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EDITORIAL

In addition to the usual array of interesting papers and reviews, this issue of the *JSE* features a debate that I consider especially noteworthy. The topic of the debate is hypnosis and the participants in the dialogue are all recognized authorities on the subject. However, the backgrounds and perspectives of the participants are also quite different, and so the discussion of the issues is commendably broad and wide-ranging.

I've often wondered whether *JSE* readers noticed and were puzzled by the fact that hypnosis has received little (if any) attention in the pages of this *Journal*. It has certainly puzzled me. Granted, unlike some of the phenomena (or alleged phenomena) discussed in the *JSE*, the existence of hypnosis is not generally disputed. However, the process and nature of hypnosis, and the implications of hypnotic phenomena for our understanding of the mind, remain acknowledged mysteries. To be sure, a small number of researchers cling obdurately to the belief (associated perhaps most often with Nicholas Spanos) that hypnosis is nothing but social compliance or role-playing designed to please the hypnotist.[1] But the transparent absurdity of that position becomes clear as soon as one considers some of the more dramatic hypnotic phenomena—for example, failing to register pain during major surgery (e.g., limb amputation, the removal of 100-lb scrotal tumors [yes, that's right], and the removal of toenails by the roots), and also the prevention of well-known involuntary responses to other noxious stimuli, such as ammonia placed under the nose and needles inserted in the mucous membranes of the eyes. Clearly, the subjects in these cases aren't simply complying with the wishes of the surgeon by (say) feigning a lack of pain. These are *paradigm* cases of genuine and profound—and poorly understood—altered states, and they're quite different from the non-reactions to relatively mild pain (e.g., hands in ice water) considered by Spanos.

Readers unfamiliar with this literature might be amused (or disheartened) to learn that Spanos and others defend their role-playing view of hypnosis by adopting the straw-man reasoning all too familiar from the skeptical literature in parapsychology—namely, generalizing from the weakest cases. Spanos's tactic was to focus on experiments which *at best* would illustrate only very modest or relatively uninteresting forms of hypnosis—that is, which even those sympathetic to hypnosis would regard as marginal and relatively easy to simulate unconsciously. Spanos systematically (and I'd say, knowingly) either ignored or passed quickly over the phenomena he

should have highlighted, the dramatic phenomena of hypnosis (e.g., hypnotic anesthesia of the sort reported by Esdaile and others).[2] These are precisely the cases most difficult to explain away as forms of social compliance.

Perhaps the mystery of hypnosis is undervalued because, unlike (say) ESP or PK, it's easily replicable and conspicuously useful. For example, one well-known study reports the effective use of hypnosis to prevent bleeding in a large number of surgical dental procedures on hemophiliac patients (Lucas & Tocantins 1964). And that's not an isolated report.[3] But how is this physiological control possible? No one has a clue, any more than we understand the details of placebo and other psychosomatic effects. Actually, for those topics there is a substantial literature (although it's not terribly strong on theory),[4] and so you'd think the scientific community would devote at least as much effort to unraveling—and not simply documenting—the puzzling mind–body connections demonstrated in hypnosis. But you won't find much serious or sustained attention either to theory or to the most challenging hypnotic phenomena in journals devoted broadly to the study of consciousness, and there's not even much in hypnosis journals or books on hypnosis. One exception, a book only modestly interesting theoretically, is Rossi and Cheek (1988); and see Barušs (2003) for a good summary of recent research and theory.

Sadly, the neglect of major puzzles concerning hypnosis is nothing new; in fact (as I'll explain below), the scientific community has a history of dropping the ball when it comes to some hypnotic mysteries. But why should these mysteries be discussed so infrequently in the pages of the *JSE*? In the early days of the Society for Psychical Research (SPR) at the end of the nineteenth century, hypnosis received penetrating and scholarly treatment in many issues of the Society's *Proceedings* and *Journal*. In those days, many believed that hypnosis was not only intrinsically interesting and poorly understood, but that along with other dissociative phenomena (such as divided consciousness) it promised great insights into the workings of the mind. These days, however, one almost never sees mention of hypnosis in parapsychological journals. It's now mostly in the hands of specialists, and accorded scholarly treatment primarily for its practical (e.g., clinical, medical, psychiatric, or forensic) consequences.

That's fine, as far as it goes. But consider this. In its early publications, the SPR frequently examined ways in which hypnosis (mesmerism) linked to various ostensible psi phenomena such as community of sensation, clairvoyance (including diagnosis of disease), and thought-transference (including willing at a distance). Many of the articles make for rewarding reading even today—perhaps especially the papers by F. W. H. Myers and Edmund Gurney.[5] One paper of particular importance is Myers's

1886 report on hypnosis at a distance (Myers 1886). It marks a stage in a strange and protracted history of noticing, and then neglecting, one of the potentially most disturbing hypnotic phenomena—disturbing because of its combination of metaphysical and ethical implications.

Hypnosis at a distance (or telepathic hypnosis) had been noticed from the beginning—by Mesmer himself. But since so much of what Mesmer and his followers were doing was strange, telepathic hypnosis didn't stand out at the time as being particularly noteworthy. Later, in the early nineteenth century, Mesmer's disciple Puységur wrote that suggestion at a distance was "magnetism's" most characteristic feature. But that was countered by a report from the French Academy of Sciences claiming that animal magnetism was nothing more than manipulation of the imagination in unbalanced people.

After several more unsuccessful attempts during the mid nineteenth century to attract sustained attention to *le sommeil à distance*, several prominent investigators—including Pierre Janet and Charles Richet—began studying a subject, Léonie, a simple peasant woman who, according to a Doctor Gibert of Le Havre, would fall asleep merely from his mental suggestion for her to do so. Janet conducted a series of successful experiments with Léonie, including having her carry out post-hypnotic commands given mentally. Richet later duplicated these results with Léonie and three other subjects.[6]

But then this research screeched to a halt. Instead of acknowledging and following up on the potential significance of what they had observed, Janet and Richet each went back to less interesting and momentous areas of research. And no one else at the time picked up where they left off. It was as if the possibility of influencing others at a distance was simply too disturbing to pursue.

Later, in the 1930s in Soviet Russia, the physiologist L. L. Vasiliev successfully demonstrated hypnotic induction at a distance in some clever experiments (Vasiliev 1976). Apparently, this work had been done furtively during the Stalin era, and so nobody outside Russia learned of it until the 1960s. But again, it seems as if researchers ran away from the implications of influencing people at a distance. No more work on the subject followed, although some efforts were made to influence lower organisms including insects, rats (of course), and fungi. For more detailed accounts of this peculiar history, I strongly recommend Jule Eisenbud's essay, "How to Influence Practically Anybody (but Fellow Scientists) Extrasensorially at a Distance" (Eisenbud 1992), and also Alan Gauld's monumental history of hypnosis (1992).[7]

You won't find telepathic hypnosis covered in the debate featured in

this issue of the *JSE*. But there's plenty of other serious work still to be done on the subject of hypnosis specifically and altered states generally, and this issue's dialogue takes a step in the right direction by addressing some basic conceptual and empirical matters. Note, by the way, that the current issue also features an interesting paper on drug-induced hallucinations and telepathy. As far as I'm concerned, the general topic of altered states likewise could be featured more regularly in the *JSE*, although as the recent Cardeña and Winkelman volume seems to indicate, that's an area of research receiving the sustained attention it deserves. Regrettably, no more papers either on hypnosis or altered states are currently in our editorial pipeline. But I hope that the spasm of attention to those topics in this issue is itself not an anomaly. We still have a long way to go before we can claim to grasp the significance of hypnosis and other altered states for our understanding of the workings of Nature generally, and ourselves in particular.

Notes

[1] See, e.g., Spanos (1983), Spanos, Weekes, and Bertrand (1985), Spanos and Chaves (1989), Spanos and Hewitt (1980).

[2] See, e.g., Esdaile (1846, 1852), Elliotson (1843).

[3] See also Swirsky-Sacchetti and Margolis (1986), LaBaw (1992), Lebaron and Zeltzer (1984), Lucas (1975), Fredericks (1967), Fung and Lazar (1983), Dubin and Shapiro (1974), Newman (1971, 1974).

[4] But see White, Tursky, and Schwartz (1985), and another valuable new addition to the literature is Cardeña and Winkelman (2011).

[5] For a representative sampling, see, e.g., Gurney (1884a, 1884b, 1884c, 1887a, 1887b, 1888a, 1888b), Gurney and Myers (1885), and Myers (1885, 1888).

[6] Janet (1885, 1886), Richet (1885, 1888). For an interesting and detailed summary of the case of Léonie, see Dingwall (1967(1):264ff).

[7] Especially pp. 466–467 in Gauld (1992).

Stephen E. Braude

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RESEARCH ARTICLE

A PK Experiment with Zebra Finches and a Virtual Predator

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Abstract—The hypothesis that PK is a biological function which can be used by prey animals to their own benefit was tested using 25 zebra finches. To test whether birds can manipulate the randomness of REG devices in a virtual predatory context, experimental and control trials were run. During the former, one subject was placed in the testing cage and could see a 25-frames-per-second, 150-second-duration video clip of a crawling predatory snake in a TFT-LCD screen. Immediately before each image, a REG device would randomly increase or decrease by one point the opacity of the subsequent image, which, as a consequence, would become more solid or more transparent. During the control test no subject was present in the testing cage. When the REG outcome was analyzed, the total number of events of image fading was found to be significantly higher for the experimental condition than for the control condition, and during the experimental condition the number of image fadings was also significantly higher than that of opacity increases (and non-significant during the control condition). Therefore, the birds might be able to influence the REG production to reduce the presence of the predator (i.e. the proposed hypothesis appears to be supported).

Keywords: birds—PK—predation—zebra finches

Introduction

The literature is abundant with reports evidencing the ability of directional psychokinesis (PK), in which deviations in random systems are produced while the actors are specifically intending to influence the situation (Chauvin & Genthon 1965, Schmidt 1971, 1973, Jahn, Dunne, & Nelson 1987, Dunne & Jahn 1992, 1995). If directional PK is of the same or similar nature as non-directional PK (subject not intending any effect), the actors' emotional state might be crucial in both cases (Bierman 1996, Lumsden-Cook 2005a, 2005b, Mason, Patterson, & Radin 2007).

There are few animal studies with sufficient number of subjects in which the PK effect may be considered as directional, involving a benefit to the potential animal PK agent. In a preliminary work with REGs using cockroaches as animal subjects, Schmidt (1970) demonstrated an effect, albeit in the opposite direction than the subjects' welfare, which was attributed to experimenter effect. PK by three species of aggressive tropical fish was also suggested by the results of Braud (1976) on the non-random display of a positive reinforcement (a mirror presentation) controlled by an REG.

Also, as a continuation of previous work on mice's psi ability to avoid electric shocks (Duval & Montredon 1968), Chauvin (1986) demonstrated that the animals were able to drive a randomly moving object (the tychoscope) whose source of randomness was electronic noise (Janin 1986) away from them, apparently by the action of PK. Using Janin's tychoscope, Peoc'h (1988), and later on Peoc'h (1995) with an improved version of it, examined the influence of young chickens on the movement of a small self-propelled robot, on which the animals had developed a maternal bond through imprinting. The result of the object moving significantly more often toward the chicks suggests the action of PK on the birds' part. The attempt by Johnson (1989) to replicate these findings obtained negative results, although in this case the imprinting object was different and the REG's effect was for the object to be presented or not to the birds, while the results of Green and Thorpe (1993), in which the chicks were imprinted on a flashing light or another chick, were inconsistent and pointed more to an experimenter effect.

If an animal would increase its life expectancy by way of PK, and if this ability would depend to some extent on genetic inheritance, its overall genetic success would also be improved. As a consequence, we should expect this ability to be widespread in nature. Levin (1996) has called attention to the fact that observation of animals in action does not support such expectancy and has reviewed the various explanations of it. While most of these explanations refer to PK as a biological function acting for the benefit of the agent, Levin (1996) has opted for considering psi (and hence PK) a function of non-material consciousness, and therefore not entirely subject to encoding by genes, and therefore to its selection.

The view that PK has biological functions (Stanford 1974) gives way to the idea that it is a product of Darwinian evolution and can be modelled by natural selection (Broughton 1988, Taylor 2003). Under this most parsimonious hypothesis with regard to strictly naturalistic grounds, since much evidence points to gene frequency affecting normal psychological traits, if PK is considered just another biological adaptation for survival, its effects can be analyzed in contexts of predation.

In a predatory situation a potential prey individual gifted with PK could be able to manipulate the predatory context to its own benefit, but so would the potential predator. According to evolutionary theory, contrasting selection pressures could possibly arise, leading to an evolutionary arms race between the two species (Dawkins & Krebs 1979, Vermeij 1987), the prey trying to evade capture and the predator attempting hunting and killing.

Under the view that PK is a biological function that can be used by living beings in natural circumstances, my hypothesis is that a potential prey individual can influence a predatory context to its own benefit by way of PK. In order to detect this effect with no interference by the potential predator, in the present study I will explore the possibility that, in a laboratory setting, a subject prey species (namely the zebra finch, *Taeniopygia guttata*) is capable of affecting the random presence of a virtual predator in the form of video clip images of a predatory snake. A previous experiment trying to detect non-directional REG randomness departure in the same bird species in an alarm situation produced no significant results (Alvarez 2011), although a psi observer effect was suggested.

Methods

The present experiment was designed to test whether the finches are able to manipulate the randomness of an REG device controlling the presence of a virtual predator (by way of its image in a video clip becoming more solid or rather fading away, thereby also virtually reducing the threat to the birds). If, when the birds are present, the images become consistently more transparent, we can conclude that the hypothesis of a PK effect caused by the birds is supported.

All subjects (25 adult female zebra finches) lived in two unisexual adult groups of conspecifics in a 3 × 3 × 2 meters aviary near Seville, Spain. The 70 × 35 × 35 centimeters testing cage was provided with four perches and a thin-film transistor LCD screen (TFT) at one end, where the stimulus in the form of a video clip was to be presented. During the sessions an Orion REG device was permanently in position on the center of the cage floor and, connected to a laptop computer in an adjacent compartment, controlled the presentation of the stimulus in the TFT screen.

The use of TFT screens to present video playback has been successful in eliciting behavioral responses in zebra finches and other estrildines (Ikebuchi & Okanoya 1999, Galloch & Bischof 2006, 2007, Alvarez 2010, 2011).

Tests were carried out under experimental and control conditions. During the trials of the experimental condition, each of the 25 birds received the test individually while the experimenter was 10 to 25 meters away from

the experimental zone (Alvarez 2011). The area with vegetation outside the testing area could be seen by the bird inside the testing cage, since being visually enclosed appears to affect the spontaneous behavior of zebra finches toward predators (Lombardi & Curio 1985). The experiments were performed between February 23 and April 18, 2011. Starting between 9:00 and 11:30 UT, the subject to be tested was taken from its group and put in the testing cage, where from the beginning it would encounter a still image of red tiles on the TFT screen. After an accustoming period of 15 minutes, 10 equal video clip segments of 15 seconds duration each (at 25 frames per second) of a predatory whip horseshoe snake (*Coluber hippocrepis*) slowly crawling from the right to the left side of the screen over the red tiles background were presented uninterruptedly in the TFT screen. Following the end of stimulus presentation, the test ended after another 15-minute period of just the still background of red tiles on the screen (Figure 1).

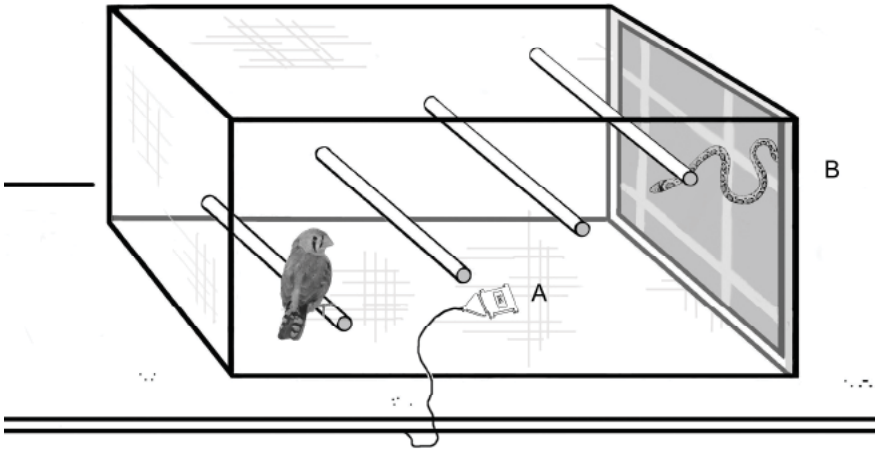

Figure 1. Testing situation. (A) REG. (B) TFT screen.

The opacity of the image of the crawling snake could vary from fully opaque (a score of 50) to completely invisible (a score of 1), and the operation of the REG on the floor of the testing cage was to randomly increase or reduce by one point the opacity of the snake image (not of the background) of each of the 25-per-second images of the video clip. The "decision" by the REG for each image to become more solid or more transparent was taken immediately before each frame was displayed. When entering the screen for the first time, the snake image was almost fully opaque (score

40, apparently fully solid to the human eye). The series of events of opacity increase and decrease was continuous along the 150 seconds of stimulation, i.e. the initiation of each of the 10 segments of the video clip, as the snake image entered from the right side of the screen, inherited the opacity of the last image of the previous segment, when the snake had ended its movement toward the left side of the screen. The background of red tiles was completely opaque throughout the 30 minutes of the experiment.

The 25 control tests were carried out following the same procedure, although in this case the testing cage was empty (no bird).

Statistical Analysis

The individual opacity score of each of the 3,750 images (the 1 to 50 scale of how solid images are) along the 150 seconds of stimulation of the experimental and the control tests, and the events of increase or reduction of the opacity of each of the images (i.e., respectively, of becoming more solid or more transparent) were registered and used for the analysis.

The nonparametric Mann-Whitney U test was used to compare the individual number of events of image fading between the experimental and the control trials, and the Wilcoxon matched pairs test was used to compare the individual number of events of images becoming more solid and of events becoming more transparent in both conditions of the experiment.

The Spearman's rank correlation test was used to compare, out of the ten segments of stimulus presentation, the mean video clip segment image opacity (i.e. how solid images are) to the mean number of events of opacity decrease (i.e. how often images faded away) per video clip segment, in both conditions, when the data from all subjects were pooled.

Linear and polynomial regressions were used to fit a model to the time series of events of opacity decrease along the ten video clip segments.

Results

The total number of events of opacity decrease obtained in the experimental condition was found to be significantly higher than that for the control condition (experimental: 51.11 percent, control: 50.09 percent; U = 191.5, N1 = 25, N2 = 25, $p = 0.019$, Mann-Whitney U test) (Figure 2), that is the snake images faded away more often when the birds were near and watching the video clip than when the testing cage was empty.

The comparison of the total number of events of opacity increase (images becoming more solid) and decrease (images fading away) for each of the 25 subjects of the experimental condition showed that the number of decreases significantly surpassed that of increases (ratio decreases–increases

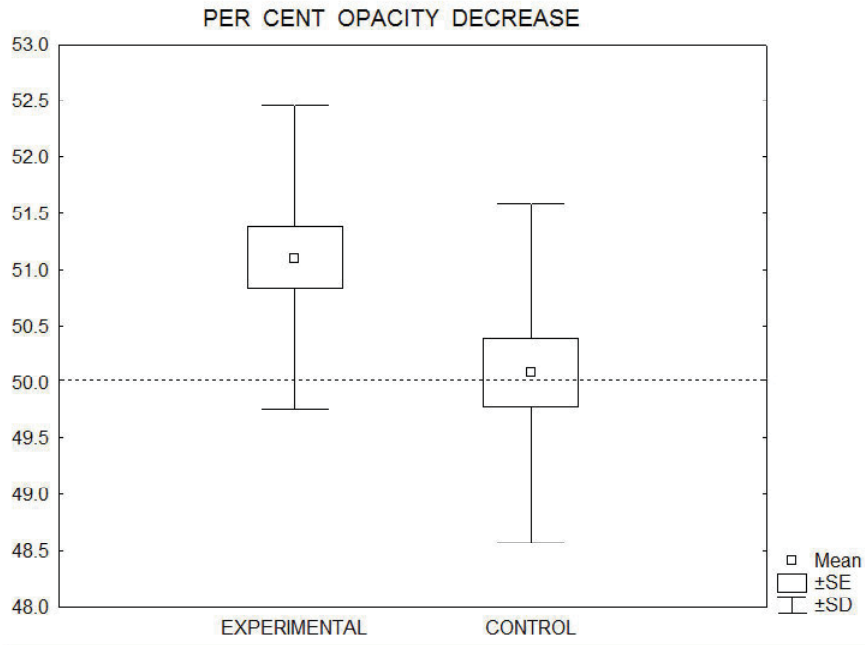

Figure 2. Percent of decreases of the opacity of all video clip images of the snake predator (i.e. images fading away) during the experimental condition (when presented to the zebra finches) and control condition (when no subject was present in the testing cage).
The 50 percent random value is indicated by the dotted line.

of 1.1:1.0) (N = 25, T = 40, z = 3.30, p = 0.001, Wilcoxon matched pairs test), while no significant difference was obtained for the control condition (ratio of 1.0:1.0; N = 25, T = 142, z = 0.55, p = 0.581, Wilcoxon matched pairs test).

The analysis of correlation between mean image opacity scores (i.e. how solid the images were) during each of the ten 15-second video clip segments and the mean number of events of opacity decrease (i.e. how often images faded away) during the same segments resulted in a significant negative correlation during the experimental condition (N = 10, R = −0.721, p = 0.019), and non-significant for the control condition (N = 10, R = 0.091, p = 0.803, Spearman's rank correlation test).

Considering the time series of opacity decreases, that is the relationship between the order of appearance of the video clip segments in the screen and the percentage of events of images fading away during the presentation of each segment, the linear regression model does not adequately describe the pattern of the scatter plot, neither for the experimental nor for the control

conditions (r = 175, N = 10, p = 0.629; r = −0.219, N = 10, p = 0.544; respectively). On the other hand, the quadratic regression model shows a better and significant fit for the experimental condition, and non-significant for the control condition (R^2 = 0.672, N = 10, p = 0.008; R^2 = −0.222, N = 10, p = 0.837; respectively).

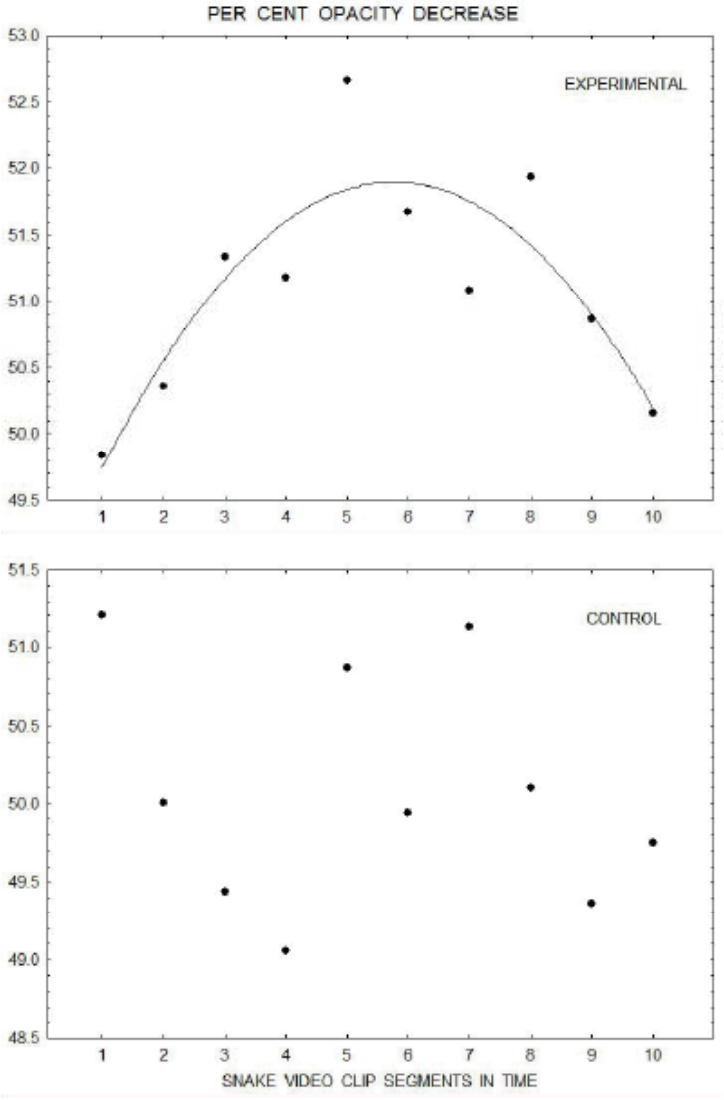

Figure 3. Time series of percent of events of the snake images fading away during the experimental and control conditions, and trend curve for the former.

The equation describing the pattern for the experimental condition is:

$$Y = 48.7532 + 1.0909\ X - 0.0947\ X^2$$

where Y is the percentage of events of image fading and X the order of appearance of the video clip segments. Corresponding to this model, the trend curve that best fits the data for the experimental condition is a frown parabola (Figure 3). As can be seen in Table 1, the ratio of total number of events of the images fading away to that of images becoming more solid rose from the lowest level at segment 1, to a peak at segment 5, and then went down.

TABLE 1

Experimental Condition: Order of Video Clip Segments, Ratio of Opacity Decreases/Increases (Events of Images Fading / Events of Images Becoming More Solid), and Comparison between Number of Decreases and Increases (Wilcoxon Matched Pairs Test)

Order of Video Clip Segments	Ratio: Decreases/ Increases	Comparison Decreases and Increases			
		N	T	z	*p*
1	**0.99**	**25**	**161**	**0.04**	**0.993**
2	**1.05**	**25**	**130**	**0.88**	**0.382**
3	**1.06**	**25**	**110.5**	**1.40**	**0.162**
4	**1.05**	**25**	**123**	**1.06**	**0.288**
5	**1.11**	**25**	**57.5**	**2.83**	**0.005**
6	**1.07**	**25**	**111**	**1.39**	**0.166**
7	**1.04**	**25**	**107.5**	**1.48**	**0.139**
8	**1.08**	**25**	**77**	**2.30**	**0.021**
9	**1.04**	**25**	**126**	**0.98**	**0.326**
10	**1.01**	**25**	**155.5**	**0.19**	**0.851**

Discussion

The significantly higher number of events of the predator images fading away in the experimental condition, as compared to the control condition, together also with the significantly higher number of events of image fading, as compared to that of images becoming more solid in the experimental

condition, and its lack of significance in the control condition, support the hypothesis that PK is taking place, and that in nature this effect could act for the benefit of the PK agent.

These results are comparable to those obtained with other bird species, namely young chickens, apparently affecting the random movement of a self-propelled object on which they had been imprinted, in the sense that PK ability would also work to the benefit of the potential agent (Peoc'h 1988, 1995). Then, with respect to the context in which the experiments were carried out, our finches' apparent capacity to make the virtual predator (totally or partially) disappear resembles more that of Chauvin's (1986) mice, which were able to keep a disturbing randomly moving object away from them, apparently by PK.

If our finches were actually able to reduce the risk of being captured, the possibilities of manipulating the predatory situation are immense, and among them the possibility of communication from the prey bird species to its potential predator (Alvarez 1993, Alvarez, Sánchez, & Angulo 2006) should not be discarded. In fact, if our finches were able to influence the REG outcome, their PK ability could as well reach any aspect of the predator's nervous system or behavior, or any component of the environment. However, since not being hunted in the case of the prey is equally important as the need to obtain food for the potential predator, there is no reason to suppose that only prey species would be endowed with PK. If certain predator and prey species would depend much on each other for survival, an evolutionary arms race would possibly arise (Dawkins & Krebs 1979, Vermeij 1987), perhaps making the PK effects less visible to outside observers.

As to the found negative correlation of the opacity of the snake image (or how solid images were) with the frequency of events of images fading away, at first sight it would be expected that the more often images fade, the more transparent they become. However, the fact that no correlation was found between those two variables during the control condition, when no bird subject was present in the testing cage, suggests that at least in part the birds were responsible for that negative correlation during the experimental condition, acting in a feedback system, so that the less solid the presence of the predator, the stronger the PK effort to make it disappear. The high influence of feedback favoring the expression of PK by humans was demonstrated by Heseltine (1977).

The pattern along the ten segments of the experimental condition of the percent of images fading away, in which values progressively go up, to reach a maximum one minute after the first presentation of the stimulus, and then descend, is best described by a frown parabola (see Figure 3), and could be regarded as a case of decline effect. Similar phenomena appear to

be common in human psychic performance (Colborn 2004), and occurred in Braud's (1976) PK experiment with aggressive tropical fish, where the PK scores went down progressively from the high level in the first to the low level in the last experimental series.

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RESEARCH ARTICLE

Revisiting the Alexander UFO Religious Crisis Survey (AUFORCS): Is There Really a Crisis?

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Abstract—This paper explores the tacit presumption that U.S. government disclosure of information regarding prior contact with extraterrestrials would precipitate a religious crisis (presuming that there is information to disclose). This issue has remained controversial since the earliest ufological writing, both government and academic, yet only minimal empirical evidence has been forthcoming. The present analysis is based on data collected as a part of the Alexander UFO Religious Crisis Survey (AUFORCS), a private study of Protestant, Roman Catholic, and Jewish clergy (N = 229) conducted in 1994 whose raw data only recently have been made public (to the author of this paper). The AUFORCS consisted of 11 statements about extraterrestrial contact, alien life, and a putative impact on religion, scaled on a five-point Likert metric requiring respondents to affirm their agreement or disagreement with each item. Findings from the AUFORCS data confirm that disclosure would not precipitate much of a religious crisis. Nor do there appear to be substantive differences in how leaders of respective religious traditions would react to such disclosure. The desirability of replicating this study through a large-scale national probability survey of the U.S. adult population is discussed.

Keywords: ufology—extraterrestrials—religion—surveys

Introduction

The possibility of extraterrestrial contact is among the most contentious, and at times lurid, subjects that can be broached among academic scientists. It represents the most marginal corner of ufology, itself a marginal field of inquiry for academics. A notable challenge for physical scientists and engineers with interests here is to identify welcoming peer-reviewed outlets for their scholarly research and writing. These are few and far between, as

we all know, but thankfully *JSE* is a notable exception, and it has published serious theoretical and conceptual analyses and reviews of UFO-related themes for two decades (e.g., Swords 2006).

For social scientists, there is less stigma attached to engaging this subject, in that it may be broached descriptively without sounding any alarms, such as to characterize the beliefs or attitudes of professed contactees or to investigate potential correlates or predictors. One significant issue has intrigued and troubled government and academic investigators since the earliest decades of study in ufology: the potential for alien contact—and government disclosure of information about alien contact—to destroy the foundations of institutional religion and thus lead to widespread panic, perhaps even undermining government authority or throwing civilization into chaos. This perception has been widespread and is nicely described by Alexander (2011) in his recent book on UFOs:

> The potential impact that the confirmation of extraterrestrials would have on religion has been raised in several articles. There are those who believe that ETs would be incompatible with Christianity and feel a revelation would threaten the foundations of the church. (Alexander 2011:241–242)

That this scenario would indeed come to pass is tacit to many experts and has long been treated as inevitable. At the very least, it is presumed, religious beliefs and attitudes would condition how such disclosure would be interpreted and experienced by people, regardless of whether they themselves would be threatened by such information. This presumption has been described in the ufological literature (e.g., Boeche 1988, Downing 1988), while noting an absence of conclusive empirical evidence. The authors of the well-known Brookings Report, published half a century ago, captured these sentiments in a subsection of their report entitled, "The implications of a discovery of extraterrestrial life":

> An individual's reactions to such a . . . contact would in part depend on his cultural, religious, and social background, as well as on the actions of those he considered authorities and leaders, and their behavior in turn would in part depend on their cultural, social, and religious environment. (Michael 1961:215)

This is an empirical question and, as such, can be tested. Interestingly, also in the Brookings Report, hidden away in a footnote, the authors added:

> It has been speculated that, of all groups, scientists and engineers might be the most devastated by the discovery of relatively superior creatures, since

> these professions are most clearly associated with the mastery of nature, rather than with the understanding and expression of man. Advanced understanding of nature might vitiate all our theories at the very least, if not also require a culture and perhaps a brain inaccessible to earth scientists. (Michael 1961:225)

This comment raises the possibility that concern over the fragility of the psyches of the American people due to religious beliefs, on the part of scientists and government officials, may be overstated. It may be scientists and government officials whose intellectual presumptions and temporal authority would stand the most to lose by disclosure of otherworldly intelligences superior to those of earthlings. But, again, the influence of disclosure on religious beliefs and attitudes, as much as the influence of such indicators on reactions to disclosure, is a matter that can be examined empirically, provided the right data are available.

While in-depth analysis of this issue has yet to occur, some documentation of public opinion, and on a national scale, has been forthcoming over the past two decades. An *ABC News* / *Washington Post* Poll, conducted in May 1994, identified a lifetime prevalence of having "personally ever been in contact with aliens" of only 0.5% (*ABC News* / *Washington Post* 1994). But other survey items showed that sensitivity to and belief in these issues may be quite higher. Questions regarding having "ever seen anything that you believe was a spacecraft from another planet" (9.8% lifetime prevalence) and affirming that UFOs are "something real [and not] just people's imagination" (57.9%) and that other folks' reports of contact involve "actual spacecraft from other planets" (40.8%) together provide a better indicator of personal beliefs than personal reports of contact. In other words, Americans are saying, this may not have happened to me, but I believe it to be true. More significantly, from the perspective of this present paper, follow-up revealed no differences in such beliefs between respondents who do or do not report being "a religious or spiritual person."

The more recent National Firearms Survey, from 1999, reused the *ABC News* / *Washington Post* item on lifetime prevalence of contact with aliens, getting a response of 0.3% (Hemenway 1999), very close to the prior finding. Analyzing responses from the cumulative file of the National Science Foundation's Survey of Public Attitudes, undertaken from 1979 to 2001, found that 10% of respondents affirmed that the statement that UFOs are "really space vehicles from other civilizations" is true (Miller, Kimmel, & ORC Macro 2004). Neither of these surveys asked questions that would enable a look at how religious identity, belief, or practice may or may not moderate or condition these responses. A national Roper Poll, conducted in 2002 for the Sci Fi Channel (subsequently renamed Syfy), asked whether

government disclosure of intelligent extraterrestrial life would precipitate a religious crisis. Fully 88% of respondents reported that such disclosure would have no impact on their religious beliefs, with higher numbers in successively older age cohorts, but still 80% said no in the youngest cohort (RoperASW 2002). An earlier Roper Poll, conducted in 1999, had found that three-quarters of respondents did not believe that Americans would panic (NIDS 1999), so perhaps the potential for a true existential crisis, apparently minimal as it may be, continues to wane.

The earliest formal effort to explore this issue was in the Alexander UFO Religious Crisis Survey (AUFORCS), conducted in 1994 (Alexander 1994). This survey was notable for several reasons: (a) it focused on the responses of a sample of U.S. clergy (Protestant ministers, Roman Catholic priests, and Jewish rabbis); (b) it sought agreement or disagreement with a comprehensive series of questions regarding potential government disclosure of UFO and alien-contact–related information (presuming such information exists); and (c) it was directed by Victoria Alexander, wife of retired Army Colonel Dr. John Alexander, member of the intergovernmental Advanced Theoretical Physics working group and a veteran of the U.S. Army Intelligence and Security Command and Los Alamos National Laboratory's nonlethal weapons program (see Alexander 2011). He is also an individual long subject to Internet-fueled rumors that he is the leader of the "aviary," an ostensibly secret government cabal tasked with covering up the truth about alien contact (e.g., Blum 1990). To be clear, the AUFORCS project was the brainchild of Mrs. Alexander and fully under her direction, not Dr. Alexander's. The project was underwritten by funding from the Bigelow Foundation.

The mission of the AUFORCS was simply to seek an answer to a question that has concerned government officials, if such stories are true, since the 1950s: Would disclosure of U.S. government contact with aliens really precipitate a religious crisis that would threaten continuity of government and even our civilization? To this end, Alexander conducted a mail survey of Protestant, Catholic, and Jewish clergy (additional details in the Method section, below), seeking their informed opinions about this issue. At the end of the study, a few descriptive results were tabulated and a report was issued that was circulated among people with interest in this subject, including selected members of the federal government, the military, the ufology community, academic researchers (including members of SSE), and, inevitably, the legions of conspiracy theorists populating cyberspace.

There the report has remained, over the years taking on a sort of mythic or legendary quality, but the data points, modest as they are, were yet to be subjected to a full empirical analysis. Only recently has the AUFORCS

data archive been made public—in the sense of being made available to an outside researcher. Mrs. Alexander graciously provided the present author with all of the raw surveys and all of the associated background material, preliminary analyses, correspondence, media reports, and so on—a complete file-dump essentially (a large box with these contents was shipped to his university office)—and he is now in possession of all materials related to the AUFORCS. While the survey data themselves are now more than a decade and a half old, and notwithstanding methodological limitations of the survey (to be discussed later), the historical value of the survey coupled with the opportunity, finally, to analyze these data in depth have encouraged the preparation of a brief empirical report for *JSE*, the most appropriate audience for these findings. This also provides an opportunity to broach the fascinating religious implications of this general issue, something that up until now has been subject to conjecture—and worry—but not yet comprehensively explored.

This present analysis was also informed by the similarly named Peters ETI Religious Crisis Survey, inspired by the AUFORCS and conducted a few years later (Peters & Froehlig no date). This was a larger survey, encompassing a wider range of religious affiliations, and focusing on adult respondents, not just clergy, and it also investigated a wider range of topics in astrobiology. A few of its results, which like the AUFORCS were released in a final report, suggested little reason to foresee a religious crisis of any serious magnitude. Moreover, the Peters report found that if there was any expectation of a religious crisis, it was on the part of respondents who self-identified as non-religious. Of these, 69% affirmed that "contact with extraterrestrials would so undercut traditional beliefs that the world's religions would face a crisis" (Peters & Froehlig no date:12). Yet despite this certainty among non-believers, when respondents who actually reported affiliation with a religion were posed the same question, only 34% agreed. In other words, according to the report, "it appears that people who embrace a traditional religious belief system do not fear for their own personal belief; nor are they particularly worried about their own respective religious tradition" (p. 13). Further, "[n]on-religious people seem to know too little about religious people, because they are mistaken in their assessment of the fragility of religious beliefs" (p. 13).

The AUFORCS, unfortunately, does not include a non-religious category—this is a survey of clergy, after all—so this particular issue cannot be followed up here. But it is raised in order to document the seriousness, timeliness, and contentiousness of the larger matter of UFOs, contactees, and religion, especially the possibility that it continues to influence any government or military calculus regarding potential disclosure (presuming, as noted, that there is actually something to disclose).

Method

Sample

The AUFORCS was a pilot survey of clergy randomly selected from three large religious bodies in the continental U.S. Names and addresses of potential respondents were obtained from Data Base American Companies' PhoneDisc Reverse Fall 1993 database. In March, 1994, questionnaires were mailed to a sample of 1,000 congregations: 563 Protestant churches, 396 Roman Catholic churches, and 41 synagogues. A total of 45 surveys were returned because of an incorrect address. By the end of April, 1994, 230 surveys had been returned (229 of these were used in the present analyses; one was excluded during data entry and cleaning for reasons related to some technical ambiguities). The present sample contains questionnaires from 133 Protestant ministers, 86 Roman Catholic priests, and 10 Jewish rabbis. The overall response rate of 24% was low, as national probability surveys go, and it is unknown whether nonresponse was random or systematic. Thus, no claim is made for overall sample representativeness of the general clergy population of the U.S. But, to be fair, this is offset by the importance of this survey—at the time, a first-ever look at a social issue of critical importance to government and military leaders—and by the historical significance of these data.

As in all large-scale social surveys, there were missing data for respective questions. In the analyses that follow, the available sample size ranged from 196 to 204, depending upon the variable(s) in question. What was distinctly unusual about this particular survey was that a subset of completely blank questionnaires was returned to the investigator, and not simply discarded. Typically, these blank returns had lengthy comments, sometimes more like sermonettes, written across the pages of the instrument. These would express disapproval of the topic, or were directed at Mrs. Alexander, with expressions of concern and quotation of Bible verses. A few questionnaires even had Bible tracts stapled to them. In all, there were 22 completely blank questionnaires: 13 from priests, 8 from ministers, and only one from a rabbi. In light of the sampling distribution, Roman Catholics were overrepresented here.

The paper questionnaires returned to the investigator were anonymous, with no personal information (e.g., name) appearing anywhere. In 2011, these were forwarded to the present author, who developed a set of data codes (i.e. variable names and values) and tasked a research assistant with entering the questionnaire responses into a SAS Dataset.

Measures and Analyses

The AUFORCS consisted of 11 statements requiring respondents to affirm their agreement or disagreement with each item on a five-point Likert scale metric (coded: 1 = "strongly disagree," 2 = "disagree," 3 = "neither agree nor disagree," 4 = "agree," 5 = "strongly agree"). The 11 statements were:

1. "Official confirmation of the discovery of an advanced, technologically superior extraterrestrial civilization would have severe negative effects on the country's moral, social and religious foundations." (referred to in the present paper as "effects")

2. "My congregation would perceive any contact made with a technologically advanced extraterrestrial civilization, direct or indirect, as a threat." ("threat")

3. "The discovery of another intelligent civilization would cause my congregation to question their fundamental concepts regarding the origin of life." ("question")

4. "If highly advanced intelligent life exists elsewhere in the universe, the basic tenets of religion would be present." ("tenets")

5. "Genetic similarities between mankind and an advanced extraterrestrial civilization would challenge the basic religious concepts of man's relative position in the universe." ("genetic")

6. "If an advanced extraterrestrial civilization had religious beliefs fundamentally different from ours, it would endanger organized religion in this country." ("endanger")

7. "Scientific confirmation of contact with an advanced extraterrestrial civilization is probable in our lifetime." ("confirm")

8. "It is unlikely that direct contact with an advanced extraterrestrial civilization has occurred or is currently ongoing." ("no contact")

9. "My congregation would question their beliefs if an advanced extraterrestrial civilization had no system of religion." ("no religion")

10. "If an advanced extraterrestrial civilization proclaimed responsibility for producing human life, it would cause a religious crisis." ("crisis")

11. "I believe my answers to the preceding questions represent the views of my congregation." ("represent")

Descriptive statistics (means and standard deviations) of all 11 AUFORCS items were obtained using the UNIVARIATE and FREQ procedures in SAS version 9.2. ANOVA analyses of religious differences in the 11 items by the three categories of clergy (Roman Catholic, Protestant, Jewish) were conducted using the GLM procedure. Pearson correlations among all 11 items were obtained through the CORR procedure.

Results

In Table 1, descriptive statistics are presented for the 11 AUFORCS items, as well as a test of any potential religious differences in responses. For 8 of the 11 items, the average response is below the midpoint—that is expressing disagreement, on average. In other words, respondents, on average, disagree that disclosure would negatively impact the country's foundations ("effects"), that one's congregation would perceive alien contact as a threat ("threat"), that disclosure would cause congregants to question their beliefs ("question"), that humanoid aliens would challenge our basic religious concepts ("genetic"), that a different religion among extraterrestrials

TABLE 1
Religious Differences in the AUFORCS Questionnaire Items

AUFORCS Items	Overall Mean (sd)	Ministers Mean (sd)	Priests Mean (sd)	Rabbis Mean (sd)	F	p
Effects	2.03 (.92)	2.12 (.98)	1.89 (.83)	1.89 (1.36)	1.47	.23
Threat	2.33 (.99)	2.31 (1.00)	2.36 (.99)	1.78 (1.09)	1.35	.26
Question	1.97 (.95)	1.88 (.95)	2.07 (1.00)	1.78 (.67)	1.05	.35
Tenets	3.86 (.87)	3.95 (.86)	3.76 (.86)	3.88 (.99)	1.11	.33
Genetic	2.07 (.94)	2.03 (.97)	2.03 (.87)	2.44 (1.01)	.84	.43
Endanger	2.23 (.98)	2.15 (.98)	2.30 (.90)	2.33 (1.58)	.54	.58
Confirm	2.55 (.95)	2.37 (.92)	2.81 (.96)	2.38 (.74)	5.07	.007
No Contact	3.58 (.97)	3.68 (.95)	3.49 (.90)	3.38 (1.51)	1.18	.31
No Religion	2.14 (.95)	1.96 (.92)	2.35 (.97)	1.89 (.93)	4.15	.017
Crisis	2.64 (1.23)	2.53 (1.33)	2.62 (1.01)	2.89 (1.69)	.42	.66
Represent	3.74 (.70)	3.82 (.70)	3.62 (.70)	3.67 (.87)	1.87	.16

would endanger our own organized religions ("endanger"), that disclosure is likely in our lifetime ("confirm"), that the absence of religion among extraterrestrials would cause congregants to question their beliefs ("no religion"), and that extraterrestrials claiming to have created humans would cause a religious crisis ("crisis"). For three items, respondents score, on average, above the midpoint; thus expressing agreement, on average, with the following statements: that intelligent life elsewhere in the universe would nonetheless have religion ("tenets"), that it is unlikely that direct contact with extraterrestrials is occurring ("no contact"), and that responses likely reflect those of one's congregants ("represent). Finally, there are statistically significant religious differences for only two of the 11 items—"confirm" ($F = 5.07$, $p = .007$) and "no religion" ($F = 4.15$, $p = .017$)—with modestly higher scores among Roman Catholic priests, but still within the same response category for each of these items.

In Table 2, intercorrelations among the 11 AUFORCS items are

TABLE 2
Pearson Correlations of the AUFORCS Questionnaire Items

AUFORCS Items[a]	1	2	3	4	5	6	7	8	9	10
1. Effects										
2. Threat	.49***									
3. Question	.41***	.45***								
4. Tenets	–.15*	.01	–.09							
5. Genetic	.36***	.31***	.51***	–.14*						
6. Endanger	.41***	.27***	.33***	–.05	.32***					
7. Confirm	–.16*	–.14*	–.03	–.06	–.11	.02				
8. No Contact	.10	.14	.09	.14	.11	.10	–.45***			
9. No Religion	.29***	.33***	.46***	–.11	.35***	.39***	.02	.08		
10. Crisis	.44***	.33***	.30***	–.11	.38***	.48***	–.09	.12	.44***	
11. Represent	–.09	–.11	–.17*	.21**	–.07	–.21**	.02	.06	–.29***	–.19**

[a] Pairwise Ns range from 196 to 204.
*$p < .05$; **$p < .01$; ***$p < .001$

presented. Out of 55 correlations, 31 are statistically significant. Most of these show moderate to strong levels of intercorrelation among items, but there are a few exceptions. For one of the variables ("no contact"), there are no significant associations with any other variables except for "confirm," which itself is only modestly and inversely related to a couple of items ("effects" and "threat"). Interestingly, this latter finding would seem to indicate that an expectation of a future danger to civilization ("effects" and "threat") is somewhat more likely among those respondents who do not believe that scientific confirmation of alien contact is likely to occur anytime soon ("confirm").

Discussion

These findings tell us two things, more or less. First, it does not appear that disclosure would precipitate a religious crisis of considerable magnitude. Second, at least in the present sample of clergy, there do not appear to be substantive differences in how leaders of respective religious traditions would react to such disclosure, for better or worse. Coupled with the other data reviewed in the Introduction to this paper, it can be guardedly concluded that most Americans are not as fragile emotionally, or spiritually, as certain authorities may presume. Since the early 1950s, the idea of disclosure of alien contact "was thought to be potentially socially unstabilizing—hence the need to manipulate the perceptions of the populace" (Petersen 2001:421–422). If existing data, limited as they are, tell us anything, it is that perhaps these presumptions are overstated.

In other words, if there is a religious crisis to be averted by government nondisclosure, such a crisis may be primarily in the minds of those less familiar with or engaged in religion. Perhaps the presumption of an inevitable religious crisis may reflect an irreligiousness or even hostility or condescension toward religion, on average, among physical scientists, engineers, military and intelligence officials, politicians and federal bureaucrats, and influential media figures—in other words, the opinion leaders on this subject, by their own presumption. If the present data, from this study and from other surveys and polls, tell us anything, it is that most Americans would just keep going about their business should the government some day choose to disclose evidence of an extraterrestrial presence on Earth. Prominent religious institutions would not collapse nor would major religious belief systems implode. Rather, perhaps, the implied authority of our leaders would be threatened: The public might stop being so deferential. Our world might not collapse, but maybe their world would. But this is only speculation.

The larger issue of the interface of ufology and religion may be

considerably more nuanced than has been able to be engaged with these data. Just what constitutes a UFO or alien contact (or "alien" or "contact") and the many ways that these topics interface with the domain of religion are complex and multifaceted. In the seminal RAND report on UFOs, the famous sightings at Fatima, Portugal, in 1917 were described as "a typical UFO phenomenology" (Kocher 1968:2), an observation that might surprise or confound church authorities. The data points, if you will, that would need to be considered in any comprehensive look at the interface of alien contact and its impact on religion, and vice versa, are surely vast.

This subject begs for national probability data, such as from a large-scale social survey of the U.S. adult population, in conjunction with sufficient questions about religious identity, belief, and practice to enable more thorough investigation of any putative religious conditioning of UFO- or alien-contact–related beliefs or attitudes. Perhaps some of the AUFORCS items could be used, recrafted as necessary according to the current standards of probability survey research methods. With sufficient funding and institutional support, the present author hopes to explore these ideas within the next few years.

