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EDITORIAL

I suppose most of us have wondered at one time or another whether a person's apparently defining character traits are fixed, or whether people are capable of meaningful and deep change. Perhaps the issue arises most frequently in the context of intimate personal relationships, but it seems also to be a recurring topic of practical or professional concern to those studying scientific anomalies.

I subscribe to several email listserves, one of which is a forum for psi researchers, and this topic routinely forms the background for discussion. It usually happens in the following way. First, some intransigent skeptic (or, perhaps more accurately, passionate psi-denier) publishes something outrageous (if not libelous) about a serious and diligent member of the parapsychological research community. It might be a severe blast of ad hominem attacks lacking even the appearance of empirical support. Or, it might be an ostensibly evidence-supported critique which only the well-informed would know to be inexcusably one-sided if not blatantly dishonest. The former happens frequently to Rupert Sheldrake. The latter is a specialty of career skeptics who posture as careful researchers themselves but who sedulously avoid discussing evidence that's most difficult to explain away.[1] When a new such attack surfaces, listserve members usually respond with a furious flurry of postings about how best to respond and whether to respond at all.

A related manifestation of skeptical intransigence is the manner in which Wikipedia biographies of parapsychologists, LENR researchers, and others have been hijacked by a collection of Wikipedia insiders who have rewritten the biographies, replaced accurate information with falsehoods, and thwarted all attempts to correct the misrepresentations. Here, too, victims of this treatment wonder how—or whether there's any point in even trying—to counter the damage.

Of course, many *JSE* readers are familiar with the manifestations of egregious and obdurate skepticism in areas of frontier science. As far as parapsychology is concerned, one need only examine the writings and public pronouncements of Richard Wiseman, Michael Shermer, James Alcock, and others, many of which are documented on the recent successor to Sheldrake's Skeptical Investigations website http://www.skepticalaboutskeptics.org/

Additional and plentiful examples of recalcitrant skepticism also deface the literature in a controversial area of research mentioned only infrequently in this journal—namely, dissociative disorders generally

and dissociative identity disorder[2] in particular. Consider, for example, the writings of Elizabeth Loftus, Richard Ofshe, and others who attacked mental health professionals treating dissociative disorders by appealing to a condition they dubbed *false memory syndrome*. The problem is not simply that this so-called condition has never been recognized in either the DSM (*Diagnostic and Statistical Manual of Mental Health Disorders*) or ICD (*International Classification of Diseases*). What many don't seem to realize is that in medicine, naming a syndrome is easy; virtually anyone can do it. And unlike the classification of *disease*, it requires no broad professional consensus. In fact, the designation of false memory syndrome has no greater antecedent credibility or authority than capricious syndromes one can easily and whimsically concoct at the drop of a hat. I've done this myself. Consider, for example, *premature seatbelt release syndrome*, describing the behavior of impatient arriving airline passengers, or *delusions of invisibility syndrome*, to characterize drivers of cars who think others can't see them while they pick their nose, or *golden voice syndrome*, characterizing those who, when singing in the shower, greatly overestimate their vocal talent.

I realize that a false memory syndrome, if real, would have more serious social consequences than those I invented (although I suppose one could argue that golden voice syndrome helps account for numerous abominations promulgated by the recording industry). What matters here, though, is that my whimsical "syndromes" probably rest on a firmer empirical foundation than false memory syndrome. As Ken Pope (among others) has noted (Pope 1996, 1997), those who allege that there's a false memory syndrome have never indicated how it was determined that there's a widespread false memory problem (much less an "epidemic" of false memory reports, as some like to claim). By contrast, there's plenty of evidence that people engage in the seat-belt unfastening, nose-picking, and shower-singing behavior referred to above. But the only way to determine whether there's a false memory syndrome is, first, to determine whether certain memory reports are false. However, there's no clear or reliable procedure to follow here, even for police investigators. And one can be sure that the vocal skeptics in question have neither conducted nor sponsored anything as thorough as police work in connection with the memory reports of therapy patients.

To support the claim that there's an excess (or epidemic) of false memory reports in therapy, one must conduct detailed, sensitive, in-depth, and sweeping studies of a kind that simply have not been carried out. In fact, those who claim that there's a false memory syndrome have not even met, much less interviewed, the majority of those who apparently recovered memories in therapy. So I'd suggest that when someone alleges that there's a false memory syndrome, a proper response is to ask how one can detect

the presence of that syndrome without ever meeting the people allegedly suffering from it. Another is to ask by what process they determined that the testimony of those denying the memory reports is more credible and less prone to motivated distortion than the testimony of therapy patients.

At any rate, the hubbub over false memory syndrome was a big deal a couple of decades ago. But despite obviously sensible criticisms from several quarters, one still finds glib skeptical critics appealing to false memory syndrome (for a critical review of one recent example, see Cardeña 2014). And for further examples of the wretched quality of skeptical reasoning surrounding the study of dissociation, see Braude (1995, 1998, 2014). In fact, there are strikingly close parallels between intransigent skepticism of psi research and skepticism of research into dissociative disorders (Braude 2014).

Now when the targets of shoddy skeptical attacks discuss response strategies, what they often ponder is whether there's any point in responding to the attacks, not just because the efforts would distract from more constructive research activity, but because there's no reason to think that anyone's opinion—or at least that of the attacker—could conceivably change, even in the face of compelling rational argument. And even more cynically, some speculate that, even if a critic's *beliefs* can be influenced by rational argument, his/her *behavior* may remain the same. That is, some critics may simply yield to overriding professional and social pressures to maintain their skeptical reputation, no matter what they believe personally. I know that many SSE members (including myself) have often noted how academicians may denounce some area of frontier science in public but confess in private to quite different points of view.

In any case, whatever psychological struggles might be occurring in the minds of hardcore skeptics, the issue here is: How should one deal with the continued barrage of poorly reasoned (if not simply dishonest) attacks on one's research, especially when there's ample evidence that the critics would never change their minds or behavior, no matter how sensibly one replies to them? After all, many anomalies researchers have noted the almost evangelical fervor of some of the criticism often directed against their work. Similarly, critics sometimes state their disagreements, their differences of opinion, as dogma, usually seasoned with a generous helping of contempt or sarcasm, and they do this with a certitude totally disproportionate to their knowledge of the facts. One would think that the transparent and intense hostility of such criticism is simply inappropriate to an objective scientific inquiry. In these cases it seems that anomalists aren't simply confronting opposing theoretical positions when they're under such emotionally charged and intellectually dishonest attack. Instead, they're apparently in conflict

with articles of faith, and engaged in something closer to a religious battle than an honest and dispassionate appraisal of rival hypotheses. But in that case, is there any point in trying to mount a rational defense and respond with data and arguments? Wouldn't that just be a futile attempt to reason people out of positions they hadn't been reasoned into?

I can't pretend to know the answer to this, but there are certainly grounds for wondering whether it's worth making a serious effort to respond to those we know are entrenched in their skepticism and not likely to play fair in a public dialogue. Consider, for example, the case of the late Charles Honorton, who spent years debating critics (primarily Ray Hyman) over the merits of ganzfeld experiments, time he could have devoted productively to further experimentation or (say) the search for superstar subjects. I seriously question whether this was time well-spent. Hyman never identified problems with the ganzfeld experiments that could be linked to those experiments' positive results. After much wrangling, Honorton and Hyman eventually jointly endorsed a revised, autoganzfeld (i.e. computerized) experimental design that was supposed to avoid the methodological "flaws"—including the risk of sensory leakage—Hyman claimed to have identified in the earlier series. But (presumably contrary to what Hyman expected) the new tests achieved scores at virtually the same level of significance as that yielded by the earlier series. Nevertheless, Hyman maintained an unwavering skepticism about the evidence for ESP, and he continued to argue that orthodox science needn't pay any attention to the work of psi researchers (Hyman 1989). So my take on the overall result of Honorton's efforts to engage Hyman in a real and honorable dialogue is that there was no positive outcome even remotely commensurate with the effort.[3] There's no reason to think that many (if any) skeptical opinions about ESP were revised as a consequence, and there's no reason to think that Honorton or anyone else learned anything new or important about ESP from all that work.

We're often reminded that patience is a virtue, but I'm not sure that even patience-virtuosi could successfully engage career skeptics in a profitable dialogue. However, I also think that anomalists have no choice but to do the best work they can and wait for the chorus of irrational critics to disappear. Hopefully, that development is something they'll live to see. But if history is any guide, this change won't happen entirely and it won't happen soon. After all, the shoddy dialectical strategies favored by opportunistic career skeptics haven't changed in more than a century (Braude 1997, 2014). Still, I have faith in the steady (if not unimpeded) progress of science, and I believe the truth will out, at least eventually. At any rate, my hope—and possibly naïve expectation—is that dishonest skepticism has no more

chance of succeeding than Mussolini's notorious and futile attempt to outlaw handshakes.[4]

. . . . . .

I would like, once again, to conclude my end-of-year Editorial with sincere thanks to my hardworking team of Associate Editors and our still regrettably small but trusty stable of referees. Despite my repeated demands on their valuable time, I count on them to help maintain the high standards of the *JSE*, and they do a really splendid job. And of course, kudos (as usual) to Managing Editor Kathleen Erickson, who keeps the whole machine running smoothly and thereby somehow succeeds at the thankless and heroic task of preserving the illusion of my editorial competence.

Stephen Braude

Notes

[1] For an expose of one such treatise, see Braude (1985).
[2] Formerly Multiple Personality Disorder.
[3] For details, see Bem (1994), Bem and Honorton (1994), and Bem, Palmer, and Broughton (2001).
[4] For those unaware of this, Mussolini thought Italians needed some toughening up, and so he decreed that handshakes were henceforth illegal and were to be replaced by the Roman salute (see Kertzer 2014).

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RESEARCH ARTICLE

Psychological Evaluation of American Children Who Report Memories of Previous Lives

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Abstract—Some young children claim to have memories of a previous life, and they often show behaviors that appear related to the memories. This pilot study examined the psychological functioning of such children in the United States. Fifteen participants, ages 3–6 years, underwent testing with the Stanford-Binet Intelligence Scale (fourth edition) and the Children's Apperception Test. Their parents completed the Survey Form of the Vineland Adaptive Behavior Scales, the Child Behavior Checklist, the Child Dissociative Checklist, and the Family Questionnaire. The children's composite intelligence scores on the Stanford-Binet were greater than one standard deviation above the mean, with relative strengths in verbal reasoning and quantitative reasoning. On the Vineland Adaptive Behavior Scales, the children scored significantly above average in daily living skills, motor skills, and on the overall composite score. Thirteen of the 15 children obtained low scores on the Child Dissociative Checklist, indicating no dissociative thought patterns in most participants. The Child Behavior Checklist averages all fell within the normal range, revealing no clinically significant behavior problems. Results on the Children's Apperception Test revealed no unusual themes, and the families did not show any distinct patterns of functioning on the Family Questionnaire. Young children who claim to remember previous lives show high intelligence, and testing revealed no evidence that their reports arise from psychopathology.

Keywords: psychological assessment—memory—pre-school children—reincarnation

In a syndrome described by Stevenson (e.g., 1960, 2000a, 2001), some young children are noted to claim memories of a previous life. They tend to start at a very early age, at 2–3 years old, and usually stop making such statements by the age of 6 or 7. During the time that they are reporting

the memories, some children show behaviors that appear connected to the reports, either by displaying fears regarding the mode of death that they have described (Stevenson 1990) or by demonstrating themes in their play that relate to the statements (Stevenson 2000b). Some say that they were deceased family members, while others describe having been a stranger. Though parents in other cultures with a general belief in reincarnation may accept their children's statements without question, American parents are upset at times by the symptoms their children display for a number of reasons. Some children become distraught in talking about their purported memories, while parents in other cases are perplexed by the idea of previous lives or are concerned that their children are abnormal. Other parents are confronted with a situation that is inconsistent with their religious beliefs.

Such cases have been reported in cultures with a belief in reincarnation for many years, initially by Stevenson and then by others as well (e.g., Mills, Haraldsson, & Keil 1994). In addition, Stevenson (2003) published a series of European cases, and reports of American cases have also been described (Stevenson 1983, Tucker 2005, 2013).

The current study is the first to include psychological evaluations of children making these statements who are from a culture without a general belief in reincarnation (i.e. the United States) and the first to test participants during the early ages when children typically report the memories. Haraldsson performed psychological testing on three groups of older subjects from cultures with a belief in reincarnation, two in Sri Lanka and one in Lebanon. In the first Sri Lankan set (Haraldsson 1995), 23 children between the ages of 7 and 13 years old who had reported memories of a previous life when they were younger were compared to 23 controls who had not made such reports. The subjects had greater verbal skills and better memory than the controls, performed much better in school, were more socially active, but were not more suggestible. They obtained a higher problem score on the parent Child Behavior Checklist (CBCL) but not on the Teacher's Report Form.

Similar results were obtained with a second group of 27 children in Sri Lanka (Haraldsson, Fowler, & Periyannanpillai 2000). They performed better in school than controls did, and they were not more suggestible. They showed more behavioral problems on the CBCL, including oppositional traits and obsessional and perfectionistic traits. On the Childhood Dissociative Checklist (CDC), they scored higher than controls, particularly in the areas of rapid changes in personality and frequent daydreaming.

In the study in Lebanon, Haraldsson (2003) found that 30 children who had reported memories of previous lives obtained higher scores for daydreaming, attention-seeking, and dissociation than the controls did,

but their overall scores on the CDC were very low (mean score of 1.47), indicating no clinically relevant problems. This group did not differ from controls on cognitive tests or school performance.

When these cases occur in areas with a general belief in reincarnation, they can be ascribed to cultural factors (Brody 1979). In the West, however, cases have often occurred in families that did not believe in reincarnation before the children made their statements, and the psychological factors involved may be very different compared to cases from cultures with a predominant belief in reincarnation. Given the lack of cultural reinforcement for the children's statements, the question arises whether the reports may occur due to psychopathology in the Western subjects, and the focus of this pilot study was to determine if the children have psychological features that would distinguish them from children who do not make such statements.

Methods

Participants

The basic inclusion criteria for the study were that a child had made repeated statements about remembering a previous life, that he or she was 36–83 months of age, and that a parent was available who had heard the statements and could complete the questionnaires of the study. Most of the participants were recruited after their parents contacted our office about their children. In addition, three parents were contacted after they posted information regarding their children on a website about reincarnation; one was referred by a colleague and one by a social contact; and one parent spoke with the first author after hearing a lecture about such children.

Of the 15 children enrolled in the study, eight were boys, and seven were girls. Their mean age was 62 months, and they came from various parts of the United States. Seven were from the Southeast; four were from the Midwest; two were from the West Coast; and two were from Colorado. Their parents' education levels varied. All 30 parents had attended at least some college, but only 12 had obtained four-year degrees. Of those 12, six had obtained post-graduate degrees, with two being doctorates and the other four being master's degrees.

Measures

Stanford-Binet Intelligence Scale, fourth edition. The Stanford-Binet Intelligence Scale is used in children as young as 2 years old to measure intelligence in several areas (Thorndike, Hagen, & Sattler 1986). Participants obtained subtest scores in verbal reasoning, abstract/visual

reasoning, quantitative reasoning, and short-term memory, along with a test composite score.

Vineland Adaptive Behavior Scales, Survey Form. Using parents as informants, the Vineland Adaptive Behavior Scales instrument measures how a child's adaptive behavior skills in several areas of functioning compare to those of his or her peers (Sparrow, Balla, & Cichetti 1984). The domain areas are communication, daily living skills, socialization, and for children under 6 years of age, motor skills.

Child Dissociative Checklist. The CDC is a 20-item parent/observer checklist that includes dissociative behaviors in childhood ranging from minor, normative dissociations such as daydreaming to more pathological ones. Each item can be scored from 0 ("not true") to 2 ("very true). When a control group of 67 girls was administered the scale, the girls generally obtained low scores, with a median score of 2, a mean score of 2.3, and a standard deviation of 2.7, though three outliers were noted (Putnam, Helmers, & Trickett 1993). In general, younger children score higher than older ones, and a score of 12 or above is considered indicative of significant dissociative behavior, particularly in older children.

Child Behavior Checklist. The CBCL is a commonly used instrument that assesses problem behaviors in the following areas: withdrawn, somatic complaints, anxious/depressed, social problems, thought problems, attention problems, delinquent behavior, and aggressive behavior (Achenbach & Edelbrock 1991). Two versions of the 1991 edition were used, the version for ages 2–3 for the one participant in that age range and the version for ages 4–18 for the remaining participants.

Children's Apperception Test. The Children's Apperception Test (CAT) is a projective test used to assess general personality functioning (Bellak 1993). It is intended for use with children ages 3–10 years, and they are asked to tell stories based on 10 drawings of animals in various situations.

Family Questionnaire. The Family Questionnaire (Fowler 1980), derived from the Family Environment Scale (Moos 1974), measures family functioning and, in particular, the dimensions of "Organization-control" and "Cohesion vs. Conflict" in families. Parents are asked how true 30 items describing features of families are for their family. This instrument was used previously in the studies of children in Sri Lanka and Lebanon noted above.

Other than the intelligence scale and the apperception test, the instruments are designed to be answered by parents. Though this means that the results may include interpretive bias on the parents' part, all the instruments have been found to be valid measures of their given area.

Procedure

The study was approved by the Institutional Review Board for the Social and Behavioral Sciences, and participants were not compensated for their participation. Preliminary histories were obtained from parents via e-mail and telephone to ensure that children qualified for the study. Participants and their parents were then seen in their homes. The CBCL, the CDC, and the Family Questionnaire were mailed to the parents prior to the meeting, and they generally completed those forms ahead of time. They completed each one once, and serial evaluations were not used.

JT obtained the history from the parents during the meeting, and DN administered the Stanford-Binet and the CAT to the children. DN then interviewed the parents using the Vineland. He did not conduct a formal clinical interview beyond the items included in the instruments, but he did talk casually with both the parents and the children in order to engage them in the evaluation process. He administered the tests in his customary way, except that the children were evaluated in their homes rather than in a clinic or office.

The authors met the families simultaneously whenever possible, but due to scheduling conflicts separate visits were required for several participants. Statistics were determined using the SPSS Statistics package.

Results

Of the 15 children who were evaluated, 11 reported memories of having lived the life of a stranger, while three said they remembered being a deceased grandparent and one a great-grandparent. In all of the cases involving strangers, no actual deceased individual had been identified whose life was thought to match the statements. Most of the parents reported that they had neutral or negative feelings about reincarnation before the cases developed, and only three described a significant prior belief in reincarnation.

While all parents cooperated completely with the evaluations, some children were reticent. With encouragement, however, all children eventually cooperated sufficiently well to complete the developmental battery that by its nature required considerable perseverance from children in the targeted age group.

The results for the group on each subtest of the Stanford-Binet Intelligence Scale are shown in Table 1. On the Stanford-Binet, each subtest has a general population mean of 100 and a standard deviation of 16. The 95% confidence intervals show that the children as a group scored significantly above 100 on three of the four subtests. They scored above average in verbal reasoning and abstract/visual reasoning and scored in

the superior range in quantitative reasoning. The only subtest in which the participants did not score significantly above the mean was the short-term memory one, and even there their scores averaged over 100. Their composite scores on the test were also significantly above average, with one child obtaining a score of 139, while four others were at 120 or above. As such, based upon the Stanford-Binet, the participants in the study overall demonstrated above average intelligence, with a number of children demonstrating superior intelligence.

The scores of the participants on the Vineland Adaptive Behavior Scales are shown in Table 2. The scales have a general population mean of 100 and

TABLE 1

Stanford-Binet Intelligence Scale Scores

Subtest	Mean	Standard Deviation	95% Confidence Interval for Mean
Verbal Reasoning	118.07	10.62	112.19 – 123.95
Abstract/Visual Reasoning	108.93	14.69	100.80 – 117.07
Quantitative Reasoning	120.67	12.06	113.99 – 127.35
Short-Term Memory	104.87	12.91	97.72 – 112.01
Composite Score	115.67	10.56	109.82 – 121.51

TABLE 2

Vineland Adaptive Behavior Scales Scores

Subtest	Mean	Standard Deviation	95% Confidence Interval for Mean
Communication	108.27	16.12	99.34 – 117.19
Daily Living Skills	110.93	10.97	104.86 – 117.01
Socialization	106.40	12.46	99.50 – 113.30
Motor Skills [a]	105.17	6.29	101.17 – 109.17
Composite	109.27	12.91	103.09 – 115.44

[a] Applies to the 12 subjects under the age of 6.

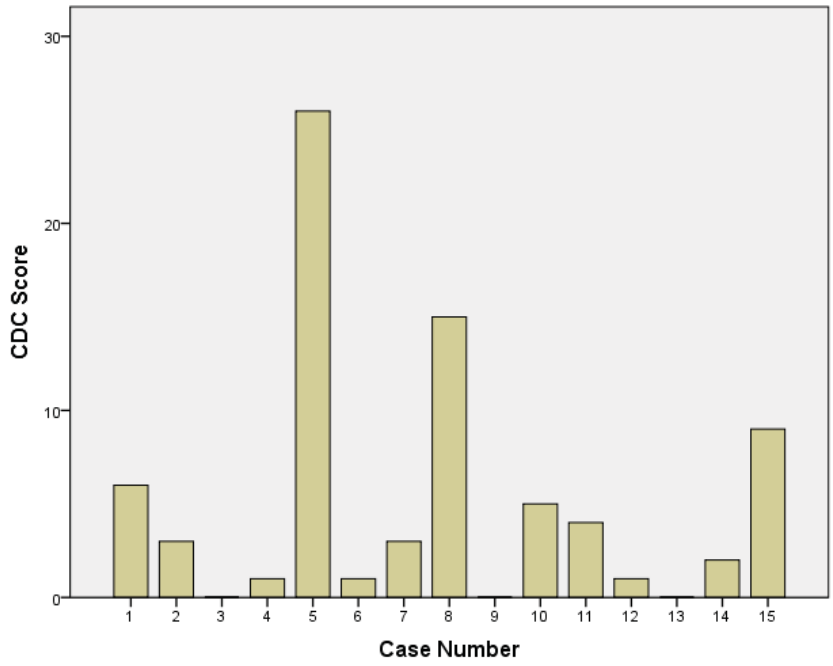

Figure 1. Child Dissociative Checklist scores.

a standard deviation of 15. The participants did well as a group. Though their averages were all within one standard deviation of the population mean of 100, their 95% confidence intervals were above 100 on the scales of daily living skills and motor skills and on the overall composite score, indicating at least statistical, if not clinical, significance. They scored slightly above average in the categories of communication and socialization.

Each participant's score on the CDC is shown in Figure 1. As the figure indicates, the children demonstrated a wide range of scores. Nine of the 15 participants scored a 3 or less, and only two of the 15 scored above the cutoff score of 12 that indicates significant dissociative behavior. Those two outliers elevated the mean score, so the mean for the group was 5.07 with a median score of 3.00 (standard deviation = 7.07). With those two participants removed, the mean score dropped to 2.69. Subject #5, who scored a 26 on the CDC, appeared to manifest a possible psychological disturbance during the evaluation. Her conversation was disjointed and tangential, and, in addition to her elevated CDC score, her test results showed significant deviation from the rest of the group in other areas as well. She scored high on the thought problems scale of the CBCL, and with her score of 99 she was the

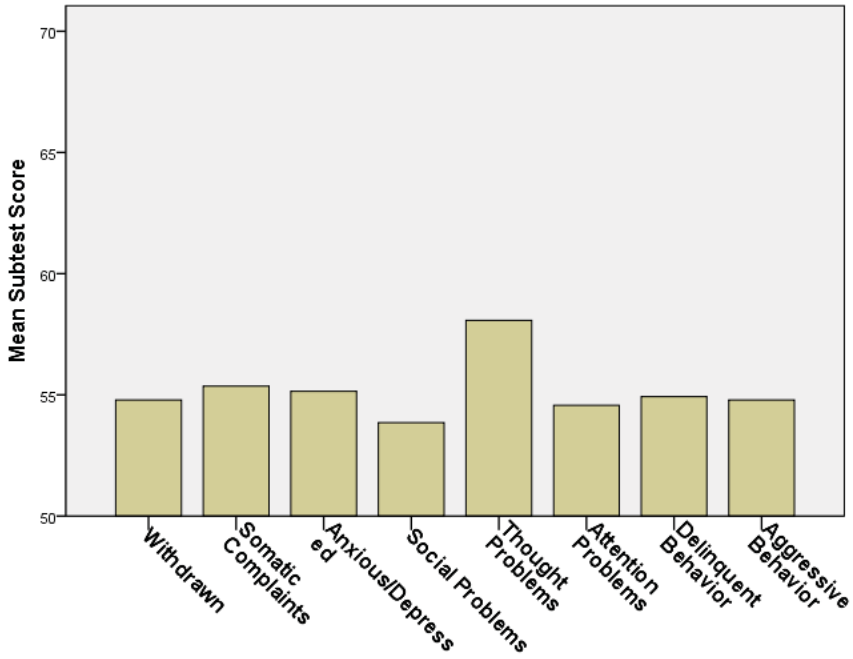

Figure 2. Child Behavior Checklist scores.

only participant to have a Stanford-Binet composite score below 100. In general, the children did not show significant dissociative symptoms, with the exceptions noted above.

On the Child Behavior Checklist, *T* scores are used, with the lowest possible score being 50 (which represents being less than or equal to the fiftieth percentile), while a score of 70 or above indicates clinically significant problems. The averages of the participants indicated no significant problems, as shown in Figure 2, but rarely did an individual child score above 70 on a particular scale. For example, as noted above, Participant #5 scored an 82 on the thought problems scale. With her score excluded, the rest of the group averaged 56, which is closer to their averages on the other scales, but even their average of 58 is well below the clinically significant cutoff.

The averages on four of the scales—social problems, thought problems, attention problems, and delinquent behavior—are based on 14 participants since the one child under the age of 4 was evaluated on the version of the CBCL for 2–3 year-olds that does not include them. It has two additional scales—sleep problems and destructive behavior—on which the participant obtained scores of 50.

TABLE 3
Family Questionnaire Scores

Dimension	Mean	Standard Deviation	95% Confidence Interval for Mean
Organization–Control	50.00	4.28	47.63 – 52.37
Interpersonal Relationships	51.13	3.56	49.16 – 53.11

In the Family Questionnaire, the dimensions of "Organization-control" and "Cohesion vs. Conflict" in families both have a general population mean of 50. The results for the families of the participants (Table 3) indicate no significant deviation from the mean, so the families do not show any distinct patterns of functioning. Likewise, the CAT results showed no unusual themes.

Discussion

This sample of young American children claiming past-life memories showed high intelligence levels, with particular strengths in quantitative reasoning and verbal reasoning. One possibility to consider is that advanced verbal skills in very young children make them more likely to verbalize mental images. Their ability to do so may intensify those images so that they become firmly established in their minds as memories.

The results on the other measures do not indicate any evidence of psychopathology for the group as a whole. Thirteen of the 15 participants showed few dissociative features. Thus, it appears that most children who report past-life memories do not show dissociative symptoms, but the two exceptions raise the possibility that children who have dissociative tendencies may be more likely than other children to make past-life reports. This possibility would need to be explored by comparing a group of children with dissociative symptoms with a group of controls to see if the incidence of reports of past-life memories was higher in the former. Further research may also be warranted to explore the possibility that two distinct populations of children report past-life memories—a minority with significant dissociative symptoms and a larger group without such symptoms. If this is the case, then future studies may need to separate the two groups, perhaps by using CDC scores, to get a true picture of each one. If the reports primarily come from children who demonstrate little psychopathology, as this study indicates,

then including outlier participants in future studies may confuse any outcomes. Subject #5, with her high dissociative score, thought problems, and lower IQ score, provides a contrast to the other participants, and while such children may be interesting to study they are likely to be a different population from the typical subjects who report past-life memories.

In addition to the CDC, the other measures—the Family Questionnaire, the Children's Apperception Test, and the Child Behavior Checklist—also showed no significant evidence of psychopathology. Thus, no typical pattern of psychopathology was seen in the evaluations, and no evidence was found to suggest that this syndrome is a pathological one. It remains possible that a non-pathological process could produce fantasies of past lives in children. However, given the lack of reincarnation themes or beliefs in the U.S. culture that might serve as an impetus for such fantasies, the question of psychopathology is a significant one.

Haraldsson (2003) has argued that some of the children show symptoms consistent with posttraumatic stress disorder, though not enough to warrant a full diagnosis. While some American cases have included nightmares and possible posttraumatic play (Leininger & Leininger 2009, Tucker 2013), those were not noted in this sample. These children appeared to be functioning quite well, and in particular demonstrated high intelligence.

Study Limitations

This pilot study used a small number of subjects, and additional studies using other Western children who claim to remember previous lives should be conducted. Since the participants in the current study were not obtained by use of random sampling, it is unclear if they are representative of young children in general who speak of previous lives. Therefore, the results, and particularly the intelligence scores, need to be interpreted with caution. Many of the parents contacted us via the Internet, and while Internet access at home or work is quite prevalent in the United States—58% of Americans had Internet access according to a poll taken during the time of the study (McCarthy 2003)—users of the Internet could possibly be more intelligent on average than those who do not use it. This, in turn, might suggest that their children would be more intelligent as well. That this could explain the significant results of the participants' intelligence testing seems unlikely, particularly given the variability in the education levels of the parents, but since all of the parents attended at least some college, it warrants consideration.

This study used young subjects. While this had the advantage of assessing children during the ages when they typically report past-life memories, the possibility exists that psychopathology that may account for

the children's statements had not fully manifested itself yet. In addition, the young age limited the available assessment tools, but evaluating the children at the age of the reports was judged to be more important than having more sophisticated test instruments. In any event, the instruments chosen have been shown to be valid and reliable, and based on the results using them only two children in the study demonstrated clinically significant pathology, with one of them appearing to be different in several ways from the rest of the group.

Clinical Implications

Parents of children who have described memories of previous lives can be told that such statements do not appear to be pathological. Such statements also do not appear to be due to influence from the parents, since few of them in this study believed in reincarnation when their children began making the statements. Thus, parents should not feel responsible for creating them. At the same time, if children make such statements, inquiring about dissociative symptoms may be reasonable since the parents of two of the 15 participants in this study reported such symptoms on the CDC. Overall, however, the present results indicate that this syndrome is largely a normal variant rather than a psychopathological condition.

Conclusion

There is no evidence to indicate that a syndrome that includes past-life memories and behaviors arises from psychopathology in American children. Likewise, the children in the current study demonstrate that the syndrome is not limited to places where cultural factors may promote them. These American children generally appeared to be functioning quite well, and past-life reports may in fact be a marker for high intelligence.

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RESEARCH ARTICLE

Facial Features of Burmese with Past-Life Memories as Japanese Soldiers

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Abstract—More than 2,600 cases of children with past-life memories have been found all over the world, and there are a number of cases in which a person's physical features corresponded with his/her past-life memories. These features included birthmarks, birth defects, physiques, postures, facial features, etc. This paper explores the possibility that in international cases, where subjects claim to have lived in foreign countries in their past lives, there can be a correlation between past-life memories and their current facial features. The subjects in the present study were Burmese who claimed to have past-life memories as Japanese soldiers during World War II. According to Ian Stevenson and Jürgen Keil, who investigated these cases, some of the subjects were said by Burmese people close to them to look more Japanese than Burmese. The present study investigated whether Japanese people judged facial pictures of these subjects as more Japanese-like than those of Burmese who claimed to have past-life memories as Burmese.

Keywords: past-life memory—mind–body connection—facial feature—Japanese—Burmese

Introduction

Ian Stevenson, founder of the Division of Perceptual Studies at the University of Virginia, studied children's reports of memories from past lives for more than 40 years. In his two-volume book (Stevenson 1997a) and elsewhere (Pasricha, Keil, Tucker, & Stevenson 2005, Stevenson 1993, 1994, 1997b), he and his colleagues presented cases in which a person's physical features corresponded with his/her past-life memories. These features included birthmarks, birth defects, physiques, postures, facial features, etc.

In international cases, in which subjects claim to remember past lives spent in another country, facial features can be particularly interesting

because sometimes people around the individuals judge that their faces look foreign to them. In this situation, it is helpful to clarify how people from the foreign country look at the facial features. If they also say the subjects' faces look like theirs, we will have additional grounds to suggest a correlation between physical features (facial features) and past-life memories.

Stevenson and his colleagues found 24 Burmese individuals who claimed to remember lives as Japanese soldiers who died in Burma (present Myanmar) during World War II (Stevenson & Keil 2005). When they were young, many of them showed a number of traits typical among Japanese, but usually not observed among Burmese: They complained about the hot climate and the spicy food of Burma. They would not wear Burmese clothes (longi), and would not observe Burmese-style postures in rituals. They also showed a strong desire to go back to Japan.

Some of these individuals were said to look more Japanese than Burmese. Because Stevenson took pictures of many of them, we can investigate whether these subjects really looked Japanese-like, as people whom Stevenson consulted claimed. If they did, or if Japanese people as well as Burmese people agree that they look more Japanese than Burmese, we will have reason for considering more seriously the connection between facial features and the claimed "past-life" memories. Stevenson was already aware of the importance of facial features in this context and presented pictures of some of the Burmese subjects, stating that "the peoples of these two nations [Japan and Burma] may have distinctive facial characteristics; a significant difference may exist, even though I cannot discern it and others who say they can discern it cannot describe it" (Stevenson 1997a).

In this paper, I will show that the Japanese consulted in this study did judge that the Burmese subjects who claimed to have past-life memories as Japanese soldiers (J-B cases) look more Japanese-like than those who claimed to have past-life memories as Burmese (B-B cases), so we have some evidence that facial features can be linked to past-life memories. I do not make any claim about the possible origin of the existence of such a correlation, but I do claim that the present findings suggest the importance of conducting further inquiry on this topic.

Methods

Picture Data

The picture data used in the present study were collected by Ian Stevenson and his colleagues in the 1970s and 1980s. Of the 24 J-B cases, 18 have pictures of the subjects, and I used all these pictures. For a control group I chose 18 B-B cases. There were 39 pictures available for this group. In

order to avoid a possible bias in selecting pictures for the control group, I used the combination of sex in past and present lives and the age when the pictures were taken as key features: I made pairs of a J-B picture and a B-B picture based on these two features so that they corresponded to each other as closely as possible. The attempt to choose pictures for the B-B group closely corresponding to those of the J-B group significantly reduced possible choices. But when there was more than one possible choice for the B-B group, I made a choice based on the case number, a six-digit number assigned to every case, and selected the picture of the case with the smallest number. This procedure yielded the 18 pairs shown in Table 1.

TABLE 1

Pairs of J-B Cases and B-B Cases

		AGE WHEN PICTURE WAS TAKEN	
	Combination of Sex "Past Life" → "Present-Life"	**Japanese-Burmese Group (J-B Group)**	**Burmese-Burmese Group (B-B Group)**
Pair 1	Male → Male	16	19
Pair 2	Male → Male	6	6
Pair 3	Male → Male	16	20
Pair 4	Male → Female	17	21
Pair 5	Male → Female	27	28
Pair 6	Male → Male	30	20
Pair 7	Male → Male	20	20
Pair 8	Male → Male	22	29
Pair 9	Male → Male	8	8
Pair 10	Male → Male	18	18
Pair 11	Male → Female	26	27
Pair 12	Male → Female	34	38
Pair 13	Male → Male	24	29
Pair 14	Male → Male	24	27
Pair 15	Male → Male	30	35
Pair 16	Male → Female	19	20
Pair 17	Male → Female	16	16
Pair 18	Male → Female	17	23

To protect the confidentiality of the subjects, the following measures were taken: (1) The pictures were shown to Japanese people without giving them any information about the people in the pictures other than their ethnicity; (2) The pictures were edited using an image manipulation program, Gimp for Mac (2.8.4p2), so that hairstyle, clothes, or the background would not give any clue as to the identity of the subject (all images were converted to black and white, and the background and clothes were painted in the same shade of color as that of the subject's hair); and (3) Each of the edited pictures was shown on a computer screen for only 10 seconds, during which time participants were busy judging how "Japanese-like" the pictures looked to them. Furthermore, the pictures were taken more than 20 years ago, so the people in the picture would not be recognizable to anyone who does not know them well.

