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SSE NEWS

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EDITORIAL

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I had the opportunity recently to referee a submission to a clinical psychology journal that examined the apparent manifestation of ESP in the psychiatric setting. I'd been solicited for this chore, not simply because of my background in parapsychology, but also because of my earlier research into dissociative identity (multiple personality) disorder (e.g., Braude 1995, 1996, 1998). The submitted paper was not awful, and commendably the author apparently had done a considerable amount of reading of relevant works in parapsychology. Nevertheless, the paper had one glaring flaw, and that flaw reminded me of one of my long-standing concerns about the current state of parapsychological research. Moreover, it wouldn't surprise me if similar concerns can be raised in other areas of frontier science.

Although this paper had a reasonably extensive (and perhaps superficially impressive) bibliography, there was not a single mention of important work on the topic done in the mid- Twentieth Century—most notably, the work of Jule Eisenbud (e.g., Eisenbud 1946, 1970, 1992) and Jan Ehrenwald (e.g., Ehrenwald 1948, 1978). I found it astonishing that the author would presume to summarize the conceptual and empirical antecedents to the research described in the paper without reference to this material.

Of course, one would expect newcomers to psi research to have only a very limited understanding of prior work in that domain. However, this episode reminded me of many other occasions on which I've seen newcomers try to publish scholarly research in parapsychology without adequate preparation for the task. In fact, all too often parapsychological newcomers seem to think that the only qualification needed in order to do responsible psi research is to have a background in some area of mainstream science, as if there were no extensive parapsychological history of enlightening successes and failures, and no serious thinking about the conceptual foundations of parapsychology, that must be mastered first. As I and other chronologically challenged SSE members have often noted, we could certainly benefit from attracting new blood into our areas of interest. But we'd also like to avoid reinventing the wheel or simply repeating past mistakes.

But in a way, that kind of newcomer naïvete or hubris is understandable—if not exactly forgivable. Consider, where is an aspiring parapsychologist to go for the necessary background education? Only a handful of reputable universities offer competent programs in psi research. And no doubt some other areas of anomalistics are in the same boat—e.g., cryptozoology, UFO studies, and astrology, all of which have extensive histories. By contrast, someone wanting to forge a career in a mainstream science can easily enroll in a program that can prepare them to do decent research of their own.

So what can educated veteran psi researchers do to ensure that the rich history of prior work doesn't simply fall victim to the gnawing tooth of time? Unfortunately, Wikipedia has been hijacked by aggressive psi skeptics. However, the Society for Psychical Research has launched an online encyclopedia, to which I and some other SSE members have contributed, which (although its audience is presumably many times smaller than Wikipedia's) takes up some of that slack. Regular conferences can help as well—such as those of the SSE (Society for Scientific Exploration), PA (Parapsychological Association), and SPR (Society for Psychical Research). I'm not sure that other areas of frontier science enjoy anything like the level of professional competence and engagement one finds at these conferences.

Another strategy would be for the *JSE* to publish occasional review articles. We already have a Historical Perspectives section in which we survey (and sometimes resurrect) important early studies. Typically, these tend to focus on late nineteenth- and early twentieth-century work—that is, research beginning slightly before or roughly around the time the SPR was formed. However, at the most recent SSE conference in Las Vegas, I was gratified to learn that many Council members would like to see the *JSE* publish review articles on more recent, and evidentially rich, areas of research. I support this initiative wholeheartedly and have already solicited some papers. I can tell you now that some survey articles will appear in due course. Moreover, if SSE members like the idea of including one or more survey-type presentations at subsequent conferences, I encourage them to make their wishes known to Council members, and perhaps open the matter for discussion at SSE conference business meetings.

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Beginning with this issue, I'm very pleased to welcome Harald Walach, Ph.D., to our team of Associate Editors. As many readers probably know, Harald works at the intersection of medicine, psychology, and

consciousness studies. He holds a double Ph.D. in Clinical Psychology, and History and Theory of Science, and he's the author of 14 books and many articles and book chapters. He's also editor of the journal *Complementary Medicine Research*, and editor of the Springer book series "Neuroscience, Consciousness, Spirituality."

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Since this is the holiday season and an appropriate time for wallowing in weltschmerz and reflecting on the year that's coming to a close, I'd like once again to acknowledge and thank our dedicated and overworked team of Associate Editors and the many reviewers on whom we rely in vetting papers for inclusion in the *JSE*. As I've noted before, producing this *Journal* poses a distinctive challenge. Because the *JSE* deals with topics either shunned altogether or dealt with shabbily by more mainstream publications, the community of qualified readers for high-level peer review is quite small. Ideally, I'd prefer to have a larger team of Associate Editors, in order to lighten the editorial load for those who—perhaps inscrutably—continue to volunteer large chunks of time to shepherding submissions through our system. However, adding members to that team inevitably subtracts from the small pool of qualified referees. So I'm deeply grateful to my largely behind-the-scenes Associate Editors, who realize the need to maintain the high standard of scientific and scholarly excellence that's characterized the *JSE* since its inception, who recognize that there are only so many people on whom the *JSE* can rely, and who accordingly and generously donate their valuable time. I'm equally grateful to our many referees, many of whom we call upon over and over again, simply because they have expertise in the relevant areas of research, and because the number of people who have both that expertise and the relevant degree of open-mindedness about new ideas remains depressingly small.

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

—Stephen E. Braude

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RESEARCH ARTICLE

Experimenter Psi Effect in Precognition Tests with Planarians

Fernando Alvarez

Tomares (Sevilla), Spain
alvar_gonzalez@hotmail.com

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Abstract—Two groups of 25 black planarians were allowed to crawl in a horizontal trough with one LED at each end, and the distance and direction of locomotion was recorded. Two blocks of trials were carried out. 1) The Active Experimenter block consisted of Experimental and Control trials. During the former, one of the two LEDs would be selected to be lit by an RNG and the experimenter would light it up. After one week the same subjects were submitted to the control trials, when exactly the same procedure was followed, although the two LEDs were disconnected from the electric source. 2) The Passive Experimenter block of trials followed the same procedure except that the computer directly lit up the LED randomly chosen by the RNG. The results of the Active Experimenter block showed that during the experimental trials, at the very moment of stimulation, the planarians were significantly more distant to the lit LED. During the control trials the subjects were also significantly more distant to the sham lit LED. The number of 5-sec periods (within the 1-min period) spent moving away from the LED to be lit was also higher. In both kinds of trials during the Active Experimenter block, the number of such periods significantly increased as the time point approached the moment of stimulation or sham stimulation. During the experimental and control sections of the Passive Experimenter block of trials, the planarians did not show any preference avoiding the LEDs to be lit or unlit, under the two criteria of distance values and number of 5-sec periods. From the results we may conclude that human activity had an Experimenter Psi Effect on the planarians or on the RNG device.
Keywords: behavior—distance measures—experimenter psi effect—planarians—precognition

Introduction

If anomalous phenomena are part of the natural world, we should expect them to follow the rules governing nature, and if those phenomena, as demonstrated to occur in humans, do not depend to a high extent on

our intellectual capabilities, but appear to be more connected to emotional or unconscious physiological states (Radin 1997, 2004, Bierman & Radin 1999, Bierman & Scholte 2002, Spottiswoode & May 2003, McCraty, Atkinson, & Bradley 2004, Sartori, Massaccesi, Martinelli, & Tressoldi 2004, May, Paulinyi, & Vassy 2005, Tressoldi et al. 2011), it would not be out of the question that such singular events also are found in the animal world, to which we belong.

In relation to anomalous anticipatory activity, although up to now the effort to bring this ability to light in animals has been scant, there are sufficient results to suggest that the phenomenon is widespread, being present not only in those animals endowed with a highly developed nervous system, such as birds and mammals (Duval & Montredon 1968, Sheldrake & Smart 1998, 2000, Radin 2002, Alvarez 2010a, 2010b) but also in primitive ones such as earthworms and planarians (Wildey 2001, Alvarez 2016).

Concerning the subject species of this study (the black planarian *Girardia dorotocephala*), recording the frequency of their Head Movements (a behavior indicative of distress or ambient exploration) showed them to be able to anticipate a noxious upcoming event (a startle sound), the effect being especially intense immediately before the presentation of the stimulus (Alvarez 2016).

When trying to understand the physiological mechanisms involved in human precognition, the use of animals endowed with a primitive nervous system could be of help. In connection with this, the use of planarians with that purpose could be of great interest, since, according to the structure and physiology of their central nervous system and cerebral ganglion, these are positioned as the most likely ancestors of the vertebrate brain (Baguñá & Romero 1984, Agata et al. 1998, Sarnat & Netsky 1985, 2002, Umeda et al. 2005, Murakoshi & Yasuda 2012).

Initially, the aim of this study was to arrive at a simple technique to study precognition in the black planarian, using distance measures instead of head movements (Alvarez 2016) for the detection of precognition, taking advantage of the planarians' strong photophobic response. Then, in view of the results of the first block of trials, the objective changed toward exploring the phenomenon of experimenter psi effect.

Before R. G. Stanford introduced the Psi-Mediated Instrumental Response (PMIR) model in 1974 and afterwards (Stanford 2015), it was assumed that in psi experiments the participant was nearly the only party responsible for the psi response. This model proposes that in the presence of a particular need the organism uses sensory and psi means to scan the environment for relevant objects or events, then using them in an instrumental way to satisfy the need. The model and the proofs on which it

stands showed that a psi effect can be produced without the agent intending to do so, provided that the psi effect favors the realization of a need or desire. Therefore, experimenters, presumably having at least a moderate interest in the success of their work, under the PMIR model would be expected to unconsciously use their own psi to fulfill their desires for the experiments. On this basis, J. Palmer hypothesized about the possible ways experimenter psi effect could occur in anomalous cognition experiments (Palmer & Millar 2015).

In addition, although the results of the long-term and worldwide experiment of the Global Consciousness Project (GCP) point to an effect by human collective emotions on the output of RNGs and the existence of a field-like Global Consciousness (GC) (Nelson 2013), a close examination of the information by Bancel (2017) appears to indicate that GC cannot explain the results, which may rather be due to a goal-oriented (GO) psi experimenter effect, associated with agents engaged with the project and aware of the pretended goal (most probably the experimenters). Nevertheless, Nelson's (2017) reply concerning the weakness of some of Bancel's assumptions (like the attribution of the effect only to an intentional source) and the structure of GCP data (like temporal and spatial parameters incompatible with the GO model) shows that GC should not be rejected outright in a successful model.

This study aims toward understanding the phenomenon of experimenter psi effect. My hypothesis was that under imminent stimulation precognition would show up in the results when the experimenter was an active participant during the tests, and would not manifest when the experimenter was passive.

Methods

A colony of black planarians was maintained in polypropylene plastic containers filled with dechlorinated (aged) tap water at 22–24 °C (Claussen, Grisak, & Brown 2003) with slow aeration, and fed on raw homogenized veal liver paste (frozen and then thawed at room temperature) and frozen and thawed larvae of chironomid midge flies, once a week, followed by water change. The planarians were allowed to feed for three hours and the water was then changed to remove any debris. They were kept on a normal diurnal cycle (exposed to diffuse natural light during the day and kept dark at night.

The use of light to test for precognition in planarians was based on their strong photophobic response (Parker & Burnett 1900, Taliaferro 1918, Halas, James, & Stone 1961, Arees 1986). The experimental chamber consisted of a 10-cm horizontal trough made of polystyrene; its transverse

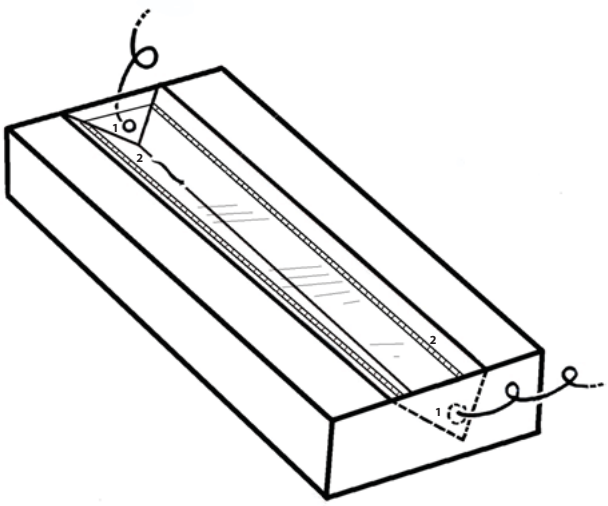

Figure 1. Experimental chamber. (1) LED. (2) Tape measure.

section was V-shaped and, in order to record the planarian's location, a millimeter tape measure was run along both sides of the trough. At each of both ends at the bottom of the trough there was a 5-mm LED-emitting blue-green light of 503–512 nm (model *verde-agua* 529PgOc, Robotecno, Spain), which corresponds to the species maximum ocellar sensitivity to 508 nm light (Brown & Ogden 1968). The trough was leveled to gravity, and during the experiments it was filled with water up to 5 mm (Figure 1).

To prevent reflections, the trough surroundings were painted matte black and covered with transparent matte acrylic medium. The trough was inside an empty 28 × 16.5 × 15 cm aquarium, surrounded by water (with a heater in it, connected to a thermostat, to maintain the temperature at 22–24 ºC) inside another, 35 × 20 × 26 cm, aquarium. The whole block was mounted on a 10-cm layer of polyethylene foam sheet to reduce any vibration. Maintaining the experimental chamber at sufficiently high temperature (22–24 ºC) would make the subjects more responsive to the onset of light (McConnell 1967).

Tests were carried out between 8:00 and 11:00 UT (apparently black planarians learn the correct response in a training task more often in the morning (Cohen 1965)). The subjects were transferred using 3-ml disposable plastic pipettes with a widened opening so as not to injure them.

The experiment was composed of two blocks. The first block (henceforth known as Active Experimenter block, carried out between January 12

and February 17, 2017, on 25 subjects) consisted of two sections of trials (Experimental section, followed after one week by the Control section, both on the same individuals). The second block (henceforth known as Passive Experimenter block, carried out between December 14, 2017, and January 14, 2018, on 25 different subjects) was also composed first of the Experimental section also followed after one week by the Control section on the same individuals.

At the beginning of each session, one subject was taken from its colony and placed exactly at the center on the trough (at equal distance from both LEDs). All trials consisted of an accustoming period of five minutes to adjust the planarians to the new environment, followed by another five minutes of light or sham stimulation.

At the end of the 5-min accustoming period of the Experimental trials of the Active Experimenter block, a random event generator (true RNG, by Orion Electronics) would determine that one of the two LEDs at the terminals of the trough would light up. Immediately after the RNG made the choice, I lighted the selected LED for the 5-min stimulation period. After one week, the same individuals were submitted to the control trials of this block, where exactly the same procedure, involving the computer, the RNG, and the experimenter, was followed, except that the two LEDs were in this case disconnected from the electric source.

The Passive Experimenter block of trials was organized in exactly the same manner as the Active Experimenter block, although in this case I (the experimenter) remained in the room but had no part in switching on the LED light, which was turned on directly from the RNG and the computer (as done in other planarian experiments (Alvarez 2016)). In the same way as for the Active Experimenter block during the Control section of the Passive Experimenter block, the LEDs were disconnected from the electric source.

After each trial, the trough was cleaned and dried, and before the next subject was tested water from its container was added. From the beginning to the end of the trials, all subjects were filmed with a 25-frames-per-second videocamera located 55 cm directly above the experimental trough (a Sony DCR-SR72E provided with a polarizing filter in order to suppress glare from the water surface). During the experimental sessions, the room was kept dark, and to allow filming in line with the species minimum evoked ocellar potential in the red (mainly at 600 nm and above, Brown, Ito, & Ogden 1968) diffuse illumination was provided by a led lamp emitting red 620–630 nm light.

During the video analysis, I measured the distance to ±1 mm from the subject's tip of the head to each of the two LEDs at the very moment of stimulation or sham stimulation, and in the previous 12 5-sec periods

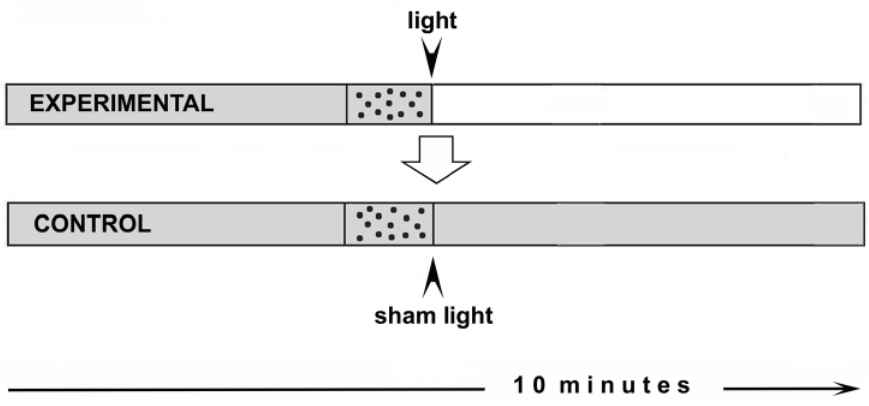

Figure 2. Order of events of the experiment. Time of darkness is represented in grey, and the stippled area corresponds to the time of observation. The diagram applies both to the Active and Passive Experimenter blocks.

(totalling one minute) I counted the number of periods in which the direction of locomotion was toward one or the other end of the trough. To obtain these two measures, the VLC Media Player computer program was used (Figure 2).

To prevent experimenter bias, after determining the exact moment of stimulation or sham stimulation and labeling each individual video clip, a third person kept the identity of each trial unknown to the author (who examined the video clips) until all analyses of each block were completed.

Statistical Analysis

Previous to all analyses, it was shown that out of the 100 cases of lighting or sham lighting in both blocks, 52 of them involved LED1 (26 to block 1 and 26 to block 2) and 48 involved LED 2 (24 to block 1 and 24 to block 2).

The distributions of distances to the LED which will light and will not light up (Experimental trials) or falsely light and will not light up (Control trials) at the moment of real or sham stimulation, and of the number of 5-sec periods spent moving away with respect to both LEDs during the 1-min period before real and sham stimulation for both blocks, did not deviate significantly from normality ($p > 0.20$, Kolmogorov-Smirnov test). Consequently, the *t*-test for dependent samples was applied to those data in order to find out if the planarians stayed farther away from the LED of the succeeding true or sham stimulus than to the other LED, as well

as comparing the distance values obtained in the experimental and control trials for the same individuals.

To ascertain the relationship between the passage of time and the behavior of moving away from the lit LED, the Pearson correlation coefficient test was applied to the order of 5-sec periods one minute before real stimulation with the mean number per period and subject of periods spent moving away from the LED to be lit up (distributions in all cases did not deviate significantly from normality).

Data analysis was done using the STATISTICA 6.0 computer program. All reported *p* are two-tailed.

Results

Active Experimenter Block

Distributions of distances toward LED 1 and LED 2 (irrespective of which one was lit) and of the number of 5-sec periods spent moving away from each of both LEDs (irrespective of which one was lit) during the Experimental and Control trials did not deviate significantly from normality ($p > 0.20$). During the experimental and control trials, at the moment of stimulation and sham stimulation, the distance toward both LEDs was statistically equivalent (Experimental: $df = 24$, $t = 0.58$, $p = 0.567$; Control: $df = 24$, $t = 1.64$, $p = 0.114$; *t*-test for dependent samples). The same lack of preference for either of the trough ends was shown in the number of 5-sec periods involving moving away locomotion (Experimental: $df = 24$, $t = 0.82$ $p = 0.416$; Control: $df = 24$, $t = 0.09$, $p = 0.928$).

During the experimental trials, when at the moment of stimulation the distance to the LED to be lit was compared with that toward the LED which would not light up, the planarians appeared significantly more distant to the lit LED ($t = 2.23$, $df = 24$, $p = 0.035$; *t*-test for dependent samples, Figure 3).

Unexpectedly, during the control trials (without any real light stimulation) at the instant of sham stimulation, the distance to the sham lit LED was also significantly higher than that toward the sham unlit one ($t = 2.32$, $df = 24$, $p = 0.029$; see Figure 3).

The comparison between distances toward the lit LED during the experimental trial and toward the sham lit LED during the control trials yielded a nonsignificant difference ($t = 0.168$, $df = 24$, $p = 0.868$; *t*-test for dependent samples).

With reference to the number of 5-sec periods within the final 1-min interval spent moving away from the LED to be lit and toward the LED which would not light up, during the experimental trials a significantly higher value was obtained for the former ($X \pm SE = 7.0$

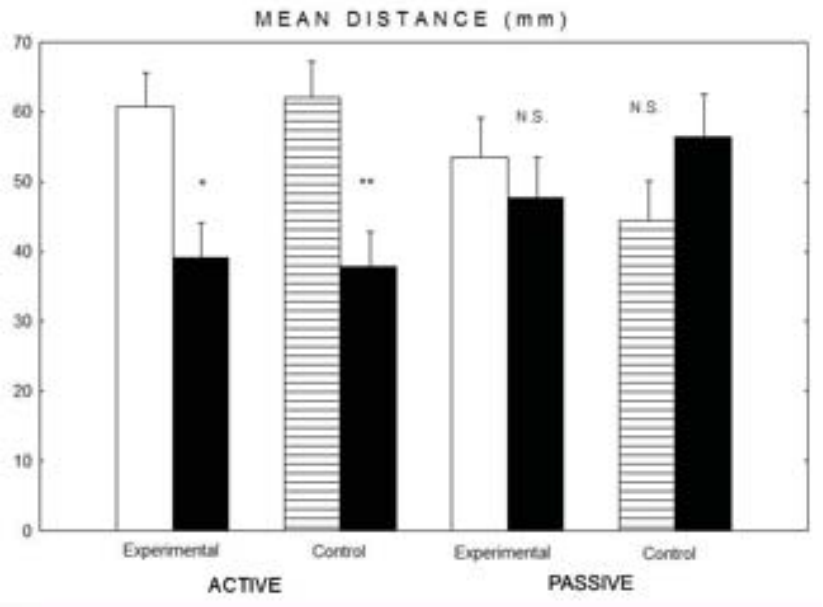

Figure 3. Mean distance (+SE) toward the LED which will light up (empty bars) and the one providing no stimulation (black bars) at both ends of the trough during the experimental trials and toward the LEDs related to sham stimulation (striped bars) and no stimulation (black bars) during the control trials at the very moment of real or sham stimulation of the Active and Passive Experimenter blocks. * $p = 0.035$, ** $p = 0.029$. N.S. = not significant

± 0.5, and 4.5 ± 0.5, respectively; $t = 2.73$, $df = 24$, $p = 0.012$), the effect being in the same direction and even slightly stronger during the control trials (7.5 ± 0.6, and 4.2 ± 0.6, respectively; $t = 3.63$, $df = 24$, $p = 0.006$; *t*-test for dependent samples).

During the experimental trials, when testing the relationship between the passage of time and the direction of locomotion away from the LED to be lit, a significant positive correlation was obtained between the order of time points of the 1-min period approaching the moment of stimulation and the mean value per subject of the number of 5-sec periods when the planarians moved away from that LED ($n = 12$, $r = 0.801$, $p = 0.002$; Pearson correlation coefficient test). Again, the effect was also positive and more intense during the control trials, when no real light stimulation took place ($n = 12$, $r = 0.873$, $p = 0.0001$) (Figure 4).

