- and software-generated random number sequences were significant when Stouffer's *Z*-scores were calculated on the basis of 600-second time intervals by averaging all 10 measurements. Audience size has positive effects on the outputs of all the RNGs. Finally, several hypotheses related to RNG biases and future tasks are discussed.

Keywords: parapsychology—RPG102—pseudo random numbers—emotion—consciousness—mind–matter interaction—PK

Introduction

Random number generator (RNG) devices produce bits (1s or 0s) during real-time processing using avalanche noise or thermal noise as the source of randomness. The products of RNGs have been used primarily for computer security, calculations in a simulation, and so on. In a different context, however, the results of RNGs have departed from expectations of randomness, as reflected in several statistics (Nelson, Radin, Shoup, & Bancel, 2002), during certain meetings (Nelson, Bradish, Dobyns, Dunne, & Jahn, 1996), broadcast events

(Radin, Rebman, & Cross, 1996), and sports events (Bierman, 1996).

In parapsychology, RNGs have been widely used to investigate the interaction between field consciousness and physical systems (Bierman, 1996, Ibison, 1998, Nelson, 2001, Nelson, 2002, Nelson, Radin, Shoup, & Bancel, 2002, Radin, 2002). These studies are referred to as "field REG/RNG" (Nelson, Bradish, Dobyns, Dunne, & Jahn, 1996, Nelson, Jahn, Dunne, Dobyns & Bradish, 1998) or field consciousness studies (Radin, 1997, Radin, 2002). In Japan, several field RNG studies have conducted measurements during the New Year celebration (Yoichi, Kokubo, & Yamamoto, 2002, Yoichi, Kokubo, & Yamamoto, 2004), at the Nebuta Festival (Hirukawa & Ishikawa, 2004), and at a baseball stadium (Ishikawa, 2004). Significant deviations were found when many people shared an emotion, a focus of attention, or a specific state of consciousness.

Movie Theater as a Field

One challenge in conducting field RNG research involves obtaining reproducible results given that different studies have focused on different events occurring at different times. Indeed, it is necessary to separate the effects of the events from the effects of the particular time at which these events occurred. However, the replication of events becomes more difficult when the events are larger, because large events such as the Olympic Games or World Cup soccer matches do not occur every year.

To mitigate the impact of this problem, this study used movie theaters as the experimental venue. To date, only one study, the CinEgg project (Varvoglis, 2006), has involved the generation of random numbers at theaters, an approach to field RNG studies that has been adopted quite recently.

In such contexts, people have many opportunities to experience and share emotions while viewing a movie. Additionally, repeated measures can be designed because multiple theaters can show the same film, which serves as a psychological stimulus, many times. It could be hypothesized that repeated measurements would show that the emotional changes experienced by different audiences would be similar. The approach adopted in the current study differs from that used in previous PK experiments in that no participant was asked to engage in a goal-oriented task. This study also differed from most field RNG studies in that the RNG was used repeatedly to measure the same event. We focused on repetitive events because the analysis could be stronger than those using non-repetitive events.

Furthermore, the maximum audience at each theater was set at 100 individuals. Although large audiences have tended to bias RNG output (Radin, 2006), the effect of smaller audiences has remained unclear. However, several studies investigating whether emotional expression could affect RNG output

have reported a bias in RNG behavior even in the context of small numbers of attendees (Blasband, 2000, Lumsden-Cook, 2005a, 2005b).

Film as an Emotional Stimulus

This study assumed that RNGs could detect the emotions experienced by humans while watching a movie at a theater. Additionally, we expected that the emotions sensed during this process would probably relate to the story depicted in the movie.

Moreover, we hypothesized that a field experiment involving repeated showings of the same movie might find a consistent bias in the RNG data if RNG deviations were somewhat reproducible. Within-movie analysis was employed to examine RNG deviations between sections of the movie during a single viewing because the storyline of the film might produce variations in these data.

We also compared the RNG data generated during the period in which participants were viewing the movie with those generated while the movie was not being shown. It would be noteworthy if the RNG data generated during viewing differed from that generated in the absence of the movie. Varvoglis (2006) examined RNG data by using two conditions, "off-show" and "off-site," as controls. The former involved an empty theater, which would have been ideal but difficult to execute. For this reason, "off-site" was the only control condition in this study.

The current study also considered the effect of audience size. The audience size for periods of non-viewing was assumed to be zero because the RNG was located outside of the theater. Thus, we regarded audience as a variable that was present only during those portions of the experiment in which participants were viewing the movie.

Pseudo Random Numbers Generated by Software

The current study used pseudo random numbers produced by software as well as those produced by the physical RNG device. Most field RNG studies have not used pseudo random numbers generated by software in their analyses.

However, it is still possible that the pseudo random numbers generated by software would be able to detect field consciousness. The results of several PK experiments (Schmidt, 1981, 1993, Schmidt, Morris, & Rudolph, 1986) showed that PK manifested in prerecorded random numbers in terms of a quasi random algorithm, even when seed numbers had been observed before the PK session. Thorough pre-inspection by an observer would otherwise inhibit the PK effect (Schmidt, 1985). At least a few studies have found positive (micro-) PK influences in real-time using algorithm-based pseudo random number

sequences (e.g., Lowry, 1981, Radin, 1982). In contrast, experiments by the Princeton Engineering Anomalies Research (PEAR) group using pseudo random sources have generally not found evidence for a PK influence on the output of such sources (Nelson, Jahn, Dobyns, & Dunne, 2000). Thus, it is necessary to examine this issue further.

If pseudo random numbers could reflect field consciousness, we would expect that the output produced by software would correlate with that produced by the physical RNG using hardware. We would predict that both instruments would show some biased behavior during only the sections involving viewing of the movie.

Experimental Controls

One advantage associated with using theaters as the field is that the experimenter can control certain variables that could otherwise contribute to measurement errors, including location, the repeated measures themselves, confederates, and so on. This study regarded these factors as error variables.

However, neither the conversations nor the expressions of audience members were measured; the only measurements obtained by confederates were those reflected in the random numbers during the viewing of the film. It might be expected that a film that strongly appealed to the mind states of many people would cause some significant deviations in RNG data if the RNG could detect human emotions (sadness, happiness, etc.) or consciousness (shared attention, mental coherence, etc.). If the RNG measurements were reproducible, data adjusted according to the time at which the film started might show consistent results in terms of *Z*-scores or *chi*-squared values.

Methods

Movie

The experiments were conducted primarily on weekends during the opening month of the movie *Departures* (*Okuribito* in Japanese). The movie, directed by Yojiro Takita, won the award for the best foreign language film at the 81st Academy Awards. The story was as follows: Daigo Kobayashi, who plays the cello in an orchestra, has just been fired. He decides to move back to his hometown with his wife, where he gets a job as an undertaker and sees himself as a sort of gatekeeper between life and death. At times humorous and at times emotional, the film paradoxically approaches the pleasures and burdens of life through death. In general, this plot attracted audiences that included many elderly people.

Dates, Theaters, and Confederates

We conducted a total of 10 RNG measurements, as shown in Table 1. The study included four movie theaters and seven confederates (the first author and six assistants, who were acquaintances of the first author). We planned a total of 10 measurements to accommodate the assistants' schedules.

During the final week, we performed measurements on both Wednesday and Friday, which represented a change in the schedule, because the theater ended the run of the film earlier than we had expected. Only one measurement per day was conducted because the pre-view and post-view control conditions were time-consuming.

Table 1
Location, Confederates, and Recorded Times in the Measurements

		FIRST			SECOND							
Theater	City	Date	Ps	N**	Date	Ps	N**	Pre-View Start	View Start	Movie Start	Post-View Start	Post-View End
Warner Mycal	Musashi-Murayama	3/21	C1*	50	3/22	C2*	60	10:48:34	13:18:34	13:43:34	15:48:34	18:18:33
Movix	Rifu Miyagi	3/28	C3	50	3/29	C1*	70	8:01:12	10:31:12	10:46:12	13:01:12	15:31:11
Cine Libre	Ikebukuro	4/4	C4	19	4/5	C2*	20	10:32:33	13:02:33	13:17:33	15:32:33	18:02:32
Cine Libre	Ikebukuro	4/11	C5	30	4/12	C1*	28	8:28:14	10:58:14	11:13:14	13:28:14	15:58:13
Cinema Sunshine	Ikebukuro	4/15	C6	65	4/17	C7	36	13:16:41	15:46:14	16:01:14	18:16:41	20:46:40

* C1, Confederate 1, the first author. C2, Confederate 2, the second author. **, the number of people in the audience.

Materials

We used a Sony VAIO Type-G (Intel Core Solo CPU U1500, 1.33 GHz) notebook PC for all measurements and attached a large battery to enable 8 hours of continuous operation.

The RPG102 hardware, produced by FDK Corporation, was used as the physical random number generator (Figure 1). It was attached to the notebook PC via a USB port, using high-speed random number generator IC RPG100 [http://www.fdk.co.jp/cyber-j/pi_ic_rpg100.htm (Japanese)].

Figure 1. FDK RPG102.

This device generates high-quality random numbers that meet the standards contained in the

federal information processing publication (FIPS PUB 140-2). We developed a GUI application with Visual C# (.Net Framework 3.5) in Microsoft Visual Studio 2008 to manage the RPG102 (Figure 2). Two types of random numbers were generated by the Visual Studio software, which can provide random numbers in two ways: "RngCrypto Service Provider" (RngCrypto) and "Random." No seed controls were provided by the application for the initiation of the random numbers.[1] Because these pseudo random number classes were not initialized by seeds, they did not represent deterministic sequences.

The application generated three kinds of random numbers, from the RPG102 (hardware), RngCrypto (software), and Random (software), at 512 bits per second, recording them into a csv text file at 2-minute intervals.

Although the Intel Centrino 2 CPU produced by "vPro technology" has a

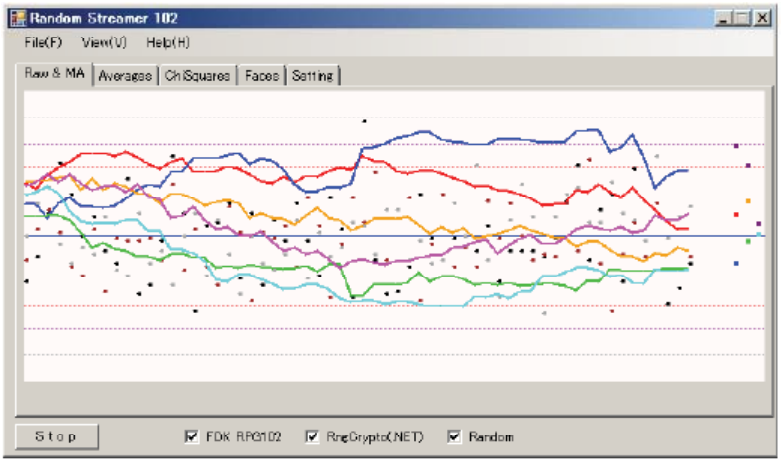

Figure 2. Appearance of the GUI application "Random Streamer 102."

physical random number generator (Levy, Kumar, & Goel, 2008), the processor in our notebook, Intel Centrino, had no mechanism for generating physical random numbers. It is difficult to generate physical random numbers with the software currently available, but future technology will make it possible to use RNG bits without attaching extra hardware.

Procedure

The application was started about 3 hours before arrival at the movie theater. The notebook was kept by one of the confederates in a bag during viewing and returned to us after the movie concluded. Before the movie started, this confederate counted the number of people in the theater (Table 1). The

numbers are approximate because it was difficult to accurately count the people entering after the theater became dark.

It was necessary to adjust the starting times of the movie between repetitions for purposes of analysis. The public time schedules of the theaters were inappropriate because the length of the advertising preceding the movie differed among theaters. The designated confederate recorded the time at which the main scene, involving a white blizzard, appeared on the screen.

The total length of *Departures* is 129 minutes. The confederates entered the theater at least 15 minutes before the start of the movie. We added 15 minutes to the beginning and 6 minutes to the end of the viewing time to make a total of 150 minutes in the "view" section. For purposes of analysis, we divided the total 450 minutes into three 150-minute sections. Moreover, the view section was then divided into 30-minute subsections (referred to as "periods" hereafter). This period factor is to test differences of average *Z*-scores from random sequences among subsections. If the sequences deviated coincident with the film story, *Z*-scores in each subsection could be different from those in the other ones.

Both pre-view and post-view sections included the time involved in traveling to and from the theater by train or car, during which time we performed no experimental procedures, nor did we control any experimental variables.

Missing Data

One measurement consisted of 450 minutes, comprising three sections (pre-view, view, and post-view). Although the total of 10 measurements would be expected to last 270,000 seconds (4,500 minutes), 5,121 seconds (about 2% of the total time) were missing from the data (264,879 seconds; Table 2).

Because the notebook was started two hours before the movie on March 21, the data on previews lasted less than 20 minutes. On March 22, some RNG bits were dropped. On April 5, an unexpected computer shutdown, of which we were unaware for an hour, occurred just before the movie ended, possibly as a result of the high temperature of the notebook, which was sealed tightly in a bag at the time. We restarted the PC and the application immediately after we became aware of the shutdown, and processing improved after we changed the notebook battery operating setting to heat-release priority mode. Most of the missing data were attributable to this shutdown.

Variables

Sampling from the RNG, a *Z*-score based on chance was calculated as follows:

TABLE 2
Number of Samples (Seconds) in Each Time Section

	Pre-View					View					Post-View					
2009	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	
3/21	728	1798	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	25926
3/22	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	26973
3/28	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	27000
3/29	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	27000
4/4	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	27000
4/5	1800	1800	1800	1800	1800	1800	1800	1800	1800	1677	0	0	1516	1800	1800	22993
4/11	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	27000
4/12	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1795	26995
4/15	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1792	1800	26992
4/17	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	1800	27000
	16928	17998	18000	18000	18000	18000	18000	18000	18000	17877	16200	16200	17689	17992	17995	264879

All sections didn't fill 1800 seconds (30 minutes) because of the "drop out" in obtaining bits, or some irregular factor such as unexpected shutdown of the notebook computer. Number "0" means range of the time 150 minutes before the movie starts to the time before 120 minutes ("1" means range of the time 120 minutes before the movie starts to the time before 90 minutes).

$$Z_{raw} = (x - np) / \sqrt{np(1-p)},$$

where p was 0.5, the probability of obtaining 1 second; and n was 512, the total number of bits per second generated by the RNG; and x was the sum of 512 bits in a second. Because x is approximately normal distributed in a binomial distribution (the expected mean of np was 256 bits in this study), standardized Z-scores are available from x.

Dependent variables were the sums of the Z-scores for 60 seconds:

$$\text{Stouffer's } Z = \frac{1}{\sqrt{60}} \sum_{i=1}^{60} Z_{raw \cdot i}^2,$$

which were based on a normal distribution.

Chi-squared values (Z_{raw}^2) could not be directly used for the ANOVA because they were distributed in terms of a *chi*-square of one degree of freedom (mean = 1, variance = 2). For purposes of normalization, Z-scores derived from *chi*-squared values (Z-*chi*) were calculated as follows:

$$Z_{chi} = \frac{1}{\sqrt{2*60}} \sum_{i=1}^{60} (Z_{raw \cdot i}^2 - 1).$$

This can be regarded as an approximately normal distribution, even though it reflected the *chi*-squared distribution ($df = 60$).

Several *Z*-scores were less than 60 seconds, due to the missing data mentioned above. In these cases, we calculated *Z*-scores according to the number of Z_{raw} instead of 60. After data processing, total samples equaled 4500 minutes (= 27,000/60 × 10 measurements), which decreased to 4,417 minutes due to the aforementioned "dropouts". Two other software variables were calculated in the same way.

MANOVA Model

For the purpose of the experiment, we tested film view effects, and period effects within view sections. This study regarded the following factors as residual variables: four movie theaters, two repetitions under the same conditions, and the confederates.

MANOVA models were designed for secondary or exploratory analyses. These models showed similarities in changes over time (Figure 3). According to the analysis using sample average scores from the 10-minute intervals, the pseudo random sequences generated by the software have similar variances to those for the random sequences generated by the hardware.

With the caveat that the analyses are post hoc explorations, these results suggest all the bit sequences might have been affected equally by a latent background factor: field consciousness evoked by the story depicted in the film. These similarities of sample averages suggest, tentatively, that pseudo random sequences could detect field consciousness. If so, the pseudo random sequences were able to respond to the emotional changes, attention, or common mental state of audience members. Thus, the current results appear to be consistent with those of the PK experiment using pseudo random sequences (Lowry, 1981, Radin, 1982) or prerecorded random number sequences (Schmidt, 1981). The similarities of sample averages revealed by our data seem to offer a preliminary indication that this phenomenon extends to field RNG effects as well.

Results

Stouffer's *Z*- and the *Z-chi* scores were calculated based on each condition (pre-view, view, and post-view) and periods within view section, respectively (Table 3). The *Z-chi* scores of RPG102 (physical random number sequences) showed noticeable values under the viewing condition ($Z = 2.01$, $p = 0.04$, two-tailed) compared with theoretical expectation.

View versus Non-View Conditions

To test the view factor, pre-view and post-view sections were integrated

TABLE 3
3 *Z*-Scores of Whole and Each Condition

		Pre-View	View					Post-View	Whole
Stouffer's *Z*	FDK RPG102	1.25			−0.65			0.79	0.80
			−1.47	2.10	−0.17	−0.72	−1.19		
	RngCrypto	−0.42			−0.83			−0.90	−1.24
			−0.11	1.57	−0.90	−1.75	−0.68		
	Random	1.49			1.65			−0.63	1.46
			1.71	2.05	−0.86	−0.23	1.00		
Z-chi	FDK RPG102	0.89			2.01			−0.02	1.67
			1.71	1.51	−0.81	1.45	0.64		
	RngCrypto	−0.53			0.67			−0.78	−0.36
			0.41	−0.82	0.42	1.00	0.48		
	Random	0.47			−1.89			−0.74	−0.26
			0.95	0.04	−0.67	−0.56	−0.20		
	N (minute)	1483			1498			1436	4417
			300	300	300	300	298		

into a non-view section. MANOVA showed no differences between view and non-view sections, in the case of using only Z-scores of RPG102 ($F(2, 4415) = 1.39$, $p = 0.248$), nor sum of all the RNGs ($F(2, 4415) = 0.65$, $p = 0.520$).

Period Effects

MANOVA with RPG102 showed no effect for period ($F(8, 2986) = 1.54$, $p = 0.138$). However, MANOVA using the sum of the three revealed significance ($F(8, 2986) = 2.23$, $p = 0.023 < 0.025 = 0.05/2$; p-value was corrected for 2 times multiple tests).

Correlation between Sample Averages

Figure 3 demonstrates the changes over time of the three kinds of random number sequences. It shows some peaks and troughs, and the variations in the two software sequences seem similar to those of the RPG102, especially in the Stouffer's *Z*-scores.

A post hoc analysis was conducted, to evaluate correlation statistics between sample averages of *Z*-scores of these random variables (RPG102, RngCrypto, and Random). We changed the time unit in exploratory analysis from 30 minutes to 5 minutes and 10 minutes, and calculated *Z*-scores and *chi*-squared scores, respectively (Tables 4 and 5). We focused on view conditions which

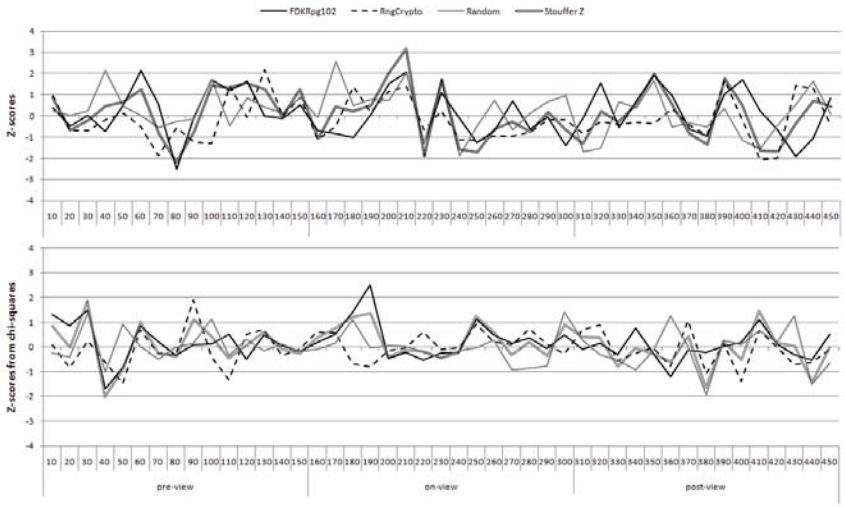

Figure 3. Variation in all *Z*-scores in elapsed time.

are the highlighted matrices in the tables. Table 4 shows that the correlations are positive during the view conditions. Table 6 examined 10-minute averages during the view condition in Table 4. Total *Z*-scores of correlation from Stouffer's *Z* showed significance ($Z = 3.051$, $p = 0.002 < 0.008 = 0.05/6$; 6 = 3

TABLE 4
Correlation Matrices of Stouffer's *Z*-Scores (5, 10, and 30 Minutes)

FDK RPG102	1.000			1.000			1.000		
RngCrypto	−0.020	1.000		0.139	1.000		0.208	1.000	
Random	0.013	0.241	1.000	0.072	0.373	1.000	0.168	0.705	1.000
	Pre-view (N=30)			**Pre-view (N=15)**			**Pre-view (N=5)**		
FDK RPG102	1.000			1.000			1.000		
RngCrypto	−0.166	1.000		0.028	1.000		0.388	1.000	
Random	0.060	0.086	1.000	0.131	0.026	1.000	0.840	0.595	1.000
	View (N=30)			**View (N=15)**			**View (N=5)**		
FDK RPG102	1.000			1.000			1.000		
RngCrypto	0.348	1.000		0.533	1.000		0.729	1.000	
Random	0.182	0.252	1.000	0.322	0.535	1.000	0.277	0.795	1.000
	Post-view (N=30)			**Post-view (N=15)**			**Post-View (N=5)**		
FDK RPG102	1.000			1.000			1.000		
RngCrypto	−0.138	1.000		−0.063	1.000		−0.509	1.000	
Random	−0.126	0.345	1.000	−0.146	0.509	1.000	−0.154	0.853	1.000

TABLE 5
Correlation Matrices of Stouffer's *Z*-Chi Scores in 5, 10, and 30 Minutes

FDK RPG102	1.000			1.000			1.000		
RngCrypto	0.074	1.000		0.134	1.000		0.132	1.000	
Random	0.160	0.095	1.000	0.166	0.020	1.000	0.489	−0.308	1.000
	Pre-view (N=30)			**Pre-view (N=15)**			**Pre-view (N=5)**		
FDK RPG102	1.000			1.000			1.000		
RngCrypto	0.162	1.000		0.259	1.000		0.119	1.000	
Random	0.124	0.079	1.000	0.259	0.068	1.000	0.490	−0.603	1.000
	View (N=30)			**View (N=15)**			**View (N=5)**		
FDK RPG102	1.000			1.000			1.000		
RngCrypto	−0.291	1.000		−0.381	1.000		−0.163	1.000	
Random	0.158	0.116	1.000	0.321	−0.421	1.000	0.630	−0.257	1.000
	Post-view (N=30)			**Post-view (N=15)**			**Post-View (N=5)**		
FDK RPG102	1.000			1.000			1.000		
RngCrypto	0.271	1.000		0.362	1.000		−0.055	1.000	
Random	0.189	0.094	1.000	−0.092	0.202	1.000	−0.587	−0.306	1.000

window sizes * 2 statistics, two-tailed). We note that this is a result from a post hoc, rather than a pre-planned, formal analysis.

Audience Size

Finally, effects of audience size were examined. Table 7 showed correlation statistics between the number in the theater audience each day ($N = 10$) and the *Z*-scores during the view section. All correlations statistics were translated into Fisher's *Z*-scores.

No relations were found using only physical RNG (Fisher's Z from Stouffer's $Z = 0.588$, Fisher's Z from Z-chi $=1.729$). Total *Z*-scores from all RNGs showed a non-significant but noticeable level using Stouffer's Z ($Z = 2.370$, $p = 0.018 > 0.0125 = 0.05/4$; 2 kinds of statistics * 2 calculations were tested). Total *Z*-scores from *Z-chi* scores showed significance ($Z = 2.669$, $p = 0.008 < 0.0125$, as in the same).

Discussion

This study involved a field RNG experiment conducted at movie theaters showing the film *Departures* (*Okuribito*). The analysis was designed to include two time-related factors, condition and period. The results showed noticeable *Z-chi* scores during the view condition in which the hardware was generating

TABLE 6
Test for 3 Correlations in 10-Minute Sample Averages Only during View

		r	Fisher's *Z*	*Z* (total)	*p*
Stouffer's *Z*	RPG102 – RngCrypto	0.533	2.059		
	RngCrypto – Random	0.535	2.068	3.051	0.002 *
	Random – RPG102	0.322	1.157		
Z-Chi	RPG102 – RngCrypto	−0.381	−1.389		
	RngCrypto – Random	−0.421	−1.555	−1.035	0.301
	Random – RPG102	0.321	1.153		
			(N=15)		

* Corrected significance levels for multiple tests were 0.008 (=0.05/6; 6=3 window sizes * 2 statistics)

TABLE 7
Correlation between Audience Size and *Z*-Scores

		r	Fisher's *Z*	*Z* (total)	*p*
Stouffer's *Z*	RPG102	0.219	0.588		
	RngCrypto	0.315	0.864	2.370	0.018
	Random	0.763	2.653		
Z-Chi	RPG102	0.574	1.729		
	RngCrypto	0.500	1.453	2.669	0.008 *
	Random	0.497	1.441		
			(N=10)		

* Corrected significance levels for multiple tests were 0.0125 (=0.05/4 statisticcs).

sequences, but MANOVAs found no significant effect for the view factor.

We also examined effects of period, which was obtained by segmenting the view condition into five subsections. It was significant only when the sum of all three random sequences was used in MANOVA. If the data were produced by human minds, audience emotions such as sadness or empathy might constitute influential factors. Large variations in Stouffer's *Z*-scores were observed for the period of time from 200 minutes to 230 minutes (Figure 3). This segment of the film included some scenes evoking much laughter in the audience (Table 8). Subsequent periods showed a little variance, but these results could not be interpreted with certainty. The last scene and the penultimate episode produced no variance.

TABLE 8
The Relation between Movie Times and Episode

Elapsed Time (min), Whole View *		Episode
160	0–10	(Participants enter the theater.) The movie starts with a scene of a white blizzard. Daigo engages in his first unsupervised operations (called *nokan*) at a funeral.
170	–20	A body turns out to be of the opposite sex from the one he had expected. The scene returns to the past, when he lost his job as a cellist in an orchestra, and decides to move back to his hometown with his wife Mika, to look for work.
180	–30	He goes to a job interview, without knowing that the position is for an undertaker. He is instantly hired by the undertaker's agent, but tries to keep his job secret from his wife.
190	–40	He plays the role of a dead person in a *nokan* video showing a group of undertakers at work (applying make-up, cleaning, and dressing). On a summer day, he has to move a decomposed corpse.
200	–50	He goes to a public bath (*Sento*) to wash himself. He coincidentally encounters his childhood friend Yamashita at the bath.
210	–60	President Sasaki says: "It is your natural calling," but Daigo is ambivalent about his job. The patriarch at a funeral becomes angry because the president and Daigo were laughing.
220	–70	President Sasaki's sophisticated efforts satisfy the bereaved family. Daigo converses with people at the public bath (*Sento*).
230	–80	In general, the undertaker's job is not as widely respected as it should be. He has to deal with his wife's and his friend's prejudices and disapproval of such a "shameless" profession. She leaves him.
240	–90	After lunch with the president, he decides to remain in the undertaker job. The opening blizzard scene is shown again. He engages in his first *nokan*.
250	–100	On Christmas night, he plays the cello at the office. Mika returns to their home because she is pregnant.
260	–110	He suddenly hears of the death of the wife of the manager of the public bath. Mika and Yamashita view Daigo's *nokan* with their own eyes for the first time.
270	–120	He hears of the death of his father, who had abandoned the family when Daigo was six years old. There had been no contact between them for 30 years.
280	–130	He decides to prepare his father with his own hands. He finds a stone letter in his father's hand.
290	–140	Ending credits; a sequence depicting the *nokan* performed by Daigo (Masahiro Motoki).

* Elapsed time from the starting data recorded.

It was also found that the audience size affected the outputs of RNGs, although this is not associated directly with the time-related factors, or the story in the film. Not only the physical RNG, but also pseudo random sequences (RngCrypto and Random), were affected to a noticeable degree in the positive direction (Table 4).

Similar Variances among Three Random Sequences

In the results from post hoc analyses, three Stouffer's *Z*-scores reflected similarities in changes over time (Figure 3). According to the analysis using sample average scores from the 10-minute intervals, the pseudo random sequences generated by the software have similar variances to those for the random sequences generated by the hardware.

With the caveat that the analyses are post hoc explorations, these results suggest all the bit sequences might have been affected equally by a latent background factor: field consciousness evoked by the story depicted in the film. These similarities of sample averages suggest, tentatively, that pseudo random sequences could detect field consciousness. If so, the pseudo random sequences were able to respond to the emotional changes, attention, or common mental state of audience members. Thus, the current results appear to be consistent with those of the PK experiment using pseudo random sequences (Lowry, 1981, Radin, 1982) or prerecorded random number sequences (Schmidt, 1981). The similarities of sample averages revealed by our data seem to offer a preliminary indication that this phenomenon extends to field RNG effects as well.

The overall findings resemble those reported by Radin and Atwater (2009), where those authors considered the effect of group coherence, showing that *Z-chi* scores of many RNGs were positively correlated with each other.

Our results have some differences from those of the CinEgg project (Varvoglis, 2006) which obtained null results. This may be due, at least in part, to our choices for the analysis. Another possible reason would be related to the film contents. *Departures* (*Okuribito*) might contribute to our results, because it has a simpler story and more touching scenes than the movies used in the CinEgg project, which range from humorous parodies to detective type stories such as *The DaVinci Code*. Varvoglis gave another explanation for null results which was that the CinEgg project prevented the experimenter effect by having the RNG installed in the movie theaters. In our study, the RNG brought by a confederate could be affected by the experimenter effect. Because the confederate or experimenter was aware of the size of the audience and also was a member of the audience, the results might be attributable at least partly to experimenter effect.

Emotion and Other Factors

Emotional expression might not constitute the only factor contributing to field RNG effects. Indeed, the findings of the current study are consistent with other explanations. The audience focused on the same screen and had a common mental state because they were viewing the same film. The film itself, *Departures*, might have led to connections among some members of the audience, creating conditions favorable to bias in RNG output.

Movie theaters seem to offer several advantages as locations for field studies of this sort. Certain movies might evoke not only strong emotions but also focused attention and a common mental state among audience members. On the other hand, sports events would evoke strong emotions, but would not elicit a common mental state among audience members due to the divided loyalties of those attending games.

Although the results of the current study support all these hypotheses, our results seem to fit best with the emotional hypothesis because the results were consistent with those of Blasband (2000) and Lumsden-Cook (2005a, 2005b), which showed that participants expressing sadness or exhibiting tears obtained low *Z*-scores.

Future Research

The current study obtained encouraging results in field RNG measurements performed in movie theaters. The significant result of the period factor within the view condition suggested that some elements of the film story might influence *Z*-scores in different ways. Emotion would be the most influential candidate element. Investigation of the emotional changes over the course of the film would have added immeasurably to this study, but we were unable to analyze the relationships between periods and the emotions evoked by the film (*Okuribito*) because transitions between scenes occurred too rapidly to address the distinct emotional features of each scene. All target periods included many different and often opposite types of emotions, rendering a quantitative approach to this phenomenon problematic. However, development of such a method constitutes an important task for future research.

Other future tasks include examining the effects of particular psychological stimuli that have evoked particular emotions (e.g., sadness, anger, happiness, etc.). We hope to arrange for a smaller audience to view DVDs or video clips to examine the deviations in the RNG output as a function of the identified emotions. Repeated measures, as performed in the current study, offer many opportunities to elucidate the subtle effects of field consciousness.

Notes

[1] In our application, the program didn't use any seed initialization when the random classes were instanced. It is perhaps dependent on some time-related variable. The program takes 512 bits per second as an array of 64 bytes. The bits are collected as fast as possible, and then it waits one second.

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RESEARCH

The Healing Connection: EEG Harmonics, Entrainment, and Schumann's Resonances

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Abstract—This study looks at interpersonal coupling or connectivity between healer and subject pairs using advanced signal processing approaches and instantaneous EEG phase coupling. Paired recordings of the healer and subject were done both with the subject at a distance and in the proximity of the healer. The EEG data were analyzed for cross-spectral coupling using the bispectrum, as well as the more traditional Fourier-based spectral analysis and EEG waveform analysis looking at phase. The healer's EEG data showed harmonic frequency coupling across the spectra, followed by between-individual EEG frequency entrainment effects, and then by instantaneous EEG phase locking. These results suggest the presence of a connection between the healer and subject. We observed the healer producing a pattern of harmonics, consistent with Schumann Resonances, with an entrainment of the subject's EEG by the healer's resonance standing waves, and with eventual phase coupling seen between the healer and subject–paired EEGs. We speculate that Schumann's resonance–based standing potential effect might serve as a connectivity mechanism underlying healing.

Keywords: EEG harmonics—entrainment—Schumann Resonances—bispectrum—EEG coupling—phase locking—distant healing

In the physical sciences, it is commonly understood that every material form has frequencies natural to it. When a form's natural frequency is applied with force, the form will also vibrate in harmonic frequencies, often in multiples of the primary frequency. When a bell is struck, harmonic frequencies fill the air, and, if it is hit hard enough, nearby bells will begin to resonate, with this

effect enhanced if they share structurally determined harmonics.

In the social sciences, it is commonly understood that in all societies people experience the waxing and waning of a sense of connectivity with other people, places, and things. The language used to describe this connection often borrows from the physical science of mechanical resonance. People describe being "in tune" with someone or "on the same wavelength," even though no previous EEG research on pairs of individuals has shown "harmonized" brain activity where the EEGs were producing the same spectral content and were "ringing" together (more technically, if they were "in phase", or phase-locked).

Indeed, conventional scientific wisdom would dictate that in the case of people, any sense of connection is purely subjective and confined to the internal biological processes within the person. Over the last several decades, though, there have been increasing attempts to correlate such subjective experiences of connection with more objective physiological and physical measurements external to the person having that experience of connection.

In terms of the subjective sense of connection from human to machines, Jahn and Dunne have widely reported that volunteer operators can significantly alter the output of random number generators in accord with their stated intention, and that the more a person feels in a "resonant bond" with the machine, the stronger the deviation from expected chance is likely to be (Jahn & Dunne, 2005). Further, if two operators work together to try to influence the output of the random number generator, a feeling of resonant bonding between the operators will produce an effect on the RNG that is stronger than the sum of the two individual operators (Dunne, 1991).[1]

Researchers have measured correlations between the brains of spatially separated people with the hope that the subjective sense of connection will be mirrored by measurable alterations in brain activity. Duane and Behrendt (1965) studied extrasensory EEG induction between identical twins, and reported in *Science* that in 2 of 15 pairs, alpha rhythms were elicited in one identical twin as a result of evoking these rhythms in a conventional manner in the other, even as they were spatially separated and had no sensory perception of the other twin. Similarly, Wackerman et al. (2003) reported in *Neuroscience Letters* that six channel EEGs recorded simultaneously from pairs of sensory-separated subjects produce significant correlations between brain activities. Radin (2004) examined thirteen bonded pairs and found that in 3 of 13 pairs, a stimulus given to one of the bonded pair produced significant EEG correlations in the partner. Standish et al. (2004) reported that correlated EEG signals were recorded in 5 of 60 subjects tested in pairs when one member of the pair received visual stimulation and the other did not. They also produced significant results using fMRIs to detect correlations between distant brains (Standish et al., 2003). Overall, Radin (2004) reports that a comprehensive review of the literature

indicates that approximately 15% of pairs of people show non-chance EEG correlations.

In these previous studies, selected brain correlations are reported in some of those studied, even while the authors lament that the mechanism by which people might "connect" and a mechanism for information transmission remain a mystery.[2]

The present study seeks to broaden the scope of previous inquiries in several ways. First, while remaining within the "bonded pair" model, we examine the EEG correlations not between friends, but between healer and subject. Second, we use a 19-channel full-spectrum EEG to look more comprehensively and with more modern analytic tools than has been done in the past. Previous studies have used simple pairwise correlation techniques looking at predefined bands. None of these earlier approaches were designed to look specifically for frequency coupling or at harmonics, as we have done through the use of the bispectrum. The bispectral analysis displays spectral harmonic coupling at a single electrode across the frequency spectrum, without predefining any bands, thus the display shows the correlated modulation of various EEG frequencies. Third, we speculate about a possible mechanism by which connectivity can be established, and thus a mechanism for possible information transfer between brains may be documented.