Participants (Raters)

Forty-six Japanese who either lived in or who had stayed in Charlottesville, Virginia, participated in the study.[1] Twenty-one were males and 25 were females. Their mean age was 39.8 (SD = 9.5; range 28 to 63). I recruited them by personal introduction. They were shown a computer display on which the faces of the 43 Burmese appeared one by one. The pictures were ordered according to the numbers assigned randomly using Microsoft Excel for Mac 2011. They were pasted on a PowerPoint slideshow file with the age of the subjects when the pictures were taken. Each picture was shown for 10 seconds using the timer function of the PowerPoint application (for Mac 2011). The participants were asked to rate how Japanese-like the faces were using five-point Likert-type scale statements: (1) not Japanese-like at all; (2) not Japanese-like, (3) neither Japanese-like nor not Japanese-like; (4) Japanese-like; and (5) very Japanese-like.[2]

All participants provided written informed consent.

The research was approved by the University of Virginia Institutional Review board (IRB #2013-0333-00).

Results

The analyses were conducted based on the mean each subject obtained from the rating for Japanese-likeness.

SPSS 20 Statistics package for Mac was used for the analyses. The Kolmogorov-Smirnov normality test revealed that the data distribution is normal in the J-B group (M = 2.80, SD = 0.59, range = 1.33 to 4.11) as well as in the B-B group (M = 2.36, SD = 0.56, range 1.11 to 4.06). The result of a paired-sample t-test showed that the J-B group and the B-B group differed

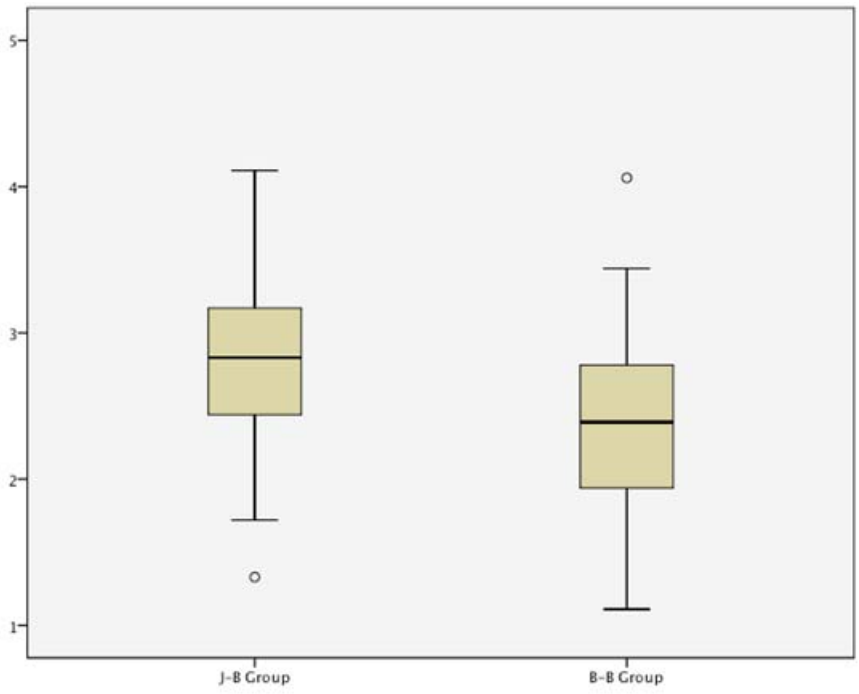

Figure 1. Difference of the rating for Japanese-likeness between the J-B and B-B groups (the circles indicate outliers).

significantly ($t = 10.634$, $df = 45$, $p < 0.001$, effect size = .85), with the former having higher scores. The box plot of the data is shown in Figure 1.

Discussion

It has been shown that the Japanese who participated in this study tended to judge the J-B subjects as more Japanese-like than the B-B subjects. Thus, we now have some grounds to suggest a correlation between past-life memories and facial features attributed to nationality.

In this study I relied on the subjective judgments of the participants. This raises an important but difficult question as to individual differences in judgment, which I cannot deal with here.[3] One possible solution might be to use rapidly developing facial recognition technology (Akamatsu 1999, Burt & Perrett 1997, Cosmides, Tooby, & Kurzban 2003, Dupuis-Roy, Fortin, Fiset, & Gosselin 2005, Lu, Chen, & Jain 2005, Takigawa, Hosoi, & Kawade 2003, Yamaguchi 2002, Yoshikawa 1999). Especially promising

for the type of investigation being conducted here seems to be an analysis employed in Akiba (2001) for identifying differences among Asian faces. I would like to explore such possibilities in the future.

Notes

[1] The selection of the participants had to be made on the basis of convenience because of the relative scarcity of Japanese living in Charlottesville. According to the 2010 census data, only 73 (0.2%) of the total population of 43,475 were Japanese (http://www.zip-codes.com/city/va-charlottesville-2010-census.asp).

[2] To examine the possible influences of sex-change on facial features, they were also asked to rate how masculine or feminine the faces looked using a five-point Likert-type scale. Following an anonymous reviewer's suggestion, I do not report that part of the experiment in this paper. I believe that the exclusion of that part does not affect the main argument.

[3] An anonymous reviewer pointed out the importance of knowing the actual facial features of the subjects and having scientific evidence (not anecdotal evidence) that these features are characteristic of Japanese rather than Burmese people, rather than that a group of selected Japanese participants judged things so. The present author believes that as an initial approach, the data based on unanalyzed subjective judgments of the participants is not without value and worth reporting as such. Identifying specific features for judgments is for the next stage and should be left for future research.

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RESEARCH ARTICLE

Parapsychological Phenomena as Examples of Generalized Nonlocal Correlations—A Theoretical Framework

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Abstract—Scientific facts are constituted as consensus about observable phenomena against the background of an accepted, or at least plausible, theory. Empirical data without a theoretical framework are at best curiosities and anomalies, at worst they are neglected. The problem of parapsychological research since its inception with the foundation of the Society of Psychical Research in 1882 was that no sound theoretical basis existed. On the contrary, the proponents of the SPR often indulged in a theoretical model that ran contrary to the perceived materialism of mainstream science, and many tried to use the data of parapsychological research to bolster the case of "mind over matter," yet without producing a good model of how such effects could be conceptualized. In general, parapsychological (PSI) research has been rather devoid of theorizing and, if anything, assumed a tacit signal-theoretical, local-causal model of some sort of subtle energy that would be vindicated, once enough empirical data were amassed. History, and data, proved this stance wrong. We will present a theoretical approach that challenges this local-causal, signal-theoretical approach by proposing that parapsychological phenomena are instances of a larger class of phenomena that are examples of nonlocal correlations. These are predicted by Generalized Quantum Theory (GQT) and can be expected to occur, whenever global descriptions of a system are complementary to or incompatible with local descriptions of elements of such a system. We will analyze the standard paradigms of PSI-research along those lines and describe how they can be reconceptualized as instances of such generalized nonlocal correlations. A direct consequence of this conceptual framework is that misrepresentations of these phenomena as local causes, as is done in direct experimentation, is bound to fail long-term. Strategies to escape this problem are discussed.

Introduction

What Is a Scientific Fact and Why Parapsychological Data Are No Such Facts

One of the biggest misunderstandings of science by popular writers and indeed empirical researchers is the assumption that a scientific fact is exclusively constituted of trustworthy and replicable observations by competent observers (Dawkins 2006, Loughlin, Lewith, & Falkenberg 2013, Martin 2004, Sheldrake 2013). One could not be more mistaken, and readers, as well as authors, of this Journal are among those who have experienced this truism (Gernert 2008, Martin 1998). This view has been haunting science since the heydays of neopositivism at the beginning of the 20th century in the Vienna circle, when philosophers of science thought that the kernel of science is observation, and that many observations are joined together to arrive at theories (Smith 1994). This crudely and purely inductive view of science has since proved plainly wrong (Suppe 1977). Hanson showed that each and every observation is theory-laden, and that no such thing as naïve, objective observation exists. Popper argued that only a deductive way of reasoning, starting from theory, or at least a hypothesis, a daring conjecture, would enable science to progress, because every inductive model of science would not be able to solve the Humean problem (Popper 1976). This consists of a circular argument: Each inductivist model has to stipulate at least one non-empirical sentence, the induction principle itself, in order to be able to use inductive observation in the first place. More historical and pragmatic approaches to science proved Popper insufficient (Kuhn 1955, Putnam 1975, Laudan 1977), and if there is any consensus among Science and Technology Study scholars at all, then it is a historical social consensus about how science operates (Toulmin 1985). It is a largely social enterprise, within which those observations are counted as facts that can be communicated well, because they are made against the background of an accepted theory, have been shown to be reasonably robust against modifications, and can be replicated by competent observers. Social–historical studies, like those of Bruno Latour, have shown that consensus about theories and observation is only a minimal requirement (Latour 1999, Latour & Bastide 1986). A scientific agent needs to be able to also draw on the benevolence of important communicators and political agencies. In the examples studied by him these were elite groups such as the French National Academy, or political decisionmakers, or important newspaper editors.

In our day, these opinion leaders outside the scientific community proper are powerful science editors of journals, newspapers, and TV magazines, funding agencies, and political decisionmakers (Emerson,

Warme, Wolf, Heckman, Brand, & Leopold 2010, Henderson 2010, Lee, Sugimoto, Zhang, & Cronin 2013, Ritter 2011).

A successful scientific theory for any class of phenomena thus consists of at least of three components:

1) There is a good theoretical model that is accepted by a majority of scientists active in the investigation of these phenomena.
2) There is a repeated and replicable observation that can be shared by competent observers and replicated within reasonable limits by them.
3) There is a communicative consensus within the scientific discourse and among those who wield the wands of power therein. This consensus has to pertain both to the acknowledgement of the observations and the acceptability of the theoretical model.

1) without 2) and 3) is only a toy model, interesting to play with, but without consequences. 2) without 1) and 3) is an anomaly at best, but normally just a nuisance. 1) and 2) without 3) constitutes a scientific fringe culture.

Parapsychology (PSI), since its inception which can be dated to the foundation of the Society of Psychical Research in 1882 (Society for Psychical Research 1882), is at best such a scientific fringe culture, without, however, really agreeing on a good and accepted theoretical background. If there was any commonality among the founders of PSI-research then it was a tacit opposition against what was perceived as the crypto-materialism of the mainstream scientific model. However, 130 years of research, some at high-profile university institutions, have not really brought us any further toward some acceptance by the mainstream. The reasons for this are debatable. Mainstream science is not convinced by a vague and undifferentiated rejection of materialism.

Moreover, critics normally point to the fact that a lot of the evidence is purely anecdotal and some of the experimental evidence fails some crucial tests, such as independent replicability and stability of observations under changed framework conditions (Alcock 2003, French 2003, Milton & Wiseman 1999). Although meta-analyses of experimental models in PSI research are generally positive overall, with stunning odds, even though effect sizes are sometimes small (Mossbridge, Tressoldi, & Utts 2012, Schmidt 2012, Schmidt, Schneider, Utts, & Walach 2004, Storm, Tressoldi, & Di Riso 2012, Tressoldi 2011), it cannot be denied that some decisive replication studies have failed spectacularly, pouring water on the mills of critics (Jahn et al. 2000, Milton & Wiseman 1999, Ritchie, Wiseman,

& French 2012, Schmidt, Erath, Ivanova, & Walach 2009, Schmidt, Tippenhauer, & Walach 2001).

Apart from this, very little attention has been paid to the theoretical background models that might hold for parapsychological effects. After some popularity of observational theories in the 1970s, most researchers seem to have turned back to a tacit local, signal theoretical concept of PSI-effects. We will explain in the following section what we mean by that. By now it should be clear why PSI is at best fringe, scientifically speaking:

- The observations communicated within and outside the PSI community are not really stable and replicable enough.
- There is no accepted/acceptable background theory.
- There is no consensus about those purported facts within the PSI-community, let alone within the larger scientific community.

In what follows we will tackle the issue of a sufficient background theory that offers a model which is, at least potentially,

1. communicable and acceptable, because it connects to the core of mainstream science,
2. capable of making clear why the empirical pattern of overall effect and failure to replicate in decisive experiments repeats itself,
3. able to make the varied phenomenology of PSI phenomena understandable.

We will use the model of Generalized Quantum Theory (GQT), which we have developed as a theoretical frame (Atmanspacher, Filk, & Römer 2006, Atmanspacher, Römer, & Walach 2002, Filk & Römer 2011). From it we can derive generalized entanglement correlations (GET) as predicted theoretical consequences, which can in turn, at least potentially and in principle, explain PSI phenomenology (Lucadou, Römer, & Walach 2007, we also refer to this publication for technical details omitted in this note). We will show with a few examples what this means. We will finally mention some framework conditions for future empirical work that can be derived from our model.

The Local-Causal Model of PSI and the Signal-Theoretical Assumptions of the Experimental Approach

Experiments are the final arbiter and authority of modern-day science, ever since Galileo and others paved the way in practical terms and Francis

Bacon laid the theoretical foundations. Experiments are precise questions to Nature, and experimental results are Nature's answer to us. Two decisive presuppositions often go unnoticed, which we should recall. One was already made explicit by Francis Bacon, the other seems trivial but is rarely discussed. Bacon defined experiments as explicitly sought experiences. "Experience remains. If it happens just in passing, we call it accident. If we seek it out, we call it experiment" (Bacon 1990:182). Experiments are willful manipulations of Nature. Observations are naturally occurring experiences, experiments are manipulated experiences. Thus experiments make the presupposition that we can actually manipulate something and still receive a valid answer. The second, even more important, presupposition is that experiments presuppose a continuity and stability in Nature. No matter by whom, where on earth, or when an experiment is made, we expect, grosso modo, the same results. We do this because we assume that experiments are detectors of stable causes, and those causes, we assume, are regular. If something works only on Mondays, and some other days, we would not count it as a regular cause. Hume had made regularity one of the hallmarks of the notion of a cause, the other being temporal precession and local contiguity (Hume 1977:Section IX:109ff). Experiments are detectors for such stable, replicable, regular causes, or at least for conditions of that type which we can use to analyze causes from them. An astronomer who observes a red-shift in a certain stellar region of a certain magnitude will expect to see this through any good telescope on earth on any good observation night, and if he communicates his observation to other astronomers he will be confident that they will also see the same amount of red-shift. This is a regular phenomenon that can be used to infer potential causes, for instance the speed of a retracting light source, or the magnitude of some deflecting source, depending on the theory.

Precisely because experiments have been so pivotal and successful in the history of modern science, it is not surprising that PSI researchers turned their hopes to experimentation. While early-days PSI research was mainly observational in nature, mapping PSI experiences of the population and observing mediums and séances, J. B. Rhine and others introduced the experimental paradigm. Thereby they transposed the tacit presuppositions of experiments—regularity, locality, availability at will—onto the subject matter of PSI. It is important and worthwhile to note that the early-days researchers did not necessarily hold such a crypto-causal theory of PSI effects. Barrett, for instance, wrote, in what was the first call to the public to help with research by offering instances of "thought reading" in the [London] *Times*:

> I shall be glad to receive communications . . . on two points—of cases of the direct action of one mind upon another giving rise to an apparent *transfusion of thought* or feeling, occurring either in abnormal conditions . . . or of cases where, under normal conditions, perceptions may seem to occur independent of the ordinary channels of *sensation.* (Barrett 1882:48, italics ours)

Note that he spoke of "transfusion of thought or feeling" presupposing some sort of correlational or connectedness model. Fifty years later he explicitly criticized his colleagues for adopting a crypto-signal theoretical model, when he wrote:

> The phrase *thought transference* is apt to be misleading, as it seems to suggest a transmission of ideas between two persons across material space; but, as I said, space does not seem to enter into the question at all. Here it may be interesting to note that in the first publication of the discovery of this super-sensuous faculty, I called it not *thought transference,* but *transfusion of thought.* We are now coming back to this idea, for telepathy is probably the intermingling of our transcendental selves or souls. (Barrett 1924: 294)

Barrett notes correctly that "thought transference" adopts a theoretical model that assumes some signal travelling through space from one mind to another, and criticizes it for its theoretical assumptions. It is exactly this theoretical assumption that has then inspired experimental research in PSI. It has not only inspired it, it was the tacit presupposition on which experimental work is predicated in very general terms.

Such a model assumes, tacitly, that PSI effects (Lucadou 1995)

1. are regular
2. are accessible at will
3. are transported by some, as yet unidentified, local-causal carrier
4. can be accumulated statistically
5. are in principle independent of meaning.

All these assumptions are in our view problematic, probably even wrong, but have rarely been debated critically. What is most important among them, though, is the locality assumption.

The Locality Principle and the Difficulties of a Local Model of PSI

"Locality" means that regions in our universe that influence each other causally need to be connected by a physical signal that exchanges energy in order to make the influence real (Reichenbach 1957). Since, according to Special Relativity, signals can only travel at the finite speed of light of

approximately 300,000 km/sec, all potentially known signals in the universe take at least some time to reach from an agent to its target. If distances are large, and if the signal is not radiated into a fixed direction but rather emitted in all directions, then signal dilution and the inverse power law come into play: The energy of a signal as collected by a detector decays by the inverse squared distance between source and detector, i.e. the further away a cause of influence, the stronger the signal has to be initially to reach its target. This is why mobile phone signals need repeaters to boost their energy.

Now, any cause that can be conceived of in our current physical world model needs to conform to this generic model and obey these presuppositions to be called a cause. In other words, in our mainstream model causes are always some kind of signal. In addition, all signals can be described by the transmission of particles, either usual particles or field quanta if the signal is conceived as the field effect. For instance, photons are the quanta of the electromagnetic field. As for the gravitational field, gravitational interactions are ubiquitous, the existence of gravitational waves is well-established by indirect evidence, for instance from double pulsars, and as a result of intensive large-scale research over several decades, gravitational detectors are expected to register gravitational waves in the near future. The detection of single quanta of the gravitational field, called gravitons, is hardly feasible: Because of the low frequencies of all known gravitational fields, the energies of the gravitons must be extremely low.

In addition to electromagnetic and gravitational interactions, the current standard model of physics knows two more kinds of fundamental interactions: so-called weak and strong interactions. Both of them have a very short range, much less than the diameter of an atom. Gravitational effects under laboratory conditions are very small indeed. So, on the basis of the Standard Model of the universe, apart from the transmission of ordinary matter, currently only the electromagnetic force is a candidate for an effective local-causal model of PSI effects. Such influences can experimentally be shielded off easily and effectively.

Every local model of PSI based on known established facts has to face very serious problems.

If any local cause is presupposed, and just for argument's sake we assume the electromagnetic force is seen as a candidate, then it becomes very difficult to understand how effects at a large distance can be conceptualized. Granted that there may be a weak signal being emitted by a brain—and the invention of the EEG was in fact predicated on just such an assumption following a telepathic experience of its inventor, Hans Berger—we can assume it is weak, in fact it is on the order of some microvolts, and hence will decay rapidly. How do we explain telepathic effects over many

thousands of miles, as have been documented? How do we explain distant healing that has been documented at least anecdotally to be independent of distance?

Any local signal is bound—by the current standard model—to travel forward in time. A vast array of PSI effects are independent of time, or even reach backward in time or forward in time. Precognition is a communication of a mind with its future state. Using a local model would mean that we can communicate faster than light. This, in turn, gives rise to paradoxes of intervention into the past that were demonstrated 40 years ago to arise if a local model of signal transfer violates Special Relativity (Fitzgerald 1971). Hence local signal-theoretical models of PSI run into severe difficulties, when it comes to explaining precognition.

One can always stipulate other or new kinds of signals that are as yet undiscovered. Such a theoretical stance comes at high cost: The scientific community is reluctant to accept such an assumption a priori, because it would mean that the whole well-proven standard model that is complex enough as it stands would have to be reworked, and no one wants to do that without a very good reason. Thus there is bound to be wild resistance against such a proposal. This is in part a social, but very important argument. Some such models have been proposed, for instance assuming multi-dimensional geometries that would allow for other types of regular signals (e.g., Zöllner 1922, Heim 1984, 1989). But for competent physicists, they can clearly be seen not to be state of the art and/or contradicting established physical facts.

We think that the locality-principle fails in PSI research for various reasons: (1) The empirical database is incompatible with its basic assumptions. PSI effects are independent of distance and time. This is a strong argument against any local model, at least within the constraints of the standard model. (2) PSI effects are also not in the same sense regular and available at will as local-causal effects are normally assumed to be. Hence, we feel, it is time to search for a nonlocal and non-causal model.

Generalized Quantum Theory, Generalized Entanglement, and a Non-Local Model of PSI

Generalized Quantum Theory

Generalized Quantum Theory was born out of two impulses: For one, there was the intuition that a theoretical structure that was so successful in explaining the material world might also be useful in other contexts. In addition, we wanted to see what a minimal theoretical frame would look like that could call itself quantum-theoretical and yet would be free of the restrictions that are typical for *physical* quantum theory proper. So, if

one generalizes quantum theory and asks the question: Exactly what is it that defines a theory as "quantum-theoretical"?, then there is a simple and surprising answer: It is the capability of the theory to handle incompatible, or complementary, or non-commuting operations (Atmanspacher, Filk, & Römer 2006, Atmanspacher, Römer, & Walach 2002, Filk & Römer 2011, Walach & Stillfried 2011, Walach & von Stillfried 2011). Our normal, classical, theories do not have that requirement: We can measure the trajectory of a cannonball and then determine its momentum, or the other way round. The measurement of one variable is independent of that of the other variable, and neither measurement necessarily disturbs the measured object or invalidates previous measurements. This is the type of theory that is applied in nearly all branches of science currently, except in the quantum realm. We call such a theory a classical theory.

However, we assume that there are many other instances where quantum-type theories are necessary. Whenever a measurement necessarily and inevitably impacts on the measured object and changes its state, we have a non-classical situation that needs to be described by a quantum-type, or a non-classical theory. In psychology this is obviously the case rather frequently. For instance, whenever a therapist directs the attention of a client to his or her as-yet-undefined bad feelings and the client then comes up with a precise description, the feeling itself has changed. This is the gist of good therapy. Whenever a patient uses the items of a questionnaire to describe some state of affairs, the answering of the questionnaire will have changed the state to some extent. Any introspection is bound to change the state of mind of the participant. Thus, a lot of psychology is in fact a good candidate for a quantum-like theoretical treatment. Learning and understanding, for instance, are non-commuting operations. Normally, we learn first and then understand, and we cannot willfully change the sequence. Clinically speaking it will make a difference whether we first try to understand a patient and then apply a battery of questionnaires or vice versa. All those operations, where sequencing effects are of importance and where a different sequence of events will yield different results, are non-classical, or quantum-type, in nature, and a quantum-like theory is useful to model them.

As already mentioned, a general formalism providing a minimal scheme in which the essential notions of incompatibility, complementarity, and entanglement (to be described later in this note) can be defined in a clear and meaningful way, without employing additional structural features necessary for quantum physics in the narrow sense, was developed under the name of "Generalized Quantum Theory" (GQT), initially called "Weak Quantum Theory" (Atmanspacher, Filk, & Römer 2006, Atmanspacher,

Römer, & Walach 2002, Filk & Römer 2011). By shedding features that are specific for quantum physics, the formalism of GQT is applicable and in fact has found many applications beyond the realm of physics. Filk and Römer (2011) provide a list of applications, and Atmanspacher and Römer (2012) applied it to sequencing of questions in questionnaires. If necessary, the formalism of GQT can be enriched stepwise to again yield the full quantum theoretical formalism.

It turns out that in fact the only and most important decisive marker of a quantum-like theory is exactly its capacity to model incompatible operations. For a complete description of GQT we refer to the original publications (Atmanspacher, Filk, & Römer 2006, Atmanspacher, Römer, & Walach 2002, Filk & Römer 2011). Here we restrict ourselves to a few hints. In GQT the notions of "system," "states," and "observables" are taken over from physical quantum theory. An observable A of a system is a feature of the system which can be observed, i.e. "measured" in a meaningful way, yielding a result that has factual validity. This means the following: If a measurement of A has yielded a result, say *a*, then immediately after the measurement the system is in an "eigenstate," in which a repeated measurement of A would yield the same result *a* with certainty. After a measurement of B following A the system is in an eigenstate of B, and after a measurement of A following B the system is in an eigenstate of A. Two observables A and B are called *complementary* or *incompatible*, if there are measured values of one of them, say value *a* of A, such that no eigenstate of A to the value *a* can be an eigenstate of B. A and B are justly called incompatible, because we cannot always define their values precisely at the same time. For incompatible observables A and B the order in which they are measured will matter. In this sense, A and B do not "commute" with each other. Observables A an B are called *compatible* if they are not complementary, i.e if their measurements are interchangeable and do not disturb one another. In a classical setting every observable is compatible with all the others. In (Generalized) Quantum Theory two observables need not be compatible but may be complementary. Whenever one of the two incompatible observables is precisely defined, our knowledge of the other observable may be reduced in precision. In quantum physics proper the Heisenberg uncertainty relationship is an expression of this situation. Yet such incompatible or complementary observables have to be employed at the same time to describe one and the same object or situation. For particles, the classical example is given by location and momentum. Previous classical theories had no need of such concepts. It was Nils Bohr and his co-researchers who were the first to discover that in order to model quantum-physical effects one had to employ two concepts at the same time that are in

conflict, yet both necessary. Bohr imported the notion "complementarity" from psychology to describe this situation conceptually (Rosenfeld 1953, 1963). Through the precise definition within quantum mechanics, complementarity became a clear notion and is in fact operationalized as incompatible or non-commuting operations. The result of our analysis of generalizing quantum theory yielded the somewhat surprising, but easy to grasp result:

The defining element of any quantum-theoretical approach is the capacity to handle non-commuting, or incompatible, or complementary operations.

If everything else is relaxed, definitions given up, precisions dropped, and the final element left intact that is necessary to define a quantum-theoretical approach, it is the handling of such incompatible variables or operations. Thus, the stipulation and the challenge of generalized quantum theory is that other situations might require such a description as well. We have above pointed to some examples from psychology. There are quite a few other areas that might require such quantum-like descriptions. For instance, it has been shown that the switching behavior of bistable images follows a dynamic that can be predicted and modeled using GQT (Atmanspacher, Bach, Filk, Kornmeier, & Römer 2008, Atmanspacher, Filk, & Römer 2004). Others have found that using a quantum-like formalism for modeling results of cognition experiments makes the modeling more precise and more closely conforming to empirical results (Pothos & Busemeyer 2013). One can speculate that other situations of our lived world contain incompatible descriptors. Typical candidates for such pairs could be

- goodness and justice
- form and content
- structure and function
- individual and community

to name but a few.

What is important to understand here is that complementary or incompatible concepts cannot be located on the same conceptual plane. Contradictory pairs of opposites can be formally modelled as negations: a = ¬b; b = ¬a such as in "warm is not cold", or "false is not true". Figuratively speaking, they can only be located on an orthogonal conceptual system, and none can be reduced to the other, but of course not all orthogonal concepts are complementary.

Whenever such candidates for complementary or incompatible pairs

are necessary, we are dealing, by default, with a quantum-like system, and a generalized quantum theory (GQT) is applicable to handle such situations.

Entanglement

One interesting consequence of GQT is of particular importance: GQT, as well as physical quantum theory, predicts a generalized form of nonlocal correlations.

Schrödinger had discovered this phenomenon in 1935 in the formalism of Quantum Theory and named it "entanglement" (Schrödinger 1935). It denotes a situation whereby elements of a quantum system remain correlated no matter how separated they are in space or in time. Suppose we have a quantum system, two twin-photons say, that have been down-converted through a beam-splitting crystal, and we were able to send one photon to alpha-centauri and the other photon to some other star, and we had a measurement apparatus on alpha-centauri that measures one of the photon's properties, say its polarization in a given direction, then we would have immediate knowledge about the corresponding polarization of the second photon that is, by definition, several light years away. Thus, no potential local signal could travel and convey the information between the two measurement apparatuses. This phenomenon occurs because the so-called entangled state of the total system is well-determined, but the polarization of neither of the single photons is determined until it is measured. Exactly which polarization value will be measured for one photon is uncertain, but once there is one value defined by measurement, the other one is immediately known. This holds independent of space and time. This correlation is called entanglement, or EPR-correlation (for Einstein, Podolsky, and Rosen, who were the first to use this situation for a thought experiment), or nonlocal correlation.

Entanglement has long remained a kind of a theoretical nuisance of quantum mechanics, but now it is an established fact with emerging technical applications. Moreover, Bell (Bell 1964, 1987) derived inequalities for correlations between disjoint parts of certain composite systems such that these inequalities should always be fulfilled in classical systems but are violated for some entangled states of quantum systems. These inequalities are experimentally testable and are indeed found to be violated, a strong argument for quantum theory and against an exclusively classical world view (Aspect, Dalibard, & Roger 1982, Aspect, Grangier, & Roger 1982). Because the experimental setup was such that a communication between the measurement apparatuses was excluded by principle, these correlations are nonlocal: No classical signal mediates this corresponding behavior. Rather, it is a consequence of the systemic setup. It has been shown meanwhile

that photons, electrons, or multi-particle systems can be entangled, and entanglement has been experimentally shown to hold over many kilometers (Gröblacher, Paterek, Katenbaek, Brukner, Zukowski, Aspelmeyer, et al. 2007, Hackermüller, Uttenthaler, Hornberger, Reiger, Brezger, & Zeilinger 2003, Kwiat, Barraza-Lopez, Stefanov, & Gisin 2001, Pan, Bouwmeester, Daniell, Weinfurter, Zeilinger, et al. 2002, Salart, Baas, Branciard, Gisin, & Zbinden 2008, Stefanov, Zbinden, Gisin, & Suarez 2002). Futuristic applications such as quantum computing and encryption are founded on this phenomenon, and proof-of-principle studies have already been conducted (Duan, 2011, Nielsen & Chuang 2000, Niskanen, Harrabi, Yoshibara, Nakamura, Lloyd, & Tsai 2007, Olmschenk, Matsukevich, Maunz, Hayes, Duan, & Monroe 2009, Parigi, Zavatta, Kim, & Bellini 2007, Petta, Johnson, Taylor, Laird, Yacoby, Lukin, Marcus, Hanson, & Gossard 2005, Reichle, Leibfried, Knill, Britton, Blakestad, Jost, Langer, Ozeri, Seidelin, & Wineland 2006, Svozil 2001, Tóth & Lent 2001).

For what follows it is important to note that we do not assume that quantum-mechanical, physical entanglement correlations are magnified and transported into the macroscopic realm. Although not impossible in principle, such a scenario is unlikely, because these correlations decay fast, as soon as interactions with other systems are happening.

In quantum physics, entanglement is normally discussed by constructing the state space of a composite system as a tensor product of the state spaces of its components, and entangled states are defined as not being factorizable with respect to the tensor product. The notion of tensor products is not available in the most general form of GQT. But, in fact, even in quantum physics the core of the notion of entanglement is independent of these technical details. The decisive feature is a complementarity relationship between global observables pertaining to the system as a whole and local observables pertaining to its parts. In the two-photon example, the global observable is an observable having the entangled global state as an eigenstate. This observable is complementary to the local polarization observables of the individual photons, whose values are in fact indeterminate in the global entangled state. Measuring one local polarization changes the entangled global state.

Now, the notion of entanglement can readily be taken over into GQT, a consequence of complementarity between global and local observables (Atmanspacher, Filk, & Römer 2006, Atmanspacher, Römer, & Walach 2002, Filk & Römer 2011, Lucadou, Römer & Walach 2007. For a detailed discussion of entanglement in GQT with many examples, see Römer 2011a, 2011b).

The genuinely quantum theoretical phenomenon of *entanglement* can

and in general will show up also in GQT if the following conditions are fulfilled:

1) A system is given, inside which subsystems can be identified. Entanglement phenomena will be best visible if the subsystems are sufficiently separated such that local observables pertaining to different subsystems are compatible.
2) There is a global observable of the total system, which is complementary to local observables of the subsystems.
3) The total system is in an *entangled state*. For instance, eigenstates of the global observable are typically entangled states.

Given these conditions, the measured values of the local observables will in general be uncertain because of the complementarity of the global and the local observables. However, entanglement correlations will be observed between the measured values of the local observables. These correlations are nonlocal and instantaneous. Einstein, trying to argue for an incompleteness of quantum mechanics, spoke about "spooky interactions" in this connection. Entanglement correlations are not due to causal interactions between the subsystems. Rather, such correlations without interactions are a witness of the holistic character of composite quantum systems: The states of the subsystems in general do not determine the state of the total system. Vice versa, the holistic state of the total system does not determine the measured values of local observables pertaining to the subsystems. The holistic character of the total quantum state resides in entanglement correlations between the subsystems which enter into the common pattern of a global entangled state.

It is not difficult to show that in quantum physics entanglement correlations cannot be used for signal transmission between different subsystems. This must also hold in GQT in order to prevent bizarre intervention paradoxes, and is formulated as an axiom "NT" ("Non Transmission") (Lucadou, Römer, & Walach 2007) in GQT. One may even turn the argument around and state that whenever correlations between subsystems can be used for signal transfer, they must be of a causal nature and entanglement must be absent or at least not dominant. Like quantum-mechanical entanglement correlations, GET correlations are not bound by space and time. Theoretically they can be even quite strong because they are not necessarily subject to the tendency of rapid decay prevailing in quantum physics.

Note two important corollaries here: The setup of GET is strictly driven by the systemic setup of the whole system and independent of its

physical makeup. The system in question could be a physical system, a mental system, or a mix of two different systems. But they have to be joined together by a strong common systemic boundary, for instance by meaning or pragmatic information (PI) that defines the system (Weizsäcker 1974). Second: The GQT model makes no predictions as to whether such correlations are ontic in nature, as in quantum physics proper, or epistemic, i.e. due to our lack of knowledge or our epistemic condition. For practical purposes this is irrelevant, but it should be noted. Some experimental PSI phenomena appear to be ontic (Schmidt 1976, Lucadou, Römer, & Walach 2007).

Application to PSI Research

Thus, whenever we have a clearly defined system that binds together subsystems whose description is complementary to the description of the whole system, we expect nonlocal correlations between the systemic elements. Let us probe the model for particular situations. We start with the usual parapsychological terminology, but it goes without saying that these concepts are attached to the model of signal-transfer, and thus the empirical and theoretical basis to use them is questionable as we argued above. The following discussion will put these phenomena in the framework of our GET non-signal model.

Telepathy

Telepathy, or "thought reading" as Barrett had called it, is the phenomenon that one mind has access to the content of another mind without classical means of knowledge or communication. This happens, typically, not with people we meet by accident, but normally only when the two persons are somehow related, as with siblings, parents and children, or are psychologically close, such as lovers or spouses. Also, doctors and therapists report these phenomena and use them as therapeutic intuition. One could make a case that therapeutic fantasies, which psychoanalytically trained therapists often refer to as "transferences," are in fact instances of such telepathic connections, and Freud is known to have been interested in these cases (Simmonds 2006); but this leads us too far astray. In all those cases we have a clear systemic boundary: The boundary is constituted by kinship and genetics, or by a ritual, as in marriage, or in a therapeutic situation. The global observable is connectedness or "organizational closure" (OC) (Varela 1981). The local observables are separation or individuality. These, we hold, are complementary, and hence the preconditions for nonlocal correlations between the two systems are fulfilled. Mental content of one system can

appear as mental content of the other system, and vice versa. Exactly when and why such an experience is bound to happen is difficult to predict, as the model is not precise enough for such predictions. Experience and anecdotal evidence would suggest that this happens mostly when one individual is in need or in danger, when the connection is very strong as in couples wildly in love, or something is bothering a person, as in unprocessed trauma or dissociation, or in strong unintegrated inner pain.

It is clear from this analysis that the process can be reversed.

Healing

This happens in instances of intentional healing, whether from a distance or with contact (Walach 2005). Here, a healer forms a strong systemic bond, normally through a ritual, cultivates an intention in his or her mind, usually supported by ritual or imagination, and, by virtue of the nonlocal correlatedness between the two persons, the envisaged situation may occur. The complementary pair is again connectedness and individuality. Likely, there is also a second complementary pair operative here: The imagination of the desired state as actual leads to a complementarity between future potentiality, or the aim of healing, and current reality, the actual situation. This may be the vehicle of operation, but clearly we need more conceptual analysis.

Clairvoyance

In clairvoyance, content is experienced mentally that is physically available elsewhere, as in remote viewing or when people guess material that is somewhere present where they have no classical access. Remote viewing studies have shown this is possible, at least in principle (May 1996, McMoneagle 2000, Puthoff 1996, Targ 1996, Targ & Katra 2000, Utts 1996). Again, we have a ritual systemic closure (OC) between an individual and the object, sometimes through a physical ritual that an envelope or something else has touched, held in the hand, or put somewhere close to one's body. Sometimes the ritual is purely mental. The same complementarity holds as above between connectedness (global variable) and separation (local variables). And by virtue of GET content may show up in the mind of the person seeking the information. Again, we do not know under which circumstances such processes work, and the classified work of U.S. intelligence has shown that it works but is not precise enough for espionage (Targ 1996, Puthoff 1996, Utts 1996). But the model can make plausible why and how this can happen.

