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AIMS AND SCOPE: The *Journal of Scientific Exploration* publishes material consistent with the Society's mission: to provide a professional forum for critical discussion of topics that are for various reasons ignored or studied inadequately within mainstream science, and to promote improved understanding of social and intellectual factors that limit the scope of scientific inquiry. Topics of interest cover a wide spectrum, ranging from apparent anomalies in well-established disciplines to paradoxical phenomena that seem to belong to no established discipline, as well as philosophical issues about the connections among disciplines. The *Journal* publishes research articles, review articles, essays, commentaries, guest editorials, historical perspectives, obituaries, book reviews, and letters or commentaries pertaining to previously published material.

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Journal of Scientific Exploration, Vol. 28, No. 1, pp. 5–6, 2014 0892-3310/14

EDITORIAL

This issue of the *JSE* is appearing rather later than originally planned, and not because (or at least not simply because) I and my editorial team members are slackers who prefer debauchery to diligent work on the Journal. The Spring *JSE* issue was supposed to be an issue focusing on physical mediumship generally and the case of the Felix Experimental Group (FEG) in particular. Some readers might recall that I've given two *SSE* presentations on my experiences with that German group, including my report, last June, on some intriguing and carefully controlled—but still inconclusive—séances at a secure private farmhouse in Austria belonging to one of the investigators. Those Austrian sessions yielded videos of a table apparently levitating in red light and the medium pulling large quantities of ectoplasm from his mouth, which then accumulated in an apparently animated heap on the floor in front of him. The ectoplasm was produced despite numerous controls, including a careful strip search of the medium, examination of the medium's clothes, and inspection of the cabinet in which he sat.

As the Spring issue approached the proofreading stage, with two long reports on the FEG ready to go, various pieces of evidence surfaced casting serious doubt on some (though not all) of the FEG phenomena. The case, constructed by one of my co-investigators, is so far largely circumstantial, but it's by no means trivial. And so it quickly became clear that the papers scheduled for the Spring issue needed to be massively rewritten, and the FEG phenomena generally carefully re-assessed in light of the recent developments. As a result, the physical mediumship issue is now planned for the summer. Of course, this last-minute decision required us to move some other already-accepted papers to this issue and then (naturally) to engage in very late and time-consuming flurries of copyediting and proofreading.

So I apologize for the delay in preparing this issue for distribution, but when the mediumship issue finally appears over the summer, the reports on the FEG will be more nuanced and authoritative, and probably more complex, than they were before.

In the meantime, you now have before you a very substantive collection of papers on a healthy variety of topics, illustrating nicely the vital diversity of issues pursued by SSE members and addressed within the pages of the *JSE*. Our lead research paper reports a study in which inexperienced remote viewers attempted to use associative remote viewing to predict the Dow Jones Industrial Average. That's followed by a paper on the biological

anomalies reported in the 1999 Hoeven crop circle and a recent effort to replicate the anomalies normally. The next paper challenges the prevailing view of how and when the Americas were settled by re-evaluating and defending the possibility of pre-Columbian transoceanic travels. These papers are followed by a characteristically well-researched historical perspective paper by Carlos Alvarado, another provocative essay by my editorial predecessor Henry Bauer, and our usual varied array of interesting book reviews.

Stephen E. Braude

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RESEARCH ARTICLE

Stock Market Prediction Using Associative Remote Viewing by Inexperienced Remote Viewers

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Abstract—Ten inexperienced remote viewers attempted to predict the outcome of the Dow Jones Industrial Average using associative remote viewing. For each trial in the experiment, each participant remotely viewed an image from a target set of two images, one of which he or she would be shown approximately 48 hours from that time. Of the two images in the target set, one corresponded to whether the Dow Jones Industrial Average (DJIA) would close up, while the other corresponded to whether it would close down at the end of the intervening trading day. For feedback, the viewers were shown only the picture actually associated with the actual market outcome. In aggregate, the participants described the correct images, successfully predicting the outcome of the DJIA in seven out of seven attempts (binomial probability test, $p < .01$). Investments in stock options were made based on these predictions, resulting in a significant financial gain.

Background and Motivation

Finding practical applications for psi phenomena will increase interest in the field. One of the more valuable applications would be the reliable prediction of future events. A form of psi that seems to offer relative dependability for predicting future events is associative remote viewing. Associative remote viewing (ARV) is a scientific protocol derived from the much-further-studied psi phenomenon known as remote viewing. The procedures for remote viewing were first developed by consciousness researcher Ingo Swann in late 1971 (Smith 2005), and further explored by Swann, Harold Puthoff, Russell Targ, and others at the Stanford Research Institute beginning

in 1972 (Targ & Puthoff 1977, Puthoff & Targ 1976). By the early 1980s, their experiments were demonstrating that remote viewing could be both reasonably and consistently successful and repeatable. However, practical applications for remote viewing were still in need of further development (Dunne & Jahn 2003). Stephen A. Schwartz developed a remote viewing protocol and pilot experiment to predict the outcome of an event with multiple discreet possible outcomes (Schwartz 2007). Puthoff and Targ independently adapted this protocol to use remote viewing to determine the outcome of a binary event. This new protocol was dubbed "associative remote viewing" (ARV).

Associative remote viewing shows promise as a practical application of psi phenomena, yet there have been relatively few published investigations into its potential uses. The research that has been carried out has in large part been successful. ARV warrants more in-depth research and the further development of a simple protocol that yields consistent and repeatable results. The intent of this experiment was to replicate previous experiments and provide additional understanding of the associative remote viewing protocol.

Summary of Relevant Research

The first published study of ARV was conducted by Puthoff in 1982. For this study, Puthoff conducted a series of 30 ARV trials in an attempt to predict the outcome of the silver futures market. He asked several novice remote viewers to describe as precisely as possible an object that they would be shown sometime after the close of the market the following day. To avoid potential remote viewing access to a pre-established target pool, each day two objects that were as different as possible were chosen at random by Adrienne Puthoff.

One object represented the market closing higher than when it opened and one represented the market closing lower than when it opened. For example, the target set might include a pencil and an apple, with the pencil standing for a higher close and the apple for a lower one. Which object represented up and which down was determined by a random event generator after the judging was completed. A judge determined which of the two objects best matched the results produced by the remote viewers during their sessions. This outcome was then used to decide what purchasing strategy should be used to invest in the market. When the outcome became known after the close of the market the following day, the remote viewers were shown the object that matched the actual outcome (whether silver futures were up or down in reference to the starting basis) as feedback.

The results of these trials were successful. Using seven naïve remote

viewers, Puthoff's experiment yielded two different, though still statistically significant, results. The first outcome was significant at $p < 1.6 \times 10^{-4}$, calculated on the basis of percent hit-rate for all individual remote viewings (127 correct out of 202). Puthoff adapted the result to apply to the market by using a "majority vote" approach that weighted the outcomes based on how many viewing results favored one target over the other in each individual market prediction. Because of the smaller trial size this produced, the p-value was less statistically significant at $p < 2.2 \times 10^{-2}$. Financially, the trials netted a profit of approximately $250,000 for their investor, of which Puthoff's share was ten percent, or more than $25,000, which he used to help fund a new Waldorf School (Puthoff 1984).

Also in 1982, Targ and Keith Harary used ARV to predict silver futures in an attempt to raise funds for their research (Harary & Targ 1985). The results for their first experiment were highly successful, earning $120,000 and a front-page article in *The Wall Street Journal* (Targ 2012, Larson 1984). A replication attempt the following year tinkered with the protocol by, among other things, shortening the time interval between trials, thus conflating the feedback by having viewers perform a subsequent trial before receiving feedback for the preceding one, and the experiment foundered (Targ 2012, Houck 1986). In 1995, Targ returned to the original protocol and again showed highly significant results for a silver futures target (Targ, Katra, Brown, & Wiegand 1995).

Other ARV experiments continue to be carried out informally or as private research initiatives. One such example is that of Greg Kolodziejzyk. From 1998 to 2011, Kolodziejzyk undertook 5,677 ARV trials to predict the market. He arranged his trials into sets to respond to 285 "project questions" designed to predict the outcome of one or another of the futures markets. Of the trials, 52.65% were correct responses, where only 50% would be expected by chance. This produced a statistical significance of $z = 4.0$. However, using the error-correction offered by larger numbers of trials per question, project questions were answered correctly at 60.3%, which is statistically significant at $z = 3.49$. Using confidence scores as a further error-correction mechanism, Kolodziejzyk achieved an overall success rate of greater than 70%, yielding a profit of $146,587.30 (Kolodziejzyk 2012).

There have been some adaptations and improvements to the ARV protocol over the years. Puthoff and Targ each used physical objects in their ARV experiments. Subsequent researchers have substituted images in place of these objects. This makes the target set easier to perceive and easier to manage, and allows for viewers and researchers to participate while in different locations.

Furthermore, ARV researchers have been testing different hypotheses

about the feedback event and its relevance to the ARV experiment. Some researchers suspect, for example, that the more significant—referred to as "numinous" (Schwartz 2007)—a feedback event is, the more likely it is that the associative remote viewer taps into the feedback event rather than the prediction event. Therefore, by increasing the emotional and perceptual significance of the feedback event, an experimenter would likewise increase the likelihood of a subject remote perceiving that event and providing a successful session.

Kolodziejzyk used heavily automated computer-based protocols in place of some of the roles usually filled by other persons. The purpose of this was to remove human subjectivity from the process as much as possible. Other private researchers have been experimenting with feedback timing, self-judging, alternative ways of providing feedback, etc., but thus far have not provided public access to their findings.

The single largest criticism that can be said about previous research into ARV is that not enough of it has been carried out, reviewed, and published.

Experimental Method

The experiment being reported here was conducted by ten inexperienced remote viewers: nine University of Colorado Students and one University of Colorado professor. The gender distribution was three women and seven men. Every few days, the number of which depended upon whether the next class was on a Tuesday or a Thursday, the viewers were tasked to remotely view a target during class. The target was always a photo that the viewers would be shown at the beginning of the next remote viewing period a few days later. The remote viewers were given five minutes to quickly describe on paper and sketch the image they would be shown in the future.

After the completion of the sessions, the judges (assigned to evaluate the results and decide which target the remote viewing results matched) would compare each remote viewing session to two previously selected targets. These targets were selected from a pool of pre-qualified picture files before the trials were carried out and could depict any object or scene. The only criterion for selection was that the two targets in any given trial should resemble each other as little as possible, so as to reduce the difficulty in distinguishing between targets when comparing results to them. The targets were printed and sealed in dated envelopes by an independent party (the spouse of the experimenter) after a coin toss was used to sort the targets into Up targets (indicating the market being predicted was up) and Down targets (indicating the market would be down). The judges did not know which outcome was associated with which image until after the judging was completed.

When judging, the judges would look for common elements between the remote viewing sessions and either target photo. The judgments were based on subjective interpretations by each of the judges involved, and did not follow any specific judging protocol. If the majority of the ten viewers' sessions were judged to most accurately describe the Up target, that was taken as a prediction that the Dow Jones Industrial Average (DJIA) would close up at the end of the next market day. If the majority were judged to describe the Down image, that would be a prediction that the DJIA would close down.

At the beginning of the next market day, the experimenter would make a decision to purchase DJIA options according to the prediction. Just before the close of the market, he would sell the options and actualize any loss or gains. At the beginning of the next trial period, the experimenter would close the previous feedback loop by showing the viewers only the picture that corresponded to how the market *actually* performed. This could be thought of as creating the then-future target event that the ten viewers were remote viewing during the previous remote viewing period. Closing the feedback loop was a crucial aspect of the experiment.

We repeated the above procedure for seven trials with the same viewer participants. Because of personal scheduling issues, the number of remote viewers fluctuated between nine and ten viewers. At the end of the seven trials, the results were then compiled. No sessions were thrown out and the results were exactly as presented below.

Experimental Results and Analysis

Of the seven trials performed, all seven resulted in correct predictions. The results are provided in Table 1. The Appendix displays two sessions along with the possible target images. One shows a clear prediction and the other an ambiguous one. Using a simple two-tail binomial probability analysis to determine the p-value, it was statistically significant at $p < .01$.

Regarding the financial results, on an initial investment of $10,000 we gained approximately $16,000 with a total of $26,000 at the end of trial 5. The first five trials were conducted on days of large market swings, therefore the potential gains were very large. Trials 6 and 7 happened on days of small market changes and, despite resulting in correct predictions, produced small losses because of the mechanics of the options trading vehicle. A timing issue in the trade of trial 7 resulted in an additional monetary loss of approximately $12,000. However, it is important to stress that this was in spite of the prediction itself being correct. (Without this timing error, total cash at the end of the project would have amounted to $38,000, or a return of almost 400% on the investment in a span of about two weeks.)

TABLE 1

Predicted vs. Actual Outcomes

Composite Results							
Trial #	**1**	**2**	**3**	**4**	**5**	**6**	**7**
Date	13-Nov-08	18-Nov-08	20-Nov-08	2-Dec-08	4-Dec-08	9-Dec-08	11-Dec-08
Predicted	Down	Down	Up	Up	Up	Up	Up
Actual	Down	Down	Up	Up	Up	Up	Up
Viewer #	**Individual Perceptions**						
1	**D-2**	**D-1**	M-1	–	**U-1**	**U-1**	**U-1**
2	**D-2**	**D-3**	**U-3**	**U-1**	**U-1**	**U-1**	**U-1**
3	**D-2**	–	*D-1*	**U-2**	**U-1**	**U-1**	*D-2*
4	M-2	**D-1**	M-1	**U-1**	**U-1**	**U-1**	**U-3**
5	M-2	*U-2*	**U-1**	**U-1**	**U-1**	**U-2**	**U-1**
6	**D-2**	*U-2*	M-1	**U-1**	**U-1**	**U-3**	**U-3**
7	**D-1**	*U-1*	*D-1*	**U-1**	*D-1*	**U-1**	**U-1**
8	**D-1**	*U-1*	M-1	*D-1*	**U-1**	**U-1**	*D-1*
9	M-2	**D-1**	**U-2**	**U-1**	–	**U-1**	*D-1*
10	–	**D-1**	**U-1**	**U-1**	**U-1**	*D-1*	**U-2**

In the Individual Results, "U" refers to a prediction of the Up image, "D" to Down, "M" to indications of both, and "–" to the absence of the viewer. The adjacent number gives the rated degree of correlation, with 1 being low, 2 medium, and 3 high. Correct predictions are shown in larger bold font, and incorrect predictions are shown in italics with an underscore.

Discussion

There are several possible reasons the experiment was successful. The associative remote viewing protocol has been established for years. However, one reason that this study could have yielded exceptional results was the number of viewers used in the trials. Most previous ARV experiments had access to fewer viewers per trial. We assume that this was because it is difficult to sustain the participation of a larger number of remote viewers for an extended series of trials. Our protocol made use of the classroom setting to guarantee the participation of relatively many viewers for the duration of our study.

By having access to many viewers, each trial had a built-in error

correction before making the prediction. Even in the case of low-quality remote viewing results, by choosing the target associated with the best of ten sessions per trial, we were more likely to choose the correct outcome. It is possible that if the study had access to 100 viewers, the accuracy might increase further.

Financially speaking, we learned a few lessons. Our loss on trial 7 shows us that one *must* sell at the end of the prediction period. The prediction is only for the time frame specified, so holding onto the options beyond that leaves the trade open to chance once again. It is important to strictly adhere to the protocol, which is decided in advance, or the outcomes will be unpredictable.

Because this study was carried out near the end of the school term, the number of trials was limited. It is not clear that the perfect success rate could be maintained during a longer trial. Decline effects have been found in many psi studies, such as another prediction experiment carried out in our laboratory, which showed a robust effect and then a steep fall to random behavior (Moddel, Zhu, & Curry 2011). However, even a moderate success rate extended over a long period of time would be significant.

Conclusions

Associative remote viewing appears to be a reasonably accurate way to predict the future of binary outcomes. An ARV project is simple to perform and, with some experimentation, it may be possible to improve upon its already fairly accurate predictive ability. If the world were to embrace the fact that it is indeed possible to reliably and consistently predict a future event with consistently greater than 50% accuracy, it could have a significant impact socially and perhaps even financially. At the very least, the stock market, along with other institutions where knowledge of the future could change system dynamics, might need to change their business models with respect to ownership and participation. Moreover, ARV has dramatic implications for how we view time and our ability to perceive the future.

This study was carried out as a class project in a course entitled "Edges of Science" at the University of Colorado in Boulder. As such, its scope and the number of trials were limited. The results were presented at the Annual Meeting of the Society for Scientific Exploration on June 11, 2010.

Acknowledgements

We are grateful to Paul H. Smith for his help in improving the background information and the text, and to Harold E. Puthoff for additional clarification on his 1982 ARV experiment.

Appendix

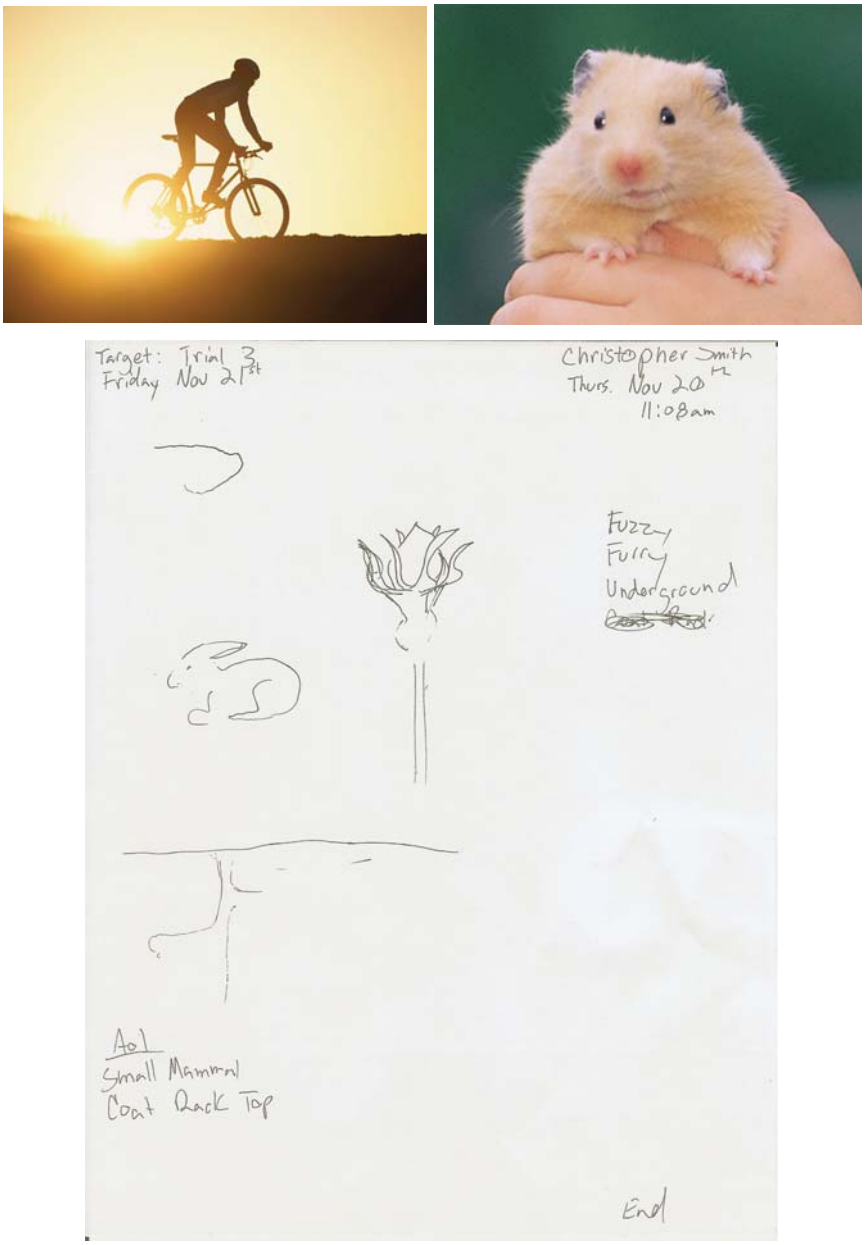

Figure 1. Example session: clear prediction. The photograph that corresponded to the actual outcome was the right-hand one.

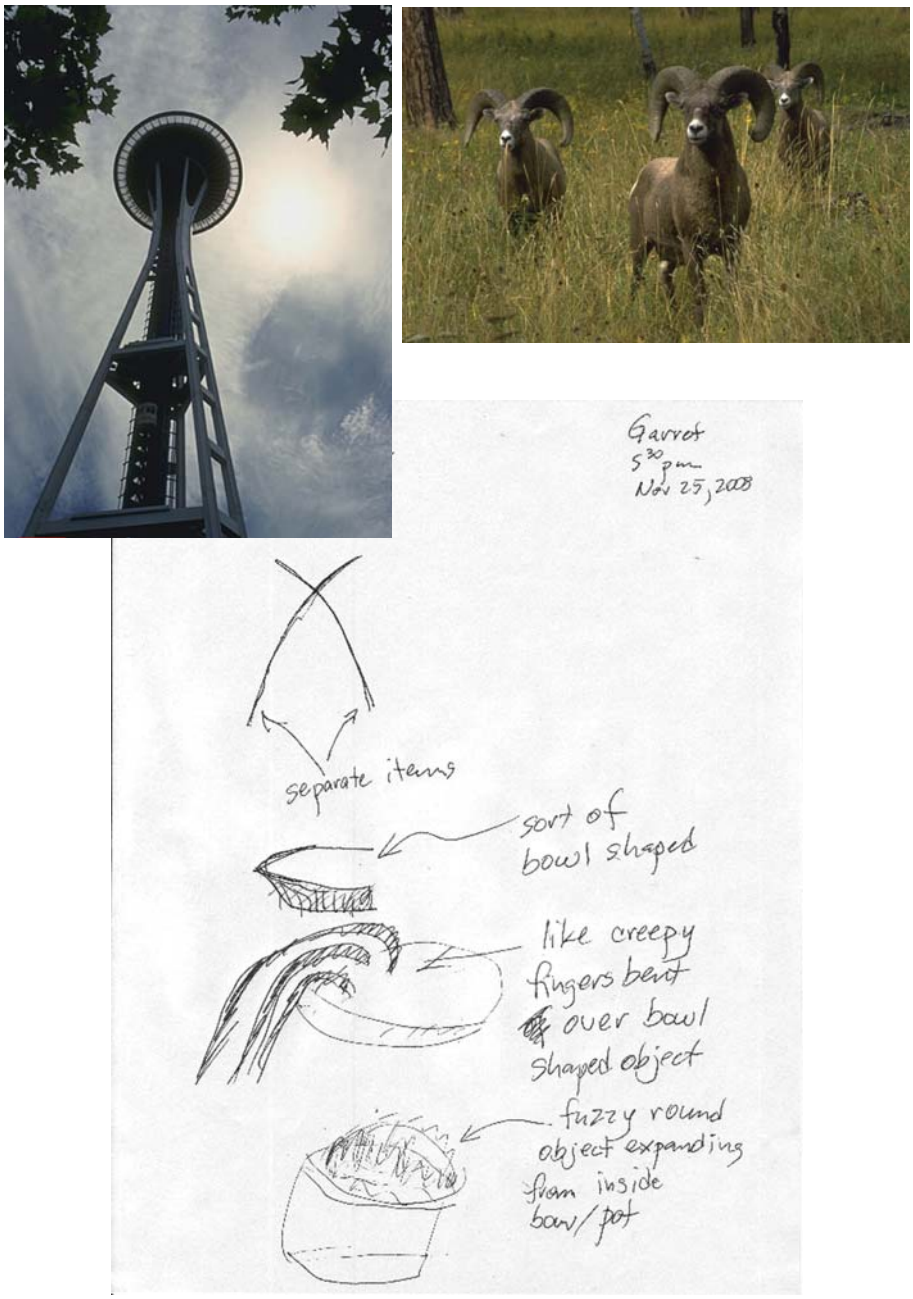

Figure 2. Example session: ambiguous prediction. The photograph that corresponded to the actual outcome was the right-hand one.

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RESEARCH ARTICLE

An Experimental Study for Reproduction of Biological Anomalies Reported in the Hoeven 1999 Crop Circle

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Abstract—This paper revisits the controversial case of a "crop circle," a circular imprint of flattened crop, which appeared in the summer of 1999 in The Netherlands in the presence of an alleged eyewitness. Sampling of plant stems at various locations in the circle revealed a strong lengthening of the growth nodes, with a symmetrical distribution that was aligned with the flattened area itself. This effect has been attributed by some researchers to the effect of electromagnetic energy. In the case of this particular crop circle, the symmetry was indeed identical to the energy distribution of a spherical radiation source, which supported the claim of the eyewitness that a "ball of light" was hovering above the field at the time the crop circle was formed. However, others have suggested the results were simply the effect of sunlight, shadows, or wind over the flattened area, or some simple natural effect related to the fact that the crop in the circle had been flattened. The authors created a man-made control circle and repeated the growth node measurements that were carried out in the original 1999 crop circle using an identical test protocol. It was concluded that the findings in the 1999 circle could not be reproduced and hence remain anomalous.

Keywords: crop circles—gravitropism—BOL model—pulvini—auxins

Introduction

Crop circles are patterns in fields created by the flattening of crops or other forms of vegetation. Their dimensions range from less than a meter to several hundreds of meters, and their designs vary from simple circular imprints to complex geometrical patterns. The number of documented cases of crop circles have increased substantially since the 1970s, whereas their origin remains subject to much controversy. Explanations include the work

of human pranksters, a natural phenomenon, and the work of an unknown intelligence.

In the summer of 1999, Dutchman Robbert van den Broeke reported that he saw a luminescent sphere hovering above a farm field while a crop circle was apparently forming underneath (Haselhoff 2001a, 1999; http://www.robbertvandenbroeke.com). This happened in the village of Hoeven, The Netherlands, and since then the Hoeven 1999 circle has become a famous and controversial case in crop circle history. It is famous because biophysical studies of plants sampled from the circle, performed independently by researchers Eltjo Haselhoff and William Levengood, revealed biological anomalies (Haselhoff 1999, Levengood 2001). These anomalies varied over the circle's area, with a symmetry similar to the radiation intensity distribution of an electromagnetic point source. These findings enticed crop circle "believers," who could finally reference a scientific argument that "crop circles were made by balls of light," and infuriated crop circle skeptics, who stated that the research methods applied by Haselhoff and Levengood were flawed and that their findings had natural explanations. The controversy ignited because Robert van den Broeke's reputation as a self-acclaimed paranormal medium was severely damaged six years later after accusations of fraud during his appearance on several Dutch television shows (http://en.wikipedia.org/wiki/Stichting_Skepsis). The result was a plethora of heated arguments and articles on the Internet, in magazines, and in the scientific literature. However, despite all the energy spent by many to re-interpret the work of Levengood and Haselhoff, little or no effort was put into the creation of new, original data. This article revisits the Hoeven 1999 formation and presents new experimental results in an attempt to find natural explanations for the alleged anomalous findings.

Node Length Increase

One morphological anomaly in cereal-type plants taken from crop circles is an abnormal increase of pulvini (growth nodes). This phenomenon, first observed in 1990 by American biophysicist William Levengood, can be easily observed and measured and is the most consistent and best-documented anomaly related to crop circles (Levengood 1994). An example of node lengthening is shown in Figure 1. The phenomenon can be easily quantified by comparing the (average) node length in any area of interest inside a crop circle with the node length of control samples taken from various locations in the undisturbed standing crop and far away from the circle. An increased node length can sometimes be explained as a natural effect that occurs when cereal-type plants are flattened. Auxins (plant hormones) in the pulvini are responsible for promoting cell elongation, a

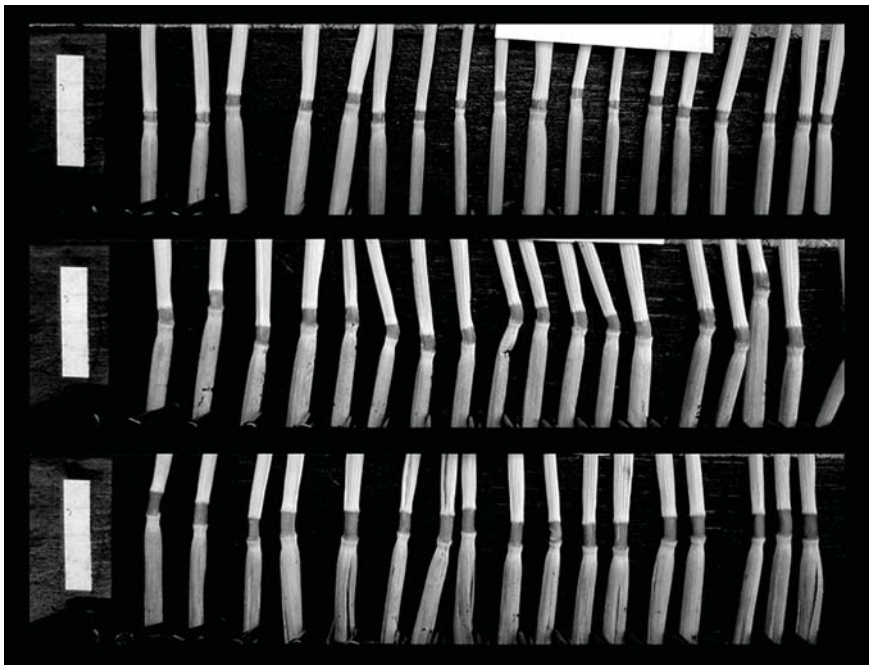

Figure 1. Node length increase in a crop circle. Shown are samples taken at three different locations from the edge (top row) to the center (bottom row) of the Hoeven 1999 circle. The pulvini (dark node section near the middle of each stem) are seen to be markedly and structurally increased in length.

process that is required before the differentiation of a cell. This occurs by promoting the intake of water and increasing the elasticity of the cell. In flattened crops, the vertical migration of auxin to the lower side of the stems caused by gravity increases cell growth rates locally.

This effect is known as *gravitropism*. In control studies, where maturing cereal grain plants were placed in a horizontal position, it was determined that gravitropism can account for a node length increase of ~20% after five days, and up to 40% after 10 days (Levengood 1994). However, reported node length increase in crop circles has been considerably higher (Haselhoff 1999, Levengood 2001, 1994). Moreover, it is not only the length increase itself that is remarkable, but also the manifestation of a structured variation of node length over the circles with a clear correlation to the physical imprints that also extends outside the visibly flattened area. Abnormal increase in node length is also found in remaining standing tufts inside flattened areas, which rules out gravitropism as an explanation.

These findings have sometimes been misinterpreted, with the conclusion that the observed node length increase is apparently not abnormal, because it manifests itself in and around crop circles both in flattened as well as in standing crop. This conclusion is not correct because the normal values for node length in cereal-type crops are well-known numbers in the various stages of a plant's life cycle, and are easily determined by considering the control samples.

Abnormal Node Length Increase in the Hoeven 1999 Formation

In the summer of 1999, Dutchman Robbert van den Broeke claimed he had seen a crop circle appear in a field of barley (*hordeum vulgare*) while a bright pinkish, almost white ball of light was hovering at a height of several meters above the field. He stated that the air around the ball of light was trembling as if it were very hot. After the light faded and disappeared, he inspected the field at the location where he had seen the ball of light and found a crop circle. The crop, the soil, and the air felt physically warm. It should be mentioned that six years later, after his appearances on a commercial Dutch television show, and subsequently, fierce and apparently appropriate criticism was brought forward with respect to Robert van den Broeke's self-acclaimed paranormal gifts (http://en.wikipedia.org/wiki/Stichting_Skepsis, http://www.colinandrews.net/Robbert-van-den-Broeke-Message-Colin_Andrews.html). We will take this for granted at the moment, because van den Broeke's testimony for this particular case took place many years later, whereas his testimony was supported by unambiguous biological changes to the flattened crop that could easily be observed and quantified (Haselhoff 2001a, 1999). We will summarize the main findings here.

On June 13, 1999, six days after its appearance, the Hoeven circle was sampled for independent biophysical analysis by one of the authors (EH). At each of the locations indicated in Figure 2, a total of 20 to 25 stems were cut at their bases, tied together, and labeled. A control set was assembled by taking 8 sets of approximately 20 stems at 9 different locations in the field, away from the crop circle, in the standing crop. After three months, during which the plants were hung in a dry room, the penultimate nodes were measured with the aid of digital photography and a computer program based on a straightforward pattern recognition algorithm. The average value of the control samples, a total of approximately 180 stems, was 2.0 mm. This value was used as a normalization factor, and all node length results in the crop circle were expressed as a percentage of this average control value. The results are shown in Figure 3. Three observations were made:

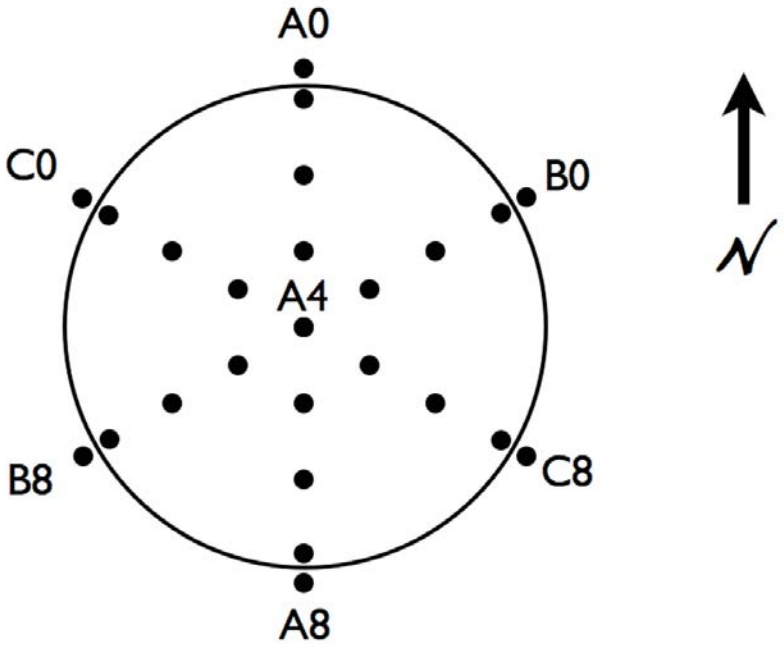

Figure 2. Sampling diagram used for the studies described in this paper. Three traces, A, B, and C, were defined, running through the circle, with the A trace running north–south. Inside the circle, samples were taken at equidistant positions (1.5 m) from edge to edge and labeled 1 through 7. Just outside the circle, samples numbered 0 and 8 were taken for each trace out of the standing crop. By symmetry arguments, samples A4 and C4 were not taken as they are represented by sample A4 (in the center).

1. There was a large increase in average node length in the flattened area. The longest nodes were 214% of the control value (in the circle's center, i.e. sample A4), considerably more than the 20% increase that can be attributed to gravitropism, according to Levengood.
2. The node lengthening was highest in the center and fell off in all radial directions, reaching control levels at the edges.
3. By simple eyeballing, it can be observed that each sampling line reveals a strong symmetry with respect to the circle's center.

The samples of the Hoeven 1999 formation were then sent to William Levengood, without the analysis report. No information was shared until

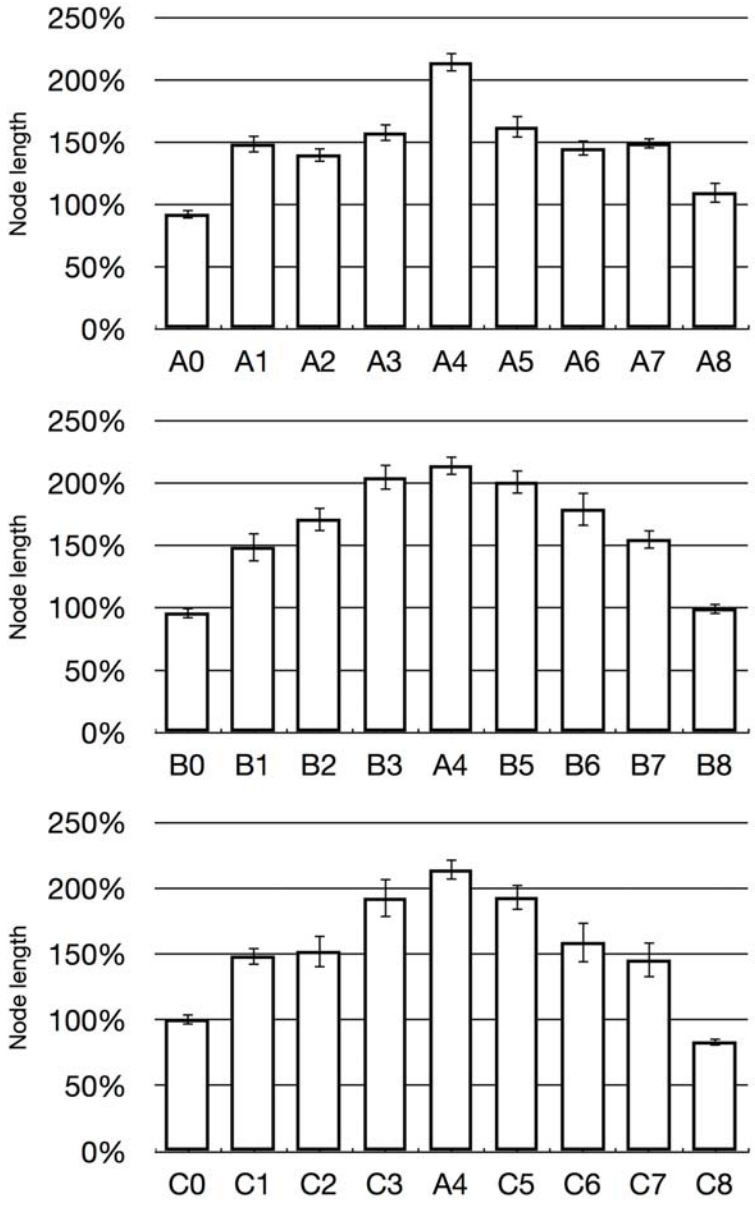

Figure 3. Average node length in the Hoeven 1999 crop circle. Each sample consisted of 20 stems taken at the locations indicated in Figure 2. The vertical bars indicate the average node length as a percentage compared with the average control value of 2.0 mm. The error bars indicate the 95% confidence intervals.

Levengood independently confirmed the measurements. The findings were considered important because for the first time an abundant sampling scheme revealed a clear radial symmetry in node length increase. Earlier studies were mostly limited to a few samples inside the flattened areas.

Model

In an earlier paper (Levengood & Talbott 1999), Levengood hypothesized that node lengthening in crop circles was the result of the viscoelastic nature of plant cell walls in combination with the heat-induced internal pressure caused by electromagnetic energy absorption. Based on the statement of the eyewitness that a "ball of light" was hovering above the field at the time of the circle's creation (Haselhoff 2001a, 1999), and assuming that the node expansion has a linear correlation with the electromagnetic radiation intensity, a simple linear regression analysis was performed for the node expansion and the electromagnetic radiation intensity of a point source located at a height h above the field, with h as a free parameter. In this case, the radiation intensity on the ground depends on the value of h. The best correlation was found for a value of $h = 4.1$ meters, with a Pearson correlation coefficient $R = 0.99$ for the B-trace (Haselhoff 2001a, 1999). These findings supported the hypothesis that the node length increase was caused by a small electromagnetic radiation source "hovering above the field at a height of several meters," as stated by the eyewitness. A revision of earlier node length measurements published by Levengood (Levengood & Talbott 1999) showed that these also correlated well to this ball-of-light model, later known as the *BOL model*. After mutual agreement with the original authors, it was decided to send a Letter to the Editor of the publishing journal (Haselhoff 2001b) suggesting the ball-of-light model as a viable alternative to the Beer-Lambert absorption model, as suggested in the original paper (Levengood & Talbott 1999).

Criticism

The scientific communication about node lengthening and the BOL model was soon embraced by crop circle enthusiasts, who now claimed that scientists had proven crop circles were made by balls of light. These statements were soon counterattacked by skeptical voices questioning the methodology and conclusions of Levengood and Haselhoff. Most of the communications took place on Internet forums and magazines directed to the general public. The traditional controversy around the study of crop circles was reflected by emotionally loaded articles and online discussions that can still be read today. Many of them, however, demonstrated an

insufficient understanding of the essentials of the scientific findings that had been published.

More elaborate objections were presented by several skeptic organizations, in particular the Italian CICAP group. In November 2003, Italian researcher and CICAP member Francesco Grassi requested that Eltjo Haselhoff share the raw data of his field studies, including the data used for his 2001 paper (Haselhoff 2001a). In the summer of 2005, Grassi published a paper in this *Journal* (Grassi, Cocheo, & Russo 2005) in which he stated that the conclusions of both BLT papers (Levengood 1994, Levengood & Talbott 1999) and Haselhoff's Letter to the Editor (Haselhoff 2001b) were invalid. Grassi and coauthors had performed their own analysis on the raw data provided by the original authors and concluded that the claims about the involvement of electromagnetic radiation during the creation of crop circles were not supported by the available evidence. Their primary concerns included the omission of important aspects in the presented BOL model, a questionable sampling strategy with respect to the choice of controls, the criteria for inclusion or exclusion of standing stems, and particularly a lack of statistical significance in Levengood's findings. Summarizing, Grassi, Cocheo, and Russo concluded that the presented findings in Levengood's papers (Levengood 1994, Levengood & Talbott 1999) demonstrated nothing but a difference in node elongation between flattened and upright plants, which could be the result of any flattening mechanism. Their concerns were rebutted by Haselhoff (2007), who stated that Grassi's most important conclusions were the result of an erroneous statistical analysis. This had happened because Grassi had not contacted the original authors prior to publication of his paper. As a result, Levengood's node length values had been incorrectly interpreted as single stem measurements instead of the average values of many stems. Consequently, Grassi missed the fact that standard t-tests had been performed and that all results obeyed the common $p < 0.05$ criteria. In addition, the positions of control samples that Grassi, Cocheo, and Russo had assumed in their analysis were wrong.