Methods

Procedure

One of the authors (WB) has developed a healing technique based upon rapid imaging which has been demonstrated to reliably produce full cures of mammary adenocarcinoma in experimental mice (Bengston & Krinsley, 2000, Bengston & Moga, 2007). The healing techniques used in this experiment have been described elsewhere (Bengston, 2007). Basically, they involve a process of extremely fast visualization of disconnected images by the person doing the healing. Images are constructed based upon very specific outcomes of meaningful goals desired by the healer. For example, if one held the desire to have a book published, the image to be cycled might be of a celebratory toast of champagne among friends, which holds the meaning that the book had indeed been finally published. A minimum of 20 such outcome images about unconnected goals are mentally visualized or "cycled" through as quickly as possible, preferably while also experiencing emotion of any kind. With much practice over time, the string of images runs automatically in the mind as background, multi-tasking activity during healing. The accelerated mental visualization activity is going on while the healer gently imagines an "energy flow" moving between the healer and subject. It is interesting to note that these

healing techniques are mechanical in nature, and do not require concentrated thought, or belief by either the healer or subject.

Three individuals known to Bengston volunteered to be subjects in this study, which would apply these healing techniques to human subjects. Two of the three had health concerns, one of whom had previously participated in brief healing sessions.

Two sets of identical full-spectra EEG amplifier instrumentation were acquired for the experiment; one for the healer and the other for the paired subject. These matched amplifiers were used to create the EEG recordings of the healer and subject pairs. We employed two separate contracted EEG technologists who were devoted to setting up and monitoring each EEG in the paired experiment. Two separate rooms were used in the experiment, a healer's room and a subject's room, which was located approximately 35 feet away. The computers in the rooms were connected via a 50 foot ethernet connection to synchronize the computers, though actual electrical contact between the healer and subject was not possible. The electrical contact via the computers or their components (via power supply) was precluded by the electrical isolation of the medical device preamplifiers and power supplies used. The medical devices are designed specifically to prevent electrical connection which could provide a potentially fatal shock hazard in hazardous medical environments (such as a "Class A electrically sensitive patient"). Both healer and subjects were fitted with individual surface scalp electrodes. The technicians used "Elefix paste" (a commercial adhesive and conductive product) and "lemon-prep" (a commercial skin cleaner/abrader) to prepare the participant's skin at the standard 10–20 electrode locations. Twenty-one (21) electrodes (10 mm Ag/AgCl with Touch Proof (DIN 42-802) style connectors) were placed on the scalp in accordance with the 10–20 placement system. One electrode was placed on the available clavicular notch area for an EKG signal measurement. Each set of electrodes was connected to an EEG amplifier with a DC-100 Hz frequency bandwidth (Mitsar-202), which was connected to a computer with a Microsoft Windows operating system.

Each channel of EEG was sampled at 500 samples per second, and the two computers were synchronized to a millisecond accuracy by synchronizing the computer clocks, so the Nyquist principle establishes that the healing session datasets could be time-matched at least to the accuracy of the 500 per sec sample rate. Once electrodes were placed, they were not removed until that person's recordings were finished. To accomplish this, an amplifier was sometimes disconnected from the computer and carried by the participant for session room changes as necessary. A wheeled cart was also used to transport the computer and amplifier when a room change occurred.

Each volunteer subject became an individual target of four sequential

recorded sessions, each session lasting at least 10 min. Each subject first did a double-baseline session, one section with eyes closed, and another with eyes open. With electrodes still in place, the subject next met for a few minutes with the healer until a subjective feeling of connection and readiness was established. The subject was then taken to the experimental room for a distant healing session, where there was not even a line-of-sight connection. Following this, the subject was brought back into the healer's room, for a hands-on healing session. Similarly, the healer did a baseline session of eyes closed and eyes open, followed by the three sets of healing, with both distant and hands-on sessions.

Analytical Techniques

Data from the subject who had previously experienced sessions with the healer were deemed most likely to show "connection", due to the earlier observation that closer relationships had more "connection", and thus one of the volunteer's data was examined first, starting with the session that was done at a greater distance. When this session revealed anomalous data, it became the target of analysis, and this session is the dataset examined in this paper. The other sessions remain to be evaluated at a later date, thus this is best viewed as a case study and demonstration of our technique and approach, and not a controlled study requiring statistical evaluation.

The EEG data on this session were processed with WINEEG software (version 2.82.30) by Luke Hendricks (Experimenter #1), under the blinded direction of Jay Gunkelman, QEEG-Diplomate (Experimenter #3). Experimenter #3 provided direction regarding what form of analysis to use, and initially instructed Experimenter #1 to look for episodes where the healer's EEG's bispectral analysis displayed visually discernible patterns of cross-spectral interaction, which would indicate the presence of harmonics.

The bispectral analysis graphic display allows easy visual identification of cross-spectral coupling. This measurement shows a correlational interaction of two or more frequencies at a single electrode site (Hagihira, Takashina, Mori, Mashimo, & Yoshiya, 2001). Bispectral analysis is ideal for displaying both harmonics and nested rhythms (the modulation of one frequency by another, even in the absence of a mathematical harmonic relationship).

The bispectral display is expected to have a 45-degree "line", which merely represents the same frequency on both the X and Y axes, which are obviously related as an "identity". When other frequencies are coupled or are interacting, they show the interaction based on the presence of non–45-degree line "nodes", such as 10 Hz coupled with 20 Hz and 40 Hz, where those nodal locations would all be simultaneously marked on the display.

The precise time along the time series of the EEG where bispectral coupling

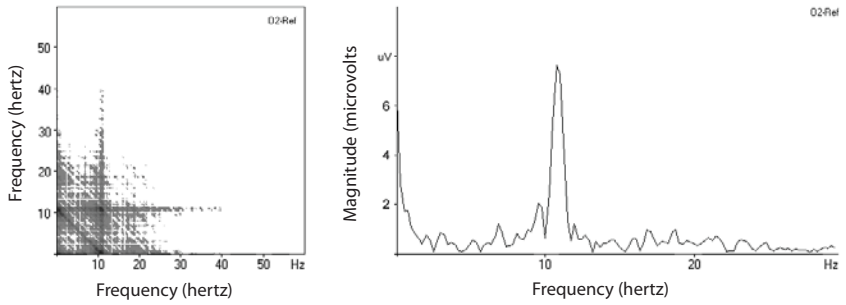

Figure 1. Typical Alpha spindle, bispectra and spectra.

(A) is the bispectral display, with the EEG frequencies from 1 to 60 along both the X and Y axes. The alpha spindle has a 10.74 Hz spectral peak, though there is no real cross-spectral "checkering" of the display which would indicate other frequencies that would be interacting with the alpha spindle.

(B) displays the spectral magnitude of the same EEG data, with the X axis displaying from 1 Hz to 32 Hz, showing the 10.74 Hz spectral peak magnitude, though no other related spectral peaks are noted. Peak values should be interpreted as midpoints of possible values of a given resolution. In this case, 10.74 is the midpoint of possible values within .125 Hz.

was initiated was identified by Experimenter #1. Experimenter #3 then instructed Experimenter #1 to switch the display to the raw EEG wave morphology, and to observe the spectral characteristics of the EEG at those points in time. The onset of the cross-spectral coupling seen in the bispectral display was observed to be time-locked with the onset of an alpha-like spindling. A spindle is an EEG waveform that ramps up suddenly, exists for a period of time (spindle duration), and then wanes. The spindles seen associated with the bispectral harmonics were seen with an oscillatory frequency of 7.81 Hz, which can be seen either "manually" in the raw EEG (by counting the waveforms), or by looking at the peak of the power spectrum display derived from the Fourier. The Fourier represents a method of deconstructing the raw complex EEG waveform into component waveforms from across the frequency spectrum, and displays these results in power-by-frequency spectral plots (see Figures 1 and 2). The frequency of a given spindle is identified by its spectral peak, which should be interpreted as midpoints within a given resolution. The data in this paper have a resolution of 0.25 Hz, so values provided should be interpreted as ± 0.125Hz.

The Fourier analysis of a segment of the EEG spindle showed the 7.81 Hz tuning of the spindle. The Fourier also identified multiple harmonic peaks in the broader EEG spectra. These harmonics had a primary frequency at 7.81, and higher frequency harmonics were seen as mathematical multiples of the primary spectral peak. The peak magnitudes of the harmonics dropped to progressively lower levels, with each successively faster peak containing less "power" (power is the EEG magnitude squared). The power was observed to

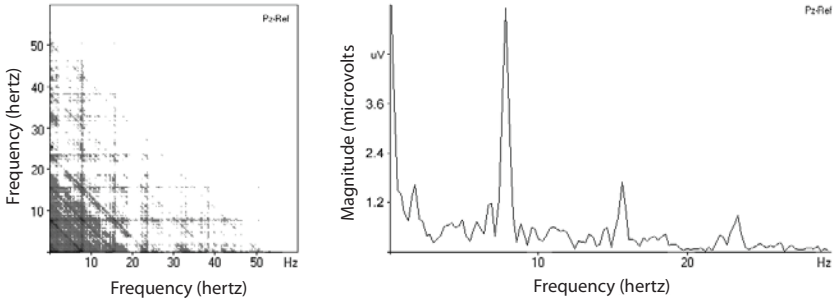

Figure 2. 7.81 Hz spindle with harmonics at 15.63 and 23.44 Hz, bispectra and spectra.

(A) is the bispectral display showing the cross-spectral interaction of the 7.81 Hz EEG activity and the other mathematically related harmonics, seen as a "checkering" of the display.

(B) displays the spectral magnitude peaks, seen with the 7.81 Hz primary peak, and then the progressively smaller magnitude responses as the doubling and tripling harmonic peaks are seen. Peak values should be interpreted as midpoints of possible values of a given resolution. In this case, 7.81 is the midpoint of possible values within .125 Hz.

be more prominent parietally, so this is the topographic area we concentrated the analysis upon.

The mere presence of an EEG spindle does not indicate a standing potential, such as the presence of alpha spindles at 10 Hz. If a standing potential is not achieved, then the multiplicity of harmonics will not be seen in the bispectrum. A standing potential is indicated only when a spindle occurs *with the tuning of the resonant chamber it is oscillating within*, and only then are the harmonics seen in the bispectrum.

It should be noted that in the experience of Experimenter #3, the pattern seen in this healer–subject data, with the bispectral coupling and the Fourier with multiple harmonics, is an uncommon one, not previously seen in well over 30 years of clinical EEG experience.

Examining the raw waveforms revealed that these high-amplitude waves happened in short bursts, with a spindle duration of 1 to 3.5 sec, with many spindles of shorter duration. These spindle bursts were observed to be distributed throughout the healing session's recording. The fourteen largest amplitude spindle bursts were identified by measuring the amplitude of frequencies in the 4 to 14 Hz range (with the aid of spreadsheet calculation).

EEG is evaluated in small time segments called epochs. We used an epoch length of 0.25 sec. A "burst" was defined as four or more consecutive epochs each exceeding 25 microvolts at parietal location Pz. Bursts were 1 sec or longer when selected by these criteria. Eye blink artifacts were eliminated using a standard artifacting formula (all quarter-second epochs with an amplitude at

the Fp1 location exceeding 50 microvolts were rejected). Following artifact rejection, predefined bursts which had fewer than four epochs remaining were rejected, thus all remaining spindle bursts were of at least 1 sec duration.

Fourteen general bursts were seen in the EEG (see Table 1), and of these ten had a spectral peak frequency of 7.81 Hz. Seven of the bursts of 7.81 Hz had harmonic peaks at double their frequency seen in the Fourier. Two other bursts had a peak frequency of 8.3 Hz, with only one showing a secondary harmonic peak. The remaining two bursts had a peak frequency of 8.79 Hz, though neither of these bursts had a harmonic spectral peak. Thus the bispectral coupling and associated harmonics appeared to be predominantly seen during the 7.81 Hz bursts.

Since the 7.81 Hz bursts were seen with spindle durations of 1 or 2 sec, the low-frequency resolution was limited by the duration of the event to 1.0 Hz and 0.5 Hz, respectively. To achieve 0.25 Hz low-frequency resolution in a Fourier spectral analysis, 4 sec of continuous data is required as the epoch length. The use of this duration length analytically would extend the "edges" of the actually observed bursts. The spectral and bispectral images of the Pz electrode site are seen below. The Pz data were compared with the data from the five parietal sites, and the data from Pz were all found to be representative of the region.

When the healer produced these bursts of spindling 7.81 Hz activity, we

TABLE 1
Summary of High-Amplitude Bursts

Start to End in Sec	Duration	Spectral Peaks	Notes
44.25–46.5	9	8.3, 16.6	
83–84.25	5	7.81, 15.63, 23.44	
120–121.5	4	8.79, 16.6	
134.75–136.5	7	7.81, 16.6	
186.75–190.25	14	8.3, 16.11, 24.41	
234–235	4	7.81, 15.63	
275.5–279.25	14	7.81, 15.63, 23.44	Blink in middle
287.75–288.75	4	8.79, 14.65	
349.5–353.25	10	7.81, 15.14	Blink in middle
366.75–367.75	4	7.81, 15.63	
416.25–417.25	4	7.81, 15.63	
473.75–475.75	8	7.81, 16.11, 23.93	
505–506	4	7.81, 15.63	
777–778.25	5	7.81, 15.63	

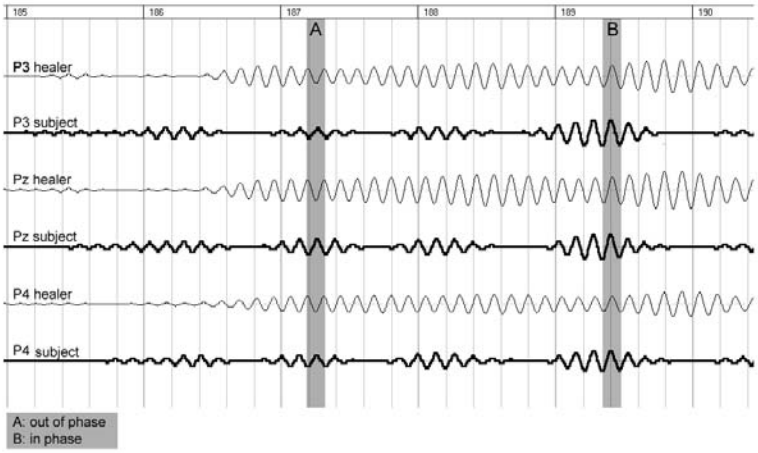

Figure 3. Spliced EEGs: healer and subject, example of entrainment to phase locking.

The figure shows waveforms revealing the 7.5 to 8 Hz frequency range in both healer and subject at three parietal locations. Early in the healer's sustained amplitude burst (A), the phase of the subject does not match the healer's. As the healer's burst continues, the subject's phase synchronizes with the healer's (B) as the subject's amplitude also increases to near its maximum for the entire 11-min session. Note: Given the healer's greater amplitude generally, the subject's entire waveform was amplified for clarity (50 vs. 15 microvolts/cm sensitivity).

looked at the time-locked data from the subject. The subject's EEG appeared to produce a similar spectral burst, though of a much lower magnitude, though with the magnitude amplified by the harmonic resonance once the EEG was in phase (phase-synchronous content is additive). The presence of generally similar spectral bursts allowed us to analyze the phase coupling (synchrony) between the healer and subject's EEG. The healer was seen to produce the primary spindle of activity first, with the subject subsequently producing the smaller bursts of similar frequency content. Though similar in frequency, the subject's EEG activity was initially out of phase with the healer's EEG activity (one waveform going up when the other is going down). The two bursts of similar frequency content achieved a phase locking within another few hundred milliseconds, with phase locking between individuals seen somewhere in the middle of the healer's sustained burst of EEG activity.

The healer's long duration burst shown below illustrates the phase "entrainment" of the subject to the healer's EEG pattern (see Figure 3).

Discussion

Previous studies were limited technically to looking at single predefined frequency bands, and how these bands were correlated in healer–subject or "interpersonal connection" research. In our study we used methods specifically designed to look for evidence of interaction between the healer–subject pair. The observation of bispectral harmonic coupling across the frequency spectrum provides evidence of an electromagnetic standing wave in the healer, with a primary tuning at approximately 7.81 Hz, and at harmonic multiples ascending the frequency spectrum.

The presence of a standing wave and the associated harmonics in the healer was observed to be stronger and to occur earlier in time than similar frequency bursts that were observed in the paired subject's EEG, though this was not initially seen with a phase-locked relationship to the activity in the healer.

A waveform that is produced at the harmonic frequency of a container will produce a "standing wave" when the resonant frequency of the container is matched, which will be seen as a stable waveform that appears not to even move or "travel". If this standing potential is present and another oscillator is available in the environment, it will be entrained into the same rise and fall time (frequency entrainment), and it will "synchronize" with the harmonic standing wave due to the reinforcement of synchronous potentials and the partial cancellation of other non-synchronous content, and this will be seen as a phase locking of the two wave oscillators.

Following the initial occurrence of similar frequencies, the phase of the subject appeared to be reset to be synchronized to the same phase relationship as the ongoing activity seen in the healer, and once tuned to the same frequency and phase, the waveform is augmented (amplified) by the identically tuned standing potential. This entrainment of the harmonics to be in-phase (or phase-coupled) was observed to occur during the healer's bursts of 7.81 Hz EEG activity seen with the bispectral coupling and spectral harmonics.

The observation from these data suggest that the standing potentials and harmonics produced by the healer make a connection between the healer and the subject, and thus provide a mechanism for phase coupling between the pair. The salient observation that merely showing a method for connectivity does not indicate the nature of the content that may be communicated via the connection cannot be overstated. The connection could be modulated (as in Morse Code), or it may be a "carrier" for higher or lower frequency spectral content (such as slow cortical potentials, DC Field potentials, or even Gamma or Tau rhythms).

With these preliminary observations having been presented in a public forum[3], it should be mentioned that others have seen similar harmonics in healer–subject paired data, but the observation had not been previously

reported. Dr. Juan Acosta-Urquidi has collected 30 pairs of healer–subject data, and following the presentation of our data, looked at the spectral content in some of his pairs and saw the spectral peaks at about 7.8 Hz and the higher harmonic peaks, though his analysis displays did not allow for higher resolution of the frequency spectra.

If we are immersed in a chamber that has harmonic characteristics, and we resonate at the harmonic tuning of our chamber, a standing potential is created that can have a distant entrainment effect on other oscillators in the chamber. The frequencies we observed empirically were noted to be suggestively close to the classical "Schumann Resonances". The Schumann resonances represent the harmonic tuning of the "chamber", which is the atmospheric cavity bounded by conductive surfaces (the ground and the ionosphere). These resonances were defined as 7.83 Hz as the primary frequency, with mathematical multiples across the spectrum, and seasonal fluctuations are seen in the data tracked by the U.S. Geological Survey in Antarctica and Parkville, California, as well as in other locations.

We suggest this as a possible method of connectivity in healer–subject pairs where distant healing effects are observed. We further hypothesize that phase coupling across the frequency spectra may be the physical representation of the felt sense of "connectedness" that permeates societal descriptions of bonded pairs. We also encourage others to utilize our methodology of looking with the bispectral display to "see" these cross-spectral interactions, and to use phase analysis to look for actual spectral coupling, and not merely to rely on general correlation of spectral power of predefined peaks.

Notes

[1] Also, in a series of exposures to scalar wave generators tuned to 7.83 Hz, Dr. Juan Acosta-Urquidi found that healers were commonly sensitive to the presence of the signal during a resting period, and experienced it as a positive felt presence (personal communication, 2009).

[2] One further aspect of the phenomenon is seen in coupling between devices and humans. Dr. Juan Acosta-Urquidi shared the data from one of his experiments with us, where he exposed individuals to a scalar generator producing the primary Schumann resonance, and observed the spectral coupling of the humans with the device. This suggests the possibility that an artificial generator can cause the human EEG to spectrally match with that of the generator.

[3] Luke Hendricks presented some of these data at the 2009 Annual Meeting of the Society for Scientific Exploration at the University of Virginia.

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RESEARCH

Laboratory Psi Effects May Be Put to Practical Use: Two Pilot Studies

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Abstract—I describe two studies that were designed to illustrate the potential applicability of laboratory-derived ESP effects in trying to predict events of practical consequence in the "real world." Both studies attempt to predict the behavior of sets of financial indices over a designated week in the future. The studies followed up on earlier work on the prediction of scoring direction and scoring extremity. Participants are asked to make repeated calls at the same set of targets, and then their responses are combined through a majority-vote analysis to generate a set of "best predictions" to be tested against the actual yoked outcomes. The results of the first study were statistically significant and powerful enough in terms of amplification to have practical consequences. The second study was less effective, but changes in experimental conditions and participant population justified a post-hoc analysis based on the assumption of overall positive scoring. In this case, results were more encouraging. The Discussion addresses possible problems of data analysis and ethical concerns about the application of psi data.

Keywords: psi application—psi enhancement—psi and mood—variance effects

Introduction

Two things are commonly said about parapsychology. One is that laboratory experimental research deals with such weak effects that it cannot be very meaningful. The other is that psychic phenomena are so unpredictable that they cannot possibly have any practical utility. Are these things true? They can both be tested at once by examining efforts that have been made to apply laboratory parapsychological effects to the task of making real-life predictions, and by carrying out further research of the same sort.

Three basic ideas with several variations and elaborations have been proposed to assist in the effort to apply laboratory psi effects. One is to collect numerous guesses at targets and combine the guesses. The second is to sample a participant's guessing and use the success rate of the sample to try to predict

the success rate of the remainder of the guesses. These two ideas both represent what might be called a "bootstrap approach," in which some characteristics of psi responses are used to try to heighten the success rate of the same body of psi responses. The third idea is not a bootstrap approach: to try to establish relationships with independent, measurable variables that have the power to predict how well a participant is performing at ESP. All of these ideas seem intuitively appealing at first blush, and I discuss each of them in turn.

The idea of combining judgments of imperfect reliability in order to cancel out some of the unreliability and improve the correctness of the averaged judgments is a commonplace and is used routinely in psychology and other disciplines whenever measurements with imperfect reliability must be used. (See, for example, Schultheiss, Scott, & Schad, 2008, in the area of measuring implicit motives, or Granhag, 1997, with regard to the reliability of forensic testimony.) A single trial in an ESP test certainly represents a situation of imperfect reliability, even with the most talented performers on their best days. Inevitably, the idea has been applied to ESP data. There it has been referred to as the "repeated guessing technique" (Scott, 1960, Thouless, 1960), the "majority vote technique" (Fisk & West, 1957), and "redundancy" (Kennedy, 1979).

As an illustration of the potential power of this approach, consider the following facsimile situation using pseudo-ESP guesses. I begin with the assumption that I have relatively gifted participants who can perform somewhat reliably above chance. Let us assume that they can perform at a rate of correctness of 60% in a binary ESP test in which chance expectation is 50%. This is high performance by laboratory standards, but still not something to use as a basis for important decisions. In this illustration, I first generated a list of 10 targets that were to be repeatedly "called" made up of 1s and 0s. Then I "asked" the pseudo participants for 10 runs at this target list of 10 binary targets, and did this by making up a little deck of cards in which 6 cards were labeled "C" (for correct) and 4 cards were "I" (for incorrect), and shuffled this deck 10 times and laid out the calls. Each time a "C" appeared I substituted the actual target for that trial of 1 or 0, and each time an "I" appeared I wrote down the non-target. Then I added up the number of "guesses" that were given for 1 and the number for 0 for each of the 10 target positions. The results of this small sample are given in Table 1.

We can see there that from a modest 60% rate of success our majority decisions have risen quickly to a perfectly usable 100% rate of success.

Notice that there are extra columns in the table. This is because this exercise also illustrates another point: that such efficiency, if it could be obtained, could be used for actual communication. In constructing the target list I did not simply assemble an arbitrary list of 1s and 0s. Instead I elected to try to "transmit" a verbal target to my pseudo participants by using Morse code. The word "cats"

TABLE 1
Totals and Majorities of Votes for 10 Targets in a Pseudo-ESP Experiment

	PSEUDO TARGET	VOTES FOR 1	VOTES FOR 0	MAJORITY
	0	9	11	0
	1	12	8	1
	0	8	12	0
	1	12	8	1
	0	8	12	0
	1	12	8	1
	0	7	13	0
	1	13	7	1
	1	12	8	1
	1	13	7	1
SUM CORRECT				10

in code is represented by dash-dot-dash-dot (C), dash-dot (A), dash (T), and dot-dot-dot (S). I then set "dash" as equivalent to 0 in the language of my ESP targets, and declared that "dot" was the same as 1. See Table 2 as an illustration of these data used as an attempt to retrieve the word.

The results in Table 2 show clearly that if a stable, above-chance rate of scoring can be assumed, then practically useful information can be obtained by combining repeated calls at targets. A modest 60% rate of scoring has been distilled to a satisfying 100% rate and the retrieval of a one-word verbal message.[1] However, a critical requirement is the above-chance rate of overall scoring. This was not appreciated in the first attempts to apply this multiple-calling, call-averaging technique. (Foster, 1943, was apparently the first researcher to report multiple-calling with his study on Plains Indians, but he did not average the calls. He would probably have had good results if he had.) Fisk and West (1956) applied the approach almost as an afterthought in a study

TABLE 2
Data from Pseudo Experiment as an Exercise in Retrieving a Word

TARGET LETTER	MORSE CODE	PSEUDO TARGET	VOTES FOR 1	VOTES FOR 0	MAJORITY	DECISION LETTER
C	DASH	0	9	11	0	C
	DOT	1	12	8	1	
	DASH	0	8	12	0	
	DOT	1	12	8	1	
A	DASH	0	8	12	0	A
	DOT	1	12	8	1	
T	DASH	0	7	13	0	T
S	DOT	1	13	7	1	S
	DOT	1	12	8	1	
	DOT	1	13	7	1	
SUM CORRECT					10	CATS

involving clock targets and mood ratings and got very encouraging results. Two more applications, however, resulted in null results (Michie & West, 1957) and significant psi-missing (Fisk & West, 1957). More recent applications of the basic approach with positive results have been reported by Brier and Tyminski (1970a, 1970b), Puthoff (1985), Puthoff, May, and Thomson (1986), and Radin (1991).

Dramatic results of real-world applications of ESP effects have been reported with the procedure of remote viewing, but almost all of these have appeared in popularly oriented books and periodicals and not in peer-reviewed journals, and details have tended to be scanty. Still, results have often appeared to be impressive, and they have ranged broadly in subject matter, including gathering military intelligence (e.g., McMoneagle, 2002, Targ & Puthoff, 1977), assisting police in solving crimes (Lyons & Truzzi, 1991), predicting silver futures (Harary & Targ, 1985, Targ, 1988), finding good real-estate opportunities (Kasian, 2004), and discovering lost archaeological sites (Schwartz, 2001). Perhaps because of the dramatic nature of the claimed results

and the paucity of details, these reports have sometimes spawned considerable controversy (e.g., Harary, 1992, Marks, 2002, May, 1998, Utts, 1996, Wiseman & Milton, 1998).

In any case, it is important to note that both the peer-reviewed reports such as Radin (1991) and the less detailed ones all rely upon participants who are being counted upon to give somewhat reliable extra-chance results. Thus they all began their averaging procedures on data that showed at least a small hitting tendency at the level of the single item (or in the case of Brier and Tyminski, also used below-chance data to deliberately amplify and transpose the negative effect). The moral holds: Sheer averaging of multiple calls will only serve to distill whatever scoring tendency is in the larger body of data. Overall scoring at a chance level will only result in more reliably chance-level scoring in the averages. A psi-missing trend in the overall data will yield a stronger rate of missing. Thus the bootstrap of repeated sampling is no panacea unless overall scoring rate can be reliably and independently predicted.

Index sampling is a technique developed to try to meet the need of directional prediction. Basically, this involves sampling some of the calling as it proceeds, scoring that, and using the scoring direction of that index sample to predict the scoring rate of the remainder of the participant's work. It was first used in a somewhat intuitive way by Cadoret (1955) and rendered more mathematically precise by Taetzsch (1958, 1962). Brier and Tyminski (1970a, 1970b), already cited, used not only repeated calls but index sampling in their application of ESP predictions to the real world of casino gambling. Results were significantly positive and apparently lucrative. Dean and Taetzsch (1970) reported a suggestively significant replication of the approach. The facts that casinos are still in business and little more research of this sort has been reported suggest that this bootstrap technique often fails as well—as reason says that it should. Index sampling by itself requires that another key assumption be met by the data to be sampled and averaged. This is the requirement that sets of the ESP data (like the runs in standard forced-choice testing) be internally consistent in scoring direction. As Schmeidler (1960) pointed out, and many others have observed, this assumption generally does not hold true for ESP data. Early attempts to find internal consistency, such as split-half reliability with ESP runs, have usually failed. Because of this, the scoring directions of index samples and remaining data are as often opposite as they are the same.

A bit of reflection will make it clear, however, that there is one way in which index sampling can be made to work as intended. Just as the overall mean-direction of a sample must be predicted for majority votes to be useful, another parameter of performance must be reliably predicted for index sampling to be useful. This is the variance of the performance around the chance expectation. If the deviation of a given set of guesses is relatively large,

then the scoring directions of any two parts of the whole set will tend to be the same. In the extreme case, if the whole run of calls is correct at 100% then the scoring direction of a sample will have to be 100%, which will match the 100% scoring rate of the remainder of the calls. Conversely, if the scoring deviation of the whole set is very small, at or very close to chance expectation, then the scoring deviations of any two parts of the whole set will tend to go in opposite directions. There is no magical bootstrap with index sampling, either; but with the assurance of large deviations from chance, or the ability to know when deviations will be large or small, it can be quite useful.

One demonstration of how stunningly well majority vote and index sampling can work was provided by Ryzl (1966). He used the work of a single hypnotically trained participant who repeatedly called the shuffled items of five lists of binary targets. The lists were coded representatives of randomly derived numbers, just as the ones and zeros in my pseudo experiment represented the letters of the word CATS. After sampling and averaging the calls, all five 10-item target lists were identified perfectly. Ryzl and his participant Pavel Stepanek achieved perfect "transmission" of the information in each of five series. It's worth remembering that Stepanek was perhaps the most reliable high scorer in the history of parapsychology. In this case, as in others, his runs of calls showed a persistent tendency to score above chance, and the run scores tended to be large deviations. He fulfilled the requirements of the techniques of majority vote and index sampling, and the flawless results demonstrated this fact.

This brings us to the third means that has been employed to assist in the problem of amplifying psi effects: finding ways to predict scoring trends (and scoring deviations), or ways to evoke them, so that redundancy, sampling, and averaging can be used reliably. Ryzl had at hand his Stepanek to work with and could safely predict that Stepanek would do as he had been doing before. With few Stepaneks around, parapsychologists have spent a great deal of effort in trying to define variables that will predict scoring, separating participants who will perform above chance from those who will perform below, and also trying to determine conditions that will reliably evoke scoring in either direction. Perhaps curiously, little of this effort has been applied to the problem of amplifying efficiency. The question with which I opened this discussion was: Can laboratory psi effects be put to practical use? The results of Ryzl and Stepanek suggest that the answer is yes. But having independent means of predicting or evoking reliable scoring directions and/or reliable scoring deviations is required. Presumably almost any independent predictor of these parameters of performance could be applied to this task.

One line of work aimed at exploring this was carried out by me some 20 years ago. I studied a set of mood adjectives that had been used in other research for describing momentary mood. Combining them with two other variables (a

sheep–goat attitude question and the California F-Scale), I derived a series of scales aimed at predicting run-score variance (sizes of deviations from chance) and hitting vs. missing (overall direction in scoring) in runs of forced-choice ESP testing with binary targets (Carpenter, 1968, 1969, 1983a, 1983b, 1991). The targets used were generally + and O. They were usually derived precognitively after all data were collected, but sometimes the targets were used clairvoyantly and coded to represent other information to be retrieved by all of the participants in that series acting in concert. In each series the participants guessed over and over at the same single list of targets without knowing it. This permitted my studies to test not only the efficiency of my predictive scales, but also my applications of the techniques of repeated guessing, index sampling, and majority votes. The content of the item collections used to predict hitting and variance evolved over the series, as larger and larger bodies of data were used to derive more reliable scales by stepwise multiple regression. I carried out 15 independent series with different groups of participants (usually university psychology students), and generally I met with some success. My final paper in this line of work reported three series in which I attempted to "transmit" by these means two words (represented by Morse code) and one set of octal digits picked by another experimenter and kept hidden from me. All three series showed statistically significant success, and clear amplification of efficiency with the repeated-sampling and averaging procedure. One of the three succeeded in retrieving the coded word PEACE with perfect accuracy.

Two New Studies

I have carried out two further studies along these lines that have not been reported. They were conducted several years ago, but never analyzed correctly until recently. Both of these studies employed revised mood scales that were generated from all previously collected data, including the last series of Carpenter (1991). One scale optimally postdicted scoring direction in the sample, and the other postdicted scoring extremity (run-score variance). In the two new studies these scales were used to predict targets unknown to me, as a demonstration of message-amplification principles to students in the Summer Study Program at the Foundation for Research on the Nature of Man (FRNM) in two consecutive years. Except for different targets, different data-collecting experimenters, and different participants, the two studies were identical. Richard Broughton served as co-experimenter in both studies, picking the targets (with the help of K. R. Rao in Study One), and, most importantly, writing a computer program that permitted an improved way of assigning and shuffling targets across runs and conducting scoring, sampling, and averaging procedures automatically. My previous series had used the same target lists repeatedly within each series, with targets in the same orders, so scoring required a great deal of work to correct

for the stacking effect caused by biases in calling patterns across participants. Broughton's program maintained the identities of targets within a list, shuffled them randomly for each run (avoiding the stacking problem), and carried out all other analyses automatically. Unfortunately, errors in using the program in the rush before scheduled class presentations led to false initial results in both cases. In Study One, extreme-quartile cut-off points intended for the two mood scales (explained in the Method section) were switched, resulting in the inappropriate inclusion and exclusion of much data. In Study Two, one of the predictors of hitting was inadvertently omitted. These problems were later realized, but the data lay in a filing cabinet for a long time before a period of leisure permitted them to be analyzed again, carefully and correctly. The results were interesting enough that I am reporting them now.

Method

These studies aimed to predict real-life events at a designated future time by the use of repeated calling at the same targets by sets of participants, and then analysis of their calls using their mood reports, their scores on the California F-scale, and their responses to the sheep–goat question. It was decided at the outset that the targets would represent the changes over a one-week period in the future of a set of twelve financial entities to be determined by an experimenter not otherwise involved in the procedure. An agreed-upon time was set that would permit the end of the week to coincide with the scheduled lecture at which the demonstration was to be described. The efforts of several parties are involved in this protocol, and after describing the materials used, I will spell out the procedures in terms of the parts played by each.

Materials

A packet of materials was given to each participant to use in self-testing to be done at home alone. The packet consisted of a page of instructions, a California F-scale, the sheep–goat question, and four calling sheets. Each calling sheet had 5 columns of 24 cells in which their guesses were to be recorded. The targets + and O were to be used. On the back of each calling sheet was a list of 57 mood adjectives, most of which were originally used to study the effects of stimulants and sedatives (Nowlis, 1961, 1965). Seven of these items were newly added for these studies for exploratory purposes, but were not used in the planned analyses. Participants were asked to respond to the questions in the packet, and then to pick four times when they could be alone for a few minutes. At these times they were to fill out each column on a given sheet with some order of +'s and O's that they felt would match the targets that would be picked later (no mention was made of coded predictions or repeated guessing). Then they

should immediately turn the sheet over and check the items in a way that would describe their mood at that moment. If an item was left unchecked, that meant it did not at all describe their mood, one check meant that it described their mood somewhat, and two checks meant that it described their mood strongly. The mood adjectives are given in Appendix A.

Based on findings from previous studies, participants were to be divided into two groups in terms of their F-scale scores. Using norms that I have carried over in this research program, those with scores of −31 or lower were held and used in further analyses, others were excluded.[2]

The collections of mood items used as predictors in these studies were as follows: Direction of scoring was predicted by combining responses positively to *amiable*, *fearless, masterful*, *retiring*, and by a *yes* answer to the sheep–goat question; and it was predicted negatively by *adaptable*, and a *no* answer to the sheep–goat question. Extremity of scoring was predicted positively by *close-mouthed* and negatively by *detached* and *witty*.[3]

Experimenter Soliciting Participants

I played this role in Study One and Kathy Dalton did it in Study Two. This person went to some group of potential participants and gave a very brief talk on ESP research and then described the procedure in which people were asked to participate. They were told that they would be asked to try to use ESP to predict targets that would be picked in the future, after all the guesses had been collected, and that this was something like the kinds of predictions that people tried to make in games of chance or gambling. They would also be asked to respond to a questionnaire on some general attitudes, and to check off words to describe their moods at the times in which they did the testing. A date was set for return of the materials, usually about two weeks hence. No payment was offered for participation.