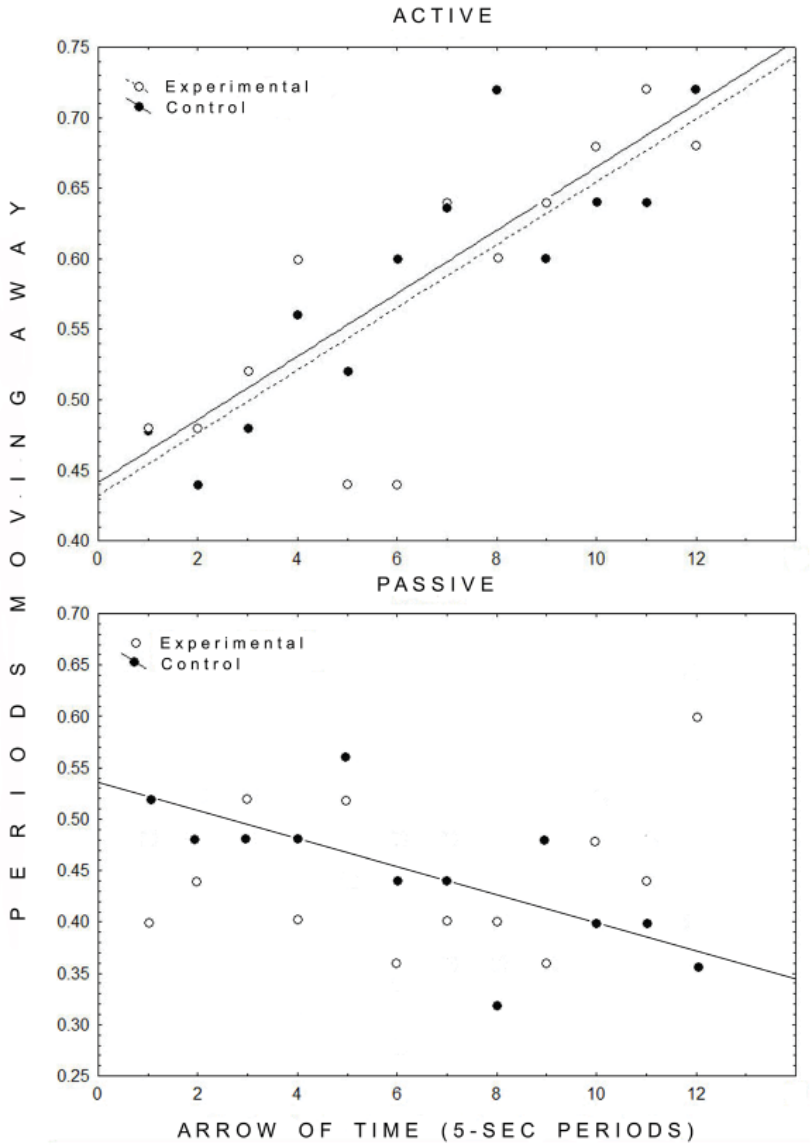

Figure 4. Scatterplots of the passage of time (order of 5-sec periods one minute before real and sham stimulation) against the mean number of periods spent moving away from the real or sham lit LED for the Active and Passive Experimenter blocks.

Passive Experimenter Block

Distances toward LED 1 and LED 2 (irrespective of which one was lit) and of the number of 5-sec periods spent moving away from each of both LEDs during the Experimental and Control trials did not deviate significantly from normality ($p > 0.20$). During the experimental and control trials, at the moment of stimulation and sham stimulation, the distances toward both LEDs were statistically equivalent (Experimental: $df = 24$, $t = 0.98$, $p = 0.339$; Control: $df = 24$, $t = 1.13$, $p = 0.270$). The same lack of preference was shown in the number of 5-sec periods moving away (Experimental: $df = 24$, $t = 0.44$, $p = 0.663$; Control: $df = 24$, $t = 0.45$, $p = 0.660$).

During the Experimental trials, the comparison of distances to the LED to be or not to be lit showed no significant difference ($t = 0.55$, $df = 24$, $p = 0.581$). The same result was obtained during the sham stimulation Control trials ($t = 1.08$, $df = 24$, $p = 0.289$; see Figure 3).

The difference between distances toward the lit or sham lit LED between the Experimental and Control trials was also nonsignificant ($t = 1.42$, $df = 24$, $p = 0.170$).

During the experimental trials, no significant difference was obtained in the number of 5-sec periods spent moving away from the LED to be lit and toward the unlit LED (5.3 ± 0.6, and 6.1 ± 0.6, respectively; $t = 0.70$, $df = 24$, $p = 0.490$). A similar result was obtained for the control trials (5.3 ± 0.5, and 6.5 ± 0.5, respectively; $t = 1.13$, $df = 24$, $p = 0.271$).

The relationship between the passage of time and the direction of locomotion away from the lit LED during the experimental trials was nonsignificant ($n = 12$, $r = 0.241$, $p = 0.4351$). Unexpectedly, in the case of the sham lit LED, the relationship was negative and significant ($n = 12$, $r = 0.698$, $p = 0.012$) (Figure 4).

Discussion

Together with the result of the Active Experimenter block of trials showing that planarians behaved in advance to avoid future real or sham light stimulation, the absence of a positive response when the experimenter was passive unquestionably points to a goal-oriented experimenter psi effect.

Then, the fact that planarians avoid in the same manner upcoming real and sham light during the Active Experimenter block of trials suggests that they are not responding to future light stimulation. During these trials the planarians were moving away from the LED to be lit or sham lit before the choice by the RNG was done, and at that time the experimenter did not consciously know yet the succeeding outcome of the choice. Consequently, if any information would have been communicated from the experimenter to the planarians, it would have to be of a retrocausal nature.

In any case, we should recognize that the effect might have acted not only over the planarian participants but also on other elements of the trial situation, perhaps making the RNG produce a non-random choice convenient to the experimenter (and the planarians'?) desires. That precognition and micro-PK effects may draw from similar mechanisms of information exchange between human consciousness and random processes has been suggested by Dunne and Jahn (1992), and actually it is not possible to experimentally distinguish precognition from PK. Pre-stated intention or awareness of the situation is known to correlate with shifts in the output of random microelectronic devices (Schmidt 1970, Bierman & Houtkooper 1975, Nelson et al. 1991, Dunne & Jahn 1992, 1995, Mason, Patterson, & Radin 2007, Ivtzan 2008, Alvarez 2012).

Although these findings and the absence of a response that could be categorized as precognitive during the Passive Experimenter block of trials would make us think that planarians are merely responding to psi indications by the experimenter, in another study with planarians where the RNG and the computer directly produced a noxious random stimulus (the experimenter remaining passive but present), the subjects showed alarm behavior significantly in advance of stimulation, suggesting the interpretation of actual precognition on their part (Alvarez 2016), although perhaps in this case the passive experimenter being present could be the actual source of psi. The same reasoning would apply to the result of a micro-PK experiment with birds (Alvarez 2012), where the experimenter was passive but present.

To the result of an experiment where animal subjects (cockroaches) were tested to see if they could influence a binary RNG device so as to avoid an electric shock, Schmidt (1970) responded suggesting that an experimenter psi effect could have occurred. The experimental situation (the experimenter was passive but always present observing the animals while the trials were run) and the obtained negative results (the cockroaches received more shocks than expected by chance) are in some way similar to those of the control section of the Passive Experimenter block of the present study, when planarians approached the imminent noxious stimulus (a negative relationship was found between the passage of time and the direction of locomotion away from the upcoming sham light).

If the experimenter psi effect actually took place in the case of Schmidt's cockroaches and in the Passive Experimenter block of trials of my planarians, it means that no great involvement by the experimenter is necessary for the effect to take place. In any case, this potential effect appears as considerably weaker than when the experimenter was present and active.

To circumvent the failure of conventional experimental approaches to produce reliable and replicable results (Kennedy 2003), Millar (2015) proposes abandoning the classical signal-based model in favor of physics-based approaches of non-local theories, which appear more in consonance with the often elusive facets of psi phenomena.

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RESEARCH ARTICLE

Human Mental Intentionality on the Aesthetics of Cooked Rice and *Escherichia coli* Growth

Alan W. L. Lai

Bonny B. H. Yuen
bonnyyuen@uic.edu.hk

Richard Burchett

Division of Science and Technology, Beijing Normal University - Hong Kong Baptist University
United International College, Tangjiawan, Zhuhai, Guangdong Province, China

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Abstract—This study examines the "intentionality hypothesis"—i.e. subjects' ability to mentally influence microbial growth in samples of cooked rice. Over a 30-day period (under triple-blind conditions), subjects focused their positive and negative thoughts ('mental intentionality') toward three randomly formed groups of cooked rice samples (positive intentionality, negative intentionality, and a control group). After 30 days, pictures were taken of the nine rice samples (three groups, each group was conducted in triplicate), which were then judged for visual aesthetic value. Findings show aesthetic ratings of 'positive' rice samples to be significantly higher than those for 'negative' and 'control' ones ($p \leq 0.05$), with no significant difference between negative and control sample ratings ($p \geq 0.05$). A further test entailed a 7-day study measuring an *Escherichia coli* strain (a type of coliform that is closely associated with food safety, whose presence often indicates food poisoning and spoilage) in vitro under the same conditions of stimuli as the rice samples. Results show positive intention to be associated with lower *E. coli* division rate when compared with the "control" and "negative intention" groups, thereby further supporting the hypothesis, as well as suggesting an emerging inference, that intentionality might be associated with microbial growth and visual aesthetic ratings.

Introduction

This paper examines what is often referred to as the 'intentionality hypothesis', a view of physical reality in which the physical environment—be it macroscopic or microscopic—can be directly impacted by a researcher's mental intent ('mental intentionality') toward specific situational variables. Although considered rather outré by mainstream research psychologists, the intentionality hypothesis has been a focus of sustained study for more than 50 years. General findings suggest that an observer's mental intentionality does seem to possess the ability to subtly impact both living and non-living elements of the immediate physical environment (Radin & Nelson 2003).

The effects of intentionality toward non-living systems has been a topic of particular interest over the past few decades. For example, meta-analytic studies examining more than 500 relevant studies have convincingly concluded that human mental intentionality is capable of affecting the 'random' generation of digital bytes by computer microprocessors (Radin & Nelson 2003, Radin 2006).

In contrast, research examining the impact of mental intentionality upon living systems is considerably more limited. There do exist, however, a number of studies that examine this topic. For example, Radin, Taft, and Yount (2004) demonstrated the impact of mental intentionality upon human brain cell growth and healing. Similarly, Roney-Dougal and Solfvin (2002) demonstrated the effect of mental intentionality upon plant growth, while Verma and Pandey (2014) extended these findings to other areas of agricultural science.

Obviously, these types of findings need to be interpreted with a high degree of caution. Nonetheless, the overall pattern of results is quite intriguing, potentially suggesting a range of practical applications. As just one example, Radin, Taft, and Yount (2004) observed that mental intentionality can positively impact the growth and development of human brain cells, implying obvious therapeutic implications from intention for the rehabilitation of brain-damaged individuals.

While many studies demonstrate the effect of mental intentionality upon the immediate external environment, not all find similar patterns of results. For example, Lenington (1979) studied the growth patterns of plants irrigated with 'spiritually' treated water and those irrigated with tap water, but found no significant difference between the growth patterns of the two groups.

However, more recent studies tend to be more supportive of the intentionality hypothesis. For example, triple-blind studies reported by Emoto (2004) and Radin et al. (2008) reported that when water molecules

were subjected to positive mental intentionality, their solid state (i.e. ice and snow) tended to be perceived as more aesthetic than molecules subjected to negative mental intentionality. Other recent studies have demonstrated the role of mental intentionality in the 'random' evolution of quantum wave functions (Radin 2006, Pitkanen, 2017).

Cited studies notwithstanding, the effects of mental intentionality upon living systems (particularly at the microscopic level) is clearly an area of study calling for further investigation. Of particular concern is the limited number of (successful) replication studies that have been conducted to date. This paucity of research is due to a number of factors, including practical difficulties in the replication of previously employed subject-selection and condition-assignment procedures, as well as the high cost and general impracticality often associated with reliability-oriented research.

For example, the replication of previous work demonstrating the effect of mental intentionality upon solid-state water molecules (i.e. snow and ice) requires researchers to judge which solid-state samples are sufficiently 'crystallized', in addition to obtaining access to a shielded research chamber and a room-sized freezer unit (Radin et al. 2006).

Random number experiments (RNE) have been considered repeatedly as a low-cost alternative in the study of mental intentionality (Radin & Nelson 2003, Radin 2006). In addition to low cost, these types of studies possess the decided virtue of minimizing, if not completely eliminating, issues regarding randomized subject selection and condition assignment. Despite being an economical model, RNE studies do not provide information regarding the interaction of mental intentionality and the behavior of microscopic living systems. However, they stimulated the idea of conducting this study at a low-cost level.

Intentionality and Micro-Organisms: Literature, Framework, and Concerns

This study aimed at testing the intentionality hypothesis on microbial growth and development. It was configured with a two-pronged approach. Firstly, the study was set up with triple-blind conditions, aimed at controlling biased sampling and the experimenter's subjective expectations—for and with both subjective and biological measures. In particular, the subjective measure was to test the "intentionality hypothesis" based on subjects' aesthetic perceptions of cooked rice samples, while the biological measure was based on bacterial samples aimed at testing the intentionality hypothesis further as well as helping to interpret the overall dataset. That said, the former examined cooked-rice cultures in terms of microbial growth and

community, and the latter the microbial growth rate of a single species of bacteria (i.e. *Escherichia coli*) but in non-rice conditions. Secondly, the entire study served as a "cost-friendly" model, requiring simple and inexpensive equipment while enabling researchers to explore the principal interest in living system contexts, thereby strengthening the reproducibility of testing the "intentionality hypothesis."

Regarding the effects of intentionality upon micro-organisms, the literature is scarce. However, a number of studies are available for reference. Despite the different species of micro-organisms involved, all the studies began with the protocol of using a single species of micro-organism as the target medium for testing the intentionality hypothesis. They also postulated that the effects of intentionality could be indicated by the density of micro-organisms (e.g., cell division rates) after being treated in different conditions. Thus, they act to provide a framework for this study. First of all, Barry (1968) reported that after ten subjects concentrated on 194 fungal cultures at a proximal distance, the growth of fungi was inhibited. Likewise, Tedder and Monty (1981) replicated the study but having the subjects concentrate on some fungal cultures at a distance of up to 15 miles. The results also showed the growth of the cultures being inhibited over sixteen of sixteen trials. Intentionality has also been found not purely in association with an inhibitive outcome. For example, Haraldsson and Thorsteinsson (1973) demonstrated that a group of health-related professionals managed to significantly increase the growth of yeast in 120 test tubes with their mental intent when compared with the controls ($p < 0.00014$) (i.e. no mental intent). Additionally, studies on mental intentionality have reported the likelihood that the behavior of microscopic systems could be inhibited or promoted. Nash (1982, 1984), for instance, showed that human intent seemed to be able to increase or decrease *E. coli*'s mutation rate to utilize lactose from "lactose negative" to "lactose positive." Put simply, the bacteria were able to mutate in all desired directions. Based on these studies, a part of this experiment that focused on biological measures adopted the protocol of using a single species of micro-organism (i.e. *E. coli*) as the target element and its growth response under different conditions of mental intentionality.

Biological measures provide a more objective way of studying the intentionality hypothesis; however, it is yet to suffice for interpreting the subjective results. To this end, this study takes the further step of creating another piece in addition to the biological measure. In general terms, it attempts to study how far aesthetic perception could be associated with the effects of mental intentionality on microbial development. Moreover, it used highly economical types of samples as the culture medium for microbial development: cooked rice and *E. coli*. This study is of high significance,

not only because it may provide valuable information about aesthetic perception and microbial growth, but also because it provides researchers with not only a low-cost approach but also a low-technology-to-acquire vehicle for studying the intentionality hypothesis.

Methods

Subjective Measure (Visual Aesthetic Study)

Subjects. The 62 subjects employed in this study were first-year students (aged 18 to 20) attending a liberal arts, four-year college located in mainland China. Students were members of a college-mandated course, and they received partial course credit in return for their participation in the study.

Materials and Stimuli Preparation. The (primary) materials used in this study include nine glass Petri dishes (100 mm × 15 mm), an electric rice cooker, an incubator, a digital camera, a slide projector, and rice. Prior to the cooking process, the rice and all dish materials were thoroughly cleansed with pure water. The Petri dishes were further prepared by autoclaving at 121 °C for 15 minutes. The rice was first cooked in the electric rice cooker at 100 °C and allowed to cool to room temperature. 60 grams of cooked rice was then placed in each Petri dish, which was immediately covered with a glass lid and sealed with parafilm. The Petri dishes were then stored for 30 days in the incubator, set at 27.5 °C.

Procedure. The nine closed and parafilm-sealed Petri dishes (each containing 60 grams of cooked rice) were randomly assigned to one of three experimental conditions: 'positive intentionality', 'negative intentionality', or 'non-intentionality' (i.e. the control condition). According to Schlitz et al. (2003), intentionality is defined as "*an attribute of a conscious and willful action . . .*" or "*. . . a property of objective actions . . .*"

For 30 consecutive days at 10 a.m., the positive intentionality Petri dishes were removed from the incubator and placed on a work bench located in a research laboratory disinfected with 70% alcohol. The primary investigator spent three minutes verbally conveying positive intentionality (i.e. words and phrases of admiration, compliments, approval, etc.) toward these rice samples, captured an individual digital image of each sample, and returned the samples to the incubator. Of note, verbalization of the intentionality functioned as a mediation tool, or a technique employed to promote the investigator's consciousness, willfulness, and awareness while performing the action that would lead to more desirable or objective results. For the negative and non-intentionality samples, the procedure was identical, except the three minutes of verbally conveyed intentionality was either negative in nature (i.e. words and phrases of contempt, reprimand, disapproval, etc.), or

the investigator simply remained silent with no intentionality given to the control rice samples.

Following the 30-day incubation period, the rice samples were removed from the Petri dishes. Two digital images of each rice sample (presented against an off-white backdrop; see Appendix 1), one on each side (top and bottom), were created and copied onto projector slides.

Subjects' aesthetic ratings of the 18 projector slide images of cooked rice served as the dependent variable for this study. To minimize error variance (e.g., experimenter/subject biases), the primary investigator had no direct contact with the research assistant who actually collected the aesthetic ratings. Similarly, neither the research assistant nor the subjects were informed of the 'intentionality' component of the study. All subjects were tested in a single session, and were instructed to make their judgments independently from those of the other subjects. A total of 62 subjects participated in the aesthetic rating study.

Each of the 18 slide images of cooked rice was presented randomly for 10 seconds. During each presentation period, subjects used a Likert scale (ranging from 0 to 4, with 0 = 'Not Aesthetic', and 4 = 'Very Aesthetic') to rate that image's aesthetic appeal. Following this rating process, subjects were thanked for their participation, completely debriefed, and escorted out of the research lab.

Objective Measure (Biological Study as the Follow-Up Protocol)

In this follow-up experiment, *Escherichia coli* (*E. coli*) was cultured in nutrient broth (dissolving 5 g peptone and 3 g meat extract in 1 L dd H_2O, pH 7, autoclaved at 125 °C for 20 minutes prior use) under different intentions: (i) positive intention (subject to 3 minutes of positive intention daily—praising words); (ii) negative intention (subject to 3 minutes of negative intention daily—mean and hateful words); and (iii) no intention (control group) for 7 days. Approximately 20,000 *E. coli* cells were inoculated into sterilized flasks, each containing 20 ml of sterilized nutrient broth, at day 0 of the experiment. A blank was included in this experiment in which the culture flasks contained only nutrient broth with no *E. coli*. The amount of *E. coli* in each culture flask was counted daily using a hemacytometer for a period of 7 days. All cultures were kept at 27.5 °C throughout the experiment. Due to the following constraining circumstances, this experiment had a duration of only 7 days (rather than 30 days as with the rice samples):

1) *E. coli* were grown in batch culture (i.e. there was no continuous input of nutrients and no removal of wastes and toxins secreted by *E. coli* cells).

2) A typical close batch culture tends to reach maximum cell density in one week and the cells begin to die off soon after due to lack of nutrients and to the buildup of toxic substances.

Statistical Analysis

One-way analysis of variance (ANOVA) was used to test the null hypothesis that intention did not cause significant changes for the aesthetic score. Two-way ANOVA was used to test the null hypotheses that (i) intention did not cause significant changes in *E. coli* growth at each time interval measured within the 7-day incubation period; and (ii) *E. coli* cell density did not change over time within the same treatment group. A significant difference was detected with $p \leq 0.05$. Statistics were performed using the statistical software SigmaPlot 12.5 (Systat, USA) with graphs plotted with GraphPad Prism 7 (GraphPad Software, USA).

Further Remarks about Six Concerns

To introduce the protocols for both measurements more clearly, there is a need to specify six specific concerns:

1) Rice samples in this study got only subjective aesthetic perception ratings. Using them for tracing microbial growth within the experimental period not only would have contaminated them irreversibly, but also would have projected other intentions onto the rice samples, hence affecting the perception results.

2) Biological measures of the rice samples at the end of the experimental period were considered. If done, that would rather have examined the microbial communities in the rice samples. However, this study postulates as others have (e.g., Barry 1968, Haraldsson & Thorsteinsson 1973, Nash 1984) that the rate of microbial growth in terms of cell division was the target of mental intentionality. That said, the identification and characterization of different microbial species in the rice samples are worthy of further investigation since the aesthetics of the cooked rice samples could be associated with different species of microorganisms present.

3) Studying a single species of bacterium in vitro allows the data to be collected and reported with some credibility. In particular, we aimed to prevent the experimenter from creating the "file-drawer" issue in which only positive or desirable effect sizes in the context of bacterial variety—if cooked rice samples were used—were reported. To avoid overestimates or underestimates of effect sizes of microbial cell division, a mono-species of cell cultures was thus employed in vitro (rather than in the rice cultures) as the key element of the followup protocol.

Figure 1. Figures 1a, 1b, and 1c show the growth of microorganisms in positive, negative, and control rice in selected samples, respectively, after 30 days of incubation at 27.5 °C.

4) *E. coli* was chosen as the test species in this study as its presence in food and drinks increases the risks of food poisoning (and death in serious cases). To ensure public safety, the presence of *E. coli* in food and drinking water is closely monitored by the USFDA and the EPA (U.S. Food and Drug Administration 1998, U.S. Environmental Protection Agency 2009) and many other governmental bodies around the world.

5) Whether evidence could be drawn from one to another, namely biological to subjective or vice versa, was a big concern. This issue has been addressed by adopting a "two-way interpretation"—i.e. by interpreting the subjective and biological measures both together and separately. This two-way system allows interpretation of data not purely for the sake of making any inferences from one to another but, as mentioned, also providing a gateway for testing the intentionality hypothesis further.

6) All experiments using these or similar procedures, either biological or not, have been reported in this paper.

Results

Subjective Measure Findings

Microbial growth was observed in all nine rice samples after 30 days of incubation, with/without positive and negative intention (see Figure 1).

The Brown-Forsythe test of variance homogeneity shows that the current dataset contains statistically significant non-equivalent error variances among the three experimental conditions ($p < 0.05$). Because of this, the non-parametric Kruskal-Wallis one-way analysis of variance of sample medians was used to interpret the data, as compared with the more traditional, parametric-bound, one-way analysis of variance of sample means (see Appendix 2).