Hypothesis

In order to explain the structured node length variation inside crop circles, Grassi suggested "the dynamics of wind near the circle borders and the behavior of circlemakers" (Grassi, Cocheo, & Russo 2005). However, it is not self-explanatory how wind could cause a radially symmetric decrease of node length in a crop circle with the longer nodes in the center, nor how the apparently well-organized node length distribution shown in Figure 3 can be attributed to mechanical plant damage caused by random footsteps. Others have suggested that the center of a crop circle receives more direct

radiation from the sun because the standing crop around the edges will cast a shadow over the flattened area, and that node length increase is somehow related to higher levels of accumulated sunlight over the day. Arguments of this kind are speculative and will never provide definite answers without new, original research.

We therefore hypothesized that the structured node length variation inside crop circles as reported earlier (Haselhoff 1999, Levengood 1994, Levengood & Talbott 1999) is the result of the geometrical symmetry of a flattened circle in a field of vegetation in combination with one or more natural effects caused by sunlight, shadows of standing crop along the edges of the circle, wind, or any other naturally occurring phenomenon, or by mechanical damage caused during the construction of the circle. If this hypothesis were correct, we would expect to find node length increases of comparable magnitude and symmetry as found in the Hoeven 1999 formation in the crop circle made by the authors.

Methods

An experiment was conducted to test the validity of the previously reported findings of symmetric variance in node length within crop laying in fields, reportedly from anomalous mechanisms. A circle was created with a well-known method used by human circlemakers. A wooden board was used, with dimensions of approximately 1.0×0.2 m^2, and a rope was attached to both ends. Holding the rope, this board can be held under one foot to trod down the crop while walking through the field. The circle was created in the same crop as the Hoeven 1999 formation (barley, or *hordeum vulgare*). The barley was at approximately the same stage of maturity (height ~60 cm) and the circle was of the same diameter (9 m) as the Hoeven circle of 1999. For the node length analysis, we used the same test protocol as for the Hoeven 1999 formation. Sampling was performed six days after the creation of the circle using the same sampling scheme and the same number of stems per sample at each sampling location (Figure 3). A control set was assembled by taking 3 sets of approximately 27 stems at 3 different locations in the field, away from the crop circle, in the standing crop. Also, the drying time was identical (three months), after which the penultimate nodes were measured with the same computer program.

Results and Discussion

If the symmetric node length distribution shown in Figure 3 had a natural explanation, one would expect similar observations for the man-made control circle. This was not the case, however, as can be seen in Figure 4.

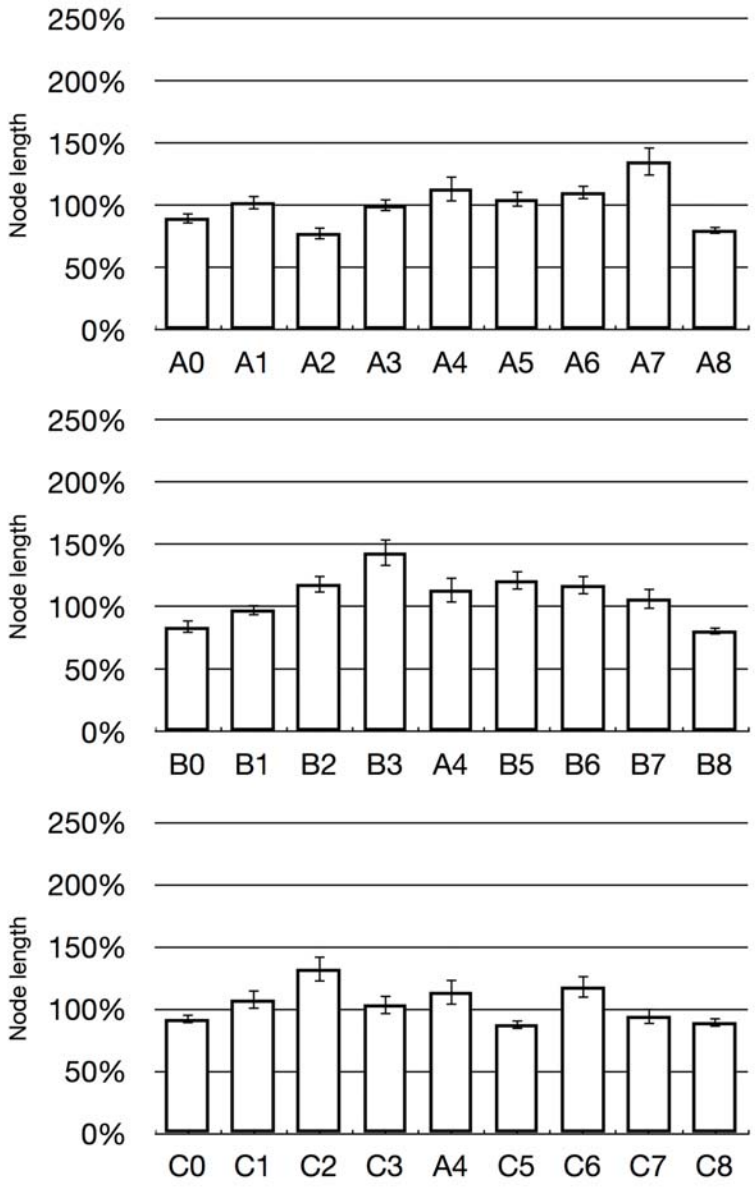

Figure 4. Average node length in a test formation created by the authors. The crop, sampling scheme, and timings were identical to those of the Hoeven 1999 formation shown in Figure 3. The vertical bars indicate average node length as a percentage compared with the average control value of 2.4 mm. The error bars indicate the 95% confidence intervals.

TABLE 1
Comparison of Node Length Parameters of the Hoeven 1999 Formation and a Man-Made Control Circle

	Hoeven (1999)	Control Circle
Average node length of all flattened stems	171%	111%
Peak node length	214%	144%
Position of peak node length	Center of circle	3 meters off-center

The average node length of the control set amounted to 2.4 mm and was used as a normalization parameter. Although in Figure 4 we do observe a significant increase of node length in the flattened crop, none of the 3 graphs show the near-perfect centric symmetry found in the Hoeven 1999 circle. The maximum node length was 144% of the average control length (sample B3). This is more than the 120% reported in Levengood's control study (1994). However, these numbers cannot be directly compared, because Levengood reported the average node length of one single sample, whereas sample B4 represents the largest average value of 25 different samples. In fact, several of our samples also showed an average node length close to 120% (for example, B5: 121%, B6: 118%, C6: 118%), similar to Levengood's findings. When the average node length of *all* flattened stems in our control circle was calculated, we found a node length of 111% of the control value, less than that reported by Levengood. In comparison, the Hoeven formation revealed a maximum value of 171% (average of all flattened stems) and a peak value in the circle's center of 214% of the control value. These results are summarized in Table 1.

The numbers presented above provide a global insight only. In order to gain insight into the regional behavior of node lengthening, the average node length in each of the sampling points in the Hoeven 1999 formation and the control circle can be directly compared in a simple correlation diagram. Figure 5 shows the result. Each point in the graph corresponds to one sampling location. The x-coordinate indicates the average node length of the control sample in that sampling location, and the vertical coordinate indicates the average node length of the Hoeven formation in the same sampling location. It can be seen that the points appear randomly distributed, which means there is no obvious similarity in the geometric distribution of node length in both circles. This is confirmed by a low Pearson correlation coefficient of $R = 0.06$. This demonstrates that whatever mechanism caused the symmetry in the Hoeven node length distribution did not manifest itself in the control circle.

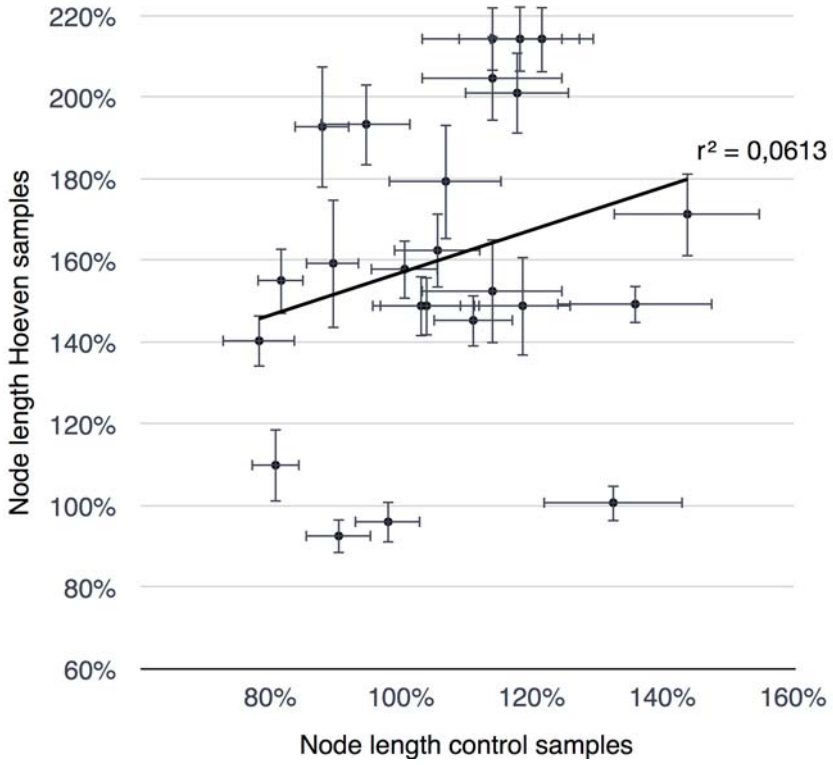

Figure 5. Regional correlation between average node length in the Hoeven 1999 crop circle and the man-made control circle. Each point represents one sampling position, with the horizontal coordinate indicating the average node length in the control circle and the vertical coordinate indicating the average node length in the Hoeven 1999 circle. The error bars represent the 95% confidence intervals for each measurement. The straight line shows the "best fit" for a linear correlation of the two datasets, which shows a very poor correlation with a Pearson coefficient of R = 0.0613.

Despite the fact that the node length distribution in the control circle lacks the clear bell shape seen in the Hoeven case, the B-trace in particular seems to reveal a slight tendency toward longer nodes in the center of the circle (see Figure 4). In order to check if this could be the effect of the varying intensity of sunlight, a straightforward computer model was created to calculate the cumulative solar energy at ground level inside the Hoeven circle, based on the sun's azimuth and elevation in June from sunrise to sunset. Figure 6 shows the result in the form of an intensity diagram. It

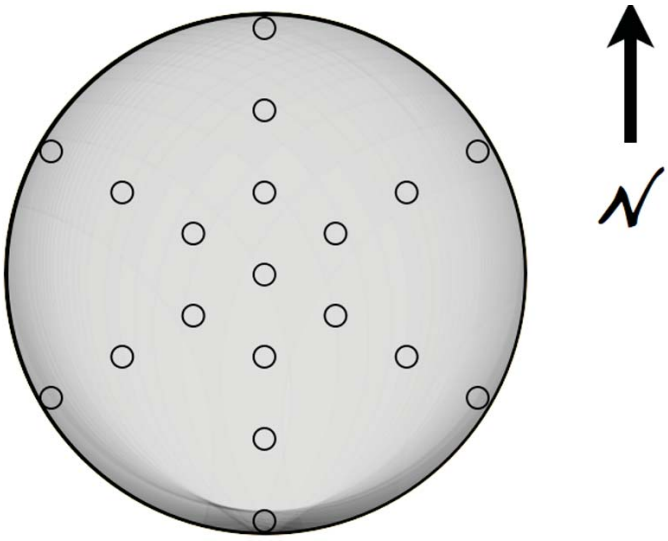

Figure 6. Relative accumulated sunlight displayed in shades of grey in a 9 m diameter crop circle between June 7 and June 13 (the dates between creation and sampling of the crop circle) at the geographical position of the village of Hoeven, The Netherlands. The height of the standing crop, casting a shadow over the flattened area, was measured as 60 cm.

shows the circle's area, with higher levels of accumulated solar energy indicated by a lighter shade of grey. It can be seen that the solar energy has a uniform distribution over the largest part of the circle with only a few shadow effects, predominantly at the southern edge, caused by the standing crop with a measured height of 60 cm. This result can be easily understood by anyone who has ever visited a crop circle on a sunny summer day. The sun shines uniformly almost everywhere in the circle, except for the area near the perimeter on the side the sun is shining from. This part is covered by the shadow of standing crop, which is usually only a small rim because of the limited height of the crop in comparison with the circle's diameter and because of the sun standing high in the sky most of the day.

Figure 7 shows a quantitative display of the relative accumulated solar energy at the positions corresponding to the sampling points of the Hoeven study, normalized to the maximum level. It can be seen that the energy is indeed somewhat lower at the edges, in particular at the southern edges (rightmost in the graphs). As was said before, this is the effect of the shadow of the standing crop along the circle's perimeter. One can only speculate as to what the net effect of direct solar radiation could be. Sunlight is known

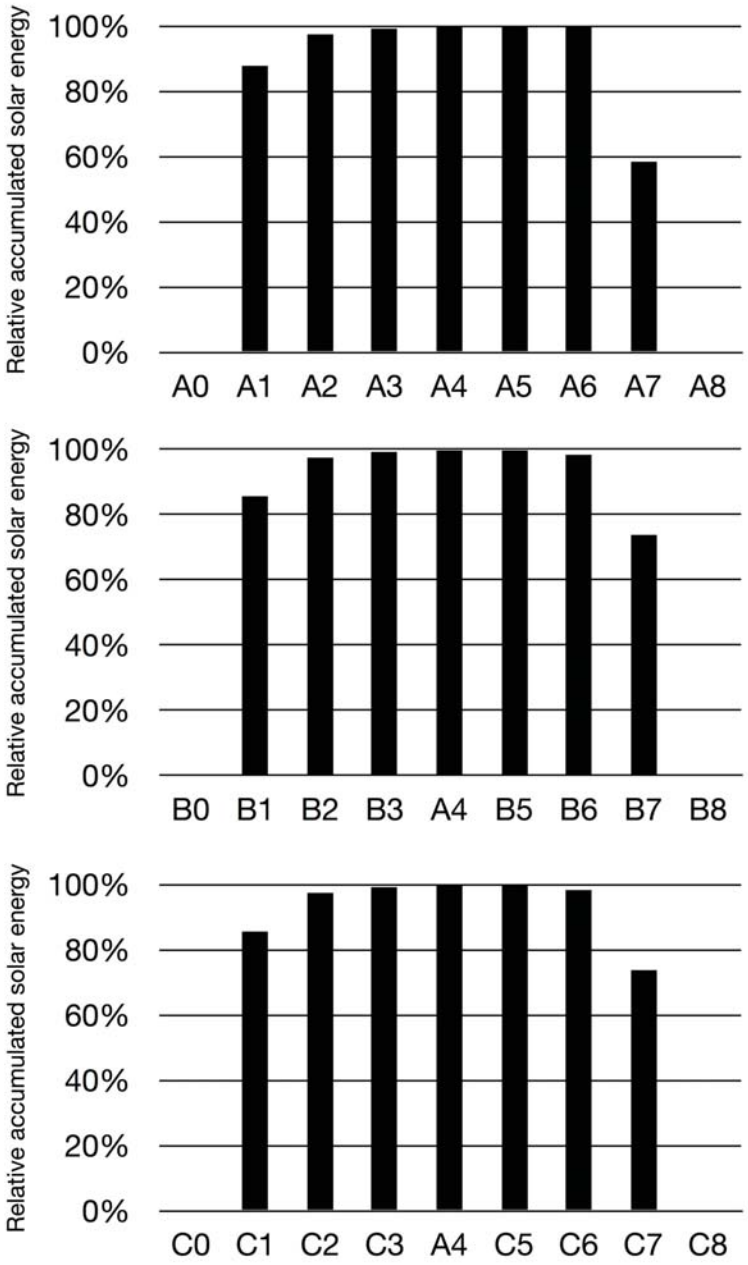

Figure 7. Accumulated solar energy at the sampling positions of both crop circles, normalized to the maximum value in the center.

to lower auxin levels and reduce soil humidity, which reduces growth, but it also increases the local temperature, stimulating growth. However, if there were a strong effect caused by sunlight it should be reflected by a structural difference between the node lengths measured in sample points A7, B7, and C7 compared with the samples numbered 1 through 6 in the rest of the circle. In Figure 4 it can be seen that this was not the case. This is confirmed by the graphs shown in Figure 8, which show the average node length in each sample location of both the Hoeven circle and the control circle as a function of the accumulated solar energy at that sample location over a day. It can be seen that for both crop circles there is no obvious dependency between node length and accumulated solar energy. It was concluded that sunlight has no significant effect on node lengthening in crop circles.

Conclusion

An experiment was conducted to test the validity of previously reported findings of node length variance within cereal crops laying in fields, reportedly from anomalous mechanisms. Therefore, a crop circle was created in The Netherlands by flattening the crop with the well-known board and rope method. The time of the year, the type and maturity of the crop, and the circle's diameter were identical to those of a formation found in Hoeven, The Netherlands, in 1999. The latter circle was allegedly created by unknown forces in the presence of a ball of light.

Stems were taken from the control circle for node length measurements. A sampling scheme was applied that was identical to the one used earlier for the Hoeven 1999 formation so that all results could be directly compared. In addition, the time span between creation and sampling and the time span between sampling and performed node length measurements were identical. The control circle did not reveal the strong radial symmetry in node length distribution that was found in the Hoeven 1999 formation. Moreover, the average node length increase was significantly less (11%, compared with 71% for the Hoeven formation), which was attributed to the effect of gravitropism, in agreement with earlier findings by other researchers. A computer simulation demonstrated that differences in accumulated solar energy over the circle area have no effect on node lengthening, which also excludes the effect of sunlight as a potential cause for the Hoeven anomalies.

Clearly, it was impossible to reproduce exactly all environmental circumstances during the six days between the creation and the sampling of the Hoeven 1999 formation. First, because these are largely unknown, but also because it is not straightforward to reproduce wind, temperature, humidity, soil composition, sunlight, and all aspects that are known to effect plant growth, over a period of six days. Future control experiments could

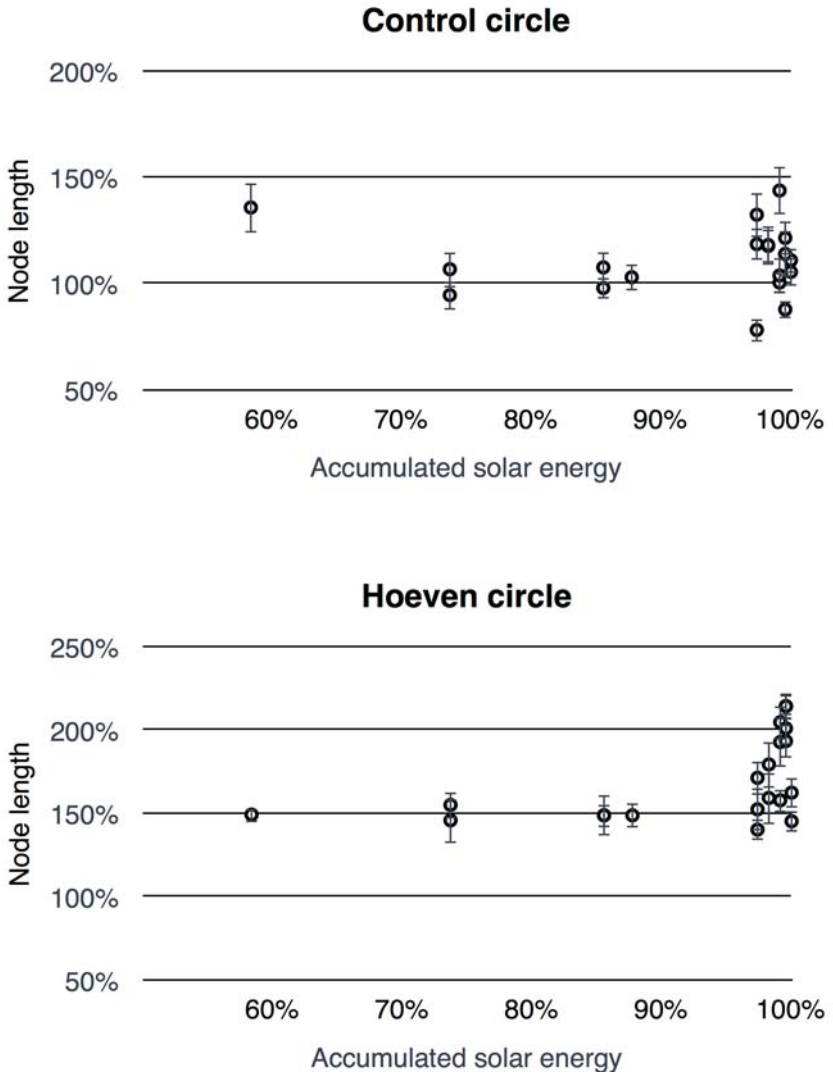

Figure 8. Correlation between node length (vertical axis) and relative accumulated solar energy (horizontal axis) in the control crop circle (top) and the Hoeven 1999 circle (bottom).

be improved by creating circles in a controlled environment that reproduces temperature, humidity, and wind based on meteorologic records of the environment of the circle to be reproduced. In addition, such an experiment could also be executed in blind fashion, meaning that the samples are measured by independent researchers with no knowledge of the sample positions. This would be particularly important when node lengths are measured by hand, and not automatically by a computer as was the case in our experiement. However, by creating an identical control formation at the same time of the year, in the same geographical area, in the same type of crop, and by applying an identical test protocol, it is fair to assume that "obvious causes" for the observations in the Hoeven 1999 formation should also be revealed to some extent by our control formation. This was not the case.

To summarize, the node lengthening in the Hoeven formation was experimentally duplicated with significant success. However, no support was found for the hypothesis that the node lengthening in the Hoeven 1999 circle was created by natural causes such as gravitropism or the effect of wind or sun. It is therefore concluded that the node lengthening found in the Hoeven 1999 formation could not be explained and remains anomalous.

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RESEARCH ARTICLE

Pre-Columbian Transoceanic Influences: Far-Out Fantasy, Unproven Possibility, or Undeniable Reality?[1]

Stephen C. Jett

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Abstract—The standard view has been that once the Americas were settled via Beringia, the human denizens of the Western Hemisphere were essentially cut off from interaction with peoples of the Old World. Here, I present multidisciplinary evidence that the hemispheres were, instead, interconnected by repeated voyages over millennia, resulting in profound influences on both sides of the oceans. I first examine arbitrary cultural traits (cosmology, calendrics, and art) and complex technologies (barkcloth/papermaking, the blowgun, metallurgy, weaving and dyeing, ceramics), then comment on likely relationships between certain Old and New World languages. A large number of cultivated plants and one or two species of domestic fowl, which could not have crossed oceans without human carriage, were shared between the hemispheres before—in most cases, long before—1492. Several tropical Old World human intestinal parasites that could not have entered the Americas via Beringia were also shared, some remarkably early. The geographical distributions of certain distinct human genetic markers imply important inputs to Mesoamerican and Andean populations from more than one overseas source. Studies of climatology, oceanography, and traditional watercraft and navigation show that early vessels were capable of ocean crossings via certain routes. These converging, essentially independent lines of evidence imply that we can no longer assume that the cultures of the two hemispheres evolved in parallel fashion in isolation from one another and according to "laws" discoverable through comparative studies.

Keywords: culture—cultural diffusion—culture change—comparative studies—technology—cultivated plants—intestinal parasites—human genetics—ocean crossings—traditional watercraft—traditional navigation

Of course, America had often been discovered before Columbus, but it had always been hushed up.

— Oscar Wilde

Introduction

The standard view of the human history of the pre-1492 Americas has long included the idea that when sea levels were lower during the last Pleistocene ice age and the present Bering Strait was dry land, one or a few migrations of pedestrian Asian hunters, following herds of game animals, walked from Asia into the unpopulated North American continent and quickly spread to virtually every inhabitable part of the hemisphere. According to this view, when the ice sheets melted and the seas rose and created a water barrier between Siberia and Alaska, owing to a lack of capable watercraft, New World peoples were essentially cut off from communication with those of the Old World, and the multifarious Native American cultures encountered by Leif Eiríksson and his Norse cohorts and by Christopher Columbus and his successors had all evolved in the Western Hemisphere from their Upper Paleolithic predecessors, without significant further input from elsewhere. Recently, agreed-upon dates of initial human entry have been pushed back a bit and the likelihood that at least some of these earlier arrivals traveled coastwise in boats rather than only overland on foot has gained numerous supporters (e.g., Dillehay 1997, 2000, Dixon 1999, Nichols 1992, 2002, see also Jett 2007a, Erlandson & Braje 2011)—although in the absence of watercraft remnants, some scholars remain reluctant to reject exclusively pedestrian movements (e.g., Meltzer 2009:130). However, aside from acceptance of a short-lived and inconsequential eleventh-century A.D. Norse presence in and around Newfoundland, the notion of pre-Columbian transoceanic contacts—to say nothing of multiple and important interinfluences, beginning in the distant past—remains almost universally, and often derisively, dismissed by archaeologists, historians, and—to a lesser degree—geographers, especially in academic America (see, e.g., Jett 2006, Kehoe 2003, 2010). Thus, the native societies of the New World are widely perceived as 1) having developed in complete or virtual seclusion and, therefore, 2) when compared with the societies of the Old World, conveniently provide a minimum of two independent cases of development from which one may generalize about universal processes of cultural evolution (cf. Trigger 2003). This isolationist stance is often labeled "independent-inventionism." Inventionists perceive multiple, historically unconnected duplicate innovations as being the principal source of cultural similarities around the world (see, e.g., Trigger 2003).

A minority contrary opinion was actively espoused during their lifetimes by, among a certain number of other professionals, eminent University of California, Berkeley, geographer Carl O. Sauer (1889–1975), by Sauer students George F. Carter (1912–2004) and Carl L. Johannessen, and by Stephen C. Jett, a Carter student (Gade 2003/2004; Jett 2000b, 2007b), as

well as anthropologists/archaeologists such as Gordon F. Ekholm (1909–1987), David H. Kelley (1924–2011), Paul Tolstoy, Wolfgang Marschall, and Alice Beck Kehoe, by the art historians Robert Heine-Geldern (1885–1968), Douglas Fraser (1929–1982), Terence Grieder, and Paul Shao, and by linguists Mary Ritchie Key (1924–2003), Mary LeCron Foster (1914–2001), Cyrus H. Gordon (1908–2001; see Gordon 2000), Bede Fahey, and Brian Stubbs. That alternative view is that not only did pre-1492, pre-1000 contacts across or around the oceans take place, they began millennia ago and were numerous and highly influential; therefore, any general theory concerning universal processes of cultural evolution that rests on the supposition that the civilizations of the Western hemisphere emerged and evolved in splendid isolation is based on a fundamental misapprehension. This point of view is a form of what is commonly termed "diffusionism." Diffusionism posits that almost all cultural change—and cultural content—is a result of interaction and cultural exchange among societies, not of repeated autochthonous innovations (see, e.g., Jett 2000a). Thus, inventionists see humans as relatively creative, diffusionists as more imitative.

The differences between these two viewpoints have generated some of the most prolonged and acrimonious debates in scholarship, particularly among archaeologists (see Fingerhut 1994, also Davies 1986).

The Critics

There are many reasons for the prevalent resistance to this idea of early and important transoceanic influences; some are fact-based, some subjective. I do not propose to detail these reasons here but, rather, will concentrate on assessing whether contacts in fact took place, which would have provided opportunities for cultural exchanges between the hemispheres.

Still, one does need to begin by gaining some idea as to what the concept of influential early ocean crossings engenders in the way of negative academic opinion. Major impediments to entertaining the notion of meaningful interconnections include beliefs that 1) owing to inadequacies of watercraft and of navigation, pre-medieval crossings of oceans were impossible except, perhaps, under extraordinary circumstances, and would have been too rare to have been influential; 2) if we accept diffusionism, we lose the pair of independent emergences of civilization that permit generalizing about cultural evolution (see above); and 3) diffusionism is racist and culturally insulting because it robs peoples of credit for inventive creativity and because it was used to justify colonialism by alleging that there was but one font of civilization and that that font's exclusive heirs were Western Europeans, who had a right and a duty to manage and civilize the Natives (e.g., Blaut 1993; see Jett 2006). Note that beliefs 2 and 3 say

nothing about the *reality* of interinfluences, only about the *palatability* of the concept, and they will therefore not be addressed here. Belief 1 is treated below.

Many scholars consider the idea of influences on pre-Columbian American cultures from across the Atlantic or the Pacific to be so implausible and/or unacceptable that they perceive it as the "far-out fantasy" of this article's title, and lump such a notion with those of sunken continents, creationism, and certain other religious/mystical beliefs (cf. Wauchope 1962), and with UFOs and space aliens' having sparked human civilization. The characterizations "cult archaeology" and "pseudoarchaeology" are employed (e.g., Cole 1980). Extremist critics speak of "off-the-wall," "pseudoscientific," "rogue professors"; these are defined as academics who may look like professors and write like professors and in fact hold the title of Professor, but who in fact play fast and loose with the evidence and are dangerous because they do so with all the trappings of scholarship (Williams 1991, p. 270, referring to George Carter and his ilk; see also Feder 2005, Wilson 2012).

Critical commentators who purvey these kinds of characterizations typically write entertainingly but with anger and/or irony and sarcasm rather than with the neutral language that is supposed to distinguish scholarly discourse. They ask, 'If there were contacts, then where are the artifacts?' but tend to dismiss every pre-Norse piece of positive artifactual evidence for contact as being some sort of fake or fraud or, at the very least, the object of misinterpretation.[2] Some of these detractors are distinguished scholars, so one must necessarily consider whether they ought not, therefore, to be taken very seriously concerning this issue. The only way to assess that question is to look at the evidence itself—something that hostile critics seldom do in depth, because they perceive such an effort as being a ridiculous waste of their time and a distraction from "fruitful" scholarship such as comparative evolutionary studies (e.g., Rowe 1966; see response by Jett & Carter 1966).

It is true that meaningful assessment of the issue requires time and effort. It also requires a broad approach: The evidence of archaeology alone is insufficient, and one must also look closely at aspects of climatology, oceanography, watercraft and navigation, linguistics and epigraphy, ethnography, ethnobotany, ethnogeography, human genetics, medicine, and so forth. Here, I take a close (if necessarily brief) glance at what the combined evidence of culture and biology may tell us concerning the reality of the postulated contacts and influences, with particular attention to relatively recent findings (cf. Jett 2003). The examination commences with a summary of some of the classically cited cultural evidence. It then proceeds to consider relatively recently forwarded relevant linguistic data,

and ends with a review of the rapidly developing biological evidence for contacts, which has been dramatically raising the debate to a new level.

The Evidence of Arbitrary Cultural Traits

"Diffusionists" may be defined as those who have concluded that humans are more "plagiarizers" than inventors, and that in most cases the contents of individual cultures are primarily the product of foreign influences rather than a consequence of independent in-situ internal innovative processes (see Linton 1936, 1971 for classic statements). Diffusionists tend to look to culture itself for evidence of cultural exchanges. In the context of transoceanic-contacts studies, diffusionist scholars have long been impressed by a variety of specific cultural traits and complexes shared by certain societies on the two sides of the oceans but absent in the northern areas over which ice-age humans are supposed to have migrated from Eurasia to America. There follow herein some explicit examples of such cultural phenomena, on which cultural historians focus, and which have generated diametrically opposed interpretations. I begin with those traits that are particularly arbitrary—what could be termed cultural oddities, not being called for, elicited by, or even favored by nature, by the medium employed, or by universal psychological characteristics or social relations, and which, therefore, seem particularly unlikely to have been "invented" more than once, especially in combination with each other. The case for diffusion is strengthened 1) when the traits concerned are complex rather than simple and easily arrived at; 2) when the traits display limited geographical distributions and thus must not be "obvious" inventions potentially universally thought up; 3) when multiple commonalities are shared between the potential donor and recipient regions—geographic clustering—the probability of the combination's being independently arrived at being significantly lower than the combined probabilities of independent invention of the individual traits; and 4) by temporal overlap of the traits concerned between the two areas, ensuring the chronological possibility of influence from proposed donor area to postulated recipient area. The presence of a developmental sequence over time in one of the regions and the abrupt appearance of the fully developed trait in the other region can suggest which area is the donor and which the recipient. (On these matters, see Jett 1971.)

Cosmology, Religion, and the Calendar

The world's many societies manifest a number of different concepts regarding the origins and layout of the cosmos and how to worship in the context of those concepts. Despite this diversity, striking similarities have

been recognized between the belief systems of ancient southern and eastern Asia, especially pre-Buddhist China, on the one hand, and pre-Columbian Mesoamerica on the other. Both realms saw the cosmos in terms of a multilayered universe with division of the earth's surface (or the domain) into four cardinal–directional quarters (plus, sometimes, a center, a zenith, and a nadir), each of which was assigned a color, a season, a deity, an animal, a wind, an element (as in air, water, fire, wood, and earth or metal), and so forth. Although the specifics of the color-directional systems varied from group to group, in certain cases the identical colors were assigned to the identical directions on the two sides of the Pacific (Nowotny 1969, Jett 1983:379–380). Independent inventionists perceive even these specific and arbitrary commonalities as emerging entirely separately owing to humans' psychological universals and limited perceptual possibilities, while diffusionists view them as strong evidence of historical connections. A third alternative is to see the similarities as being a result of parallel development from common ancient Paleolithic roots (e.g., Chang 1992).

An elaborate timekeeping system is a part of this complex. The Université de Montréal archaeologist Paul Tolstoy provided a thoughtful statement in this connection:

> . . . the series of 20 day-names on which the Mesoamerican calendar is based . . . shows multiple and elaborate correspondences with the Eurasian lunar zodiac and its associated deities as identified in China, India, and the Near East. . . . This system's mere presence in Mesoamerica, in view of its arbitrary features, would seem persuasive evidence of contacts between the higher civilizations of both hemispheres. Moreover, it is but one element of an elaborately networked set of correspondences that includes mathematics (e.g., position numerals, the zero), calendrics (e.g., permutation time counts), communication devices (e.g., writing, books, papermaking), and conceptions of the world (former and present mythological worlds, world quarters and their colors, the latter with such diverse ramifications as the patolli/parchise game and state administration). To these may be added ritual practices (various forms of sacrifice, the use of water and incense, the *volador* [pole-swinging] ceremony); symbolism based on felines, snakes, and trees; and insignia of rank such as fans, parasols, and litters. (Tolstoy 1974:132–133)

The University of Calgary Mayanist David H. Kelley (1960, 1972, 1974, 2008, 2011–2014), Showa Women's University East Asian linguist David B. Kelley (1995, 2008, 2011–2014), and others who have studied calendar systems have pointed out commonalities involving a combination of deities and their attributes, associated animals and concepts, and order of occurrence in sequence. For example, D. H. Kelley identified seven in-order primary correspondences between the gods of the 28 Hindu lunar mansions

and the deities of the 20 Aztec days, and nine in-sequence correspondences between the Mesoamerican day names and Asian lunar animals.

Regarding rain worship specifically, Dennis Wing-sou Lou (1957; cf. Shao 1998) pointed out that China and Mesoamerica had the following, often arbitrary, beliefs and practices in common: 1) serpent deities (dragon/feathered serpent) associated with sky and water, the cardinal directions, and the latter's colors and winds; 2) twin-snake rain deities who are also gods of Heaven and Earth and are the precursors to humans; 3) two forms of torch-bearing Chinese thunder gods and Mayan rain gods (*chac*s), one with a human head and the other with a long-nosed head (of an elephant, in China), who ride serpents, are associated with the directions and with the S or reverse-S sign, which in turn is associated with a + sign; 4) the association of frogs with rain; 5) the concept of the raven of the sun and the rabbit/hare in the moon, along with a woman in the moon associated with medicine and childbirth; 6) four-directional rain-worship altars, with a directional rain god and its element worshiped according to the season, coupled with the gathering of snakes and the performance of a serpent dance; 7) large bonfires whose smoke elicits rainclouds; 8) mountains as rain deities; 9) the plaiting of mats in connection with rain ritual; 10) human sacrifice, including by heart excision, drowning, burial alive, and immolation; and 11) dog sacrifice.

In a summary statement, D. H. Kelley (1974:136) gave the following as the principal Asian cosmological/religious trait constellations for which there were corresponding ones in the Mesoamerican system:

(a) the Eurasian animal cycle
(b) the Hindu deity cycle
(c) the system of world ages and their associations with colors
(d) Hindu and Greek four-element theory and relationship to the world ages
(e) the use of an astronomical and cosmological era base
(f) the association of cataclysmic catastrophes with the era base, with planetary revolutions, and with eclipse calculations
(g) the use of zero in calculating the era base
(h) the use of the nine-day planetary week
(i) iconographic items such as the *makara*/*cipactli* [composite-monster] parallel and the *makara* tree.

It takes a confirmed skeptic indeed concerning contact to see this kind of complex, detailed, and arbitrary correspondence as something naturally and independently arising here and there. Although in light of the multitude

of co-occurring arbitrary concepts involved, historical transpacific—or circumpacific—connection seems to me to be the only plausible explanation, such skeptics are nevertheless numerous. Some see mere coincidence in these commonalities, others the manifestations of universal human psychological characteristics dealing with similar materials, questions, and challenges. Harvard's late K. C. Chang (1992), the pre-eminent American interpreter of Chinese archaeology, felt that Chinese/Mayan resemblances reflected the common ancient Asian background of the two cultures, both of which, over time, built similar conceptual edifices on this assumedly shared pre–Bering-migration Paleolithic foundation. "[W]e can empirically establish a Maya–China cultural continuum based on real and powerful archaeological and textual data," he observed. All that notwithstanding, his mind was made up: "no amount of illustration can convince us that these similarities were the result of cultural contact . . ." (p. 218).

Art Styles

Artistic style is another area of culture that often involves arbitrary traits. One pair of comparable decorative styles is the Eastern Zhou/Chin style of Bronze Age China and the Tajín style of late pre-Columbian Veracruz, Mexico (Figure 1), whose close resemblances have long been recognized. The following observation comes from the pen of the prominent architectural and art historian of pre-Columbian Mesoamerica and talented artist Tatiana Proskouriokoff:

> Many observers have noted striking parallels between some of the Veracruz designs and those that were used on early Chinese bronzes. Not only are the two arts very similar in general conception, with their dragon forms almost lost amid intricate tracery, but there are [also] specific and complex forms in the two styles so nearly alike that it is hard to believe that they were independently invented. (1971:571)

Having said this, Proskouriokoff then explicitly backed off from selecting an explanation for the similarities. A major difficulty concerning these two styles is that they are separated by half a millennium of time, and no temporally intermediate similar examples have been discovered. This, say isolationists, shows that similar styles can and do emerge independently. Yet diffusionists contend that these styles' detailed correspondences cannot conceivably be independent, so an ancestral Tajín-like style must have endured the time gap concerned, perhaps in perishable materials that did not survive (e.g., Heine-Geldern 1959).

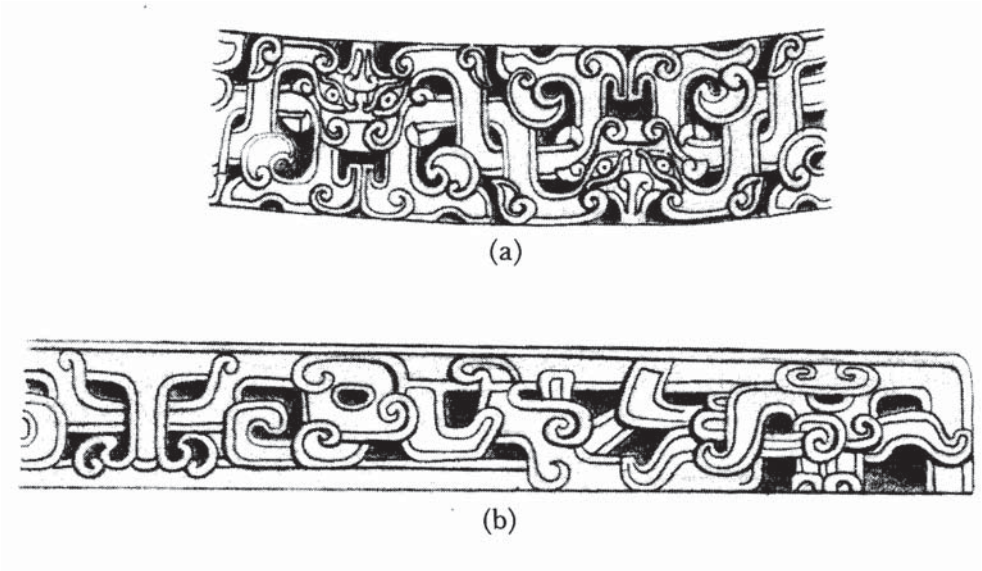

Figure 1. Drawing (by Rulon Nielson) of Chinese and Mexican interlaced, outlined-band designs: (**a**) from a Zhou Dynasty bronze vessel, first millennium B.C.; (**b**) from a stone frieze, El Tajín, Vera Cruz, circa A.D. 900. Note the semi-camouflaged monster masks (from Jett 1983:357).

Technological Systems as Evidence of Contacts

Whereas the emergences of technological systems are constrained by those systems' purposes and by physical law and are therefore far less arbitrary than are cosmological concepts and iconography, nevertheless some technologies that are shared between the two hemispheres are so complicated and/or peculiar that it is surprising that they were *ever* devised, anywhere; it would be more than doubly surprising to discover that they arose twice, independently, since particular complex enabling sets of environmental, economic, cultural, and historical circumstances are never closely duplicated. Furthermore, geographically these technologies appear in coherent distributions, not randomly here and there, suggesting that historical diffusion has been at work rather than disparate invention; an outward expansion of the complexes over time can often be demonstrated archaeologically, as would be expected from dispersal outward from a hearth of innovation. Therefore, diffusionists tend to think in terms of there having to be a historical relationship among the geographically separated occurrences of any such technology. I next describe five such technological complexes that have been studied thoroughly.