The AUFORCS project and other prior efforts to bring quantitative data and empirical analysis to bear on this issue are undeniably admirable, if not quite up to the current state of the art of academic social research methodology. These studies were done on a shoestring budget and, without the financial and human resources of a social research shop, they cannot be expected to match the sampling frame, response, and technical expertise of mainstream national probability surveys. On the other hand, mainstream social research efforts typically do not evince interest in the subject domain investigated by the AUFORCS and the other studies cited here. So Mrs. Alexander is to be highly commended for her foray into this topic, and the present author is grateful for the opportunity to maximize what could reasonably be extracted from these data.

This exercise has proven useful not just as a preliminary take on this issue, but also as a constructive look at the barriers to be surmounted in conducting survey research on the religious consequences of alien contact. To wit, the surveys returned blank or with religious tracts stapled to the response sheet along with plaintive handwritten invitations to the investigator to surrender her life to the Lord. Clearly, these are unusual responses, even by the standards of research on anomalous phenomena. As noted at the start of this paper, within this marginal area of inquiry—namely study of anomalous phenomena, which includes the field of parapsychology and some domains of ufology—research on contact with extraterrestrials may be its most marginal corner. Folding in consideration of a topic as

contentious as religion makes this a subject that many perhaps would choose to avoid. But, as indicated earlier, this subject concerns an issue that may be of pressing national security interest, if certain reports are to be believed. If so, then academic scientists and scholars ought not shy away from more in-depth investigation of the religious correlates and consequences of beliefs and attitudes about disclosure and of disclosure itself.

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RESEARCH ARTICLE

Hallucinatory Telepathic Experiences Induced by *Salvia divinorum*

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Abstract—Scientific investigations of extrasensory perception are scarce despite the fact that anomalous paranormal experiences are common in psychiatric and general populations. This report investigated self-reported cases of telepathy-like experiences induced by smoking *Salvia divinorum* or by ingestion of LSD. Trip reports posted on a recreational drug website have been saved and analyzed for the presence of anomalous subjective effects. Telepathy-like experiences were reported both by subjects smoking *S. divinorum* and by subjects ingesting LSD, frequently in combination with other psychoactive substances such alcohol and marijuana. Descriptions of telepathy differed in the content and the audibility of the experience. Phenomenological differences suggest that telepathy-like experiences may have different etiology. The findings are discussed in relation to the activity of mirror neurons, empathy, hallucinations, and thought disorders. The paper also proposes a theoretical framework and a questionnaire designed to investigate the phenomenology of telepathic experiences.

Keywords: *Salvia divinorum*—telepathy—thought disorders—insertion—empathy—broadcasting—mindreading—empathy—mirror neurons—LSD

Introduction

Scientific studies of extrasensory perception are scarce (Brugger & Mohr 2008) despite the fact that paranormal experiences, such as telepathy, are common in the general population (Glicksohn 1990, Ross & Joshi 1992). It has been proposed that paranormal beliefs can arise from misinterpretation of coincidence and randomness, erroneous interpretation of normal experiences, some psychological factors such as fantasy proneness, dissociativity, absorption, and susceptibility to false memories, and from

the occurrence of anomalous experiences (Glicksohn 1990, Glicksohn & Barrett 2003, Brugger & Mohr 2008, French, Santomauro, Hamilton, Fox, & Thalbourne 2008). Paranormal experiences were previously associated with some psychedelic drugs (Luke 2008), and it has been found that drug users express stronger belief in paranormal phenomena (Kumar, Pekala, & Cummings 1993, Luke 2008). This report contributes to this field by investigation of hallucinatory extrasensory perception induced by smoking *Salvia divinorum* (*S. divinorum*), which is a short-acting hallucinatory plant (Vortherms & Roth 2006).

Methods

During the study of subjective effects induced by *S. divinorum* I have found descriptions of hallucinatory extrasensory perception. To explore in more detail the frequency and phenomenology of these unusual experiences, I have analyzed "trip" reports posted on the neurogroove.info website (http://neurogroove.info). All trip reports tagged for *S. divinorum* have been saved and analyzed for concomitant ingestion of other drugs. Reports of the combined effects of *S. divinorum* and other psychoactive substances were rejected. Remaining reports have been screened for unusual experiences such as knowledge about other people's thoughts and the sensed presence of external objects which were perceived without the use of any known senses such as vision, hearing, or touch. Additionally, I have also collected trip reports describing effects induced by LSD in order to compare the hallucinatory telepathic experiences induced by *S. divinorum* with experiences induced by other psychoactive drugs. The analysis of subjective effects induced by LSD has been performed on trip reports posted on the neurogroove.info website from December 1999 to October 2005.

Results

Salvia divinorum

There were 72 trip reports tagged for *S. divinorum*. Nineteen reports were rejected from further analysis because they described combined effects of *S. divinorum* and other psychoactive substances. The remaining 53 reports contained descriptions of 81 trips induced by smoking (79 trips), chewing (1 trip), or combined chewing and smoking *S. divinorum* (1 trip). The wordcount of all analyzed trip reports was 40,184. Extrasensory experiences were present in 16 reports describing 20 separate trips. In most cases, the extrasensory experience has been induced by smoking *S. divinorum* with the exception of one subject who both chewed and smoked the leaves. Selected reports contained descriptions of 13 cases of sensed presence of a human,

TABLE 1
Telepathic Experiences Induced by *S. divinorum*

Subject Number	Trip Number	Form of Drug	Experience Description
1	1	E5	He heard their thoughts flowing within the lines of energy (*connecting objects in the room*). He heard thoughts in the same way as one can hear somebody's voice but in fact there were no words but only shapes. He heard shapes. They were talking and commenting on his every movement.
2	1	DL	When his friend was cutting something with a saw, he had a strange impression that Nature was revolting, that it wanted total quiet.
2	2	DL	It seemed to him that she wanted to send him a message it was not yet time for.
3	1	E10	He sensed the opinion of Salvia that they (*hallucinated numbers and signs produced by his room*) were always present but only now was he able to perceive them. In his head and in his entire body he felt repeated words in another language, the language of thoughts and feelings. . . . Salvia started to explain to him the entire matrix using her language, but he was not able to follow the translation of feelings into thought and words. He sensed around him a dispassionate assurance from the higher creature that he always wanted to be there. He had a thought . . . and in response he received silent/unspoken disapproval. The message "it was how it was and it was all" emanated from Salvia's world.
4	1	E5	In his head appeared thoughts and he replied aloud. It was a very strange feeling for him, no voices or anything like that, just such a strange feeling that was impossible to describe. He asked himself whether that is how telepathy looks?
4	2	E5	He had again this feeling of strange telepathy. Something just appeared in his head and he replied aloud.
5	1	DL	He knew that they wanted to tell him that he would not take another hit (of salvia). He replied to them that he would take it.
6	1	DL	He felt the presence of two silent persons and he knew that they were friendly and that they wanted him to follow them.
7	1	E5	She (*hallucinated girl*) sent thoughts to him. He heard her voice, although her lips were not moving.
8	1	E10	He felt that he was sent a clear message to keep away from this plant.

DL—dried leaf; E5—extract x 5; E10—extract x 10.

human-like creatures, or objects, 10 cases of telepathy (Table 1, Appendix A), and 4 cases of other unusual extrasensory perceptions.

LSD

There were 72 reports describing 79 trips induced by LSD. One of the reports has been published twice with different titles. Thirty-five trip reports contained information about ingestion of other psychoactive substances

such as marijuana, ecstasy, amphetamine, and alcohol, which could affect the effects induced by LSD. The high incidence of concomitant ingestion of LSD and other drugs resulted from long latency and long duration of the effects induced by LSD (usually lasting for several hours). Because of the difficulty of finding descriptions of experiences induced only by LSD, all collected reports have been used for comparison with the effects of *S. divinorum*. The word count of all analyzed trip reports was 73,595. Nine trip reports contained descriptions of hallucinatory extrasensory perception. Two subjects reported the experience of sensed presence (friends standing behind the doors / presence of evil), whereas 8 subjects reported telepathy (Table 2, Appendix B). In most cases, the telepathy-like experiences were reported by subjects who ingested both LSD and other psychoactive substances (Table 2).

TABLE 2
Telepathic Experiences Induced by LSD or by Combined Ingestion of LSD and Other Drugs

Subject Number	Ingested Drugs	Experience Description
1	LSD	They looked at a branch and heard the rustle of leaves. The leaves whispered words in an unintelligible language. After a while, they understood that the branch wanted to play with them and that it wanted to make friends with them.
2	LSD	It seemed to him that his friends were waiting for him behind the door and that they wanted to greet him as a newborn person. He was surprised when he opened the door and there was nobody.
3	LSD/Mj	The subject had a problem cutting the bread because the knife somehow did not want to be taken by him.
4	LSD/X	The subject mentioned the appearance of metaphysical thoughts and telepathic communication with friends.
5	LSD/Alc	The subject was talking with a friend but they could not communicate properly. It seemed to him that the friend was able to understand him without words but in fact the friend could not understand what he was talking about.
6	LSD/Mj	The subject felt that he and his dog could understand each other very well.
7	LSD/Alc	The subject looked at other people. Some of them were good but others were not so much OK. Next, the author mentioned that it was an intense extrasensory perception.
8	LSD/Alc	He hugged the tree and felt that it was alive and good. The tree \'told \' him where he should go. He could see the map of the forest with the eyes of the soul.

Alc—alcohol; Mj—marijuana; X—undefined drug; \'\'—unusual quotation marks used by the author of the trip report.

Discussion

Extrasensory perception is rarely described in medical literature because of the difficulty of classifying such phenomena and because items such as extrasensory perception, sensed presence, and telepathy are often not present in psychiatric questionnaires. Although numerous authors have investigated the effects induced by *S. divinorum* (Siebert 1994, Gonzalez, Riba, Bouso, Gomez-Jarabo, & Barbanoj 2006, Dalgarno 2007, Albertson & Grubbs 2009, Vohra, Seefeld, Cantrell, & Clark 2009, Baggott, E. Erowid, F. Erowid, Galloway, & Mendelson 2010, Sumnall, Measham, Brandt, & Cole 2010), only two papers reported effects relevant for the present study. Dalgarno (2007) reported a crippling experience of sensed presence in 1 out of 10 interviewed users of *S. divinorum,* whereas Sumnal, Measham, Brandt, and Cole (2010) reported that intoxication induced by *S. divinorum* was associated with the experience of thoughts belonging to somebody else. The present study confirms previous occasional reports (Dalgarno 2007, Sumnall, Measham, Brandt, & Cole 2010) and extends them by providing detailed descriptions of the telepathic experiences. The analysis of the effects induced by *Salvia divinorum* shows that the feeling of telepathy differs in audibility of the experience. In some cases, there is just a knowledge or feeling, whereas in other cases the subjects are able to hear the thoughts sent by others (Table 1). Another distinction can be made on the basis of the content of the telepathic experience. In some cases, the experience resembles conversation, whereas in others it is more a knowledge about the emotional state or intention of another person. Telepathic experiences were also reported by subjects ingesting LSD, often in combination with other psychoactive substances. These experiences were, however, less bizarre than in the cases of smoking *S. divinorum* and usually were related to knowledge about the intentions of other people, animals, plants, or objects.

Previous studies showed that the experience of telepathy is not restricted to the effects induced by hallucinogenic drugs. An example of telepathic experience related to the attitude of another person has been reported recently in an epileptic patient who experienced the feeling that the hallucinated unknown girl, sitting with her back turned toward him, wanted to talk (B. Kasper, E. Kasper, Pauli, & Stefan 2010). Nonetheless, the phenomenology of telepathic experiences is rarely described in the literature. More information is available on the frequency of telepathic experiences both in the general population and in psychiatric patients (Greyson 1977, Ross & Joshi 1992). The study performed by Ross and Joshi (1992) revealed that 15.6% of people from the general population experienced telepathy, whereas Greyson (1977) reported that 13% of the nonschizophrenic and

40% of schizophrenic patients admitted to the psychiatric unit claimed the ability to read other people's minds. The higher prevalence of telepathic abilities among schizophrenics was associated with a higher prevalence of thought insertion, thought withdrawal, and thought broadcasting (Greyson 1977). These results suggest that the experience of telepathy can be associated with thought disorder, and similarity between telepathy and thought insertion has been already noticed by Mullins and Spence (2003). The phenomenon of thought disorder helps us to understand the occurrence of telepathic experiences. It is, however, not sufficient to completely explain them because the experience of thought insertion can be perceived both as telepathy and as external mind control. Therefore, there is a need for an additional mechanism differentiating the experience of telepathy from the feeling of external mind control. Furthermore, because there are differences in audibility and in the content of telepathic experiences, it can be assumed that the experience of telepathy may have different etiologies. In some cases, the experience of telepathy can stem from auditory hallucinations (Podmore 1909, Dubal & Viaud-Delmon 2008, French, Santomauro, Hamilton, Fox, & Thalbourne 2008, Wackermann, Putz, & Allefeld 2008), whereas in others it can be a thought disorder (Mullins & Spence 2003, Sims 2003). Finally, the telepathic experience can also be explained as a delusional mindreading resulting from abnormal activation of neuronal networks underlying the natural ability to guess the mental states of other people based on their behavior. Hypothetical mechanisms underlying the telepathic experiences are summarized in Figure 1. The proposed model is consistent with previous suggestions that telepathy may be associated with empathy and with activation of mirror neurons (Donovan 1997, Heyes 2010). The model is also congruent with studies that found an association between paranormal belief and schizotypy, psychosis-proneness, and hallucination predisposition (Thalbourne & French 1995, Wolfradt, Oubaid, Straube, Bischoff, & Mischo 1999, Levine, Jonas, & Serper 2004, Goulding 2005, Dubal & Viaud-Delmon 2008). Although telepathic experiences can be related to different phenomena, there is a need for a common mechanism attributing the experience to another person. Therefore, I propose that this common mechanism may be the activation of a neuronal network underlying the mindreading (mirror neurons). Interestingly, the network implicated in mindreading (mirror neurons) includes the temporal cortex and the adjacent region of the anterior insula (Agnew, Bhakoo, & Puri 2007, Bastiaansen, Thioux, & Keysers 2009, Samson 2009). Therefore, there is an anatomical overlap of neuronal networks implicated in mindreading and brain regions related to telepathic experiences, auditory hallucinations, and thought disorders (Mace & Trimble 1991, Reutens, Savard, Andermann,

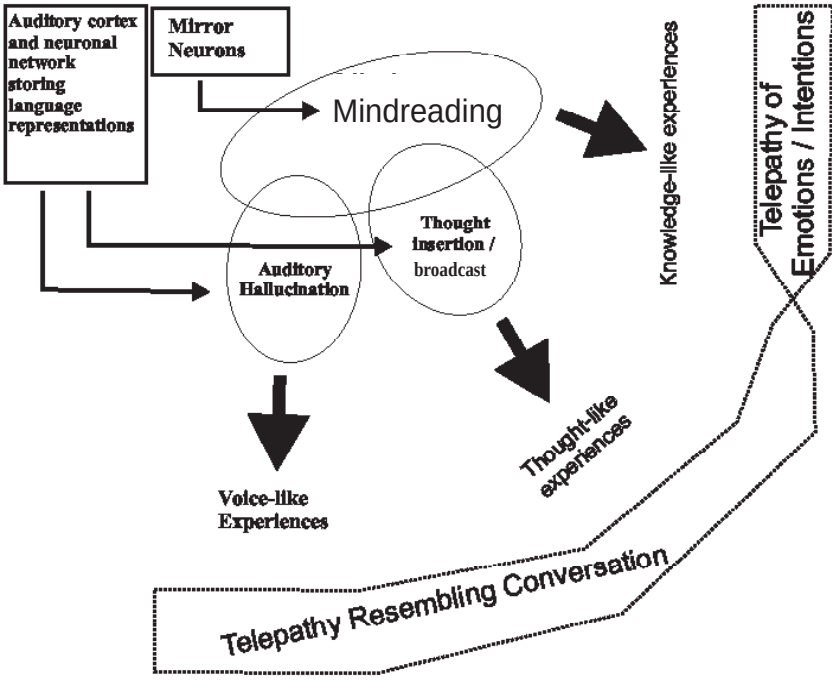

Figure 1. The model of telepathic experiences.

Dubeau, & Olivier 1997, Mizukami, Yamakawa, Yokoyama, Shiraishi, & Kobayashi 1999, Asheim Hansen & Brodtkorb 2003). According to the proposed model (Figure 1), the auditory hallucination associated with concomitant activation of mirror neurons will be perceived as a telepathic experience during which the subject can hear the thoughts of another person. Simultaneous thought insertion and activation of mirror neurons will be perceived as a telepathic experience without auditory quality, whereas thought insertion that is not paralleled by activation of mirror neurons will be perceived as a feeling of external mind control. When there is a predominant activation of mirror neurons, the subject will experience the delusional mindreading related to the emotional states and intentions of others. A similar approach based on the concomitant occurrence of different perceptual components has been applied recently to the problem of auditory hallucinations (Sommer, Selten, Diederen, & Blom 2010). The model presented in Figure 1 constitutes a theoretical framework that explains different telepathic experiences using already-existing theories of hallucinations and thought disorders in combination with the mechanism of

TABLE 3
Questionnaire Screening for Telepathic Experiences and Terminology Used to Describe Different Experiences

Number	Question	Proposed Phenomena
1	Do you know what kind of emotions other people are experiencing when you look at them?	Normal mindreading
2	Do you know the intentions of other people when you look at them?	Normal mindreading
3	Have you ever had the feeling that you know what kind of emotions somebody experienced, although this person was not with you and you did not know what this person was doing at that moment?	Delusional mindreading
4	Have you ever had the feeling that you know the intention of somebody else, although this person was not with you and you did not know what this person was doing at that moment?	Delusional mindreading
5	Have you ever had the feeling that somebody else communicated with you by means of telepathy and the experience resembled conversation but instead of hearing other people's speech you could hear their thoughts.	Telepathy with auditory component
6	Have you ever had the feeling that somebody else communicated with you by means of telepathy and the experience resembled conversation but instead of hearing other people's speech you received their thoughts / their thoughts appeared in your mind?	Telepathy without auditory component
7	Have you ever sent messages to other people by means of telepathy?	Delusion of telepathic control of others

mindreading. To foster further progress in understanding the neurobiology of telepathic experiences, I have prepared a questionnaire (Table 3) which is based on the phenomenology of collected reports and is consistent with the proposed theoretical framework. Although there are several available questionnaires designed to screen for paranormal experiences (Eckblad & Chapman 1983, Thalbourne & Delin 1993, Gallagher, Kumar, & Pekala 1994, Kumar, Pekala, & Gallagher 1995, Houran, Thalbourne, & Lange 2003, Tobacyk 2004), none of them investigates the phenomenology of telepathic experiences. The proposed questionnaire can be applied in combination with already-used psychiatric questionnaires such as CAPE and SAPS (Andreasen 1984, Brenner, Schmitz, Pawliuk, Fathalli, Joober, Ciampi, & King 2007) to screen psychiatric and neurological patients, the general population, and drug users for telepathic experiences in addition to other psychic experiences.

Conclusions

It has been found that *S. divinorum* and other psychoactive substances induce in some subjects hallucinatory telepathic experiences, which differ in the content and the audibility of the experience. It is proposed that phenomenological differences may be important for understanding the mechanism underlying paranormal experiences.

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APPENDIX A
Reports Containing Description of Hallucinatory Telepathic Experiences Induced by *S. divinorum*, and Nicknames of Authors

Subject Number	URL	Subject Nickname
1	http://neurogroove.info/trip/sd-juz-was-rozumiem-abli	Abli
2	http://neurogroove.info/trip/pani-salvia	Wronek
3	http://neurogroove.info/trip/szkola-zycia-z-szalwia-altwet	altWET
4	http://neurogroove.info/trip/salvia-8211-co-mi-w-pamieci-utkwilo-retrospekcje	unknown
5	http://neurogroove.info/trip/atak-malych-szalwiowych-ludzikow-z-szalwii	unknown
6	http://neurogroove.info/trip/salvia-odkryjmy-lepszy-swiat	unknown
7	http://neurogroove.info/trip/szauwia-dywanora-czip-riipoot	unknown
8	http://neurogroove.info/trip/bliskie-spotkanie-z-ekstraktem-10x	unknown

APPENDIX B
Reports Containing Descriptions of Hallucinatory Telepathic Experiences Induced by LSD, and Nicknames of Authors

Subject Number	URL	Subject Nickname
1	http://neurogroove.info/trip/moj-pierwszy-papierek	unknown
2	http://neurogroove.info/trip/pieklo-i-niebo	unknown
3	http://neurogroove.info/trip/moj-pierwszy-kwas	unknown
4	http://neurogroove.info/trip/baba-na-rowerze	unknown
5	http://neurogroove.info/trip/luxus-szczescie-dekadencja	unknown
6	http://neurogroove.info/trip/zajecia-pozalekcyjne	unknown
7	http://neurogroove.info/trip/hoffman-12	unknown
8	http://neurogroove.info/trip/principia-lysergia	unknown

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RESEARCH ARTICLE

Hypnosis Reconsidered, Resituated, and Redefined

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Abstract—The two-hundred-year history of hypnosis and its predecessor, animal magnetism, is replete with stories of unusual phenomena. Perhaps surprisingly, a close reading of that history reveals that investigators and students of hypnosis have been unable to achieve an agreed-upon definition of their subject matter. Because of this failure to describe the essential nature of hypnosis, they resorted to lists of hypnotic phenomena as a means for confirming the presence of a hypnotic state in clinical and experimental situations. However, identification and enumeration of hypnotic phenomena proved to be problematic. The content of these lists varied from era to era and from practitioner to practitioner, and the selection of phenomena seemed to be an arbitrary process. With no agreed-upon definition and no definitive list of phenomena that would apply to hypnosis and hypnosis alone, there was no way to ensure that the "hypnosis" that was being studied in clinical and experimental work was the same entity in all cases. Although hypnosis research in recent decades has yielded important insights, significant difficulties and disagreements remain. It is the contention of this article that this confusing state of affairs came to pass because the discussion of hypnosis in the literature was wrongly situated and that there is a need to step back and gain a new perspective on hypnosis and hypnotic phenomena. The proposed fresh look at hypnosis situates hypnosis as a subspecies of *trance* as defined in a very specific way: a state of profound focus on something accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources. *Hypnosis* is then defined as an inner-mind trance characterized by rapport. This new approach and its implications are discussed in some detail.

Keywords: hypnosis—trance—rapport

Brief Historical Sketch of Hypnosis

This history of hypnosis is very brief. It is intended to highlight certain events that will help place in context the issues to be dealt with in what follows. In this section there is no attempt to critically comment on these events. For a fuller history, see Laurence and Perry (1988), Gauld (1992), and Crabtree (1993).

Hypnosis has a venerable history. As a term (in its earliest form *neurohypnotism*), it goes back to 1842, but as a human experience that could be induced at will and studied, it has its beginnings in the ideas and healing practice of Franz Anton Mesmer (Mesmer 1779) and most especially his pupil Armand Marie Jacques de Chastenet, the Marquis de Puységur (Puységur 1784). Mesmer developed a healing technique and accompanying theoretical framework that he called "animal magnetism." He used "magnetic passes" or sweeping motions of the hands over the body of the ill to apply the healing power of what he called "magnetic fluid," a vital energy that he believed pervaded the universe.

Puységur, after learning to use animal magnetism from a seminar offered by Mesmer in 1784, soon noticed that something odd seemed to happen to those he magnetized. Many entered into a state with these characteristics: 1) a sleepwalking kind of consciousness, 2) a "rapport" or special connection with the magnetizer, 3) suggestibility with heightened imagination, 4) amnesia in the waking state for events in the magnetized state, 5) ability to read the thoughts of the magnetizer, and 6) a striking change in the personality of the magnetic subject (Crabtree 1993:38–45). The magnetized person seemed to be asleep, but was awake enough to communicate with the magnetizer. Rapport meant that the subject was connected both mentally and, it seemed, physically with the magnetizer. The magnetic subject was ready to follow the suggestions of the magnetizer and experienced a heightened ability to imagine things vividly. Amnesia for events occurring during magnetic somnambulism upon returning to the normal state (which Puységur believed to be a feature present in all cases) led to the notion that everyone possesses a *divided consciousness*, and he regarded the waking and magnetized states as "two different existences" (Puységur 1784:90). Ability to read the magnetizer's thoughts was augmented over the years of his practice to include other paranormal capacities, such as being able to perceive objects and situations not available to the senses and the ability to exercise a "sixth sense" by which magnetic somnambulists could diagnose their own illnesses or those of others and prescribe effective remedies. The magnetic subject's personality was sometimes altered so radically that he or she seemed to be a different person when magnetized.

Mesmer had previously noted that some of his magnetic subjects went into a "swoon" during his ministrations, but he did not consider that state significant and simply had them placed in a separate room where they could recover. Mesmer believed that the fainting was merely part of the natural healing process. When he began to use animal magnetism, Puységur immediately saw the importance of this state, which he called "magnetic sleep" or "magnetic somnambulism," and made a careful study

of it for the rest of his life. As it turned out, Puységur's discovery was to have momentous consequences for the subsequent history of psychology and psychological healing (Crabtree 2003, Ellenberger 1970). Puységur believed that magnetic somnambulism was the same thing as natural somnambulism or sleepwalking, with the important difference being that the magnetic subject was in a state of rapport with the magnetizer, whereas the sleepwalker was in rapport with no one (Puységur 1811). Puységur considered that the somnambulistic state had a healing virtue and that remedies for the illness being treated, which were suggested by magnetic somnambulists, were effective when applied.

In the decades following Puységur's initial findings, animal magnetic healing split into two streams, one continuing along the lines of Mesmer's practice and the other emphasizing the psychological dimensions demonstrated by Puységur. Puységur spoke only well of Mesmer and accepted the reality of magnetic fluid, but it was not central to his magnetic work. Over time Puységur's approach dominated magnetic healing practices and opened previously unimagined doors to the inner psyche.

Animal magnetism survived the negative findings of two French commissions set up to investigate the phenomenon in 1784. The commissions' investigations centered on whether or not there was such a thing as "magnetic fluid," and, except for one dissenting report, found against it. Nevertheless, the number of practitioners using animal magnetism as a healing approach continued to grow, and spread from Paris to the rest of Europe, to England, and eventually to the United States.

In the fifty years after Puységur's discovery, practitioners of animal magnetism used their own experiences to add to his list of six somnambulistic phenomena. They wrote about analgesia and anesthesia, "travelling clairvoyance" (which involved leaving the body and finding oneself at another location), precognition, magnetizing at a distance, discernment of the magnetic fluid, and ecstasy. Other phenomena related to the state of rapport and included being responsive to the magnetizer's mental commands, experiencing the magnetizer's physical sensations, and being influenced by the magnetizer's movements (Crabtree 1993:41).

Beyond the explanations offered by Mesmer and Puységur, a number of explanatory schemes were developed to account for the phenomena of animal magnetism. Spiritistic schools believed that the phenomena were produced through the intervention of spirits. Others believe they could be explained as the result of "sympathy" (a notion derived largely from Renaissance medicine) combined with the belief in a universal world-spirit that connects all things. Still others held that the efficacy of animal magnetism derived from the power of suggestion. One theory claimed the

phenomena were due to an accumulation of electrical fluid in the body, particularly the brain and stomach area (Crabtree 1993:113–127).

This was the state of affairs when a new understanding of magnetic somnambulism arose in England in the 1840s. Although animal magnetism had made some inroads in England in earlier times, it only became well-established there around 1830 through promotional demonstrations given in London by the well-known French magnetizer the baron Jules Du Potet. Then in 1842, Manchester physician James Braid attended a stage demonstration of animal magnetism given by Charles Lafontaine and immediately became interested in magnetic phenomena. At the demonstration he saw the deaf cured, paralytics given the ability to move, and sight restored to the blind. A skeptic at first, Braid soon came to believe that something real was going on (Braid 1842). However, he did not accept the magnetizer's explanation that the phenomena were produced by magnetic fluid. Instead he developed his own explanation and a new nomenclature (Braid 1843).

Braid believed the phenomena he witnessed had a purely physiological cause: When fatigued by a prolonged sensation of some kind, the mind "slips out of gear" producing a state of "somnolency," and "a peculiar state of the brain and mobility of the nervous system, which render the patient liable to be directed so as to manifest the mesmeric phenomena" (Braid 1842:321, Crabtree 1988:450). His theory posited a "new agency," one that he found to be particularly effective in his medical practice. He called this agency "neuro-hypnotism," later shortened to "hypnotism," and the practitioner of neuro-hypnotism a "hypnotist." As he developed his theory over the following years, Braid concentrated on hypnotism as a form of monoideism or focused thought, and gave increasing attention to the role of suggestion in the induction of hypnotism and the effects produced by the hypnotized subject (Crabtree 1988:465)

Hypnotism did not catch on in England during Braid's lifetime. However, the French were impressed by his ideas, particularly his use of suggestion. About 1860, four French physicians, Eugène Azam, Paul Broca, Jean Demarquay, and M. A. Giraud-Teulon, began to experiment with hypnotism as a surgical anaesthetic. Their researches came to the attention of the provincial physician Ambroise Liébeault, who began to use Braid's hypnotism in his general medical practice. He was unusually inventive in his use of hypnotism for healing, and his six books on the subject were instrumental in making hypnotism or "Braidism" known throughout Europe and the United States. One of the people who visited Liébeault's clinic to learn about hypnotism was Hippolyte Bernheim, a physician who practiced at Nancy. He became an adept at hypnotic practice and wrote seven books that further added to the fame of hypnotism. Liébeault and Bernheim founded

what came to be known as the Nancy School of hypnosis (somewhere in the 1880s the term *hypnosis* began to be used as the equivalent of *hypnotism*). Bernheim claimed that hypnosis centers around suggestion. He believed that suggestion was involved in practically all human interaction and that phenomena such as paralyses, anesthesias, sensorial illusions, and hallucinations, as well as automatic obedience, automatic movements, and post-hypnotic hallucinations, could be produced by hypnotic suggestion. He also held that many phenomena could be produced by suggestion even without hypnosis, and that, as a matter of fact, hypnosis did not enhance suggestibility.

The Nancy School found itself in opposition to the school of famed neurologist Jean-Martin Charcot, which came to be called the Salpêtrière School of Hypnosis. Charcot's view was that hypnosis was a manifestation of nervous states, each distinguished by a particular symptomology: 1) the cataleptic state, 2) the lethargic state, and 3) the somnambulistic state. He held that these states were organically determined, and not the result of suggestion. For Charcot, hypnosis was an artificially created neurosis essentially identical with hysteria. The Nancy and Salpêtrière schools flourished in the 1880s and 1890s, but by the late 1890s the Nancy School had come to dominate thinking about hypnosis. Bernheim, however, eventually developed serious questions about whether hypnosis was a special state at all.

He was not alone in having doubts. While many acknowledged the usefulness of "induced sleep" in the treatment of illnesses, not all considered that state unique. At the same time, suggestion was treated as a phenomenon in its own right and not related essentially to the hypnotic state. The potency of suggestion was fully acknowledged, but it was considered to be as effective in the normal waking state as in the state of "induced sleep." In the same period, Oskar Vogt offered an intriguing idea (Gauld 1992:370). He believed that hypnosis should be thought of as a sleep-like state with rapport. Since sleep is essentially an inhibition of consciousness, we can think of hypnosis as a state in which consciousness is inhibited except with regard to those ideas associated with the hypnotist.

It was noted by many authors that there were a variety of states not generally called "hypnosis" that were somehow related to it. Alan Gauld (1992:517) describes the characteristics of these states: 1) reduced awareness of the outer world with heightened awareness of the inner, 2) heightened responsiveness to suggestion, 3) enhancement of some psychological or physiological functions and restriction of others, 4) amnesia, complete or partial, upon returning to the normal state from the unusual state, 5) memory of events in the state and past instances of the state. Examples

of such "cognate states" were sleep, spontaneous somnambulism and somniloquism, dual or multiple personality, certain drug- or alcohol-induced states, and mediumistic states. Considerable methodological difficulties were encountered when attempting to develop criteria for distinguishing between these states and hypnosis, and discussion of these issues was inconclusive.

Attempts to deal with these matters and clarify the nature of hypnosis continued up to the beginning of World War I. After the war there was a long fallow period in which there was little progress in the understanding of hypnosis. This time of relatively little exploration ended about 1960, when fresh discussions of theories of hypnosis emerged, and researchers again began to wrestle with the proper methods for its study.

The new wave of research was initiated by a paper written by Harvard professor Robert White, fittingly (as it turns out) entitled "A Preface to the Theory of Hypnotism" (White 1941). He wrote that hypnosis should be thought of as an altered state of consciousness that occurs in an environment characterized by high levels of motivation. He reframed hypnotic behavior as meaningful, goal-directed striving, the goal being to behave like a hypnotized person as continuously defined by the operator and understood by the client. This key idea influenced many experimental researchers in hypnosis, including Martin Orne, T. X. Barber, and Nicholas Spanos.

Martin Orne, long-time editor of the *Journal of Clinical and Experimental Hypnosis*, expanded White's idea and used this perspective in his experimentation with hypnosis. He saw the hypnotic subjects of these experiments as intelligent people who understood a great deal about the situation they were in. Further, he considered that the experimental environment interacted with the personal goals of the subjects—an interaction that was expressed in terms of "demand characteristics" that indicate how the subject is expected to perform (Orne 1962). One of Orne's conclusions was that hypnosis did not enable subjects to transcend normal limits of human performance. In his experiments he developed the use of the control group as a means of identifying the genuine presence of hypnosis, as opposed to simulation of that condition.

During this same period, T. X. Barber conducted research that indicated to his satisfaction that certain marks of the presence of hypnosis, such as amnesia and arm levitation, could be attained without the aid of hypnosis. As to other phenomena judged more difficult to accept, such as hallucination, age regression, and hypnotic blistering, he was inclined to dismiss them as the result of bad observation and inaccurate reporting (Barber 1969). Unfortunately, this state of affairs seemed to leave hypnosis in a kind of no-man's land, since the traditional "phenomena of hypnosis" were not specific

to hypnosis alone. Given this state of affairs, the question arose as to how it would it be possible to do any meaningful hypnosis research.

Barber was determined to solve this problem, and as the result of carefully designed experiments, he concluded that all the way along the history of hypnosis, researchers had been self-deceived about their methods. They believed they were inducing a distinctly identifiable state called hypnosis, but in fact they were unwittingly creating a situation in which their subjects would respond in certain preordained ways to their instructions through subtle indications of the hypnotist's expectations. Barber insisted that the way to go about it was not, as some had proposed, to first define hypnosis and then study it. What was needed instead was to begin with the phenomena of hypnosis, the data, and attempt to explain them by relating them to their antecedent conditions. This, he insisted, was the first step in any scientific explanation (Barber 1967).

Later Barber notably modified his understanding of hypnosis, introducing a "three-dimensional" theory of hypnosis, which he considered a new paradigm (Barber 1999). He identified the first dimension as associated with a small group of individuals who are prone to fantasizing. The second he associated with a small group of amnesia-prone individuals who tend to forget memorable events in their lives. The third he saw as a larger group of individuals who have strongly positive attitudes, motivations, expectancies, and cognitions toward the hypnotic situation. This three-factor approach was supplemented and made more subtle by the introduction of three more dimensions: the social psychology of the psychological experiment, the dimension of the hypnotist, and the effects of suggestion on hypnotic responsiveness.

Another approach to defining hypnosis was developed by Ernest Hilgard: the "neodissociation" theory (Hilgard 1977, 1992). Like other theories, Hilgard's approach identified hypnosis as that condition that exhibits objective and subjective phenomena of the type found in the hypnotic literature. Hilgard would agree with Barber that it is not possible to arrive at an agreed-upon definition of hypnosis, and so settled for specifying the "domain of hypnosis." That domain is the collection of accepted subjective and objective hypnotic phenomena. As examples, he mentions ideomotor movements, sensory distortion, hallucinations, and post-hypnotic amnesia, so Hilgard's specification of the domain of hypnosis is accomplished by simply drawing up his preferred list of hypnotic phenomena. To explain these phenomena, he introduced the concept of dissociation originally developed by Pierre Janet (Janet 1889). But instead of talking about dissociated subconscious centers of consciousness, as Janet had, Hilgard described a different kind of dissociated element. He said that people are equipped with

hierarchically arranged cognitive subsystems that perform certain important functions in their lives. These constitute parallel streams of consciousness, separated from the main body of consciousness by an amnestic barrier. At any moment some are latent and some active. Even in ordinary situations, conflicts can arise between subsystems, and Hilgard postulated that in order to avoid chaos, there must be a "central control structure" or "executive ego" to look after things. For Hilgard, hypnotic inductions facilitate dissociative experiences. In hypnosis parts of the central control structure are handed over to the hypnotist, and the subject will do what the hypnotist suggests. In this situation, the subject has experiences and performs actions that are not in the control of his executive ego, so that the phenomena of hypnosis are essentially dissociative phenomena. Hilgard's ideas met with a great deal of criticism (e.g., Spanos 1991) and some spirited, though revisionary, defense (Bowers 1990, 1992, Woody & Sadler 2008).

A very different approach, based on the sociocognitive perspective, was built around an analysis of the social and situational environment in which hypnosis takes place. Perhaps the chief spokesperson for this perspective was Nicholas Spanos, who began as a student of Barber and was influenced significantly by his views, although he said that his deepest roots were in the ideas of Robert White. For Spanos, the hypnotized person enacts a "role," one defined both culturally and by subtly communicated expectations from the experimenter. In the hypnotic situation, the subject's attempts to fulfill the expectations constitute a feedback to the experimenter. Wittingly or not, the experimenter then gives cues to the subject about how well he or she is performing the role. In this way the situation becomes a complex web of largely unrecognized interactions that create the hypnotic result. In playing the part of the hypnotized person, the subject produces expected hypnotic phenomena. The subject may very well mistakenly believe that these phenomena emerge spontaneously or automatically, but, according to Spanos, the sociocognitively aware observer will realize that this is not the case. The mistaken view arises from misdescription of the hypnotic subject's private experiences or from deception and/or reinterpretation by the subject (Spanos 1991). Hypnotic subjects are characterized by "their willingness and ability to use their imaginal and other cognitive skills to create the subjective experiences called for by suggestions (Spanos 1996:20)." The keen observer will realize that there is no creation of a "state" of hypnosis, but a subtly choreographed interaction between subject and experimenter that produces the impressive but familiar dramatization that we call hypnosis.

Deeply affected by the change in perspective created chiefly by the work of White, Barber, Spanos, and other sociocognitivists, hypnotic

researchers have in recent times attempted to more fully come to terms with their subject matter. Particularly in the past two decades, increasing attention has been paid to the complexities involved in identifying hypnosis and hypnotic phenomena. André Weitzenhoffer examined these issues for more than forty years and shrewdly formulated crucial questions relating to these matters, attempting

> . . . to bring about some kind of sensible order . . . in what otherwise is becoming an increasingly amorphous and chaotic field. (Weitzenhoffer 2000:8)

He wrote that there is an assumption that the old hypnosis of the 19th century and the modern version have the same phenomenology. Although there are common elements, "that there is a full identity is questionable and basically untestable" (p. 3). For Weizenhoffer, this and many other questions relating to the definition of hypnosis, hypnotic phenomena, experimental methodology, clinical effectiveness, and other significant issues remain unanswered. His exposition of these problems stands as one of the most helpful guides for those who are similarly fascinated and befuddled by the present state of affairs around hypnosis.

Order and Disorder

The attempts of Weitzenhoffer and other researchers "to bring about some kind of sensible order" from the disorder we are confronted with in the field of hypnosis have thus far been only partially successful. In recent years, there have been concerted attempts to provide a framework for understanding what hypnosis is and how to effectively experiment with it. There are several tasks that must be undertaken to establish such a framework. One is to establish what type of data will be acceptable for investigators of hypnosis. There seems to be agreement that the data will consist of observable actions, physiological changes, and self-reports from the hypnotic subject (Kihlstrom 2008). Another is to establish a definition for hypnosis, one that distinguishes hypnosis from other phenomena by a description of its general characteristics. One approach is to distinguish "hypnosis-as-procedure" from "hypnosis-as-product" (Barnier & Nash 2008). In this schema, hypnosis-as-procedure involves making use of suggestions and consists of two steps: an introduction that invites the subject to participate with the experimenter in the production of imaginative experiences, and the application of a suggestion for an imaginative experience, which serves as the actual induction. The resulting state or condition (hypnosis-as-product) will be presumed to be hypnosis when the

subject produces both objective and subjective evidence that meet certain criteria. The objective evidence involves motor responses that have come to be accepted as standard phenomena of hypnosis, such as arm levitation and arm catalepsy. The subjective evidence is the self-report of the subject of the experience of altered sensations of the type generally accepted as standard phenomena of hypnosis, such as visual and auditory hallucinations and amnesia.

Hypnosis thus understood involves two people: the hypnotist and the hypnotic subject. This means that the concept of *self-hypnosis* is in certain ways problematic. Although self-hypnosis and hetero-hypnosis are highly coordinate, questions remain pertaining to what constitutes self-hypnosis and whether it is identical with hetero-hypnosis. One way to think about the matter is to say that in self-hypnosis, the subject takes on both social roles (hypnotist and subject) so that in effect self-hypnosis and hetero-hypnosis turn out to be the same thing (see Kihlstrom 2008:38).

In recent years greater emphasis has been laid upon individual differences that exist in the ability of people to experience hypnosis. This has led to the belief that in analyzing the hypnotic experience, a componential approach is best. This means recognizing that different hypnotic experiences require different components of underlying abilities and that to produce a particular kind of response one or more components may be necessary (McConkey 2008). We see elements of this kind of thinking in Barber's three-dimensional theory of hypnosis (Barber 1999). It is also in evidence in Cardeña's study of the phenomenology of deep hypnosis (Cardeña 2005). Here the author took a multifactorial approach to the phenomenology of physically passive and active hypnosis and found that the results did not indicate the presence of a single hypnotic state, but "various commonly experienced modalities of experiencing" (p. 51).

In virtually all experimental work on hypnosis, standardized scales of hypnotizability are used to determine the hypnotic abilities of subjects. Chief among them are the Stanford Hypnotic Susceptibility Scales, Forms A, B, and C, and the Harvard Group Scale of Hypnotic Susceptibility, Form A. These use as criteria certain hypnotic phenomena that have become conventionally accepted as indicative of the hypnotic state.

In my opinion, the principle reason that, according to Weitzenhoffer, hypnosis has become an "increasingly amorphous and chaotic field" (Weitzenhoffer 2000:8) lies in the fact that all approaches to defining hypnosis and determining when an individual is in a hypnotic state use the standard criteria based on a list of "hypnotic phenomena" that have been produced from time to time throughout the history of mesmerism/hypnosis, and which have in recent times become fixed and canonical in the field.

Kirsch and Lynn (1995:846) note that there is an emerging consensus about the basic phenomena of hypnosis. They also point out that there is growing acceptance of a definition of hypnosis-as-procedure as given by the American Psychological Association (APA) Division of Psychological Hypnosis: a procedure wherein changes in sensation, perception, thoughts, feelings, or behavior are suggested. Hypnosis-as-procedure thus understood depends directly on that "emerging consensus" about what the phenomena of hypnosis are, for these constitute the specific suggested "changes "to which the APA definition alludes. Barnier and Nash acknowledge this state of affairs and point out that when hypnosis-as-procedure has been applied in an experimental situation, one cannot necessarily assume that hypnosis-as-product has been elicited (Barnier & Nash 2008:6–10). They say that hypnosis-as-product is a particular state. That state can be said to be present when certain motor responses of a hypnotized subject are publicly measurable (p. 11). What are these responses? They are the hypnotic phenomena about which Kirsch and Lynn say there is an emerging consensus, and which, they say, include those phenomena that Hilgard believed specifies the domain of hypnosis, such as muscular movements, sensory distortions, hallucinations, posthypnotic amnesia, and hypnotic dreams (Kirsch & Lynn 1995:846)

Kirsch and Lynn point out that, despite a great deal of discussion about the nature of hypnosis throughout its history, theorists remain as contentious as ever (p. 847), and they discuss the methodological, sociological, and philosophical context of this contentiousness. I would like to add an additional reason for the present situation: All discussions begin with and are based on the identification of crucial hypnotic phenomena. I believe that lists of conventionally accepted phenomena can never provide an adequate basis for this discussion. I would like to suggest that the resituating and redefining of hypnosis that I propose in this article create a framework for achieving a clarity in the discussion of hypnosis that has for so long eluded researchers.

Problems Relating to the Phenomena

When Barber asked the question of what constitute the phenomena of hypnosis, he answered: They are the phenomena that "have been specified by Bernheim, Moll, Bramwell, Weitzenhoffer, and many other investigators" (Barber 1967:112). He offered as example a list drawn from Weitzenhoffer: suggested age regression, amnesia, analgesia, blindness, catalepsy, color blindness, dreams, hallucinations, hypermnesia, negative hallucination, strength enhancement, and time distortion. At the time Barber was writing, it was generally accepted that hypnotic phenomena had been definitively specified in the 1930s, in connection with work on the development of

hypnotic susceptibility scales carried out at that time (Edmonston 1986:324 ff.). Those researchers chose phenomena from among those mentioned in the mesmeric and hypnotic literature stretching back some 150 years. So for Barber and most researchers after him, the issue of what constituted the phenomena of hypnosis was considered settled, and the first step in their scientific explanation could be taken. But investigators generally ignored the fact that all these lists of phenomena varied and were compiled by picking and choosing from among the mass of phenomena mentioned in the literature. This arbitrariness in selecting crucial phenomena inevitably created problems for hypnosis research.

Since 1960, the conventional list has been narrowed down in practice to a few phenomena that are relatively easy to produce in laboratory settings. Typically they included suggestibility, ideosensory and ideomotor activity, catalepsy, age regression, hypermnesia, post-hypnotic responses, analgesias and anesthesias, time distortion, release of inhibitions, ease of fantasy, literalness, and amnesia. What was not acknowledged about these purported hypnotic phenomena—by the researchers of the 1930s or anyone else—was the arbitrariness of the canonical list. On what basis was the selection made? To some extent it seems that bizarreness was one of the criteria, ease of production another, conventional thinking that a priori excluded paranormal phenomena yet another. We find this difficulty exacerbated by the fact that, in the literature of clinical and experimental hypnotic practice, it is often recognized that it is possible to have instances of hypnosis that lack many of the phenomena of the accepted list. This creates the problem of having no consistent basis for choosing which combination of phenomena is sufficient to indicate that hypnosis is present. At the time Barber was writing about these things, there was no discernible debate about the matter, creating the impression that it was a relief to have established the subject matter of research so scientists could get on with the job; this state of affairs still holds true.

There are still more problems around the phenomena. For one, all of the conventionally listed phenomena are ones that also occur in "non-hypnotic" conditions (Gauld 1992:517–536). To add further to the difficulty, they all occur in some form or other in everyday life. It is possible to identify examples of everything from amnesia and anesthesia to positive and negative hallucination in ordinary human experience. The attempt to determine the object of study of hypnosis on the basis of such phenomena has, in my opinion, been the cause of a great deal of confusion. The confusion has led to endless disputes in clinical and experimental settings as to what positions on hypnosis qualify as legitimate. As a result, an examination of hypnotic literature over the past fifty years brings little clarification to the problem of what hypnosis actually is.

If we were to put ourselves in the place of a late 19th-century theorist who held the "special state" view of hypnosis of the time, how would we attempt to definitively separate hypnosis from other states of consciousness? According to Spanos and Chaves, in the 19th and early 20th centuries, the special state view of hypnosis was "sustained by the belief that hypnotic procedures produced *highly unusual behaviors that transcended the capacities of non-hypnotized individuals*" (Spanos & Chaves 1989:10). To illustrate this we might choose some physiologically observable phenomenon that would be strikingly obvious to onlookers. Take, for instance, immunity to pain, such as that described by Esdaile in his accounts of surgical operations (the amputation of limbs, removal of huge tumors, etc.) performed while the subject was in a "mesmeric" state (Esdaile 1846). I have myself witnessed a dentist who, in one session, with the aid of hypnosis alone, pulled six front teeth in the upper jaw of a patient, who experienced no pain, and then stopped the flow of blood with a command. Our hypothesized theorist might say that such feats could only be accomplished while the patient was in a deep state of hypnosis, and that this phenomenon, along with similarly striking phenomena, would serve as infallible indications of the presence of hypnosis. Unfortunately, that would not be the case, for there are many reported incidents of individuals who have not been hypnotized who nevertheless exhibit comparable analgesia. One such was related to me personally. It involved a railway worker who attempted to rescue a co-worker who was in danger of being crushed by a runaway railway car. As he rushed to save his colleague, the car ran over his own foot, severing all his toes. During the incident he felt no pain and had no awareness that his toes had been amputated, until after he had succeeded in pushing his co-worker out of harm's way. This man had not undergone any mesmeric or hypnotic procedure, yet he exhibited what would, in the special state theory, be one of the phenomena uniquely associated with the hypnotic condition. I believe that bizarreness or extraordinariness of phenomena alone cannot provide a way to indicate the presence of the hypnotic state, since even the most astounding hypnotic phenomena may also occur in normal life. As I will discuss below, the extraordinary phenomena that human beings exhibit under hypnosis are not due to an extraordinary state or condition. Rather, the phenomena of hypnosis are the phenomena of everyday human experience. I would have to say, however, that sociocognitive theorists are also in trouble when dealing with extraordinary or bizarre phenomena of this kind, but on different grounds. The notion that the railway worker was enacting a role suggested to him by his interpersonal environment seems to me to leave common sense far behind.

There is much truth in the sociocognitive view that:

> the phenomena of hypnosis that have figured most prominently in the history of hypnosis . . . coalesced into a coherent social role (the role of the hypnotic subject) not because of any intrinsic correlation among these different behaviors, but instead because they were conceptualized as being related in eighteenth- and nineteenth-century special process theories of mesmerism and hypnosis. (Spanos & Chaves 1989:437)

I fully agree that the forms hypnotic phenomena take are conditioned by the socio-interpersonal environment in which they are produced. This situation creates an insuperable problem: There is no basis, no underlying rationale for saying that there is (or could be) some definitive list of hypnotic phenomena which applies to hypnosis and hypnosis alone. However, this is not because there is no identifiable state of hypnosis, as the sociocognitive theorists believe, but because the way of talking about that state has been improperly situated in discussions of hypnotic theory.