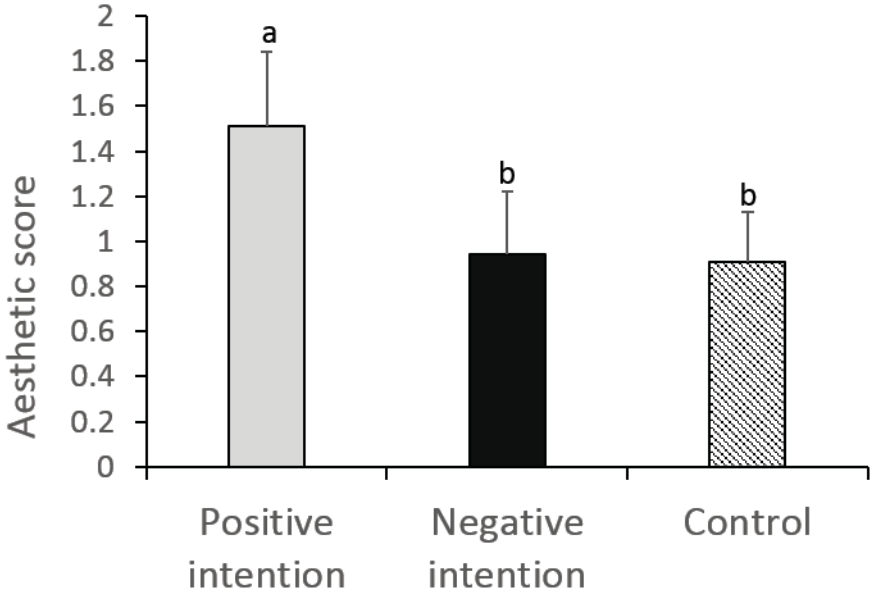

Figure 2. Average scores of aesthetics perceived for all images of rice. Different treatment groups marked with the same letter are not significantly different from one another ($p \geq 0.05$); treatment groups marked with different letters are significantly different from one another ($p \leq 0.05$). Data are expressed as mean ± S.E.M.

The initial analysis of variance shows a statistically significant overall test statistic; $H(2) = 32.12$, $p \leq 0.05$, indicating one or more significant differences between average aesthetic ratings for the three different groups (i.e. positive, negative, and non-intentionality) of cooked rice cultures. A Student-Newman-Keuls post hoc multiple comparison test specifies that the average aesthetic rating for the positive intentionality rice cultures is significantly higher than those obtained for the negative and non-intentionality cultures (Figure 2). No significant difference was found for the aesthetic ratings between the negative and non-intentionality rice cultures.

Biological Measure Findings

Two-way parametric ANOVA was performed to investigate if different intentions caused significant differences in *E. coli* growth in batch culture (Figure 3 and Appendix 3). After incubation for 24 h at 25 °C, *E. coli* cell

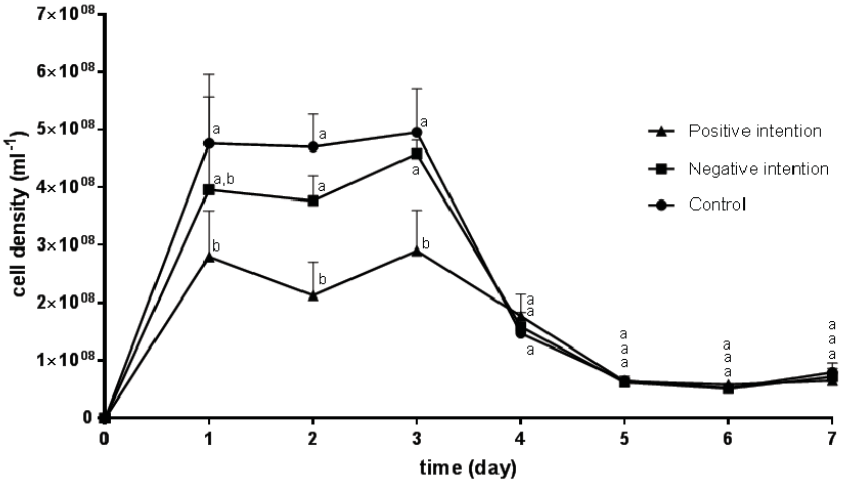

Figure 3. Growth of E. coli in batched culture under different intentions for 7 days. Within each time interval, different treatment groups marked with the same letter are not significantly different from one another ($p \geq 0.05$); treatment groups within the same time interval marked with different letters are significantly different from one another ($p \leq 0.05$). Data are expressed as mean ± S.E.M.

density (cell/ml) was significantly higher in the "control" group than that of the "positive intention" group ($p \leq 0.05$). At day 2 and 3 of the incubation period, significantly higher cell density was observed in both the "control" and "negative intention" groups when compared with the "positive intention" group ($p \leq 0.05$). Cell density among all three groups ("control," "negative," and "positive" intention groups) declined significantly at day 4 of the study, mainly resulting from the build-up of toxins and the run-out of nutrients in the culture medium. No cell was observed in the blank group throughout the experimental period.

Discussion

Searching the Internet, one can find sites touting accounts of an interesting nature with regard to the use of rice claimed to be intentionally treated. While this interest in testing the intention hypothesis is thought to be anecdotal or not abiding by rules of science, our study tapped into this interest and found that there seems to be a plausible relationship between microbial

growth patterns and intentions, i.e. human intentions may inherently affect microbial growth and populations. In this study, the aesthetic ratings for the 'positive' rice group is higher than for the 'negative' and control ones. Seeing the differences, we continued with a follow-up study, which shows that the positive intentionality lowered the growth rate of *E. coli* in that group compared with those of the negative and control groups. That said, it provides some hints on the rice sample study that the higher aesthetic rating of the 'positive' rice cultures might be associated with cell divisions (i.e. microbial changes) of harmful microbes inhibited by positive mental intentionality. However, since this inferential interpretation is based on the assumed premise that cell division modality was the target of mental intentionality, we have yet to examine the diversity of micro-organisms that would also affect the appearance (also known as the aesthetic score) of the 30-day cultured rice samples. As mentioned, this aspect of concern shall be the objective of future research.

More importantly, though, the subjective and biological findings could (or should) also be interpreted separately (i.e. not using the biological measure to make inferences to the subjective one). When interpreted in this way, both approaches separately support the intentional hypothesis in terms of perceived or actual differences. In other words, positive mental intentionality was associated with a higher perceived level of aesthetic ratings in the rice samples and a lower density of *E. coli* in vitro.

If the subjective and biological measures show emerging evidence to demonstrate mind–matter interactions—beyond chance, as indicated in the current study—then the discussions mentioned below are worthy of being explored.

First, it remains too soon to confirm that there has been any interaction between mind and matter simply based on this study. However, it contributes to the extant evidenced-based literature (Radin et al. 2008, Radin, Michel, & Delorme 2016, Pitkanen 2017) arguing that fewer data are found to deny the hypothesis under concern. In its wake, more research testing the hypothesis is warranted.

Second, the authors unexpectedly learned that the subjective and biological measure scores of the control groups, although not significantly different from the 'negative' groups, were the lowest. To recap, the two control groups were the ones treated without any intention, meaning that they were "ignored" at all times. It thus stimulated our thinking about whether being ignored is more detrimental than receiving contemptuous, reprimanding, and disapproval treatments. This result may relate to current studies in psychoimmunology, pointing out that the effects of intentionality on health are not merely from thinking positively or negatively but also

from being ignored (which has the worst outcome).

Third, the influences of mental intentionality on cell growth, divisions, or microbial changes have been topics drawing some attention for research purposes. This study on rice and non-rice conditions seems to warrant more study—particularly for therapeutic concerns on two fronts:

1. *The mind–matter role of intentionality in terms of health*: Microbial changes or growth of micro-organisms are conditions of micro-living systems affecting health. In light of both subjective and biological findings as evidence, this study tends to support other mind–matter studies which involved micro-organisms such as fungi (Barry 1968, Haraldsson & Thorsteinsson 1973, Tedder & Monty 1981) and bacteria (Nash 1982, 1984), etc., thereby suggesting that therapeutic intention for controlling the growth of micro-organisms for health purposes deserves further attention.

2. *Impacts of intentionality*: Intentionality appears to be gaining its evidence-based stance regarding its possible impact on microbial modalities (at least from this study and others mentioned). As such, any research regarding human diseases due to micro-organisms might need to take intentionality into account since it might bring to light certain risk factors for subjects. Whether it should be taken into account also for therapeutic purposes needs further investigation.

Conclusion

This study presents findings that support the "intentionality hypothesis." In addition, it has demonstrated a low-cost and low-technology-to-acquire approach. It proves the point that testing the mental intention hypothesis in living systems contexts could be as inexpensive as the RNE model, and hopefully will thereby encourage more researchers to replicate the approach or to keep testing their interest in finding an approach that is replicable and reproducible.

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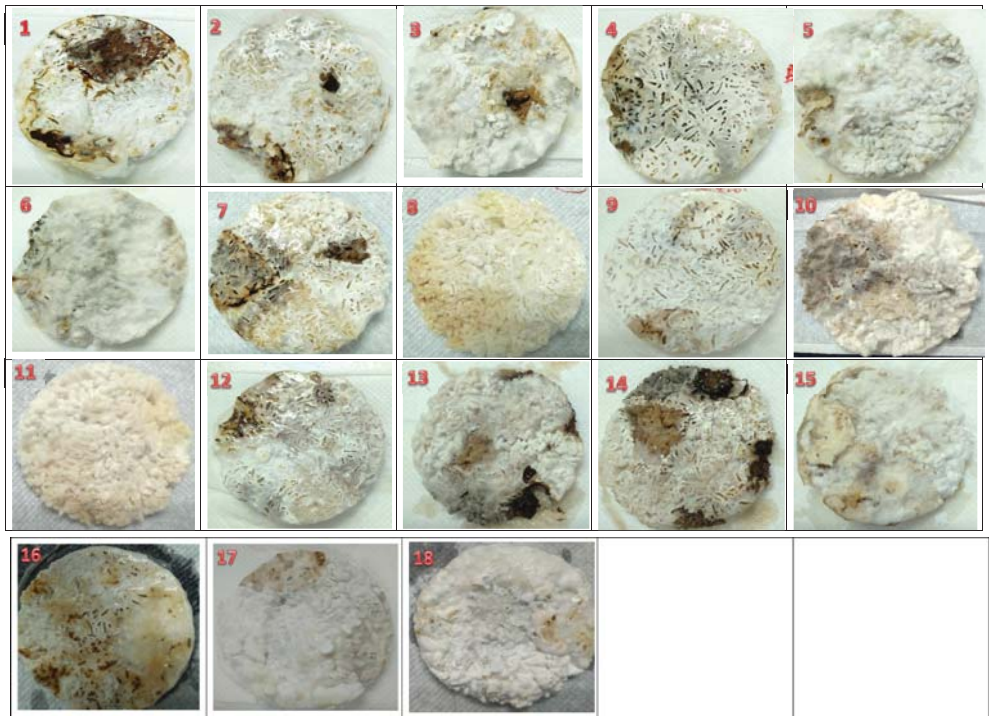
APPENDIX 1

SLIDES OF RICE SAMPLES AFTER REMOVAL FROM PETRI DISHES, USED FOR JUDGING OF AESTHETIC RATINGS BY SUBJECTS



Positive intention: Image numbers:	7, 8, 10, 11, 16, 18
Negative intention: Image numbers:	1, 4, 5, 6, 12, 15
Control "no" intention: Image numbers:	2, 3, 9, 13, 14, 17

Note: Each photo was projected onto the screen by a teaching assistant (who was not informed of the nature of the experiment), and each student had 10 seconds to score the image on a 5-point Likert scale (0 to 4). When rating, the judges independently and blindly assessed the images' overall aesthetic appeal, where 0 meant "not aesthetic" to 4 "very aesthetic."

APPENDIX 2

ONE-WAY ANOVA—RICE AESTHETICS SCORE

Result from Sigma Plot

One-Way Analysis of Variance October 09, 2017, 15:03:55
Data source: Data 1 in Rice experiment
Normality Test (Shapiro-Wilk): Passed ($P = 0.514$)
Equal Variance Test (Brown-Forsythe): Failed ($P < 0.050$)
Test execution ended by user request, ANOVA on Ranks begun

Kruskal-Wallis One-Way Analysis of Variance on Ranks October 09, 2017, 15:03:55
Data source: Data 1 in Rice experiment

Group	N	Missing	Median	25%	75%
Negativity	62	0	0.917	0.500	1.375
Control	62	0	0.917	0.500	1.333
Positivity	62	0	1.500	1.000	2.000

H = 32.118 with 2 degrees of freedom. ($P = <0.001$)

The differences in the median values among the treatment groups are greater than would be expected by chance; there is a statistically significant difference ($P = <0.001$)

To isolate the group or groups that differ from the others, a multiple comparison procedure is used.

All Pairwise Multiple Comparison Procedures (Student-Newman-Keuls Method):

Comparison	Diff of Ranks	q	*P*	*P* < 0.050
Positivity vs Control	3034.000	7.157	<0.001	Yes
Positivity vs Negativity	2817.500	9.956	<0.001	Yes
Negativity vs Control	216.500	0.765	0.589	No

Note: The multiple comparisons on ranks do not include an adjustment for ties.

APPENDIX 3

TWO-WAY ANOVA—E. COLI GROWTH RATE

Two-Way Analysis of Variance August 24, 2018, 11:16:07
Data source: Intentions on *E. coli* growth rate (7-day study)
Balanced Design Dependent Variable: log10(cell density)
Normality Test (Shapiro-Wilk) Passed ($P = 0.081$)
Equal Variance Test: Passed ($P = 0.585$)

Source of Variation	DF	SS	MS	F	*P*
day	7	533.294	76.185	4135.640	<0.001
treatment	2	0.133	0.0663	3.602	0.035
day x treatment	14	0.318	0.0227	1.232	0.285
Residual	48	0.884	0.0184		
Total	71	534.629	7.530		

The difference in the mean values among the different levels of day is greater than would be expected by chance after allowing for effects of differences in treatment. There is a statistically significant difference (P = <0.001). To isolate which group(s) differ from the others, a multiple comparison procedure is used.

The difference in the mean values among the different levels of treatment is greater than would be expected by chance after allowing for effects of differences in day. There is a statistically significant difference (P = 0.035). To isolate which group(s) differ from the others, a multiple comparison procedure is used.

The effect of different levels of day does not depend on what level of treatment is present. There is not a statistically significant interaction between day and treatment. (P = 0.285)

Power of performed test with alpha = 0.0500: for day: 1.000
Power of performed test with alpha = 0.0500: for treatment: 0.485
Power of performed test with alpha = 0.0500: for day x treatment: 0.129

Least square means for day:

Group	Mean
d0	0.000
d1	8.523
d2	8.511
d3	8.594
d4	8.194
d5	7.796
d6	7.721
d7	7.841

Std Err of LS Mean = 0.0452

Least square means for treatment:

Group	Mean
Control	7.196
neg	7.154
pos	7.092

Std Err of LS Mean = 0.0277

Least square means for day x treatment:

Group	Mean
d0 × control	0.000
d0 × neg	0.000
d0 × pos	0.000
d1 × control	8.666
d1 × neg	8.492
d1 × pos	8.411
d2 × control	8.666
d2 × neg	8.570
d2 × pos	8.298
d3 × control	8.683
d3 × neg	8.660
d3 × pos	8.438
d4 x control	8.166

d4 × neg	8.189
d4 × pos	8.227
d5 × control	7.807
d5 × neg	7.790
d5 × pos	7.791
d6 × control	7.705
d6 × neg	7.697
d6 × pos	7.760
d7 × control	7.876
d7 × neg	7.838
d7 × pos	7.808

Std Err of LS Mean = 0.0784

All Pairwise Multiple Comparison Procedures (Duncan's Method):

Comparisons for factor: **day**

Comparison	**Diff of Means**	**p**	**q**	***P***	***P* < 0.050**
d3 vs. d0	8.594	8	189.950	<0.001	Yes
d3 vs. d6	0.873	7	19.297	<0.001	Yes
d3 vs. d5	0.798	6	17.631	<0.001	Yes
d3 vs. d7	0.753	5	16.641	<0.001	Yes
d3 vs. d4	0.400	4	8.842	<0.001	Yes
d3 vs. d2	0.0824	3	1.820	0.231	No
d3 vs. d1	0.0706	2	1.560	0.276	Do Not Test
d1 vs. d0	8.523	7	188.390	<0.001	Yes
d1 vs. d6	0.802	6	17.737	<0.001	Yes
d1 vs. d5	0.727	5	16.072	<0.001	Yes
d1 vs. d7	0.682	4	15.081	<0.001	Yes
d1 vs. d4	0.329	3	7.283	<0.001	Yes
d1 vs. d2	0.0118	2	0.261	0.855	Do Not Test
d2 vs. d0	8.511	6	188.129	<0.001	Yes
d2 vs. d6	0.791	5	17.477	<0.001	Yes
d2 vs. d5	0.715	4	15.811	<0.001	Yes
d2 vs. d7	0.671	3	14.821	<0.001	Yes
d2 vs. d4	0.318	2	7.022	<0.001	Yes
d4 vs. d0	8.194	5	181.107	<0.001	Yes
d4 vs. d6	0.473	4	10.455	<0.001	Yes
d4 vs. d5	0.398	3	8.789	<0.001	Yes
d4 vs. d7	0.353	2	7.799	<0.001	Yes
d7 vs. d0	7.841	4	173.308	<0.001	Yes
d7 vs. d6	0.120	3	2.656	0.081	No
d7 vs. d5	0.0448	2	0.990	0.487	Do Not Test
d5 vs. d0	7.796	3	172.318	<0.001	Yes
d5 vs. d6	0.0754	2	1.666	0.245	Do Not Test
d6 vs. d0	7.721	2	170.653	<0.001	Yes

Comparisons for factor: **treatment**

Comparison	**Diff of Means**	**p**	**q**	***P***	***P* < 0.050**
control vs. pos	0.104	3	3.770	0.014	Yes
control vs. neg	0.0418	2	1.507	0.292	No
neg vs. pos	0.0627	2	2.263	0.116	No

Comparisons for factor: **treatment within d0**

Comparison	Diff of Means	p	q	P	P < 0.05
control vs. pos	0.000	3	0.000	1.000	No
control vs. neg	0.000	2	0.000	1.000	Do Not Test
neg vs. pos	0.000	2	0.000	1.000	Do Not Test

Comparisons for factor: **treatment within d1**

Comparison	Diff of Means	p	q	P	P < 0.05
control vs. pos	0.255	3	3.252	0.033	Yes
control vs. neg	0.174	2	2.227	0.122	No
neg vs. pos	0.0804	2	1.026	0.472	No

Comparisons for factor: **treatment within d2**

Comparison	Diff of Means	p	q	P	P < 0.05
control vs. pos	0.368	3	4.690	0.002	Yes
control vs. neg	0.0957	2	1.221	0.392	No
neg vs. pos	0.272	2	3.469	0.018	Yes

Comparisons for factor: **treatment within d3**

Comparison	Diff of Means	p	q	P	P < 0.05
control vs. pos	0.245	3	3.130	0.040	Yes
control vs. neg	0.0231	2	0.294	0.836	No
neg vs. pos	0.222	2	2.835	0.050	Yes

Comparisons for factor: **treatment within d4**

Comparison	Diff of Means	p	q	P	P < 0.05
pos vs. control	0.0612	3	0.781	0.608	No
pos vs. neg	0.0382	2	0.487	0.732	Do Not Test
neg vs. control	0.0230	2	0.294	0.836	Do Not Test

Comparisons for factor: **treatment within d5**

Comparison	Diff of Means	p	q	P	P < 0.05
control vs. neg	0.0175	3	0.223	0.884	No
control vs. pos	0.0161	2	0.205	0.885	Do Not Test
pos vs. neg	0.00139	2	0.0177	0.990	Do Not Test

Comparisons for factor: **treatment within d6**

Comparison	Diff of Means	p	q	P	P < 0.05
pos vs. neg	0.0629	3	0.803	0.597	No
pos vs. control	0.0550	2	0.702	0.622	Do Not Test
control vs. neg	0.00796	2	0.102	0.943	Do Not Test

Comparisons for factor: **treatment within d7**

Comparison	Diff of Means	p	q	P	P < 0.05
control vs. pos	0.0681	3	0.870	0.567	No
control vs. neg	0.0384	2	0.490	0.731	Do Not Test
neg vs. pos	0.0298	2	0.380	0.789	Do Not Test

Comparisons for factor: **day within control**

Comparison	Diff of Means	p	q	P	P < 0.05
d3 vs. d0	8.683	8	110.809	<0.001	Yes
d3 vs. d6	0.978	7	12.483	<0.001	Yes

d3 vs. d5	0.876	6	11.178	<0.001	Yes
d3 vs. d7	0.807	5	10.296	<0.001	Yes
d3 vs. d4	0.518	4	6.605	<0.001	Yes
d3 vs. d2	0.0174	3	0.222	0.884	No
d3 vs. d1	0.0169	2	0.216	0.880	Do Not Test
d1 vs. d0	8.666	7	110.593	<0.001	Yes
d1 vs. d6	0.961	6	12.267	<0.001	Yes
d1 vs. d5	0.859	5	10.963	<0.001	Yes
d1 vs. d7	0.790	4	10.081	<0.001	Yes
d1 vs. d4	0.501	3	6.389	<0.001	Yes
d1 vs. d2	0.000503	2	0.00641	0.996	Do Not Test
d2 vs. d0	8.666	6	110.587	<0.001	Yes
d2 vs. d6	0.961	5	12.261	<0.001	Yes
d2 vs. d5	0.859	4	10.956	<0.001	Yes
d2 vs. d7	0.789	3	10.074	<0.001	Yes
d2 vs. d4	0.500	2	6.383	<0.001	Yes
d4 vs. d0	8.166	5	104.204	<0.001	Yes
d4 vs. d6	0.461	4	5.878	<0.001	Yes
d4 vs. d5	0.358	3	4.573	0.003	Yes
d4 vs. d7	0.289	2	3.691	0.012	Yes
d7 vs. d0	7.876	4	100.513	<0.001	Yes
d7 vs. d6	0.171	3	2.186	0.151	No
d7 vs. d5	0.0691	2	0.882	0.536	Do Not Test
d5 vs. d0	7.807	3	99.631	<0.001	Yes
d5 vs. d6	0.102	2	1.304	0.361	Do Not Test
d6 vs. d0	7.705	2	98.326	<0.001	Yes

Comparisons for factor: **day within negative**

Comparison	**Diff of Means**	**p**	**q**	***P***	***P* < 0.05**
d3 vs. d0	8.660	8	110.514	<0.001	Yes
d3 vs. d6	0.963	7	12.290	<0.001	Yes
d3 vs. d5	0.870	6	11.107	<0.001	Yes
d3 vs. d7	0.822	5	10.491	<0.001	Yes
d3 vs. d4	0.471	4	6.016	<0.001	Yes
d3 vs. d1	0.168	3	2.148	0.158	No
d3 vs. d2	0.0900	2	1.149	0.421	Do Not Test
d2 vs. d0	8.570	7	109.365	<0.001	Yes
d2 vs. d6	0.873	6	11.141	<0.001	Yes
d2 vs. d5	0.780	5	9.958	<0.001	Yes
d2 vs. d7	0.732	4	9.342	<0.001	Yes
d2 vs. d4	0.381	3	4.867	0.002	Yes
d2 vs. d1	0.0783	2	0.999	0.483	Do Not Test
d1 vs. d0	8.492	6	108.366	<0.001	Yes
d1 vs. d6	0.795	5	10.142	<0.001	Yes
d1 vs. d5	0.702	4	8.959	<0.001	Yes
d1 vs. d7	0.654	3	8.343	<0.001	Yes
d1 vs. d4	0.303	2	3.868	0.009	Yes
d4 vs. d0	8.189	5	104.498	<0.001	Yes
d4 vs. d6	0.492	4	6.273	<0.001	Yes
d4 vs. d5	0.399	3	5.090	0.001	Yes
d4 vs. d7	0.351	2	4.475	0.003	Yes
d7 vs. d0	7.838	4	100.023	<0.001	Yes

d7 vs. d6	0.141	3	1.798	0.237	No
d7 vs. d5	0.0482	2	0.615	0.666	Do Not Test
d5 vs. d0	7.790	3	99.408	<0.001	Yes
d5 vs. d6	0.0927	2	1.183	0.407	Do Not Test
d6 vs. d0	7.697	2	98.225	<0.001	Yes

Comparisons for factor: **day within positive**

Comparison	**Diff of Means**	**p**	**q**	***P***	***P* < 0.05**
d3 vs. d0	8.438	8	107.679	<0.001	Yes
d3 vs. d6	0.678	7	8.651	<0.001	Yes
d3 vs. d5	0.647	6	8.254	<0.001	Yes
d3 vs. d7	0.630	5	8.036	<0.001	Yes
d3 vs. d4	0.211	4	2.694	0.087	No
d3 vs. d2	0.140	3	1.782	0.241	Do Not Test
d3 vs. d1	0.0265	2	0.338	0.812	Do Not Test
d1 vs. d0	8.411	7	107.341	<0.001	Yes
d1 vs. d6	0.651	6	8.313	<0.001	Yes
d1 vs. d5	0.620	5	7.916	<0.001	Yes
d1 vs. d7	0.603	4	7.698	<0.001	Yes
d1 vs. d4	0.185	3	2.356	0.122	Do Not Test
d1 vs. d2	0.113	2	1.444	0.312	Do Not Test
d2 vs. d0	8.298	6	105.897	<0.001	Yes
d2 vs. d6	0.538	5	6.869	<0.001	Yes
d2 vs. d5	0.507	4	6.472	<0.001	Yes
d2 vs. d7	0.490	3	6.254	<0.001	Yes
d2 vs. d4	0.0715	2	0.912	0.522	Do Not Test
d4 vs. d0	8.227	5	104.985	<0.001	Yes
d4 vs. d6	0.467	4	5.957	<0.001	Yes
d4 vs. d5	0.436	3	5.559	<0.001	Yes
d4 vs. d7	0.419	2	5.342	<0.001	Yes
d7 vs. d0	7.808	4	99.643	<0.001	Yes
d7 vs. d6	0.0482	3	0.615	0.686	No
d7 vs. d5	0.0171	2	0.218	0.878	Do Not Test
d5 vs. d0	7.791	3	99.425	<0.001	Yes
d5 vs. d6	0.0312	2	0.398	0.780	Do Not Test
d6 vs. d0	7.760	2	99.028	<0.001	Yes

A result of "Do Not Test" occurs for a comparison when no significant difference is found between two means that enclose that comparison. For example, if you had four means sorted in order, and found no difference between means 4 vs. 2, then you would not test 4 vs. 3 and 3 vs. 2, but still test 4 vs. 1 and 3 vs. 1 (4 vs. 3 and 3 vs. 2 are enclosed by 4 vs. 2: 4 3 2 1). Note that not testing the enclosed means is a procedural rule, and a result of Do Not Test should be treated as if there is no significant difference between the means, even though one may appear to exist.