Experimenter Picking Targets

Richard Broughton played this role in both studies, with the help of K. R. Rao in Study One. He picked 12 financial entities the one-week changes in which would generate the targets for the study. This list was kept secret. When the day arrived for the target week to begin, Broughton consulted *The Wall Street Journal* for the baseline values of the entities and recorded them. On the last day of the week, he recorded the values of the same entities and noted the direction of change over the week for each entity. These *gain* or *loss* targets were coded as + and O, respectively, for the ESP test. Then when the predictions generated by the analysis of the guesses were unveiled, he unveiled these targets as well for a check on how the procedure had fared.

Experimenter Analyzing the Calls

After collecting the last of the data at least two weeks prior to the target week, I keyed (or got help in keying) all of the participants' guesses into a spreadsheet along with the participants' F-scale scores, sheep–goat response, the page number and run number of each run, and all mood-item responses. Then I used some high-temperature numbers from that day's local newspaper to pick an entry point into a table of random numbers from which I selected 12 digits. I converted the digits to + if the digit was odd, and O if it was even. These digits were to be used as index targets for the series, to be used to help predict the actual content of the 12 precognitive targets. All of the participants' responses along with the index targets were entered into Broughton's scoring program.

This program maintained the identity of each of the 24 targets used in each run (12 predetermined index targets and 12 unknown precognitive targets) and randomly shuffled the 24 positions anew for each run of calls using a software pseudo-random function. Thus "Index Target One" or "Precognitive Target Five" kept their identities across runs, even though they appeared in different actual run positions in different runs. Then the program scored the mood scales for each page of runs. It excluded the data of high–F-scale participants from further analysis and retained only the low-F cases. It tabulated the mood scale scores for each page of runs and printed them out for the experimenter who then calculated the nearest quartile cut-off points in each scale (only extreme-quartile scores on the scales were used as generating a prediction of direction or extremity. Mid-range scores were omitted from further analysis). These quartile cut-off points were entered back into the program which then used the mood scales to segregate the data into subgroups for two repeated-guessing analyses—one based on directional predictions and the other based on extremity predictions. The logic used was as follows.

Consider first the simpler case of using directional predictions. If a mood-scale score gave a prediction of psi-hitting for its page of ESP runs, then all the calls made to the 12 precognitive targets were tallied as they stood. If the mood scale gave a psi-missing prediction, then all calls were reversed (+ calls became O's, and vice versa) and these reversed calls were tallied. Then all guesses across all pages that had yielded the mood predictions were tallied together for a final set of "votes" for + and for O.

In the case of extremity predictions, the 12 index targets were used in an intermediate step. All index calls in all runs were scored against the index targets. Then in the case of a large-RSV prediction for the page, runs in which index calls scored above chance (7 or more) generated a psi-hitting prediction for the rest of the trials in the run, so the calls on the remaining 12 precognitive targets were entered as they were into a tally. On the other hand, if the index

scoring was below chance (5 or fewer), precognitive calls were reversed and then entered into the tally. Index scores exactly at chance with 6 hits yielded no prediction, and the calls on the precognitive targets were omitted for that run. When the mood scale for the page predicted small RSV, a procedure opposite to that used with large RSV prediction was carried out. Above-chance index scores generated a prediction of psi-missing for the other trials of the run, so they were reversed and tallied, while below-chance index scores generated a psi-hitting prediction for the rest of the run, so those precognitive calls were tallied as they were. Both of these analyses (one using directional mood-scale predictions and the other using extremity predictions) were carried out for all usable data. Then the two sets of tallies were themselves combined for a final set of 12 best guesses as to the identity of the precognitive targets.

At the end of the one-week target period, the target identities predicted by the participants' efforts are revealed and matched against the actual targets determined by the week's financial activity, and a number correct and incorrect can be determined.

Study One

Participants

Volunteers were solicited from Summer Study students and from members of two meditation classes and one yoga class being taught in the community whose instructors were interested in parapsychology. I chose these groups because I expected that they might contain a relatively large proportion of persons with low-authoritarian attitudes. The cut-off points that had separated median groups of university students 10 years earlier (and which I chose to continue to use) had come to select smaller groups of participants over time as attitudes of university students apparently drifted in a more authoritarian direction. This made it inefficient to use this unselected student population. A small sample of 58 participants agreed to take part, of which 25 met the low-F criterion. It was not expected that such a small sample would yield very reliable results, but time did not permit soliciting more participants. Of the 25 low-F participants, 19 were female. The low-F group ranged in age from 18 to 52, with a median age of 21. These low-F participants contributed 495 runs.

Soliciting Experimenter

I played this role in Study One, and maintained what had become my normal routine: a very brief talk about the meaning of ESP testing, a dispassionate statement that the questions involved were interesting but still quite mysterious to science, and a courteous request for their help. My attitude was deliberately neutral and routine, as experimenters typically behave in other psychological

research in which they hope to make their own contributions to the situations as standardized and neutral as possible.

Targets

The one-week behavior of a set of market values and industry group comparisons was chosen to determine the targets. Six were simple comparisons from the beginning day to the last. A rise in value was called a +, a decline was an O. The other six were proportional measures. These were chosen in case a general drift in the market over the week could cause too many positive correlations in behavior and a disproportionate number of targets of the same type. The value of a pair of industry groups was compared at the beginning of the week and then again at the ending day. If the relative value of one over the other was maintained in direction over the week, the trial was called +. If the advantage between them switched to the other over the week, the target was an O. The specific target indices and their actual values and the targets determined are given in Table 3.

Results

The final tally of both repeated-guessing analyses of the low-F data was rather successful. Eleven of the twelve items were predicted correctly. See Table 4.

The overall data of low-F participants scored at a very slightly below-chance rate of 49.9%, which would have made a simple majority vote procedure with these data a waste of time. The votes cast by the two analyses combined were correct at a higher rate of 51.7%, and the decisions made by the votes were more efficient still at 92% correct ($\chi^2 = 8.33$, one degree of freedom, $p = .004$). Only the last item (the behavior of banks central vs. banks west over the period) was called incorrectly. Had actual investment decisions been made at the beginning of the week based on these predictions, the outcome would have been positive for the investor. Of the two mood scales used to generate predictions, the one for scoring direction contributed much more. It correlated positively (but not significantly) with the ESP scores: $r = .15$, $p = .08$. Statistical significance is not always required for practical utility. The extremity scale gave a correlation with run-score variance that was very slightly in the wrong direction: $r = -.05$.

Study Two

Participants

Participants for this study were drawn from classes in acting and creative writing at the University of North Carolina Chapel Hill. This population was

TABLE 3
Economic Indices Used as Precognitive Targets for Study One

Trial	Item	Beg. Value	End Value	Change	Target
	Markets				
1	Price of Gold on CMX (spot)	369.7	364.4	Declined	0
2	Oil, West Texas Int. Crude	22.15	21.5	Declined	0
3	Sugar cane raw, World	.0956	.0989	Increased	+
4	Dow Jones Industrial Avg.	2978.76	2966.23	Declined	0
5	Dollar vs. Yen (preced. day)	.07301	.007273	Declined	0
6	Bond Yield – Corp. Master	510.41	513.03	Increased	+
	Industry Group Comparisons				
7	Banks (money centers) vs. Casinos	B: 195.22 C: 596.95	B: 192.93 C: 568.59	B declined less	+
8	Industrial Tech. vs. Aluminum	I: 290.68 A: 322.23	I: 291.83 A: 322.86	IT gained more	+
9	Energy vs. Financial	E: 281.11 F: 301.77	E: 276.34 F: 297.93	Fin. declined less	0
10	Building Materials vs. Railroads	B: 404.02 R: 371.91	B: 398.15 R: 381.18	R increased, BM decreased	0
11	Office Equipment vs. Diversified Technology	O: 265.99 D: 269.75	O: 262.23 263.75	OE declined less	+
12	Banks Central vs. Banks West	BC: 499.99 BW: 299.36	BC: 493.7 BW: 287.92	BC declined less	+

On **Markets**, if value was higher at the end than at the beginning of the period, the target was coded as 1 (or +), if lower at the end it was coded as 0 (or−). For **Industry Group Comparisons**, if the first item did better relative to the second over the week (improved more or declined less), the target was coded as + (or 1); if the second did better, the target was coded as 0 (or−).

chosen in part to assure a higher proportion of low-authoritarian participants, as in Study One, but also because we thought it would be of interest to see how a group expected to be more psi-productive might respond to this procedure. Previous research had strongly suggested that more creative persons are especially likely to demonstrate psi effects (e.g., Anderson, 1966, Moon, 1974, Moss, 1969). I reasoned that their mood reports might discriminate their scoring patterns especially effectively. The sample for this study was smaller even than the one before: 47 volunteers, of whom 22 were low-F. They contributed 440

TABLE 4
Tallied Votes and Prediction-Decisions in Study One

Cell	Votes +	Votes O	Decision	Target
1	193	208	O	O
2	194	207	O	O
3	206	195	+	+
4	194	207	O	O
5	196	205	O	O
6	203	198	+	+
7	215	186	+	+
8	226	175	+	+
9	194	207	O	O
10	200	201	O	O
11	212	189	+	+
12	193	208	O	+

2,490 votes correct, 2,322 incorrect: 51.7% correct.
Underlining, total votes associated with actual target.
Gray shading, correct decisions. 11 decisions correct 1 incorrect: 92% correct.

runs. Those low-F participants ranged in age from 18 to 58, with a median age of 22.5. Thirteen were female. Time constraints again limited solicitation of participants.

Soliciting Experimenter

Kathy Dalton played this role in Study Two. Although I instructed her briefly in my normal approach to soliciting participants in class groups, discussion with her later made it clear that she also gave some room in the situation to more expression of her own personality. She made it a point to

TABLE 5
Economic Indices Used as Precognitive Targets for Study Two

Trial	Item	Beg. Value	End Value	Change	Target
	Markets				
1	Price of Gold on CMX (spot)	339.30	336.10	Declined	0
2	Oil, West Texas Int. Crude	19.90	20.75	Increased	+
3	Sugar cane raw, World	.0956	.0985	Increased	+
4	Dow Jones Industrial Avg.	3338.77	3333.18	Declined	0
5	Dollar vs. Yen (preced. day)	.007454	.007491	Increased	+
6	Bond Yield – Corp. Master	568.21	568.70	Increased	+
	Industry Group Comparisons				
7	Banks (money centers) vs. Casinos	B: 239.79 C: 650.17	B: 237.41 C: 674.10	Casinos increased	0
8	Industrial Tech. vs. Aluminum	I: 316.32 A: 316.82	I: 322.42 A: 320.08	IT gained more	+
9	Energy vs. Financial	E: 250.13 F: 346.98	E: 260.36 F: 346.00	Energy up, Financial down	+
10	Building Materials vs. Railroads	B: 467.39 R: 476.96	B: 477.32 R: 501.67	Rail did better	0
11	Office Equipment vs. Diversified Technology	O: 307.72 D: 287.98	O: 306.81 D: 282.43	OE declined less	+
12	Banks Central vs. Banks West	BC: 624.5 BW: 384.4	BC: 623.52 BW: 390.67	BW up BC down	+

On **Markets**, if value was higher at the end than at the beginning of the period, the target was coded as 1 (or +), if lower at the end it was coded as 0 (or−). For **Industry Group Comparisons**, if the first item did better relative to the second over the week (improved more or declined less), the target was coded as + (or 1); if the second did better, the target was coded as 0 (or−).

clearly express her interest and enthusiasm and lively faith in the creative and intuitive abilities of the students. She followed the letter of the procedure, but added more lively spirit.

Targets

I asked Broughton to select targets as before, picking some financial indices whose behavior over a one-week period would generate 12 precognitive binary targets. We agreed upon the target week, again a time that would conclude on a

day in which I would be giving a lecture on this subject at the FRNM. Without telling me so, he elected to use the same indices as the year before: six changes in markets and six industry group comparisons. Changes would generate + and O targets as before. See Table 5 for the targets assigned along with the actual financial values recorded at the beginning and end of the target week.

Results

Overall results were unusually positive for this series. In fact, this is the first case among the 17 series conducted in this program of repeated-guessing work, that significant psi-hitting was observed overall. Scoring on the precognitive targets overall (irrespective of F-scale or mood scores) was 5,661 hits where 5,520 were expected by chance: $z = 2.42$, $p = .016$. On the other hand, in terms of the analyses of most interest, in contrast to Study One the decisions generated by the repeated-guessing procedures were not particularly successful. See Table 6.

TABLE 6
Tallied Votes and Prediction-Decisions in Study Two

Cell	Votes +	Votes O	Decision	Target
1	168	147	+	O
2	153	162	O	+
3	169	146	+	+
4	160	155	+	O
5	167	148	+	+
6	165	150	+	+
7	161	154	+	O
8	157	158	O	+
9	160	155	+	+
10	145	170	O	O
11	164	151	+	+
12	150	165	O	O

1,926 votes correct, 1,854 incorrect: 51% correct. 7 decisions correct, 5 incorrect: 58% correct.
Underlining, total votes associated with actual target.
Gray shading, correct decisions.

The unit majority votes were correct at only a 51% rate, barely above-chance expectation, and only seven majority decisions were correct, with five incorrect ($\chi 2 = .33$, one degree of freedom, $p = .56$). Thus, the results are on the expected side, but only weakly so. Practical investment decisions made on the basis of these predictions would have beaten sheer chance, but not by very much.

This result would be expected by the rather poor performance of the mood-scale predictors, along with the small number of cases. The cluster intended to predict scoring direction yielded a relationship almost exactly equal to chance: $r = .01$. This time the cluster predicting extremity performed better, but not significantly: $r = .11$, $p = .155$.

An Exploratory Analysis

Overall, psi-hitting was not expected in this study, but I decided to carry out an exploratory majority-vote analysis as if it had been. For this analysis, all

TABLE 7
Study Two Analysis Using No Scales (Simple Majority Vote)

Cell	Votes +	Votes –	Decision	Target
1	427	497	O	O
2	460	464	O	+
3	457	467	O	+
4	458	466	O	O
5	478	446	+	+
6	471	453	+	+
7	456	468	O	O
8	493	431	+	+
9	468	456	+	+
10	453	471	O	O
11	477	447	+	+
12	446	478	O	O

5,684 votes correct, 5,404 incorrect: 51.3% correct. 10 decisions correct, 2 incorrect: 83.3% correct.
Underlining, total votes associated with actual target.
Gray shading, correction decisions.

data across all participants are pooled and tallied into simple votes for + and O for each target position. The results are given in Table 7.

As might be expected from the overall psi-hitting in the data, and with the much larger number of votes, this analysis is more efficient than the last one. The unit majority votes rose in accuracy to 51.3%, and 10 of the 12 majority decisions were correct: $\chi^2 = 5.33$, one degree of freedom, $p = .021$. With an accuracy rate of 83.3%, our investor would be doing better in this case.

Discussion

Some Possible Problems

One problem that bears mentioning hinges on the fact that with almost any "real-world" set of targets, randomness cannot be assumed. For example, there are times when financial entities such as the ones studied here drift up or down in a correlated manner. This has a bearing mainly on simple majority-vote analyses such as the last unplanned analysis done in Study Two. It may be highly unlikely, but it is not inconceivable that, given a high degree of correlation among the targets, some similar excess of calls that happened to be given by participants in the weeks before the target period could cause a spurious relationship between majorities and targets. If most targets happened to be + because of increased value, and most calls happened to be "+" because, say, of some period of exuberance during the testing, then an excess of hits would appear that would not represent ESP. This does not seem to have happened in the case of this particular analysis of Study Two. Only the first six targets are involved in the question, since the last six were determined by relative comparisons, precisely in order to avoid the problem of correlated performance. Among these six targets there may have been some degree of correlation, in that 4 were "+" and 2 were "O." The majorities reached from the participants' calls tended in the opposite direction: 4 "O" and 2 "+." The fact that there were still four hits among the trials was in spite of the two contrary tendencies, not because of them.

While randomness of targets is an understood prerequisite for parapsychological research in general, it is because of the need to be assured that the statistical evaluation of results is appropriate. In the context of wishing to predict real information, statistical evaluation is less important than pragmatic accuracy. Even if the exactly best statistical model for assessing significance cannot be known because of target non-randomness, results may still be practically useful.

Still, the non-randomness of targets does add an unnecessary difficulty in interpreting results, so future studies planning a simple majority vote should eliminate it. This could be done simply on the part of the experimenter who

picks targets by his or her adding one additional step to the procedure. After picking the targets, and observing the actual behavior that was being predicted, before submitting to the second experimenter for scoring, one more pass could be made on the targets by randomly switching about half of them to their opposites. It would be understood at the outset that the "correct target" would be this final, randomly coded set. Participants appear to use their ESP to reach the correct target, whatever it is, without regard to such contingencies, so there seems to be no reason to expect that any problem would arise from adding this step.

The objection might also be raised that the choice of the responses "+" and "O" are too transparently linked to the idea of ups and downs of market performance, and somehow lead to spurious relationships. It is difficult to imagine why this might be so. In this design, participants never know that their responses are yoked to any future outcomes of any sort, only that targets will somehow be selected and they are trying to predict them. In any case, with regard to the primary analyses of these studies involving independent predictors of performance, and not simple majority votes, the manipulations of calls prior to tallying majorities results in about half of the calls being actually rendered into their opposite content before they are used.

Some Implications

These two small studies offer some support for the idea that even relatively weak laboratory psi effects can potentially be put to practical use in predicting unknown future events. The scales of mood items and attitude items used here have shown modest reliability over a number of studies. The findings also underscore the reality that procedures that depend upon such relatively small effects may not be counted upon to always work, particularly when small sample sizes are employed. However, this report is primarily a demonstration of a principle, and it is important to note that such mood items are not the only predictors that could be used in such a way, and they are probably far from the strongest that we might find to use. As they are, they make some psychological sense. Low-authoritarian persons have been found to give more valid self-reports in other settings, as mentioned above. Persons who believe that ESP is possible in the conditions of the study have often been found to score better than those who do not believe that. A factor analysis of the mood items was carried out by Carpenter (1991) and showed that items predicting hitting tended to represent factors of detached relaxation and inward focus, freedom from self-doubt and cognitive analysis, and freedom from anxiety—all things that have been found throughout our literature to effect psi performance (e.g., Carpenter, 2004, Palmer, 1978, 1982, Schmeidler, 1988). Items predicting extremity tended to represent factors that implied a non-analytical and holistic state of

mind along with a freedom from distraction and cognitive work. Such things have been proposed by Carpenter (2004, 2005, 2008) to facilitate a singularity of unconscious intention that is theorized to increase scoring extremity.

Whatever their meaningfulness and predictive power, however, these means are certainly not the only ones for making use of redundant psi data. And they are not the simplest and most direct. The secondary analysis of Study Two illustrates that nicely. Some experimenters (and perhaps some experimental approaches) appear to be more psi-facilitative than others (e.g., Wiseman & Schlitz, 1997), and Kathy Dalton has gone on from her work in this study to do other work that suggests she can be one of those inspiring experimenters, at least with artistic participants (Dalton, 1997, Morris, Dalton, Delanoy & Watt, 1995). At the time this study was done, there was already evidence that persons engaged effectively in creative work can be counted on to score above chance in ESP tests with some reliability, and the ensuing years have also added to that evidence (Dalton, 1997, Morris, Cunningham, McAlpine, & Taylor, 1998, Morris, Summers, & Yim, 2003, Moss, 1969, Schlitz & Honorton, 1992). Thus, while I did not predict above-chance overall scoring in Study Two, I certainly might have done so legitimately. If I had, a straightforward confirmation would have come forth. We should make such direct predictions of scoring when we have reason to.

The main point I wish to make is that any means of predicting scoring direction could be put to work in a majority-vote paradigm. The targets given to participants can be yoked to some "real-world" event which we wish to predict. The yoking seems to present no obstacle to persons demonstrating their psi apprehensions as they normally do, all in the context of our predictors. Similarly, any means at hand of predicting scoring extremity reliably can be put to work as well and used to interpret the scoring implications of index sampling. Will extraversion as measured on the Eysenck Personality Inventory reliably predict nonverbal ESP performance (Roe, Henderson, & Matthews, 2008), or self-rated luckiness reliably predict psi-hitting (Luke, Delanoy, & Sherwood, 2008)? Will self-rated openness to experience show a robust relationship with preference choices linked covertly with psi targets (Luke, Roe, & Davison, 2008)? All of these possibilities are drawn from the most recent journals I have at hand. There are many others. Some will prove to be more robustly reliable than others, and they can all be put to work in the practical ways illustrated in these two studies.

Should psi effects be applied? This is like asking if we wish to have more powerful access to knowledge by any means at all. Should Galileo have figured out the basic equations of refraction and developed the telescope? Should Pascal have built a mechanical calculator? Should the Wright brothers have worked out some basic principles of aerodynamics? In fact, we generally desire new access to knowledge and fear it at once. More knowledge is more power,

and we wonder if our wisdom and humanity will be equal to the challenges of more power. In any case, scientifically derived parapsychological effects await application and will probably be put to work.

Ideas such as "psychic development" and "psychic application" currently tend to be left mostly to practitioners who teach self-development techniques in "mind control" or "intuition", with dubious results. Even if such approaches have some success, they are rather analogous to trying to see greater distances by vision training, or to increase computational power by teaching arithmetical shortcuts, or to solve the problem of human flight by developing the techniques of training high jumpers. It is scientific work that has made the enormous leaps in our capacity in these areas, and it will be scientific work that eventually leads to the reliable application of psi.

Notes

[1] This is a straightforward application of the Law of Large Numbers (Feller, 1968), which holds that the estimate of a population value drawn from averaging samples will more closely approach the true value as the number of samples increases.

[2] In earlier studies I had used the F-scale as a moderating variable, on the assumption that persons lower in authoritarian attitudes were more reliable reporters of their own internal states (Barron, 1953, Scodel & Mussen, 1953, Thayer, 1971) and hence should give more valid mood reports. Lower-F participants were found to give more useful data in my previous ESP studies.

[3] Since these clusters are derived by stepwise multiple regression, which selects for orthogonal contributions of items to a prediction, no unifying conceptual themes would be expected among the items.

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LETTER TO THE EDITOR

Only Ostensibly Anomalous

I read with fascination Adrian Parker's excellent review (Parker, 2010) of Archie Roy's *The Eager Dead: A Study in Haunting* (Roy, 2008). Parker cites an example from the 1974 book *The Challenge of Chance* (Hardy, Harvie, & Koestler, 1974), in which co-author Arthur Koestler states that several top-secret codewords for the 1944 D-Day invasion showed up in crossword puzzles in *The Daily Telegraph* immediately prior to the military operation. Subsequent events not mentioned by Parker shed light on this remarkable scenario, which is a cautionary tale for anyone investigating ostensibly anomalous phenomena.

The codewords that appeared in the crosswords immediately prior to D-Day were those for the American landing beaches (Omaha and Utah), the Allies' floating harbors used in the landings (Mulberry), the naval assault phase (Neptune), and the overall term for the operation (Overlord). In previous months the words Juno, Gold, and Sword had also appeared, all codenames for beaches assigned to the British. This understandably triggered fears at MI5 that the Germans were being tipped off by a British spy. However, when Leonard Dawe, the crossword puzzle designer, was arrested and interrogated, he proved utterly clueless about the invasion.

So where did the codewords come from? Had Dawe unconsciously dipped into "the Jamesian cosmic reservoir" or the "universal memory repository," which Parker mentions in his review, and unknowingly extracted the codewords?

Apparently not. According to an article in *The Daily Telegraph* in May 2004 (Gilbert, 2004), the sixtieth anniversary of D-Day, the mystery of how the codewords came to appear in the crosswords was solved in 1984, when Ronald French, a property manager in Wolverhampton, came forward. According to French, Dawe, headmaster of Strand School, a boys' grammar school in South London, occasionally invited his pupils to help fill in the blank crossword patterns as an intellectual exercise (James Lovelock, the scientist who originated the Gaia hypothesis, was a former pupil at Strand). Dawe would then create the clues to solve the words. French, who was 14 at the time and a student at Strand, claimed that during the weeks before D-Day he had learned the operation's codewords from American and Canadian soldiers camped nearby, awaiting the invasion. "I was totally obsessed about the whole thing," he said. "I would play truant from school to visit the camp and I used to spend evenings with them and even whole weekends there, dressed in my Army cadet uniform. . . . Everyone knew the outline invasion plan and they knew the various codewords. Omaha

and Utah were the beaches they were going to. They knew the names but not the locations. We all knew the operation was called Overlord. Hundreds of kids must have known what I knew," French said. The soldiers talked freely in front of him because it was obvious to them that the boy was not a German spy.

"Soon after D-Day, Dawe sent for me," French explained, "and asked me point blank where I had got the words from. I told him all I knew and he asked to see my notebooks. He was horrified and said the books must be burned at once. He made me swear on the Bible I would tell no one about it. I have kept that oath until now," French said in 1984.

MI5 also grilled Dawe's senior colleague Melville Jones, the newspaper's other crossword compiler. Dawe almost lost his job, but not quite. "[T]hey eventually decided not to shoot us after all," he said later.

The Challenge of Chance was published in 1974, so Koestler would not have known about the mundane explanation of these events that surfaced a decade later.

So it appears that Dawe was not unconsciously dipping into the great cosmic soup after all. One is reminded of T. H. Huxley's observation, "The greatest tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact" (Huxley, 1894).

Thanks to Adrian Parker for reminding us.

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ESSAY REVIEW

"Denialism": The New "Pseudo-Science"

Denialism: How Irrational Thinking Hinders Scientific Progress, Harms the Planet, and Threatens Our Lives by Michael Specter. Penguin Press, 2009. 304 pp., $27.95. ISBN 9781594202308.

Special Report: Living in Denial by Michael Shermer, Debora MacKenzie, Richard Littlemore, Jim Giles, and Michael Fitzpatrick. *New Scientist*, 15 May 2010, pp. 36–45.

Bertrand Russell (among others) remarked on the sad fact that it takes ever so many more words to correct a false assertion than it takes to make that false assertion in the first place. Specter's book and the Special Report series of articles in *New Scientist* illustrate that—for volumes could be filled with analyses of the ignorance displayed by these writers.

In a nutshell: These writers fit the category of self-styled "skeptics" whom Marcello Truzzi accurately described as ***pseudo***-skeptics: They label as "pseudo-science" any view that they regard as wrong, without displaying any felt need to demonstrate why that label might be appropriate. That would have required defining pseudo-science and showing how the particular item being criticized satisfies the definition; and that has never been done, for one thing because neither philosophy of science nor any other pertinent discipline has ever been able to agree on how to define "pseudo-science"—or for that matter, "science." Innumerable attempts have failed to establish criteria that distinguish science from other knowledge-seeking attempts, or that distinguish good, proper science from bad, improper or spurious science. For an authoritative account regarding that failure, Laudan (1983) is accessible as well as sound. For some unsuccessful attempts to define "pseudo-science," see "Pseudoscientists, Cranks, Crackpots" (Bauer, 1984/1999). For discussion of a number of topics often called "pseudo-science" and a review of the lack of validity in that labeling, see *Science or Pseudoscience: Magnetic Healing, Psychic Phenomena, and Other Heterodoxies* (Bauer, 2001). Yet Specter claims that "the line between science and pseudoscience was deliberately blurred" (p. 160): What line? That non-existent, undefined, undefinable line?

Pseudo-skeptics are largely, perhaps always, unwitting defenders of scientism, the erroneous ideology which holds that science and science alone can capture objective truth; pseudo-skeptics attack true skeptics just because we

question a contemporary mainstream consensus in science.

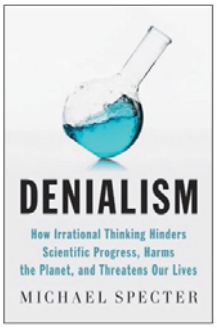
Of late the vigilantes of scientism have adopted the term "denialism" rather than "pseudo-science" to describe the questioning of received scientific wisdom, and they have taken to calling the questioners "deniers" or "denialists." This change in terminology was spurred plausibly because "denialism" carries more emotional charge than "pseudo-science," since "denialism" first came into popular usage in connection with denials that the Holocaust was a deliberate and not-far-from successful attempt to eradicate from the planet all those of Jewish descent. "Denialism" is as intellectually barren and invalid a term as "pseudo-science."

Michael Specter's book, and the article series in *New Scientist*, illustrate all this admirably, which is to say sickeningly: They are intellectual garbage.

Bear in mind Bertrand Russell's cited insight: It's impossible to demonstrate everything that's wrong in these writings short of many hundreds of pages. So I can only illustrate, which leaves me open to the charge of taking out of context or over-generalizing. To such a charge, I can only say: "Be my guest. Read all of that book and those articles." If you do that, you will declare me innocent of any deception or exaggeration.

Specter's book is a muddle, a mish-mash, unfocused, incoherent, and intellectually shallow to say the least. He doesn't define "denialism," but his remarks about it show that it is the questioning of anything that Specter believes to be true. Most of the book is a paean to the wonders of science. Periodically he acknowledges reality, for instance by admitting that genetic engineering and genetically modified (GM) foods are not without risk: But then he immediately muddles that admission by labeling as denialists those who, because of those real risks, have argued and acted against widespread deployment of GM foods and seeds.

Specter is an award-winning science writer, yet he commits such rubbish as "unmistakable connection between 'conventional' plant breeding and genetic engineering" (p. 118). What true skeptics about GM foods point to is the unmistakable *disconnect*. Conventional plant breeding works via whatever results hybridization and mutation bring about *by means of naturally evolved mechanisms that we do not fully understand*; genetic engineering by contrast is a hit-*and*-miss injecting of genetic material in hopes that it will somehow lodge in a place where it can be "expressed" in the intended manner. That procedure ignores just about everything that's been discovered about genetic mechanisms since the elucidation of DNA structure (Ast, 2005), namely: that "genes" aren't single entities that always do the same thing (make the same protein); that intricate signaling determines when "genes" are switched on and off and how they divide themselves into sub-units that coordinate with sub-units of other "genes" to do the right thing at the right time; that "junk" DNA—which constitutes the



largest part of the human genome—has something to do with the signaling, as well as incorporating evolutionarily acquired "fossils" of retroviruses (human endogenous retroviruses, HERVs) that may protect against exogenous viruses (and which have likely been responsible for the mistaken "identification" of "HIV" as the cause of AIDS). Genetic engineering has yet to understand all these things well enough to know what bits of DNA need to be placed exactly where if they are to do what's wanted, nor has a means even been found to place bits of DNA exactly where intended. And yet Specter asserts that "Genetics and molecular biology are simply tools to help scientists choose with greater precision which genes to mix (and how to mix them)" (p. 117).

So Specter knows his science as little as he knows his philosophy of science. The shallowness of Specter's understanding of science might be epitomized by this: "Francis Bacon invented what we have come to regard as the scientific method (and Galileo began to put it to use)" (p. 9). It was painful to read this book, which is replete with such nonsense: "we are either going to embrace new technologies, along with their limitations and threats, or slink into an era of magical thinking" (p. 16). "Science and religion have always clashed and always will" (p. 17). On topics where a reader is informed, Specter's ignorance is likely to be manifest all too clearly. For example, I know quite a lot about HIV/AIDS, including that after nearly three decades it remains a mystery how HIV purportedly destroys the immune system, since all of the suggested mechanisms have so far been disconfirmed: "It is not clear how much of the pathology of AIDS is directly due to the virus and how much is caused by the immune system itself. There are numerous models which have been suggested to explain how HIV causes immune deficiency" (Cann, 2005); yet Specter writes: "Without the tools of molecular biology, we wouldn't have a clue how the AIDS virus works" (p. 20). In fact we don't have a clue, and perhaps that explains why all attempts to make vaccines or microbicides have failed.

At any rate, the book is about "denialism" only as part of an obsequious obeisance to "Science": Anything that questions current dogma is thereby "denialism." Even the titles of the book's chapters expose Specter's prejudices: "Vioxx and the fear of science"; "Vaccines and the great denial"; "The organic fetish"; "The era of Echinacea"; though the last two, "Race and the language of life" and "Surfing the exponential," are somewhat obscure until you've scanned their texts.

The book is muddled everywhere. Eric Topol is described as someone who was right about the dangers of Vioxx and who became thereby "an outcast in his own profession, shunned for his warnings and eventually driven from the

department he made famous" (p. 31). In other words, Topol was a denialist who was right when the mainstream was wrong! Nowhere does Specter try to explain why "denialism" is bad when denialist Topol is good and should have been attended to. To muddle things even further, Specter concludes the Vioxx chapter with this rather monumental non sequitur: "When we compare the risk of taking Vioxx to the risk of getting behind the wheel of a car, it's not at all clear which is more dangerous" (p. 55).

In the chapter on vaccines, Specter fails to make the crucial distinction between two quite different matters: vaccination in principle on the one hand, and on the other the use in practice of preservatives such as organic-mercury–containing thimoserol and such non-specific allergenic or toxic "adjuvants" as squalene. Many "denialists" question the latter, not the former, but Specter tars them all with the same brush. He also lauds Gardasil (p. 100) as an effective cancer vaccine, when we are decades away from being able to assess that.

"Race and the language of life" interested me because it recognizes that the markedly different tendencies to be asthmatic between West-Coast Hispanics and East-Coast Hispanics obviously has a genetic basis—just as does the tendency to test "HIV-positive" (Bauer, 2007). But then the chapter becomes unfocused, like the rest of the book, and it's not clear what Specter is getting at. He even fails to criticize the extraordinary albeit widespread foolishness of presuming that all "Latinos" or "Hispanics" share a common cultural or "ethnic" heritage. Unlike Ruth Benedict (Benedict, 1942/1983), Specter and those he cites don't appear to understand that "race" is a matter of biology whereas racism is a matter of culture, and there's nothing racist about investigating genetic aspects of race, indeed it can improve medical treatment, for example by appropriately varying drug dosages.

"Surfing the exponential" is about synthetic biology and presumably is meant to suggest speeding along a wave into the future. Synthetic biology seeks to create entirely new, human-designed organisms by putting together strings of DNA that Nature never put together, in order "to redesign the living world." Here Specter illustrates what he means by denialism: "Synthetic biology provides what may be our last chance to embrace science and reject denialism" (p. 226). In other words, to call "Halt" to even the most far-fetched experimenting that has unforeseeable, enormously far-reaching consequences—experimentation by people who literally don't know what they're doing—is denialism, just so long as those experimenters could be said to be doing science. Here are some other examples of Specter's lack of clarity as to what denialism is supposed to be: "Denialism is at least partly a defense against that sense of helplessness" in the face of "highly sophisticated technology we can barely understand" (p. 33); "denialism [is] at the core of nearly every alternative approach to medicine" (p. 158). "Denialism provides a way to cope with medical mistakes like Vioxx and

to explain the technical errors of Chernobyl or Bhopal" (p. 47); I wondered, "How does that work?," but Specter didn't enlighten me.

Or, denialism results from disappointed high expectations (p. 51); "willful ignorance . . . [is] the driving force of denialism" (p. 118)—as in the case of Topol, above, perhaps?

"When people decide that science can't solve their problems, they reject its principles." Where does that generalization come from? "Denying the truth becomes a habit" follows that sentence immediately (p. 127). So Specter is asserting that rejecting the principles of science—whatever that might mean—is a denying of truth. The book is brim-full of such silly assertions and non sequiturs.

"Denialists shun nuance and fear complexity, so instead of asking how science might help resolve our problems, they reject novel strategies even when those strategies are supported by impressive data and powerful consensus" (p. 4). Doesn't he know that "impressive" data are in the eyes of the beholder, and that even the most "powerful" consensus has often been wrong?

"Holocaust deniers and AIDS denialists are intensely destructive—even homicidal—but they don't represent conventional thought and never will" (p. 4). Of course, neither does breakthrough science represent conventional thought.

The "most remarkable act of denialism" by President George W. Bush was "to devote one-third of federal HIV-prevention funds to 'abstinence and marriage' programs" (p. 8). Stupid, perhaps; ineffectual, probably; but denialist?! Just because Specter says so?

* * *

The essays in *New Scientist* are of a piece with Specter's ignorant muddle. The lead author is Michael Shermer, who introduces the series by proclaiming, "I am a sceptic, but I'm not a denier." The difference, he says, is that Shermer takes "a scientific approach to the evaluation of claims." This is the Shermer who dismissed my book *without having read it* because the overwhelming majority of medical scientists regard the connection between HIV and AIDS as overwhelming; apparently he doesn't even know that hundreds of medical scientists and medical practitioners have expressed their disagreement with that "consensus." Shermer continues by asserting that climate skeptics have looked at the evidence whereas climate deniers had their position staked out in advance—an egregious, atrocious calumny on the thousands of true climate skeptics—competent and appropriately qualified climatologists and meteorologists and atmospheric scientists and geologists, and the like—who have signed petitions asking for the evidence to be looked at properly, that is, the actual evidence and not the outputs of computer models that mainstream

dogmatists of human-caused global warming keep pushing on gullible policy makers and media pundits.