Bark-cloth and Primitive Paper Manufacture

Certainly the best-known example developed by a student of possible transoceanic transfers is the Université de Montréal Mesoamericanist archaeologist Paul Tolstoy's analysis of the making of bark-cloth—known as *tapa* in the Pacific islands—a paper-like material produced by felting the inner-bark fibers from certain kinds of trees, particularly those of the mulberry family; in its advanced form, a primitive paper is the product. Bark-cloth is used for clothing and, in its refined form, for writing on. In the opinion of the late American Museum of Natural History Mesoamericanist archaeologist Gordon F. Ekholm (1955:104) and a number of others, "bark cloth manufacture has, in general, the appearance of something that is not a very obvious thing; it is not a discovery which would be likely to be made more than once . . ."

Manufacture of bark-cloth involves the following steps: 1) stripping bark from an appropriate tree; 2) usually soaking or retting the bark to remove the sap; 3) separation of the outer bark from the inner to obtain the bast (phloem) of the inner; 3) beating the bast to felt its fibers; 4) optionally, boiling in an alkaline solution to facilitate firmer felting by 5) a second beating; 6) drying; plus, optionally, 7) polishing and 8) sizing (Needham & Lu 1985:51–53). Tolstoy (1963, 1966, 1972) ascertained that of the 121 analyzable traits found within the world's bark-cloth and primitive papermaking industries, 92, or 76%, were shared between Southeast Asia and Mesoamerica. Forty-four of these shared traits are

> not required by any of the other steps in the procedure of which they are part or by the goal itself of making bark-cloth . . . Even when essential, many of these traits are still but one of several known alternatives . . . [37 of the traits] are redundant, i.e., they co-occur with their alternatives, thus casting doubt on their comparative advantage or determination by function. (Tolstoy 1972: 385)

Bark-cloth–beating implements of the Indonesian island of Sulawesi, which derive from a type originating in Guangdong, China (Cameron 2008:206–07), and those of pre-Columbian Mexico are essentially identical (Figure 2). In addition to these bark-cloth manufacturing commonalities is the making of screen-fold books from the material produced by both the Maya of Mesoamerica and certain peoples of Thailand and Burma in Southeast Asia (Grieder 1982:173, 175–77).

The Blowgun

In the context of examining possible early Indonesian influences in tropical America (Jett 1968), the University of California, Davis, cultural

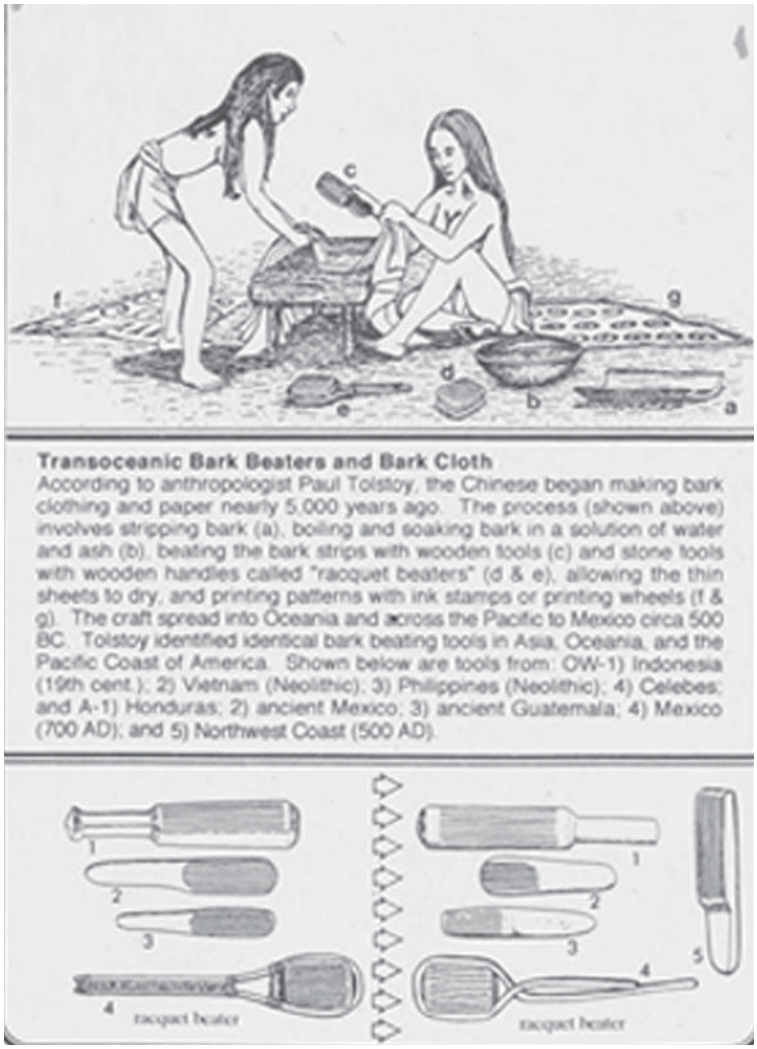

Figure 2. Drawing of Southeast Asian and American bark-cloth beaters (by Gunnar Thompson 1992:224).

geographer Stephen Jett conducted a global review of blowguns—those tubular weapons with which hunters shoot darts or pellets at small game (Jett 1970, 1991)—which archaeology shows to be pre-Columbian in both hemispheres. The developed blowgun is closely associated with Indonesian speakers in the Old World but is also widely distributed within the tropical

and subtropical Americas, where its greatest elaboration centers on the region where Ecuador, Colombia, and Peru conjoin.

Jett concluded that there is compelling evidence of a historical relationship between these two blowgun complexes. His global analysis identified 55 traits for comparison. Of these, 32, or 58%, were shared between Island Southeast Asia and tropical South America. But whereas 82% of the 39 elements described for the Americas also appear in Asia, the Old World complex is more evolved and only 67% of its traits also occur in the Western Hemisphere—which implies an Asia-to-America direction of transfer, prior to later elaboration in Indonesia.

Here are some of the more notable characteristics held in common: 1) single-tube weapons using a naturally hollow plant stem or one whose pith has been pushed out; 2) single-tube blowguns created by splitting a length of wood, incising half of the bore into one of the split halves and half into the other, then gluing and binding the two halves together; 3) double-tube blowguns, with one tube inside the other; 4) sights, mouthpieces, and muzzle rings; 5) projectiles in the form of clay pellets and darts, with fiber wadding for the latter carried in a gourd; 6) bamboo dart quivers tied to the waist by a cord; and 7) preparation and use of a cardiac tree-sap dart poison and of a poison made from lianas of genus *Strychnos*, for which salt is a supposed (but not real) antidote.

Metallurgy

In elaborated form, the technology of metal-making is exceedingly complex. Even in fairly basic form, the *chaîne opératoire* of metal-artifact production involves the following: 1) prospecting, by inspection of minerals, plant growth, and water color and taste; 2) the collecting or mining of ore, mining requiring manufacture and the use of hammers and picks, excavation of pits, shafts, drift tunnels, etc.; 3) ore-processing or benefaction, which involves crushing with tools and then sorting; 4) acquiring materials for, and building, drying, and preheating a crucible or furnace of the correct dimensions and providing the crucible/furnace with draft, as either wind or as breath blown through properly placed and employed blowpipes or as air-flow generated with bellows, which require previous construction; 5) finding and selecting the appropriate type and size of fuel (usually, charcoal from certain woods, which requires its own long and elaborate preparation and even woodland-management), placing the fuel in correct position and proportion to the ore, and timing the addition of more ore (additionally, in the case of sulfide ores, roasting to replace the sulfide radical with oxygen prior to smelting); 6) reduction to free metal by smelting with the carbonaceous fuel for the proper length

of time at the proper temperature; 7) refining (re-melting and removing remaining impurities, preferably in the presence of charcoal to prevent re-oxidization); 8) alloying (if required), with control of the proportions of two or more ores or metals; 9) annealing (cold-hammering, perhaps alternated with heating); 10) construction of a mold and then casting, if required; and 11) smithing, including hammering, grinding, polishing, and decorating (optional) to produce the finished artifact. Considerable organization and overall guidance are needed to achieve success, and most of the individual operations require experienced specialists (de Barros 1997, Ottaway 2001, Roberts et al. 2009). This complicated set of physicochemical procedures is surely not something that would naturally be stumbled upon again and again, even incrementally (Forbes 1950:12).

Beyond the fundamental technology just described is the sophisticated elaboration of it, as well as the forms of the objects produced. The Austrian art historian Robert Heine-Geldern (1972) addressed these matters with regard to two adjacent metallurgical areas of pre-Columbian northwestern South America. Technological commonalities with Southeast Asia included copper-ore smelting, the wind furnace, the blowpipe, granulation, solid and lost-wax casting, the manufacture of a copper–gold alloy (*tumbaga*), tin-bronzemaking, surface-coloring of gold–alloy objects by chemical processes (*mise-en-couleur* and wash gilding), and soldering. Heine-Geldern compared the forms of metal objects from these areas with those of the Dong Son (Đông Sơn) culture of northern Vietnam. Regarding the Colombian–Ecuadorian region, he found in common with Southeast Asia: small globular bells, openwork scenes framed with simple or plaited rope designs with spiral appendages plus dangles, frogs decorated with the plait motif, and a stress on double spirals (Figure 3). In Peru, he noted other traits shared with Dong Son: socketed axes and spades, tweezers, bracelets or rings whose open ends form spirals, and S scrolls. His conclusion was that, somehow, Dong Son metallurgists had sailed to South America two millennia ago. West Mexico, which had connections by sea with Ecuador, participated in this metallurgical complex as well (Heil 1998).

Weaving and Dyeing

Although we take loom-woven textiles very much for granted today, they are, as one archaeologist observed, "one of the strangest inventions ever produced by man" (Rubín de Bobolla 1964:3), an invention that seems unlikely to have arisen in multiple times and places.

Cloth-weaving on hand looms is a technological complex that involves a number of stages, devices, and procedures: 1) domesticating and raising fiber plants or animals; 2) harvesting, cleaning, and carding the fibers;

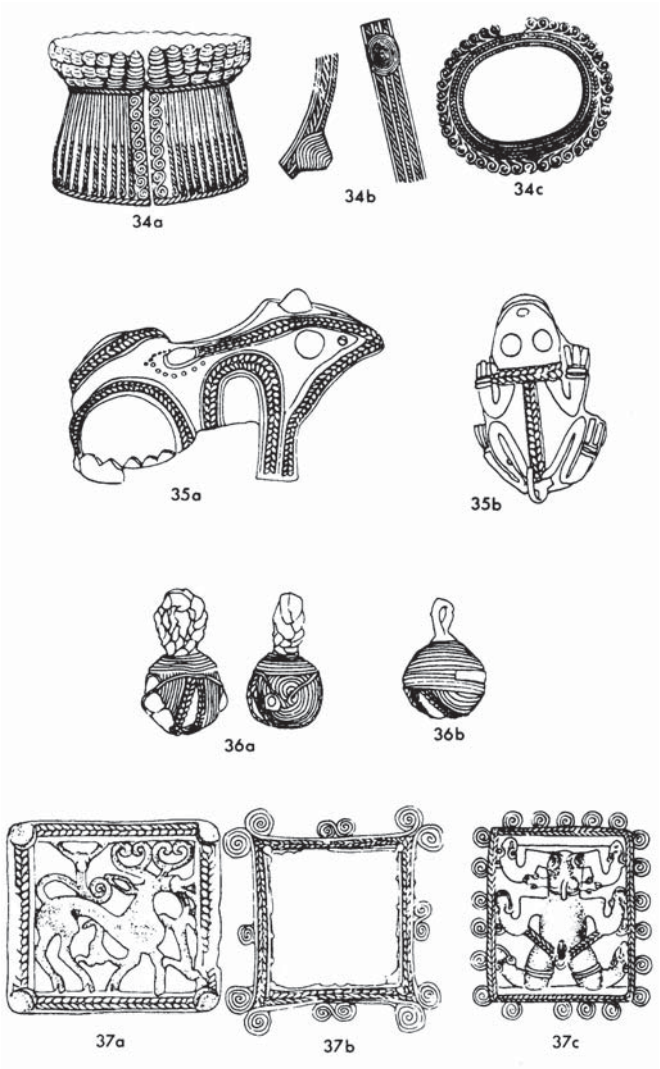

Figure 3. Drawing comparing metal objects from the Dong Song culture of Indochina (second half, first millennium B.C.) and from pre-Columbian Colombia and Panama:
(34a, b) bronze amulets, Cambodia; **(34c)** gold ornament, Colombia;
(**35a**) bronze frog effigy, Indochina; (**35b**) *tumbaga* (copper–gold–alloy) frog effigy, Colombia;
(**36a**) bronze bells, Laos; (**36b**) *tumbaga* bell, Colombia;
(**37a**) bronze belt buckle, Caucasus Mountains; (**37b**) bronze ornament, Indochina; (**37c**) gold ornament, Panama (from Heine-Geldern 1972:804).

3) using the principle of the flywheel to spin the fibers into thread, and perhaps plying threads into yarn; 4) assembling and setting up the loom frame; 5) stringing the warp threads/yarns under tension; 6) separating the warp leaves using heddle rod and heddles and inserting the wefts through the resulting shed and then the countershed; and 7) beating the warps into place. A host of woven structures—some ingeniously complex—were devised to produce patterns. Weaving may be followed by making the cloth into tailored garments, which requires cutting to shape and assembling by sewing. If one adds to all this the chemical technology of dyeing with colors, often highly technical and complicated and involving not only laboriously produced dyestuffs but also fiber-roughening and chemically binding mordants, one has an extraordinarily elaborate physical and chemical system of production—in fact one of humankind's most amazing pre-modern achievements. Regarding dyeing alone, whose recipes were often kept secret, one writer asserted, "Making dyes was once a treasured craft, something akin to sorcery, and the recipes were often so complicated and obscure that most tribes chose certain people to do nothing but gather the necessary roots, nuts, leaves, fruits, and insects" (Specter 2000:49).

At least four important genres of dyestuffs were shared between the hemispheres before 1492. Madder-root reds were produced in southern Asia and in early Peru. Indigo was a specialty of northwestern India and of the high cultures of the Americas. Red dyes from various species of tiny coccid insects were also made in Southwest and South Asia (kermes and lac) and in Mexico and, later, Peru (cochineal). The labor-intensive use of shellfish-purple dyes was centered in the eastern Mediterranean but also included the Red Sea and Atlantic Morocco in the Old World, and (in less laborious form) Middle America and Peru in the New (Jett 1998b).

But that is not the totality of potential complexity. In addition to manipulating structure to create pattern, including with yarns of different colors, in both hemispheres non-structural means were utilized as well, including embroidery, freehand painting, and printing—the last two employing either direct painting/printing or the application of mordants to cause the dye to be absorbed only in the mordant-painted areas. As if this weren't enough, methods of resist dyeing were also developed to create design: preventing the dye from reaching certain parts of the thread/yarn or cloth during the bath. These hugely laborious shared resist methods included 1) *ikat*, the pre-dyeing tying off, with impermeable cord, of some areas of the as-yet unwoven warps, wefts, or both; 2) *tie-dye* or *plangi* and *tritik*, the tying or sewing off of some areas of the woven cloth prior to dyeing; and 3) *batik*, the coating of parts of the cloth with starch, resin, wax, or the like before dyeing to repel the dye from those areas. Then there is the question

of color palettes and design styles, in themselves often complex and quite diagnostic, even emblematic, of particular cultures (Jett 1999).

There are three ancient loom types with distinct regional associations in the Old World that also occur in the New: 1) the horizontal staked ground loom of North Africa and Southwest and Central Asia is also found in the Lake Titicaca basin of Bolivia and Peru and in Northwest Mexico; 2) the vertical two-bar (tapestry) loom of southwestern Asia is widely distributed in the Americas as well; and 3) the backstrap loom of Southeast Asia is also common in the Western Hemisphere, especially in the tropics (Heyerdahl 1978:76, Broudy 1979, Teague 1998:106–24). With regard to the heddle complex (a rod-and-multiple-string device for separating the warp leaves), *the* historian of ancient textiles (and a weaver herself), Occidental College's Elizabeth Wayland Barber (1994:41) wrote that from its region of Neolithic origin in northern Iraq or Turkey, "the idea must have spread slowly to Europe, to the Orient, and eventually by boat to South America [circa 2000 B.C.]. It is such a difficult concept that it may have been invented only once."

Needless to say, these several elaborate cloth-related technologies did not arise spontaneously in many places here and there but only in a few areas where a permissive combination of factors happened to co-occur. From these centers of innovation, the technologies spread outward until they became widely distributed in both the Old and the New Worlds but with coherent patterns of geographical distribution as well as some archaeological support for spread over time from areas of invention. I am inclined to conclude that the textile traditions of the two hemispheres are historically as well as technologically closely related to each other and a consequence of multiple overseas contacts between peoples of southern Asia and tropical America.

Ceramics

The making of even simple, non-wheel–turned, non-molded ceramics is not an uncomplicated matter. Suitable clay must be identified, dug out, transported home, and worked to remove inclusions. It must be mixed with the proper proportion of water and tempering material such as sand, crushed potsherds, or organic material, to limit shrinkage and prevent cracking. The vessel must be hand-formed (usually by coiling and scraping) to a uniform degree of thinness. Optionally, the surface may be decorated by beating with a cord-wrapped paddle, incising, punctating, appliquéing, or some other method while still damp, and/or be slipped and painted when dry. Fuel must be gathered and the "green" pot fired with the right amount of cover to create either a reducing or an oxidizing atmosphere at the proper temperature. Altogether, it is not a technological complex that would naturally and easily have arisen in multiple locations.

Furthermore, certain New World pottery vessel forms are strikingly similar to certain forms in the Old World, e.g., ceramics of the American Formative and those of Neolithic southeastern China (Tolstoy 1974:133,134).

There are many other areas of material and nonmaterial culture in which close correspondences may be seen (see Sorenson and Raish 1996). The abundance, arbitrariness, complexity, and geographical and temporal clustering of many such correspondences are enough to cause diffusionist scholars to have few doubts about the existence of historical relationships (see, e.g., Fraser 1965, Tolstoy 1972, Jett 1971). However, such commonalities are not sufficient to persuade everyone; in fact, the majority of scholars remain convinced that, because humans all have the same kinds of brains and must deal with the same kinds of physical and social challenges, such similarities demonstrate not contact but the potential of completely separated societies to independently invent the same solutions anywhere that they are faced with the same general circumstances: If people could devise some trait or another in one place, other people could do the same thing somewhere else. This belief is particularly strong in the transoceanic context, since it is widely assumed that the Atlantic and the Pacific were essentially uncrossable before the European development of adequate ships and navigation in the 1400s (see below). The data that diffusionists perceive as demonstrating contact strike independent-inventionists as proving that unconnected societies can and do create very similar innovations. Clearly, then, cultural indications and theoretical arguments alone are unlikely to resolve these differences of viewpoint; we must search for confirmatory or refutatory evidence in non-cultural realms. More particularly, we must ask whether there exists *objective* and *non-cultural* proof of significant contacts. If such can be identified, then the *opportunity* for cultural exchange will have been demonstrated and the burden of proof will definitively shift from the diffusionist to the independent-inventionist.

Linguistic Evidence of Contacts

Languages and Writing

Among all the different categories of cultural phenomena, language and written inscriptions provide the most potentially useful indicators of contact, and American inscriptions in Old World alphabets and languages were of much interest to the Texas A&M geographer George Carter, who sparked the sometimes wild "American epigraphy" movement as led by the marine invertebrate zoologist H. Barraclough "Barry" Fell, a brilliant but methodologically flawed researcher and popularizer (e.g., Fell 1976).[3] Although much of Fell's work is unreliable, some of his identifications have

been accepted by Mayanist epigrapher David H. Kelley (1998a, 1998b, 1998c) and certain others (e.g., McGlone, Leonard, Guthrie, Gillespie, & Whittall 1993, but see Vastokas 2004).

Regarding comparisons between Old and New World languages, although these have long been viewed as not demonstrably related to one another, in 1967 the linguist Aert H. Kuipers did point out striking lexical and grammatical resemblances to Indo-European on the part of the Squamish language of northwestern North America's Coast Salish stock; but, as far as I am aware, this has never been followed up on. In more recent years, a few maverick professional and avocational linguists have forwarded some intriguing additional indications of certain other American Indian languages being closely related to particular Old World tongues—for example, the Andes' Quechumaran and Mexico's Uto-Aztecan, Tarascan, and Mixe-Zoquean languages being allied to or creolized with Afro–Asiatic idioms (Foster 1998, Stubbs 1998), of many tropical South American languages containing Austonesian elements (Key with Richards 1984, Key 1998, Stubbs 2014), and of Mayan being a Sinitic language (Fahey 2004, 2005/2006/2007). Two other proposed Asian/American language connections may also be mentioned: North America's Na-Denean and Siberia's Yeniseian (Ruhlen 1998, Kari & Potter 2010) on the one hand and Cal-Ugrian (Siberia's Ob-Ugrian and western North America's Penutian; von Sadovszky 1996); however, these two transfers would not have involved long open-sea voyaging.

In addition to some notable grammatical correspondences, these scholars have presented impressive lists of seeming lexical cognates that display systematic phonetic shifts. However, the linguistic and epigraphic evidence and arguments are too technical to present briefly and cogently, so I will move on to a distinct category of clues, that of biological evidence. The beauty of biology is that it is based on genetics rather than on cultural invention interpretable as coming from the universally shared human psyche interacting with people's common experiences and concerns.

Biological Evidence of Contacts

Cultivated Plants and Domesticated Fowl

More than a century ago, U.S. Government botanist O. F. Cook recognized that every cultivated plant could have been taken into domestication only where its wild ancestor(s) existed, and, that said, ancestors were confined to one hemisphere or the other. Therefore, such plants—most of which were incapable of diffusing via the Arctic, of leaping oceans on their own, or even of surviving without human intervention—could be used as objective

tracers of human movements, including movements across oceans. He also pointed to the names of these plants as having often traveled with the species (Carter 2002). Although botanists generally ignored Cook's notions, the cultural geographer Carl Sauer took up these ideas, and one of his students, Johns Hopkins University geographer George Carter, carried them farther than Cook or Sauer ever did (Gade 2003/2004, Jett 2007b).

Beginning in the 1930s, but especially in the decade after World War II, an increasing number of indications began to crop up that several cultivated plants had been shared between the hemispheres and between the Americas and Polynesia before 1492. Carter (1950, 1953) became the first to assemble all of the then-available information and present it as a whole (see also Heine-Geldern 1958). Until recently, however, the evidence of pre-Columbian transfer of these species was mostly circumstantial and therefore subject to dispute. But in recent years, the data have mushroomed. Another Sauer student, University of Oregon cultural-plant geographer Carl L. Johannessen (Gade 2003/2004), has been at the forefront in gathering and presenting the new findings (e.g., Johannessen & Parker 1989, Johannessen 1998, Johannessen with Wang 1998). The Brigham Young University anthropologist John L. Sorenson and Carl Johannessen have together compiled copiously documented information, which the authors characterize as providing

> . . . conclusive evidence that nearly 100 species of plants, a majority of them cultivars, . . . were present in both the Eastern and Western Hemispheres prior to Columbus' first voyage to the Americas. The evidence comes from archaeological, historical and linguistic sources, ancient art, and conventional natural science studies. Additionally, 19 species of micro-predators and seven other species of fauna were shared by the Old and New Worlds. The evidence further suggests the desirability of additional study of at least 75 other organisms as probably or possibly bi-hemispheric in pre-Columbian times. (Sorenson & Johannessen 2009:1)

That, it must be acknowledged, is a breathtaking statement. Because it is so very far from the general consciousness concerning pre-Columbian plant distributions and exchanges, we are bound to ask: Are Sorenson and Johannessen's assertions really supportable? I have sampled their original sources and studied all the archaeological reports in detail and have found that the short answer is: Yes, they are abundantly supportable. Johannessen's work has stressed pre-Columbian temple carvings in India that clearly show maize ears (Figure 4) and, somewhat less obviously and abundantly, the sugar-apple, sunflower, and certain other American plants. The Indian art historian Shakti M. Gupta (1996) has independently

confirmed many of these identifications. For those not convinced by carvings, literary references (e.g., to tobacco; Ashraf 1985), and the like, but only by truly "hard" evidence in the form of actual pre-Columbian plant remains, one may mention a number of reports of Eurasian–Pacific archaeological specimens—some of great antiquity—of the following American crop plants: the sweet-potato widely in Polynesia (Hather & Kirch 1991, Pearthree 2003, Ladefoged, Graves, & Coil 2005, Horrocks & Rechtman 2009, Barber 2010); the peanut in Neolithic China (Johannessen with Wang 1998, Sorenson & Johannessen 2009, citing Wenhua 1994); the common bean, the lima bean, the phasey bean, amaranth, the sugar-apple, and *Datura* at early levels in India (Johannessen with Wang 1998:22–25, Saraswat, Sharma, & Saini 1994, Pokharia & Saraswat 1999, Pokharia 2008); third millennium B.C. peanut, sugar-apple, maize, and chili pepper on the East Indies' island of Timor (Glover 1977:43,46, 1986:55, 102, 132, 229–230, Oliveira 2008:218,178,182); and agave in Cyprus (Steffy 1985:84, 1994:56).

In the New World, the Asian variety of the bottle gourd is archaeologically ancient in many regions (Erickson, Smith, Clarke, Sandweiss, & Tuross 2005), and the Indian Ocean coconut has been reported archaeologically in Guatemala (ca. A.D. 700), in Honduras (ca. A.D., 400; Robinson et al. 2000:843), and in Peru (Heyerdahl 1953:458). Half of the chromosomes of pre-Columbian American domesticated cottons are from an African species (Hutchison, Silow, & Stevens 1947, Johnson 1975), although likely a result of natural dispersal (Wendel & Cronn 2003), and there is more equivocal archaeological evidence for plantain and certain other Old World crops. Small culinary dogs in China and the Americas share the same gene for hairlessness (Drögemüller et al. 2008, Jett 2008–2010); although the animal is ancient in America, its age in Asia is unknown.

Molecular genetics has, in recent times, been applied to questions of the geographic sources of certain of these "out-of-place" crop species. Genetics indicates that the aboriginal Polynesian sweet potato most likely came from the Ecuador/Peru region (Roullier Benoit, McKey, & Lebot et al. 2013). The bottle gourd is more problematic; morphologically, the New World ones are like African gourds, while some geneticists feel that the affinity is with Asiatic ones (Erickson et al. 2005, Clarke 2009:199).

George Carter (1971, 1998) and Carl Johannessen (Johannessen & Fogg 1982, Johannessen, Fogg, & Fogg 1984) also presented much circumstantial biological and cultural evidence of Asiatic chickens having been introduced to pre-Columbian America, but they were unable to verify any pre-1492 chicken remains. However, in 2007 an international archaeological team

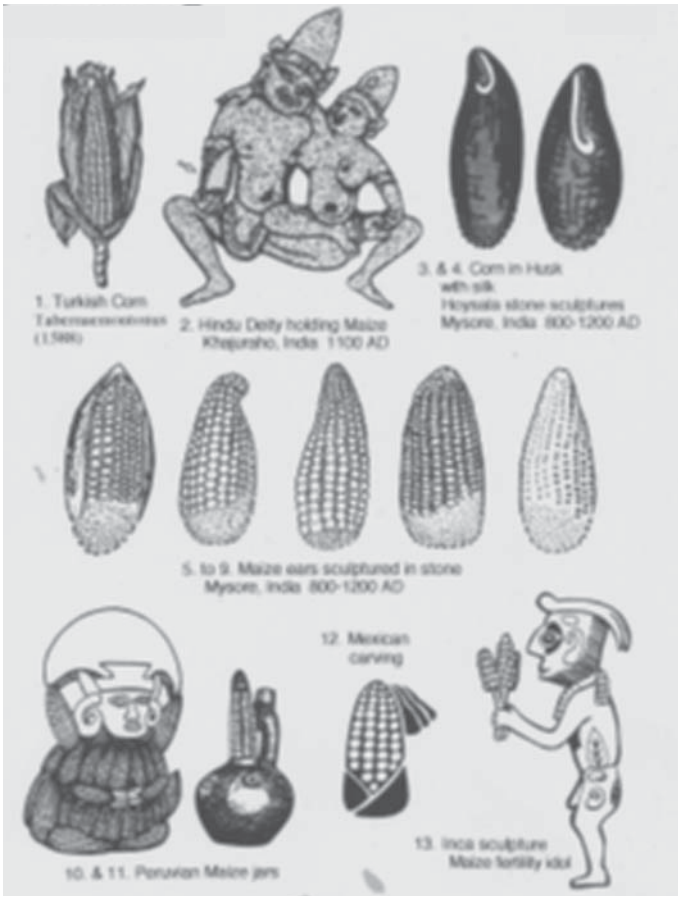

Figure 4. Drawings of pre-Columbian sculpted maize representations in the Old World, notably from Hindu temples in Karnataka (formerly, Mysore) state, India, and in Mexico and Peru (by Gunnar Thompson 1992:240).

led by Alice A. Storey announced the discovery of late pre-Columbian West Polynesian–type chicken bones in coastal south–central Chile, representing a minimum of five birds; additional bones were obtained later (Storey, Quiróz, & Matisoo-Smith 2011; for demurrers, see Gongora et al. 2008, refuted in Storey et al. 2007, 2011, Thompson et al. 2014). Medieval-period bones of the American turkey have been reported from Europe as well (Bökönyi & Jánossy 1959), although they have been disputed as being from peacocks (Schorger 1966:472).

One fascinating matter, which Jett has investigated in depth, is

reports, mainly by a German team of forensic pathologists led by Svetlana Balabanova (beginning with Balabanova, Parsche, & Pirsig 1992), that residues of nicotine and cocaine occurred in the bones, hair, and tissues of a multitude of ancient Egyptian mummies, and nicotine alone in numbers of other pre-1492 Old World burials. Since tobacco and coca—major ritual, medicinal, and indulgent plants of the Americas—are the only plausible sources for these alkaloids, we are obliged to conclude that transoceanic drug-trafficking occurred. This conclusion is reinforced by the discovery of the occurrence of residues of THC from Asian-origin hashish in a number of pre-Columbian Peruvian mummies (Parsche, Balabanova, & Pirsig 1994; for comprehensive coverage, see Jett 2002, 2003/2004; see also Görlitz 2002, 2011).

Although this fact has not yet been widely absorbed in the scholarly community, the quantity and quality of the evidence for inter-hemispheric transfer of domesticates is now such that it is hardly disputable that multiple roundtrip pre-Columbian contacts and plant transfers took place; the "undeniable reality" of this article's title must be accepted.

Human Parasites

The human louse (*Pediculus humanus*), known archaeologically from Europe and Africa, has been identified on pre-Columbian Peruvian mummies dating to circa A.D. 1225, in the form of clade B (one of its three clades), which may have originated in Africa but which is now global (Raoult et al. 2008).

There are certain tropical/subtropical intestinally parasitic worms that had always been thought of as being confined to the Eastern Hemisphere before 1492, most having evolved along with domestication and urbanization there and consequently being too late to have entered the pre-Columbian Americas with the founding migrants (Reinhard 1990:159). Most of the few human intestinal parasites of earlier vintage were thought to have been filtered out by cold, as paleolithic hunters entered via arctic Beringia. Except for pinworms, said archaeopathologist Karl T. Reinhard, "So late as 1981, parasitologists in general believed that the [pre-Columbian] New World was essentially free of human parasite disease" (Pringle 1998:1776). In the intervening years, that impression has changed dramatically.

In 1974, the first American archaeological find of the hookworm *Ancylostoma duodenale*, from a Tiahuanaco mummy of circa A.D. 900, was reported from Bolivia. Sixteen years later, many more such finds had been made, in both South and North America, not only of *A. duodenale* but also of another hookworm in Brazil, *Necator americanus*, and the whipworm (*Trichuris trichura*) in various parts of South America (at approximately

6670 B.C. in Brazil). Similarly, the hookworm (*Ancylostoma duodanale*) was found circa 5250 B.C. in eastern Brazil. Other parasites unearthed include the hairworm (*Strongyloides*) and the giant roundworm (*Ascaris lumbricoides*), the last being attested in Peru at about 2300 B.C. (Fernando Ferreira, Araújo, & Confalonieri 1988:65–67, Verano 1998:221, Horne 1985:300–303).

Brazilians have been active in this research, citing *A. duodanale* from two places in Brazil, at circa 800 and 5250 B.C., respectively. These Brazilians concluded that transoceanic contacts were necessary to account for these occurrences and that the presence of various helminths among "Paleoindians" placed the oldest of such contacts much earlier than even most diffusionists have proposed (Fernando Ferreira, Araújo, & Confalonieri 1988:20–23, Confalonieri, Fernando Ferreira, & Araújo 1991:864–865). The pre-Columbian New World presences of these Old World warm-region intestinal parasites are among the stronger evidence of actual seaborne contacts across the oceans in early times. The only alternative explanation to transoceanic—or, at least, rapid boat-borne littoral transfer—that comes to mind is of carriage via the Bering Strait area during an interglacial, when conditions would have been warmer—although whether or not they would have been warm *enough*, I am uncertain;[4] in any case, that would require both an early initial entry of humans into the hemisphere—the possibility of which is debatable—and emergence of the parasites before the rise of agriculture, which is contrary to present understanding.

Human Genetics

The study of human genetics, especially biochemical and molecular genetics, is technical and fast-developing. Sampling has so far not been anything like geographically universal; thus, many conclusions remain tentative. And because genetics is so complex a subject, I cannot present a clear and comprehensive picture in the space available. Nevertheless, the field seems certain to provide critical evidence relevant to transoceanic investigations. The biologist Austin L. Hughes (2002) has written, "Molecular-biology data offer the promise of at last unlocking the prehistories of our . . . species." The anthropologist Kenneth Tankersley was of the same mind:

> Genome variation is rapidly becoming a powerful tool that is leading toward a quantum leap in our knowledge of human migrations and origins. . . . It is becoming increasingly evident that genetics in the twenty-first century will have as a profound effect on American archaeology as radiocarbon dating did during the twentieth century. (Tankersley 2000:75)

What gives such genetic studies the advantage over traditional physical

anthropological ones is the specificity, numerousness, variability, and high degree of mutual independence of the items involved, resulting—as long as sampling is done properly—in statistically unassailable matches. For purposes of reconstructing contacts, distinctive and uncommon genetic markers that involve polymorphisms with no apparent phenotypic functional or adaptive advantage may be most revealing, just as in the cultural realm minor but distinctive and highly arbitrary cultural traits may indicate contact and introduction. It is the presence and limited geographic distributions of such distinctive polymorphisms, not pooled averages that show the degree of overall genetic distance among populations, that count in this context. These polymorphisms are the "trace elements" of biological anthropology, and can signal contact and gene flow even when small numbers of migrants or visitors were involved.

Although the ABO blood groups are the best-known single-gene–controlled factors, they are of limited use for our purposes because the different blood groups provide differing degrees of resistance to various diseases and therefore can be selected for or against relatively rapidly. However, other blood factors are far more useful, in that the numbers of genotypic variants are much greater and have no demonstrated adaptive differences among variants. As far back as the 1960s, the Diego factor was found to be abundant among South American Indians and common in southern and eastern Asia but absent in the Bering Strait region (Garn 1965:45–46). Certain haplotypes of the Rhesus and Kell systems also display such suggestive distributions among living Amerinds. Transferrins have variants with similar distributions as well. All of this suggests contacts between the hemispheres via the oceans, separate from any via the Arctic. (The absence of the Asian mitochondrial–DNA haplogroup B in the north may reflect the Late Pleistocene littoral movements into the hemisphere's bypassing the then-frozen north or scarce early northerners later being genetically swamped, as well as later, Holocene, transoceanic inputs directly to more southerly areas (Jett 2007a).)

The American organic chemist James L. Guthrie (2000/2001) made an extensive study of the present-day distributions of the above factors, particularly of human leukocyte (lymphocyte) antigens (HLAs), which are components of the histocompatibility system. Although no HLA data are available for pre-Columbian times, these contemporary data are nevertheless particularly useful owing to the great number of variants and the rarity and geographical restriction of certain of them, and the low likelihood of their presence reflecting post-Columbian admixtures.

I can only summarize Guthrie's findings. Many of the "foreign" HLAs and other factors do not occur in the northern regions of Asia or America but

do occur in America's lower-latitude zones of high culture. Mesoamerica and the Andean region share many of these factors with each other but not with Central America, while a number of those present in Central America fade out to the north and to the south, implying related outside inputs to Mexico and Peru and a separate input or inputs to the intermediate area. A number of "Afro–Asiatic" HLAs—characteristic of the Mediterranean/southwestern Asian realm—show up, many together, in South America, especially in the Andean region, as well as among Uto-Aztecan speakers of Mexico and adjacent countries; these distributions fit nicely with the aforementioned fairly recent proposals that Uto-Aztecan and the Andes' Quechumaran languages are in some way related to the Afro–Asiatic linguistic stock. In addition, there are southern Asian HLAs in parts of aboriginal America, suggesting Southeast Asian/Oceanian input. Certain European HLAs also appear among Uto-Aztecans and Andeans. The patterns of "foreign" HLAs found among living indigenous American peoples are not, in most regions, what would have prevailed if their source had been the post-1492 European colonizers. Nor are independent mutations a plausible explanation for these patterns. I cannot credit that the co-occurrence of these and other "foreign" genes in the Andean and greater Mesoamerican regions—exactly where multifarious foreign influences are most suggested by cultural and linguistic evidence—does not reflect pre-Columbian human intrusions from the Eastern Hemisphere; if anyone can suggest another encompassing explanation, I would be most interested to hear it.

One relevant genetic system is that of the genetically stable but highly diverse polymorphic *Alu* sequences of short interspersed repetitive elements (SINEs). Heterozygosity (an index of multiple contributors to a population) is maximal in Peru, high in North America including Mexico, and minimal in Central America. The study of *Alu*s shows that a notable correlation exists between the Chinese and the Mayans and their neighbors (Novick et al. 1998)—dovetailing with Bede Fahey's case that the Mayan and Chinese languages derive from a common ancestor as well as D. B. Kelley's concerning some of the calendrical data (see above).

Supremely useful though they are in tracing past human movements, with the exception of the finding that Asian mitochondrial-DNA haplogroup B is absent in the north of Native North America (Cann 1994), uniparental DNA studies have contributed little to the Holocene transoceanic-contacts question. Geneticists studying Native American origins typically look only at "Native American" DNA (haplogroups A, B, C, D, and X) and eliminate from consideration "foreign" DNA, which they reflexively attribute to post-Columbian admixture. Thus, these studies normally fail to report the very data we need for the question we are asking (future advances in ancient-

DNA studies may eventually come to our aid). In addition, most ancient contacts from overseas are highly likely to have involved only men and not women, so we would not anticipate mtDNA (female-inherited) to be useful in this context (male-inherited MSR DNA has significantly fewer polymorphisms, and there are many fewer Y-DNA than mtDNA studies).

A partial exception to the limited utility of recent DNA studies in identifying transoceanic inputs is the Near Eastern/European mitochondrial-DNA haplogroup X, of which variant X2a occurs, sometimes at fairly high frequencies, among a number of northerly North American Indian groups (Smith et al. 1998, Brown et al. 1998). The European and American haplogroup-X variants appear to have split from each between 17,000 and 13,000 years ago, giving some additional credibility to theories of Pleistocene ice-edge transatlantic migrations from France and Spain: the "Iberia, not Siberia" hypothesis for the ancestors of carriers of the early North American Clovis culture, ancestors who are hypothesized to have been Solutrean Europeans (Stanford & Bradley 2012; for an early contrary view, see Straus, Meltzer, & Goebel 2005; too, X may not have been in Western Europe this early).

Interestingly, a sample from the Cherokee included not only notable frequencies of mtDNA X but also six additional non-Native American haplogroups of Levantine/European origin, most in significantly higher percentages than among the non-Indian populations of the Southeast and including haplotypes unique to the Cherokee, suggesting considerable age; these patterns seem to essentially preclude attribution to post-Columbian European/African admixture (Yates 2012).

Watercraft and Navigation

Transoceanic diffusionists have always thought that the detailed and arbitrary cultural commonalities shared between the two hemispheres were sufficient to show that contacts had taken place and that influences had occurred. All this was reinforced by the circumstantial evidence for the pre-Columbian sharing of certain cultivated plants and the chicken. Therefore, so diffusionist thinking went, despite a lack of direct evidence of adequate watercraft with which to effect crossings, such craft *must* have existed—contrary to historians' assertions that Old World "discovery" of the open oceans and the Americas awaited European Medieval/Renaissance developments such as adoption of the magnetic compass, capacious multi-masted ships, and the stern rudder (Jett 1998a, 2008). Isolationists simply said, "We don't think contacts could or did occur; *show* us the boats, and then *show* us Old World artifacts professionally excavated from pre-Columbian New World sites, and then perhaps we'll consider the possibility

of influences. Meantime, we will assume independence."

Littoral adaptations, including the use of watercraft, are now thought to have been important in the spread of modern humans for at least 150,000 years (Erlandson 2001). Archaeology has proven humans to have made significantly long ocean voyages to settle the islands of Near Oceania beginning more than 40,000 years ago, showing that well-developed seagoing watercrafts, presumably sail-powered, as well as celestial navigation were in use far earlier than previously suspected (e.g., Gamble 1994, Irwin 1992). Okinawa, in the Ryukyu Islands and never connected to the mainland, has yielded human skeletons dated back to 30,150 B.C. In Japan proper, obsidian was being obtained from Kozushima in the Izu Islands 34 miles off Honshu as long ago as 28,000 years B.C., showing that Paleolithic voyaging was occurring in East Asia as well (Ikawa-Smith 1986:204); by 1000 B.C. or earlier, long-distance seaborne trade was taking obsidian some 2,175 miles westward from New Britain and the Admiralty Islands to Borneo (Fredericksen 1997:376–377). In the West, the Mediterranean islands were populated even earlier: Sardinia as much as 300,000 years ago and Crete 170,000 years ago (Bednarik 1997, Broodbank 2006).