Psychokinesis

Psychokinesis, spuk, or poltergeist phenomena happen whenever an inner mental process affects a physical system directly without the mediation of classical local causes (Lucadou 1995). The more spectacular cases are called poltergeist, where visible events in the macro-world happen without apparent causes. Documented cases report tables whirled around and toppled, bookcases fallen over, fires started and extinguished by themselves, knives, stones, and other heavy objects thrown around, etc. (Imich 1995, Roll 2003, West 1990). Phenomenologically speaking, such situations seem to require an "agent", someone who suffers from a—usually—unconscious conflict that cannot be and must not be known and expressed. In such a situation the poltergeist phenomenon seems to "express" the mental content phenomenologically. One of us was involved in a poltergeist-resolution where a young female secretary was strongly focused on her boss, a relationship which was impossible to express, because the boss was happily married and had no interest in pursuing a relationship. In short: The spuk started when the boss had to go on a business trip. He said to his employees: "Only call me if there is fire!" Sure enough, after the boss had gone on his trip, fires started in his office. The boss had to return. Later, the shutters of the windows, without anybody setting them ablaze began to burn when his wife came to the office. As a funny aside, the German word for *shutters* is *jalousie*, derived from the French, meaning *jealous*. Thus, this particular poltergeist also had the phenomenological wisdom to express the inner dynamics of the jealous secretary, who likely was jealous of the wife.

How can such a strange situation be conceptualized? Again, we have a strong systemic closure (OC) that ties together various systemic elements. We normally have poltergeist phenomena within families. Here we have it within a company and within a subsystem of the company formed by the boss and his secretary, who, however, has no chance of expressing and fulfilling, perhaps not even admitting or being aware of her feelings. This forms a strong subsystem between the secretary and her boss. Again, complementarity between connectedness and individuality holds, describing the global and the local observables. Strong emotional material, usually disavowed or disconnected from the inner life, seeks some form of expression. As it happens, the expression is found in the outer reality that bears some symbolic connectedness with the total system. Thus, a nonlocal correlation becomes operative that exists between elements of a system by virtue of a strong systemic boundary. Exactly why material objects are involved, and not, say, only mental content as in clairvoyance, is a point for debate. One could speculate that, had the boss been more

receptive and felt the strong connection, verbalized this, and helped the secretary express and live through her feelings, the poltergeist would not have been necessary. In that sense, we conceptualize poltergeist as a more massive form of nonlocal correlation that is normally felt in telepathy, that becomes operative if telepathy fails, or perhaps under yet-to-be-defined other boundary conditions.

Micro-PK as is used in experimental realizations, when voluntary subjects are to influence random processes, is simply a more artificial setting using the same processes.

Precognition and Presentiment

Precognition is, conceptually speaking, the most challenging phenomenon, because it defies, by definition, a local explanation. In it a mental system receives content about its future state. Even if precognition is targeted at future events, as in classical prophecies, it is still a relationship of a mind with its future state, as the events can only be relevant as known or otherwise mentally present. A slight variation is presentiment, where the content is not consciously known but subconsciously felt and made visible by, for instance, monitoring autonomic arousal. But if we adopt a wide notion of "mind" and "mental content" to also comprise subliminal mental material and all elements processed by our neuronal system, then we can also include presentiment.

We have again a systemic boundary that comprises the mental system and its future state. The boundary is set here by meaning (PI). Precognitive events and presentiment effects are not arbitrary, but happen for a reason. In presentiment they have been experimentally discovered in a situation where the individual is about to face potentially threatening situations and can thus be thought of as a warning system. In other precognitive situations, as in precognitive dreams, we observe, phenomenologically speaking, the same thing. They usually either have a warning or a preparatory function that help the individual deal with dangerous or important situations. Thus the systemic closure is one of meaning and relevance. As an interesting aside, this can only be defined by the future event that actually will happen in the distant future. However, if it forms a systemic boundary with a present mental system, then, by definition, a future meaning has an effect in the present, pointing to a deficient current notion of time anyway. But this is just an aside. Systemic closure is produced by meaning and importance, or the pragmatic information that is being processed. The complementarity that is operative here seems to be one between potentiality, the global descriptor, and actuality, the local descriptor. This forms the basis for the entanglement between the present moment state of the mental system and its future state.

Thus, we have covered the major instances of PSI or anomalous cognition that form the basis of the various parapsychological phenomenologies. We have shown that one and the same model can form the basis of an understanding of such phenomena in terms of generalized nonlocal correlations within a generalized quantum theory. Obviously, the key issues are twofold: We need to nominate a clear candidate for a strong systemic boundary. In all instances, such systemic boundaries are either given or intentionally set. And we need a pair of complementary observables that describe the system and its components. In most cases the complementarity between connectedness and separation will be sufficient to fulfill this requirement. Wherever some willful or involuntary action in the real world is part of the phenomenology, it might be the case that a second complementarity between actuality and potentiality comes into play. And it might be the case that this acts as a driver.

Consequences, Empirical Observations, Future Directions

One consequence of this model should be immediately obvious: Generalized entanglement correlations are nonlocal and hence will eschew any detector system long-term that is geared toward detecting regular, local causality, such as classical experimentation is. This is the reason why we have postulated the no-signal-transfer axiom (NT axiom). In quantum physics proper it is clear and has been proven that entanglement correlations cannot be used to convey classical signals (Lucadou, Römer, & Walach 2007). If this is done or could potentially be done, entanglement breaks down. While this can be formally proven for the quantum physical case, in the generalized case we simply assume it as an axiom. This has two consequences: Whenever we set out to "prove" PSI effects using classical experiments, we are in fact coding a signal. The results of the first experiment can be used, in principle, to code a signal the second time the experiment is repeated. Suppose we always see a rise in an EDA-curve (Electro Dermal Activity), shortly before a threatening image is presented. We develop the smart idea to build a danger-sensing system for soldiers, for instance, by attaching the EDA of a subject to an analyzer (Mossbridge, Tressoldi, Utts, Ives, Radin, & Jonas 2014). Whenever the EDA rises repeatedly above a threshold defined by previous experimentation, we call it a hit. And the hit moves the subject to stop, for instance. That way, we could use entanglement correlations that are nonlocal to code a signal that would be causal, and because derived from nonlocal correlations not bound to the locality conditions of special relativity. Apparently, nature does not allow such a scenario (due to the intervention-paradox), and the prediction from the NT-axiom would be: Such a device will be unreliable. Not in all instances where the EDA-signal goes

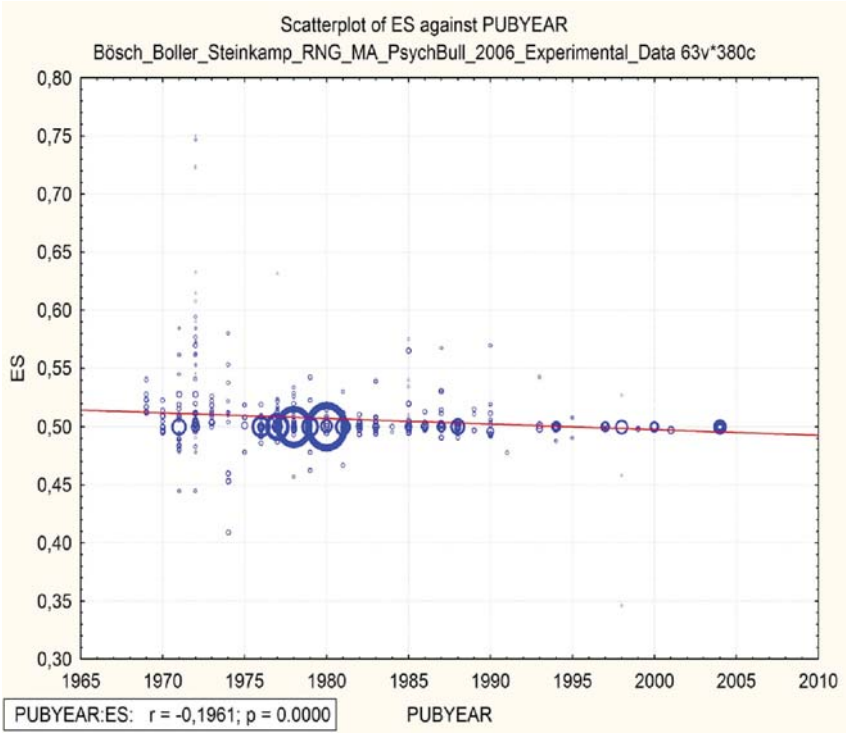

Figure 1. Scatterplot of correlation of Effect Size (ES; mean chance expectation = 0.5) versus publication year, weighted by study size that is indicated by the size of the bubbles, showing a clear significant negative correlation indicating a decline effect.

up, will there be danger, and in some dangerous situations the EDA signal will instead go down, killing the bearer of the device and demonstrating that nonlocal correlations cannot and must not be misinterpreted as causal signals. This is exactly what classical experimentation does, and this is, in our view, the reason why some decisive replications failed. Granted, overall and across experiments, meta-analyses show effects, although also here it is debated whether there is not a decline of effects.

For instance, the largest and longest sequence of comparatively identical experiments of micro-PK analyzed by Bösch, Steinkamp, and Boller (2006) clearly exhibits such a decline effect (Figure 1).

One could argue that decline effects are also expected when stricter control conditions are applied. We don't think that this is a valid argument in this case, as the experiments have been conducted the same way most of

the time and hence methodological aspects are unlikely explanations for the decline. Decline effects would also be expected as a consequence of experimental testing and thus misrepresentation of correlational effects. Hence, in the very long run, the strategy of amassing experimental evidence and distilling out a true effect size using meta-analysis might be treacherous. It can only be used if there is such a thing as a true effect size in the sense of a causal signal. Our expectation would be that this will not work long-term, because there is no causal effect in the first place.

This is also the reason, by the way, why pragmatically speaking the most robust advice one can give to victims of spuk phenomena is to observe and document the effects as closely as possible, with cameras covering all angles. This restriction of the degrees of freedom of the effect seems to have the consequence of destroying the correlations. It turned out, that in practice, this method is very successful.

Sometimes one can hear the argument: Why? In physics, entanglement correlations have been experimentally proven. Why not for the generalized case? It is important to analyze how the experimental test in physics was done. In what we term "experiment" in this paper, an experimental condition is tested against an *artificially created* control condition. This gives rise to the potential signal coding in a replication experiment. In physics, entanglement correlations were proven against a theoretical prediction that was derived from a precise theory. That is, in the physical entanglement experiments, two streams of data were generated, polarization measurements of stream A and analogous measurements of stream B. Their correlation function was then compared not against another, artificially produced control condition, but against the theoretical expectation derived from Bell's inequalities. This is a completely different experimental and theoretical situation. For in no way could the correlation function measured in this data stream in any way be used to generate a signal.

Thus, in order to construct an experimental proof in the generalized situation, we must stop classical experimentation. Some experimenters instinctively do the right thing: They never repeat experiments exactly the same way, but always change some parameters. The problem only arises with exact replications. As soon as changes are introduced—new parameters, new variables—the system is, technically and conceptually speaking, a new system. But for scientific acceptance, identical replicability of experimental paradigms is key to accepting a phenomenon as a fact.

A way out is to design an experiment which is indirect. We did that by using a matrix approach to analyzing a micro-PK experiment. In this experiment a classical micro-PK situation was generated, instructing volunteers to influence a display that was driven by a random number

generator. A classical experiment such as those conducted by the PEAR (Princeton Engineering Anomalies Research) lab, would look at the mean shift against expectation values. We constructed a large array of potential correlations using 5 physical variables derived from the experiment and 5 psychological variables, such as number of key presses and time used for the runs. Since each experiment consisted of 9 runs, we had a matrix of 45*45 cells which gives a huge array of 2,025 potential correlations between physical and psychological variables. Now, in any correlational analysis one would expect a certain number of significant correlations by chance. However, if entanglement correlations are also operative, we would expect more significant correlations than by chance. Furthermore, we constructed a negative control by letting the system run empty and pasting the psychological variables into the physical matrix, correlating these empty runs with the psychological variables. This experiment had already proven replicable in four previous attempts and was now successfully replicated by an independent replication (data in preparation for publication).

Thus it seems, if we obey the framework conditions of the NT theorem and build an experimental setup, that, in principle, cannot be used to distill a signal out of the experiment when identically replicated, GET effects seem to be amenable to experimental analysis. The correlational matrix approach obeys this boundary condition. For it is completely irrelevant which cell of the matrix will exhibit the significant correlations as long as they are more numerable than expected by chance and more than seen in the control condition. Only if we were to fix the effect and predict which cell it will show up in would we be on the trajectory of defining signals and would fail. This would, incidentally, also constitute an empirical test between the two models, the nonlocal and the local one. A local model would predict that the cells stay the same. The nonlocal model would predict that the cells have to change, but the effect overall stays the same. This is already true for the five experiments conducted so far: The effect stays the same, but the cells in the matrix with significant correlations jump between cells across experiments.

Another way to test these models against each other would be to run a series of replications of the matrix experiment. While the local model looks at the mean shift and expects a replicable mean shift over experiments, this is exactly what the nonlocal model prohibits. It would predict that correlations stay the same, but the effect in mean shift will decline toward zero.

With some ingenuity, other experimental models can be adapted such that it becomes operationally impossible to code signals from experiments and their replications. Then this would be our prediction, that GET effects can be replicably shown.

In sum: We have shown that a theoretical model that is predicated on generalized entanglement correlations derived from a generalized quantum theory can be used to model PSI effects of all kinds. This makes it preferable over other models that can only cover certain types of phenomenologies. We have also shown that such a model explains why local assumptions fail in PSI research. It makes understandable why we have exactly the data structure in the field that we have. This makes the model preferable over any tacit or explicit local signal-theoretical models. We have also shown why experimentation has to proceed in indirect ways, and we point toward future development of the field.

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RESEARCH ARTICLE

Aberrant Salience and Motivation as Factors in the Formation of Belief in Scientifically Unaccepted Phenomena

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Abstract—The aim of this study was to investigate relationships between the endorsement of beliefs in scientifically unaccepted phenomena and two psychological domains, namely proneness to aberrant salience and fundamental dimensions of human motivation. The project was undertaken as an online survey of 104 university students. "New Age" or paranormal beliefs were found to be related both to a proneness to aberrant salience and to a need for intimacy, but no such relationships were evident for traditional religious beliefs. The findings are discussed in terms of the psychological mechanisms that may underlie the development of beliefs in paranormal phenomena.

Keywords: scientifically unaccepted beliefs—aberrant salience—need for control—need for intimacy

Introduction

Research into the bases of belief and disbelief in paranormal phenomena is crucial to the assessment of human testimony as evidential support for paranormal phenomena and to an understanding of skeptical commentators' reactions to claims of such phenomena. Both parapsychologists and their critics therefore have devoted considerable effort to the empirical investigation of correlates of paranormal and related beliefs (for a survey of this literature see Irwin 2009). Two potential correlates were investigated in the present study, namely, aberrant salience and basic dimensions of motivation.

Broadly speaking, either of two major cognitive systems (Stanovich & West 2000) appear to be implicated in the formation of a belief in a paranormal phenomenon. The first system engages a good deal of rational reflection. Thus, many parapsychologists and some skeptics assert that their belief or disbelief is founded on a critical reading of the relevant empirical literature (Irwin 2014), and some members of the general

population also endorse a paranormal belief after thoughtful analysis of a personal experience or a secondary source (Blackmore 1984, Kennedy 2000). A second cognitive pathway is much less analytical and involves a rapid intuitive decision that some anomalous event warrants a paranormal interpretation. A substantial amount of research indicates that among the general population the intuitive–experiential route to a paranormal belief is particularly dominant (e.g., Aarnio & Lindeman 2005, Irwin & Young 2002). This view is consistent with growing evidence that in the general population the formation of paranormal beliefs has much in common with the mechanisms independently identified to underlie the development of clinically defined delusions (Irwin, Dagnall, & Drinkwater 2012a, 2012b). This is not to imply that paranormal beliefs are intrinsically false. The latter construction of delusions has now been officially abandoned (American Psychiatric Association 2013); rather, a delusion is currently defined as a belief formed without adequate scrutiny of supportive evidence and persistently held in the face of any conflicting evidence. Thus, like delusions, belief in paranormal phenomena has been found to be associated with a deficiency in subjecting inferences to adequate reality testing; specific biases in reasoning such as emotion-based reasoning and catastrophizing; inferential confusion or the inclination to draw inferences on the basis of remote theoretical possibilities; proneness to conformation bias or the neglect of disconfirming information; and distinctive metacognitive beliefs or attitudes toward one's thinking, particularly the tendency to focus attention on thought processes (cognitive self-consciousness) and negative beliefs about thoughts concerning uncontrollability and danger (Irwin, Dagnall, & Drinkwater 2012a, 2012b).

A behavioral characteristic increasingly promoted as a key marker of proneness to clinically defined delusions is known as *aberrant salience*. A section of the cerebral cortex dubbed the "salience network" (comprising the bilateral insula and anterior cingulate) has been shown to play a role in activating relevant brain regions for processing sensory inputs. When an anomalous experience occurs, a dysfunction in the brain's dopamine system has been proposed to make even a small amount of evidence for an inference about the experience seem unusually salient, leading to premature conclusions and instigating psychotic perceptions and beliefs (Kapur 2003, Lau, Wang, Hsu, & Liu 2013, Palaniyappan, Mallikarjun, Joseph, White, & Liddle 2010, Poletti & Sambataro, 2013, Smeets, Lataster, van Winkel, de Graaf, ten Have, & van Os 2013, Winton-Brown, Fusar-Poli, Ungless, & Howes 2014). Aberrant salience thereby plays a crucial mediational role in the development of psychosis and other conditions in which delusions are predominant (e.g., Balzan, Delfabbro, Galletly, & Woodward 2013, Cicero,

Becker, Martin, Docherty, & Kerns 2013, Poletti & Bonuccelli 2013). This mechanism reportedly is entailed also in the formation of subclinical delusional beliefs (Balog, Somlai, & Kéri 2013). Taken in conjunction with the diverse evidence that cognitive processes associated with delusions also underlie the formation of paranormal beliefs in the general population (Irwin, Dagnall, & Drinkwater 2012a, 2012b), there are grounds here for investigating the relationship between paranormal beliefs and aberrant salience. In addition, a recent study by Irwin, Schofield, and Baker (2014) reported a positive correlation between aberrant salience and an inclination to attribute anomalous experiences to paranormal factors, a behavior which closely implicates belief in the paranormal. The following hypothesis therefore was formulated.

Hypothesis 1: The intensity of paranormal and related beliefs is positively related to proneness to aberrant salience.

A second psychological domain investigated in relation to paranormal beliefs in this study was motivation. Some instructive theoretical reviews of the possible motivation for paranormal beliefs among the general population (e.g., Krippner & Winkler 1996, Schumaker 1990) have been undertaken. Kennedy, for example, has proposed that such beliefs may be

> influenced by motivations to have control and efficacy, to have a sense of meaning and purpose in life, to be connected with others, to have transcendent experiences, to have self-worth, to feel superior to others, and to be healed. (Kennedy 2005:263)

Relatively little empirical attention has been focused on this topic, but the principal motivational factor identified in the associated body of research is the need for a sense of mastery or control over life events (Greenaway, Louis, & Hornsey 2013, Irwin 1992, 2000, Keinan 2002); that is, having paranormal beliefs seems to engender an impression that one potentially has the power to control unanticipated hazards that may arise in life or to foresee such events and thereby be mentally prepared for them. The major shortcoming of this body of empirical research is that the need for control has been studied in isolation, with no account of other motivational factors for comparative purposes. The present study therefore undertook a multifactorial approach to motivation in relation to belief in the paranormal.

What are the major elements of human motivation? People have some basic bodily needs that must be met for survival; thus, we all have a need for food (nutriments), a need for water, and so on. Such physiological needs

are essentially universal and as a consequence they are of limited interest to psychologists seeking to understand the motivation of more complex human behaviors such as endorsement of paranormal beliefs. Higher-level, more uniquely human, motivations show greater variation from one person to another and thus potentially have more utility for explaining how complex behaviors are energized and directed. The taxonomy constructed by McClelland (1961, 1987) nominated the so-called "Big Three" human motives to be the need for achievement, the need for power, and the need for affiliation (see also Heckhausen & Heckhausen 2008). The *need for achievement* comprises a desire for significant accomplishment, mastering of skills, and setting high standards; the *need for power* concerns a desire for status and prestige and for having an influence over other people; and the more prosocial *need for affiliation* relates to a desire for warm interpersonal relations. The need for affiliation has since been dichotomized into the *need for affiliation* and the *need for intimacy* (e.g., see Sokolowski 2008), with the former now focused on a desire for a sense of belonging and for getting along with people in general, and the latter concerned more with the desire to establish close emotional relations with certain select people. McClelland (1987) also acknowledged that these motives can have directional components, one pertaining to the pursuit of a positive desired goal and the other pertaining to the avoidance of states that negate the positive goals. Thus, a need for achievement, for example, might be driven mainly by the desire for a sense of accomplishment or alternatively, by a desire to avoid failure. The present project took account of the four motives for achievement, power, affiliation, and intimacy and included a check for the directional component of these. In light of the dearth of multivariate motivational investigations of paranormal and related beliefs, the following prediction was formulated as an exploratory hypothesis.

Hypothesis 2: The intensity of paranormal and allied beliefs is related to some combination of a need for achievement, a need for power, a need for affiliation, and a need for intimacy.

Method

The project was a correlational study conducted as an online questionnaire survey.

Participants

The survey was completed by a convenience sample of 104 Australian university students enrolled in an Introductory Psychology course; the

students received course credit for their participation in the project but were not under any compulsion to undertake this specific survey. There were 19 males and 85 females in the sample. The mean age was 33.90 years (*SD* = 10.24), with a range of 19–65 years.

Materials

The survey inventory included a measure of aberrant salience, a measure of the intensity of beliefs in scientifically unaccepted phenomena, and finally, a multifactorial questionnaire on motivation, plus a few items on basic demographic characteristics. All participants completed these questionnaires in that order.

Proneness to aberrant salience was assessed with the *Aberrant Salience Inventory* (ASI; Cicero, Kerns, & McCarthy 2010). The ASI has 29 dichotomous (Yes/No) items surveying experiences of aberrant salience (e.g., "Do normally trivial observations sometimes take on an ominous significance?"). A total ASI score is computed as the total number of affirmative responses over the 29 items. Cicero, Kerns, & McCarthy (2010) report the scale has satisfactory convergent and discriminative validity, as well as high internal consistency.

Beliefs in scientifically unaccepted phenomena were indexed with *The Survey of Scientifically Unaccepted Beliefs* (SSUB; Irwin & Marks 2013), labeled the "Survey of Popular Beliefs" for general use. The SSUB is a 20-item, self-report, interval-level measure of the intensity of paranormal and related beliefs and was generated through a factor analysis of a wide range of beliefs in scientifically unaccepted phenomena. Responses to the SSUB items are made on a 5-point scale (1 = Strongly disagree, to 5 = Strongly agree). The SSUB comprises two scales denoted New Age Beliefs (NAB, 15 items) and Traditional Religious Beliefs (TRB, 5 items). Scores on each scale are computed as the sum of responses to the items in the respective scale and then converted to scores with interval-level measurement using the conversion table provided by Irwin and Marks (2013:Appendix 3). Scores for NAB may range from 13.37 to 36.53, and those for TRB, 15.62 to 34.12. The Rasch measures for both scales have been standardized with a mean of 25 and a standard deviation of 5. Irwin and Marks (2013) have documented the dimensional purity and other psychometric characteristics of the SSUB, and generally these appear satisfactory.

Dimensions of human motivation were surveyed with the *Unified Motives Scales* (UMS; Schönbrodt & Gerstenberg 2012). The UMS was derived from an analysis under Item Response Theory of several pre-existing motivation questionnaires supplemented by a few new items. The

statistical techniques of Item Response Theory identify a small number of items for each factor that reliably index performance on the complete set of items in that factor, thereby substantially reducing administration time in subsequent applications. Thus, the UMS measures the strength of the Achievement, Power, Affiliation, and Intimacy motives with a mere three items for each factor. For each item responses are made on a 6-point scale (depending on the item, either 0 = Strongly disagree, to 5 = Strongly agree; or 0 = Not important to me, to 5 = Extremely important to me). For each factor a total score is computed as the sum of responses to the items in that scale, and thus may range from 0 to 15. Psychometric characteristics of the four factor scores are impressive (Schönbrodt & Gerstenberg 2012). In addition, the UMS provides an additional item for assessing the directional component of performance on each of the Achievement, Power, and joint Affiliation–Intimacy dimensions; these are dubbed the Fear of Failure, Fear of Losing Control, and Fear of Rejection, respectively.

Procedure

The questionnaire inventory was administered as an online survey compiled using *Qualtrics Survey Software* (Qualtrics Labs, Provo, UT; see http://www.qualtrics.com). The stated aim of the study was "to survey various popular beliefs and relate them to aspects of psychological style and personal experiences" which would "help us to appreciate the role of these beliefs in people's lives." People aged at least 18 years were said to be eligible to take part, and their participation was anonymous and voluntary, with withdrawal from the exercise permitted at any time. The need for frankness in responding was stressed. The Qualtrics system automatically prevented participation more than once by the same person.

Recruitment was terminated soon after the target of 100 completions had been achieved.

Results

Descriptive statistics for the principal variables of the study are given in Table 1. The distribution of some of the UMS variables was significantly skewed; bivariate relationships therefore were indexed by Spearman correlation coefficients. Table 1 also presents the Spearman correlations between components of the SSUB (NAB and TRB) and both the ASI and the four factors of the UMS (Achievement, Power, Affiliation, and Intimacy). Where appropriate, Bonferroni corrections to the significance levels of these correlation coefficients have been applied on a hypothesis-by-hypothesis basis (Abramson, Wolfson, Marcotte, Grant, & HNRC Group 1999, Shaffer

TABLE 1
Descriptive Statistics and Spearman Correlations of Scientifically Unaccepted Beliefs (SSUB) with Aberrant Salience (ASI) and Motivation (UMS)

Variable	M	SD	Range	Skewness	Spearman rho NAB	TRB
SSUB Components						
NAB	23.57	2.27	18–29	.04	–	.37***
TRB	23.40	5.05	16–34	.24	.37***	–
ASI	15.51	6.32	4–29	–.06	.37**	.03
UMS Factors						
Achievement	10.87	2.49	5–15	–.06	.02	.06
Power	7.32	3.14	0–15	.06	.11	.07
Affiliation	8.33	2.73	0–14	–.49*	.07	.16
Intimacy	11.18	2.68	1–15	1.03***	.25**	.21*

*p < .05; **p < .01; ***p < .001 (where appropriate, corrected within each hypothesis)

1995); given that Hypothesis 2 was exploratory, however, the significance levels of correlations between SSUB components and UMI factors remain uncorrected.

Under Hypothesis 1, the intensity of beliefs in scientifically unaccepted beliefs (NAB and TRB) was proposed to be positively predicted by proneness to aberrant salience (ASI). This prediction is supported by the correlation between NAB and ASI shown in Table 1 (*rho* = .37, corrected $p < .01$), but not by the correlation between TRB and ASI (*rho* = .03, $p = .75$). However, given that paranormal beliefs often vary with age and gender (Irwin 2009), a more rigorous statistical test of the hypothesis would best take account also of these demographic variables. Hypothesis 1 therefore was assessed through a multiple regression analysis of NAB scores on three predictors, namely, aberrant salience (ASI), gender, and age. The regression equation was significant [$F(3,100) = 10.74$, $p < .001$; adjusted $R^2 = .221$], with independently significant contributions to the regression made by ASI (*partial r* = .46, beta = .47, $p < .001$), (female) gender (*partial r* = .30, beta = .27, $p < .01$), and to a borderline extent, age (*partial r* = .30, beta = .18, $p = .054$). The correlation between TRB and ASI (*rho* = .03) does not encourage a similar regression analysis for TRB, but for the sake of completeness it may be reported that the regression of TRB on ASI, gender, and age, was not significant [$F(3,100) = .43$, $p = .73$; $R^2 = .013$]. Hypothesis 1 is confirmed for New Age or paranormal beliefs but not with respect to traditional religious beliefs.

Under the exploratory Hypothesis 2, the intensity of beliefs in scientifically unaccepted beliefs (NAB and TRB) was proposed to be

predicted by the UMI dimensions of human motivation. This prediction is supported by the correlations (see Table 1) between the Need for Intimacy and both NAB (*rho* = .25, uncorrected $p < .01$) and TRB (*rho* = .21, $p < .05$), although there appears to be no independent relationship for any of the other types of motivation. Again, however, a more rigorous statistical test of the hypothesis would best take account also of the demographic variables of gender and age. Hypothesis 2 therefore was assessed through multiple regression analyses of NAB and TRB scores on the four UMI components, gender, and age. As the distribution of two of the UMI factors was significantly skewed (see Table 1), bootstrapping was utilized in the analysis (1,000 samples with bias corrected and accelerated analyses); bootstrapping is a procedure for using the original sample data to estimate a variable's distribution in the population and thereby avoids the need to meet the statistical requirement for a normal distribution (IBM 2011). The regression equation for NAB was significant [$F(6,97) = 2.25$, $p < .05$; adjusted $R^2 = .068$], with independently significant contributions to the regression made by the Need for Intimacy (*partial r* = .22, beta = .27, $p < .05$) and (female) gender (*partial r* = .22, beta = .22, $p < .05$). The regression equation for TRB, on the other hand, was not significant [$F(6,97) = 1.29$, $p = .27$; adjusted $R^2 = .017$]. Hypothesis 2 is supported in that there is a relationship between New Age beliefs and the Need for Intimacy. This result nevertheless needs to be checked for the directional component of the Need for Intimacy. A post hoc computation found a significant Spearman correlation between NAB and the Fear of Rejection item of the UMI (*rho* = .37, $p < .001$); that is, the "need for intimacy" in this context may well have been driven at least in part by the negative factor of a fear of rejection.

Discussion

The statistical analyses for Hypothesis 1 confirm that aberrant salience is a predictor of the intensity of "New Age" or paranormal beliefs, but not of the intensity of traditional religious beliefs. These findings are consistent with the observation by Irwin, Schofield, and Baker (2014) that the inclination to attribute anomalous events to paranormal processes is linked to the characteristic of aberrant salience. Given that aberrant salience is a key marker of proneness to clinically defined delusions (e.g., Winton-Brown, Fusar-Poli, Ungless, & Howes 2014), the data provide additional support for the view that in the general population the cognitive processes independently identified to underlie the formation of delusions are implicated also in the formation of beliefs in paranormal phenomena (Irwin, Dagnall, & Drinkwater 2012a, 2012b). I reiterate that this does not mean either paranormal beliefs or paranormal phenomena themselves

are intrinsically baseless; rather, the appropriate inference is that many people in the general population endorse paranormal beliefs essentially for emotional reasons rather than on the basis of critical rational scrutiny of the available evidence. At the same time the findings do signal caution in relying on human testimony about anomalous experiences as evidence for paranormal processes; the latter require empirical scrutiny from quite a different perspective.

Cognitive processes involved in the predisposition to paranormal beliefs are clearly fundamental to an understanding of this behavior, but the associated indications that paranormal beliefs spring in part from emotional considerations point in turn to the necessity of taking some account of motivational factors. There is insufficient evidence here that the fundamental human motives play a role in the formation of traditional religious beliefs; despite the significant correlation between TRB and the need for intimacy ($rho = .21$, $p < .05$), this association was no longer evident when due account was taken of the demographic variables gender and age. For New Age or paranormal beliefs, on the other hand, the need for intimacy was found to be a relevant factor. Further research is required to clarify whether this relationship principally reflects the positive component of the desire to establish close relations or the negative component of a fear of being rejected by significant others. Nonetheless, the observed relationship with a need for intimacy does affirm the needs-serving role of belief in the paranormal. In addition, this appears to be the first study to identify explicitly the potential role of an intimacy motive in the development of paranormal beliefs, and future research could appropriately take greater cognizance of this factor. The effect size found for this variable ($rho = .25$) was not high, so the contribution of the need for intimacy should not be overstated, but one must remember that many factors other than motivation are known to contribute to the development of paranormal beliefs (for a review see Irwin 2009).

The observation that a need for power was not a significant predictor of paranormal belief is surprising in light of past research suggesting the role of a need for control in this context (e.g., Greenaway, Louis, & Hornsey 2013). This is not to claim that the earlier research is refuted by my findings, but perhaps one key facet of a need for control here relates to a desire for power over other people as a means to gain and to maintain intimacy. This interpretation is consistent with Irwin's (1992) report that the motivational element of paranormal beliefs is a need for specifically *interpersonal* control. In any event the present findings do reinforce the insufficiency of studying the role of a need for control in isolation from other potential motives.

The study's limitations must also be acknowledged. The sample cannot

reasonably be regarded as strongly representative of the general population. Thus, perhaps Psychology students by their nature tend to have both strong paranormal beliefs and a high need for intimacy, although this possibility is somewhat at odds with participants' relatively skeptical performance on the SSUB (for NAB, sample $M = 23.57$ as compared with the normative population $M = 25.00$). Nonetheless, replication of the study with a more representative sample would be appropriate. In addition, the ASI, SSUB, and UMI questionnaires in the survey inventory were not counterbalanced for their order of administration. Future replications might usefully take account also of this procedural shortcoming. Finally, the UMI scales address explicit motives only, that is, the person's conscious self-perception of his or her goals and values. There remains a possibility that various implicit or ineffable motives (McClelland, Koestner, & Weinberger 1989) are also important to energizing the endorsement of paranormal beliefs.

A further potential limitation may arise from the operation of response sets. A reviewer of this paper notes that the ASI comprises items worded only in the positive direction and thus may be vulnerable to "acquiescent" response sets; that is, participants may realize they give the same answer to the first few items of this questionnaire and decide to give the same answer to subsequent items with insufficient regard to the items' content, or they may bring to the test session a habitual tendency to agree with propositions rather than taking issue with them. If a similar vulnerability obtained in the other measures was used in the survey, the observed relationships between the ASI and these measures might then be a mere artefact of the operation of these response sets. On the other hand, the only context in which such an artefact could have been problematic concerns the correlations of the ASI with the SSUB scales, and the latter comprise items that are counterbalanced for the direction of their wording, that is scores on these scales are not substantially affected by acquiescence response sets. Be this as it may, further investigation of the role of aberrant salience in the formation of scientifically unaccepted beliefs might usefully employ a performance measure of aberrant salience such as that devised by Roiser, Stephan, den Ouden, Barnes, Friston, & Joyce (2009).

In a rejoinder to this analysis the reviewer argued that an acquiescence response bias may constitute the inclination "to agree with plausible-sounding items irrespective of their specific content." This perspective seems to reach far beyond the usual definition of an acquiescence response bias as occurring "when respondents agree to, or endorse items without regard to content" (Furr 2011:23), given that the plausibility of an item must necessarily take some account of content. Nevertheless, the reviewer concedes that an acquiescence response bias is unlikely to account for

the observed relationships between ASI and SSUB scores, given that the relationship was null for the Traditional Religious Beliefs SSUB scale which might be presumed to comprise relatively plausible-sounding items. The reviewer also suggested that the operation of an acquiescence response bias could be tested by comparing the ASI–SSUB relationships across gender and age on the assumption that such a response bias may on average be stronger among women and older participants. Post hoc statistical analyses found no significant variation in the relationships between ASI and SSUB scores with these demographic variables.