Among Shermer's more amusing assertions is that "good scientists are sceptical." No! The greatest achievements in science have come from strong-willed individuals who paid little or no attention to ideas or claims that conflicted with their own pet notions. They were skeptical just as Shermer is, toward everything except their own notions.

I am among those who have been called a denialist as to HIV/AIDS. According to Shermer, that means that I am "automatic[ally] gainsaying . . . a claim regardless of the evidence"; because, if I'm a typical denialist then I'm "driven by ideology or religious belief." Had he read my book, he would have learned that I was driven by a collation of the mainstream data on "HIV" tests to deny the HIV–AIDS connection, *to my own initial astonishment* (Bauer, 2009a).

What Shermer has written here is a just-so story describing his firm belief that he can see the truth when others cannot, and feeling therefore at liberty to call those with different views "deniers" or "denialists." At the same time, Shermer asserts that science is not a matter of belief but of facts: Evolution or Big Bang either happened or they didn't, and "both matters can, in principle, be solved with more data and better theory." As to "evolution" he's wrong because that word needs to be defined very precisely before anything can be said on that score, much of the disputation being the result of a lack of such precision; as to Big Bang, of course that can never be finally decided by human beings no matter how much data might be accumulated or how many abstruse theories might be thrown into the mix.

"Sceptics," Shermer concludes, "change their mind. Deniers just keep on denying." But most of those I've met and heard from who deny the connection between HIV and AIDS were converted from a prior acceptance of such a connection, whereas "sceptics" like Shermer just keep on denying the plain evidence that has accumulated over more than two decades since that connection was first asserted on the basis of highly dubious inferences.

Shermer's nutshell illustrations of "True Disbelievers" further illustrate his ignorance of what he presumes to write about. AIDS denialists, he says, call themselves "AIDS truthers"! Utterly wrong, as the most rudimentary attempt to fact-check would have discovered. We call ourselves AIDS Rethinkers or HIV Skeptics. "AIDStruth.org," by contrast, is the website of the most intemperate vigilantes for the mainstream view.

Debora MacKenzie, the *New Scientist* correspondent in Brussels, contributes a piece that is even worse than Shermer's. As an example of "denial" she cites the proposition that the swine flu pandemic was a hoax; yet it has become quite plain that the dangers of swine flu were vastly overstated and that no pandemic

eventuated despite the lack of widespread vaccination. She notes that "denial finds its most fertile ground in areas where the science must be taken on trust." This from a correspondent for a science journal? When and why should any "science" ever be taken on trust? And, MacKenzie holds that "all denial is essentially the same"; when in reality one can only judge any specific issue on its own merits and the arguments over evolution, human-caused global warming, HIV/AIDS, Big Bang, etc., have to do with quite different sorts of evidence and different reasons for doubting the mainstream dogmas. MacKenzie cheerfully cites a vaccinologist who diagnoses the mental and emotional character of vaccine deniers, as though he were a psychologist or sociologist. She asserts that Seth Kalichman spent a year infiltrating denialist groups when he did nothing of the sort (Bauer, 2009b). Many denialist movements "originate as cynical efforts by corporations": an apt description rather of HIV/AIDS activist groups, most of them funded by pharmaceutical companies and which campaign for widespread use of antiretroviral drugs. "[D]enial is often driven by an overtly political agenda"; but then, of course, so are "mainstream" assertions. That "HIV deniers . . . have massive but mysterious funding" will come as a shock to us "HIV deniers" who have yet to see any of it, say, as we pay our own way to conferences while pharmaceutical companies pay mainstreamers to go to their conferences. MacKenzie also keeps suggesting a commonality among all "denialisms," namely, conservatism, while paying the usual lip-service to not committing that innuendo of guilt by association.

Jim Giles's essay, "Giving life to a lie," follows the usual course of simply assuming the mainstream consensus to be always right, fleshing that out with such banalities as "we seldom bother to check the veracity of what we are told," which actually describes precisely what Giles himself and other unthinking groupies of mainstream views do habitually.

The last essay, by Michael Fitzpatrick, promised to be different: "Don't mention the d-word." Labeled "Opinion"—unlike the other essays!—this "argues that branding your opponent a denier is a convenient way of ducking difficult questions." YES! At last an independent and thoughtful piece!

Indeed, Fitzpatrick criticizes Michael Specter for his assertion that denialists "replace the rigorous and open-minded scepticism of science with the inflexible certainty of ideological commitment" and points out that "the concept of denialism is itself inflexible, ideological, and intrinsically anti-scientific . . . used to close down legitimate debate by insinuating moral deficiency in those expressing dissident views. . . . crying denialism is a form of ad hominem argument." Bravo!

I wish Fitzpatrick had stopped there. Unfortunately, he continues that

"the popular appeal of pseudoscience is undoubtedly a problem," and as one example cites Peter Duesberg's claim that HIV doesn't cause AIDS, and makes the following errors about that:

- that Duesberg couldn't substantiate his hypothesis, whereas in reality he has documented it copiously;
- that his supporters include "disaffected scientists, credulous journalists, charlatans, quacks and assorted conspiracy theorists and opportunistic politicians." No doubt there are some of those, but defenders of the mainstream also include demonstrably some "credulous journalists, charlatans, . . . assorted conspiracy theorists and opportunistic politicians";
- that mainstream scientists made "a comprehensive rebuttal of Duesberg," albeit only five years later. But where is that rebuttal? Fitzpatrick doesn't cite it, and we dissidents haven't seen it despite innumerable requests to mainstream-adherents that it be cited to us.

To cap that off, Fitzpatrick cites as authoritative, Kalichman's book.

But perhaps his essay is redeemed by its last two paragraphs. Using the terms "pseudoscience" or "denialism" amounts to labeling certain views as "a secular form of blasphemy," and it is "illiberal," "intolerant," "ineffective." "What we need is more debate, not less." Amen.

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ESSAY REVIEW

Reflections on Frederic Myers' Romantic Psychology

Immortal Longings: F. W. H. Myers and the Victorian Search for Life After Death by Trevor Hamilton. Imprint Academic (Exeter, England), 2009. 300 pp. £16.96. ISBN 9781845401238.

History is what contingencies allow us to record and remember. Great things from the world of art and thought are probably routinely lost in time, vanished without a trace, or lying in dustbins, unnoticed and forgotten. It is often one person who pulls a genius from oblivion, as Thomas Wentworth Higginson seems to have done with Emily Dickinson. Melville's *Moby Dick* almost vanished before it was rescued from oblivion. Sebastian Bach's work hovered at the edge until Felix Mendelsohn recalled him to music lovers. The name and the work of Frederic Myers have by no means vanished from educated consciousness; in most history books, however, reference to him is rare. Outside the small community of psychical researchers, Myers has been largely forgotten. Now and then he was acknowledged in the twentieth century; Colin Wilson wrote a chapter about Myers' "forgotten masterpiece," *Human Personality and Its Survival of Bodily Death* (1903). Andre Breton called attention to Myers as one of the inspirations of Surrealism, which relies on automatisms, Myers' great domain of expertise. And importantly, Henri Ellenberger gives a good account of Myers' role in the discovery of the unconscious (Ellenberger, 1970). Myers introduced the writings of Freud to the English-speaking world, discussing his work and publishing an important essay of Freud's on the unconscious. The new century has produced two major books about Myers, one that reviews mainstream psychology in light of Myers' ideas and data, in considerable detail, and concludes that by comparison mainstream psychology looks deficient and inadequate (Kelly, Kelly, Crabtree, Gauld, Grosso, & Greyson, 2007).

The second volume is the subject of this review, the first full-length biography of Frederic Myers, by the historian Trevor Hamilton. We are indebted to the author for placing the career of Myers in the context of his social and historical world, a period of convulsive transition. We get to see Myers in his early years, his relationship to a rather forbidding mother, his career as a student, as a swimmer and runner, and as a poet who for a while had grown a national reputation; his ardent friendships with fellow researchers, a dalliance with homosexuality; his upwardly mobile marriage to Eveleen Tennant, who

was brilliantly portrayed by artist John Everett Millais; his previous, strangely Dantesque love episode with the married Annie Marshall who, like Poe's Ligeia, dies and becomes the supreme icon in his inner sanctum, becoming perhaps the chief force that drove him to found a new science.

Hamilton is even-handed and fair-minded with a large quantity of controversial material. Myers drove some writers to attack him unfairly. When Myers' full biographical statement, *Fragments of Inner Life*, was published in 1961, Archie Jarman mounted a campaign of slander against Myers, arguing that Myers, the married author of "Honor," made Anne Marshall pregnant, which is what caused her to commit suicide. This was false, and Hamilton lays out the evidence; detailed, referenced discussion puts the issue in perspective. Much of the biography centers around Myers' work: his investigations of spiritualism, encounters with mediums; his role in founding the English Society for Psychical Research (SPR) in 1882; his networking with scientists of high caliber: William Crookes, Oliver Lodge, and William Barrett, philosophers Henry Sidgwick and William James, and psychologists such as Charcot and Janet, and Richet the Nobel Prize–winning physiologist. Myers was not a reclusive eccentric genius like C. S. Peirce but an inveterate socializer, traveler, and promoter who conducted numerous first-hand investigations, spoke in public, and presented his work at international science conventions (at a time when the internationality of science was coming into its own).

Chapters cover his personal and literary life, the founding and personnel of the SPR, the scientific ratification of telepathy, the crucial work on automatisms and the "multiplex personality," research on hypnotism, and so forth. Chapter Six concentrates on some of the great mediums, the uncelebrated heroines of this Victorian new science of the soul. The next chapter examines some hurdles the new science had to face in haunted houses and in the personality of the Miss Goodrich Freer. The rest of the chapters consider Myers as a psychologist, the scientific status of Myers' project, and finally his legacy—still, an open question, unfolding.

Hamilton provides a reliable, often insightful, review of Myers' world, his social, intellectual, and amorous life, his mission to create a new science of spiritual consciousness, and his conviction that he had proven, at least to himself, that survival was a fact. Myers fought against the prevailing tide of reductive materialism for the increasingly suspect concept of postmortem consciousness. He seems to have had a precocious metaphysical imagination. His mother recorded a conversation with him at age five about heaven and hell. "But can you tell me Mamma, why God made only two places—one so very good as heaven, & the other so very bad as hell—& why not another, not *quite* so bad, for those who are a *little* good?" (p. 12). How thoroughly sane the five-year-old Myers sounds compared to so many adult fundamentalists who

promote an unforgiving moral dualism. Besides sanity, Myers possessed an intensity and sensitivity that sometimes worked against him. In *Fragments of Inner Life*, Myers recalls a shock he had when seven or eight years old. His mother, in one of their philosophical discussions, suggested that wicked people might simply be annihilated at death in lieu of being installed in hell. "I remember where I stood at the moment," he writes, "and how my brain reeled under the shock" (p. 7). It was the first time he imagined the possibility of annihilation awaiting us after death. As a young man, he underwent a more profound disillusionment, passing through different philosophical moods, Hellenism, Christianity, Agnosticism (all found wanting), wrote poetry to console himself, and continued to suffer from want of "evidence." Thus he lapsed into a "dull pain borne with joyless doggedness, (which) sometimes flashed into a horror of reality that made the world spin before one's eyes—a shock of nightmare-panic amid the glaring dreariness of day" (p. 30). This is quickly followed by an equally dark sentiment, worthy of Leopardi: "In that foreseen futility of the life of individual and race, sympathy itself seemed a childish trifling thing with the universal despair." This mood of nihilism, of existential depression, was coeval with the "first flush of triumphant Darwinism." Myers felt himself being swept away by the "camp of negation." A new constellation of active ideas and forces was rising: "It must be remembered that this was in the very flood-tide of materialism, agnosticism—the mechanical theory of the Universe, the reduction of all spiritual facts to physiological phenomena." This existential crisis became the matrix of Myers' conception of a new science—a science dedicated to exploring the limits of consciousness that came to be known as psychical research. The shock of disillusionment drove him to look at psychic phenomena as the one residual thread that might lead back to the fountain of spiritual re-enchantment.


Eveleen Tennant-Coombs Myers

Hamilton reminds us on more than one occasion that Myers was a snob and (although not bellicose) comfortably imperialist in outlook. Myers doubted that the lower classes could be relied on to give trustworthy testimony; so, we should not look to him for insight into the socially ravaging consequences of his crusade against materialism. His interests were private, esthetic, spiritual, (and as we shall see, romantic); so one is advised to turn to Dickens or William Blake for insight into the suffering of the lower classes.

Chapter Six dwells on Myers and the mediums. In this new science, as conceived by the English founders, *other people* become the instruments of knowledge, the vehicles of insight and perception, and sometimes of deception

and seduction. It was William James who discovered one of the great mental mediums, Eleanora Piper. Confounded by her intimate knowledge of his family life, James was convinced of her powers, and enlisted her to cooperate with psychical researchers such as Myers, Lodge, and Hodgson.

Forming the right kind of relationship between medium and researcher was key to the progress of psychical research. More than just critical intelligence is required. Since we are talking about human beings and not machines, emotional intelligence is also required. Based on extensive experimentation, Kenneth Batcheldor (1979) has written about the special group dynamics that facilitates positive results in PK studies. Among the founders of psychical research, Myers (I would have to say implicitly) pursued a conception of science in which the balanced fusion of intellectual and emotional energies is required of investigators. (In this context, the inquisitorial language of the "investigator" and the "investigated" needs to be carefully reconsidered, especially in view of our increased understanding of the omni-influence of language. This implicit (sensitive to the nuances of emotion) methodology is crucial in working with mediums. Of course, any conception of science that recognizes feeling as part of the instrument will be suspect to the practitioners of sciences such as astronomy and chemistry, which are sciences of dead matter. In a science of living matter and human behavior, we must recognize the inevitable role of feelings and intuition.

Unfortunately, relationships between mediums and scientists were sometimes difficult because of clashes of temperament. For example, it must have been difficult for Myers and his refined Platonic eroticism to deal with the somewhat gross and forthright Eusapia Palladino. Eusapia was the one medium who different scientists kept returning to, and closely studied. Myers' and Sidgwick's first encounter with Palladino produced positive results, and Myers never backed down on his initial findings. Myers' feeling functions might occasionally zone out, but he also had a powerful intellect and a well-honed Victorian "will power." Hodgson, however, flush from his (alleged) triumphant exposure of Madame Blavatsky's frauds, was convinced merely from reading the reports that Myers and Sidgwick had been imposed upon by Palladino. Hodgson thought Myers went soft on Eusapia.

Clearly, the English investigators found it hard to work with Palladino. No doubt cultural differences were in play, for the highly cultivated and fastidious English found it difficult to be with the short, stout, homely, vulgar, uneducated Italian orphan who was overtly sexual in her manner and (God save the Queen!) who cheated. Eusapia's Continental investigators all knew that under certain circumstances, she would try to bring about effects with her hands (it was largely involuntary, occasionally playful, or just spite against ennui); they also knew that her best performances were typically executed under the most

stringent controls. The Continentals realized they had to adapt themselves to the vagaries of Eusapia's personality—as if she were, say, an opera star.

Myers realized the importance of the Palladino phenomena, and decided to lodge the medium in his house where he could observe her extraordinary effects. The prospect of this did not appeal to Myers and certainly not to his wife, Eveleen. Hamilton reports that although they were kind and hospitable, and duly astonished by Eusapia's paranormal manifestations, Frederic and Eveleen were terribly "bored" by the unsavory foreigner. They did their best to keep her occupied but at a safe distance; the Sidgwicks, in their home, fed her in the servant's quarters.

I am surprised by the bored indifference to the personality of the medium they were studying. Sidgwick is quoted as saying, "It [spending time with Eusapia] will be rather a bore, and I fear, tiring to my wife" (p. 215). In one sitting with Eusapia held at Richet's Ile Roubaud where conditions were tight, Myers witnessed some extraordinary phenomena, for example, the materialization of John King's hand ("a big, five-fingered, ill-formed thing it looked in the dusk" (p. 214). Myers wrote to Eveleen about this: "All so wonderful! Eusapia herself an intolerable bore" (p. 215). Granting the difficulties with English, and granted she was not up to the aesthetic or intellectual standards of the English aristocracy, Myers and Sidgwick might have been curious about the history, the beliefs, the inner world of the person who was producing these "wonderful" effects. It was as if their sole wish was to observe some strange kind of miracle-making machine, rather than behavior that could be correlated with a unique inner life.

The Continental investigators had the more sensitive and constructive approach to Palladino. Perhaps the best single book on her and also the keenest in understanding her peculiar psychology was by the American Hereward Carrington (1909). In 1908, Carrington, with W. W. Baggally and E. Feilding, carefully tested Eusapia in Naples, with ample positive results. Skilled in conjury, Carrington knew all about Eusapia's tricks. And yet, according to their unanimous testimony, the tighter the physical controls, the more extraordinary the phenomena. Now and then she unconsciously or perversely employed some legerdemain, and produced effects that were clearly pedestrian by comparison with her genuine, more spectacular feats. "We discovered that the more rigorous the control, other things being equal, and the greater the contact with the medium's body, the better the results" (Carrington, 1909:310). Most of her investigators knew how to handle her; as performer, she exhibited the quirks of a prima donna. She was practicing her art (Eusapia was famous on several continents), but she could not perform in a vacuum; she needed a responsive audience and other actors to play their parts.

Maxwell blamed Hodgson for Palladino's failure: ". . . he and his friends

were responsible for her frauds, and almost wholly responsible for the failure of the experiments. They appear to have neglected the psychological side of a medium's role, and forgot that a medium is not a mechanical instrument" (Carrington, 1909:55). The proper procedure was to gain the medium's confidence and sympathy and try to establish rapport. Subtle human variables, moods, and attitudes are crucial to results in this field of research. The challenge is how to effectively combine the requirements of science with due appreciation of the delicate dialectic of human relations. The latter needs to be looked at more closely.

In the course of Hamilton's account of Myers' work, two important themes come up, psychological automatisms and multiplex personality. Myers' range of interests was immense, but these two were central for him. Although the spectrum of his research topics led insensibly to the issue of post-death, automatisms and multiplex personality are areas of research with independent value. They represent possible domains for further development in their own right, and lead us toward exploring the depth of our present being rather than the extent of our future being. We could say that like Columbus, in seeking the most expeditious route to the Other World, Myers stumbled on new worlds that beg for exploration.

Myers was convinced that a more comprehensive and powerful self was in principle open to human consciousness. This greater form of potential consciousness he stipulated to be the *subliminal self*. Myers inferred this enlargement of the concept of the self from thousands of case studies that illustrate experiences of expansion. Human evolution, he speculated, was about exploring the little known world of the subliminal mind; shamans, poets, and prophets have in the past led the way—Myers was convinced science had to pick up the gauntlet.

In 1933, Andre Breton, a physician and leading theoretician of Surrealism, wrote: "Among Freud's antecedents I continue to think that, in spite of unfortunately widespread ignorance of his work, we remain more indebted than we generally believe to what William James so aptly called the *gothic psychiatry* of F. W. H. Myers" (Breton, Eluard, & Soupault, 1997). Surrealism's debt to Myers is twofold. First, there was the emphasis on new forms of "nondirected expression," in short, automatic writing, drawing, and other forms of involuntary expression. The second point is more radical. For Breton the surrealist project entailed a revolution of consciousness; in brief, its stated aim was to somehow achieve a concrete fusion of dream and reality. This concrete fusion, in Myers' language, was the essence of creative genius; a state in which supraliminal and subliminal mental life interact, interfuse, and regenerate each other. This connection with Myers' psychology raises surrealism from the status of just another art style or movement; there are possibilities here for further development.

The second idea often discussed by Myers I want to mention as containing the seeds for greater development is his idea of the multiplex personality. A close student of Janet's work on psychological automatisms, Myers invented the term "secondary personality" to describe what may seem to emerge during emotional crisis, hypnotic induction, or mediumistic trance: an intelligence, a voice, a set of talents and mannerisms, phenomenologically different from, and sometimes functionally superior to, one's customary personality. Myers understood that the formation of these new patterns of personality might be pathological (dissolutive) *or* something new, *more* functional, or (what he called) *evolutive*. In short, going against the prevailing tendency (Freud, Janet, etc.), he chose to *de*pathologize the notion of multiple personality; instead, he saw these multiplex manifestations as opportunities to recreate the personality. Myers, quite apart from pathology, suggested that normal human beings could learn to awaken and assimilate new creative elements from the subliminal mind, while also learning to discard, or, at any rate, dismantle the old and harmful traits. Myers' view of the human personality was not only richer and more many-layered than Freud's or even Jung's, his prospectus for human development was more optimistic and challenging. In an original book by Adam Crabtree (1985), we see Myers' multiplex self come to life in contemporary psychotherapy.

In the broad field of twentieth-century self-help and spiritual transformation studies we may also see evidence of Myers' legacy, as Hamilton notes in some useful detail. See also, along Myers' lines, another recent paper that explores the notion of self-creation in art and therapy (Grosso, 2010). Myers' theory of the multiplex self provides the raw materials for a new type of psychotherapy. It does not set its sights merely upon the goal of adjustment to everyday life; it seeks rather to furnish the tools for evolving the personality toward Myers' regulative ideal of genius as co-ordination of all one's gifts and talents, known and latent. In short, Myers offers an unfashionably romantic, heroic, and optimistic take on what is humanly possible and realizable.

Trevor Hamilton provides a wide-ranging assessment of Myers' career. The impression I got from the author, without it being altogether explicit, was that here is an open (by no means discredited) field that may yet reach something like its hoped-for goal.

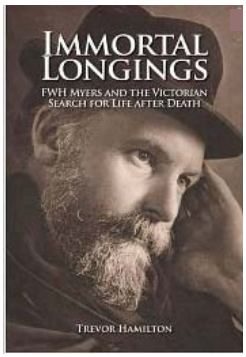
In assessing Myers' work, one question needs to be raised about Myers' idea of the "subliminal self," an idea that has been criticized for different reasons. Myers used it in different ways, always oriented around the possibility of some type of creative advance in consciousness. Myers inferred the idea of a larger subconscious or subliminal identity of self from the extraordinary experiences he investigated. Sometimes he used it as a scientific hypothesis, or more rhetorically as an image with psychically releasing effects. It could be formulated as the basis of a program for experimentation. For me the concept of

a *subliminal self* is a reminder that I am probably underestimating my internal resources. I might also think of it as a rationale for lowering my psychic defenses and becoming more receptive to those untapped internal resources.

One critic Hamilton cites said that the subliminal self was just a throwback to the guardian angel. There is this truth in the remark. The subliminal self, understood as the undefined storehouse of human psychic potential, is a construct that allows us to understand how people might interpret paranormal events as the work of guardian angels. Benvenuto Cellini was in prison, and in despair he set up a device to commit suicide when something physically stopped him and appeared in the guise of a beautiful angel. Flournoy argued that this event was explainable in terms of a teleological automatism produced by Cellini's subliminal mind (Flournoy, 1911). If this is correct, we have here the basis of a new psi-mediated hermeneutics for interpreting various "miraculous" phenomena. In my opinion, such an approach to interpretation would serve to deconstruct the attached religious myth but preserve the transcendent character of the experience.

Myers' subliminal self also has experimental potential. Myers understood scrying, dowsing, automatic writing, drawing, and speech as techniques for lowering the supraliminal threshold and facilitating the influx of subliminal consciousness. It is by means of more careful penetration into realms of the subliminal mind that we can begin to assess the utility of this theoretical construction of Myers. As an overly abstract, somewhat nebulous idea, the explanatory value of the subliminal self may be slight; but as a hypothetical springboard for experimental exploration of exotic regions of mental life, the concept could prove to be useful. Its value is not a given, but something we have to be clever enough to use in a way that becomes valuable. Something like this seems true of any scientific or speculative hypothesis; we have to be ingenious at testing it to reap its potential benefits. It seems especially so in this hypothesis. In Myers' romantic psychology, every experiment is an initiation; rather unlike normal science, it must be prepared to honor (and benefit from) the rare, the exceptional, the singular, perhaps the unrepeatable. We will never know, or be able to assess, what convinced Myers of Anne Marshall's survival. But it was of crowning importance to Myers. The Romantic psychology of Myers is a psychology of creative breakthrough, of singularities, of evanescent events; of necessity it is awash in the whirlpool depths of subjectivity.

There is another basic question we have to ask about Myers' romantic—or shall we say quixotic?—quest to solve the riddle of life after death with the aid of science. Trevor Hamilton points out that the intense research, and new science that he helped launch, did leave *him* with the conviction of survival. Myers went to his death fully convinced he would survive and meet Annie Marshall in a next world. William James witnessed the death of Myers, and spoke in glowing



terms of the man's courage, confidence, and apparent joy. Myers' death in a way surpasses Socrates'; Myers died beautifully in the pain of illness; Socrates from a painless poison.

But, beyond personal heroics, did Myers persuade—or even make a strong impression on—the scientific world? We have little hard data on this, but we can be sure that the research has not made much impression on the scientific world. But that is not because the evidence is lacking; it is because the evidence is ignored, or dismissed, more or less a priori by the prevailing scientific world.

We can distinguish four possible positions on the survival question.

1) Conscious survival is not a coherent idea; there can be no evidence for it; it is impossible.

2) Survival is logically possible, but there is no good factual evidence for it.

3) There is good evidence for survival, but it is not compelling.

4) The evidence is compelling; not to affirm survival is irrational.

I think most critically informed people believe 2) or 3), though I am sure some people subscribe to 1) or 4).

In my view, the evidence en masse from Myers and company supports 3): There is evidence, but it's not compelling. Position 3) is two steps forward from the null start point. That should count as something. Myers also advanced the survival hypothesis by helping to create a scientific society designed to investigate phenomena that directly and indirectly relate to survival. This, in turn, furnished a cornucopia of counterexamples for the deconstruction of materialism. While we cannot say that Myers (and his progeny) have made much impression on the scientific consensus about life after death, the kind of systematic research he launched provides an enormous variety of psychophysical phenomena that render the metaphysical conceit of materialism very difficult to sustain. There is certainly something romantic about the Davids of psychical research battling against the bloated behemoth of materialism.

As to the romantic coloring of Myers' project, some further comments on the following seem in order. Before marrying Eveleen Tennant, Myers had a profound emotional encounter with Annie Marshall, a troubled married woman. To savor the uniqueness and complexity of this story, read the pertinent chapters in Hamilton's biography. Suffice to note that Myers was smitten by this woman in a way that became a turning point in his life. In *Fragments of Inner Life*, he wrote: "In 1873 there dawned upon me a new knowledge of what divineness can lodge in a woman's soul" (Myers, 1961:17). The effect of meeting this woman produced a "buoyancy which lifts beyond the clutch of fate; the sheer exultation that in the Universe such a creature could breathe and live. Then,

as love grew . . .” (Myers, 1961:17). The two of them sustain an on-and-off relationship for about three years, honorable and consciously modeled after the Platonic ideal, or so one gathers from written accounts, when suddenly an ugly fate intervenes and the unfortunate Annie Marshall, under general family pressure (as Hamilton suggests), commits suicide.

This dramatic and extraordinary loss propelled Myers’ quest for evidence of survival. It was no abstract possibility of knowledge that lured him onward in his research, in his wish to achieve certainty. It was intensely personal. Myers was passionate about reuniting with Annie Marshall. Hamilton suggests that Myers’ essay on Mazzini reveals something of Myers’ feelings about himself. Invoking Dante as a model of love that will not be crushed by contingency, Myers wrote after the death of Annie: “. . . a love like Dante’s . . . grows more pervading through self-control, and more passionate through the austerity of honor, and only draws a stronger aliment from separation, anguish and death” (Myers, 1921:281). Through the eyes of cold reason, of course, this can only appear as madness. In a letter to Sidgwick, Myers even imagines that this lofty honor-shaped form of love will transcend possessiveness. Myers seems to have discussed these ideas with his wife, and suggested to her that in death he and she would meet up with Annie in a place “where no loves are mutually exclusive, but each intensifies all” (p. 56). Indeed, a heavenly prospect. Myers, in short, tried to coax his wife into entertaining the idea of a *ménage a trois* in the world to come.

But Eveleen Myers would have none of it; she was jealous of his intense attachment (however spiritual) to the memory of Annie Marshall and jealous of the mediums who claimed to be in touch with her after death. As we know from Lodge and James, Mrs. Myers tried to interfere with anything involving Annie in Myers’ life becoming public. It wasn’t until 1961 that the original memoir, *Fragments of Inner Life*, saw the light of day. Mrs. Myers could not bear to have her husband’s exalted feelings about the long-deceased Annie published for all the world to pry into. Also, she apparently destroyed the written reports of Myers’ late experiments with Mrs. Piper and Mrs. Thompson, both of whom produced evidence *convincing* him that Annie survived death. Could Myers have convinced himself on feeble evidence in the last act of a desperate imagination? I don’t see how it’s possible for anyone to know. If we had the documents, it would be easier to determine.

Myers’ afterdeath persona lived on for about thirty years in the guise of what are known as the “cross-correspondence” materials. These documents represent a prolonged exercise in group mediumship. According to one interpretation, Myers survived death, exactly as he, on the basis of his research, predicted. If Myers did survive he would want the world to know it; so he would attempt to prove it. Moreover, he and his colleagues whom he presumably joined after

death knew all about the problems of survival research. So maybe they did come up with a new and clever idea. On the other hand, one never knows if the medium(s), combining histrionic talent and ESP, made up the whole survival show. According to the correspondence cases, Myers and company figured out a way to deal with the histrionic subconscious of the medium. The experimenters, residing in the next world, convey information to several mediums pretty much at the same time. They give bits and pieces to each medium, puzzling teasers. It dawns on researchers that patterns of meaning seem to be coming from a single intelligence outside the circle of mediums. To see how that works in detail can only be a difficult challenge, requiring various specialized skills, such as knowledge of classical languages and their literature, along with considerable intuitive, symbol-sensitive, and acutely logical thinking abilities. It is an interesting game these disembodied scholars would play with us. Their meaning at first escapes being noticed. Eventually, the pattern, message, quote, image, reference, etc., become apparent. These were the cross-correspondence tests, and they went on for three decades.

Students of this material disagree on interpretation: one group regards cross-correspondence cases as perhaps the best evidence for survival (for two reasons): 1) by conveying messages proving identity through several mediums, in a way that suggests the operations of an external intelligence and avoids the superpsi unconsciously motivated argument; 2) The evidence of identity, the peculiar classical erudition and linguistic esoterica, seem characteristic of Myers and his buddies now on the other side.

Others take a less optimistic view of this material. The meaningful information that seems to emanate from beyond is the product of imagination, or at best the product of an intricate unconscious group process orchestrated by living mediums; so that the interactive collective unconscious of the mediums is what creates the appearance of an over-riding intelligence. Myers and company only *seem* to be speaking; in fact, there's nobody there. The dead are dead. Hamilton provides enough details for both points of view and skillfully leaves the big question open, whether Myers made it to the other world, or not. One thing is certain: A Myers-persona, a wandering soul, or literary ghost, keeps showing up postmortem in the automatic writings of mediums. One wonders if we are here witnessing certain semi-mythical, ontologically hybrid beings, in the process of being created, entities with dubious status like Moses, Orpheus, or Pecos Bill.

I find it useful to describe Myers as a type—rare and perhaps unrepeatable—of *romantic* psychologist. The word has many meanings, popular ones as in "Isn't this romantic?" followed by "More candlelight, please!" The term is also academically worked over, parsed, and analyzed, describing a literary and artistic movement from the last half of the eighteenth century through the first

half of the nineteenth. Myer was *romantic* in the popular *and* the academic sense. In the popular sense, Myers was an amorous man who as Hamilton likes to remind us truly savored the company of attractive women. In his memoir, Myers reports that he was "converted" to Plato's philosophy of love and death. Myers' Platonizing *eros* taught him to spiritualize and (hopefully) eternalize the passions by focusing on the beautiful. The *Phaedo* teaches that one must "practice for death," in short, learn to liberate one's soul from the passions of the body. In Plato's philosophy of love, one's soul is also draw upward from physical beauty to the realm of the immortals. By worshiping the beauty in other human beings we practice separating our psyches from the more binding passions of the body. The Platonic practice of death and the Platonic ladder of *eros* lead to the same place, perhaps the key to Myers' psychological quest: ecstatic release from the lower passions and the ignominious mortality of the flesh. Divining this ecstatic state as possible, Myers imagined a next world where bliss was an anonymous possession, a feature (as it were) of the very atmosphere of consciousness itself.

Something hinging on this idea came together for Myers in his probably unconsummated love affair with Annie Marshall. He wrote in his brief but extraordinary memoir: "My history has been that of a soul struggling into the conviction of its own existence, postponing all else to the one question whether life and love survive the tomb" (Myers, 1961:36). Myers here refers to "love" in a general way; but we know from the *Fragment* that he has one particular love in mind, the love that taught him how the "divine" could lodge in a female body. One sees why Mrs. Myers fought to prevent this memoir from reaching the public. She must have been humiliated at the thought of it being published. The reason was not to prevent the public from supposing that Myers was carrying on with a married woman. The hurt would have been deeper and subtler. Myers describes a love for Annie that, however "honorable" and "Platonic," might make the most secure, self-possessed woman furious with jealousy. It's clear from his prosaic utterances and his poetic effusions that Myers' whole life work was built around the hope of reunion with his deceased lover. This linkage of high-minded love with death is of course an old romantic motif.

One passage would surely have galled Eveleen: where he reviews his life, marriage, family, splendid home, and exquisite Lake District—his whole world—as *not* where his heart or destiny truly lay. His true hope is "elsewhere," in the next world, the world after death, basking in the glorified presence of Annie (whom he calls Phyllis), his divine inamorata. She it was who inspired him to believe in Platonic love as the *eternal* begetter of beauty. Myers actually had counted the number of days he and Annie had met face to face (426, to be exact), days that to him were nothing less than a "prelude of Love's unimaginable

day" (Myers, 1961:40). How painful it must have been for Eveleen to read this part of her husband's memoir.

In calling attention to some romantic motifs driving Myers' complex personality, I in no way mean to imply that they undercut the intellectual virtues of his work. Far from undercutting, I believe they inspired, propelled him forward, and made him more daring and dauntless—but not less critical. I disagree with Hamilton who cites "narcissism" as possibly tainting Myers' scientific intelligence. I think the evidence carried him to his conclusions, not just his hopes or desires (which doesn't rule out that he was wrong). Still, we might ask: If Myers was so passionate about proving survival, can we trust his judgment? My reply: Myers would have been a Spiritualist if he were merely an enthusiast and could be satisfied with mere faith. In fact, he chose the unemotional, witheringly logical Sidgwick to join him in his quest for scientific knowledge in the great questions. Myers and Sidgwick both thought science was the last hope of saving some remnant of their collapsing spiritual universe. Myers was moved by his metaphysical passions; but he was also weaned on the Greek and Roman classics. And he had a snobbish sense of truth and honor he couldn't get rid of even if he tried. Aggravated by Sidgwick's corrosive questionings, he recalls periods of doubt, the thought that survival of death is an illusion. He resigns himself to his duties and tries not to think about "the blackness of the end." He then adds, "As I have implied, the question was for me too vital to admit of my endeavoring for a moment to cheat myself into a false security" (Myers, 1961:41).

My sense is that Myers' romantic science bifocally honors the rules of reason and as well the depths and heights of feeling. (Most of us favor one or the other.) The romantic domain of consciousness embraces intuition, feeling, sentiment, intense subjectivity; ecstasy, rapture, manic-depression—all the polarities of mental experience, all the possible altered states: genius, artistic inspiration, psychosis, paranormal group dynamics, dreams and somnambulism, visions and apparitions; all the anomalies, and indeed all the ontological outlaws and hybrids. This edgy stuff, the wild flirtations with love and death, is part of the funky tonality of Myers' romantic psychology. (Not everybody will like it—but so what?) It is not less science, it is richer, more complex, albeit elusive, science. It is also democratic science, for it favors the altered, the multiple, the outcast, the outlaw, the different, and the alien.

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BOOK REVIEWS

Dictionnaire des Miracles et de l'Extraordinaire Chrétiens [Dictionary of Miracles and the Christian Extraordinaire] edited by Patrick Sbalchiero. Paris: Fayard, 2002. 880 pp. €59.00. ISBN 2-213-61394-X.

The nineteenth century inherited a previous skepticism, embedded within the rationalistic tradition, in the actual occurrence of the miracles of Christianity. This is what historian William Edward Hardpole Lecky referred to in the opening chapters of the first volume of his *History of the Rise and Influence of the Spirit of Rationalism in Europe* as the "declining sense of the miraculous" (Lecky, 1887). Many intellectuals and scientists seemed to believe that the miracles of Christ, and the healings, levitations, bilocations, inedia, and luminous phenomena associated with saints were stories invented for the purpose of conversion, or had a variety of conventional explanations, such as suggestion, as in the case of stigmata. A later example of this tradition was James H. Leuba's *The Psychology of Religious Mysticism* (1925) where he stated that levitation was due to a loss of bodily sensation giving the illusion of floating.