In the late twentieth century, the study of traditional watercraft and their performances—including, importantly, by the geographers Edwin Doran (e.g., 1971) of Texas A&M University and Clinton Edwards (e.g., 1972) of the University of Wisconsin-Milwaukee—plus maritime archaeology, led by the Texas A&M University nautical archaeologist George Bass—vastly expanded our knowledge of ancient watercraft, and a number of specialists now feel that many kinds were quite capable of crossing oceans; in fact, history and archaeology tell us that traverses of the Indian Ocean greater in length than the width of the Atlantic were routinely undertaken in Antiquity.

The building of reproductions of ancient craft and the submission of them to rigorous sea trials has increased our respect for early vessels' durability, seaworthiness, and handiness. Experimental voyaging in replica watercraft has repeatedly demonstrated the transoceanic capabilities of everything from reed bundle craft and log rafts to skin boats, sailing canoes, and Chinese junks (e.g., Crumlin-Pedersen & Vinner 1986, Capelotti 2001). Multiple solo crossings in minute modern boats have reinforced the conclusion that almost any craft is capable of crossing an ocean, even by simply drifting (many post-1492 transoceanic drifts have been recorded; Jett 1971:13–15; Kehoe 1971, 1990). Rainwater and wild foods obtainable at sea were usually more than adequate for survival (Jett 2005/2006/2007).

Also, indigenous navigational methods have been studied (e.g., Gladwin 1970, Thomas 1987, Lewis 1994) and tested, particularly by University of Hawaii anthropologist Ben Finney's (e.g., 1979, 1994) team, and found to

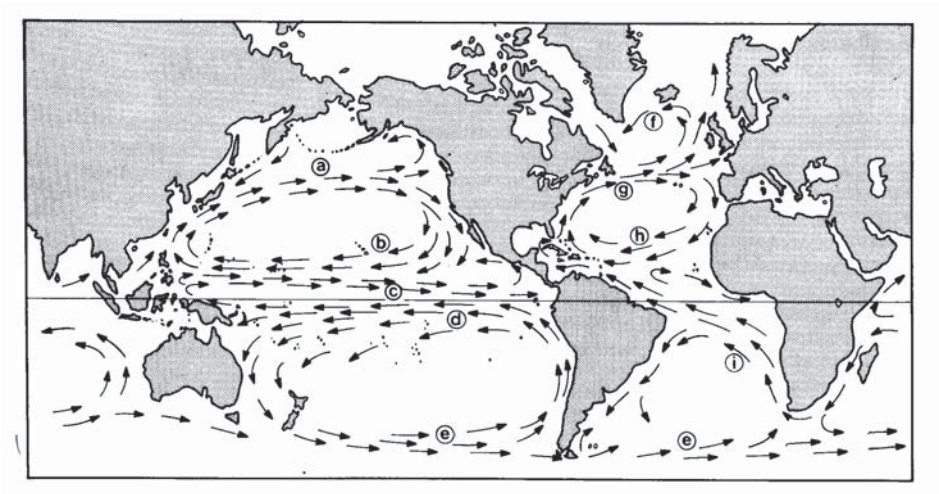

Figure 5. Generalized map of the principal surface ocean currents:
(a) Japan/North Pacific currents; (**b**) California/North Equatorial currents;
(**c**) Equatorial Countercurrent; (**d**) Peru/South Equatorial currents;
(**e**) Antarctic Drift; (**f**) Irminger Current;
(**g**) Gulf Stream/North Atlantic Current;
(**h**) Canaries/North Equatorial currents;
(**i**) Benguela/South Equatorial currents.
Note: The map projection distortion exaggerates area and distance in northerly regions.

be serviceable (Jett 1998a, 2008). In addition, our growing understanding of the world's winds and surface ocean currents, including, importantly, the El Niño–Southern Oscillation, has revealed more about plausible avenues and timings of travel (Figure 5; for drift simulations, see Callaghan 2003, 2005, Montenegro, Hetherington, Eby, & Weaver 2006). The view of a necessarily hermetically sealed-off New World can no longer be sustained. With what we now know of early watercraft and navigation and of climatology and oceanography, it would not only be unsurprising if many overseas contacts turn out to have happened, it would be astonishing if such contacts did *not* occur.

Summing Up

If significant contacts between the hemispheres did take place, then there existed the opportunity for biological and cultural inter-influences. Because it is easier to emulate than to invent, diffusion rather than independent invention is the more economical hypothesis to explain the multitude of specific cultural similarities between the Old World and the New World,

and may also plausibly account for many more general resemblances.

Research into the possibilities of pre-Columbian transoceanic travels has rapidly advanced in recent years, and signs of more widespread acceptance have appeared (note, for example, Huyghe 1992, Schoch & McNally 2003, Jones, Storey, Matisoo-Smith, & Ramírez-Aliaga 2011, Stanford & Bradley 2012). Access to relevant information has been greatly facilitated by the publication of a massive annotated bibliography on the subject (Sorenson & Raish 1996, see also Fingerhut 1994) and by initiation in 1998 of *PreColumbiana: A Journal of Long-Distance Contacts*.

A variety of independent lines of evidence now converge on the conclusion that these facts can, separately and especially jointly, be explained only by a long sequence of influential pre-Columbian transoceanic contacts, between and among several Old and New World areas. For decades, a plethora of cultural evidence has existed. The now-massive biological evidence shows clearly that repeated and significant encounters, involving at least notable plant and human-genetic exchanges, did happen. In fact, even many of those researchers not fully acquainted with this evidence have seen enough to acknowledge that a few contacts must have taken place—while usually still denying that the interactions had much impact or importance, remaining reluctant to give up the notion that the New World represents an independent "laboratory" of cultural evolution, and/or remaining committed to an optimistic and "egalitarian" humanist view of mankind's great inventiveness (see Jett 2006).

To the diffusionist, on a culture-by-culture basis the role of inter-influences has been far more significant to human cultural history than has local innovation. The inter-hemispheric exchange of economic plants and of culture seems so massive as to have played a fundamental (if so-far largely unrecognized) part in the histories of the Eastern and Western hemispheres, especially the Western. It is not that the various cultures of these continents lacked their own distinctiveness and styles or were mere passively uninventive receptive vessels. But the civilizations of the two hemispheres may have evolved, to a considerable degree, in tandem, involving significant and continuing inter-influences from early times onward, mostly unrecorded in written history but no less real for that. In fact, the inter-influences may have been critical in stimulating cultural innovation and elaboration—in both the New World and the Old, but especially in the New.

Unquestionably, Late Pleistocene/Early Holocene human migrants from the Old World to the Americas brought with them basic Paleolithic technologies and non-material culture that formed the basis for later developments within the New World. Certainly, too, there was some innovation among descendants of these founders as they applied universal

human abilities and mental characteristics in adapting to a variety of New World environments, as well as to environmental change. However, as long as these peoples remained out of touch with the larger world, their cultures remained more static than innovative. I conclude that although a terminal Pleistocene transatlantic input of European Solutrean Paleolithic technology may well have occurred in otherwise isolation, relatively little altered culturally in earlier-Holocene America. Because archaeology and history have increasingly made clear that the peoples of the ancient Old World were linked in networks of travel, trade, and cultural exchange from at least Neolithic times onward and that major innovations tended to develop in only a few hearths at cultural crossroads (e.g., Southwest Asia, between three continents and several seas) and subsequently to diffuse from those centers like ripples on a pond, it would, in my opinion, be erroneous to omit the New World from this overall picture. When inter-hemispheric contacts did (as I see it) become established, not only did cultural and biotic imports take place, but spurts of stimulus to innovate locally occurred, sparked by the possibilities of combining novel traits with pre-existing ones as well as opening minds to the possibility of true invention rather than simply taking the status quo as a given. This is analogous to the principle, in organic evolution, of punctuated equilibrium (see Gould 2002), punctuations in this instance arising from overseas interactions. However, these postulated cultural and proven biological imports did not create clones of Old World societies in the New World; American cultures developed in often highly distinctive ways and created their own unmistakable styles—although making only a handful of exclusively American technological breakthroughs and, despite postulated major interaction, failing to adopt or invent many Old World ones.

The ancient Greeks spoke of the Ecumene—the known inhabited, particularly civilized world. We may increasingly be obliged to think in terms of a global Ecumene, enmeshing the more elaborate pre-Columbian cultures on both sides of the seas (Jett 2000a, see also Gordon 1971), and to think of the ancient oceans less as barriers and more as highways for watercraft-users, linking distant shores and peoples (Jett 2008).

Notes

[1] This is an adaptation, expansion, and updating of a paper read as the inaugural George F. Carter Lecture, Emeriti Professor Colloquium Series, Department of Geography, Texas A&M University, 19 November 2004 (Schilling 2004). I acknowledge with gratitude the efficient collegiality of the TAMU Geography graduate students, especially Wendy W. Patzewitsch. Early versions of certain parts of this article appeared in

Jett (2003, see also Jett 1993). Some of this material is also treated in a forthcoming book (Jett 2014). Many colleagues have contributed to my knowledge and to the gestation of my ideas. Thomas D. Dillehay has made suggestions specific to this article.

[2] An excellent case has been made for a circa A.D. 220 Roman terracotta head found in-situ in a late-pre-Cortesian pyramid at Cholula, Mexico (Hristov & Genovés 1999).

[3] Fell established the *Epigraphic Society Occasional Papers*, which continues to be published long after its founder's death.

[4] Uniquely, Hawdon and Johnston (1996) have suggested the possibility that the warmer microclimates of dwellings and a hypothesized dormant stage of the parasite could have allowed the passage of hookworms through the Arctic. But this begs the question of why the organisms are absent in the North today. Again, apparently uniquely, Fuller (1997) opined that the hookworm was misidentified in South America.

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HISTORICAL PERSPECTIVE

G. Stanley Hall on "Mystic or Borderline Phenomena"

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Abstract—G. Stanley Hall (1844–1924) was one the most prominent of the early American psychologists and an outspoken skeptic about the existence of psychic phenomena. This article presents a reprint of one of his critiques on the topic, a little-known paper entitled "Mystic or Borderline Phenomena" published in 1909 in the Proceedings of the Southern California Teacher's Association. Hall commented on some phenomena of physical mediumship, as well as on apparitions, telepathy, and mental healing. In his view all could be explained via conventional ways such as trickery and the workings of the unconscious mind. The paper is reprinted with an introduction and annotations providing biographical information about Hall and additional information and clarification of the points he made in the paper. It is argued that Hall's paper represents an instance of boundary-work common at the beginning of organized psychology, representing an attempt to give authority to the discipline over fields such as psychical research.

Over the years there have been many examples of attempts to reduce psychic phenomena to conventional explanations presented by students of the mind (see the discussions of Alvarado 2009, Coon 1992, Oppenheim 1985, Plas 2000). A representative of this point of view, and the topic of this paper, is American psychologist G. Stanley Hall. In this paper I present a reprint of a neglected article written by him to criticize psychic phenomena (Hall 1909).

In addition to Hall's unquestionable importance for the development and history of American psychology, I had several other reasons to choose this article. The paper is a good summary of Hall's negative views about psychic phenomena and psychical research and represents the opinion of other psychologists at the time. Furthermore, and perhaps because the paper was published in the proceedings of an education conference, and

not in a psychology or a psychical research journal (Hall 1909), the paper is not well-known among students of psychic phenomena and deserves to be remembered as representative of negative opinions about psychic phenomena discussed by a prominent American psychologist. However, the reader should keep in mind that I am not presenting a study of Hall nor a detailed survey or analysis of his thinking. This paper is instead a reminder of Hall's views about psychic phenomena through the reprint of one of his articles, presented with some general biographical and contextual information.

As I will discuss below, Hall's paper is an example of the attempts of many early psychologists to separate their emerging field from psychical research. Admittedly, writings by other authors fulfilled a similar function (e.g., Jastrow 1889, Münsterberg 1899) and also could have been singled out as examples of separating both fields. But none of the other candidates were as respected and as well-established in American psychology as Hall was.

I am also presenting Hall's paper as a reminder of the importance of remembering critics and criticism in our discussions and understanding of the past developments of psychical research. This is because many historical articles published by workers in the field tend to focus on proponents of, or on defenses of, the "reality" of psychic phenomena (on this issue see Alvarado 2012:624–626).

Psychologists and Psychic Phenomena

As stated by Coon (1992) in her paper about American psychologists:

> Experimental psychologists studied the mind, its limitations and its capabilities. Many perceived their own science as the most fundamental of the sciences because it was only through the mind that knowledge was possible. Belief in spiritual and psychic phenomena was to these psychologists only the secular ghost of a religious past, but a malevolent ghost preventing public confidence in scientific naturalism. Psychologists, as experts of the mental realm, would therefore expose fraud, credulity, and deception in matters psychic and spiritualistic. They would offer alternative naturalistic explanations and would be the self-appointed guardians of the scientific light. (Coon 1992:149–150)

Some reduced phenomena to conventional mechanisms, for example Pierre Janet (1859–1947) and Alfred Binet (1857–1911) in France, who wrote about automatism and dissociation to explain mediumship (Binet 1892, Janet 1889).[1] In Germany, Wilhem Wundt (1832–1920) argued that phenomena such as the influence on the mind at a distance had normal

psychological and physiological explanations or was due to "superstitious self-deception or intentional fraud" (Wundt 1897:275).

In the United States, where G. Stanley Hall operated, there were questions about the scientific character of psychical research (Mauskopf & McVaugh 1980, Moore 1977), as seen in the writings of several psychologists. For example, Joseph Jastrow (1863–1944) argued that psychical research "has . . . contributed an interesting chapter to the natural history of error . . . " (Jastrow 1889:81). Similarly, Edward W. Scripture (1864–1945) complained in his book *The New Psychology* about "unscientific methods of experimentation and . . . the air of occultism in which the whole is enveloped" (Scripture 1897:69).

All of these authors were engaging in boundary-work. They were actively separating their activities and concepts from those of others "for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science" (Gieryn 1999:4–5). Méheust (1999) has argued that many physicians in France developed a variety of conventional explanations (such as hyperesthesia) to separate hypnosis from psychic phenomena such as mental suggestion. Others have discussed a variety of strategies designed to separate psychology from psychical research (e.g., Coon 1992, Parot 1994, Sommer 2012, Wolffram 2009). Such boundary-work was related to the development of psychology as a scientific discipline and to its professionalization, which included defenses of the expertise of psychologists over the facts of the mental realm (see also Sommer 2013). In Coon's view:

> Psychologists were stationed at the periphery of science, and therefore they were the most threatened by challenges to the boundary and the most susceptible to cultural anxieties about what it meant to be 'scientific.' (Coon 1992:150)

Of course there were other issues involved. Psychologists were reacting to phenomena such as telepathy and mediumship that questioned the dependency of thought, and consciousness in general, on the nervous system. Such views seemed to many to run counter to what had been learned about the localizations of sensory and motor functioning during the nineteenth century (Clarke & Jacyna 1987).

In the paper reprinted here, Hall illustrates well the process of boundary-work by presenting an authoritative account of what he believed were the problems with accepting evidence for mental healing, telepathy, mediumship, and other phenomena. Hall attempted to discredit these topics by pointing out that many of them could be explained by trickery, particularly phenomena of physical mediumship such as raps and slate-

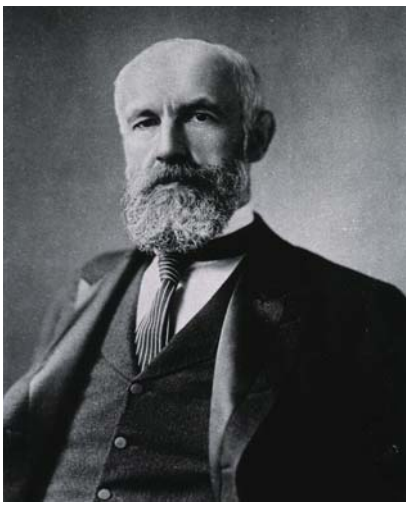
writing. He also argued that those who believed in the existence of psychic phenomena lacked knowledge about the productions and capabilities of the mind (e.g., hallucinations, dissociation). In his conclusion he suggested that the functions of the unconscious mind could account for psychic phenomena in conventional ways. Although not directly stated, he seemed to argue that a field such as psychical research was not necessary because psychology could explain the phenomena in question without recourse to a new field.

G. Stanley Hall

Psychology

Granville Stanley Hall (1844–1924) was active in American psychology during the early nineteenth century when the discipline developed as a systematic field separate from philosophy (O'Donnell 1985).[2] He obtained the first American psychology Ph.D. degree at Harvard University under William James in 1878 and was the holder of the first philosophy professorship at Johns Hopkins University in 1884 (Green 2007).

Hall was very productive, as seen in his books *Youth* (1907), *Educational Problems* (1911), *Morale* (1920), and *Life and Confessions of a Psychologist* (1923). His book *Adolescence* (1904) has been discussed in recent times (Arnett, 2006). Hall was involved with topics such as child development, education, teaching, and philosophy. A bibliography of his writings published in 1914 listed 328 publications, 285 of which were listed before the article (published in 1909) reprinted here (Wilson 1914). Some of Hall's papers were about topics such as hypnosis (Hall 1881), experimental psychology (Hall 1885), fears (Hall 1897), and anger (Hall 1899), among many other subjects (see the references presented by Ross 1972 and Wilson 1914).



G. Stanley Hall

In addition to teaching and research, Hall's importance in American psychology was evident at the end of the nineteenth century. According to a historian of psychology: "Until about 1894, Hall was the unrivaled leader of American psychology" (O'Donnell 1985:141).

Sigmund Freud, G. Stanley Hall, Carl G. Jung (front row), Abraham A. Brill, Ernest Jones, Sandor Ferenczi (back row), at Clark University in 1909.

Hall was a founder of journals, among them the *American Journal of Psychology* (1887) and the *Pedagogical Seminary* (1891). He was instrumental in the formation of the American Psychological Association in 1892 and president of Clark University (1888–1920). Hall created in this institution the first autonomous psychology department in the country, where he granted several psychology doctorates. Furthermore, Hall organized at Clark a conference in 1909 to celebrate the 20th anniversary of the founding of the university, for which he brought Sigmund Freud (1856–1939) and Carl G. Jung (1875–1961) to the United States, among others (Rosenzweig 1992).

However, by many accounts Hall was a difficult man to deal with. He had many conflicts with William James (1842–1910) and with several other psychologists (Ross 1972, Rosenzweig 1992). According to Christopher Green: "First and foremost a self-promoter, Hall was determined that everyone acknowledge his supremacy over American psychology" (Green 2007:315).

Psychic Phenomena

By the time Hall had become an influential figure in American psychology, there was a considerable literature about psychic phenomena and psychical research in existence, even if it was only a recently developed discipline. This is clear not only in developments in English-speaking countries (Moore 1977, Oppenheim 1985) emphasized by Hall, but in developments from countries such as Italy (Biondi 1988), France (Plas 2000), and Germany (Wolffram 2009).[3]

Throughout his career, and as seen in his article presented here, Hall was highly critical of psychical research (e.g., Hall 1887, 1895, 1908, 1909,

1910). His early involvement in the topic included his presence at a meeting held on September 23, 1885, to consider the formation of the American Society for Psychical Research. He became a member of the Society's council and one of its vice-presidents (American Society for Psychical Research 1885, Formation of the Society 1885). As early as September of 1885 Hall stated that he was skeptical about thought-transference. He was conducting tests in his house "but with no trace or shadow thus far of any thought-transference in the sense of the English Society" (Miles & Miles 1929:334).[4] He also wrote that he had seances with mediums, and that he searched for mediums in Philadelphia, New York, and Boston (Hall 1910:xv–xvi). The latter took place during the late 1880s.

In a widely cited long review of publications issued by the Society for Psychical Research (SPR), Hall (1887) strongly criticized the Society's work on spontaneous and experimental telepathy. In his view, telepathy "lacks everything approaching proof save to amateurs and speculative psychologists will be allowed to lapse to forgetfulness" (p. 146). Such a position, however, was highly exaggerated. His argument that the SPR researchers were "amateurs and speculative psychologists" was more a dismissal, and one associated with name-calling, than a critical examination of the problem. The SPR pioneers, as seen in Gauld's (1968) study, were enthusiastic, but they were aware of and vigilant about various conventional explanations and artifacts associated with research. An example was the discussion of criticisms of the evidence for spontaneous telepathy in the first major work of the Society, *Phantasms of the Living* (Gurney, Myers & Podmore 1886:Vol. 1:Chapter 4).

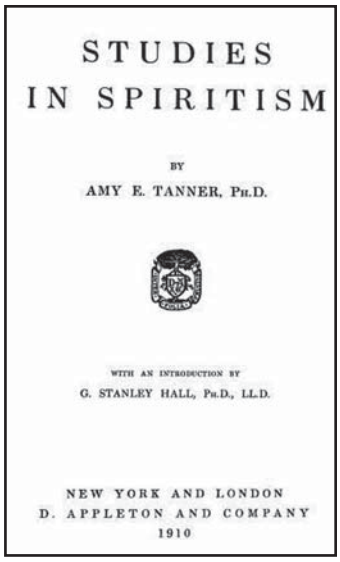
In a later review he characterized some of the literature of this field as having

> . . . deep unconscious bias of prejudice, in the form of hunger for immortality, which weights every die of fact, where the atmosphere, though clearing up, is still murky with traces of nearly every form of superstition that the world has ever seen . . . (Hall 1895:141)

Unfortunately, Hall lacked perspective on this issue as well, failing to examine his own biases and prejudices. It may even be argued that the psychical researchers were more self-critical and showed more cognizance of alternate viewpoints than critics such as Hall.

A similar negative stance appears in his book *Educational Problems*. In a chapter dealing with the lies of children, he mentioned cases of witchcraft, poltergeists, and mediums as examples of a "chronic diathesis of falsehood . . . . most common among barely pubescent or pre-pubescent girls" (Hall 1911, Vol. 1:350; see also Hall 1908:680).

Hall (1908:681) believed "there is no clairaudience or clairvoyance save through the regular channels of the sense" and that evidence for spontaneous veridical experiences was not to be trusted due to coincidence and other factors such as "errors in verification, [and] the fallibility of human testimony" (p. 682). Characteristically, he failed to acknowledge the detailed attention some early psychical researchers paid to such problems (e.g., Gurney, Myers, & Podmore 1886:Vol. 1: Chapter 4).


STUDIES
IN SPIRITISM

BY
AMY E. TANNER, PH.D.

WITH AN INTRODUCTION BY
G. STANLEY HALL, PH.D., LL.D.

NEW YORK AND LONDON
D. APPLETON AND COMPANY
1910

Tanner's *Studies in Spiritism*

Hall also had seances with the celebrated medium Leonora E. Piper (1857–1950), as reported in Amy E. Tanner's *Studies in Spiritism* (1910:177–185, Chapter 16).[5] He wrote in the introduction of the book: "I for one can see nothing more in Mrs. Piper than an interesting case of secondary personality with its own unique features" (Hall 1910:xxxi). The ideas he presented in Tanner's book were strongly criticized in the psychical research literature (Hyslop 1911, Lang 1911). Hyslop pointed out many problems with Hall's writing and suppositions, as well as with Tanner's. For example, he said it was not true that, as Hall claimed, Piper read the records of her trance statements (p. 70 of Hyslop's critique). He also believed that Hall, as well as Tanner, were learning to study mediums as they proceeded in their examination of Piper and presented assertions without evidence. Both authors, Hyslop maintained, insinuated many things that were untrue, trying to present a negative picture of psychical researchers. In Hyslop's words:

> The authors are forever telling us that the "controls" are extremely suggestible and often remind us that the subconscious is a very delicate affair. That is what psychic researchers like Dr. Hodgson always said or acted upon, but we are not told this fact, as if no one knew it but antipsychic researchers . . . As to suggestibility I would only say that was apparent to more people than these experimenters and the psychic researchers have always recognized that this was either a difficulty to be overcome or a necessary condition in the obtaining of the supernormal. (Hyslop 1911:87–88)

With this background, there is no question that Hall's self-appointed role as a critic of psychical research was problematic at best and that many of his evaluations need to be taken with a grain of salt. His general strategy,

both in the article presented here as in his other writings, was to offer all kinds of suggestions about possible artifacts and conventional explanations that in his view could account for the phenomena reported without informing his readers that such critiques were not new nor that they were well-known and discussed by psychical researchers. Consequently, uninformed readers most probably received the impression, particularly those that knew the comments were coming from an eminent psychologist, that psychical researchers were incompetent and that evidence for psychic phenomena was very weak or nonexistent. Nonetheless, Hall remains a good example of the rejection of psychic phenomena by American psychologists at a time that deserves to be remembered today as an historical example of skeptical views on the subject.

Reprint of Hall's 1909 Article

The paper reprinted below was one of three papers presented by Hall in December of 1908 at the Southern California Teacher's Association. In the program, the paper was listed with the title "The Psychology of Hypnotism, Telepathy, Spiritism, etc.," but it was printed in the *Conference Proceedings* in 1909 as "Mystic or Borderline Phenomena" (Hall 1909). I have added to the paper several footnotes that present further information about the topics discussed by Hall.

Mystic or Borderline Phenomena

This topic is an omnibus one, which includes spiritualism, mesmerism, hypnotism, crystal-gazing, mind-reading, Eddyism,[6] clairvoyance, telepathy, all the mancies, magic, sleight of hand, the Emmanuel Movement,[7] primitive medicine, and now certain subtle forms of mental arrest and perversion.

I. A word about raps or typtology. This was introduced in this country by the Fox sisters more than fifty years ago; and it was supposed that by answering questions by two raps No, by three raps Yes, revelations from the spirit world could be communicated to man. The phenomenon of these girls marks the outbreak of what might almost be called a spiritualistic epidemic, as a result of which many of the most eminent people in this country were convinced that there were veritable apparitions, that the souls of the dear departed hovered around us, that the spirit shore was near, and that our deceased friends could give indubitable proof of their post-mortem existence. Not a few of these mediums confessed; some of them are known to have had the power of snapping the joints at the toes, fingers, elbows, and even knees.[8] Moreover, one can purchase now various kinds of apparatus that can be concealed about the person and which give either muffled, dim and mystic notes, or shrill, resonant ones; and these have often been described as very remarkable.[9] We have yet to find, however, any kind of raps not explicable on physical principles, provided only the investigator can

control the conditions; and most investigators are more and more reluctant to conduct their researches unless they can do so. Female mediums cannot be searched; but the credulous still believe, and the skeptics still doubt.

II. Slate-writing can be produced in many ways, which are bought and sold by firms that deal in sleight of hand apparatus. Slade, the great medium, could not perform when he had sciatica, and was thought to write with his toes as the result of a great deal of practice.[10] There were often spirit drawings as well as messages. A college graduate and a professional man once called on me, and I am only an amateur conjurer, and wished to see slate-writing. I gave him two slates, which he cleaned and tied and held; then I placed my hand upon them in daylight, and soon a message appeared from Mary, which he recognized and by which he was deeply affected. I told and showed him how I had written the message, but in invisible ink which would not wash off, and brought it out by an acid gas, which I had palmed in a rubber capsule, and squeezed out from a hypodermic needle which I pushed between the frames. He was unhappy, and was finally frank enough to tell me that he believed I really did it by the aid of spirits, but that it was more becoming in a college professor to give him this kind of scientific patter.

There is almost nothing tricks cannot do, aided by skill and practice. There are many codes: for instance, reading cards can be done by two confederates, one of whom catches the heart rhythm as the toe or a crossed leg moves, and counts off the suit and the card, marking the beginning of the count by any rustle or noise of the foot, hem, sniffle, or any other sign, which the observers never detect. Probably hundreds of these tricks are well known and are found in the copious literature on this subject;[11] but the victim is entirely in the hands of the one who knows the secret or has the apparatus. So even mediums sometimes deceive each other, even in the same trick. My contention is that every investigator should know what are the resources of sleight of hand.

The English Psychical Research Society have recorded over seven hundred ghost stories, and the French Society many more, where it would seem that real spooks, wraiths, apparitions, spectres or something of the sort appeared to one or more senses. We must, however, insist that the investigator in this field must also know something about abnormal psychic phenomena, such as visions, optical illusions, etc. He must realize that sleep is often very partial, and that a part of the mind and one or more of the senses may dream while the others wake. He must understand hallucinations and hypnotism or induced sleep.[12] A great many so-called mediums are perfectly honest but simply deluded. Very interesting in this connection are reports of the French investigations by Vaschide and also by Viollet, who have studied the same phenomena but from a totally different standpoint: viz. from that of the physician.[13] They describe and interpret many cases of spiritistic hallucinations and illusions, some of which are entirely outgrown and vanish as the patient becomes more normal; and, conversely, some of their patients come to believe more intensely in spirits and also in more spirits as their mental disorders become more grave. Belief in spirits is profoundly

engraved upon our very nervous system. For instance, I am an utter sceptic, not about immortality, for all these studies leave that great question just where it was before, but about objective materialization of ghosts and their power to appear to any of the senses. When I was a boy and lived a mile from the village in the country, I used to run past the grave-yard surrounded by a high, black wall, with a black gate and shaded by moaning pines, with my heart in my mouth, for it stood remote from dwellings. What was my surprise a year or two ago, on walking over the same track, alone, late at night, to detect a little of the old shudder. I forced myself, therefore, to brave it, and climbed over the gate, marched to the middle of the grave-yard, lit a cigar by scratching my match upon a tomb-stone, and looked boldly around and walked deliberately out as I came; but what I was surprised to find was that my nerves and muscles were very tense and that it had cost me a great deal of nervous energy to thus face the old superstitions of my childhood which were still potent in my automatism.

Of course, it is hard to realize that our friends are really dead, and one purpose of funerals has been that the survivors may actually see them encoffined and entombed, and therefore that by all these sad ceremonies the unconscious depths of their souls might completely realize and feel that their friends were indeed dead. For only if this is done, are we secure against the intrusions of their ghosts. It is a remarkable circumstance that many young girls in the *backfisch*[14] stage have been the centre of spiritistic phenomena, and have deceived their parents and other adults, sometimes scientific men, to the very top of their bent, in one or other aspect of this domain. I have a list of nearly a score of such cases. It is a peculiar age, when the imagination is sometimes as vivid as the senses are, and when young girls, who have the lying diathesis, can do things that escape detection in a remarkable way.[15] What about spirit clothes? Did anyone ever see or hear or read of a nude ghost?[16] or must we agree with a recent writer who declares that, while the ghosts are real, their clothes are products of hallucinations? The sceptic asks if the clothes are not the ghosts themselves.

Then there are the phenomena of possession and trance, or mediumship proper. The Watseka wonder made the parents of a little dead girl near her age believe that the spirit of their daughter had come back to her, and acted the role for weeks in the house of the dead girl; and in a trancoidal state found possession of her own soul and wanted to go back to her own home.[17] Not merely the ghosts of dead friends but of strangers, not only those of recent but of ancient time who, if one theory of perisprit[18] is right, should have transcended the mortal sphere, come back and taken possession of the souls of mediums. Quite often spirits from Mars or Saturn, Lucifer, Raphael, and even mythological personages have been named as co-respondents in these psychic rapes. Dual and multiple personalities are pretty well known, as in the remarkable studies of Prince upon Miss Beauchamps [sic], or of Flourney [sic] upon Mlle. Smith.[19] One medium I know is possessed at times by the spirit of God Himself; and very often the spirit controls important proof, as in the case of Mrs. Piper who developed from phiniut [sic] up to imperator, rector, etc.[20]

Take all that the spirits have ever communicated through all the dark mediums, and on slates and planchettes, etc., what has it added to human knowledge? Dual personality is exaggerated change of mood; while crystal-gazing shows that the slightest, transient, unconscious impressions may not only be received but, by exceptional persons, be reproduced from their filmy traces, while the medium remains honestly unconscious that such impressions have ever been received. Fugies or impulsive runaways, or those who forget their past lives suddenly and start new careers, belong to this field.[21]

As telepathy, or the transmission of impressions from one person to another outside the ordinary channels of sensations, this, although firmly established in the conviction of many people wiser than I, still seems to me undemonstrated. Like others, I have tests which I have elaborated and which, if successfully met, would convince me. Several in past years have attempted to meet my tests, but not often. There are now funds, the controllers of which often advertise for the demonstration of telepathy as well as of spiritualism, but these tests have never been made, because a scientific man demands that he, and not the medium, shall control the conditions under which they are made.

The method of probabilities argues that, although one honest man's experience in seeing a ghost may not be convincing, that of ten honest men would be more so, and that of thousands would establish a presumption which, in the end, would be irresistable. Evidence is compared to a bundle of sticks which, if small, would be easily broken, but which could be so large that nothing could break it; but surely this is bad logic.[22] Before Copernicus the whole world believed that the sun went around the earth. Again, who has not had experiences of levitation, floating, swimming, hovering? but does that make one ounce of difference with anybody; still more could such evidence be convincing? Proofs for such things must be weighed, not counted.

As to Eddyism, it has several kernels of precious truth with a vast amount of chaff. The influence of the mind on the body is very great,[23] possibly more in our age than ever before; and who does not welcome the attempt of the Emmanuel Movement to set a back-fire, reduce the error, and produce a truth? The Emmanuel Movement, however, has greatly lost cast among medical people especially, partly because it has entered upon a public propaganda which seems very much like advertising, which is against the medical code and medical honor. Moreover, the cheapness of the training these people give of only a few weeks cannot possibly qualify classes to practical mental-healing. My criticism, however, lies against the scientific quality of the Emmanuelists: not only are they theologians with a rather limited knowledge of philosophy and pyschology [sic], but not one of them begins to have adequate experience in this field.[24] Meanwhile, the Binnets [sic], Grasset, Freud, Bleuler[25] and their school have spent laborious years in working out a rationale here, and they themselves feel that the field is large and that a great deal of the work needs to be done. So that, on the whole, I think we must conclude that it is premature; that this new junction

> which is so much to be desired between religion and medicine is not likely to be made by this new movement.
>
> The fact is that the unconscious part of the soul is vastly larger than the conscious part, and is often far more sensitive; it is like an ice-berg, nine-tenths under water.[26] Man's soul is a great museum of which consciousness lights only a few rooms. The brain is too large and too complex for the mind to use all of it. We are getting at a radically new conception of the human soul which, there is reason now to fear, is to make most of our current systems more or less obsolete.

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Notes

1 On the work of these and other men regarding dissociation and mediumship see Alvarado (2010), Alvarado, Machado, Zangari, and Zingrone (2007), and Le Maléfan (1999).

2 The most complete study about Hall is Ross (1972). See also Bringmann (1992), Goodchild (1996), Hogan (2003), Hulse and Green (1986), and Sokal (1990).

3 See also the large number of books about psychic phenomena listed by E. Abbott (1864) and Crabtree (1988).

4 This is a reference to throught-transference work of the Society for Psychical Research, which was well-known during the nineteenth century. This work has been summarized by Podmore (1894; see also Luckhurst 2002). On the Society in general see Cerullo (1982) and Gauld (1968).

5 Mrs. Piper was of key importance for the development of American and English psychical research. Her life and work has been discussed by her daughter (Piper 1929) and by many other writers (e.g., Gauld 1982:Chapter 3, Tymn 2013). Early work with the medium includes that reported by Hodgson (1892, 1898), Hyslop (1901), James (1886, 1890), Leaf (1890), and Lodge (1890). Hall (1910:xxxiii) described Amy E. Tanner (1877–1964) as his assistant at Clark University. However, Tanner was a psychologist in her own right (Pettit 2008).

6 This refers to Mary Baker Eddy (1821–1910) and her Christian Science movement (Gill 1998, Podmore 1909).

7 The Emmanuel Movement was an American psychotherapy and spiritual guidance method (Caplan 1998:Chapter 6, Gifford 1997). Its leaders said its purpose was "*to bring into effective co-operation the physician, the psychologically trained clergyman, and the trained social worker in the*

alleviation and arrest of certain disorders of the nervous system which are now generally regarded as involving some weakness or defect of character or more or less complete mental dissociation" (Worcester & McComb 1909:48–49, italics in original). The movement received much discussion in leading American intellectual reviews (e.g., Gage 1909, Powell 1909)

[8] For mid-nineteenth century discussions about the Fox sisters and early American mediumship, see Capron (1855) and Capron and Barron (1850). Weisberg (2004) presents an overview of the Fox sisters and the controversies surrounding them. On raps said to be produced by the Foxes' joints, see Flint (1851).

[9] For a discussion of fraudulent means to produce raps, see Carrington (1907:Chapter 5).

[10] This is a reference to the highly controversial American physical medium "Dr." Henry Slade (ca 1835–1905; Podmore 1902:Vol. 2:87–91). On fraudulent ways to produce slate writing, see Carrington (1907:Chapter 6).

[11] Two examples are D. P. Abbott (1908) and Carrington (1907). Here, as in other writings, and in other parts of the article, Hall presents his comments without acknowledging that psychical researchers were aware of the issue of fraud and of techniques of fraud from the beginning of the movement. Gauld's (1968) discussion of the early SPR presents many examples of this regarding physical mediumship and other topics. As seen in this, and in other instances through the essay reprinted here, Hall had a tendency to offer advice and issue recommendations under the apparent assumption that his points had not been considered before. While this may have been true among some, such as members of the general public engaged in seances, it did not apply to most psychical researchers.

[12] See also Hall (1920:60). Hall failed to acknowledge that many psychical researchers knew about and explored these topics (du Prel 1885/1889, Gurney, Myers, & Podmore 1886, Hyslop 1906, Myers 1903). There is a small but growing literature about the contributions of psychical researchers to the study of the subconscious mind and of dissociation (e.g., Alvarado 2002, Crabtree 1993, Plas 2000).

[13] These are references to Roumanian psychologist Nicolas Vaschide's (1874–1907) skeptical study of telepathic experiences (Vaschide 1908) and French physician Marcel Viollett's study of mediumship from the point of view of abnormal mental health (Viollet 1908). On this last topic see Alvarado, Machado, Zangari, and Zingrone (2007), Alvarado and Zingrone (2012), and Le Maléfan (1999).

[14] This refers to their teen years.

[15] Frank Podmore (1856–1910), an early SPR researcher, had argued similarly about poltergeists years before (Podmore 1896).

[16] Yes, a few people have. See Lang (1897:69, 137, 280)

[17] On the Watseka Wonder case, see Stevens (1878), and the later discussion of Anderson (1980).

[18] The perispirit was the concept of a semi-physical subtle body believed to bridge the physical body and the spirit discussed by French spiritist authors (Delanne 1897, Kardec 1863, see also Alvarado 2008).

[19] This refers to American physician Morton Prince's (1854–1929) study of Christine L. Beauchamp (pseudonym of Clara Ellen Fowler, b. 1873; Prince 1905) and Swiss psychologist Théodore Flournoy's (1854–1920) study of medium Hélène Smith (pseudonym of Catherine Élise Müller, 1861–1929; Flournoy 1900). Both books are classics in the study of the dynamics of multiple personality and mediumship.

[20] I mentioned Mrs. Piper in my introductory comments (see Note 5). Phinuit, Imperator, and Rector were her spirit controls at different times. On her controls, see Sidgwick (1915).

[21] This is a reference to fugue states, such as the case of Ansel Bourne (Hodgson 1891). Hacking (1996) has discussed aspects of the history of the concept.

[22] This is a reference to a point made in *Phantasms of the Living* (Gurney, Myers, & Podmore 1886:Vol. 1:169–170). However, once again Hall does not represent psychical researchers accurately. Their actual claim, as stated in *Phantasms of the Living*, was somewhat different. It was that the consideration of specific cases as sticks depended

> not on its being so flawlessly strong, as evidence for our hypothesis, that no other hypothesis can possibly be entertained with regard to it, but on the much humbler fact that any other hypothesis involves the assumption of *something in itself improbable*. Third-hand ghost-stories, and the ordinary examples of popular superstitions, have no claim to be regarded as sticks at all . . . and no multiplication of their number could ever make a respectable faggot. But in every one of the examples on which we rest the telepathic hypothesis, the rejection of that hypothesis does . . . involve the assumption of something in itself improbable; and every such example adds to the cumulative force of the argument for telepathy. The multiplication of such examples, therefore, makes a faggot of ever-increasing solidity. (Gurney, Myers, & Podmore 1886:Vol. 1:169–170)

On the statistical study of apparitions, see H. Sidgwick, Johnson, Myers, Podmore, and E. M. Sidgwick (1894).

[23] An example of this, discussed in Hall's *American Journal of Psychology*, was the phenomenon of faith healing (Goddard 1899). Hall was surely aware of the use of hypnosis to produce physiological effects (e.g., Beaunis 1887:Part 1, Chapters 3–9).

[24] An example of a critique of the movement from the medical point of view was presented by Gage (1909). Hale (1971) has argued that the insistence on the subconscious in the Emmanuel Movement associated their enterprise to the occult in the eyes of "psychologists who prided themselves on their scientific Wundtian heritage" (p. 249).

[25] Alfred Binet was a psychologist trained in law and in the natural sciences, while Joseph Grasset (1849–1918), Sigmund Freud, and Eugene Bleuer (1857–1940) were physicians who specialized in mental phenomena and conditions.

[26] Hall's ideas about the unconscious have been discussed by Fuller (1986:68–69). There were many such ideas before and around the time Hall was writing (Münsterberg, Ribot, Janet, Jastrow, Hart, & Prince 1910, Myers 1892; see also Crabtree 1993, Ellenberger 1970, Fuller 1986; and Nicholls & Liebscher, 2010).