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HISTORICAL PERSPECTIVE

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

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I believe that I have really found the relationship between gravitation and electricity, assuming that the Miller experiments are based on a fundamental error. Otherwise, the whole relativity theory collapses like a house of cards.
—Albert Einstein, in a letter to Robert Millikan, June 1921 (Clark 1971:328)

The effect [of ether-drift] has persisted throughout. After considering all the possible sources of error, there always remained a positive effect. —Dayton Miller (1928:399)

Abstract—The author reviewed the experimental ether-drift experiments and publications of Michelson–Morley, Dayton Miller, Michelson–Pease–Pearson, and more recent others, from the late 1800s through the present. Many of these historical studies presented positive results in detecting a cosmic ether, and ether-drift through space. Among these experiments, the most widely cited Michelson–Morley experiment of 1887, which did show a slight positive result (and never the claimed "null"), was found to be the least significant or robust in terms of experimental procedures and actual data collected, as compared with the far more important 1920s' study by Miller on Mount Wilson near Los Angeles, California. Most ether-drift experiments yielding claimed negative results were plagued by various unwarranted assumptions about the capacity of an ether-drift to penetrate dense materials such as stone buildings or metal shielding, or that ether flow would "contract" matter, including measuring instruments, leaving the ether-drift undetectible. Some obtained positive results, but the authors chose to interpret them as "negative" due to unwarranted assumptions demanding extremely fast ether-drift velocities near to the Earth's surface. Miller was the first to experimentally account for these issues, his most important study made atop Mount Wilson in a thermal shelter, with the largest light-beam interferometer ever constructed, and where the light-beam path was enclosed only by light glass or cardboard. His procedures account-

ed for a matter-retarded cosmic ether-drift with a reduced velocity closer to the Earth's surface. Miller thereby obtained significant positive results over four epochs of study. Albert Einstein also was aware of these issues, and admitted openly that if Miller was correct, then his own relativity theory would "collapse like a house of cards." In subsequent years, however, the followers of Einstein defeated this evidence for the cosmic ether by public ridicule and political tactics, not too different from the modern "skeptic" movement. The Shankland, McCuskey, Leone, and Kuerti article claiming to have reviewed Miller's Mount Wilson data, well after all the old ether-drift experimenters were dead and could no longer defend their findings, were also specifically reviewed and found to *not* support their own stated conclusions, thereby leaving the question of a cosmic ether and ether-drift as an open and unresolved question, or one which positive evidence indicates has been proven out. More recent ether-drift experiments from the last quarter of the 20th and early 21st Centuries, notably by Galaev, Múnera, and others, using radiofrequencies, light-beam interferometery, and other novel methods, have provided further proof for the existence of a cosmic ether in space.

Introduction

The history of science records the 1887 ether-drift experiment of Albert Michelson and Edward Morley as a pivotal turning point, where the energetic *ether of space* was discarded by mainstream physics. Thereafter, the postulate of "empty space" was embraced, along with related concepts that demanded constancy in light-speed, such as Albert Einstein's relativity theory. The now-famous *Michelson–Morley Experiment* is widely cited, in nearly every physics textbook, for its claimed "null" or "negative" results. Less known, however, is the far more significant and detailed work of Dayton Miller.

Dayton Miller's 1933 paper in *Reviews of Modern Physics* details the positive results from more than 20 years of experimental research into the question of ether-drift, and remains *the most definitive body of work on the subject of light-beam interferometry.* Other positive ether-detection experiments have been undertaken, such as the work of Sagnac (1913) and Michelson–Gale (1925), documenting the existence of light-speed variations ($c + v > c - v$), but these were not adequately constructed for detection of a larger cosmological *ether-drift,* of the Earth and Solar System moving through the background of space. Dayton Miller's work on ether-drift was so constructed, however, and yielded *consistently positive results.*

Miller's work, which ran from 1906 through the mid-1930s, most strongly supports the idea of an ether-drift, of the Earth moving through a cosmological medium, with calculations made of the actual direction and magnitude of drift (Miller 1930, 1934). By 1933, Miller concluded that the

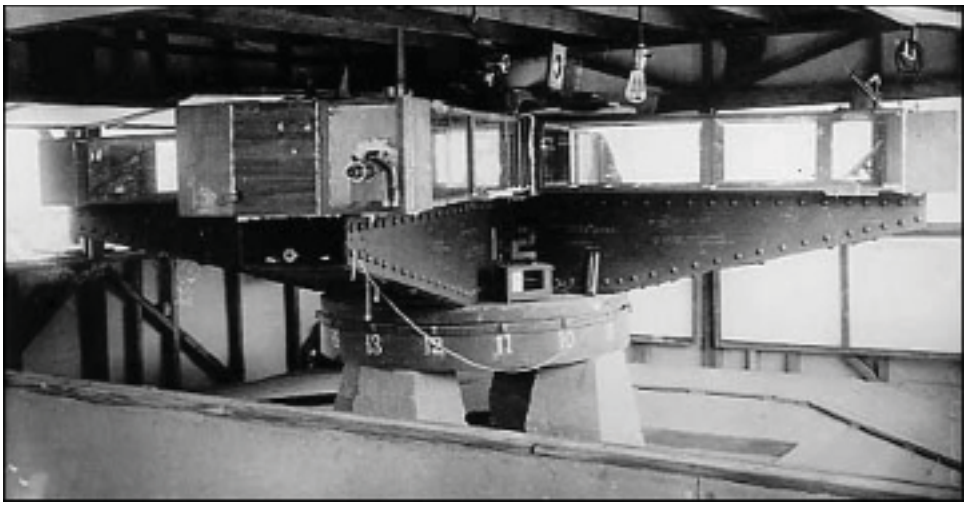

Figure 1. Dayton Miller's light-beam interferometer, at 4.3 meters across, was the largest and most sensitive of this type of apparatus ever constructed, with a mirror-reflected roundtrip light-beam path of 64 meters. It was used in a definitive set of ether-drift experiments on Mt. Wilson, 1925–1926. Protective insulation is removed in this photograph, and windows were present all around the shelter at the level of the interferometer light-path (see below).

Earth was drifting at a speed of 208 km/sec toward an apex in the Southern Celestial Hemisphere, toward Dorado, the swordfish, right ascension 4 hrs 54 min, declination of −70° 33′, in the middle of the Great Magellanic Cloud and 7° from the southern pole of the ecliptic (Miller 1933:234). This is based upon a measured displacement of around 10 km/sec. at the interferometer, and assuming the Earth was pushing through a stationary but Earth-entrained ether in that particular direction, which lowered the velocity of the ether from around 200 to 10 km/sec at the Earth's surface. Today, however, Miller's work is hardly known or mentioned, as is the case with nearly all the experiments that produced positive results for an ether in space. Modern physics today points instead to the much earlier and less significant 1887 work of Michelson–Morley as having "proved the ether did not exist."

While Miller had a rough time convincing some of his contemporaries about the reality of his ether measurements, he clearly could not be ignored in this regard. As a graduate of physics from Princeton University, President of the American Physical Society and Acoustical Society of America, Chairman of the Division of Physical Sciences of the National

Research Council, Chairman of the Physics Department of Case School of Applied Science (today Case Western Reserve University), and Member of the National Academy of Sciences well-known for his work in acoustics, Miller was no "outsider." While he was alive, he produced a series of papers presenting solid data on the existence of a measurable ether-drift, and he successfully defended his findings to not a small number of critics, including Einstein. His work employed light-beam interferometers of the same type used by Michelson–Morley, but of a more sensitive construction, with a significantly longer light-beam path (Figure 1). He periodically took the device high atop Mount Wilson (above 6,000′ elevation) in Los Angeles, California, where Earth-entrained ether-theory predicted the ether would move at a faster speed than close to sea level. While he was alive, Miller's work could not be fundamentally undermined by the critics. However, toward the end of his life, he was subject to isolation as his ether-measurements were simply ignored by the larger world of physics, which was then captivated by Einstein's relativity theory.

After his death in 1941, Miller's work was finally "put to rest" in the publication of a critical 1955 paper in *Reviews of Modern Physics* by Robert S. Shankland, S. W. McCuskey, F. C. Leone, and G. Kuerti (hereafter referred to as the "Shankland team" or "Shankland" paper), which purported to make a fair and comprehensive review of Miller's data, finding substantial flaws.

Lloyd Swenson's *Ethereal Aether* (1972) presents a cursory discussion of Miller and his "inexplicable" positive results, giving a high degree of significance to the Shankland team's critique. Swenson wrote:

> . . . . Shankland, after extensive consultation with Einstein, decided to subject Miller's observations to a thoroughgoing review. . . . Einstein saw the final draft [of Shankland's pre-publication manuscript] and wrote a personal letter of appreciation for having finally explained the small periodic residuals from [Miller's] Mount Wilson experiments. (Swenson 1972:243)

In August of 1954, Einstein replied to Shankland:

> I thank you very much for sending me your careful study about the Miller experiments. Those experiments, conducted with so much care, merit, of course, a very careful statistical investigation. This is more so as the existence of a not trivial positive effect would affect very deeply the fundament of theoretical physics as it is presently accepted. You have shown convincingly that the observed effect is outside the range of accidental deviations and must, therefore, have a systematic cause [having] nothing to do with 'ether wind', but with differences of temperature of the air traversed by the two light bundles which produce the bands of interference. (Shankland 1973a:2283)

From the above accounts, it certainly would appear that the case was finally closed on Miller, and that all the lose ends were finally cleaned up. With the strongest support for cosmological ether-drift swept aside as the alleged product of temperature errors, Einstein's theory of relativity continued to grow in popularity and dominance.

Here, I will compare the Shankland team's 1955 criticisms to what is actually contained in Miller's published works, notably his 1933 paper which summarized his work on the subject. It is my contention that the Shankland paper, published 14 years after Miller's death, attempted to resurrect speculative criticisms which had previously been raised and rebutted when Miller was alive and not given serious credibility except among anti-ether fundamentalists. The Shankland paper also misrepresented Miller's data in several ways, and furthermore misrepresented itself as a definitive rebuttal, which it most certainly was not. In order to properly address this major issue of science history, I will also recount the central facts of Miller's work.

The basic principles of light-beam interferometry for detection of ether-drift are described in most textbooks, albeit with typical factual errors (i.e. the slight positive result of the Michelson–Morley experiment is nearly always misrepresented as a "null" or "zero" result) and so will not be repeated here. However, there were novel methods introduced by Miller into the discussion of ether-drift, along with interferometer construction features and principles of operation that are not widely known—these will be detailed.

Miller's Work with Interferometry

Miller began his work on the question of ether-drift and light-beam interferometry with Edward Morley, from 1902 to 1906, using an apparatus three times as sensitive as the original interferometer used by Michelson–Morley in 1887 (Morley & Miller 1905, 1907). In later years, from 1921 through 1928, Miller made additional refinements for sensitivity in his interferometer, obtaining increasingly significant positive results. His interferometer was the most massive and sensitive ever constructed, with iron cross-arms 4.3 meters across, and standing 1.5 meters in height. Four sets of mirrors were mounted on the end of each cross-arm to reflect light beams back and forth 16 times horizontally with a total roundtrip light path of 64 meters, starting from the same light source, and finally recombined to form interference fringes whose movement relative to a pointer was read through a magnifying telescope. The large apparatus was floated inside a circular tank of liquid mercury, providing a frictionless base for rotation. Fringe-shift movements (in tenths of a fringe, plus or minus in direction) were observed by one person who walked around with the apparatus while

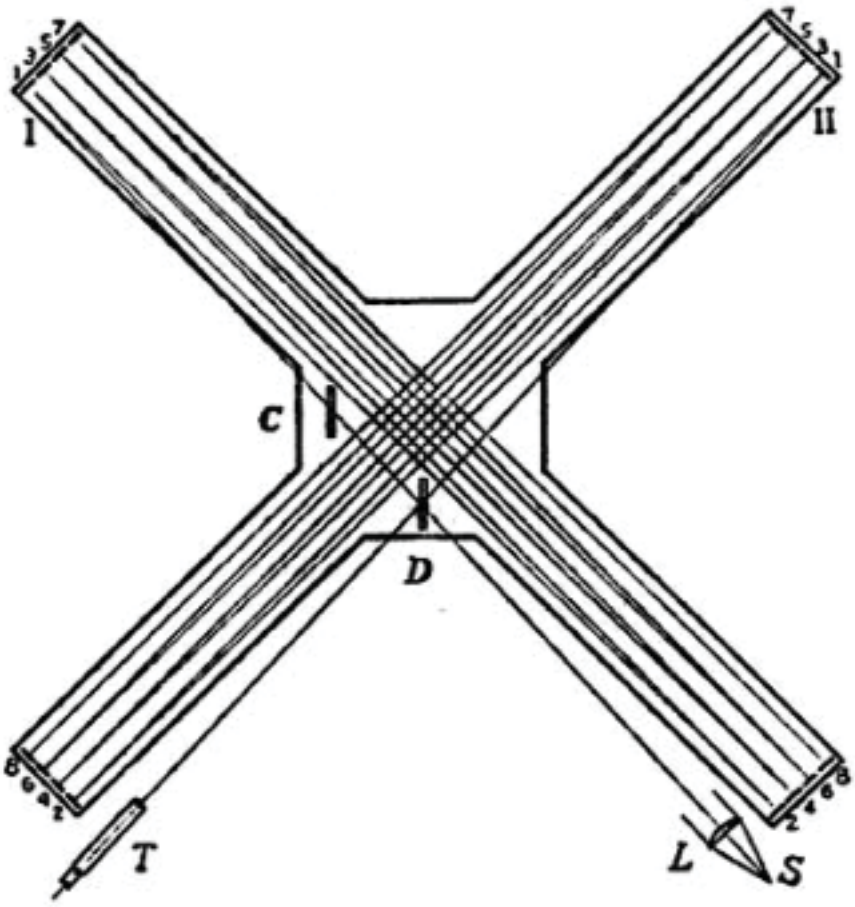

Figure 2. Light Paths of the Michelson–Morley and Miller Interferometers, as seen from above. Source (S) generates light which passes through lens (L) and is then split by half-silvered mirror (D). Beams then reflect back and forth along beams (I and II) to mirrors (numbered 1–8) before finally being recombined by half-silvered mirror (D) and reflected to small telescope eyepiece (T) where interference fringes are observed.

it turned, speaking out the readings at the ring of a bell which automatically sounded when electrodes made contact at 24° intervals (dividing the circle into 15 parts). An assistant then noted the readings on paper. The readings from consecutive turns of the apparatus were then organized into "sets," which were made at different times of day and at different seasons of the year. Datasets were then averaged according to a sidereal time clock, which

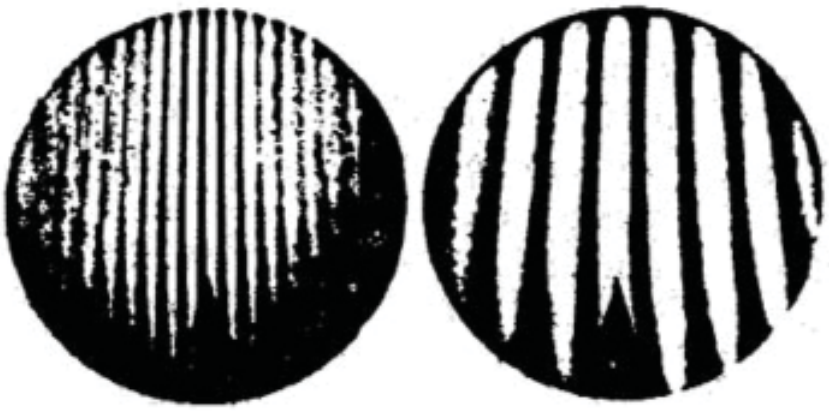

Figure 3. Light-interference fringes as seen in the interferometer telescope. Magnified by an eyepiece with precise graduated markings, one could observe the lateral movement or shifting of fringes as the instrument was rotated. Miller's larger apparatus used a 50x telescope, allowing magnified readings down to hundredths of a fringe, though readings were typically recorded in tenths.

was correlated with external celestial coordinates. Miller became convinced of an ether Earth-entrainment effect, which necessitated using the apparatus at higher altitudes (to reduce the anticipated entrainment-effect of sea-level environments), and he additionally undertook the experiments in structures where the walls at the level of the light-path were open to the air, covered with canvas (Figure 2, Figure 3). Only glass or glass and light paper covers were used along the light-beam paths, with all wood or metal shielding removed. By contrast, the original Michelson–Morley interferometer (Figure 4) had a roundtrip light-path of around 22 meters (Michelson 1927:153), and the experiments were undertaken with an opaque wooden cover over the instrument, situated in the basement of one of the large stone buildings at the Case School in Cleveland.

In his 1933 paper, Miller published the most comprehensive summary of his work, and the large quantity of data which supported his conclusions. A total of more than 200,000 individual readings were made, from more than 12,000 individual turns of the interferometer, undertaken at different months of the year, starting in 1902 with Edward Morley at the Case School in Cleveland, and ending in 1926 with his Mount Wilson experiments (Figure 5). These data do not include many rigorous control experiments undertaken at the Case School Physics Department from 1922 to 1924 (Miller 1922, 1925). More than half of Miller's readings were made at Mount Wilson using the most sophisticated and controlled procedures, with

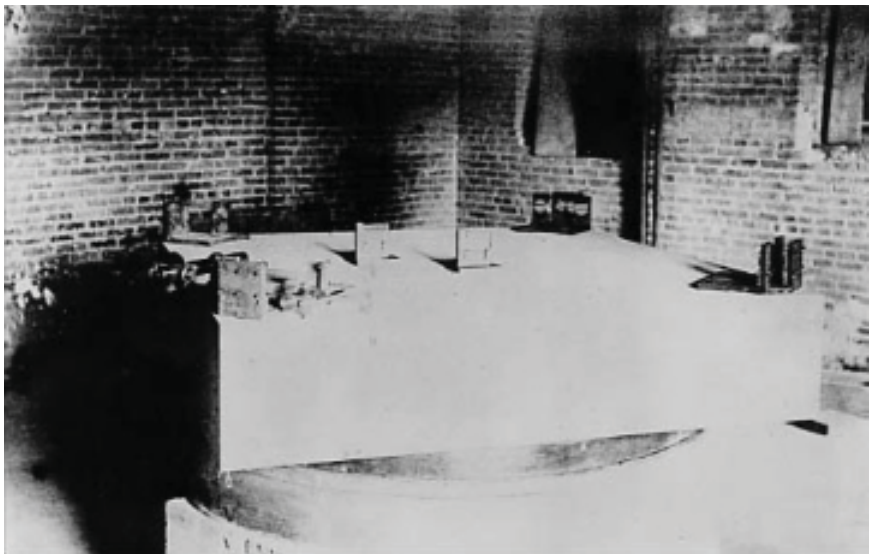

Figure 4. The original Michelson-Morely interferometer with an approximate 22-meter roundtrip light-beam path, mounted on a concrete platform in the basement of the old Case School Physics building (today, Case Western Reserve University). This interferometer was about one-third as sensitive as the 64-meter interferometer constructed later by Miller. A protective wood cover over the light-beam paths is removed for this photograph. Such dense coverings and stone-basement shielding, as Miller showed, slowed down the movement of the ether. These problems, along with a relatively short light-path, and placement at a relatively low altitude basement location, virtually guaranteed only a small (but never "null") measured result.

the most telling set of experiments in 1925 and 1926. By contrast, we can mention here, the original Michelson–Morley experiment of 1887 involved only *six hours of data collection* over four days (July 8, 9, 11, and 12 of 1887), with a grand total of only 36 turns of their interferometer. Even so, as shown below, Michelson–Morley originally obtained *a slight positive result* which has been systematically ignored or misrepresented by modern physics. As stated by Michelson–Morley:

> . . . the relative velocity of the earth and the ether is probably less than one-sixth the earth's orbital velocity, and certainly less than one-fourth. . . . The experiment will therefore be repeated at intervals of three months, and thus all uncertainty will be avoided. (Michelson–Morley 1887)

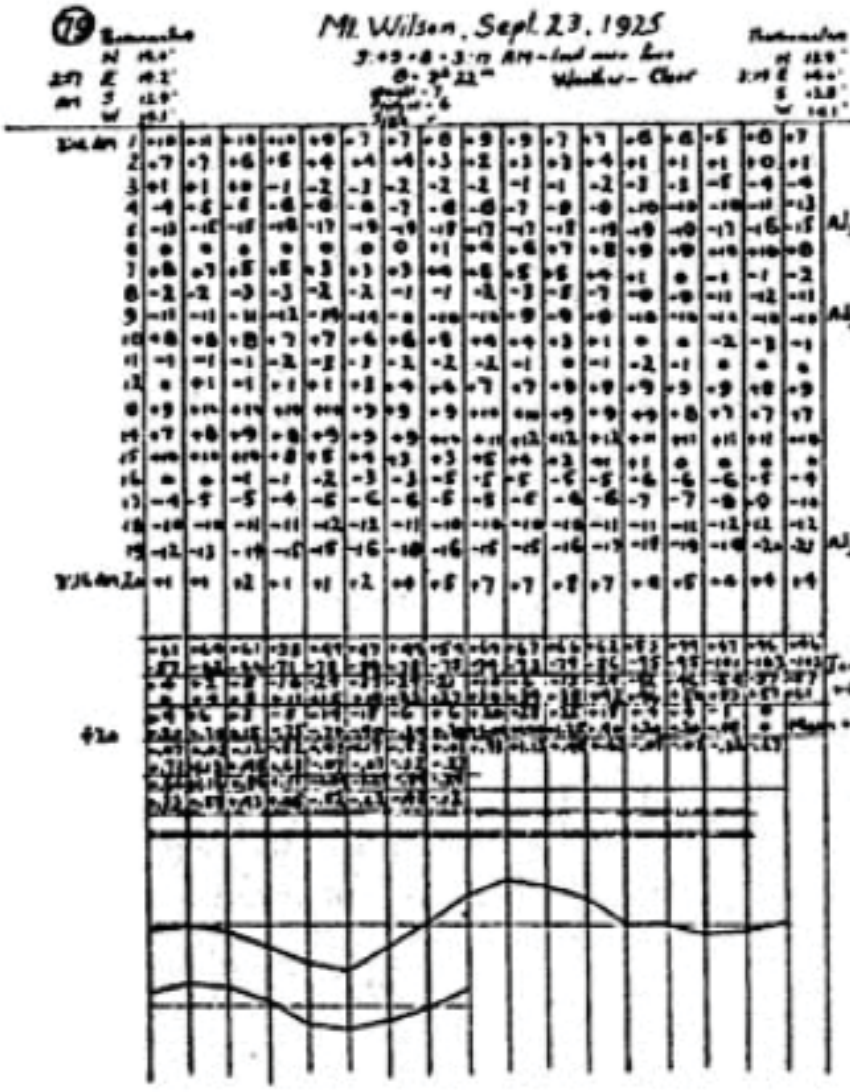

Figure 5. A typical datasheet recording 20 turns of the interferometer, in this case, on 23 September 1925, 3:09 to 3:17 a.m. at Mount Wilson. More than 300 of these datasheets were recorded by Miller at Mt. Wilson alone, covering more than 6,000 turns of the interferometer.

Unfortunately, and in spite of all claims to the contrary, Michelson–Morley never undertook those additional experiments at the different seasonal configurations, to "avoid all uncertainty." However, Miller did. Over many years, he developed increasingly sensitive apparatuses, using them at higher altitudes and in open structures, making clear and positive detection of the ether. His experiments yielded systematic periodic effects which pointed to a similar identifiable axis of cosmic ether-drift, though of a variable magnitude, depending upon the season, time of day, density of materials shielding or surrounding the apparatus, and altitude at which the experiment was undertaken. He argued that basement locations, or interferometers shielded with opaque wood or metal housings, yielded the most tiny and insignificant effects, while those undertaken at higher altitudes and in less dense structures yielded more readily observable effects. The Michelson–Morley experiment, by comparison, was undertaken in the basement of a stone building closer to sea level. Even so, it produced a slight positive result which was in agreement with Miller's results.

Miller's observations were also consistent through the long period of his measurements. He noted, when his data were plotted on sidereal time, they produced

> *. . . a very striking consistency of their principal characteristics . . . for azimuth and magnitude . . . as though they were related to a common cause. . . . The observed effect is dependent upon sidereal time and is independent of diurnal and seasonal changes of temperature and other terrestrial causes, and . . . is a cosmical phenomenon.* (Miller 1933:231)

Debates with Einstein

There are several newspaper accounts indicating a certain tension between Albert Einstein and Dayton Miller, since the early 1920s at least. In June of 1921, Einstein wrote to the physicist Robert Millikan:

> *I believe that I have really found the relationship between gravitation and electricity, assuming that the Miller experiments are based on a fundamental error. Otherwise, the whole relativity theory collapses like a house of cards.* (Clark 1971:328)

Privately, in letters and in spoken words, there was a struggle going on for philosophical dominance, and occasionally this struggle surfaced into public view:

GOES TO DISPROVE EINSTEIN THEORY
Case [School] Scientist Will Conduct Further Studies in Ether Drift
Einstein Discounts Experiments
Speaking before scientists at the University of Berlin, Einstein said the ether drift experiments at Cleveland showed zero results, while on Mount Wilson they showed positive results. Therefore, altitude influences results. In addition, temperature differences have provided a source of error. "The trouble with Prof. Einstein is that he knows nothing about my results," Dr. Miller said. "He has been saying for thirty years that the interferometer experiments in Cleveland showed negative results. We never said they gave negative results, and they did not in fact give negative results. He ought to give me credit for knowing that temperature differences would affect the results. He wrote to me in November suggesting this. I am not so simple as to make no allowance for temperature." (*Cleveland Plain Dealer*, 27 January 1926)

The above newspaper account is significant, as it demonstrates that Einstein was pushing the "thermal artifact" argument against Miller's results as early as 1926. There are other accounts of Einstein's discontent with Miller's results in "Conversations with Albert Einstein" written by Robert Shankland in the years after Miller's death (Shankland 1963, 1973b).

Miller's Control Experiments

Miller was fully aware of the criticisms being made against his findings, that his interferometer was responding to one or another mechanical, magnetic, or thermal influence. Given its large size and sensitivity, it required a careful setup procedure prior to each use. Setting screws with extremely fine threads were used to adjust the mirrors, and the final adjustment could isolate 100 wavelengths of light by just a 16° turn of the screw. Even this was insufficient for the final adjustment, which was made by adding small weights of around 100 grams to the ends of the cross-beam, which was sufficient to cause a micro-flexing of the iron supports by only a few wavelengths. Only then would the interference fringes come into view. And once in view, additional care had to be taken to prevent distortions from mechanical vibrations. Consequently, from the very beginning of the ether-drift experiments, Miller undertook extensive control experiments and procedures to guard against laboratory artifacts, and to objectively determine just how sensitive his apparatus was to external influences.

Especially between 1922 and 1924, Miller's control experiments were most rigorous, aimed at addressing the criticisms he had received following the earlier work, to make the apparatus as sensitive as possible only to ether-drift. A special interferometer of aluminum and brass was constructed, to

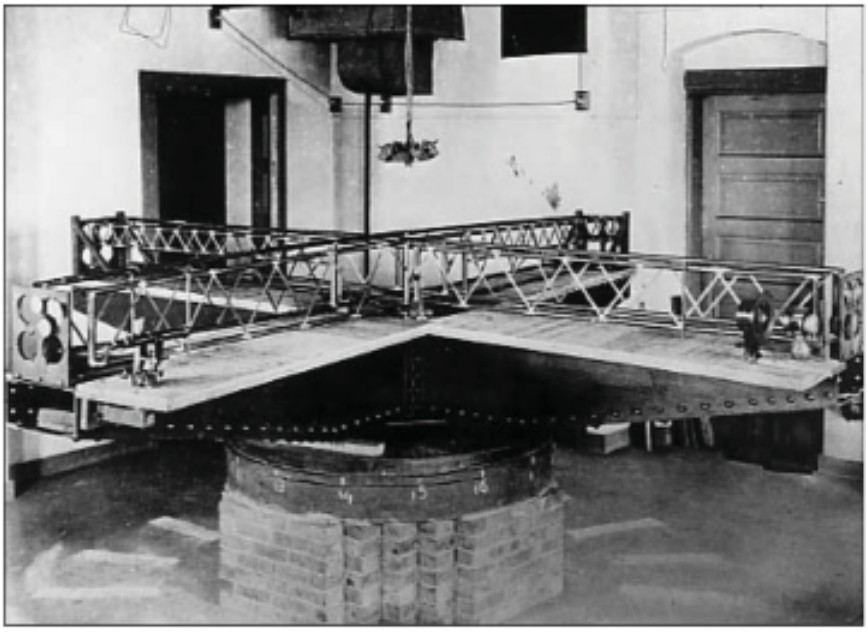

Figure 6. Miller's Control Experiments. A wooden platform has been supplied for the mirrors and optics of the interferometer, inside a building at the Case School.

guard against the possible effects of magnetoconstriction (the measured periodic ether-drifting was the same as with the original iron interferometer). Procedures were made to judge the effects of mechanical vibration—such as using a loose or tight centering pin. Bases made of wood, metal, or concrete were floated in the mercury tank, to judge and correct for the effects of strain and deformation (Figure 6, Figure 7). The apparatus was not touched when operating, but rather gently pulled in a circle by a thin string, slowly accelerated to the desired velocity of rotation while floating in the mercury tank. Different light sources were tried, mounted at different locations on the apparatus. Light sources outside the structure were also tried, including sunlight, but finally an artificial light source located above the central axis of the instrument was used.

Possible temperature effects were evaluated by using radiant parabolic heaters to artificially heat the apparatus and the air through which the light-beam passed. These experiments showed that the interferometer clearly was sensitive to artificial heating, and so steps were taken to eliminate the effect. Strong radiant heat sources, it was learned, would badly skew the apparatus if focused upon only one arm or pair of arms of the iron cross-

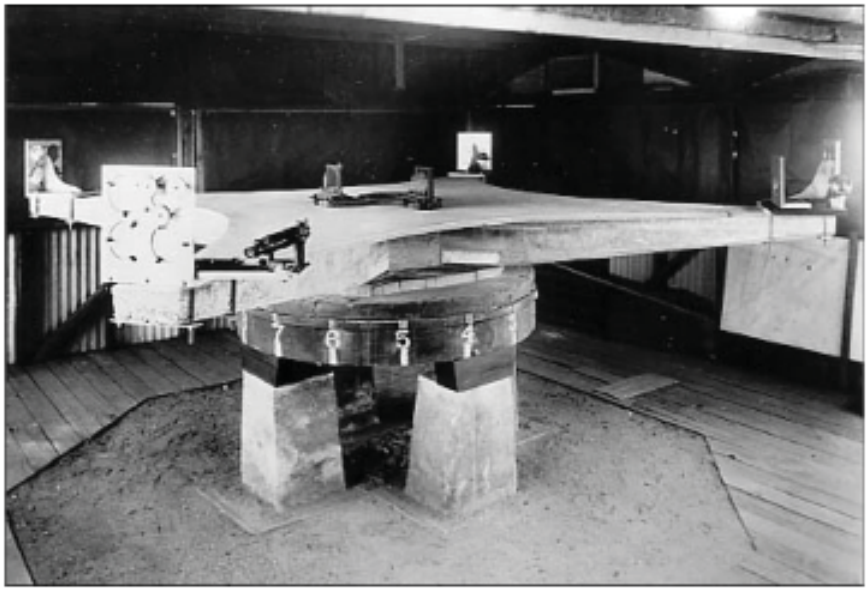

Figure 7. Miller's Control Experiments. A concrete platform supports the mirrors and optics of the interferometer inside a small shelter on the grounds at the Case School.

beams. Equal heating of the apparatus had no such effect, but the metal arms were nevertheless covered with a one-inch cork insulation to guard against radiant thermal effects (Figure 8). The lightpath was given a glass housing, which stabilized the temperature inside, and later a light corrugated paper cover was added over the glass cover, which did not affect the ether-drift but further protected against possible temperature variations. Low-level thermal effects were also evaluated, as from human body heat, by having the recording assistant stand in different locations while the apparatus was turned and operated.

Temperature effects from the larger environment were evaluated as well. Early ether-drift experiments, including those of Michelson–Morley and Morley–Miller, were undertaken inside basement locations with relatively stabilized temperatures, but shielded from the ether-drift as well due to heavy and dense building materials. Miller's ether-drift experiments atop Mount Wilson required a different approach, and a special house was constructed to shelter the interferometer. It had a floor, walls, and roof, and canvas-covered windows all around at the level of the interferometer light-beam. During his last set of Mount Wilson experiments in 1925–1926, a tent-like covering was erected over the roof and walls to provide additional

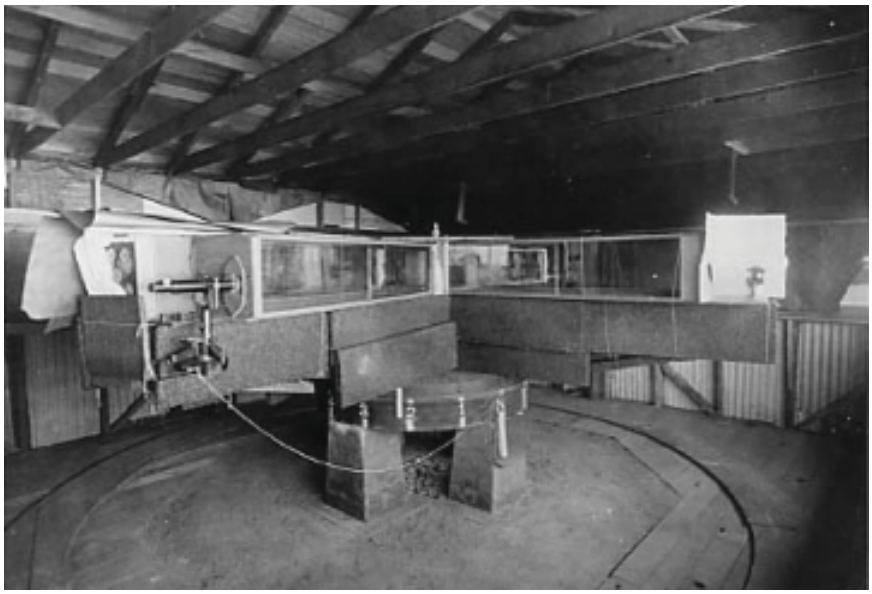

Figure 8. Miller's fully-insulated interferometer as it was finally employed at Mt. Wilson, c. 1925, fitted with 1″ insulating cork panels covering the metal support structure, and glass and light paper coverings along the light-beam path (paper removed for the photograph). These steps eliminated any significant influences of ambient temperature differences upon the apparatus and the air within the light-beam path, but still allowed the movement of ether-drift.

shielding from direct sunlight, to diminish thermal variations or radiant heating effects from the walls (Figure 9).

Miller noted that at no time during his entire work on the question did he ever observe any periodic effects expressing themselves according to civil time coordinates, as would be present if a thermal effect was radiating from a specific wall, related to solar heating. Since the measurements were made at different times of day, and at different seasons, their amplitude would vary, but the direction of the ether-drift would shift only to the same average points along a sidereal azimuth. This is graphically demonstrated in Figures 10, 11, and 12. The measurements were latitude-dependent as well, and when analyzed with attention to the Earth's rotation, axial tilt, movement around the Sun, and the Sun's movement through galactic space, finally revealed a common sidereal cosmological axis of ether-drift (Figure 13).

From reading his publications, one gets the impression of Dayton

Figure 9. Miller's interferometer house on Mt. Wilson. With canvas-covered windows all around, insulating "beaver-board" walls (wood fiber composite), and fitted with a tent cover to further stabilize temperatures.

Miller as a very careful and exceptionally patient experimentalist, someone who took every possible precaution to ensure his apparatus was detecting only the phenomenon of interest. He also appeared to be quite content with the possibility that, having undertaken all the various controls to shield the apparatus from thermal effects in the measurement room, he might finally get a true "null" or "zero" effect—he did not appear to be a "believer" in ether-drift who would succumb easily to bias. He was a genuine scientist, dedicated to finding the truth of the matter. A null result was not observed, however, and his efforts to control out mechanical and thermal artifacts never eliminated the observed periodic sidereal variations, which persisted throughout his experimental work. More will be said about Miller's control procedures below.