Similar to the current state of the claims of parapsychology, the study of such miracles from the assumption that the phenomena were real in the sense of requiring more than conventional explanations was left to a small number of individuals, but not to the mainstream. Nonetheless, over the years there have been scholars who have taken seriously the reality of the phenomena beyond its purely historical and symbolic aspects. A nineteenth-century example was *Die christliche Mystik*, translated into French as *La Mystique Divine* (Görres, 1836–1842/1854–1855). This was followed in the next century by works such as *The Physical Phenomena of Mysticism* (Thurston, 1952), and by more recent works such as *Encyclopédie des Phénomènes Extraordinaires dans la Vie Mystique* (Bouflet, 2001–2003). The *Dictionary* reviewed here, although not a study by a single author, is an important reference book about the Christian literature on the topic.

Dictionnaire des Miracles et de l'Extraordinaire Chrétiens is edited by Patrick Sbalchiero, historian of religion, and author of other works such as *L'Eglise Face aux Miracles* (Sbalchiero, 2007). In the Introduction to the *Dictionary*, Sbalchiero refers to the "Christian extraordinaire," or a group of psychological and physical phenomena recorded throughout the history of Christianity. He classifies the phenomena in four groups: (1) Biblical manifestations (e.g., Jesus' virginal birth, and his miracles); (2) phenomena presented by Christian mystics, such as physical manifestations (e.g., stigmata and levitations), phenomena tak-

ing place around the moment of death or after (e.g., fragance, incorruptibility), and mental phenomena (e.g., bilocation, clairvoyance); (3) the "Christian marvelous," consisting of hagiographic accounts not included in the *Bible*; and (4) the diabolical (e.g., possession, and some visions). The *Dictionary* presents more than 800 entries written by more than 230 authors. In addition to many contributions by the editor, there are essays by theologians, historians, physicians, and, to a lesser extent, parapsychologists or individuals with knowledge of the field. They all bring interdisciplinary perspectives as well as a variety of viewpoints centering on the historical and religious aspects of the subject matter. The well-known student of Marian apparitions and other phenomena, Father René Laurentin, contributes the Preface in which he clearly states that the domain in question is one that has been neglected and marginalized. He writes: "For the sciences the 'extraordinary' does not have prestige. It is but an interference of accidental and spectacular causes" (p. xv, this and other translations are mine). The *Dictionary* reviewed here, Laurentin says, is an attempt to find a place for the topic in "scientific order as well as in the hierarchy of theological values, a place that is currently humble and marginal. . . ." (p. xvi). It seeks to throw light on the subject and to indicate in what measure we are dealing with illusions or with something deserving scholarly attention. But Laurentin also represents the religious worldview of other writers in the *Dictionary* when he says: "The supernatural is interior to nature, since it is a gift from God that gives rise to our own existence . . . from the vital extension of 'nature'" (p. xix).

The entries range from short ones, limited to a few sentences, to long ones going for a few pages. A good number of them focus on phenomena of different sorts. These include those written by the editor such as apports, exudation (of liquid substances from dead bodies and statues), fragrance, glossolalia, incombustibility, invisibility, odor of sanctity, prediction, and stigmata. Regarding the latter, Sbalchiero notices that "all genuine cases of stigmatization are accompanied by diverse mystical phenomena" (p. 755). He says that the most frequent ones are ecstasy, visions, and internal locutions such as messages received from Mary and Jesus. Less frequent phenomena, Sbalchiero states, include inedia, odors of fragrance, luminous manifestations, and the faculty of being able to distinguish a blessed object from non-blessed ones.

This tendency for mystics and saints to show more than one type of phenomenon is evident in writings about many figures. For example, in the monumental compilation of evidence for the beatification and canonization of Teresa de Jesús there are accounts of levitations, luminous manifestations, and apparitions, among other phenomena (Silverio de Santa Teresa, 1934–1935). However, this does not mean that all phenomena take place in every mystic and saint, nor that there is a one-to-one relationship between specific phenomena. The latter is illustrated in a list of Christian levitators compiled by Joachim

Bouflet in the first volume of his study of extraordinary phenomena in mystics (Bouflet, 2001–2003(1):67–70). My analysis of the list, which is limited to 66 individuals who lived during the nineteenth and the twentieth centuries, shows that 45% of them had stigmata. While this shows some overlap, there are certainly many cases where levitation did not coincide with stigmata. Hopefully, more detailed quantitative analyses may be conducted in the future with a large number of mystics and saints to increase our empirical knowledge of the patterns of interrelationship of these and other manifestations.

Essays about phenomena by other authors include bilocation (Philippe Wallon), clairvoyance (Paul-Louis Rabeyron), ecstasy (Philippe Lemairie), elongation of the body, levitation, precognition (all by Bertrand Méheust), incorruptibility (Pierre Delooz), inedia (Alexia Levrat), multiplication of food (Pierre Haudebert), mystical and near-death experiences (François Brune), hauntings, poltergeists, psychometry, raps (Djohar Si Ahmed), out-of-body experiences (OBEs) (Christine Hardy), telekinesis, telepathy (Pascal Ide), and xenoglossy (Bernard Peyrous). Many of the parapsychology-related entries are focused on old research (such as the one on precognition), and barely present any research findings (telekinesis and telepathy). The entry on xenoglossy does not include parapsychological research such as Stevenson's important (1974) study.

The high number of entries about phenomena does not mean that they are the main topic of interest in the study of Christian mystics. For example, Pierre Miquel stated about Christian mysticism: "The Christian tradition has never based sanctity . . . on extraordinary phenomena. Miracles . . . may accompany sanctity, they may confirm it, but they do not prove it" (p. 561). The author of the entry on sanctity (André-Mutien Léonard) made a similar point. He wrote that Christian sanctity "does not require any manifestation of miraculous supernatural phenomena" (p. 698). Léonard argues that sanctity is defined by an extraordinary life. While miracles are part of the life of many saints, they are not an integral part of sanctity.

Many other entries are about mystics and saints. Among them are Francesco d'Assisi (by Jean-Baptiste Auberger), Teresa de Ávila (Bernard Sesé), Yvonne Beauvis (René Laurentin), Hildegard of Bingen (Ventura Sella Barrachina), Juan de la Cruz (Bernard Sesé), Natuzza Evolo (François Brune), Gemma Galgani (Georges Daix), Padre Pio (Jean Derobert), Bernadette Soubirous (Bernard Billet), and Francisco Xavier (Sbalchiero). Furthermore, there are entries about many other topics. These include psychological and psychiatric concepts such as hysteria (Philippe Wallon), religious denominations such as the Amish (Sébastien Fath), phenomena in groups such as the Dominicans (Marie-Ancilla), events such as the miracle of the sun at Fatima (Sbalchiero), fields or areas of study such as eschatology (Guy Lobrichon), religious entities such as Satan (Dominique Cerbelaud), texts such as *Ars Moriendi* (Marc Zuili),

institutions such as the Medical Bureau of Lourdes (Patrick Theillier), and procedures such as canonization (Jean Évenou).

I particularly enjoyed the biographical entries about past students of saintly and mystical phenomena. One of them is Sbalchiero's essay about Herbert Thurston. There are many other students of the subject who need to be remembered but who are forgotten not only because their publications are old but also, in the case of current scholars operating in the English-language tradition, because they were published in languages other than English. I am referring to individuals discussed in the *Dictionary* such as Joseph von Görres (Pierre Deghaye) as well as the following (written by Sbalchiero): Pope Benedict XIV (whose name was Prospero Lambertini), Albert Farges, Antoine Imbert-Gourbeyre, Olivier Leroy, Jérôme Ribet, and Joseph De Tonquédec. Some of these authors, such as Lambertini, Farges, and De Tonquedec, were concerned with the distinctions between truly supernatural, parapsychological, and conventional causes (e.g., hallucinations, suggestion) to explain observed phenomena.

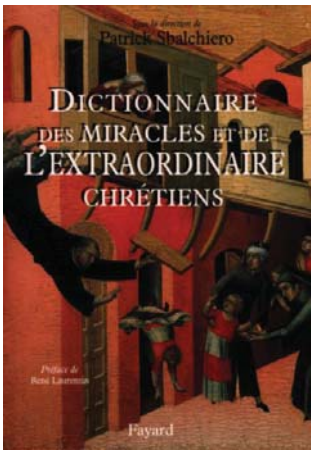
The *Dictionary* also includes some famous psychologists and psychiatrists. Among them are Jean-Martin Charcot (Pierre Morel), Sigmund Freud (Louise de Urtubey), and Pierre Janet (Philippe Loron). Furthermore, there are biographical entries about a few French psychical researchers such as Camille Flammarion (Pierre Lagrande) and Charles Richet (Sbalchiero). But why are so few psychical researchers discussed and why only French ones? There could have been entries about other figures whose writings touched on the relationship between parapsychological phenomena and religion. Two examples are Cesar de Vesme and J. B. Rhine.

Several entries are devoted to parapsychological concepts. Mario Varvoglis has some brief but very clear and informative ones about extrasensory perception, percipient, psi agent, and psi subject. There is an interesting article by Rabeyron about the anthropology of the paranormal. While he mentions classic authors such as Ernesto de Martino, the entry would have benefitted from the inclusion of later work (see, for example the review by Giesler, 1984).

Some of the topics discussed have more than one article. Examples are the entries about miracles. The first article is about general aspects of the concept, by René Latourelle. He sees miracles as a religious wonder, a special intervention of God, a "sign of the presence in the world of its word of salvation" (p. 531).

Răzvan Andrei Ionescu discusses miracles related to the Greek and Syriac Father of the Church, while others discuss miracles in the *Bible* (Hervé Tremblay), in contemporary culture (René Latourelle), and from contemporary perspectives (Pierre Delooz).

Other terms with more than one entry are supernatural and paranormal. The latter has two articles about general aspects (Paul-Louis Rabeyron) and about



spiritual dimensions (Éric Raulet). Rabeyron states that the term paranormal is a complex and controversial concept sometimes associated with the supernatural. This happens, he writes, due to lack of knowledge of parapsychological studies. Rabeyron argues for the consideration of the concept as one representing the "'non-ordinary natural'" (p. 599). Raulet's discussion focuses on near-death experiences. He argues that in our current state of knowledge we cannot affirm that the phenomenon is produced by any known brain mechanism and states that they "escape the definition of hallucination" (p. 599). Paranormal phenomena, Raulet says, deserve to be received with an open mind. They are indicative of the limitations of our current knowledge.

Another example of multiple articles are the essays about apparitions. Topics represented are general aspects (René Laurentin), biblical accounts (Pierre-Marie Delfieux), Marian apparitions in the Middle Ages (Sylve Barnay), and apparitions of saints (Sbalchiero).

There are several entries that I found particularly interesting. Sbalchiero presents a fascinating article about Eucharistic miracles involving the consecrated host. The manifestations are said to show the "supernormal power of the host on life and matter" (p. 540). They include healing and conversion effects, power over the elements, and the induction of such phenomena as ecstasy and levitation. In addition there are instances of hosts that fly, that bleed, that cannot be destroyed, and that show the semblance of Christ on their surface. After a brief discussion of the symbolism behind the host as the "presence of Christ in a sacramental mode" (p. 542), Sbalchiero discusses the theoretical difficulties: "Should we talk about unexplained wonders, or about authentic miracles . . . ? Can such wonders be produced from non-consecrated hosts?" (p. 542). He is also puzzled about the lack of reports of these phenomena from the Eastern Christian tradition.

Sbalchiero wrote other fascinating entries. One is about hemography, or the formation of "figures, drawings or names traced by one or several trickles of blood . . . discharged fortuitously from the skin of a person, without any human intervention" (p. 347). An account in English of some of these phenomena with the Italian Natuzza Evolo appears in Marinelli's (1978) pamphlet. Another entry written by the editor is the one about multilocation, or the "simultaneous presence of the same person in several places" (p. 556). Other interesting entries are those about possession and psychiatry (Michel de Boucaud),

and spiritism and literature (Patrizia D'Andrea). The author of the latter article presents a discussion of mediumistically produced writings. She points out that the ambiguity of the source of the material is the main problem regarding these literary productions. On the negative side, many well-known writings of this sort are not included in the entry, and Ernesto Bozzano's (1947/1998) study on the subject is not even mentioned.

There is no question that this is a very useful reference work. In fact, I would argue it is a unique one. I am not aware of another contemporary book that presents information about the above-mentioned phenomena and different aspects of the Christian literature on the subject. The bibliography about religion, theology, and mysticism presented at the end of most entries, mainly in French, is very useful, covering a variety of publications.

Nonetheless, some of the discussions are somewhat problematic. For example, the entries about OBEs (Christine Hardy) and decorporation (meant as OBEs, by Pascal Ide) do not cover the literature on the subject. It is possible that the writers of the entries were asked to limit their contributions to the topics they do cover. But the end result is a lack of information that weakens the book as a reference work.

While it is clear that most of the *Dictionary* is written from the point of view of Christian mysticism, theology, and hagiography, I believe that the book could have been improved, including more systematic discussions of the relationships between parapsychological studies and religion. Over the years a literature has developed about such interactions (e.g., Berger & Thompson, 1988). Aspects of this include discussions of the importance of psychic phenomena in the development and maintenance of religious concepts, a topic discussed by Andrew Lang in his book *The Making of Religion* (1898). Frederic W. H. Myers argued that psychical research brought an empirical approach to the main tenets of religion: "We do not seek to shape the clauses of the great Act of Faith, but merely to prove its preamble. *To prove the preamble of all religions*; to be able to say to theologian or to philosopher: "'Thus and thus we demonstrate that a spiritual world exists . . . .'" (Myers, 1903(2):297). Part of this effort has been the empirical study of phenomena suggesting survival of bodily death, such as mediumistic communications and apparitions.

Furthermore, some parapsychologists have defended the idea of a nonphysical mind. An example was J. B. Rhine: "The psi researches show the natural human mind can escape physical boundaries under certain conditions . . . Accordingly a distinct difference between mind and matter, a relative dualism, has been demonstrated by the psi experiments . . ." (Rhine, 1947:205). In fact, Rhine went on to suggest that these findings supported some of the tenets of religion regarding the existence of personal agency not recognized by science. In his view: "The relationship of parapsychology to the field of religion is,

theoretically at least, much the same as that of physiology to medicine, or that of physics to engineering" (Rhine, 1947:209).

In addition to the above, other topics deserving more detailed discussion in this work are the history of the involvement of the Catholic Church (Resch, 1989), and of specific clerics (Nicol, 1966), in the study of psychic phenomena. I also believe that a whole entry could have been devoted to the arguments of those—such as De Tonquédec (1955), Omez (1956/1958), and Gonzalez Quevedo (1996)—about the relationship between miracles and psychic phenomena, and their distinction. An example of the latter that could have been included are the ideas French Jesuit priest Joseph De Tonquédec presented in his book *Merveilleux Métapsychique et Miracle Chrétien* (1955). De Tonquédec regarded Christian biblical and hagiographical phenomena as superior and of a higher order than the parapsychological, which he called, in characteristic French usage, the metapsychic. In his view, metapsychic phenomena only showed simple and worldly concerns. In contrast, the miracles of Christ and his followers "are framed in an extremely high moral and religious plan" (De Tonquédec, 1955:48), that of issues such as the salvation of humankind through Christ, and virtues.

In contrast to the low character and limitations of mediums, Jesus was described by De Tonquédec as a "model of calm, simplicity, ease, of serene majesty sure of himself. Jesus does not enters into trance to exert his power. . . . For him, miracles do not require any preparation . . . any exceptional state . . .; he does not tire out nor get fatigued. . . ." (De Tonquédec, 1955:50–51). Compared to non-religious phenomena, the clairvoyance of Jesus and his followers shows constancy and control, as well as a "moral and religious purpose." They seem to be "ruled by a superior intelligence" (De Tonquédec, 1955:60, both quotes). Metapsychic phenomena were seen by De Tonquédec as deterministic, or related to "natural, physical, physiological or psychic causes and condition . . . ." (De Tonquédec, 1955:65). Regardless of how we feel about such comparisons, these ideas could have been discussed in detail in the *Dictionary* because they are part of both the history of the Catholic Church's interactions with parapsychology as well as of attempts to demarcate both fields and to keep the sacredness of the religious perspective.

Some of the entries are problematic because its authors seem more interested in presenting their religious views than in giving us a summary of the various concepts related to particular phenomena. Ide is a case in point when he cites Catholic theological dogma about the soul to support the idea that real decorporation, where the soul is supposed to leave the physical body, does not take place, because "the separation of the soul from the body is irreversible" (p. 212). This same author discusses telepathy and writes that the "immediate action of a human soul over another soul is impossible" (p. 786).

The one-sided content of some of the entries limits the value of this work. Nonetheless, the amount of information compiled by Sbalchiero is prodigious, even considering his statement in the Introduction that the work is an invitation to continue investigations on the subject. There is much to learn from this work not only about specific phenomena, but about the history of Christianity, and a variety of movements, concepts, and individuals.

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UFOs: Generals, Pilots and Government Officials Go on the Record by Leslie Kean. Crown, 2010. 352 pp. $25.99 (hardcover). ISBN 9780307716842.

"Militant agnosticism" is an unlikely pairing of loaded words, but the idea emerges from Leslie Kean's *UFOs: Generals, Pilots and Government Officials Go on the Record*, with a blue-chip gallery of advocates. And it may be the best hope for unclogging America's intellectual arteries of a pathological aversion to a legitimate policy debate since the Air Force officially terminated Project Blue Book 40 years ago.

Kean, a former public radio investigative reporter who worked to expose human rights abuses in Burma during the 1990s, took a radical career jag in 1999 by plunging headlong into the pitiless UFO mystery. After a decade-long struggle for federal documents, of occasional victories in persuading corporate media to give the issue a fair hearing, and of banging against the inertia of mainstream science, Kean has produced the most important book on the phenomenon in a generation. *UFOs* delivers exactly what its full title promises. Hopefully, so-called "skeptics" who refuse to review this book will have the integrity to excuse themselves from the controversy it intends to provoke.

UFOs . . . on the Record is not merely a procession of authority figures reciting personal encounters and attitudes. It mines something far more abiding and insidious—the corruption of science in arguably the most bizarre incarnation of American exceptionalism on the books. With France leading the way, 13 nations from Europe to South America have published previously withheld UFO data since 2004; in 2007, 22 American and international pilots, scientists, and aviation experts signed a petition lobbying for the U.S. to start a new investigation of this global and potentially dangerous reality.

Washington failed to respond to that widely covered conference at the National Press Club. And as its rigid silence confronts mounting evidence for a high-technology component to the phenomenon, America finds itself the source of accelerating international exasperation. When retired Maj. Gen. Denis Letty, who organized a landmark French UFO study published in 1999, asked the U.S. to join its investigation, he and his colleagues received no reply. Retired Gen. Recardo Bermudez Sanhueza, who ran Chile's government UFO project from 1998 to 2002, requested U.S. assistance through its embassy. "To be frank," Bermudez writes, "we've had no response from the United States any time we've tried to enlist its cooperation" regarding UFOs.

This is not an academic exercise. Former NASA senior scientist Richard Haines founded the National Aviation Reporting Center on Anomalous Phenomena (NARCAP) in 2000 in response to concerns over aviation safety.

"According to our statistics," Haines writes, "in an average career of commercial flying, a pilot has about the same chance of seeing a UAP (unidentified aerial phenomena) as he does of striking a bird in flight or of encountering extreme wind shear." Haines goes on to cite three mysterious cases in which worst-case scenarios may have already occurred.

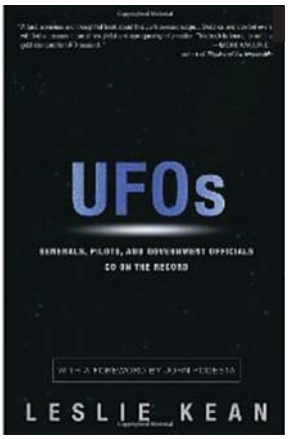
But without government channels to sanction the reporting of in-flight incidents, American pilots operate in a dysfunctional vacuum with implicit career risks. In one of NARCAP's most notable investigations—the 2006 Chicago O'Hare International incident, in which witnesses reported a UFO slicing a circular hole through a low cloud ceiling when it departed—not a single United Airlines employee dared to go on record with the story.

Contrast this with the more professional culture in Brazil, where retired Brig. Gen. Jose Carlos Pereira declares "Our civilian pilots are not afraid to speak up, and they always do, because they don't want to lose their jobs for *not* reporting unusual events." Or with active-duty Chilean Capt. Rodrigo Bravo Garrido, who was assigned by the Air Force to investigate a harrowing encounter reported by an Army aviation crew. Writes Bravo, "It was because of my involvement in this pivotal case that I was asked to study the unconventional topic of UAP in order to graduate from my pilot training program." And it never occurred to civilian airline pilot Ray Bowyer to shut up about his 2007 encounter with two massive UFOs over the Channel Islands, which he reported promptly to British authorities—without suffering repercussions.

Perhaps the more progressive attitudes outside American borders are best summarized by retired Maj. Gen. Wilfried De Brouwer, who staged a press conference in 1990 after Belgian F-16s proved incapable of intercepting triangular UFOs in its airspace. "It is not easy to admit that authorities in charge of air defense and airspace management are not capable of finding an acceptable explanation," he writes, "but in my opinion this is better than issuing false explanations."

In advocating a new government study designed to scrub the stigma of UFOs from American culture, Kean regards classified U.S. government research—the alleged X-Files stuff—as virtually irrelevant to the conversation. "Any behind-the-scenes endeavor would have to be so exclusive, so entirely covert, that in effect its existence would make no difference to our government or country, to the people who know nothing about it, which is essentially everyone." A clean slate, she argues, is the best way to proceed. Enter militant agnosticism.

Kean concludes with some thoughts from two political science professors, Dr. Alexander Wendt of Ohio State University and Dr. Raymond Duvall with the University of Minnesota. Revisiting a largely overlooked coauthored paper



called "Sovereignty and the UFO" published in a 2008 Political Theory journal, they examine the roots of America's refusal to confront the data in a public and transparent forum. And they offer a way out.

"By 'agnostic' here we mean that no position on whether UFOs are extraterrestrial should be taken until they have been systematically studied," write Wendt and Duvall. "Resistance must be agnostic because, given our current knowledge, neither denial nor belief in the extraterrestrial hypothesis is justified; we simply do not know." And then: "To be *politically effective*, however, resistance must also be militant, by which we mean public and strategic. Indeed, purely private agnosticism about UFOs, of the kind that people in the modern world might have about God, does nothing to break the spiral of silence that surrounds the issue and so in effect contributes to it."

That sort of middle-ground activism—between the conspiracy paranoia and the flat-earth ostriches—is responsible for the much-buzzed-about Foreword by former Clinton White House Chief of Staff John Podesta, who also steered Barack Obama's presidential transition team. It also generated book-jacket raves from the likes of theoretical physicist Dr. Michio Kaku, Dr. Rudy Schild of Harvard–Smithsonian Center for Astrophysics, and former Clinton White House Office of Science and Technology Director Dr. Neal Lane. If the U.S. hopes to overcome what Yves Sillard—former director of the French equivalent of NASA—described as its "intellectual blindness," more figures of their ilk will have to step up.

"We ask those on the two sides of this outmoded contest between unwavering believers and nonbelievers to realize the fallacy of both positions," Kean writes, "and to accept the logic, necessity, and realism of the agnostic view. Scientists must disavow the untenable claim that we have no evidence other than eyewitness reports, which are to them—of course—unreliable."

The stage is set for an adult conversation. Unfortunately, that means its fate is now largely in the hands of the ailing American mainstream media, whose sense of identity and purpose has never been in a more acute condition.

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Controlled Remote Viewing for Scientific Investigations: Student Workbook by Michael Van Atta and Susan Van Atta, with Melvin L. Morse. Sacred Mountain Retreat, 2009. $34.95.

Michael and Susan Van Atta are remote-viewing enthusiasts who took a course in remote viewing from David Morehouse (a former military remote viewer) and then—with some assistance from Melvin L. Morse—wrote a how-to manual explaining their practices that they identify as "Controlled Remote Viewing." It is unclear what role Melvin Morse (whose research we have followed with great interest for many years) had in writing the book. Our review of this book is limited to the book itself, and our comments do not address the activities or teachings of Mr. Morehouse, who indeed may find this volume as problematic as we do.

The term *Controlled Remote Viewing* is normally considered a proper name, and it identifies a style of remote viewing that was developed by Ingo Swann in his work with U.S. government–sponsored, remote-viewing projects originally conducted at Stanford Research Institute (later renamed SRI International). A few retired military personnel who worked with Army Intelligence currently teach remote viewing to civilians, and these trainers often identify their style of remote viewing as "Controlled Remote Viewing" or "CRV." The methods of remote viewing that are described in this volume differ dramatically from what was developed for and used by the military, and it is profoundly unfortunate that the authors of this volume have identified their version of remote viewing as "Controlled Remote Viewing." Moreover, researchers committed to the scientific method will likely be very troubled with the approach to remote viewing that is described in this book. Make no mistake about it: Despite the title, this volume should never be used in connection with scientific investigations, in our opinion.

The problems with this volume from a scientific perspective are so many and so profound that a review such as this would be too long if we were to attempt a complete listing. Nonetheless, we point to some heuristic examples below that demonstrate the overall problems that we have with the book.

Few scientists would dispute that remote viewing is a form of nonphysical perception, where "nonphysical" means without the use of the normal five senses of hearing, touch, sight, taste, and smell. Some might also add that it is a nonlocal phenomenon that bears some resemblance to phenomena that occur on the quantum level, such as entanglement. In most scientific studies of which we are aware, remote viewing is described as a method that assists in the transfer of nonphysical perceptual data across time and space. For the authors of this book, however, the term "remote viewing" is used more as a catch-all phrase to refer

to many things of a psychological and/or psychic nature, including resolving emotional issues, canceling "Karmic contracts," visiting dead people and pets, building astral sites, visiting "vortices" to explore energy patterns, etc. This conflicts with the trend in the scientific literature to use terms with more narrow or specific meanings.

These authors also see a dark side to remote viewing that we feel many scientific researchers will find objectionable, even superstitious. According to the authors, one must approach remote viewing with the greatest of care in order to protect one's health. Here is a caution they offer: "[I]f the target is a virus or a harm causing vibrational substance the Monitor should never direct the Viewer to stand inside it or become the target, for that point of view may lead the Viewer to acquire the virus remotely and bring it back to the physical world. NEVER move a Viewer into a sensitive target, or have the Viewer become the target without protection because it may harm the Viewer" (p. 59). They suggest envisioning a 10-foot diameter "white light" around oneself when remote viewing to offer protection from harm's way. They also suggest that a viewer's "guides" (presumably spirit guides) patrol and guard the white light to keep one safe. Readers of this review should note that remote viewing as we understand it is normally supposed to be done "blind," which means that the viewer should have no knowledge of the target when remote viewing. It seems fully contradictory for someone to be considered "blind" to a target when they are told to go through all manner of protective ritual to avoid getting sick and while orchestrating white lights and patrolling spirit guides. Moreover, we do not know of anyone who has ever caught, say, HIV, or even a common cold as a result of remote viewing, although we admit that the authors may be able to find someone who believes he or she did so. With sufficient effort, it is often possible to find someone who believes just about anything.

These authors have similar worries about remote viewing people. According to the authors, if a remote viewer moves his or her "apparitional body" into a person, the remote viewer will assume some or many of the personality characteristics of the person. The authors state, "WARNING: If you become another individual, you bring home their memory, hates, angers, and fears into your psychological make-up. It will harm the Viewer to become the individual. Putting the Viewer inside the individual, the Viewer becomes that individual's personality—speaking as they speak, thinking as they think and causing the Viewer to take up the character flaws of the remote individual" (pp. 53–54). In general, a near-paranoiac fear of the remote-viewing process pops up in numerous places in this volume. It is hard to see how any remote viewer can attend to the matters of recording perceptions in a deliberate and emotionally neutral manner if the viewer is worried about catching both diseases and personality flaws during a session.

The authors do make a spotty attempt to connect with the scientific community in this book. In one instance, they include a complete copy of an article by James Spottiswoode titled "Apparent Association between Effect Size in Free Response Anomalous Cognition Experiments and Local Sidereal Time" that was published in the *Journal of Scientific Exploration* in 1997. The article is essentially used as a chapter in the book.

Among the most troubling procedural aspects of the entire volume is how the authors describe initial remote-viewing training. In their Appendix A, they go through what they call a "first remote viewing experience." They offer an exercise that they suggest will help the student grasp the difference between memory and true remote viewing. A monitor instructs the student viewer to close his or her eyes and then mentally move up 500 feet, and then move to the Washington Monument, staying at a 500-foot altitude. The viewer is then instructed to travel mentally to the San Francisco Golden Gate Bridge, and then to Mother Cabrini's Shrine in Golden, Colorado, all by name. Observations are recorded along the way. Again, the monitor is telling the viewer to go to these places by stating the names of the locations to the viewer. This violates the most basic rule of training in remote viewing of which we are aware, which is to keep the viewer blind to the target at all times. Identifying a target by name invites the viewer's imagination to control everything that happens in the session. Even if a person knows nothing about a target except the name, the name alone will evoke the imagination. For example, assuming that a viewer has never been to Mother Cabrini's Shrine, it nonetheless would not be difficult for the viewer to correctly guess many aspects of this target based on only the name. Impressing the student that he or she can accept such guesses as legitimate remote-viewing data conflicts with training procedures that are standard for scientific, military-derived, remote-viewing methodologies.

Some nitpicky points. The literature of remote viewing that is referenced in this volume is woefully incomplete. The writing is sloppy. For example, the name of one of the authors of this review is misspelled. When the authors reference the physicist David Bohm, they incorrectly identify him with Stanford University. Grammatical laxity is a problem throughout the volume. A list of such troubles could go on and on. The volume should have been sent to a good copyeditor before being published.

Again, if this were a volume written by remote-viewing enthusiasts for unscientifically minded remote-viewing enthusiasts, this could be seen as a fun book. We should always expect that people newly exposed to a phenomenon as interesting as remote viewing will want to write articles and books about it. We would have fewer objections to this book if the title did not identify it as an explanation of "Controlled Remote Viewing" and its application "for Scientific Investigations." In our opinion, this volume is not an optimal approach to

teaching remote viewing, nor should this style of remote viewing be used in scientific investigations without serious qualifications. Indeed, skeptics of remote viewing specifically, and psi functioning more generally, will find easy cannon fodder in using a book like this as an example of unscientific thinking and methodology. The title of this volume also makes it easier for skeptics of remote viewing to debunk the authentic military version of "Controlled Remote Viewing," ignoring the science surrounding the development of the latter and further confusing an uninformed public.

It is not an exaggeration to note that a number of scientists have risked their scientific reputations by conducting research into the remote-viewing phenomenon. Great strides in our understanding of psi functioning in general, and in the remote-viewing phenomenon more specifically, have resulted from this research. The military-derived procedures for remote viewing offer great potential for use in scientific studies. But scientists must exercise care with respect to the types of training and methodologies used in such studies. The volume reviewed here is an example of what scientists should avoid.

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Out-of-Body and Near-Death Experiences: Brain-State Phenomena or Glimpses of Immortality? by Michael N. Marsh. Oxford University Press, 2010. 336 pp. $110 (hardcover). ISBN 9780199571505.

Michael Marsh is a British physician, a mainstream biomedical research scientist who late in his career took the unusual step of obtaining an advanced degree in theology. This book, based upon his D. Phil. thesis and published as an Oxford Theological Monograph, purports to provide an objective and critical examination of out-of-body (OBE) and near-death-experience (NDE) phenomena from these perspectives. Regrettably, it reveals instead a person so committed a priori to these divergent orthodoxies that he is unable to come fully to grips with the phenomena and issues at hand.

The central arguments are clearly foreshadowed in a brief Introduction. He will assess the state of things by examining eight popular books dealing with NDEs and OBEs (which he refers to collectively as "extra-corporeal experiences" or ECEs) and their putative implications for topics such as post-mortem survival, religion, and the nature of the cosmos. These books, which he refers to repeatedly and incorrectly as the "canonical" literature of the field, include two books each by Moody, Ring, and Sabom, one by the Fenwicks, and one by Margot Grey. Note that this is already a rather odd procedure for a supposedly scientific review, given that none of these books is less than twelve years old, and that the relevant literature now includes not only many additional books and book chapters but large numbers of peer-reviewed papers in refereed journals as well.

The bulk of the book will consist of his detailed response to the challenge by Ken Ring and others to provide a comprehensive neurophysiological explanation of ECEs, a task which Marsh clearly regards himself as better-equipped than all previous commentators to pursue. In contrast to previous misguided talk about dying or effectively dead brains, he will offer a "reviving brain" hypothesis which construes NDEs as brief and wholly illusory conscious accompaniments of the disordered and idiosyncratically patterned return to full normal competence of brains that have been subjected to prior cardiovascular trauma or other functional insults. The theological perspective he brings to bear later in the book will complement this analysis by showing that the narratives provided by ECE experients cannot be regarded as reports of genuine spiritual experiences.

In Chapter 1 he mainly introduces ECE phenomena along with the eight targeted books and their authors. However, he also begins to introduce his own very different point of view through brief commentaries on hellish NDEs (emphasizing their diversity, and sudden changes in affective tone), supposedly visual NDEs occurring in congenitally blind persons (which he uses to disparage the quality of NDE interviewing and reporting), and Sabom's celebrated case of Pam Reynolds (which he dismisses for transparently inadequate reasons as "most unimpressive" (p. 26; see also below)).

Chapter 2 provides an additional historical and cross-cultural sampling of ECE reports, apparently designed to emphasize their diversity, cognitive/cultural dependence, and dreamlike character while raising further doubts about the investigative and reporting techniques of NDE researchers. I will comment only that the small sample of cases provided is far from genuinely representative of NDE/OBE phenomenology.

In Chapter 3 Marsh continues these themes, attacking certain interpretations which he thinks the targeted authors have unjustifiably imposed upon their data. He faults Moody and Ring in particular for the suggestion that there is anything

resembling a canonical sequence of events, or a core NDE experience of meaningfully measurable "depth." This attack is largely unwarranted, however. For one thing, the artificiality of Moody's original scheme has long been recognized by practically everyone active in the field. More importantly, the meaningfulness of the concept of a core NDE experience has been abundantly confirmed through the development and subsequent widespread application of Greyson's reliable and valid NDE scale (Greyson, 1983). Greyson's original work has also recently been supplemented by a rigorous psychometric demonstration that this instrument provides a unidimensional measure of NDE depth or intensity with interval-scaling properties (Lange, Greyson, & Houran, 2004). The reality of NDE phenomenology, that is, is neither the unlimited idiosyncrasy claimed by Marsh nor the stereotypy suggested in some early popular books but something in between.

Marsh goes on to fault Ring, Grey, the Fenwicks, Sabom, and anybody else who takes seriously the idea that ECEs contain anything veridical (other than reflections of aspects of an experient's actual physiological state, such as pain or fever), or that they provide meaningful evidence of post-mortem survival, or indeed that anything *could* provide such evidence. These views, in my opinion, reflect the author's near-total ignorance of the larger context in which a truly scientific appraisal of ECEs needs to be framed, and they go to the heart of my dissatisfaction with this book.

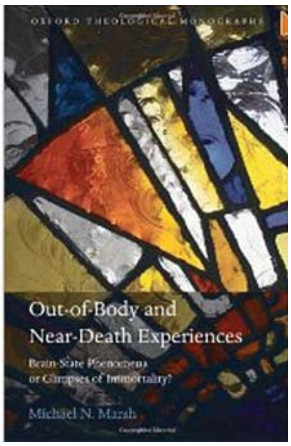
Take for example the many reports of spontaneous paranormal events in conjunction with ECEs. In my view the reality of psi phenomena has been demonstrated beyond reasonable doubt by a century-plus of high-quality experimental, case, and field studies, and what is unusual about ostensible paranormal events associated with ECEs concerns only the circumstances in which they occur. Marsh will have none of this, however; he summarily dismisses the entire history of psychical research (p. 65), and in that light regards the reported psi events as mere "anecdotes," each standing entirely on its own, and none sufficiently well-documented to deserve the slightest interest or respect. Like other determined psi-deniers, he offers alternative explanations for events he thinks he can explain in conventional terms—no matter how far-fetched those explanations—and dismisses or ignores the rest. Thus for example he asserts that Pam Reynolds actually heard the surgical saw (by bone conduction, even though she was already deeply anesthetized at the time), regards the Maria case as nothing but "hearsay," and neglects to mention van Lommel's case of the spontaneous cardiac-arrest patient who reported observing removal of his dentures by a particular nurse involved in the resuscitation procedures (nor does he ever describe or discuss the main results of that important prospective study itself (van Lommel, van Wees, Meyers, & Elfferich, 2001)).