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RESEARCH ARTICLE

Brief Research: A Follow-Up Study on Unusual Perceptual Experiences in Hospital Settings Related by Nurses

Alejandro Parra

Instituto de Psicología Paranormal
rapp@fibertel.com.ar

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Abstract—The aim of this study was to determine the degree of occurrence of certain unusual perceptual experiences in hospital settings often related by nurses, in a follow-up study at 36 hospitals and health centers in Buenos Aires. 344 nurses were grouped as 235 experiencers and 109 non-experiencers. The most common experiences are sense of presence and/or apparitions, hearing noises, voices or dialogues, crying or complaining, and intuitions and extrasensory experiences as listeners of the experiences of their patients, such as near-death experiences, religious interventions, and many anomalous experiences in relation with children (Parra & Giménez Amarilla 2017).

Introduction

It is important to note that the findings related to anomalous experiences by nurses (Barbato et al. 1999, O'Connor 2003) have also been reported by doctors (Osis & Haraldsson 1977, 1997) and other caregivers in hospital settings (Brayne, Farnham, & Fenwick 2006, Katz & Payne 2003, Kellehear 2003, Fenwick & Fenwick 2008), and in care homes (Katz & Payne 2003) around a number of anomalous events such as deathbed visions (Barret 1926, Betty 2006, Brayne, Farnham, & Fenwick 2006, Brayne, Lovelace, & Fenwick 2006).

A previous, up-to-date study on anomalous experiences in hospital settings (Parra & Giménez Amarilla 2017), recruited nurses (n = 100) who reported a number of anomalous experiences from one health center in Buenos Aires, Argentina. Four potential traits that could "modulate" anomalous experiences in nurses are work stress, hallucination proneness, heightened attentional capacities (psychological absorption) associated with

a number of anomalous experiences, and "openness" to such experiences. It could be argued that the psychological pressure of the working conditions of nurses triggers such anomalous perceptual experiences, but a comparison between nurse experiencers and a control group in terms of work stress (i.e. nurses who reported these experiences tended to experience greater work-related stress) was not confirmed (Parra & Giménez Amarilla 2017).

Parra and Giménez Amarilla (2017) also found that those who reported a combination of unusual perceptual experiences and a high level of psychological absorption—a state of heightened imaginative involvement in which an individual's attentional capacities are focused in one behavioral domain (Tellegen & Atkinson 1974)—tended to score higher for anomalous experiences compared with those who did not report such experiences. In fact, a better predictor than work stress and hallucination-proneness was psychological absorption in experiencers compared with the control group (Parra 2015, Parra & Argibay 2012). Of the 100 nurses they surveyed, 61 of them reported having had at least one anomalous experience in a hospital setting, the most common feelings reported were a sense of "presence" (30%), hearing noises and finding no source (17%), and knowing intuitively what was wrong with a patient without knowing their medical history (14%), patients with near-death experiences (19%), patients recovering quickly and completely from disease after religious intervention (e.g., prayer group) (18%), and anomalous experiences where children were involved (15%).

The present study represents a replication utilizing a larger number of nurses from a wider range of hospitals and health centers in Buenos Aires. The main aim was to determine frequency and percentage of anomalous experiences across multiple hospital/health centers instead of from just one.

Methods

Participants

A total of 450 questionnaires were sent to nurses in 36 different hospitals and health service departments. Of these, 344 (76%) usable questionnaires were returned. The nurse participants were recruited with the cooperation of their Research and Teaching areas of the Nursing Department of each (the Principal Nursing Officers). They gave us permission to administer the set of questionnaires, which were distributed through the Nursing Officers to each nurse in the hospital, depending on the number of nurses working in each hospital and health center (Mean = 100; Rank = 5 to 300 per hospital approximately).

The Nursing Officers verbally explained the research to each nurse on all shifts. The Nursing Officers were also contacted through announcements

in hospitals and the Internet (briefly stating the main aims of the research), and many nurses interested in spiritual/paranormal topics contacted the Nursing Officers in their hospital, some of whom also showed interest in the topic. Another group of Nursing Officers also was contacted by the researcher who briefly stated the main aims of the study, but no hypothesis was given to the nurses employed in the hospitals. All nurses completed the questionnaires in isolation and they returned them at the same time (the time to complete the set of questions was forty minutes). Some nurses (n = 80) were also recruited from courses and seminars through nursing schools and health centers seminars, where the questionnaires were completed in a classroom setting with the permission of their teachers and directors.

Consent Form

The set of questionnaires included a consent form. The nurse participants were informed that we were recruiting information on anomalous and/or spiritual experiences and they signed an appropriate consent form. They all received significant information about the procedure and were free to decline to participate. All data collected were treated confidentially.

Categorization Procedure

The following criteria were used to split the sample into two groups: Nurses who indicated "one time" and/or "multiples times" for (at least) one of the 13 items were categorized as the Nurse Experiencers "NEs" group (n = 235), and Nurses who indicated "never" for all 13 items were categorized as the "Control" group (n = 109). Eight items of the *Anomalous Experiences in Nurse & Health Workers Survey* were used to create an Index of total experiences, that is, nurses as anomalous experiencers themselves, but nurses as listeners of the experiences from patients and other nurses were excluded (five items).

Participants

Nurse experiencers (NE). The sample consisted of 235 nurses, of which 183 (78%) were female and 52 (22%) were male. The age range was 19 to 68 years (Mean = 39.19 years; SD = 11.15 years). Nurses scored a mean of 11 years in their work in hospitals (Range = 1 to 48 years; SD = 10.52). 39 (16.6%) of them worked a morning shift, 51 (21.7 %) an afternoon shift, and 45 (19.1%) the night shift (just 6 work in two shifts, 2.6%). 66 (31.8%) worked in other shift modes such as continuous shift (37, 15.7%) and weekend shift (29, 12.3%), and 28 did not mention the shift (11.9%). The main work areas surveyed were patient rooms (24.3%), guard station

(13.2%), intensive care ward (22.1%), neonatology (7.7%), others (22.2%, i.e. ambulances, surgery, etc.), and undefined by the respondent (8.9%).

Nurse controls, nonexperiencer nurses (NC). The sample consisted of 109 nurses of which 89 (81.7%) were female and 20 (18.3%) were male. The age range was 19 to 69 years (Mean = 38.94 years; SD = 11.62). These nurses scored a mean of 9 years in their work (Range = 1 to 39 years; SD = 8.97). 21 (19.3%) worked a morning shift, 16 (14.7%) an afternoon shift, and 27 (24.8%) a night shift (just 1 worked in two shifts, 0.9%). 28 (25.7%) worked other shifts, such as continuous shift (15, 13.8%) and weekend shift (13, 11.9%). 16 did not mention their shift (14.7%). The main work areas were patient rooms (28.4%), guard station (15.6%), intensive care ward (16.5%), neonatology (10.1%), others (18.1%, i.e. ambulances, surgery, etc.), and undefined by the respondent (11%).

Anomalous Experiences in Nurse & Health Workers Survey

The Anomalous Experiences in Nurse & Health Workers Survey was used, which is a self-report that has 13 yes/no items designed (Cronbach's alpha = .78) following a previous study (Parra & Giménez Amarilla 2017). Items of anomalous (or spiritual) experiences during hospitalization include sense of presence and/or apparition, floating lights, or luminescences, hearing strange noises, voices or dialogues, crying or moaning, seeing energy fields, lights, or "electric shock" around or coming out of an inpatient, etc. Other indications might include having an extrasensory experience, a malfunction of equipment or medical instrument with certain patients, or a spiritual form of intervention (e.g., prayer groups, laying on of hands, rites, images being blessed).

The survey also evaluates age, length of service, shift (morning, afternoon, or night), hospital area (patient rooms, guard station, intensive care, neonatology, others), and name of institution (confidential). Email or phone information was optional.

The questions were also split into two types: Type 1: Nurses as listeners to the anomalous experiences from patients (i.e. near-death or out-of-body experiences) and from other (trustworthy) nurses (items 1, 2, 6, 12, and 13), and Type 2: Nurses as experiencers themselves of anomalous experiences (items 3, 4, 5, 7, 8, 9, 10, and 11). This separation is important to provide information to help us understand anomalous experiences.

Results

The most common anomalous experiences reported under Type 2 (as experiencers) are sense of presence or apparitions (28.8%), hearing strange noises, voices or dialogues, crying or complaining (27%), knowing

the patient's disease intuitively (20.6%). Under Type 1, the rankings of experiences as listeners of experiences of their patients were: near-death experiences (25.6%), religious intervention (20.1%), and anomalous experiences in relation with children (12.2%) (see Table 1).

TABLE 1
Of 235 Nurses Who Reported Anomalous Experiences, the Number and Percentage Who Answered Yes to These Questions

Item	Type	Question	N	%
#1	1	Patients admitted to my clinic have reported near-death experiences (or something similar) during hospitalization or during clinical interventions (e.g., surgery).	88	25.6
#2	1	Patients in my health center have reported out-of-body experiences.	33	9.6
#3	2	During intensive therapy, I witnessed events of the kind with a sense of "presence," an apparition, floating lights or luminescence, or unexplained movements of objects.	99	28.8
#4	2	In my clinic, I witnessed events such as hearing strange noises, voices or dialogues, crying or moaning, and found no source for them.	93	27.0
#5	2	In my clinic, I had the experience of seeing energy fields, lights, or "shock" around, or coming from, a hospitalized patient.	20	5.8
#6	1	Patients admitted to my clinic have reported extrasensory experiences (for example, knowing things about people or situations that they could not know because they were interned and isolated).	27	7.8
#7	2	I have had a strange experience such as knowing about the situation of a patient I had seen in in my clinic while being at home, or on vacation.	47	13.7
#8	2	In my clinic, I have had the experience of seeing medical equipment failing consistently with certain patients while not with others.	28	8.1
#9	1	In my clinic, I observed that after some form of intervention (e.g., prayer groups, laying on of hands, rites, or objects, images of beatified saints, rosaries), some patients recovered quickly and completely from disease and/or trauma.	69	20.1
#10	2	I have had the experience of "knowing" intuitively what is wrong with a patient just by seeing him/her, or even before, or even without knowing his/her medical history.	71	20.6
#11	2	I had an experience that could be defined as "mystical" or a special "connection" in the context of my clinic.	27	7.8
#12	1	I have heard of, or met, trusted peers who have witnessed experiences like the ones above, in a medical context only.	104	30.2
#13	1	In my clinic, I witnessed unexplained events in relation to children.	42	12.2

Discussion

The results showed that of the 235 nurses who reported having had at least one anomalous experience in a hospital setting, the most common anomalous experiences reported as experiencers, are sense of presence or apparitions, hearing strange noises, voices or dialogues, crying or complaining, and knowing the disease intuitively; and as listeners of experiences of their patients/peers, near-death experiences, religious intervention, and anomalous experiences in relation with children.

Hence, in the context of this study, the distinction between purely subjective experiences and those considered paranormal (veridical) is irrelevant. Even veridical experiences may depend on the same psychological predispositional factors as do non-veridical experiences. A high prevalence of anomalous experiences in nurses at work could lead them toward acceptance of the voices sometimes reported by clients. Nurses may listen to these experiences and seek to understand them by perceiving them as similar to their own, rather than fundamentally different, incomprehensible, or even schizophrenic. It could lead nurses to explore where, when, and how the experiences took place. As nurses have anomalous experiences, too, professionals can begin to understand the experience as not inherently bad and in need of elimination—rather, it is a common experience that we can accept and try to make sense of.

Generally speaking, there are a number of drawbacks connected with this research in hospital settings as they are conservative institutions, unlikely to be open about their population and even more so with respect to providing information relating to the subject of this investigation. The nurses did reveal their personal and professional experiences and those of their patients, noting that they considered experiences of paranormal phenomena within a hospital setting not to be infrequent or unexpected. They were not frightened by their patients' experiences, or their own, and exhibited a quiet confidence about the reality of the experiences for themselves and for the patient or dying person. Acceptance of these experiences, without interpretation or explanation, characterized their responses.

By reassuring them that the occurrence of paranormal phenomena is not uncommon and is often comforting to the dying person, we may assist nurses to be instrumental in normalizing a potentially misunderstood and frightening experience. There is evidence that the sensed presence is a common concomitant of sleep paralysis particularly associated with visual, auditory, and tactile hallucinations, as well as intense fear. Recent surveys suggest that approximately 30% of young adults report some experience of sleep paralysis (Cheyne, Newby-Clark, & Rueffer 1999, Fukuda et al. 1998,

Spanos et al. 1995). Visions of ghosts may be related to cognitive processes involving fantasy and cognitive perceptual schizotypy proneness, which are correlated with each other (Parra 2006).

Although the recruiting procedure of the survey was voluntary instead mandatory, it might be skewed toward people with more interest in the subject, particularly as a result of their own experiences. However, future studies will be conducted using a qualitative study to explore palliative care nurses' experiences, to reflect on the influence of these experiences on the care of dying patients and their families and friends, and to contribute to the limited nursing literature on the topic. The response of health professionals, specifically nurses, to anomalous experiences is an area not widely reported (Kellehear 2003). Even palliative care literature is mostly silent on this topic. Indeed, the study of anomalous experiences is an area of much contention in many fields.

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COMMENTARY

Experimental Replicability

Henry H. Bauer

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Editorials in the last two issues of this *Journal* have discussed flaws inherent in the notion that observations or measurements must be reproducible or replicable if they are to be accepted as properly scientific. The arguments and examples are compelling, and I would like to add more emphasis to one of the points that the Editorials touch on implicitly more than explicitly. That point is that human beings differ from one another in innumerable ways, as well as to varying degrees in those capabilities that they may have in common; even more important, human beings are not exactly the same ***from moment to moment***—they are inherently unreproducible.

To demand reproducibility in observations or measurements presumes that what is being studied is itself so stable or regular as to be able to reproduce itself on demand, at least in principle. That is simply not the case with phenomena involving human beings; indeed, it is not the case for phenomena that concern the behaviors of any living systems.

Demanding that being scientific requires observations and measurements to be replicable would therefore bar, from being accepted as scientific, not only parapsychology but all of medical science and social science.

A related and no less basic issue concerns what type of knowledge is even attainable. In chemistry and physics, dealing with inanimate objects that are identical with one another and that do not change over time, it has been possible to derive quantitative universal laws and fundamental constants that are Nature-given: Planck's constant, the speed of light, and so forth. Furthermore, the degree of attainable replicability and the limited number of relevant variables are such that it is often possible to identify specific, often unique, causes of an observed phenomenon. By contrast, in the social and behavioral and medical sciences, the attainable knowledge is of a statistical nature, whereby the attempt to identify causes is fraught with all sorts of difficulties: Association or correlation never proves causation.

In my view, the current fuss about failures of replication in medical science and in the social sciences misses the point, which is that we want knowledge of definitive causes, even though that sort of knowledge is often simply not attainable in the medical and social sciences to the degree that has often been gained in physics and chemistry; that's not because of any failure of researchers to perform properly reproducible studies, but because of the nature of what is being studied.

These points are treated in more detail in my recently published book *Science Is Not What You Think: How It Has Changed, Why We Can't Trust It, How It Can Be Fixed* (McFarland Publishing, 2017; https://mcfarlandbooks.com/product/science-is-not-what-you-think/)

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HISTORICAL PERSPECTIVE

Table Turning in the Early 1850s: The Séance Reports of Agénor de Gasparin

Carlos S. Alvarado

Parapsychology Foundation
carlos@theazire.org

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Abstract—The phenomena of table turning flourished during the 1850s, providing for many people a context for belief in spirit action, and for the development of explanations such as unconscious muscular movements and the exteriorization of nervous forces from the sitters. This paper consists of the presentation of excerpts from the classic study of these phenomena by Agénor de Gasparin, who reported his work on the subject in his book *Des Tables Tournantes* (1854, 2 volumes, translated into English in 1857). De Gasparin believed that unconscious muscular action could not explain the movements of tables, and postulated the emission of a force from the sitters around the table to account for the movements. I present a long excerpt from de Gasparin's book in which he described the phenomena he obtained, preceded by a short review of interest in table phenomena in the 1850s, and followed by critiques showing the general skepticism about these phenomena during and after de Gasparin's lifetime.

Keywords: Agénor de Gasparin—table turning—table levitation—unconscious movements—nervous forces

Most people shrug their shoulders when we speak to them of Turning Tables, but they find it very simple to believe in the infallibility of the electric telegraph, and in the fact that physical and moral resemblances are transmitted from them to their children! The tables could not escape the common fate. Absurd to-day and evident to-morrow, they will have their theory, a theory, scientific and official, before which I respectfully bow in advance.

(Agénor de Gasparin 1857:Volume 1:99)

Writing in June 1853 in the *Illustrated London News*, an anonymous author mentioned a delusion threatening to become epidemic: "Thousands of people in Europe and America are turning tables, and obstinately refusing to believe that physical and mechanical means are in any way concerned in the process . . ." (Anonymous 1853a:481). Another commentator, physician and mesmerist John Elliotson (1791–1868), wrote: "Every body now sees the tables turn in his own dwelling, be it Buckingham Palace or a room which serves for kitchen and parlour and all . . ." (Elliotson 1853:191).

These were references to the appeal table turning held for many in some societies. The practice was, in fact, one of the most influential ones in the development of middle Nineteeth-Century belief in Spiritualism, and one that led a writer to state that the phenomenon "is as well-established as any fact in history or science" (Dibdin circa 1853:2).

The purpose of this paper is to reprint descriptions of table turning séances, those published by Agénor de Gasparin (1854; English translation and later abridged edition: de Gasparin 1857, 1889). My purpose is not to discuss their evidential value today, but to remind current students of this phenomenon, and of this important pioneering work and its importance in psychic history. While these séances have been discussed in various historical overview works (e.g., Evrard 2016:Chapter 2, Gutierez & Maillard 2004:20–23, Inglis 1992:218–219, Podmore 1902:Volume 2:187–188), de Gasparin's work does not seem to be well-known today, and has not been cited by more recent writers covering the topic of physical phenomena (e.g., Nicol 1977, Robinson 1981; for an exception see Gimeno 2015). Furthermore, these studies deserve to be better-known because they were very influential at the time they appeared. Although there are many reports and discussions of table phenomena throughout the late Nineteenth Century and later (Willin 2015), I focus my comments in most of this paper to material published in the 1850s. I extend the later discussion at the end of this paper to the reception of the work in later periods.

Table Turning in the 1850s

Table turning, also known as table moving, table talking, table tipping, dancing tables, and by various other names, was widely discussed during the Nineteenth Century to the point that in some places it became a fashionable social practice.[1] In addition to being entertaining, in some places the tables had fit into the prevailing political and religious contexts (Monroe 2008). The practice spread fast in the United States and in Europe for many reasons. Furthermore, it may be argued that the terrain for such acceptance had been prepared by highly publicized accounts of unusual physical phenomena. These inluded physical effects from the early literature of

American Spiritualism (Capron 1855, Capron & Barron 1850), and reports of poltergeist-type cases.[2]

Here is a fairly typical account of table turning:

Table Turning

> I witnessed at the house of an American gentleman in Paris, a series of experiments . . . It may be proper to say that some fifty persons were present . . . , In the first place, a light mahogany tea-table, with six legs, was placed on the waxed floor of the saloon, and the palms of the hands of four persons (two ladies and two gentlemen) were placed upon it . . . In three minutes, the table cracked, undulated, and then moved on being directed by the will of one of the party; it moved along the floor slowly or rapidly to the right or left, forward or backward, when thus directed it also rose on two legs, and resisted strong pressure before it would come down. While standing on two legs, it also turned round to the right and the left, as directed by the will. (Anonymous no date:4)

But not all the effects were purely physical. Some presented veridical information, as in the following account:

> The writer has seen a gentleman enter a room who was a perfect stranger to the medium, and ask if the table would spell his name. The alphabet was called over, the table tipping to the different letters which spelt his proper name. It then spelt, in the same manner the name of his deceased sister, the name of the disease with which she died, told many events in her life, &c. (Wharton 1853)

According to a commentator, lawyer André Saturnin Morin (1807–1888), the behavior and tone of the communications produced by the tables varied greatly. Some were graceful and novel, others presented platitudes. They showed gaiety, mocking, and solemnity, and could be bad-tempered and pedantic. Similarly, they presented various political and religious views (Morin 1860:354–355). This, and other issues such as knowledge of literature, was in Morin's view a function of the composition of the circle. In

his view, communications were "in harmony with the ideas of the operators and it [the table] does not express knowledge other than that belonging to the latter" (p. 359; this, and other translations, are mine).