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ESSAY

Anomalistics, Pseudo-Science, Junk Science, Denialism: Corollaries of the Role of Science in Society

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Abstract—Recent decades have seen a number of public disputes over scientific anomalies and unorthodoxies, typically framed as science versus pseudo-science. This Essay suggests historical and intellectual context for these controversies. The main point: There is no universally applicable, objective, impartial formula for distinguishing good science from bad science or real science from pseudo—the devil is always in the details. Anomalies and unorthodoxies are defined implicitly by the contemporary state of the art in mainstream science; "pseudo-science" is a pejorative aimed at non-mainstream claims in defense of the authority of established, mainstream science. WWII was a game-changer: In its aftermath, science achieved unprecedented influence over public policies. As the stakes became high, "pseudo-science" seemed no longer a sufficiently powerful pejorative and was superseded by charges of "junk science" and "denialism."

Introduction

"[M]ost cryptids are brand-spanking new." I was rather bemused that this statement was apparently intended as a noteworthy insight, in a purportedly authoritative book about cryptozoology, science, and pseudo-science published by an august university press (Loxton & Prothero 2013).

Of course cryptids[1] were new in the 20th century. Cryptids, like anomalies in general, are by definition things that official science doesn't (yet?) countenance. They had no *raison d'être* until science had become a social authority allowed to proclaim not only that certain things are so but also that certain other things are *not* so. "Science" only became such a nay-sayer in relatively recent times. This Essay describes how the growth and progression of science led to the creating of such categories as "pseudo-science," "junk science," and "denialism."

Anomalistics concerns areas of potential knowledge that are ignored by science—and ignoring shades into denigrating. If an anomalistic topic is persistently investigated and attains public visibility and potential respectability, it is likely to be branded "pseudo-science" by proponents or

defenders of contemporaneous mainstream science. The category "pseudo-science" was created in defense of prevailing scientific beliefs; it is a sociopolitical category, not an intellectual one.

The term "pseudo-science" came into general usage at about the same time as science became a profession and a career (Ross 1962, Daniels 1967), and it became more widely deployed as science became an increasingly influential social force. The parallel histories of science and of pseudo-science show how the conventional wisdom has come to incorporate what the scientific establishment believes.

The stronger the influence of science within society, the higher its status, the more anxious become the true believers in "Science" that there should be no effective challenges to it. Nowadays science is almost universally regarded as the ultimate intellectual authority on which other social institutions, including political ones, depend for their beliefs; correspondingly, challenges to scientific authority are beaten back with extreme vigor.

Corollary to fiercely resisting threats is that the defenders become ever more unyielding and increasingly dogmatic; they can less and less afford to suspect that they might not be right on even the smallest detail. Thus questioning widely held scientific beliefs—"the mainstream consensus"—becomes equivalent to heresy. As mainstream science has become increasingly dogmatic in matters of great public import—DDT, the ozone layer, climate change, HIV/AIDS—the term "pseudo-science" apparently seemed no longer a sufficiently powerful pejorative and had to be replaced by the emotionally more evocative "junk science" and "denialism."

Pseudo-Science Is Defined by Science

Pseudo-science is not the same as non-science: Literary criticism, say, is not science, but it is also not pseudo-science. Pseudo-science constitutes an implicit or explicit challenge to science: It presumes to have *scientific* grounds to question the completeness or validity of prevailing science and even claims to command authentic knowledge that differs in some way from what is claimed by established or mainstream science.

Challenges to science are resisted. Observers as well as scientists have often framed the ensuing controversies in intellectual or rational terms as the "demarcation question": Can one identify objective attributes that science possesses which pseudo-science does not? Best known among proposed candidates are the scientific method, falsifiability, and avoidance of ad hoc modifications to theories (Ptolemaic "wheels within wheels"). None of those candidates have stood the test of time, however. Historians and sociologists in particular have found it easy to locate cases of universally

accepted science that was not done under such rules (Bauer 1992). Laudan (1983) went so far as to declare "The end of the demarcation problem."

There are no objective intellectual criteria to characterize pseudo-science either. Specific topics that have been called pseudo-science at various times have no commonality other than that they were not countenanced by contemporaneous science. In the 1950s, a seminal critique of pseudo-science as subversive of real science (Gardner 1957) mentioned such diverse topics as flat-Earth and hollow-Earth theories; the theories of Velikovsky, Donnelly, and Hörbiger; Forteana;[2] UFOs (then more commonly known as flying saucers); crankish attempts to disprove relativity theory; dowsing; orgonomy (Wilhelm Reich's universal energy) and eccentric sexual theories; young-Earth creationism; Lysenkoism; racism; Atlantis and Lemuria; farfetched properties attributed to the pyramids and their builders; homeopathy, naturopathy, osteopathy; medical quacks such as William Koch (cancer cure) or Edgar Cayce (remote diagnosis by psychic means); Dianetics (which was just then becoming Scientology); Korzybski's general semantics; phrenology, physiognomy, palmistry, graphology; extrasensory perception (ESP) and psychokinesis (PK); and the case of Bridey Murphy, which encapsulated reincarnation and hypnotic regression.

There is no trace of intellectual commonality among these diverse topics: clearly, the criterion of pseudo-science is simply and solely what—according to the author—does not fit with what science knows. The book specifically avoided such topics as astrology as being "so far removed from anything resembling science" (Gardner 1957:14), thereby acknowledging implicitly that the criterion for inclusion as pseudo-science is an overt and not immediately and obviously implausible challenge to science; pseudo-science is "a historically relativistic [category] . . . whatever the scientific establishment of the time—for whatever reason—is trying to discredit. . . . [A]n epithet hurled by members of the scientific *and social establishment*" [Mauskopf 1981; emphasis added]. That emphasized phrase recognizes the fact that by the 1950s, influential social institutions had become stakeholders in what counts as proper science. This is illustrated as the pejorative "pseudo-science" as deployed by politicians, social activists, lawyers, and others who really do not understand what science is and what it is not; they simply parrot what the mainstream scientific consensus happens to be because it belongs to their social clique.

The absence of objective criteria explains why what is called pseudo-science at some times and in some societies might not be called pseudo-science at other times and in other places: It all depends on what the state of established science is at a given time and place. "If you want to know what science is or has been, show me the contemporary pseudoscience"

(Gordin 2012:3). Some topics, for example alchemy or astrology, were socially accepted in the past but later became pseudo-science; Newton (1643–1727), an iconic figure of early modern science, spent much effort on Biblical exegesis and studies of alchemy; leading scientists in the 19th century made extensive investigations of mediumship and other claimed psychic manifestations that are nowadays branded pseudo-science; and sea serpents, too, were respectable subjects of investigation not much more than a century ago. Other phenomena, for instance meteorites (Westrum 1978), were once pseudo-science but later became science. Other matters again have experienced several back-and-forth classifications as science or pseudo-science, for instance biological effects of electricity and magnetism (Bauer 2001a:119–136).

Hegemony of Science and Public Frustration

Science attained even greater prestige than earlier as a result of its role in World War II, when it delivered victory-bringing atomic bombs, radar, penicillin, and many other less prominent advances. Government support for scientific and medical research and education expanded enormously through the newly founded National Science Foundation and the greatly expanded National Institutes of Health. Public media became replete with items about matters scientific.

The long-held view is that science can make the natural world comprehensible. Frustration then ensued when, in this modern age with science taking tremendous strides to greater knowledge, science appeared nevertheless to have no useful information about matters of great public interest: What are those "flying saucers"? What are the big creatures in Loch Ness, one of which was captured on film in 1960? What is science's quarrel with the interesting scenario that Immanuel Velikovsky inferred from innumerable historical and geological sources? What is not scientific about the rigorous experiments by Rhine at Duke University that demonstrated a human capacity for extrasensory perception?

Frustration led to the founding of a variety of new organizations aiming to acquire knowledge about these things that science failed or even refused to offer: the Parapsychological Association (1957), the Loch Ness Investigation Bureau (1962), a number of groups interested in UFOs (Aerial Phenomena Research Organization [APRO] (1952) and others now-defunct; as well as still active ones such as the Mutual UFO Network [MUFON] (1969) and the Center for UFO Studies [CUFOS] (1973)). Roger Wescott (1980) recognized this intellectual agitation by coining the umbrella term "anomalistics."[3] Some well-established scientists launched the Society for Scientific Exploration[4] in 1982 as an encompassing organization to

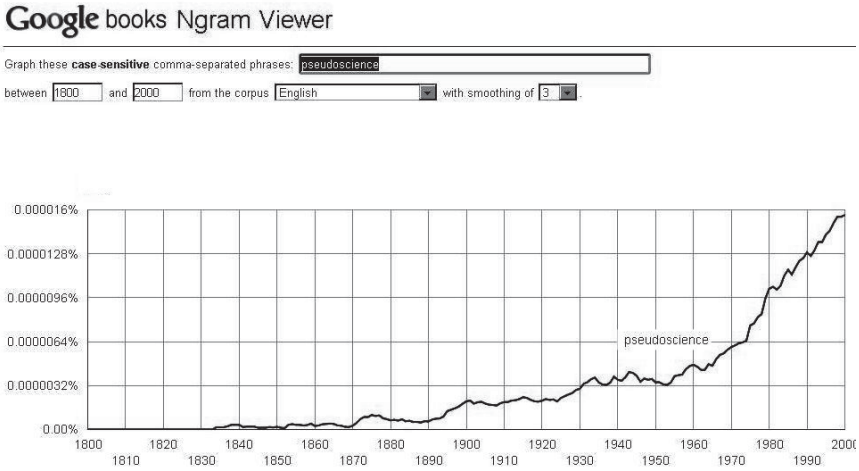

Figure 1. Presence of the term "pseudo-science" in books in English. It became steadily more frequent from the early 19th century into about the middle of the 20th century, then increased more rapidly.

foster rigorous consideration of these matters that were being ignored by mainstream research; the *Journal of Scientific Exploration* began publication in 1987 and in 2009 was joined by *EdgeScience*.

Devoted advocates of mainstream science did not take kindly to these ventures. Specifically to combat the perceived flourishing of such "pseudo-science," in 1976 philosopher Paul Kurtz founded the Committee for Scientific Investigation of Claims of the Paranormal (CSICOP; the name changed in 2006 to Committee for Skeptical Inquiry, CSI).[5] Its journal, *The Zetetic*, also began publication in 1976, changing its name to *Skeptical Inquirer* in 1978.

The increase in perceived challenges to science after WWII is reflected in the more frequent appearance in print[6] of the defensive epithet "pseudo-science" (Figure 1). Pseudo-science had earlier been of concern primarily or even solely to the scientific community. But as science became increasingly influential in public policy after WWII, challenges to science began to be seen as challenges to the social order, not just to matters internal to the scientific community.

Scientists had grounds to fear the consequences of challenges to scientific understanding from outside the scientific community. Nazi Germany had enacted "Aryan science" which forbade anything attributed to Jews, including relativity theory (Lenard 1938). In the Soviet Union, chemistry and physics had been forbidden from employing the insights of quantum mechanics, and genetic science was crippled by Lysenkoism.

Gordin (2012, especially chapter 3) has argued that the latter example in particular stimulated prominent scientists to react violently when Velikovsky's science-challenging books met widespread public acclaim in 1950 and later years.

This was the intellectual milieu in which spokespeople for science—self-appointed spokespeople with sometimes doubtful credentials for it—began to agitate against "pseudoscience" as a perceived threat to the social order. When Gardner (1957) published the first version of his compendium of pseudo-science in 1952, he could point to only three similar earlier efforts, by Daniel Hering (1924), David Starr Jordan (1927), and Joseph Jastrow (1936). By contrast, critiques of pseudo-science became increasingly frequent and vehement from the 1950s on, not only in the *Skeptical Inquirer* but also in books, for example Cohen (1965), Moore (1972), Sladek (1973), Fair (1974), Evans (1975), Story (1976), White (1976), Marks and Kammann (1980), Abell and Singer (1981), and Gardner (1981). Prominent scientists took leave from their science to attack Velikovsky (Bauer 1984) and to propose definitions and examples of what constitutes pseudo-science (Bauer 1984:Chapter 8).

Two matters of semantics need to be noted, relevant in particular to groups like CSICOP: the mis-use of "skeptical" when debunking is actually meant, as in "Skeptics" societies and publications; and the mis-use of "paranormal" to include not only everything not accredited by contemporaneous science, even the possible existence of perfectly material and natural entities, but often also anything of a religious bent—a fairly natural corollary of CSICOP's founding under the initial auspices of the American Humanist Association.

Science in Earlier Times and the Relative Lack of Pseudo-Science

Many notable "scientific" discoveries and technological inventions had been made in the centuries and millennia preceding what is regarded as "modern" science, but they had not challenged prevailing social authorities. Builders of megaliths and pyramids several millennia ago understood much about astronomy, applying what they knew in the service of established beliefs and authority. Greek philosophy, not Greek science, was sometimes seen as a threat to the social order. The notable achievements of Arabic science produced no authority-challenger analogous to Galileo. "Modern" science is generally considered as beginning in the 16th century in Western Europe, but it did not present a serious challenge to established authority until the middle of the 19th century.

The iconic figure in the birth of modern science is Galileo, widely viewed as an empirical proto-scientist in opposition to a non-empirical, non-

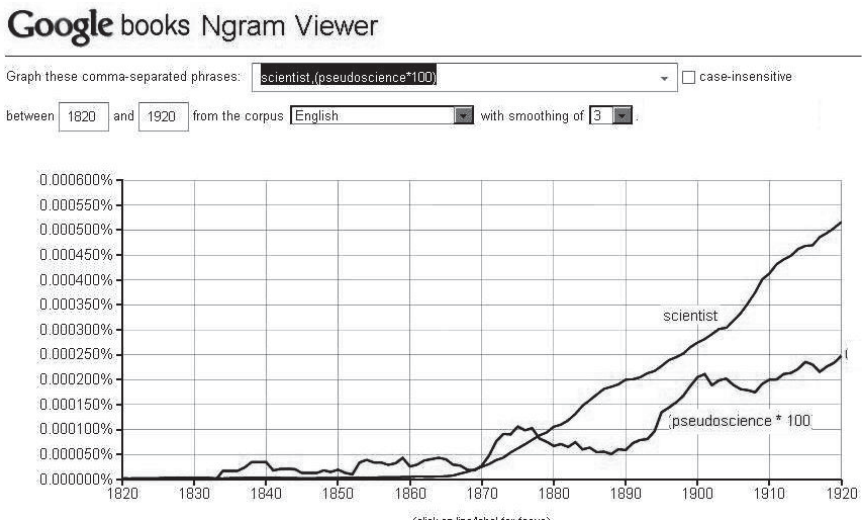

Figure 2. The defensive pejorative epithet "pseudo-science" came into use at about the same time as "scientist" and when the latter could describe a career and then the member of a professional guild (Ross 1962, Daniels 1967).

scientific Church. Historians argue for something more nuanced, not so much differences over heliocentricity or empiricism or "science" but centrally over the question of *intellectual authority*: Galileo's insubordination would have been just as unacceptable to his Pope had it been over something other than whether the Earth goes round the Sun.

Although the Catholic Church discerned Galileo as a challenge to its authority, it was only an individual's challenge. During the 17th and 18th centuries, increasing knowledge about Nature laid the groundwork for science to mount an *institutional* challenge to religious authority on questions about the workings of the world. In the 18th century, proto-geologist James Hutton concluded that geological formations had resulted from very long, slow processes. Also in the 18th century, Galvani had found that he could make frogs' legs seem alive by stimulation with his batteries ("piles"). But it was only in the 19th century that the accumulation of such discoveries led to inferences that challenged organized religion. It was also in the early 19th century that the word "scientist" (Ross 1962, Daniels 1967) first came into use, corresponding with the time when science could be a career rather than an avocation; and it seems unlikely to be coincidental that the term "pseudo-science" began to enter public discourse at about the same time (Figure 2).

Galvani's work inspired speculation that life might be breathed into clay by electricity as well as by God, as in the 1818 novel *Frankenstein, the Modern Prometheus* (commonly but perhaps mistakenly [Lauritsen 2007] attributed to Mary Shelley). In 1828, Friedrich Wöhler demonstrated that the "inorganic" ammonium cyanate could be converted readily into the "organic" urea: Living beings were seen to be made of the same stuff as makes up inert matter. The challenge to religion reached its crisis when Darwin proposed that evolution resulted from natural selection. As Charles Lyell pointed out, Hutton's stratigraphic analyses indicated that the Earth was old enough to accommodate Darwin's proposal. Thus traditional religious views were challenged directly over the age of the Earth as well as over the genesis of species including human beings.

The apparently *mindless, purposeless* mechanism of natural selection constituted a direct challenge to the belief that Godly purpose governed everything on and outside the Earth, and it was anathema to most religious authorities (as it still is to some). Before that, from about Galileo's time until about the middle of the 19th century, science had been a largely descriptive enterprise often described as "natural philosophy." Among those active in its pursuit had been clergymen to whom learning about Nature was synonymous with worshipping the Creator. After Darwin, however, church people faced novel problems in reconciling scientific knowledge about the natural world with religious teachings and traditions. Eventually the great proportion of Christian groups came to allow science authority over natural phenomena, restricting religious authority to matters of human behavior (though small fundamentalist sects remain recalcitrant; and evolution is still not taught in such Islamic institutions as universities in Turkey as well as fundamentalist colleges in the USA).

From the middle of the 19th century, "Science" progressively supplanted religion as the ultimate recognized intellectual authority. Science was now a career, a profession (Ross 1962, Daniels 1967), and it eventually became an influential social institution. Tremendous scientific advances accumulated during the second half of the 19th century, which an historian aptly described as The Age of Science (Knight 1986): Understanding grew about electricity and magnetism, atomic theory, and the regularities in properties and compounds of the chemical elements as revealed by Mendeleev's Periodic Table (ca. 1870). By the end of the 19th century, Science had acquired high intellectual authority, not to say hubris, as when T. H. Huxley proclaimed Science's victory over religion by preaching "Lay Sermons" for "the Church Scientific" (Knight 1986:3–4). Huxley's enthusiasm might be seen as the founding of *scientism*, the view that science and only science offers a reliable path to Truth. Some enthusiasts ventured that science had

already unraveled all the major mysteries and what remained was to fill in details.

Scientism almost at once suffered a setback. In what has been called the Second Scientific Revolution, at the turn from the 19th into the 20th century confidence in amassed knowledge was shaken by entirely unexpected discoveries: radioactivity, atoms that were sometimes unstable, transformations of one element into another, X-rays, and quantum phenomena and associated discoveries.

Science Supersedes Religion as Social Authority

The staggering scientific advances during WWII brought science back onto a pedestal similar to its status at the end of the 19th century. Our times are sometimes called an age of science, but it might more appropriately be called an age of scientism. Although not many people will acknowledge, probably not even to themselves, that scientism is their faith, many actions and inferences reveal that scientism nowadays has many adherents within and without the scientific community.

For the general public, the force of science is seen in the degree to which "scientific tests have shown" trumps the rhetorical impact of "tests have shown," even though the two statements have the same intrinsic meaning (Bauer 2001b:Chapters 1–3). CSICOP and "Skeptics" groups reveal clearly enough through their writings and initiatives that their only touchstone of trustworthiness is what science happens to be saying. Another indicator is the fact that some religious sects felt the need to enlist "science" in support of their religious beliefs: "Creation science" or "scientific creationism" emerged in the 1960s as an attempt to have science support fundamentalist Biblical interpretation (Whitcomb & Morris 1961); and "intelligent design theory" represents the same ambition.

Before science became the touchstone of intellectual authority, that role had been filled by religion and religion-sanctioned political bosses. In such circumstances, those who harbor nonacceptable beliefs are traitors or heretics. When Science supplanted Religion as the ultimate intellectual authority, challenges to social authority were less frequently condemned as heretical and more frequently as pseudo-scientific (Figure 3).

Science and Politics

Following WWII, scientists began to see the active influencing of political policies as part of their proper role. An early example was the *Bulletin of the Atomic Scientists*, concerned primarily but not only with military and peaceful applications of atomic energy. Academe also began to recognize

Figure 3. As science superseded religion as the ultimate intellectual authority, challenges to social authority were pejoratively labeled pseudoscience rather than heresy.

the rising public significance of science by developing courses and programs on "science and society"; these fostered interactions among historians, philosophers, sociologists of science, and political scientists as well as engineers and "hard" scientists, which led eventually to the field nowadays recognized as Science & Technology Studies (STS).

The atom-bomb project not only underscored the potential national importance of science; it also represented for scientists a precedent for the initiation of significant public policy from within the scientific community. The feasibility of atomic weapons had become known to the national government not through the usual channels but directly from the scientific community, exploiting the high prestige of Albert Einstein to gain access to the President. Also from WWII onward, the President routinely includes a Science Advisor as part of the White House staff. In recent decades, several initiatives of national and global importance have started as a result of claimed scientific discoveries, for example concerning the claimed destruction of the Earth's ozone layer by refrigerants and similar chemicals and the effect of carbon dioxide on global climate.

The influence of science on public policy brought a degree of collateral damage. In the 1960s, the perceived participation of "science" in The Establishment made antagonism against science part of the "counterculture," but the most serious damage has come through attempts to make science serve partisan political purposes. Publicly professed opinions about matters of science thereby become determined by political affiliation.

Differing political views and ideologies find specific interpretations of scientific matters more to their liking or less to their liking, with the result that differences over interpretation of scientific data become politicized. For example, the hypothesis that human generation of carbon dioxide is responsible for climate change (AGW, for anthropogenic global warming) is welcome to environmental activists and unwelcome to people concerned with industrial economics. As a result, scientists who question the evidence for AGW are accused by AGW advocates and activists of being right-wing collaborators with conflicts of interest that allegedly vitiate their professional expertise. Liberals and Democrats are supposed to regard AGW as proven, Conservatives and Republicans are supposed to deny that (Bauer 2012a). Similarly, arguments divisive along political lines have concerned research on human stem cells and the question of what defines the beginning of human life. The teaching of biology is beset by continuing controversies about how or even whether evolutionary theory should feature in textbooks and classrooms.

How Modern Science Has Changed

It is not widely recognized that "modern" science, acknowledged as originating half a millennium ago, is nowadays a different *kind* of thing than it was then. The conventional wisdom maintains a view of science based on something like the earliest days of modern science, namely, that science is an objective, disinterested pursuit of authentic knowledge by people of outstanding intellect whose only aim is to uncover the best possible understanding of the natural world. But pervasive conflicts of interest, external control of research directions and funding and publication, and politicization of the interpretation of scientific data make today's science nothing like the science of even a century ago. Those who appear plausibly to speak for science (Ross 1962, Daniels 1967) are still treated by media, public, and policymakers as though they were disinterested purveyors of objective understanding when instead they are increasingly self-serving agents of commercial or political forces as well as of their own status and the prestige of their profession.

There have been three distinct eras of modern science (Bauer 2013). From the 16th century into the early 19th century, science was an avocation. Beginning in the 19th century, it became a profession (Ross 1962, Daniels 1967). From about the middle of the 20th century, science has been an institution just as influential as the institutions of economics, finance, media, politics, and religion. Nowadays scientific experts influence national and international activities through such institutions as the World Health Organization, UNAIDS, and the International Panel on Climate Change,

just as economic experts influence national and international activities via the World Bank and the International Monetary Fund.

The change from profession to institution was in essence from a largely self-organized cooperative activity of independent intellectual entrepreneurs—which is how the conventional wisdom still thinks of "science"—to a centrally organized activity controlled largely by factors and forces not intrinsic to the pursuit of scientific research.

As science gained social prestige, preferment, and access to greater resources, the social sciences sought overtly to align themselves with the "hard" physical sciences in order to qualify as "Science" and enjoy the associated benefits in resources and prestige. A corollary has been the tendency to treat "experts" in every field as though they had access to knowledge as reliable and usefully applicable as knowledge in the physical sciences is agreed to be.

However, in economic matters it is universally understood that the experts suffer conflicts of interest owing to their political views: left- and right-wing economists, or progressive and conservative economists, draw significantly different conclusions from any given set of data or "facts." By contrast, it has not so far been widely understood that analogous conflicts of interest play a role in the interpretation of "scientific" data. In an increasing number of fields within science, a single point of view has gained hegemony and become dogma, and proponents of that view have been able to enlist institutions outside science to enforce that dogma through suppression of competent professional minority opinions (Bauer 2012b).

That a purportedly "scientific" viewpoint should attain hegemony to the extent of suppressing differing interpretations held by competent researchers and observers runs counter to the traditional view of science as an empirical enterprise that never attains final closure. Proponents of the dogmas resort to rhetorical devices like claiming a "consensus" of experts, which Michael Crichton aptly labeled

> the first refuge of scoundrels. . . . Whenever you hear the consensus of scientists agrees on something or other, reach for your wallet, because you're being had. . . . The greatest scientists in history are great precisely because they broke with the consensus. There is no such thing as consensus science. If it's consensus, it isn't science. If it's science, it isn't consensus. Period. . . . Consensus is invoked only in situations where the science is not solid enough. Nobody says the consensus of scientists agrees that $E = mc^2$. Nobody says the consensus is that the sun is 93 million miles away. It would never occur to anyone to speak that way. (Crichton 2003)

Crichton's understanding of science and common sense have not yet

penetrated the halls of power, however, so monolithic viewpoints have shaped national and international policies over matters of science and medicine, for example global warming and HIV/AIDS. Minority views on these and similar questions represent a most threatening challenge: to the mainstream scientists, who have inveigled policymakers and national leaders into expensive, far-reaching actions and whose careers and social status are in jeopardy if their advice turns out to have been flawed; and to the decisionmaking national leaders whose reputations and careers would be in jeopardy if it turned out that they had allowed themselves to be misled *even as competent dissenting experts had tried to warn them of it*.

The potential threat is so profoundly damaging that the old pejorative "pseudo-science" seems no longer adequate: In the last few decades, when "science" has been responsible for major public policies and actions, for example over purported destruction of the ozone layer or the dangers of carbon dioxide, questioning mainstream views is now labeled as "junk science" (for example, Huber [1991] and Agin [2006]), and its proponents are called "denialists" (for example, Kalichman [2009] and Specter [2009]), a deliberate emotionally evocative analogy with those who deny that the Nazis had perpetrated a Holocaust of genocide against Jews and gypsies; see Figures 4 and 5.

Dissenting scientists find themselves ostracized, disinvited from conferences and interviews, unsuccessful when seeking research funds, and rarely able to get their work published in top professional journals (Bauer 2012b:Chapters 2 & 3).

Some activist dissidents have turned the tables and returned fire by pointing to the "denialism" of HIV/AIDS theorists regarding the toxicity of antiretroviral drugs (Bauer 2010a, 2010b) and the "junk science" of AGW and the dangers of second-hand smoke[7] (Milloy 2001).

In a Nutshell: The Interplay of Science, Anomalistics, Pseudo-Science, Junk Science, and Denialism

As Science became an integral part of the Establishment, it acquired constraints, including what topics it could choose to study. External influences now control what science is done and published, and those external influences do not have the background to make the best judgments about where research is likely to be most fruitful. Anomalistics became necessary to fill the role that natural philosophy, early modern science, had played—namely, the pursuit of knowledge about everything that interests human beings.

In that sense, anomalistics and science are complementary just as official medicine and alternative medicine are in principle complementary.

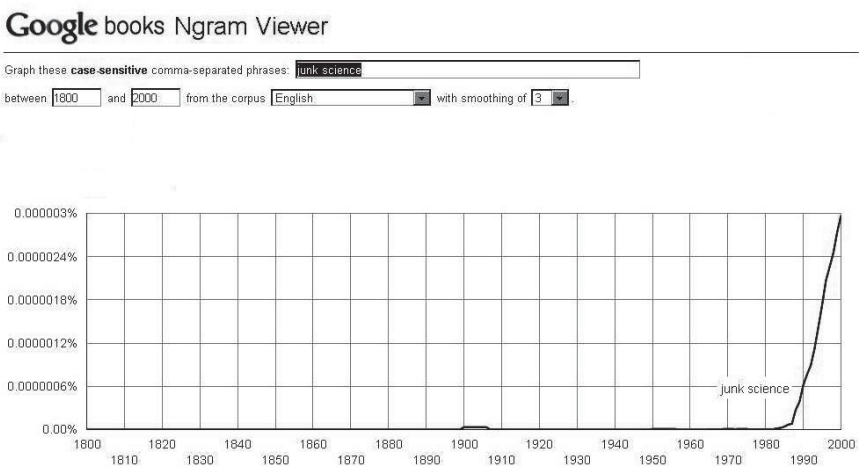

Figure 4. The epithet "junk science" started to be deployed when questions were raised about the predictions of doom from carbon dioxide emissions and the supposed destruction of the ozone layer by refrigerant chemicals.

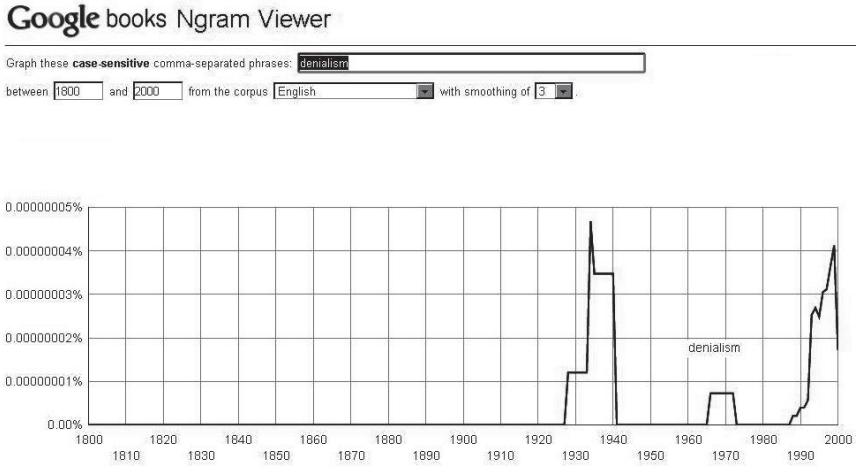

Figure 5. The pejorative "denialists", earlier associated with denying the horrors of the Nazi regime, became deployed against those who raised questions about the predictions of doom from carbon dioxide emissions, from supposed destruction of the ozone layer by refrigerant chemicals, and whether HIV had been proven to cause AIDS.

However, practitioners of the mainstream ventures see their complements as challenges rather than potential allies. Perhaps that is inevitable, just as political hegemonies seem unable to make even the smallest accommodations to minority wishes.

At any rate, current circumstances find the mainstream consensuses over matters of science and medicine exerting effective control over research and dissemination of information, to a degree that extends to active persecution of those who hold different views. A contributing factor to these circumstances is the widespread ignorance about the nature and history of science: that "science" has changed out of sight during the last century, and nowadays is the captive of conflicting interests; that science has always progressed by overturning mainstream consensuses; that what is denigrated and persecuted as "pseudo-science" and "junk science" and "denialism" just because it challenges the consensus might become the accepted mainstream consensus of a future time.

Not *all* challenges to mainstream dogmas necessarily have merit, of course. Implying that they do has been aptly described as the Galileo Gambit or Galileo Fallacy.[8] But that fallacy is widely understood as such, while the unwarranted and increasing dogmatism of mainstream science and medicine remains little-recognized.

Notes

[1] Cryptids are creatures known only from human testimony, looked for by cryptozoologists, and which may or may not exist: Nessie, Bigfoot, and the like.

[2] Named after Charles Fort, who published collections of events or phenomena awaiting scientific explanation.

[3] Wescott apparently used the term in an oral presentation in 1973 (Wescott 1975) but in print only in 1980 (Wescott 1980).

[4] http://www.scientificexploration.org

[5] http://www.csicop.org

[6] Data about frequencies of usage have become available through the Google Ngram Viewer.

[7] JunkScience.com

[8] http://rationalwiki.org/wiki/Galileo_gambit

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LETTER TO THE EDITOR

Exaggerated Emphasis

In the Fall 2013 issue of this Journal, Jerome Clark reviewed my book *Zones of Strangeness: An Examination of Paranormal and UFO Hot Spots* (*Journal of Scientific Exploration 27*(4):735–737). I was surprised to see that so much of the review is taken up with an obsessively pedantic diatribe against my use (misuse, in Clark's view) of exclamation marks. (Without naming the person, he also accuses another writer of the same 'offence.')

One could argue that exclamation marks add color and nuance to writing, but Clark seems to have an almost phobic aversion to them, since he notes: "Exclamation marks in other than quoted material, including the title of a book, can be found under my byline. In each case, I was not responsible, and the marks were inserted editorially without my consent."

With respect to my book, Clark is guilty of gross exaggeration, since he writes:

> When one removes end notes, bibliography, and index, one is left with 490 pages of text. Barely one is deprived of an exclamation point, and many boast multiple ones, at times in succeeding sentences.

From that, a reader might wrongly infer that the text contains some 490 or more exclamation marks. In fact, the book as a whole contains 173 (including exclamation marks within quotations).

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BOOK REVIEW

UFOs, ETs, and Alien Abductions: A Scientist Looks at the Evidence
by Don Donderi. Charlottesville, VA: Hampton Roads, 2013. 231 pp. $22.95. ISBN 978-1571746955.

Many books about UFOs appear each year, yet few of these books are worth reading; *UFOs, ETs, and Alien Abductions* is one of those few.

The author, Don Donderi, holds a doctorate in psychology and spent most of his career at McGill University in Montreal as a professor, dean, and researcher. His specialties are human visual perception and memory, with several books and more than one hundred research papers and technical reports to his credit. He began to read about UFOs when he was ten years old. The interest has stayed with him throughout his life and motivated him to investigate several sightings as the opportunities arose. In 1968 he participated in a review of occupant cases as a consultant for the National Investigations Committee on Aerial Phenomena (NICAP), then the leading U.S. civilian UFO investigations organization. In the 1990s, when abductions dominated ufology, he consulted on how to interpret the results of a Roper Poll designed to uncover the prevalence of abduction-like experiences in the general public, and participated in major meetings such as the 1992 Abduction Study Conference held at MIT. He further lent his psychological expertise to a personality test for separating simulated abduction claims from honest experiential reports, and to an experiment that compared symbols reported by abductees with symbols imagined by non-abductees.

In short, Donderi writes about UFOs from a position of long familiarity with the subject and experience with hands-on UFO research, while also bringing the expertise of a seasoned academic psychologist to issues of who sees UFOs and how to distinguish truth from error in observation. This combination equips him with the rare perspective of scientific objectivity combined with an informed and sympathetic curiosity. He also states his general position with refreshing open-mindedness:

> When our senses turn up something new in the world, there *is* something new in the world, and it is an obligation of a trained professional who understands the human senses to report on it.

Though he is aware of the pitfalls of human observation and memory, he does not dismiss the anecdotal evidence of eyewitnesses out of hand. The reader can look forward to a balance of critical rigor and fairness toward UFO evidence seldom found in scientific treatments.

The book can be divided into two general arguments: The first establishes UFOs as a distinctive phenomenon worthy of acceptance as an extraordinary reality and the second considers why government and scientific authorities have largely ignored, denied, or disparaged the subject. Donderi founds his case for the reality of UFOs on the striking observations that responsible people have continued to report from 1947 to the present. The original "flying saucer" sighting of Kenneth Arnold on June 24, 1947, introduced the mystery of seemingly metallic vehicles flying at impossibly high speeds, which no nation on earth possessed the technology to build. Consistently over time competent but startled witnesses would report further encounters with flying objects that defied conventional explanation yet suggested the presence of superior technology. One example is the case of the luminous saucers that flew twice over Washington, D.C. in the summer of 1952, where radar tracked them, witnesses on the ground watched them, and jets chased them to no avail. Another case occurred in 1957 when the crew of an RB-47 tracked an unknown, radar-emitting object over some 800 miles from the Gulf Coast of Mississippi across Texas and into Oklahoma. In 1973 an Army Reserve helicopter piloted by Lt. Lawrence Coyne nearly collided with a cigar-shaped object bearing several lights. The cockpit filled with green light and the helicopter climbed nearly three thousand feet even while Coyne held the control lever for descent.

Cases of this caliber recur throughout UFO history. With multiple high-quality witnesses, instrumental confirmation, consistent patterns, and no credible conventional explanation, such reports ought to demonstrate a significant UFO phenomenon beyond a reasonable doubt. In reality this has not been recognized. Government authorities and official science continue to treat the phenomenon as if no worthwhile evidence existed. No other motive for this behavior was implied in the 1950s than the narrow Cold War concern of governmental and military authorities' desire to prevent public alarm, while scientific rejection owed much to hubris and the unwillingness to pay close attention to the evidence. These habits of thought have carried over even as more and more "touchstone" cases have confirmed not only the existence of UFOs, but their probable extraterrestrial origin.

Donderi characterizes the history of UFOs from 1947 until about 1980 as a "chronology of doubt," a time when UFO reports created shifting, often entangling currents of uncertainty among interested parties. During this period responses to UFO evidence missed the mark from nearly every

quarter. The public took a keen interest in the subject and civilians shouldered an investigative task that military and scientific authorities usually shirked, but UFOs also attracted charlatans, hoaxers, self-promoters, and religious seekers along with well-intentioned but inept amateurs. As a result, civilian ufology sewed together a crazy-quilt of studies, claims, and speculations that ranged from meritorious to loony and created an incoherent image of what UFOs encompassed. Official science approached the subject with doubt and too often issued condemnations based on the antics of its proponents rather than on the merits of the evidence. A new breed of professional skeptics emerged, beginning with Harvard astronomer Donald Menzel, who explained UFOs in conventional terms but seldom confronted the reported facts of the best cases. These seemingly authoritative but spurious "explanations" gave other scientists license to ignore the phenomenon out of hand but exasperated informed members of the public and shook their trust in scientific authority.

The military took an early and serious look at this new phenomenon. As early as 1948 one faction of Air Force Intelligence concluded flying saucers were most likely extraterrestrial vehicles; a second faction concluded that no evidence indicated the saucers threatened national security, manifested exceptional new technology, or came from outer space. By 1953, the CIA-sponsored Robertson Panel essentially transformed doubt into policy by declaring that UFOs were not real and needed to be stripped of the public interest they had "unfortunately" acquired. Before Robertson, when Edward J. Ruppelt headed the Air Force's Project Blue Book, he carried out serious investigations and amassed evidence for a genuine mystery. Afterward, in obedience to the Robertson conclusions, UFOs ceased to be a matter to investigate and became no more than a public relations headache for the Air Force. No meaningful effort was made to understand UFOs even as new cases fascinated the public and mocked the flimsy explanations concocted by low-ranking personnel with little thought and less evidence.

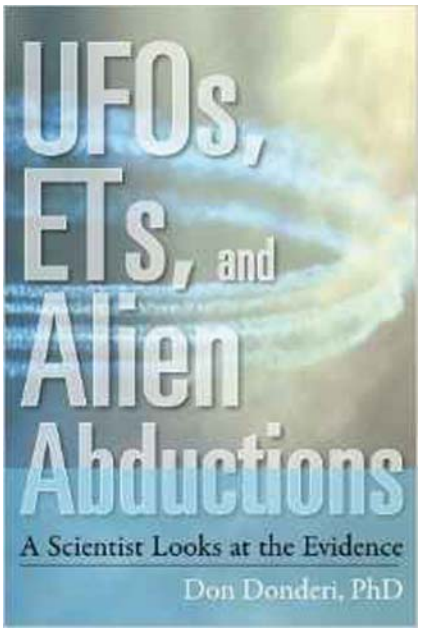
The climax arrived in the mid-1960s when prolonged waves of UFO activity attracted favorable media attention and provoked Congressional leaders to call for investigations. Long anxious to get out of the UFO business, the Air Force agreed to fund a scientific study led by physicist Edward Condon at the University of Colorado. This Condon Committee promised to satisfy military, scientific, and civilian interests in a rigorous but fair study, only to fall apart in a fiasco that ended in 1968 with Condon writing that there was nothing to UFOs even as he ignored the contrary evidence collected by project investigators. Outraged ufologists felt betrayed and gave up on the scientific establishment for any meaningful answers.

In the aftermath of this disappointment, a new ufology emerged. Some dissident scientists, most notably James McDonald and J. Allen Hynek, were already speaking out against the prevailing scientific verdict with evidence and argument that UFOs were both real and perhaps otherworldly. New organizations such as Hynek's Center for UFO Studies (CUFOS) set out to fill the vacuum left by official negligence. The UFO phenomenon itself was also acquiring a different complexion. In the 1950s most UFOs were described as distant flying disks or lights in the night. A gradual change toward close encounters grew throughout the 1960s and 1970s as the era of doubt transitioned into an era of certainty.

By the 1980s, two changes in the phenomenon helped to solidify the case in its favor: one was the growing prevalence of extremely large UFOs, some round and some V-shaped, flying low and slow over the landscape. For several years during the early 1980s, lighted "boomerang"-shaped objects "wide as a football field" appeared to hundreds of witnesses along the Taconic State Parkway in New York and Connecticut. In 1989 large triangular objects with lights invaded Belgium and jet fighters went in pursuit, while in 2008 another large UFO flew near Stephenville, Texas, and was tracked on radar and (perhaps) pursued by jets. Multiple motorists along a 200-mile stretch of highway in the Yukon saw a giant tub-shaped UFO in 1996. The object was lit up like a Christmas tree with multiple lights, and triangulation indicated a diameter as large as one mile. UFOs seemed to have traded their elusiveness for deliberate and even provocative display.

The second major change was the emergence of abductions. Reports of humanoid beings associated with UFOs date back to the 1950s, but a definite pattern and purpose took shape only with the "interrupted journey" of Barney and Betty Hill in 1961. A UFO followed them through a remote mountain valley in New Hampshire as they drove at night, finally drawing near, then abruptly disappearing. The Hills continued their drive home but arrived some two hours later than expected. Betty soon experienced nightmares and Barney suffered from anxiety that worsened over the following months until he sought medical attention. Hypnosis then uncovered memories from both the Hills that a party of humanoid beings had stopped them, escorted them aboard the landed UFO, and subjected them to a medical examination. Some communication followed with one of the beings, then the Hills continued their journey and lost conscious memory of their onboard experiences. As strange as the story sounded, the Hills held excellent reputations and their doctor was a distinguished psychiatrist; physical evidence lent further support.