We can certainly all agree that more and better documentation of veridical

psi events in NDEs is desirable, and it is true that attempts at controlled studies have so far largely failed (Holden, Greyson, & James, 2009, Chapter 9), but to dismiss the entire subject as Marsh repeatedly does in contemptuous fashion is surely unwarranted. He also seems unaware that a considerably larger amount of evidence, including some experimental evidence, already exists for veridical events in conjunction with OBEs (see for example Hart, 1954, and Kelly, Kelly, Crabtree, Gauld, Grosso, & Greyson, 2007, Chapter 6). More seriously still, he appears to be almost totally ignorant of the very large body of direct evidence for post-mortem survival itself, deriving principally from systematic studies of trance mediumship, apparitions, and cases of the reincarnation type (Braude, 2003, Gauld, 1982, Stevenson, 1977, 1997, Tucker, 2005).

Marsh's "objective" analysis depends at bottom upon the fact that in his scientific phase anyway he is a conventional reductive physicalist, committed a priori to the view that mind and consciousness are entirely the products of, or supervenient upon, or identical to, electrochemical processes occurring in the brain. He is candid about this in his Introduction (p. xx), where he acknowledges that it is a *premise*—not a conclusion—of his investigations, and numerous statements scattered throughout the book confirm that this is the case. Thus for example he asserts that Pam Reynolds could not have had conscious experience of any kind during the deepest part of her hypothermic cardiac arrest procedure, because "that, of course, is clearly impossible, viewed from any physiological perspective" (p. 25). More generally, nobody can have conscious experiences under conditions such as deep general anesthesia and/or cardiac arrest, and persons who report having had such experiences therefore must have had them at another time (as they awakened, he believes), notwithstanding anything they might report, veridical or otherwise. Similarly, all reported ECEs must be cerebral in origin, he declares repeatedly, simply because the experients *remember* them (pp. 25, 53, 72, 78, 261; but see also Kelly et al., 2007, Chapter 4). This of course all begs the central scientific question whether the standard mainstream "production" model of the mind/brain relation is correct.

From Marsh's conventional perspective, the task is now simply to figure out how the phenomenological properties of NDEs and OBEs might be explained in terms of known or possible brain mechanisms. This he proceeds to attempt in Chapters 4 through 9, and although this exercise constitutes the empirical heart of the book I can be mercifully brief in presenting and discussing it. Marsh is more systematic and thorough than his predecessors in pursuing this task (and this detail is welcome), but in fact very little of what he presents is new, and none of it compelling. The basic strategy, as usual, is to identify physiological agents, processes, or circumstances that result in phenomenological features analogous to features reported in NDEs and/or OBEs, on the presumption that the former must be causative of the latter.



Marsh begins in Chapter 4 by arguing that NDEs *must* occur just before the recovery of full consciousness by a previously disordered brain. In doing so, he simply ignores the large number of NDEs that have occurred in the absence of recognized brain injury or pathology, as for example in near-accidents and brief falls, and those containing verifiable time-anchors placing the experience at points remote from time of recovery, such as the reports of Pam Reynolds (Sabom, 1998), van Lommel's spontaneous cardiac-arrest patient (van Lommel et al., 2001), and many others.

Chapter 5 is mainly devoted to demonstrating the "projective" or virtual-reality character of some perceptual experience, using the phenomenology of phantom limbs as an illustration. Marsh's main point here is that human beings can have vivid and subjectively impressive hallucinations of things that aren't really there, in conjunction with specific types of brain malfunction. This is certainly true, and in fact it is true of ordinary perception in perfectly healthy persons as well, as argued in particular depth by Velmans (2009). It is not self-evidently true, however, as Marsh assumes, that the brain itself *produces* all such experiences. In fact, a large and diverse body of evidence strongly suggests that this conventional "production" model of the mind/brain relation is *false* (Kelly et al., 2007).

In Chapters 6 through 9 Marsh extends his analysis by examining a number of specific candidates for relevant neurophysiological mechanisms. Most of these, contrary to what he seems to think, have been proposed before. Chapter 6 focuses on the temporo–parietal junction (TPJ) and its role in normal and disturbed perceptions of the body and its location in phenomenological space, drawing heavily on the work of the Blanke lab (and consultations with Peter Brugger and Christine Mohr (p. ix)), but ignoring the many significant problems previously identified in this work (Kelly et al., 2007, Chapter 6). Chapter 7 compares NDE phenomenology to that of ordinary dreams and especially to hypnagogic/hypnopompic states arising at the border between sleep and wakefulness. Chapter 8 reviews supposed connections between NDEs/OBEs and disturbances in temporal lobe function, whether the result of overt pathology or various kinds of cortical stimulation. Here Marsh lays special emphasis on the potential of *latent* temporal lobe dysfunction to interact with other pathophysiological events and thus perhaps to explain why some people do have while many others do not have NDEs under what look to be

very similar conditions such as deep general anesthesia and/or cardiac arrest. Note, however, that this also conveniently provides him with a generalized escape mechanism for cases lacking evidence of overt pathology of any relevant sort. In addition, he again overlooks the many significant problems previously identified in this comparison (Kelly et al., 2007, Chapter 6).

In Chapter 9 he examines a variety of further topics already touched upon by the "canonical" authors, including the possible role of endorphins, the physiological basis of "tunnel" phenomenology (where he rejects Blackmore's analysis), blood–gas disturbances such as hypoxia and hypercarbia (which he downplays), anesthetic agents such as ether, nitrous oxide, and ketamine, and reductions of posterior and basal cerebral circulation known to contribute to certain hallucinatory experiences (peduncular hallucinosis). Surprisingly, given his general outlook, he specifically declines (p. 139, Note 30) to discuss psychedelics such as LSD, on grounds of (unidentified) phenomenological dissimilarities to NDEs.

Summing up his exertions, Marsh declares triumphantly (p. 262) that the neurophysiological challenge put forth by Ken Ring (and others) "has been completely neutralized, if not eradicated, by my pursuit and deployment of in-depth neurophysiological explanation and possibility." With all due respect I categorically disagree: In addition to the more general issues already raised above—his blanket dismissal of psychical research, and his generalization to all ECEs of forms of explanation that apply in principle only to some of them—there are additional serious problems in these phenomenology-driven comparisons themselves. First, the individual comparisons are in general very strained, and routinely ignore major phenomenological *dissimilarities*, good examples here being the experiences produced by centrifuging pilots (Whinnery) and TLE or electrical stimulation of the brain (Blanke, Penfield, Persinger). Second, the suggested mechanisms have to get strung together ad hoc in arbitrary combinations, and without supporting evidence, to "explain" the composite, integral character of real NDEs. Finally, and most importantly, none of the suggested neurophysiological models results in experiences having anything like the profound transformative impact routinely associated with genuine, deep NDEs.

It is especially ironic that Marsh overlooks this last objection to his reductive neurophysiologizing, because one of his main claims in the following three chapters, in which he examines NDEs from his newly acquired theological perspective, is that NDE researchers have signally failed to appreciate the impact such experiences have on recipients' subsequent lives! More work can certainly always be done along these lines, but his generalized claim here is simply false (e.g., Holden, Greyson, & James, 2009, van Lommel et al., 2001).

One can almost hear the gears grinding as Marsh makes the transition

from biomedical-scientific to theological critic of NDEs, and much of what he says in these final chapters seems to me not relevant to a scientific appraisal. Nevertheless, there are several interesting connections between these two main parts of his book that seem worth highlighting here.

The first appears in Chapter 10, where he suddenly adopts the viewpoint of Christian theology. Why Christianity should be privileged in this way is not discussed, but in any case it now becomes clear that he is not after all a thorough-going physicalist in the mold of people such as Dennett, Searle, the Churchlands, and most other contemporary philosophers of mind, neuroscientists, and psychologists. We are more than just "packs of neurons"—answering a question first posed but not addressed in Chapter 5—and some form of immortality now seems to him at least doctrinally possible. This subject has a long history in Christian thought, it turns out, involving ongoing controversy between two extreme positions, one favoring end-time resurrection of the body as the appropriate model, the other conceiving immortality as some form of continuing post-mortem existence of minds, personalities, or souls. NDE reports, of course—like the survival evidence generally—tend strongly in the latter direction; and for the same sorts of reasons driving his scientific evaluation of NDEs—at bottom, his acknowledged inability as a committed reductionist to conceive any credible way in which a mind or personality or soul could function in the absence of a working brain—Marsh feels driven to the resurrection model of Christian immortality, and to acceptance of the resurrection of Jesus as literal historical fact.

In Chapter 11 he addresses the further question whether NDEs can be regarded from a Christian point of view as genuine spiritual experiences. He of course wants to argue that they cannot, on grounds that they are nothing but endlessly variable brain-generated hallucinations, inconsistent with each other and with received Christian doctrine. But here he faces a problem: Many Christian mystical experiences, which he accepts as genuine, look as though they too might be explainable in reductive neurobiological terms, and many neuroscientists in fact presume they are (Saver & Rabin, 1997); so what's the difference? To address this, Marsh sets up a straw-man comparison between carefully selected examples of classic deep mystical experiences, as presented for example by William James and W. P. Alston, and "randomly" (?) selected bits of NDE narrative, emphasizing the idiosyncrasy, anthropomorphism, and banality of the latter. This procedure certainly makes garden-variety NDE reports suffer by comparison, but it utterly fails to do justice to the full range of NDE phenomenology, which clearly overlaps in particular with extrovertive mystical experiences of the sorts encountered by ordinary persons under a wide variety of precipitating circumstances (see especially Marshall, 2005). It is true, as Marsh points out, that mystical experiences typically lack certain

features such as barriers and tunnels that occur commonly in NDEs, but other and more significant features—for example, unusual experiences involving light (Kapstein, 2004), the sense of immediate contact with some sort of overpowering higher reality or *mysterium tremendum*, and transformative impact—are common to both.

Marsh's relentless trivialization of NDEs also has repercussions in the following chapter, in which he turns to their consequences for human lives and personalities. For if these experiences are really nothing but meaningless and banal brain-generated hallucinations, as he has argued throughout the book, why should they have the profound transformative power they so evidently do? Marsh never really faces up to this paradox and its implications, acknowledging it merely as "intriguing" (p. 243).

The book ends with an overview and recapitulation of the main arguments, followed by a glossary of technical terms in both neuroscience and philosophy (!), a very useful 29-page bibliography, and a not-so-useful index.

In conclusion, there is certainly some good here: The book is strongly written, and it contains a wealth of physiological information of which this review captures only a few highlights. Marsh also consistently and sensibly advocates for our need to acquire more and better information about what is actually happening to persons undergoing these powerful experiences, and about their effects on experients' subsequent lives. He also advocates—again correctly in my view—against premature and undisciplined speculation about the possible religious and cosmological significance of ECEs.

Despite these virtues, Marsh's analysis remains in my opinion fundamentally flawed by virtue of his pre-existing commitments to both scientific and religious orthodoxy. Indeed, it exemplifies all previously identified ways of evading full engagement with the NDE/OBE evidence plus some novel ones deriving from his added theological perspective. He firmly rejects any possibility of post-mortem survival of mind or personality, as suggested by ECEs, yet he has made no contact with the large scientific literature dealing with psi and survival, and despite his theological interests he is unable even to consider the possibility that alternative non-physicalist understandings of the mind/brain relation could potentially do justice not only to the full range of NDE/OBE phenomenology, but to post-mortem survival and to leading-edge neuroscience and physics as well (Kelly et al., 2007). He is certainly entitled as a private citizen to believe in the resurrection of Jesus as his only hope of immortality, despite the scanty evidence for that event, but he is not entitled as a scientific reviewer to dismiss out of hand the entire mass of far better evidence for post-mortem survival of ordinary humans. Moreover, I deplore his uncharitable and unseemly readiness to invalidate, because they fail to conform to the scientific and religious dogmas he has personally embraced, the life-changing experiences of thousands of his

fellow human beings. Readers seeking genuinely scientific appraisals of NDEs and OBEs, in short, will be better served elsewhere.

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The Purpose-Guided Universe: Believing in Einstein, Darwin, and God by Bernard Haisch. New Page Books (Franklin Lakes, NJ), 2010. 222 pp. $19.99 (hardcover). ISBN 9781601631220.

This review must begin with a disclosure: I have in press a book (Batten, 2011) in which I cover much of the same ground as Haisch does. Our approaches are different, but we have the same basic message: There is nothing in modern science that precludes at least some forms of religious belief. We cite several of the same authors, sometimes the same passages from them, to which we react in similar ways. There are even some of Haisch's sentences that I wish I had written myself; on page 35, for example: "It is intellectually dishonest to discount out of hand that our Universe appears special because, well, it happens to be special." My prejudices in favor of Haisch may be tempered, however, by the fact that we are competitors!

Bernard Haisch hardly needs any introduction to long-time readers of *JSE*, since he was for many years its Editor-in-Chief. Although he was once a candidate for the Roman Catholic priesthood, this book shows that he has been heavily influenced by the Perennial Philosophy, especially as it is described in Aldous Huxley's (1944) book, and by the mystical tradition in all religions. His thesis can be fairly easily summarized: The physical universe does not constitute the whole of reality and is indeed subordinate to consciousness. Citing the Hindu identification of Athman (the individual consciousness) with Brahman (the universal and creative consciousness), he maintains that each one of us is a spark of God. Recent developments in quantum mechanics, he argues, show unequivocally that "reality" is created by our measurements. The properties of fundamental particles do not exist until a measurement is made. As Haisch himself puts it (p. 168): "Consciousness creates reality." Given this interpretation of science, and mystical rather than the organized religion which he has left, he sees no conflict involved in believing in Einstein, Darwin, and God. I can agree that "Consciousness creates reality," if that is not interpreted as our individual consciousnesses doing so. I do not believe that we ourselves create the universe, nor am I completely persuaded that we are "sparks of God." I think that there is a real physical universe independent of our limited minds, even if all we can say of it is what Eddington (1928:291) famously said of the electron: "Something unknown is doing we don't know what."

Although I neither cite Huxley's book nor refer explicitly to the Perennial Philosophy, I, too, argue that there is more to reality than just the physical universe revealed to us by our senses. I find it convenient to use the word "transcendent" to denote those aspects of the universe not revealed by our senses, a word that Haisch sometimes uses, too. The word is not entirely

satisfactory because of some of its connotations, but the same can be said of all other candidate words. Haisch sees evidence for the transcendent in the experiences of mystics, especially in the thought and writing of that great astronomer and mystic Sir Arthur Eddington. (Haisch is kind enough to refer to an article I wrote on Eddington.) Again, I agree, although I would suggest that music and the visual arts also provide us with such evidence.

I have sometimes thought that we need to rescue the word "God" from those who believe in God. Similarly, Haisch points to the distinction made by many mystics between "God" and "the Godhead" (pp. 120*ff*). He is clearly much impressed by the experience of unity with the Godhead that these mystics report. There is, however, another kind of mysticism, the so-called *nature mysticism*, in which the percipient experiences a sense of unity not with the Godhead, but with the whole created order. I rather suspect that Eddington's mysticism was of the nature variety. The evidence is not clear-cut, but a sentence in *The Nature of the Physical World* (Eddington 1928:321) seems to me to support that interpretation.

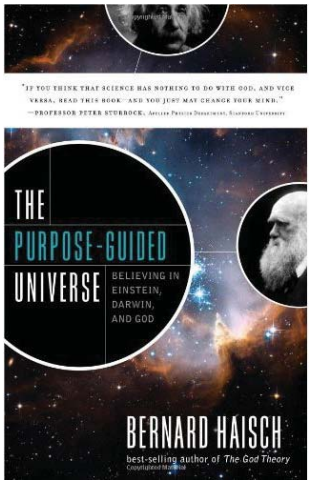
My quotation in the first paragraph of this review from page 35 will alert readers to the prominent role played in Haisch's arguments by the so-called fine-tuning of the universe. He contrasts two possible explanations: Either there are many "universes," some of which will, by chance, be suitable for the development of life; or this universe has been deliberately created to be special by God. He points out, correctly in my view, that the latter explanation is by far the simpler. (A third possible explanation, that our universe is just one tremendous fluke, is dismissed, again I think correctly, as far too unlikely to be taken seriously.) Although they do not affect the argument in any important way, I could not help noticing two technical errors, or at least obscurities, in that Chapter. On page 69, it seems to be implied that the Moon is only 180,000 miles from the Earth, about three-quarters of the correct value, and on pages 75–76 figures are given for the amount of dark matter in the universe that, on the face of it, appear to be inconsistent. Dark matter is said to constitute 25% of the universe on one page, while on the next we are told that there is about six times as much dark matter as ordinary matter. I suspect the first figure takes account of both matter and energy, while the second refers to matter alone, but the point could have been made more clearly.

While Haisch has no difficulty reconciling mystical religion and the Perennial Philosophy with modern science, he states on page 31 that he believes organized religion cannot be so reconciled. Here I do not agree, having managed (sometimes with difficulty) to stay within organized religion, while recognizing its many faults, which frequently include abuse of power. Nevertheless, churches, synagogues, and mosques are often the loci of many good works, and, on the whole, I believe that our society would be the poorer

if they did not exist. The vast majority of us, after all, do not have mystical experiences, however much we may aspire to them, and organized religion helps to fill the gap. Presumably, living a disciplined life and regularly engaging in certain modes of thought, whether we term them prayer, meditation, or spiritual exercises, will predispose people toward the mystical experience, but many who do those things never have the experience, while, as Haisch points out (p. 148), that experience sometimes comes out of the blue to others who have never prepared for it.

Haisch devotes some space to the thought experiment that has come to be known as "Schrödinger's Cat." Schrödinger was no more willing than Einstein to accept the Copenhagen interpretation of quantum theory, and his thought experiment, which he himself described as "diabolical," was intended as a *reductio ad absurdum* of that interpretation every bit as much as the EPR experiment which Haisch also discusses. Walter Moore (1989) gives an insightful account of the connection between the two thought experiments in his biography of Schrödinger. In the experiment, the cat's life depends on whether or not one radioactive atom decays within a given time. The Copenhagen interpretation implies that the cat was neither alive nor dead until an observer opened the sealed box in which it had been ensconced. Schrödinger thought that conclusion absurd. We can only speculate whether or not he would have changed his mind if he had lived to see the modern experimental results to which Haisch refers. Most quantum physicists, however, now embrace the uncertainty, as Haisch does. Moreover, he goes on to say that not only is the cat neither alive nor dead until the observer opens the box, but even the atom upon which the cat's life depends neither decays nor remains stable until the box is opened. In other words, the observer causes the decay of the atom although the observation comes after the decay! This kind of backward causation in time is also discussed by Paul Davies (2006) in the context of the two-slit experiment; Davies goes on to suggest that, similarly, our presence in the universe has caused it to have a history that makes it "fine-tuned" for the emergence of life. Interestingly, in another recent book, J. Scott Turner (2007) also suggests that this kind of backward causation can occur in biological evolution. This is the kind of consideration that leads Haisch to his conclusion already quoted, that "Consciousness creates reality."

Haisch also quotes from Schrödinger's (1944) well-known little book *What Is Life?*, the Epilogue to which reads almost like a précis of Haisch's. In his opening chapter, Schrödinger stated his commitment to a reductionist explanation of the material processes of life and his belief that quantum uncertainty had no biological relevance, both positions disputed by Haisch, yet the two men experienced rather similar spiritual developments from organized Christianity (in Schrödinger's case Lutheran) to an appreciation of



the Perennial Philosophy and of Indian religion with its insistence on the identity of Athman and Brahman. (Huxley's book had not been published when *What Is Life* was first published, but Schrödinger commended it in later editions.) Haisch would probably not entirely approve of my own reaction to this aspect of his book. I feel that the Christian doctrine of the Trinity has something to be said for it, since it tries to hold in balance three incompatible ideas about the Godhead: transcendence, so important in both Judaism and Islam, incarnation in human form, that recurs over and over again in Hinduism, and immanence, important in mystical religion.

I agree with Haisch that some of the criticisms brought by people such as Dawkins and Hitchens against the traditional notions of God are justified. Those who insist on the literal sense of the Bible do not seem to understand the moral conundrums it often poses. If, instead, the Bible is understood as a record of one particular people's evolving understanding of God, those moral problems largely disappear. Once again, Haisch's distinction between God and the Godhead is useful. On the other hand, Haisch borrows the title of his earlier book *The God Theory* (which I have not read) for one of his chapters in this book. I think that this phrase is a little unfortunate. For many believers, God is not a theory but a reality they have experienced. This was particularly true of Eddington and must, I suspect, be true of mystics in general. There is a helpful discussion of this point in a book by Mikael Stenmark (2004).

Another chapter is provocatively entitled "Staying out of Heaven." Haisch does not like the idea of everlasting bliss, maintaining that anything that goes on forever will sooner or later cloy. There is, of course, some ambiguity as to whether "everlasting" and "eternal" mean quite the same thing. Eternity may be more like "timelessness," of which Haisch writes quite approvingly. The traditional imagery of Heaven and Hell does not carry very much conviction these days except, again, to those who suppose that the Bible must be taken literally. That imagery, however, was an attempt to describe the Beatific Vision (or its absence), which surely can be a dynamic process. Still, I sympathize with Haisch's dislike of the idea that one short lifetime—at most about a century—will determine our fate for all eternity. His solution is again to revert to Eastern religions and adopt the notions of *karma* and reincarnation, and to suppose that we shall have several lifetimes until we are eventually reunited with the Godhead of which he believes we are a part. The idea of reincarnation is not without its

attractions and, like Haisch, I have been impressed by the sheer quantity of data amassed by the late Ian Stevenson, much of which has been published in *JSE*. Nevertheless, repeating all the frustrating experiences of infancy and childhood is not a very appealing prospect to me. I hope for an existence after death in which one can continue to develop and to learn almost indefinitely. Of course, one must also expect to atone for what we have done amiss. If the universe has a purpose, it must surely be a moral one—Haisch and I agree totally on that. Even a Hitler or a Stalin, however, should not, it seems to both of us, be condemned to *everlasting* torment. Monstrous as their crimes were, they were necessarily finite. I oscillate between one of two solutions: Either Hitler and Stalin were so evil that they killed off all within themselves that was capable of surviving the death of the body, or third-century Alexandrian Origen was correct in his belief that, in the end, all people would be saved.

Toward the end of his book (Chapter 9), Haisch discusses the future importance of the physical and biological sciences. He sees physics as having had its golden age, at least for the time being—perhaps a rash prediction with Large Hadron Collider just coming on line! Yet he is undoubtedly right in saying that biology is currently the most productive area. This, of course, is partly because advances in molecular biology and genetics are likely to have direct effects on our everyday lives. I am not sure, however, that this is going to transform science as much as he suggests. He argues, for instance, that the "seemingly indispensible requirement of repeatability [of experiments] will have to yield" (p. 177). This would certainly be true if we were talking of old-fashioned field biology—natural history—but is less obviously so of molecular biology. Moreover, it was somewhat surprising to see this remark being made by a fellow astronomer. Astronomical observations have *never* been repeatable, and astronomers have long known that they have to do the best they can with unique observations often made under less-than-ideal circumstances. The controversy that has surrounded Eddington's original demonstration of the gravitational deflection of light predicted by Einstein arises, I believe, partly because physicists have never fully appreciated the difference between controlled experiment and observation.

Haisch also appears to think that the increasing importance of biology will lead scientists away from reductionism, but many who work in molecular biology and neuroscience adhere to something very like the nineteenth-century version of materialism, determinism, and reductionism. Francis Crick's *The Astonishing Hypothesis* (1994) is a good example. Haisch and I each quote the same passage from that book, which illustrates my point nicely. It is an interesting paradox that physicists, who work with inanimate matter and have come to understand how elusive its true nature is, are much more open to arguments such as the fine-tuning of the universe than are biologists, who work

with living organisms. At least this appears true of the majority of those in each discipline who write books for the general public.

My reservations, however, are few and are far outweighed by my agreements with Haisch. This is a book to stimulate thought—the length of this review shows how much it has stimulated me—and that is the most important test of a good book. I strongly recommend it. Unfortunately, those who most should read this book, namely, those who are certain that they know the truth, whether they believe that truth to be revealed religion or scientific materialism, are the least likely to read either this book or mine. Richard Dawkins is a very successful popular writer and deserves the success and fame that his books have brought him, but it is a poor reflection on our media that this book by Bernard Haisch, better informed on religion than anything Dawkins has written, will not receive the sort of publicity that the latter can immediately command.

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Authors of the Impossible: The Paranormal and the Sacred by Jeffrey J. Kripal. University of Chicago Press, 2010. 332 pp. $37.50 (hardcover). ISBN 9780226453866.

Jeffrey Kripal is the J. Newton Rayzor Professor of Philosophy and Religious Thought at Rice University; he has written a book one may choose to take as medicine meant to revive a mortally ill academic discipline. It is generally known that departments of religious studies in university life are run by atheists, materialists, and Marxists, folks who pretty much share the same stultifying assumptions about the divine, the sacred, the afterlife, the spiritual life, psychical research, mysticism, extraordinary healing, the super-rational, the creative power of the imagination, and a whole spectrum of ignored, damned, and forbidden branches of human experience.

In direct opposition to that form of exclusive reductionism, Kripal has written a book that insists on bringing the psychical dimension into the discourse about religion and spirituality. He wants to open up religious studies to the extraordinary, the fantastic—*the impossible*. What Kripal means by his title—on the surface it is merely enigmatic or ironical—the reader discovers in context. His peculiar notion of "the impossible" needs to be exhibited in its living context, if one hopes to grasp its meaning.

Crucial to Kripal's argument, as it was for James, Lang, Myers, and DeVesme, was the claim that as you reach back to the origins of what we call "religion" you are likely to arrive at a point of origin in some extraordinary, psychical experience, mystical, paranormal, or both. The origins of all the great religions are replete with tales of supernormality in action. For historically intelligible reasons, however, there is little research or writing on the psychical origins of religion.

There is of course a problem with the idea of studying "religion" objectively and rationally; people are emotionally invested in their religious views and anti-views. What happens when ideology undermines the objective standpoint demanded by academic standards? For example, it seems reasonable to think that the academic study of religion requires a well-rounded sensitivity, a feel for the subject matter, for the stories, the concrete phenomenology, and for the accounts of the exceptional, often strange or bizarre, events that drive religious belief systems. Suppose, for example, your main passion is the political, racial, and gender side of any story? Harold Bloom kvetches about "The School of Resentment" in English studies. Professional studies of literature become battlefields for partisan politics, and the poetics of spirit, miracle, adoration, ascetic self-mastery, and mystical rapture are damned. Or consider the academic study of psychology (now called neuroscience): The notion of psyche

is reduced to an item of folklore; the neuron and the computer are deified. With a faint glimmer of hope, consciousness lingers on as mysterian irritation. Finally, I might mention professional philosophy, no longer interested in "love of wisdom"—which is what the word *philosophy* means. What's going on here? Are these items of self-alienation symptoms of what Vico called the "barbarism of reflection"—the perversions that arise from the inhuman use of reason and rationality—the ultimate disenchantment of life?

Kripal's animating thought, as I read it, is that there is a way to fight this psychical disenchantment, this decay of vital imagination. The way back to the lost dimension of the sacred is through the portal of the paranormal. (The subtitle of the book is *The Paranormal and the Sacred*.) Kripal emphasizes throughout the need for a bifocal view, "The One in Two" ethos of coinciding opposites: The scholar, any pilgrim of the impossible, must be a delicate receiving apparatus with, at the same time, the eagle's critical eye at all times scanning the great horizon. In solemnly willed acts, one must lay aside reductive biases, advert to pure consciousness, and look to the moments of extraordinary experience: the sacralizing, energizing, expansive, exalted—*the most transformative experiences*—as the starting points for one's scholarly, rational, imaginative response to the great issues of religion. (Scholarly loathers of religion typically begin with the most degrading.)

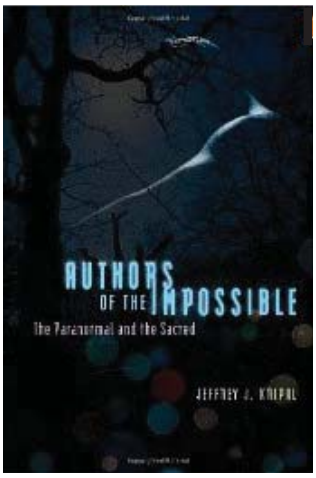
Now, these moments of creative advance that constitute the soul of "religion" sometimes crystallize with special vividness in and around certain individuals. Kripal considers four case histories that point to models of spiritual evolution for the future. Four authors of the "impossible" are presented and discussed. This is a book for people interested in re-imagining and re-ensouling some of the life-serving ideas of "religion." Jeffrey Kripal, like William Blake, wants to restore not just imagination but the majesty of the imaginal to the old gray house of religious studies. He would have religious scholars hone their visionary skills. He would rouse them to erotic participation with their subject matter, urging them to graduate from Apollonian episteme to Dionysian gnosis. Kripal quotes in a kindly vein Henri Bergson's jolting utterance that the universe is a machine for creating gods.

Kripal's four authors of the impossible are Frederic Myers, Charles Fort, Jacques Vallee, and Bertrand Meheust. These are authors whose work aids Kripal in authoring himself, and he is happy to make the process of that as transparent with detail and nuance as possible. One of Kripal's *topoi* that he keeps returning to is postmodern, even for that matter a theme central to analytic and linguistic philosophy. Perhaps it goes back to Heidegger who said that *Die Sprache is das Haus des Seins*, "Language is the house of being" (Heidegger, 1957). So, Kripal reminds us, we must ask, Who is writing our story? Are we not being written, written over, written off? Are we not in thralldom to prevailing concepts,

assumptions, scripts, codes, symbols, thus cornered in our little cultural niche. What writes us is the merely possible; to write ourselves, tell our own story, we have to break free from the dead weight of history, and accomplish the impossible. This, if I understand our author, is what "religion"—the aspect of it in tune with evolutionary advance—is most authentically about.

Each of the four authors contribute something to the larger process that interests Kripal, namely, the authoring of the impossible, which is Kripal-code for inducing breakthroughs of consciousness beyond the mundane to the sacred. Myers' contribution is to build a "spectrum" psychology that ranges from the normal to the abnormal and supernormal. Myers is a fitting ally for Kripal's impossible authorings; Myers was a poet, a classical scholar, and a literary critic. One of his great contributions was the terminology he invented for the study of consciousness. Myers invented (or brought to prominence) the terms *telepathy*, *supernormal*, *subliminal*, *preversion*, *mythopoeic*, *imaginal*, *secondary personality*, *phantasmogenetic*, and so forth. These terminological additions are tools for registering, ordering, and integrating impossible realities. Myers was not only a poet but also a romantic psychologist and erotic thinker. The eroticism and romanticism of Myers come together in his creation of a new science, a new psychology that stretches our conception of the human personality to quite impossible (and therefore fascinating) dimensions; the human personality that emerges from the crucible of Myers' spectrum psychology is steeped in transcendent heights and depths, reaching into unknown worlds even after the death of the body. According to Kripal, Myers was inspired to create this new science of consciousness because of his dead lover, who embodied for Myers a Platonic vision of eternal beauty and creativity. Myers lived an impossible life, impeccably married to his wife, Eveleen, while madly in love with the image of Annie waiting for him in the beyond. Some might regard this as more of a danger than a plus, but Myers' "subliminal Self" is a conceptual tool that permits us to imagine impossible love, and the complexities of passion in the metaphysical world of Andrew Marvell. In general, says Kripal, Myers's vastly enlarged model of the human psyche, unlike Freud's and even Jung's, helps us "explain religious experiences without explaining them away" (p. 62).

Kripal's discussion of evolution and the paranormal in Myers is useful; Myers' challenge was to incorporate evolutionism without swallowing materialism. He rejected the idea that telepathy arose from some juggling of hereditary factors; rather, evolution led to a "revelation" of telepathy when it rose from the subliminal to the supraliminal level of consciousness. Kripal underscores the spiritual and erotic dimensions of telepathy as Myers understood and lived them. Myers' intense love affair with Annie Marshall sheds light on his passionate pursuit of the psychical as the key to the mysteries of existence. Looking for the authentic roots of religiosity in the erotic and



psychical dimensions of consciousness marks an important trend toward a more full-bodied, anti-dualist conception of religious reality. It might well turn out to be the crucial step toward new forms of self-conceived religiosity. Kripal keeps pushing the envelope, alerting us to the need—and the possibilities—for world-re-enchantment.

Kripal's second exemplar of authoring the impossible is the American philosopher and collector of scientific anomalies, Charles Fort (1874–1932). Fort caught the imagination of several literary figures in the early twentieth century (Booth Tarkington, Theodore Dreiser, etc.), and one can see why: The Bronx original, at his best, wrote scientific criticism that sometimes sounded like the poet Arthur Rimbaud. (We might perhaps call him the Walt Whitman of the Impossible.) Fort, I would say, wrote the kind of surrealist narratives that Breton imagined, seamless fusions of dream and reality. Here is the first sentence of *Lo!* "A naked man in a city street—the track of a horse in volcanic mud—the mystery of reindeer's ears—a huge, black form, like a whale in the sky, and it drips red drops as if attacked by celestial swordfishes—an appalling cherub appears in the sea—*Confusions*."

It goes on like this for a while, and we read: "A naked fact startles a meeting of a scientific society—and whatever it has for loins is soon diapered with conventional explanations" (Fort, 1974:541). Kripal writes a searching account of the four main books of Fort, highlighting a number of imaginative ways of viewing the world conducive to the possibility of the sacred, naming it "Scattering the Seeds of a Super-Story." Kripal's chapter title reminds us of a point of contention in current intellectual life. It is fair to say that in the postmodern mood, one is obliged to look askance upon all talk of grand historical laws, trends, or destinies; no one great, all-powerful point of view that explains and fancies it can control everything. So the idea of a "super-story" may seem suspect to many. Kripal, I am sure, is not interested in foisting some one super-story, sacred or profane, on the wild diversity of humankind.

As individuals, however, we may need to form, however fallible and makeshift, some one, or several, super-stories for ourselves. We all need rafts, as the Buddha said, to ford the sea of existence; it must, however, be our own raft. Kripal thinks that a good way to free yourself from the deadening script you have been given is to write your own script, arming yourself with the widest possible range of facts, including the wildest of anomalies, but insisting

upon your own reading, interpretation, and reconfiguring. The point is to live our super-story. ("Religion" is always intensely personal when it's authentic.) While not coming off as preachy, Kripal, an adversary of spiritual dullness, invites us to freely rewrite the story imposed on us (in the double sense of *imposed*). It was a great day when Jefferson verbalized our inalienable right to life, liberty, and the pursuit of happiness. Kripal, speaking loud and clear to all fanatics, fundamentalists, and reductionists, is now telling us that we each have a right to author our own impossible lives. Jefferson would have approved.

Among the many ideas discussed in this chapter, I'll mention one that speaks to the current scene. Fort imagines three Dominants or Eras. The first Era is that of traditional religion, exclusive, and creed-driven; second, today's Dominant is scientific materialism, obsessed with explanation and control. And beyond now there is *tertium quid*. The New Dominant, Kripal writes, "he (Fort) associates with the epistemology of expression or acceptance and the professionalism of a new brand of individuating wizards and witches" (p. 113). A mark of the New Dominant is its spirit of radical inclusiveness. According to Kripal, "it (the New Dominant) builds an open-ended system and preserves it through the confusing inclusion of data, theoretically *all* data, however bizarre and offending, toward some future awakening" (p. 113). Kripal seems intent on opening new pathways from the paranormal to the sacred. The last extraordinary sentence points to a model of peace arrived at by the road of creative chaos—a road that travels toward fusion through fructifying confusion. Fort provides the second exemplary opus for authoring the impossible.

The remaining two authors are contemporary Frenchmen. Jacques Vallee is a scientist, novelist, computer entrepreneur, and leading figure in ufological studies. The UFO phenomenon is linked in many ways to the psychical and the sacred, and, as Vallee showed, to the realm of folklore. The religious connections are confusing. I have a written account of an American artist who joined her religious friend on a pilgrimage to Medjugorge. In an area where the Virgin appeared to certain young visionaries in the 1980s, Miss X observed a structure of lights hovering in space that appeared to her like a spaceship; she drew a picture of it, a geometric pattern of lights; it certainly looks like the classic light-grid craft of ufology. It turned out that others in the group also saw the strange lights, but were at first too disturbed to discuss it. In the literature of alien abduction (Mack, 1999), abductees invariably describe telepathic communication with the aliens. The vast UFO lore and literature is saturated with reports indicative of all kinds of psychic hijinks.