Michelson, and Others, Confirm an Ether-Drift

Miller's work did finally receive indirect support from Albert Michelson in 1929, with the publication of "Repetition of the Michelson–Morley Experiment" (Michelson, Pease, & Pearson 1929). The paper reported on three attempts to produce ether-drift fringe shifts, using light-beam interferometry similar to that originally employed in the Michelson–Morley

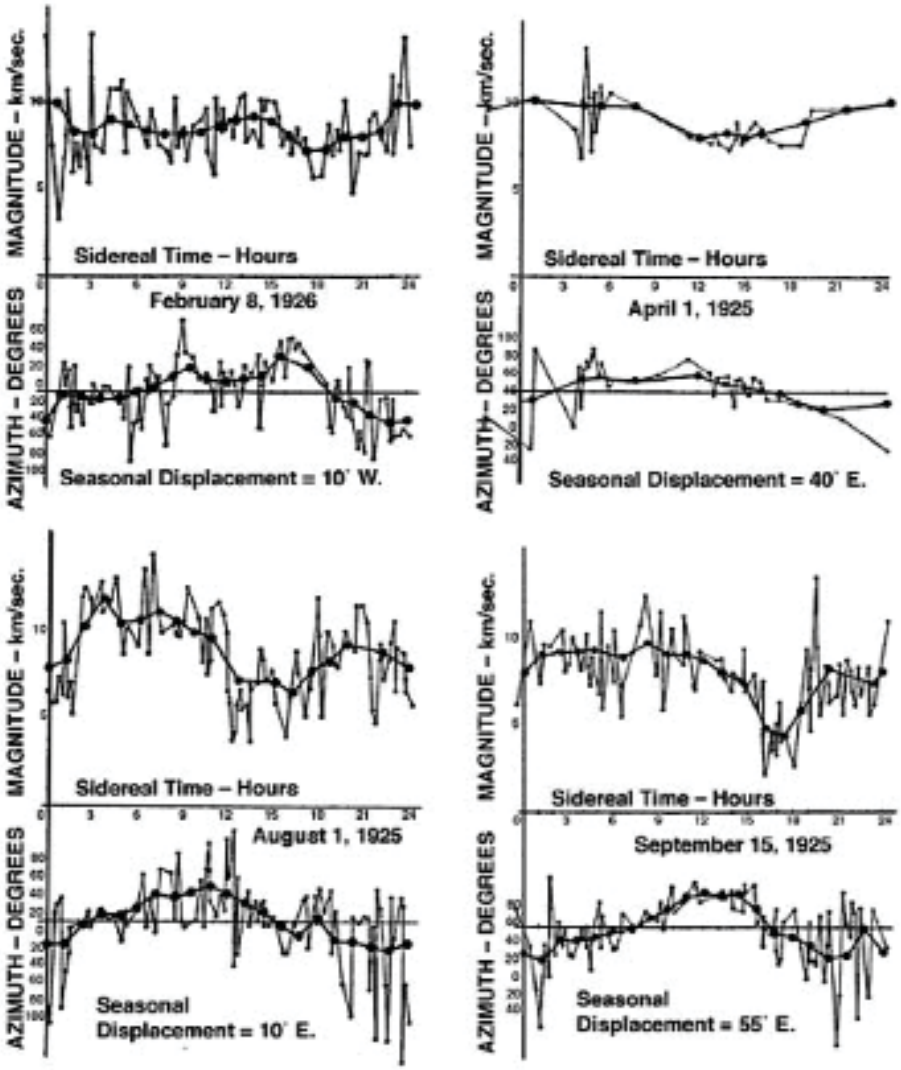

Figure 10. VELOCITIES AND AZIMUTHS OF ETHER-DRIFT, from the four 10-day epochs of measurement at Mt. Wilson, 1925–1926. The caption is redrafted from the original in Miller (1933:229).

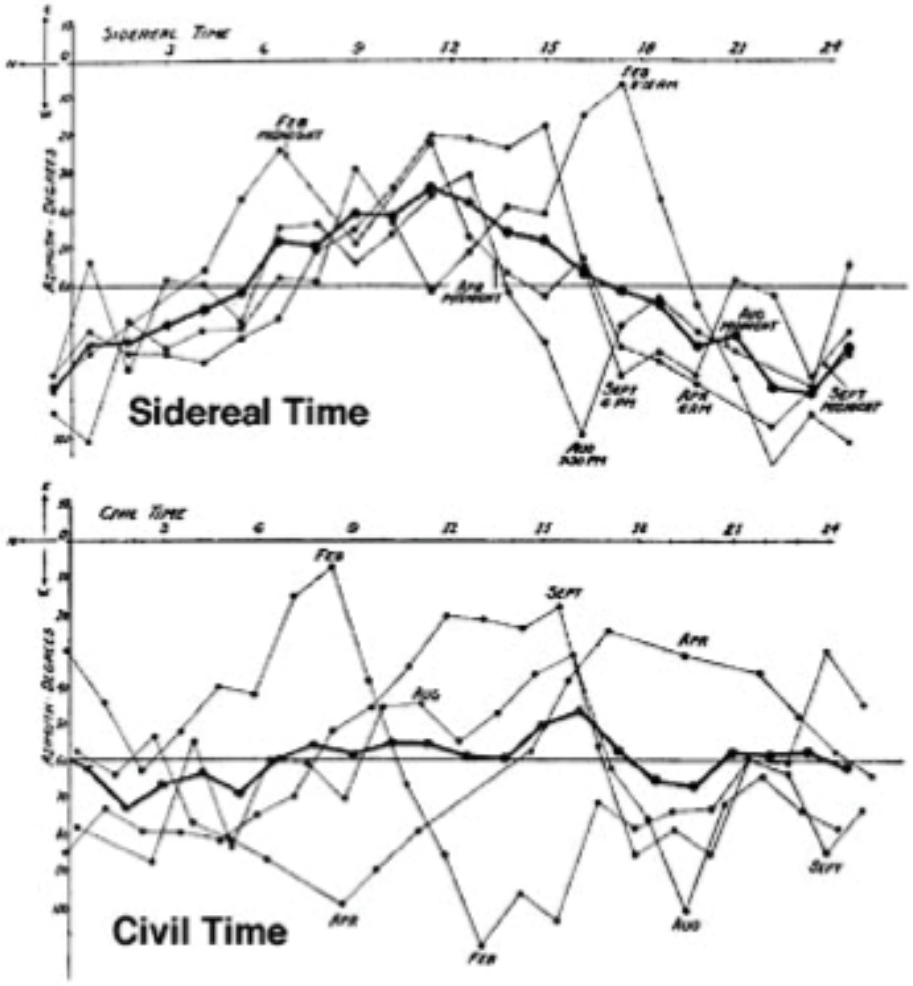

Figure 11. PERIODICITY OF GLOBAL ETHER-DRIFT, from Dayton Miller's Mount Wilson Ether-Drift Experiments, 1925–1926. The *Top Graph* above plots data from four separate months or epochs, measured at different times of the year and organized by sidereal time, showing a definite periodic curve. The heavy line is the mean of all four epochs. The *Bottom Graph* above plots the same data organized by civil clock time coordinates; here the plotted data spreads out along the graph, without apparent periodicity. This demonstrates that the detected axis and periodicity of ether-drift is the same for different times of year, but can only be seen when the data is viewed within a cosmological, sidereal coordinate system (from Miller 1928:362). These data curves are organized along azimuthal means that were later recomputed for Miller's 1933 publication, as given in Figure 10.

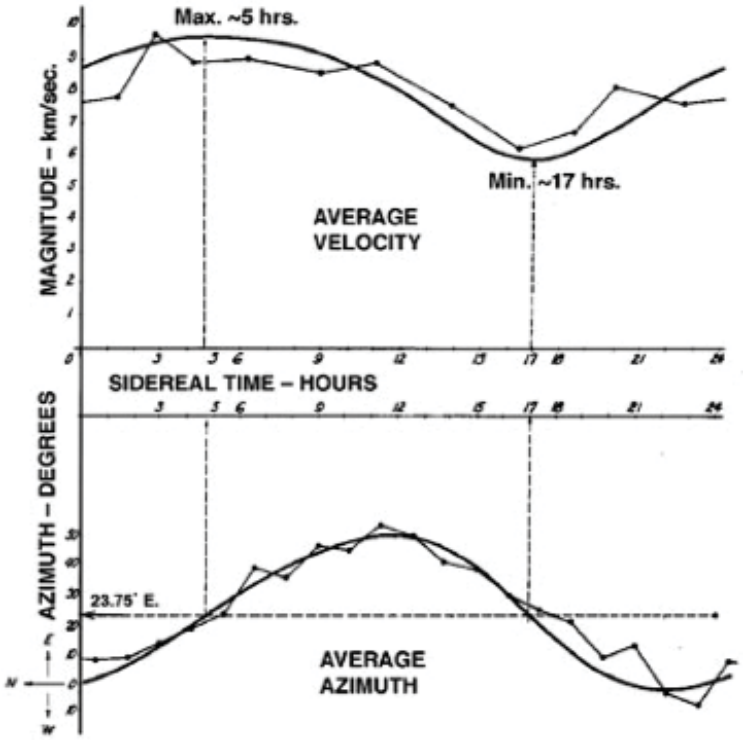

Figure 12. AVERAGE VELOCITY AND AZIMUTH OF GLOBAL ETHER-DRIFT from Dayton Miller's Mount Wilson ether-drift Experiments, 1925–1926. *Top Graph:* Average variations in observed magnitude of ether-drift from all four epochs of measurement. Maximum velocity occurs at about 5 hours sidereal time and minimum velocity occurs at about 17 hours sidereal. While Miller's 1933 paper assumed the Earth was *pushing through the ether* and moving toward Dorado, near the southern pole of the Plane of the Ecliptic, the movement and direction of ether-drift past the interferometer was *exactly opposite* to this, toward Draco near the northern pole of the Plane of the Ecliptic (17 hours right ascension, declination of +68°). It is important, *from the standpoint of his working theory,* to clarify the concepts of the "net motion of the Earth" versus the "direction of ether-drift." However, *if the ether itself is in motion, acting as a cosmic prime-mover, the direction of ether-drift and the net motion of the Earth would be identical, though at different velocities. Bottom Graph:* Average variations in observed azimuth readings according to sidereal time. This graph uses the same average data curve from Figure 11 (top graph) published by Miller (1928:363) but at the time was given a different baseline average. The same graph is presented here, for the first time, using Miller's revised seasonal averages as published in 1933 (p. 235), which help define the axis of ether-drift. Amazingly, the independent averages for the four epochs provided by Miller (Feb. = −10° west of north, April = +40° east, Aug. = +10° east, Sept. = +55° east) together yield *a mean displacement 23.75° east of north* (Figure 13). This is close to the Earth's axial tilt of 23.5°, and can hardly be coincidental. More discussion is in DeMeo (2002). Adapted from Miller (1928:363, 1933:235).

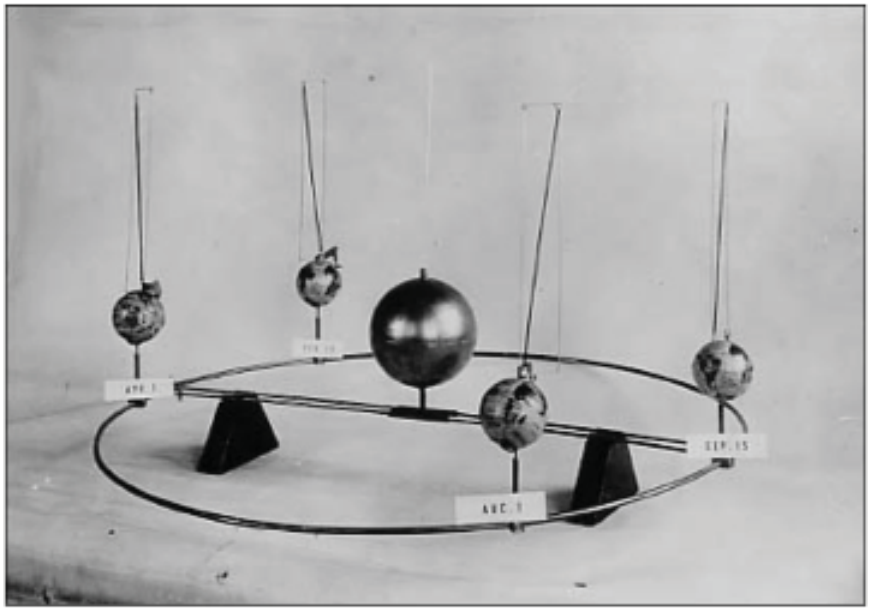

Figure 13. A model constructed by Miller, displaying the axis of ether-drift for the four seasonal epochs of the Earth moving around the Sun. The axis of drift, in this model, appears to be roughly perpendicular to the plane of the ecliptic.

(M–M) experiments. In the first experiment, undertaken in June of 1926, the interferometer was the same dimensions as the original M–M apparatus, with a roundtrip light path of around 22 meters. A fringe shift displacement of 0.017 was predicted, but the conclusions stated *"No displacement of this order was observed."* The second experiment, undertaken on unspecified "autumn" dates in 1927, employed a slightly longer roundtrip light path of around 32 meters (given as 53′ for an assumed one-way distance). Again, *"no displacement of the order anticipated was obtained,"* and the short report did not give details about the experimental surroundings or locations.

The third experiment was undertaken on an unspecified date (probably 1928) in *"a well-sheltered basement room of the Mount Wilson Laboratory."* The roundtrip light path was further increased to approximately 52 meters (given as 85′ for an assumed one-way distance). This time, having moved the apparatus to a higher altitude and using a longer light-path, a small quantity of ether-drift was detected which approximated the result observed by Miller, although *the results were unjustifiably reported in negative terms:*

> . . . precautions taken to eliminate effects of temperature and flexure disturbances were effective. The results gave no displacement as great as one-fifteenth of that to be expected on the supposition of an effect due to a motion of the solar system of three hundred kilometers per second. These results are differences between the displacements observed at maximum and minimum at sidereal times, the directions corresponding to . . . calculations of the supposed velocity of the solar system. A supplementary series of observations made in directions half-way between gave similar results. (Michelson, Pease, & Pearson 1929)

One fifteenth of 300 km/sec. is 20 km/sec., a result the authors dismissed as they apparently had discarded the concept of an Earth-entrained ether, which would move more slowly closer to sea level. A similar result of 24 km/sec was achieved by the team of Kennedy–Thorndike in 1932, however they also dismissed the concept of an entrained ether and, consequently, their own measured result:

> *In view of relative velocities amounting to thousands of kilometers per second known to exist among the nebulae, this can scarcely be regarded as other than a clear null result.* (Kennedy & Thorndike 1932)

This incredible statement serves to illustrate how deeply ingrained was the concept of a static ether.

Michelson, Pease, and Pearson went on to make speed-of-light measurements in a one-mile-long partially evacuated steel tube lying flat on the ground, oriented roughly southwest to northeast. While the purpose of these experiments was not to measure any ether-drift or variation in the speed of light, such variations in fact were observed and reported in their paper (Michelson, Pease, & Pearson 1935). A newspaper account of these experiments, published after Michelson's death in 1931 but prior to their final publication of results reported:

> *Dr. Pease and Mr. Pearson say the entire series of measures, made mostly between the hours of 7 and 9 p.m., show fluctuations which suggest a [variation] of about 20 kilometers per second.* (Dietz 1933)

Miller commented on these results, suggesting they would have measured a stronger ether-drift variation if they had taken their interferometers outside of the basement structures and steel pipes (Figure 14):

> If the question of an entrained ether is involved in the investigation, it would seem that such massive and opaque shielding is not justifiable. The experiment is designed to detect a very minute effect on the velocity of light, to

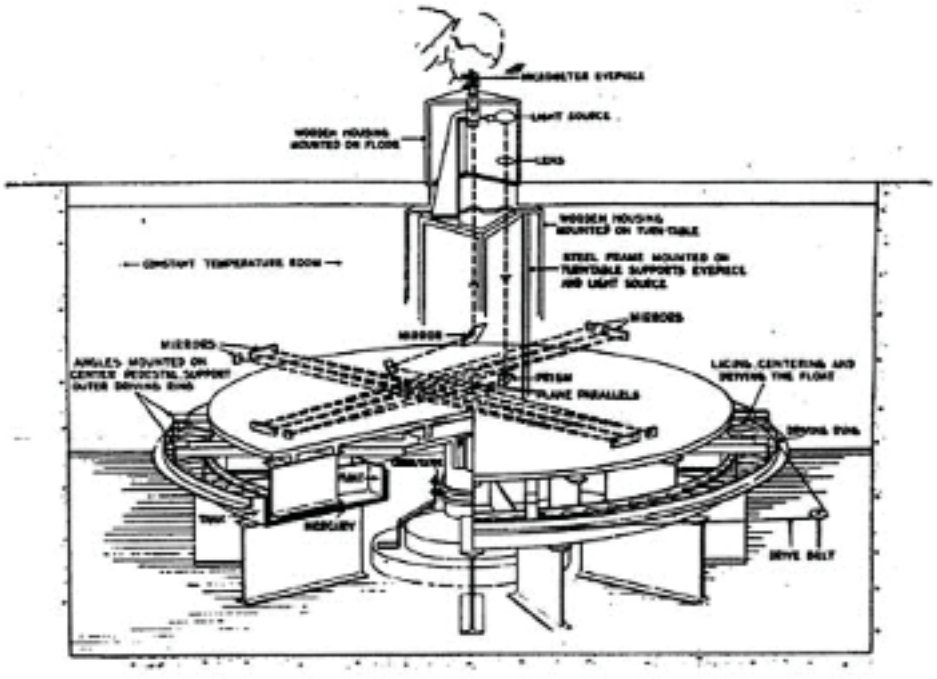

Figure 14. Apparatus used by Michelson–Pease–Pearson in their successful detection of an ether-drift of some unspecified quantity just under 20 km/sec. at Mt. Wilson, as reported in their 1929 paper. This positive result was inappropriately dismissed as a "negative" result because the experimenters had prematurely discarded the conceptual implications of an *Earth-entrained ether*. This experiment used the largest light-beam interferometer ever constructed by Michelson, with a 52-meter round-trip light path, coming close to the sensitivity found in Miller's 64-meter interferometer. It is shown here, situated in a basement location, in the ground, which, by itself, would also predictably reduce the measured result.

> be impressed upon the light through the ether itself, and it would seem to be essential that there should be the least possible obstruction between the free ether and the light path in the interferometer. (Miller 1933:240)

Miller had, by this time, acquired a lot of experience working on Mount Wilson, using his large interferometer in the specially constructed interferometer house. With a light path of 64 meters, Miller's apparatus was still significantly more sensitive than the best apparatus of Michelson–Pease–Pearson. Given that Michelson–Pease–Pearson did make some small

detection of an ether-drift from their efforts at Mount Wilson, in spite of the fact that it was located in a basement location, their report of detectable sidereal fringe displacements supports Miller's findings. It is also notable that this was the second time Michelson's work had significantly detected an ether, though in the first instance of Michelson–Gale (1925) the apparatus could only measure light-speed variations along the rotational axis of the Earth. These papers by Michelson and also by Kennedy–Thorndike have conveniently been forgotten by modern physics, or misinterpreted as being totally negative in result, even though all were undertaken with far more precision, with a more tangible positive result, than the celebrated Michelson–Morley experiment of 1887. Michelson went to his grave convinced that light speed was inconstant in different directions, and also convinced of the existence of the ether. The modern versions of science history have rarely discussed these facts.

Shankland Team's 1955 Critique of Miller

As previously pointed out by Swenson, Shankland's 1955 critique of Miller's work was undertaken with "extensive consultations" with Einstein, who like Newton and others before him had assumed only a static or stagnant ether, through which the Earth passed without material affect and, hence, without entrainment close to the Earth's surface. Shankland in fact was Miller's student for many years, and only emerged to become a professional advocate of Einstein's relativity after the death of Miller in 1941. Shankland became Chairman of the Physics Department at Case following Miller's retirement and death, building his professional career upon publications misrepresenting the Michelson–Morley experiments as the most solid evidence on the question, and publishing widely read interviews with Einstein (Shankland 1963, 1964, 1973a, 1973b). Shankland later took up administrative positions within government agencies developing nuclear energy—he rarely discussed Miller's positive ether-drift measurements in any of these papers except in the 1955 paper under discussion here. In this sense, it is legitimate to view Shankland and other members of his team (all Einstein advocates from Case) as biased reviewers of Miller's work.

The very first sentence in the Shankland team's 1955 paper began with the falsehood, now widely parroted in nearly every physics textbook, that the Michelson–Morley experiments had a "null" result. The third sentence in the Shankland paper was similarly false, claiming that *"All trials of this experiment except those carried out at Mount Wilson by Dayton C. Miller yielded a null result within the accuracy of the observations."* This kind of chronic misrepresentation of the slight positive results of many interfero-

meter experimenters, including Michelson–Morley, Morley–Miller, Sagnac, Michelson–Gale, and Michelson–Pease–Pearson, suggests an extreme bias and deliberate misrepresentation. The fact that this is a popular bias does not excuse it. By redefining all the positive results observed by what may in fact have been the majority of ether-drift researchers, as mere expressions of "observational inaccuracy," Shankland narrowed his task considerably.

These and other sentences in the Shankland paper revealed its bias from the get-go, and gave it the spirit of an autopsy, where Miller was dissected without careful concern, and certainly where no advocate of ether theory appeared to be involved in the process. It is possible that by the 1950s there was nobody left who could fill Miller's shoes to make an adequate defense. Ether-theory was then being compared to "the search for perpetual-motion machines" (Swenson 1972:239), and such ridicule surely must have had a silencing effect upon the entire fields of physics and astronomy. Swenson also suggests that, during his later years, Miller was largely ignored and isolated. This appears to be correct, as according to an interview with Shankland made in 1981, shortly before Miller died, he gave all of his interferometer datasheets—hundreds of pages of measurements—to his one-time student Shankland, with the somewhat bitter statement that he should "either analyze the data, or burn it" (Kimball 1981:2). In that same interview, Shankland also blamed Miller for having blocked the awarding of a Nobel Prize to Einstein for his relativity theory—clearly, Miller's work was a major obstacle to the Einstein theory of relativity, and for that reason may have given Einstein and his followers sleepless nights.

The title of the Shankland paper, and its overall representation suggests the authors had made a serious review of "*the* interferometer observations" of Miller, to include some kind of comprehensive and inclusive evaluation—but this was not the case. There were two basic approaches to the Shankland team's analysis: 1) a search for random errors or statistical fluctuations in Miller's data, and 2) a review of selected datasets which they claimed demonstrated significant thermal artifacts in the data. We can review these claims.

Shankland Team's Evaluation for Random-Statistical Variations

The Shankland paper did present a statistical analysis of a portion of Miller's published 1925–1926 Mount Wilson data, concluding that his observations *". . . cannot be attributed entirely to random effects, but that systematic effects are present to an appreciable degree*" and that "*the periodic effects observed by Miller cannot be accounted for entirely by random statistical fluctuations in the basic data"* (Shankland et al. 1955:170). Also, the Shankland team admitted they *". . . did not embark on a statistically*

sound recomputation of the cosmic solution, but rather [looked for] . . . local disturbances such as may be caused by mechanical effects or by nonuniform temperature distributions in the observational hut" (Shankland et al. 1955:172). In short, they admitted the harmonic patterns in Miller's data could not be due to any systematic measurement error, nor result from any mechanical flaws in the interferometer apparatus itself—while simultaneously admitting a disinterest in computation of any potentially validating ether-drift axis ("cosmic solution") from his data. These were important admissions, as the suggestion is that unless they could find some other fatal flaw in his data, Miller had really got it right, and measured a real Earth-entrained ether-drift.

Of interest from the perspective of the politics of science, is the fact that *this statistical analysis was not undertaken by any of the four members of the Shankland team listed as authors of the paper!* The analysis was in fact undertaken by Case physics student Robert L. Stearns, for his Master's Thesis (Stearns 1952)—Stearns was given only a footnote credit in the Shankland paper.

Stearns, who performed the analysis, informs us about the large amount of data gathered by Miller. He mentions (Stearns 1952:15–17) the existence of "316 sets of data . . . by Miller in 1925–1926" for the centrally important Mount Wilson experiments. Each dataset was composed of 20 turns of the interferometer, with sixteen data points per turn (a total of 320 data points per dataset). Miller noted his work at Mount Wilson was undertaken at four different seasonal "epochs," each of which encompassed a period of around ten days, centered on the following dates: April 1st, August 1st, and September 15th, 1925, and February 8th, 1926 (Miller 1926, 1933). It must be kept in mind, that these Mount Wilson data from 1925 and 1926 provided the most conclusive and foundational observations for Miller's ether-drift calculations and conclusions, as presented most clearly in his 1933 paper. As detailed below, the Shankland team mentions these Mount Wilson data, but in a manner which confuses them with his earlier and less significant efforts, including various control experiments conducted at the Case School. The significance of this confusion of dates will be highlighted momentarily.

Shankland Team's Assertion of Temperature Artifacts

Regarding possible temperature artifacts in Miller's data, this objection was raised early on in the history of ether-drift interferometry, and specifically rebutted by Miller when he was still alive. A letter exchange between Miller and Georg Joos from a 1934 issue of *Physical Review* records part of this debate, and appears to be one of the few *published* criticisms on

the temperature issue Miller ever received while still alive. Miller had this to say about the problem: *"When Morley and Miller designed their interferometer in 1904 they were fully cognizant of this . . . and it has never since been neglected. Elaborate tests have been made under natural conditions and especially with artificial heating, for the development of methods which would be free from this [thermal] effect"* (Joos & Miller 1934). The Shankland critique never made any systematic evaluation of possible thermal artifacts using a larger set of Miller's data, as was done with the statistical evaluation. Instead, they appear to have "gone fishing" in Miller's data for something by which they could simply dismiss him. For example, Miller's own 1923 temperature-control experiments were brought into discussion, where radiant parabolic heaters were used to artificially create a general doubling of the size of interference fringes. Miller describes these experiments:

> Several electric heaters were used, of the type having a heated coil near the focus of a concave reflector. Inequalities in the temperature of the room caused a slow but steady drifting of the fringe system to one side, but caused no periodic displacements. Even when two of the heaters, placed at a distance of three feet from the interferometer as it rotated, were adjusted to throw the heat directly on the uncovered steel frame, there was no periodic effect that was measurable. When the heaters were directed to the air in the light-path which had a covering of glass, a periodic effect could be obtained only when the glass was partly covered with opaque material in a very nonsymmetrical manner, as when one arm of the interferometer was completely protected by a covering of corrugated paperboard while the other arms were unprotected. These experiments proved that under the conditions of actual observation, the periodic displacements could not possibly be produced by temperature effects. (Miller 1933:220)

Perhaps without intending to do so, after examining Miller's laboratory notes for the Cleveland temperature control experiments, the Shankland team confirmed Miller on this point:

> In the experiments where the air in the optical paths was directly exposed to heat, large second harmonics (0.35 fringe for one heater, and about twice this value for two heaters) were always observed in the fringe displacements, and with the expected phase. Shifting the heaters to a different azimuth produced a corresponding change in the phase of the second harmonics. *When the optical paths and mirror supports were thermally insulated, the second harmonics were greatly reduced to about 0.07 fringe*. (Shankland 1955:174; emphasis added)

This statement confirmed the wisdom of Miller's approach. The added insulation reduced the thermal effects from a nearby radiant heater to only

20% of the un-insulated readings. I have an ordinary commercially available electric radiant parabolic heater at my home, and it gets so hot you cannot stand closer than 12″ without burning yourself, or possibly catching your clothing on fire. If Miller had used a parabolic heater even half as strong as this, it would certainly have been a source of heat much stronger than anything present in his Mount Wilson experiments, particularly at night, during foggy or overcast conditions, and when the entire interferometer house was covered over with a tent, with the apparatus and light-beam path covered with cork, glass, and paper insulation. Consider a radiant heater at several hundred degrees C, creating a steep thermal gradient but only a 0.07 fringe shift in the insulated interferometer. How much *less* of an effect would be produced by a human body, or even from the inside of a solar-heated wall? Assuming an environmental thermal effect only one-tenth that seen with the parabolic heater (a wood composite wall radiating inside the structure at perhaps 50 °C?), fringe shifts of *only 0.007* would have been produced, *well below observational detection*. Miller's datasheets, for example, recorded observations *"in units of a tenth of a fringe width,"* though readings down to hundredths of a fringe were possible with care. Overall accuracy of the ether-drift measurements approached a hundredth of a fringe after mathematical averages of many readings were extracted. The Shankland paper nevertheless used these control experiments as a weapon against Miller, claiming without evidence that heater-type effects *might* have occurred in his Mount Wilson experiments, even where no such heater or remotely similar heat source was present. But why would the Shankland team shy away from undertaking a more systematic evaluation for temperature artifacts? They could have, for example, evaluated only Miller's daytime interferometer experiments, and looked for a thermal effect from the southerly wall of the structure during the various epochs—if they could have shown an effect present in daytime data which was not present at night, it would have devastated Miller's claim, and proved their case. However, this obvious analytic procedure was not performed, or, if it was done, not reported.

The Shankland paper also resurrected the temperature criticisms by Joos and Miller (1934), but without reference to Miller's rebuttal in the same published exchange. If the periodic effects observed by Miller were the product of temperature variations, as was claimed by Shankland and Joos, then why would that variation systematically point to the same set of azimuth coordinates along the celestial *sidereal clock*, but *not* to any single terrestrial coordinate linked to civil time (see Figure 11)? Miller repeatedly asked this question of his critics, who had no answer for it. The Shankland team likewise evaded the question.

It is clear Miller had been deeply engaged in the problem of temperature effects, and worked hard to know exactly how they might be produced, and how to eliminate them. The Shankland paper, however, seized upon Miller's open acknowledgment of fringe-shifts from air heating by powerful radiant heaters during control experiments, and a few other sentences written in his lab book, and tried to claim thermal anomalies were probably the source of whatever periodic effects were subsequently measured by Miller at Mount Wilson, when no radiant heaters were used, and when the empirically developed control procedures were put in place. Without some kind of independent experimental evidence to support such a claim of a thermal influence, their dismissal was illogical.

The Shankland paper also went through a series of arguments about the interferometer house, how the wall materials, roof angles, interferometer glass housing, etc., might result in a definable effect upon the air temperature in the light-beam path, concluding only that they could not rule out such an influence—that it *". . . is not in quantitative contradiction with the physical conditions of the experiment"* (Shankland et al. 1955:175). Given their ignoring the sidereal nature of the periodicities, this statement could hardly be taken seriously, and certainly did not constitute a rebuttal of Miller's data.

The Shankland paper finally attempted to correlate several selected daytime interferometer runs with temperature measurements made at the same time. They acknowledged difficulty in correlating low fringe-shift values with low temperature differentials, but found one set of high fringe-shift values correlated with slightly higher temperatures, even while noting another set where high values correlated with lower temperatures. Finally, they complain that *". . . no temperature data are available to reveal thermal conditions at the roof, which may be responsible for the large fringe displacements at the times of highest altitudes of the Sun"* (Shankland et al. 1955:176). If this sounds confusing, a reading of the full original text provided little clarification.

Failing to show anything damning from daytime datasets, when temperature gradients inside the interferometer house might be expected to be at a maximum, they turned their focus to nighttime datasets. Once again, only a few of Miller's datasheets were selected out to prove their case. Data from two nights (30 August 1927 and 23 September 1925) with stable air temperatures were reviewed—these nights showed very clear and systematic fringe variations (Figure 4 in Shankland et al. 1955:176), but because the azimuth of the fringes changed minimally over the approximate 5 hours of observation, the critics complained "it *would be extremely unlikely if the fringe shifts were due to any cosmic effect"* (Shankland et

al. 1955:177). Apparently, the Shankland team was so locked into the older "static ether" assumptions of the original Michelson–Morley experiment, they were unclear about what they should have seen in Miller's data. In 1927, at a *Conference on the Michelson–Morley Experiment* held at Mount Wilson Observatory, where Michelson, Lorentz, Miller, and others made presentations and engaged in open debate, Miller addressed this question: *"Observations were made for verifying these [static ether] predictions . . . but it did not point successively to all points of the compass, that is, it did not point in directions 90° apart at intervals of six hours. Instead of this, the direction merely oscillated back and forth through an angle of about 60° . . ."* (Miller 1928:356–357). The reason for this is that Miller's detected axis of ether-drift is oriented reasonably close (within 60°) to both the Earth's axis of rotation and the axis of the plane of the ecliptic.

Another important fact which nearly escapes detection in the Shankland paper is that the 30 August data were made in Cleveland, while the 23 September data were from Mount Wilson, and *neither were a part of the published Mount Wilson data Miller used for calculations of the ether-drift*—both dates are well outside of the 10-day epochal periods identified by Miller. Furthermore, not all of the interferometer datasheets for a given date—which presumably would have had similar weather and temperature conditions—were included by the Shankland team for critical review. They selected only those datasets that appeared to support their argument of a claimed thermal anomaly. For example, they selected *"ten sets of observations, Nos. 31 to 40 inclusive, made in the hut on the Case campus between midnight and 5:00 a.m. on August 30, 1927"* and *". . . runs 75 to 83 inclusive taken from 12:18 a.m. to 6:00 a.m. on September 23"* (pp. 176–177). Other than making the *claim* that these selected data gave them the *impression* of being the result of temperature errors, they had no other stated criterion for bringing them into discussion. This biased data selection, or rather *data exclusion,* procedure forces one to ask*: What about datasets No. 1 to 30, and runs 1 to 74?* Similar unexplained data selections or data exclusions occur throughout the Shankland paper, leaving one to wonder if the *unselected and excluded* data, which constituted the overwhelming majority of it, simply could not provide support for their criticisms. One can imagine the howl of protest which would have occurred if Miller had taken this approach, arbitrarily excluding data from his calculations which superficially suggested something other than a real ether-drift. A third dataset from 30 July 1925 was highlighted by the Shankland team as it contained one extremely large peak where Miller noted "Sun shines on interferometer." This data does appear to have been a part of Miller's published Mount Wilson analysis. However, the Shankland team extracted

only *"observations Nos. 21 to 28 inclusive, made between 1:43 a.m. and 6:04 a.m. on July 30, 1925."* Obviously, at around 6:00 *a.m.* the sun rose and caught Miller and his assistant off-guard. What about observations Nos. 1 to 27, or other early-morning data, where the sun *didn't* shine on the interferometer? These other data were not brought into discussion, except they did note that the runs prior to the sunshine incident demonstrated *". . . an extremely erratic behavior . . . we have no ready explanation for this apparent departure . . . "* Here, the Shankland team basically confesses their grab-bag of "ready" explanations was empty, and the idea that those data were expressing a real ether-drift was simply too "impossible" for them to consider. The fact that Miller included the note about the Sunlight on this datasheet speaks to his honesty.

The Shankland team also identified datasets Nos. 56–58 from 8 July 1924, which was part of Miller's control experiments made in a basement location at Case physics laboratory—the temperatures were very stable, and the fringe oscillations were quite small, and they argued these data were a proof for thermal effects on the apparatus. However, it was this very problem of basement and dense surrounding materials that led Miller on the path to use the apparatus in locations not subject to significant ether shielding or Earth entrainment. After 1921, Miller only used the Case School laboratory to undertake control experiments, and that is why those particular data were never published.

The Shankland paper concluded its temperature criticisms by discussing a few additional datasets: Nos. 113–118 from April 2nd, Nos. 88–93 from August 8th, 1925, and Nos. 84–91 from February 11th, 1926, (Shankland et al. 1955:177). Here, the amplitudes and phases were claimed to have been "nearly alike," but insufficient detail was given to allow a review of the critic's claims, and it did appear they were once again incorrectly misinterpreting Miller's data along the lines of static ether assumptions.

As in almost all the cases given above, *none of these data were analyzed systematically*, nor were they presented in such a manner that the author's criticisms could be factually reviewed. I got the impression that they simply scanned through a pile of Miller's datasheets, and with a wave of the hand, picking and pointing to only selected parts, dismissed it all as the product of thermal artifacts. Miller's detailed control experiments were basically ignored, as was the fact that, for all these experiments, the interferometer was enclosed in a small house covered over with a tent, while the apparatus was shielded with cork insulation, and the light-beam path covered with glass and paper panels—with a full rotation occurring in less than a minute, one is left to wonder how any observable thermal variations could develop within Miller's data, especially variations with a sidereal-cosmic component.

For the casual reader, who had not undertaken a careful review of Miller's original experiments, the Shankland paper might appear to make a reasoned argument. However, the Shankland paper basically obfuscated and concealed from the reader most of the central facts about what Miller actually did, and in any case was so unsystematic and biased in its approach, excluding from discussion perhaps 90% or more of Miller's extensive Mount Wilson data, as to render its conclusions meaningless.

As a final note, after undertaking my research into the archives of both Miller and Shankland at Case University, and urging them and the faculty of the CWRU Physics Department on the importance of the original Miller datasheets, they were finally located and placed into the CWRU Archive.

Conclusions

My review of this important but sad chapter in the history of science left me both astonished and frustrated. Miller's work on ether-drift was clearly undertaken with more precision, care, and diligence than any other researcher who took up the question, including Michelson, and yet his work has basically been written out of the history of science. When alive, Miller responded concisely to his critics, and demonstrated the ether-drift phenomenon with increasing precision over the years. He constantly pointed out to his critics the specific reasons why he was getting larger positive results, while others got only small results, or no results. Michelson and a few others of the period took Miller's work seriously, but Einstein and his followers appeared to view Miller only as a threat, something to be "explained away" as expeditiously as possible. Einstein in fact was catapulted into the public eye following the end of World War II. Nuclear physics was then viewed as heroic, and Einstein fast became a cultural icon whose work could not be criticized. Into this situation came the Shankland team, with the apparent mission to nail down the lid on Miller's coffin. In this effort, they nearly succeeded.