By itself the Hill case stood as an isolated curiosity, but it did not remain



in isolation for long. Other people experienced intense fears without apparent cause and associated in some way with a light or object in the sky. Two camp counselors one afternoon in 1968, four campers in Maine in 1976, and two women driving across Kansas one night in 1989 underwent "missing time" experiences like the Hills's, and these people recovered memories of capture and examination similar to the Hills's. Many more creditable reports confirm a recurrent pattern of complex events and similar descriptions of the entities and the interior of the UFO. The standard arguments against the abduction evidence cannot stand up: Hypnosis does not "cause" the memories because many abductees recall part or all of their experiences without resort to it; an investigator with an agenda does not instill false memories because other investigators with different agendas uncover the same story. Psychological tests of abductees reveal that they represent a normal cross-section of society without any tendency to psychopathology or characteristics such as fantasy-proneness. They act more like people who have had unnerving experiences than people susceptible to imagining strange things. The consistency of the abduction story, its compatibility with what else we know about UFOs, the normalcy of the abductees, and the availability of supporting evidence such as independent recollections of unpublicized symbols all converge in pointing toward the abduction phenomenon being a reality.

Donderi has guided the reader through the clutter of UFO data to the truly informative pieces, then fits those pieces together into a meaningful whole: Countless, often high-quality witnesses around the world have observed UFOs for decades. Instrumental evidence affirms that UFOs are physical objects with solidity and weight. UFOs are unlike any known aircraft. They are capable of great speed and maneuverability and they also can cause physical effects on human bodies and interfere with electronic devices. UFOs are strange but limited in their strangeness; they manifest recurrent patterns of appearance and action. The occupants that sometimes

accompany UFOs suggest that these objects are crafts flown by unearthly beings. The activities of these beings show that they take an interest in humans, perhaps as scientific specimens or for some other program, but in any case the numerous reports of abductions indicate purposeful interaction of these visitors with humans.

Science as we learn about it in schoolbooks starts with the observation of an anomalous phenomenon. A process of repeated observations that leads to generalizations about the phenomenon is induction, and similar observations from diverse witnesses provide evidence for objectivity. Discovery proceeds to understanding from recognition of consistent patterns and elimination of competing explanations to establish a firm hypothesis or theory for further testing. UFOs have fulfilled these requirements and convinced many people. Scientists are not among them, and, in what seems almost as strange as the phenomenon itself, they show no interest despite the cumulative evidence. In one short but enlightening chapter Donderi offers pointed explanations for this baffling oversight.

In reality, science does not work according to the straightforward popular ideal. As Thomas S. Kuhn pointed out in his study of scientific revolutions, most science is "normal" science, a process that operates within a theory or system of theories that comprises a prevailing paradigm of understanding. This paradigm has proven so successful that it stands as "truth as we know it," and most scientific activity serves to apply this paradigm to more and more of the natural world. However, along the way, some parts don't seem to fit in. These anomalies present puzzles that scientific effort solves by finding a place for them within the paradigm, or in rare instances, the anomalies accumulate and present a serious conceptual challenge to accepted understanding. Such a crisis may lead to the growth of a competing paradigm that embraces not only the explanations of its predecessor, but also the anomalous evidence. The new and better paradigm then overthrows the old, like the Copernican system replaced the Ptolemaic view of the universe in a famous scientific revolution.

UFOs seem like just the sort of anomalies to revolutionize current understanding, but no one should expect scientists to welcome them. The psychologist and philosopher William James pointed out that science is extremely conservative. Scientists do not like evidence that doesn't fit, and they are much more likely to ignore it than to expend time and effort on it, much less risk the loss of a paradigm that has served them so well throughout their careers. Scientists are human, prone to reject UFOs because of a vested individual interest in preserving the current paradigm, and because of social pressure not to buck a congenial status quo. Real or not, UFOs have an uphill fight of the most serious kind to attract scientific favor. In one of

his most significant insights, Donderi characterizes modern science as not interested in asking *does* something exist, but rather asks *can* it exist, and, if so, how does it work? Such an approach makes scientists sound like the priesthood that once denied the earth moved around the sun, but this attitude is a direct result of the outstanding success of scientific practice based on paradigms that place understanding first and observation second. UFOs clearly land on the wrong side of the question because they are strong in observation and experience but run afoul of accepted expectations for what is and what can be.

The place of the U. S. government necessarily returns to mind before a survey of the UFO mystery can draw to a close. Surely the government has to stay on top of the situation and know more about it than anyone else, yet the only statements given to the public say UFOs are not real and pose no threat, and no investigations continued after Project Blue Book closed in 1970. The clearest evidence that official words and deeds do not add up is the Roswell crash. Investigators have interviewed hundreds of people who witnessed or knew something significant about the incident, and a compelling picture emerges that something remarkable and probably extraterrestrial crashed, while military authorities strove with considerable success to plug all leaks of information in the aftermath of the event. One blatant example of coverup was the replacement of genuine wreckage with balloon debris for newspaper photographers so that they could spread the word that only a balloon had crashed.

With the Roswell incident following Kenneth Arnold's "flying saucer" sighting by less than two weeks in 1947, the government must have known all about UFOs almost from the start. The subject must also have been a matter of highest concern, not just a public relations problem or a burden on personnel and communication systems. From this perspective, the history of UFOs stands in a very different light. Those years of doubt, denial, and low-level Air Force investigations amounted to efforts on the periphery by people largely in the dark about what was really going on, or diversions intended to hide the real answers. The truth was already in hand and the real official investigations occurred—and continue to occur—out of sight as "black ops." When significant sightings happen, mainstream media take too close an interest, or anyone gets too close to sensitive information, the documented responses include the disappearance of vital information or the spread of disinformation. Such practices can result only from government policy, and the effort to maintain secrecy implies a secret important enough to keep. The UFO puzzle comes together if a governmental elite with little trust in the wisdom of the masses conceals the knowledge of ongoing extraterrestrial visitation, controlling public perceptions out of patriotic but

undemocratic motives to preserve order and a sense that "all's well."

Donderi wraps up the multitudinous parts of the UFO mystery into a meaningful whole. He emphasizes the importance of the phenomenon as the foundation for all else, the primacy of those raw experiences of objects that appear out of nowhere and disappear back into it, to leave no trace in the air but a profound sense of surprise and amazement on the witnesses. Too often in ufological writing the emphasis shifts from this root cause to government responses and efforts to divine what the authorities are hiding, or to aliens and their mission, but here the spotlight rightly stays center-stage on the phenomenon itself. Without this substantive mystery of the actual sightings, all the rest has no purpose. With the phenomenon and its extraordinary implications in mind, the rest falls together and makes well-defined sense.

In this book, as in his public lectures, Donderi maintains a lucid and well-crafted argument. The reader never loses sight of what matters, why it matters, and where the parts belong in the overall scheme. For these reasons the book could serve as a valuable introduction to the subject of UFOs for newcomers, but equally well as a reminder to veterans of where the basic evidence lies and how all the rest provides auxiliary support. Anyone familiar with the literature will read of events and controversies already familiar to them, but they stand to benefit from seeing them ordered according to their proper proportions in a cohesive overview. The result is a convincing statement of the rational case for UFOs.

Donderi's discussion of the psychology of science brings a new perspective to most ufologists. They have long regarded ufology as a scientific enterprise and aspired to scientific acceptance, then resented mainstream scientists for withholding it. Donderi replaces the image of stubborn and arrogant scientists with valuable understanding of the practices and mindset behind their rejection of UFOs, and such understanding provides a starting point from which to heal frustrating attitudes on both sides. The reader may notice that Donderi's ufology does not readily embrace conspiracy theories. He prefers psychological and social explanations for the actions of scientific and governmental authorities and pragmatic motives for hiding evidence and the human weakness of rejecting the challenge of revolutionary new knowledge. He builds his history of growing UFO evidence without factoring in the implications of Roswell, introducing the crash and what it must have meant to official policies and behavior only late in the book. To accept Roswell is to obligate a radical rewriting of history, and not just UFO history. Donderi acknowledges the extraordinary importance that ufologists attribute to this event, but he does not seem to have fully reckoned with its consequences. The result is a book that is at odds with itself, with the

first and larger part telling a story of UFO events and a human struggle to understand the unknown, while a short final section reveals that some authorities knew the secret of UFOs from the beginning and the rest of the history unfolded in full awareness of what the events were all about.

Donderi's exposition presents ufological history as advancing from uncertainty through adversity toward the triumph of truth. This positivistic course lays out a thrilling plot line and suggests a happy ending to the story, but in fact the plot has not reached its resolution. After the new hope in the wake of the Condon fiasco and the emphasis on abductions and Roswell during the 1980s and early 1990s, the ufological bandwagon seems to have lost its way and run out of steam. The book pays little attention to the past 25 years or so, and maybe that is just as well, because the story becomes one of ufological organizations closing and trustworthy information being supplanted by rampant nonsense and misinformation on the Internet, of charlatans and "personalities" replacing experienced and trustworthy ufologists as spokesmen for the field. Little research goes on, few bright lights of integrity remain, and the chance for UFOs to gain respectability seems farther away than ever. A desire not to end the book on this low note is understandable, but for the sake of accuracy he probably should not have overlooked this disappointing state of affairs.

Even an argument of the finest crystalline structure is no stronger than the evidence that supports it. If the evidence proves false then the argument fails as well, and for this reason the choice of cases is of utmost importance when arguing in favor of UFO reality. Anyone familiar with ufology knows that most reports are mistaken identities, with Venus or earthly aircrafts as common culprits. The evidence for a significant UFO phenomenon lies in the high-quality reports that do not resolve easily into conventional phenomena. Donderi rightly wastes no time on readily soluble reports and concentrates on the strongest examples, well-known and provocative cases with proven staying power against criticism. Yet even these select exemplars are far from infallible.

For example, the Yukon giant "mother ship" of 1996 received an excellent investigation and one TV series ranked the case as one of the ten best UFOs of all time. The composite illustration looks impressive and leaves a casual viewer to wonder how anyone could doubt that such an object came from another world. Yet skeptics have also investigated this case and explained it as the reentry of Russian space debris. The ufologist who investigated the Yukon sightings was aware of the reentry on the same night as the sightings but dismissed this solution on the grounds that the reentry should not have been visible to the witnesses, but the skeptics sought out a leading expert on reentries, who declared that the sight should

have appeared low in the north, just where the witnesses reported it. Further confirmation comes from the fact that witnesses who actually checked a clock during the sighting reported a time that coincided with the reentry, and from the reports of witnesses all along the north-to-south highway that the UFO flew west to east. Other known reentry cases have misled witnesses into thinking that burning fragments high in the atmosphere were lights on a solid body close to the ground, and much evidence points to the Yukon "UFO" being another example of the same illusion.

The skeptics argue that if some high-quality cases resolve sooner or later into conventional phenomena, all UFO reports will eventually go the same way and we may as well conclude that no genuine UFOs exist. This skeptical faith may stretch too far. Strong cases still support the presence of a remarkable phenomenon in the skies, as Donderi has ably outlined. What the failure of significant cases should teach us is that the evidence is more confusing than ufologists like to admit, and the search for truth calls for all the expertise available. In other words, ufologists could benefit from a cooperative relationship even with skeptics, rather than ad hominim attacks on the intelligence and integrity of anyone who expresses doubts about UFOs. In Donderi's effort to explain the thought processes of scientists when they reject UFOs, he might have admitted that ufologists have seldom presented a satisfactory case. They too often accept questionable reports, or fail to give proper credit to negative evidence. As long as too little certainty and too much advocacy surrounds UFO evidence, doubting critics find good cause to consider their rejection a rational choice, and ufologists to regard themselves as their own worst enemies.

These quibbles aside, Donderi has authored a book with a clear and succinct argument to take UFOs seriously. It will not convince the skeptic, but it is worthy of reading by everyone interested in the subject or even curious about it, if for no other reason than it provides a fine statement of why many ufologists maintain a commitment to their subject. They know their reasons or at least feel them, but only a few have stated their case half as well as Donderi has done here.

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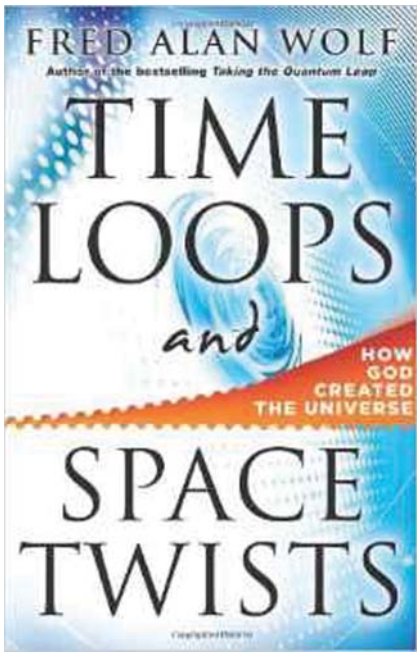
BOOK REVIEW

Time Loops and Space Twists: How God Created the Universe by Fred Alan Wolf. Hierophant Publishing, 2013 (hardcover 2010), xii + 286 pp. $18.95. ISBN 978-1938289002.

Reading this book, I could not help comparing it with another covering some of the same ground, which I had read recently, namely, *A Universe from Nothing: Why There Is Something Rather than Nothing* by Lawrence M. Krauss (2012). Each author has an agenda. Krauss wishes to persuade us that the universe will arise from nothing quite naturally, without any need for a Divine Creator, while Wolf wishes to persuade us that consciousness determines reality and that the very same phenomena that led Krauss to atheism actually provide evidence for the existence of a Divine mind at the heart of everything. Each author attempts the difficult task of explaining quantum-field theory in non-technical terms in order to bolster his case, but they draw diametrically opposed conclusions from the physical data. In my opinion, Krauss' argument fails because it is circular, but his book is not under review here and I shall not go into details. Wolf is more subtle and low-key in his presentation. Despite the subtitle, and apart from some tantalizing references to the Vedic scriptures and a few scattered mentions in the body of the book, God does not make an appearance until the last chapter. There, Wolf presents us with his own personal interpretations, but he makes no claim to have provided a scientific proof of the existence of God and freely acknowledges that many of his colleagues will disagree with him.

Wolf takes us on a journey through special relativity and quantum field theory, promising to use no more mathematics than readers will have learned in high school. He keeps the promise, although I am not sure how successfully he gets his message across. Mathematical symbolism, after all, is only a kind of shorthand, although a long and rigorous training is required to understand it. In my student days I was familiar with partial differential equations and even enjoyed working with them, but any abilities to do so that I may have had have atrophied from lack of use—they have not been required in the areas of science in which I have worked. So I am not much better off than Wolf's intended readers when I try to understand quantum field theory, although I do have some appreciation of the difficulties of the task he has set himself.



The shorthand of mathematics is succinct, and many words are needed to explain equations that look simple. Wolf's literary style, on the other hand, tends to the repetitious and he supplements it with many diagrams—so many, in fact, that one is often reading the text on one page that explains a diagram on another, which does not make for easy comprehension. Although the diagrams themselves are often helpful and sometimes amusing, the general impression is analogous to that created by a PowerPoint presentation in which the lecturer has too many slides. The early diagrams are concerned with explaining special relativity and are very similar to ones that I find simpler and easier to follow presented by Eddington (1928) in his book *The Nature of the Physical World*. I also have reservations about the use of the terms "space-vibes" and "time-vibes." The latter term is simply a synonym for frequency and is proportional to energy; the former is related to wave-number and proportional to momentum. The concepts of energy and momentum are common enough, even if many readers will not fully understand the precise sense in which those terms are used in physics, and I do not see what is gained by introducing trendy phrases in their place.

From special relativity, Wolf proceeds to the behavior of the fundamental particles that clearly are his main interest. He introduces us to *tachyons*, particles moving faster than light, which is the same as moving backward in time. A positively charged particle moving forward in time with positive energy is the same as a negatively charged particle moving backward in time with negative energy. It is important to remember here that Wolf is talking about *kinetic energy* although he does not always make this clear. In our everyday macroscopic world, kinetic energy can only be positive, since it is proportional to the square of a body's velocity. *Potential energy* or *gravitational energy*, on the other hand, is conventionally considered to be negative. Non-specialist readers who venture to read both Wolf's book and Krauss' may be confused here, since Krauss makes much of the fact that the

total gravitational energy in the universe exactly balances its kinetic energy. After his long treatment of tachyons, Wolf mentions that some physicists, including Richard Feynman, prefer to speak of *virtual particles*, but that he prefers to think in terms of tachyons. Krauss uses the virtual-particle formalism and I find his discussion the easier to follow at this point.

The above critical remarks are, however, of only minor significance. The last chapter of the book is probably the most important one. Wolf emphasizes the role of mind or consciousness in quantum physics, which is surely familiar to most readers of this *Journal*. The traditional objectivity of science which separates the observer and the observed does not apply at the level of sub-atomic particles where the act of observation appears to affect what is being measured. This has been brought out, for example, by Paul Davies (2008) in *The Goldilocks Enigma* where he discusses the famous two-slit experiment. It is possible to modify that experiment to determine whether a given photon has behaved as a wave or a particle. A slightly different setup is needed for each of those possibilities and the experimenter can delay the choice of setup until after the photon has passed through the slits. Yet, whichever setup is chosen, the photon will obligingly display the appropriate behavior. Consciousness plays a role in determining reality! Wolf goes on to suggest that mind does not reside in the brain but that there is, rather, a "mind-field" permeating the physical universe. Tentatively, he identifies this field with the Higgs field (he was writing before the claimed discovery of the Higgs boson) which, he further suggests, could be identified with the mind of God. That would certainly guarantee God's omnipresence and possibly explain both His omniscience and omnipotence!

Although Wolf is clearly conversant with the Hebrew Bible in its original language and also quotes the *Qabala* (his preferred spelling), his conception of God appears to be different from that found in the Abrahamic religions. Indeed, as I have already hinted, he is also clearly influenced by some aspects of Hinduism. It is at this point that Wolf stresses that he is offering a personal opinion and does not try to tell us that we must inevitably come to his point of view—and his honesty on this account is much to be applauded. He admits that many of his colleagues will disagree with him, and he must know that some of them will dismiss his ideas as "mysticism" in the pejorative and incorrect sense in which that word is often used. Indeed, Wolf's ideas have much in common with the correct sense of the word "mysticism."

I like Wolf's suggestion because it gives the lie to those who try to persuade us that quantum theory inevitably leads us to a godless universe, but I have some hesitations about embracing it fully. Newton wrote of space as the "sensorium of God" and was criticized for it in his own lifetime.

Nowadays, probably only a few historians of science remember the remark. More importantly, I recall a sentence from the penultimate paragraph of Eddington's book (1928) cited earlier:

> The religious reader may well be content that I have not offered him a God revealed by the quantum theory, and therefore liable to be swept away in the next scientific revolution.

Quantum theory, of course, has developed far beyond the stage known to Eddington, but he saw a danger that is still present. Cosmologists and theoretical physicists alike (they are often the same people) seem confident that they have approached a final understanding of the natural world; that a "theory of everything" will soon be discovered. I believe this confidence to be misplaced. Both scientific cosmology and quantum theory are approximately a century old and that seems hardly enough time to unravel mysteries that have been with us since the first human beings began to think. To tie our notions of God even to the Higgs field may be too limiting and is, perhaps, a form of that idolatry against which the Hebrew prophets railed so stridently.

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BOOK REVIEW

Science, the Self, and Survival After Death: Selected Writings of Ian Stevenson, edited by Emily Williams Kelly. Rowman & Littlefield, 2013. 424 pp. $65 (hardcover). ISBN 978-1442221147.

Ian Stevenson (1918–2007), professor of psychiatry at the University of Virginia, was the most prominent and prolific psychical researcher of the 20th century. Stevenson devoted his research activities to a large extent to phenomena relevant to the question of survival and to spontaneous phenomena. This he did while J. B. Rhine, the other most prominent reseacher of the 20th century, had abandoned research concerning the survival question and became exclusively an experimentalist.

This volume consists of a collection of important papers published by Stevenson during his long and productive career. His long-time assistant, Emily W. Kelly, collected the texts for this volume, and writes a general introduction and a few brief introductions to his major works. She writes:

> No one can hope to express Ian's ideas better then he himself did, and so, rather than write a biography summarizing his work, I have chosen to let him tell the tale. The remainder of this volume therefore consists primarily of selections from his own published work, either full papers or excerpts from papers and books, that I think convey most effectively what he was trying to do and why.

The book consists of 34 chapters of varying length, the oldest of them Why Medicine Is Not a Science published in 1949, and the latest What Are the Irreducible Components of the Scientific Enterprise? published in 1999. Note that half a century passed between these two publications. Still there were later publications, the last being autobiographical, Half a Career with the Paranormal, an Essay that he published in the *Journal of Scientific Exploration* in 2006.

Stevenson's focus was on the large questions about the nature of human personality and unanswered question of the mind and the brain relationship. He saw the aim of psychical research as being precisely his own: to apply the methods of science to the still-unanswered questions.

The 34 chapters are organized into five sections: New Ideas in Science, The Nature of Human Personality, Psychical Research—Principles and

Methods, Research on the Question of Survival After Death—Reviews and Representative Case Reports, and Implications. The section on research on the question of survival is by far the longest.

Stevenson became best-known for his studies of children who claim memories of a past life (reincarnation cases). They are duly presented in several chapters such as Some New Cases Suggestive of Reincarnation: The Case of Ampan Petcherat, Three New Cases of the Reincarnation Type in Sri Lanka with Written Records Made before Verification, Cases of the Reincarnation Type with Birthmarks and Birth Defects, and Birthmarks and Birth Defects Corresponding to Wounds on Deceased Persons. The important paper Reincarnation, Field Studies, and Theoretical Issues: 1977 is published in full, whereas most of the above chapters are not, where only excerpts—the essential or crucial part of the papers—are published, often preceded by a short commentary or overview by the editor. That way Kelly is able to cover a wide area of Stevenson's work and still limit the text to some 400 pages.

The editor writes that the view of

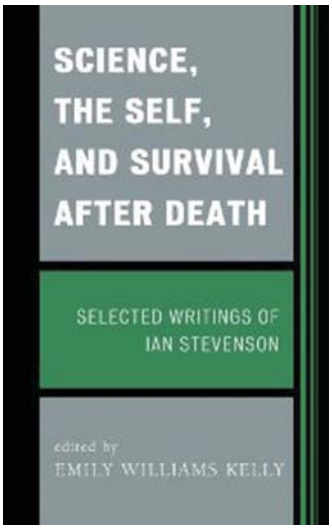
> [human personality] as something that interacts with but is neither identical with nor wholly dependent on the biological body, provided Ian with an avenue to understanding the problem that he called the 'leitmotif' of his career—namely, the source of individual differences, both in character and in susceptibility to particular diseases. (p. 61)

This source is in his view the *third factor* that molds human personality, the other two being the generally accepted genes and environment.

Chapters are devoted to Ian's contributions to the study of apparitions (Modern Apparitional Experiences), death-bed visions, out-of-body and near-death experiences (Cardiac Arrest Remembered), and mediumship (A Communicator of the 'Drop in' Type in Iceland: The Case of Gudni Magnusson, that was co-authored by this reviewer). Only a few of his papers on apparitions are mentioned above.

Which papers to select must have been a difficult task considering Stevenson's voluminous work, because of the breadth of his interests and the great variety of research projects that he worked on during his six-decades-long active career. One misses his research on special subjects, such as Ted Serios and Stephan Ossowiecki. As the title indicates, the emphasis is on Stevenson's survival research and his approach to science, including his criticism of how many fellow scientists looked at science as a set of fixed assumptions but not as a neutral methodology.

Science, the Self, and Survival After Death, *Selected Writings of Ian Stevenson* concludes with an Index that the reviewer would have liked



to be more thorough. An example: One author who is mentioned a few times in the book is listed only once in the Index. An Appendix gives a complete bibliography of Stevenson's publications, beginning with his 15 published books, some of which have become classics and been translated into numerous languages, such as *Twenty Cases Suggestive of Reincarnation*, and *Children Who Remember Previous Lives. A Question of Reincarnation*. His last major books were his two volumes *Reincarnation and Biology: A Contribution to the Etiology of Birthmarks and Birthdefects Volume 1: Birthmarks* and *Reincarnation and Biology: A Contribution to the Etiology of Birthmarks and Birthdefects Volume 2: Birth Defects and Other Anomalies*. He also wrote a less technical summary of the material that was published as *Where Reincarnation and Biology Intersect*. In these books he argues strongly for the existence of the *third factor*. His last major work was *European Cases of the Reincarnation Type* published in 2003.

Science, the Self, and Survival After Death, *Selected Writings of Ian Stevenson* gives an excellent overview of Stevenson's work. It is important that his great, outstanding contributions be remembered and brought over to the next generation of researchers. Emily Williams Kelly deserves praise for this highly recommendable and readable volume.

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BOOK REVIEW

Medical Mysteries and Conundrums —When Doctors May Not Help

Understanding Hashimoto's Encephalopathy: A Guide for Patients, Families, and Caregivers by Nicola Nelson, Susan Foley, and Shari Lawler for the Hashimoto's Encephalopathy SREAT Alliance. CreateSpace, 2013. 458 pp. $21.95 (paperback). ISBN 978-1484883099.

Some number of people feel ill, yet mainstream medicine tells them via their doctors that there is no discernable reason for their symptoms and therefore no clear way to offer any real help. Sometimes the sufferers may be told that it's psychological stress, "all in the head," that there's nothing "really" wrong even though the physical pains are indubitably felt; or perhaps that what they think they suffer from doesn't even exist.

Consider Chronic Lyme Disease, for example:

> The term 'chronic Lyme disease' is not recognized in the medical literature, and most medical authorities advise against long-term antibiotic treatment for 'chronic Lyme disease'. Studies have shown that most patients diagnosed with 'chronic Lyme disease' either have no objective evidence of previous or current infection with *B. burgdorferi* or are patients that should be classified as having post-Lyme disease syndrome, which is defined as continuing or relapsing non-specific symptoms (such as fatigue, musculoskeletal pain, and cognitive complaints) in a patient previously treated for Lyme disease. (Wikipedia; Science-Based Medicine)

Or chronic fatigue syndrome (CFS):

> Chronic fatigue syndrome is a complicated disorder characterized by extreme fatigue that can't be explained by any underlying medical condition. The fatigue may worsen with physical or mental activity, but doesn't improve with rest. The cause of chronic fatigue syndrome is unknown, although there are many theories—ranging from viral infections to psychological stress. Some experts believe chronic fatigue syndrome might be triggered by a combination of factors. There's no single test to confirm a diagnosis of chronic fatigue syndrome. You may have to undergo a variety of medical tests to rule out other health problems that have similar symptoms. Treatment for chronic fatigue syndrome focuses on symptom relief. (Mayo Clinic)

> Researchers have not yet identified what causes CFS, and there are no tests to diagnose CFS. Moreover, because many illnesses have fatigue as a symptom, doctors need to take care to rule out other conditions, which may be treatable. (Centers for Disease Control and Prevention)
>
> CFS may also be referred to as myalgic encephalomyelitis (ME), post-viral fatigue syndrome (PVFS), chronic fatigue immune dysfunction syndrome (CFIDS), or by several other terms. Biological, genetic, infectious, and psychological mechanisms have been proposed, but the etiology of CFS is not understood and may have multiple causes. (Wikipedia)
>
> CFS may in some cases be associated with immune deficiencies similar to those in AIDS. (Blogspot)

Or irritable bowel syndrome:

> In many cases, you can control irritable bowel syndrome by managing your diet, lifestyle, and stress. (Mayo Clinic)
>
> There are treatments that attempt to relieve symptoms, including dietary adjustments, medication and psychological interventions. Patient education and a good doctor–patient relationship are also important. (Wikipedia)

Those suffering from such symptoms or syndromes are unlikely to find any of these official pronouncements particularly helpful. They would be well advised to do some research for themselves. Sometimes, it turns out, they can come to understand what's wrong and even fix it, as Mohammed Aziz did when a family member turned out to have a deficiency of vitamin D (Aziz 2012, Bauer 2012a). With the officially non-existent Chronic Lyme, some sufferers have benefited from long-term antibiotic treatment offered by a few intrepid, empirical, evidence-based physicians who risk being disciplined by their profession and by government authorities (Bauer 2010).

One of the children in our family became annoyingly unruly, strangely obsessive, a "problem child" in school. A succession of doctors advised counseling or medication with the ADHD favorite Ritalin or its ilk. The parents thought it might have had something to do with a recent infection, but the medicos pooh-poohed the idea. The persistent parents did their own research, and it turns out that their daughter suffered from PANDAS:

> PANDAS is an acronym for Pediatric Autoimmune Neuropsychiatric Disorders Associated with Streptococcal infections, a rare disease that usually appears in children. This term describes a hypothesis that there exists a subset of children with rapid onset of obsessive-compulsive disorder (OCD) and/or tic disorders and these symptoms are caused by group A beta-hemolytic

> streptococcal (GABHS) infections. The proposed link between infection and these disorders is that an initial autoimmune reaction to a GABHS infection produces antibodies that continues to interfere with basal ganglia function, causing symptom exacerbations.
>
> The PANDAS hypothesis was based on observations in clinical case studies at the U.S. National Institutes of Health and in subsequent clinical trials where children appeared to have dramatic and sudden OCD exacerbations and tic disorders following infections. There is supportive evidence for the link between streptococcus infection and onset in some cases of OCD and tics, but proof of causality has remained elusive. The PANDAS hypothesis is controversial; whether it is a distinct entity differing from other cases of Tourette syndrome (TS)/OCD is debated.
>
> PANDAS has not been validated as a disease entity; it is not listed as a diagnosis by the International Statistical Classification of Diseases and Related Health Problems (ICD) or the Diagnostic and Statistical Manual of Mental Disorders (DSM). Pediatric acute-onset neuropsychiatric syndrome (PANS) is a 2012 proposal describing another subset of acute-onset OCD cases including "not only disorders potentially associated with a preceding infection, but also acute-onset neuropsychiatric disorders without an apparent environmental precipitant or immune dysfunction."(Wikipedia)

Even if personal research turns up no real "scientific" certainty or actual cure, suffering individuals may get considerable help by learning of other people in similar situations and how they cope. And occasionally it may be that such contacts suggest the possibility of a known ailment so rare that it escaped the attention not only of local doctors but even that of such renowned institutions as the Mayo Clinic. That happened to someone who eventually discovered her problem to be Hashimoto's Encephalopathy:

> Hashimoto's Encephalopathy ('HE'), also commonly referred to as Steroid Responsive Encephalopathy Associated with Thyroiditis ('SREAT'), is a rare and devastating autoimmune disease in which a patient's antibodies mistakenly turn on and attack the patient's brain. The results of the attack may include severe cognitive impairment, speech disorders, seizures, memory loss, impaired balance, movement disorders, and sometimes psychosis. Patients sometimes fall into a coma and in rare cases die. Unfortunately, HE is not well understood and patients often face an enormous struggle trying to find the correct diagnosis for their confusing constellation of symptoms. Many neurologists have never even heard of the condition, although that has begun to change in the last few years. Misdiagnosis is the rule more often than the exception, at least in the early months of illness.

Those words introduce the book under review. Part I describes Hashimoto's Encephalopathy. Part II contains 35 personal testimonies from

individuals diagnosed with HE, at ages as young as 17 and as old as 66, in Australia, Denmark, England, Scotland, and the United States. Part III has 5 stories of children diagnosed as young as 10, as old as 17. In Part IV, friends and family of HE sufferers share their experiences. Then there are Resources: support groups (pp. 454–456), informational websites (pp. 457–458), and scientific articles (pp. 403–453).

Obviously this book is invaluable for anyone with HE, but most particularly for those who don't yet know that they have HE. It deserves to be common knowledge that a variety of debilitating symptoms might (but in rare cases only!) be signs of HE, symptoms easily mistaken for "mental" problems and difficult to diagnose:

> patients typically present with several of the following symptoms: cognitive impairment (including concentration and memory problems); emotional, behavioral, and personality changes; speech difficulties (including aphasia or dysphasia); tremor; muscle jerking (myoclonus); dizziness, vertigo, and impaired coordination and balance (ataxia); headaches; malaise, weakness, and fatigue; fluctuating consciousness; disorientation, confusion, or dementia; seizures or seizure-like events; stroke-like episodes; partial paralysis (often on the right side); sleep abnormalities such as insomnia or excessive sleepiness; sensory or motor difficulties, often on one side; status epilepticus; psychosis (including hallucinations, delusions or paranoia); coma.

Not necessarily helpful, since any of those can occur for a variety of reasons. Even worse: Some people experience episodes, in other words remissions and relapses, whereas others suffer progressive worsening. Consequently, HE is diagnosable only by excluding a whole host of other possible conditions, some of them also quite rare.

One of the first symptoms seems often to be like a stroke or a seizure. Brain MRIs sometimes appear like those of much older individuals (e.g., p. 280). The book gives details about a number of tests and what they can and cannot prove. The report of a consulting neurologist, reproduced in full at pp. 269–775, offers insight into the difficulty of diagnosing and treating HE. It took 7 years after her first bad episode before Kelly got her diagnosis of HE (pp. 276–277).

Many will find it reassuring that "up to 90% of HE patients respond to steroid treatment" (p. 19). When seizures occur, anti-seizure drugs are appropriate. Thyroid hormone replacement may help when there is pronounced hypothyroidism.

The personal reports make emotionally difficult, heart-rending, shocking reading—extraordinarily debilitating experiences of mental confusion, physical pain, feeling completely ***lost***, described sometimes as

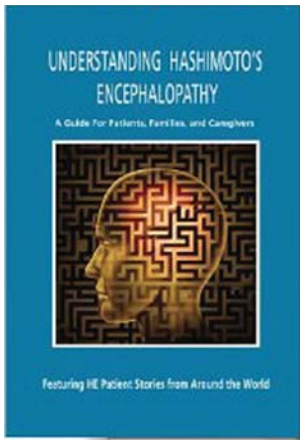
having an out-of-body experience, or feeling abducted by aliens, or suddenly experiencing full-blown dementia, or being on the point of dying—or even beyond it. Unable to talk, or unable to think, having no motivation to do anything at all, not knowing how to drive a car. Shari gives a very detailed description at pp. 82–97.

There is, however, a silver lining: These people somehow managed to carry on and eventually to understand what the problem is and often to go further with such ventures as this book and using social media to bring positive help to other people. There are tales here of astonishing courage and determination, for instance from Allison, who did much research, essentially developed her own treatment as a full partner with her doctors, survived a husband leaving her while she was ill, and not only held HE at bay but managed at the same time to have the baby she had always wished for (pp. 174–183).

Each tale is unique, but there are such common elements as long periods of tests, mis-diagnoses, innumerable doctors, and specialists. Often the doctors concentrate on what "objective" tests show while paying little attention to what the patients know about what they are feeling. One possible reason or excuse may be that patients often present with such a variety of complaints that hypochondria is a ready guess. In one case, Canadian doctors would not accept an HE diagnosis from the Mayo Clinic (pp. 363–364). Shari offers useful advice at pp. 99–116.

The adage that "hard cases make bad laws" cannot be applied to medicine: Systems should be designed with the needs of ***individuals*** in mind, just as democracies should care about minorities and avoid the tyranny of the majority. This book illustrates that the American "Health Care System" is worse than inadequate in several ways, routinely because there is little if any coordination among specialists and family doctors (see pp. 78–79, for instance), and disgracefully in individual cases when, for example, an insurance company refuses to pay for a diagnostic visit to the Mayo Clinic after local doctors have exhausted their expertise (p. 46); or when attending doctors could not get approval to do a PET scan (p. 60) when they were desperately seeking to diagnose a patient who had them puzzled; or having to wait more than a month to be seen by an appropriate specialist (p. 74), or being frustrated by bureaucratic paperwork (p. 77). In one instance,

> now that she is covered by Medicare it has opened doors to treatment options she didn't have with private insurance (that she paid for/worked for all of her life)! (p. 150)



—illustrating what's wrong with free-market, profit-driven healthcare and private insurance. There are the tens of millions like Tiffany (age 28; p. 256), who have no health insurance because they cannot afford it. For more such horror stories, read what Grace (p. 331 ff.) reports about insurance, bureaucracy, and not being allowed to defer payment on student loans. Or about a wait of 2 years for an appointment with a pediatric neurologist (p. 362).

That "up to 90% of HE patients respond to steroid treatment" is not unmitigated good news because the "side" effects can be dangerous in themselves, and some of the personal tales recount an astonishing number and variety of medications used to mitigate the consequences of the primary treatment, which is basically immune suppression. Allison, for example, was taking 21 drugs at one stage (p. 175).

The thing to remember always about "side" effects is what Frank Ofner, MD, used to say: "'Side' effects are ***main*** effects that doctors don't want to talk about." Everything a drug does is an effect. That "side" effects are anything but negligible or ignorable is illustrated as drugs approved for one particular use are then ***often*** claimed to be effective in entirely different ailments. Drug companies like to "reposition" their drugs in this manner because it is cheaper and easier than having an entirely new drug approved: Once a drug has been approved earlier as "safe," fewer clinical trials are needed when approval is sought for the different use (Ashburn & Thor 2004, Healthtech 2012, Bauer 2012b). Of course, that a drug "works" does not necessarily mean that it is treating the actual cause of an ailment. As I read somewhere, the fact that aspirin "cures" pain doesn't mean that the pain was owing to lack of aspirin. Larry (p. 189) found that an anti-inflammatory and an anti-psychotic worked the same benefit for him in treating his HE, and asks the excellent question what that means.

Individual doctors are permitted to prescribe *any* approved drug for *any* condition, though drug manufacturers are not allowed to proselytize for such "off-label" uses. Well-informed patients may be able to persuade their doctor to try treatments for which only anecdotal evidence exists. A couple of the stories in this book mention naltrexone or low-dose naltrexone, officially approved to treat alcoholism but attested by a number of people as "helping those with HIV/AIDS, cancer, autoimmune diseases, and central nervous system disorders" (LDN).

The personal experiences published here are surely invaluable for medical professionals as well as for individuals suffering from mysteriously erratic, unpredictable, capricious collections of symptoms.

> [S]ince everyone who has HE has different symptoms, it's sometimes easy to dismiss some of my symptoms. I did that while I was in a period of denial. Now, I realize that the only way I can get better and other people can get help is if we all keep track of our symptoms and share them so that we can learn from each other and doctors can learn. (p. 163)

A priori assumptions don't work. For example, HE is an autoimmune disease; there is a genetic predisposition to some autoimmune ailments; autoimmune diseases run prominently in Lisa F.'s family; yet HE hit her only at age 40, while individuals with no such family history have been affected as early as age 10. Some early symptoms are reminiscent of paranoia or schizophrenia (see Larry at p. 183, for instance). The difficulty of ascribing characteristics to HE are redoubled because treatment always begins with, and has sometimes even preceded, diagnosis, so that some symptoms might be drug "side" effects rather than indicative of HE. Or, they may have to do with neither: Larry (pp. 196–197) twice had a toe turn blue and then black underneath the nail, then the nail dropped off and was replaced with a new one, and thought this might have been related to episodes of HE—but I had such an experience while perfectly healthy and attributed it to too tight a shoe.

HE seems more common among women than among men, but the reported ratio varies between 2-to-1 and 6-to-1 (pp. 9–10). Only 7 of the 35 personal stories in this book are from men, and only one of those was younger than 45 at diagnosis of HE. All 5 children's stories are about girls.

Before diagnosis of HE had been accomplished, some older HE patients were sometimes told that changes they had noticed were just age-related: not finding the right words, difficulty concentrating, decreased motivation, poor balance, general anxiety, loss of short-term memory while long-term memory remains. Those are indeed things that come with age, of course at quite different ages and with different intensities in individual cases; I (age 82) could empathize—fortunately to only minor degrees—with about half of the first 20 symptoms of HE listed by Larry at pp. 185–187. Essentially all the personal stories report that life is now quite different than before HE, which can also resonate with much older people who have seen their capacities decline and who spend almost all their time just coping with everyday matters. Anyone who has experienced a family member with Alzheimer's or other age-related dementia will appreciate how similar can be the symptoms of HE; those who have not had this experience can read

about the typical symptoms in the story of James (pp. 165–174).

This is a very important book. Mention it to everyone you know. The fact of very rare and difficult-to-diagnose diseases ought to be known to everyone, and that a large proportion of doctors and specialists might be ignorant about these conditions. Awareness of those facts might assist the few actual victims get help earlier than did so many of the individuals who tell all in this book.

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BOOK REVIEW

Thinking, Fast and Slow by Daniel Kahneman. Farrar, Straus, & Giroux, 2011. 512 pp. $16. ISBN 978-0374275631.

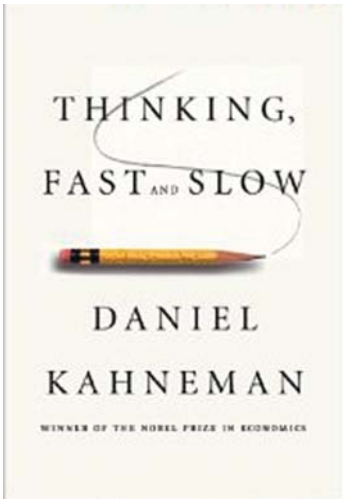
Passing through an airport recently, I happened to notice *Thinking Fast and Slow* on a rack in the bookstore, alongside *The Racketeer* by John Grisham, *Inferno* by Dan Brown, and *Fifty Shades of Grey* by E. L. James. Having read Kahneman's book, I knew that it lacked the sex, action, and pop appeal that these other books offered, but that what it was short of in these characteristics it made up for in depth and revelation, offering an unfamiliar form of intrigue—something personal rather than scandalous—altogether riveting.