So the connections with psi and religion are present. Vallee's account of what is going on in UFO-land provides a conceptual apparatus we can use to deconstruct the prevailing worldview and reconstruct the space of the possible. For one thing, the multi-dimensionality of existence must be a shock to people

of flatland sensibility. In the ancient world, Socrates administered shock therapy to the prevailing consciousness through a new form of language use called dialectic. Later on, not that far away, another shock was administered to human consciousness; the news spread relentlessly—a man was raised from the dead by a divine power. In the Resurrection, a new belief was created, which promised eternal life. This became the nucleus of a movement still going strong. And now what? Jung relates UFOs to the death of the gods, the return of the repressed. The UFO phenomenon—a devastating form of Socratic *aporia*—imposes itself upon witness or abductee. But what is it? One's sense of reality is attacked. One is forced to ask fundamental questions about what is—*really* is. One is forced to enroll, as it were, in Jacques Vallee's Invisible College. What is the UFO phenomenon? A creature of the twilight zone, betwixt and between, on the border between fact and fiction? Inner or outer? Super science or science fiction? A hyphenated mode of being? Either? All? None? Confusion, as Fort said.

To describe this edge of reality demands a new syntax, a new koan, a new *Finnegan's Wake*. Here is how Kripal broadly suggests we consider our cognitive vapors: "The point is not to reduce one 'false' register to the other 'true' one. It is to confuse and destabilize *both* registers. Put more radically, the point is not to adopt this or that symbolic system as somehow literally true. The point is to be simultaneously sympathetic to and suspicious of *all* symbolic systems, and then finally to entertain the impossible possibility that the controlling intelligence communicating with us through all these systems is a human one, that is, a form of human consciousness far beyond our present, hopelessly materialistic and restrictive notions. We are not who we seem to be."

In reflecting on Kripal's vision of the self-authoring–authorizing process, it must be hard if you suffer from a fundamentalist cast of mind, one feature of which is the obsession with disjunctive logic. Kripal's fourth model of instigating psychical revivalism is Bertrand Meheust, who pursues his impossibilities in the multiple, unpredictable depths of somnambulism—dream walking, ambulating in sleep. The title of the chapter is clear enough: "Returning the Human Sciences to Consciousness." Kripal writes of the "naïve objectivism of the scientific method with respect to paranormal phenomena" (p. 227) whose unfortunate effect is to kill the phenomena. Nietzsche of course was right; "God is dead; *we* have killed him." The objectifying approach of the rational scientist probably creates the milieu that is lethal to paranormality.

In studying the variety of dissociated, "magnetic," or mesmerized states—with a special focus on Alexis Didier—Meheust observed that the phenomena are anarchic and fugitive. Many will find this discomfiting. However, there is a vital point that Kripal wants to make: The "phenomena, this boundless reservoir of potentialities, should profoundly transform our image of the human being,

and consequently render any final model of human nature, and so any general or universal method of therapy, impossible" (p. 227). On the same page, Meheust is quoted as stating this key point for practicing authors of the impossible, " . . . the phenomena of somnambulism are not invariable manifestations of the human soul, but . . . they should be thought of as the actualization of hidden virtualities—an actualization rendered possible in certain contexts, and therefore variable." This elasticity of effect ought instantly to enlarge confidence in our latent powers. However, beside the erotic and the numinous, there is a dark side somnambulism, or walking into the world of dreams. The Fortean and ufological dimension, along with somnambulism, nightmare, and hagging, enlarge but also darken ambiguously our understanding of the repressed sacred. Myers of course knew that the subliminal self housed a dark side, but believed, some would say with Victorian overconfidence, the light of intelligence would show the way. In any case, Kripal is right to pay attention to the dark, subterranean, deceptive side of the sacred world. One cannot get very far there without bumping into all sorts of shadow figures, adversaries, and devils of all stripes. All this new, edgy personnel is grist for authoring and self-authorizing. It seems almost common sense to say that any conception of the evolution of self without integrating the Shadow will fall short.

This book, despite an occasional extempore breeziness, is densely scholarly, well-argued, and boldly intuited. Kripal is interested in bringing literary studies into paranormal hermeneutics. Above all—and here he seems to be addressing his comrade "religion" scholars—there is a Blakean call for Imagination. He is also interested in reading the signs of popular culture for their sacred or profaning events, images, and trends. Isn't that what the prophets of old did? Since popular culture, its many worlds and personae, is a creation of the human psyche (no matter how many clever machines mediate that creation), *its* products also have religious significance. In Kripal's generous vision, a more wildly motley crew of possibles will be allowed to enter into the world of conversation.

Besides hermeneutics and imagination, Kripal respects the neurophysical challenge to his theory of religion, which relies on consciousness and some of its stranger effects. Kripal is armed and ready to deal with this, and throughout refers to, and sometimes elaborates on, a theory of the mind–brain relationship that several writers have expounded upon. Sometimes referred to as the transmission model, or "filter" theory, the main appeal is that it's consistent with psychical data; whereas, if we accept the mainline views the whole enterprise that speaks of the mental, the conscious, and the spiritual is more or less dead in the water.

So Kripal interprets the brain as an instrument for focusing the flow of consciousness on the business of biological survival (brain doesn't

create consciousness); surrounding this narrow, contracted field of profane consciousness is an indefinitely vast zone of possible experience. It is traffic with this vast, circumambient mind at large that constitutes the well of "religious" experience. So, in light of this mind–brain theory, when Augustine said "If you seek the Truth, go within," we can take him to mean lower your defenses, dismantle the filters, and let the higher spirit and intelligence overflow into waking consciousness. This squares with Eastern thought, as when the Taoist advertises the virtues of *wu wei* or "non action"; i.e. an invitation to drop one's normal intellectual and emotional defenses in a very radical and deliberate way. Wisdom here is to do nothing to support the habitual repression, the narrowing and filtering of one's consciousness.

Influx may occur spontaneously, as it did in a famous case of a neuroscientist who had a stroke that wiped out her left-brain functions. Or one may try to induce the onset of influx, gradually, by techniques of meditation, fasting, or controlled breathing. Jeff Kripal's book provides a vision and a proliferation of concept-juggling exercises, all designed to inject new life into the sagging, academic bones of religious scholars. For friends of the fantastic, and daring devotees of the impossible, this book might serve as a breviary for metamorphosis.

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Mind Before Matter—Visions of a New Science of Consciousness edited by Trish Pfeiffer, John Mack, and Paul Devereux. O Books, 2007. 224 pp. $24.95. ISBN 9781846940576.

A century ago Quantum Physics began breaking down the classical view of reality by which most of us live our lives. In this view, reality is external and "objective." We observe it but our observation does not affect it. The discovery of quantum physics led to the "Copenhagen Interpretation" by which the observer "collapses the wave function." This was the first admission by science

that the observer can affect a scientific measurement.

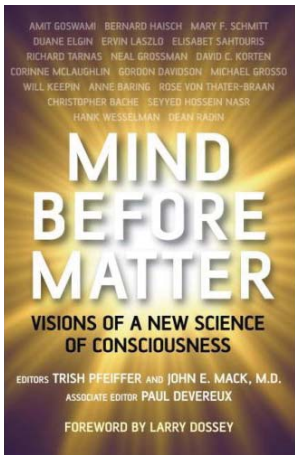
In this description, a quantum system is described as a superposition of basis states. A quantum system is said to be partially in each of the several possible states. Only if an observation is made does the scientist know which state it was "actually in" at a given moment. However, it was still assumed that the relative probabilities of the various basis states (eigenfunctions or eigenstates) were not affected by the observation (if many measurements are made). In this way, consciousness may be said to exert a "weak" influence on an experiment. Observation was assumed to cause "wave function collapse" but not to change the relative probabilities of the various basis states making up the wave function.

Recent experiments are finding that consciousness can exert much stronger effects than this. Experiments with highly trained Qigong masters, and other experiments in highly "conditioned" spaces in which the energy of consciousness has been accumulated, indicate that consciousness can exert much greater changes on a physical system (Yan, Lu, & Tiller, 1997).[1] The basis states which make up a physical system can themselves be altered by conscious intention. In some experiments, radioactive decay rates have been altered (Yan, Lu, & Zhu, 2000, Yan, Lu, Ren, Zhu, & Hu, 1990, Yan, Lu, Zhang, Wang, & Zhu, 1988). In others, the properties of water bonding have been changed. In others, chemistry has been altered (Yan, Lin, Li, Traynor-Kaplan, Xia, Lu, Fang, & Dao, 1999), while in still others, laser propagation has been affected (Yan, Li, Yu, & Lu, 1988).[2]

These effects of consciousness cannot be explained by the mere collapse of the wave function. They imply the alteration of physical conditions leading to wave functions which are themselves profoundly altered. Such experiments elevate consciousness itself to the level of a "force." In Professor William Tiller's terminology, consciousness has "raised the gauge," which implies that the physical conditions are altered. The basis states of a quantum system can change. In such a circumstance it is no longer sufficient to say consciousness "collapses the wave function." In these cases, consciousness alters the wave function in a profound way. The laws of physics themselves are changed by the presence of a conscious, active observer.

If consciousness can exert such profound changes on a scientific process, then it is no longer valid to separate it from the experiment, as traditional physics has done. The experimenter is truly an active component of the system.

These discoveries are leading to a new paradigm, as science continues the dialog begun a century ago between Einstein and Bohr. Consciousness can play a much deeper role than they imagined. Protocols and philosophy must adapt to this deeper understanding of the role of the conscious observer. These changing conditions represent an unfolding scientific revolution.



This book edited by Trish Pfeiffer, John Mack, and Paul Devereux makes a valuable contribution to this important dialogue. It presents thoughtful and thought-provoking essays by a variety of experts on the emerging new paradigm. As we move beyond the old Newtonian (and Einsteinian) view of an objective external reality, what issues must be resolved as consciousness is elevated to take its rightful place as an equal or perhaps even dominant component of scientific knowledge? How do we think about the role of the scientist when, instead of standing apart making his "objective measurement," his role may more closely resemble the shaman "dreaming the dream?" For the past century mainstream science has avoided these questions, but in view of new experiments these issues must be faced.

In a collection of essays by various leading thinkers, *Mind Before Matter* considers these implications from many viewpoints. Physicist Bernard Haisch observes that mainstream science is very conservative on this issue: "The perspective on consciousness within the mainstream community of physical scientists is in direct opposition to the possibility that consciousness may be primary, rather than being merely an epiphenomenon of the brain." Yet, even in this community he observed "glimpses are beginning to be seen. . . ."

On the other hand, what if consciousness is primary and the perceived physical world merely a secondary consequence, as suggested by Ann Baring: "Could the idea that consciousness rather than matter is the ultimate reality—the ground of all being—initiate a metaphysical revolution which would shake the foundation of human thinking?" And if this is true, then our consciousness has enormous and unguessed power. She quotes Paul Coelho: "We have the power to change the collective dream." And if consensus reality is based upon our collective consciousness, there is virtually no limit to what consciousness can accomplish.

And if this is true, then is science even possible at all? And finally, what are the consequences to society, as our view of ourselves expands to absorb these insights? Non-local consciousness appears to be one consequence. The continuance of consciousness beyond the physical body is another. And the secondary, almost illusory nature of matter in these theories is another possible consequence. These are some of the questions to be addressed as the issue of integrating consciousness with science proceeds.

Mind Before Matter offers a rich selection of observations on these topics.

It contains essays by approximately two dozen contributors whose backgrounds are wide-ranging. Among the contributors are Bernard Haish, Amit Goswami, John Mack, Dean Radin, Duane Elgin, Michael Grosso, Elizabet Sahtouris, and Paul Devereux, as well as many others.

In the Introduction, Devereux notes:

> I have placed each of the essays . . . into one of four sections—Science, Philosophy, Psi, Communion—as seems best fit its content. However, let it be noted that the essays tend to be so wide ranging that this has had to be something of a token exercise. . . .
>
> Perhaps the most generally underlying thought is that mainstream Western science will simply have to find room for the subjective, for human experience. It needs to understand consciousness as being "inside" of the objective universe. Without it our Western model of reality will forever be incomplete, and dangerously so.

Devereux further observes:

> Those who already believe that consciousness is the prime foundation of reality will find that the visions in these pages broaden that understanding; skeptics are urged to read all the essays in this anthology before hardening their viewpoint. This book represents the start of a discussion, not some kind of "last word." What it is intended to do is inspire, challenge, and provoke the reader, leading to further, ever-deepening discussion.

Anthropologist Hank Wessel notes that the public is increasingly aware that ". . . consciousness, not matter, is the ultimate reality and thus the ground of all being. . . ." He compares the new evolving view to that of tribal peoples he studied in Africa:

> . . . the perception that the multi-leveled field of the dream is the real world; that we human beings are actually dreaming twenty-four hours a day; and that the everyday physical world came into being in response to the dream, not vice-versa.
>
> Through my ongoing participation in these groups. I came to conclude that the different levels of reality on which the shaman operates are simultaneously levels of consciousness as well as levels of experience. By intentionally expanding their conscious awareness, shamans are able to transcend the physical world and change their level of experience, effectively shifting from one level of reality to another.

The shamanic idea of many parallel realities may play an important role in the evolving model of consciousness, since these realities can also correspond to possible quantum states or to various theories of "multiverses." Such a viewpoint may play a crucial role in the eventual unification of physics and consciousness.

Ann Baring sums up these themes:

> It seems that whatever name we give this ground, whether we use scientific or metaphysical language—Quantum Vacuum, Zero-Point Field, Creative Energy, Universal Intelligence, Ground of Being, Sacred Mind, Cosmic Soul, God or Spirit—the Primal Consciousness is the origin or source of our being. All aspects of life, visible and invisible, are interconnected and interdependent: All life is one. Death is an illusion born of our fragmented consciousness: There is nothing beyond death but life. In the light of this new understanding, the physical brain or even the entire mind/body organism is not the source of consciousness but the exquisitely fine-tuned vehicle of an invisible reality in which we all exist, the means through which it can come to awareness of itself in this material dimension. It could be said that this new vision marks the return of a very ancient insight known to the Vedic seers of India and summed up in the words in the Bhagavad Gita: "All is the Divine Being."

The great challenge of the integration of consciousness with science is that it is truly profound and far-reaching. It brings into question and into focus how we know anything. It leads us to examine the roots of all knowledge, and even the basis of how we observe and the nature of awareness. In this way, it will lead us to a more profound understanding of who we are and to our place in the universe. For the reader wrestling with these important questions, *Mind Before Matter* is an excellent source of wisdom and ideas.

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Notes

[1] For a more complete bibiography, see Swanson, 2010, Lu, Zhu, Ren, & Hu, 1993, Tiller, Dibble, & Fandel, 2005, Tiller, Dibble, & Kohane, 2001, Tiller, Dibble, Walter, & Kohane, 2002, Tiller, 1997, 2007.

[2] An excellent summary of many of these citations, as well as further discussion of consciousness models, is found in Swanson, 2010.

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Consciousness and Its Objects by Colin McGinn. Oxford University Clarendon Press, 2004. 272 pp. $35.00 (hardcover). ISBN 9780199297634.

Here we have a collection of papers written by McGinn over the period 1993–2001. It is intended as a followup to his 1990 book *The Problem of Consciousness*, in which he argues that the philosophical problem of the relation of mind to body can never be solved due to an inherent limitation of the human concept-forming capacity, which he refers to as *cognitive closure*.

McGinn calls this position *epistemic mysterianism*. It is not to be confused with ontological mysterianism, which holds that the problem would require a supernatural explanation. In McGinn's view, the relation between mind and body must be a natural one even though the human cognitive closure prevents us from understanding it. Therefore he also calls his view *transcendental naturalism* (p. 182). The mind–body relation transcends human cognitive capacity but nonetheless "the world itself is as smoothly natural and seamless as one could wish" (p. 64).

While the papers gathered here are very much in the context of professional philosophical discourse, the thesis impinges on the borders of neuroscience, cosmology, biology, and psychology. Aside from its possible interest to philosophers and scientists, those who would feel spiritual or metaphysical interest in the idea of profound universal mystery may find his arguments compelling. McGinn writes in a particular philosophical style with clever turns

of phrase and a variety of approaches, making the text (hopefully) accessible to a wide spectrum of readers.

We find, for example, a fantasy scenario in which McGinn has a humorous dispute with an alien being whose views are opposed to his own, a methodological excursion aiming to show that his theory predicts the sorts of dilemmas characteristic of the most knotty philosophical problems, a curious thought experiment outlining a theory of mind on the analogy of ancient Greek atomism, and a journey to the more recent past, where the basis of the argument rests largely on a recitation of Bertrand Russell's sense-data–oriented views of 1912.

In this review I will undertake particularly to develop a sense of the philosophical context within which I believe McGinn is operating, and suggest a remedial way of looking at things.

McGinn's Account of the Problem

For McGinn, the mind–body problem is the problem of explaining how conscious states are caused by the brain. It is actually a mind–brain problem (pp. 56–57). The argument below is, I think, a fair distillation of his theory.

(1) We know that conscious states cannot be reduced to brain states.
(2) But conscious states *are* caused by brain states.
(3) Therefore we feel a sense of an inescapable philosophical problem.
(4) The concept-forming ability of the human brain is limited by a cognitive closure.
(5) The mind–brain causal relation is outside that limit.
(6) Therefore we can never solve the mind–body problem.

This argument has two parts. The first two premises yield (3), while the fourth and fifth yield (6). If we compare (3) and (6), it would appear that we are in a kind of ongoing existential crisis, because we feel a sense of an inescapable problem that can never be solved. On the other hand, McGinn seems to think that this acknowledgment of a fundamental limitation of the mind may have a kind of liberating effect: "We need simply to accept our deep ignorance" (p. 69). This has been the advice of skeptics at least as far back as Pyrrho (ca. 360 BC–ca. 270 BC). We can't solve the problem, so why worry? But rather than adopt either a skeptical or an existentialist viewpoint, perhaps we should probe a bit further.

Essential to McGinn's argument is (2). The basis for this premise is the evidence supplied by neuroscience for correlations between activity in the brain and conscious states: "Brain states cause conscious states—that is what observation suggests" (p. 57). But (2) is contradicted by (1). So (1) is the source

of our sense that there is a mind–body—or rather mind–brain—problem. And (1) is supported by an argument which invokes the view of perception put forward by Bertrand Russell in 1912.

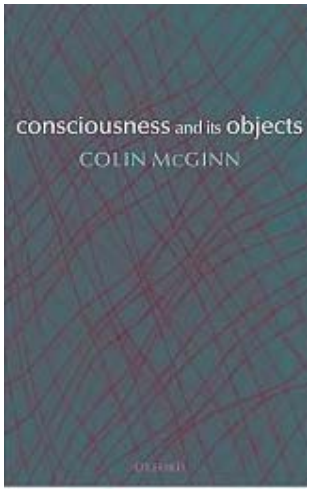
According to Russell, when we see and touch a physical object such as a table, we don't really see or feel the table. What we really see is variously shaped patches of color, and what we feel are various sensations of touch, etc. These are sense-data. We infer that there is a table on the basis of these data. And we know the sense-data *directly* by "acquaintance" rather than *indirectly* by "description." Now the special quality of things that are known by acquaintance is that you just *know* them, and that's that. "I know the color perfectly and completely when I see it, and no further knowledge of it itself is even theoretically possible" (p. 6, McGinn quoting Russell).

McGinn seizes on this to point out that since we have knowledge by acquaintance of our conscious states, we must know these states in the way Russell describes, perfectly and completely, with no further knowledge of them being "even theoretically possible." McGinn then makes the remarkable assertion that our perfect and complete knowledge of our own conscious states tells us that the brain cannot cause consciousness.

> . . . if we know the essence of consciousness by means of acquaintance, then we can *just see that* consciousness is not reducible to neural or functional processes. (p. 9, my italics)

This then is the justification for (1) and, when confronted by (2), the source of the mind–brain problem. And I have to confess that I really don't know how to take this statement. McGinn appears to be justifying (1) by appealing to some kind of direct intuitive knowledge similar to the untenable rationalist position of Descartes regarding "clear and distinct ideas." What McGinn *can just* see "perfectly and completely" by introspecting his own states of consciousness seems little removed in lack of verifiability from the rationalist intuitions of Descartes.

Against this idea I would point out that over the course of history many individuals have spent a great deal of time pursuing direct knowledge of consciousness by introspective meditation, and frequently such individuals report just the opposite. The dualism of mind and body disappears and instead a profound unity embracing all of existence, physical, mental, and spiritual, becomes clear. Why is McGinn a better source of information about the nature of pure consciousness than, say, a Buddhist monk? The answer is that when one appeals to truth as revealed by personal introspection, there is no objective standard. (It is for this reason that I rather suspect McGinn's underlying justification for (1) is conceptual rather than intuitive, i.e. based on a presupposition of mind–body dualism to begin with.)



However for the time being let us put this aside and instead yield the point to McGinn, that by introspection he knows the essence of consciousness, which means he knows perfectly and completely that consciousness cannot be reduced to brain states. If so, why wouldn't that be the end of the matter? Conscious states are not caused by the *brain*. Premise (2) is false. I heartily agree with this, although not because I accept (1). But McGinn wants to hang on to (2). After all, if he loses (2), what does he do then about the observations provided by neuroscience, that when certain conscious states occur, these are associated with activity in certain regions of the brain, which proves that the brain causes consciousness?

The Mediatory Brain

Well, one would have to conclude that a large body of neuroscientists and cognitive scientists are mistaken. This indeed is what a growing number of philosophers and cognitive scientists are thinking (Rockwell, 2007, Noë, 2009, Chemero, 2009, Shapiro, 2010). These individuals happen also to be the sort of philosopher who is not enamored by the sense-data theory. They, like myself, agree that conscious states are not caused by the brain—at least not in the sense of a one-to-one or "atomistic" causality (one single physical state of the brain causes one single mental state). And McGinn ought also to agree with this, except for those pesky neuroscientists who keep telling him otherwise.

Suddenly it begins to look as though the ground is trembling a bit beneath McGinn's (philosophical) feet, and the tremors are increasing. To make the point in depth let us take a look at McGinn's concept of memory.

> We know perfectly well how it is possible for the brain . . . to harbor as large a memory store as it does. . . . the number of memories is identical to the number of a (subset of the) brain's states. (p. 13)

McGinn here espouses the data-storage theory of memory. Inside the brain (metaphorically) there are a number of pigeonholes for memories, and the number of these pigeonholes that are filled is equal to the number of memories one has. This theory, despite its wide acceptance among neuroscientists, has been decisively shown to be deeply flawed. (If you are not sure of this, ask a neuroscientist to tell you how many memories you've got.) Anyway, the more

probable alternative is that the role of the brain in memory is mediatory. As one critic puts it,

> . . . it's one thing to say that the brain *mediates* the capacity to remember, and another to say it *stores* memories. The former view (more likely the correct one) takes the brain to be an *instrument involved in the expression of memory*; the latter view turns out to be deeply unintelligible. (Braude, 2006, my italics)

McGinn ridicules such an idea. He assumes this must imply that the brain is an "interface" between the "real basis" of consciousness and "bodily behavior." This is apparently because he believes that the memories must be "stored" *somewhere*. The "real basis" must be some sort of *realm* which is "nothing like anything we have ever encountered in nature," and furthermore where can it be? Perhaps it is "up in the sky," or "underground," and so on (p. 57). To hold that the brain's role in consciousness is mediatory is to subscribe to this sort of absurd fantasy.[1]

This is of course a "straw man" argument, but now McGinn says something quite revealing. He argues that this would put consciousness in a strange *location*. Conscious states would be located where the real basis is located, "since the location of the mind is parasitic on the location of the physical basis" (p. 57). And of course if the real basis is underground, or in the sky, then that's where the mind is located. Ergo, *reductio ad absurdum*.

I will return to this point later. First however it is necessary to deflate McGinn's restrictive idea of what might be meant by calling the brain's role mediatory. To do this is to reject that aforementioned narrow idea of causality, in which some single event in the brain stands in a direct causal relation to some single mental state. W. T. Rockwell for example argues against this atomistic causality, citing Mill's view that causes cannot be separated from their context of conditions (Rockwell, 2007:54).

What then is the proposed context of conditions within which the brain has an important, but not atomistically causal, function? We are moved here into the realm of the contemporary theory of Extended Cognition (EC), which is the theory that the physical basis of mind includes not only the entire nervous system and the body that supports it, but also a person's engagement with the world through activity in the world. Alva Noë, citing "evidence that the brain gives rise to consciousness by *enabling an exchange* between the person or animal and the world," (my italics) says

> What emerges . . . is a new conception of ourselves as expanded, extended and dynamic. . . . Where do you stop, and where does the rest of the world begin? There is no reason to suppose that the critical boundary is found in our brains or our skin. (Noë, 2009:67–68)

Someone might object to this on the ground that even if the "real base" of consciousness is such a larger nexus, it is still "physical" and so the problem remains. What the critic is missing here is the term "dynamic." Rockwell speaks of such a dynamic system as a "behavioral field" involving environmental interactions, time, and energy (Rockwell, 2007:86). This kind of system is of a different order than that of the brain by itself. McGinn's point is well-taken: Consciousness would somehow be *located* in such a dynamic system, which perforce overlaps with multiple other such systems and may extend over a very wide field in space–time. In such a scenario the concepts of "cause" and "location," and even perhaps the concept of "physical basis," may well be irrelevant to the issue or at least in need of revision. (A fundamental problem of the sense-data theory is that it excludes the role of *action in the world* from its concept of cognition and substitutes a kind of passive observer, i.e. the philosopher sitting at his desk looking at patches of color inside his brain.)

But it is not my purpose here to attempt to account for the way in which a dynamic interactive spatiotemporal system of this sort involves the existence of consciousness. The direction I am going is rather different. What I hope to make clear is the radical difference between McGinn's perspective and that of the proponents of a quite different line of thought exemplified by the nondualistic views of American Pragmatism and particularly the views of John Dewey, whose philosophy is a key inspiration for EC theory. To put it succinctly, it is more or less a waste of time to try to deal with McGinn's convoluted argumentation within his own frame of reference. The cards are stacked against you in advance. Really to understand what is going on one must move to a quite different frame, one which Dewey referred to refreshingly as the "open out-of-doors air and light of day" (Dewey, 1930).

McGinn's Conceptual Straitjacket

The actual cognitive closure involved in McGinn's epistemic mysterianism, I believe, is not the limiting factors which everyone might agree must exist for human knowledge relative to any given time and any given state of science. It is instead a closure resulting from the set of concepts which McGinn himself utilizes in adumbrating the problem and subsequently arguing that it cannot be solved. And this set of concepts is nothing new. It has persisted in philosophy, in science, in religion, and in the popular mind. McGinn is operating within a conceptual framework that might even be called a kind of "folk metaphysics." And what it is, is mind–body dualism.

McGinn himself agrees to this interpretation. He refers to the "conceptual dualism inherent in our introspective and perceptual concepts" (p. 21). He thinks dualism lies at the foundation of our consciousness, and by including *perceptual concepts*, he reveals that he believes there is an inescapable dualism here as

well. Indeed, the extreme subjectivism of the Russellian view of perception, to which McGinn subscribes, divides the perceiving subject from the world and puts perception and knowledge of a physical (outer) world into irreconcilably separate categories. All of McGinn's argumentation and conceptual structuring is characteristic of an underlying dualistic view of things. Not only does he not deny this, he relies entirely upon it in order to make his argument stick. So if it weren't for those darned neuroscientists with their mind–brain correlations, we would have no problem. We would just be dualists.

His resolution of this dilemma is to invoke cognitive closure and transcendental naturalism. *We* can never escape the dualism, but somewhere within another conceptual framework the existential problem of (3) must be resolved. With this I heartily agree; except that what appears to McGinn to be "transcendental" may not be so far away after all, as I will suggest momentarily and as may be implicit in his chapter on "Consciousness and Space."

Cognitive Closure and Hints of Rehabilitation

We have yet to look more closely at the general idea of cognitive closure, the subject of the last three premises. The fourth premise, that the human cognitive capacity has an absolute limit beyond which it cannot go, lies at the heart of McGinn's thesis. The notion of a limit to human knowledge is one which few would want to deny. Surely it is presumptuous to say there is no problem, now or in the future, that we cannot eventually solve. It is palliative to espouse a philosophical humility.

On the other hand, it seems to me to be equally presumptuous to assert without qualification that this or that problem is beyond some absolutely determined cognitive limit—except for one very important factor. For it turns out that premises (1), (4), and (5) really collapse into a single premise. The introspective knowledge by acquaintance of consciousness, in telling McGinn that his conscious states cannot be caused by the brain, and the sense-data analysis of perception, are seen by McGinn as establishing that a dualistic mindset is fundamental to human consciousness itself (p. 21). We can never escape it. So the argument is really something more like this.

(1a) Our knowledge by acquaintance of our own conscious states, and our analysis of the process of sense-perception, tells us that our concept-forming capability is fundamentally dualistic.
(2a) A fundamentally dualistic mind can never conceive of any nondualistic reality.
(3a) Therefore we can never solve the mind–body problem.

Even though in his view premise (1a) is pretty much self-evident, McGinn turns his attention from Bertrand Russell to Noam Chomsky in a somewhat different argument to support his thesis of cognitive closure. In his

chapter "The Problem of Philosophy" he outlines Chomsky's combinatorial view of language and argues that this implies an absolute limit to cognition. "Cognitive accessibility . . . turns upon the applicability of the combinatorial paradigm supplied by language" (p. 189). When a system begins with some pre-programmed set of principles (or units, or atoms), there must be a limit to the applicability of the possible combinations thereof. But Chomsky's ideas are also highly controversial, and in any case given (1a) the argument from Chomsky is unnecessary.

However, at this point McGinn offers a possible solution to the entire cobweb. He has held throughout, that despite our inability to conceive of how the brain can cause consciousness, the brain nevertheless does so. Therefore McGinn suggests two possibilities. The first of these is that the production of conscious states may lie hidden within "subconscious self-monitoring representations employed by the brain as it goes about its business." He kindly spares us the agony of having to cope with an explanation of whatever this means by saying it would take too long to expound. Instead he offers a second possibility. The ability of the brain to produce consciousness must be founded on the biological basis of life: the genetic code.

> Since . . . the genes work symbolically, by specifying programmes for generating organisms from the available raw materials, they must contain whatever information is necessary and sufficient for this feat of engineering. . . . They must, that is, specify instructions adequate for creating conscious states out of matter. (pp. 193–194)

In bringing up the gene possibility, McGinn is looking for a conceptual framework outside the ordinary capability of the human brain that will allow accounting for consciousness. He is suggesting that such a conceptual framework is somehow contained as information within the genes. He does not consider pushing the origin further back in evolutionary time. Genes, after all, have come from matter and energy over the course of cosmic evolution. So what is preventing us from speculating that the information necessary to create the consciousness-creating genes is not somehow embedded in the fundamental structure of matter (i.e. energy) itself? This may be thought of as implying some sort of panpsychism, but that is just a name at this point.

Another option however is implied by McGinn's discussion of "Consciousness and Space" (Chapter 5). In this chapter, which in my opinion is the very best chapter in the book, he undertakes an analysis of our conception of space, and of consciousness as being non-spatial. Within the common conception of space, "Clearly, the space of perception and action is no place to find the roots of consciousness!" (p. 112, his punctuation). His conclusion is that "we need, at a minimum, a new conception of space" (p. 105). McGinn is

asking for a paradigm shift, but because in his view that our consciousness is structurally dualistic he believes a paradigm shift of this order is impossible for human cognition (pp. 21–24).

It is significant that in speaking of space, McGinn only pays lip-service in a parenthetical afterthought to time (p. 113). But "the space of perception and action" perforce involves time. Contrary to his assumption that such a space is no place to locate consciousness, space *is* the place. A dynamic spatiotemporal field, or "behavioral field," is precisely where the proponents of EC theory *do* locate the roots of consciousness. Furthermore, the subjectivistic encapsulated ego implicit in the sense-data theory of knowledge is just what theorists such as Rockwell and Noë reject in favor of the notion of an "expanded, extended and dynamic" self. It is precisely *action*, and action in a spatiotemporal energetic field, that generates all our scientific knowledge in any case.[2] The move away from introspective, private perceptual space into the active space of scientific inquiry is what has generated our rapidly evolving cognitive capabilities and allowed us to solve problems such as those that Pyrrho, for example, thought could never be solved because of what appeared to him at the time to be an inherent limitation of our perceptual capacity.

Indeed, McGinn's speculation about information contained in the genes could equally be applied to Alva Noë's suggestion that the purpose of the brain is not to cause consciousness directly, but to allow for the higher-level interactions between persons and the world that make possible our cognitive capacity.

Long ago Dewey was calling for precisely the paradigm shift that McGinn is, in effect, asking for. Speaking of the gulf that under the dualistic paradigm separates *experience* and *nature*, Dewey wrote

> To many the associating of the two words will seem like talking of a round square. . . . I know of no route by which dialectical argument can answer such objections. They arise from associations with words and cannot be dealt with argumentatively. One can only hope in the course of the whole discussion to disclose the meanings which are attached to "experience" and "nature" and thus insensibly produce, if one is fortunate, a change in the significations previously attached to them. (Dewey, 1929/1958:1a–2a)

At this point, then, I leave it to the reader to decide which way may lead to a richer, more productive reality.

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Notes

[1] Something like what McGinn suggests here is Ervin Laszlo's theory that memories are stored in the quantum vacuum by means of "quantum holograms" and are accessed by the brain (Laszlo, 2007).

[2]The earmark of contemporary scientific investigation is that it is collective in nature. Communication, peer review, experimentation are necessarily collective enterprises. In these enterprises various safeguards against self-centered egoism are accepted as de rigeur. A recent study documents the existence of collective intelligence among groups of people who cooperate well, showing that such intelligence extends beyond the cognitive abilities of the group's individual members (http://www.physorg.com/news205076011.html). McGinn's introspective conclusions would seem irrelevant and even false when considering this kind of collective cognition.

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Carl Sagan: A Biography by Ray Spangenburg and Kit Moser. Prometheus, 2009. 145 pp. $16.98. ISBN 9781591026587.

Arguably, Carl Sagan was the premier science popularizer in the world in the second half of the twentieth century. His monumental PBS TV *Cosmos* episodes were seen around the world by millions of people. He motivated countless young students to follow the sciences, and many important scientists today credit Sagan for inspiring them to make their careers in scientific fields such as astronomy, geology, and biology. Unfortunately, Carl Sagan died young, and the world lost his gift and pathos to explain the universe the way it is and not the way we wish it to be.

Several biographies have appeared describing his life, but I find the recent book *Carl Sagan: A Biography* by Ray Spangenburg and Kit Moser a concise, well-written, easy-to-read, and uncomplicated treatise of his life. The book correctly emphasizes Carl Sagan's two most important life questions, "Is there

life on other planets?" and "What is the rest of the solar system made of?" His research work took him from biology to physics and astronomy and contributed greatly in the quest of planetary exploration, he being a leader in many of NASA's early planetary missions.

His academic affiliations included the University of Chicago, where he received his Ph.D., Berkeley, Harvard, and from 1968 Cornell. At Cornell he did most of his productive work in laboratory experiments in exobiology and planetary exploration, as well as authoring several books and popularizing astronomy and space sciences. The author of this book review was Chair of the Department of Astronomy and Space Sciences at Cornell for most of Sagan's tenure until his untimely death in 1996. Sagan and I were close friends and colleagues, and I agreed with and applauded his efforts in science education.

In his youth Sagan was sympathetic to the claims of UFOs and wanted to find any evidence that would prove their extraterrestrial origins. He never did, and he remained a skeptic. Perhaps his greatest scientific work was to recognize that the planet Venus has a large greenhouse effect that keeps the surface of the planet at about 800°F, too hot to sustain life. He then turned his focus to the planet Mars, but the Mariner Mission that landed on Mars only showed a vast reddish desert. Prior to that Sagan was hoping to see the photos of Martians dancing in front of the Mariner cameras. Yet he was at once in front of the TV cameras providing exciting commentary to the public.

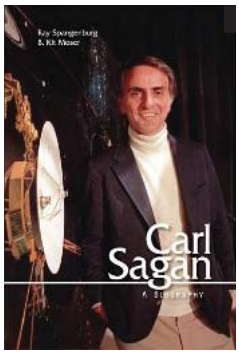
He enthusiastically participated in trying to send messages to other extraterrestrials by producing various messages, some attached to spacecraft such as Pioneer 1 and 2, which are now so distant that they have left the solar system.

The book also details the many discussions that Sagan had with other SETI (Search for Extra Terrestrial Intelligence) enthusiasts and describes how one can estimate the number of intelligent communicative civilizations in our galaxy. Our galaxy alone has some 200 billion stars—Sagan liked to emphasize that as "Billions and Billions of stars." Just based on probability arguments, we may not be alone in the galaxy. Today it would be appropriate to say "Billions and Billions of Galaxies."

His many TV appearances made him a celebrated public figure, but a trusted one.