The Hypnotic Situation

I would like next to say a few words about what I consider a central issue for preserving the credibility of experimentation in hypnosis. If we recognize, as I believe we must, the crucial role of the social and interpersonal features of hypnosis in both clinical and experimental settings, then there must be some account taken of two aspects of that structure that, to my mind, have not been sufficiently investigated. The first is the fact that the demand characteristics to which the hypnotic subject is responsive are not only those that occur in the laboratory or the consulting room. They are at work forming the individual's expectations long before he or she becomes part of those situations. Subjects are steeped in impressions of the nature of hypnosis and hypnotic phenomena through their encounters with the news media, magazine articles, books on hypnosis, television dramas that involve hypnotic themes, documentaries, opinion programs, conversations with friends and colleagues, etc. The resulting attitudes vary greatly from individual to individual, and no two people can be expected to have the same set of preconceptions.

Clinicians who use hypnosis in their therapeutic work know that they must find out what ideas about hypnosis their hypnotherapy clients bring with them and attempt to correct the usual inevitable fund of false or distorted information they have accumulated. Even when a determined effort is made in that direction by the hypnotherapist, he or she will still find that incorrect notions remain behind and will only be corrected through the client's personal experience over time.

What is true of the clinical setting must be equally true of the experimental.

The notion of "demand characteristics" must be expanded. It seems to me that this state of affairs must be addressed by experimenters, using whatever means might be available to identify these hidden conditionings and either correct them or take them into account in evaluating the results of experiments. Of course experimenters realize that people bring preformed notions of hypnosis and hypnotic responding to the table. But there does not seem to be much evidence that they specifically identify them and make allowance for them in evaluating their findings.

Also, in discussion of the interactions between experimenter and subject, there is much said about the effects of the experimenter on the ideas and expectations of the subject, but little about the effect of the subject on the experimenter. The experimental situation is a living human interaction, and human interactions always go both ways. Freud recognized this fact in his concept of countertransference, and Jung was famous for insisting that the psychotherapist is as much affected by the client as the client is by the therapist. Today the intersubjective school of psychoanalysis explores these two-way effects routinely, and clinicians from other schools of thought are becoming more and more aware of the cogency of this view. Also, it is increasingly acknowledged that these exchanges occur as much on subconscious levels of communication as conscious. If awareness of these factors is part of the routine concerns of experimenters in hypnosis today, I have not come across information to that effect, but I would be relieved to know that this dynamic is routinely taken into account in the laboratory.

Resituating Hypnosis: A Fresh Start

The reason the discussion of hypnosis has reached its present inconclusiveness is that it has taken place within a framework riddled with too many hidden assumptions and unexplored contradictions. What is needed, I believe, is to back up a step and establish a new perspective.

What I am proposing in this article is a fresh start. It is my intention to resituate and redefine hypnosis. The new approach I am suggesting both applies to *all* the phenomena of hypnosis found in its 200-year history and situates hypnosis in the broad context of human experience. I believe that it makes possible a way to discuss and explore hypnosis freed of much of the disorder that has so far prevailed. Barnier and Nash correctly remark about hypnosis that "almost everything flows from definition" (2008:6), and that is where I will start.

The approach I suggest involves seeing hypnosis as a subspecies of *trance* as defined in a very specific way. My definition of *trance* is: a state of intense focus on something, accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources. My

definition of *hypnosis* is: an inner-mind trance characterized by rapport. Both of these definitions require explication.

Trance is an old word, used with a variety of meanings over the centuries. One of its original meanings is a state that involves absorption in something and abstraction from, or obliviousness to, other things. It is a version of this meaning of trance that I employ in this discussion.

Trance involves *intense focus on or absorption in something*. That thing constitutes the center of the mind's attention. The object of focus may be a person, place, thing, situation, idea, feeling, etc.—anything that a person may direct his or her attention to. The focus may be brief or prolonged. Attention may shift from focus to focus in a fluid way or remain fixed for some period of time. This is the first constitutive element of trance.

Of its very nature, focus on something entails a corresponding *diminished awareness of everything else*. The more intense the focus (the more complete the absorption) on something, the more awareness of other things decreases. This is the second constitutive element of trance.

There is a direct relationship between degree of focus and diminishment of attention elsewhere, and the depth of the trance. In the deepest trance, awareness of things not part of the focus approaches the vanishing point.

There is no extinguishment of consciousness in the trance state; in fact, awareness of the object of focus remains constant and can be very vivid. The mistaken notion that consciousness is diminished or disappears in trance states is largely due to the fact that sometimes memory of the trance experience is lost with the change in the object of focus.

Trances are not mysterious, misty, or transcendent states of mind. They are characterized not by diminished but by heightened awareness, at least in the area of concentration.

Trances do not turn people into automatons. In trances individuals do not lose their ability to make their own judgments, although the narrowed awareness of trances may significantly affect those judgments.

The third phase of the trance state, *evocation of appropriate subliminal resources*, occurs automatically. Once the focus is established, the organism immediately responds with the resources that the focus requires. Focus on something calls for action with regard to that thing. Our mental/emotional/biological apparatus is constructed in such a way that input stimuli evoke an action or a response of some kind, and the response may be mental or physiological. In trance, whatever is needed for the action is made available. This is the third constitutive element of trance.

The response to the object of focus is *appropriate*, in the sense of fitting. Appropriateness is determined by the responsive mechanisms of the individual. To the onlooker the response may seem inappropriate, but

for the entranced individual considered as a whole organism the response will be the one that is judged appropriate. The judgment is made on many levels, and the process of making that determination is to a great extent unavailable to consciousness. The response draws upon the individual's physical/biological/emotional/mental resources. These resources have their roots in evolutionary biology, cultural influence, and personal experience and learning. In the manifestations of the resources, the subconscious mind and unconsciously embedded dispositions play a significant role.

Trances are part of everyday life. By this I mean that the notion of trance I am proposing provides a perspective on the entire range of human experience. Everyone is susceptible to trance, except for individuals whose mental state, temporarily or long term, precludes focusing. In the conduct of our affairs, we are constantly shifting from one center of focus to another as we move from one activity to another or one concern to another. Here the state-dependent property of memory comes into play, and we might find it difficult to clearly recall our experience of one state of trance after we have moved on to another.

There are many kinds of trances, depending on the type of object being focused on. For the sake of convenience I have divided trance into four categories: situational trance, interpersonal trance, inner-mind trance, and group-mind trance. Each has a different kind of object of focus, as will be explained. For the moment, I want to call attention to the inner-mind trance. The focus of this trance is the inner world of the mind with its thoughts, ideas, feelings, memories, symbols, impressions, intentions, subconscious dynamics, etc. There are many subcategories of inner-mind trance. The one I want to concentrate on now is hypnosis—which I define as *an inner-mind trance characterized by rapport.*

Hypnosis is in its very nature an interpersonal thing. It involves a hypnotist or trance inducer and a subject. The hypnotist plays a central role. Throughout the duration of the hypnotic state, the subject is aware of the hypnotist. As a matter of fact, the hypnotist is incorporated as an inseparable part of the focus of the subject, and is in this way introduced into the inner world of the subject. This is what is called *hypnotic rapport*—a unique connection between hypnotist and hypnotized. In this role, the hypnotist serves as trance inducer and guide for as long as the person persists in the hypnotic state. To understand the nature of hypnosis it is crucial to identify the role of *rapport* in the hypnotic situation. Rapport is both the means by which suggestion enters into the situation and the reason suggestion in hypnosis is so effective. The incorporation of the person of the hypnotist into the subject's hypnotic focus means that the subject experiences the suggestions of the hypnotist as coming from him or herself. This gives

those suggestions an aura of trustworthiness that opens the subject to those suggestions in a uniquely effective way.

In this schema, there is no need for lists of the "phenomena of hypnosis" to establish the presence of hypnosis. In fact, the notion of "phenomena of hypnosis" as conventionally understood is misleading, for the phenomena of hypnosis actually consist of all phenomena that can occur in human experience. We find in the literature of hypnosis, ancient and modern, the broad recognition of the fact that any and all of the phenomena that occur in hypnosis also can be found in normal human life. The approach proposed here not only allows for this state of affairs, but actually requires it.

I would like to clarify the use of the term *state* in my definition of *trance* in general and *hypnosis* in particular. *State* has come to be used in two different senses: the strong sense, in which causal properties are attributed to the altered condition of mind, and the weak sense in which no causal properties are attributed to it as such. I use the term in the latter sense.

The state of hypnosis is specifically identifiable, not because it manifests conventionally agreed-upon phenomena, but because it exhibits a state of focus, the object of which is the subject's inner mental world, which temporarily includes the hypnotist, accompanied by a diminished awareness of everything else. The resources evoked for the subject are those that pertain to that inner world and allow interacting with it in a way that the subject consciously or unconsciously determines to be useful.

What the subject judges useful may very well be compliance with the expectations of the hypnotist or hypnotic situation. This judgment may escape awareness, but it will be the determinant of what "hypnotic phenomena" occur.

Responses in the state of hypnosis may be experienced as *automatic*, happening without conscious intention. The reason is that the evocation of appropriate resources that occur in this type of trance involves tapping largely unconscious (physiologically based) and subconscious (mentally based) hidden resources. For that reason, the subject does not have access to the source of the judgment that determines what response is called for. The hypnotist, from his or her position of rapport, makes suggestions and the person responds appropriately, but why the response is judged appropriate and what produces the response escapes the subject's awareness. Hypnosis might rightly be considered the most mysterious of trances precisely because it so obviously draws on inner capacities of which the subject has so little knowledge and over which the subject has so little control. The subconscious mind with its peculiar dynamics is still largely unexplored territory.

Weitzenhoffer wrote:

> Too little is known regarding the hypnotic state to allow one to devise induction of hypnosis procedures from scratch with any certainty that they will work. For this reason, the general practice has reasonably been to use procedures that have been known to be most often associated with the production of hypnotic effects. (Weitzenhoffer 2000:13)

It is my belief that induction has been thought of as a chancy matter at least to some degree because the hypnotist did not know precisely what an induction was supposed to do. Using my proposed definition of trance, induction of trance becomes a straightforward matter. Any person, thing, thought, or situation that can create a focus can produce a trance. This simple approach helps clarify the fundamental nature of the induction of hypnotic trance. The destination is focus and diminished awareness, accompanied by the evocation of subliminal resources. The focus of *hypnotic* trance is the inner world, and the induction must provide a way to direct attention there. The possible paths to this destination are limited only by the ingenuity of the hypnotist. The induction process is facilitated by keeping in mind a principle often reiterated by the most ingenious trance inducer of the 20th century, Milton Erickson. It is called his "utilization principle": Make creative use of whatever behavioral patterns or emotional concerns are presented by the individual hypnotic subject. The subject himself or herself will show what will be the most effective focus. This approach to hypnosis induction makes obsolete those often-cited verbal induction patterns that are aimed at the "average" hypnotic subject (Edmonston 1986). It also suggests a reevaluation of those schematized approaches to hypnotic induction used in experimentation with hypnosis.

How might one look at experimentation in hypnosis working within this new definition? The *induction* to be used in the experimentation is very straightforward, built right into the definition: Anything that brings about the state of hypnosis as defined is a valid induction. Establishing that the induction has succeeded and that the subject is in a state of hypnosis involves noting: 1) indications that the subject is in a focused state with diminished awareness of everything but the object of focus; 2) indications that the focus of the subject is his or her inner world; 3) indications that the subject has incorporated the hypnotizer into that focus (is in a state of rapport). Depth of hypnosis depends on the degree of focus on the inner world and corresponding diminution of awareness of everything else.

One of the principal tasks of the experimenter should be to explore the nature and extent of the inner resources that are evoked in the hypnotic state. As already pointed out, those resources will be evoked which the subject consciously or unconsciously finds useful. The particular manifestation of

the resource is a "phenomenon of hypnosis." If the experimenter (consciously or unconsciously) cues the subject about what is expected, that subject may very well comply. So if the experimenter is looking for phenomena found on one of the traditional lists, that is what he is likely to find. On the other hand, if the experimenter wants to clearly direct the experiment toward the production of a specifically chosen phenomenon of interest, he will be able to do so. In the absence of such direction, spontaneous phenomena will occur, such as a memory, a symbol, a feeling, a perspective, or one or the other traditionally occurring phenomena, possibly including "paranormal" phenomena.

To sum up, the concept of *trance* offers a specific perspective on all the phenomena of human experience; the concept of *hypnosis* applies that perspective to a particular kind of trance—an inner-mind trance with rapport. Hypnosis has come to be seen by many as mysterious, even undefinable. The reason is that trance states, including hypnosis, can tap the full, incredibly rich spectrum of inner resources available to all human beings. So if there is a mystery here, it lies not in the trance state itself, but in the unfathomable depth of human capacity that is revealed in the trance.

Four Categories of Trance

Over the years, to clarify things for myself, I have devised four categories of trance (Crabtree 1997). The choice of these categories is my own, and I do not claim that they constitute the best possible way to distinguish the various types of trance. The division is not based on theoretical grounds, but empirical and practical ones. Categories are assigned in terms of *the various kinds of objects of focus*. It could well be that there should be more categories than four, or that distinctions between them should run along different lines. I include these categories in this article principally to provide an opportunity to present examples of what I mean by trance states in everyday life.

Situational Trance: Here the focus is some situation, activity, project, or action. Reading a book, threading a needle, acting in a play, and teaching a class are examples of this kind of trance. The famous Russian ballet dancer Vaslav Nijinsky, considered by some the best of the 20th century, wrote that when he performed he was in a trance (Nijinsky 1937:49). Pablo Picasso described his state when concentrating on painting as a trance. Speaking about painting in the illumination of a spotlight at night, he said, "There must be darkness everywhere except on the canvas, so that the painter becomes hypnotized by his own work and paints almost as though he were in a trance" (Gilot & Lake 1964:116–117). Athletes performing in the "zone"

are in a state of focus that well exemplifies situational trance. Our daily lives are interweaving tapestries of situational trances. From making coffee in the morning to planning a home renovation in the evening, from writing a paper to correcting an exam, situational trances continually manifest. We are evolutionarily equipped to flow with relative ease from one situational focus to another.

Interpersonal Trance: Interpersonal trances involve relationships between persons. All personal relationships are trances insofar as they entail mutual concentration and focus. The focus is on the other person, things connected to that person, and the interpersonal interaction that occurs. The interpersonal trance is experienced intermittently as the persons involved interact with each other. Intensity of relating varies and so interpersonal trances vary in depth. The most absorbing and the most meaningful relationships establish deep trances, while passing relationships involve light trances. From the relatively light trance of a trivial conversation with a friend to the deep trance of lovemaking, interpersonal trances are found everywhere in ordinary human interactions.

Group-Mind Trance: Group-mind trances involve focus on the social dynamics of experience. A group mind is what results when many individuals gather together and focus on one idea or activity. It embodies the ideas, emotions, intentions, and values that characterize the group. Once constituted, it exerts an influence over its members that is to some extent consciously identifiable, but to a greater extent exercised through subconscious interactions. The influence is on both the thinking and acting of the members. Sometimes that influence induces thoughts and actions out of character for the individual members when separate from the group. Striving to maintain one's own thinking and values in a group context can be very difficult. Group-mind trances involve individuals becoming absorbed in the group thinking and attitudes, and experiencing a diminished awareness of their thinking and attitudes in other contexts. Examples of more enduring group-mind trance situations are the family, the church, the staff, the corporation, and, in the broadest manifestation, the culture at large. Examples of temporary group-mind trances are rock concert audiences, soccer crowds, and lynch mobs.

Inner-Mind Trance: The inner world is where you go when you close your eyes and think about or imagine something. This trance provides access to a broad variety of inner experiences. The inner world is always available and its exploration always a possibility. An inner-mind trance involves focus on the arena of inner mental and emotional richness, and diminished awareness of the outer environment. Inner-mind trances take up a great deal of space in our everyday lives, and appear in the form of everything from

worry to meditation, from reverie to dreaming. Hypnosis is a special kind of inner-mind trance, for it involves not only focus on the inner world, but also rapport, a special connection that incorporates the hypnotist into the focus. An inner-mind trance *without* rapport should not be called hypnosis at all. For that reason, the notion of *self-hypnosis* makes sense only if the incorporation of the hypnotist into the inner world that occurred in previous hypnotic sessions stays with the person in memory with sufficient strength that it can be reestablished in the imagination when the person attempts hypnosis alone. This would amount to the establishment of rapport with an absent person. This concept was already developed in the writing of the Marquis de Puységur in his therapeutic work with his young client Alexandre (Crabtree 1993:79–82). Rapport with an absent person may also be used as a model for understanding neurotic attachments.

Evocation of Appropriate Subliminal Resources

Key to my proposal about hypnosis is the notion that in trance states there is an automatic evocation of resources that the individual possesses but that lie latent within until focus on the trance object mobilizes them. I use the word "subliminal" in the sense intended by Frederic Myers (sub-*limen*—below the threshold of consciousness) in his discussion of the "subliminal self" (Kelly, Kelly, Crabtree, Gauld, Grosso, & Greyson 2007:577–607). The evoked resources are appropriate to deal with the object of the trance focus. This state of affairs holds for all trances, including the trance of hypnosis. Evoked responses arise infallibly and immediately once a focus is achieved.

By "appropriate" I mean those resources which, from the subject's perspective, are needed to deal with the focus at hand. "Appropriate" does not necessarily mean "best." Others may well believe that what is evoked in the subject is not the best possible response. Whether it is "best" or not is irrelevant to what I mean by appropriate. Appropriateness is determined by the subject's particular understanding of the unique circumstances of this particular moment, responding from the conditionings that are currently in place. For that reason, a "neurotic" response, for example, may be "appropriate" because as the person is now constituted, consciously and subconsciously, that is what the organism as a whole judges to be fitting. As mentioned, this judgment largely escapes conscious awareness.

What accounts for this phenomenon? Why is it that the appropriate responses are inevitably brought into play? I believe we are constellated in such a way that as soon as we perceive something, we are stirred to action of some sort with regard to that thing. We are built for action; we attend to something and we seek some way to interact with that thing. Versions of

this view are found in the writings of such philosophers as William James (1890) and Henri Bergson (1912).

Responses may be physiological, emotional, or mental. The resources available to be tapped may be placed in six categories: 1) genetically embedded responses, 2) overlearned habits, 3) unconscious connections to surrounding reality, 4) dynamic subconscious mental/emotional resources, 5) preconscious memories, and 6) something that might be called the creative faculty. All six are present in all trances to some degree. As already pointed out, conscious intention has some part to play in deciding which resources are evoked, but the decision occurs mostly outside conscious awareness.

For example, if my focus is on *designing* a wooden bed, my creative imagination supplies me with a series of possible structures to consider, my memory provides information about its optimal dimensions and the stresses to which it will be subjected, and drafting skills, learned long ago and now become habitual, come forward to aid with my sketches. If I subsequently focus on *building* that bed, a different array of responses comes into play. I immediately have access to a variety of overlearned habits relating to using carpentry tools. I retrieve memories of the design I had arrived at. I imagine modifications that need to be made as I fabricate the parts that I will assemble. The more intensely I concentrate on this task, the more efficiently each resource becomes available and the more skilled my work.

Moving from the ordinary to the extraordinary in evoked resources, I would like to mention evoked subliminal resources that relate to anomalous phenomena. From the first instances of magnetic somnambulism to contemporary hypnotic practice, there have been frequent reports of anomalous phenomena occurring in the trance state. I will here limit the present discussion of this type of phenomena to instances of what are called paranormal phenomena: telepathy, clairvoyance, precognition, and psychokinesis. The reality of these phenomena is, of course, an empirical question, and their genuineness is demonstrated by applying the same scientific criteria that are used for all investigated phenomena. It is my opinion that the existence of these phenomena has been adequately demonstrated in the abundant relevant literature of the past two hundred years. I will not go into that question in detail here, but would refer the reader to *Irreducible Mind* (Kelly et al. 2007) as a starting point for studying the most relevant research in this area. For the purposes of this article, I am going to accept that anomalous phenomena sometimes occur for individuals in trance states. Given that the phenomena are genuine, it is sufficient to say that they point to a particular type of subliminal resource that may be evoked in a variety of situations. Virtually all these situations involve formal or informal trance

inductions. That trance states may evoke paranormal phenomena follows naturally from the proposed definition, for if paranormal abilities exist, they are going to be experienced in some kind of focused state. However, just how it comes about that the subliminal self possesses this resource and how it is able to mobilize it are yet to be discovered.

There is no question that hypnosis proved a uniquely effective access to certain types of subliminal resources, as the history of animal magnetism and hypnosis testifies. Hypnosis is also a form of access that can be reliably brought about through specifically identifiable means, thereby making those resources available to systematic study. It follows that one of the principal tasks of experimentation with hypnosis is to seek out the psychological, neurological, and biological concomitants to the evocation of these resources.

Here I would simply like to reiterate my belief that the real mystery of hypnosis is not the state or condition or process that may be involved, but how it is that human beings are capable of producing the type of phenomena that have been conventionally associated with hypnosis over the past two centuries. The state or condition or process we call hypnosis does not of itself provide the answer to this question, for these same phenomena—all of them—can be seen to occur in the absence of hypnosis. So, as it turns out, the phenomena of hypnosis are the phenomena of life, and Orne's belief that hypnosis does not enable subjects to transcend normal limits of human performance is true, because normal human performance includes the most extraordinary things.

Trances as Universal Experiences

Weitzenhoffer, describing the field of hypnosis as "chaotic," said that "part of the problem lies in the ubiquity of the slippery state of hypnosis" (Weitzenhoffer 2000:8). He knew that anyone who defines hypnosis by attempting to specify it in terms of unique phenomena runs into serious problems, for all of the phenomena of hypnosis are found to occur *naturally*. The only way around this, he believed, was to emphasize the *artificial* nature of hypnosis, and make that its defining feature. However, defining artificiality and specifying what that means with regard to hypnosis entails many problems of its own.

The problem identified by Weitzenhoffer is removed by resituating hypnosis as a trance, as I have defined it. As already mentioned, the understanding of trance states I am proposing here has as one of its consequences that trance states are a normal part of life, that they are in play in every type of human experience. This means that all of us are familiar with them in practice, even though we may not have explicitly recognized their

place in our lives. That is why deliberately induced trances, such as hypnosis, are ordinarily not experienced by the subject as particularly alien or strange.

Although trances may manifest in a simple form, in most of life's situations we are involved with *clusters* of trances, each with an identifiable focus, and each focus possessing meaningful relationships to the others—in other words, a cluster is a unified, cohesive grouping of trance states. The sub-foci of a cluster are related to one another by the fact that there is a larger focus, a palpable unity that characterizes the cluster as such and holds together the sub-foci (as illustrated in my bed-building example). These sub-foci each have an important part to play, contributing in their own unique way to the one larger focus. Some trance clusters become stable and difficult to disrupt. Typically clusters come and go, but normally a well-established trance cluster, one that has become habitual, can be fairly easily reconstituted as needed. Nevertheless, most clusters do alter over time. An example of such a cluster is the grouping and flowing interactions of trance states brought to bear in teaching a class on a particular subject.

There are larger groupings of clusters which have their own unity and coherence that tend to persist over time. Such groupings may be called *constellations*—identifiable groups of related members. The most familiar of these constellations is what might be called the baseline normal consciousness of daily life. This is the grouping of all those trances and clusters of trances that habitually come and go in day-to-day living. We are familiar with them and are not surprised when they appear and disappear within our field of consciousness.

This baseline constellation is difficult to disrupt, having become stable through frequent use and the familiarity of its clusters. The reason the same clusters tend to recur is their practical usefulness, embodying, as they do, tried and true ways to get along in the world. There is a definite feeling of dependability about trances within the normal-consciousness constellation, and consequently a certain sense of security when that constellation is in play.

This everyday-life constellation is made up of elements of all four types of trance. These trances are experienced as "ordinary" and "normal." This judgment is made on the basis of criteria derived from a combination of our culturally formed beliefs about what is normal, and beliefs we arrive at through personal experience. People who live within the same cultural context tend to develop similar everyday-life trance constellations. Because this constellation is by far the most familiar one, and the most stable, it may fittingly be called our *normal* constellation, the one against which we measure all others. An example of another kind of trance constellation is our dream world.

Our normal constellation constitutes the fabric of ordinary life and we thrive within its familiarity and stability. Although we may at times move off into other constellations, we know sooner or later we will find ourselves back at this one. It is the home base to which we inevitably return after trips away. But what about those trips? What about those other groupings of trances and clusters that we occasionally visit? These non-ordinary groupings are what Charles Tart popularized under the name "altered states of consciousness" (see Tart 1969), and in the heady psychedelic days of the 1960s they were indeed referred to as "trips." They are "altered" states in that these trances and clusters differ from those of the baseline, normal, everyday constellation of trances.

In the conduct of normal life, we experience a certain ease and fluidity as we move from focus to focus, from trance to trance, and from cluster to cluster. Why that fluidity is possible is an important question. We find a clue to the answer in the exposition of the concept of focus and fringe in the writing of William James. In James's view, we become aware of things in such a way that each object of experience has a center of attention, or focus, and a fringe of which we are only dimly conscious. He speaks of that fringe as a "psychic overtone" that gives us a sense of relations that exist beyond the central focus of attention. The concept of "relations" is critical here. In James's philosophy of radical empiricism, relations between the things we perceive are as real as the things themselves. These relations are not added by the mind, but exist apart from our perception of them. He wrote,

> Of most of its [the object's] relations we are only aware in the penumbral nascent way of a "fringe" of unarticulated affinities about it. (James 1890:I:259)

It is precisely because of this "penumbral nascent" awareness that we can form a sense of *where to go next*, what new focus to move toward, as we live our daily lives. In perceiving the relations attached to the object of our attention, we have a sense of the way it is connected to all other things. This way of perceiving allows us to shift easily and, for the most part, appropriately from one center of attention or focus to the next, all with little or no conscious awareness of why we are making that move.

Contributors

It is a truism that in the evolution of ideas in human culture no "new" idea is really new, and that progress occurs only when previous progress is incorporated into the new. The new is really a new perspective on what is already given. This is certainly true of my proposal about hypnosis.

All of the elements of my proposal can be found in the rich literary tradition of hypnosis. What is new is the combination of those elements and the perspective under which they are viewed. Most significantly, the situation of hypnosis, as I define it, in the broader context of trance, as I define it, is, I believe, new, as is my take on the notion of evocation of appropriate subliminal resources. It also seems to me that the resulting altered view of the "phenomena of hypnosis" and of hypnotic induction offers something new.

I would like to say a word about the influences that have affected my thinking on these matters. First of all, I have been instructed by my psychotherapy clients, with whom I have used hypnosis over the past forty years, as well as by my own personal experience of hypnotherapy.

Beyond these concrete experiences, I have been greatly assisted in my attempts to rationalize hypnosis and hypnotic practice by many researchers in the field. In the early 1990s, when I was looking for a way to talk about hypnosis that overcame the ambiguity and confusion found everywhere in the literature, in desperation I opened *Webster's Collegiate Dictionary* and looked under the entry "trance." It said, among other things, that a trance is a state of *abstraction* or *absorption*. I thought that it would make sense to combine both aspects and see trance as a state of absorption in something and abstraction from everything else.

Shortly after this, I was reading Milton Erickson's account of his hypnotic work with a woman who had come to seek his help with pain. He described speaking intently with her about her pain and focusing her attention more and more on describing that pain in detail, and on his words to her. He got her to sit and notice every aspect of her pain and describe it. He said that he believed that if his secretary would have come into the room and played the drums, the woman would not have noticed. Yet she would notice immediately if he rustled a piece of paper or looked at his watch. She was extremely aware of everything he did and said, but totally unaware of everything else in the environment. Then he said,

> As far as I was concerned, as far as the therapeutic situation was concerned, this woman was in an utterly light trance. But in relation to alien reality, to *irrelevant* reality, she was in a very profound trance because she was so completely inattentive to it. (Erickson 1983:111)

Reading this account, I was put in mind of the definition of trance I had worked out—absorption and obliviousness—and I realized that Erickson's notion of trance was exactly that. The only difference was that he concentrated on the inattentiveness or obliviousness of trance, whereas I believed that the

absorption aspect was also part of the meaning of trance. So I would have said that this woman was in a profound trance which had as its focus her attention to her pain and to Erickson's suggestions, and as its obliviousness everything else in the environment. So she was not in *two* trances at once—a light and a deep one—but *one* deep one. This important insight, gained from Erickson's story, solidified my thinking and provided the starting point for my perspective on hypnosis from that time on.

I liked the idea of using *trance* as the key to understanding hypnosis. It allowed discussion of hypnosis to occur relatively free from the baggage that it had acquired over the previous one hundred fifty years. It also provided a broader context in which to situate hypnosis—as a subspecies of trance. I noticed that Erickson had a predilection for the word *trance*, and wondered if perhaps he wanted to use this more venerable term (going back to at least the 15th century) to evoke a more open-ended approach to understanding hypnosis.

Many years before my attempt to resituate hypnosis, I had studied and written about the history of animal magnetism (mesmerism) and early hypnotism (Crabtree 1988, 1993). The work of the Marquis de Puységur had struck me as revolutionary and a development of Mesmer's vision far beyond anything Mesmer ever dreamed of, into the realm of the psychological. Puységur discovered artificial somnambulism, which he called "magnetic sleep," and the development of his original insights eventually led to the psychodynamic understanding of the human psyche that made possible our modern psychotherapy of the subconscious (Crabtree 2003). One of the things Puységur insisted on was paying attention to the insights exhibited by subjects in the somnambulistic state. He believed that somnambulists were able to tap an inner knowledge relating to disease and healing that was totally reliable. This was the first hint in the mesmeric–hypnotic literature of the remarkable inner resources that reside within human beings outside normal awareness, what James called "beyond the margin" of ordinary consciousness (see Crabtree 1993:116–119).

A hundred years after Puységur, Frederic Myers took this concept to its ultimate conclusion in writing, in the 1890s, about the "subliminal self," the region "below the threshold" of consciousness, which is the font of the most remarkable human capacities. Myers's vision (see Kelly et al. 2007:66–97) influenced my thinking in developing the notion of "evoked subliminal resources." In the same period, William James published his *Principles of Psychology*. Among other contributions to my thinking, his ideas about "focus and fringe" in perception helped me to understand the way we effortlessly shift from trance to trance in daily life.

There are a number of modern authors in the literature of hypnosis

who have shaped my thought. T. X. Barber insisted that experiments must begin with the data—the phenomena of hypnosis. He attempted to lay out what these phenomena were in terms of those mentioned in the literature. Thinking about this way of specifying the phenomena, I realized that any such specification was patently arbitrary and put thinking about hypnosis on the wrong footing. I felt that the sociocognitive school of thought came nearer to the truth. The sociocognitive perspective on hypnosis recognized that the forms hypnotic phenomena take are, and always have been, determined by social and interpersonal expectancies, and attempts on the part of hypnotic subjects to fulfill them. This calls into question every list of the "phenomena of hypnosis" from every period of its history. Conventionally accepted lists are made up of phenomena that were expected by the researchers and practitioners of mesmerism and hypnosis of the era, and could not provide a basis for a "definition" of hypnosis. The nature of hypnosis cannot be grasped in terms of such lists.

However, sociocognitive theorists insisted that hypnosis has no "nature" at all, in the sense of being a specified state of the human psyche that could be distinguished from other states. In this, I believe, they were mistaken. I certainly agree that hypnosis is not a "thing," and that the phenomena of hypnosis are socially molded. Nevertheless there is some recurring reality that can be legitimately named (we have called it hypnosis), that manifests a potential that evolution has embedded in the very constitution of human beings, and that is not a purely socially conditioned complex. Whatever culturally conditioned forms the phenomena may take, they manifest something consistent and enduring. Hypnosis is not an evanescent mist of relations, but something that actually has some substance. It is that substantial reality that intrigues everyone who engages in hypnosis research.

Conclusion

The discussion of hypnosis over the many years of its history has resulted in a state of affairs that has diminished the usefulness of the term. It has become a discussion not of an identifiable state or condition, but of an arbitrarily selected and constantly shifting group of "phenomena" which are said to be associated with an undefinable something, "hypnosis," which seems to have a kind of phantom existence. What is needed is to bring a new perspective to bear, one that will provide us with a larger context which will allow us to make further progress in exploring hypnosis. That context is, for me, based on an understanding of trance. Trance is susceptible to a practical definition in a way that hypnosis as commonly conceived has not been. In this wider context, a new situating of hypnosis can occur that allows for its meaningful definition and a reinvigorated approach to its study.

In the end, as with all theorizing, the value of my proposal for understanding trance and hypnosis will be determined by the empirical evidence. The best theory is one that is consistent within itself, is not unnecessarily complex, does not contradict well-established principles in related fields of research (in this case, for example, neurology), and applies to the broad spectrum of the data found in the literature. I believe my understanding of trance and hypnosis qualifies and actually fits the data better than approaches previously used.

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COMMENTARY

A Proposal That Does Not Advance Our Understanding of Hypnosis

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Abstract—In his paper Hypnosis Reconsidered, Resituated, and Redefined (*JSE 26*(1):297–327), Adam Crabtree, a distinguished expert in the history of hypnosis, maintains that contemporary hypnosis research suffers from conceptual disorder. In his words, he attempts to redefine hypnosis in order to provide a stronger ground for future research. We find that his proposed reconsideration of hypnosis as a form of "trance" characterized by a focus on internal stimuli and involving the recruitment of appropriate subliminal resources is neither novel nor helpful to our current understanding of hypnosis. Among the problems we find with his paper is that it is conceptually unclear and is not informed by current research and theory; for instance, it disregards well-established findings such as individual differences in responsiveness to hypnosis and the importance of suggestion for the elicitation of hypnotic phenomena. Historical knowledge of a field is invaluable but is alone insufficient to understand its current status or propose pathways for future research and theory.

Keywords: hypnosis—trance—suggestion

Adam Crabtree is a distinguished historian of hypnosis who has made a number of important contributions to this area including a thorough bibliography of early research on animal magnetism, hypnosis, and putative psi phenomena (Crabtree 1988, see also Dingwall 1967–1968), and a scholarly work on the history of animal magnetism (Crabtree 1993). We believe that knowledge of a field's history can greatly aid one's understanding of contemporary issues and controversies (for an excellent example see Laurence and Perry 1988), and it is with this in mind that we looked forward to Crabtree's (2012) proposal on the need to redefine

hypnosis. However, his historical expertise covering up to the first half of the 20th century does not serve him well when commenting on the current status of hypnosis theory and research. Indeed, we are troubled that Crabtree by and large did not attempt to integrate his musings with contemporary research and theory. In what follows we describe our principal concerns with his ideas and outline why his proposal does not advance our current understanding of hypnosis.

History of Hypnosis

Before delving into the substance of his proposal for a new definition of hypnosis, a couple of comments on his summary of the history of hypnosis are worthy of brief mention. By necessity, Crabtree had to limit his coverage of historical issues, but it is worth reminding the reader of this *Journal* that probably the first examples of controlled, masked trials to evaluate a clinical treatment occurred in the context of testing Mesmer's theory of animal magnetism (Best 2004). One of them consisted in "magnetizing" one of the five trees in Benjamin Franklin's garden. A susceptible patient was brought to the line of trees and promptly became magnetized when in the vicinity of the "wrong" (control) trees. This simple experiment demonstrated that the individual's own beliefs and expectations, and not the putative magnetic fluid, caused his responses. More than 200 years later there are still researchers of complementary and alternative treatments and similar phenomena who fail to include basic controls for demand characteristics and experimenter and placebo effects.

The second comment involves an imprecise account of one of Martin T. Orne's contributions to hypnosis. His development of the simulating control group, in which unhypnotizable participants are instructed to feign the role of a "hypnotized" person, was not to identify "the genuine presence of hypnosis, as opposed to simulation" (Crabtree 2012:302), but

> to recognize which aspects of a S's response, if any, were due to hypnosis, as opposed to those that were the result of a combination of the S's prior knowledge and expectations in conjunction with cues provided by the situation. (Orne 1979:523)

Trance

In his proposal Crabtree seeks the "essence" of hypnosis, an attempt that has eminent precedents (e.g., Orne 1959, Weitzenhoffer 1980). His central thesis is that contemporary hypnosis research is in a state of disorder because of unresolved conceptual issues regarding the way in which hypnosis is defined. We firmly disagree that the field is best characterized

in this way, and we note that considerable progress has been made in integrating the positions of rival factions in the hypnosis community (e.g., Lynn & Green 2011). Crabtree sets himself the goal of overcoming the supposed disorder in the field by reconceptualizing hypnosis. The core of his proposal is to define hypnosis "as a subspecies of trance . . . a state of intense focus on something, accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources" (Crabtree 2012:311). There are many problems with his adoption of the word *trance* and with his specious claim that this account is somewhat novel. For the sake of brevity, we address only the most problematic. First, with regard to the statement that his definition constitutes a "fresh start," the idea that hypnosis involves focusing on something and disregarding everything else harkens back at least to Braid's 19th century theory of *monoideism* (Crabtree 1993). Closer to our time, Barber defined hypnosis as a situation in which individuals are purposefully guided by carefully chosen words and communications (suggestions) to "let go" of extraneous concerns and to feel–remember–think–imagine–experience ideas or events that they are rarely asked to experience. (Barber 1984:69). However, an important distinction between Barber's and Crabtree's definitions is that the former includes suggestions about *experiencing* unusual events (see also Tellegen 1981), whereas Crabtree opines that "Trances are part of everyday life" (Crabtree 2012:313).

As for the use of the term *trance*, one of us looked at the various senses of the word in the *Oxford English Dictionary* and concluded that a term that is used to refer to unresponsiveness to stimulation, sleeplike states, spirit possession, ecstasy, dread, and other phenomena muddies rather than clears the conceptual waters (Cardeña & Krippner 2010). This explains why many if not most current hypnosis researchers and theoreticians avoid using the term. It is somewhat ironic that despite stating that theory should be based on research, Crabtree makes no effort to integrate his work with recent attempts to operationalize "trance" during hypnosis using self-report measures (e.g., Pekala & Kumar 2007). Although we have reservations about such endeavors (see Terhune & Cardeña 2010), Crabtree's proposal would have carried greater weight if it were at least grounded in this research.

Leaving aside the issue of adopting a very vague term, Crabtree defines trance as "intense focus or absorption in something" which in the case of hypnosis is "an inner-mind trance characterized by rapport" (Crabtree 2012:313). There is a substantial literature on the construct of absorption (Tellegen & Atkinson 1974; for a recent review, see Ott 2007), defined as openness to absorbing and self-altering experiences, and which seems to involve at least two orthogonal dimensions: a processing or narrowing of

attention, and a focus on "internal" or "external" foci (Tellegen 1992). The research on absorption has provided a more nuanced and rigorous account of the relationship between absorption and hypnotic responding than Crabtree's, so it is unfortunate that he did not seek to improve his account by further developing the ideas of Tellegen and others. Nonetheless, we can ask whether absorption is related, and all there is, to hypnosis, and whether hypnosis only involves a narrow, internal focus.

Regarding the first question, research has shown that absorption correlates mildly to moderately with responsiveness to hypnotic suggestions (Roche & McConkey 1990, see also Council & Green 2004); thus, absorption cannot be all there is to hypnosis. There is also some experimental research linking attentional abilities with hypnosis (Crawford, Brown, & Moon 1993, Egner & Raz 2007), but again those abilities do not wholly explain hypnosis, and highly hypnotizable individuals vary considerably in their attentional state following a hypnotic induction (Terhune, Cardeña, & Lindgren 2011a). As for the second question, Crabtree disregards the use of, and research on, hyperempiric and other hypnotic induction techniques that actually emphasize focusing on "external" stimuli (Cardeña, Alarcón, Capafons, & Bayot, 1998, Gibbons 1976). Moreover, responding to certain hypnotic suggestions (e.g., positive visual hallucinations) will require attention to exogenous stimuli. Thus, the *direction* of attention is not necessarily as important as Crabtree assumes. Finally, Jamieson and Woody (2007) make the case that, contra Crabtree, states of absorption may reflect poorer, rather than superior, attention.

What Ever Happened to Suggestion and Individual Differences?

When it comes to hypnotic responding, we find Crabtree's account both confusing and confused since he neglects the absolutely fundamental roles of suggestion and individual differences in responsiveness to suggestions. We address these omissions in turn. Crabtree repeatedly refers to hypnotic phenomena and responses, but never actually mentions the types of responses to which he is referring. Following a hypnotic induction, there are two distinct types of responses that are fundamentally different, those that are spontaneous (Cardeña 2005, Pekala & Kumar 2007) and those that are suggested (Woody & Barnier 2008). Crabtree throughout his paper confounds these two types of responses. It is well-established that a hypnotic induction, even one with minimal suggestions (Cardeña 2005), can produce a wide variety of spontaneous experiences such as alterations in body image, temporal perception, and affect, and that responses vary qualitatively according to the level of hypnotizability (Cardeña, Lehmann, Jönsson, Terhune, & Faber 2007, Pekala & Kumar 2007).

The second, and better-studied, type of response is that which follows a suggestion for some type of motor, perceptual, or cognitive–emotional change. Suggestion is what enables a whole host of hypnotic phenomena and is almost completely neglected in Crabtree's account. How is it that absorption facilitates responses to suggestions? Why are high hypnotizables also highly responsive to suggestions outside of a hypnotic context (Braffman & Kirsch 1999)? These are questions of fundamental importance that are ignored.

Relatedly, there is no discussion in his proposal of individual differences in responsiveness to hypnosis. Individuals vary in both their spontaneous response to a hypnotic induction (Pekala & Kumar 2007) as well as to hypnotic suggestions (Woody & Barnier 2008), with approximately 10%–15% of the population meeting criteria for high hypnotizability (McConkey & Barnier 2004). Individual differences in responsiveness to hypnosis have been recognized at least since Faria (1819) and systematically researched since the early 20th century (Laurence, Beaulieu-Prévost, & du Chéné 2008). Today, that there are vast individual differences in hypnotizability is the most widely recognized fact of hypnotic responding. Crabtree mistakenly refers to suggestibility as one of many hypnotic phenomena rather than as a fundamental source of variegation in response to hypnosis. His failures to acknowledge individual differences in responsiveness to hypnosis or the role of an ability underlying individual differences evidence his disconnection from current research and theory on hypnosis.

Furthermore, Crabtree places emphasis on the role of interpersonal context but neglects variability across contexts. For instance, he maintains that hypnosis is a single thing and makes the mistake of assuming that "[w]hat is true of the clinical setting must be equally true of the experimental" (Crabtree 2012:310). This is an oversimplification. Patients are often more motivated than research participants and the dynamics present in clinical settings differ greatly from those of the typical research setting. This helps explain why the correlation between hypnotizability and treatment success is only moderate (r = .44, Flammer & Bongartz 2003).

Crabtree also promulgates ideas regarding the effect of an induction that are not accepted by experimental researchers. For instance, he writes: "[h]ere the state-dependent property of memory comes into play, and we might find it difficult to clearly recall our experience of one state of trance after we have moved on to another (Crabtree 2012:313)." This statement seems to be based only on the author's intuitions and is completely at odds with the finding that spontaneous posthypnotic amnesia is extremely rare (Hilgard & Cooper 1965), even among high dissociative, highly hypnotizable individuals (Terhune, Cardeña, & Lindgren 2011b).

Crabtree elsewhere expresses ideas regarding hypnosis that further demonstrate a fundamental misunderstanding about some of its most basic aspects. For example, he writes that the development of rapport facilitates an experience wherein "the subject experiences the suggestions of the hypnotist as coming from him or herself" (Crabtree 2012:313). Rather, the opposite is the case. Participants experience the suggestions as coming from the hypnotist; this, in turn, produces the extra-volitional phenomenology of hypnotic responding—the experience that the responses are controlled by an external agent rather than by the person him/herself (Bowers 1981, Spanos & Gorassini 1984). He also claims that Ericksonian-type tailored suggestions should make us reevaluate schematized approaches to hypnosis although there is no empirical support for his claim (Matthews, Conti, & Starr 1999). Finally, he asserts that his four categories of trance are empirical (Crabtree 2012:316), but provides no evidence for this assertion.

More Comprehensive Theoretical Models

Crabtree rightly considers that an interpersonal dimension is an essential part of hypnosis but does not develop the idea very much. In contrast, in 1962 in the context of his and others' dissatisfaction with unidimensional theories of hypnosis, Ronald Shor argued that there are three dimensions of hypnosis that include cognitive, emotional, and cultural processes: hypnotic role-taking involvement (e.g., conscious and unconscious personal and cultural expectations), trance (i.e. alterations in conscious experience), and archaic involvement (i.e. the influence of the socio–emotional history of the individual with significant others on his or her response to the hypnotist) (Shor 1962). He further explicated his notion of "trance" as involving a fading of the "generalized reality orientation," which is consistent with Barber's later definition of hypnosis and goes farther than just the mention of focusing of attention with disregard of other concerns.

A similar three-dimensional model of hypnosis was advanced by Brown and Fromm (1986). Specifically, they emphasized the roles of an altered state of consciousness, specific hypnotic suggestions, and expectation/suggestibility. The aspects delineated by Shor and by Brown and Fromm have produced a substantial amount of theory and empirical work in hypnosis, although transferential and countertransferential issues have received only scant attention (but see Nash & Spinler 1989). For instance, just with regard to the experiential domain, Cardeña and Spiegel (1991) discussed three basic phenomena: increased absorption, a sense of automaticity, and spontaneous alterations of consciousness (see also Cardeña 2005, Pekala & Kumar 2007). Thus, a number of more comprehensive and sophisticated models than that advanced by Crabtree have been proposed for almost 50 years, not to

mention other ones involving response sets, dissociated control, and so on.

More recently, increasing emphasis has been placed on the idea that even among high hypnotizables (contra Crabtree there are substantial behavioral, experiential, and physiological differences in the hypnotizability of individuals) there are two or three different subtypes (Barber 1999, King & Council 1998, Terhune, Cardeña, & Lindgren 2011a) and/or different componential abilities underlying different hypnotic responses (Woody, Barnier, & McConkey 2005).

Crabtree also disregards recent theoretical work on the role of unconscious processes in hypnotic responding in favor of vague speculations. He argues that hypnosis evokes "subliminal resources" (Crabtree 2012:318) but provides no evidence, nor does he fully explicate what he means by this phrase. A better approach would have been to relate his work to the recent proposal that hypnotic responses are supported by unconscious intentions (Dienes & Perner 2007).

Another fundamental problem with Crabtree's proposal is that he extends so much his concept of trance ("trance states . . . are in play in every type of human experience" [Crabtree 2012:320] and in "All personal relationships" [Crabtree 2012:317]) that he ends up dissolving hypnotic phenomena into irrelevance. If they are an aspect of every experience, there is no reason to even suppose that there is a distinct domain of hypnotic phenomena, and there is nothing special about them (contrary to the observation of *unusual* phenomena that have made the study of hypnosis tantalizing). Of course, this does not deny the fact that episodes of absorption occur in everyday life, but that is not the same as stating that trance states are part of every human experience (which logically would mean that we are perpetually in "trance," whatever that means), or that absorption experiences are all there is to hypnosis. Crabtree's ideas are unhelpful and at odds with our current understanding of hypnosis and altered states of consciousness more generally (e.g., Cardeña & Winkelman 2011, Vaitl, Birbaumer, Gruzelier, Jamieson, Kotchoubey, et al. 2005).

Then there is the unfalsifiability problem of his notion that trance evokes "appropriate subliminal resources" (Crabtree 2012:35). One could object that precisely the propensity to inappropriately enter hypnotic–dissociative states helps explain in part post-traumatic and dissociative symptomatology (see Cardeña, Butler, Spiegel, & Reijman 2012), but Crabtree has a reply in that even "neurotic" responses can be considered appropriate from the individual's conscious or unconscious motivations. Thus, his proposal becomes unfalsifiable because one could always envisage some type of "unconscious" rationale, and because it preempts consideration that there are likely a number of processes evoked by hypnosis, some of which are

appropriate and others not (e.g., negative emotional sequelae following particular hypnotic suggestions, which may be caused by associations with previous negative stimuli, see Cardeña & Terhune 2009).

Finally, Crabtree also makes a categorical mistake by stating that unconscious processes are "physiologically" based whereas subconscious processes are "mentally based." By definition, subconscious processes are those that are below the level of conscious awareness, and are thus unconscious, and we expect mental processes to have both physiological and mental substrates (e.g., neurophysiological responses to sub-threshold meaningful stimulus).

Conclusion

Despite his undeniable contributions to the history of mesmerism, hypnosis, and their relation to psychical research (Crabtree 1988, 1993), we do not think that Crabtree's proposal adds helpful new ideas or advances to the study of hypnosis. Rather, it actually does a disservice to the field because non-specialists may assume that his account is a fair description of the field as it currently stands. We hope we have given readers some pause for thought. A thorough analysis of the wealth of theories and findings in hypnosis research over the last few decades would have been necessary to advance a useful critique of assumptions and hypotheses in the field and make a strong case for a novel account. Crabtree's account is neither novel nor well-informed; regrettably, the proposal could have been written in the 1950s with only minor differences. His descriptions are overly vague and simplistic, offer no novel substantive predictions, and neglect a vast amount of relevant research. We consider it unlikely that any account that does not recognize individual differences among highly hypnotizable individuals (McConkey & Barnier 2004, Terhune, Cardeña, & Lindgren 2011b) or acknowledge a wider variety of processes (e.g., response expectancies, motivation, and individual differences in propensity for automaticity) will provide a comprehensive account of the fascinating set of phenomena called hypnosis. Knowledge of its history is of substantial value to understanding a field but is alone insufficient to address its current issues and controversies.