Since its first release in 2011, *Thinking Fast and Slow* has received an overwhelming number of glowing reviews. One pronounces Kahneman "the world's greatest living psychologist;" others describe the book as "one of the greatest and most engaging collections of insights into the human mind" and "a masterpiece." While these are well-deserved accolades, what earned Kahneman the Nobel Prize in economics and has gotten his book onto the rack of popular best-sellers isn't just the theories or the research but rather the surprise and self-reflection his pulling together of this amazing body of knowledge evokes.

More than 100 years ago, Sigmund Freud's book *The Psychopathology of Everyday Life* drew a similarly widespread public response. Freud's book identified the key signs in ordinary life of the underlying "pathology" of the psyche: forgetfulness, "lapsus linguae" (slips of the tongue), and "parapraxis" (apparently random actions). And it made the "unconscious" and such familiar notions as "Freudian slips" part of our everyday thinking; it had, and still has, a profound effect on how we think about, and understand, ourselves.

In much the same way, but in what seems a far more inviting, and refreshingly contemporary, style, Kahneman lays out for his readers a system by which to identify the irrational cognitive mechanisms of everyday life. To achieve this, he weaves decades of his, and others', research with common life events with which readers can easily identify.

Much as Freud did with contrasting notions of conscious and unconscious and *Id* and *Ego*, Kahneman does with his two 'systems' which he emphasizes are just heuristics invented to make it easier to talk



about how we think. One is intuitive and emotionally influenced, making it a fast and generally effective "machine for jumping to conclusions." The other serves to more rigorously check and correct the first system's conclusions.

System 1, *fast thinking,* is something that we do all day long at an automatic and largely subconscious level. Not only is it what underlies our snap decisions and intuitive opinions, it is the source of explicit beliefs and deliberate choices that our conscious mind uses to explain our behaviors. As Kahneman acknowledges, System 1 is generally pretty good at carrying out this role, leading us to trust it. However, we pay a high price for speed, by simplifying tasks, assuming that what we know is all we need to know, and jumping to conclusions based on limited (and sometimes insufficient) information.

System 2, *slow thinking,* on the other hand, is effortful, focused, and deliberate, and not something we do as often as we might think. Since it requires intention and mental energy, System 2, which is the kind of thinking often required for good (truly thought out) decisions, tires us easily and we are naturally reluctant to invest more effort than is strictly necessary. Thus, we are inclined to be 'lazy' and to go along with whatever our fast-thinking mind suggests.

To illustrate and contrast these two systems, Kahneman offers these activities:

> answering 2 + 2 = ? vs. calculating 17 × 24 = ?
> driving on a quiet road vs. making a left turn in dense traffic
> orienting to a loud voice in a crowd vs. listening to the loud voice's conversation

Once his theme is established, Kahneman presents a collage of findings spanning more than forty years showing how the sleek and fast System 1, that works well enough under most circumstances, can lead to irrational biases and intruding effects. A talented writer as well as a competent researcher, Kahneman carries us from demonstration studies to events in the real world, to activities in our own internal mental worlds through which we, the readers, can observe what happens.

Filling much of the remaining 300 pages are too many observations to even attempt to summarize. Here are a few examples:

- When experienced German judges roll a pair of dice loaded to give either a low or high number, those with the higher number impose longer jail sentences in immediate trials—the *Anchoring Effect* or influence of irrelevant numbers
- Students, after being presented with a set of words which evoke thoughts of old age (Florida, forgetful, bald, gray, and wrinkled), walk more slowly than usual—*Priming Effect* in which thoughts and behavior are unknowingly influenced
- Professional golfers "try harder and are more successful when putting for par (to avoid a bogey) than when putting for a birdie"— *Loss Aversion*
- The trading records of 10,000 brokerage accounts show that: "on average, the shares that individual traders sold did (substantially) better than those they bought"—*Illusion of Validity*
- Individuals are more likely to opt for surgery if they are told that the "survival" rate is 90 percent, rather than that the mortality rate is 10 percent—*Framing*
- When asked whether "Ford (F) stock is a good investment," subjects base their answers on available, but not really relevant, data such as whether they like Ford cars—*WYSIATI* ("what you see is all there is")

In later chapters, Kahneman shifts his focus, introducing two more heuristics: the "experiencing self, which does the living" and the "remembering self, which keeps score and makes the choices."

He discusses how, in the 1990s when happiness became a popular focus for the emerging Positive Psychology movement, researchers relied on retrospective polls about life satisfaction. In contrast to these questionnaires which attempted to measure "remembered" well-being, Kahneman offers an alternative that assesses "pleasure" (or pain) from moment to moment which can then be summed over time. These two approaches produce very different results with the "remembering self" rating an experience by the peak (or valley) of the experience and by the feelings when it ended, rather than by the duration or extent of the feeling.

For example, patients undergoing painful colonoscopies were divided into two groups. Group A got the regular procedure while Group B unknowingly received a few extra minutes of less painful discomfort at the end of the examination. Although they received more total discomfort, the less painful ending led Group B to view the whole affair as less unpleasant.

In the same way, vacations and overall life itself are judged/remembered by the peaks and valleys, and conclusions rather than in their totality leading Kahneman to write: "Odd as it may seem, I am my remembering self, and the experiencing self, who does my living, is like a stranger to me."

Toward the conclusion, he raises the question as to whether a lifetime of research (and all this knowledge about thinking) has made Kahneman himself a better thinker. No, he writes, confessing that

> except for some effects that [I] attribute mostly to age, [my] intuitive thinking is just as prone to overconfidence, extreme predictions, and the planning fallacy as it was before [I] made a study of the issues.

And as if to challenge us, he warns that "it is much easier, as well as far more enjoyable, to identify and label the mistakes of others than to recognize our own."

If you choose to read *Thinking Fast and Slow* (and I genuinely think you should), be prepared to question your own decisions and opinions both past and present. Your mind (and my mind and seemingly everyone's mind) pretends to be cautious and thoughtful when often it simplifies, confabulates, and wildly jumps to conclusions. It doesn't just let you remember what you experienced but rather what it chooses as your memories.

This book is about how your mind has a mind of its own; it's a fascinating read—an unusual best-seller and something that even Freud's ghost would find provocative.

Tana Dineen

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BOOK REVIEW

Dogmatism in Science and Medicine: How Dominant Theories Monopolize Research and Stifle the Search for Truth by Henry H. Bauer. McFarland, 2012. 301 pp. $24.99, Kindle $14.74. ASIN B008AHNIGS.

Dogmatism in Science and Medicine (*DSM*) by Henry H. Bauer is about the corruption of modern science. For practicing scientists it is a disturbing book to read. Medicine is bitter, yet we put up with it to get better. *DSM* is bitter medicine intended to improve the health of science.

Overview of the Book

DSM describes "knowledge monopolies" (KMs) which can be thought of as Kuhnian paradigms that have been hijacked to carry out nonscientific agendas—political, economic, or governmental—with disregard for the substantive scientific content. KMs subvert science for nonscientific purposes, thereby suppressing alternative scientific interpretations that threaten the hegemony of the KM; hence the monopoly aspect. KMs are bad since they repress the hallmark activities of science: modification of ideas based on honest, open critique of evidence acquired and interpreted based on technical and theoretical competence.

Several chapters are dedicated to detailing the three main examples of KMs: HIV/AIDs (which Dr. Bauer studied in detail (Bauer 2007), anthropogenic global warming, and the Big Bang Theory. Chapter 4 provides shorter descriptions of thirteen other KMs including, for example, antidepressant drugs, migration to America, dinosaur extinction. Perhaps surprisingly to some, the Special Theory of Relativity is even included as a KM.

The chapters alternate between broader analyses of KMs, and detailed analyses of specific sciences and official reports from national and international bodies. The broader analyses include the general features of KMs (Chapter 2), some historical context of KMs (Chapter 4), and the consequences of KMs (Chapter 10). Detailed analyses include an interesting discussion of the cancellation of the Elsevier journal *Medical Hypotheses* (Chapter 3), and detailed critiques of reports from UNAIDS and The World Bank on the global HIV/AIDS epidemic (Chapter 8). Scientific peer review is critiqued in a variety of contexts. Chapter 7 gives a wonderful discussion on the misuses of statistics that should be required reading for all professional scientists.

The various discussions are intelligent, thoughtful, and meticulously documented. Dr. Bauer treats the Reader as intelligent. The bitter pill of the book is that it relentlessly plows the reader with examples of the disinformation, incompetence, and dishonesty engendered by KMs. The relentlessness may wear down the Reader. The final chapter offers possible solutions, but, generally, the cons of implementing them outweigh the pros, and the book concludes in an unresolved state.

Critique of the *DSM*

My critique of *DSM* revolves around the issues that a work addressing the corruption of modern science: (1) will have a hard time communicating to its target audiences, and (2) cannot cover all aspects of relevance.

Target Audience

DSM is grounded in "science and technology studies" (STS); the academic disciplines of the history, philosophy, and sociology of science. *DSM* contributes to this literature, but does not strictly adhere to its formalities because Dr. Bauer is not an STS worker. This is an advantage: First, as Professor of Chemistry, then Dean of Arts and Sciences at Virginia Tech, Dr. Bauer has first-hand experience in the trenches of science. This experience provides a personal and relatable element throughout the book. In his discussions of unreasonable peer reviews or faulty data analysis, I thought to myself, "Ugh, this has happened to me, too."

However, the reliance on the insights of STS puts Dr. Bauer in a catch-22 with potential target audiences. It is hard to anticipate how STS practitioners might evaluate *DSM* given the broad variety of schools of thought in STS. Regarding practicing scientists, Dr. Bauer repeatedly states that STS is not part of formal science education, a conclusion my experience also supports. My familiarity with STS is due to reading in my spare time the works of Kuhn, Merton, Feyerabend, Popper, Marie Boas, etc. Lacking STS background, practicing scientists have little basis to appreciate Dr. Bauer's positions. I expect the average practicing scientist would be emotionally defensive and not assimilate *DSM*. Dr. Bauer recognizes that the average scientist will not appreciate the realities discussed in *DSM* until their own research runs them afoul of dominant forces in their specialty. Nonspecialists are at a major disadvantage: They lack professional scientific experience, STS knowledge, and, importantly, are subject to the scientific propaganda described in *DSM*.

While the target audiences have much to gain from reading *DSM*, Dr. Bauer is certainly in a spot in attempting to educate these groups. The reader who will appreciate the book most readily is the practicing scientist with

some knowledge of STS literature or with experience of the effect of KMs in his or her career.

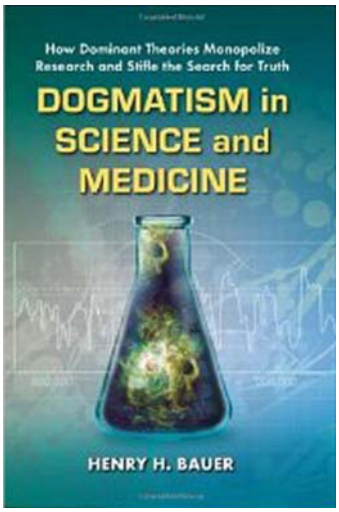
STS Considerations

As the basis of *DSM* is STS, I offer two points where the book can be critiqued from an STS standpoint.

The Demarcation Problem. The demarcation problem (Popper 1962) asks: What distinguishes, or demarcates, science from any other human activity? There is no consensus on this question (Pigliucci & Boudry 2013), being one of the most formidable open issues, if not *the* core issue in STS. Dr. Bauer does not directly address the demarcation problem. In Chapter 6, he presents the idea of "knowledge filter," which is the closest he comes to the demarcation problem. The "knowledge filter" depicts the psychological and social processes by which scientific knowledge becomes more reliable over time. The schema is sensible and provides a nice summary of processes that undergird scientific activity. But the schema implicitly assumes that science is, somehow, demarcated from other human activity. However, with slight modification, one could apply the knowledge filter idea to, for example, various arts (i.e. computer programming) and technologies (i.e. computer manufacturing) that also have become more efficient over time.

The problem with failing to address the demarcation problem is that many of the core issues surrounding KMs are demarcation problems of how science interacts with the greater society. It was possible to "read between the lines" and to see Dr. Bauer implicitly struggle with the demarcation problem, most obviously when he contrasted the classical idea of scientific knowledge as objective information, with recent sociological formulations of science as social construction (i.e. as in Woolgar 1988). He suggests, not unreasonably, that KMs are science bent too far toward the pole of social construction, but otherwise the issue was left open. This is not the place to go deeper into how KMs relate to the demarcation problem, but it is an important issue left unaddressed in *DSM* that I believe plays a key role in the rise of KMs.

Knowledge Monopolies as a Typological Construct. Dr. Bauer does not explicitly use the technique, but effectively treats KMs in a Weberian fashion as an "ideal type." An ideal type is an intellectual device used in sociology to describe and compare social phenomena, serving as a conceptual "measuring rod to ascertain similarities as well as deviations in concrete cases" (Coser 1971). The 16 main case studies presented in *DSM* provide a more or less successful fit with the ideal features of a KM. Some examples were canonical, including global warming and HIV/AIDS. However, other examples felt as if the Author was trying to shoehorn a particular example into the ideal type.



For example, the idea that Alzheimer's disease (AD) is caused by beta-amyloid plaques was presented as an instance of a KM. Amyloid plaques are a well-established symptom of AD, but whether they are cause or effect is unknown. As a member of the National Institutes of Health study section NOMD at NINDS, I served with AD experts, and AD-focused applications were routinely reviewed. I saw no indication of a KM when evaluating such applications. There was no bias toward a particular causal mechanism. A variety of mechanisms were equally considered, ranging from free radicals to cerebrovascular dysfunction. So AD research, in itself, is a less successful fit with the KM ideal type. That said, however, the broader discussion in Chapter 10, Disasters of Cartel Sciences: Medical Malpractices, gets more to the heart of endemic problems in modern biomedicine that affects all specialties from AD to cancer to antibiotics to HIV/AIDs. These discussions were spot on in my experience, and point to more general pathologies in biomedicine that, while significantly illuminated by the KM ideal type, probably require additional scope to fully characterize the pathologies.

Similarly, the critique of String Theory relied on well-known String Theory critics Lee Smolin and Peter Woit. While String Theory has dominated academic physics for the past 20 years, its ascendency was not arbitrary as compared, for example, with computer models of global warming that altogether lack a firm theoretical basis. There were natural reasons String Theory arose and these reasons appear to be running their course, especially given the latest LHC findings (Schellekens 2013). Thus, String Theory, as an intellectual monopoly in academic physics, seems to me closer to a regular Kuhnian paradigm than a KM.

Finally, there is one outstanding KM that *DSM* failed completely to mention. Newtonian physics dominated Western thought from about 1675 until Einstein's Special Theory of Relativity in 1908. It is now uncontroversial history (Hall 1980) that Newton, on being appointed head of the Royal Society in England, took as his first official action chairing the committee that investigated Leibniz for ostensibly plagiarizing calculus. Newton himself is believed to have written the document making these accusations. The effect of Newton and his cabal of cronies constitute one of the earliest and longest-lasting KMs in Western science. Leibniz's advocacy

of the relativity of space and time (e.g., as espoused in the Clark–Leibniz correspondence (Ariew 2000)) was suppressed for almost two centuries. Had there been representation of dissenting views in this instance, something like Special Relativity might have emerged much earlier than it did, not to mention the intellectual havoc wreaked for two centuries by treating Newtonian mechanics like an absolutist religion. Only now is the scientific depth of Leibniz' ideas being rediscovered by scientists, as opposed to by philosophers (Calude 2007).

These examples are meant not to undermine the KM idea but to reinforce it. The breadth and depth of modern specialized scientific knowledge almost guarantees that no one can command many diverse fields. Nonetheless, to understand the corrosion of modern science demands the attempt, and Dr. Bauer is to be lauded as an exemplary trailblazer. For the sake of accuracy and credibility, it seems advisable to explicitly consider the KM as an ideal type, and to be sensitive to the goodness of fit in specific instances.

Historical Changes Correlated with the Rise of KMs

There are two critical historical changes that correlate with the rise of KMs which Dr. Bauer did not consider, but to my mind factor centrally in any remedy to the problem of KMs.

Deindustrialization of First World Countries. It is not difficult to link deindustrialization of the first world (Roberts 2012) to the rise of KMs. In economies contracting in terms of real wealth, scientific funding also constricts. A feature of KMs is the reliance on increasingly monolithic funding sources which can constrain the scientific agenda to be "economically productive" and force scientists away from "basic research." Concomitant with deindustrialization has been a rise in bureaucracies, particularly in health insurance and academia, and these too have eroded the independence of medical and scientific institutions.

Economic constriction also facilitates corporate mergers as, for example, in media. One hundred years ago there were thousands of independent media voices in the US; today there are 6 or so trans-national mega-media conglomerates (Bagdikian 2004). Throughout *DSM*, "mainstream media" is often invoked as a force maintaining KMs. It was therefore surprising that media consolidation was not considered as a factor in the rise of KMs.

Recognizing the historical facts of deindustrialization, with the associated rise in monopolies and bureaucracies, would have given a deeper historical context to the rise of KMs. It also would have allowed the proposal of more substantial solutions, because those proposed were not informed by this history. For example, calls for dirigist, as opposed to oligarchical, economic policies would be expected to foster real economic growth, including

scientific investment. Anti-trust actions against media conglomerates would be expected to dilute the effect of KMs favored by specific media monopolies.

Decline of Tenure/Tenure Track Positions in U.S. Universities. According to a report from the American Association of University Professors (Beaky & Besosa 2013), in 2009 75% of faculty appointments at U.S. universities were not tenured or on tenure track and 61% were part-time appointments. To quote:

> Though many people inside and outside of higher education think of tenure-track appointments as the norm, in reality tenure-track faculty are a dwindling minority on American campuses: While in 1975, tenure-track faculty accounted for 45.1 percent of the instructional staff, by 2009 they accounted for only 24.4 percent.

At my own medical school, only 18% of ~700 medical school faculty are tenure/tenure track, and for the University as a whole only one-third of all faculty are tenure/tenure track. It seems to me that the decline in academic freedom and independence may be the key factor in the rise of KMs. Again, the solutions offered in the final chapter were not informed by the erosion of academic independence in the core source of scientific knowledge: the universities.

Both of these historical facts—first-world deindustrialization and the decline in tenured university professors—bring us back to the demarcation problem. Science is embedded in society, and what happens in the greater society intimately affects this ill-understood activity that we pretend to understand when we call it "science."

Conclusion

Dr. Bauer does a professional, competent, and important job bringing the corruption of modern science into the light. The criticisms offered above do not detract from the fundamental correctness of the picture *DSM* paints, but instead underscore its seriousness, and the need to further refine the picture. To scoff at *DSM* or to think it is off-base is merely to reveal that the scoffer is woefully uninformed about the transformations that have occurred in science over the past decades. If one is a practicing scientist, or a concerned citizen of good will, one ignores this book at one's own peril.

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BOOK REVIEW

Dogmatism in Science and Medicine: How Dominant Theories Monopolize Research and Stifle the Search for Truth by Henry H. Bauer. McFarland, 2012. 301 pp. $24.99, Kindle $14.74. ASIN B008AHNIGS.

While the ridiculing of new ideas and their consequent suppression is not a new phenomenon (as for example happened with Semmelweis's proposal that disease could be reduced if doctors who delivered babies washed their hands first), changes in the nature of scientific activity have introduced new and rather sinister aspects into the phenomenon. Bauer cites the case of a letter sent by Duesberg to the journal *JAIDS*, disputing the number of deaths due to AIDS in South Africa quoted in an article criticizing his stance on the subject, suggesting that in that article the number of deaths had been inflated by a factor 25 relative to the official statistics. Notwithstanding the fact that the appointed referees had made no attempt to dispute his analysis, the submission was refused publication. As Bauer points out, publication of a letter alleging serious inaccuracy in a journal article would normally be automatic, unless the allegation could be refuted, but that principle was disregarded in this case.

Subsequently, Duesberg's letter was accepted by the journal *Medical Hypotheses* and posted online. But soon afterward it was withdrawn, pending an 'investigation,' allegedly of claims such as the possibility of potentially libelous material. Months later Duesberg learned that external reviewers had recommended that the withdrawal of the paper be permanent. Publication of the reviewers' comments was forbidden, but Bauer summarizes their content, suggesting that the reasons provided for rejection were incompetent, giving the impression of having been hastily prepared with the aim simply of providing excuses for rejection. A paper by different authors, relating to the risk of HIV infection from dissection of cadavers, was withdrawn by *Medical Hypotheses* at about the same time, apparently by Elsevier's Vice-President in response to complaints, bypassing the Editor who was 'replaced' a month later. The new Editor changed the Journal's policies so it no longer freely accepted innovative ideas but made them subject to peer review, which many consider has significantly diminished the value of the journal for publishing controversial material.

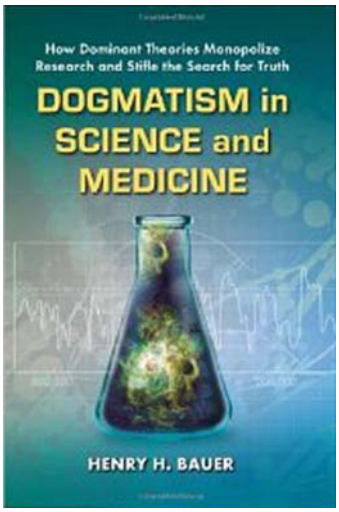
How should one assess what was happening here? It seems that the real reason for rejection in this case was fear that publication might cause doubts in people's minds as to the connection between AIDS and HIV, which might have had public health consequences. But this would have been the case only if the official position were correct; if it were not correct then it would be valuable from the point of view of public health for this fact to be known. There would be benefit for the issues to be discussed in an attempt to determine the truth, rather than have discussion closed down.

But closing down discussion is what happens in highly controversial cases; as the author points out, in such cases orthodoxy behaves like a religious authority, treating dissent as heresy to be excommunicated. I have become aware of this myself on a couple of occasions, once when a conference invitation was withdrawn by an organizer on the grounds that he had become aware that I was interested in parapsychology. Another time, in a meeting on energy, the chairman in a discussion session got up and shouted "Stop! You can't talk about that!" when I made reference to cold fusion in a comment.

Cold fusion is an interesting case: As Bauer points out, the term "has become as iconic of nonsense as 'Loch Ness Monster'." That fact seems to have been due to the fact that if something is repeated often enough it becomes regarded as being true, regardless of whether it is true or not. Here Bauer states incorrectly that a committee set up by the US Department of Energy in 1989 concluded that the claim was mistaken: Rather, it is the fact that it is often summarized as such that has led to the belief that is what the conclusion was (the committee merely concluded that the evidence was not convincing, but also accepting that certain claims were difficult to explain away). Editors of journals such as *Nature* and *Science* then refused to publish papers on the subject and, in a vicious circle, the resulting non-publication in these journals is widely taken as proof that there is no good research on the subject.

This premature closing off of the field of cold fusion will almost certainly be seen as a serious failing of the scientific community some time in the future. The present state of development of the technology, where a number of companies have been able to generate substantial amounts of energy, practical application currently being held up mainly by the need to control the process sufficiently well that reactors can run reliably unattended, might have been achieved many years ago had the normal processes of science applied, with all the evidence having been made available in the main journals to make proper evaluation possible.

Bauer's emphasis is less on the question of whether heresies are correct or not than on the serious failure of the scientific community to address



such issues appropriately. For example, in the context of climate change, computer models are taken to be correct despite the fact that in them there are many factors that are not taken into account. He suggests that people tend to read only summaries of reports and ignore the detail, and that these summaries may be the work of technical writers whose aim may be to put on the actual evidence and conclusions "the best possible spin to reinforce the bureaucracy's viewpoint, and emphasize the importance of the bureaucracy's activities." In cases such as these, however, it is impossible in the absence of fuller information to determine how accurate Bauer's own analyses may be. Certainly I have found myself doubting some assertions in the book, such as the suggestion that "there is still no good treatment for any cancer." A related issue is that of passive smoking, where the author asserts that the evidence for it being dangerous is very weak, and that the belief that it is dangerous has come about as a result of factors relevant to knowledge monopolies generally. That may be so, but Bauer curiously does not mention the important point that absence of proof is not the same as proof of absence, a point that might have diluted his case had he pointed it out. Again, Bauer's account of "flaws in Special Relativity" would seem to point more to flaws in his own understanding of relativity than to any in the theory.

The media play their role because of the way they select news, preferring to publish "what they believe the public want to hear about," and also assuming (as do journal editors) that the prevailing scientific opinion is correct. Propaganda also plays a role in determining what people in general think, often disguised by official sounding names for the organizations concerned (readers of this journal will doubtless be familiar with the propagandizing Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), whose clear bias caused one of its founding members, Marcello Truzzi, to leave the organization). Another factor is funding: If funding bodies take it that alternatives to the consensus can be ignored, then these alternatives will not get funded, to their detriment. Equally, if knowledge that a scientist believes in something heretical can be detrimental to his or her career, this can also be a barrier to proper evaluation of the subject of the belief. The movie *Expelled:*

No Intelligence Allowed (which can be viewed on YouTube) shows what has happened in the case of intelligent design, a topic not among those reviewed by Bauer but of great interest for the way it is simply assumed to be false by the scientific community, without any discussion being needed.

At the end of this fascinating book, Bauer asks the question: Can 21st century science become trustworthy again? He suggests that change must come from outside the existing institutions, which merely serve to perpetuate knowledge monopolies, but first the need for change must become generally recognized. Possibilities discussed include a Science Court; independent, publicly funded institutions that can assess scientific claims of public importance; and designated funds for non-mainstream research. Something of this nature is clearly needed.

Brian Josephson

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BOOK REVIEW

Science and the Afterlife Experience: Evidence for the Immortality of Consciousness by Chris Carter. Rochester, VT: Inner Traditions, 2012. xiv + 369 pp. $18.95. ISBN 978-1594774522.

Science and the Afterlife Experience is the concluding volume of a trilogy that began with *Parapsychology and the Skeptics* (Carter 2007; reissued as *Science and Psychic Phenomena*, Carter 2012) and continued with *Science and the Near-Death Experience* (Carter 2010). These books provide handy introductions to parapsychology, psychical research, and allied concerns (such as the near-death experience) for a new generation of readers. They may best be described as quasi-scholarly, aimed primarily at a general (non-academic) audience, although they include notes, reference lists, and indexes. Carter, who holds an M.A. from the University of Oxford, England, identifies himself as a philosopher and here and there addresses philosophical concerns, such as the implications of "paranormal" phenomena for concepts of personal identity. One of the hallmarks of the series is the attention given to materialistic skeptical positions, extended in the volume under review to include super-ESP.

This new installment consists of 19 chapters arranged in four parts, Reincarnation, Apparitions, Messages from the Dead, and Conclusions, preceded by a Foreword by philosopher Robert Almeder, an Introduction by Carter, and a chapter entitled "Psychic Phenomena and the Near-Death Experience: Background" which summarizes the preceding volumes in the series. Carter does a good job of surveying the classic survival literature and presents a strong case for the persistence of the human personality after death, but this is a book primarily for beginners. More advanced readers may find the survey interesting but are likely to be annoyed by the failure to consider recent works, both of evidence and of criticism.

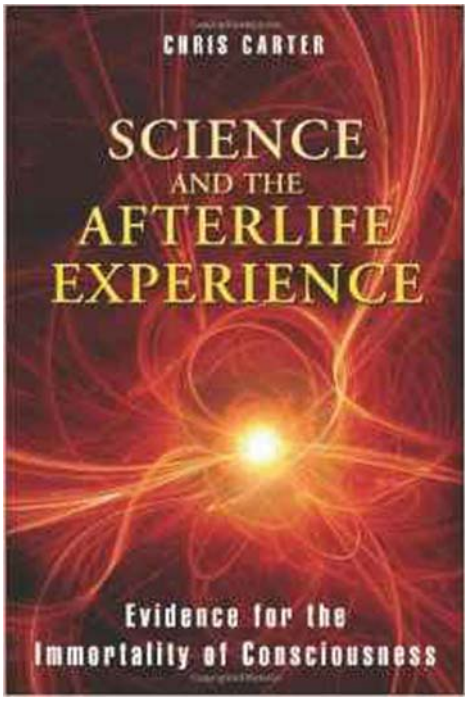
The first chapter sets up the argument of the book by examining the challenge to materialism posed by the experimental findings and theory of quantum physics. Materialism is the philosophical position that all of physical reality, including biological systems, can be explained entirely in terms of material elements and processes. Materialism identifies consciousness with, or reduces it to an epiphenomenon of, brain states, thus ruling out of hand the possibility that it might survive the body's demise. But in the last century quantum mechanics demonstrated that the materialistic

view is incomplete and ultimately unfounded. Some versions of quantum theory, including the orthodox or standard interpretation, see consciousness as standing apart from physical reality. This opens the door to a mind/body dualism or dualistic interactionism that allows for the survival of consciousness after death and means (as theoretical physicist Henry Stapp (2009) has pointed out) that the judgment about whether or not survival occurs must be based on evidence and not on a priori assumptions.

Carter does not say why he chose to start his survey with reincarnation. The decision may strike some readers as strange, because he has not yet established the likelihood of survival, which reincarnation implies. In order for there to be something (let's agree to call it consciousness) to reincarnate, it must first have survived death, and Carter would have us accept survival before providing the evidence for it. The placement of these chapters seems odd also because the reincarnation data are by and large more recent than those on apparitions and mediumistic communications. Apparitions and mediumship were concerns of SPR investigators from the earliest days, but reincarnation began to be researched intensively only in the 1960s, by Ian Stevenson. The argument might be made that the reincarnation data are at present our strongest evidence of survival, so that data from other areas should be assessed with that in mind. Carter, however, does not make this point, nor does he mount as strong a case for reincarnation as he might have done, principally because he relies on a few relatively old sources and fails to take account of recent work. There are in addition numerous omissions and errors of fact that likewise suggest a superficial acquaintance with the literature.

This assessment applies to the book as a whole but is most apparent in the chapters on reincarnation. In Chapter 1, Evidence from India to England, Carter notes the widespread geographical and historical presence of the belief and introduces the reader to Stevenson's research through three case studies, but he makes several mistakes along the way. In reviewing historical accounts of past-life memories (p. 20), he wrongly attributes them to the Indian Tulsi Das, who recounted the experiences of another person, not of himself. Carter asserts that no cases appeared between the early 19th-century case of Katsugoro and a series of Burmese cases published in 1898, whereas Wortabet (1860) described an interesting Syrian Druse case in the interim. He states that most cases reported from India between 1900 and 1960 involved single cases, yet the majority were contributed by two investigators, seven by one (Sahay 1927), and four by the other (Sunderlal 1924).

In describing the 1960s British case of the Pollock twins (pp. 23–25), who behaved in strikingly different ways in line with the deceased older



sisters whose lives they recalled, and only one of whom had birthmarks, Carter misses the very significant fact that they were shown to be monozygotic. Dizygotic or fraternal twins are no more similar than any two siblings, but monozygotic or identical twins share the same DNA and should not (according to materialistic biology) reveal such differences. Later (p. 37), in discussing the case of Ma Tin Aung Myo, a Burmese girl who claimed memories of a Japanese army cook, he notes that she cross-dressed into adulthood but neglects to mention that she became an open lesbian. Role confusion is common in reincarnation cases in which the subject and previous person are of opposite sexes, but pronounced gender dysphoria of the sort exhibited in this case is extremely rare.

All of the references in Chapter 2, Characteristics of Reincarnation Cases, are to a single work, the 1987 edition of Stevenson's *Children Who Remember Previous Lives* (Stevenson 1987). Carter does not seem to realize that this was superseded by a second edition in 2001 (Stevenson 2001). Thus, he says (p. 36) that Stevenson found phobias in 50% of cases in which the death of the previous person was violent. This is true per the 1987 edition, but in the 2001 edition, drawing on a larger sample, Stevenson gives the figure as 36%. Carter repeatedly states that children "almost always" (p. 33) stop talking about the previous lives between 5 and 8 years of age, apparently unaware of a study by Haraldsson (2008) which showed that 38% of Sri Lankan subjects who spoke about previous lives in childhood claimed to retain at least some memories into middle adulthood. His brief discussion of intermission memories (memories of the period between lives) (p. 35) would have been enhanced by reference to a paper by Sharma and Tucker (2004). In discussing the continuation of Stevenson's research (p. 43), he acknowledges a 1994 replication study but not the many later papers by its authors (Antonia Mills, Erlendur Haraldsson, and Jürgen Keil) and

others who have greatly expanded our knowledge of reincarnation cases and their subjects. The evidence no longer rests as heavily on Stevenson, so Carter arguably is wrong when he says on p. 53, "If Stevenson's cases are seriously flawed, then of course the case for reincarnation collapses."

Chapter 3 takes up Alternative Explanations of the Reincarnation Evidence, from fraud to "cultural fantasy." By "cultural fantasy" Carter means the idea that reincarnation cases can be explained as cultural constructions in the context of prevailing beliefs in and about reincarnation. Chapter 4 is devoted to The Objections of Paul Edwards, a skeptical philosopher. It begins with an error in the first paragraph, in which Carter declares that "the writer most frequently criticized in the book is, not surprisingly, Stevenson" (p. 51). Actually, that distinction goes to Elisabeth Kübler-Ross, who receives 38 pages to Stevenson's 30 pages. Chapter 5, Reincarnation in Review, concludes Part I of the book. This short (five-page) chapter is mainly concerned with showing why ESP does not adequately account for apparent past-life memories. Carter does not address the super-psi possibilities outlined by Stephen Braude (2003) or David Ray Griffin's (1997) idea of retrocognitive inclusion, which holds that persons with past-life memories are exercising retrocognitive ESP to access the memories of deceased persons and then incorporating these memory streams into their minds and behaviors.

Part II, Apparitions, is better than Part I, although it too is dated in its coverage. Chapter 6, Strange Visits, introduces the subject with cases drawn largely from Green and McCreery's *Apparitions* (1975). In Chapter 7, "Characteristics and Theories of Apparitions," Carter presents the standard classification of apparitions of the living, crisis apparitions, postmortem apparitions, and haunting apparitions, then considers theoretical approaches under the headings, "The skeptical theory," "The telepathic theory," and "The physically real theories." In a lengthy Chapter 8, What Underlies Ghostly Visions?, he supplies additional cases studies. He concludes Part II with a brief Chapter 9, Final Thoughts on Apparitions.

Carter's selection and presentation of apparition cases is generally good and conveys well what is most exciting about the best cases—the suggestion that personality and dispositions, will and intentions, persist beyond death. He is less good in his handling of theory. He conflates Gurney's earlier and later views and does not deal adequately with the ideas of F. W. H. Myers or with Alan Gauld's (1982) useful evaluation and extension of them, nor does he address Griffin's (1997) assessment in terms of super-psi or Braude's (1986, 2003) theory that apparitions are produced by psychokinesis, much as apparently genuine (non-fraudulent) séance materializations are thought to be. Carter clearly prefers the animistic position (what we might call the

there-really-is-something-there position) but does not seem to be aware of Gauld's (1982) probing criticism and dismissal of it. The there-really-is-something-there position may be fundamentally correct nonetheless, but to be fully convincing it would require a defense beyond the naïve level of analysis Carter provides.

Part III, Messages from the Dead, includes seven chapters and is the longest and best section of the book. In Chapter 10, Ancient Evidence, Carter highlights anthropological and early historical accounts of mediumship. In Chapter 11, The SPR Investigates, he describes work with two classic mediums, Leonora Piper and Gladys Osborne Leonard. In Chapter 12, Alternative Explanations, he discusses the possibilities of "conscious fraud," "subconscious fraud," and "ESP and subconscious fraud," before turning to a consideration of the "difficulties with ESP as an explanation." Chapter 13 asks, *Super*-ESP as an Explanation? Here Carter shows why a very extreme ESP would be required to explain the better mediumistic communications collected during the pre–World War II heyday of psychical research. That the best cases point to the survival of personality and cognitive skills comes through clearly once more.

Carter spends some pages on one of the most interesting of recent cases, involving a deceased chess grandmaster, but fails to consider other studies (e.g., Beischel & Schwartz 2007, O'Keeffe & Wiseman 2005, Robertson & Roy 2004). He includes an account of a case from psychic Arthur Ford, evidently not realizing that Ford was posthumously exposed as a fraud (ironically in one of the sources he cites for the case). His notion of "super-ESP," moreover, is badly outmoded, having been replaced by the super-psi of Braude (1992, 2003), Griffin (1997), and Michael Sudduth (2009).

Chapters 14 through 16 are devoted to the Cross-Correspondences. These were complex networks of motifs and literary allusions communicated through two or more mediums in such a way that their sense was apparent only when all the messages were considered together. These puzzle cases were ostensibly devised by Myers after his death and carried on by other deceased investigators of the early SPR for a period of about thirty years, from 1902 until about 1932. They are considered by many authorities to be among the finest evidence for survival after death, because they suggest not only the persistence of character but of the ability to think and plan in the afterlife. Carter does a good job of introducing the subject, but again his presentation is diminished by his failure to consider recent criticisms, such as those of Braude (2003) and Moreman (2003).

The last three chapters of the book form Part IV, Conclusions. The title of Chapter 17, How the Case for Survival Stands Today, leads the reader to think that he has before him an up-to-date assessment of the survival

evidence. But this book could have been written in the main twenty years ago. Moreover, because he misses so much of the recent evidence for reincarnation, Carter underestimates the present strength of the case for survival in general. Recent work on apparitions and mediumship have done little to resolve the stalemate between survival and super-ESP (or super-psi), but the richly complex reincarnation data provide a much stronger challenge. To handle the reincarnation data, super-psi must explain not only the statements children make regarding previous lives, but also the associated emotions and behaviors as well as the physical signs that link the children to the people they talk about.

Chapter 18 asks, Is Survival a Fact? Here Carter avers that the evidence "proves" survival "beyond all reasonable doubt," backing up this conclusion not with a summary of the evidence but with a "theory of knowledge." Basic to this epistemology is the idea that we can never be absolutely certain that something is true but only that it is most likely true because we cannot demonstrate otherwise. Knowledge thus becomes a "*category* of belief" (p. 281, italics in original), and a well-founded belief in survival is tantamount to knowledge that survival occurs. Chapter 19, "What the Dead Say," considers what mediumistic communicators have had to say about the process of dying and the state of being called death. The first part of this chapter makes an interesting and original contribution to the survival literature, but the second part is based solely on Myers' communications through Geraldine Cummins, published in the 1930s.

I have emphasized this book's shortcomings, but I do not want to leave the impression that it is a thoroughly bad book. Carter has gotten the big picture right and he makes a powerful case for the persistence of personality beyond death. Readers wanting a casual introduction to the survival literature will learn a great deal, and if the book succeeds in getting them to go further into the literature it will have served an important purpose. But readers should not expect an up-to-date survey of the subject, nor should they look for a careful weighing of the evidence for survival, pro and con. Readers whose tastes tend toward more academic writing and sophistication in analysis would do better to start with Griffin (1997) or Braude (2003), both of whom also end up endorsing survival as the most satisfactory interpretation of the data in toto.

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BOOK REVIEW

Medical Qigong: Fact or Fiction?

Chinese Medical Qigong edited by Tianjun Liu and Kevin W. Chen. Jessica Kingsley Publishers, 2010. 656 pp. [2013 edition $29.95. ISBN 978-1848190962]

Qigong, China's ancient system of exercise, meditation, and energy therapy ("External Qi"),[1] is, by sheer numbers, the most popular form of Complementary and Alternative Medicine (CAM) in the world. There are at least 100 million practitioners in China and millions worldwide, including more than 625,000 in the United States (NIH 2008). If we include Tai Chi, which is both Qigong and a martial art, there are an additional 2.2 million practitioners in the U.S. alone, and these numbers are increasing. One no longer needs to go to "New Age" or "Pseudo-science" sources to find healing reports. Rather, typical of readily available literature is the meta-analysis published by the American Medical Association in 2004, in which data indicated benefits in "balance and strength, cardiovascular and respiratory function, flexibility, immune system, symptoms of arthritis, muscular strength, and psychological effects" (Wang, Collet, & Lau 2004), though it is difficult to draw firm conclusions because of limitations or biases in some of the studies. More recent reports include impressive evidence of Qigong or closely related mind–body disciplines preventing oxidative stress and enhancing positive genetic expression (Dusek et al. 2008) as well as significant corroborating evidence for benefits previously reported (Jahnke et al. 2010).

Unfortunately, Western scholars and researchers have generally had little access to primary source material from China. This situation is partly remedied by the publication of *Chinese Medical Qigong,* the *only* work on the medical applications of Qigong officially approved by the government of the People's Republic of China (PRC). It has become the standard Qigong textbook in Chinese healthcare programs and schools of Traditional Chinese Medicine and, because of its status, is growing in usage in the United States. The English-language edition was a collaborative effort of more than 30 Chinese scholars, the U. S. Editor-in-Chief Kevin W. Chen, and a team of U.S. consulting editors.

Translators in China did a preliminary English version of the Chinese

text, perhaps with the help of a computer translation program, as most of the material was incomprehensible. This initial draft was sent to a group of U.S.-based editors, all of whom had a background in Qigong or Chinese medicine, who were told to "correct" it for grammar and style. I was part of the team of "Final Consulting Editors." Luckily, I had a copy of the original Chinese edition in my library. I re-translated and edited the Introduction, Chapter 1, Chapter 9, and the Glossary.

I know that readers may find it unusual that I am writing a review of a book with which I am associated. Is there a conflict of interest? I believe not, and am grateful that the *Journal of Scientific Exploration* accepted my reasoning: I did not write the original text. I have, thus far, not received any royalty, though a small one, divided among all the authors and editors, is possible if publication costs are recapped. I do not have any connection with the authors or institutions associated with the book. Thus, I declare no competing loyalties or interests that could bias my judgment.