In 1988 we hosted the Society for Scientific Exploration at Cornell University where the keynote speaker was Carl Sagan. He was an entertaining speaker, but had a strong message that the public needs to be skeptical about paranormal phenomena. He said extraordinary events need extraordinary evidence. Soon after, he published the important book *The Demon-Haunted World: Science as a Candle in the Dark*.

Later in life, working with his colleagues, he described "Nuclear Winter,"



the devastating scenario that could result from a global nuclear war that may bring the end of our civilization. He advocated an end to nuclear weapons. A global nuclear war will spread ashes in the earth's upper atmosphere, blocking sunlight for a long time and cooling the surface of the earth, preventing any food production.

Among his many books, in 1977 he published *The Dragons of Eden: Speculations on the Evolution of Human Intelligence*, where he explored the brain and how it evolved, a subject clearly connected to intelligent life on other planets. He was honored with the Pulitzer Prize for this work. Later in his career he also wrote a fiction called *Contact* (naturally to do with contact with another extraterrestrial civilization) that later became a successful movie.

This book on Sagan describes his private life in some detail with his three marriages and five children and makes it clear that he was most happy with his third wife Ann Druyan.

One shortcoming of the book is the abbreviated discussion on the PBS TV 13 episodes of *Cosmos*. There is little, if any, about the contents of the subject matter in this spectacular series that was Sagan's most important contribution to society. Yet the book gives an excellent account of Sagan's life and personality.

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La Télépathie: Recherches Expérimentales by René Warcollier. Préface by Charles Richet. Paris: Alcan, 1921. 363 pp. (63 figures). Free at http://books.google.com/books?id=XR5WAAAAMAAJ&printsec=frontcover&dq=La+Telepathie:+Recherches+Experimentales+by+Rene+Warcollier. Nabu, 2010. 394 pp. $33.75 (paperback). ISBN: 9781142292607.

Before presenting this work, a few words about the author and the context of his writings within the tradition of French psychical research or "métapsychique" (Richet, 1905). Born in 1881 in Ormonville-La-Rogue, on the northwestern coast of France, René Warcollier successfully combined two careers, as a chemical engineer and a "métapsychiste." Well before the creation

of the Institut Métapsychique International (IMI), he was in close contact with French psychical researchers and collaborated in the Review *Les Annales des Sciences Psychiques* (founded by Dr. X. Dariex and Professeur C. Richet in 1891). So, he had belonged to the small circle of scholars and scientists who, in 1919, founded the IMI in Paris. While he did not appear in the initial IMI Board, he soon became IMI treasurer and later editor of *La Revue Métapsychique*. From 1946 until 1950, he was IMI's vice-President, and from 1950 up to his death in 1962 he held the position of President.

Warcollier obtained a degree in chemical engineering in 1903, and became a brilliant and prolific inventor as well as head of a major corporation. His patents include the synthetic manufacture of artificial pearls and gemstones (sapphires, emeralds, and rubies), the first super-luminous cinema screen, and an original method for the extraction of potassium permanganate.

It is tempting to see in his early personal and professional interests in light and in the shimmering of colors (in precious stones and in images on a movie screen) the roots of his psychical research, which focused on the characteristics of visual mental imagery implicated in all its forms in telepathic communication. No doubt his attraction to visual impressions may have influenced his psi research and his focus upon cognitive processes, specifically in those underlying telepathic transmission of drawings.

His early colleagues and collaborators were illustrious figures in paranormal research, including Richet, Flammarion, Bozzano, de Vesme, C. Lombroso, Lodge, Maxwell, etc. Most of them were on the editorial committee of *Les Annales des Sciences Psychiques* and published regularly in it, such as Geley (1914) and Osty (1914), the later directors of IMI. Many of them were psychologists or physicians sharing a fascination for human abilities that appeared in association with somnambulism and hypnotism. In such states of consciousness, it was observed that individuals could mobilize and upgrade resources and reveal psychic capacities inaccessible in the ordinary awakening state. These functions were indeed well-described by Myers (1892, 1903) in his theory of the subliminal self. In addition, *Les Annales des Sciences Psychiques* was echoing the questions of survival and spiritualism (Bozzano, 1906) in line with mainstream SPR researchers.

Both personal psi experiences and intense curiosity contributed to the young Warcollier's strong involvement in psychical research. He wrote his first articles on the subject in *Les Annales des Sciences Psychiques* in 1905 and 1906 (Warcollier, 1905, 1906a, 1906b). Later, he made an analysis of the "fluid motor" device discovered by Count de Tromelin, which consisted of a paper cylinder suspended from its central axis by a needle, so that it is stable but easy to rotate. De Tromelin's experiment supposedly demonstrated that the cylinder could be moved at a distance by a hypothetical "fluid" emanating from human

hands. Warcollier's analysis was quite critical of this experiment, arguing that the movement could be attributed to ordinary thermal currents (1908). Three years later, he published an article entitled "Conditions expérimentales dans l'étude de la télépathie" (1911a), in which he critically discusses the theory of Usher and Burt, English researchers who had proposed a physical explanation for telepathy via some sort of waves radiating from the brain.

In fact, Warcollier had serious doubts about all theoretical models of telepathic phenomena, which relied on a supposed physical medium. He claims: "It is best to abandon any theory to deal only with facts and to study successively the experimental conditions."

Another article (Warcollier, 1911b), with reference to Duchâtel's study of psychometric cases, is about 52 psychometric experiments with four psychics that Warcollier co-organized with de Vesme from 1910 to 1911. According to Warcollier, misperceptions are due to contamination by the shape of the object, and the successes are the result of thought transmission. It emphasizes the extremely low number of successes and the considerable amount of errors and inconsistent results from one medium to another. Nevertheless, he sees in psychometry an interesting method to quantify paranormal perception.

His intuition about the importance and function of imagination on our body and mind led him to co-author (with Edmond Duchâtel) *Les Miracles de la Volonté* (Duchâtel & Warcollier, 1912), which presents a number of phenomena intensively studied in the nineteenth century, and still popular in the early twentieth century: auto-suggestion, collective hallucinations, dreams, materializations, thought photography, etc.

As we see, the cultural, theoretical, existential, and experiential background in which Warcollier evolved has three aspects: discoveries about psychology and the unconscious (Myers, Janet, and Freud), a strong passion for spiritualism and the question of survival, and finally a great interest in these enigmatic phenomena grouped under the name of métapsychique. All these subjects were regularly treated in *Les Annales des Sciences Psychiques.*

Because of his fundamental interest in the visual, Warcollier centered his research on the mental image. This fitted well with the ideas of his time and was directly or indirectly linked to the emerging disciplines of the period—including those dealing with the psychology of form and perception, and the psychology of the subconscious and the unconscious.

La Télépathie was actually already complete by spring 1914; but with the advent of World War I, Warcollier had to postpone its publication. It finally was published in 1921, two years after the foundation of the IMI. Warcollier writes in his short prologue:

> This research was practically entirely conducted in 1914. But, among the ru-

> ins of an epoch it seemed anachronistic to me. Those investigations were left behind, when I saw with surprise the public mind coming back to psychical research, even more eager than before the war, as if it distinguished there darkly the moving forces able to elevate Human being in its wholeness.

La Télépathie starts with a Preface from Warcollier's friend, Charles Richet, who says: "Here is a book of science, true science. Telepathy, which is one of the core chapters of the métapsychique, has been treated (here) very methodically, as a scientific monograph." Amid all other psychical phenomena, Warcollier chooses to present in this volume his long experience of more than ten years research on Telepathy (this word is used by him in its broadest meaning: transmission of sensations, thoughts, ideas, emotions, etc.). This work is dedicated to specific questions, questions that Warcollier intends to be scientific: What is telepathy? What are the mechanisms of transmission from agent to percipient, or from a group of agents to a group of percipients? "Lift every corner of the veil of the Universe . . . discovering in the darkest parts (of the psyche) the key to the enigma . . . exploring the depths of human thought" (p. XIII). These are some of Warcollier's proposals that had a high interest for him.

As a chemical engineer and as a scientist, he focused on the data, and the experimental surroundings under which these phenomena can be obtained, trying in a way to separate the pure telepathic transmission from the "noise ratio," i.e. what prevents a clear reception, by distortions or disturbances of the message. Like any learned man of his time and facing a relatively unexplored field of investigation, he collects the facts, sorts and categorizes them, and finally offers interpretations in which the "subconscious" and neurophysiology held a special place. He does not fail to note like Myers had already noticed (1903) that sleep or drowsiness has a positive role. From this point of view, and surprisingly for an engineer, Warcollier was very aware of contemporary theories of consciousness, perception, and memory.

The book is divided into three big sections and ten chapters.

The first section, devoted to spontaneous telepathy, begins with a review of inquiries on spontaneous telepathy (Chapter 1). Warcollier refers to the numerous cases collected by the SPR in England (reported in the voluminous book *Phantasms of the Living*, 1882) and in France by Camille Flammarion (1914). On the basis of a personal telepathic case (pp. 15–17), as well as the cases published in France and in England, Warcollier objects to the negative conclusions of Vaschide (1908) who discussed and denied the accuracy of some of these results. He then puts forth several conditions favoring the emergence of spontaneous telepathy, according to the percipient's or agent's states of consciousness (sleep or awareness).

In Chapter 2, Warcollier tackles the problem of unintentional telepathic transference. Establishing his argument on the psychological theories of memory of his time (Abramowski, 1914, Bergson, 1919, Janet, 1889), he postulates the unconscious origin of telepathy, noting that it can be expressed not only through words or images, but also through bodily sensations (pain in the heart, throat contractions, etc). Yet, he adds:

> To avoid the complication of involving the Intelligence or the subconscious will, admitted by Myers in *Human Personality* (1903), by Geley in de *L'Inconscient au Conscient* (1919), I prefer to accept the temporary hypothesis that the telepathic transmission occurs as light, heat, electro-magnetic waves or sound, in all directions. The section "collective hallucinations" in *Phantasms of the Living* (Gurney, 1886, 1891) allows admitting this. (pp. 38–39)

According to this model, Warcollier postulates that the psychic waves are produced by brain neurons, even if there is not a specific center of telepathic emission.

He then moves on, in Chapter 3, to a classification of mental images. Agreeing with the English authors (Gurney, 1886, 1891), he notices that in spontaneous telepathy we find more visual than auditory hallucinations (271 vs. 85). Even though clumsily, Warcollier, in these early writings, is already bringing up the probability of a link between unconscious memory and telepathy. He writes:

> We must admit, as a starting point, that the images which appear to the mind of the percipient under the form of hallucinations, dreams, or more or less well-formed images, spring exclusively from *his own mind*, from his own conscious or subconscious memory. *There is no carrying of the visual impression from the agent to the percipient*, anymore than there is actual carrying of a letter of the alphabet from the sending apparatus of a telegraph office to the receiving office. The transmission of the message consists in making the same letter of the alphabet *appear*, but it already exists at the receiving apparatus, along with the others, before the transmission takes place. (pp. 46–47, italics in original text)

The deployment of extrasensory messages is thus analogous to the recollection of repressed memories from the depths of memory: Representations and skills unavailable in the ordinary waking states of consciousness re-emerge through a kind of resonance between the telepathic message and memories.

Warcollier then classifies visual imagery according to its relevance and usefulness to spontaneous telepathy; he distinguishes nine types of imagery, ranging from those that are quite frequent for habitual states of consciousness, but of little use for the emergence of telepathic information, to those that are more likely to help the percipient receive a message: dreams, positive or negative illusions, hypnagogic and hypnopompic illusion, hallucinations.

Of course, to contemporary psychologists or parapsychologists this may seem to be obvious, but we must recall that we are talking about ideas developed between 1906 to 1914, by a chemical engineer who had devoted a large part of his life to psychical research.

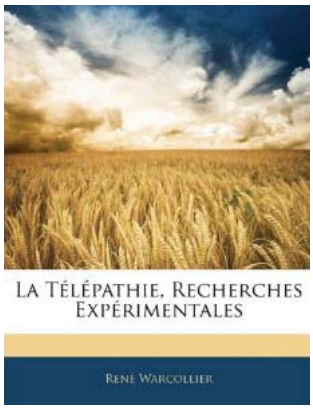
Following these chapters on spontaneous telepathy, Warcollier analyzes, in some detail, ways to provoke artificial imagery: stimulating the eye mechanically or electrically, utilizing magnets, crystal-gazing, and the action on nerve centers of alkaloids such as mescaline, or peyote (based on the work of Havelock-Ellis, 1898, and Rouhier, 1919).

In Chapter 5 he returns to spontaneous telepathy during the normal sleep state. Based on his own experiences, and experiences of friends and relatives, he seeks to outline differences between dreams that are mistaken as telepathic vs. those that most likely to be genuinely telepathic. Here's an example of one of his wife's telepathic dreams:

> One morning, when my mother came into our bedroom, my wife wakes up. She told us the following dream: She saw our housemaid introducing a young man in the house, and this man left the house about 2 a.m. During the day, my mother reminds me of this dream, which impressed her very much. In fact, a neighborhood person had warned her yesterday that the maid had brought a young man home. She never suspected the housemaid to whom we entrust the baby. Very moved by this news, she had been thinking about it all night and was so surprised this morning to hear my spouse telling her dream. We found that the maid had really brought into the house a butcher the previous day. (pp. 116–117)

Curiously, Warcollier concludes this section stating that dreams are "unfit to prove the existence of telepathy" although sleep is seen by him as a good state facilitating the emergence of extrasensory messages.

Starting with Chapter 6, the second section is dedicated to experimental telepathy. It begins with a description of transference and reception of so-called "daily telepathy," when for example two persons concomitantly express the same ideas. Warcollier gives a comprehensive summary of all possible experimental protocols exploring this kind of telepathy, including playing cards (Richet, 1884), automatic writing, dowsing, lottery balls (Schrenk-Notzing, 1891). Following a detailed analysis of positive and negative results obtained by each of them, he concludes: "Telepathy is not an intellectual power. . . . When intellectual activity is at its height, neurons communicate by contiguity: The lines are busy, according to the expression devoted in telephony" (p. 186). This is no doubt intended as a mere metaphor, rather than neurophysiologic reality, but Warcollier did attach much importance to the parallel between telephones and "mental radio," as did Sinclair (1930). Like other scientists of his time, he was fascinated and amazed by the technological prowess of the wireless



transmission. It is therefore normal that this model compels recognition for everything regarding the transfer of information from one mind to another or from one brain to another brain.

To illustrate telepathy during sleep-onset, he relates his early experiences, in 1906, trying to develop "inner vision" by observing his hypnagogic imagery (Chapter 7). This leads him to suggest how to develop paranormal capacities by relaxing mind and body. Then he makes a statistical and comparative analysis of his own results of seven trials, especially with Mr. Archat as an agent and Warcollier as a percipient. These series include short and long (790 Km) tests, from September 1906 to July 1907 (pp. 192–208).

The third section is devoted to an interpretation of the results. After contrasting his results with those obtained by Usher and Burt (1909) in their investigation of long-distance transmission of card symbols, he examines the different components of telepathic transmission: ideas, sensations, visual or kinesthetic feelings, numbers, concepts, states of mind, moods. He concludes that it is not the representations themselves that are transmitted, but rather the affective states associated with them. He describes characteristics of "positive" drawings, which could be good targets, such as those with contrast and strong feeling. This positive quality is related to the idea of movement and moods, in the drawing, as well as in the personality of the agent. In the same context, representations, which are too abstract, based on numbers, general concepts, and symbols, are considered to inhibit transmission and reception.

For Warcollier the *primum movens* of telepathy is the transmission of sensation and the possibility for the message to enter into resonance with images stored in the percipient's memory. The message sent by the agent emerges in the percipient's mind by successive waves, a process which is responsible for message fragmentation. As we said before, Warcollier argues against the hypothesis of direct transmission of the image, through some form of mental projection—viewing this as impossible as the transference of a "pure idea." He gives far more credence to the hypothesis of resonance, and attempts to illustrate this again by the physical phenomenon of sound waves and radio transmission. He concludes that "only the elements common to the agent and percipient" are transmitted, a bit like the vibrations from a tuning fork will vibrate in resonance to another tune. "No transfer of new knowledge occurs from the agent to the percipient, but rather an awakening, a phenomenon of resonance, vibratory

states similar or identical" (p. 288). In this context, distance between agent and percipient does not affect the transmission/reception of telepathy.

Warcollier points very often in this book to similarities and differences between TSF (wireless telegraphy) and radio waves: The latter are clear and direct, but sensitive to distance, while telepathic transmission is less direct, but independent of distance. He tries to analyze, through this approach, the case of Leonie, who was remotely hypnotized by Janet at a distance of two kilometers (in 1893).

Warcollier ends this chapter with reflections about the relationship between mind and matter, and expresses his intuition that the riddle of what is going on between agent and percipient will be resolved at the subatomic level.

In the next chapter, Warcollier summarizes the difficulties that may be encountered in telepathic transmissions. The frequently observed distortions of messages are due to the conditions of the experiment and the flow of free thought association in both the agent's and the percipient's minds.

Warcollier, who had extensive experience with telepathy, was able to pinpoint the conditions of its transmission, conditions opening the psyche of the agent and percipient to the subconscious, thus promoting access to the "subliminal self" of Myers who said that "the telepathic message generally starts from, and generally impinges upon, a subconscious or submerged stratum in both agent and percipient" (Myers, 1903(2):5). Among those facilitating conditions are the role of surprise, the hypnotic state, the focus of attention, the emotional aspect of the message, sympathy between senders and receivers.

Mentioning the works of Abramowski (1898) on paramnesia and Claparède (1903) on free association, Warcollier shows, in the Conclusion**,** that we find the same difficulties in experimental telepathy as in experimental psychology.

Against a spiritualist interpretation of telepathy, he shows the plausibility and efficiency of a hypothesis that telepathy is a phenomenon occurring between human minds. Speculating about the consequences of telepathy on groups and society, he concludes, in a very lyrical way:

> Telepathy is the natural law which we unconsciously obey when we seek to form groups, to assist one another, to join together. We communicate, we do not excommunicate. . . . We are the same man, I am you and you are me. Or to express it in modern terms, we are the electrons of the atom of Humanity. (p. 353)

I hope it is apparent by now that *La Télépathie* is an impressive work, a patient and sustained collection of cases, tests, hypotheses, and analyses, focused on both spontaneous and intentional telepathic phenomena. For the next forty years of his life up until his death in 1962, Warcollier continued his telepathy investigations—confirming, modifying, retracting, developing,

improving, and deepening his early and "anticipatory" visions, as expressed in *La Télépathie.* His elaborations on telepathic phenomena, through thousands of trials, contributed to a more comprehensive understanding of the process underlying the sending and the receiving of drawings.

Certainly, there were a number of conflicting or contradictory ideas in Warcollier's work: This is understandable, given the richness and complexity of the mechanisms he sought to describe over the course of several decades. But that notwithstanding, *La Télépathie* must be considered as a major precursor to the scientific investigation of this subject. Warcollier succeeded in establishing in this book the foundations for a psychological study of telepathic transmission of drawings. His research contributed to the recognition of telepathy as a clinical reality—so much so that he could propose an entire nosology, thus preparing the way for later researchers, whether scientists or (as he hoped) psychologists.

When he joined IMI in 1919, he naturally continued his investigations. In this Institute during this period between the two wars we find three personalities conducting psychic experiments over long periods of time: Gustave Geley (1919–1924), Eugene Osty (1924–1938), and Rene Warcollier (1921–1962). In the field of qualitative parapsychology, Warcollier rapidly became a figure as eminent as Rhine in the field of quantitative parapsychology. In 1922, after an appeal to 500 persons, he managed to obtain a stable and highly motivated group of 20 persons who agreed to participate in "methodical experimentation." They met regularly once a week for at least two years, participating in experiments that involved transmission of feelings, printed words, and playing cards, but above all of drawings. Then, Warcollier held a telepathic training group until the end of the 1950s.

During his psychology studies at the University of Colombia, Gardner Murphy (American psychologist and parapsychologist) had heard of Warcollier and his work and groups. In 1923, he decided to take advantage of a Congress of Psychology at Warsaw to meet René Warcollier in Paris. In his obituary of Warcollier, Gardner wrote: "I had the privilege and pleasure to read *La Télépathie*, by Rene Warcollier. It was obviously the important work in the field of experimental studies in telepathy" (1962). Their meeting was the beginning of a great friendship and long collaboration, through transatlantic telepathic experiments between New York and Paris. Thanks to Murphy, a part of *La Télépathie* and some articles (of the 56 published by Warcollier in *La Revue Métapsychique* between 1924 and 1962), were made available for the English-speaking world (Gardner, 1938, 1948, Warcollier, 1938, 1948).

He was also ahead of his time by situating telepathy within a functional model of memory, a model some parapsychologists use today to explain the ESP process (Irwin, 1979, Roll, 1966). Warcollier postulated a resonance between the message sent and subconscious information in the mind of the

percipient. This hypothesis introduced some difficulties for Warcollier, in the way he dealt with telepathic information and the role of feelings and emotions. It seems obvious today that strong affect has an impact on the transmission, which allows or distorts the contents of the message; Warcollier wanted to underestimate the role of affect in this dynamic of sending/receiving, devoting his attention to more "controllable" aspects, such as color, movement, contrast, etc. Indeed, the use of drawings in telepathic sessions generally allowed him to keep strong emotions at a distance.

Very soon in his career as a metapsychiste, his experimental method and what he found made Warcollier a pioneer who opened the way to further discoveries: His own he recounted in *La Revue Métapsychique*, as well as those of other parapsychologists. Some researchers today consider him as the *true father* of remote viewing, which owes many, if not most, of its techniques to his work, but also to others (Targ & Harary, 1984, Targ & Katra, 2001, Swann, 2001). Regarding the nature of the telepathic transmission (its essence, its substratum, or its *motus operandi*), Warcollier can only offer speculative assumptions. He was however an astute observer and has demonstrated a great relevance in the study of the emergence and deployment of message in the percipient mind and drawings. Similarly he could point out the conditions for this emergence and deployment and show the similarities between telepathy functioning and the psychology of perception.

In France, following Warcollier's work, Henri Marcotte (1977) has facilitated training groups in telepathy. Instead of drawings, Marcotte proposed stories and scenarios, introducing the time dimension. In parallel, he refined the techniques of transmission and reception. Following him, I went further by introducing in the group telepathic training (Si Ahmed, 1990, 2006), a psychoanalytic understanding of the dynamics and the processes involved in sending and receiving messages.

To end this review, I would like to quote Eileen Garrett: Warcollier "had never wavered from his preferred area of research, and current trends indicate a re-examination of the work in which he had been a pioneer" (1962).

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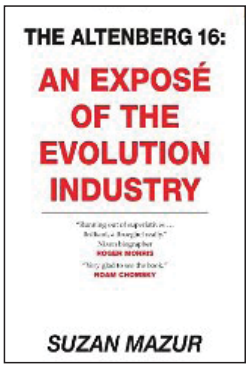
Further Books of Note

The Altenberg 16: An Exposé of the Evolution Industry by Suzan Mazur. Scoop Media (New Zealand), 2009; North Atlantic Books, 2010. 343 pp. $25 (paperback). ISBN 9781556439247.

This is a profoundly disappointing book. Its genesis is an article of March 2008 (which forms Chapter 2 of this book) breaking the news of an impending meeting of "16 biologists and philosophers of rock star stature—**let's call them 'the Altenberg 16'**—who recognize that the theory of evolution which most practicing biologists accept . . . is inadequate in explaining our existence."

That promised much, and when this followup book was announced I was eager to read it. Unfortunately, the book doesn't deliver on the article's promise. As a result, the author really has nothing useful to add to the article, and the book is replete with expressions of resentment that she was not invited to the actual meeting. Apart from the reprinted article, the book consists of interviews Mazur had at various times with some of those at the meeting and also with other evolutionists. Trivialities of email greetings are reproduced; there was apparently no effort to edit out irrelevancies. Nowhere are things pulled together to deliver a meaningful message. Chapter 7, "The One and Only Richard Lewontin," illustrates that there is actually nothing startlingly new going on.



Despite that, the book's Introduction suggests a major intellectual breakthrough: "the scientific community . . . knows that natural selection has little to do with long-term changes in populations. And that self-assembly and

self-organization are real, that is, matter can form without a genetic recipe." But nowhere does a lay reader discover what those "self-" things actually are, how they are supposed to work. And the official statement issued at the end of the conference (http://j.mp/a7yh0S) doesn't mention self- anything: It describes ideas for incremental improvements in esoteric details of the theory within which natural selection plays the same role as it has been said to do for decades; there is no abandonment or overthrow of a paradigm:

> The new concepts include (but are not limited to): evolvability, developmental plasticity, phenotypic and genetic accommodation, punctuated evolution, phenotypic innovation, facilitated variation, epigenetic inheritance, and multi-level selection.
>
> By incorporating these new results and insights into our understanding of evolution, we believe that the explanatory power of evolutionary theory is greatly expanded within biology and beyond.

I heartily dislike writing such negative reviews, but potential readers enticed by the hype, as I was, deserve to be forewarned.

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The Lost City of Z: A Tale of Deadly Obsession in the Amazon by David Grann. New York: Vintage Departures, 2010. 428 pp., illus., maps, refs., index. $15.95 (paperback). ISBN 9781400078455. Random House, 2009. $27.50 (hardcover). ISBN 9780385513531. [Review reprinted from *Pre-Columbiana: A Journal of Long-Distance Contacts,* 4(3&4), 2008–2010].

Col. Percy Fawcett (1867–ca. 1925) was the quintessential English amateur explorer of the first half of the twentieth century, a man of incredible personal toughness and persistence—and an impossibly exigent taskmaster, brooking no weakness from his subordinates in the field. He became fascinated by Spanish-colonial chronicles of the search for El Dorado in Northwest South America and was further stimulated by the American politician Hiram Bingham III's (1875–1956) 1912 rediscovery of the Incan "lost city" of Machu Picchu in Peru's

eastern Andes (Heaney, 2010). Having himself seen a manuscript in Brazil's National Library that spoke of a ruined city of stone near high mountains in the interior of the country, viewed in 1753, Fawcett was inspired to search for this reported mysterious abandoned settlement. He dubbed the supposed site "Z," and his quest to find Z occupied most of his middle years, ultimately leading to the loss of three lives: his own and those of one of his sons and the son's best friend.

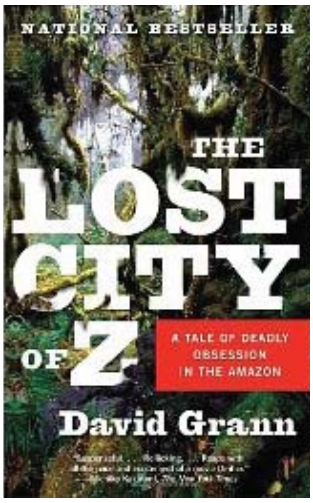
Fawcett's fascination with forgotten treasure had been first sparked by an experience as a young military man in Ceylon (Sri Lanka). There, a local man presented him with a treasure map that led him to seek a purported cache of uncut gems and gold hidden in a cave in the interior. Fawcett found the cave but nothing of interest in it; nevertheless, his fire had been ignited.

Fawcett served as a Lieutenant Colonel in combat during World War I, and after experiencing that conflict's horrors he took up Spiritualism. Two Spiritualist mediums assured him that Z existed and that the lost city was full of jewels. It may be noted at this point that Fawcett was a friend of H. Rider Haggard (1856–1925), the enormously popular author of fantastic adventure novels such as *She* (1887), which involved discovery of the lost African Kingdom of Kôr. Haggard's more famous novel, *King Solomon's Mines* (1885), involved the search for, and discovery of, the ancient Jewish monarch's fabled mines with their heaps of gemstones.

Fawcett clearly suffered from narcissistic personality disorder, with paranoid and delusional tendencies. Over time, his early efforts as a simple field surveyor and scientist along the Bolivian border were replaced by obsessive treks into Brazil's notoriously resistant Matto Grasso jungles. As his brutally difficult expeditions turned up no Z, and as he aged, Fawcett's initial vision of modest scientific investigation evolved into one focused on imagined treasures and then into one in which he perceived his elusive Z as being an Atlantean cradle of civilization and one of Theosophist Madame Helena Blavatsky's "White Lodges" where one could attain transcendence. (Beginning in the late twentieth century, Fawcett's handful of occult writings have aroused a number of cultic and individual seekers of this alleged magical portal.)

The New Yorker staff writer David Grann became intrigued by the Fawcett story, which had been a journalistic big deal at the time of the explorer's vanishing in back-country Brazil in 1925. A number of others had endeavored to ascertain what had ultimately happened to the Englishman during his final expedition, but all had failed. Grann decided to take up the task once again. He took into account the fact that Fawcett had deliberately falsified coordinates of his routes in order to prevent potential "claim-jumping."

In the 1950s, Fawcett's son Brian had flown over the area of his father's disappearance and had spotted a "stone city" on a ridge; but the "city" turned



out to be merely naturally eroded sandstone formations. On Grann's expedition decades later, that writer traveled northward from the Brazilian frontier settlement of Cuibá and then eastward into the upper Rio Xingú drainage, making local inquiries and following his reconstruction of the Fawcett route. At one point, he arrived at a plateau reminiscent of the mountain mentioned in the report of 1753. Atop, his guide showed Grann natural rock columns and a natural arch; these could have inspired the eighteenth-century tale of a ruined city.

The Kalapalo Indians of the region came to admit that they had killed Fawcett, his son, and the companion. But later they denied this, alleging instead that the party of Englishmen had gone on eastward from Kalapalo territory, toward the Rio das Mortes ("River of the Dead Ones"), a tributary of the upper Rio Araguaia. It was in these neighboring lands, occupied by hostile natives, that the killing had been perpetrated, now said the Kalapalo. At any rate, it seems clear that the Fawcett party was indeed murdered by Indians in this general part of Brazil, presumably without their ever having found Z or any other abandoned city.

Was there such a city out there? If one is thinking of a Machu Picchu–like community featuring monuments and dwellings in stone masonry, the answer is almost certainly no. However, large sites of human settlement involving extensive earthworks have, for decades, been coming to be recognized in regions of South America that earlier had been perceived as always having been sparsely populated by relatively primitive peoples. Such earthworks—ridged fields, canals, causeways, and mounds—were mostly in low-lying plains subject to periodic flooding and were difficult to detect on the forested ground. They were first recognized from the air, in the Mojos of eastern Bolivia, more than 40 years ago (e.g., Denevan, 1966, Mann, 2005:3–12). Remarkably, such works have also been discovered in the upper Xingú drainage (Grann, pp. 310–14, Heckenberger, 2005, Mann, 2008), particularly surprising in view of the fact that in post-1500 times that region has been a refugium for some of the most primitive tribes of the Amazon Basin. More recently still, vast areas of the upper Purús drainage of Brazil near the Bolivian border have been recognized as displaying huge, precisely geometrical, human-made, ditch-and-embankment "geoglyphs" as well as arrow-straight roads (Pärssinen, Schann, & Ranzi, 2009). These all are regions in or near which Fawcett explored; but, ironically,

he (like everyone else at the time) never recognized that he was traveling past striking manifestations of an advanced ancient culture.[1]

Notes

[1] These rainforest societies seem to have depended on raised fields in the flood-prone flats and, atop the bluffs, on the recently archaeologically recognized *terra preta do indio*, a dark, partially artificial soil that had resulted from past peoples' augmenting the natural low-nutrient lateritic soil with nutrient-absorbing charcoal and other organic materials.

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Articles of Interest

Creation of a Bacterial Cell Controlled by a Chemically Synthesized Genome by D. G. Gibson, John I. Glass, Carole Lartigue, Vladimir N. Nosko, Ray-Yuan Chuang, et al., *Science, 329*(5987), July 2, 2010, 52–56.

Researchers led by J. Craig Venter (a pioneer in genome sequencing) recently published a landmark paper in the journal *Science*. While Dr. Venter himself was very precise when speaking about this advance in interviews, the popular press has claimed the "Creation of Life." It's important to discern

precisely what has, and has not, yet been accomplished. What the group did was to take the text of an entire bacterial genome, residing on a computer, and use this information to synthesize a long piece of DNA corresponding to this data sequence. They then took the DNA out of another bacterial strain, and instead put in the synthetic DNA they had made, showing that the resulting hijacked organism is viable, and, as expected, takes on the behavior of the donor bacterium as the host's proteins are replaced over time with the products of the newly inserted DNA. Alongside the numerous tricky practical details that their work had to overcome to make the procedure work, the major advance here is the ability to create a whole, functional genome, from scratch, using the information contained on a computer.

The ability to construct working genomes from a computer-stored sequence is hugely important. This proof-of-principle application paves the way to the near future where genetic material is readily manipulated by computer-aided tools in silico, and then produced as real DNA for use in real organisms. While the former is already routine, the latter will really take off when the arduous steps described in this *Science* paper are inevitably encapsulated into a turnkey system that can make any arbitrary DNA sequence of genome-length. This is somewhat analogous to the ability to manipulate text in a word processor and print it out as books, or define mechanical parts in a computer-aided-design software package and download the information to a 3D printer to have them manufactured for use in the real world.

As important as this advance is, it is not creating life. First, the genome used (stored on the computer) was not designed from scratch—it was obtained by sequencing an existing organism (the donor bacterium). Second, the genetic material was inserted into an existing cell, with all of its unimaginable complexity of physiological processes necessary to interpret the DNA and carry out its instructions to perform the functions of life. The fundamental efforts of workers in this field are akin to reverse-engineering a complex artifact arriving from an advanced civilization. We are learning about how the information and matter–energy streams of living systems work, but are not yet at the point of building everything from first principles. The advance is similar to finding a set of alien supercomputers, and figuring out how to copy the software from one, store it on a disc, make minor changes to remove/add a few pieces of known function, and download it to another of their computers. A superb advance no doubt, but not yet equivalent to writing the code oneself, or building one of the advanced machines in our own workshops.

Richard Feynman said that we really only understand something when we can make one ourselves. Thus, the two fundamental hurdles which must still be pursued are 1) the ability to put together an organism from scratch—not merely rearranging and making minor edits to an existing genome, but

rationally designing the contents of the genome to produce an organism with desired functionality, and 2) the ability to assemble an environment within which a piece of DNA can be anything more than "static code"—to make a cell that can read, and carry out, DNA. Both of these (especially #2) are well beyond current possibilities, facing important (but likely not insurmountable) barriers of technology and fundamental limits imposed by complexity theory and dynamical systems theory (chaos). Truly creating life requires the ability to understand and manipulate the genetic and epigenetic information networks that control biological systems, and the capability to put together "devices" of astounding complexity in three-dimensional space. While existing living beings are operated by genetic material that is the result of the progressive tinkering of evolutionary processes, a fundamental shift in the biosphere will result from the ability to rationally design information sequences that can be carried out by a massively parallel distributed system such as the cell and give rise to predictable (and desired) behavior. Pursuit of these efforts will enable fascinating advances in basic biology and unimagined advances in biomedicine.

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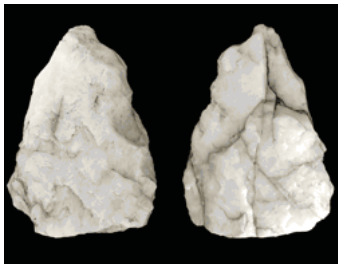
Bon Voyage, Caveman by Brendan Borrell. *Archaeology 63*(3), May/June, 2010, 9, 54.

In recent years, a revolution has been taking place in our understanding of the antiquity of the use of seagoing watercraft on the part of humans—not only behaviorally modern *Homo sapiens* but also more primitive species of hominens. Archaeology and genetics now support the scenario that groups of *H. sapiens* were moving out of Africa and along the southern coasts of Asia by boat before 70,000 years ago, reaching Australia in but a matter of a few millennia. Up until recently, the earliest known clear traces of human presence on Mediterranean islands such as Crete was about 12,000 years ago. Now, however, that date has been pushed back 117,000 years.



This quartz hand-ax, which was left on the island of Crete between 130,000 and 700,000 years ago, shows that pre-modern humans boated across open seas. (Photo: Thomas Strasser)

Stone hand axes, scrapers, and cores found on Crete are Acheulean in style, representing types of tools that are associated with *sapiens* predecessor *Homo erectus*. Geological dating puts the manufacture of these tools at no less than 130,000 years ago, with the possibility (on the basis of style) that they may be as much as 700,000 years old. To reach Crete, a minimum water crossing of 64 kilometers (40 miles) of open sea would have been required, strongly suggesting use of watercraft. Flores, Indonesia, furnishes even older evidence of *H. erectus* waterborne travel, at circa 840,000 years ago. To make the crossing from the closest other Indonesian island, *erectus* would have required either watercraft or a freak accidental drift on a natural raft of vegetation. The fact that many non-human creatures did not cross Wallace's Line, the first maritime water gap on the way from the low–sea-level Asian mainland to Flores, makes the accidental drift hypothesis less likely (Strasser, Panagopoulou, Runnels, Murray, Thompson, et al., 2010).

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