In some communications, such as those the famous writer Victor Hugo (1802–1885) and his family and friends had in Jersey in the early 1850s, the tables produced messages from communicators identifying themselves as Galileo, Molière, Mozart, and Shakespeare, among others. There were also more abstract communicators who called themselves The Drama, The Idea, and the Shadow of the Sepulcher (Simon 1923).

Many writers believed that spirit agency accounted for table phenomena, be it Satan (Godfrey 1853) or deceased spirits. In a defense of the latter, it was argued that spirits of the dead could charge the table with their perispirit, a vital principle allowing the spirit to cause intelligent movements of a table (and other objects) (Kardec 1862).

The previous idea is a variant of the concept of a force coming out of the body of mediums and sitters, kind of a nervous force that some believed was unrelated to spirits. This idea was proposed by many and was in a way an extension of the concept of animal magnetism (e.g., Charpignon 1848, Deleuze 1813).[3] In fact, several writers discussing mesmerism included table turning in their writings (e.g., Baragnon 1853, Bersot 1864, Gathy 1853). One of them wrote that, in his opinion, table turning was "an undeniable magnetic phenomenon" (Baragnon 1853:358), while another stated that the magnetic agent affecting tables was the same principle that was used to magnetize people (Du Potet 1853:581).

Within the context of American Spiritualism, this force was discussed:

> Tables are *moved* by a mysterious power, when a circle of interested spectators, with a medium, are seated around it . . . Stretch forth your arm, and grasp a heavy weight and raise it. How mighty that power put forth! Trace it back to its origin, and how wonderful! You *willed* to perform that act. Instantly in your brain, as in a Leyden jar, a nervous influence was generated, which, coursing along your nerves as on metallic wires, entered your muscles and there the mere shrinking of the fibres of a little muscle, the shortening of a small cord, drew up the large weight in your hand. How immeasurable, how unaccountable, such a power! And now think of that circle around the table. When they first sit calmly down, no movement is seen; none can be produced. But when for a few moments in intense mental action, a nervous energy has been generated in the frame of each, until, like a circle of Leyden jars, a whole battery is surcharged, and there are negatives as well as positives in the circle, who can wonder if currents of nervous influence should leap over from one to the other, and if tables, chairs, or anything else intervening, should be moved? We should not wonder at any phenomena which might show themselves under such circumstances.

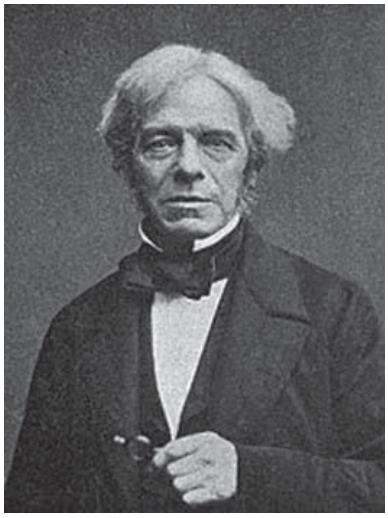
> We should only fear that, like inexperienced experimenters in electricity, we should thoughtlessly inflict upon ourselves an incurable injury . . . If I overcharge myself with it, if I strain the vital organs which generate it, I may weaken my own energies for life. (Oldfield 1852:37–38)

As we shall see, de Gasparin wrote along similar lines. In addition to Oldfied (the pseudonym of George W. Samson [1819–1896]), many others discussed similar concepts of force during the period in question. This was the case of Marc Thury (1822–1905) (Thury 1855) and others (Rogers 1853, Mahan 1855)[4]

A somewhat later example from France was Alphonse Chevillard, a professor at the École des Beaux-Arts. He argued that when the medium's magnetic fluid charged the table, this acted like a limb of the body, obeying the medium's will (Chevillard 1869:15). This idea was also discussed by de Gasparin.

In addition to articles in journals dedicated to psychic phenomena (e.g., Kardec 1859, Gathy 1853), there were discussions in the general press (e.g., de Gasparin 1853b), some of which were humorous. This was the case of cartoons and other humorous writings published in France (Doré & Paulin 1853; see also Monroe 2008:22–26). In France, a commentator wrote that the tables are not only trained to talk but they beat people who do not accept their statements (Kendall 1853).

Furthermore, some discussions of table turning were published in medical and science journals (e.g., Anonymous 1853b, Seguin 1853). In Spain a physician justified the inclusion of the topic in the *Boletín de Medicina, Cirugía y Farmacia* saying that turning tables may have important theoretical implications for physiology, pathology, and therapeutics (D 1853).



Michael Faraday

Perhaps the most famous publication by a scientist was that of English physicist Michael Faraday (1791–1867), who conducted tests that suggested that the movement of tables observed was caused by small unconscious movements (Faraday 1853a),[5] an idea defended as well by several others (e.g., Babinet 1854, Carpenter 1853, Chevreul 1854, Orioli 1853).

One version of this idea, with some psychological concepts, was that of Scottish physician James Braid (1795–1860). He postulated that various phenomena could be produced provided there was a state of concentration opening the person to the influence of suggestion and dominant ideas. He wrote:

> Thus, let the mind of the person be engrossed with the notion, that he is to be irresistibly drawn, repelled, or paralysed, or catalepsed, and the ideo-dynamic or ideational condition of the muscles corresponding to this idea will take place, without any conscious volition of the subject to that effect. It is this very ideational or unconscious muscular action which is the cause of "Table-moving," . . . The experimenters perceive the fact that the table moves; but not being conscious of putting out any voluntary effort, they imagine that the table is drawing them, whilst all the while their own muscles are imparting the requisite impulse to the table, although they are unconscious that they are doing so. (Braid 1853:38)

Such ideas of unconscious movements were developed in opposition to explanations involving spirits and psychic forces of various kinds. But they were in turn criticized as well by others. For example, one writer considered ideas of unconscious movements to be "ingeniously ridiculous explanations" (Morin 1854:676). This was probably because proponents of unconscious movements ignored the various reports of movements of tables without physical contact (for some examples of phenomena, see Capron & Barron 1850:6, Capron 1855:349, Edmonds & Dexter 1853:426, Hare 1855:46).[6]

Interestingly, and one of the reasons to reprint de Gasparin's work in this article, is that most of the literature on table turning consists of discussions of the topic or informal accounts of séances. Not many individuals conducted what may be described as tests with some controls. In addition to the studies of Faraday and de Gasparin, there were other tests. These include the accounts of Thury (1855) and a few others (e.g., Delgras 1853, Orioli 1853, Terzaghi 1853).

Agénor de Gasparin and Table Turning

The son of Count Adrien de Gasparin (1783–1862), Minister of the Interior in France, statesman and author Count Agénor Etiénne de Gasparin (1810–1871), was born in Orange, France, and later lived in Switzerland.[7] His mother was Adèle de Daunant (1784–1834), and he married Valérie Boissier (1813–1894), a well-known writer on social and religious topics (Gilman, Peck, & Colby 1907:471).

An obituarist referred to him as a noble and chivalrous man who showed "grace that charmed his adversaries as well as his friends" (N

Agénor de Gasparin

1871). He held various political appointments, such as a member of the Chamber of Deputies from Bastia (Corsica) in 1842. Furthermore, de Gasparin was interested and active in issues related to economics, history, politics, and religion.

A biographer presented de Gasparin as a man always willing to fight for causes, such as the abolition of slavery (Borel 1879). This was evident in his various books, among them *De l'Affranchissement des Esclaves et des Rapports avec la Politique Actuelle* (1839), *Intérêts Généraux du Protestantisme Français* (1843), *Les États-Unis en 1861* (1861), *L'Amérique Devant l'Europe* (1862), *La Liberté Morale* (1868), and *L'Égalite* (1869).

Although de Gasparin's book about table turning appeared in 1854, he had discussed the topic in print before that in some letters published in various reviews (de Gasparin 1853a, 1853b). In one of these he expressed his approach. He wrote:

> I denounce . . . scientific intolerance that concludes without examination, that seeks to stifle under anathema and sarcasms a physical phenomenon that troubles it. (de Gasparin 1853b)

Entitled *Des Tables Tournantes: Du Surnaturel en Général et des Esprits* [On Turning Tables: The Supernatural in General and Spirits], de Gasparin's (1854) book consisted of two volumes, which were translated into English a few years later (de Gasparin 1857; there was also a later abridged French edition in 1889). The first volume has two parts. The first one, and the one relevant for this article, is about table turning. Here he presented accounts of séances which I reproduce here, and discussed various methodological and evidential considerations.

The second part is about "The Supernatural in General." The third part, entitled "Spirits," is about the "supernatural apocrypha." Chapters about miracles, sorcery, animal magnetism, and spirits appeared in the second volume. The discussion consists of attempts to explain the supernatural, including the action of spirits, in natural terms. This includes things such as fraud, testimony problems, nervous excitement producing physiological and psychological changes, and hallucinations. For some cases, the author considers fluid action. This was not confined to physical effects but also included mental ones, such as obtaining information from the thoughts of others.

De Gasparin was critical of all spiritual agency arguments to account for phenomena. Among others, he was critical of French writer about miracles and psychic phenomena Jules Eudes de Mirville (1802–1873), a well-known defender of satanic agency (e.g., de Mirville 1854).[8]

"My deductions," wrote de Gasparin, "have been of a nature to destroy all superstitious fables, modern as well as ancient, and, at the same time, to strengthen historical, scientific, and religious certainty" (de Gasparin 1857:Volume 2:469). In this sense de Gasparin's work was similar to that of previous critics who attempted to reduce all unusual phenomena to deception, physiological and psychological explanations (e.g., Dendy 1841, Newnham 1830).[9] But his emphasis on the action of a nervous fluid outside the physical body separated him from this line of thought and placed him in line with mesmeric ideas and the ideas of other nervous force critics of Spiritualism (e.g., Rogers 1853).

The analyses of previous traditions led French physician Auguste Debay (1802–1890) to remark that the book, unlike other works, was not an "indigestible compilation" of information (Debay 1854:370). He believed de Gasparin's analyses were "a monument of reason, against the disorders of the imagination" (p. 370), kind of an antidote against superstition.

Writing about this nervous force, de Gasparin referred to it as a hypothetical one consistent with the facts. He stated that although he did not accept all the mesmeric ideas of a universal fluid, he thought that an "hemato–nervous fluid" could explain much in mesmerism and Spiritualism (de Gasparin 1857:331). He wrote further:

> If my brain, acting like a Leyden jar, emits and directs a fluid current along my nerves, if this fluid is also emitted by the other members of the chain, it is evident that our combined action will soon form a sort of electric battery, the influence of which will be felt conformably to our thought; we shall communicate a rotation, we shall produce, even at a distance, energetic elevations. (de Gasparin 1857:Volume 1:430–431)

The fluid was seen as an agent connected to the will of the sitters.[10] As mentioned above, this principle was seen by him as related to body movements. He wrote that

> the table identifies itself in some sort with us, becomes one of our members, and executes the motions conceived in our minds, in the same way that our arm does. (de Gasparin 1857:Volume 1:98)

De Gasparin also discussed in the first part of his book the objections to new phenomena. He noticed how new facts were rejected by many without examination. According to a reviewer of his work, he examined in detail many objections, and refuted them "with a clarity, a reason, a dialectic superiority which makes this part of his work a masterpiece of an essay; he victoriously crushes his opponents" (Morin 1854:676).

The section about séances reprinted here includes reports on 12 meetings held by de Gasparin and his collaborators at Valleyres, Switzerland, from September 20 to December 2, 1853. These do not represent all the séances conducted, since there were many held before and after the ones that appear below. But those extra séances were not reported. Not much is said about the sitters, but de Gasparin (1857:Volume 1:178) stated:

> The persons engaged in these experiments were two scientific botanists, MM. Muret and Reuter, M. Tachet, the clergyman, M. Boissier, several domestics, three children from eleven to fifteen years of age, my wife, and myself.

The number of sitters varied between 8 and 12 sitters, but was generally 10. Thury (1855) also attended some séances.

The Count's wife, Valérie Boissier, wrote in a letter to her father dated March 31, 1853: "The day before yesterday, we placed Agénor on the table, and under this weight of nearly a quintal, she rose on two legs and turned" (Barbey-Boissier 1902:243). In a later letter she stated that the table danced the waltz, as well as the gavotte and the minuet.

Excerpt about Table Turning

Presented here are the séance accounts from the English-language translation of de Gasparin's 1857 work.

Sitting of September 20th

I leave out . . . everything that has not been sufficiently studied, everything that ulterior experiments have rendered in the slightest degree doubtful, everything that is merely a repetition of the facts already stated. This deduction performed, there still remain some results to notice.

And first, to speak of the table that has served us most frequently. The top is of ash, about 32 inches in diameter, and rests upon a heavy pillar from which project three feet, 22 inches distant from each other. Another table, the top of which is a little larger, the pillar less heavy, has also been employed. In fact, we have sometimes put in motion tables with four feet, both round and square, all of respectable dimensions. The number of experimenters forming the chain at a time is ordinarily ten; it has varied between the two extremes of eight and a dozen. The rotation usually manifests itself after five or ten minutes. In certain cases—very rare—we have waited nearly half an hour.

On the 20th of September, then, we desired to put to the proof the pretended faculty of divination ascribed to the tables. For this purpose, we submitted to the one around which we were sitting, and which operated to admiration, the most elementary question assuredly, that can be proposed to a spirit. We placed three nuts in the pocket of one of the experimenters; the table, interrogated as to their number, promptly struck nine blows! The same person, after having succeeded in obtaining several numbers indicated by his will—among which was a cypher—entered upon a contest with his vis-à-vis [facing person]. This constituted a particularly interesting experiment, which we termed *the balance of forces*. It cannot be said in this case that the motion was communicated by the vis-à-vis acting as a lever, for the interests were opposed. The vis-à-vis struggle against each other, the one wills a large number, the other a small number. Were the impulsion of a mechanical nature, the champion of the small number would determine to cease furnishing the balance from the moment his number had been struck, he would even lean in such a manner as to obtain judgment! But, no! The most powerful operator carries it; if he is charged with the high number, the high number is attained. One thing must be remarked, however, that from the moment his adversary's limit is passed, and the wills have ceased to coincide, the blows become less strong; the foot which previously obeyed both thoughts is no longer sustained by more than one.

We then changed the conditions of the struggle. A coalition was formed to the advantage of the small numbers; they were confined to two, afterwards to three members of the chain, and it was only then that the knight of the large numbers was vanquished, and the foot in front of him (a foot over which he was deprived of all mechanical action), ceased to follow the impulsion of his will, in spite of the experimenters opposite, who alone would have been sufficient to put and maintain it in motion, had that motion been produced by muscular force.

It is to be taken for granted that different combinations were tried and produced results not less decisive. We made a variation in the feet, sending the blows from one foot to another. We inverted the *roles*—the most powerful experimenter was in his turn charged with the small numbers; and he regularly succeeded in stopping his adversaries, no matter which foot was designated.

It was at last proposed to try the counterproof of one of our most conclusive experiments; that which consists in making the table turn and knock while supporting the weight of a man weighing 174 pounds. The man was placed upon it; the twelve experimenters, taking care not to form the chain, applied their fingers to the table and exerted themselves to obtain by the tension of their muscles what they had obtained some days previous without tension or effort. The energy with which they worked was astonishing! and yet—nothing! The rotation took place in a feeble degree, scarcely turning half round; the poor table all the time trembling and creaking as though it were about to split into pieces. To raise it from the ground was out of the question. Not one of the feet would give the least sign of docility. It is useless to add, that, for the strongest possible reason, we gave up all hopes of obtaining the complete revolution, which our simplest commands had effected but a short time before.

September 22nd Sitting

We have not established any new fact worthy of mention here; but among the old facts reproduced, I think it useful to describe the motions of the table while bearing the same person who was placed on it three days before. The inutility of muscular action had then been seen; we were this time about to see the power of the fluid, or whatever physical agent it may be, of which the operators dispose when they form the chain and when command with a firm will.

We were indeed very glad of the opportunity to make this comparison.

In the habit of criticizing our experiments, and not willing to accept as a certainty what we had observed only once or twice, we were anxious to begin by placing ourselves in the identical positions. The success has this time been complete. The table has turned; it has struck several blows; it has stood entirely upright, so as to throw off the man.

I desire, in passing, to be permitted to record a general remark. We had already held numerous meetings; our experimenters, among whom were several young, delicate women, had acted with uncommon perseverance and energy; their physical fatigue, at the termination of each sitting, was naturally very great; it might consequently have been expected that nervous accidents, more or less grave, would have occurred. If the explanations based upon the involuntary acts accomplished in a state of extraordinary excitement, had rested upon any real foundation, we should have had ecstasies, almost possessions, and in all cases nervous attacks.[11] Now, it did not happen, during the five months we thus met, animated and noisy as our experiments frequently were, that one of us, for a single moment, experienced the slightest discomfort.

Still further, when one is in a state of nervous tension, he becomes absolutely incapable of acting on the table. It must be taken cheerfully,

briskly, with confidence and authority, but without passion. This is so true, that the moment my interest in it becomes too great, I cease to make it obey me; and in all our sittings, I invariably found that whenever, by reason of the public discussion in which I was engaged, I allowed myself to desire success too ardently, and became impatient at our numerous delays, I lost my influence over the table.

September 26th Sitting

Our début was discouraging enough, and led us to think that the entire results of the day would be limited to the two following observations, which are in fact well worth their price, and which our practice has not ceased to confirm: First, there are some days when we can do nothing, however numerous, strong, or animated we may be. This proves that the motions of the table are not obtained by fraud, nor by involuntary pressure of the muscles. Second, there are persons (among others, those who are unhealthy or fatigued) whose presence in the chain, is not merely useless, but injurious; themselves deprived of fluid, they seem also to hinder its transmission and circulation; their good will, their faith in the table, go for nothing; so long as they are there, the rotations are feeble, the elevations are languid; the commands are not executed, the foot placed in front of them is particularly affected by paralysis; induce them to retire, and immediately, life reappears and everything succeeds as by enchantment.

It was not, indeed, until after we had taken this course that the movements became as free and energetic as usual. We had already met with several checks, and especially when the purpose was to dislodge a man placed on the table. In vain did we issue our commands impressively and with spirit; no rotation, no perpendicular motion! We were forced to substitute a child for the man, and then alone could we succeed in producing action.

We were thus almost disheartened, when the purification of which I just now spoke was tried, and immediately, what a metamorphosis! Nothing seemed difficult to us; those even, who, like me, ordinarily succeeded only tolerably well, now caused the numbers indicated by our thoughts to be correctly rapped out, with the occasional exception of one rap too many, resulting from the tardy issue of the mental order which should have arrested the blows.

Finding that everything progressed according to our wishes, and determined to attempt the impossible, we undertook an experiment which marks our entrance into quite a new phase, and puts our previous experience under the guarantee of an irrefutable demonstration. We were about to forsake probabilities for evidence. We were about to make the table move *without touching it*.

Our first success was brought about as follows:

Choosing a moment when the table was impelled by an energetic and truly spirited rotation, we all raised our hands at a given signal; then, maintaining them united by means of the little finger, and continuing to form the chain at about an inch above the table, we pursued our course, and, to our great surprise, the table also pursued its course, making thus three or four turns!

We could scarcely believe in such a success; the witnesses of the experiment could not refrain from clapping their hands. And not less remarkable than the rotation without contact, was the manner in which it was effected. Once or twice, the table had ceased to follow us, because the accidents resulting from our change of place had separated our fingers from their regular position above the margin; once or twice the table came to life again, if I may dare thus to express myself, as soon as the revolving chain returned to its proper relative position. We all had a perception that each hand had carried, by a sort of attraction, the portion of the table underneath it.

September 29th Sitting

We were naturally impatient to submit the rotation without contact to a new proof. In the confusion incidental to a first success, we had not thought either to vary or renew this decisive experiment. Since then, we had reflected on it; we had felt that it was important to do the thing over again more carefully, and in the presence of new witnesses; that it was especially important to produce the motion in place of continuing it, and to produce it under the form of elevations, rather than confine ourselves to the rotations.

Such was the programme for the meeting of the 29th of September. Never was a programme more implicitly followed.

First of all, we resumed our experiments of the 26th. The table being in full rotation, the hands were separated and continued to turn above it, in forming the chain. The table followed, making sometimes one or two revolutions, sometimes half, or nearly a quarter of a revolution. The success, more or less prolonged, was certain. We verified it several times.

But it might be said that the table being already started, preserved a certain impetus which it mechanically obeyed, while we imagined it to obey our fluid power. The objection is absurd, and we would have challenged anyone to obtain merely a quarter of a revolution without forming the chain, no matter how great the velocity of the rotation; we would especially have challenged them to succeed in renewing the race, after it had been momentarily suspended. Nevertheless, it is well in such matters to anticipate objections, however absurd, as long as they are plausible: and this might appear so to the eyes of the careless observer. It was necessary, therefore, to produce rotation from a condition of complete repose.

We did so. The table being motionless as well as ourselves, the chain of

hands separated from it and began to turn slowly a short distance above its margin. After a moment, the table made a slight motion, and each person endeavoring by his will to incite the portion underneath his fingers, we drew the body of the table after us. The same circumstances then occurred as in the preceding case; it is a difficult matter to maintain the chain in the air without breaking it, without removing it from the edge of the table, without moving too quickly and thus interrupting the established relation, that the rotation is often arrested after one, or even less than one, revolution. Nevertheless, it is sometimes prolonged during three or four.

We expected to encounter still more obstacles, when it came to the point of raising it without contact. But we were agreeably disappointed—the fact was entirely otherwise, and we accounted for it in the following manner: There being in this instance no circular movement demanded from us, we found it much easier to retain the normal position of the hands above the table. The chain then being formed a short distance above the top of the table, we ordered one of the feet to rise, and it instantly obeyed.

We were in raptures. This beautiful experiment was renewed many times. We ordered the table, likewise without touching it, to stand erect, and to resist the witnesses who should attempt to bring it to the ground. We ordered it to turn over, and it fell with the feet in the air, although our fingers at no time touched it, but always remained at the same distance from it.

These were the essential results of this meeting. They are such that I hesitate to mention by their side other incidents of secondary importance.

I will merely add in passing that the sitting had commenced very discouragingly; that not only had it been necessary to send away some new operators, but several of the old ones were deprived of their usual enthusiasm. The table obeyed badly; blows were struck feebly, and as if with regret the numbers demanded were not expressed. Therefore, we took a new approach, from which good results flowed without number: We persevered and persevered cheerfully; we sung, we made the table dance, we banished from our minds all new experiments, insisting upon easy and amusing operations. After a certain time, the order of things was changed, the table overflowed with activity and willingness, its obedience almost anticipating our commands; we were prepared to approach matters of grave import.

October 6th Sitting

Notwithstanding the distraction created by too many spectators, and the lassitude caused by the stifling heat, we obtained in this long sitting the most essential confirmation of previous results.

Numbers indicated by the thought, the balance of forces, the elevation and resistance of the table, all were renewed. With regard to resistance in particular, we measured it. A weight of 80 lbs. did not suffice to lower the

table over which we made the chain, when it formed with the floor an angle of 35 degrees. The same table forming the same angle, fell heavily under the force of a weight of about 60 lbs. when not sustained by the influence of the chain. Note, moreover, that the hands placed opposite the weight of 80 lbs. had been raised, and did not again touch the table while it continued to resist. But I do not offer this as a conclusive experiment, because I know that there is a certain point of equilibrium, where a table the most destitute of fluids, would, of itself, resist a considerable pressure; notwithstanding, therefore, the difference above established, I discard the fact (very real to my eyes) that I have just related, for I am determined to adduce only such proofs as cannot be controverted.

We tried also to set in motion the table bearing the weight of a heavy man. The rotation was at that time impossible, but the feet struck several heavy blows.