The authors are to be commended for the breadth of material covered. Readers will discover a great deal of new information and an integration of data that, previously, could be found only through laborious searching through academic and scientific journals, generally in Chinese. There is a good summary of Qigong research including a timeline showing the evolution of research from single cases, to thousands of cases, to impressive experimental and clinical studies. The authors, although not ruling out psychological factors such as placebo, cite in vitro and animal studies that effectively eliminate placebo as the mechanism of effect. For example, a group of 30 nude mice injected with hepatic cancer were randomly assigned to a control group, a group treated by a Qi healer, and a sham treatment group treated by untrained individuals. After four forty-minute treatment sessions once every other day, only the group treated by the actual Qi healer demonstrated statistically significant reduction in tumor growth. Electron microscope analysis of the cancer cells confirmed morphological changes in the Qi-treated group. Many of the animal studies cited are especially relevant to integrative treatment strategies, as External Qi or personal practice is often combined with appropriate drug therapies. As an example, when tumor-bearing mice were given cyclophosphamide, those treated by External Qi had slower-growing tumors and demonstrated different natural killer cell, macrophage, and interleukin-2 activity compared with controls. We assume that mice do not believe in the healing skill or authority of the healer! However, if professional skeptics still cling to "placebo," the in vitro experiments should certainly frustrate them to no end. In vitro studies have been performed in which the Qi healer effectively destroys both gram-positive and gram-negative bacteria and numerous types of cancer cells (pp.160–161).

Issues and variables that have been ignored by Western scientists, and thus confounded research results, are highlighted. For example, the text notes that when a Qigong master attempted to emit Qi to various chemicals, light absorption (measured through circular dichroism (CD) spectroscopy) was affected by the practitioner's state of mind (p. 158). In later chapters, the authors also describe other, often overlooked, influences on Qigong treatment outcomes, including individual compared with group teaching and practice, the influence of a mountain environment and beautiful scenery, and the importance of directional orientation, such as facing North compared with South while practicing Qigong techniques.

In Western medicine, both the type and dosage of medicine must be tailored to the condition of the patient. *Chinese Medical Qigong* provides important details about how to create an appropriate Qigong regimen, including consideration of gender, age, individual character and physique, and lifestyle (sedentary vs. active). Numerous Qigong methods are described, though rarely with illustrations or with enough details to be able to practice on one's own or with confidence in accuracy of technique. And a full half of the book is devoted to Qigong exercise and treatment "prescriptions" for many of the most common diseases, including hypertension, heart disease, cancer, diabetes, depression, back pain, obesity, insomnia, chronic fatigue syndrome, and much more. The last section of the book is a detailed overview of Qigong literature with selected translations. This will be invaluable for future researchers who are looking for a reliable reference list.

In spite of these merits, the objectivity and validity of many portions of the text were tainted by an excessive emphasis on the medical and scientific nature of Qigong. Perhaps this is to be expected in a country where religion and spirituality are considered incompatible with the prevailing Communist ideology.[2] *Chinese Medical Qigong* attempts to backdate medical Qigong in a kind of revisionist history. Ancient dances and Daoist meditations that for most of the world would be the domain of shamanism or theology are here considered the origin of medical Qigong. Worse still, we see in this book the lingering effects of China's Cultural Revolution (1966–1976). Sadly, during this period many Chinese citizens lost touch with their own cultural traditions. Rather than lose face by admitting ignorance or searching for reliable scholarship outside of China, the authors of the original *Chinese Medical Qigong* text frequently make up information, cite myths as fact, and promote stereotypical views of the past, without apparent knowledge of relevant scholarly literature.

> Our ancestors in the New Stone Age lived a tough life in the struggle for survival but were content with their lot. Their simple lifestyle and peace-

> ful mental state kept their metabolism slow and mood/mind calm, so they were not subject to pathogenic Qi. This is a veracious description of primitive Qigong for the purpose of life-nurturing. (pp. 34–35)

When I read this, I wondered, "How do we know that ancient people were "content with their lot" and maintained a slow metabolism and calm mood? I would find it difficult to remain calm if I returned from a poor hunting expedition at the beginning of winter or knew that a saber-toothed tiger lurked nearby! The authors are also careful to reframe information to make it politically acceptable. Hence, yin/yang theory becomes "Marxist dialecticism." As one of my colleagues, another co-editor, put it—"this book is essentially a work of Chinese nationalism."

Yi: Is It Medical?

The original title of *Chinese Medical Qigong* is *Zhong Yi Qigong Xue*. It is unlikely that the word *yi*, generally translated "medicine, medical, or doctor" had the same connotations as the English word "medical" before the Republican period, 1911– 1949. Rather the meaning was closer to "healing," as it covered physical, psychological, and spiritual dimensions of health. A Chinese doctor focused more on wholeness, balance, and prevention than on combating disease. At the heart of Chinese medicine there was still a link between healing and the natural or animistic powers invoked by shamanism. Indeed the word for doctor was once drawn with the component for shaman (*wu*). The terms "Qigong" and "Medical Qigong" *Yi Gong* were coined and popularized by two friends, Liu Guizhen (Daoist, acupuncturist) and Hu Yaozhen (Daoist priest, martial artist) around 1950, in an attempt to protect this Daoist art from the label *fan dong* "counter-revolutionary." In other words, "medical Qigong" was a political expediency, a fact that is omitted by the authors of *Chinese Medical Qigong* and other similar works on the subject.

Although there were certainly some Chinese medical practitioners who incorporated or prescribed Qigong, in general *Dao Jia Yang Sheng Shu*, Daoist Arts of Nurturing Life (or *Yang Sheng* for short, an ancient name for what is now called "Qigong"), emphasized systemic well-being, longevity, and spiritual transformation. Yang Sheng was no more "medical" than prayer or talisman writing is "medical." Yet this does not exclude the realization that an ancient art can be put to modern medical usage.

In the U.S. there are legal ramifications to stressing the "medical" side of Qigong or claiming ancient *Yang Sheng* as a medical discipline. If it is fundamentally medical, then Qigong becomes the domain of licensed medical practitioners, such as chiropractors, medical doctors, and, in most

states, acupuncturists. Instead of being a primary therapy for the spiritual and "energy" side of illness (with "cure" a hoped-for side effect), Qigong healers, like acupuncturists, would be legally restricted in the scope of their practice. In California, for example, acupuncturists are allowed to treat pain, nausea, and other side effects of radiation and chemotherapy, but are not permitted to treat cancer, *even though their methods have demonstrated great efficacy in the laboratory.* If Qigong were licensed, intuition, a hallmark of the Qigong master's ability to tune in to the needs of the student, would deteriorate in favor of standards set by boards, insurance companies, and the paranoia of "better safe than sueable." There are also numerous financial quagmires along the path to licensure: the need for malpractice insurance, the various state statutes against sliding scales among medical practitioners, and the requirement of co-providers to accept Medicare once they are approved by the insurer (whereas conventional medical practitioners such as doctors and dentists may refuse to accept it if the reimbursement policy is unacceptable to them).

Although there is certainly a master/student (or client) hierarchy in Qigong, it is qualitatively different from the status of doctor compared with patient. Qigong healers and teachers expect their clients/students to adhere to certain behavioral and ethical standards. If clients are clearly disruptive, dishonest, unappreciative, or fail to follow instructions (including practicing Qigong techniques), they may be dismissed, hopefully with some tact and compassion. Medical professionals, on the other hand, are not allowed to dismiss a patient; although in rare instances it is permitted if they follow set protocols, such as referrals or when closing or selling a practice. The patient can abandon the practitioner, but not vice versa.

At present Qigong is somewhat protected because the National Institutes of Health (NIH) in its classification of CAM modalities does not consider it a medical system, but rather a form of energy healing, closer to Reiki or therapeutic touch. Similarly, Qigong is classified in some states as a form of spiritual healing. Practitioners may still need to register with regulatory agencies and provide informed consent forms to clients that describe their training, ethical standards (including confidentiality), precautions, scope of practice, and how to register complaints. But this is a very different level of oversight from that required for medical practitioners. As long as Qigong practitioners are not practicing medicine, they cannot be sued for practicing medicine without a license. The concept and practice of "medical Qigong" threaten the protections that "healing" Qigong now enjoys.

In China, the situation is different. Right or wrong, Qigong therapy is a category of medical practice. Qigong healers' medical records are virtually identical to those of doctors of Western Medicine, except that

their "diagnoses" include both Western and Chinese categories of disease, and their treatments may include a "prescription" of Qigong exercises. The parameters of medical practice are limited by ideological concerns rather than by the economic and lobbying power of the medical, pharmaceutical, and insurance industries so common in the United States. My personal opinion is that the legal, political, social, educational, and economic differences between China and the United States make it ill-advised to promote "medical Qigong" here. With this caveat in mind, let's proceed to a more critical look at the content and style of *Chinese Medical Qigong*.

Critical Commentary

Readers will enjoy the contrasts between ancient and modern diagnostic categories, such as diabetes mellitus characterized as an endocrine disease that in Traditional Chinese Medicine is known as *Xiao Ke* (Extreme Thirst) resulting from prolonged Yin deficiency. The juxtaposition of old and new terminology along with modern explanations of archaic concepts is very helpful for those not steeped in China's rich, ancient literature. Unfortunately, these and other merits of *Chinese Medical Qigong* are often hidden from Western readers because of poor translations. As mentioned earlier, the team of U.S. editors was sent an incomprehensible English text and told to "edit it." Most of the English edition editors did not read Chinese or have a copy of the original text and relied on, as one put it, "best guesses." Sometimes even these best guesses were overruled by editors in China who had final proofing rights. For example, I changed "Bionic Qigong" (*Que Gu*) to "Qigong Fasting" or "Grain Avoidance" and was happy that my corrected translation was retained. An ancient text, *Jing Ming Zong Jiao Lu* is translated Ana [sic] of Pure and Bright School. I recommended "Records (or Annals) of the Clear Bright Sect." The meaningless word "Ana" was kept in the published book (p. 14).

Some poor translations were simply the result of nuances and connotations of which Chinese editors were unaware. We see this in what was perhaps the most important translation of all, the definition of Qigong: I submitted the translation, "Qigong is a mental and physical skill and practice that adjusts body, breath, and mind, bringing them into unity" with an added editorial note that the word "tiao" (adjust) may also be translated "tune" as in tuning a piano. The published version has, "Qigong is the skill of body–mind exercise that integrates body, breath, and mind adjustments into one" (p. 15) The English draft contained numerous examples of nonsensical sentences such as this, from the Daoist classic *Taiping Jing* (Classic of Great Peace), "(You) empty the room, paint Five zang [yin organs—liver, heart, spleen, lungs, kidneys] organs corresponding to their color and the Qi

of four seasons. Then hang it up on the window light to ponder." I changed this to: "In an empty room, imagine the five zang organs [lungs, heart, liver, spleen, kidneys] in their respective colors and in accord with the Qi of the seasons." This was one of the rare instances when I did not have a copy of the source text (*Taiping Jing*) in my library, and, frankly, without spending a full day searching through the text to find and read the pages surrounding the quote—the context of these sentences—I could not be absolutely certain of my translation.

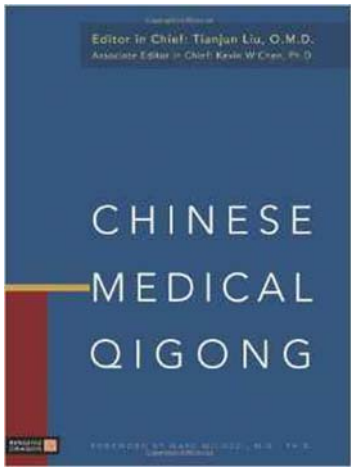
I hate to say it, but I suspect that some readers who praised this book either didn't read it carefully or confused difficulty with depth. Here is a typical paragraph, found near the opening of a chapter called Psychological Effects of Qigong:

> Mental activities in Qigong practice are quite distinct from those in everyday life mainly in respect to the thinking form. The rudimental requirement for the thinking maneuver in mind adjustment is, therefore, transforming the basic thinking form from abstract (language-based) and imaginal thinking into the pattern mostly based on concrete thinking. (p. 149)

Grammar, extremely poor in the draft translation, was only occasionally corrected pre-publication. The work is replete with run-on sentences and redundancy. Some stylistic norms in Chinese writing, such as unnecessary repetition, are the vestige of rote learning and the memorization required for pre-Republic civil service exams. I tried to eliminate such repetitions when it seemed advisable and hoped that other editors would do the same.

To clarify obscure ideas, expand discussions, or suggest other viewpoints, in the chapters I translated/edited I added various editorial notes in parentheses as "Editor's note:" It was disappointing to discover that in the published version, my notes were included as footnotes without any acknowledgment of my authorship. This implies that the footnotes were in the original Chinese text.

Book titles and technical Qigong terminology are translated without regard for established Western academic standards. "Form is not different from emptiness" (*Se bu yi kong*), a well-known phrase from the Buddhist *Heart Sutra*, is translated "Visible is equal to invisible" (p. 587). "Triple Burner/ Triple Heater" is rendered "Triple Energizer." The classic text *Lushi ChunQiu* is translated *Collection of Eclectics*. I had been overruled in my attempt to change this to the standard *Springs and Autumns of Master Lu*. Thankfully my Chinese colleagues accepted my rendering of Qu Yuan's poem *Yuan You* as "Distant Wandering" rather than their "Get Away" and *Balanced Instructions on Spirit and Life (Xingming Guizhi)* instead of *Life Sundial*. Some of the most glaring mistranslations are found in the



"translation" section at the end of the book, where, again scholarly corrections were overruled by non-English speaking editors in China.

The original Chinese authors seem to have little familiarity with critical works on traditional Chinese literature. For example, "Confucianism stressed the importance of 'quiet sitting' or 'sitting meditation' (*jingzuo*), as seen in the book, *Zhuangzi*" (p. 39). Confucianism did not stress quiet sitting, and *Zhuangzi* uses these sections to make fun of Confucianism. To take such spurious references literally would be as absurd as citing the *Hua Hu Jing* (Classic on Converting the Barbarians) as proof that Laozi taught the Buddha! *Chinese Medical Qigong* has a general tendency to portray conjecture as fact, for example calling Qu Yuan's *Yuan You* a work of "Qigong poetry," rather than folk belief, or labeling Tao Hongjing as a Qigong scholar rather than, primarily, a Daoist alchemist (and one who died young from taking his mercury-laden elixirs). Similarly, the authors suggest that the Jade Pendant Inscription (one of the earliest literary references to breathing techniques) may belong to a particular Qigong School. Yet there were no "schools" of Qigong during the 4th or 5th Century B.C. Or when the authors speak about "scientific progress" during the Tang Dynasty contributing to the evolution of Qigong theory, how do they define "scientific progress"? China certainly has a great scholarly tradition, which makes these errors all the more puzzling.

There are also confusing historical inconsistencies; the reader may not know what to believe. For example on page 48, we read about the origin of the Tiantai sect of Buddhism: "At the close of the Southern and Northern dynasty, the Buddhist Tiantai sect began to take form. It traced its philosophy back to Nagarjuna (Longshu), an Indian monk and philosopher, and regarded him as the founding master. Huiwen and Huisi were the second and third masters, during the Northern Qi dynasty. The fourth master, Zhiwei, brought the sect to its full development during the Chen and Sui dynasties." Then on page 51 Zhi Yi is identified as the sole founder of the sect. It is also a serious oversight by the authors to not include the relationship of Qigong to Buddhist theories of disease, as discussed in Zhi Yi's writings. (Zhi Yi's works also contain one of the earliest references to the popular *Liu Qi Fa* Six Healing Sounds Qigong, a fact not mentioned by the authors.)

I was especially interested in how the authors would describe shamanism, the substratum of Daoism, Chinese medicine, and Qigong. Indeed the word doctor, *yi*, was originally drawn with a key component meaning "shaman," and another component meaning a quiver of arrows, suggesting that the shaman/healer removed the arrows of disease, perhaps through physical gestures or prayer. *Zhu you*, "exorcising illness through prayer," was once a major branch of Chinese medicine. In *Chinese Medical Qigong*, descriptions of the link between shamanism and Qigong are inaccurate, misleading, and demeaning to both indigenous people and the scholars who study them. In the first draft of the historical chapters, the Chinese authors translate *wu* as "wizardry" rather than the standard "shamanism" and refer to practitioners as witch doctors, witches, or wizards. The latter two terms belong to Western paganism. Was Merlin, perhaps, a practitioner of Chinese wu-ism? Only after much persistence was my recommended translation for wu ("shamanism") accepted, though the terms "wizardry" and "witchdoctor" still appear on page 37. The Chinese official view of shamanism reminds me of early American missionaries' cants against North American Indian spirituality.

> During the period of feudal society, a few distinguished shamans became rulers and some became 'medical' doctors, but most turned into quacks or swindlers. These charlatans carried out their performances in the name of the medical knowledge and techniques now called Qigong and consequently disgraced the reputation of both medicine and Qigong. (p. 37)

The authors suggest that it was only "witch-doctors'" knowledge of physics, chemistry, and medicine that allowed them to "perform tricks so as to obtain the trust of their audiences" (op. cit.). When the authors wonder if "perhaps shamanism was the only medical service available to primitive humans" (p. 36), they imply that given the choice, any modern or rational person would certainly choose a prescription over a prayer. Two major problems here: (1) Much of the world's population depends on shamanism as primary healthcare, even when given the choice of therapies. This includes some six million Hmong in China, among whom shamans are held in high regard. In Central and South America, Curanderos (male) and Curanderas (female)—a Meso-American equivalent of Siberian shamans—have used their combination of physical and spiritual therapies as front-line responders during national disasters such as earthquakes and hurricanes. According to U.S. National Health Statistics, in a typical year, approximately 812,000 Americans visit traditional healers, which includes 329,000 to Curandero/as (combining the categories of Curandero, Yerbera–Herbalist, and Sobador–Indigenous Massage Therapist), and

224,000 visit Native American healers. (2) There is strong evidence for the healing benefits of music, ritual, prayer, and other shamanic techniques, some of which can be analyzed scientifically, but much of which appears to be acausal, nonlocal, and beyond explanation. To dismiss phenomena as "trickery" simply because the mechanism is unknown or in defiance of current paradigms is not science but scientism.

Chinese Medical Qigong contains many other less troubling oversights or inaccuracies. For example, in their discussion of the origin of modern Qigong, the authors mention the important role of Liu Guizhen but not his colleague Hu Yaozhen, also considered the father of modern Qigong. Also, the authors cite 1979 as the beginning of the "second high tide" of Qigong and the beginning of Qigong's spread abroad. Neither is entirely true. The Cultural Revolution's prohibition against Qigong was not fully lifted until about 1982, largely because of the endorsement of nuclear physicist-turned-Qigong-exponent Qian Xuesen, whose omission here is problematic. Also, Qigong had spread abroad many years earlier during pre–Cultural Revolution days.

Here are some of the important and well-known facts in the Western Qigong timeline, which should have been noted. Choy Hok-P'eng (1885–1958) taught Tai Chi in San Francisco beginning in 1939, followed by his son, the famed Choy Kam Man (1920–1994) who, in the 1950s, was the first to teach large numbers of non-Asians. Daoist Master Share K. Lew (1918–2012) arrived in the U.S. in 1948 where he trained and mentored thousands of students. Da Liu (1904–2000), who had an impeccable lineage in Daoism, Qigong, and internal martial arts, taught at the United Nations and in other New York City locations beginning in 1955. Kuo Lien Ying (1895–1984), with a distinguished background in Qigong, Tai Chi, Shaolin, and other martial arts, taught in San Francisco beginning in 1965. Cheng Man-ch'ing (1902–1975), carrying the Yang Style Tai Chi lineage and Daoist Qigong from Zhang Qinglin, moved to Manhattan in 1964. Ironically, while Qigong was illegal in China, it was becoming increasingly popular in the United States.

Other Sins of Omission

Knowledgeable readers are likely to be concerned about the "sins of omission:" particularly the limited number of diseases and Qigong methods mentioned and the lack of information about the history of the various styles (e.g., Dr. Ma Litang in connection with the "Six Syllable Formula;" Guo Lin with New Qigong Therapy, Walking Qigong, and the revival of interest in Five Animal-Play; and Wang Xiangzhai with methods of Standing and visualization created by him). But, in all fairness, it is impossible to

cover everything in one text, and there is indeed a wealth of information here. Readers should carefully consider the many Qigong prescriptions offered, but not assume that they all are safe or generally recommended by a majority of Qigong clinicians. For example, page 271 has a description of a standing meditation posture called "Subduing Tiger" (part of Master Wang Xiangzhai's *Yi Quan* Qigong method, not noted in the text). Positive effects on the muscles, joints, nerves, and stability are described correctly. However, the authors also recommend Subduing Tiger for its therapeutic effects on "prolapse of the lumbar intervertebral disc, chronic strain of lumbar muscles, sacroiliac joint dysfunction syndrome, and injury of the knee joint and anklebone…" (p. 271) In my experience, these are the very conditions for which Subduing Tiger is generally *not* recommended. Some methods are so counterintuitive that, without case studies or at least more explanation, I am not convinced. For example, on page 478, an anti-cancer method is described that consists of inhaling, then holding the breath and moving the diaphragm rapidly up and down to increase abdominal pressure and heat. The heat and pressure are mentally directed to the tumor. Then, with a rapid exhalation and deliberate abdominal tension, toxic Qi is emitted in a sudden "burst." In the United States, massage therapists are warned about putting pressure on tumors, lest they burst and metastasize. Is Qigong so very different?

As disturbing as the lack of information is the lack of attribution. China is not known for its protection of intellectual or artistic property. Various translations scattered throughout the book are strangely inconsistent with the poor grammar and translations elsewhere. I cannot help wondering about their source. When I received the galley proof of *Chinese Medical Qigong*, I found that my own copyright had been violated because one of the other U.S.-based editors had copied much of the Glossary section from my book *The Way of Qigong: The Art and Science of Chinese Energy Healing* (Ballantine Books, 1997) without acknowledgment and without my knowledge. At my insistence, a note was added to the final published text that I gave permission to copy definitions from my book.

The Bottom Line: *Chinese Medical Qigong* is a tome filled with information that will be new for most Western readers, including details about the history and development of Qigong, the current state and view of Qigong, descriptions of various techniques, a solid overview of research conducted in China, and, very importantly, how Qigong is applied in the treatment of disease. It is an ambitious work, perhaps overly ambitious.

Notes

[1] Similar to Therapeutic Touch, though generally applied without physical contact.

[2] During the last fifteen years, there has been an increase in PRC regulation of Qigong as a medical discipline, in part as a reaction to the Falun Gong movement, a late 20th century religious cult that recommended Qigong to its members as a way of unifying people through both ideology and body-based practices. Falun Gong is not a style of Qigong but, rather, a religion that includes Qigong, in the same way that Christianity is not prayer, but a religion that includes prayer. Falun Gong is a messianic cult that claims that the earth is "the trash heap of the universe." The founder of the movement, Li Hongzhi, names Jesus, Buddha, and other religious figures as saviors who lift people out of their decrepitude. He suggests that in this age, he is the new savior.

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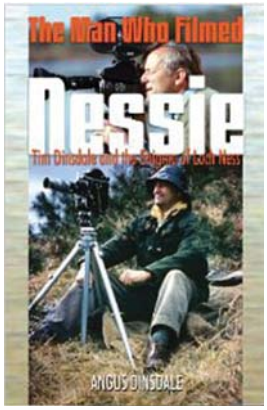
Further Books of Note

The Man Who Filmed Nessie: Tim Dinsdale and the Enigma of Loch Ness by Angus Dinsdale. Surrey, BC, Canada: Hancock House, 2013. 256 pp. $25.95 CAD (paperback), $9.99 (Kindle). ISBN 978-0888397270.

The strongest evidence that Loch Ness Monsters are real animals is the film taken in 1960 by Tim Dinsdale. It remains unique and remains the evidence that debunkers have most determinedly attempted to undermine. In trying to assess the merits of the film and of the criticisms, one wants to see the film itself and to learn about the circumstances of the filming and the trustworthiness of the photographer. This book by Dinsdale's youngest child is illuminating about the circumstances and the photographer, and the book's website (http://www.themanwhofilmednessie.com/) presents the film in informatively annotated fashion. The book includes more than 30 pages of color photos showing people and historical events associated with the search for Nessie.



This reviewer is utterly biased, having come to know Tim Dinsdale as a trusted friend. Readers of this book are presented enough plain facts about Tim to appreciate the genuine enthusiasm he had for establishing Nessie's reality and identity, and to learn about the support from his wife Wendy that allowed him to carry on his quest without neglecting his family of four children; to the contrary, the Nessie hunt was a family adventure.

Of course the book itself is also biased. Angus, the author, himself accompanied his father on Nessie hunts and himself had a sighting. But bias in favor of the reality of Nessies does not detract from the book's recounting of events during the 1960s and 1970s, which recaptures the considerable public interest during those years that saw a variety of individual Nessie hunters as well as organized expeditions of volunteers led by the colorful Member of Parliament David James, already well known in earlier years for his adventurous escape from a German POW camp during WWII.

Anyone who has been at all interested in the Nessie controversy will find this book highly enjoyable reading. Anyone who has not taken much interest in the matter before can get a marvelous feel for several decades of

happenings from this readable account, enlivened by tidbits about the host of memorable characters who have had their hand in Nessie-seeking over the years.

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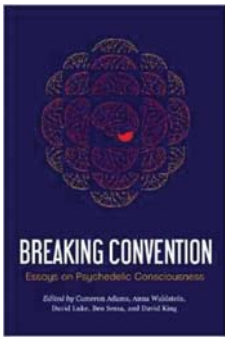
Breaking Convention: Essays on Psychedelic Consciousness edited by Cameron Adams, Anna Waldstein, Ben Sessa, David Luke, and David King. Berkeley, CA: North Atlantic Books, 2014. 320 pp. $15.12 (paperback). ISBN 978-1583947715.



Psychedelic drugs disappeared as quickly from the public eye in the early 1970s as they had burst into the consciousness of the 1960s. At first touted as a key to mental illness, creativity, and spirituality, LSD and other psychedelic drugs became anathema due to their association with civil unrest, and psychiatric and other medical morbidity. As a result, clinical and other research into their effects ceased with their placement into an inaccessible legal status in the U.S. and in Europe in 1970. The current resurgence of medical research with psychedelics has received generally positive media treatment and benign government oversight. As a result, psychedelic drugs are once again being discussed in academic circles.

This selection of 20 essays comes from a 2011 conference in the UK called Breaking Convention: A Multidisciplinary Conference on Psychedelic Consciousness. The topics covered are broader than simply consciousness itself and include legal and research questions that are associated with the study and use of psychedelic plants and drugs. The majority of essays come from the social sciences, philosophy, literature, and history. A smaller number report on or about clinical research with these substances.

The editors in their Introduction clearly state their "pro-psychedelic" stance, and this is the basis of my only misgivings about this book. There is little attention in this collection directed toward how psychedelic drugs may be harmful and misused, increase wrongheadedness, or may provide little more than a fleeting sense of meaningfulness. My only recommendation for

a second iteration of this project, should one take place, would be to include contributors with a more cautionary perspective.

The essays are generally well-written, engaging, and optimistic for the future of the field. As a snapshot of how the contemporary, primarily non-medical, academic community is approaching what appears to be a second flowering of research into the psychedelic drug experience, this book is highly recommended.

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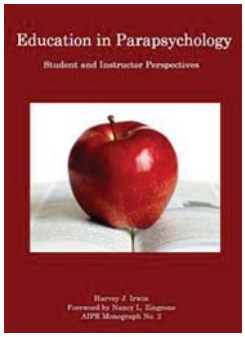
Education in Parapsychology: Student and Instructor Perspectives (AIPR Monograph No. 2) by Harvey J. Irwin. Gladesville, New South Wales, Australia: Australian Institute of Parapsychological Research, 2013. 105 pp. Australian $25. (paperback). ISBN 978-0987077219.



Harvey Irwin has had more than 30 years of involvement in both teaching and researching parapsychology within a university setting. In this monograph he offers, with much modesty, advice for students and instructors based on his valuable experience in the field. It feels like a parting gift, as he states in the Preface: "As I near the end of my own career in parapsychology it is timely that I proffer to others the benefit of my considered experience to use or not to use as they wish" (p. viii). The monograph consists of two parts, the first directed toward prospective students of parapsychology, and the second toward prospective instructors. While these may seem like two distinct audiences, as Irwin notes there is much for the student to gain from considering the perspective taken by an instructor as he or she plans a course in parapsychology, and speaking from my own experience as an educator in this field, I am confident that from now on I will be recommending this book to every prospective student who approaches me.

In the first part of the monograph, Irwin pulls no punches in addressing misconceptions about parapsychology, motives for studying parapsychology, and practicalities and benefits of studying this topic. He gives a frank and realistic appraisal of what it means to study parapsychology, and provides wise advice on how to do so while minimizing the risk of damaging one's future career prospects. In addition to advice to educators on designing a parapsychology course, the second part gives practical advice based on the (not unreasonable) assumption that the prospective instructor is situated

within an academic environment that may be hesitant or even openly hostile to the idea of teaching students about such a controversial topic.

In sum, I think this monograph will be of great value to readers who wish to become actively involved in learning or teaching parapsychology. It is succinctly written, but packed full of treasures. Fittingly, Irwin dedicates the book to his students, from whom he has learned so much about the fascination of parapsychology.

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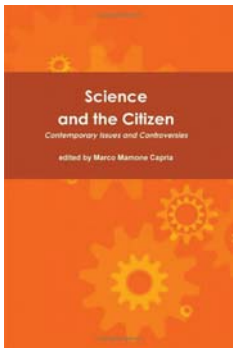
Science and the Citizen: Contemporary Issues and Controversies
edited by Marco Mamone Capria. lulu.com, 2013. 450 pp. $25.99 (paperback). ISBN 978-1291446838.

Capria organizes the Science and Democracy conferences in Italy (http://www.dmi.unipg.it/~mamone/sci-dem/sci&dem.htm), which cover a wide range of topics typically critical of mainstream ideas and actions. This book contains a selection of essays presented at a conference or posted on his website. Four pieces grouped under the heading Brave New Science and Its Discontents address societal interactions of science with economics and corporatism. Experts and Participatory Democracy includes questions of risks, environment, nuclear energy. Corporate Medicine includes my essay, Evidence-Based Medicine? Wishful Thinking, David Rasnick on AIDS Drugs Cause AIDS and Death, Martin Walker on The Bigger the Lie—The Wakefield Case, and Rocco Maruotti on Ethics, Surgeons, and Transplantation. Public Opinions, Official Lies, and Whistleblowers focuses on conspiracy theorizing, explanations of the 9/11 events, an interview with the activist mathematician Serge Lang (now deceased), and Lang's file on suppression of dissent in contemporary science. The final essay, by Anthony Liversidge, is a delightful description of the ambience of the Science and Democracy conferences. Every anomalist and contrarian should find some things of interest in this volume. Libraries should be urged to add it to their collections.



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The 33rd Annual North American SSE Meeting
June 5–7, 2014, San Francisco, CA, USA

Program Chair: Adam M. Curry (adam.curry@psyleron.com).
Local arrangements are being coordinated by Jerry Gin (jerry@ginclan.com).
Conference hotel: Hyatt Regency San Francisco Airport, Burlingame, California, USA, 94010. http://sanfranciscoairport.hyatt.com. Telephone: 1-650-347-1234. SSE hotel rate: $139 per night (single or double) including up to 3 days before and after the meeting. Reservations must be made by May 20 to receive this rate. Reserve rooms at http://resweb.passkey.com/go/SSE2014. You can also make a reservation by phone at 888-431-1442. Be sure to mention you are with the Society of Scientific Exploration to receive the special rate. The cutoff date for special rates is 5/20/2014.

Welcome Reception: Wednesday, June 4, starting at 6 p.m. at the hotel.
Field Trip: Computer History Museum in Mountain View.
Conference Topics: THE MYSTERIOUS UNIVERSE (all talks will be in the Hotel Ballrom)
Theme 1: The Conscious Lab: New Research in Consciousness
Dean Radin, Institute of Noetic Sciences (Invited Speaker) — Mind–Matter Interaction Experiments Involving Light
Theme 2: Future Energy: Horizons of Energy Research
Vittorio Violante, Rome Tor Vergata University—Material Science Challenges to Define the Fleischmann and Pons Effect by Applying the Scientific Method
Theme 3: Earth and Beyond: Evidence of the Mysterious Universe
Gerald H. Pollack, Univ. of Washington (Dinsdale Award Lecture)—The 4th Phase of Water
Gary Nolan, Stanford University (Invited Speaker)—DNA Analysis of the Atacama Humanoid
Evening Panel: A free-wheeling discussion of program topics will be the focus of an evening panel of members and invited speakers.

Workshop: The SSE will be holding its first full-day workshop, on June 8 at the hotel, in which Roger Nelson of the Global Consciousness Project will discuss using randomness to study consciousness.

Students: The Society encourages the attendance of students and young investigators, and the conference is open to the public. There is a special, reduced registration fee for students at this event. Please encourage students from your area to come.

CONTRIBUTED PAPERS

Contributed Papers by Full members on any topic of interest to the Society are welcome. Papers related to the themes of the conference will be grouped with relevant invited talks when possible. A poster session may be provided for selected papers or on request. Titles and abstracts for contributed papers should be sent to the Program Chairman: Adam Curry, adam.curry@psyleron.com. Electronic submission is required. The Title should be short and informative. Please include Author name and Affiliation, and contact information. Abstracts should be 300 to 500 words, and should summarize the main points of the paper. Plain text as the body of the e-mail is preferred. If special formatting is required for intelligibility, please submit a Word document. If selected for presentation, please plan on a 15-minute talk with 5 minutes for questions. Submissions by Associate members must be sponsored by Full members. (Full members do not require sponsorship.) The cutoff date for submissions is April 30, 2014. Submissions received subsequent to that date may be considered for presentation subject to the availability of time in the program.

FULL-DAY WORKSHOP

Using Randomness to Study Consciousness: REGs and RNGs in Scientific Research Applications—Presented by Dr. Roger Nelson

This is a workshop for people who have been intrigued but perhaps mystified by random number generator technology in consciousness research, or who want to dive in and do their own experiments. RNGs, also known as REGs, evidently can be affected by intention, and they are responsive to deep emotions and group coherence. We will work to reduce the mysteries and produce both scientific and practical clarity. It will be an opportunity to explore your questions in detail.

Dr. Roger Nelson was a member of the PEAR team from 1980 to 2002 when he retired from Princeton University. In 1997 he created the Global Consciousness Project, which he continues to direct. His experience with REG/RNG technology spans laboratory and field research, and his expertise reaches from design and calibration of the instruments to experimental and statistical design to analysis and interpretation of results. He is willing to step into the deep waters of explanatory theory, but with some necessary caveats.

The full day workshop will begin at 9:30 and end around 4:30, with two breaks. Please contact Dominque Surrel for questions about the workshop. Email: lumierebl@aol.com. Additional information on the conference will be announced via email, and provided on the website (www.scientificexploration.org). Registration and fees will be announced shortly, and available on the website.

TRANSPORTATION and HOTEL

Hotel Airport Shuttle: The Hyatt Regency San Francisco Airport complimentary shuttle is available every day, 24 hours a day and runs every 10 to 15 minutes. At San Francisco International Airport (SFO) take your luggage to the Departures Level, center island, and look for the area marked "Hotel Shuttle." The shuttle is a large bus marked "Hyatt Regency ."
Hotel Amenities: The hotel offers a fitness center, pool, and business center. Restaurants: Swiftwater Café, Swiftwater Deli, Knuckles Sports Bar, and Cascades Wine Bar. Parking: The SSE has arranged a special rate for overnight self-parking at the Hyatt of $10.00 overnight.
To Downtown San Francisco: BART (Bay Area Rapid Transit)—Take the complimentary Hyatt Shuttle Bus to and from SFO Airport International Terminal to connect with BART. Cost from the BART station to downtown San Francisco is approximately $8.10 one-way.
SFO Airporter Shuttle: Service is offered between SFO Int'l Airport and the Embarcadero or Union Square in San Francisco. Take the complimentary hotel Shuttle Bus to and from SFO Airport to connect with the Airporter. Airporter costs $10.00 one-way, $15.00 roundtrip.
To Downtown Burlingame Trolley Service: Daily free trolley service is available from Hyatt Regency San Francisco Airport to downtown Burlingame.

The 10th Annual European Conference of the Society for Scientific Exploration

November 13–15, 2014, Leiden, The Netherlands

Program Chair: C. M. Chantaral Toporow, cmc.toporow@mac.com
Local Arrangements Coordinator: Erik Schultes, schultes@hedgehogresearch.info

Meeting Venue: Leiden University Medical Center. All sessions will be held here.
Parking: The SSE has arranged a special rate for overnight self-parking at the LUMC garage, Albinusdreef 2, 2333 ZA Leiden. The organization will have a parking ticket available at the registration desk on site. You can park your car first and the leave the parking garage by using that specific ticket. For information and route description, go to:
http://www.lumc.nl/0000/12299/71127110446221/?setlanguage=English&setcountry=en

Hotel: Hotel Ibis, Stationsplein 240, 2312 AR LEIDEN, email H8087-RE@accor.com Telephone: 071 – 516 00 00. A large block of rooms has been reserved for the SSE at a special conference rate of € 84.00 including breakfast. Excl. € 2.00 tourist tax p/p p/night (€ 97.00 for a double room). Reservations must be made by 1 August 2014 to receive this rate. Please call directly to the hotel or send an email and mention this reference number—BOER121114—to reserve your rooms at the special rate. Hotel Ibis is a basic hotel within walking distance to both the Leiden University Medical Center where the conference will be held, and to the city center and all the museums. The Hotel has a nice lobby with a bar.

Transportation: There is a good direct train connection (20 minutes) between Schiphol Airport (Amsterdam) and Leiden Railway Station. For information about the arrival hall floor plan: www.schiphol.com
To & from Schiphol Airport and for the train schedule, go to www.ns.nl (English)

Registration: Please go to the following website to register:
https://www.boerhaavenascholing.nl/pages/Boerhaave/ShoppingCart?addactivity=30860&lang=en_uk

Students: The Society encourages the attendance of students and young investigators, and the conference is open to the public. There is a special, reduced registration fee for students at this event. Please encourage students from your area to come.

Welcome Reception: Wednesday, November 12th, starting at 6 p.m. at the hotel.
Field Trip: Leiden Boerhaave Science Museum, http://www.museumboerhaave.nl/english/
Dinner: The Dinner will be on November 14th at Koetshuis, in the Leiden city centre
http://www.koetshuisdeburcht.nl/

CALL FOR PAPERS

The overall theme of the conference is "Physical and Biological Correlates in Alternative Healing Modalities." Invited speakers will help define themes to be developed further by our own SSE membership, and the program will include papers assessing progress and social/political issues in areas of longstanding interest to SSE members. All conference sessions will be held in the Leiden University Medical Center (LUMC).

Theme 1: Life, Water, and Light

Theme 2: Physical and Biological Correlates to Alternative Healing Modalities

Theme 3: Our Universe: Exploring Concepts of Non-Locality

Evening Panel: November 13th

CONTRIBUTED PAPERS

Contributed Papers Contributed papers by full members on any topic of interest to the Society are welcome. Papers related to the themes of the conference will be grouped with relevant invited talks when possible. A poster session may be provided for selected papers or on request.

Titles and abstracts for contributed papers should be sent to the Program Chairman: Dr. C. M. Chantal Toporow (cmc.toporow@mac.com). Electronic submission is required. The Title should be short and informative. Please include Author name and Affiliation, and contact information. Abstracts should be 300 to 500 words (one page of single spaced text), and should summarize the main points of the paper. Plain text as the body of the e-mail is preferred. If special formatting is required for intelligibility, please submit a Word document. If selected for presentation, please plan on a 15-minute talk with 5 minutes for questions. Submissions by Associate members must be sponsored by Full members. (Full members do not require sponsorship.)

SUBMISSION DEADLINE: **September 1st, 2014.** Submissions received subsequent to that date may be considered for presentation if the program allows extra time slots.

JOURNAL OF SCIENTIFIC EXPLORATION
A Publication of the Society for Scientific Exploration

Instructions to Authors
(Revised February 2013)

All correspondence and submissions should be directed to:

JSE Managing Editor, EricksonEditorial@gmail.com, 151 Petaluma Blvd. So., #301, Petaluma CA 94952 USA, (1) 415/435-1604, *fax* (1) 707/559-5030

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AIMS AND SCOPE: The *Journal of Scientific Exploration* publishes material consistent with the Society's mission: to provide a professional forum for critical discussion of topics that are for various reasons ignored or studied inadequately within mainstream science, and to promote improved understanding of social and intellectual factors that limit the scope of scientific inquiry. Topics of interest cover a wide spectrum, ranging from apparent anomalies in well-established disciplines to paradoxical phenomena that seem to belong to no established discipline, as well as philosophical issues about the connections among disciplines. The *Journal* publishes research articles, review articles, essays, book reviews, and letters or commentaries pertaining to previously published material.

REFEREEING: Manuscripts will be sent to one or more referees at the discretion of the Editor-in-Chief. Reviewers are given the option of providing an anonymous report or a signed report.

In established disciplines, concordance with accepted disciplinary paradigms is the chief guide in evaluating material for scholarly publication. On many of the matters of interest to the Society for Scientific Exploration, however, consensus does not prevail. Therefore the *Journal of Scientific Exploration* necessarily publishes claimed observations and proffered explanations that will seem more speculative or less plausible than those appearing in some mainstream disciplinary journals. Nevertheless, those observations and explanations must conform to rigorous standards of observational techniques and logical argument.

If publication is deemed warranted but there remain points of disagreement between authors and referee(s), the reviewer(s) may be given the option of having their opinion(s) published along with the article, subject to the Editor-in-Chief's judgment as to length, wording, and the like. The publication of such critical reviews is intended to encourage debate and discussion of controversial issues, since such debate and discussion offer the only path toward eventual resolution and consensus.

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