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COMMENTARY

Comments on Crabtree's "Hypnosis Reconsidered, Resituated, and Redefined"

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I was very stimulated by Adam Crabtree's article, and also a little embarrassed. I am always preaching to colleagues that you should be sensitive to the implicit and cultural assumptions you make, so how in the world could I have been so blithely ignorant of the cultural assumptions built into the process of defining hypnosis by biased lists of phenomena?

I began reading extensively in the hypnosis literature as a young man, and by the time I was in my second year of college was quite well-informed about what was known at that time. I had seen many lists of hypnotic phenomena, and while intellectually I questioned the idea of "Authorities" in general—what young person doesn't?—at that age I was still pretty accepting that the Authorities[1] on hypnosis knew what they were talking about. I was aware that modern writers on hypnosis prejudicially left out any mention of the apparent parapsychological aspects of hypnosis, but I assumed they were otherwise accurate.

The Fallibility of Authorities

I received a major shock in my acceptance of Authorities in general and particularly the Authorities on hypnosis when the best-selling book *The Search for Bridey Murphy* was published in 1956 (Bernstein 1956). The author, Morey Bernstein, a businessman and amateur hypnotist, reported on his experiments in regressing an anonymous woman back before her birth, where, in a number of sessions, she reported various descriptive items about living a life in Ireland as one Bridey Murphy. In a fast reaction to the book, a number of the most prominent Authorities on hypnosis published "A Scientific Report on *The Search for Bridey Murphy*" (Kline 1956). I opened that book with great interest, since I had found the Bernstein book quite interesting, and was looking forward to seeing what

scientific parties could add. What I found instead was that people who were otherwise genuine authorities about hypnosis, psychology, medicine, etc., simply became totally angry and irrational when it came to the topic of reincarnation. Their criticisms of what they claimed had been said in Bernstein's book were so inaccurate that I had to go back and reread the book. I couldn't believe that their anger at the very idea of reincarnation could cause them to become so distorted in their perceptions and thoughts, but it had.

Note that the vast majority of my mainstream colleagues in hypnosis research constantly stressed that there were no weird things like recall of past lives associated with hypnosis, hypnosis was scientific and science had no place for things like that. Past life recall was not included in the various lists of defining hypnotic phenomena. I don't think readers of this *Journal* will automatically feel angry that I am mentioning hypnosis and past life recall, but if any of you feel some of that, keep that feeling available to inspect in the last part of this Commentary . . . it's relevant.

Nevertheless I did not really question the phenomena lists as a primary way of defining hypnosis, although in my own development of my systems theory approach to altered states of consciousness many years later (Tart 1975) I paid little attention to specific phenomena and looked at the dynamics and interrelationships of various subsystems of consciousness instead. But in the back of my mind, the definition of hypnosis by prominent phenomena was still intact. So I want to again thank Adam Crabtree for making me realize how questionable this approach is, especially when it is implicit, beyond rational questioning.

Crabtree proposes a fresh look at hypnosis that

> . . . situates hypnosis as a subspecies of *trance* as defined in a very specific way: a state of profound focus on something accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources. *Hypnosis* is then defined as an inner-mind trance characterized by rapport. (Crabtree 2012)

I certainly think this will be a useful approach, although, as he recognizes, it's going to require a considerable increase in our knowledge of things like "rapport," "trance," and what the "subliminal" is. As I think these are important realities that have been too long neglected in contemporary psychology, that's excellent! I don't know that Crabtree's approach will turn out to be the definitive approach, but it's certainly worth trying.

A Related Problem—Hypnosis is Weird and Eerie

I want to add one extra consideration to Crabtree's stimulating discussion, though, and that is that perhaps some of our difficulty in understanding hypnosis is that there is something just plain "eerie" about it. Eerie in and of itself, not simply because of historical associations with the occult.

I concentrated on hypnosis research in the early part of my career, and for all the intellectual understanding I had of various aspects of it, and for all the ways I could appear to be (and was, by contemporary standards) an authority in talking about it, I know that I still basically don't understand what was happening in hypnosis, and what was happening could be downright weird.

In Ernest Hilgard's hypnosis research laboratory at Stanford, for example, where I did my postdoctoral training, one of our standard hypnotizability scale items for talented hypnotic subjects was anosmia to ammonia. After going through the procedure to hypnotize subjects—and since these were highly selected individuals I will assume they were genuinely hypnotized at this point—we had standard instructions, to be read to the subject for half a minute or so, to the effect that the subject could not smell anything. I would announce then that I was going to put something odorous under her nose and ask her to take a good sniff, but the deeply hypnotized subject would not smell anything. When this worked, I would be quite amazed, although, as a professional, I didn't show my reaction to the subject. I saw many people take a deep sniff of the bottle of household ammonia or even stronger ammonia one inch away from their nostrils *and not show the slightest reaction*. When asked if they smelled anything they would say no, but the lack of overt reaction was far more impressive.

Ammonia is not simply a strong smell, *it is extremely painful* in high concentration. When I would try to sniff it my head would snap back to get away from the pain! I could imagine someone training themselves over long periods to suppress their reaction to this kind of pain, but not when it came for the first time in their life, unexpectedly.

If you want to get a good feeling for how eerie this kind of reaction is, take a sniff of household ammonia yourself. But I strongly suggest that you start from several inches way and don't inhale very deeply! It hurts!

Theories as Defense Against the Eerie?

Insofar as I am correct that we can have uncomfortable emotional reactions to some of the phenomena of hypnosis, to the strange and eerie, analgesia and ostensible past life recall to mention just two, this also means that a lot of the conventional theories of hypnosis have a hidden agenda, namely to

"explain away" the mysterious. A way of saying "There's nothing strange going on here, we are in control, we understand everything important!" While there is much truth in the role-taking type theories for many subjects—some subjects are indeed primarily acting the role of the hypnotized subject—seeing someone as "just acting" is a lot easier on our social conditioning than believing someone really can become immune to, say, extreme pain, or regress to an earlier age, or, to bring back the suppressed, show clairvoyant knowledge of the world, or retrieve valid memories of a past life.

Experimenter Bias

Of course we're all very attached to our self-concept as objective scientists, who are just getting at the facts, not being influenced in our observations or theorizing by implicit or explicit emotions. Yes, that's our goal as scientists for collecting the facts, but thinking that's the end of the story is a recipe for disaster. I personally take the attitude that while I have a strong motivation to want the truth about things, I am biased in most things I do, including formal experiments, so I must allow for the possibility of some kind of bias distorting results, and see what I can do to prevent that from happening. If instead I simply take refuge in a self-concept of being objective, I allow biases a wide play.

One of the most amazing things in my career as a psychologist was to see that the question of *experimenter bias* and *demand characteristics* raised by investigators like Rosenthal (1963) and Orne (1962) was so central to our scientific enterprise—and then to see how quickly interest in it disappeared! My own small contribution to studying such bias showed that experienced professionals, knowing they were being checked for bias in a hypnosis experiment, nevertheless showed such bias (Troffer & Tart 1964). When I look at the way most interest in experimenter bias simply disappeared from mainstream psychology, I can't help but think of suppression and repression in the service of the ego, in the service of supporting our belief in our superiority because of our vaunted objectivity. So besides thanking Crabtree for bringing one of our assumptions to consciousness where it now seems to be quite questionable, I would like to add the recommendation that we start looking at our experiments on hypnosis and other phenomena with openness to the fact that we may be biased in various ways, and asking what we can do about that.

Note

[1] I will capitalize authority, viz. Authority, when I want to emphasize the implicit, irrational reactions we often have to authority figures.

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COMMENTARY

Regarding "Hypnosis Reconsidered, Resituated, and Redefined": A Commentary on Crabtree

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As I prepare to make comments about Crabtree's paper, I find it difficult to know exactly where to begin. It is hard to decide whether this paper has said a lot or has, in fact, said nothing other than the most obvious, or simply used different language to state what others have said. On the other hand, perhaps my struggle is indicative of something more. Since I am struggling to figure this out and am unclear in my thinking, does this indicate Crabtree has presented something new and significant? Despite this quandary, there are a number of specific points I would like to address, and, then, later return to considering the larger questions raised by my struggles.

I would like to frame the context for my Commentary. I was trained in hypnosis and hypnotherapy in 1968. I have done active clinical practice for forty years and used hypnosis in various clinical ways. For 10 years, I taught doctoral students a course in hypnotherapy. I am a practitioner and not a researcher or theorist of hypnosis. I have published a fair amount in the areas of dissociation and have a background in philosophy, in particular phenomenological philosophy. In my comments I want to be fair and balanced, but I want to address what was problematic from my point of view since in the long run that might be most useful.

I would like first to applaud Crabtree's creative and theoretical tour de force: He defines hypnosis from the inside, from the perspective of mental phenomena and not from the operational definitions of hypnotically emitted actions (his list of hypnotic phenomena) or of the behaviors of hypnotists. He has created an overarching theory that explains a range of phenomena and answers important questions. How is it that we can observe hypnosis-like behavior in people who are not in a hypnotic state? How is it possible for people in a hypnotic state to engage in hypnotic behavior? What happens when someone goes into hypnosis? What is the link between hypnosis, group trances, and other rituals seen worldwide, rituals that evoke

trance-like states similar to hypnotic states? How does experience seem to flow connectedly from thing to thing? His theory answers these questions and brings them together seamlessly. Unfortunately, I believe he generalizes too broadly and has established concepts which founder when examined closely.

Is there something unique about hypnosis? There must be, otherwise a unique word would not label it. Alternatively this statement has been questioned by those who assert hypnosis is not a unique state but a social enactment, having no independent reality as a state. Crabtree adequately critiques this dismissal of hypnosis being a unique state. Crabtree is clearly a scholar of the history of hypnosis and conversant with current theory and research. His paper describes attempts by current theorists, practitioners, and researchers to more clearly define and specify what hypnosis is. Psychology has struggled with the dilemma he describes: How can a researcher or theorist empirically measure a mental phenomenon? How do we define a mental phenomenon so others can know what that is? This difficulty led to defining mental phenomena as a function either of specific behaviors, brain activities, or of particular measurement activities called operational definitions. I am reminded of my own initial foray into this dilemma in the 1960s when, as a graduate student, I concluded that psychology had "lost its mind." All psychological phenomena were defined by behaviors, and mind or consciousness did not exist. When dreams were finally connected to REM sleep, psychology as a discipline, constrained by its methods and assumptions, had to acknowledge mental phenomena—in this case, dreams. In an analogous fashion, were specific brainwave activities linked to hypnosis, then this research result might lead to a different conceptual and experimental approach. I believe I have read about this specific research result in the past five years (unfortunately I cannot find the reference), and, although I am not an expert, such data would suggest an empirical way to specify what hypnosis links to experimentally. If this is correct, I do not know the implications for Crabtree's theory.

Before addressing specific issues, as I have reflected on Crabtree's theory, I would like to consider what I have called the *domain of hypnosis.* He has attempted to globally explain all hypnosis-like phenomena. I wonder, however, whether he has been too inclusive or uncritical by including all historical reports of hypnosis. How does one evaluate the quality of those reports? Given the present-day dilemma in knowing what is hypnotic and what is not, how can he be assured those reports are accurate and also that they are hypnotic? From another perspective, there are phenomena in the hypnotic domain not addressed by his theory. Although he refers to individuals who cannot be hypnotized because of difficulties

with attention, he does not consider differences in hypnotizability or the possibility that some individuals might not be hypnotizable at all. A non-hypnotizable individual does not accord with how Crabtree later describes everyday trance experience or how those individuals seem to experience, in particular how they focus. I will return to this later when addressing focused attention. Another issue pertains to the distinction between the process of being hypnotized and the state of being in hypnosis. As an individual enters into hypnosis, the state deepens over time. There is a transition from a non-hypnotic to a hypnotic state. When does that transition occur? What happens when it does? Finally, Crabtree does not consider post-hypnotic suggestion. The previously hypnotized person, no longer hypnotized, enacts the suggestion. How does that take place according to his theory?

I repeatedly struggled with Crabtree's definitions and meanings. In particular, the cornerstone definition on which *hypnosis* rests is *trance*. His theory must rest firmly on that concept; if it does not, it is not supportable. Colloquially and professionally individuals use *trance* interchangeably with *hypnosis*, or use the phrase *hypnotic trance*. As I have been writing this Commentary, I frequently find myself intending to write *trance* as a substitute for *hypnosis*. Crabtree, for example, refers to Erickson's use of *trance* as a synonym for the *hypnotic state.* An online dictionary provided the following definitions:

1. a half-conscious state, seemingly between sleeping and waking, in which ability to function voluntarily may be suspended.
2. a dazed or bewildered condition.
3. a state of complete mental absorption or deep musing.
4. an unconscious, cataleptic, or hypnotic condition.
5. Spiritualism. A temporary state in which a medium, with suspension of personal consciousness, is controlled by an intelligence from without and used as a means of communication, as from the dead.

Trance as a core definition for hypnosis, is confounded by its identification with hypnosis. In this regard, I found myself sometimes asking, "How is trance different from hypnosis?"

Crabtree addresses this in his definition of trance and hypnosis:

> a state of intense focus on something, accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources. My definition of hypnosis is: an inner-mind trance characterized by rapport. (Crabtree 2012:312)

Striking about these two definitions (*trance* and *hypnosis*) is what I consider the unusual use of the term *trance* by Crabtree. *Trance* per dictionary definition includes hypnosis (as mentioned above) but also bewilderment, half-consciousness, daze, unconsciousness, and catalepsy. Crabtree describes *trance* as consciously intended, an "intense focus on something," a state described in only one of the five definitions—"complete mental absorption or musing." His use of the term does not include being dazed, half-conscious, or unconscious—clearly not intentional states. Likewise, one would be hard-pressed to describe a state between waking and sleeping as "intense focus." Some uses of the term *trance*, such as "zoning out," involves being unresponsive to the environment (subjectively blocking out everything else, as per Crabtree's definition) yet does not involve an intense focus on anything at all. From my perspective, then, one problem with using the word *trance* is its unusual use, a use which leads others (in this case me) to understand it in a fashion different from that intended by Crabtree.

Not surprisingly, given the preceding discussion, one of the difficulties that I have is Crabtree's use of language: Does it add conceptual clarity to use the word *trance* and not the word *hypnosis*? Is this simply a linguistic substitution or is there a substantial and significant reconceptualization embedded in his language? I assume Crabtree would choose the second option; I am not sure. A similar difficulty involves Crabtree's notion of accessing subliminal capacities. Is this different from activating "unconscious responses via hypnotic procedures"—the latter being language one might use? Once again, does his language add something that extends and clarifies? This issue will be addressed later.

A second difficulty from my perspective is that everyday phenomena do not accord well with Crabtree's definition of *trance*. Later in his paper, for example, he asserts that everyday experience is a series of trances.

> This everyday-life constellation is made up of elements of all four types of trance. These trances are experienced as "ordinary" and "normal." (Crabtree 2012:321)

Using his language, then, everyday experience is a series of states "of intense focus on something, accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources." Granted, elsewhere in the paper, he adds that intensity of focus can be more or less, leading to different subjective experiences. On the other hand, in terms of everyday experience, do these words accurately describe day-to-day experience? As I go about my day-to-day activities, though I focus on various things, I would hardly say my focus is consistently intense.

In this past moment, for example, I am mulling on my Commentary as I look across the room. I am aware of a diffuse internal question relating to how my experience relates to intense focusing. I have not had clear and focused thoughts but rather a generalized wondering. My attention shifts from inside to outside and I slowly become aware that I have been seeing, as I have been mulling, the wall and furniture across the room on which a plate rests on a stand. My seeing the room has been present all along and now I notice it, once again in a diffuse way, realizing I could focus on the plate or not. And I then wonder, "If I focus on the plate, would that be trance?" According to Crabtree, I would then be in trance. And, according to my own subjective state, I would not have been in trance previously. From my experiential perspective, focusing on the plate would not be trance. I will later address his notion of focus in the context of figure–ground perception.

But let me continue. The doorbell rings, intruding into my attention. I rise, still diffusely reflecting on my Commentary as I walk to the door. A person's dark outline shifts on the opaque glass in the door and then disappears. I open the door and a package sits there. A delivery person walks away toward the street.

My focus, during this 30-second event, is hardly intense or sharp. I would describe it as floating and diffuse, evoked as much by the outside as by my intention. My attention has been "pulled" by the "outside." It would seem to me that the progression of experience is not that of sequences of intense focuses. That simply does not accord with what happened. Were Crabtree to assert, in disagreement, that I was, in fact, in trance and focusing, just less intensely, I would rejoin that he is establishing this by assertion and not argument or evidence.

Crabtree's description of designing a wooden bed is an example of what he means about everyday trance. The fabric of our experience is, as he describes it, a series of perceptions, first this and then that, which continue on and on, linked together in some kind of meaningful fashion, guided by implicit meanings at the fringe. What is added to our understanding by labeling this trance? He also never states whether any everyday experience (as I previously described) is not trance. If such a distinction is the case according to Crabtree, then what distinguishes non-trance from trance experiences? I assume that any time someone focuses on anything, that focusing leads to trance. According to Crabtree, is this correct? And if it is, does it add to our understanding to describe such experiences as trance?

Crabtree vacillates in his presentation between asserting that everyday experience is mostly trance to characterizing everyday experience as sometimes or partly trance. See the following quotation from Crabtree (my italics).

> *Trances are part of everyday life. By this I mean that the notion of trance I am proposing provides a perspective on the entire range of human experience. Everyone is susceptible to trance, except for individuals whose mental state, temporarily or long term, precludes focusing. In the conduct of our affairs, we are constantly shifting from one center of focus to another as we move from one activity to another or one concern to another.* (Crabtree 2012:313)

Let us grant, for the sake of this argument, that hypnosis is trance and that hypnosis is an inner-mind trance that can include all possible everyday experience. Based on this set of assertions, can we then conclude that everyday experience is also trance? I would answer that "No, we cannot draw that conclusion." Crabtree does not present his theory in this fashion: Rather, he begins by defining trance in a way that allows him to assert trance characterizes everyday experience and then defines *inner-mind trance*. The point I am making, however, is that hypnosis can be trance without the requirement or implication that everyday experience also be trance. From my point of view this is an important observation, since it would allow Crabtree to keep trance as a central concept vis à vis hypnosis without complicating his exposition by making everyday experience trance. If, by analogy, we equate trance with dreaming, all possible everyday experience can be dreamed. That does not imply that everyday experience is a dream. It seems to me that dreaming has some special and different quality, distinct from everyday waking experience. The same can be claimed about hypnosis—hypnosis has some special and different quality, distinct from everyday experience, yet can include all possible everyday experiences.

From the opposite perspective and in contradistinction to the prior discussion, Crabtree might need to have everyday experience characterized as trance for his theory to "work." Here is why I have concluded this. Based on his theoretical exposition of everyday trance, I infer that the crucial issue underlying his theorizing is how to explain the evoking of appropriate subliminal resources. Crabtree writes,

> To add further to the difficulty, they [hypnotic phenomena] all occur in some form or other in everyday life. It is possible to identify examples of everything from amnesia and anesthesia to positive and negative hallucination in ordinary human experience. (Crabtree 2012:308)

Though we discover hypnotic phenomena in everyday life, the phenomena do not occur frequently. Most people would react to their occurrence as unusual, and, though occurring in the midst of everyday experience, hardly an "everyday occurrence." This suggests to me that there must be something in the everyday circumstances that evoke the

"hypnotic response." The railway worker who is not aware of his pain or severed foot is in an extreme situation that is not "everyday." Given these comments, I conclude that Crabtree needs to explain why these hypnotic phenomena occur and he does so by "making everyday experience trance." My reconstruction of the possible underlying logic is as follows: Since, according to his exposition, hypnosis is a trance state in which subliminal resources are evoked, and since hypnosis also entails all possible everyday experiences, then evoking those resources must occur during everyday experience, and, since evoking resources requires trance, everyday experience must also be trance. Should my analysis be correct, I would question the necessity for these logical connections.

In a later portion of my comments I will address evoking subliminal resources, but, in this context, might it be possible to theorize that hypnosis by its nature allows for subliminal resources to be evoked? Might there be something different about the evocation of resources during hypnosis than for their evocation during everyday experience? Returning to "designing a bed," an equally viable model to understand what is evoked for him at each step is the "elicitation of memory." What he describes are not skills (like increased strength, sharper visual perception, or heightened tactile sensitivity) but recall of relevant information or procedures. And, I would note, the elicitation of memory associations occur without conscious intention—they simply appear in mind.

Crabtree distinguishes hypnosis from everyday experience by the object of focus, "inner mind trance" as opposed to "everyday trance," and the presence of a hypnotist with whom there is rapport. As I try to untangle these terms and ideas, I seem to find myself caught in definitions that point back and forth to themselves such that they explain, by fiat, the phenomena and are exempt from further consideration. Consider *trance* which is defined to mean an intense focus on some object with accompanying diminished wareness of everything else. Is this different from figure–ground distinctions in perception? When one perceives a figure, for example, it pops out from the ground which recedes and becomes less prominent than the figure. In other words, the object of focus, the figure, becomes central and what is not the object of focus, the ground, recedes and awareness of it diminishes. Does this not seem to capture the phenomena Crabtree describes as "trance"? In other words, the way perception seems to operate, shifting from figure to figure with accompanying shifts in perception of the ground, seems similar to what Crabtree defines as trance. Why use the term *trance*? To ask this differently, does the word *trance* add anything? And, from another point of view, does it confuse and complicate? Clearly my own struggle to understand the theory suggests that "trance" confuses

and complicates. Later I will discuss the "background," similar to James' "fringe" in the context of focusing.

Continuing to consider "trance" in terms of everyday experience, Crabtree considers everyday experience to be a continuous series of trances which shift or flow, the one to the next. The significant variable seems to be intensity of focus, with the most intense focus leading to the greatest loss of awareness of everything else. Crabtree uses intensity as a variable, a deeper or more absorbed to a less absorbed state linked to depth of trance. He comments on trance as "a state that involves **absorption** in something and abstraction from, or obliviousness to, other things [emphasis added]" (Crabtree 2012:312)

Absorption has been used for decades as an explanatory intervening variable for hypnosis, significantly so by Josephine Hilgard. Although I am not an expert on current research, I believe that there is equivocal evidence supporting its central role in hypnosis as currently defined. Absorption has not adequately "explained" the phenomena. If current research on a narrow definition of hypnosis, conceptually a subset of the universe of possible definitions, has not obtained support, then this variable is also equivocal for a larger and more inclusive definition. Yet this is what Crabtree does; he makes absorption a central concept. Absorption, as I understand it, is a capacity that allows for hypnosis to occur. In its absence hypnosis would not occur. In this regard, Crabtree's definition of *absorption* is reminiscent of precisely the kind of state my clients need to enter on the way to trance. But the question that arises for me is the utility of adding trance as a concept that includes absorption. Does this add to our understanding? Clearly I am not sure.

Another issue vis à vis hypnosis is his phrase *inner-mind trance* which would suggest that the absorption is to the "inner-mind." He does not define what *inner-* or *outer-mind* mean. I find myself puzzled about what outer-mind might be. Rather than I making a conjecture, Crabtree, it would seem, should clarify the distinction. In the following quotation, he implies that "inner-mind" is the "subject's inner mental world."

> The state of hypnosis is specifically identifiable, not because it manifests conventionally agreed-upon phenomena, but because it exhibits a state of focus, the object of which is the subject's inner mental world, which temporarily includes the hypnotist, accompanied by a diminished awareness of everything else. (Crabtree 2012:314)

I find focusing on the "inner-mind" particularly problematic in relation to usual hypnotic procedures. For example, a standard procedure involves staring at a dot or a point on the wall. In this situation, trance is induced while

focusing on the external world. Furthermore, there are usually words the hypnotist says that also "come from the outside," such as "You are listening to my voice." If Crabtree were to counter that at first focus is outside but then inside, then he acknowledges that the hypnotic induction is done with outside focus. That would imply that outside focus somehow establishes hypnosis, in conflict with his theory.

The subsequent paragraphs in which Crabtree links intensity of focus to diminished awareness of everything else jives directly with my experience of what is involved in depth of trance. In this regard, his description matches precisely my clinical experience. From another perspective, however, it would seem trance is being substituted for hypnosis. Once again, I ask: Does the word *trance* add to our understanding?

Crabtree continues by developing an explanation for hypnotic phenomena by asserting that the "evocation of appropriate subliminal resources, occurs automatically (Crabtree 2012:312)" when trance occurs. He continues, "Focus on something calls for action in regard to that thing. . . . In trance whatever is needed for the action is made available" (Crabtree 2012:312). This occurs in deep trance when a suggestion has been responded to by the subject, for example a hallucination, anesthesia, or amnesia. In this regard, I believe Crabtree has not addressed one of the most distinctive features of hypnosis: Hypnotic suggestions occur on their own, without conscious intention on the part of the subject, and are usually not within the intentional control of the subject. In this regard, we know that hallucination, anesthesia, and amnesia occur for some (though not all) individuals during everyday experience. But most non-hypnotized people cannot hallucinate or be amnestic or anesthetic at will. Hypnotize the individual and that person can hallucinate or become anesthetic or amnestic. And that person, after the fact, will frequently be surprised or shocked about this having happened because it is NOT part of their everyday experience and not something consistent with what the person can do intentionally.

Another way in which Crabtree understands hypnosis to be like everyday experience can be seen in the following quotation.

> . . . trance states are a normal part of life, that they are in play in every type of human experience. This means that all of us are familiar with them in practice, even though we may not have explicitly recognized their place in our lives. That is why deliberately induced trances, such as hypnosis, are ordinarily not experienced by the subject as particularly alien or strange. (Crabtree 2012:320)

I disagree with Crabtree's conclusion. His example of the railway worker who does not experience pain is not everyday and could be construed

as alien or strange. Moreover, when a person acts like a chicken on stage during a hypnosis act, most would construe the behavior as strange. And I would further note that these observations make clear that hypnotic phenomena are not everyday experiences at all.

Considering hallucinations further, they are experienced as outside, in the world and not in the mind. Many hypnotic phenomena relate to how the individual experiences and responds to the world. This, it would seem, conflicts with the concept "inner-mind" trance. I would posit further that one of the conceptual difficulties leading to this conflict is that Crabtree's theory assumes a mind–body dichotomy, a distinction which characterizes Western philosophy.

Let me add another observation about being hypnotized. When an individual is in a hypnotic state, that person shifts perception from object to object, listening to the hypnotist, performing actions, or reacting to the environment. All of these shifts in focus occur while the individual is hypnotized. Hypnosis, therefore, cannot be the shifting of perception or the shifting of focus—but must be considered a meta-state, a context or something experientially more inclusive within which these shifts in focus take place.

There are other issues associated with Crabtree's concept of intensity of focus. My clinical experience is that individuals who focus too intensely or too "tightly" are not hypnotizable, in contrast to Crabtree's theory. These are individuals who are intellectualized, compulsive, rigid, or too reality-focused. They seem to focus too well. To allow themselves to go into a hypnotic state requires their relaxing or loosening their intense focus and becoming receptive or allowing—being less intentional and more passive. My clinical experience is that intense focus, as Crabtree defines it, interferes with hypnosis. Hypnosis most frequently requires relaxed receptivity.

There is another aspect of "intense focus" that jars with my experience of hypnosis. Apparently, Crabtree takes "intense focus" to imply "intentional" or "intended" focus on the part of the individual. I conclude this from the way he describes intense focus.

> The object of focus may be a person, place, thing, situation, idea, feeling, etc.—anything that a person may **direct** his or her attention to. (Crabtree 2012:312)

I have bolded the significant word, *direct*, to emphasize the intentional aspect of focusing. As a result, focusing would be intentional on the part of the individual and, in this way, actively involve the subject or the sense of self. Most notable about hypnotic experience is that the individual

observes what occurs but does not intend it, and, when challenged, cannot stop the hypnotic suggestion from happening. As a result, there is a marked shift in how a hypnotized person's sense of agency operates—that is, it changes from agent to observer. This is a profound alteration in how the individual functions. Crabtree does not address how this shift occurs and, I would claim, that "intentionally focusing intensely" strengthens agency. This brings me again to note that hypnosis alters a larger context or frame within which self, mind, body, memory, and perception arise such that they function differently.

Linked to these observations is auto-hypnosis and the necessity for rapport with the hypnotist. Years ago when I was first learning about hypnosis, I taperecorded various inductions and listened to them. Moreover, I read inductions and found myself going into a hypnotic state. In both of these situations, I never imagined a hypnotist, either myself or another. My memory of this is that I followed the directions or suggestions, either heard or read. And then I observed that certain hypnotic experiences followed. Obviously I knew that I had recorded the induction or was reading the words. But does that imply I was in rapport with myself as an outside hypnotist with whom I was absorbed? That was not my experience. The suggestions were directions I followed or heard but did not consciously experience them as from another person either in fact or imagined. My experience was simply following instructions set down by the induction. It would seem that the necessity of having an external hypnotist limits what is possible and constrains self-hypnosis.

Rapport as Crabtree defines it, experiencing the hypnotist as not different from oneself or as a part of oneself, does not accord with how I experience hypnosis. I have inferred from his concept of "rapport" that this is an attempt to understand why suggestion works. In other words, since the hypnotist is an extension of my self, suggestions are not self-discrepant, and therefore, I do them. If I am correct, this is an intriguing analysis; although I consider it problematic.

As with hypnotizability, suggestibility lies along a continuum. In other words, people are more and less suggestible. This is the case independent of hypnosis or the individual being hypnotized. I doubt that non-hypnotized individuals who respond to a suggestion from another experience it as coming from someone with whom they have rapport. Of course, if I feel a connection with another, I might be more likely to go along with the suggestion—partially supporting Crabtree's idea in this context.

I would like, however, to explore this in a different fashion. When Erickson describes inducing a trance, he engages the subject in such a way that the individual becomes receptive, open to what might follow, and at

that point begins to make suggestions. In other words, my understanding of Erickson's approach is to foster open receptivity which then allows for suggestions to work. Although this accords with how I facilitate hypnosis, this analysis does not accord with Crabtree's. Crabtree would argue that these conditions are not sufficient for hypnosis; rapport is required in addition. He might argue that for a suggestion to "work" rapport is necessary. But to simply assert this is not a persuasive counterargument. From my perspective, this argument clarifies some of the confusion about rapport. For example, when Crabtree writes "that the subject has incorporated the hypnotizer into that focus (is in a state of rapport)" (Crabtree 2012:315), he does not clarify what *incorporate* means. Does he mean "take into one's body" implying that the hypnotizer and the subject become indistinguishable? That the hypnotizer is not different from my self?

Furthermore, when I am doing hypnotic work with a client, I note that when I am attuned to my client's process, such as subtle nonverbal cues, this facilitates the deepening of trance. I would say I am "in synch" with my client. When I am not attuned or when I miss what is going on, my client does not respond. I would assert however that my client is not experiencing me as an extension of self. When the client does not respond, the client has remained in a hypnotic state. Yet my suggestion does not lead to a response. Given Crabtree's view of rapport, the suggestion should work. How does one explain this dilemma from Crabtree's theoretical perspective?

Furthermore, I know of various ceremonies performed in Western and non-Western cultures (drumming, spinning, chanting, and so on) which lead to trances in which hypnosis-like behaviors occur. These are clearly explainable via Crabtree's group trance definition. Yet these group or situational activities seem to generate states very similar to hypnosis. This puts into question, again, the necessity of having a hypnotist for a hypnotic state.

Continuing to consider the evocation of appropriate subliminal resources when in trance, does this evocation imply that cure should naturally follow from focusing on a problem in trance? See the following quotation.

> The response to the object of focus is *appropriate*, in the sense of fitting. Appropriateness is determined by the responsive mechanisms of the individual. To the onlooker the response may seem inappropriate, but for the entranced individual considered as a whole organism, the response will be the one that is judged appropriate. The judgment is made on many levels, and the process of making that determination is to a great extent unavailable to consciousness. (Crabtree 2012:312–313)

I have some difficulty with this, not that it does not fit, but rather with the

necessity of trance for it to occur. To be fair to Crabtree, he never discusses the possibility that subliminal resources might be evoked in a non-trance state. If that is the case, however, would this not put into question how they are evoked during hypnosis? Assuming, however, that Crabtree agrees that trance is necessary to automatically evoke resources, it is important in theory not to fall victim to circular reasoning: That is, if appropriate responses occur, then that occurrence means the individual was in trance. Rather, there would need to be some kind of trance-like state clearly defined before that happens. I recently had a training experience in which I simply thought about what might have happened between ages 3 and 4, diffusely focused on my body, and discovered various sensations and other processes arising, which, as I continued attending, changed, brought up pictures that seemed to fit that age span, brought up emotions and bodily sensations, and then it all seemed to eventually fade. This sequence clearly mirrors what Crabtree has described, fitting responses associated with a specific focus. But I would argue that I did not need to be in trance for this to happen: Rather, I thought about a young age and then openly attended to what might occur experientially. I did not continue to focus. I simply observed receptively. Sensations, emotions, fragmentary pictures, and symbols came and went and eventually faded away. As mentioned in a prior section, memory or associations are as viable an explanatory mechanism for this as trance is.

Another difficulty I have with Crabtree's notion of trance comes from my experience of doing EMDR. For readers who are unfamiliar with EMDR, the process is as follows. The client brings to mind something of concern by thinking about a picture, an emotion, and a negative belief in relation to the issue. The client brings to awareness these aspects of the issue, some might be clear and intense, others might be hazy and almost unnoticeable. The client then lets that awareness go and notices whatever occurs in experience while the therapist provides bilateral stimulation (moving the eyes, listening to sounds, or feeling taps) which the client follows either with eyes, with ears, or by sensory awareness. The client's instructions are to let happen whatever happens: to track whatever arises internally whether a thought, sensation, emotion, memory, insight, or nothing. The result, according to an EMDR model, is the evocation of memory traces that lead to a resolution of distress and negative beliefs.

What seems significant about this process in relation to Crabtree's theory is that the evocation of a response could be memory traces or associative networks. There is, in this regard, a match between this EMDR model and Crabtree's theory, although the explanation for the response is the activation of memory and emotion. The precipitating situation is not one of trance or of intense focus. In fact clients are asked not to focus intensely on their issue

but rather to focus briefly and then to let it go and allow whatever happens.

One of the omissions in Crabtree's discussion of hypnosis is the relationship between hypnosis and dissociation. The phenomena associated with dissociation are almost identical to those in hypnosis. Dell (2009) argues that dissociation and hypnosis have not been adequately distinguished, that neither have been clearly defined, and there is some kind of yet-to-be articulated connection between them. Interestingly, Dell (2009) observes that dissociation is involved with every kind of human experience, an observation Crabtree makes about hypnosis. Two important conceptual issues derive from this connection. Dissociation occurs apparently in the absence of two significant variables Crabtree emphasizes as necessary for hypnosis: the interpersonal nature of hypnosis and the role of social expectations or context in guiding the hypnotic response.

One of the important reasons to emphasize dissociation is the similarity of dissociative and hypnotic phenomena. Disassociation occurs spontaneously and without the intent of either the subject or somebody else creating the dissociative state. This implies that self-hypnosis might have nothing to do with the person or another, but rather provides a context within which this responsive capacity is activated.

In my own research and theory on dissociation (Beere 1995), I developed a phenomenologically based theory that is similar to what Crabtree describes as necessary for hypnosis. According to my theory, dissociation occurs by focusing on a specific perceptual figure and blocking out aspects of the "background," a technical term to be described shortly. Crabtree refers to figure and fringe as informing his own theory. I believe that *fringe* as he uses it is equivalent to *background* as I have used it. *Background* is the experiential container for figure–ground experience. Every perception occurs in a larger context which includes the experience of time, the world, the body, the sense of having a mind, and a self or identity who perceives. These background phenomena are constants yet in the background for every perceptual experience. The background seems similar to fringe phenomena.

Dissociation, according to my theory, arises when someone attends with such intensity that background features are blocked out and thus experienced dissociatively. Body or object size could change. One could observe the body from outside. Time could change. All of these experiences are also those that occur in hypnosis. Consequently, to develop an adequate theory, the connection between hypnosis and dissociation needs to be made.

In one of my research studies (Beere & Pica 1995) we looked at distractibility and the capacity to attend in relation to dissociation. The capacity to attend correlated significantly with distractibility but not with dissociation. In other words, attending and not being distractible was

not associated with dissociation. If, as Dell is asserting, dissociation and hypnosis are connected, then, based on my research results, hypnosis also is not associated with not being distractible and the capacity to attend. This does not support Crabtree's foundational definition of *trance* which underpins his definition of *hypnosis*.

Reflecting on the preceding comments clarifies the struggle described at the beginning. Crabtree has created a comprehensive theory that attempts to solve various problems with prior theories and approaches. Clearly I have been impressed. Unfortunately, as I considered various elements of his theory, almost every one had a practical, theoretical, or logical flaw. My conclusion is that his theory requires additional refinement. My wish is that my comments can assist in furthering his theory, a task well worth the endeavor.

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COMMENTARY

Reply to Three Commentaries on "Hypnosis Reconsidered, Resituated, and Redefined"

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In responding to the comments (pp. 327–359) on my article (pp. 295–325) in this issue of the *JSE*, I will take up the issues raised by each of the three Commentaries separately. I take this approach because their concerns seem to be quite different, and by treating them individually I can best do justice to each.

Tart's Comments

Tart's comments are directed mainly to the first half of my article, where I call into question the way *hypnosis* has been defined, particularly over the past seventy-five years, in terms of arbitrarily compiled lists of hypnotic phenomena. He makes the point that the lack of sensitivity to implicit and cultural assumptions found in the case of hypnosis illustrates a problem that too often goes unrecognized in other areas of scientific inquiry. Tart calls attention to the fact that scientists can be vulnerable to becoming attached to a self-concept of objectivity that excludes the possibility of being influenced in their observations or theorizing by unrecognized biases. He points out that the recognition of the possibility of unacknowledged influences operating in hypnotic experimentation, formulated in the 1960s in terms of experimental bias and demand characteristics, was something that he himself researched, and he notes how quickly that kind of important scientific self-examination disappeared.

It is precisely the need for this kind of self-examination with regard to hypnosis that led me to write the article. I was confident that a call for the examination of unrecognized assumptions would be welcomed by those who appreciate the importance of hypnosis and want to make hypnotic research and clinical practice as effective and fruitful as possible. It was with some surprise, then, when I read the comments of Cardeña and Terhune, who not only disagree with my conclusions (which is their prerogative) but also convey the impression that all is well and in order in the field of

hypnotic research and that any discussion that presumes to re-evaluate basic assumptions is not welcome. For that reason, I would like to respond at some length to their comments.

Comments of Cardeña and Terhune

To begin I would like to say something about the comments of Cardeña and Terhune on the historical segment of my article. Their description of the experiment carried out by the Franklin commission set up to investigate animal magnetism is accurately outlined. It was known from the very first years that effects of animal magnetism could, at least in some cases, be attributed to suggestion, and this possibility is mentioned in the earliest magnetic literature. The observation of Cardeña and Terhune about my take on Orne's experiments, however, puzzles me. It must be obvious that Orne's experiments to see which responses were due to hypnosis and which to prior knowledge surely are geared to attempt to ascertain whether hypnosis is genuinely present in any particular responses.

Commenting on trance, Cardeña and Terhune misunderstand my view of the field of hypnosis at present. I do not say, as they seem to imply, that there has been no progress in the field of hypnosis. On the contrary, I consider the work on hypnosis, both experimental and clinical, that has been carried out over the past 200 years to be most impressive and to have contributed important benefits to human science. However, I do claim, along with Weitzenhoffer, that the field is presently in a state of disorder. Unfortunately, it appears that Cardeña and Terhune are so intent upon defending the reputation of experimental hypnosis as conducted up to this point (although I do not see the need for such a defense) that they seemed to miss the substance of what I was saying.

The view of Cardeña and Terhune that my account of trance is "somewhat novel" is puzzling (especially considering that later they will claim there is nothing new in my proposal). Of course associating hypnosis with trance is not novel at all. Many have done it—most notably Milton Erickson. What is somewhat novel is the combination of elements I include in my definition of *trance*. Also, I define *hypnosis* in a very specific way as a subspecies of trance: an inner-mind trance that includes rapport. Proposing this definition of *hypnosis*, situated within my very specific meaning of *trance*, is, I believe, indeed novel.

It seems that Cardeña and Terhune take my call for a "fresh start" in defining *hypnosis* to mean I am talking about starting from scratch. It must be obvious from my article that I do not take that attitude. My ideas arise from and depend upon the whole rich and fruitful tradition represented in the hypnotic literature.

I find it interesting that Cardeña and Terhune object to the use of the word *trance* on the grounds that, in addition to the ancient meaning of the word that I use as my starting point, many other meanings have, over the ages, been ascribed to it. I chose the word to provide a starting point for clarifying the state of affairs in the hypnotic literature that has led to so many differing and contradictory definitions of *hypnosis*, which are difficult for the intelligent reader to make sense of. Having made that decision, I clearly defined the meaning of *trance* that would be most useful to the task at hand. There is nothing vague in my statement of its meaning in this context.

Cardeña and Terhune ask whether hypnosis, as I define it, only involves a narrow internal focus and specifically whether this means that focus is all there is to hypnosis. I attempted to clarify this issue in my treatment of the crucial role of "rapport" in my definition of *hypnosis*. I believe that rapport involves, in an essential way, the hypnotic subject's incorporation of the hypnotist into the focus, so that the hypnotist becomes an internal presence. From this it naturally follows that suggestions given by the hypnotist are experienced as coming from within and are, therefore, very influential. I must disagree with Cardeña and Terhune that this approach excludes external stimuli from the hypnotic experience. The hypnotist may, and often does, draw external stimuli to the attention of the subject. In so doing, these too are incorporated into the subject's internal focus. Later in their comments, Cardeña and Terhune say,

> Participants experience the suggestions as coming from the hypnotist; this, in turn, produces the extra-volitional phenomenology of hypnotic responding—the experience that the responses are controlled by an external agent rather than by the person him/herself. (Cardeña & Terhune 2012:334)

I agree with this statement, for subjects do experience suggestions as extra-voluntary. However, this evaluation on the part of the subject is in terms of his or her conscious understanding. This does not rule out the possibility of a subconscious evaluation of a different kind, as Pierre Janet's experiments with hysterics demonstrated (Janet 1889).

Cardeña and Terhune claim that I confound spontaneous and suggested responses, and that I do this throughout my article. This is a reading that I find difficult to respond to, since a substantial part of my explanation deals with the broader category of trance rather than the more restricted one of hypnosis. However, I would say that in my approach to trance states (one type of which I define as hypnosis), I indicate that both spontaneous and suggested responses occur. To say they both occur is not to say there is no difference between them, as Cardeña and Terhune seem to imply.

When considering what Cardeña and Terhune say in their comments

about suggestion, I am reminded of why I wrote this article. In these paragraphs they illustrate precisely what I think is confusing and misleading in discussions that take place around hypnosis. They ask the question, "Why are high hypnotizables also highly responsive to suggestions outside of a hypnotic context?" First of all, I find problems in the use of the term *high hypnotizables*. The notion of high hypnotizables is derived from and determined by a way of defining *hypnosis* that in my opinion simply does not work, for very good reasons. I find it surprising that Cardeña and Terhune say little about one of the main contentions in my article: that "hypnosis" simply cannot be defined in terms of lists of hypnotic phenomena. In contemporary literature, every attempt to experimentally determine the presence of the hypnotic state in the experimental subject is based on some version of a list of phenomena which the subject is supposed to manifest. And it is from this definition, and from the identification of the chosen phenomena in the behavior of the experimental subject, that the experiment is said to be about hypnosis. Without attempting to recapitulate my treatment of this important issue, let me here point out that when one considers the history of hypnotic literature, it is clear that these lists are made up of phenomena that are arbitrarily selected from among many that could have been chosen. As such, they do not and cannot constitute a dependable basis for identifying the presence of hypnosis. When "high hypnotizables" are identified, it is on the basis of these criteria. For that reason, when statements are made about the characteristics of "high hypnotizables," I do not experience great confidence in the claims being made about them. To respond to their question thus formed, let me also ask: How does one determine what a "hypnotic context" is, except through the application of the same questionable phenomena-list criterion already mentioned? So I would respond that, given my objections, their question simply cannot be answered in its present form.

The query of Cardeña and Terhune about the role of individual differences in hypnotic experimentation is subject to the same criticism. I must ask how does one reliably determine responses based on individual differences resulting from "hypnotic induction" when there is no reliable way to know that a hypnotic induction has taken place? Again, I must express my dismay that Cardeña and Terhune have not in any way responded to this central point of the article: that the definition of *hypnosis* that is current in experimental hypnotic work is confusing and impossible to fruitfully employ in precise discussions of hypnotic experiences. Instead their criticisms are largely from a point of view apparently based on full acceptance of what I find unworkable. I would like to have heard some plausible reasons for denying my position. Instead, it seems to me, it is dealt with by ignoring it.

With regard to "variability across contexts," I am surprised to see that Cardeña and Terhune seem to be unaware of the context in which I said "what is true of the clinical setting must be equally true of the experimental." It is obvious that the "what is true" phrase referred to the fact that an expanded notion of demand characteristic must be applied in both areas, and that this was not a general insistence on the equivalency of clinical and experimental situations.

I am not surprised that Cardeña and Terhune may have misunderstood my statement about state-dependent memory, for I did not elaborate my views about post-hypnotic amnesia in this article. In establishing my position I was not relying on intuition, but the literature of state-dependent memory (e.g., Overton 1964, Fischer 1971, Eich, Macauley, & Ryan 1994, Woike, Bender, & Besner 2009). My particular view is that, in contrast to the almost ubiquitous presence of post-hypnotic amnesia in the first decades of magnetic somnambulism, the modern incidence is extremely low, and I fully concur with this finding of Hilgard and Cooper. However, I believe there are sociological reasons for this decline of post-hypnotic amnesia over the past two centuries. This does not affect the validity of the notion of state-related memory or militate against my belief that this phenomenon operates in many situations in ordinary life.

Cardeña and Terhune rightly point out that a great deal of research has been done concerning various important factors that affect the form of hypnotic phenomena, including conscious and unconscious personal and cultural expectations. My emphasis on the importance of these factors was not meant to imply that no one had taken notice of them over the long and winding road of hypnotic history. My concern was that although such elements have been here and there acknowledged, I do not believe that they have been adequately taken into account in the procedures of hypnotic research in general.

I must take exception to the claim of Cardeña and Terhune that I deny that "there are substantial behavioral, experimental, and physiological differences in the hypnotizability of individuals." This is simply not true. That was not said in my article. Such a position is both contrary to what I believe and is in no way implied in my redefinition of *hypnosis*.

Although Cardeña and Terhune do not care for my use of the term *subliminal resources*, I must point out that this term is not new and the reality to which it refers, as expressed in the article, was often discussed by Frederic Myers, William James, Pierre Janet, Morton Prince, and many other experimenters in the era in which psychodynamic psychology was taking form. The fact that recent hypnotic research has not followed through with these important early insights is not a reason to deny their usefulness.

When Cardeña and Terhune take issue with my extending the notion of trance to everyday phenomena, they say that this dissolves hypnotic phenomena into irrelevance. They say that "if they are an aspect of every experience, there is no reason to even suppose that there is a distinct domain of hypnotic phenomena, and there is nothing special about them." In expressing surprise that anyone might hold such a position, Cardeña and Terhune show that they have missed the whole tenor and central argument of the article. There I took pains to point out that the way the domain of hypnosis has been determined over the past decades has been through applying canonical lists of phenomena that supposedly set hypnosis off from other conditions. But this way of determining the domain of hypnosis simply cannot work. I will not repeat here the discussion in which I give reasons for this position. But in taking this position I did not intend to say that therefore hypnosis has nothing distinct about it. On the contrary, I gave *hypnosis* a definition that makes it fully distinct from every other human state or condition. It is my belief that this is the first definition that actually accomplishes this task.

As to the belief of Cardeña and Terhune that my explanatory approach is unfalsifiable because "one could always envision some type of 'unconscious rationale'" that would account for the hypnotic response, this would be true only if there were no possibility of determining which specific psychological motivations may be operating subconsciously in individual subjects. I have to admit that I am more sanguine about making such a determination than Cardeña and Terhune may be.

Cardeña and Terhune contend that I make a "categorical mistake" in the way I use *subconscious* and *unconscious*. Much psychological and philosophical ink has been spilled over this complex problem, and I do not believe it is possible to do justice to that difficult discussion here.

It can come as no surprise that I disagree with the Conclusion of Cardeña and Terhune. To evaluate my position without even acknowledging, much less responding to, my concern that conventionally accepted definitions and domain identifications of hypnosis are fatally flawed, leaves me perplexed. It would have been of great relief to me had they provided me with good reasons for believing that my worries were misplaced, but I am afraid they have denied me that comfort. Rather, it is as if I had never spoken; so I can only conclude their counsel to be that if one pretends the disease it not there, perhaps one will be cured of it.

With regard to their referring to my definition of *hypnosis* as "vague" and "simplistic," I can see no justification for such a view. My definition is just the opposite of vague; it is clear, unambiguous, and definite. On the other hand, I cannot imagine anything more vague by way of definition

than the one I call into question in my article: that hypnosis is a condition of some uncertain type that is defined by its manifesting an artificially limited number of arbitrarily chosen and canonized hypnotic phenomena which have been selected from a vast array of phenomena which show up not only in the hypnotic literature, but also in non-hypnotic states. This, it seems to me, makes a rather shaky foundation for experimental work, while at the same time providing little help to the clinical worker, especially those engaged in psychotherapy. As to the statement that my definition is simplistic, it is worth pointing out what that word means in this context. *Simplistic*, when applied to a scientific theory, means "unable to deal with the full richness of the data involved in the field in question." That certainly does not apply to my theory. I might add, however, that it is indeed "simpler" than that one that has been in vogue. But simplicity should not be a mark against a theory. The criticism of a theory should, on the contrary, be based on its inadequacy to the data. I believe that simplicity is in fact an advantage, particularly if it helps to bring clarity to a field of endeavor that finds itself in some disarray. I might add that it is precisely such simplicity that, when developed with a view to experimental confirmation, should facilitate devising means for empirically deciding its worth.