Passing then to the counterproof, we remarked that when we act mechanically, precisely the contrary result takes place. By energetic muscular efforts, a slight rotatory movement is obtained, but it is impossible to raise the feet.

Finally, we resumed the great experiment, that of motion without contact.

It seemed at first that we were not in a condition to obtain good results. But soon, however, we succeeded in continuing the rotation and in producing it from a state of repose. Its most remarkable feature was, that our commands effected a small rotation, about one-quarter of a revolution, although we ourselves remained entirely motionless. The table thus gently glided from under our fingers.

The perpendicular motions without contact were produced many times and with energy. The table, influenced by our hands, which were extended a short distance above it, stood erect, resisted efforts to lower it, and turned itself over completely several times.

October 7th Sitting

Another long and fatiguing *réunion*. It was principally devoted to the trial of divers pieces of mechanism, which had no success: metal rings, frames of canvas or paper placed above the table, platforms turning on pivots, and the keyboard of a piano. Whether a view of the machines in question suppressed the emission of the fluid in the operators, whether the machines themselves suppressed its circulation in the table, whether, in fact, the natural conditions of the phenomenon were disturbed in another manner, it is certain that the results were either nothing or questionable.

Only one new experiment succeeded. A platform turning on a pivot supported a bucket. After filling the bucket with water, I and two others

plunged our hands into it. There we formed the chain, and began to turn round, avoiding touching the bucket; it was not long before the bucket also put itself in motion. The same thing was done several times in succession.[12]

As the objection might be offered that the impulsion given to the water was sufficient to impel a thing so easily moved as a bucket, we immediately proceeded to the counterproof. The water was agitated circularly, and with much more rapidity than when we formed a chain in it, but the bucket did not stir. It remains to know, doubtless, if one of us three did not touch the interior of the bucket in order to influence its motion. To this I reply, first, that the manner in which our hands were plunged into the bucket was evident proof that none of our fingers could, corporally, touch the bottom; second, that being careful to form the chain in the center, we might as easily have brought our fingers in contact with the walls of the room.

The doubt, however, not being absolutely inadmissible, I continue to rank this experiment among those of which I do not pretend to make any use. I desire to show myself difficult on the point of proofs.

That which is furnished by the expression of numbers indicated by the thought, is still one of the most substantial in my estimation.

What rendered it particularly convincing, in the sitting of which I speak, was that each of the ten operators, in turn, received the communication of a sum in writing, from some member of the audience, the others having their eyes closed. Now, of the ten, all, with one exception, obtained perfect obedience from the foot designated by the most suspicious witnesses. Whoever reflects on the above-mentioned experiment, will see for himself that it is entirely beyond the circle of things admissible, that fraud could not have any agency in producing the combination of motions here communicated. The objection needs to invent a prodigy far more surprising than ours.

Let us return to the demonstration *par excellence*—the elevation without contact. We began by accomplishing it three times. Then, as it was suggested that the presence of witnesses exercised a more certain influence over a small table than a large one, over five operators than ten, we caused a round table, made of spruce, to be brought in, and which the chain reduced by one-half, sufficed to put in rotation. Whereupon, the hands being raised, and all contact having ceased, the table elevated itself perpendicularly seven times at our command.

October 8th Sitting

This sitting was accomplished under such circumstances that I ought, perhaps, to pass over it in silence. The death of a valued friend had plunged us all into profound grief, and the moral depression resulting from it took away the fluid power even of those in whom it was usually most abundant.

Had it not been for the presence of a visitor, who had come a long distance to assist at our experiments, and who could not prolong his stay, we should certainly not have attempted to act at such a moment.

Among the new trials, I will mention one, the object of which was to raise entirely from the ground a table suspended from a pulley and balanced by a counterweight. Only one of its feet touched the ground, and the weight to overcome it was reduced to a trifle. The chain having been formed, the foot that touched the ground rose clear from it, and the table thus accomplished some vibrations without encountering the floor.

Had it been raised? I am far from affirming this to be the fact. It might have been simply impelled by the fluid, so as to change the mode of suspension, and put a space between the earth and its foot. It might also have been that the action of the hands on it was purely mechanical, that the cord which sustained it had been removed from the vertical, and that the friction had ceased because the table was forcibly drawn to the right or left, at the precise instant when its foot would have been impelled to strike the ground.

Consequently, this fact possesses no value either in favor of or against my theory. I will say as much of various analogous experiments, and also of the keyboard of the piano, over which we formed the chain anew without obtaining any rotation. The fluid is probably lost in this labyrinth of springs and platforms; moreover, the confidence and will are weakened.

To conclude with something less negative, I will state two more facts confirmatory of the preceding results.

Among the numbers called for, the malice of a witness had placed a cypher, and the foot designated for its expression at the left of the operator, beyond the sphere of his muscular action. Now, the command having been issued without producing any response, we were all extremely annoyed, convinced as we were that our powerlessness for the time being was so great as to prevent our obtaining even the simple elevation. I confidently assert that if the experimenters placed in front of the foot ever were tempted fraudulently to apply mechanical action, they were at that moment. Our *nerves* were intensely excited, and our impatience was at its height; nevertheless, no motion was observed, and to our great relief the figure was announced to be a cypher.

We at length twice effected the motion without contact. At such times, it was great movement, and we considered ourselves happy in having accomplished it.

October 27th Sitting

I relate things as they occurred, and have no wish to describe ourselves as more triumphant than we actually were. The reader must judge for himself. I confess that here again is a sitting by no means brilliant.

Moreover, this lack of uniformity is of interest. We were, for example, deprived of a great portion of our power, by the single fact of the indisposition of the person who has the most influence over the table. Now, let the enemies of the tables try to explain that! If it were the result of mechanical action, it and we would have succeeded as in the past, for our muscular force was not diminished.

If it were the result of fraud, we should also have succeeded equally well, for our *personnel* was the same, and nothing prevented the dishonest hands from performing their office. If it were the consequence of unconscious and involuntary motion, the success ought likewise to have been complete, for never had we been more ardent and energetic. But there we sat, real objects of pity, sometimes passing an entire quarter of an hour without obtaining a rap or a simple rotation.

Nevertheless, we finally arrived at some results, which were as follows:

Seeing that we did not succeed in effecting the perpendicular motion without contact, starting from a state of immobility, we contented ourselves with effecting it under the more modest form of a continuation of the motion; thus, we commanded the table to strike eight blows; at the third, the hands were raised, and the table, no longer touched by any one, pursued its task, at one time striking four, at another five, and at another eight.

Such was our principal exploit. I will cite another, the exact value of which I do not pretend to determine.

It had been objected to in our experiment in which the table was made to strike while it bore the weight of a man, that this man could lend himself to the motion, and in a measure provoke it. As earnest seekers after truth, we felt that there was plausibility in this objection, and consequently decided to give it our particular attention. The living being, endowed with intelligence, and consequently subject to suspicion, was replaced by inert matter; retorts, filled with sand, were put on the exact center of the table, which was then summoned to display its skill.

But the day was badly chosen. After having thus deposited, one upon the other, two retorts, weighing 130 lbs., we found that we were incapable of producing the elevations; we were obliged to content ourselves with continuing them; the retorts were therefore removed, the table set in motion, and the retorts replaced while the rotation was going on did not check it; they were jostled about with considerable force, and the sand was spilled out on all sides. The remainder of the sitting was devoted to new experiments on the pretended power of divination. Let me here recapitulate the results of those we had attempted in this and in other sittings.

When the table is requested to divine anything that is known to one of the members of the chain, it happens frequently enough, and very naturally, that it divines. The operation is the same as that of numbers indicated by the thought, neither more nor less.

When the table is asked to divine something which is known to one of the audience who takes no part in the chain, it sometimes happens that it divines. This occurs when the person in question is endowed with great fluid power, and can exercise it at a distance. We obtained no such demonstration; but others have succeeded, and their testimony appears too well-established to be called into question.

Up to the present time, we perceive not the slightest trace of divination; fluid action, either near or distant, accounts for those results which at first glance would appear to resemble it.

If tables divine, if they think, if they are under the control of spirits, we ought to obtain conclusive responses under circumstances where the facts are not known, either in or out of the chain. With the problem thus stated, its solution is not difficult.

Take a book; do not open it, but invite the table to read the first line of any page you may choose to designate—page 162 or page 354.[13] The table will not recoil; it will strike blows and you will compose words. It is thus, at least, that we have always been treated. Be that as it may, one thing is certain: no spirit, either here or elsewhere, now or at a future time, however cunning or clever he may be, has read, or will read this simple line. I recommend this experiment to the partisans of the thinking tables and of mysterious evocations.

As for the examples of nuts, pieces of money contained in a purse, the hours, playing cards, the tables conform themselves exactly to the calculation of probabilities, they divine just as much as you and I do. As regards the question of small numbers of which we get a proximate idea, the circle of possible combinations is very little extended; the mind fixes upon a figure, which has tolerable chances of being correct; the proportion between the failure of the table and its success, is about the same as it would be, independent of all miraculous divination. We are here very far from those uniform results obtained by fluid action: numbers indicated by the thoughts, for example, which succeed ten or twenty times in succession, during moments of excitement. This cannot certainly explain itself by any casual conjunction of circumstances.

November 9th Sitting

We were in haste to take our revenge; and it far anticipated our hopes.

Before commencing my relation of this sitting, the most remarkable of all, I wish to observe that neither the thermometer nor the compass have furnished the slightest, interesting indication. I have thought it my duty to note this in passing, in order to show the reader that we have not neglected to employ instruments which, it would seem, might have put us on the road to a scientific explanation. In general, I pass over in silence the various trials that have remained under the conditions of the trials and have led to nothing positive.

Our first care was to renew the experiment of raising an inert weight. This time it was agreed that we should begin with a condition of absolute immobility. The question was to produce, not to continue the motion.

The center of the table having been determined with precision, a bucket, filled with sand and weighing 42 lbs., was placed upon it. The feet raised themselves easily as soon as the order was given.

A second bucket weighing 38 lbs. was then placed in the center of the first. They were both raised, less easily, but very distinctly.

A third and smaller bucket, weighing 26 lbs., was likewise added, and placed upon the other two. The elevation took place.

We had prepared, in addition to these, some enormous stones, weighing about 44 lbs. We put them on the third bucket. After considerable hesitation, the table raised each of its three feet successively and several times, with a force, a decision, and a spirit which surprised us. But its strength, already subjected to so many trials, was unequal to this. Staggering under the energetic impulse communicated to the entire mass of 150 lbs., it suddenly gave way, and its pillar was rent from top to bottom, to the great peril of the operators on the side toward which the load fell.

I do not pause to comment on such an experiment; it covers the whole ground. Our muscular force would not have sufficed to determine the motion that took place. An inert and noncomplying weight had replaced the person whose complicity was to be feared. In fine, the three feet having been raised, each in its turn, there could be no excuse for insinuating that we had put the weight more on one side than on the other.

Our poor table was wounded on the field of honor. Not being able to cure it immediately, we took a new one strongly resembling it, but which was in reality a little larger and a little lighter.

It remained to be seen if we should be obliged to wait until it had become charged with fluid. The occasion was favorable to the resolution of an important problem: Where does the fluid reside? In the operators or in the table? The solution was as prompt as decisive. Hardly had our hands, forming the chain, been placed on the table, than it turned with the most unexpected and comical rapidity. Evidently the fluid was in us, and we were free to apply it to any other table.

Our time had not been lost. In the condition in which we found ourselves, the motions without contact ought to have succeeded better than ever. We were not deceived in supposing it.

The rotations without contact were first obtained up to the number of five or six. The motion under our fingers, and under the will that attached itself to this or that particular point of the table, was slow at the beginning, gradually accelerating toward the end; several rotations lasted during three or four revolutions.

As to the elevations without contact, we discovered a new process

that rendered success easy. The chain, formed a short distance above the bed of the table, is so arranged as to pursue its course in the direction of the point where the motion is expected to take place. The hands nearest the foot called upon to rise are outside the bed of the table, which they gradually approach and pass over; while the hands opposite, and which at first had advanced toward the same foot, move off to one side, drawing it with them. It is during this progression of the chain, while all the wills are fixed on one particular spot in the wood, and the orders to rise are uttered with force, that the foot quits the earth and follows the hands to the point of overturning the table if not prevented from doing so.

This is not an isolated result. We reproduced it about thirty times. We caused it to be executed by each of the three feet successively, in order to deprive the critic of all pretext for cavil. We, moreover, watched the hands with scrupulous attention; and when it is observed that this watchfulness was continued during thirty operations, without surprising us with the slightest contact, it will be concluded, I think, that the reality of the phenomenon is henceforth established beyond all reasonable contestation; especially, if it be added that during the last elevations one of the spectators, kneeling down, applied his eye to the plane of the table in such a manner as to assure himself that it was the whole time free from touch or other improper influence.

One word more. It seemed to us that the table once made a movement forward instead of perpendicularly, and that it had thus followed on the floor the progress of the chain. This was a fact to verify.

Confirmed, as will be seen by our subsequent experiments, it manifests under a new form, the impulse to which the table yields. It is curious to see it submit to our action from a distance, and glide over the ground, when it has not force enough to rise. In fact, the same thing occurred when the hands rested on it. If the fluid power does not suffice for the elevation demanded, the table takes flight and makes its escape, sometimes in a straight line, sometimes by commencing an unlooked-for rotation—now in one direction, now in another. The impulse communicated, whether great or small, produces a proportionate effect.

November 24th Sitting

The peculiar characteristic of this sitting was the absence of the person who exercised the greatest authority over the table. By operating without him we were enabled to establish two things: the first, *an experimenter cannot with impunity be dispensed with*; the second, *that he can be dispensed with in case of extremity, and that success, although less brilliant at first, is not impossible*. I underline this last point, along with the frequent modifications caused by our *personnel*, for the benefit of the suspicious portion of the community, who, not knowing the moral value of the persons in question, would be

disposed to impute to their dexterity, results to which they, themselves, essentially contribute.

First of all, and when there was in none of us any fluid developed, we desired to ascertain if it could not be produced by the simple process of mechanical rotation. Applying, then, our hands to the table without forming the chain, we turned it rapidly for nearly a quarter of an hour. We then commanded the table to resume this motion of itself; we commanded it to raise one foot, and although our fingers rested on it the whole time, it was impossible for us to obtain the feeblest movement.

Still more significant was the fact that having formed the chain, but having determined its rotation by the mechanical action of our hands, we were thus able to continue it for a quarter of an hour, without inducing any fluid manifestation; in vain did we address various orders to the table—not one of them was obeyed. We exercised no power over it.

It is consequently clear that the phenomenon is of a mixed nature; that a given position and a circular course are not of themselves sufficient to call it into existence. There must still be another force—the will.

Our wills being finally brought into cooperation with the other powers, and the muscular pressure having ceded its place to the pressure of commands, we produced the fluid rotation after five or six minutes concentration of our thoughts. We clearly felt that we lacked some person of importance, and that we did not possess all our usual power; nevertheless we were determined to overcome the obstacle, even at the price of greater moral fatigue.

The great difficulty, motion without contact, was thus attacked frontally.

The rotations without contact were obtained three times. I should add that they were very incomplete, a quarter or a half revolution at most.

The success of the elevations without contact was more decisive; but it was bought by the expenditure of a very considerable amount of strength. After each elevation we were obliged to take a rest, and when we had reached the figure nine, yielding to lassitude, we were compelled to stop entirely. It is necessary to go through with such experiments in order to know how much attention and energy they exact, to what degree it is indispensable to will, to will absolutely that such a knot in the wood of the table follow the extended fingers that attract it from a distance.

Be that as it may, our attempt was crowned with success, and we felt at liberty to terminate the sitting by exercises less exhausting.

The idea then occurred to us to make the trial on a large table with four feet. It had often been claimed that the round tables with three feet alone lent themselves to our operations; it was time to furnish demonstrative proof to the contrary. We therefore selected a table whose diameter was 3½ feet, and the half of which, independent of the foot that supported it when drawn out, folded up at will.

Hardly had our fingers touched it, than it surrendered itself with a loud noise to a rotation, the vivacity of which surprised us, thus showing that tables with four feet diameter were not more rebellious than others. It furnished, besides, a new argument in favor of one of our preceding observations; the fluid is in the individuals, not in the tables. Indeed, the motion was produced almost immediately, and before the large table could be considered as charged.

It was afterwards requested to strike blows with its different feet. We began with those that supported the top and one of the leaves; they were three in number. They raised themselves, two by two, with such force as to break one of the casters in splinters . . . Now it would be difficult to accept the idea that the intensity of this motion could result from the fraudulent action of the fingers as a lever upon so heavy a piece of inert matter, or that they could impel it to such a height.

It remained to try the foot that was independent of the bed of the table. We thought it would obey as readily as the others; but no! In vain we lavished the most pressing invitations; it did not once consent to rise, whether in company with its neighbor on the right, or whether associated with its neighbor on the left. Supposing that this reluctance might be owing to the persons placed near it, we changed the position of the members of the chain. Useless efforts! All the combinations were doomed to be successively foiled.

We already anticipated an important consequence from this fact. But as it was afterwards proved incorrect, the rebel foot yielding us its perfect obedience on another occasion, I shall not confide our process of reasoning to the public; I will only beg them to remark two things: first, the care which we constantly took to confirm the accuracy of our proofs by repeated experiments; second, the impossibility of having recourse to explanations based on muscular action. This action could have been exercised as easily to raise the foot that was independent of the table, as to raise the feet confined to it; and yet, by the operation of some unknown cause, evidently foreign to all mechanical laws, the latter alone consented to move.

November 28th Sitting

We were all assembled; but two or three of the operators were slightly indisposed. In fact, from some cause or other, the meeting was only remarkable because of the almost total absence of fluid power. For a single moment we had a little half an hour of action, and then two hours and a half of inertia.

I always state the fact as it is; first, out of respect for truth, and also because it seems to me that nothing better refutes the vulgar objections than to show that the same individuals are incapable of constantly obtaining the

same results. Their muscles have not changed; their susceptibility is as great; their dexterity in fraud (we need not fear to speak thus) has not vanished, and yet, behold them unable to do that which but a short time previous they had done with extreme facility.

Our wounded victims had been cured; the old table reappeared with its pillar repaired; the large table with four feet was supplied with a new caster. It was with this that we commenced. Inauspicious beginning! That which the other day had whirled and leaped about with so much vigor, now scarcely stirred. And as for inducing either of the feet to strike a single blow, we were compelled to renounce the idea.

Then, passing to the table with three feet, we entered upon our phase of animation, which did not, however, long continue.

Nevertheless, we profited by it to the extent of effecting five elevations without contact. After which, our slender provision of fluid being exhausted, it was no longer possible to effect anything. The rotations without contact, as we had foreseen, were utterly out of the question.

Nothing could be more lamentable and curious at the same time, than to see us sitting round the various tables, passing from one to the other, resorting to all sorts of expedients, and yet unable to obtain more than a languid rotation, which soon ceased entirely.

December 2nd Sitting

I should have been sorry to close the relation of my experience with a report so little brilliant. Happily, the result of our last meeting gives me the right to leave quite a different impression on the mind of the reader.

We were all in excellent spirits; to which the fine weather perhaps contributed, and this is not the first time I have remarked the coincidence. One thing is certain, that the same persons who, on the 27th of November, had obtained but a half-hour of success, passing the rest of the sitting in vainly soliciting for something better than poor, imperfect rotations, or languid blows, ruled the table today with an authority, a promptness, and if I may be allowed the expression, with an elasticity that left nothing to be desired.

The large table, with four feet, had been put in motion, and this time the facility with which the foot at liberty raised its portion of the top, proved that we were right in not drawing from its preceding refusal a too positive conclusion.

We did not succeed in raising this table without contact, or in folding up its movable leaf. None of us were surprised at this, for the weight was very considerable; our attempts, however, were not entirely without fruit, for they brought about a result of which we had not dreamed.

Each time that we endeavored to elevate without contact the portion

of the table farthest from me, I felt the foot, whose neighbor I was, gradually approach, and lean itself against my leg. Struck with this fact, which was repeated several times, I inferred from it that the table was sliding forward, in consequence of not having enough force to raise itself. We thus exercised a sensible action upon this large table without touching it in any way.

In order better to assure myself of the fact, I left the chain, and observed the progress of the feet of the table on the floor. It varied from less than an inch to several inches. Having afterwards tried to fold up without contact, the movable leaf of a card table, covered with cloth, we obtained the same result. The top did not yield to our influence, but the whole table was carried forward in the direction of the ordered motion. I should add that it was far from easy for it to slide along thus, for the floor of the hall in which we carried on our experiments is rough and uneven.

It is not less interesting to note here, the moment when the movement usually occurs. It is precisely the same as that in which the elevation without contact takes place whenever it is effected. When the portion of the chain which presses forward is about to pass beyond the edge of the table where it is brought back, and the portion of the chain that draws, is about to pass over it in making a retreat, then is manifested, either the ascensional, or in default of that, the sliding motion. Our fluid power is at its maximum, just at the point where our mechanical power is at its minimum, where the hands that push have ceased to be able to act (supposing fraud is intended), and where the hands that draw cannot yet act.

Returning to the table we generally employed, we tried to produce the rotations and elevations without contact. Our success was complete.

The rotations numbered three. We obtained the elevations, one after another, with the most satisfactory regularity. Setting aside as uncertain four movements which, although real, did not terminate in a complete elevation of the top (even leaving out of consideration two energetic overtures that were separately produced), we effected an uninterrupted series of fourteen elevations, and so emphatic, in general, that we were several times obliged to catch the table thus subdued, in order to prevent its becoming wrecked.

The reader is now as well-acquainted with the results of our sittings as though he had personally assisted at them. I have concealed nothing; I have related the best and the worst, the experiments that were failures, and the experiments crowned with success.

(de Gasparin 1854/1857:Volume 1:43–66)

[Added Observations in the Preface]

Some distinguished men of science to whom I communicated the results obtained, were unanimously of the opinion that the elevations without contact would possess the character of absolutely certain proof, provided

we could succeed in verifying them by any material process. They said:

> Strew some flour over the table the instant the hands are separated from it; in these conditions, cause it to effect one or several elevations; then, if the layer of flour does not bear the impress of fingers, or give any other evidence of having been touched, there can no longer be a word offered in objection to your theory.

Well! we have recently and on several occasion, performed this very experiment. I briefly present a few of the details.

Our first attempts were most unsuccessful. Making use of a coarse sieve, which it was necessary to move about over the entire surface of the table, we met with a double inconvenience; first, of suspending for too long a time, and consequently, annulling the action of the operators; second, our layer of flour was much too thick. The enthusiasm of will was weakened, the fluid action impeded, the ardor of the table diminished; in short, nothing progressed. The effect was even so injurious that the table not only refused elevations and rotations without contact, it almost refused ordinary elevations and rotations.

After a while, a brilliant idea suggested itself to one of the operators. We possessed a pair of bellows, such as are used in sprinkling sulphur over vines infested with *oidium*. Substituting flour for sulphur, we renewed the operation.

We were in the most favorable conditions; the weather was dry and warm, the table bounded beneath our fingers, and, indeed, before the order to raise the hands was given, the majority of them had spontaneously ceased to touch the table. The command being issued, the entire chain separated from the table, which was, at the same instant, covered by the bellows with a light cloud of flour. Not a second had been lost, the elevation without contact had already taken place, and, in order to leave no doubt in our minds, it was repeated three or four times in succession.

That done, the table was scrupulously examined: It bore not the faintest token of having been touched or even grazed.

The fear of inadvertently touching it was indeed so great among the operators as to cause them to raise their hands much higher from the table than in the previous sittings, without, however, producing any diminution of the fluid action. I should also mention that we resorted to none of the maneuvers, none of the passes of which we had made use at other times. Remaining in its place above the table to be raised, the chain had preserved its form; it had scarcely effected a slight motion in the direction of that which it provoked at a distance.