The Shankland conclusions against Miller were clearly negative, but the one systematic statistical analysis of his Mount Wilson data merely confirmed what Miller said all along, that there was a clear and systematic periodic effect in the interferometer data. The Shankland paper also confirmed Miller's contention that this periodic effect was *not the product of random errors or mechanical effects.* The Shankland team subsequently searched for temperature artifacts in Miller's data, but they failed to undertake any systematic analysis of his centrally important Mount Wilson data in this regard. Instead, they made biased selections of a few published datasets and unpublished datasets obtained from different periods in Miller's research, from different experimental locations, and

including from his control experiments at the Case School.

Miller's most conclusive 1925–1926 Mount Wilson experiments encompassed a total of 6,402 turns of the interferometer, recorded on more than 300 individual datasheets. That was the data the Shankland team should have focused on and evaluated systematically. Instead, only a few of Miller's datasheets from these most centrally important experiments were selected—certainly less than 10% of the data available to them was brought into discussion—and then only after being firstly dissected to extract only those data that could most easily be misconstrued as evidence for presumed temperature anomalies. For certain, some of the data held up for public critique came from Miller's control experiments at Case, or possibly from trial runs when technical "bugs" were being worked out in the apparatus and building. Miller is no longer alive to inform us about his data, but the Shankland team willy-nilly lumped together both published and unpublished data, without comment.

Even though they were content to pick and choose data as they wished, they could not come up with a coherent and solid critique by which Miller's work could be conclusively dismissed—some of the data they selected merely confirmed Miller, though the Shankland group seemed ignorant of the basic ether-drift astronomy by which such an interpretation could be made. When alive, Miller openly stated he had addressed and corrected for thermal effects upon the apparatus, and yet the periodic elements of his measurements persisted—the Shankland paper ignored Miller on this important point.

The Shankland group undertook no new experiments of their own, neither on the question of ether-drift, nor on the subject of thermal perturbations of light-beam interferometry—they made essentially an "armchair analysis" of Miller's data. Only *some* of Miller's original data was carefully selected to make a rather unbelievable claim that small natural ambient temperature gradients in Miller's Mount Wilson observation hut *might* produce fringe shifts in the insulated interferometer similar to what Miller himself previously observed in his control experiments using strong radiant heaters. The Shankland paper argued there *must have been* "thermal effects" in Miller's Mount Wilson measurements, but provides no direct evidence of this.

At no time did the Shankland group present evidence that temperature was a factor in creating the periodic cosmic-sidereal fringe shifts observed by Miller in his published data, even though this was their stated conclusion. In fact, they presented evidence from Miller's own lab notebooks which implied thermal gradients in the Mount Wilson interferometer house would have been *below the observational limits* of the insulated apparatus.

The larger issue of periodic or harmonic effects in the data, expressed in nearly identical cosmic sidereal coordinates at different seasons and at all hours of the day, was never addressed or evaluated by the Shankland group. Neither was any attempt made to show *exactly how* an external temperature phenomenon could affect the interferometer readings to yield such a systematic sidereal effect. This issue was almost totally avoided by the Shankland team.

A reading of Miller's 1933 paper shows the picayune and biased nature of the Shankland team procedure, as the systematic sidereal periodicities observed by Miller expressed themselves nearly uniformly across the board, though at differing magnitudes. From 1906 to 1926, Miller undertook more than 200,000 separate readings, more than 12,000 turns of the interferometer demonstrating harmonic periodicities constantly pointing to the same general axis of ether-drift in the cosmos—a factor which was completely independent of the time of day or season of year in which the experiments were undertaken. At best, the critics provided only an *ad hoc argument*, a claim or suggestion without substance, that some small part of Miller's data *might* contain an undefined temperature effect.

From all the above, it appears the Shankland group, with some degree of consultation with Einstein, decided that "Miller must be wrong" and then set about to see what they could cherry-pick in his archive to support their *a priori* conclusion—which is not a scientific method.

As I have discussed previously, Miller found the ether-drift effect to be stronger at higher altitudes and also to be small when the experiment was undertaken in heavy stone buildings or when the interferometer light-path was encased in wood or metal shielding. In my studies over the last 40+ years, I've found many examples from the fields of biology, meteorology, and physics that independently support the assertion of a subtle energetic force with similar altitude-dependent and metal-reflective properties—notably in the works of Wilhelm Reich, Giorgio Piccardi, and Frank Brown (DeMeo 1979, 1989a, 1989b, 2000, 2002, 2004). Likewise, there are many new findings in astrophysics, where anisotropy of cosmological factors have been discovered, which are congruent with Miller's identified axis of ether-drifting (Miller 1933:241, Allais 1997, 2002).

Notable in this respect are the experiments of Cahill (2006a, 2007) of the Chemistry, Physics, and Earth Science Department at Flinders University in Australia; DeWitte (in Cahill 2006b) working with the Belgian telecommunications company Belgacom in Brussels; Galaev (2000a, 2000b, 2001, 2002) at the Institute for Radiophysics and Electronics, National Academy of Sciences of Ukraine; Múnera (2002, 2009) of the Physics

Department, Universidad Nacional de Colombia at Bogotá; and Múnera, Deckers, Arenas, and Alfonso (2006) and Múnera, Deckers, Arenas, Alfonso, and Lopez (2009) from diverse institutions including the Physics Department at Bogotá and the Max Planck Research Center in Hamburg, Germany.

All of these newer studies have basically confirmed the Miller results, including its general axis of ether-drift and sidereal-day velocity components, *"down to the details"* (as expressed by Galaev).

To close, I ask the reader to imagine that Michelson–Morley's 1887 experiment, which ran over only 6 hours on four days, had resulted in a claim that "the ether has been detected," and that Dayton Miller had undertaken his years of work with 200,000 observations showing "the ether cannot be detected." It does not take much consideration to conclude that—in such a fictional case—Miller would today be cited in every physics textbook as having "proved the ether did not exist," and nobody would refer to Michelson–Morley. The fact that the present-day situation is totally opposite of this fictional example is a testament to the intensely political nature of modern science, and how major theories often develop into *belief-systems*, which demand the automatic suppression of any new finding which might undermine the faith and "popular wisdom" of politically dominant groups of academics. And that "wisdom" today is: ***Space is empty and immobile, and the universe is dead.*** I submit that these are unproven, and even *disproven* assertions, challenged in large measure by Dayton Miller's exceptional work on the ether-drift.

I should also add how modern astrophysics today accepts without hesitation many theoretical concepts which basically fill the vacuum of cosmic space, and appear either superficially or dramatically similar to the Miller type of tangible and measurable cosmic ether, in spite of their detection difficulties. Examples are the "dark matter," the "neutrino sea," the "intergalactic or interstellar medium," and "cosmic plasmas" (DeMeo 2011). The fields of parapsychology also could find a potent explanatory mechanism in such a cosmic medium, for transmission of sensory or inertial impulses via as-yet-unclear excitations or mechanisms more similar to old wave-theory than modern particle-based or consciousness-based "intentional" postulates.

By Ocam's Razor, if not also by the similarities of their properties, I postulate these are all one and the same thing, as per the well-known example of ten blind scientists in a room with an elephant, each one grabbing ahold of, and describing what only *appear* to be uniquely different parts of a unitary cosmic anatomy.

Acknowledgments

My thanks to the Case Western Reserve University (CWRU) Archivists, and to Bill Fickinger of the CWRU Physics Department, for their help during the research phase of my investigations, and also for help in locating and securing in archives Miller's 1000+ original datasheets, which had been lost. Most of these sheets were recorded in pencil, which unfortunately does not photocopy very well. Consequently, a visit to the CWRU Archive may be necessary for accuracy, in case others may wish to review them. This problem also makes it nearly impossible to re-review Miller's findings based upon those original datasheets, so I must defer to his own concluding statements in his published papers. In my view, rather than another re-analysis of Miller, this subject begs for a serious replication atop Mount Wilson, performed in both a critical and open-minded spirit which takes into account all the necessary parameters for the possibility of a matter-inhibited and Earth-entrained ether-drift. All photos courtesy of the CWRU Archive. The author has also developed a comprehensive webpage list with download links, of historical articles on the ether-drift experiments, including for most of the papers cited in this article at http://www.orgonelab.org/energyinspace.htm

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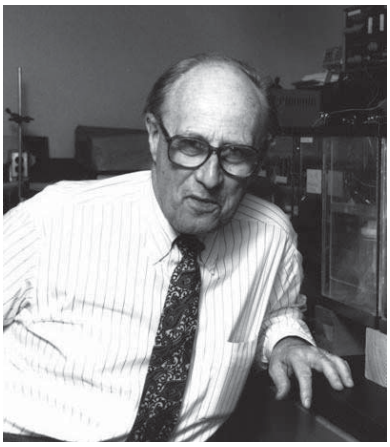
OBITUARY

Professor John O'M. Bockris, 1923–2013



Bernhardt Patrick John O'Mara Bockris arrived on January 5, 1923, in Johannesburg, South Africa, and left on July 7, 2013, in Florida after spending 90 years on this planet. In between these events, he explored this world with intensity, courage, and creativity. He sought education at Brighton Technology College and obtained his Ph.D. from the Imperial College of Science and Technology in England. Later, the University of London awarded him the additional distinction of a D.Sc. degree. With these tools in hand, he went on to become one of the world's experts in Electrochemistry and taught many students most of what they know about the subject through his lectures, many papers, and books. Much of this pioneering work was done while he taught at the Imperial College in London (1945–1953), at the University of Pennsylvania (1953–1972), and with a short stay at Flinders University in South Australia (1972–1978). As a result, he was awarded much recognition for his contribution to this growing, but conventional science, and is now considered the father of modern electrochemistry.

But then John crossed a forbidden boundary when in 1989, while teaching at the University of Texas in College Station, he started a study of what is called cold fusion—but first some background.

Martin Fleischmann, a student of Bockris when he taught at the Imperial College, who later became a famous expert in electrochemistry, and Stanley Pons, the Chairman of the Chemistry Department at the University of Utah, announced to the world in March of 1989 that they had initiated a fusion reaction, with the resulting heat and nuclear products, using only an electrolytic cell (Fleischmann, Pons, & Hawkins 1989, Fleischmann 2008). This discovery had and still has the potential to change life as we know it and to solve some of the pressing problems created by the excesses of modern life. Consequently, John and hundreds of scientists in laboratories

all over the world attempted to replicate the claim. Many were successful, but most were not. Nevertheless, because of his skill and information he obtained from Fleischmann, John was among the first to successfully make tritium using the electrochemical method (Kainthla et al. 1989, Packham et al. 1989, Lin et al. 1990). His success was shortly followed by many other successful efforts at major laboratories all over the world. As a result, the discovery has been shown to be a real effect (Storms 2007, 2010) that is on its way to commercial application. But in 1989, and even now but to a lesser degree, the claim was rejected by conventional science.

John's troubles started when Gary Taubes, an author of a book (Taubes 1993) about the discovery, accused John's graduate student of adding tritium to the cell and then pretending it had been made by the cold fusion reaction. This accusation was published in *Science* (Taubes 1990) without any proof, such was the hostility and certainty of the scientific establishment that the claim was false. The resulting investigation by the university could find no evidence to support the accusation (Anderson, Bockris, Worledge, & Taube 1990), but John's reputation was now in question and rivals at the university smelled blood. His troubles only deepened when he tried to convert mercury to gold using a method suggested by Joe Champion (1994). This led to his increased interest in transmutation as a result of the cold fusion effect (Bockris 2004), which now has a rich literature of support (Srinivsan, Miley, & Storms 2011). As a result, professors at the university attempted to strip John of his Distinguished Professor Award and force him out of the university (Bockris 2000). Fortunately, calmer heads prevailed, but the experience made John an outcast and brought into question the level of academic freedom possible at Texas A&M. In spite of these distractions and the resulting pain to him and his family, John continued to study cold fusion and was awarded the Preparata Medal in 2012 by his grateful friends and students in the growing field now called Low Energy Nuclear Reaction (LENR). John also contributed to the development of hydrogen (Bockris 2011) as the fuel of the future and started to take an interest in natural gas (Bockris 2012). As can be expected of such an intelligent and creative person, John spent time during his final years trying to understand the event of death and what happens next. Naturally, he took a scientific approach that avoided much of the distraction and myth contributed by religion. I'm sure John now knows whether his understanding is correct or not because he expected awareness to continue after death. Regardless of what happens next, his example of great courage and curiosity is rare and will be missed by everyone who knew him.

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BOOK REVIEW

Crimes of Reason: On Mind, Nature and the Paranormal by Stephen E. Braude. Rowman & Littlefield, 2014. 234 pp. $70.00 (hardcover). ISBN 978-1-4422-3575-5.

Here we have a collection of Stephen E. Braude's challenging philosophical forays into the criticism and evaluation of conceptual frameworks endemic to current trends in science of mind and reality (including the paranormal). Some of the essays in this volume are expanded and updated versions of previously published ones, and some have been written especially for this publication. It is good to welcome the new essays, and good also that the earlier ones should not be lost sight of, as all of them have ongoing value in the ferment surrounding questions of who and what we humans are, what is our relationship to the world, and how much of that relationship can be explained by a science committed to mechanism.

Because of the scope and variety of the included essays I will not attempt to discuss each in turn or even to discuss all of them. I will instead give a general sense of what Braude is up to, what is the overall structure of the book, and what a reader may expect to encounter. I will then take just one of the many possible directions a reviewer might take in considering such a multifaceted topic. That particular direction is of considerable interest to me personally and may also, I believe, be of interest to many readers. But there are lots of other directions that might be taken among the variety of topics and ideas to be found in this volume.

Each essay is a unique contribution in itself. Taken together they fall roughly into four not entirely unrelated categories. (1) The first two essays deal with the inadequacy of mechanistic explanations in the behavioral and life sciences. The first of these is "Memory Without a Trace," Braude's definitive rejection of the theory that memory consists of data stored in the brain. The second is Braude's review of Rupert Sheldrake's controversial *A New Science of Life*, where he uses a gentle touch (he considers Sheldrake a personal friend) but eventually must conclude that Sheldrake's theory is "seriously flawed." (2) The third essay, "In Defense of Folk Psychology: Inner Causes versus Action Spaces," takes a penetrating look at the inadequacy of psychological explanation when it tries to attribute human behavior to an "inner cause" (i.e. to brain states). I will be exploring this essay at some length in what follows. (3) The fourth and fifth essays,

which are "The Creativity of Dissociation" and "Multiple Personality and Moral Responsibility," add to the author's already extensive analyses in his book *First Person Plural* (Braude 1995) dealing with issues of multiple personality. (4) The sixth and seventh essays discuss the nature and limits of human abilities as seen from the viewpoint of parapsychological studies. The sixth, "Parapsychology and the Nature of Abilities," is a penetrating look at the failure of clarity in "the scholarly community" regarding "the nature of human (and other organic) abilities." The topic, of course, involves the nature and frequency of occurrence of psi abilities in the populace as a whole. This topic spills over into the seventh essay, "Some Thoughts on Parapsychology and Religion," which I found especially stimulating. I will devote some time below to this essay. There is also an eighth essay, "Credibility under Fire: Advice to the Academically Marginalized," which in my opinion would best be read first. It is a proper introduction to all those which precede it.

Braude's commentaries are at a high level of professional philosophical analysis. Some who may not be familiar with the most sophisticated equipment in the philosopher's toolbox may find the intricacy occasionally requires re-reading a passage. On the other hand, Braude writes with an eye toward clarity, gives many examples which increase understandability, and at times provides delightful humor based on his own life experiences. It would be a mistake to bypass Braude's meatiest essays—especially those in the first two categories—and to turn only to the pages harboring the more exotic topic of parapsychological explanation. The two areas of interest, mechanistic explanation in the sciences and methodological concerns in parapsychological research, are closely related. In both cases, the danger lies in focusing primarily on the materials and methods of laboratory experimentation at the expense of the way things are in the broad scope of lived human experience. Braude makes this point especially in the third essay, "In Defense of Folk Psychology," and in the seventh, "Some Thoughts on Parapsychology and Religion."

> . . . formal experiments in principle tell us virtually nothing about the nature of psychic functioning. . . . It's clear to me that at least some of the most interesting, compelling, and illuminating parapsychological data comes from venues outside the lab. (p. 183)

This perspective reflects a far more general issue having to do with the proper relation between science and human experience. It is a topic taken up almost universally, for example, in philosophical critique of mechanistic conclusions in cognitive science. Trying to compress the study of human life and behavior into the constricted atmosphere of any particular methodology

may distort understanding of the results to the point where the world *as it is experienced* is devalued—or worse, written off as unreal.

Nothing illustrates this point better, perhaps, than the analysis Braude undertakes of the "Inner Cause Theory" of human behavior (ICT). This theory, espoused by many in cognitive science, attributes the cause of behavior exclusively to the activity of the brain, and assumes that an "explanation" of behavior consists in locating the brain states causing the behavior. (Of course, under the computer interpretation of the brain, those brain states in turn would be caused by the supposed programming of the brain as it responds to some sensory input, so ultimately any explanation of behavior would have to rely on the capability of the programming.) Braude's approach to criticism of ICT is exemplary of his overall approach to the issues taken up in the book. I will focus first on how this approach develops in the third essay, and later on how this may relate to the seventh essay.

The Concept of "Action Space"

In the third essay Braude holds that folk psychology, peremptorily dismissed in cognitive science, frequently provides satisfactory and productive explanations of behavior, while ICT is incapable of doing so. In his discussion of folk–psychological explanation, Braude comes up with a somewhat curious term, *action space*, referring to a "space" accommodating aspects of behavior necessary for explanation which are inaccessible to ICT. I find this notion of "action space" to be of considerable interest. Residing at the core of his analysis, it deserves close attention.

"Action space" is the "space" wherein certain features of human activity necessary for explaining behavior take place. Braude would naturally seem to be using "space" here metaphorically. If we were to speak literally about the "space" within which the causal factors in any particular instance of behavior are located, for ICT it would of course be the *physical* space within the cranium occupied by certain neurons; or, since the behavior in question must be decided upon by the brain's programming in response to sensory input, the inner cause might be thought of as located in a *metaphorical* space, i.e. the "programming space" within which the computer-brain dwells—something like the metaphorical space Jeff Bridges runs around in when he is caught inside a computer in the movie *Tron*.[1]

That "programming space" may seem large, but actually it is miserably confined compared to what Braude calls action space. Action space is the lived milieu within which human behavior takes place. By focusing on this wider landscape Braude carries out his conviction that to understand behavior we must leave the confinement of the lab and look at the

experienced world in its full dimensionality. But now a question arises (at least in my inquisitive mind): Is the "space" in "action space" metaphorical, or literal? Is it a kind of space other than physical space?

This may seem a peculiar question to ask, and Braude does not take it up. In his discussion, "action space" clearly has the role of a useful metaphor. Ordinarily, when an agent performs some action, it is assumed this behavior takes place in the same physical space as that within which science locates objects and events. But the problem that arises on reflection is that certain things Braude lists as occurring in action space are not things that the space of physics is able to accommodate. There seems to be a distinction to be made between the two; and I believe considering this distinction has very interesting implications in relation to paranormal issues as they are taken up in Braude's other essays in this book.

So what goes on in action space, and what does this have to do with explaining behavior?

Braude outlines four requirements, which are those things relevant to explanation that are going on in action space on the occasion of some particular instance of behavior. (1) the *intentions* of the agent engaging in the behavior; (2) the larger social–environmental *context* within which the behavior occurs (including the intentions, memories, and character traits of any others involved); (3) the stage of personal *development* of the agent such as the agent's degree of maturity or immaturity (and that of any others involved); and (4) the background of experience that would allow for *anticipations* on the part of the agent as to what the consequences of various choices might be (and the anticipations of any others involved) (pp. 64–67).

Clearly then, the "action space" within which behavior takes place is a very complex affair. And most often it involves other individuals than just the immediate agent of the action. Braude shows how the representational theory of mind assumed by cognitive science as necessary for explaining how ICT can work cannot, even in theory, handle such a task (p. 77). Furthermore (although Braude does not explicitly make this point) on the mind–brain identity theory, which must rely on a mechanistic interpretation that eliminates purpose in causality, the factor of an agent's *intentions* would have to be eliminated from the explanatory framework entirely. Such things as intentions, and for that matter *agents* having intentions, and the determining character traits of such agents as they manifest at the present stage of that agent's development, belong to the "folk psychology" which cognitive science dismisses. But it is precisely there, Braude holds, where we must look for satisfactory explanations of behavior.

We see then a stark contrast between the space within which the cognitive scientist expects to find explanations of behavior (whether

physical space or computational space), and the space within which human life really takes place and which Braude insists must be taken into account in order for behavior to take place at all. Braude refers to this "action space" as one that can accommodate the *larger patterns of action* to be taken into account in any explanation (p. 59).

So now the dimensionality of "action space" begins to loom rather large. It looks less and less like a metaphorical space and more and more like a literal kind of space. Just as physical space accommodates energies and material particles and is defined geometrically, action space accommodates different sorts of things, such as intentions, agents, personality characteristics, and so on, that do not fit into the geometric, purposeless nature of physical space—and that is exactly why consciousness itself is generally problematic and often even denied existence in reductionist accounts. Braude's bringing action space into the discussion vividly highlights this critical point. To put the idea rather bluntly: *You cannot reduce action space to physical space*. And certainly the next question would be: What then *is* the relation between action space and physical space? And how might one answer such a question?

These, I believe, are very profound questions right on the cutting edge of the problem of the relation of science to humanity. And this brings me to the main point I would like to make. Taken not in isolation, but as a whole body of careful philosophical work, the essays in this book are literally overflowing with what I might call "idea potential." I could not read any of them very far without being stimulated to think more about this or that point in this or that essay, and also about the potential for new ideas stemming from the relations *between* the essays. So I must beg the pardon of the author as I use this Review to illustrate this point by engaging in a kind of thought experiment which comes to mind when considering the potentials inherent in Braude's concept of "action space" and its relation to his essays on paranormal phenomena—particularly to the seventh essay

Action Space and Situations

The "larger patterns of action" accommodated by action space I understand to be the *situations* wherein behavior occurs: Action space is the space that accommodates *situations*. Behavior does not occur in a vacuum. It is always found within a *situation*. In a folk–psychological explanation of behavior, understanding the situation is an absolute necessity. This is essentially the reason for Braude's conclusion that ICT cannot come up to the mark. It cannot handle situations.

A situation in this sense is not a static rigid entity, such as the placement of actors in various positions on a stage prior to raising the curtain. It only

becomes a *situation* when the actors move, speak, exhibit intentions and traits of character, and respond to one another as the course of their separate and concurrent actions and intentions unfolds in time. These actions also unfold from the past, reflecting what *was* going on, what brought them there in the first place and from whence they came. A situation is a *temporal existence*, within which each actor is moving—both physically and psychologically—in the fulfilment of individual motivations, interests, intentions, and reasons. And their anticipations of possible future results are also part of the situation. To describe a situation is to describe a temporal field of human experience.[2] Duplicating such situations in a laboratory setting, or in the form of some sort of preprogrammed logical structure inside the brain is patently impossible, as Braude points out, not only because of the variables involved but also because such things as agents and the intentions of agents do not (and cannot) compute. Braude effectively argues that no programming of a computer-brain, however sophisticated, can deal with such a situational space, and he has chosen "action space" to describe the wider and deeper milieu necessary for adequate explanation.[3]

To this point of Braude's I would add: A machine is never in a situation.

Esoteric and Paranormal Considerations

Now then, we are ready to take a look at what happens if we consider action space from a *non-metaphorical* standpoint. We suppose, for the sake of argument, that action space has an ontological existence; that it is a dimension of reality in addition to the scientifically recognized three dimensions of physical space and one of time.[4] Then Braude's concept of action space will reveal a possible relation to the discussion of paranormal phenomena found in the seventh essay. To suggest that there is yet another dimension (and possibly more than one other) which is not a geometric dimension and which accommodates fundamental properties of human behavior would be to shake the foundations of the scientific view of the universe, wherein such a thing as a *situational* space having the ontological status of a dimension must be nonexistent and must remain entirely metaphorical—if for no other reason than that it is by definition capable of containing agents having intentions.

Let us be daring enough, then, to extrapolate from Braude's idea and suggest that action space might be an additional but unrecognized (non-geometric) dimension with true ontological status.[5] This must bring us to consideration of the relation between action space and the "spaces" called in esoteric and clairvoyant literature "planes of being" such as the so-called astral plane.[6] I don't know whether this extrapolation would meet with the author's approval, but after all it is his idea that brings it to mind. And if a

connection between action space and the astral plane were to be speculatively entertained, it seems to provide a link between Braude's analysis of what can *go on* in action space (i.e. in *situations*), the testimony of psychics and clairvoyants, and the paranormal phenomena discussed in his seventh essay.

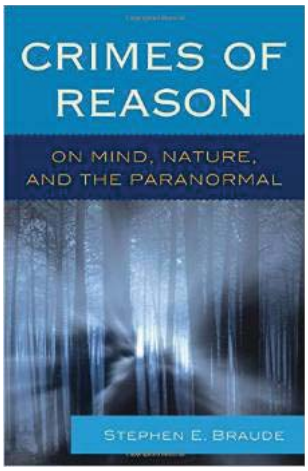
To take one example, consider this passage from that essay. The seventh essay is not confined to intellectual analysis and conceptual criticism; it is creative speculation. In it Braude considers the possibility that occurrences of PK (psychokinesis) and ESP (extrasensory perception), as well as of other psi phenomena, may be more prevalent and active in ordinary social situations outside the laboratory context than is ordinarily thought, and he explores the implications of such an idea.

> . . . if we accept the best nonexperimental evidence for observable (or macro) PK, then we have reason to believe that humans can intervene in day-to-day occurrences . . . everyday PK might blend smoothly and imperceptively into ordinary surrounding events, and real-life PK might affect or cause events of a sort that we usually believe are independent of PK (e.g., heart attacks, car crashes, good or bad "luck," ordinary decisions and volitions, and both healing and illness). (pp. 184–185)

Braude further speculates that psi events like ESP and PK may be *unconsciously* carried out by those involved in a social situation—even to the extent of being triggered by the unconscious purposes and emotions of individuals in that milieu, for positive or destructive reasons (p. 183).

> . . . we might be living in a world where we need to fear a profound lack of mental privacy, the direct psychic influence of others' malevolent thoughts, and the potential and daunting responsibility for the psychic efficacy of our own uncontrollable unsavory impulses and desires. (pp. 187–189)

What is remarkable about Braude's speculation is that his concept of action space very closely matches the descriptions in occult literature having to do with the alleged astral plane: The sorts of things that happen in action space and those alleged to happen in the astral plane have a rather startling similarity. I must pause here however to dispel one likely misinterpretation. I am *not* suggesting that the concept of action space somehow supports or verifies the metaphysical menagerie of the occultist. I would rather suggest that what the occultist clothes in obscure metaphysical guise may be demystified, or normalized, by Braude's notion of action space. But with this I am opening a very large field of study, which I will not be able to explore in great detail. All I can do here is to make some suggestions. I will therefore try just briefly to outline the similarities I have noticed. I will focus



on just one text of esoteric or occult literature, the detailed kabbalistic interpretation of the Tarot by Mouni Sadhu (pen name of the polish occultist Mieczyslaw Sudowski) in his book *The Tarot* (Sadhu 1962).

Suppose we look closely (with our third eye, if you please) at the following hypothetical situation. We are at the site of a plane crash in a remote location. A group of surviving passengers is gathered around one of their number who is a doctor and who is deciding whether she should operate immediately to save the life of a severely injured passenger (CEO of a large corporation), or whether it is too dangerous to attempt such an operation in these circumstances.

Now let's look at this as a *situation* in action space. Is the doctor's decision—and her ultimate behavior—separate from the wishes, anticipations, and concerns of the surrounding group? One of the group is agitating for them to form a protective perimeter for protection against a native tribe whose frenetic drumming can be heard in the distance. Another is begging for the doctor's attention to her child who has a nosebleed and is in hysterics. Others are giving encouragement and unwanted advice to the doctor. Can we describe this situation as a kind of *vortex* of energies in action space? And isn't the ultimate action to be taken by the doctor either in opposition to some of the distractions in this vortex, or concordant with some of the supportive energies that are swirling about?

Well, that swirl or vortex of impulses, agitations, desires, motivations, and energies matches a kind of existence on the astral plane that is called by the occultist a *tourbillon*, described as a kind of "astral" vortex, a "creation of force" which has to be managed by someone caught up within it by "finding the point of support for the tourbillon on the physical plane" (Sadhu 1962:29). I would say that this "tourbillon" is what I have called a *situation*. The doctor must find the pivotal point of her decision within the turbulence of the action space and manifest it in behavior in physical space.

Now let's add Braude's idea, in his seventh essay, of various psychic influences that may be swirling around the doctor (and perhaps also emanating from the doctor, and from the patient as well), including manifestations of ESP and PK that are being produced unconsciously by different individuals in the group. And suppose that at least one, or maybe several, individuals in the group happen to be sworn enemies of the patient

and their combined psychic energy is unconsciously exerting a PK influence that might cause the patient to die—an influence that the doctor, in action space, must find a way to resist.

Braude's speculations include such negative impulses as well as benign ones, all of which could be happening in the action space, or the astral plane, or whatever you want to call it. In the esoteric metaphysics of the astral plane, all kinds of forces, reified as entities, are named, and many of them are inimical. The "body" of a person as it exists on the astral plane is called an astrosome. Translating to an action space context, we can say that this "body" is what we understand as a self, having intentions and all the other properties that make up a living human being. (The physical body of this self can be described by physical science, but the astrosome of the person cannot.)

There are aggregates (called egregors) of astrosomes, which are collective entities (e.g., like corporations or clubs), some of which may have destructive purposes. The Magus (a person skilled at perceiving astrosomes and egregors) can learn methods for warding off these destructive entities. Translating, the doctor may be able to deal with the crying, nosebleeding child and her mother's demands, and may be able to focus her concentration enough to resist the PK impulses being directed toward her by the egregor of those present who would like to kill the patient. And so on. Sadhu's book outlines all these sorts of things that exist in what we can call action space, and even gives a way in which the assailed individual can make a "magic sword" with which to combat them—actually a metaphorical "sword" implying enhancement of concentration (which is just what the doctor needs to save the patient) (Sadhu 1962:60).

Well, regrettably I do not have enough space here to go into all the parallels between what is said by the occultist and what Braude describes as happenings in action space. I will however add one very interesting factor. In discussing action space, Braude says that both behaviorist and ICT explanations of behavior fail because "both bodily movements and the posited inner causes [in those respective kinds of explanations] occur at too low a *level of description* to adequately classify and systematize behavior" (p. 61, my emphasis).

So where we have in occultism the three chief planes of being, the Physical, Astral, and Mental, in Braude's analysis we find different "levels of description." But what are these "levels" and how are they related? I have seen this device, appealing to levels of description, many times in such discussions, but I have always found that the question as to the relation of these "levels" (call them planes?) to reality and to each other is not forthcoming. Perhaps we might look at what the occultist says:

> The astral plane is, according to definition, mixed with the physical and mental planes. They interpenetrate one another. So in the astral there must be visible the reflections of those planes, which correspond to its sphere. (Sadhu 1962:47)

So Braude might have proceeded a step further. Instead of three planes of metaphysical reality, we would have dimensions of the world to be dealt with on three levels of description: physical space, action space, and mental space. These, following Sadhu, are "mixed" and must "interpenetrate one another." But "mixed" and "interpenetrate" are unexplained metaphors. I cannot take this further here because of space limitation, but I will give a hint. The nature of the relation between the "planes" or levels of description is contained in the final sentence of the Sadhu quote above about how each of the levels is "reflected" in the others.

This must bring my Review to an end. I hope that I have shown the depth of the waters which Braude has stirred up by his penetrating analyses, and encouraged others to follow yet other paths through the essays in this book.

Notes

[1] *Tron* (1982), produced by Walt Disney Studios, directed by Steven Lisberger, and starring Jeff Bridges, was nominated for two Oscar Awards.

[2] For a corresponding analysis of the nature of a situation with regard to explanatory value, see Dewey (1938:66–67).

[3] Philosophers make a distinction between mere movement and action. An action is a movement with intent. A machine makes movements, but only an agent can perform actions. As a result, the expression "action space" would indicate a space that can accommodate agents with intentions.

[4] The dimensions of physical space are defined as directions 180 degrees apart used in determining the location of a physical object. The dimension of time has been in effect subsumed under the same definition, as really a fourth "direction" for the determination of position. An action space, in contrast, would not be found by "going in yet another direction," but by looking for a different layer of reality.

[5] What I mean by non-geometric here, is a dimension not defined by one or more additional "spatial" dimensions, such as the ten dimensions in string theory, where the extra dimensions are geometrically defined (see note 4).

[6] I would caution the reader, though, not to assume that this means acceptance of the esoteric metaphysical representations of the "astral

plane" as it is usually understood on the part of afficionados of occult practices.

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BOOK REVIEW

Las Alas de Psiqué: Extender la Mente Más Allá de los Límites [The Wings of Psyche: Extending the Mind Beyond Limits] by Alejandro Parra. Buenos Aires, Argentina: Antigua, 2014, 2 vols. Vol. 1, 247 pp., Vol. 2, 243 pp. ISBN 978-987-3707-03-2 (for both volumes), Vol. 1: 978-987-3707-04-9, Vol. 2: 978-987-3707-05-6.

In this book, Dr. Alejandro Parra, an Argentinean psychotherapist and parapsychologist, and past President of the Parapsychological Association, presents essays by the author about parapsychological topics that have been published in popular magazines. This is a unique book in that the Spanish language literature presents very few serious discussions of parapsychology by Latin American authors.

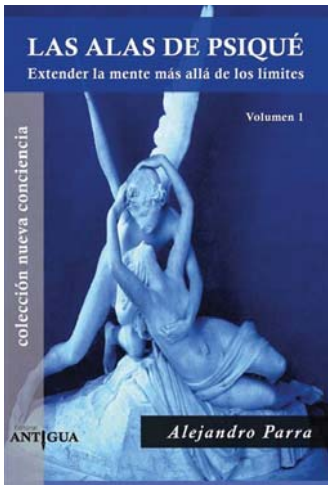
The wings of psyche, the author explains in the first volume, represent the "subtle, transcendent, spiritual and transhuman aspect" (p. 15) of the mind. Parra also states:

> Psychic abilities are the wings with which Psyche unfolds to take the challenge of its other nature. This ancient Greek myth represents nothing else than the effort of men of science to defy the limits of their knowledge so as to understand other realities . . . (Vol. 1:15)

In addition to an Introduction and an Epilogue, the first volume has 19 chapters organized in four sections: Doing a Bit of History, Varieties of the Extrasensory Experience, The Power of Mind Over Matter, and Who Is Afraid of the Paranormal? The first chapter takes care of preliminaries, definitions, and the importance of a scientific approach. Parra writes:

> It is difficult to predict parapsychological behavior. . . . But in spite of this the crisis of parapsychology is but a disguise of its change toward better understandable forms of observation aimed at enlarging this reality, keeping in mind that psychology in general has as many 'dark spots' in the scientific study of consciousness as parapsychology does. (p. 22)

There are too many chapters in this first volume to list them all. Some of the ones I find more interesting are On Spirits and Mediums in Ibero America, Exceptional Psychics and Seers: Ingo Swann and Eileen Garrett, Light Buds: The Experience of Seeing the Aura, Online Minds: The Unity



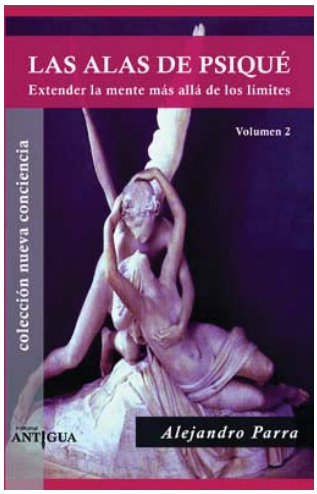
of Consciousness to Modify Reality, Is there a 'Global Consciousness' to Avoid Catastrophes?: The Psi Effect of September 11, and 'Not All Gifts are Blessings': Analyzing the Movie *The Sixth Sense*.

The second volume also has 19 chapters, not counting an Introduction and an Epilogue. Like volume 1, it has sections about The Variety of Spontaneous Experiences (e.g., influence of early childhood on psychic experiences, an interview with Larry Dossey about precognitive experiences) and The Power of Mind Over Matter (e.g., phenomena with St. Giovanni Bosco and with medium Eusapia Palladino). The rest of the chapters are in the section Death Is Not the End. The latter includes near-death experiences, materializations, experiences associated with organ transplants, and children who remember previous lives. The second volume ends with an interview with Charles Tart.