Cardeña and Terhune worry that non-specialist readers may assume that my account is a fair description of the field as it currently stands. The great American philosopher and scientist Charles Sanders Peirce wrote,

> We may as well acknowledge it, [scientific men] are, as such, mere specialists. . . . We are blind to our own blindness; but the world seems to declare us simply incapable of rising from narrowness and specialism to take broad view of any facts whatsoever. (Peirce 1935:376)

Cardeña and Terhune seem to take the position that only those who are themselves engaged in hypnotic experimentation (the specialists) are in a position to make a judgment about what is happening in their field, and that therefore those who are not (the non-specialists) must be content to let their opinion in the matter be formed by those who see themselves as spokespersons for their specialty. This is a kind of scientific puffery that is, unfortunately, more common among scientific writers than one might hope (Lewontin 1997). I believe it is important for specialists to have some confidence in the intelligence of non-specialists. My position, expressed in this article, is that although hypnosis research has provided valuable insights into the nature of human experience, it has at its core a problem which, while not totally invalidating what has been done so far, urgently needs to be corrected. I do not expect this to be too great a shock for intelligent non-

specialist readers, nor would I think it beyond their capacity to treat this discussion as a stimulus to expand their readings to include other relevant literature.

I must agree with Cardeña and Terhune that this proposal could have been written in the 1950s; unfortunately, it was not. The problem was already evident at that time, but it seems that my particular dissatisfaction and my specific solution were not yet in evidence.

I would like to make one final comment on the response of Cardeña and Terhune. It seems to me that knowledge of the history of hypnosis includes knowledge of the research that has occurred during its course. Although Cardeña and Terhune may not agree with my identification of problems relating to certain aspects of more recent hypnotic experimentation, sometimes the wider view of the historian and clinician can provide a perspective on the key issues that may be denied one with too great proximity.

Beere's Comments

In contrast to the approach of Cardeña and Terhune, Beere's comments respond directly to my concerns about the understanding of hypnosis currently in vogue and also to the substance of my proposed alternative approach. His comments and questions are stimulating and are the kind of thoughtful reflection that is needed to create a dialogue which can, in his words, "assist in furthering his [Crabtree's] theory, a task well worth the endeavor." It is with a sense of that dialogue that I respond to his comments in some detail.

Terms and Definitions

The most important terms to be defined are *trance* and *hypnosis*. My intention in the article was to define these terms as clearly as possible so that the ensuing discussion could be as free of ambiguity as can be reasonably expected.

My definition of *trance* is: a state of intense focus on something, accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources. I have been developing this definition for fifteen years and it has undergone several revisions. But from the first it was inspired by a definition of *trance* from *Webster's Collegiate Dictionary*: a profound state of absorption or abstraction. Since I considered *absorption* and abstraction to be complementary terms, my first definition was "a state of absorption and abstraction," in which my meaning of *absorption* was "focus" and *abstraction* "diminished awareness." I realize that there are

many meanings of *trance* that have developed over the past five centuries or so. My intention in working out this definition is to distinguish what I mean from all other meanings of the word. For that reason, when Beere asks how *trance* as defined in other places is included in my definition, I have to say that those other meanings are not what I intend, and I certainly do not mean to say that my definition includes them. So when I use the word *trance* in this article, I mean it strictly according to my specific definition. This will, I hope, prevent the reader from thinking that I am going to discuss any other usage.

My definition of *hypnosis* is: an inner-mind trance characterized by rapport. Whenever I talk about my usage of that word in my proposed framework it always has that meaning and that meaning only. Others use the word with many other meanings. My explanations concern only what falls within my specific definition. Trance is the broader category; hypnosis is only one type of trance. I am concerned that in several places Beere seems to believe that I am saying that all *trance states* are a type of *hypnosis.* That is certainly not my meaning. Trance includes all instances of hypnosis, but hypnosis does not include all instances of trance. I also want to emphasize the crucial importance of the use of the word *rapport* in my definition. Any inner-mind trance that does not involve rapport is not hypnosis, in my meaning of the term.

Clarifications

Beere raises an important issue when he points out the problem of establishing the domain of hypnosis. We must be critical in accepting which reported cases of hypnosis in the literature of the last two centuries were indeed hypnosis and in deciding whether the phenomena reported actually occurred, as opposed to being the result of bad observation, fraud, etc. In this I fully agree with Beere. Moreover, the difficulty in establishing the domain of hypnosis is precisely what led me to write my article. I did not state that all the reports of hypnosis were genuine, but, quite the contrary, that the list of the phenomena connected with hypnosis has evolved over time and that no particular list can be considered canonical in deciding what hypnosis actually is. So there is no way that I or anyone else can be assured that the phenomena reported over the past two hundred years are in fact hypnotic. The approach that I use in my paper does not depend on making such a judgment, but emphasizes that fact that many researchers *do* make such judgments, without realizing how arbitrary those judgments are.

Beere is correct in pointing out that I did not develop my article in the direction of explaining differences in hypnotizability, but I will say something about that here. Differences in hypnotizability in my theory are due to differences in the ability of subjects to achieve a state of inner focus

while in rapport with a hypnotist. Rapport is a key element of hypnosis, and there can be many reasons why that rapport may vary from one hypnotic situation to another. These reasons include, among others, the possibility that the subject cannot achieve rapport because a history of abuse, for example, makes openness to that kind of connecting (which involves a degree of trust) difficult, or the possibility that the hypnotizer, for various subjective reasons, is better at establishing rapport with some types of hypnotic subjects than others. I believe such factors may be so inhibiting that some individuals may not be hypnotizable at all. But this in no way means that those same individuals will not be subject to *trances* in my definition of the term, particularly those trances we experience in everyday life. It is important to distinguish between hypnotizability and the capacity to go into non-hypnotic kinds of trances.

I agree with Beere that during induction there is a transition from a non-hypnotic state to a hypnotic one. That transition occurs when the everyday trance with which the person begins (the non-hypnotic state) is disrupted and replaced by another focus that engages the person in his or her inner world (in this I agree with Charles Tart's description of the hypnotic induction process, Tart 2008). A further step then occurs in which that new focus is replaced by a more or less passive state that awaits automatisms from the subconscious mind, which provide a new, engaging focus. For a psychotherapist, that is the moment at which hypnotherapeutic work begins.

I must comment on Beere's concern when I refer to Erickson's use of the word *trance* as a synonym for *hypnosis*. I have learned a lot from Erickson, but my usage of the terms *trance* and *hypnosis* are not identical to his. He sees these terms as synonyms. I certainly do not. That is why I have taken pains to make their respective definitions as clear as possible. Beere seems to mistakenly think that I agree that they are synonyms and from this misunderstanding he wonders how *hypnosis* can be applied to the many possible meanings of *trance* that he cites from his online dictionary. The term *hypnosis* cannot be applied to any of many other kinds of trance that may exist. It can only be applied to one kind of trance—that which I have spelled out in my definition of *hypnosis*. For that reason, he is also mistaken when he says that I see "'trance' as a core definition of 'hypnosis'." He continues, "In this regard, I found myself asking, 'How is trance different from hypnosis?'" These statements show that he continues to confuse the two and is not mindful of the clear definitions I have given them. I see trance as the broad category which has many subspecies, only one of which is hypnosis. So Beere is correct in saying that trance includes hypnosis and also many other kinds of mental states. But hypnosis is not an example of those other kinds of states.

In this context, I must disagree with Beere that what is called *zoning out* does not involve focus. The unresponsiveness to the environment that characterizes that state is due to some real, although perhaps not verbally expressible, inner focus. I believe that the same would apply to those states Beere refers to with the terms *dazed* and *half-conscious*. I would add that I do not apply the word *trance* to states of total unconsciousness, as Beere seems to think I do.

I believe it is because of these misunderstandings of my use of the terms *trance* and *hypnosis*, that Beere has arrived at some mistaken conclusions about the implications of my proposal. He asks whether my definitions of *trance* and *hypnosis* add clarity to the discussion. If his understanding of my proposal were correct, they probably would not. But it is not correct, and given my actual usage of those terms, I would have to say they do add clarity. In my proposal it should now be clear precisely what hypnosis consists of and how to recognize it not only through its observable phenomena, but also, and most importantly, through its subjective experience.

With regard to my phrase "the evocation of appropriate subliminal resources," Beere wonders whether this is the equivalent of the commonly used "activating unconscious responses via hypnotic procedures." In my proposal, these are not equatable because my phrase refers to the definition of "trance" in general, whereas the latter phrase refers only to hypnotic trance.

Beere presents a very thoughtful examination of my notion that trances are everyday phenomena. He wonders whether my definition of trance accurately describes day-to-day experience. His first question has to do with my use of the word *intense*. He asks whether, as he goes about his daily activities, his focus on various things could be described as intense. Beere's question gives me the opportunity to clarify this matter here. I conceive of "intense" as admitting of degrees, of greater and lesser intensity. I wonder, however, about Beere's description of the experience in which his thoughts are characterized by a "generalized wondering." He describes shifting from inner thoughts occurring as he stares at the wall and furniture, ending with a plate resting on a stand, saying that the room was present all along and only now does he really notice it. He says that, according to me, he was not in a trance until he actually focussed on the plate. I would, in fact, not say that. I would say he was already in a trance, but the focus of that trance was his inner thoughts. His trance shifted when he began to notice the plate, and left his inner mulling aside. This is the kind of shifting from trance to trance that I describe at some length in the article. When Beere talks about being aware of something in a "diffused" way, he seems to be using that word in two senses. In one sense he seems to mean that what he is diffusedly aware

of is what I could call something on the "fringe" of attention, in precisely the way *fringe* and *focus* were used by William James (James 1890). But "diffused" attention for Beere also seems to be used to denote an awareness in which the focus is of very low intensity. In my way of looking at things, a "diffused" or "floating" awareness, when used in this latter sense, does not refer to having *no* focus, but a low degree of focus which moves rapidly from object to object until something really grabs the attention, at which point the focus is more intense.

Just what might this focus/fringe experience look like? Let us say I am staying at an old inn on a lake. In the morning I look at myself in the antique bathroom mirror. I wonder if my beard needs a trim. I examine it closely and decide it does. I turn and reach for my trimmer, and when I look back at my reflection in the mirror, I notice that the mirror has a number of small spots where the silvering has disappeared—a clear sign of age. I had not noticed the spots before; all I saw was my face and beard. If I had been called out of the room as I reached for my trimmer and asked whether the mirror was suitable and clear, I would have responded, yes. But now, for the first time I notice the spots and turn my attention to them. I look closely at them and note their positions, their shapes, their color. I see that they form a peculiar pattern—an arrangement of distinct triangles—that interests me. As I focus on the spots and the patterns that they form for me, I become more and more absorbed in them. For a moment I become so focussed on the mirror and its defects, I have almost no awareness of my beard and my face reflected in the mirror. I continue to concentrate on those odd spots and speculate how old the mirror is, whether this kind of defect occurs in all older mirrors, and what are the chances that this fascinating pattern of spots would eventually repeat itself in a mirror of the same manufacture. In the meantime, not only have I lost awareness of my beard and face, I do not notice the passage of time, and even momentarily forget who I am—I am totally focused elsewhere. Now I began to realize that I have totally lost awareness of my face, even though I am staring at its reflection. I have also lost awareness of the trimming project I had set for myself. My initial scrutiny of myself in the mirror has shifted to a fascinated examination of the spots on the mirror I am looking into. My focus has shifted, and my face and beard now form the fringe of my awareness. Now I am intrigued by what has just happened and reflect on the general fact that what I concentrate on at any moment is my focus, and what I am not concentrating on becomes the fringe of my awareness. These reflections become a new center of focus and now both beard and mirror are pushed to the fringe of my awareness. I can shift at will the focus/fringe structure of my experience: Now beard as focus with the mirror (as such) as fringe; then it is the mirror with its spots as focus with

my beard as fringe; and finally, my thoughts about *focus* and *fringe* take over as the center of attention. With each shift of focus the previous focus is forced to the periphery of my awareness and my awareness of it diminishes. Each new center of attention creates a new set of fringe elements. Expressed in terms of my specific definition of *trance*, I would say that each new focus creates a new set of things of which I have a diminished awareness.

Beere thinks that I "vacillate" about whether everyday experience is mostly trance and only partly so. This impression apparently arises from my statement: "Trances are part of everyday life." My meaning was that one aspect of all everyday experience is trance. I thought my meaning would be clear, but I am glad to have the chance to remove any ambiguity.

I am puzzled by Beere's reading of my text when he says, "Let us grant, for the sake of argument, that hypnosis is trance and that hypnosis is an inner-mind trance that can include all possible everyday experience." True, hypnosis is a sub-species of trance, but it in no way follows from this that hypnosis can include all possible everyday experience. Hypnosis is only one, very specific kind of trance, one rarely experienced in ordinary everyday living. The conclusions Beere draws from this misreading of what I wrote cannot stand.

Beere then suggests that hypnosis can be trance without the requirement or implication that everyday experience also is trance. That might be hypothetically true, but it is not consistent with my proposal. Given my definition of *trance*, everyday experience must also be instances of trance.

When I point out that all hypnotic phenomena occur in some form or other in ordinary life, I am simply echoing the problem, recognized for many years now, that there is no phenomenon that is attributable to hypnosis and hypnosis alone. I did not mean to make a judgment about how *frequently* they occur in everyday life, as Beere seems to think, only that they do occur in everyday life. Beere then says, "This suggests to me that there must be something in the everyday circumstances that evoke 'the hypnotic response'." I am afraid that once again Beere attributes to me assertions that I have never made and with which, in fact, I disagree. I do not believe that everyday experiences evoke the "hypnotic response." Again Beere seems to think I am saying that all everyday experiences are hypnotic. This is contrary to what I hold and contradicts what I have written. My position is that everyday circumstances evoke "trance" responses, very few of which will be "hypnosis" (unless, for example, the individual involved is in hypnotherapy). For that reason, Beere's subsequent "reconstruction of the underlying logic" is incorrect.

Beere says that in reading my article he seems to "find himself caught in definitions that point back and forth to themselves such that they explain

the phenomena by fiat and are exempt from further consideration." I can only conclude that this feeling arises from his misreadings of what I have written, which I have pointed out above. There is no circular logic in my actual exposition.

Beere's discussion of "absorption" as I have used it with regard to trance is very helpful. In my earlier writings, I used "absorption" as an element of my definition of trance. I no longer do. In psychological writings the term *absorption* has become a technical term. I think it is problematic to use that term in its technical sense as a defining element. For that reason, I think that Beere's criticism of my use of the term in this context—even though it is not part of my definition—is justified, and for that reason I will no longer employ it in my discussions of the nature of trance.

I also find Beere's discussion of my use of the term *inner mind* useful. Following his suggestion, I would like to clarify my meaning of *inner mind* and *outer mind*. I use *inner mind* as a phenomenological term. The inner mind operates in the world of interior impressions. Its meaningful reality consists of the thoughts, imaginations, fantasies, memories, feelings, and emotions that we experience as occurring in the mental world we describe as private. We experience them both when awake and when dreaming. When we focus on any of these things, we establish an inner-mind trance. There are many other kinds of inner-mind trance besides hypnosis, such as meditation, daydreaming, and worrying. We experience *conscious* awareness of our inner world, but we also discover that there are mental dynamics operating outside our normal awareness. This subconscious aspect of life operates dynamically to reveal itself in conscious awareness in various ways, and the boundary between the contents of subconscious and conscious awareness continually shifts. Insights about these interactions make up the foundation of what Ellenberger called dynamic psychiatry (Ellenberger 1970), and the history of psychodynamic psychotherapy reveals how these insights evolved (Crabtree 1993, 2003).

The outer mind, phenomenologically speaking, is the aspect of our mentality that experiences the world as publicly available and is largely active in the practical aspects of daily living. It is "in its element" in the physical, social, interactive environment of our lives. The public world is its home, its theatre of operations, the place where it is active. The job of the outer mind is to find the best way to deal with worldly affairs. The outer mind's meaningful reality is not just the physical world and its occupants, but also the expectations, rules, and protocols that operate there.

Beere reaches a false conclusion when he presumes my views are based on a mind–body dichotomy. My phenomenological approach to "inner mind" and "outer mind" experiences makes no such metaphysical presumptions.

I have no problem with Beere's belief that hypnotic induction can begin with an outside focus. However, at some point that focus must shift to the inner world, otherwise hypnosis will not take place.

Beere's discussion of suggestion is also helpful. I agree that suggestions result in phenomena that are experienced as occurring without conscious intention on the part of the subject. Suggestions are effective in hypnosis. But as a matter of fact, as Hippolyte Bernheim said, they also occur in waking life and phenomena such as paralysis and anesthesia, and hallucinations can be obtained through suggestion without hypnotism (Bernheim 1884). There is controversy about whether a person is more suggestible in hypnosis than in waking life, but in this I concur with Beere that suggestion is more effective in hypnosis.

Beere makes a good point in saying that, in the example of the railway worker, his experience of having no pain when his toes were amputated would, on subsequent reflection, be considered alien or strange. However, his comment that "these observations make clear that hypnotic phenomena are not everyday experiences at all," is another example of his misreading of my statements about the matter. I have never made the point that *hypnotic* experiences are everyday experiences, only that *trance* experiences are everyday experiences.

To a certain extent I agree with Beere that the flow of experience presumes some kind of "meta-state" or context that is more inclusive. I see that meta-state as required to provide our experiences with a unity. The work of Janet with hypnosis in the late 1800s led to the positing of some rock-bottom, fully inclusive awareness that brings all the piecemeal experiences of various hypnotic states together, and he believed that it would theoretically be possible to reach a perfect subterranean stream of consciousness which would embrace the whole conscious life of the individual (Janet 1889:335).

In my article, I discuss the fact that in trances of ordinary life we direct our attention to things that become the object of focus—that conscious intention is involved. Beere seems to think I said that is what happens in hypnotic induction. That is not what I wrote. The place of automatisms in hypnosis (and in everyday trances) is central. I have discussed elsewhere the matter of automatic responses in hypnosis and other altered states (Crabtree 2007).

Beere describes his experience of going into a "hypnotic state" without any rapport being involved. I have no doubt he went into a self-induced inner-mind trance state. But it is not what I would call *hypnosis*.

My notion of the importance of rapport in hypnosis was not conceived in an attempt to explain why suggestion works, as Beere surmises. Rather

it derived from my study of the history of animal magnetism/hypnosis. Rapport was first mentioned and researched by Puységur in 1784, and it has remained a central feature of the history of hypnotism ever since. The fact that rapport can help make sense of the effectiveness of hypnotic suggestion is something I became aware of only in recent years.

Beere is right in describing Erickson's view of suggestion in terms of receptivity. In fact, when Erickson observed that a person was a good listener and attentive to others, he knew that person would be a good hypnotic subject. When a person is in a hypnotic state, he or she will experience automatisms, some of which may come as the result of suggestion. But I do not believe, as Beere thinks I might, that for suggestion to work rapport is necessary. I do recognize the power of suggestions elicited in the non-hypnotic or "waking" state.

To answer Beere's question, when I talk about the hypnotic subject incorporating the hypnotizer into his inner focus, I do not mean to say that this occurs bodily. Neither do I believe that the hypnotizer is experienced as an extension of self. Rather there is a sense of intimate presence in the same inner space. Beere seems to think that I have said that, because of the special sense of connection, suggestions should infallibly work. My only contention is that suggestions are *more likely* to be successful.

Commenting on my notion of group-mind trance, Beere again shows that he confounds trance and hypnosis. I do not talk about group-mind hypnosis; that notion does not make any sense to me. Rather I talk about group-mind trance.

When Beere describes his experience of sensing what things would be like for a child between three and four, he says that he diffusedly focussed on his body, discovered various sensations and other processes arising, he became aware of pictures that seemed to fit that age span, and experienced emotions and sensations that fit that age. He says that this experience showed that one did not have to be in a trance state to carry out this exercise. My conclusion is the opposite. What he describes is focus, followed by the coming forward of various sensory and motor automatisms in response to that focus. This is precisely what I mean by the evocation of appropriate subliminal responses in the trance state. Similarly, EMDR seems to me to involve a trance state. Clients focus briefly on the issue they are concerned with and then let it go and allow to happen whatever happens. To my way of looking at things, this is an example of what I define as a trance state, complete with evoked subliminal resources.

Beere's discussion of figure–ground distinctions in perception is intriguing. He finds this way of describing perception helpful and says that he notes similarities between this approach and William James' focus–

fringe idea. Beere takes this understanding of perception as the basis for his theory of dissociation. In his theory, dissociation "arises when someone attends with such intensity that background features are blocked out and thus experienced dissociatively." I find his ideas about dissociation intriguing and consonant with my own therapeutic work with dissociative disorders. He states that the experiences of dissociation are also those that occur in hypnosis. He also says that Dell states that dissociation is involved in every kind of human experience, and notes that this is "an observation Crabtree makes about hypnosis." Unfortunately, once again Beere attributes to me a position that I have never taken. If he is to accurately express my position, it should be phrased, "an observation Crabtree makes about *trance*."

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I would like to conclude my responses with a comment on Beere's statement, "Unfortunately, as I considered various elements of his theory, almost every one had a practical, theoretical, or logical flaw." I believe that most of what Beere considers flaws in the theory are in fact due to his misreading of the text. I have pointed out many instances where this misreading occurred. In fact, in reading Beere's comments I have formed the impression that we actually agree on many things with regard to the phenomenology of hypnosis, but that certain key misreadings of my text have obscured that basic agreement. It is my hope that we may some day have the chance to discuss these things more thoroughly. For now I will be content if our exchange helps to clarify my views about hypnosis for the reader.

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HISTORICAL PERSPECTIVE

The Sorcerer of Cobenzl and His Legacy: The Life of Baron Karl Ludwig von Reichenbach, His Work and Its Aftermath

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Abstract—Karl Ludwig von Reichenbach was a well-known and controversial personality in the 19th century. The controversies largely centered on his theories concerning a universal and all-permeating force he claimed to have discovered—the "Od." In this article, I highlight important events in von Reichenbach's life and his explorations into the frontiers of science. Subsequently, I present an overview on lines of experimentation that have addressed two of his propositions, namely (a) that the effects of Od can be directly detected by macroscopic movements of objects such as compass needles, and (b) that (electro-)magnets can be detected visually in the dark due to the emission of odlight.

Keywords: Reichenbach—Od—magnet—compass—light—visual perception

The more inexplicable a phenomenon appears, the deeper is it rooted, the greater is the significance it bears, the bigger is the interest adhering to it, and the more pressing is the challenge for science to examine and to explain it.

— Karl von Reichenbach (1854–1855(1):xxvi)

Introduction

This paper presents an historical overview of the life and work of Karl von Reichenbach (1788–1869) and some of the attempts to replicate his findings. Von Reichenbach was a well-known and controversial personality of his time, the controversies largely centering on his theories concerning a universal vital principle or force he claimed to have discovered, the "Od." He developed his theories in numerous publications between the years 1845 and 1867. Von Reichenbach derived the word "Od" from the ancient Germanic all-permeating principle termed *Wodan*, also modulated into *Odan* and *Odin*, which as well was personified as the Germanic god (von

Reichenbach 1852:198). Thus, the Od in von Reichenbach's writings refers to a dynamic principle permeating all nature.[1]

Although the overall reaction of academia toward von Reichenbach's claims was rather negative during his lifetime (e.g., Braid 1846/1970, Fechner 1856, Gouge 1846, Vogel 1863, Vogt 1854:322, von Liebig 1852:18f), his writings were very influential on later developments in the context of mesmerism, animal magnetism, and spiritualism during the second half of the 19th century. For example, Mesmerists implied that the discovery of Od validated the concept of animal magnetism (e.g., Lee 1866), and authors such as Beecher (1853), Brittan and Richmond (1853), and Rogers (1853) cited von Reichenbach's work in their discussions about forces that may account for the phenomena of physical mediumship. In France, Albert de Rochas popularized the concept of Od and associated it with the exteriorization of sensitivity and to the idea of the "double," a replica of the physical body thought to consist of subtle energies or matter that can be separated from the physical body (de Rochas 1895/1909). In the German-speaking countries, von Reichenbach's concept of Od continued to be widely adopted in theories about life and spiritualism until the end of the 19th century, most notably by the influential philosopher of spiritualism Carl du Prel (e.g., du Prel 1899), but were also promoted after the turn of the century (Feerhow[2] 1914, Kröner 1938, Quade 1924). Numerous other authors from the contexts of spiritualism, psychical research, but occasionally also from mainstream science, tried to experimentally replicate the findings of von Reichenbach. Some of them will be introduced in this paper; for a brief overview on Od and related concepts of human radiations see also Alvarado (2008). Von Reichenbach seemed largely unaware of the literature on mesmerism, somnambulism, and spiritualism when he started to get involved in research into Od, but quickly learned that it had much in common with these older concepts that also implied a universal vital principle which permeated everything and could be utilized by human beings. However, he distanced himself from these earlier concepts and mentioned them only occasionally, usually very critically. He regarded them as largely ill-founded and confusing, shrouding a true core with a mass of useless if not misleading details which had been built on inadequate experimental methods. His aim was to elaborate a theory of Od solely on the grounds of his own findings, and he claimed that only in the framework of his new system would parts of the older theories find their proper place in science (e.g., von Reichenbach 1854–1855(1):xxixff).

In recent decades, von Reichenbach's work has not received much attention and is usually discussed from a historical perspective (Alvarado 2008, Bischof 1995, Erdbeer 2008, Ingensiep 2001, but see also Baldwin 2006).

Karl Ludwig von Reichenbach, about 70 years old.

At present, details of von Reichenbach's life and writings seem to be not well-known among researchers actively exploring the frontiers of science, let alone among mainstream scientists. Moreover, many of the attempts to replicate von Reichenbach's findings seem forgotten today. In this paper, I present a brief overview on his life and on two particular lines of investigation that I consider of interest, namely that (a) effects of Od can be directly observed by macroscopic movements of objects such as compass needles, and that (b) (electro-)magnets can be detected visually in the dark due to the emission of odlight.

The Life of Karl Ludwig von Reichenbach (1788–1869)

Early sources providing biographical data of the life of Karl Ludwig von Reichenbach include Schrötter (1869) and von Wurzbach (1873); a recent biography including a list of further references and a bibliography was compiled by Ferzak (1999). Reichenbach was born on February 12, 1788, in Stuttgart (Germany) and died on January 19, 1869, in Leipzig (Germany). Between these dates spans the eventful life of a creative and spirited man who climbed the highest peaks of scientific reputation and financial prosperity, but died lonely and comparably poor in a hotel room far away from his former home. Karl was the eldest of four children. After some unsteady years in early manhood, he married his only wife, Friederike Luise Erhard. She gave birth to five children, all of whom died without giving birth to children of their own. Because his wife came from a wealthy family, Reichenbach took the opportunity to study fields of interest to him before he determined his own professional future. The fields he found most fascinating were metallurgical processes and the carbonization of wood. Soon, he developed an industrial oven to burn wood faster than traditional

models did while simultaneously improving the quality of the produced coal—an achievement that earned him much respect from experts in the field and resulted in further occupations. In 1821, Count Hugo von Salm-Reifferscheid (1776–1836), owner of large metallurgical factories in and around Blansko in Moravia (then in the Austrian Empire), employed Reichenbach as the supervisor of manufacturing operations. The factories prospered and enabled Reichenbach to purchase several estates and additional industrial plants. With well-equipped chemical laboratories at hand, Reichenbach commenced pioneering studies analyzing byproducts of wood carbonization, namely tar, from 1825 onward. He discovered and identified a number of important substances, most notably paraffin, but also several other substances such as creosote (a mixture of antiseptic phenols), the gasoline-like eupione, and pittacal (the first synthetic dyestuff to be produced commercially). Between 1830 and 1836, Reichenbach published 23 papers on organic chemistry in the most noted German chemistry journals, thus substantiating his reputation as a gifted chemist. He maintained good contact with leading chemists of his time, including Jöns Jakob Berzelius (1779–1848), Friedrich Wöhler (1800–1882), and Justus von Liebig (1803–1873). Reichenbach's interest in chemistry declined thereafter and was followed by passionate explorations into two other fields of research: first, studying meteorites, and subsequently the Od. His interest in meteorites was raised in 1833 after a meteorite had crashed into the Earth's surface near Blansko. Reichenbach recruited a team to systematically search the land for the projectile and was successful on the 11th day. Gradually, Reichenbach compiled one of the largest private collections of meteorites and performed pioneering explorations into their analyses. He developed a classification system for meteorites depending on their contents and structure, and coined the still-used terms Kamacit, Taenit, and Plessit for components of iron meteorites. Between 1835 and 1865, Reichenbach published 28 treatises on meteorites, and, despite his controversial publications on Od, advanced to be an authority in the field.

The years 1835 and 1836 were pivotal for Reichenbach's private life. His wife died in 1835, and his benefactor Count Hugo von Salm-Reifferscheid followed her in 1836. Moreover, Reichenbach, by then a pecunious man, bought Reisenberg Castle, close to Vienna, in 1835, commonly named "Cobenzl" after the former owner Count Philipp von Cobenzl. In 1839, Reichenbach was raised to the rank of Baron by King Wilhelm of Württemberg, Germany, due to his excellent contributions to science which also allowed for practical applications in the area of technology. Yet, von Reichenbach's employment as supervisor of the industrial plants in Blansko ended abruptly in 1841 when he was discharged from all positions by the

son of Count von Salm-Reifferscheid who accused von Reichenbach of deceptive business management. A lawsuit followed that von Reichenbach won in 1846. It provided him with a financial settlement.

Free from professional occupations and financial restraints, Reichenbach's involvement with Od began in 1844 and persisted until his death in 1869. However, his struggle was doomed to fail. In his publications about Od, the Baron often neglected to present adequate documentation of the experimental settings and the exact way he had obtained his results. Moreover, he seemed to ignore and to underestimate alternative explanations for many of the claimed phenomena, such as (auto-) suggestion. When confronted with critique or accusations, he would respond with rumbling trivialities and counter-accusations (e.g., von Reichenbach 1855, 1856) instead of carefully and level headedly explaining his experimental conditions, presenting detailed clarifications, and improving his style of experimentation and publication. Among scientists, even former friends such as von Liebig, who had published von Reichenbach's first treatise on Od in his chemistry journal, turned their backs on him and became critics of his work. In addition, the public began to fear von Reichenbach, the "Sorcerer of Cobenzl," a tall man of imposing stature, usually dressed in dark coats, who performed mysterious experiments with his "sensitive" subjects in darkened rooms full of magnets, wires, and crystals behind heavy black curtains, and who even took his sensitives to graveyards at night to examine purported odic emanations from rotting corpses. In the 1860s, "Cobenzl" Castle must have been a lonely place. In a reprise on von Reichenbach, Bauer (1907) described how he found him sitting alone at an immense table after having ingested a meal. The Baron loved to walk in the forests surrounding his castle on self-made paths, the entries to which were hidden from public view. Financial ruin already loomed over Cobenzl Castle. Several of von Reichenbach's factories suffered from adverse side effects of the war at the Baltic Sea, the insurrection in India, unusual summer draughts in Austria that dried the rivers required to ship tree logs needed in the factories, and from simple mismanagement. Von Reichenbach wrote desperate letters to influential personalities in Vienna to secure his stay at his beloved Castle, and to save his enormous library, the laboratories, and the huge naturalist collections. He feared he would not survive leaving the Castle. But to no avail. In July 1867, von Reichenbach had to leave Cobenzl. Sickly and almost 80 years of age, he abandoned Austria and moved to a hotel in Leipzig. Knowing that his days were numbered, von Reichenbach restlessly sought to convince at least one influential personality of his time, Gustav Theodor Fechner (1801–1887), of the reality of Od. The erstwhile Sorcerer of Cobenzl died in his hotel room in Leipzig on January 19, 1869.

Seven years later, Fechner (1876) published an account of his experiences with von Reichenbach. I will present essentials of it later.

Reichenbach's Work on Od

In the following section, I present sketches of von Reichenbach's major works on Od. The first publication about Od was printed in 1845 as an addendum in a respected journal of a friend of his, Justus von Liebig's *Annalen der Chemie und Pharmacie* (von Reichenbach 1845). Initially welcoming von Reichenbach's intriguing descriptions of the observations related by the sensitives, von Liebig refused to publish further manuscripts in his journal due to increasing skepticism from his colleagues and on his own part. Four years later, the Baron published a revised version of these texts and added a second volume exclusively dedicated to the luminous phenomena of magnets that were reported by his sensitives (von Reichenbach 1849). In the beginning of the first volume, the author described how he happened to investigate the curious phenomena associated with magnets. In March 1844, he was consulted by a Viennese physician to visit a sick woman, Miss Nowotny. She suffered from severe headaches and repeated cataleptic fits. Moreover, she had developed an intense hypersensitivity to light and preferred to lie in darkness. In her darkened room, she stated she was still able to perceive everything as in dim light. Von Reichenbach, at that time interested in theories about the origins of the northern lights that sometimes grace the night skies of the polar regions, became curious. It came to his mind that a person with such a heightened visual sense might perhaps be able to perceive lights around magnets, as it was known that northern lights were influenced and perhaps caused by magnetic effects. He suggested performing tests with Miss Nowotny, and they resulted in positive feedback. Miss Nowotny claimed she could indeed see light emitted from the two poles of a large horseshoe magnet, but only when it was open. When its poles were closed with the armature she perceived no light. When a much smaller magnet was shown to her without informing her of this experimental change, she correspondingly described much weaker luminous effects. Now, von Reichenbach was hooked and continued to experiment with Miss Nowotny. However, he had only a couple of days left. She recovered rapidly from her disease and lost her hypersensitivity and her ability to perceive light around magnets as her state of health improved. Thus, von Reichenbach sought independent verifications of her descriptions and began to search for other persons who were sensitive enough to perceive lights around magnets. Within a short time, he was successful in finding a handful of individuals who allegedly described identical phenomena without being informed about what might be expected. In particular, he

found persons who seemed much more sensitive than Miss Nowotny, such as Miss Reichel. For these sensitives, the entire magnet seemed to glow in the dark, not only the area above its poles (Figure 1a). All the sensitives stated that the two luminous flames above the poles of a horseshoe magnet did not attract each other or bend together, different from the magnetic field spanning the poles. All agreed furthermore that those flames were always emitted straight in the direction in which the magnet was held. They didn't behave like candle flames which always curl upward irrespectively of the

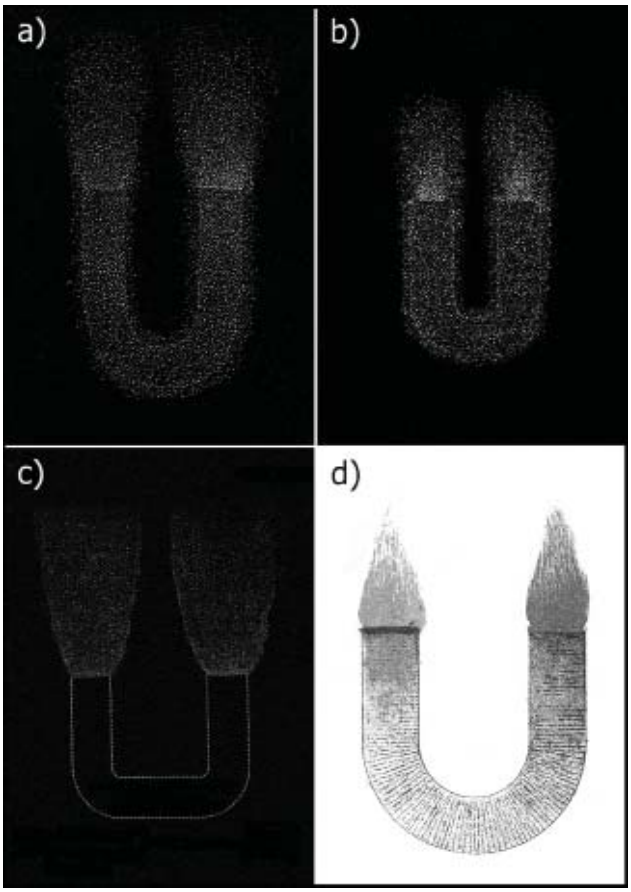

Figure 1. Visual impressions of horseshoe magnets in the dark described by a) sensitives of von Reichenbach (1849), b) Neumann and his sensitives (Neumann 1857), c) three persons studied by the Reichenbach Committee of the SPR; the body of the magnet was not visible to them (Barrett & Collaborators 1882–1883), and d) hypnotized persons of de Rochas (1895/1909). All figures were digitally revised by M. Nahm.

direction in which a candle is held. Moreover, the test persons reported that electromagnets produced the same type of luminous emanations as permanent magnets. In both cases, the sensitives described the quality of the lights as slightly different above each magnetic pole, thus von Reichenbach concluded the phenomenon must be of a polar nature. All higher sensitives additionally confirmed that these lights were strong enough to illuminate objects in the surroundings of the magnets, and that they left an after-image in their eyes. In the later stages of his experimentations, von Reichenbach took pains to make sure that the darkness in the room was complete, and the sensitives had to spend up to several hours in this darkness to accommodate to it before the experiments started.

Throughout the rest of the first volume of this book (1849(1)), von Reichenbach described numerous further experiments, many of which concerned the bodily sensations described by the sensitives. Furthermore, he aimed at showing that identical emanations and sensations like those reported from magnets can be found in crystals and human hands, that these effects can be transferred to water and many other materials, and that electricity, heat, friction, chemical reactions, sunlight, and moonlight also serve as sources of this principle, which must ultimately be regarded as a universal and all-pervading force of nature, a universal adjunct of all matter. He suggested using the short word *Od* for it.

The second book volume (1849(2)) is exclusively concerned with the luminous phenomena of magnets described by the sensitives in the dark room. Von Reichenbach began this volume by presenting the names, and often the exact addresses, of almost 60 new sensitives he recruited among all social strata. They included three professors and four physicians. He then proceeded with describing what each of these sensitives claimed to have observed in the dark room, thus countering the voices criticizing that he had only worked with a handful of ailing women before that. Still, these accounts are almost bare of technical descriptions of the exact circumstances of the experiments. Some sensitives reported a variety of bodily sensations, but seemed unable to perceive luminous phenomena. The extent to which those persons were able to perceive luminous emanations was also varied and ranged from faint impressions to claims of seeing all objects shining in considerable light. Although most of these new sensitives were healthy persons, it appeared to von Reichenbach that sickly persons are generally more sensitive to the perceptions of Od. Moreover, the sensitivity of certain individuals seemed to vary depending on the state of health, in the case of women it seemed more pronounced during their menses. Some sensitives allegedly perceived the odlight of objects that were invisible to the Baron, and led him straight toward these objects. The largest part of

the second book volume (1849(2)) is dedicated to detailed analyses of the different forms that luminous odic emanations can allegedly assume. Next, von Reichenbach presented the reports of sensitives who observed odic emanations of magnets in different media such as in a vacuum bowl and in a water bowl. Apparently, all agreed that lower air pressure resulted in more extended luminous phenomena, and that immersion into water resulted in dramatic declines of the luminous aura around the magnets. Toward the end of the book, von Reichenbach argued that northern lights are likely to represent an immense manifestation of odic emanations.

Von Reichenbach's next publication, *Odisch-Magnetische Briefe* [*Odic-Magnetic Letters*] (1852), consisted of a collection of articles, recycling the ideas of his former work specifically addressing the public. New elements consisted of the explicit discussion of light emitted by plants in the dark, notably by their flowers. An often-cited episode concerns Stephan Endlicher (1804–1849), a highly respected professor of botany at the university of Vienna and director of the botanical garden. At first, he ridiculed von Reichenbach's sensitives, but he turned out to be a sensitive himself. Apparently, he was able to perceive distinct luminous emanations of plants in the dark. When a pot with plants was put in front of him in full darkness on one occasion, he seemed to recognize them solely by their glow and exclaimed "It's a blue flower, it is a gloxinia!"—this was correct (von Reichenbach 1852:56). Von Reichenbach also stressed the importance of training the ability to perceive odlight, which could improve over the course of several years.

In the years 1854 and 1855, von Reichenbach published his two-volume major work *Der Sensitive Mensch und Sein Verhalten zum Ode* [*The Sensitive Human Being and His Relation to Od*], a massive treatise of almost 1,700 pages in which he continued to report the results of his investigations—by then, allegedly amounting to 13,000 experiments with large numbers of sensitives. The two volumes brim with ideas and experiments. Those included in volume one mainly concern bodily reactions of sensitives to certain stimuli. The second volume focuses again on visual impressions of Od, but also on its effects on other senses. I will touch on only a few topics, which fit into the context of this paper. For example, the book contains a whole chapter on odic emanations of plants. Von Reichenbach listed the names of more than 40 persons with different degrees of sensitivity who claimed to see light emitted from plants in the dark, notably from their flowers. Some of the higher sensitives stated that the flowers illuminated the whole room so that they were able to distinguish objects in it, and some, like the mentioned professor Endlicher, described minute details of blossoms and were even able to determine which plant species was brought to them in

the dark chamber. The odic emanations were only visible with fresh or living plants, withering plants lost their shine. A few years later, von Reichenbach published a whole book on plants and Od (von Reichenbach 1858). Among the most curious findings he reported in his major work (von Reichenbach 1854–1855) was the claimed ability of several sensitives to see through metal. Von Reichenbach was led to investigate these claims after occasional reports of some highly sensitive persons who stated not only that they saw magnets or metal wires shine in the dark, but that these objects also seemed translucent. After he had inserted metal plates in the window shutters of his dark chamber, about 40 sensitives noticed these plates, although they were not informed that these plates had been inserted. The lower sensitives only recognized a luminosity at these spots, but several high sensitives including Endlicher reported they were able to see through these metal plates which to some appeared as transparent as glass. These individuals were able to depict the outside scenery correctly. Von Reichenbach also described successful experiments on table-tilting with some of his highly sensitive persons, who reported various luminous phenomena associated with it. Moreover, he acknowledged that some sensitives displayed telepathic abilities, but remained highly skeptical toward purported future predictions.

In a later publication, von Reichenbach (1866) summarized a series of fundamental experiments that were supposed to prove the existence of Od, including experiments which aimed at demonstrating that Od also possessed the ability to move objects. He described how a pendulum could start moving inside a bottle solely by putting one's finger on the thread the pendulum was fastened with on the bottle top, how objects such as paper cards and bar magnets could be set in turning motion on the fingertips of sensitives, and other experiments. He also described other experiments on table tilting. To exclude the possibility that the sitters could move the table with their hands, he fastened drooping ropes to its sides which were held by the sensitive sitters at their other end. Thus, mechanical contact with the table was avoided, whereas a physical bridge between body and table was still maintained. Von Reichenbach reported this experiment had worked well.[3]

In the last publication issued during his lifetime, von Reichenbach (1867) again stressed the importance of movements that were apparently induced by odic influences. He maintained that typical arguments such as suggestion or fraud could be refuted easily by his experimental designs. He again described some of the experiments in his previous publications, but going more into detail. For example, the Baron devoted 40 pages exclusively to table turning and added the information that the ropes were fastened only very lightly to the table, so that any drawing movement on

behalf of the sitters would have disconnected them immediately from the table. He also claimed that heated tables seemed to work better than cold tables, and that laying one's head on the table would also result in table movements—interesting hypotheses that might be tested one day. Although von Reichenbach does not state it explicitly, it appears by his descriptions of what he had observed and how the persons had behaved that these experiments were all performed in good or normal light.

Od on Trial

Soon after the Baron's first publication on Od in 1845, the controversy about the nature of the described effects began to stir. I will present an overview of the efforts to replicate von Reichenbach's experiments in the following, discussing only two of the many facets of Od that have been reinvestigated: its ability to (a) move small objects such as compass needles and to (b) produce luminous effects in the dark that are visible to sensitives.

(a) Object Movements: The Refutation of the "Suggestion" Argument

A prominent witness who published his experiences with the Baron and some experimental results was Gustav Theodor Fechner, famous for his pioneering work on "psycho-physics" in which he argued for a parallelism between the human mind and brain physiology, but who had also authored several other influential philosophical treatises. Fechner was in contact with von Reichenbach starting in 1845 and had criticized von Reichenbach's work (Fechner 1856), but he remained considerate in his formulations. When the Baron left Vienna in 1867 and moved into the hotel in Leipzig, it seems that von Reichenbach deliberately chose this town to convince Fechner of the reality of Od—knowing that there was not much time left in his life. In the same year, von Reichenbach (1867) had published the already-mentioned book in which he stressed that sensitives could move objects in inexplicable ways by using Od, and that the often-raised argument that all observations attributed to Od were based on suggestion became futile in this light. However, he had focused on describing his studies on table turning, and the dubious experiments with the pendulum or turning magnetic needles on fingertips, whereas he seemed unaware of the enormous significance of the simple experiments he had the sensitives perform with compasses. It was Fechner who first stressed the importance of these experiments. In 1867, the Baron paid Fechner unannounced visits in Leipzig and tried to convince the rather reluctant philosopher to participate in joint investigations, who at one point grumblingly agreed. Fechner (1876) published an account of his meetings with von Reichenbach

seven years after the Baron died. Both performed a variety of experiments in daylight with von Reichenbach's housemaid, apparently a moderately sensitive person. Some experiments clearly failed, but others worked well without exception. Fechner was particularly impressed by the ability of the woman to deflect a compass needle simply by nearing her fingers or elbows to the compass. The experiments were successful on three different days. On the last occasion Otto Linné Erdmann (1804–1869), professor of chemistry in Leipzig, had joined Fechner. It seemed to them that they had excluded all possibilities of fraud. Fechner was stunned by these observations. Although he remained highly skeptical toward the speculative and multi-faceted theory of Od, Fechner regarded it of great importance to follow some of the Baron's experiments, as they might prove to be of great value for science. He tried to repeat the compass experiments with several other persons, but his attempts invariably failed. He also performed a literature survey and found one earlier source in which a somnambulant woman had purportedly deflected a compass needle (Bähr & Kohlschütter 1843; for another early source see Burdach 1840; for other examples, see Durville 1895–1896/1912). In collaboration with scientists at Leipzig University, Fechner developed an electrical apparatus to test if human fingers can be magnetized or electrically charged—it seemed impossible. The puzzle remained. Another colleague of Fechner's at Leipzig University, professor of astrophysics Johann Karl Friedrich Zöllner (1834–1882), had paid attention to Fechner's reports. When Zöllner started to work with the medium Henry Slade (1835–1905), his first test for potentially unusual abilities consisted of repeating Fechner's compass experiment.[4] Indeed, Slade was successful on three different days (but not on each day) and even succeeded in magnetizing steel knitting needles solely by holding them in his hands. It seems that Slade was unaware of his ability to move compass needles without touch before this visit to Germany (Zöllner 1878:329). Similar experiments continued to be performed by other experimenters with apparent success. Among the authors who reported on successful deflections of compass needles without touch were Harnack (1905), de Rochas (1906), Grunewald (1920, 1922), von Rechenberg-Linten (1921), and Zeller (1925). Two noted Polish mediums, Stanislava Tomczyk (Ochorowicz 1909) and Franek Kluski, were also reported to be able to deflect compass needles.[5] Kluski was said to be able to move the needles of compasses holding his hands 12 centimeters above them. In experiments performed in 1924, the needles of three compasses reacted sensitively to his fingers and toes, but also to his chest and stomach regions (Okolowicz 1925; for a brief mention of these experiments see Weaver 1991–1992). In 1939, long after Kluski had stopped serving as a medium for spiritistic sittings in 1925, he was

apparently still able to rotate compass needles without touch (Thorsen 1950). Another medium, German Heinrich Melzer, was also said to have moved compass needles without touch (Hess 1935). Similarly, a Greek woman with seemingly psychic abilities who was studied at the University of Athens repeatedly succeeded in deflecting a compass needle (Tanagra 1932, Tanagra, Walther, & Herbert 1972), and a noted Hungarian writer, Count Alexander Berényi, was reported by a team of scientists to be capable of performing these actions (Röthy 1936). Also, psychic Annie Abbott was reported to have moved the magnetic needle by moving near it, particularly her right hand (Cross 1939). In more recent years, reports of further successful results produced by persons with psychic abilities such as Nina Kulagina (Keil, Herbert, Ullman, & Pratt 1976), Felicia Parise (Honorton 1974, Watkins & Watkins 1974), Matthew Manning (Owen 1974, Owen & Whitton 1974), Uri Geller (Hasted 1981), a girl called "Lena" (Mattuck 1977), and a mention of the psychic Geoffrey Boltwood (Scofield & Hodges 1991) were published. Although the observations of all these authors were similar in that a compass needle was moved in an inexplicable way, they differed in details. In some cases, the left and right hand caused movements in opposite directions, but in other cases both hands deflected the needle in the same direction. Moreover, it seemed that sometimes the hands acted upon the compass by magnetic influence, whereas on other occasions the movements were apparently not effected by magnetic or electric forces but by plain psychokinesis. In sum, it seems these are simple but intriguing experiments which might contribute to establishing the reality of abilities often regarded as "paranormal." Such experiments are easy to perform and to control, and, as Fechner had noticed long ago, seem well worthy of further investigation.