I add, in conclusion, that we did not rest contented with one experiment. We produced several elevations in succession, at the close of which, a minute

examination of the flour that covered every portion of the surface of the table, convinced us that it had been absolutely untouched.[14]

(de Gasparin 1854/1857:Volume 1:xix–xxi)

Perspective: Critiques and Influence

De Gasparin wrote about the table movements: "Thus the fact is established. Multiple experiments, various irrefutable proofs, mutually supporting each other, give to fluid action an entire certainty" (de Gasparin 1857:Volume 1:81). But he also discussed various other things about the séances, such as the use of instruments, psychological conditions, occasional unproductive séances, and the implausibility of the issue of fraud and unconscious movements.

But how did others react to this work? Here I limit myself to table turning, and not to examinations of other phenomena in different time periods nor to views of de Gasparin's use of the concept of the fluid.[15]

There is no question that de Gasparin's tests can be considered "classic" studies. Castellan (1960:53–55) opened her chapter about "The Classic Period in Europe" in her brief history of psychical research with this work,[16] which traditionally has been included in older and more recent overviews of these topics (e.g., Podmore 1902:Volume 2:187–188, Inglis 1992:218–219).

In addition, de Gasparin's work frequently has been mentioned in encyclopedia entries (e.g., Felton 1898:673, Sidgwick 1890:407), and has been summarized in various textbook overviews (e.g., Holms 1927:276–277, Richet 1922:521), as well as in other psychical research books (e.g., de Rochas 1896:317–320, Flammarion 1907:Chapter 6).

Thury (1855) was certainly positive about the work in question. In his view, the experiments at Valleyres established that the will "can act at a distance on inert bodies by means different than muscular action" (p. 11). Students of Spiritism and magnetism were also positive about the séances. One stated that the main aspect of the work was "the evidential constatation, indelible, REAL FACTS . . ." (Auguez 1857:101). Another was also positive, stating he found the work imposing. But he regretted it had little impact on scientists. In summary, "no attention was paid to him [de Gasparin]" (Morin 1860:378).

In fact, various writers in later years discussed explanations of table turning and unconscious movements without mention of de Gasparin's work (e.g., Hahn & Thomas 1883:286–287, Maira & Benavente 1887:204–209, Maury 1861:419–424). Two further important examples were French physicist Jacques Babinet (1794–1872) and English physiologist William B. Carpenter (1813–1885), whose writings (or reprints) on the topic published after the appearance of *Des Tables Tournantes* in 1854 did not

mention de Gasparin (Babinet 1856:9–56, 231–254, Carpenter 1877:97–100, 1882:292–311).

G. Mabru (1858) stated that

> [if de Gasparin had used] the Faraday apparatus to control his experiments, he would have avoided the trouble of writing two volumes on things which do not exist. He would have seen that the movement of the tables was not due to any supernatural cause, that it comes simply from the impulse of the fingers of the operators. (Mabru 1858:386)

No mention was made of effects inconsistent with the unconscious movement explanation.

A further example was Joseph Grasset (1849–1918) discussing his model of polygons, consisting of subconscious centers of superior and inferior mental functioning capable of managing cognitive, motor, and other functions, including unconscious movements related to table turning (Grasset 1908:105–111). While Grasset mentioned de Gasparin in relation to the importance of having confidence in the phenomena (p. 110), he did not mention de Gasparin's evidence regarding actual movement of tables, as opposed to coordinated polygonal activity of the sitters pushing the table around.

Skepticism was the tone of the reviewer in *Harper's New Monthly Magazine*. The writer suggested that testimony of sitters would not be convincing to establish facts that science now considers to be in the realm of the supernatural. "The monks who imprisioned Galileo," he wrote, "only evinced the bigotry of common sense. With their light they were entitled to consider him an impostor; and with ours, we laugh at turning tables" (Anonymous 1857b:772).

Louis Figuier

Such also was the case of French physician and popularizer of science Louis Figuier (1819–1894), who discussed de Gasparin's studies in the fourth volume of his classic examination of

psychic-related topics *Histoire du Merveilleux dans des Temps Modernes* (Figuier 1860). He started out assuming that the movement of tables without physical contact was not possible and that the phenomena was never produced beyond his small group of sitters. Then he speculated that a too-zealous sitter in the circle produced such phenomena fraudulently. While not doubting the honesty of de Gasparin, he wrote:

> All that can be said is that he saw the movement without contact occur, without being able to recognize the secret engine. But in order to admit the scientific reality of this fact, it should have been reproduced several times, and at will, in later experiments, at the hands of other experimenters. Now this is what has never happened . . . (Figuier 1860:306)[17]

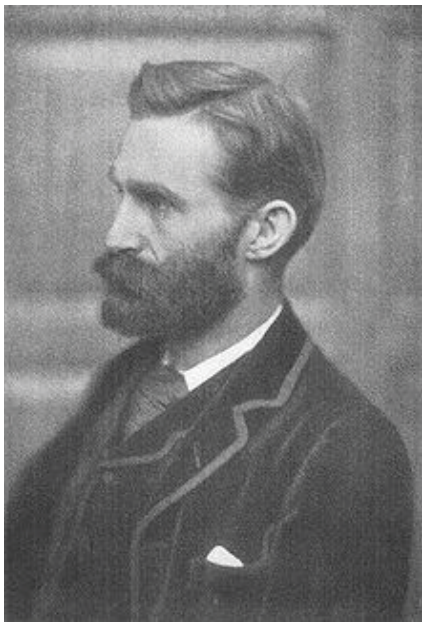
The issue of fraud, something discussed by de Gasparin, was also mentioned by others. Although A. Petit d'Ormoy (1856) wrote that de Gasparin's descriptions of the test seemed to exclude some sources of error, it was not impossible to conceive that some sitters played pranks on the others. Another skeptical response came from Adrien Delondre (1857). He started by raising suspicions about the sitters, saying that in situations like those described by de Gasparin there could a strong desire to cheat. Then he argued that the sitters could deceive themselves via unconscious muscular movements, to which he added suggestion and dissociation as complicating factors. While Delondre did not deal with movements without contact, he decided that de Gasparin's rejection of unconscious movements obtained when no one was touching the table led "the astonished to reasonably ask whether such a statement is serious" (p. 21).

Finally, Delondre suggested that sitters may have hallucinated the movements of the table without contact. He preferred "the marvels of hallucination" to "the prodigies of the volitive fluid" (Delondre 1857:277).

The possibility of fraud was also raised by Frank Podmore (1856–1910), a well-known critic of mediumship and other psychic phenomena. In his book *Studies in Psychical Research*, Podmore stated that de Gasparin did not control well for potential fraud, as some sitters could have used their knees to move the table (Podmore 1897:47). In a later work, Podmore wrote:

> It will be seen that the results depend for their acceptance on the sufficiency of the precautions taken to exclude action of the hands, feet, knees, and other parts of the person below the table. These precautions appear to me, so far as can be gathered from the scanty records, to have been wholly insufficient. The records of the experiments are extremely brief, and hardly any detailed accounts of individual experiments are given; the

> names of the assistants nowhere appear; but we learn that the persons present numbered on some occasions as many as twelve, that this number included several servants and children, and that, generally speaking, the children were found to be more successful operators than their elders.[18] Neither de Gasparin nor Thury appear to have sufficiently appreciated the possibilities of unconscious muscular action or of fraud; nor the extreme difficulty of detecting the kind of half-conscious fraud which later experience has shown that children and young persons are prone on such occasions to practise.[19]
>
> (Podmore 1902:Volume 2:188)



Frank Podmore

Positive comments came from William Crookes (1832–1919), well-known for his studies of mediums, who mentioned de Gasparin's work as the "only good series of test experiments" of physical phenomena he had found (Crookes 1874:5). Some later discussions suggest that the work was influential and held in high regard (e.g., de Rochas 1896:317–320, Flammarion 1907:Chapter 6). One writer considered the work to have been conducted under rigorous control, and that "the movement of heavy bodies without mechanical contact was recognized, proved, and demonstrated" (Aksakof 1895:10).

Morin (1854) reviewed *Des Tables Tournantes* in the *Journal du Magnétisme*, praising de Gasparin's scientific spirit. He concluded saying: "Honor to the courageous athlete who defends with equal energy and talent the rights of reason and truth!" (p. 684).

Two writers confessed that their skepticism was diminished after reading about de Gasparin's séances (Debay 1854:347, J 1855). In addition, an author who was critical of de Gasparin's use of the concept of the fluid to explain things such as miracles and spiritualistic phenomena, characterized his research with the tables as "scrupulous" and as providing an "incontestable service" for truth (Gougenot de Mousseaux 1860:253).

Comments also came from American magician and student of the history of magic Henry Ridgely Evans (1861–1949). While he believed that most mediumship was accomplished via tricks, he included in his

debunking book *Hours with the Ghosts* the work of de Gasparin (together with the work of Crookes and others) as "a class of cases not ascribable to trickery" (Evans 1897:207).

Concluding Remarks

De Gasparin's work was ignored by many, particularly by strong defenders of the unconscious movement explanation. While the reports could have been more detailed, something not common at the time, the critics ignored aspects of de Gasparin's results inconsistent with unconscious movements and simple fraud explanations. This is a pattern that has been common in the evaluation of past work, such as that conducted by Crookes and others. While it may be argued that there are issues of interpretation in terms of the evidential strength of séance reports such as the ones discussed here, I would also argue that it is the duty of the critic to criticize keeping the argument close to the evidence, and at least taking into account aspects of the phenomena inconsistent with the critiques.

The assumption here, and again a common one in psychical research, is that there is nothing to consider, no testimony worth credence to support the existence of unconventional psychic action. Carpenter (1853) argued that the testimony of believers was worthless because they were possessed by their own ideas, a form of insanity. Similarly, later versions of this also declared positive testimony in favor of mediumistic phenomena as delusional and the effects of a weak mentality, as discussed by Brown (1983) and Le Maléfan (1999).

To account for the rejection of de Gasparin's work, it has been stated that he "made the mistake of reopening the old controversy of animal magnetism which had been closed by a final veredict fom the Academies" (Sudre 1962:33). Such association with the magnetic fluid must have certainly been an important factor in the perception of the work as tainted. But other contributing factors to this rejection must be considered. The tables, Monroe (2008:Chapter 1) has shown, were associated with entertainment and ridicule, not with scientific analysis. Similarly, they were also connected to the development of mediumship in Western societies, and to the spread of American Spiritualism, topics that were not appealing to many people.

Several commentators on de Gasparin's work—Delondre, Figuier, and Podmore—raised the issue of fraud. While this has to be considered, it is important to recognize that there was no actual evidence for such an explanation. But as a consequence of this situation, the work was not generally accepted, something quite common in the history of physical mediumship and other areas of psychical research.

Although by modern standards, de Gasparin's accounts could have been more detailed, his work was an improvement in reporting in terms of providing support for nonconventional explanations. His contribution would have been more valuable, however, if he had reported more séances and had he worked with others in addition to Thury (1855). But I have the impression that de Gasparin, who was not a scientist, did not seem particularly interested in further careful work. In fact, his interests seemed to lay elsewhere. The séances in question take only a short section of his book. His concerns seemed mainly exegetic, as seen in his explanations of miracles and various other phenomena recorded through history in the greater part of the book. Furthermore, he was not only interested in séances. As seen in his biography (Borel 1879), most of his time after the publication of the tables book seems to have been taken by work and writings about social, religious, and political issues (e.g., de Gasparin 1861, 1868).

Regardless of evidential considerations, de Gasparin's work was certainly important in many ways. He contributed to rescuing table turning from the casual discussions in the press and popular books, and from the "learned" attempts to reduce all phenomena related to tables to delusion and unconscious muscular movements. It is less clear, however, how influential he was on later studies of table movements (e.g., Crawford 1916).

Regardless of his lack of further work, de Gasparin, it has been stated recently, upheld "reproducible experimentation" and showed in his work "an empiricist will denouncing prejudices and refusals of examination" (Evrard 2016:86). Because of this, he raised the bar in various ways in the study of table turning. This work presented important instances of tests attempting to counter objections empirically, and emphasizing phenomena inconsistent with the unconscious movement explanations of others. While a few conducted tests to see if they could support this hypothesis (e.g., Faraday 1853a), *many* others just accepted the argument without empirical examination (e.g., Chevreul 1854).

Even if de Gasparin's work was not successful in countering skepticism at the time, his effort inspired some like Thury (1855) and Crookes (1874) to conduct later work.[20] In addition, it is probable that it provided an impetus both for the development of then-current ideas of nervous forces, and more important the empirical approach typical of later developments in psychical research.[21]

Notes

[1] For discussions of the topic, see Crabtree (1993:Chapter 12), Figuier (1860:Volume 4:Chapters 14–17), González de Pablo (2006), Monroe

(2008:Chapter 1), Nisbet (1973), Podmore (1902:Volume 2:Chapter 1), and Wantuil (circa 1957).

[2] Some examples were accounts of knockings and movement of objects in the Friederike Hauffe (1801–1829) case (the famous Seeress of Prevorst, Germany, Kerner 1845), the Angelique Cottin case (France, Tanchou 1846), the Phelps and family case (Stratford, Connecticut, Capron 1855:Chapter 7), and the Cideville case (France, Owen 1860:272–283). See also the summaries of cases presented by Owen (1860:Book 3) and Podmore (1902:Volume 1:Chapter 2). Furthermore, the idea of physical phenomena connected to human beings was supported by claims of the influence of animal magnetism on plants and on instruments (e.g., Picard 1845, Rutter 1851).

[3] I have discussed this rich tradition of unorthodox concepts of force—fluids, human radiations, magnetism, nervous forces—in various papers (e.g., Alvarado 2006, 2009, 2016, Alvarado & Nahm 2011). For ideas about animal magnetism, see Gauld (1992).

[4] Another writer argued that the tables were affected by a nervous fluid directed by the will. This was the same will and fluid "which puts your fingers in movement when you write, and your legs in action when you walk" (Debay 1854:355; see also Alvarado 1981). For some later examples of nervous and vital force speculations, see my papers listed in Note 3 (Alvarado).

[5] Faraday's work received much publicity, and was reprinted in publications appearing in many countries, such as Australia (Faraday 1853b), France (Faraday 1853c), and the United States (Faraday 1853d). See also Alvarado (2000).

[6] In addition to veridical table communications mentioned above, there were other phenomena hardly explained by unconscious movements, among them difficulties in raising a table (Capron & Barron, 1850:69). One author mentioned that a young girl could raise a table but men and women could not (Tiffany 1851:200).

[7] For biographical information, see Anonymous (1871:332–333), Borel (1879), N (1871), and Ripley and Dana (1868:104).

[8] De Mirville (1855) answered many of the critiques throughout his book *Question des Esprits*. There were other critiques about de Gasparin's remarks about clairvoyant and other phenomena (Almignana 1889), spirit agency (Hare 1857), and sanctuaries in Palestine (Mislin 1858:485–513). An anonymous reviewer of the English translation felt that de Gasparin's comments were testy and lacking in calmness (Anonymous 1857a). Samson (1860:175–183) summarizes de Gasparin's arguments to naturalize the supernatural (see also Anonymous 1857c).

[9] It may be argued that de Gasparin's work is also part of Nineteenth-Century attempts to present overarching interpretations of many phenomena, as seen in the works of Crowe (1848), de Mirville (1854), and Gougenot de Mousseaux (1860).

[10] De Gasparin may have been influenced by similar previous discussions of the will in relation to animal magnetism (e.g., Chastenet de Puységur 1809:247, Deleuze 1813:Volume 1:90). Others writing about table turning also referred to the directing role of the will (e.g., Roubaud 1853:39). Rogers (1853:179) argued that the force responsible for phenomena

> is not, as a general rule, controllable by the will; not at all directly, as it is the agent of the unconscious organs, and plays its part automatically, as the organs of the brain are affected. That it is not acted upon, therefore, directly by the will, but indirectly.

[11] Silas (1853:20, 22, 23, 25) presents some possible cases of nervous reactions related to table turning, but the cause of these reactions is ambiguous at best. Another writer stated that somnambules were the best table turners, followed by persons of nervous and sanguine temperaments (Ogier 1855:114). Physician Félix Roubaud (1820–1878) (Roubaud 1853) stated that women in morbid states made tables move more than other sitters (p. 42), and that in rare cases headaches were associated with table turning. But he observed no relationship with nervous or epileptic attacks (p. 92).

[12] Crookes (1874:30, 36) and Hare (1855:Plate 3, 48–49, 51) also used water in some tests.

[13] Book tests became an important part of the study of mental mediumship years later (e.g., Thomas 1922).

[14] Ogier (1855:120) wrote that he covered a table with cork sawdust and that it moved without disturbing the dust. For another mention of talc, see Hébert (de Garnay) (1854:84). In a letter sent to Abbot François Moigno (1804–1884), chief editor of the popular science magazine *Cosmos*, a professor Stroumbo from the University of Athens (perhaps D. S. Stroumbo) communicated some tests he conducted in which sitters placed their hands on a plate over a table. In a summary of the tests, Moigno (1853:94) wrote that when dust was placed on the plate the table did not move. It was assumed that the dust prevented the adherence of fingers Stroumbo believed was necessary to produce table movements.

[15] See Dingwall's (1921) discussion of de Gasparin's fluid in terms of W. G. Crawford's (1921) later work.

[16] After mentioning de Gasparin, Castellan summarized aspects of the work of French researchers, among them Camille Flammarion (1842–1925),

Albert de Rochas (1837–1914), and Joseph Maxwell (1858–1938).

[17] Years later Figuier (no date:579) repeated his accusation, once again, without presenting supporting evidence. Both the Countess de Gasparin (1889) and Marc Thury (1889) criticized Figuier, arguing he had omitted important information from the séance reports that did not support his assertions.

[18] Sidgwick (1890:407) pointed out that de Gasparin did not present the testimony of his sitters.

[19] Podmore wrote earlier on about naughty children and poltergeists (1896).

[20] For later work with tables, including physical effects without contact, see Willin (2015).

[21] Guy Lyon Playfair, who refereed this paper, pointed out that Kenneth Batcheldor's "work was strongly influenced by de Gasparin's book, of which he had a copy and knew well." It is a matter of speculation how much he was also influenced by other early table-turning literature, as well as by later students of table phenomena such as Crawford (1916).

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LETTER TO THE EDITOR

JSE Tributes to Bob Jahn

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I sincerely appreciate the kind tributes to Bob Jahn in the Spring and Fall issues of *JSE* by Henry Bauer, York Dobyns, Roger Nelson, Tony Edwards, William Bengston, and Peter Sturrock. As you all know, SSE was very close to Bob's heart, for its community as well as its mission. He always looked forward to the annual (and European) meetings and the opportunities they presented for reconnecting with old friends, and making new ones, as well as for the intellectual stimulation they provided. And he took great satisfaction in being able to serve on the Council and to support the organization in any way he could. I know he would be deeply touched by all your warm remembrances, as am I.

—Brenda J. Dunne

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ESSAY REVIEW

Fake Medical News

Doctoring Data: How To Sort Out Medical Advice from Medical Nonsense by Malcolm Kendrick. Layerthorpe, York, UK: Columbus Publishing/YPS, 2015. 296 pp. $16.99 (paperback), $8.99 (Kindle). ISBN 978-1907797460.

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Fake medical news can kill. Unfortunately, much of the medical news disseminated everywhere is indeed fake in the sense of not trustworthy, perhaps most dangerously in what is generally accepted as sound medical practice. Much of the peer-reviewed mainstream medical literature is not to be trusted, as pointed out by John Ioannidis among many others.[1]

This book explains how to improve health and extend lifespan by exercising informed skepticism. The author, Malcolm Kendrick, M.D. (University of Aberdeen, 1981), practices in Cheshire (England). He had earlier published *The Great Cholesterol Con* (John Blake Publishing, 2008), and he posts regularly at https://drmalcolmkendrick.org.

When someone is obviously ill, or damaged physically through accident or warfare or other physical violence, modern-day medical practice can be splendidly effective. On the other hand, modern-day medical practice can be dangerously harmful for symptom-free people whose numbers on any of a variety of tests happen to deviate appreciably from a population average. So-called preventive medicine, almost exclusively based on prescription drugs, has a very high risk-to-benefit ratio, not least because the purported benefits have rarely—if ever—been demonstrated in actual practice.

Doctoring Data has 10 chapters described as "tools for establishing the truth":

1. Association does not mean causation
2. Lives cannot be saved; we are all going to die
3. Relative mountains are made out of absolute molehills
4. Things that are not true are often held to be true
5. Reducing numbers does not equal reducing risk
6. Challenges to the status quo are crushed—and how

7. Games are played and the players are . . .
8. Doctors can seriously damage your health
9. Never believe that something is impossible
10. "Facts" can be, and often are, plucked from thin air

Each chapter elaborates appropriately on its title, with true stories that drive the lessons home. Some of these instances would be unbelievable, were they not fully documented. I marked as worth quoting so many points that I can only urge that everyone read this book, often enough to absorb its lessons well enough to apply them whenever it is suggested that one take some prescription drug to ward off possible future harm. Difficult and onerous as it may be to seek out the pertinent specific information for oneself, there is simply no possible substitute, no available shortcut. There exist no authoritative sources that can be relied on safely for every topic. In my own experience, only after much reading on each topic could I identify trustworthy sources and individuals on specific issues: HIV/AIDS, global warming, Loch Ness Monsters; but those are three distinct categories, and those who are reliable about HIV/AIDS are not necessarily reliable about global warming or about Nessies.

To illustrate further the need for personal effort: I had read a great deal about claims that HIV does not cause AIDS, yet after about a decade I was still not sure one way or the other. Then a particular assertion stimulated a lengthy search of the literature that forced me to recognize that the results of HIV tests demonstrate unequivocally that what is being measured is not an infectious pathogen. My analysis (Bauer 2007) is available in documented detail for anyone to examine, but I doubt that it can carry the same conviction as it did for me as I was uncovering the primary data for myself and recognizing patterns.

That need for personal effort is the central lesson of Kendrick's book. Most people would surely find it unbelievable a priori that contemporary official statements about preventive medicine could be so ungrounded in evidence as they in fact are, and that the literature of medical research is so full of unsound material as it is. Only through personally encountering a sufficient number of examples does this become believable. It helps, too, to recognize the drastic increase in recent decades in sheer dishonesty, in science including medical science (p. 24 ff., see also Bauer 2017:Chapter 1).