Some unique aspects of both volumes are the combination of experimental and non-experimental work, and the discussion of unusual and controversial phenomena. This includes topics such as materializations, spontaneous combustion, appearance and disappearance of objects, raps, mediumistic art, and the bleeding of an image of Christ. The authors of most modern overviews of parapsychology rarely discuss these phenomena, but they are still reported. I also enjoyed the chapter about mediums in Ibero America, which includes mention of cases in Brazil and Argentina.

The author avoids controversies and does not actively endorse many of the phenomena discussed, limiting himself mainly to presenting the topic. This can be seen throughout the book, examples being the chapters about raps and previous lives (Vol. 2). In the case of mediumistic paintings, Parra alludes to the difficulties of deciding on explanations dealing with discarnate agency and the subconscious abilities of mediums. He writes that the issue is a "question that will remain as open and unsolved as the philosophical debate carried on by monists–dualists, believers–atheists, and spiritualists–materialists" (Vol. 2:184). But Parra also presents conventional explanations suggested for some phenomena, as seen in his chapter about orbs in the second volume.

In other instances, the author presents critiques made by researchers about explanations and phenomena. For example, in the chapter about



the controversial materialization medium Kai Muegge he cites the worries of some researchers, but does not presents the specific objections of either Stephen Braude or Michael Nahm—perhaps this book was in press when the papers of these researchers were published in the *Journal of Scientific Exploration*. Interestingly, Parra does not include his own observations of the phenomena which took place in conditions he could not control and that were reported elsewhere.

On other occasions Parra includes some of his own work in the discussion. This is the case in the discussions of auras (Vol. 1:130) and of psychomantheum research (Vol. 2:167–168, 170–171). In the chapter about the disappearance of objects, he writes that he surveyed 560 psychology students, to whom he asked in a questionnaire:

> Have you had the experience of having left an object (e.g., a key ring or a watch) in a particular place and when you looked for it later you could not find it? But some time after you found it in the same place where you left it, even though you had checked the place several times? (Vol. 2:88)

He found that 46% of the respondents had the experience, that 47% found nothing unusual about the event, and that the rest considered the whole thing unexplainable. Only 5% considered a possible paranormal explanation.

In the above-mentioned discussion of the psychomantheum, based on Raymond Moody's research, Parra summarizes research he conducted with his colleague Jorge Villanueva in which he tested for ESP in psychomantheum vs. no psychomantheum conditions. Each of their subjects went through both conditions. It was found that there were more ESP hits in the psychomantheum (looking at a mirror while a target was being sent) than in the non-psychomantheum condition. However, the reason for the difference is not clear in the sense of actual cognitive effects produced by the psychomantheum, or its possible expectation effects.

Some of Parra's opinions are presented in the Epilogue to the first volume entitled The Era of the Consciousness Revolution Has Arrived. He presents overviews of development in recent decades, including work with the ganzfeld and geomagnetic activity. Parra clearly believes that parapsychology has shown the existence of ESP and PK, and is in the

forefront of thought regarding important questions. In his view this research may lead us to a situation in which "science must admit that it should enlarge its conception (maybe spiritual, maybe non-physical) of the nature of man" (Vol. 1:241). Parapsychology, Parra states, can have profound implications for other sciences, and may also change our ideas about human beings and their lives.

One general problem with the book is that the author mentions many investigators and investigations, and presents quotes, but generally does not provide references, something that would assist those interested in learning more about the topic by studying the literature themselves. This applies to his own studies mentioned throughout the book. There is a bibliography of books at the end of each volume (the same list in both), but it would have been better to provide the primary source articles for the research mentioned. While the book may be addressed to the general public, there are always intelligent and inquisitive individuals who may benefit from these sources if they want to pursue the topic in more depth.

Las Alas de Psiqué is an interesting collection of essays on a wide range of topics. It is written in a serious, yet non-technical, way, that brings information to the general intelligent public from international parapsychological research. This, as pointed out before, is not common in the Spanish-language literature, something that increases the value of Parra's contribution.

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BOOK REVIEW

The Spiritualist Movement: Speaking with the Dead in America and Around the World edited by Christopher M. Moreman. 3 volumes. Santa Barbara, CA: Praeger, 2013. 236 + 299 + 270 pp. $163 (hardcover). ISBN 978-0-313-39947-3.

The Spiritualist Movement is divided into three hardback volumes (handsomely produced) which between them contain 43 chapters on assorted aspects of the subject, the boundaries of which are generously interpreted. Most of the chapters are between 15 and 25 pages in length. The topics covered can be relatively narrow or relatively broad, and not all of them will be of keen interest to every reader. The contributors (who range from Ph.D. candidates to emeritus professors) are of varied academic backgrounds and attitudes toward the phenomena or alleged phenomena of Spiritualism. Some are pretty skeptical, others less so—indeed (interestingly), several are or have been practicing mediums. However, excessive partisanship, pro or con, is refreshingly absent.

The editor, Christopher Moreman, makes clear in his Introduction (which is printed in each of the three volumes) that his "collection" (he also refers to it as an "anthology") does not aim to provide a "comprehensive coverage of scholarship on Spiritualism," but rather to "illustrate the complexity of the movement and the ways it might be open to academic consideration." And this is the light in which we must consider it.

I shall begin with a brief characterization of each volume.

Volume 1 is subtitled American Origins and Global Proliferation, which is a touch misleading, because, although the contents are largely historical and the immediate impetus, from 1848, for the modern Spiritualist movement was American, the origins of the movement go much further back, and there is in fact very little about the United States in the volume.

Volume 2 answers well to its subtitle—Belief, Practice, and the Evidence for Life after Death.

Volume 3 is subtitled Social and Cultural Responses, and is something of a miscellany (not to say a hotchpotch). It contains chapters (mainly historical) on the interrelations of the Spiritualist movement with other religions, with occult groups and societies, with issues of gender and race, and with an assemblage of other cultural matters.

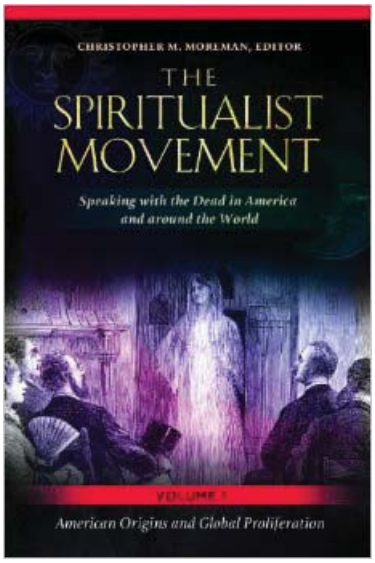
It should be noted that the same or related topics may crop up in more

than one of the volumes and can if desired be followed from one to the other through the indices.

Each volume is divided into several sections. Volume 1 begins with two chapters on Pre-Spiritualist Mediumship. The first of these is by Jordan Paper, who speculatively traces—and it is a line of speculation of obvious interest—what one might call quasi-spiritualistic practices back to hunter–gatherer shamanism of a good many millennia BCE, and from there through more stable early horticultural societies with their propensity to ancestor worship, to the first written records of such practices, which come from the China of 3,000 or so years ago (female mediums were predominant even then). Finally he moves quickly and briefly to the pervasive continuance of mediumistic possession states in various post-industrial societies.

The second is by David Gordon Wilson, who has himself been through mediumistic training. Like Jordan Paper, Wilson sees analogies between Spiritualist mediumship and other shamanisms, and indeed claims (p. 21) that "Spiritualism is a traditional shamanism." I will not enter into questions of how *shamanism*, a term of multifaceted and somewhat fluid meaning, should be defined. M. & S. Aldhouse-Green (2005), in their learned examination of early shamanism, spend some eight pages giving us a few indications. But it is clear, I think, that certain traditional shamanic practices may well have persisted in variant forms for centuries or even millennia.

Who first drew parallels between shamanic phenomena and the activities of Western mediums I do not know, but such parallels are drawn without introducing the word *shamanism*, in, for instance Howitt (1863(1):389–390, 398–409) and Brevior (1864:402–419). In 1879 Wundt disparagingly referred (p. 292) to Spiritualists as "pitiable victims of exotic shamans." By 1933—considerably before the earliest date suggested by Wilson—Lawton, in his substantial study of the Spiritualist religion, assumes (x–xi, 480n) that the comparison with shamanism, whether or not wholly viable, is at any rate obvious. A principal reason for this assumption—an assumption which some scholars would dispute—was doubtless that detailed accounts of Siberian shamanism (for example those translated from Russian sources by Czaplicka (1914)) had made it apparent that the range of skills and aptitudes displayed by shamans and by Spiritualist mediums—including trance, healing, possession by spirits, prediction, the production of spirit voices in darkened chambers, even the unexplained movement of objects—have certain similarities. It is, one should note, rather easy to exaggerate the overlap here. Wilson, however, holds that a, or rather the, distinctive similarity between Spiritualism and shamanism is that these skills, and the important background knowledge of the rituals and conventions that go with them, have to be shaped and honed by apprentice-participation until



they reach a level acceptable within the particular group or culture involved. From this basis he proceeds to an account of what he calls the formal protocols of mediumship as exhibited in and learned from Spiritualist Church services and from encouragement and training in the development circles organized within many such churches.

It seems to me, however, without going into details, that while some well-known mediums (particularly those destined to become 'platform mediums' or public performers) have indeed begun their careers with a period of apprentice-participation nearly as long, though hardly as painful, as that undergone by many shamans, others equally renowned have not, but have discovered their talents in private home circles, and have never ventured far or at all into platform mediumship.

The second section of Volume 1 is devoted to Spiritualism's Spread in European History, though the limited number of countries covered—Italy, Germany, Denmark, and Iceland—would not encourage one to think that its spread had been very wide.

Massimo Biondi devotes his chapter (3) on Spiritualism in Italy largely to the repeated denunciations of Spiritualism by the Vatican and the Catholic press. These began soon after the table-tipping and rapping craze that swept through Europe in 1853 reached Italy, and they went on, sometimes waxing or waning a little, until well into the twentieth century, and at times included excommunications and the banning of Spiritualist books and practices (though the scientific investigation of séances might still be tolerated, despite the frequent Catholic claims that the spirits under investigation were devils in disguise).

The Catholic Church was at this time increasingly under political, doctrinal, and even military pressure, and in 1869 Pius IX, hoping to strengthen his power, called the notorious First Vatican Council at which the doctrine of papal infallibility was proclaimed. The predictable result was the consolidation of an anticlerical and largely republican opposition, with which Spiritualists, as believers in free thought and free speech, were on the whole more at ease than they were with the authoritarian Catholic Church. During the later parts of the nineteenth century and the early parts

of the twentieth century a growing number and variety of Italian mediums appeared on the scene (Biondi has some interesting pages here on the early days of the celebrated Eusapia Palladino), and so did Italian savants and scientists prepared to investigate them seriously. However, the path since then has been, Biondi seems to think, chiefly downhill, and he remarks that Spiritualism has largely disappeared from the Italian scene.

Spiritualism in Germany is interestingly tackled by Andreas Sommer. His chapter (4) centers around a particular late-nineteenth century episode, which, however, he introduces against the broader context of earlier German romantic philosophy, and what, following Roy Porter, he calls the "Enlightenment crusade" (originating in eighteenth-century France) "to banish reports of supernatural phenomena." In Germany this crusade was represented from the 1840s onward by Hermann von Helmholtz, physicist and physiologist, the physiologist Emil du Bois-Reymond, and a group of their scientific friends, who more or less systematically gave time to bringing the gospel of scientific materialism to a wider audience. Among later adherents was Wilhelm Wundt, widely if perhaps erroneously regarded as the founder of experimental psychology in Germany, indeed the world.

This crusade had its scientific opponents, for example Karl Friedrich Zöllner, an astronomer and admirer of Schopenhauer, and Gustav Theodor Fechner, a surviving nature-philosopher of the old style, and a founder of psychophysics. In 1877 and 1878 Zöllner undertook a series of experiments with Henry Slade, an American medium who had left England in haste to evade a charge of fraud. Zöllner persuaded Fechner and two others, a physicist, Wilhelm Weber, and a mathematician, Wilhelm Scheiber, to join him in investigating Slade. Between them they witnessed many curious physical happenings under conditions they regarded as impeccable. Publication of their findings led to a sometimes unedifying war of words by supporters and principals of both sides, the details of which need to be read in Sommer's lucid analysis. Wundt maintained that conjuring can be shown to underlie the more startling phenomena of Spiritualism, but offered no indication how, and complained that scientists unnecessarily dignify Spiritualism by seriously investigating it and that doing so will corrupt the minds of the young. He emerges as a would-be panjandrum who thought that his *ipse dixit* on such questions, though unfortified by evidence or solid argument, should convince all reasonable men.

Unfortunately, Zöllner, in his reply, gave way at times to such indiscriminate fury that some thought him mad, while Fechner's calmer but firm response remained partly hidden in a private letter to Wundt. Wundt outlived them both and resumed his attack. But, as Sommer demonstrates, his continual evasiveness on crucial issues might well merit a less polite word.

Before long, apparent echoes of this dispute could be found in the United States. At that time it was customary for American graduates wishing to pursue an academic career to seek further education in German universities, and this was not less the case in psychology than in any other subject. It seems not unlikely that some portion of the hostility stirred up in U.S. academia at the turn of the nineteenth and twentieth centuries by William James's insistence that certain examples of supposed psychic phenomena were worthy of serious scientific study can be traced back to the pervasive influence of Wundt on German-educated staff members. That influence has long dissipated, but somewhat similar disputes are still a feature of the European and American scenes.

In Chapter 5, Jesper Vaczy Kragh discusses Danish Spiritualism from 1853 (when the table-tipping craze arrived there) until the present. Before long, as in the cases of Italy and France, it commonly took the reincarnationist form propounded by the Frenchman Allan Kardec. It had some links, not especially powerful, to political and social reform, and before the end of the century took a distinctly scientific turn according to which mediumistic phenomena can and should be investigated under strictly scientific conditions. Unfortunately, this laudable approach hit the buffers in the earlier part of the twentieth century when two 'physical' mediums, Einer Neilson and Anna Rasmussen, who had been taken up by the Danish Society for Psychical Research, came under strong suspicion, or more than suspicion, of fraud. Since then, it seems, Danish Spiritualism has persisted only in a relatively enfeebled condition.

Spiritualism in Iceland (Chapter 6 by Corinne G. Dempsey) is apparently in a more vigorous condition. From its beginnings in the early twentieth century it presented itself as a scientific rather than a religious movement. It had the good fortune to discover in its very early days a truly remarkable medium, Indridi Indridason (1883–1912), who produced a great variety of phenomena and was willing to submit to strict test conditions. His death at the early age of 29 caused a hiatus, but Dempsey attributes the survival and continued strength of Icelandic Spiritualism to the social respectability of its members and leaders and to its ties with other kinds of New Age movements. Possibly the need to fill in long dark winter nights may have had something to do with it.

Dempsey also notes two special characteristics of Icelandic Spiritualism. The first is a widespread belief that Spiritualism's resilience in Iceland is due to the existence there of certain people with a special gift for accessing the spirit realm. She collected a number of cases of skeptical persons who possessed this gift without realizing it and feared that they were becoming insane until they made reassuring contact with a Spiritualist community.

The second is the apparent prevalence there of the spirits of departed persons in need of rescue from the darkness of self-hatred in which they are lost. These unfortunate entities are prone to importune psychically gifted individuals for assistance, and it is a recognized function of many Icelandic mediums and their supposed spirit 'guides' to hold trance sessions devoted to helping such hapless souls. Clearly Iceland should be a regular port of call for psychical researchers.

Section Three of Volume 1 is devoted to some of Spiritualism's key historical figures. It begins with a chapter (7) by Trevor Hamilton on F. W. H. Myers, William James, and Spiritualism. Myers and James were investigators of Spiritualism rather than Spiritualists themselves. Though Myers came in the end to believe in human survival of bodily death, his theoretical position was far more sophisticated and his canons of evidence much stricter than those of most Spiritualists, who found him something of a puzzle. James, a close friend of Myers, never attained any full conviction of survival, and had indeed a strong aversion to the sort of afterlife promised by Spiritualism—he described spirit teachings as "a kind of philosophy and water." But both were important to Spiritualism, Myers for the vast case-collections embodied in his *Human Personality* (1903), and James, a world-famous philosopher and psychologist, for his readiness to state, in the teeth of much academic disapproval, that the strange phenomena involved (or certain of them) not merely deserved fuller investigation but cried out for it. Thus both merit their places in this book. Hamilton (the author of a biography of Myers) gives a good short account of Myers's involvements in psychical research, and an excellent one of James's.

Conan Doyle's right to a place in this book could not be denied, and Roger Straughan (Chapter 8) does him full justice. Doyle was a man of absolute honesty who felt it a moral duty to lay his newly found belief in Spiritualism before the world. This he did with considerable success in the decade or two following World War I, but in the process he sacrificed a comfortable retirement, his health, and a good deal of his fortune. Whatever one thinks of his beliefs, and of the basis for those beliefs—and as a Conan Doyle *aficionado* I have to deplore how in his late pro-Spiritualism novel, *The Land of Mist*, he had the formidable Professor Challenger of *The Lost World* convert to Spiritualism on really quite feeble evidence—one has to admire the man, his industry, his dedication, and his many fine qualities.

I cannot quite see why Susannah Crockford's Chapter 9, on the influence of animal magnetism on the development of Spiritualism in France from 1840 to 1870, is included in this section rather than in the preceding one on the spread of Spiritualism in Europe, or even in the concluding Volume 3, for though it contains much interesting information about Louis-Alphonse

Cahagnet and his 'magnetic' somnambules, most notably the remarkably gifted Adèle Maginot, and goes on to discuss the influence of Swedenborg (or his spirit) on Cahagnet, and the possible influence of Cahagnet on Allan Kardec, the founder of reincarnationist Spiritism, it is not exactly a specialized biography of any of them.

Chapter 10, on Jung and the Spirits, by Francis X. Charet, would I am sure be of considerable value to enthusiasts for and biographers of Jung (to neither of which categories do I belong), but it does not seem to me to be properly placed in a volume on the development, progress, and spread of Spiritualism.

The final section of Volume 1 presents four chapter-length case studies of Spiritualist Groups, Churches, or organizations from different parts of the world. Two of the chapters relate to what might be called the margins of Spiritualism, another seeks to test a particular sociological theory using a Spiritualist subject pool, and only one relates to what might be thought of as a more or less mainstream Spiritualist community.

The title of Rebecca Moore's Chapter 11, Angels Among Us in Four San Diego Spiritualist Churches, sufficiently indicates its contents. It appears that in these churches (to which the author and her assistants paid numerous site visits) traditional spirit guides (presumed to be discarnate human beings) have been largely replaced by intermediary angels, who similarly communicate through or are channeled by the various mediums involved. The function of these angels is to help, advise, teach, warn, encourage, heal, comfort, and so forth. It is certainly clear that angels are on the up and up, especially in some parts of the U.S.A. What is not clear is what rational grounds the faithful have for believing (as they do) that angels are actually present, both at the church services and wherever else they may be needed.

In chapter 14 Cristina Rocha writes about John of God, a Brazilian Spiritist healer, medium, and visionary, with a large international organization. John of God asserts (p. 209) "that he is the medium of the spirits of deceased doctors, surgeons, healers, saints, and people who were remarkable in their lifetimes." He (or his spirits) specialize, as do some other Brazilian mediums, in surgical operations, which are of two kinds, visible and invisible. Visible operations are carried out on stage with scalpels or scissors and without anesthetic or asepsis, which are held to be superfluous. Invisible operations may take place almost anywhere in or around the healing center and apparently without warning, though their occurrence may be signalled by pains, pins and needles, and even by the appearance of small cuts and bleeding.

It is clear that John of God meetings are hugely well-attended and

emotional, but although Rocha narrates in some detail the fortunes of one particular family during several visits and several operations (which deeply affected them), there is nothing within this chapter that would enable a reader to assess or even begin to assess the nature and extent of the ailments they (or indeed any other patients) suffered from, the extent and genuineness or otherwise of the presumed cures, the gullibility of the patients, or their openness to suggestion and placebo effects.

Chapter 13, Spiritualism in a Globalized Ireland by Olivia Cosgrove, is very different from the two preceding chapters. It attempts to test, with respect to the growing exploration of Spiritualism by the Irish, a Global Field Theory to the effect that in this highly interconnected and global modern world individuals are becoming increasingly aware that their lifestyle is just one option among many, leading to a growing sense of social detachment which they try to adjust by turning to a relatively nondoctrinal religion of their own choosing. In pursuit of this thesis, Cosgrove subjected the five willing subjects, out of 27 active Spiritualists approached, to a semistructured interview based on her review of the relevant literature. She interprets their answers as supporting the theory.

Chapter 14, by Paul Biscop, is entitled The Anomalous Anthropologist, presumably because the Canadian author is both a trained anthropologist and a practicing Spiritualist Medium, which certainly puts him in a good position to carry out fieldwork as a participant observer. His accounts of the organization, activities, and membership of a fairly typical Spiritualist church, of the beliefs of the members, and of the task and point of view of the mediums, are brief and to-the-point, and he is the only contributor to this volume to emphasize the centrality of good evidence to the whole Spiritualist enterprise (a topic to which I shall return).

Volume 2 has sections on Spiritualist beliefs, on Spiritualist practices, and on the evidence for life after death. Section 1 begins with a chapter by Stamford Betty which summarizes from an extensive literature fifty claims (the "Nifty Fifty") widely made by communicating spirits as to the nature of the next world and of life therein. He is commendably cautious in his approach, but ends (p. 18) with a nice analogy:

> [Spirit communications] are like maps. . . . When Lewis and Clark began their famous expedition from St. Louis to the Pacific Ocean in 1804, they depended on a few extremely rough maps. As they proceeded westward . . . their maps got better and better. But it was not until they reached the Pacific that they really knew what they had been seeking.

In Chapter 2, Cathy Gutierrez writes interestingly on Spiritualism and the Dismantling of Hell, mostly with references to spirit communications

received in the 1850s by the New York State Supreme Court Judge John Edwards and the later English Spiritualist Florence Marryat. For Spiritualists the departed are subject to a law of progression—even the worst sinners are the victims of poor circumstances and adverse surroundings and will one day be rescued from their wretchedness. Christian ideas of original sin, and the Calvinist doctrine that baptism is a prerequisite for entry into heaven, were particularly obnoxious to women as child-carers in an age of high infant mortality. Well-known female mediums were in the fore of the denunciation of such ideas. The upshot was concepts of an afterlife with various grades or levels through which even the blackest souls could ultimately progress toward the light.

Nonetheless, according to some the bottom level could be a pretty frightful place, a kind of hell, into which the most criminal and degraded would be drawn through like-with-like affinities. Some spirit guides seemed to dwell almost with relish on the torments which these vicious souls might inflict on each other. Gutierrez gives various examples, and seems to think that being possessed by (or acting out the roles of) such wretched beings might have given the female mediums involved the opportunity of behaving in unrestrained ways that would normally have been frowned upon (to say the least) by respectable society.

The next chapter (3) by Andrew Singleton, asks how far Spiritualist ideas about the afterlife have influenced contemporary beliefs. He notes at the outset the important point that central to the Spiritualist movement is the idea that it can provide *proof* of life after death (which is what has led to the not-infrequent claim that Spiritualism is a *scientific* religion). And if contact with the dead can be thus demonstrated—by for instance the purported communicators through mediums giving verifiably correct information about their earthly lives—it lends weight to whatever statements the spirits may make about the next world and their lives in it. The next world, as Singleton points out, seems on this showing to be rather like this one, with some of the ills of the earth gone, and not much of the contact with God or Jesus expected by Christians.

I think it would be fair to say that a good many in Western society have some general idea of the basic beliefs and practices of Spiritualism. When these turn up in films or on television (which is not uncommon), most of the audience knows what is going on. But how many audience members have any degree of belief in the rather cozy Spiritualist ideas about the afterlife is another question, which Singleton approaches by citing the findings of various surveys along with data from interviews he conducted himself in Australia with 52 persons of assorted religious beliefs or none. Together these suggest to him that there are continuities between traditional

Spiritualist beliefs about the afterlife and what quite many people believe today, and that (p. 47) Spiritualism "must be credited, at least in large part, with popularising this personal, anthropocentric [as opposed to theocentric] vision of the life to come."

Section 2 opens at Chapter 4 with a chapter by Elizabeth Roxburgh and Chris Roe who write about mental mediumship from the medium's perspective. They adopt a process-oriented (rather than an evidence-oriented) approach to what they term the "pathways to mediumship," and explore the phenomenology of how mediums experience communication with the departed. After some slightly, and to my mind unnecessarily, defensive remarks about the use of qualitative methods, they take up (p. 55) the matter of "mediums' own understanding of how their abilities originated and developed." They tackle this question by studying the auto/biographies of certain noted mediums, and by interpretative structural analysis of in-depth semistructured interviews conducted with ten approved mediums from the Spiritualists' National Union (Great Britain). It could not be said that anything very novel emerges. For many mediums the pathway to mediumship began in childhood with visions (often veridical) of known or unknown persons or unknown scenes. Often they had invisible playmates. The pathway was much eased if the children concerned came from families in which such experiences were accepted and treated as normal. Families who feared or disapproved of such matters or thought them signs of possible insanity would upset or alarm the children, though later contacts with another medium or a Spiritualist church or a development circle might reassure them and put them on the pathway again.

Clearly these children often are, or are likely to become, gifted fantasizers or fantasy-prone personalities (a topic not mentioned by Roxburgh and Roe) of the sort first brought to extensive notice by Wilson and Barber (e.g., Wilson and Barber 1983). Gifted fantasizers not infrequently believe themselves to possess psychic gifts and may come from homes in which their talents and creativity have been actively encouraged. On the other hand quite often they turn out to have had repressive, lonely, unhappy, or even abusive childhoods, which caused them to create and live as much as possible in their own worlds of rich fantasy. I have talked with more than one medium whose background was apparently of this kind, and in fact a rather similar connection was proposed by Lawton (1933:490–494).

Roxburgh and Roe's account of mediums' experiences of communicating with the deceased is of interest mainly for the range of differing kinds of experiences reported. Mediums may see or hear spirits as real or see them in the mind's eye or hear them speak into the mind's ear, or smell or be touched by them, or all or any of these in combination. Or they may simply

feel a presence and know things about the individual concerned. Or again they may sense in their own bodies pains or unpleasant sensations indicative of the ante-mortem sufferings of the deceased. Or they may take on their mannerisms, or way of talking, or urges. Or relevant information may come in symbolic form, with symbols peculiar to the medium in question, or as flashes of relevant or analogous memories from the medium's own mind.

Nothing is said here about full trance mediumship, perhaps because it is rather rare, or perhaps because such mediums have only a scanty recollection of what passed during the trance.

In chapter 5, Joan H. Hageman and Stanley Krippner ask, "Are there cultural differences in the practice of mediumship?" It is tempting to reply, "Of course there are." However, what this chapter is concerned with is hardly what one would immediately suppose from the title. The chapter (pp. 69–70) "compares the absorption and dissociative propensities between Afro–Brazilian mediumistic practices and other esoteric multicultural groups that involve ritualistic practices for spiritual transformation and/or healing." It reports data from the administration of the Tellegen Absorption Scale and the Dissociative Experiences Scale to 591 participants, mediums and non-mediums, from various religious or religio-philosophical groups from Brazil, the United States, and Korea. Various significant differences emerged between groups, though not between genders, but the authors' interpretations are so hedged about with caveats that, for a study involving so many subjects, the overall effect is disappointing.

Todd Jay Leonard writes (Chapter 6) about the backgrounds, religious and other, of 54 ordained Spiritualist Ministers and the circumstances in which they found their calling. Again nothing very surprising emerges. The majority of these ministers were middle aged and 70% of them were female. Only one in nine were full-time ministers, 12% were lifelong Spiritualists; most had tried out a variety of religions, with Catholicism being the most frequent. The "call" to become a minister most commonly took the form of a "message" heard by the recipient. It should be noted that achieving ordination involves a great deal of dedication and hard work.

Heather Kevan takes on (Chapter 7) the task of writing about Channeling. Channeling may be an offshoot of the Spiritualist movement but whether one should call it a part of that movement is an open question. Channelers in general do not link with spirits who establish their credibility by providing ostensible evidence of their identity with formerly incarnate human beings. They have no governing body to establish standards. The entities channeled present themselves as elevated or hieratic personages much given to spouting what often strikes cynics like myself as vapid twaddle. And as Kevan remarks (p.118), the "most obvious risk of channeling is delusion on

a grand scale." Nonetheless, the practice of channeling, and the audience for it, seem to be steadily growing, and many people have apparently been cheered by the teachings and helped by the advice that have been channeled. Kevan's task is thus to say the least a complicated and controversial one. She has produced one of the best chapters in the book, comprehensive, fair-minded, level-headed, clearly written, and thoroughly to be recommended.

The third and final section of Volume 2 is on Spiritualist Phenomena and the Debate over Evidence for Survival. It opens appropriately with Carlos Alvarado's Chapter 8, on Mediumship and Psychical Research, which covers the period from the mid-nineteenth-century until the 1930s. To survey, as Alvarado does, the extensive relevant literature from this period in just 17 pages, would have struck me in the abstract as nearly impossible. However, he has made a remarkably good shot at it. Detail of course was impossible, but he outlines, and gives pointers to the literature on the history of mental and physical mediumship, methodological and instrumental problems confronting investigators in these areas, qualitative and quantitative assessment of the findings, and emerging theoretical issues over the question of survival of death, over the existence of some kind of psychic force, and over the concept of the mediumistic subconscious, particularly as set forth by F. W. H. Myers.

The following chapter (9), by Philip K. Roth, deals with the conflict and cooperation of Science and Spiritualism over much the same timeframe as Alvarado's chapter. He notes (p. 152) that Spiritualism should not be conflated with "the broader investigative field" of psychical research, in part because many psychical researchers were not Spiritualists. Proposals that Spiritualistic phenomena should be investigated started early, and some scientists, most notably Sir William Crookes, began to put the idea into practice. Such attempts encountered growing hostility from the increasingly materialist representatives of orthodox science. However, there were those, such as Sir Oliver Lodge, who argued that science should not proscribe the investigation of certain sorts of alleged phenomena just because, if they really occurred, they would not fit in with currently accepted science. Roth goes into these old arguments in some detail, and the sad thing is that they still continue today on much the same terms.

Chapter 10, though it still sticks with issues to do with mediumship, moves from generalities to particular investigations. Its author, Gary Schwartz, describes some of his own (previously published) researches extending over a decade and a half and using 15 "research-oriented" mental mediums and some 50 "research-oriented" sitters who had personal and/or scientific reasons for participating. The experiments involved subgroups of sitters, and experimental designs were either single-blinded (medium blind

to the identity of the sitters), double-blinded (sitters in addition blind to the identity of their personal readings **or** medium blind to information about the sitters' deceased loved ones), or-triple blinded (for example the research assistants who received the transcripts and interacted with the sitters were blind to which readings appertained to which sitters). All the experiments eliminated visual clues, and some additionally eliminated auditory ones. Each sitter received transcripts of all the sittings and scored (on a 7-point scale) all the items of information in all of them as applied to themselves. Overall the scores were considerably higher for the sitters' own sittings than for the sittings of others.

Schwartz concludes that taking the results as a whole, the best explanation is what he calls the SOC (survival of consciousness) theory which he prefers to any theory of complex telepathy between persons still living, or (presumably) any theory of the post-mortem preservation of memories in some sort of non-conscious record. He apparently prefers the SOC theory because in some cases the information received by the mediums does not appear to be "dead" but (p. 163) "seems like communication with a living person. The information appears as if it is 'intentional.'" He gives what he regards as a couple of examples of this, curious but too long to be gone into here.

The following chapter (11), by Chris Roe and Elizabeth Roxburgh, takes up the topic of the cold reading and related strategies developed and used by pseudopsychics for convincing people that they are being told more about themselves and their concerns than the psychic could possibly have learned by ordinary means. It is a valuable survey, which I would happily recommend to anyone contemplating a visit to a psychic or medium. The authors do not advance any rules of thumb for guarding against being thus taken in, though reading their article would doubtless be helpful. Personally I would suggest that before reaching a conclusion about the nature of any *soi-disant* psychic's performance one should obtain or prepare a full transcript of all that was said and afterward go though it critically and carefully with the circumstances of the sitting in mind and if possible this chapter in hand.

The next two chapters switch from mental to physical mediumship. Chapter 12 is by Walter Meyer zu Erpen and concerns three Canadian home circles, one being the Philip Group, run in Toronto in the 1970s by George Owen, a mathematical geneticist, and his wife Iris. Philip was a deliberately created fictitious spirit from the seventeenth century, who nonetheless proved able to communicate through raps and table movements in the traditional manner. Another home circle, treated at much greater length, was one run in Winnipeg from 1918 to 1935 by Glen Hamilton, a local medical man of some distinction. To call it a home circle is an

oversimplification, since (p. 208) "In addition to the [non-professional] mediums and neighbours, the Hamilton group included doctors, lawyers, clergy, engineers, teachers, and other prominent members of Winnipeg society." The circle was essentially one for physical phenomena, but communicators also spoke through the mediums, who were generally in trance. From the start (p. 210), Hamilton took precautions to preclude the possibility of fraud. Ultimately, these included "a battery of 11 cameras and remote control apparatus, shorthand recording of the proceedings, special scrutineers, and examination of the medium and sitters before and after an experiment." The experiments in question were principally photographic ones involving flash exposures. In the earlier years they included apparently successful attempts to photograph small tables levitating without contact. Later came photos of teleplasms, the term given by Hamilton to the somewhat amorphous whitish masses (more often referred to as ectoplasm) that seemed to emerge from the medium's mouth and nostrils and that sometimes incorporated small-sized quasi-photographic images of recognized deceased individuals.

The Hamilton circle soon became well-known and photographs of the teleplasms were published in various places. But the original publications are hard to obtain, and even if one has them it is hard to know what to say about the phenomena. It is apparent that Meyer zu Erpen has gone more deeply than anyone into the history, the records, the archives, and the photographic collections of the circle, and that he himself is quite convinced of the genuineness of the alleged phenomena. But this chapter is far too short for him adequately to convey the strength (or otherwise) of the basis for his conviction. It is a pity that his overview of the case, The Quest for Immortality, is not available in durable printed form.

Chapter 13 is somewhat curiously entitled Becoming Bodies and the Birth of Ectoplasm. It is mainly about supposed ectoplasmic manifestations in relation to female anatomy and female sexuality as exemplified by two well-known mediums of the earlier twentieth century, Eva C. (Marthe Béraud) and Margery (Mina Crandon). The author's approach to these two cases is summarized early on in her chapter as follows (p. 230):

> Although these otherworldly performances were probably fraudulent and the mediums who produced them often relied on erotic misdirection, this supernatural stage allowed these women to transgress rigid sexual and social boundaries, improve their material conditions and, for better or for worse achieve celebrity status among scientists, Spiritualists, and psychical researchers.

"Improvement in material conditions" probably applies more to

Eusapia Palladino, who is briefly mentioned, than to the others. Delgado seems particularly interested by the apparent quasi-parturient emergence of ectoplasmic structures from the mediums' vaginas, and rather readily interprets these structures and some others with uncertain points of origin as having a phallic character (including, p. 231, one of Palladino's that "after great extension had a hand at its extremity"). It is certainly true that Eva's and Margery's sittings could (as two erstwhile sitters intimated to me) have decidedly erotic overtones. But there is I think some danger that readers of this chapter might be tempted to generalize too readily on the basis of its contents. I have seen various supposedly ectoplasmic manifestations—the mediums, as it happened being mostly male, and the ectoplasm's source where visible the mouth—and, while a few (both mediums and manifestations) were so obviously fraudulent that I could not understand how anyone could take them seriously, and others were at best rather dodgy, there were some that were certainly quite puzzling. And this seems to mirror the overall history of the subject.

In chapter 14, Anita Stasulane writes about the Raudive Voices, those curious recordings named after the Latvian Konstantin Raudive, who investigated them and wrote extensively about them in the 1960s and 1970s, ostensible spirit voices that intruded upon certain radio transmissions. The chapter is essentially a useful concise biography of Raudive, with emphasis upon his involvement with the voices, but offers no assessment of the phenomenon itself. The author mentions, but does not pursue, the more recent expansion of this electronic voice phenemenon (EVP) into instrumental transcommunication (ITC), the supposed communication with spirits through the use of a whole range of electronic devices, including video recorders, video cameras, televisions, and word processors. I should mention, however, that my own, admittedly limited, experience of such phenomena has been uniformly far less than convincing.