(b) To See or Not to See

A second potentially valuable line of investigation is provided by the purported luminous emanations that were said to be emitted from magnets. Even before von Reichenbach's descriptions of the luminosity of inanimate objects such as magnets and crystals, it was long established in the literature of animal magnetism and somnambulism that certain sensitive persons can perceive luminous effects around and within living organisms or objects. For instance, Armand de Chastenet de Puységur (1811) commented on a somnambulant man who refused to use lights in a cellar because all objects would shine for him in the dark, and Philipp Heineken described the case of a sick somnambulant woman who was able to see well in total darkness (Heineken 1818:43). Heinrich Bruno Schindler (1857:152) pointed to other individuals, starting with Roman Emperor Tiberius, who had claimed to be

able to see or even to read in darkness. Schindler regarded the awareness of odlight as the induced and selective perception of the "magical light" said to permeate all objects in the concepts of Kabbalism, Neoplatonism, Gnosticm, Sufism, Vedanta, and also somnambulism (Schindler 1857:146ff). Moreover, a few magnetizers seem to have discovered that some psychic individuals claimed to see magnets or objects charged with electricity glow or emit a luminescence of bipolar nature independently of von Reichenbach. For example, French physician Jules Charpignon (1848) published reports of experiments performed with somnambules who correctly distinguished several magnetic objects due to peculiar bipolar luminous emanations. Similarly, Joseph Haddock (1851) reported experiences with a woman who perceived colored light issuing from magnets, the lights being brighter above the north pole of the magnets than above the southern pole. Haddock stated that he had conducted these experiments without knowledge of von Reichenbach's work, and had only learned of it in 1850 when he visited William Gregory, professor of chemistry in Edinburgh, who translated the Baron's first book into English.[6] Unusual luminous phenomena were also discussed in the early literature on experiences such as hauntings and apparitions. For example, Catherine Crowe (1848) related the luminosity of apparitions to the light reported by somnambules, and reported the case of a young girl of highly nervous temperament who was repeatedly punished because she claimed to see luminous flames issuing from organisms and objects (Crowe 1848(2):165). Similarly, one of von Reichenbach's most sensitive subjects, Miss Reichel, claimed that she had perceived luminous emanations of objects and living beings since her childhood (von Reichenbach 1849).

Extensive attempts to replicate von Reichenbach's experiments were performed in 1846 by a committee of Viennese physicians who tested a few sensitives, mainly Miss Reichel, in 22 sittings during the course of six months (Gouge 1846). The committee failed to record successful results with the exception that Miss Reichel seemed able to discern magnetic and non-magnetic metal objects held in her hands. Overall, the committee concluded that the sensitives were largely subject to delusions and on occasion resorted to fraud. Von Reichenbach (1849(2)) opposed their conclusions and criticized the methods employed, which he regarded insufficient for a variety of reasons. However, the experimental series performed by the Viennese physicians rank among the most rigid and valuable replications of von Reichenbach's experiments, highlighting the numerous problems associated with experiments in darkened rooms and difficult-to-handle sensitives.

Another critical article was published in the same year by James Braid

(1846/1970), who had already rejected concepts of magnetic forces or fluids in earlier publications. He tested the ability to see luminous effects around magnets in "several" persons whom he led into a darkened closet in which he had put a magnet. Similar to the findings of the Viennese committee with Miss Reichel, all were able to perceive luminous effects only after respective leading questions were offered by Braid, and they also continued to report these effects when the magnet had been removed. Consequently, Braid concluded that the phenomena described by Reichenbach's sensitives were mental delusions due to a leading external stimulus and/or excited imagination—possibilities that the Baron appeared to neglect and to ignore, perhaps naïvely, but, as it seemed to some who had witnessed the Baron experimenting, sometimes even deliberately (Vogel 1863).

Yet, a positive eyewitness testimony from Gustav Brabbée, who had attended more than a dozen meetings in von Reichenbach's dark room, was later included in Albert von Schrenck-Notzing's (1891) foreword to a posthumous publication of von Reichenbach. Brabbée described how a highly sensitive woman saw all objects in the room as distinct as in daylight, never erring with her descriptions of various flowers, magnets, or of the number of digits of a hand that was raised into the air, or hidden from her to mislead her, and so forth.

Confirmations of the reported luminous Od-effects were also published shortly after the first critical reports by other authors. Both early translators of the Baron's first book into English, William Gregory and John Ashburner, a physician in the tradition of animal magnetism, soon experimented with magnets according to von Reichenbach. Both Gregory (von Reichenbach 1850) and Ashburner (von Reichenbach 1851) reported in comments of their translations that some individuals were indeed able to perceive luminous phenomena around magnets as the Baron had described. According to Ashburner, they were able to do so "without being informed of the purpose for which they were introduced" into a darkened room (von Reichenbach 1851:12). In Germany, Ludwig Büchner (1854), famous for his soon-to-follow classic treatise promoting materialism, *Kraft und Stoff* [*Force and Matter*] (Büchner 1855), published results on his investigations of Od that he had performed with about 100 persons. He set out to replicate von Reichenbach's experimental findings thinking that they indeed seemed odd, but that they nevertheless might be of importance and should be tested before dismissing them for purely theoretical considerations and superficial accusations. With regard to the dark chamber, he led "various persons of both sexes" into it, unfortunately not giving the exact number (Büchner 1854:36). He conducted 11 sittings in the dark, each with a duration of one to three hours. It appears that most persons perceived nothing, that

some made dubious claims Büchner was inclined to regard as subjective illusions, but that eight individuals reliably perceived luminous phenomena. Of these eight sensitives, four seemed to perceive an entire horseshoe magnet glowing and emitting light from its poles, this light being differently colored on each pole. One person even claimed to see a luminous smoke curl up to the ceiling, just as some of the Baron's sensitives had reported. According to Büchner, these sensitives were entirely unaware of von Reichenbach's publications. The higher sensitives also claimed to see other persons glow in the dark, and one was apparently able to locate a flowering bush correctly due to its light emissions. Büchner (1854) also mentions another researcher who had worked with a sensitive who claimed to see light of different colors emanating from the poles of a magnet in the dark (1854:43). In 1856, physician Albert Constantin Neumann, himself a sensitive who declared he could see persons, metallic objects, and magnets shine in darkness, wrote to von Reichenbach that he had by then discovered 32 other sensitives who perceived luminous phenomena in his own dark room (von Reichenbach 1856), and he published a brief summary of his work shortly after (Neumann 1857). An outline of how he and his sensitives alleged to perceive a horseshoe magnet is presented in Figure 1b. In England, the interest in Od continued in spiritistic circles. In a lecture given to the London Dialectical Society in 1869, Cromwell Varley, a renowned electrical engineer closely involved in laying the transatlantic telegraph cables in the 1860s, reported that his wife possessed mediumistic abilities and was capable of perceiving odic flames issuing from magnets, crystals, and human beings. He had experimented with her and stated that he had achieved "abundant and conclusive evidence" in favor of these phenomena (Anonymous 1871:167). In 1871, Varley's friend Lord Lindsay performed an experiment with the famous medium Daniel Dunglas Home (1833–1886) in his private laboratory in London along with three other guests: Lord Adare, Dr. Bergheim, and his brother-in-law.[7] Lindsay placed a large permanent magnet on the floor of a completely dark room a considerable distance from the door. Home was then brought into the room and remained standing at the door for some moments. Then he claimed to see a sort of light on the floor. He took the hand of Lord Lindsay, walked him across the room, stooped down and placed his hand directly on the magnet (Lindsay 1871).

In 1879, Sidney Billing (1879) reported how he had accidentally seen a white oscillating flame on a book table in the library of a friend. When he ascertained its cause, he found a large upright magnet from the poles of which the light seemed to proceed. His friend, the owner of the magnet, was not able to see this light (Billing 1879:355). Also in 1879, amateur

scientist and astronomer John Rand Capron reported that five persons in a dark chamber did not succeed in perceiving luminous magnetic effects during an experimental session in the dark when a hidden operator silently connected and disconnected a battery linked to an electromagnet (Capron 1879). Later, he published a note on a man who had accidentally noticed weak flames around the poles of a large ordinary magnet glowing in the dark, having never heard of von Reichenbach's work before (Capron 1884).

The most carefully conducted and documented experiments up to that time were performed by the "Reichenbach Committee" of the Society for Psychical Research (SPR), headed by physicist Sir William Barrett (Barrett & Collaborators 1882–1883). Alfred Russel Wallace, co-founder of the theory of evolution by means of selection of the best-adapted individuals, was convinced of the reality of the luminous phenomena described by von Reichenbach, but held that they should be retested. In a letter to Barrett in 1876, he enticed him to repeat such experiments, because this issue could easily be tested and settled (Marchant 1916(2):197). In 1877, he repeated his incitement and recommenced to use an electromagnet that could be switched on and off (Marchant 1916(2):198). It seems likely that the establishment of the Reichenbach Committee by Barrett was to some extent invoked by the stimulation of Wallace. After the foundation of the SPR in 1882, this committee had screened the ability to perceive luminous emanations of permanent magnets and electromagnets in 45 persons after they had spent at least one hour in the dark to allow for visual accommodation. Of these 45 persons, three men who had declared entire ignorance of von Reichenbach's work professed to perceive luminous appearances around magnets (Barrett & Collaborators 1882–1883). In subsequent experiments, two of these men were tested and seemed to display the ability to detect correctly when an electromagnet was switched on and off in irregular intervals without their knowledge. All three described the magnetic light of a horseshoe magnet similarly to those outlined by von Reichenbach's sensitives, the flame above the magnetic north pole appearing slightly brighter. This detail is not apparent in the picture the committee included in their report (Figure 1c). Barrett (1883) also stated that he held a permanent horseshoe magnet in different positions in front of one of the sensitives. Apparently, this person was able to correctly describe Barrett's actions such as holding the magnet upward, downward, or moving it around. Moreover, two of the sensitives and one member of the SPR research committee felt peculiar sensations in heads and faces when placing their head between the poles of the electromagnet, and were at times able to correctly determine whether it was excited or not (Barrett & Collaborators 1882–1883). Barrett (1884) repeated this experiment successfully shortly after with one of the

sensitives. In addition, a rough and preliminary sketch of experiments performed in a dark chamber by another experimenter left slightly positive but inconclusive results due to the unsatisfactory experimental methods applied (Stewart 1884). It seems these experiments were not pursued. All in all, the Reichenbach Committee tested about 100 persons, but found no more sensitives than the three reported on previously (Barrett 1886).

Around the same time, members of the American Society for Psychical Research aimed at replicating the British experiments with magnets. William Pickering (1886) reported that he was not able to see luminous emanations from a powerful electromagnet, and Joseph Jastrow with George Nuttall (1886) tested the ability to determine whether a powerful electromagnet was excited or not by sensations in the head. Apart from participating in the experiments themselves, Jastrow and Nuttall tested "eight students, young men in good health" (1886:124). All ten individuals failed to display a sensibility for a magnetic field.[8]

Two French researchers who performed extensive studies following in the footsteps of von Reichenbach were Albert de Rochas (1837–1914) and Hector Durville (1849–1923). Both largely confirmed the findings of the Baron. Assessing the ability to detect luminous emanations from magnets visually, de Rochas reported many successful experiments in which an apparently highly sensitive hypnotized man, "Albert L.", was able to correctly describe the status of different magnets. To test if the visual impressions of the man were objective observations, de Rochas designed an apparatus in which an electromagnet could be handled in three different ways: switched off with no electric current flowing inside, and switched on with a current flowing from pole A to pole B, or flowing in the opposite direction from pole B to pole A. When an experiment was performed, the apparatus was brought into a random position the state of which de Rochas himself was not consciously aware of. Albert L. was then asked to describe what he saw. After that, de Rochas tested his statement by nearing a compass to the electromagnet to detect its status. Albert L. was tested on several occasions, sometimes a couple of times on one day, and sometimes with an interval of a couple of days. The experiments were performed between 2 and 4 p.m., the daylight being dimmed by a curtain. In 22 experiments, Albert L. was correct in determining the status of the electromagnet on each occasion, reporting two differently colored emanations from the poles which enabled him to discern the direction of the electric current in the case where the magnet was switched on (de Rochas 1895/1909:20). A drawing of the luminous emanations as described by de Rochas' hypnotized sensitive persons is given in Figure 1d. De Rochas also tested if active suggestion can influence the hypnotic subjects, and on occasion obtained confirming results.

Thus, he stressed the importance of avoiding any influence whatsoever on the sensitives and suggested posing only one question when asking for a description of their perceptions, namely "What do you see?" (de Rochas, 1895/1909:41).

The studies and findings of Durville were similar to those of de Rochas in many regards. When testing the ability to perceive light emanating from magnets, Durville worked with sensitives who were able to describe a large horseshoe magnet in the dark. He confirmed that for highly sensitive persons, the entire magnet seemed to glow and that its emanations appeared to reach the ceiling of the room and to spread there, providing sufficient light to read a newspaper in the vicinity of it. The light emanating from the two poles was described as being of different quality. He included a drawing of the magnet as described by his sensitives, which is largely identical to Figure 1a and 1b of the present paper and thus is not reproduced here (Durville 1895–1896/1912:324).

In 1907, Dutch researcher Floris Jansen (1907) published the results of what appears the most advanced study addressing the perception of magnet light ever performed. After briefly working with de Rochas in Paris, Jansen returned to The Netherlands in Spring 1906 and founded in Amsterdam the first laboratory for experimental parapsychology. In particular, he aimed at elucidating the relation between biology and psychology, and considered parapsychological phenomena an important link between the two (Kramer 2006). In the course of conducting his experimental tests, Jansen tested in total about 120 persons. He reported at length on the results of the first 83 persons, 54 men and 29 women aged between 18 and 60 years. He concluded that 13 persons proved to be able to correctly distinguish the periods in which an electromagnet was activated or not by perceiving visual impressions at its poles. Jansen ensured that the methodological and experimental setup could be reliably controlled, and implemented a completely automated test procedure. The test persons sat alone on a chair in a totally darkened room, a pole of the electromagnet positioned at eye level about 70 centimeters in front of them. The magnet was switched on and off in irregular intervals by an automated device in the adjacent control room of the laboratory. Because Jansen himself was not aware of the on–off intervals during a given trial, it was possible to exclude telepathic influence as a potential means of affecting the reactions of test persons. The test person was to press a button when he or she perceived a visual impression around the electromagnet. The signals of both channels, the one recording the on/off state of the magnet and the other recording the response given by the test person by pushing the button, were automatically recorded. The graphs

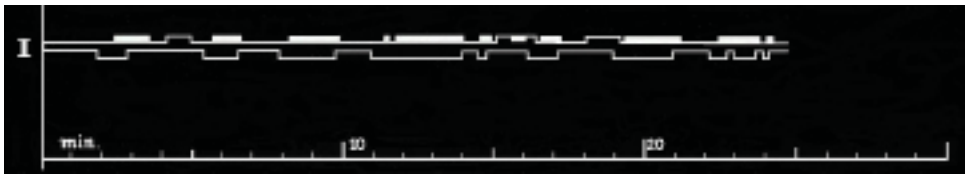

Figure 2. Automated recording of a sitting with a sensitive of Jansen's (1907). The line at the bottom is the time scale in minutes. The middle line shows the status of an electromagnet that was switched on and off in irregular intervals with an automated device, the dropping of the line indicating the excited state. The magnet was positioned in a dark room with the sensitive person who pressed a button when he thought he perceived a visual impression (upper line). The white blocks indicate that he perceived an impression, and they correspond well to the states of the excited electromagnet.
The figure was digitally revised by M. Nahm.

of a trial with one of his best test subjects are displayed in Figure 2. The patterns of the two lines show a notable parallelism, indicating that the man was able to perceive if the electromagnet was active or not after a certain "reaction time" (Jansen 1907). Unfortunately, Jansen was forced to give up his laboratory in 1908 due to severe financial strains. He could not continue with his promising experiments, and could not even issue the detailed report he was intending to publish (Kramer 2006).

After this largely unknown study by Jansen, attempts to replicate Reichenbach's experiments with magnets became even rarer. In Mexico, Gustav Pagenstecher (1924) reported on experiments with a woman who reacted sensitively to a magnet, and who gave descriptions of bipolar luminous phenomena around humans in the dark similar to those described by the sensitives of von Reichenbach. Rudolf Tischner (1950) reported successful experiments with a woman who reliably detected water treated by his hands, but mentioned in passing that persons he tested in dark rooms were not able to perceive luminous appearances around crystals and plants. Yet, it is of interest that reports of unusual observations that von Reichenbach would have claimed to be of odic origin continue to be published occasionally. For example, Owen (1972) reported that five witnesses had independently and simultaneously observed a luminous aura or a kind of blurring of the air around a dowsing rod which was the focus of intense concentration of two psychic persons. This unexpected observation was made in full light. The experiment was successfully repeated with different persons who were not informed about the nature of the event, but the luminous appearance

around the dowsing rod was not visible on a photograph taken. And, like so often before, the way the experiment was performed did not entirely exclude the possibility that suggestion or even telepathic influences among the participants played a role in generating these visual impressions. Only recently, Göte Andersson (2009) has published a summary of experiments he had performed with a Swedish boy named Pontus who was able to reliably distinguish the two different poles of magnets, even in double-blind test settings. To Pontus, the two poles of magnets seemed to emit differently colored light, and he reported similar emanations around humans. He has stated that the experiments of von Reichenbach and his successors came to his knowledge no earlier than spring 2010 (Göran Brusewitz, personal communication with Andersson on March 6, 2011).[9]

Concluding Remarks

In this paper, I have reviewed important stages of Karl von Reichenbach's life and some of his findings related to the supposedly universal force he termed Od. Although many of his findings are likely to be attributed to inadequate experimental protocols and performances that may have allowed for self-deception and suggestion, if not fraud, it might be too early to conclude that *all* of his findings can be attributed to these factors. If von Reichenbach's experiments concerning unusual visual perception in complete darkness could be replicated with modern equipment and would yield positive results, they would provide important clues for a better understanding of the nature and functioning of our senses, and perhaps also of the *aura* that some people claim to see around human beings and objects. Moreover, should some persons indeed be able to turn compass needles simply by nearing their fingers, assessing the detailed circumstances might also constitute an important step forward toward understanding peculiar motor abilities long attributed to some human beings. Such findings would also indicate that certain aspects of historical concepts on (human) radiations and forces might still be of relevance today, and would serve as a reminder that other treasures might also be unearthed in the writings of numerous pioneers who explored border areas of science in the past.

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Brusewitz for summarizing from the original Swedish source the basics of the experiments performed by Göte Andersson with Pontus. Likewise, I am grateful to Kaare Claudewitz for a summary of the compass experiments that were performed with Franek Kluski in 1924, derived from a Polish original source, and for providing relevant references. Finally, I thank Carlos S. Alvarado and Andreas Sommer for helpful comments on this manuscript.

Notes

[1] In the English-speaking countries, the Od became also known as *Odyle* following the influential translation of von Reichenbach's first book into English by William Gregory, a professor of chemistry in Edinburgh (von Reichenbach 1850)

[2] Feerhow is a pseudonym and an anagram of (Friedrich) Wehofer.

[3] Fritz Grunewald (1920) has designed a similar table and reported remarkable success, but these intriguing experiments have to my knowledge so far not been replicated by the groups that have experimented with table turning more recently.

[4] Zöllner, who published under his third forename Friedrich, ranked among the most respected and innovative scientists in Germany. When he started experimenting with Slade, he was searching for experimental verification of his theory concerning a fourth spatial dimension. Slade was reported to be able to provoke numerous large-scale paranormal phenomena under conditions of good light. When Zöllner reported successful experiments with Slade, he was portrayed as being insane by influential academics. Rumors of fraud had also been put forward by critics of Slade, but were hardly substantiated. For brief summaries of the experiments Zöllner performed with Slade see Inglis (1992) or Randall (1982), for an extensive compilation of the original reports written by Zöllner about his experiments with Slade and a commentary about his critics see Tischner (1922).

[5] Stanislava Tomczyk was extensively studied by French researcher Julian Ochorowicz, and also by Albert von Schrenck-Notzing in Germany (von Schrenck-Notzing 1920). Probably the most frequently discussed phenomena reported with her are controlled levitations of small objects in full light. Franek Kluski, his real name being Teofil Modrzejewski, was particularly known for the human limbs and animals that were reported to materialize during his sittings, but he also seemed to possess mental psychic abilities. For an overview on Kluski's mediumship see Weaver (1991–1992), for often-discussed original reports of sittings with Kluski see Geley (1924/1927).

[6] Indeed, the first edition of his book (Haddock 1849) contained no reference to von Reichenbach. Haddock only described that his subject was able to clairvoyantly locate a magnet, and that she perceived objects in "bright light" when in the somnambulant state.

[7] Even today, Home ranks among the most important mediums for physical mediumship. He usually worked under conditions of full light, and numerous high-ranking witnesses voted for the genuineness of his phenomena. For an overview on the mediumship of Home, see Braude (1997).

[8] Although many experiments aimed at assessing the ability of humans to consciously and directly detect (electro-) magnetic fields have yielded negative results in the past, the recent literature on possible and largely unconscious influences of electromagnetic fields on organisms of humans and animals is vast (for a brief review, see Brusewitz 2010).

[9] It should also be mentioned that there have been numerous efforts to obtain photographic imprints of odlight. However, it is safe to state that odlight from even powerful magnets has never been photographed when the conditions were sufficiently rigid. A late experiment often regarded as decisive was performed by the fourth Lord of Rayleigh (1938–1939). He used highly sensitive photographic plates which were not affected by the magnet even after an exposure of 150 days. However, in contrast to the unsuccessful attempts to catch the odlight of magnets on photographic plates, the literature on human emanations and fluids contains numerous reports in which such emanations were seemingly captured on plate, mostly protruding from hands (for examples see Aigner 1921, Feerhow 1914, Krauss 1995, Ochorowicz 1911–1912, 1912). In addition, unexplained lights emanating from human bodies have apparently been recorded on film or photographs on occasion (Alvarado 1987, Schimberg 1947).

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OBITUARY

William Roll
Leader in Parapsychology Research

January 9, 2012, saw the loss of one of Parapsychology's luminaries, William G. Roll. He passed away at age 85 at a nursing home in Normal, Illinois.

Born of an American father and Danish mother in Germany in 1926, William George Roll became a major player in Parapsychology, though probably best remembered for his work with poltergeist phenomena.

While in his teens in Denmark, Roll began having out-of-body experiences which led to his more academic interests. After gaining his B.A. from the University of California, Berkeley, in 1949, he moved to Oxford, England, to study under H. H. Price and to begin making his mark on the field. He remained at Oxford for eight years, in part due to grants from the Society for Psychical Research and the Parapsychology Foundation. He received the M. Litt. Degree from Oxford.

In 1957, he was invited by J. B. Rhine to join the Duke Parapsychology Laboratory, working with Rhine until 1964. In early 1958, he encountered his first poltergeist case with J. G. Pratt, a case that set him on a direction leading quickly to what would become a major focus for his career and research. Out of that case came the term *recurrent spontaneous psychokinesis* (RSPK), which crystallized the lens through which parapsychologists view poltergeist cases.

After the Psychical Research Foundation was created in 1961, Roll was appointed the Project Director in charge of research (Roll 2010). His work at the PRF over the years included several high-profile poltergeist cases, most notably "The Miami Disturbances" (Roll & Pratt 1971) and the controversial case of Tina Resch, the "Columbus Poltergeist," later the subject of the book *Unleashed: Of Poltergeists and Murder: The Curious Story of Tina Resch* (Roll & Storey 2004). 1972 saw the publication of his best-known work, *The Poltergeist* (Roll 1972).

His other work included research on OBEs, psychometry, and ESP, theoretical models for psi in general and for apparition and haunting cases, and field research and investigation of hauntings with consideration for anomalous electromagnetic fields. His theoretical work includes his Psi

Field Theory, first presented in 1964 as Roll's Presidential address to the Parapsychological Association.

In 1986, he joined the faculty of West Georgia State College, which later became the University of West Georgia. Roll received his Ph.D. from Lund University in Sweden in 1989 for a dissertation dealing with Survival Research. The Parapsychological Association (Parapsychological Association 2012) awarded him their "Distinguished Career in Parapsychology" Award in 1996. He received SSE's Dinsdale Memorial Award in 2002.

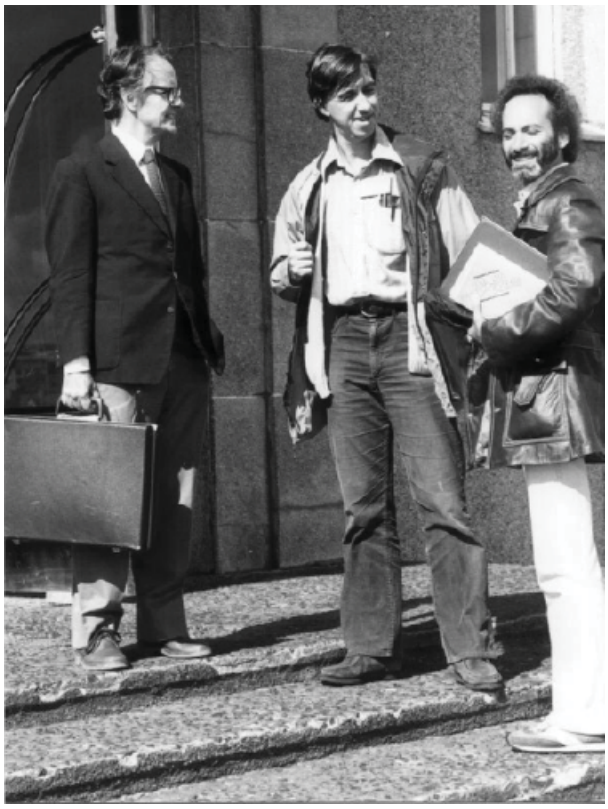
While others have contributed to the RSPK model of poltergeists, it is William G. Roll's name that is most associated with it, and rightly so. His work with poltergeist agents and their families has furthered our understanding of spontaneous PK and the connection between the unconscious and PK, and set some excellent standards for field investigators of the phenomena. He also had much to say about how and why field researchers must consider the mental health of the agent and family (or other witnesses) in the process of the investigation. This latter focus helps researchers understand the motivations for those reporting such incidents in their lives, what they really want, and what they need from us as investigators.

When I interviewed him for one of my own books in 1985, he had this to say:

> People essentially don't want an investigation, they want to be rid of "it." We have been able to provide some understanding to people with open ears. "It's not demons, it's RSPK, and PK's a natural sort of thing." Then we would suggest that they see somebody after we left, some psychologist in the area that's open to these sorts of things. We haven't been able to do that to the extent to which we would like to have it done. These people really suffer; the families suffer and the central persons suffer. (Auerbach 1986: 385).

Roll's work with poltergeists, along with his haunting and apparition cases, truly has had a lasting impact on parapsychological field research and investigation methodology and ethics. With rare exception, the RSPK model as delineated by Roll and others has shown itself to be a practical working model not just for how the phenomena run their course in cases, but as a way to achieve resolution for the people who experience the phenomena. His work with apparition and haunting cases, and his attempts to discern a working model for those as well, has raised some important questions about how people experience such things and the role the environment plays in the experiences.

With his published books, work as an editor in the field, more than 100 scientific papers, and contributions to various anthologies, William G.



Bill Roll, Charlie Tart, & Steve Braude at the Parapsychological Association Convention in 1980, in Reykjavik, Iceland

Roll has indeed made his mark on the field of Parapsychology (L. Roll 2012). However, the general public also came to know his work as well. Numerous appearances on network and cable television, coupled with other appearances in the news media cemented Roll in the public mind as a leading investigator of paranormal phenomena.

On a personal note, Roll had a great influence on my own education and work and continues to do so.

Loyd Auerbach

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LETTER TO THE EDITOR

Erroneous Expert Judgments

The Editorial in *JSE* 26:1 suggests the need for a scientific analogue of Slonimsky's one on erroneous and intemperate judgments in music (*Lexicon of Musical Invective: Critical Assaults on Composers Since Beethoven's Time*). Something like that is already available in Cerf and Navasky's *The Experts Speak: The Definitive Compendium of Authoritative Misinformation* (1984/1998) and *Mission Accomplished! Or How We Won the War in Iraq: The Experts Speak* (2008). Highly enjoyable.

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BOOK REVIEWS

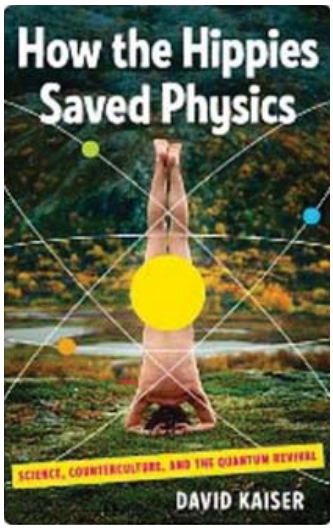
How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival by David Kaiser. W. W. Norton, 2011. 372 pp. $26.95. ISBN 9780393076363.

Kaiser's thesis is that quantum information science, which is beginning to have application to subjects such as cryptography, came into existence only as a result of the activities of a counterculture movement, or "hippies." The curiously ambiguous nature of the connection between the mathematics of quantum theory, and reality as observed, had led the founders of the theory to an interest in deeper issues, but the budget cuts of the Cold War period led to a more exclusive focus on practical aspects, as epitomized in the instruction "shut up and calculate!" Dissatisfied with this restriction in what one was supposed to spend one's time thinking about, a group of people centered on the Berkeley campus of the University of California got together to form the "Fundamental Fysiks Group."

Much of the attention of this group focused on the non-locality demonstrated by the work of J. S. Bell, work that appeared to demonstrate that any picture of what is going on cannot be a local one, that is to say that connections at a distance must be involved. Might these connections produce observable effects: could they explain paranormal phenomena? It was far from clear that this would be so; indeed quantum mechanics appeared to show that such influences as there might be would not act as a signal. Freedom to speculate was supported financially by organizations such as the CIA, no doubt interested in intelligence applications, and the "New Age" Esalen Institute.

The group never clearly showed links between physics and paranormal phenomena, but its work did lead to experiments to test for non-locality. These experiments, rather disappointingly, confirmed the existence of non-locality, while also confirming the predictions of quantum mechanics and thereby the impossibility of using that non-locality to transmit information at a distance.

The group was able to get publicity for its ideas in various ways, despite the fact that *Physical Review* had responded to the submission of such views by banning papers on the interpretation of quantum mechanics. These connections included books such as Fritjof Capra's *The Tao of Physics*.



There was also interest in some quarters in paranormal phenomena and their possible applications.

While paranormal issues were not taken up by mainstream science, the fact that experiment confirmed non-locality in *some* senses of the word opened up scientists' minds to the value of looking for other counterintuitive anomalies, leading ultimately to developments such as quantum encryption.

Kaiser argues that the "hippies saved physics" in three ways: making speculation and philosophizing once again more acceptable; putting a focus on the question of non-locality; and discovering the "no-cloning theorem" on which quantum encryption is based. Did they really "save physics," though? Tremendous prejudices remain, as exemplified by the way an invitation to a physics conference I had been planning to attend was withdrawn on account of my interest in the paranormal, as if I might contaminate the conference even if I did not lecture on the subject. Because of the taboo nature of the subject, the large number of scientists who surveys have shown accept the reality of paranormal phenomena mainly come to the conclusion that it is best to keep quiet about this belief. The present focus is still very much on calculating, even if horizons have now expanded somewhat.

All in all, this is a very instructive book, telling of historical events that are not very widely known.

Brian Josephson

Physics on the Fringe: Smoke Rings, Circlons, and Alternative Theories of Everything by Margaret Wertheim. New York: Walker & Co., 2011. 323 pp. $27 (paperback). ISBN 0679774009.

I don't get it. I just don't get it. This book is supposed to be about the relation between insider physics and outsider physics. It isn't.

As I read *Physics on the Fringe*, I was increasingly disappointed and frustrated at the amount of space given to Jim Carter, not only his "fringe physics" but also his personal doings. Admittedly he seems quite an interesting person, fun to be with, admirably self-motivated, helpful to others, a good citizen—not unlike quite a lot of other people. But his "circlon theory" isn't an exemplar of fringe physics, it's way-way-out pseudo-science. And Carter is not even typical of way-way-out crackpots: As Wertheim says, Carter atypically is a successful entrepreneur and has a sense of humor.

So the book's title misleads, and unfortunately the book's substance also misleads about science and fringe science and pseudo-science in a number of ways.

For one important thing, there's absolutely no justice in science as concerns the relationship between being an admirable person and producing admirable science, or between having good intentions and producing good science, or between recognizing the failure of modern science to make itself widely comprehensible and being able to do something about it. I could make quite a long list, off the top of my head, of scientists who accomplished great things, and even won Nobel Prizes, and yet were in many ways quite despicable people—self-centered, arrogant, self-important, nasty to others, ungenerous, without sense of perspective or self-knowledge; Nazis, fascists, racists . . . . And I've known and liked quite a few good people trying to do science whose accomplishments are zero, or in some cases worse than that by cluttering the literature with rubbish.

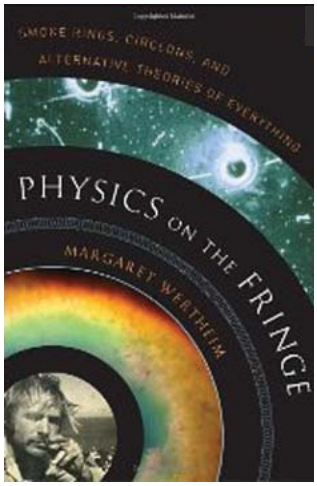
As Wertheim came to appreciate the admirable human being Jim Carter, it appears that her liking of him superseded objectivity about his circlons. The Appendix that summarizes the assertions of circlon theory encapsulates the evidence that it's pseudo-science: purporting to be science but having none of the characteristics of science, in particular its tight interplay between evidence and theory which brought modern science into being starting about half a millennium ago. Near a waterfall, Carter drops a stick and tells Wertheim that it is not gravity that pulls the stick down, it is that the expanding earth moves up to the stick. So (I would have asked), the earth expands to different extents in different places at different times as

various people drop various things? And why was the water falling relative to all the other objects around it?

By focusing on Carter, much of the book's purported inquiry into big questions becomes incoherent. The treatment of Steven Rado (pp. 56–57) similarly confuses questions of personality and of science. In a somewhat similar vein, the book juxtaposes Carter's experiments blowing smoke rings and similar experiments carried out 150 years earlier by such authentic scientists as William Thomson (later Lord Kelvin) and Hermann von Helmholtz; but the accumulation of knowledge during those 150 years makes the juxtaposition substantively meaningless, just a superficial coincidence. Still, I enjoyed learning that volcanoes sometimes emit massive smoke rings and that dolphins generate and play with bubble rings.

The incomprehensibility of frontier physics *is* a social problem, and perhaps it does motivate some way-out amateur speculation—but there are other consequences too, and more serious ones. The state of science education *is* parlous, and it is woefully matched by parlous coverage of science in the mass media—but the existence of fringe science and of pseudo-science are not the chief causes or consequences, or even the socially most important ones. Wholesale ignorance about straightforward, reliable, uncontroversial 20th and 21st century scientific understanding wreaks economic and political havoc because, as John Burnham pointed out,[1] "superstition won and science lost": Gullible belief in the dogmatic pronouncements of official spokespeople for science and medicine replaced authentic popularization of science. Ignorance of elementary axioms of probability and statistics, the lack of competent science journalism, and the coming into being of a bureaucratic science–government–industry complex have led 21st century civilization to waste untold effort and resources on such counterproductive ventures as attempts to control global climate or to treat long-standing African diseases as though they were caused by a non-existent retrovirus.

Perhaps my frustration with this book owes something to my background as a chemist. The periodic table was a revelation to me at age 16, as I realized that one could understand and predict an enormous amount of chemistry from the simple notions of valence shown by the numbering of the columns and the easily visualized progression of atomic size down each column, rationalized perfectly and easily by the pairing and exchanging of electrons between atoms. Carter's circlonic rendering of the table has none of those uses, it even masks them. Perhaps my background in chemistry is also why I reject the assertion (p. 262) that theoretical physics "is supposed to be the bedrock of scientific ground": We chemists sometimes call ours "the central science," because without it there is no biochemistry



or physiology or geology, and these are the sciences that really matter to human beings in ever-present practical ways. Physics gained much or most of its modern cachet from the successful creation of atomic bombs, but that creation resulted from the work of chemists and engineers and mathematicians more than from that of physicists. Certainly physics can claim the equation $E = mc^2$, but it was chemists Meitner and Hahn who discovered the energy-releasing phenomenon of nuclear fission.

Wertheim had been struck by the similarities between a conference on string theory and a meeting of the Natural Philosophy Alliance, and apparently drew the opposite of the right conclusion: She seems to take the pseudo-science of string theory as justifying the scientific status of "fringe physics," instead of recognizing that string theory is an emperor without clothes, even though she cites the works by Smolin and Woit which demolish the pretensions of string theory. Philosophy owes us a discussion of the limits of feasible human understanding. It seems to me that we can understand—that we can *feel* that we understand—only things and interactions that have a recognizable connection, analogy, similarity to human-scale phenomena; so infinity, multiple universes, and ultimate origins are simply outside possible human comprehension. String theorists and their ilk are attempting the impossible.

Wertheim is quite right—and it is a point worth noting—that the ready availability of computers, PowerPoint, and the like makes it possible to project all the externalities of professional conferences without any of the substance. That illustrates perhaps the most crucial point about trying to judge whether a venture is potentially useful science or not: There is absolutely no substitute for digging into the pertinent evidence and the arguments pro and con. There are no valid shortcuts, not "falsifiability" nor "scientific method" nor "consensus" nor any of the other proposed approaches: The demarcation problem is insoluble.[2] I felt embarrassed for Wertheim for the suggestion that Carter's theory of gravity qualifies as scientific because it is falsifiable (p. 226). Her discussion of criteria for credibility is certainly correct on the point that formal credentials, degrees, do not suffice; but I demur from Wertheim's open-ended question, whether theoretical physics belongs to a category like brain surgery where credentials and experience matter or to the category of art and literature, "open to anyone who wants to have a go."

If theoretical physics is science, then its criteria are not those appropriate to art and literature, because physics has to jibe with external material reality. I also demur from the suggestion that "one of the purposes of science is to help us feel 'at home in the universe'," and that Jim Carter and the NPA are calling for a reformation of science analogous to the religious Reformation instigated by Martin Luther et al. Science is simply impersonal.

The book gives a useful history of the Natural Philosophy Alliance (NPA); but citing NPA as exemplar misleads by dodging the central issue of potentially believable versus rank pseudo-science and the long spectrum between those extremes. Wertheim cites the NPA's website and its listing of "dissident scientists" which lumps together highly competent insiders who happen to espouse unorthodox views but are nevertheless acceptable in mainstream circles (for instance, Maurice Allais, Hermann Bondi, I. J. [Jack] Good); highly competent insiders who espouse some unorthodox views not acceptable in mainstream circles (Kary Mullis, Nobel Prize for inventing Polymerase Chain Reaction; Martin Fleischmann among other researchers of "cold fusion"; Halton Arp and other non-Big-Bang scientists; Tom Van Flandern) but also others whose activities left and will leave no mark in the advance of science (say, Wilhelm Reich[3] and several of his acolytes; Immanuel Velikovsky and several prominent neo-Velikovskians—as well as a host of names that Google knows nothing about). Many on the list have no warrant to be called scientist at all. (I should mention that my own name appears on the list. I had not been asked or invited.)

If this book's aim is to illuminate fringe physics, the bibliography ought to help readers to other works dealing with the fringes of science, yet it lists only two: De Morgan's *Budget of Paradoxes* from 1872 and Jeremy Bernstein's essay collection from 1993. There have been quite a few others in the last half century or so, following Martin Gardner's *Fads and Fallacies in the Name of Science*.[4] Just as Jim Carter wants to do physics ab initio, so this book sets out to do philosophy of science and science studies ab initio.

I dislike writing so negative a review, especially when this book is an easy read with much interesting narrative. To reassure myself that I was not being unfair, I looked for other reviews of the book; and I was astonished to read such comments as "fascinating, bizarre, and provocative new book . . . brilliant thesis: that the 'cranks' and 'crackpots' lurking on the fringes of the scientific establishment are manifesting the same esthetic impulses that drive outsider artists . . . . finely wrought, sympathetic, and stimulating survey of gonzo ingenuity in the service of science";[5] or "very thought-provoking book . . . . an important book, one which raises in an interesting way fundamental issues about how people think about and conduct research into fundamental theoretical physics,"[6] from Peter Woit, who has revealed

string theory as pseudo-science. Michael Shermer, otherwise a fierce debunker of pseudo-science, calls the book "enlightening. In an elegant narrative Ms. Wertheim has taken on one of the knottiest conundrums in the philosophy of science, the demarcation problem—that is, how to find criteria to define the boundary between science and pseudoscience. . . . Ms. Wertheim has convinced me that I may be too hasty in pre-emptively dismissing . . . especially . . . Jim Carter."[7] If Shermer cannot dismiss circlon theory, then he has no business posing as a judge of what is and what isn't science or of what is and what isn't pseudo-science. Eventually I was reassured by finding some other reviews that express the same reservations as I have: by Michael Gordin in *American Scientist*,[8] by Jesse Singal in the *Boston Globe*,[9] and indeed Peter Woit, having declared the book "thought-provoking," admits that he has "essentially zero sympathy for this kind of thing [Carter's work] as science."[6] I'm unable to shake the sense that the laudatory comments about this book stem from empathy with the author, for whom this was clearly a work of love and fascination. If so, this is a sort of condescension and not a service to readers of book reviews.

I do agree with the favorable reviews that the book is fetchingly written and that Carter is a fine fellow worth knowing. We also learn about some interesting but obscure tidbits in the history of science. But the book does not illuminate the differences among mainstream science, unorthodox views within mainstream science, and the outsider claims that range all the way from possibly valid to blatantly nonsensical. Nowadays journalists seem increasingly to regard their job as gathering information by interviewing people; but science journalism calls for looking into the substantive evidence, for without that the journalist cannot judge the degree to which the interviewed experts, insiders or outsiders, can or cannot be trusted.

Notes

[1] John C. Burnham, *How Superstition Won and Science Lost*, Piscataway, NJ: Rutgers University Press, 1987.

[2] Larry Laudan, "The demise of the demarcation problem," pp. 111–127, in *Physics, Philosophy and Psychoanalysis*, edited by R. S. Cohen & L. Laudan, Dordrecht: D. Reidel, 1983.

[3] Reich's orgone energy is nonsense. It is irrelevant that his approach to psychotherapy has been of practical help to some number of individuals.

[4] Martin Gardner, *Fads and Fallacies in the Name of Science*, Dover, 1957. [First edition, *In the Name of Science*, G. P. Putnam's Sons, 1952]

[5] Paul Di Filippo, In the Margin column, *The Spectator*, May 25, 2012, barneshttp://bnreview.barnesandnoble.com/t5/In-the-Margin/Physics-on-the-Fringe/ba-p/6101

[6] Peter Woit, December 24, 2011, http://www.math.columbia.edu/~woit/wordpress/?p=4246

[7] Michael Shermer, "On the margins of science," Bookshelf column, *The Wall Street Journal*, December 10, 2011.

[8] Michael D. Gordin, "Everyman's physics," *American Scientist*, Jan/Feb 2012, 81–83.

[9] Jesse Singal, Book Review, *The Boston Globe*, December 26, 2012, http://articles.boston.com/2011-12-26/arts/30556119_1_physics-outsider-theory.

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A Scientific Adventure: Reflections on the Riddle of Relativity by Ian McCausland. Montreal: Apeiron, 2011. iv + 252 pp. $20 (paperback). ISBN 9780986492662.

This book is highly recommended reading for anyone interested in scientific controversies about firmly accepted mainstream beliefs. It illustrates cogently how proponents of a mainstream view fail to engage substantively even with tightly argued and logical critiques. Although the polemical tactics are quite typical, the substance of this controversy is untypical: The sole point at issue is whether the special theory of relativity (STR) is inconsistent, whether it is based on a logical inconsistency. By contrast, in almost all other such arguments the questions concern the nature of evidence, the reliability of observations, the designs and protocols of experiments.

McCausland has published two articles in the *Journal of Scientific Exploration* (one of them while I was Editor). He had attended the 1991 SSE meeting where he met Jack Good, with whom he subsequently had a long exchange over the validity of STR. I had reviewed favorably McCausland's earlier book, *The Relativity Question* (*Journal of Scientific Exploration*, *3* [1989] 217–219). The present work is in some sense an update of that one. The earlier book focused chiefly on Herbert Dingle's role in questioning STR; this one recounts McCausland's continuation of Dingle's struggle.

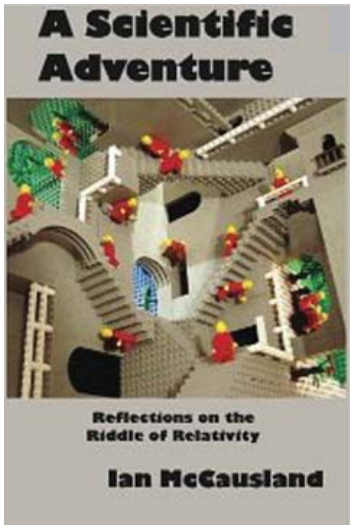
Dingle had been an early proponent of special relativity, but grappling with the twin paradox or clock paradox eventually led him to assert that the theory is inconsistent. Here is the issue:

The special theory deals with uniform relative motion. Two identical synchronized clocks move relative to one another. The theory's mathematics calculates that the faster-moving clock runs slower.

The trouble is that the theory postulates that there is no absolute frame of reference to specify the state of rest, so there is no way to identify the faster-moving clock. Hence the paradox: Each of the clocks runs slower than the other.

A popular attempt at resolution of the paradox is that each clock only appears to the other clock to be running slower; but this contradicts the original Einstein publication as well as asserted experimental proofs that moving clocks *actually* run slower.

Such experimental proofs constitute another common defense of the theory. However, McCausland argues convincingly that experiment is irrelevant to the question of self-consistency of a theory. He adduces much support on this point, for example from Karl Popper: If a theory contains



an inconsistency, then any result at all can be derived from it, and so the theory is useless and uninformative.

Some of the claims of experimental proof refer to situations where forces and accelerations are present, variables specifically excluded by the postulates of the special theory. A similarly unsound defense is the sometime assertion that the special theory is right because the general theory is right; but the two are independent of one another.

Perhaps most striking is that defenders of the special theory have offered a number of different and sometimes mutually incompatible arguments—without actually addressing directly Dingle's question, "Which of those two clocks runs slower?"

As long as I can remember, the special theory has seemed to me too difficult to understand, so Dingle's conclusion is congenial to me: The twin or clock paradox is actually a contradiction, not a paradox. A theory that postulates nothing but symmetry surely cannot lead to an asymmetrical conclusion.

I found helpful here the point that Einstein's formulation is mathematically identical with that of Lorentz. Which of the associated physical interpretations is preferred cannot therefore be decided by experiment. The more general point, all too often neglected by practicing scientists, is this: The ability to make calculations that describe phenomena accurately says nothing about the physical interpretation of the mathematical variables in the given equations. The Newtonian view of gravity—action at a distance—is not proved by the successful calculations that continue to be made with Newton's equations. The success of calculations based on general relativity do not entail that gravity is really a curvature of space or of space–time. The success of quantum-mechanical calculations does not establish any particular physical interpretation of such things as wave functions.

Observers of controversies over anomalies will recognize the generality of the stories related in this book: Journals that reject manuscripts without review, without giving reasons, or giving inappropriate reasons, and which refuse criticized authors the opportunity to respond (see especially Chapter 15 on censorship). In this connection, *Nature* and John Maddox pop up

several times in an unfavorable light (see Chapter 6 in particular). Several defenders of the mainstream refused McCausland permission to publish what they had written in argument against him or against Dingle; as McCausland points out (p. 59), not only does censorship prevent a viewpoint from being presented to the scientific community as a whole, such refusals even make it difficult to describe the censorship.

An important point (pp. 127–128) seldom made is that science lacks the sort of incisive criticism that has long been part of art and literature: Criticism that is substantively insightful yet intellectually independent of those who created the work being considered.

Dissenters from relativity theory are quite often cited by mainstreamers as examples of crackpots. McCausland demonstrates that quite a few of the dissenters are perfectly rational and clearheaded, so this book is likely to be relished and to bring solace to other people who are labeled crackpots, cranks, denialists just because they see flaws in some dogmatically held mainstream belief. Worth remembering is the general point that when the experts disagree among themselves, they cannot all be right but they could all be wrong. Worth quoting and re-quoting is McCausland's insight that "the strongest and most frequently used argument . . . [by mainstream experts], an argument which is singularly difficult to rebut . . . , [is] complete silence" (p. 121).

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The Science Delusion: Freeing the Spirit of Enquiry by Rupert Sheldrake. Coronet, 2012. 400 pp. €19.99 (paperback). ISBN 9781444727937.

Dr. Sheldrake, an eminent biologist and creative thinker, astounded the scientific world in 1981 with his first book, *A New Science of Life.* Sheldrake posited the view that nature contains within her breast, fields that guide and change life forms. He called them *morphogenetic fields*, which I will label simply as MG-fields. In September, 1981, John Maddox, a senior editor of *Nature*, published an Editorial concerning Sheldrake's opening opus entitled "A Book for Burning?" In it, Maddox said:

> Sheldrake's argument is an exercise in pseudo-science. Many readers will be left with the impression that Sheldrake has succeeded in finding a place for magic within scientific discussion—and this, indeed, may have been a part of the objective of writing such a book.

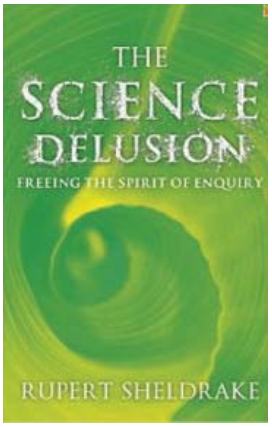
Maddox did not act concerned by the criticism his "burning" comments received, and elaborated on his views: "Sheldrake's [view] can be condemned in exactly the language that the Pope used to condemn Galileo, and for the same reasons: It is heresy."

Therein lays the crux, as they say. Scientists, perhaps exhibiting the very same heretical morphogenetic field of the past clerics surrounding Galileo's time—who abhorred any publishing of anti-clerical views—have seemingly adopted a similar abhorrence in their reluctance to accept within scientific legitimacy anything to do with what cannot be demonstrated by means brought forward via materialistic demand. Or perhaps scientists are in morphogenetic resonance with the field of Bruno at the burning stake; they have adopted a closed and fearful mindset when it comes to going beyond the bounds of materiality. No one will strike the match of illumination to examine the boundary between materialistic and non-materialistic causation, and thus all enquiries in this matter are to be left unstated; "Don't ask, don't tell" is alive and well within the "sacred" halls of science.

Again Sheldrake fearlessly rises to face the burning stake and take materialists to the Inquisition, this time asking them to prove their own claims of materialistic domination. At the end of each chapter he posits questions that need to be answered by materialists as scrupulously as they appear to be asking non-materialists for answers.

I loved this book. Not only is it a delight to read, it makes very logical and clear points that any scientist should be considering in their assumptions concerning the universe, in particular non-materialistic causation.

What remains astonishing to me in this 21st century quantum age of



reason is how any serious scientific investigator can take 19th century materialism and all that it professes to include seriously. With the discoveries of quantum physics, materialism was well buried in the past more than one hundred years ago with the discoveries of the *standard model* of quantum field theory (circa 1930–1980) and the special theory of relativity (circa 1905–1910). The most fundamental elements of material are now known to be nonmaterial yet energetic excitations of fermion quantum fields. As such, these excitations making up the families of fermions—quarks, electrons, neutrinos, muons, and tauons—can be viewed as moving at lightspeed until they encounter interaction with another field; i.e. the sacrosanct masses of all the particles and the sub-particles that make them up are known to be excitations of these fermion quantum fields in interaction with an apparently invisible Higgs field (the Higgs particle is still being sought for with greater confidence).

So it is that Sheldrake asks us again to consider his MG-field. It is perhaps no more surprising to think that an MG-field gives form and character to living matter than that the Higgs field does so to "dead?" matter. What's new and heretical about that?

This leads to many consequent boundary illuminations involving how such fields can be detected as causative actors in biological matter including humans and animals. Sheldrake goes on to posit that perhaps we search in the wrong places to expect to find memory within brain and nervous matter when we already know that the memory of matter itself lies within the quantum fields that produce it. Once a material particle comes into existence, the field that produced it and the particle that materialized out of it continue to interact. Without this continual interaction, our universe could not and would not ever come into existence. You can think of this as a resonance that, for example, keeps fermions minding their own business and excluding each other from entering into the same state (known as the Pauli Exclusion Principle). Good thing that, for without this "resonance" atoms could not and would not exist. Nor would you or I.

Taking Sheldrake's views seriously then leads to new explorations of the material/nonmaterial boundary. For example, take the age-old battle between mind and brain theorists. Does the mind arise as an epiphenomenon from electrical hydrogenated matter? Or is it merely the MG-field acting in resonance with the brain tissue that came from it that produces mind? Examining many cases both anecdotal and experimental, the author

concludes that mind is indeed extended beyond any spatial or temporal boundaries—a conclusion I came to as well in my studies of the dreaming brain and the timing of conscious experiences.

Just how this field works is still a bit mysterious, and I can only add my own speculative thoughts. Previously it seems to me that Sheldrake took it that only the past acts causatively in affecting living matter through the MG-field. In this sense perhaps Sheldrake was still himself caught up in the old mechanistic views of Newton and today's modern materialists (cause before effect). The MG-field involves a kind of tuning of brains with past experiences beyond the brains that seemingly encompass them.

In *Delusion* he does consider telepathic communication as being part of the effects of the MG-field including

> future emotional events [that] seem able to work 'backwards in time' to produce detectable physiological effects.

So it may seem that quantum physics which posits such actions plays a greater role in the production of the MG-field than Sheldrake's earlier MG-field theory considers. If we take quantum field theory seriously enough, then it appears that both future and past spacetime boundaries play roles in the arising of mind within matter—something I discussed in greater detail in my own books and papers.

Sheldrake considers many other areas of scientific enquiry normally contained within the purview of objective science and finds them lacking as well. Discussions of how the viewpoints of scientists altered and skewed their findings according to expectations are well-covered here. Even the role played by our educational institutions is taken to the stake. When a "scientific fact" is well-established, there is greater reluctance to publish results that disagree with the "facts." Science students are encouraged to get the "right" answer in experimental class studies rather than the answer they get, and so tend to "fudge" their own work. He questions whether we should take it on face value that the so-called "constants" of nature are really so or perhaps are merely constructs of our own thinking.

Delusions is very well written and enjoyable to read and ponder. *JSE* readers will find it of great interest I am sure. Unfortunately, the book would be better suited for the audience who would still consider it a book for burning and would remain closed to its contents as surely as they would fail to open the book to examine them.

Fred Alan Wolf
Author of The Dreaming Universe, The Spiritual Universe, Mind into Matter, *and* Time-Loops and Space-Twists: How God Created the Universe

Comets and the Origin of Life by Janaki Wickramasinghe, Chandra Wickramasinghe, and William Napier. New Jersey/London: World Scientific, 2010. 232 pp. $70 (hardcover, 2009). ISBN 9789812566355.

This volume is the latest in a series of books and articles stretching back more than three decades on a theme quite startling in its claims and implications: that terrestrial life did not originate on Earth but arrived in the form of cells or bacteria from outer space. The idea of "panspermia," that the seeds of life are spread from planet to planet, dates to the 19th century with the ideas of Lord Kelvin. It was championed by the Swedish physicist, chemist, and Nobelist Svante Arrhenius at the beginning of the 20th century. Once scientists recognized the difficulties of life surviving in the conditions of interplanetary and interstellar space, by the 1960s a neo-panspermia became popular: not life itself, but prebiotic chemicals were the new seeds of life, made more likely by the discovery of numerous complex organic molecules in meteorites, comets, and interstellar molecular clouds. But the difficulties of synthesizing anything more complicated than amino acids in the wake of the famous Miller-Urey experiment in 1953 kept alive the idea that life itself may be spread throughout the universe.

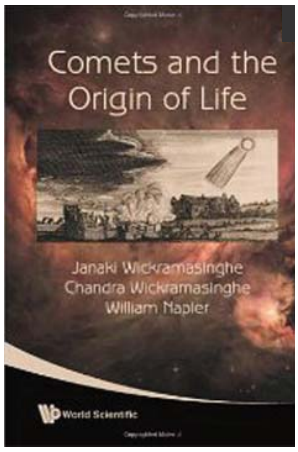
At the center of this work is Chandra Wickramasinghe, a research student of the maverick astronomer Fred Hoyle. In 1962 Hoyle became interested in the origin and nature of interstellar dust, in particular as found in dense molecular clouds, and he and Wickramasinghe set to work on the problem. They became convinced that dust could not form inside molecular clouds, but must have originated in the atmospheres of cool stars, protoplanetary discs, or supernova ejecta, a theory now widely accepted. It was the next steps that became increasingly controversial: that the spectroscopic signature of dust was best explained by complex biomolecules such as cellulose; that biomolecules were assembled into still more complex forms inside comets; and that the living cells and bacteria generated there were responsible for the origin of life on Earth. And not only that: Hoyle and Wickramasinghe argued that the delivery of bacteria from space continues, affecting both the origin and the ongoing evolution of life, and may even be responsible for certain diseases on Earth. These theories were not only reported in reputable scientific journals such as *Nature*, but also in popular books including *Lifecloud* (1978), *Diseases from Space* (1979), and *Evolution from Space* (1981). Biologists were not impressed; Lynn Margulis, not known for the timidity of her own theories such as endosymbiosis, called the first book "wanton, amusing, promiscuous fiction."

The current volume, based on a Ph.D. thesis at Cardiff University

completed in 2007 under the supervision of William Napier, does not go so far as the disease claim, but limits itself to the latest evidence for what we might call the "microbial life panspermia hypothesis," whereby life itself is spread throughout the universe via comets. The germ of the hypothesis originates with, and is given initial credence by, two well-known facts: First, life on Earth originated shortly after the so-called "late heavy bombardment" of the planet by planetesimals about 3.8 billion years ago; and second, the Oparin–Haldane–Miller theory of the spontaneous origin of life on Earth from non-life has defied all attempts at laboratory synthesis beyond the amino acid stage—a long way from life. In the view of the authors, their hypothesis is proven by a variety of spectroscopic evidence. Their conclusion, that comets harbor primitive microbial life and are the agents for the distribution of life on a galactic scale, not only accounts for the origin of life on Earth, but also offers a sweeping vision of a universe full of life. It does not, of course, solve the problem of the original origin of life, but necessitates only one origin somewhere in the galaxy, or even the universe.

As with so many other questions, the validity of this sweeping theory comes down to the nature of evidence. And it is here that many critics find the argument wanting. Two examples will suffice to show the uncertainty of the arguments. In Chapter 2 on "cosmic dust and life," the authors discuss evidence supporting biological dust grain models, in other words dust grains with a possible biological provenance. In one case they describe the Stardust mission, which captured dust particles from the tail of comet Wild 2 in January 2004. The results included the detection of hetero-aromatic organic molecules rich in nitrogen and oxygen, which the authors conclude "could be a tell-tale sign of degraded material, biology being particularly rich in such structures" (p. 57). It could also be something else. In this sense the argument is reminiscent of those made for nanofossils in the Mars meteorite ALH84001: The magnetite in the Mars rock could be biogenic, but not necessarily. Most scientists have concluded that even with three other independent lines of evidence, it is unlikely that the rock bears evidence of past life on Mars.

In a second argument the authors discuss the capture of stratospheric dust in the Earth's atmosphere via U2 aircraft, and compare a carbonaceous structure in one of the particles to a 2-million-year-old microbial fossil found in the Gunflint cherts of Minnesota, concluding that "in view of the striking similarity seen between the two images . . . the most reasonable explanation might be that the particle . . . was a partially degraded iron-oxidising bacterium" (p. 60). The possibility of contamination aside, the words "might be" hang heavily over the claim; the particle might just as easily be something else. Such morphological arguments have a long



history of controversy, ranging from the Claus–Nagy controversy in the 1960s over "organized elements" in meteorites (still alive today in the claims of Richard Hoover et al.), to the Brasier–Schopf controversy over the 3.45-billion-year-old microfossils of the Apex chert formation in western Australia. Schopf (one of the main skeptics when the Mars rock nanofossils were announced in 1996) claimed certain structures in this formation as evidence for the oldest fossils on Earth; in 2001 Brasier and his colleagues argued that they might not be fossils at all but deposits of graphite or organic molecules produced abiotically. Many scientists now prefer the latter interpretation.

The authors also must argue for the origin of life inside comets, and for the viability of microbes under extreme conditions for long periods needed for panspermia to be effective. They reason that molecular clouds and comets can shield any interior microorganisms from ultraviolet radiation. Ionising radiation is more damaging, but they argue that only a minute number of microbes would have to survive for cometary panspermia to work. Moreover, extremophiles on Earth increasingly demonstrate how rugged life can be.

Neither biologists nor astronomers have been impressed with the Hoyle–Wichramasinghe arguments over the last 30 years, and they are unlikely to be convinced by the new evidence presented here. Molecular biologists consider the gap between non-life and bacteria to be very large even under the relatively stable conditions of Earth, reasoning that it is even less likely that it could have happened spontaneously inside comets or molecular clouds. For their part, astronomers have not been convinced by the spectroscopic evidence. While the reader will learn a great deal about comets, interstellar dust, and molecular clouds in this volume, and while the authors may be correct in their panspermic conclusions, their less-than extraordinary evidence has not convinced the scientific community of their extraordinary claims. As Carl Sagan reminded us in another of his pithy quotes, "what is called for is an exquisite balance between two conflicting needs: the most skeptical scrutiny of all hypotheses that are served up to us and at the same time a great openness to new ideas." At stake here, as in so many other areas, is maintaining that balance.

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Mathematics: The Loss of Certainty by Morris Kline. New York: Fall River Press, 2011 [originally published in 1980 by Oxford University Press]. 464 pp. $19.95 (paperback). ISBN 9781435136069.

In 1980 Morris Kline wrote this engaging book, in which he took on many of the myths about the nature and history of mathematics. This new edition will probably be as seldom read as the original, which is too bad because it contains important messages, including perhaps some comfort for anomalies researchers. I will briefly present an overview of the book's contents, and then say what I think these comforts are.

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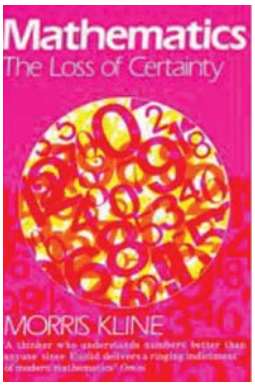
The ancient Greeks developed the seed of what we now think of as mathematics. Kline points out that their mathematical concepts arose from consideration of the natural world, and then the fact that numbers, shapes, and relationships corresponded to things in the real world convinced them that reality itself was in some mystical way generated by numerical principles. The regular patterns that they found in geometric forms and simple integers reflected the regularities of nature, and so provided keys to understanding how things were, and why they were that way. The faith that mathematics lay behind the mundane world of observations became an unquestioned truth, at least as important as the practical techniques the Greeks devised, and passed along through the Middle East and the medieval period to modern Europe.

Of course Euclid's *Elements* was the foundation of the Greek legacy. There is no question about the fact that it was designed in order to describe the spatial aspects of the world we live in. It was not a hodge-podge of facts bound loosely together because they all pertained to space, but rather an intricate structure, in which one started with definitions that clearly applied to real things (points, lines, and so on), and then through the power of deductive logic alone one discovered and even proved things that could be observed in the real world. As a model for what a deductive system should look like, it persevered well into the modern period in Europe. But perhaps more important than its specific insights and theorems, it justified a view of mathematics as an engine with which the human mind could understand the natural world. It was a short step from there to believe that the natural world was designed and created on the basis of Euclid's geometric truths. Since the religionists of modern Europe were quite eager to obtain a monopoly on the truth, they had little difficulty convincing themselves that the hand of the Creator was to be seen in this remarkable relationship between apparently abstract mathematics and concrete reality.



The idea that one learns about nature through observation and experiment developed slowly during the early modern period, but, as Kline argues, if mathematics represents truth, and truth is exemplified in scientific observations, then mathematics must be the appropriate language for talking about science. Therefore, one whole strain of the development of mathematics, from the 15th to the beginning of the 19th centuries involved the increasing mathematization of science. The success of this enterprise had the effect of bolstering the belief that mathematics and truth were necessarily bound together.

The first crack in this world view came in the 17th century, when a number of mathematicians simultaneously developed calculus (although Newton and Leibniz usually receive most of the credit). Kline does not mention it, but the basic ideas of calculus go back to Archimedes, who failed to invent it primarily due to an inadequate number system. All versions of calculus involved taking ratios of things where both the numerator and denominator tend to 0. The problem was in claiming that this operation had some kind of legitimacy. One might say that something like 1/0 could be interpreted as infinity (whatever that was), but 0/0 would not yield to any sensible interpretation. The overwhelming fact about calculus was, however, that it was immensely useful. From the logical standpoint, this was a muddle, since one seemed to be performing nonsensical steps to consistently obtain correct answers. Virtually all of the arguments about the nonsensical steps were metaphysical, both on the side of Newton, Leibniz, and their adherents, as well as on the side of the opponents, notably Bishop Berkeley. As Kline points out, this unsatisfactory situation continued quite persistently for at least two centuries, until Cauchy provided the modern definition of a "limit."

Despite the saving of calculus by Cauchy, Kline sees an even further unraveling of the logical status of mathematics in the 19th century. The first difficulty with the "mathematics = truth" equation was created by Hamilton in 1843 when he invented quaternions. He was trying to address exactly the same kinds of problems as Euclid, the description of three-dimensional space, but using algebraic methods rather than deductive geometry. Quaternions are intimately bound up with rotations, and as anyone familiar with Rubik's Cube has discovered, rotations in three dimensions are not commutative (the order in which you perform a sequence of rotations is important to the result). Mathematicians in Hamilton's time were so committed to the idea

that arithmetic (as they had learned it) was truth, that it was illogical (if not blasphemous) to talk of multiplication being non-commutative. Despite the historical importance of quaternions, they tended to fall by the wayside, only to be rediscovered recently in applications to aircraft electronics and video games.

The second difficulty was closer to a disaster. The lore of centuries had held that Euclid's geometry was the one and only true geometry. But there were a few things that were not entirely clear. One was whether Euclid's axioms were independent of each other. Of particular concern was the "parallel postulate," which can be stated several different ways. This axiom seemed to many mathematicians to be less self-evident than Euclid's other assumptions, and since doubt appears as the enemy of truth, it was important to clear the matter up. One way of looking at the problem was to ask whether the parallel postulate could be deduced from the other axioms. If so, then it could be discarded as an axiom, and all the rest of Euclid's work would remain as it was. But if not, the possibility presented itself that one might be able to state Euclid's geometry in a form that simply did away with the parallel postulate, or perhaps replaced it with a different version. This latter step was taken by a number of 19th century mathematicians, creating a variety of "non-Euclidean" geometries. Again Kline points out that most mathematicians rejected these geometries as novelties, because they held to the "mathematics = truth" belief, and the real world was obviously Euclidean. When a special case of Riemann's elliptical geometry was seen to apply to the surface of a sphere (where "straight line" means "great circle"), then because the sphere was also a part of Euclid's geometry, the tide turned in favor of acceptance of non-Euclidean geometries.

Having seen their discipline pass successfully through several challenging storms, the mathematicians of the early 20th century expressed supreme confidence that all of the potential logical problems with mathematics had been dealt with, and all that remained (in the words of Lord Kelvin) was to fill in the details. Figures such as Bertrand Russell and David Hilbert undertook the task of putting all of mathematics on a solid foundation of some version of logic. Hilbert in particular was certain that his "proof theory," which we now call "formal systems," was the correct way forward, and so he proclaimed that the end of the period of uncertainties in mathematics was at hand.

But once again fate conspired to dash such noble hopes. In the early 1930s Kurt Gödel proved that any system at least as complicated as arithmetic (in the mathematical sense) was either inconsistent (one could deduce contradictions) or incomplete (there were true statements that could never be proved from within the system). For centuries one of

the most troubling uncertainties that had bothered mathematicians was whether or not the systems of thought they had inherited were consistent. If Euclidean geometry was inconsistent, then it could not describe space (which evidently is consistent), but how could we tell? What was needed was a methodology for testing whether a given system was consistent. But Gödel's result then said that if you achieved this for some system, you would have simultaneously found that there were truths in the system that could never be discovered, within the system. Applying this to all of mathematics, Gödel had shown that Hilbert's program of reducing mathematics to formal systems was doomed. While on the one hand we can see Gödel's result as a triumph of mathematics, on the other hand the victory seemed remarkably Pyrrhic.

Kline finishes his narrative with observations on the foundations of mathematics, especially the "axiom of choice" and Cantor's "continuum hypothesis." His opinions come out most strongly in the later chapters where he rails against the modern tendency of mathematicians to value abstract, literally useless creativity, as opposed to the direction of mathematics back to its roots, the solution of actual problems.

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Why should any of this be of interest to anomalies researchers? I think that much of it is, for the simple reason that woven in among the major themes I've described above, Kline includes rich detail about just how confused most mathematicians have been throughout most of the history of their discipline. He points out how vague Euclid's actual definitions and axioms really are, and how much trouble this created for those who wanted to see geometry as being logically tight. He also emphasizes a fact that so far as I know has been completely omitted from mathematics texts and virtually all histories. Although negative numbers were known in medieval times, and the necessity of the square root of minus 1 since the 15th century, it was not until perhaps the late 18th century that mathematicians began to accept these as numbers. For the complex numbers, one can perhaps understand the reluctance, although Euler had already shown that they were no more complex than the two-dimensional plane. But the inability to conceive of the use of negatives shows a truly remarkable failure to employ the one feature of mathematics that everyone agreed to—that it should portray reality. It is even more astonishing to realize that Newton developed calculus while believing that subtraction of a larger number from a smaller one was a meaningless operation. Even further, insofar as mathematics being the model of a logical deductive system, all of the great mathematicians used complex numbers, and sometimes even negative numbers, to obtain their results. Again we see logic sacrificed for the sake of practicality, and

the hypocrisy of claiming the infallibility of the results because they were supposed to arise from a logically pure source.

It does not stop there. Consider the fundamental point in the definition of calculus, that ratios of quantities each approaching zero gives an uninterpretable 0/0. Consider the ratio $(1 - x^2)/(1 - x)$. As x goes to 1, the numerator and denominator each go to 0. But the expression is equal to 1 + x, and everyone agreed that this goes to 1 as x goes to 0. In other words, there are pathetically simple examples to demonstrate that there is not necessarily any problem with the Newton/Liebniz infinitesimal ratios. Moreover, Newton believed that all continuous functions were differentiable, flying n the face of truly trivial realistic counterexamples. I have seen this same pattern come up in how scholars of this and subsequent eras dealt with questions about probability. Often they endlessly debated points with barrages of philosophical arguments, when a few simple examples would have made the situation abundantly clear.

Although Kline mentions the fact that even into the 19th century mathematicians were confused about discontinuous functions, he does not mention the famous story about Fourier. In his investigation of the propagation of heat, Fourier asserted that any function could be approximated by a series of sines and cosines. His assertion so offended the leading lights of his day that its publication was blocked for more than a decade. (He was almost right; the notion of pointwise convergence needs to be replaced by convergence in L_2 norm). Kline does, however, devote a section to how confused even the great mathematicians were about series (of numbers, not even functions).

Here is the lesson that I take from Kline's history. In mathematics we have an excellent example of a method of thinking that laid claim to absolute truth, while it was in fact often wallowing in confusion and error. The situation was complicated by the fact that much of the mathematics that was created was both subtle and incredibly useful. But this turned out to be a double-edged sword, since every advance brought with it further confidence in the underlying logic, and simply postponed the day of reckoning. The proponents of mathematics went vastly beyond the facts in their crowning of it as the "queen of sciences," and were thus largely blind and resistant to most of the attempts to remove the evident problems. The history of mathematics is not what is taught in elementary science classes, an inexorable march of progress, but instead it lurched from success to disaster to success . . . and so on for centuries.

As a final example of the hubris of conventional science, we can cite the topic in dynamic systems theory somewhat inappropriately called "chaos." This could not have been covered by Kline, because Edward Lorenz did

not make his celebrated rediscovery of the phenomenon until several years after Kline's book was published (and Kline died in 1992). But it would have suited Kline's purpose admirably, since it was Henri Poincaré, just after the turn of the 19th century who discovered and fully appreciated the essential unsolvability of certain easily stated physical problems. Poincaré turned away from the abyss, and for 70 years no one else sneaked up to the edge to take a peek. And that, I think, helps to define the role of anomalists in the 21st century; they are the ones who go up to the edge and peek.

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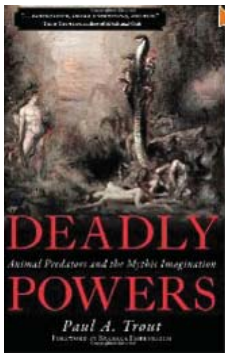
Deadly Powers: Animal Predators and the Mythic Imagination by Paul A. Trout. Amherst, NY: Prometheus Books, 2011. 325 pp. $26.00. ISBN 9781616145019.

The question of how myth, folklore, and religion originated has exercised the scholarly imagination at least as far back as the Greek philosopher Euhemerus. The answers often have depended more on imagination than evidence. In the Victorian era, the German philologist Max Muller promoted "solar mythology" as the solution for all such origins, arguing that the movements of celestial bodies, seasonal events, and weather phenomena preoccupied the minds of primitive peoples, and their natural poetic abilities distorted these observations into fanciful anthropomorphic tales of gods and heroes. Freud applied psycho-analytic theory to the problem and found that myths began in dreams as the psyche struggled to resolve Oedipal and other developmental conflicts throughout the course of life. Structural anthropologists regarded myths as templates arising to reconcile logical contradictions in the concrete thinking of the primitive mind.

A sweeping explanation for origins may gain widespread acceptance for a time, but this dominance seldom endures for long. Anthropological fieldwork typically uncovers exceptions and alternative possibilities that overthrow the theory. In retrospect, it appears obviously wrong, and worse still the evidence and arguments that once seemed so convincing come to look as embarrassing as "Ancient Astronaut" speculations. Once burned, scholars shy away from the subject and frown on any efforts in its direction. Yet the problem of origins never stays down for long. Because it remains one of the "big" questions of human science, the origins problem continues to tease scholarly curiosity and sooner or later another ingenious proposal comes forward.

In the last few decades the issue of origins has returned from two directions, despite considerable disciplinary resistance. One approach has tried to solve limited problems rather than provide a comprehensive theory. A readiness to look for an experiential basis behind even extranormal traditions, a position argued long and well by folklorist David Hufford, has backed these efforts with persuasive evidence. Strong cases have emerged that the catastrophic eruption of the Santorini volcano about 1600 BC gave rise to the Atlantis legend, that misunderstood observations of the phenomena of bodily decay provided the basis for vampire beliefs, and that discoveries of dinosaur fossils by pre-modern peoples led to stories about giants and monsters. An even bolder appeal to experiential origins



has attempted to explain religious ideas of heaven and hell as the result of near-death experiences.

The second approach pays close attention to the circumstances and cognitive capabilities of humans over their evolutionary career. Significant changes in thinking abilities accompanied the transition from nonhuman primates to hominids and from the earliest hominids to *Homo sapiens*, with development of language, tool-making technology, and abstract thinking serving as momentous milestones along the way. The environment of our ancestors challenged them to meet never-ending needs like food and shelter, while the social environment obliged adaptations to internal group dynamics and relationships with external groups. Various scholars have sought the origin of myth and religion in humans becoming hunters and their transition to a diet dominated by meat. The successful hunter became a figure of power, a master of prey animals, and, as dispenser of food, the arbiter of life and death. The leap from human to divine seems not very great since these same characteristics are also prime attributes of primitive gods.

With foundations in archaeological research, primate behavior, cognitive evolution, and language development, current theories on origins surpass all predecessors in sound evidence and defensible argument. Paul Trout joins this debate, accepting much of the human past as recent scholarship reconstructs it, but finds the key to myths and religion in an experiential reality recognized but not fully confronted in ongoing discussions. His argument in *Deadly Powers* identifies the crucial fact of life for our prehistoric ancestors as a stark prospect of death by predators of overwhelming strength and ferocity. This day-to-day fear left its mark on our ancestors, a mark so consequential that it shaped our stories, habits, and religious beliefs and lingers with us today.

The idea of man as prey has not appealed to many scholars. “Man is not cat food!” proclaimed archaeologist Louis Leaky; but Trout counters that this statement is true only in the sense that man was not food for cats alone. He was on every large predator’s menu throughout the Pleistocene, when our distant ancestors were not burly Neanderthals or muscular Cro-Magnons but small, weak creatures with no means of defense. They faced a menagerie of man-eaters that consisted not just of lions, tigers, crocodiles, and sharks, but also larger and more terrifying killers like saber-toothed cats, dire wolves, cave bears, and gigantic forms of snakes, lizards, and eagles. The most dramatic (and traumatic) fact of life was sudden death that threatened from land, water, and air; and that death was an appalling one of

roaring, screaming, tearing, dismembering, and nothing left but blood and scraps, terrible for witnesses as well as for victims.

Trout emphasizes that our ancestors lived in constant fear. This fear sharpened our intellect to become aware of predators or the possibility of predators, to recognize their characteristics and read the signs of their presence, also to imagine how these enemies thought and to anticipate their actions. Though fear helped our ancestors survive, it also burdened them with insupportable stress that they had to manage and alleviate. Trout argues that one outlet was to "act out" the predator, through mimesis during pre-verbal times and later through storytelling. These performances communicated knowledge of survival value to the audience, but they also helped to control fear by demonstrating that sometimes the prey escaped to tell the tale. In this way the prey gained a small measure of control over their adversaries.

Once humans acquired the capacity for language, they also developed a cognitive fluidity that allowed the integration of previously isolated domains of knowledge. Words had a flexibility that memories of concrete objects did not, opening unprecedented possibilities for imaginative creation. The basic components of mythic thinking—attributing human properties to a non-human agent (anthropomorphism), seeing all objects as living things (animism), and believing that one being or object can transform into another (metamorphosis)—had antecedents in pre-verbal thought processes, but began to flourish only once language provided a conceptual medium for imagination.

Mythic thinking supplanted the strictly experiential world with an imaginative substitute, necessarily in close attunement with reality yet amenable to altered relationships with the sources of fear. Imagination created monsters, not the specific predators of experience but hybrid sums of multiple fearful creatures that added to the total of reasons to be afraid. These imaginary monsters could be huge, powerful, even supernatural; but they included human properties that allowed imaginative solutions like appeasement and negotiation. Humans began to identify themselves as animals, as the shaman able to communicate with animal powers, as descendants of a totemic ancestor, and as killers themselves by internalizing the ways of predators. The fear of real predators diminished as the mythic substitutes developed, while stories of heroes slaying monsters or tricksters able to outwit stronger adversaries helped control fears born from reality and imagination alike.

A plausible origin for much of myth and religion becomes understandable by taking fear of the predator as a template. The hero's quest in myths and folktales typically includes conquest of fearsome monsters such as dragons

or cannibal ogres. The gods of primitive religion often take the form of predatory animals or monsters, while rituals of sacrifice and supplication echo tactics to fend off predator attack. Initiation rites may depict the candidate being swallowed and dismembered, then emerging from the belly of the imaginary beast remade as a full adult. If deities humanized over the ages, such primitive traits as the angry god that had to be appeased persists even today, while such imagery as the maw-like mouth of hell, death as a stalker, and disease as a devourer preserve the awe and fear that the predator inspired. The danger of predation continues to grip modern audiences of movies like *Jaws*, *Jurassic Park*, and *Alien*, where the threat may be a shark, an alien, a zombie, or a crazy killer, but the emotional charge derives from the age-old fear of being eaten and the hormonally induced "survival ecstasy" that comes from living through the attack.

A proliferation of research into the physical, cognitive, and social evolution of the human line has opened new approaches for understanding cultural beginnings. Trout takes advantage of these opportunities as he synthesizes theory and evidence to build a wide-ranging account of the origin of myth and religion. While he depends on prior research, his book is valuable to lay readers as an introduction to the explanatory potential of current theories. At the same time, *Deadly Powers* is not merely derivative but a strong argument for the importance of predation in the history of human evolution. Trout's most original contribution insists that the predator and fear of its death-dealing powers have served as the primary agents of natural selection, shaping both mental and cultural evolution. He takes a long view of formative influences because to start with man as hunter or storyteller begins too late. By then our ancestors had been prey for millions of years and the damage was already done. This emphasis on the experiential challenge our lineage faced not only sounds persuasive at face value, but accounts for a great many attributed consequences like myths being about fear, primitive gods taking the form of monstrous animals, rites mimicking survival techniques before predators, and the continuing fear and pleasure connected with stories of being eaten alive.

The subject of cultural origins has traditionally been rather jealously guarded by scholars with their favored theories and preferences, so Trout's advocacy of predation as the key has the hurdle of prejudice to overcome. In more concrete terms, his book offers no balanced review of competing theories and becomes liable to accusations of selective presentation of evidence. A critic might argue that other forces besides predation might have taken a hand, like natural disasters, or that social and psychological factors deserve more credit than he gives them. Trout points toward many consequences attributed to the predator experience but he rarely follows up

with detailed evidence, and as a result fails to balance breadth with depth in arguing his case.

If not every assumption and deduction is right, the fact remains that *Deadly Powers* takes on one of the big questions and responds with a big answer that is at once lucid, plausible, and well-supported. The reader comes away with thought-provoking insights into the development of human thought and the nature of cultural institutions. Anomalists can benefit from a theory of origins for some monsters of legend and the experiential factor in creating some stories about terrible creatures. Trout is convincing enough that future research into the origin of myths and religion will have to reckon with his assertion that long ages of being low on the food chain made us what we are.

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Yoga and Parapsychology: Empirical Research and Theoretical Studies edited by K. Ramakrishna Rao. Dehli: Motilal Banarsidass Publishers, 2010. 507 pp. $54.75 (hardcover). ISBN 978-8120834736.

Those familiar with the history and the literature of parapsychology will recognize the name of K. Ramakrishna Rao, director of the Rhine Research Center after Rhine's retirement, and a decades-long contributor to the literature of parapsychology. Now head of the Indian Council of Philosophical Research, Rao is a natural choice to produce a book on the topic of yoga and parapsychology. The 19 papers (plus the Preface to the book) come from a 2006 conference at Andhra University. A helpful glossary is also included.

The theme of the connection between yoga and parapsychology is a natural one, as several authors point out, since, on the one hand, Indian philosophy takes paranormal powers (*siddhi*) to be a natural outcome of yoga practice, and on the other hand, meditation and other noise reduction exercises have proved to be psi-conducive in Western empirical research. Yet, there has been too little cross-cultural fertilization, so a conference on the subject and the subsequent book are welcome.

The title of the book, however, is somewhat ambiguous; it can mean one of three things: 1) how yoga and parapsychology intersect, 2) parapsychology as a separate topic, or 3) yoga as a separate topic. Given the East–West emphasis of Rao, as well being the subject matter that contributes more to new ideas, the first topic is the one that I expected and hoped the book would be about. That is not fully the case, though, as fewer than half of the chapters intentionally inter-relate both yoga and parapsychology (with another several topics relatable by implication), and Rao authored three of these chapters. I could not discern a strong organization of the articles—certainly none is made plain—so for this review I propose to impose my own organization and discuss the articles under the three headings above. I will spend the most time on the articles in the first category since they seem to me the more groundbreaking and integrative. The other articles will be interesting to different researchers more specifically dedicated to Eastern thought or to parapsychology research.

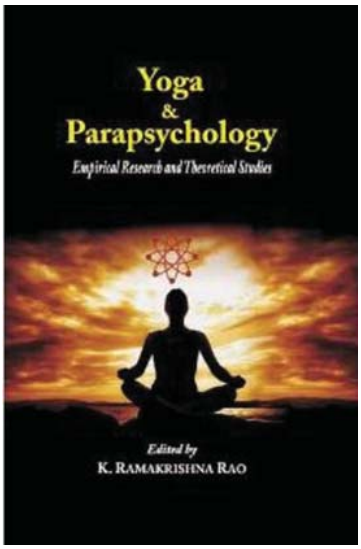
Yoga and Parapsychology

Perhaps it is too much to ask practitioners of yoga and of parapsychology to be well-versed enough in the other subject to combine the views of both East and West. After all, one of the problems the book attempts to

ameliorate is the disconnect between the two worldviews, especially Western knowledge of Eastern philosophy. All three of Rao's contributions (plus his Preface) contribute to the discussion of the interrelation (or lack of it) on the conceptual level. As Rao points out, yoga is a philosophy and a practice, while parapsychology is a Western science, and each has a different set of assumptions and approaches, although parapsychology can be viewed as a science of the *siddhis*. While the East has focused on a science of the inner, the West has focused on a science of the outer, material world. But, Rao wonders whether there is an alternative "to the neurocentric conception of consciousness and the mind that could conceivably bridge the epistemic asymmetry between objective science and subjective experience" (p. xvi). He suggests that yoga might offer a "new paradigm for studying the mind, one that would reconcile the scientific demands as well as spiritual aspirations" (p. xvii). And he further argues that as long as *siddhis* (psi phenomena) are viewed by parapsychology as anomalies (as opposed to being natural events, as they are in yoga), parapsychology will always remain simply a study of anomalies, and it will not gain scientific legitimacy. Rather, parapsychologists must move beyond this traditional Western view and accept that psi is part of a broader psychology, where parapsychology studies exceptional natural phenomena rather than attempting to naturalize the supernatural. Within such a view, Rao points out that an expanded psychology is offered by the *Yoga-sutras* of Patanjali.

Matthijs Cornelissen's article contrasts the Western mainstream view of consciousness (which he identifies with Searle) with the Vedic view presented by Sri Aurobindo. While the Western view discusses consciousness as being possessed by individual persons, the Vedic tradition examines the spiritual experience of consciousness as Absolute, in which both matter and individual consciousness are manifestations of a larger consciousness, which is the essential nature of reality. Aurobindo's view of reality differentiates a hierarchy of subtle worlds, with consciousness and knowledge penetrating every level of the world. Much like Rao, Cornelissen also makes the point that psi events will cease to be anomalous when they are understood in a more comprehensive view of reality. Richard Hartz offers a study of Aurobindo's diaries which detail putative psi experiences (which it seems to me Aurobindo accepts as paranormal in far too generous a fashion), and his explanation that psi events are simply a conscious experience of processes going on all the time.

William Braud presents a masterful article doing exactly what his title suggests, "Patanjali *Yoga-Sutra*s and Parapsychological Research: Exploring Matches and Mismatches." Along with Rao, he seems most at home in dealing with both traditions. After pointing out that studying *siddhi*s could



help parapsychologists "elaborate the nature of some psi manifestations already familiar to psi researchers" (p. 247), as well as direct us to other possible psi events, Braud offers a table connecting the eight areas of yogic practices with areas of psi research. Many of the approaches in somatic quietude, sensory restriction, and cognitive quietude have played a prominent role in Braud's own research and thinking. Braud also concludes that parapsychologists should advance their study beyond a study of more traditional psi manifestations to more spiritual matters.

A third article by Rao examines the question of postmortem survival. After an examination of the empirical evidence, which concludes with the general consensus that empirical evidence is at best suggestive, he asks the question, "What could survive?" Contrasting the Western view of the survival of individual consciousness with the Indian view of underlying consciousness as non-individual and as devoid of content, Rao suggests that the Western view of survival does not make sense, or at least it needs recasting in light of yogic and Buddhist literature. Arseculeratne approaches a similar task, asking how we can explain Stevenson's findings about reincarnation, and he argues that the Buddhist conception of *annata* (absence of a soul) might suffice.

Two other articles fit nicely into this category, but rather than being conceptual they are reports of empirical studies. The first is a reanalysis of data collected on psi success and yogic practice by Jerry Solvin and Serena Roney-Dougal. These post-hoc explorations are meant to be helpful for future research. As expected, swamis scored better than novices on a psi task, but only based on effect size and the fact that less-practiced meditators scored in the psi-negative direction. They list a number of methodological improvements and suggestions that future researchers should employ. The other empirical study, carried out by Rao on Indian students, seeks to see if there can be large-scale group screening to select good subjects for further psi research. In particular, Rao sought to figure out which variables might be appropriate in selecting subjects, using a number of psychological instruments. Rao draws two conclusions: a) the students were led in a relaxation procedure before performing the psi task, and he believes that

this procedure was responsible for the positive scores, and b) it is possible to screen subjects on a mass scale, even on a free-response test.

Parapsychology

Two kinds of article fit into this category. Four articles can be thought of as transition—they deal with empirical psi research, but they have broader implications for consciousness studies or for religion, but they don't specifically tie their research to yoga. The other two articles present purely parapsychological research. Roger Nelson summarizes the results of the Global Consciousness Project, which suggests some sort of universal mind; although Nelson thinks that Bohm's theory of active information may explain the results, one suspects that an Eastern approach might also suggest contending explanations. Two further articles, by Fernandes and Marwaha, and by Hill-Clark, study the relationships among religion, personality, and paranormal experiences, seeking to see the interrelations among them. The first study uses Hindu students as subjects, while the second uses American students from a conservative southeastern U.S. university. Neither paper finds a strong interrelationship among these three topics. J. E. Kennedy examines the capricious nature of psi, such as psi missing and decline (or loss of) effects, and he suggests these cannot be overcome with any statistical analyses, even meta-analyses. Rather, he proposes that psi experiences may be intended to wake us up, as it were, to a greater sense of interconnectedness and meaning in life, and a greater spirituality.

Two chapters that are purely empirical are included. Suitbert Ertel argues that his ball-drawing methodology (drawing ping pong balls out of a bag) done privately by individuals is a sound methodology to investigate individual differences in ESP. He argues that post-cautionary controls are sufficient to meet concerns about fraud, and these studies have supported the psi-star hypothesis, that psi is unevenly distributed among the population. May, Paulinyi, and Vassy argue that Decision Augmentation Theory (where someone chooses non-random sequences within a larger random set to begin the experiment) rather than presentience is the best explanation for success in pre-stimulus tests.

Yoga

Finally, four chapters early in the book focus exclusively on Eastern thought and are only related to parapsychology because they give an alternative view of reality to the Western view, a view focused on consciousness (as opposed to a focus on the individual mind). They tend to offer more of an exegesis of Indian thought than an argument for it. Insofar as many of us

in the West are not sufficiently familiar with these approaches, the chapters offer a window into specific ways of thinking about reality, although the articles can seem didactic.

Sangeetha Menon explicates the *yoga-sutra* of Pantanjali. Discussing *purusa* (pure consciousness) and *prakrit* (matter), five mental planes, five cognitive modes, and five pains, among other things, Menon explains that yoga can settle the afflictions and dysfunctions of mind, and thus yoga leads to systematic health and well-being.

Arjuna DeZoysa examines four cases of alleged reincarnation in Sri Lanka and Thailand and argues that the Vedic and Buddhist conceptions of a continuing consciousness, albeit not individual, could be a better starting point for explaining reincarnation than Western notions of "Abyss of Nothingness" at death.

H. R. Nagendra argues that a consciousness-based approach to understanding the world offers greater possibility for answering questions in science than a matter-based one. Such a view articulates five layers (sheaths) of existence, as well as a level of pure consciousness (*Brahman*). In such a world, the *siddhis* arise naturally and are ultimately meant to promote social harmony.

Contrasting consciousness as the primal state versus the mind, viewed as a modification of consciousness, K. M. Tripathi elucidates the components of cognition in Pantanjali, arguing that yoga aims to lead to a transcendental state of the psyche.

In summary, the book offers a wide variety of articles, many of which attempt to bridge the gap between East and West. Rao argues that both yoga and parapsychology can benefit from such engagement, and the articles give sufficient reason to support this point. Depending on one's initial cultural and research orientation, some articles will be more enlightening than others, but the collection offers food for thought for people in both traditions. The book can be a useful tool for a number of researchers, although the rather substandard binding may require repair if the book is used often.

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Fringe-ology: How I Tried to Explain Away the Unexplainable—And Couldn't by Steve Volk. New York: Harper One, 2011. 312 pp. $25.99 (hardcover). $15.99 (paperback, 2012). ISBN 9780061857713.

It was a summer night, almost 20 years ago, when my father-in-law had his one and only paranormal experience. It still haunts him though. And even though he's told himself countless times that it was a dream, just a dream, he's never quite been able to convince himself of that.

He was sleeping away the dark night hours of June, at his tidy ranch home in California's San Joaquin Valley, when suddenly he startled awake, sitting up in bed so abruptly that he woke his wife. "What?" she said. "What is it?"

"Dan's here," he replied, referring to a cousin who lived a few hundred miles away. "He's calling my name." They both sat there in the country silence. Not a sound, not a call. Just a dream, she said, and they returned to their pillows. But a few minutes later, he was up again. The voice was closer.

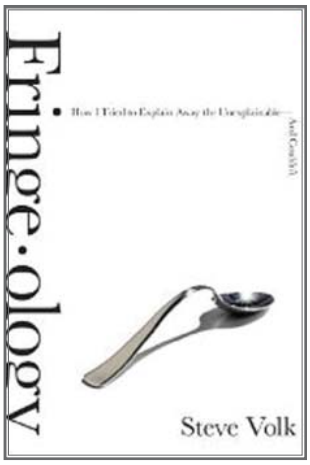
"Dan's outside," he said. "I'll go find him." And he was up, pulling open the sliding doors that opened from bedroom to patio, searching for his cousin, startling some sleepy birds into flight. But only the birds stirred, maybe a few leaves fluttered, nothing else.

He returned to bed, wondering—as we tend to do—if something he'd eaten, something spicy, twisted up his dreams. But he was almost relieved to be up with daylight. And he was still fighting that odd anxiety when his cousin Dan's son called to tell him his father was dead. Dan had shot himself to death during those exact minutes that my father-in-law had heard his cousin calling in the dark.

I'm telling you this story because it leads directly into why I like Steve Volk's book *Fringe-ology* so much. Of course, it helps that it's just a really good book: smart, incisive, funny, readable. But, more than that, it captures what an entirely human experience we're talking about—this exploration of our natural world, this journey in which we try to make sense of the fantastical universe that we inhabit, this patchworked understanding built of both our doubts and our beliefs.

"In the coming pages," Volk says in his Introduction,

> I write about near-death experiences (NDEs), mental telepathy, quantum consciousness, UFOs, a mystic astronaut, ghost hunting, and a pair of scientists doing their level best to study aspects of human experience often derided as paranormal. But this book is about more than any of these things. This book is about us. (p. 4)



And, thus, while I start by telling you a personal story, Volk weaves through the book an inquiry into his own childhood experience of living in a house apparently haunted by thumps and creaks and bumps in the night. In fact, each chapter in the book is framed by an individual's encounter with the paranormal, from dedicated investigators to everyday citizens caught up in inexplicable events.

One of my favorite chapters focuses on the question of UFOs, beginning and ending with a rather shy man from a small Texas town who in 2008 found himself standing under a rather spectacular and still unidentified flying object, a chain-link of lights in the sky. "I think I saw something that I shouldn't have seen," the man near tears tells his local police officer (p. 113).

And from this perspective, Volk opens an exploration of the event itself—seen by a startling large number of residents in the Texas community of Stephenville, their shock and uncertainty in the face of a media frenzy, government mismanagement (at best) of the response, and cultural attitudes toward such Spielberg-like close encounters. The word *attitudes* is mine here; Volk characterizes it more as combat between skeptics and believers.

In general, throughout the book, he portrays believers a little more gently than skeptics. That's not to say that he doesn't provide some excellent examples of psychic scam work. And that's not to say that Volk himself can be called a believer in the classic sense. He maintains a kind of journalistic distance from his subjects, testing his ideas, weighing and balancing them.

But he has more sympathy, I think, for the willingness to consider possibilities than for determined denial of those things that lie outside the scientific rulebook. And he makes an eloquent point of the fact that scientific believers and well-known skeptics are also capable of devious behavior. He elegantly makes this point in the case of the well-known magician-turned-debunker James Randi, providing examples of times when Randi has apparently altered facts to strengthen his points. "It seems to be that any truly rational group would not have James Randi as a member," Volk notes (p. 73).

Fringe-ology is not, though, a judgmental book. Most of his people studies are of those trying to make sense of what they've seen or heard or even done. He looks at near-death encounters through the often troubled story of Elizabeth Kübler-Ross, at the fascinating subject of lucid dreaming

through both the research and the workshops run by researcher Stephen LaBerge, at the path that led Apollo 14 astronaut Edgar Mitchell to found the Institute for Noetic Sciences with its focus on science and spirituality.

Volk neatly connects all these different tales into the fabric of his story, each person, each inquiry adding to his exploration of what's real and what's not and whether we will ever fully define the difference. In the end, he argues against entirely rigid definitions of reality, and for finding a comfort level with uncertainty. And he argues—with real success—that allowing for uncertainty lets us find a better understanding. "The question," Volk says, "is whether or not we're prepared to accept a world in which science and spirituality really do serve each other" (p. 264).

Which brings me back to my father-in-law's story of the voice in the night. People hearing that tale have assured him that it was, indeed, his cousin saying goodbye. And other people hearing that tale have assured him that it was just a coincidental dream. When I've repeated the story to my more skeptical friends, they've pointed out that we probably have many dreams involving friends and family that we just don't remember, that the circumstances of this one vaulted it into unforgettable territory.

But I find myself shaking my head when they tell me that. My father-in-law is such a grounded personality, such a natural skeptic. Yet this, he says, was like no dream he ever had. We don't have the scientific method at this point to prove it was a dream—or to prove it wasn't. And until we learn how to do that, I find myself on Steve Volk's side, willing to allow for possibility. If nothing else, it makes our uncertain universe a more interesting place.

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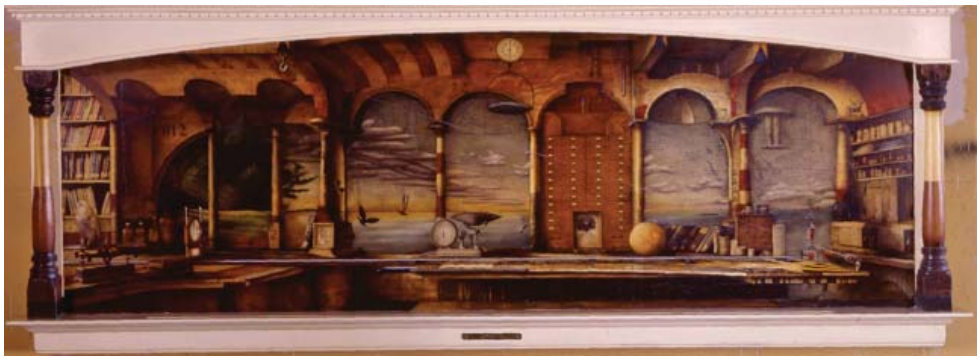
The Psychology of Science and the Origins of the Scientific Mind by Gregory J. Feist. New Haven: Yale University Press, 2008. 336 pp. $25 (paperback). ISBN 9760300143270.

On the second floor of the National Academy of Sciences headquarters in Washington, D.C., there is a most impressive painting by Robert Van Vranken, *Untitled (Where Do Thoughts Come From, Where Do They Go?*). In my mind this amazing picture, a huge panorama of a scientific laboratory, encapsulates everything the psychology of science is about. The book under review could well be seen doing the same task.