Chapter 15, by Trevor Hamilton, is the most perceptive, lucid, and informative short account I have so far seen of the celebrated cross-correspondences, the huge and often intractably complex series of interlinked automatic writings (in one case there were also spoken communications) produced in the early twentieth century by a number of ladies connected with the Society for Psychical Research. These scripts purportedly emanated from several of that Society's deceased early leaders, who had created puzzles that could only be understood if the writings of several different automatists were compared. My only critical comment would be that the précis of the *Ave Roma Immortalis* case which Hamilton uses as an example of a complex cross-correspondence is so heavily abridged that few without access to the original will be able to follow it. As he points out,

the cross-correspondences had, because of their often highly literary and esoteric content, a relatively slight influence on the Spiritualist movement in general.

This brings us to the third and final volume, which is devoted to "the ways in which a number of groups have related or responded to the Spiritualist movement." The links between some of these groups and the Spiritualist movement are tenuous. The first section begins with Andrew P. Lynch's chapter on Catholicism's response to Spiritualism, which to an extent follows on from Massimo Biondi's chapter in Volume 1. Lynch points out (p. 11) that after the mid-nineteenth century, Spiritualism presented the Church with a tricky problem. Science was starting to challenge the religious worldview on a number of fronts. Thus the Church had to be careful in dismissing the spiritual claims of the mediums lest in so doing it seem to offer sustenance to the materialist camp. But equally it had to take a stand against Spiritualism because Spiritualist beliefs about the afterlife were in flat contradiction to those of Catholicism.

In 1962 came the Second Vatican Council, at which the Church instituted a sustained attempt to reach out to, and perhaps reach an accommodation with, a changed world and new religious movements, particularly the curious hybrid ones that had sprung up in some parts of South America and the Caribbean. But the incompatibility of Catholic doctrines about the afterlife with Spiritualist beliefs still remains an insurmountable barrier to rapprochement.

All this is (up to a point) relatively straightforward. In the next chapter (2), Roddy Knowles takes on the much more complicated task of expounding the Christian theological attacks on nineteenth-century American Spiritualism, particularly over the issues of bodily resurrection, post-mortem judgement, and the continuance of miracles into post-apostolic times. The complexity is worsened because the various denominations and sects, Bibles in hand, not merely differed with each other on such questions but were liable to have internal differences, too. Arguments spread widely and could become quite acrimonious, particularly in the early days of the Spiritualist movement when its growth in numbers alarmed Christians, and, as Knowles forcefully puts it (p. 20), they "found that Spiritualism had spewed steam through fissures of their theologies, calling increased attention to existing debates and launching dormant issues into the public forum." Knowles does well to impose some order on this tangled historical episode.

Chapter 3, by Patricia Likos Ricci, is the longest and one of the most scholarly chapters in the set. It is essentially a brief biography of Mary Baker Eddy (1822–1910), the Founder of Christian Science, preceded and

accompanied by a good deal of relevant background information. What remains doubtful is whether the Spiritualist movement ever had any serious influence on the development of Christian Science or Christian Science on Spiritualism. It seems likely that in her earlier days, when Spiritualism was first burgeoning, Eddy "took on the symptoms of a 'medium'" with some enthusiasm. Later she always denied that she was or ever had been a Spiritualist, though she admitted (p. 51) that her life had always been attended by "phenomena of an uncommon order, which Spiritualists have mis-called mediumship." Her remarks on the subject tended toward the elusive. She could in fact (p. 52) "never extricate herself or Christian Science from an association with Spiritualism." Thus insofar as Spiritualism had an influence on Christian Science, it was probably through harmful rumors that there was or had been just such an influence.

Chapter 4, by Jane Williams-Hogan, is about the influence of Emanuel Swedenborg on the Spiritualist movement. Now there never has been any doubt among historians of Spiritualism that Swedenborg (1688–1772), and the influential New Church founded after his death, had a powerful influence on the beliefs and practices of the subsequent Spiritualist movement. Williams-Hogan outlines the teachings that Swedenborg drew from his own otherworldly visions, and notes how appealing the Swedenborgian account of the next world could be in comparison with the often much bleaker eschatology of many Christian denominations.

Although Swedenborg strongly discouraged others from attempts to contact spirits, for those who became aware of his claims the pull to experiment and follow his path (p. 71) became too strong, and well before Spiritualism emerged in the late 1840s there were small groups of Swedenborgians whose activities might be described as quasi-spiritualistic. Later there emerged certain well-known and as it were transitional figures, in that their Spiritualism, such as it was, was also—or so they thought—heavily Swedenborgian. Such were Andrew Jackson Davis (1826–1910) and Thomas Lake Harris (1823–1906). Both were strongly disapproved of by Swedenborgians, and it appears that Williams-Hogan dislikes them also, particularly Harris. She contrasts Swedenborg's indifference to personal gain or fame with (p. 78) "the energetic recruiting done by Spiritualists: the economic gain they often sought, the attempts to control their disciples that they employed, and the self-promotion found in their written works, their lectures, and their preaching." It might be pointed out, however, that Swedenborg had anyway no need of gain or fame, and that these remarks would apply only to a small number of Spiritualists (Harris amongst them).

The next two chapters, 5 by Alison Butler and 6 by Leo Ruickbie, are about the response of certain occult groups (notably ritual magicians) to

Spiritualism and its claims. Butler focuses particularly on Aleister Crowley (1875–1947) and Dion Fortune (Violet Firth, 1890–1946), the former of whom regarded Spiritualist séances as invitations to demons to enter and take control, while the latter favored some kind of rapprochement. Both Crowley and Fortune might be described as renegades who had branched out from the ambience of the Order of the Golden Dawn. This chapter would have been easier to follow if Butler had incorporated more of the significant dates, especially of letters, into her text instead of consigning them to endnotes. Ruickbe's topic is the celebrated Hermetic Order of the Golden Dawn (founded in 1887), whose members by and large vehemently opposed Spiritualism. In this, Ruickbie thinks they were perhaps influenced by the teachings of Madame Blavatsky. According to the (majority) Golden Dawn point of view, the medium (p. 112) "does not communicate with the dead, but with elementals or mere shells." This did not, however, stop some members from experimenting with Spiritualistic practices—after all, the Golden Dawn was dedicated to contacting spirits, albeit supposedly of a higher kind.

Both these two chapters are interesting in their own right, but so far as the influence of the Spiritualist movement is concerned, their scope is very narrow.

One might expect that the scope of the chapters in the next two sections, headed respectively Gender and Race, would not be subject to this criticism, but such is not the case. Chapter 7, by Elizabeth Lowry, is about Mrs. Leah Underhill, eldest of the three Fox sisters around whom, in 1848, what are reckoned the initiating phenomena of the modern Spiritualist phenomena, the 'Hydesville knockings,' had broken out. The focus is on Underhill's autobiography, *The Missing Link in Modern Spiritualism*, published in 1885. Lowry (who teaches composition at the university level) describes her approach to this volume as follows:

> I analyze [Underhill's] unique manner of enacting a feminine identity capable of operating both within and counter to common institutional discourses of that era. Underhill controls and shapes public memory by contextualizing her 1885 autobiography within a chronology of personal correspondence and newspaper reviews, reclaiming agency by "talking back" to her critics . . . she is able to construct a domesticized feminine self by strategically negotiating often contradictory cultural prohibitions pertaining to gender and corporeality.

Nonetheless I think her analysis of Underhill's book is largely correct. That lady was exceptionally clever at subtle self-presentation and self-vindication while just about managing to avoid overstepping the margins

of what was, or was becoming, socially acceptable. But what, if anything, this has to do with the special influence of the Spiritualist movement on issues of gender I am not clear. The female author of a book on, say, life on the frontier in the mid-century, might have exhibited a similar cleverness without having had any contact with Spiritualism.

I am even less clear about this question with regard to the Chapter 8, in which Deborah K. Manson discusses three fictional Edgar Allan Poe stories from 1644–1645. In all three stories the male mesmerizers are able to build up a strong rapport and intimacy with their invalid male patients, and the stories illustrate how such relationships could develop without any naughty rumors being spread around as to their nature. Manson thinks that they foreshadow (p. 141) "the freedom from gender norms as well as the authority and inspiration that scores of women would experience through Spiritualism." But why Poe's stories should be thought in any special way to illuminate this development I do not know. The annals of mesmerism contain many long and detailed accounts, some going back a good sixty years before the dates of Poe's stories, of prolonged treatments given by male mesmerizers to female patients with whom they were certainly on intimate terms, even though no intimacies took place.

This brings us to Section 3, which contains two chapters on the Spiritualist movement in connection with issues of race. The first of these, Chapter 9 by Margarita Simon Guillory, involves both race and gender. It centers around the city of Rochester, New York State, which, in the middle two quarters of the nineteenth century was a stronghold of (*inter alia*) both the abolitionist and the Spiritualist movements. In the case of abolitionism (concerning which Guillory gives a lot of information), Rochester antislavery societies were at first run exclusively by white males, though in the 1840s African American men began to play an effective part. White women formed their own societies, which made common cause with local antislavery men. African American women, however, were not offered membership in white women's societies. Undaunted, they set up their own abolitionist societies and groups, which became involved in the wider abolitionist scene. However, the activities of these ladies has been largely passed over by historians of Rochesterian abolitionism. Their activities, says, Guillory (p. 161), have been "historically repressed."

She seems to be of the opinion that something rather similar happened in connection with Spiritualism, and that (p. 156) "Spiritualist activities of African American women have been excluded from the historical annals of Rochesterian Spiritualism." I have two problems with this. One is that, so far as I can see, she presents no evidence that there were any African American Spiritualists, male or female, in Rochester prior to the

foundation of a Spiritualist church there in 1926 (African American women have been the pastors of this church since 1947). The second is that the Spiritualist movement, though quite possibly not in its early days free of such discriminatory practices, has certainly never been noted for them. So why is it picked on here to illustrate them?

The following chapter (8), by Kathryn Troy, features the manifestations between 1857 and 1888 through various mediums of Black Hawk, a well-known Native American chief who had died in 1838. Troy has extracted a good many accounts of these sittings from Spiritualist periodicals of the time. Black Hawk's repertoire included speech (or rather speeches) and (in darkened rooms) footsteps of different kinds, movement of small objects such as musical instruments, touches (indicative of varying garbs), and once at least an alleged materialization in broad daylight. None of this amounted to adequate evidence of identity. Spiritualists thought that in the later years of his performances Black Hawk's messages became increasingly intelligible, increasingly spiritual, and increasingly helpful toward whites.

Troy suggests (p. 183) that undoubtedly "the Indian ghosts that allegedly appeared at séances were recognized on some level as a symbol of the sins and consequent guilt of the United States in its dealings with Native Americans." There may be some truth in this, but at the same time there may be truth in the old idea that the religions of many of these native peoples had points of resemblance with Spiritualism, so that Spiritualists who came to know something of them would have believed that North American Indians would make suitable spirit guides. After all, Indian spirit guides were also much favored in Britain where guilt over the treatment of North American Indians can hardly have been widespread.

Section 4 of Volume 3, the last section of the whole work, is headed Other Cultural Issues, and is a sort of mini-miscellany of contributions that do not quite fit in elsewhere. The first, Chapter 11, is Sophia French's British Spiritualism and the Experience of [the First World] War. French is impressively well-read and covers a lot of ground—Spiritualist remembrance ceremonies, the psychic photography of Ada Deane, the war-time Spiritualistic activities of Sir Oliver Lodge and Sir Arthur Conan Doyle, the dilemma posed by the war for Spiritualists, who were in general strongly opposed to violence and war but consoled themselves with the thought that this could be the war to end all wars (a hope forever dissipated in 1939). Surprisingly, she does not tackle what seems to me to be the most interesting and indeed most important effect of WWI on British Spiritualism—not just the quite understandable increase among the general populace of interest in Spiritualism and the possibility of contacting the departed, but the remarkable emergence in Britain during the decade or two

following World War I of a surprising number of mediums widely agreed to be of unusually high quality. Why this should have been I do not know, but it is obvious to anyone who considers the immediately preceding state of things or (most particularly) the state of things now.

Chapter 12, entitled Negotiated Seeing, is by Chera Kee. Much of her article is concerned with the now more or less defunct practice of spirit photography, which she compares to the phantasmagoria shows that preceded it. Spiritualism claimed to be a scientific religion and the camera soon became a tool of science, and so it is not surprising that a marriage between them quickly came about. The resultant spirit photographers were soon producing photographs of their customers that showed not just the usual background studio props, but ghostly figures of spirit extras hovering hard by the customers. Of course customers might be aware that in principle it was not too difficult to doctor a photographic image and begin to wonder what exactly had gone on. In that event, as Kee observes, seeing became a process of "negotiating among the claims of what one saw, what one was told about what one saw, and what one believed, all at the same time." All this is in some sense true, and one could throw into the mix negotiating with the claims or insinuations of the photographer or perpetrator. Something analogous is also true of Barnum-type performances and of quite a few Spiritual séances. But all this is quite obvious to reasonably level-headed persons, though they might not use the term *negotiation* in saying so. I would have preferred it if Kee had used her knowledge of the history of the photographic arts to spell out the methods of fraud successfully used by psychic photographers despite the efforts of clients to catch them out. There is a large literature on this.

Concerning Chapter 13, by Laura K. Hoeger, on the nineteenth-century French Spiritualist artist J. J. J. Tissot's adaptation of traditional Catholic iconography, I do not feel qualified to write.

The final chapter (14) on Mediums and Stars, by Simone Natale, argues that "star mediums" are to a large extent accountable for the popularity of Spiritualism in the nineteenth and early twentieth centuries. "Star" here is conceived in a showbusiness or cinematic way as a person who is a charismatic crowd-puller. And it is certainly true that there have been multiple connections between the stage and Spiritualism, and that some well-known or once well-known mediums have shown remarkable charisma and a gift for controlling large audiences. But as far as accounting for the erstwhile popularity of Spiritualism is concerned, the 'star' theory has limited application, unless indeed one is going to extend the reach of the varied meanings of that term to cover almost all likely cases. And that is very easy to do. Consider Mrs. Leonora Piper, perhaps the most outstanding

mental medium of the late nineteenth to early twentieth century. Some of her phenomena were truly remarkable, and she figured in several lengthy articles by various *savants*. Because of these articles, she was written about in popular books, and figured with increasing frequency in newspapers. But she was a quiet family person, who did not regard herself as a public medium, and never sat for large groups. Was she a 'star'? Or take John Sloan, who may well have contributed a lot to British interest in Spiritualism in the 1930s. He was a retiring Glasgow working-man who became well-known because as a physical medium (who, incidentally, made no charge) he was a central figure in one of the best-selling books on Spiritualism ever written, Arthur Findlay's *The Edge of the Etheric* (1931). Was he a 'star'? The answer is that neither Piper nor Sloan had star quality, the sort of charisma that pulls crowds and gains title roles, but both became well-known and were in some basic sense stars, because they were leading lights in a given, at least roughly definable constellation or context. Such a context does not have to be large or the star widely famous. The winner of a tiddlywinks tournament could be a star in that particular setting. And both sorts of mediumistic star, the ones who like big halls, bright lights, and applause, and the ones who are happier with a small audience and quiet surroundings, have contributed to the spread of Spiritualism when it has been spreading. The latter sort, however, has contributed most to the proper recording and publishing of the kinds of evidence that may be of longer-term interest.

I imagine that few persons other than reviewers (and perhaps not all of those) will aspire to read these volumes in their entirety. Nonetheless they contain some excellent chapters, and a good many that may safely be recommended. Though there are some that, to this reviewer, appear misplaced, other readers may think differently. Most people with a serious interest in the Spiritualist movement, whether their principal concern is with the curious phenomena or alleged phenomena that have been central to it, or with Spiritualism as a social or religious movement with many historical ramifications, will find much to interest them in this collection.

It would be easy to criticize the Editor for the absence of topics that one would like to have seen included, but this would be pointless, for, as he says, this collection is in effect an anthology, organized under headings but certainly no textbook. It is quite clear that the range of topics (which is in fact considerable) has been limited by the number of areas for which qualified persons willing to offer contributions have been available. However, there is a hint that there may be further volumes in the future. Let us hope so.

Alan Gauld

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BOOK REVIEW

The Spiritualist Movement: Speaking with the Dead in America and Around the World edited by C. M. Moreman. 3 volumes. Santa Barbara, California: Praeger, 2013. 236 + 299 + 270 pp. $163 (hardcover). ISBN 978-0-313-39947-3.

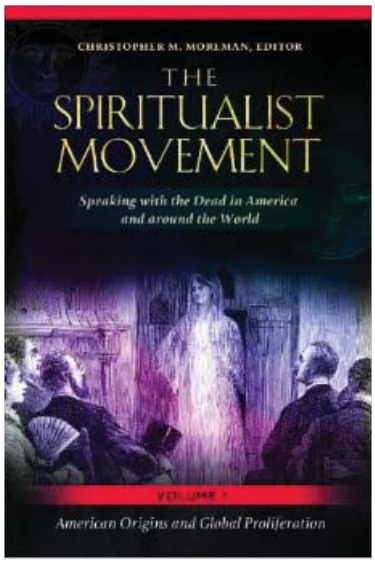
Christopher M. Moreman, Associate Professor in the Department of Philosophy at the University of East Bay, edited this three-volume anthology comprising 43 chapters contributed by as many authors. Each of the three volumes is divided into three or four sections, with two to eight thematically grouped chapters. Each volume is separately indexed. Photographs, tables, and other relevant illustrations are provided in limited numbers.

The initial impetus for this project came from the work of the "Death, Dying, and Beyond program unit" at the American Academy of Religion, established in 2004 by Moreman. He tried to focus on Spiritualism as a specific religious movement, "not to be confused with general spirituality or with mediumship in a broader global and historical context" (Volume 1:x). Defined as such, Spiritualism is a relatively young movement that appeared in the mid-nineteenth century in America and rapidly developed worldwide.

There has not been such a large analysis of the worldwide Spiritualist movement since Sir Arthur Conan Doyle's two-volume *The History of Spiritualism*, first published in 1926, and recently reedited in French (Doyle 2014). But Moreman's anthology affords a scholarly approach to this topic without any attempt to persuade of the correctness or wrongness of spiritualist doctrines. Even as Spiritualism has remained popular, it received relatively little academic attention: Such an anthology recognizes the lacuna and tries to rectify it. By doing so, it also contributes to a better understanding of all related areas, including scientific approaches to spiritualistic phenomena.

Rather than summarizing all 43 contributions (done very well by Walter Meyer zu Erpen 2014), I will attempt to extract the essence of the book, as my reading allows me, by quoting from a selection of chapters. I must clarify that I am not a specialist on this topic and have never been an adept of spiritualist practices or doctrines. This book thus forced me out of my habits, and asked of me a fair curiosity: I began each chapter with the naïve question of what it was going to teach me, and I was often surprised.

My first reaction was to face the lack of unity in what was called "the spiritualist movement." I find it better to speak in the plural: They



are "spiritualist movements" with, as a possible common ground, the "interactions with the dead." This feature is marginally present in many monotheist religions and other religious movements, but here it is a core principle around which are articulated *the creed* and *the community practices*. Besides, it may have been interesting to separate these two aspects as some spiritualist practices (like "turning tables") were adopted without accepting the spiritualist interpretation. An example of that is the Protestant theologian and politician Agénor de Gasparin, for whom "turning tables" was a scientific anomaly which was not well understood through the doctrines of the "necromancers" (Gasparin 1854). I do not think it is scientifically adequate to reduce this heterogeneity of the Spiritualist movements to local or personal "coloring" of a genuine American-born movement. Instead, works on the "plasticity of the psyche" in comparative history of religions showed that these singular appropriations are fundamental for the vitality and the effectiveness of these "orthopractices" (Mancini 2006, Mancini & Faivre 2011).

The first volume (14 chapters) examines Spiritualism's "American Origins and Global Proliferation." It contains excellent chapters by well-known historians of parapsychology (Massimo Biondi, Andreas Sommer, Trevor Hamilton). The plurality of spiritualist movements can also be seen in the fact that this anthology never succeeds in a unifying synthesis. All of the contributions are cases studies. Some are made about local arrangements (old cases in Italy, Germany, Denmark, and Iceland, and contemporary cases in United States, Canada, Ireland, and Brazil in Volume 1, Chapters 3 to 6 and 11 to 14), others are about some important figures' interactions with several aspects of Spiritualism (Myers, James, Doyle, Jung, in Volume 1, Chapters 7 to 10), and still others about a transversal aspect as a point of doctrine or the use of a technology (in the other volumes). The work was divided among all the contributors, but there is a lack of integration of their contributions. I don't say that such a synthesis is easy given the scope of the book, examining Spiritualism in all times and cultures!

The second volume (15 chapters) discusses "Belief, Practice, and Evidence for Life after Death." The final section of this volume is the most

interesting for those interested in the scientific debate about evidence of survival, remembering that Spiritualism claims to be an evidence-based religion. Gary E. Schwartz summarized the contemporary experimental approach of mediumship (Volume 2, Chapter 10), Walter Meyer zu Erpen described experiments in séance-room phenomena (Volume 2, Chapter 12), Trevor Hamilton analyzed the Cross-Correspondence Automatic Writings (Volume 2, Chapter 15), Carlos Alavarado and Philip K. Wilson presented the development and reception of psychical research with mediums (Volume 2, Chapters 8 and 9). There is an excellent overview of "Cold Reading Strategies" by Chris A. Roe and Elizabeth C. Roxburgh which describes a complete model that still required some additional empirical validation (Volume 2, Chapter 11; see also their contribution on the subjective perspective of mediums, Volume 2, Chapter 4). All these contributions come from experts in the field. This final section also includes a biographical study of Konstantin Raudive and his instrumental trans-communication (Anita Stasulane, Chapter 14) and a sexualized account of the production of ectoplasm by well-known female mediums (L. Anne Delgado, Chapter 13). This last chapter disappointed me because Delgado used the erotic side of these researches as an argument against their empirical validity, multiplying over-interpretative assumptions and making some historical mistakes (for example, by not checking Lambert's claims, see Evrard 2014).

The third volume (14 chapters) examines the "Social and Cultural Responses" to Spiritualism both during the heyday of the American spiritualist movement and today. Contributions are again diverse: interactions between Spiritualism and other religious traditions (Catholicism, Christian Science, Swedenborgism, Occultism, Golden Dawn), gender studies (Leah Fox's rhetoric; male medium in Poe's mesmeric fiction), race studies (African American; American Indian); and other cultural issues (wars, visual technologies, show business, art).

Meyer zu Erpen (2014:191–192) pointed out some topics that could have been further elaborated, but, from my point of view, this anthology addresses Spiritualism in a fairly comprehensive way and with a consistent quality. It will now be a must for any researcher who claims to work on this issue. It's a big step toward a transdisciplinary scholarly approach that does not give precedence to one or the other perspective. Even those who are only interested in the scientific aspects of so-called spiritualistic phenomena will learn something—but this anthology gives clues as well about the past and present context in which these phenomena occur and are studied.

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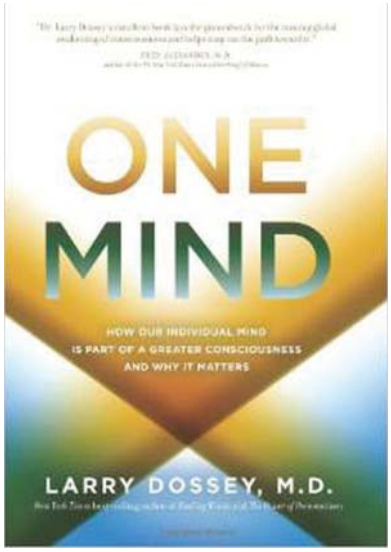
BOOK REVIEW

One Mind: How Our Individual Mind Is Part of a Greater Consciousness and Why It Matters by Larry Dossey. Hay House, 2014. 339 pp. ISBN 978-1401943158.

It seems that every cultural and spiritual tradition has some form of the proposition that we are all part of a greater whole. This goes by many names: Oneness, Unity, Wholeness, Godhead, Brahman, . . . , and it is something philosophers and teachers tell us in many ways. Alan Watts is quoted saying, "You and I are all as much continuous with the physical universe as a wave is continuous with the ocean." Swami Vivikenanda says, "All differences in this world are of degree, and not of kind, because oneness is the secret of everything." And from Osho, "We are all different expressions of one reality, different songs of one singer, different dances of one dancer, different paintings—but the painter is one."

Larry Dossey is an integrator, who sees the commonalities and fundamental connections of apparently separate conceptions of the world. He brings this ability to bear in books from which I learn new things and creative ways of thinking about old ones, about ideas that have been around for a long time. It is a talent or a gift he has, but I think, as Edison said, it is 1% inspiration and the rest perspiration. Of course people who are so effective in their work love what they do, so it's no sweat. His most recent book, *One Mind*, is a gathering and culmination of work by the author, who has touched on the same themes many times before. These are the connections that empower *Healing Words* and those which underlie *The Power of Premonitions,* two of his earlier books about ideas that have been on his mind and growing in clarity since his days as a battlefield surgeon in VietNam. Now Dossey is putting it all together, so that his readers can shortcut the process that engages so many of us, trying to integrate our spiritual and experiential lives. He asks who is in charge of the One Mind, and waves away the long list of gods and gurus and recommends to us that "as understanding grows, all descriptions of the Absolute are eventually transcended."

In his latest book, Dossey looks at a special form of the oneness proposition, namely that we are all integrated in One Mind. As always, Dr. Dossey presents the broad scholarly picture, with an amazing array of ideas and extracts from a wide spectrum of literature resources. The result



is a persuasive case for the overarching idea—that our minds are not isolated and separate even though we tend to perceive the situation this way. There is just one mind, and it is the source of all individual consciousness. That we don't know this or have any real ability to assess the matter directly is an issue we should consider, and this is the point or one important point that this excellent book addresses. Dossey believes that an acceptance and understanding of our participation in one mind is essential—it has the potential to lead humanity away from the divisions and perceived enmities which are on track to destroy the world we know. What we need is recognition and practice of our better selves, and that means recognition that we are truly interconnected and interdependent. We have urgent issues to address and they are so daunting that we feel helpless. But when we move as one, we do move mountains. This is Dossey's motivation: to help us see, accept, and act with the collaborative wisdom of One Mind.

The vehicle for Dossey's argument is a series of vignettes with the common theme of nonlocal effects of consciousness and mind. He recounts experiences that are unaccountable without a concept of links between people separated in time and space. The anecdotes are about the extension of mind into the world outside our bodies, where our connections become clear because we experience a sharing of ideas and emotions that is simply not explainable in the ordinary language of science. And on the other hand, he also looks to some of the most respected of scientists, people such as Erwin Schroedinger, William James, Albert Einstein, and David Bohm, for expressions of the theme in terms that are scientifically precise, but integrative across the whole range of human experience. Dossey says, "If we are to have a ghost of a chance of understanding the One Mind . . . we are going to have to learn to think nonlocally, not locally." The language of quantum physics may help us to do so. Dossey quotes Shroedinger's biographer, Moore: "Schroedinger and Heisenberg and their followers created a universe based on the superimposed inseparable waves of probability amplitudes. This view would be entirely consistent with the Vedantic concept of the All in One." We need the help of our most creative

minds still, for the classical view of separation is deeply ingrained. Eben Alexander, a neurosurgeon who was profoundly affected by his own near-death experience says, "There's something going on . . . about consciousness that our primitive models don't get. It's far more profound than I ever realized before."

The book is filled with examples and quotes, and an enormous amount of information. Dossey covers the territory of many specialists in such depth and with such clarity it is in some ways better than reading the original works. Naturally, he would want us to do the latter, and he provides the means. The book includes almost 50 pages of endnotes and references plus a terrific index that itself is almost 30 pages.

Here is a sampling of concepts that you might not think about without the urging of someone who has paid attention to interconnection and nonlocality. Empathic resonance may be one of the most potent drivers of the linkages across time and space. Medical intuitions, when they are respected, save lives. Ecological validity helps researchers look at the actual world, not a confusing abstraction. Ineffable factors play a large, but unacknowledged role in the thinking of both ordinary folks and professionals in science, medicine, and academia. Interconnection occurs at deep levels in a natural and normal medium we usually cannot see, much as a fish cannot see the water in which it swims.

Dossey touches on some of the difficulties that the construct of One Mind faces. Most of the Western world has a notion of individuality that is ingrained by education, leading to something close to fear of the merging implied by ideas of interconnection. We worry about "who gets the credit" for a new idea that in so many cases emerges from shared thinking, from "standing on the shoulders . . . ." Fortunately, there are vast numbers of messages from our best thinkers assuring us that such worries are misplaced. As Dossey says, "the urge to become absorbed into something greater . . . underlies the drive of many highly creative individuals." Arthur Koestler, reflecting on his insights while imprisoned, became certain that a higher order of reality existed, and that "the isolation, separateness and spatio-temporal limitations were merely optical illusions." It is an important matter, this illusion. Dossey gives the example of a conservative director of nursing, angry about her staff taking a weekend course in Therapeutic Touch (a healing practice). When the staff came back to work, they discovered a sign saying "There Will Be No Healing in this Hospital!"

His breadth of thinking and documentation is wide, but there is an area of relevance that is not covered in this book and I wish Dossey had given these questions some attention and research. How does the fact that some people have no interest in sharing in the One Mind affect the proposition,

and how can we accommodate those who may actively avoid interactions or any truck with the rest of us? I can see it might be argued that there is no choice; that every mind is integral to and sourced in the One Mind, but it seems a bit like expecting a family of sharks to be able to integrate with a school of dolphins. A few years ago I set up a survey using an interesting methodology, requiring selection of the preferred answer to a difficult question from pairs randomly selected from a pool. The question was, "How can we learn to talk with people on the other side of the fence?" The results showed most clearly that nobody has a singularly effective answer. It is common knowledge that people of privilege simply have no inkling of the life experiences suffered by people in poverty. That's because there is practically no communication across the fence. There is no shared consciousness at any level we can envision. I wonder how that can be, if we are all in and of the One Mind. To be sure, this question is mixing levels—the mundane direct experience versus the cosmic scheme of things—but I think it is germane because it speaks to the potential applications of the insights in Dossey's book. I hope many people take the message from the book that we are both capable of and responsible for efforts to activate our participation in an effective new level of consciousness that partakes of the One Mind proposition. And I hope people from all walks of life, including the other side of the fence, will read this excellent book.

Let me end with an extract from a speech of Vaclav Havel, the first president of the Czech Republic, quoted by Dossey.

> Without a global revolution in the sphere of human consciousness, nothing will change for the better in the sphere of our being as humans, and the catastrophe toward which this world is headed—be it ecological, social, demographic, or a general breakdown of civilization—will be unavoidable.

Havel sounds pessimistic, but he sees a way forward through:

> Transcendence as a deeply and joyously experienced need to be in harmony even with what we ourselves are not, what we do not understand, what seems distant from us in time and space, but with which we are mysteriously linked because, together with us, all this constitutes a single world.

Roger Nelson

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BOOK REVIEW

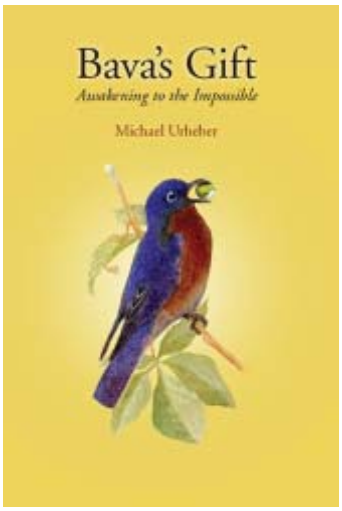
Bava's Gift: Awakening to the Impossible by Michael Urheber. Princeton, New Jersey: ICRL Press, 2014. 158 pp. $12.95 (paperback). ISBN 978-1936033102.

I must admit that I had some reservations about reading this book, as all I was told was that it chronicled the author's signs received from a discarnate friend. The prospect of wading through yet another book about pennies sent from deceased loved ones seemed onerous and a task to which I did not look forward. Not that I am averse to such manifestations and after-death communication, quite the contrary. I just prefer the evidence to be more convincing. Finding a coin that has become so devalued in our currency that half the people who drop them no longer bother to pick them up doesn't do it for me. However, to my great delight, Michael's message is no more about the signs than calligraphy is about the paper.

The death of a loved one is often a trigger for exploration and a search for meaning, and it certainly was for the author. Following the death of a close friend, he was witness to an unfolding symphony of communications, first dismissed as coincidences, but eventually embraced and treasured in an awakened consciousness. In this case, a journey began as the result of the appearance of a simple marble that was owned by the deceased. After Michael's friend Frank had passed, he decided to engage in an experiment by placing the marble with Frank in his coffin, specifically in his suit pocket. His initial motivation was simply to return the treasured item to its owner, but in the recesses of his mind Michael also contemplated a possible confirmation from his friend that he still survived in some fashion. He wrapped the marble in the paper on which Frank's eulogy was written, and on the top of the paper wrote "If you are ever around, toss this back."

The author was familiar with the work of physicist David Bohm and his theory of a multidimensional universe, and Michael saw his marble experiment in the same context. He writes,

> Later, I would think of Frank's marble, folded in his pocket, and all of it, including Frank, folded into the ground from where, reaching its source, and directed by a greater consciousness, it would unfold again, and again, as needed, delivering the requested information. (p. 30)



And unfold the marble did. In a continuous and somewhat playful dance with Frank and the universe, marbles starting appearing to Michael, his friends, acquaintances, co-workers, and strangers. However, it was not the appearances themselves that were significant, but the effect that these marbles and stones had on their finders. Invariably each marble discovery occurred at significant times and brought great comfort to the new owners, as if each marble possessed an ethereal luminosity, something that transcended the known senses.

Initially, Michael was not content to simply accept each marble discovery as a gift from beyond. He felt obligated and compelled to investigate each circumstance and consider material explanations. He evidently felt like many who doubt things that make no apparent sense, as one must try to discern fact from fiction if they are ever to reach any type of knowing. When the evidence reached a point where Michael finally relented, he describes

> . . . . A place where coincidence abandons any obligation to probability and becomes the partner of another reality, one that only the heart can fathom. Skeptics and quants can attribute such claims to 'magical thinking,' to the 'power of suggestion' or to the 'statistics of large numbers.' But who cares? Let them adjust their tables and interpret them however they choose. (pp. 60–61)

Michael was able to marvel at the "fun" of it all as he stepped back and observed all of the seemingly separate and unrelated events, each one individually able to be explained as coincidence, emerge into a collective transpersonal pattern orchestrated by an entity that resided outside the physical realm. He now saw Einstein as being mistaken, as God or some guiding force not only does play dice with the universe but revels in the game as we catch on and become part of the dance.

What made the evidence extraordinary for Michael was the fact that he controlled the experiment. He made a request and received a direct and swift response, as the marble was tossed back as requested. The effect that this had on Michael was not unlike those described by near-death experiencers and others who report being forever changed by glimpses of

unseen forces at work. However, despite the fact that he now felt imbued with a new vitality and deep knowledge of an interconnected universe, the nagging question of what to do about his shift of consciousness persisted. It's a question that has also been faced for centuries by experiencers of all types, whether the epiphany comes from a near-death experience, out-of-body experience, deathbed or shared vision, electronic voice phenomena, mediumship communication, meditation, afterlife encounter, or altered state of consciousness. In this case, Michael became convinced that the right course of action would be to openly share what he now knew. His decision was not an easy one, but it was the sense of it being something that he had to do, as if guided by a higher purpose that now took control. This meant giving up a productive and lucrative career in the corporate world as he ventured into a new life guided by heart and inner knowledge as opposed to brain and materialistic philosophy.

Bava's Gift is not a book simply about pennies and marbles, as it contemplates our role in the universe and at the same time probes the answer to why more of us are blind to things we cannot perceive with our physical senses. Michael writes that after Frank's passing,

> I had to agree that in his passing he created an opportunity for us to embrace a much larger dimension by making it visible, by reaching out across the veils of space and time and thereby revealing the shabbiness of our commonly held notions, and why our obstinacy in accepting greater truths is often an expression of fear. (p. 132)

It is a well-written and insightful work, and one that urges us to begin to see between the dots and recognize the miracles at play. I found it interesting that after the author's life change, he wound up befriending Bob Jahn and Brenda Dunne as he learned about their work at the PEAR (Princeton Engineering Anomalies Research) lab at Princeton University. One could imagine Michael's reaction when he learned that their consciousness experiments involved the use of 9,000 marbles. The universe at play once again.

Bob Ginsberg

Society for Scientific Exploration

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