It is very difficult to disagree with the ambitious aim that Gregory Feist has set for this book, and that is to provide a foundation and a *Tour d'Horizon* for the psychology of science. One might certainly think it is just that, since this book received the William James Prize of the General Psychology Division of the American Psychological Association in 2007. Yet it is an uneven book, which in spite of its considerable contributions needs substantial improvements.

About the title. Obviously, there is not one "scientific mind" any more than there is one "scientific method." Scientific brains and scientific thinking take many forms. One thinks, for instance, of Ian Mitroff's book *The Subjective Side of Science*, whose examination of thought and research of the Apollo moon scientists shows very different patterns for the theorists and empiricists in the sample. Yet though Mitroff is not mentioned in this book, I am sure that Feist would see this sort of diversity as being basic to understanding how science works. "The scientific mind" is thus a metaphor rather than an assertion of fact.

Having set an ambitious aim for himself, can Feist be said to have accomplished it in this book? I believe the fair answer is, "not yet." But, as J. Alfred Prufrock says in Eliot's poem, "Let us go and make our visit."

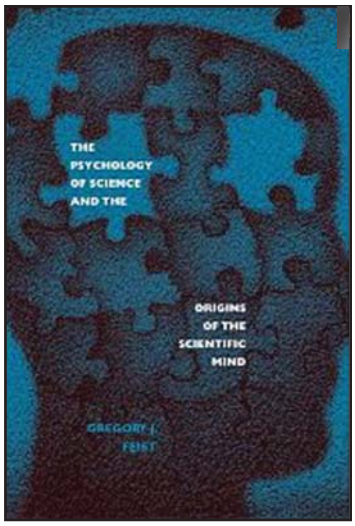
Our first visit is to Chapter One, in which Feist looks at the "meta-disciplines" associated with studying science: The Philosophy of Science, The History of Science, the Sociology of Science, and the Psychology of Science. Having once been a practitioner of the sociology of science, I will put the heaviest emphasis on it. To begin with, one would have thought that his timeline for sociology of science would include Alphonse de Candolle's *Histoire des Sciences et des Savants depuis Deux Siecles* (Geneva, 1873), a truly sociological work in spite of its title. But this is a minor quibble.

More serious is his perception that the heavy hitters in the sociology of science were Robert Merton's students Jonathan and Stephen Cole and Harriet Zuckerman. I believe that one could argue, instead, that along with Bernard Barber, the Mertonians were in fact a prologue to the serious sociology of science. That sociology was centered in a journal called *Social Studies of Science*, and strongly influenced by the Center for Science Studies of the University of Edinburgh. Eventually many scholars founded The Society for Social Studies of Science (4-S) (which later included "and Technology"—SSSST), which of course was not restricted to sociology, but included people from the other meta-disciplines as well. Whereas the Mertonians studied things like promotions and reward patterns, the next generation of sociologists of science spent much more time on the actual processes by which science is produced. Among the many insights this later work produced, and in no particular order, we find the following:

1. The scientific paper is often partly fictional (Knorr)
2. Laboratories engage in the social construction of scientific facts (Latour and Woolgar)
3. Science is heavily political (Latour, Nelkin, Boffey)
4. The Mertonian norms are often honored "in the breach" (Mulkay)
5. Division between science and non-science is arbitrary (Collins, Pinch, Barnes)
6. The peer review process is often biased and flawed (?)

Now both Social Studies of Science and the 4-S (or SSSST) appear on Feist's timeline, but he does not appear to appreciate their importance, or make many citations to them. In addition there was also Science, Technology and Human Values, and Science Studies. What was particularly important about the 4-S is that it put the various meta-disciplines together.

With regard to the psychology of science, he neglects the role of the Subgroup on Social Psychology of Science, whose newsletter *Social Psychology of Science* I edited (later with help from Robert Rosenwein) for at least twelve years. This was a Subgroup of the 4-S. The newsletter did not start "in the 1990s" (p. 155) but in 1982, more or less in synchrony with



the Subgroup itself, and did not last "a few years," but until 1994. One can scan the newsletters themselves to see the large number of psychologists, sociologists, historians, etc. (There were dozens of subscribers early on, and 125 subscribers in 1994.) Panels at 4-S meetings on psychology of science were often to draw from those on the subscription list, as did conferences. The newsletter included book reviews and the addresses of subscribers. It helped to showcase the work of such eminent science historians as Stephen Brush, whose work on theory acceptance has proved so important (it is not mentioned in Feist's book at all, even in the section on Planck's Principle). This newsletter was ephemeral, indeed, but the fan mail I got showed how many people appreciated it.

A final remark on this chapter. With regard to the history of science, multi-volume histories of astronomy (e.g., by Bailly and Delambre) were in full bloom by the 18th century, and I suspect earlier as well.

Chapters Two and Three deal with neurological and developmental issues. Here I believe Feist does better. His discussion of Sulloway's findings on birth order and on Planck's Principle (ageing and openness) are good. His introduction of the findings (his own work) from various Westinghouse Science Competitions are particularly interesting. At the same time, however, I feel he spends too much space on "scientific" abilities in children, and too little on the peculiarities of adult scientists.

For instance, there is the curious matter of high concentration. Quite a few inventors and scientists have shown an uncanny ability to shut out the world around them as they concentrate on their projects. Inventor Bill McLean was quite capable of sitting on a couch, concentrating on some weapon system, being called to dinner, and eating quietly, then returning to the couch, and suddenly jumping up and saying, "OK, I've got it, when do we eat?" This concentration is often referred to as, from its side effect, absent-mindedness, but as William James said, absent-minded people are often "present-minded somewhere else."

In his discussion of Keith Simonton's findings about creativity and age, I believe that Feist underplays another feature of Simonton's data, which is that the "early peaking" of creativity is most marked in the most theoretical sciences, and least marked in those that heavily depend on facts and skills. In

particular, inventors tend to peak later, and often keep inventing into periods often thought to be affected by senility. Jack Rabinow, for instance, came up with the world's first pickproof lock when he was about 80. It is also interesting to note that Rabinow, who came up with about 2,000 inventions (and patented 230 of them), kept a timeline on his lifetime creativity, noting the number of ideas per year. He found that the highest level was reached when he was running his own company, and could rapidly turn his ideas into prototypes because of his skilled technicians. Rabinow's chart in his *Inventing for Fun and Profit*, might find a place in Feist's book, since it is a far more differentiated and sensitive indicator than the simple division of "young" and "old" that Feist employs.

Chapter Four, on cognitive approaches to the psychology of science, is the most satisfactory chapter in the book. Feist does a good job of reviewing experiments, historical case studies (though Darwin is covered in the most detail), and materials on modeling, clearly an extremely important aspect of how scientists think. It is here that one finally gets to mechanical intuition or ingenuity, so important in physics, chemistry, and zoology. He has read widely, and he takes pains to consider most of the issues. He leaves out, however, Morris Stein's work on animistic metaphors, which I have found particularly useful. I have certainly observed that inventors and scientists alike tend to make what they study "come to life" by endowing atoms, molecules, etc., with feelings and even thoughts. For instance, I was sitting one day with Paul MacCready, aeronautical inventor, and he described the Bernoulli effect to me as if the molecules of air were actually alive. Similar comments were made by inventors Jacob Rabinow and Raymond Damadian. The latter insisted that "all scientists" use such metaphors to think. Bill McLean, inventor, was described by a colleague as "grunting and sweating and trying to get the third derivative" as he felt his way into the manner in which the Sidewinder missile responded to the airflow. Karen Knorr has also discussed the physicality of scientific thinking. In discussing Sara Mednick and remote associations, Feist fails to note that Mednick's Remote Associates Test, for instance, correlates with patent disclosures. The RAT test figures in Gerald Gordon's study showed research groups with Hi-RAT members, paired with high differentiating leaders, to be the most productive. It also figures in the classic study by Pelz and Andrews, *Scientists in Organizations*, in assessing the intellectual quality of members of R&D groups.

In Chapter Five, the author tackles the relationship of personality to science achievement. There are some very striking findings here, both about the personalities of those who choose science and those who are good at it. Some of the more valuable findings in the chapter come again from Feist's own research. His meta-analysis of scientists' personalities

shows, for instance, that scientists tend to be more dominant, arrogant, hostile, self-confident, autonomous, and introverted. This is even true of women scientists. Yet the personalities of high achievers are different from low achievers. For instance, while conscientiousness is strong for the average scientist, it is much less strong for the highly creative scientist. I was surprised, however, that when Feist discusses the Myers-Briggs test, he doesn't point out the most typical personality profile for productive scientists: INTJ, introverted intuitive thinkers with judgment (mentioned by Mary MacCauly, a professional tester). I remember giving the MBTI to a group of radar engineers and scientists. There was only one member of the group who wasn't essentially INTJ!

Chapter Six on the social psychology of science is relatively good with regard to "how future scientists are influenced to go into science." Gender issues are handled in some detail. One would like to see a little bit more on the dynamics of science graduate schools. Furthermore, it is surprising that he does not mention the work of Joseph Ben-David and similar scholars on the growth of disciplines. As far as the dynamics of scientists in groups are concerned, one really needs to go somewhere else. His statement that Industrial/Occupational social psychology (in the psychology of science) is undeveloped is a joke. About half of the "scientists" in the United States, I believe, are in the private sector. Feist pays little attention to them, but the literature is enormous. My library has several books on scientists in private organizations from the *1950s,* that is, before Pelz and Andrews wrote their classic *Scientists in Organizations*. As for the dynamics of R&D groups, I would guess that there are at least a half-dozen journals that focus on this, to say nothing of individual case studies, in book form or otherwise. The literature on groups in creativity (including scientists) is also extensive.

Chapter Seven is about the psychology of science and what might be done with it. He gives a very knowledgeable discussion about various forms of aptitude testing, and competitions such as the Westinghouse one. He does mention Howard Gardner's "Multiple Intelligences" without noting that C. W. Taylor had earlier proposed the same thing (with a cute chart to boot). This then leads into a discussion on the future of the psychology of science, which I found unconvincing. The fact that Feist and several other scholars have created a journal and an association to study psychology of science, however, is very germane to this question. Feist has been a leader in pushing the psychology of science recently, and its future success may well depend on his ability to bear this mantle.

Chapters Eight and Nine are about "the evolution of the human mind" and "the origins of the scientific thinking." I am not expert in either of these areas, but was unimpressed with both chapters, especially the latter. I believe

that as a popular treatment of the issues in question, it is not bad. Yet for the more discerning mind, it is not satisfactory. His knowledge of the specialist literature seems modest, at best. He cites Will Durant rather than Lynn Thorndike, for instance. I have no reason to doubt his anthropological sources (in Chapter 8), but if one is looking for history of science (Chapter 9), or even the prehistory of science, these chapters may not be the place to start. I looked in vain for any number of standard sources that were simply not there (e.g., Marrou, Partington). In earlier chapters he quotes Howard Gruber on Darwin, but does not seem to have read Janet Browne's definitive biography.

Chapter Ten is on pseudoscience, anti-science, and postmodernism. Although I am inclined to agree with his negative judgments on postmodernism, the part on pseudoscience is simply incompetent. Imagine writing a chapter on this subject without knowing about the SSE, about *Zetetic Scholar*, about the Mars Effect debate (and the Starbaby scandal), etc. Feist has read the skeptical literature and apparently nothing else. He has done no original research in this area, and is obviously unfamiliar with the literatures of UFOlogy, paranormal research, near-death experiences, and so on. I found particularly disturbing his statement that along with flat-earth theory and alchemy, ESP and UFOlogy are not more fundamentally advanced than they were thirty or forty years ago. His sweeping statement is not correct. He has simply not done his homework. This might be acceptable if he were a bench scientist who wanted to blow off steam on this subject. It is not acceptable from a social scientist who should be better informed.

Bottom line. So what has Feist accomplished with this book? I believe that he has brought together a large amount of literature and arranged it in a logical way. Given his knowledge of a good deal of the psychological literature, he has performed a service in bringing it together. As one gets into the details, however, there are many respects in which this book does not measure up, and especially in the ambitious goal of founding a psychology of science. He has not read everything he needs to have read. Furthermore he does not seem overly familiar with actual scientists. I don't recall any materials from interviews he did, or any anecdotes from biographical studies which he himself carried out. I grew up with a father who was an experimental scientist, and even as an outsider to his world learned a substantial amount of lore about how scientists operate. It is not clear to me how familiar Professor Feist is with such lore.

Many of the problems might be corrected with a second edition. But between the first and second editions, there is a lot of work to be done.

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The Myth and Mystery of UFOs by Thomas E. Bullard. Lawrence, KS: University of Kansas Press, 2010. 417 pp. ISBN 9780700617296.

UFO books run the gambit from slapped-together pulp jobs with little thought to accuracy to Ph.D. dissertations that rely on scholarship without a thought to style. Thomas (Eddie) Bullard's book, *The Myth and Mysteries of UFOs*, is one that walks the fine line between over-the-top scholarship and bottom-of-the-barrel trash. His is a book that belongs on everyone's shelf because of the scholarship and the readability.

It was, for me, confusing at first. I wasn't sure where he was going with his scholarship. He wrote about sightings that most of us inside the UFO community knew about, but he often answered the questions about their reality. Or maybe I should say about their extraterrestrial nature. Clearly something had happened, but Bullard seemed to provide us with answers for those strange cases.

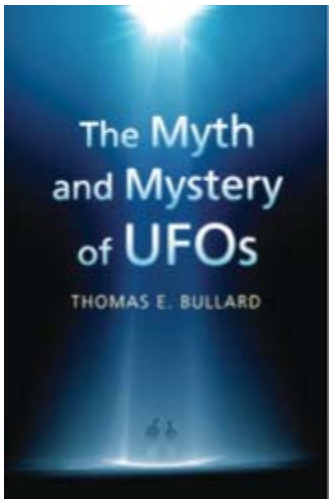
The massive sightings of March 3, 1968, in which a number of witnesses described a cigar-shaped UFO with windows on the side, for example, was explained as the re-entry and breakup of the Zond IV spacecraft launched by the Soviet Union. This has become an accepted explanation throughout the UFO community and one not without merit.

The case is interesting because, as Bullard notes, while some thought of alien craft, there were those who recognized the sighting for what it actually was. Bullard suggests that the reason there weren't more reports of this with the proper answer is because those who properly identified it felt no compulsion to report it. Those who thought of it in terms of an alien craft did.

But the real importance of the sighting was how it applied to other, similar reports. In 1948 two airline pilots saw something that they described as cigar-shaped with square windows. This was, of course, the same thing said about the Zond IV reentry. A cigar-shaped craft with square windows. Of course there was no returning space debris in 1948, but there were bolides, very bright meteors, that could give the same impression and often do.

Bullard looks at the UFO phenomenon through the eyes of a folklorist who is studying the legends and myths of the human race. He notes that humans, from the very first, were reporting the strange apparitions in the sky that we now call UFOs or flying saucers. He looks at the history of those myths.

But he is not telling us that all UFO sightings can be explained with such a study, only that science might learn something about human nature, about how we view the world as opposed to how our ancestors viewed the world,



and that there is something real happening. Some of the sightings aren't based only on our perceptions, but on something concrete and tangible flying, or floating, through the sky.

He acknowledges many of the answers for what people have seen, but also makes it clear that these answers do not cover everything that is seen. It is impossible to write off a UFO sighting that was witnessed by dozens, especially when the object, or objects, are detected by radar or have been photographed or leave traces on the ground.

In other words, Bullard sees something of value in the study of UFOs. There is science that can be applied, and science has been negligent in what it has done with UFO reports. Rather than be intrigued by them, science simply ignored them.

This is a book that has been needed since 1969 when the Air Force–sponsored study at the University of Colorado, popularly known as the Condon Report, rejected the idea of UFOs. The Condon Committee found that not only did UFOs not pose a threat to national security, one of the Air Force's requirements for the inquiry, but, more outrageously, nothing of scientific importance could be learned by studying them. Skeptics have cited this investigation as if it is the final word on UFOs since it was released.

Bullard's book, however, is the important and long-needed counterpoint. He's not arguing that UFOs are extraterrestrial, though it appears in some places he has reached this conclusion. No, he's arguing that some UFOs demand scientific study. They might not lead to alien spacecraft but they will certainly add to our knowledge of the world around us. While national security might not be an issue, scientific understanding of our world is.

While alien abduction might not be extraterrestrial creatures taking humans into their craft for examination, neither is it explained by sleep paralysis. While sleep paralysis may, in fact, explain some abduction tales, it does not explain them all.

Bullard's argument here, then, is that UFOs deserve academic study. Hufford's study (1989) of the Old Hag, as outlined in *The Terror That Comes in the Night*, which is about bedroom visitation, led us to a more complete understanding of the phenomenon related to sleep paralysis. The study of the UFOs might lead us to a better understanding of our psychological makeup.

With that said, Bullard is also suggesting that there are UFO sightings that are sufficiently strange, sufficiently documented, with sufficient eyewitness testimony, to demand study. This might lead us right into the extraterrestrial.

Bullard is suggesting that we stop dismissing UFOs by saying the witnesses were drunk, uneducated, unsophisticated, or simply of below-average intelligence, and apply our science to them. He looked at the UFO phenomenon with the eyes of a trained folklorist and found much that required study. He is saying that other sciences, both physical and social, might benefit from a similar academic analysis.

Ridicule is not a way to learn something new. Ridicule is a way to dismiss something without having to know anything about it. Bullard tells us that now is the time to stop ignoring UFOs and actually try studying them with the same sort of academic precision that is devoted to other types of anomalies. Now is the time to begin the real science and not the pseudo-science that has gone before.

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Reference

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UFOs: Myths, Conspiracies, and Realities by John B. Alexander. New York: Thomas Dunne Books, St. Martin's Press, 2011. 305 pp. $25.99 (hardcover). ISBN 9780312648343.

It is said that the truest test of a man's intelligence is how much he agrees with you, and I find that Dr. Alexander and I share a great number of opinions. I looked first at the chapter about Philip Corso, who claimed an inside knowledge of the Roswell UFO crash and the government plans to exploit the find by seeding recovered material into American industry. Here Alexander writes not only from his experience in the Pentagon and classified operations, but as a friend of Corso. He spoke with him in the weeks prior to Corso's death. But Alexander found many holes in the stories spun by Corso, and in the end, while acknowledging Corso's long military career, did not truly believe him. Here Alexander and I agree.

What was more fascinating was Alexander's discussion of Congressional hearings about UFOs, and what disclosure would accomplish. Writing as an insider who has experience in this arena, Alexander suggested that neither hearings nor disclosure was going to happen for reasons he carefully lays out.

One of those reasons was what almost any of us have observed ourselves. UFOs are a third rail in politics (though Alexander suggests they are tar-like), meaning that almost any expression of belief is the same as admitting to a belief in the Easter Bunny. He provided examples of what happened after UFOs were mentioned in a debate with former presidential candidate Dennis Kucinich. From that point on, while commenting on Kucinich's political ideas and theories, pundits found they had to remark on UFOs, always in a derogatory way. The UFO connection might have nothing to do with Kucinich's political statements, but it was brought in anyway, as a means to discredit him.

On disclosure, the idea that the U.S. government has many classified UFO documents to release, Alexander noted that there was nothing to actually disclose (an idea reinforced by a recent White House announcement that the government held no classified UFO files). The official investigation of UFOs by the Air Force had been released decades ago, and a great number of the files and records from the now closed Project Blue Book are online, available to everyone.

Alexander scoffs at the idea of MJ-12, that is, the super secret committee supposedly created by President Truman after the Roswell UFO crash. Unlike so many others who suggest the documents are faked based on analysis of the documents themselves, Alexander's attack is about the way they entered into the public consciousness. Using Watergate as an example, he notes that the Watergate investigation was built on solid

evidence from sources known to the reporters, while MJ-12 is built on anonymous documents sent to an obscure movie producer. In leaks of real documents, those documents can be examined, the sources verified, and the information corroborated. With MJ-12, there are no original documents, there are no sources, and the information seems to be a hodge-podge of real data taken from historical sources rewritten to include references to MJ-12. Here again, Alexander and I agree.

And we certainly agree on his analysis of the Air Force–sponsored University of Colorado study of UFOs known to many as the Condon Report. He chides science for its refusal to look critically at the results of the study, which he describes as badly flawed. He notes that scientists continue to use the study to prove that nothing of scientific interest could be learned from a true examination of UFOs, when the contrary is true. He suggests that many of the case studies cited by the Condon Committee were cursory at best and certainly inadequate for a true scientific analysis. Although he doesn't mention it, one of the cases in the Condon Committee report was concluded suggesting that it was caused by a phenomenon so rare that it had never been seen before or since. They don't bother to identify that phenomenon. Alexander suggests that scientists actually read the report before relying on it to prove there is nothing of value in UFO research.

Where we part company is in his analysis about the Roswell UFO crash case. He writes that he now subscribes to the Project Mogul answer. According to him, "While the Air Force report, *Case Closed*, provides conflicting information regarding classification, most of those involved agree it [Project Mogul] was both Top Secret and strictly compartmented." While the ultimate purpose, to spy on the Soviet Union, was classified, the balloon launches and the equipment in New Mexico were not. For the launches in June, 1947, the balloons were standard neoprene weather balloons and the radar targets were foil-covered devices known as rawins. The name of the project, contrary to what has been said many times by many other sources, was not classified and appeared in Dr. Albert Crary's unclassified diary published in the Air Force study. Announcements of the launches were required by the CAA (forerunner to the FAA) because the balloon arrays could be a hazard to aerial navigation. Pictures of the balloon arrays were published in newspapers around the country on July 10, 1947. So much for a highly classified project.

What struck me most about this short segment of the book was how he let the sources get away from him. In other places, he carefully named sources and their credentials. As an example, when writing about an intercept of a UFO by an American pilot stationed in England, he told us it was Lieutenant Milton Torres, who eventually earned a doctorate in

mechanical engineering, that Torres taught at the university level, and he was a very credible source who had been sworn to secrecy about his UFO encounter. We learn all that we need so that we might verify what Alexander has written if we feel the need to do so.

With Project Mogul, we are not so blessed. In writing about the strange symbols reported by Jesse Marcel, Jr. (whose credentials are also carefully laid out for us), Alexander said,

> What was learned was that on the reflecting panels had been placed a specially designed code that could only be read by the people with access to the key. More important, it was stated that this code was not alphanumeric as are most that are frequently employed, but entailed the use of glyphs.

In all my discussions with project engineers and others associated with Mogul, including Charles Moore who claimed he had "launched the Roswell UFO," this was never mentioned. The best the Air Force could do was suggest that a flowered tape from a novelty company had been used to reinforce part of the rawin targets, but they produced nothing to prove it. If I wanted to verify Alexander's new claim, I could not. Alexander did not provide the source for this unique bit of information.

For me, this discussion of Roswell was the big disappointment here. While Alexander chastised others for accepting much of the nonsense published in the UFO field including those scientists who make statements without bothering to learn the facts, this seems to be what Alexander has done in the Roswell case. He accepted the story of glyphs without proper analysis.

That said, this is a book that needs to be read and understood by all those inside the UFO community and by everyone who has an interest in these topics. Yes, he is going to annoy everyone regardless of their personal beliefs with his opinions about UFOs. His insider status and his knowledge of how things work in both the world of congressional hearings and in the world of military classification provide an interesting insight that those pushing for congressional hearings and full disclosure should read.

For the most part, his use of names, dates, sources, and personal experiences lend an even stronger note of credibility to his work. While he doesn't use footnotes, he provides the source material in the text. It is clear that he knows what he is talking about, and that he has, for the most part, the sources and data to back it up.

Here is a book about UFOs that is a must read for everyone. And if we disagree about the Roswell case, well then, we disagree about Roswell.

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Flying Saucers over The White House: The Inside Story of Captain Edward J. Ruppelt and His Official U.S. Airforce Investigation of UFOs by Colin Bennett. 172 pp. $16.99 (paperback). New York: Cosimo, 2010. ISBN 9781616404543.

Defying the pundits of the past that the flying saucer craze would fade into history as a popular culture anomaly, the issue is still with us more than sixty years later. Indeed in terms of sighting reports it has quieted down a good deal from the almost hysterical headlines of the 1940s through the 1960s when every odd light in the sky was reported and published, no matter how lacking in detail. From the 1970s onward we saw the rise of strangeness in the reports. Stories became more detailed in describing close encounters, contacts, abductions, physical examinations of humans by aliens, and even hybrid breeding of man with extraterrestrials. This increase in reports of the intrusion of flying saucers into the lives of people so overtly certainly catches one's attention, but with the downside of being less believable with, at least to date, the lack of any physical evidence to support the remarkable claims. We have settled into a period of the saucers becoming an amusing sidebar in life, with the daily news ignoring most reports of old-fashioned distant sightings in favor of features relegated to the "Lifestyles" sections of whatever medium is reporting. Flying saucers are not as alarming as they once were, yet it is undeniable that the phenomenon is with us forever in the collective consciousness.

Enough time has passed for the topic to be regarded as history. Most of those who were there from the beginning of the modern UFO era in 1947 are no longer available to answer questions about those times. We have to rely upon retellings and reinvestigation to attempt to clarify those odd tales. Sometimes we learn new things. Sometimes old information is found not to be as unusual as was once thought. And sometimes we need to be reminded of what was so disturbing to those who preceded us.

Books on UFOs have always ranged widely in credibility. It was especially difficult in the 1950s for witnesses and investigators trying to define a new genre of strangeness. We know such reports have existed throughout history, but for reasons still debated it came together as a distinct topic of research in 1947. As with any new phenomenon, its beginnings were laced with misinterpretation, exaggeration, and outright hoaxing. The books of the time preserved those problems along with facts, and often the two could not be kept separate.

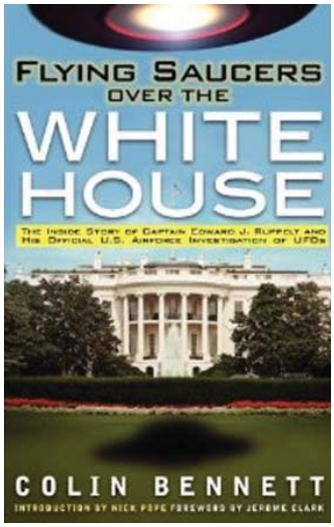
One book stood above the saucer fray. Edward Ruppelt was the head of the Air Force's Project Blue Book, their investigation of flying saucers, between 1951 and 1953. The Air Force was certainly not a rubber stamp for furthering

saucer hysteria. It was, well, the government. And the government had said up to the time of Ruppelt's term that saucers were not a concern. After leaving the service, Ruppelt decided to write a book about his time in Blue Book. The 1956 release was titled *The Report on Unidentified Flying Objects*, and he suggested that some of the reports he investigated were truly puzzling. When someone like Edward Ruppelt said that flying saucer reports were mysterious, it was eye-opening. The Air Force in fact refused to endorse the book for the reason that it contradicted official policy that no report was evidence of anything unusual. A new release (Cosimo, 2011) of Ruppelt's original book *The Report on Unidentified Flying Objects,* with an Introduction by Bennett, followed the release of *Flying Saucers over the White House*.

Author Colin Bennett has decided to re-examine Ruppelt's time in dealing with flying saucers, 50 years after Ruppelt's untimely death from a heart attack in 1960. For those conspiracy theorists out there: No, there is no evidence he was bumped off by the Air Force. Bennett walks us through Ruppelt's writing, commenting on the sightings and events that developed during those Blue Book years. This is not the first detailed look at Ruppelt, preceded by *Captain Edward J. Ruppelt: Summer of the Saucers—1952* by Wendy Connors and Michael Hall (Rose Press International, 2000). With the relative scarcity of the Connors/Hall work now, Bennett's book will be more available to general readers.

While Ruppelt's original book is not a difficult read, it is useful to have a modern reminder and update of that fascinating history of Air Force UFO investigations which were both detailed and inept, sometimes both at the same time. The question for this work is how useful and in what direction the overview goes in the author's commentary. When he sticks to straight discussion of the events of Ruppelt's experiences in his time leading Blue Book, Bennett is well-grounded. But scattered through the text is the temptation in his analysis to link those past incidents to what Bennett calls "The New Ufology." He describes a UFO as a "liminal form" that is presently "half-in and half-out of directly received experience," sort of a nether world of existence.

This is where the discussion gets sticky because it presumes that the events related by Ruppelt are all literally true as described, without error in reporting and without any additional detail to be discovered to support such a concept. Even a moderately experienced investigator/researcher into UFOs knows that a seemingly solid UFO report of high strangeness can blow apart in an instant with refined analysis and newly unearthed detail added to the mix. Many examples of this abound as old reports are reinvestigated with more energy than originally applied. And Ruppelt's inquiries into UFO reports weren't always that diligent. I recall his retelling of a visit to a group



of General Mills' balloonists doing upper atmosphere research in Minnesota. They reported having seen strange flying objects repeatedly during a variety of flights. Ruppelt made a point to visit the group personally to ferret out details, but he only managed to alienate the men. As he listened to their stories, he would occasionally offer alternative explanations for the sightings. The balloonists saw this as a dismissal of what they thought were inexplicable events, and, as Ruppelt related in his book (p. 120), he felt like he was going to be tossed into a snowbank outside. Ruppelt's meeting ended with his leaving a stack of UFO sighting report forms for the balloonists to fill out in the event of future sightings. It was highly unlikely that such future reports would have been forwarded by a group of trained observers who saw Ruppelt as less-than-overwhelmed by their sightings. It wasn't one of his finer moments.

On page 42 Bennett discusses the matter of the 1948 Air Force "Estimate of the Situation," a Top Secret document drafted for approval with the conclusion that flying saucers were interplanetary. He says that the Estimate was destroyed in 1948, but also mentions that Ruppelt saw it. If Ruppelt saw it, it couldn't have been destroyed. In fact, many years later Dewey Fournet, the Air Force's Pentagon liaison with Project Blue Book, said in a letter to a UFO researcher that he had a copy of the Estimate in the ATIC (Air Technical Intelligence Center) branch files as part of the current intelligence records that he oversaw. Ruppelt likely saw the document during his dealings with Fournet in 1952, five years after the alleged destruction of the Estimate. The document stayed in the files when Fournet left. There is every chance it still exists, buried in approximately 600 shelf-feet of current intelligence records at the National Archives in Washington, D.C. And it is still classified.

As one reads along further in this book, there continues to be a tendency of escalating exaggeration in describing Ruppelt's experiences in the context of Bennett's New Ufology. He discusses Ruppelt's visits to various military and scientific groups to give briefings on Project Blue Book. The existence of the briefings caused Bennett to feel that "we begin to take the ideas of Stanton Friedman, Kevin Randle, and the Woods about MJ-12 more seriously" (p. 84). Friedman and Randle have been advocates of the Roswell incident as the crash of an alien vehicle, and Friedman and the Woods have

promoted the reality of a secret government investigation of crashed saucers, Roswell included, called "Majestic 12." Bennett seems to minimalize the fact that there is a vast and incriminating body of evidence that "MJ-12" is little more than an out-of-control hoax, using crudely manufactured "official" documents as proof of the claims of its advocates. Ruppelt's briefings in no way, shape, or form support a notion that MJ-12 should be taken seriously. But Bennett's view presses the idea that a pseudo-agency, created long after the events, lurks behind the Roswell incident, which makes an inevitable appearance here even though Ruppelt never addressed the matter at all as part of his history. The Woods' mid-1990s version of MJ-12 is considerably less credible than the original MJ-12 story created in the 1980s, and this doesn't say much about the original story either.

On page 56, referring to Air Force officers' actions in dealing with the saucer phenomenon as those of a creaking organization, Bennett observes that because these "amateurs" couldn't have handled the matter of a crashed flying saucer and coverup like Roswell, there must have been "quite a different (possibly hybrid) outfit altogether" responsible for the recovery operation. New Ufology rears its head again. In other words, none of the identifiable military people in Ruppelt's history was capable of pulling off our modern version of a crashed saucer coverup. This was despite the fact that those mentioned by Ruppelt were undeniably part of real military UFO investigative history. By Bennett's thinking, a new cast of previously unknown characters instead were all involved in a mysterious MJ-12–like pseudo-history that New Ufology has caused to be manufactured.

Bennett's enthusiasm for UFO reality frequently gets the best of him in his discussions of Ruppelt's writings. On page 94 he begins with a report on an unnamed Air Force fighter base where a UFO dodged fighter pursuit in 1952. A report on the sighting was written by the base intelligence officer. From this point on Ruppelt was given an incredible runaround, being called into the fighter base to hear of the UFO report, described by the intelligence officer as "the most fascinating UFO report I have ever seen." But hear of it was all he could do. The officer told Ruppelt he couldn't have a copy of the report because it was going to be destroyed by order of the base commander. There was within the Air Force during this time different views on UFOs, much as there always was with the public. Some believed UFOs should be disregarded and some believed them to be worthy of serious investigation. It is not hard to figure out what is happening here. The base commander, a disbeliever, wants the incident swept away. Perhaps reporting flying saucers reflects badly on his personnel as crazy, drinkers, etc., so the event is of no value. But the intelligence officer, thinking saucer reports were significant, informs Project Blue Book of the details before he is forced to destroy them

per orders of his skeptical commander. In hindsight, Ruppelt might have written down the details from the report before handing it back, but in the 1950s orders were orders. Were flying saucer reports worth administrative problems, even though they would seem to have national security implications unrecognized by the higher brass? Apparently not! On page 101, Bennett deals with this, suggesting that the reader should "think right out of the box." He cites the possibility that "for a short time something very strange happened to this group of highly trained and reasonably intelligent men." Somehow the men, including Ruppelt, appeared to go "mentally limp" as if under temporary hypnosis. He adds that the men were no longer part of the Air Force machinery, not realizing they had been "attacked."

I looked for a deeper metaphor in this as Bennett couldn't have meant that a reasonable explanation for the fighter base coverup was that it was orchestrated by the UFO. But he doubled down. "For a short time, the UFO was running the base, the men and the technology," he continued. What he means is that the actions of the characters are part of the New Ufology concept of the Escher-Penrose state, a behavioral form of the UFO as a liminal object that is only partly of the real world. It is a part of the UFO experience, according to Bennett, to encounter such peculiar behavior coupled to UFO sightings. I wonder though if this is a better explanation than Air Force officers sometimes being obstinate, careless, and mindlessly obedient.

So the trend of the book is that it is a commentary on the pluses and minuses of government handling of UFOs from an almost mystical viewpoint. On page 48 Bennett suggests that UFOs (and Bigfoot too) "belong to our world only as partial and somewhat intermediate constructions." I read this as meaning that you won't be able to get to the bottom of UFOs and Bigfoot, without adopting and adapting to New Ufology. The author clarifies this by saying the "interdimensional hypothesis is a better explanation for UFOs than the extraterrestrial one" (p. 49). This is a common approach for modern UFO advocates in that the phenomenon is in a realm beyond our ability to understand without much more knowledge of the frontiers of physics. The same argument was offered during the 1950s for flying saucers, but in a slightly different form. Saucers, they said, were from other planets. Mars and Venus were the usual culprits cited as the home bases. Those places were at the time beyond our ability to fully understand without having the means to go there and find out what they were like. The Martian canals were still believed by many to have been artificial constructions. The clouds of Venus hid a temperate environment with a functioning civilization below. In more recent times the "Face on Mars" serves as another example, said by advocates to be a sculpture by Martians.

When we finally acquired the technology to go into space and pulled the

curtains back on what these mysterious places were like, such exotic origins for flying saucers evaporated. The Martian canals never existed and the cloud-shrouded civilized surface of Venus was instead a hellish wasteland of volcanoes and mind-numbing temperature and atmospheric pressure. The Martian Face, with clearer photographs, became an eroded hill that only resembled a face under poor imaging conditions. It was a disappointment to the advocates of these ideas but they pressed on and pushed theories out of reach of current knowledge once again. New Ufology kicks the latest UFO can down the road.

The July 1952 saucer wave was perhaps the biggest story during Ruppelt's Blue Book tenure. It culminated in the Pentagon press conference on July 29th with an array of Air Force brass. The press conference had been called by the head of Air Force Intelligence, Major General John Samford. The reason for the press conference was that the wave had run wild in the media for two weeks, including two weekends of Washington, D.C., skies being virtually invaded by unidentified radar tracking of strange lights. The wave as reported by the press was unusual in that unlike earlier periods of activity when the government was quick to debunk the sightings as significant, the 1952 wave went practically unchallenged by any official pronouncements for most of the two-week span. All of this activity threatened to draw into question the military's ability to deal with embarrassing security penetrations, especially over Washington.

Two things could have happened from the press conference. The Air Force could have admitted they were helpless to stop the saucer reports, not a very likely option. Or they could have dismissed the activity as due to misidentifications, weather phenomena, or a lack of further detail for a positive evaluation. And most important, that they posed no threat to national security. This second option is what happened at the press conference. There wasn't much choice for the Air Force.

Beginning on page 130, Bennett's take on the press conference was nothing short of remarkable. He launched into an attack on General Samford's capabilities and intelligence that went far beyond what I recall about the event from film and transcripts, or what others have said about it. Claiming Samford "lacked all personality and field leadership qualities" and was "out of his depth," Bennett tears into Samford's career. Citing Samford's appointment to the NSA in 1956, he calls it the "National Security Administration" instead of the "National Security Agency," and slams Samford for making mistakes, observing that he "did not have anything like the great synthesizing brain required (presumably!) for such a post."

I looked at the footnotes for evidence of such negative facts and opinions about Samford and was unable to find any in the single biographical source

cited by Bennett. Bennett never knew Samford so where did these notions come from? General Roger Ramey, also a conference participant, was called "the only really sinister element" there. Why? Because according to New Ufology the Roswell crashed saucer coverup was orchestrated by Ramey. These character assessments were uncomfortable to read, but amazingly they became even more hyperbolic.

Samford was favorably compared to Nazi armaments minister Albert Speer as a good "techno-bureaucrat," with Samford able to make UFO reports vanish as efficiently as the Nazis made millions of corpses vanish! This is about as ugly an analogy as I've seen in a long time. Yet, Bennett persisted in the Nazi comparisons. He calls Captain Roy James of the Technical Analysis and Electronics Branch of the Air Technical Intelligence Center at Dayton, Ohio, and a conference participant, one of the "greatest liars in history." This is said to be because if Stanton Friedman is correct about his theories on Roswell reality (and Bennett felt he "almost certainly" was), then James stands along with all-time liars like Nazi propaganda minister Josef Goebbels, Joseph Stalin, and President Bill Clinton!(?) All because we know Roswell is real, according to Bennett, and these people were part of a monstrous conspiracy to hide the truth.

On page 137 Bennett says that ground observers took still photographs of saucer-shaped objects in the night sky over the White House. I had spent a great deal of time poring through newspaper microfilm of this time, literally hundreds of titles reporting on the July wave. In none of them do I recall seeing still photographs of saucers over the President's residence. There is one picture claimed to have been taken in 1952 over the Capitol. It first appeared in a tabloid newspaper in the 1970s and was quickly identified as lens flares from the dual light poles in front of the Capitol. Nevertheless, it has been widely reproduced in countless later UFO writings as genuine. The alleged White House photos are not referenced in the text, nor footnoted or discussed further. Where are they?

At this point I had really had enough of this book. It came off as an aggrieved UFO believer going off on familiar events of UFO history after having had a few beers with friends at the local bar. Bennett's rhetorical excesses were so over-the-top that this cannot be regarded as good UFO history, or even bad UFO history. It is terrible UFO history. He tosses incredible insults at people like a patron throws beanbags to dunk the clown at the carnival. Ruppelt doesn't deserve this treatment however well-intentioned Bennett was. I trust these problems will be repaired for future volumes.

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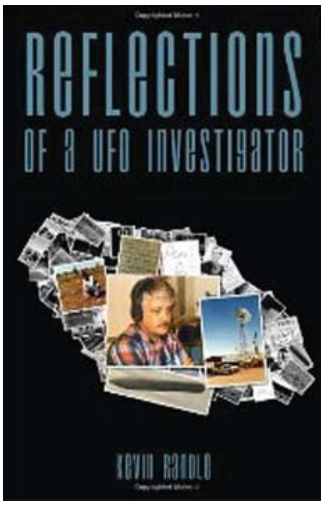
Reflections of a UFO Investigator by Kevin Randle. San Antonio, TX: Anomalist Books, 2012. 270 pp. (paperback). $15.95. ISBN 9781933665566.

This book was a "fun read" since it provided new (to me) details of the life and investigations of a man who has been investigating UFOs longer than I have, even though he is younger. Although this is not an autobiography, he presents enough of the key events in his life so that one can see how his thinking about the UFO phenomenon developed as the years went on. Of particular importance in this book are the cases he discusses and his present opinions of them.

He devotes a considerable amount of space to what may be his longest and most important investigation, that of the "Roswell Incident" (whatever may have crashed at Roswell, New Mexico, in June or July 1947). Although I have not been a Roswell investigator, I have followed the story as it developed starting in 1979. Kevin became interested in Roswell about ten years later and subsequently, with Don Schmidt, carried out an investigation that was largely independent of the investigation already carried out by Stan Friedman, Bill Moore, and others. Kevin and Don had published two Roswell books in the early 1990s, so I was pleased to see presented in this book an overall review of what has been discovered since then, as well as rebuttals of the arguments put forth by the skeptics. Information in this book shows why his opinion still is that, at the very least, it was not a Mogul (or any type of) balloon and could have been some sort of "ET" craft. (This had been my opinion since 1980 when I first learned of the testimony by Jesse Marcel, the Roswell base security officer who handled the material found at the crash site.)

Kevin's and my investigations crossed paths in the case of the crash story told by Robert Willingham. I included this story in my article entitled "Immediate High Alert" because researcher Todd Zechel had told me that he had, in the latter 1970s, checked Willingham's story and had even made a videotaped mini-documentary (with the help of Japanese documentarian Jun-ichi Yaoi) of Willingham being flown over the area of the supposed crash. Furthermore, according to Zechel, the crash took place on or about December 6, 1950. This date was important because it fitted with a bonafide FBI file document, dated December 8, 1950, that said the Army Counterintelligence corps had been placed "on immediate high alert" for any information related to flying saucers. Furthermore, there actually was a high alert in the Pentagon and White House (Truman Administration) during the morning of December 6. The reader can find the whole story on this mysterious alert at http://www.brumac.8k.com/IHAlert/IMMEDIATEHIGHALERT.html



To date no one knows (so far as I know) whether or not the government-wide alert was a result of flying saucer sightings, and no one knows why the FBI was told about the immediate high alert, but the important things to note here are that the alert was real and was not related to or based upon Willingham's story, which Kevin has shown to be not true. So, if you read the above article, ignore the part about Willingham.

One further correction: In the last chapter Kevin points out that most of the cases he has investigated have turned out to explainable one way or another, but there are a few which aren't. One case he cites is the Trent photo case (McMinnville, Oregon, May, 1950) which I extensively investigated over a period of several years in the 1970s. This case includes photos that are so clear that either they are a hoax or they show the "real thing" (disc-shaped flying object). Kevin correctly points out the skeptics argued that it must be a hoax because shadows on the eastern garage wall could only have been made by the sun in the early morning, thus invalidating the claim by the Trents that the pictures were taken in the evening. Kevin has written that I claimed that the shadows were a result of "random light scattering." This is not what I claimed. As part of my detailed analysis, I discovered that the shadows could have been made by a brightly illuminated cloud east of the garage, and I also showed that the shadow size and brightness were not consistent with sun illumination in the morning. Details of this long investigation are at http://www.brumac.8k.com/trent2.html and http://www.brumac.8k.com/trent1.html

To conclude, UFO researchers and investigators will appreciate the updates on the numerous sightings discussed in the book, and everyone will appreciate Kevin's insights gained from his years of studies and investigations of the UFO phenomenon.

Bruce Maccabee
http://brumac.8k.com/

Further Books of Note

Qué es Sensibilidad Psíquica? [**What Is Psychic Sensibility?**] by Alejandro Parra. Buenos Aires: Deva's, 2011. 190 pp. US$12. ISBN 9789875821088.

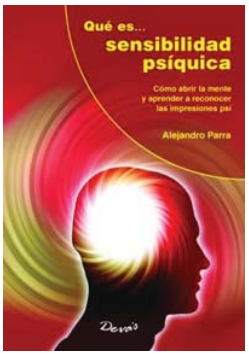
Alejandro Parra is an Argentine researcher and writer in the field of parapsychology. He has written a series of popular books on this topic over the years, each of which is a model of clarity and accuracy. This book, written in Spanish, *What is Psychic Sensibility?*, is no exception.

Parra begins by introducing his readers to the concept of psychic phenomena, providing a brief history and some vignettes from his file of case histories. Early in the book, he answers the question posed by the title. For Parra, "psychic sensibility" is the ability to interpret certain events in a way that allows the extension of one's senses to incorporate information in a form that cannot be explained by mainstream science.

Parra also gives brief but accurate descriptions of laboratory experiments such as the dream telepathy studies conducted at Maimonides Medical Center in Brooklyn, New York, the remote viewing research conducted by Edwin May and others, and the "presentiment" investigations of Dean Radin and Dick Bierman, during which participants displayed discernible physiological reactions just before an erotic image was quickly flashed on a screen (non-erotic images evoked no physiological change). Parra also provides examples of when psychic sensibility appeared to have life-saving qualities, citing cases collected by Louisa Rhine (during which both realistic and symbolic dreams stimulated dreamers to change behaviors that would have been disastrous) and Ian Stevenson (regarding the remarkable number of ticket cancellations prior to the *Titanic*'s doomed voyage).

Parra tackles the issue of how psychic sensibility can be differentiated from psychopathology on the one hand and from fantasy on the other. He regrets that the American Psychiatric Association includes such characteristics as "hearing voices" and "magical thinking" as symptoms of mental illness without giving at least a footnote to possible exceptions. He could have added that these and other psychiatric "symptoms" are commonly reported by many members of indigenous groups around the world.

To utilize psychic sensibility, Parra urges his readers to find periods of time free from distracting stimuli, occasions that do not demand logical thinking, and mood states free of anxiety and depression. He discusses



the role that psychic sensibility can play in creative inspiration, imagination, and intuition. To stimulate psychic sensibility, Parra provides sensible exercises in visualization, relaxation, free association, sensory deprivation, and pychometry. In the latter procedure, people attempt to obtain information about the owner of a common object, simply by seeing, touching, or holding the object. All of a person's response can then be checked out for accuracy.

To his credit, Parra advocates skepticism when using psychic sensibility. He cautions his readers not to accept claims as evidence and to keep written records of "hunches" that can be followed up with reality checks. It is only too common for advocates of psi phenomena to conveniently forget the dozens of dreams that did not foretell future events in favor of the one that did. One could add that scoffers might have a dream that forecast an actual event only to insist they probably had numerous dreams that did not come true. Alan Vaughan was a well-known psychic claimant who kept meticulous records of his dreams. Toward the end of his life, at the request of James Spottiswoode, he selected 61 dreams characterized by at least three correspondences between the dream and the event it apparently foreshadowed. The dates on which these dreams had occurred were found by Spottiswoode to have occurred on nights of remarkably low geomagnetic activity (in comparison to other nights of the week). This association was found in other dream studies as well (e.g., Persinger & Krippner, 1989) and could become a "marker" that might be put to practical use by future dreamers because geomagnetic activity is a matter of public record.

However, Parra does not limit psychic sensibility to glimpsing the future. He also suggests that it can help his readers see overall patterns when faced with complex phenomena. Whether his readers will take the time to keep records or not, Parra urges them not to abandon analytical thinking and logical problem-solving. Instead, he suggests that psychic sensitivity can guide analytical thinking rather than working against it. This is one example of the balance that characterizes this book, a perspective that keeps Parra's readers interested rather than being bored by a pedantic presentation of this type of material. At the same time, Parra refrains from the sensationalism that characterizes too many books on psychic phenomena. In this way, he becomes a model of the very approach to psychic sensibility that he recommends his readers employ.

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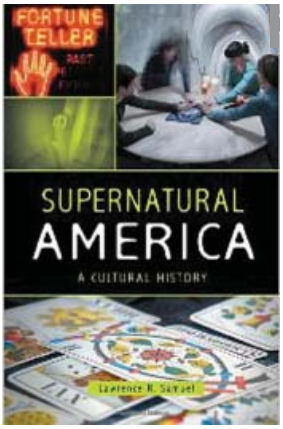
Supernatural America: A Cultural History by Laurence R. Samuel. Santa Barbara, CA: Praeger, 2011. 217 pp. $49.95. ISBN 9780313398995.

Laurence R. Samuel, a Fellow of the Smithsonian Institution's Museum of Natural History, has written a sprightly book, *Supernatural America: A Cultural History.* The word *supernatural* is generally connected with deities or spirits, and Samuels could have avoided this connotation by using "occult" or "paranormal." Samuel's position is that "there is not a shred of definitive evidence" that any such phenomenon has ever occurred, yet belief in the supernatural is as high as it has ever been over the last century. He further asserts that "It was a mistake for researchers to try to turn the supernatural into a science," referring to parapsychologists whose work Samuels sometimes describes accurately and sometimes misrepresents; his paragraph describing Maimonides Medical Center's laboratory investigation of dream telepathy contains no fewer than five blatant errors. Most of Samuel's book deals with topics bypassed by most parapsychologists, namely astrology, auras, fire walking, fortune telling, Ouija boards, UFOs, and witchcraft. This portion of the book, an historic panorama filled with colorful characters and dramatic (and often humorous) anecdotes, is well-written and entertaining. He points out how these topics can be explained by what anthropologists and cognitive scientists have discovered about human nature and the human brain. Finally, the book might have been titled *Supernatural White America* as there is no mention of Chinese astrology, Hispanic Spiritism, Mexican–American curanderismo, New Orleans voodoo, or African–American cults.



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