Preventive medicine is aimed by definition at people who are not aware of any troubling symptoms. It is based currently on presumptions as to what are a healthy (or "normal") blood pressure, a healthy body–mass index, and healthy levels of blood sugar and cholesterol. This practice of prescribing drugs based on numbers and not on tangible symptoms began less than a

century ago (Greene 2007). This practice is irrational in presuming that the same numbers are normal or healthy for everyone irrespective of all the characteristics in which human beings vary. Furthermore, it has never been demonstrated that such regulation of numbers brings the purported benefits (e.g., Järvinen et al. 2011). Despite that, there is a continuing initiative—whose ultimate impetus comes from the pharmaceutical industry—to modify guidelines steadily in the direction of medicating ever more people. Thus "high" cholesterol meant >7.5 mmol/l (UK; in USA, 290mg/dL) in 1981, reduced steadily over the years to the current "optimum" of 4.4 mmol/l (170 mg/dL); yet some 85% of adults in the Western world have a higher level than that (pp. 7–8, 148). Moreover, the average cholesterol level of French people is slightly above the European average, yet theirs is the lowest rate of heart disease in Europe (pp. 84–85). The "Swiss . . . have the highest average cholesterol level in Europe . . . 250 mg/dl . . . . But . . . they have the second lowest rate of heart disease in Europe."[2]

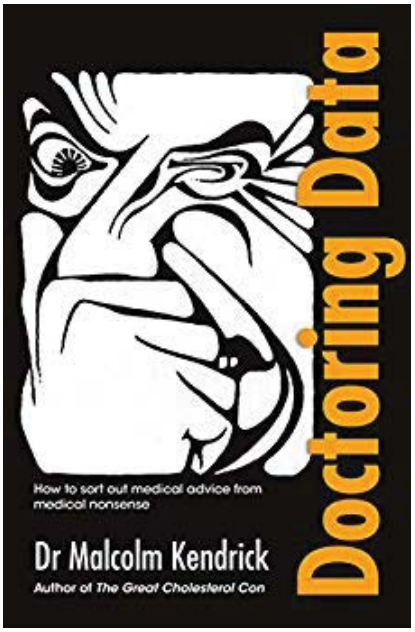
Similarly irrational are the guidelines relating to blood pressure, which ignore the long-established fact that blood pressure increases normally with age (Bauer 2012a).

Chapter 1 of *Doctoring Data* describes how current medical science routinely transgresses the basic fact that statistical association does not demonstrate causation.

Simple example: Yellow fingers are associated with, but do not cause, a higher risk of lung cancer: Smoking brings about both.

More intricate examples: Consumption of red meat was found to be *associated* with increased risk of death, particularly from cardiovascular disease and cancer, superficially supporting the standard "red meat increases cholesterol and cholesterol is bad" story. Yet the data also showed that consuming red meat was associated with *lower* levels of blood cholesterol. The missing, crucial, variables were that eating lots of red meat was also associated with more smoking, less exercise, higher intake of calories, and incidence of diabetes. Eating red meat may be entirely harmless if one does not smoke, is not obese, and is moderately active physically (p. 27 ff.). Again, a statistical association had brought the recommendation of hormone replacement therapy for women after menopause, said to reduce the risk of heart attacks—until, many years later, a proper clinical trial revealed the very opposite (pp. 31–32).

Chapter 2 exposes as hype and spin any claim that something "saves lives." When data actually showed only that people at high risk of heart attacks and strokes lived on average 3 months longer when treated with statins, multiplying by population size led to a press release asserting that if 10 million people were put on statins, "about 50,000 lives a year—that's



a thousand a week" would be saved (pp. 34–37).

Another type of number-inflating ruse is the subject of Chapter 3: reporting *relative* rather than *absolute* risk, a widespread ploy for misleading over the benefits of drugs. Say a certain medication decreased mortality by 50% in a one-year clinical trial: Obviously, everyone should take it. But if the trial had enrolled 20,000 people, 10,000 taking the drug and 10,000 not; and among the latter, two people died, among the former only one; 50% reduction, as stated. But the individual risk of death has been reduced only from 2 in 10,000 to 1 in 10,000—and the drug's side effects might well carry a mortality risk far outweighing that benefit. *Relative risk reduction should never be accepted as grounds for taking a medication*. What doctors and patients should attend to is NNT: the ***N***umber of people ***N***eeded to be ***T***reated for one of them to benefit. Yet even here there are pitfalls: If the intended benefit includes a large enough number of possible outcomes—avoiding not only death but also mere hospitalization, minor non-disabling stroke, angioplasty, bypass surgery—then the NNT can be made to seem much lower than is meaningful from a patient's point of view (pp. 46–48). Here Kendrick fails to make the important point that NNT should best be compared with NNH, which measures the harm done by a treatment's side effects—NNH = ***N***umber of people ***N***eeded to be ***H***armed before one of them succumbs.[3] However, that lacuna in Kendrick's presentation is of little or no practical importance at present since NNTs and NNHs are almost never reported.

Citing decreased mortality achieved by some treatment for, say, cancer, is misleading in much the same way as reporting relative rather than absolute results. ***Overall*** mortality is the critical variable. After all, death *from cancer* could be reduced to zero if the treatment for it—surgery or chemotherapy or radiation therapy—caused 100% mortality (p. 48 ff.).

In this connection Kendrick cites one of his genuinely shocking exposés of how the medical literature misleads through selective reporting. A trial of statins resulted in significantly lowered all-cause mortality; but an

examination of the data revealed that the benefit applied only to men—more women using the statin died than among the women on placebo; "in no statin study done has there been an [favorable] impact on overall mortality in women" (pp. 51–52; also p. 71).

Chapter 3 continues by pointing to ways in which standard statistics (frequentist, null-hypothesis, *p*-value) can mislead and do so routinely: "There are hundreds of different ways of choosing how to present data to make them seem to say certain things" without actually lying (p. 53). Such tactics are described in some of my favorite books: Huff (1954), Best (2001, 2004).

Chapter 4 deals generally with how and why some ideas not based on sound evidence become quickly and widely accepted while others, based on solid evidence, are pooh-poohed; there are many delectable quotes about this from a variety of authors. Illustrations include cholesterol-cardiovascular-disease theory, hormone-replacement therapy, and such dietary fads as wine/resveratrol and chocolate = good/bad.

Chapter 5 cites the copious data showing that reducing numbers does not reduce risk. The mistaken belief that it does stems from the reliance on surrogate endpoints or biomarkers: blood pressure and cholesterol level as though they measured a state or level of cardiovascular disease, blood sugar as though it measured a state of diabetes. Beyond that, blame also rests on "the linear model," the assumption that potential harm is linearly proportional to dosage, that risks to blatantly ill individuals are also present in proportionately milder form for everyone. In other words, that it may be beneficial to lower the blood pressure of individuals who suffer such tangible symptoms as extreme headaches means that it will also be beneficial to prescribe blood-pressure–lowering drugs when the pressure is only a bit above the population norm. That is an implausible assumption in the first place; further, the evidence against it includes the well-established phenomenon of hormesis (Calabrese 2004), whereby low doses of some poisonous substances, and also of radiation, actually improve health—plausibly, by stimulating the immune system into activity.

How far can a patient rely on expert opinion? In a study of how treatment impacted the quality of life, the doctors were sure that it improved, whereas relatives of the patients were sure of the very opposite; the patients' opinions were in the middle. A rather striking illustration of seeing what one hopes to see.

Chapter 6 is about the sad circumstances of people who swim against the tide, about which there is a copious literature (Bauer 2012b, 2017, and sources cited there). Experts by definition are those who propound the mainstream view; and peer review also serves to entrench what is generally

accepted and to dismiss and denigrate dissenters. When results go against the accepted view, researchers quite typically engage in semantic euphemism to hide the fact, by presenting the results as only *seeming* or *appearing* to contradict accepted doctrine.

Chapter 7 describes some of the deceptions that have become so common because research is almost exclusively funded by the pharmaceutical industry. Should anyone doubt whether the deceptions are deliberate, consider what the drug companies themselves say, for example, "effectively manage the dissemination of these data in order to minimize any potential negative commercial impact." That was advice from SmithKline Beecham to Glaxo Wellcome before they merged into the present-day Glaxo-SmithKline, in connection with their promotion for children of an anti-depressant, paroxetine, despite the associated risk of suicides (p. 155). The subsequent fine that GSK paid, *three billion dollars*, represented only one-quarter of what sales of paroxetine had brought in over the years (p. 156).

As to how authoritative guidelines are generated, consider the National Cholesterol Education Program (NCEP) in the National Heart, Lung, and Blood Institute of the National Institutes of Health. The listed financial ties of Committee members to drug companies cover more than a page of the book's text (pp. 160–161). The Committee recommended "Aggressive LDL lowering for high risk patients [primary prevention] with lifestyle changes and statins"; whereas the independent Cochrane Collaboration, reviewing almost the same set of studies, advised that "Statins have not been shown to provide an overall health benefit in primary prevention trials" (p. 162).

Big Pharma also creates and supports "public-interest" groups, "consumer advocacy" groups, and "charities" whose aim in truth is to promote drug sales. Thus HEART UK is Britain's "cholesterol charity" whose "vision" is "To prevent avoidable and early deaths caused by high cholesterol" (p. 172); the charity's supporters include such drug companies as AstraZeneca, Bayer Schering, Boehringer-Ingelheim, Bristol Myers Squibb, Merck, among others (p. 173).

Peer review does not keep things honest. Deceptive clinical trials are reported deceptively, as attested by Richard Smith (2005), long-time editor at the *British Medical Journal*, in "Medical journals are an extension of the marketing arm of pharmaceutical companies"; a view seconded by Richard Horton, editor of *The Lancet*, and by Marcia Angell and Jerome Kassirer, former editors at the *New England Journal of Medicine*. There are innumerable ways in which the results of clinical trials can be assured to support a desired outcome, and all of them are used routinely. Thus only a publicly funded trial found that statins provide no benefit at all (p. 145).

Chapter 7 concludes by urging potential consumers of presctiption

drugs to be skeptical and to seek out contrarian claims and voices before accepting the standard mainstream view. In Chapter 8, Kendrick reverses a common saying to urge, "Don't just do something, stand there" (p. 184). The stage is set by recalling some things that were accepted medical practices at one time but are now regarded as absurd and harmful, say, blowing tobacco smoke into the rectum (p. 183) or prescribing strict bed rest, which may have killed millions of people from 1912 into the mid-1960s (p. 187); just as AZT killed some unknown number of people after misleading clinical trials during the initial hysteria over AIDS (p. 189 ff.).

It is a dilemma for doctors: They wish to help people, and that makes it extremely difficult to admit that any standard medical practices might ever have been harmful rather than beneficial, though the evidence is clear enough that it has happened quite often.

In this connection, Kendrick believes that cardiac bypass surgery and angioplasty are performed far too often. I suppose he means when patients are not experiencing tangible symptoms. I personally benefitted from angioplasty after having fainted; and from a quintuple cardiac bypass 10 years later after being almost unable to breathe. What may be appropriate in extreme cases may also be unnecessary and inappropriate in the absence of tangible suffering.

On average, too many people take too many drugs. Especially with older people, health can often be dramatically improved simply by stopping many of their medications (p. 200 ff.). On the other hand, some forms of dementia may be associated with malnutrition: B vitamins might even help stave off Alzheimer's (p. 211, citing Douaud et al. [2013]).

"Never believe that something is impossible" is the tantalizing title of Chapter 9. Kendrick would replace the category of impossible by three choices: probable, possible, or unlikely (p. 213). He believes that harmful consequences of mitochondrial dysfunction[4] might include Chronic Fatigue Syndrome (CFS), Myalgic Encephalomyelitis (ME), and Autism Spectrum Disorder (ASD). One study indeed found "measurable mitochondrial dysfunction which correlates with the severity of illness" in people complaining of ME/CFS (p. 230). One of the co-authors of that study became so much non grata to Britain's National Health Service (p. 231) that she went into private practice.[5]

Nor does Kendrick discount the possibility that vaccination might indeed lead to autism. He cites a specific case in which vaccines were officially acknowledged to have significantly aggravated an underlying mitochondrial disorder, causing damage with features of ASD; the vaccine didn't, however, *cause* autism, it was said, but merely "resulted" in it (p. 223). Kendrick had published a number of times in a British journal for

general practitioners, but when he wrote a piece criticizing how Andrew Wakefield has been demonized, the journal simply would not publish it (p. 227).

Among things long labeled impossible or quack by medical establishments but that work: Pulsed electromagnetic energy is used to heal bone, and it may also work against depression and against migraines (p. 235).

Chapter 10 illustrates how "facts can be and often are, plucked from thin air"; for instance, the common dietary advice to consume five portions of fruit and vegetables daily was made up at a meeting of fruit and vegetable companies in 1991 in California (p. 243). So too with the cutoff values for body mass index where above 25 means overweight and above 30 means obese; Kendrick points out that such exact round numbers should always arouse suspicion (p. 245). Another shocking example is cited of how data on cholesterol and mortality were fudged to point to the very opposite of what they mean (p. 249).

Everyone should read this book.

Notes

[1] See periodically updated bibliography *What's Wrong With Medicine* at http://henryhbauer.homestead.com/WhatIsWrongWithMedicine.pdf or https://mega.nz/#!oOAhVaxA!BwxcAEUqYP4V5eDDwtPnWGwoJvkUpp5NVaPPD0akNHs

[2] What Is Your 'Statin by Date'? https://drmalcolmkendrick.org/2013/11/19/what-is-your-statin-by-date/

[3] How (Not) To Measure the Efficacy of Drugs. https://scimedskeptic.wordpress.com/2015/02/19/how-not-to-measure-the-efficacy-of-drugs

[4] Mitochondrial dysfunction has been suggested as the basic underlying cause of aging: "Bioenergetic therapy for aging: Mitochondria hold the key to cellular life and death." *Life Extension Magazine*, Cover Story, February 2001; citing A. W. Linnane, *Lancet, 1*(1989):642–645; *Biochimica et Biophysica Acta, 1271*(1995):191–194; *Annals of the New York Academy of Science, 854*(1998):202–213.

[5] Sarah Myhill. About My Practice: A Private Medical Practitioner—To Be or Not To Be? http://www.drmyhill.co.uk/wiki/About_my_practice

—Henry H. Bauer
Professor Emeritus of Chemistry & Science Studies, Dean Emeritus of Arts & Sciences
Virginia Polytechnic Institute & State University
hhbauer@vt.edu, www.henryhbauer.homestead.com

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BOOK REVIEW

The Star Gate Archives. Volume 1: Remote Viewing, 1972–1984 compiled and edited by Edwin C. May and Sonali Bhatt Marwaha. Jefferson, NC: McFarland, 2018. 546 pp. $95.00 (paperback). ISBN 978-1476667522.

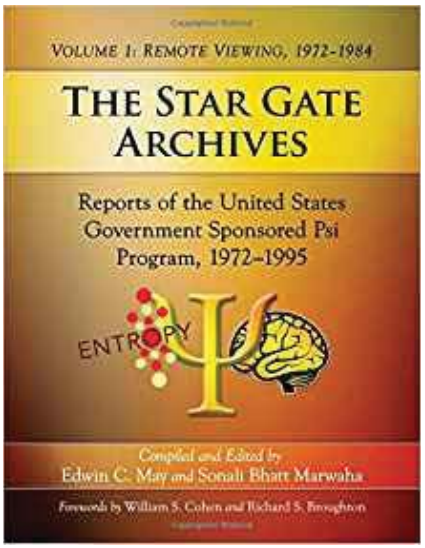
DOI: https://doi.org/10.31275/2018/1371

Remote viewing (RV) has been defined as ". . . the ability of a person to perceive, by an intellectual process, remote physical locations blocked from ordinary perception by distance or shielding" (p. 228). This was long the focus of research at the Stanford Research Institute (SRI) by Harold Puthoff, Russell Targ, and Edwin May, all with backgrounds in physics. May joined the researchers at SRI in 1976 and directed the research from 1985 until the closure of the Star Gate program in 1995 (the text on the back cover is misleading). Star Gate was the last name used for a military RV program that was initiated in 1977. That name was chosen because it ". . . invoked the feeling of exploration, a sense of reaching beyond our ordinary capabilities, of expanding the boundaries of our human potential" (Graff 2002:8). During the period covered by the anthology, the names used for the program were Gondola Wish, Grill Flame, and Center Lane.

Although the researchers were engaged in classified work, they attended conferences and presented some findings in the open literature. *The Star Gate Archives Volume 1* consists of a collection of unclassified reports and declassified documents. The anthology also includes no fewer than nine appendixes, a list of abbreviations, an extensive glossary, an author index, and a subject index. Brief comments about all reproduced documents can be found in the Introduction. The Editors, May and Sonali Bhatt Marwaha, have really tried to be helpful.

The Backstory

The U.S. and the Soviet Union had long been engaged in the Cold War after World War II. During the 1960s, U.S. intelligence agencies became puzzled and somewhat concerned because in the Soviet Union parapsychological research was once again being undertaken after a long hiatus. The CIA had dabbled with parapsychology earlier and funded Stephen Abrams (Black



2001), but had not developed any operational applications of psychic abilities.

Although exchanges with Western parapsychologists had been tolerated (though no doubt monitored) for some years in the Soviet Union, at the end of the 1960s the Soviet attitude changed as if the research had become classified. The sensational book *Psychic Discoveries Behind the Iron Curtain* (Ostrander & Schroeder 1970) became a best-seller in the early 1970s. This and other publications awakened the fear that the Soviets were ahead of the Americans in a psychic arms race. Ebon, almost alone, argued that ". . . American and Soviet researches in parapsychology are actually more complementary than competitive" (Ebon 1971:8). Several reports concerning the developments in the Soviet Union were requested (e.g., LaMothe 1972, Hamilton 1977, Wortz et al. 1976). The researchers at SRI, too, made threat assessments, mainly by doing their own experiments.

In 1972, first Targ and later Puthoff reached out to the CIA. Puthoff wrote a letter concerning an experiment at SRI with Ingo Swann, now known as the father of RV. That letter arrived at the right time and the CIA provided funding for additional experiments with Swann. In 1973, Swann and Patrick Price accidentally remote-viewed a National Security Agency facility (the West Virginia Site). In 1974, Price remote-viewed a site in the Soviet Union, at Semipalatinsk: "Several hours of tape transcript and a notebook full of drawings were generated over a two-week period" (p. 93). The reproduced documents cover all this, but not the evaluations—many remain classified (but see Kress 1977/1999, Richelson 2001, Stillman 1975). Wilhelm (1976, 1977) revealed to the world that the CIA had been interested in the research at SRI. However, by that time, Price was dead and the CIA no longer funded the research (they nevertheless continued to task RVers).

Introduction

Parapsychological research funded by intelligence agencies and psychic spying were bound to be controversial from the start. However, the RV program had some supporters in high places. Among them were Senators

William Cohen, Claiborne Pell, John Glenn, and Charles Rose. Cohen has written a brief Foreword: "I believe it was a mistake for us to abandon the effort to explore the power of the mind" (p. 1). The parapsychologist Richard Broughton has contributed a generous Foreword about the history of parapsychology and the RV research.

The Editors have written an informative Introduction, which covers the backstory and outlines the history of the RV program. The Editors have included commentary about important reviews of the program, timelines, detailed information about funding, and a glossary. In an appendix the Editors list subcontractors, which included parapsychologists such as Charles Honorton, William Braud, and Robert Morris. It seems clear that parapsychology benefited from the belief that the U.S. and the Soviet Union were engaged in a psychic arms race. Although the Editors acknowledge that the research provoked criticism, they provide no details.

Controversies

The first reproduced document is a proposal, now mainly of historical interest, addressed to the NASA Jet Propulsion Laboratory. Targ and Puthoff wrote

> Ultimately we would hope to obtain both an understanding of the ESP phenomena, and the skill to train subjects sufficiently to provide them with an ESP ability functioning at a useful operational level. (p. 34)

They got funding and their later report, concerning research with an ESP teaching machine, is reproduced. The study was criticized by Gardner (1975), and later he republished his article and the subsequent correspondence, including letters from the researchers (Gardner 1981). Apparently, people at the CIA also studied the report and were unable to explain the results (Kress 1977/1999). However, in light of their goal the results were disappointing, though a gifted subject was identified, Duane Elgin, who later participated in RV experiments.

In the 1970s, Uri Geller was famous for metal-bending, clairvoyance, and telepathy. A magician, James Randi, became well-known for his attempts to convince the public that Geller was just a magician and not a psychic. Geller was tested at SRI, and although many strange events occurred in his presence (Targ & Puthoff 1977/2005), in their report to the CIA the researchers concluded: "It was always necessary for him in the experimental situation to have physical contact with any metal he bent" (p. 66). Their films were ". . . insufficient to determine whether metals are being bent by normal or paranormal means" (p. 66).

Geller also participated in ESP tests, and in some he was to reproduce drawings. The first trials were single-blind (at least one experimenter knew the target): "Geller made seven almost exact reproductions of the seven chosen target pictures, with no errors" (p. 65). The results encouraged the researchers to have Geller participate in better-controlled experiments. They published the results of their research in the prestigious journal *Nature* (Targ & Puthoff 1974)—the publication was bound to provoke controversy. The Editors have reproduced the article. However, the issue that included the article also included a critical Editorial that is not reproduced. Randi (1975) argued that Geller must have fooled the researchers. In response, they circulated a Fact Sheet and in their popular book, *Mind-Reach* (Targ & Puthoff 1977/2005), commented on Randi's speculations.

In addition to the experiments with Geller, the article in *Nature* included the results of nine RV trials with Patrick Price—this, too, provoked controversy. Marks and Kammann (1978) discovered that the transcripts contained cues about in which order the trials had been carried out and argued that the cues had helped the judges. The researchers tested this hypothesis. Charles Tart edited the transcripts and the series was rejudged by a new judge—". . . seven of the nine were again correctly matched" (Tart, Puthoff, & Targ 1980:191). However, years later, when Marks and Scott (1986) were finally allowed to see the edited transcripts, they found that not all cues had been removed. It is unfortunate that the Editors do not discuss this controversy and other methodological problems in RV studies (e.g., see Kennedy 1979).

The Research

During the period covered by the anthology, 1972–1984, the focus was on evaluating the operational utility of RV and finding practical applications of psychic abilities. For example, the researchers tested whether Price could detect which envelopes contained secret writing. While two RVers were onboard a submersible, an attempt was made to use RV to communicate (by associating the targets with different messages), but only two trials were carried out. Twice RVers were provided with just a driver's license, despite this "excellent results were obtained" (p. 160). Studies involving search tasks were also conducted, but the researchers eventually concluded: ". . . on average, both the laboratory experiments and operational use have been disappointing" (p. 501). Nevertheless, the researchers gradually assembled an impressive body of evidence for RV which suggested that it could be useful now and then. However, the documents often only summarize experiments and operational RV sessions, and many of the descriptions are spare and inadequate for making assessments.

Much of the early research was exploratory, but there is a noteworthy exception, namely the research with Hella Hammid. The researchers became interested in her when they analyzed the results of an EEG experiment. The basic idea is that a sender is stimulated, in this case with a flashing stroboscopic light, and elsewhere a receiver is supposed to react. Similar studies had already been conducted, including one funded by the CIA (i.e. Duane & Behrendt 1965). Formal replication attempts of their pilot study were made with Hammid as the receiver. This research is described in several of the reproduced documents (some descriptions give the reader the impression that either four subjects or just Hammid participated in the first study when, in fact, six subjects participated). When taken together, the results are inconclusive. That said, Hammid turned out to be a good RVer (but see Marks 1981, 1982, Puthoff & Targ 1981).

> In our experiments, we have never found anyone who could not learn to perceive scenes, including buildings, roads, and people, even those at great distances and blocked from ordinary perception.
>
> (Targ & Puthoff 1977/2005:5)

Even CIA personnel occasionally participated in experiments and were able to RV. However, after having conducted mass-screening studies (Lantz & May 1988, Trask, Lantz, Luke, & May 1989), the researchers concluded: "Approximately 1% of the general population possesses a natural remote viewing ability" (p. 495). One is reminded of how J. B. Rhine and his associates early on discovered several gifted subjects, but later on had to work with ordinary subjects (Pratt 1975). If the researchers are to be believed, many people are able to RV now and then, but gifted subjects are rare.

Once the CIA realized what gifted subjects such as Swann, Price, Elgin, and Hammid could do, they wanted to know everything about them. One of the reproduced documents concerns the results of the extensive testing they undertook. Neuropsychologists may find the results interesting, but no clear profile emerged:

> Several years of observation by workers in the field has, however, led to an informal guide . . . successful remote viewers tend to be confident, outgoing, adventurous, broadly successful individuals with some artistic bent . . . (p. 352)

This is essentially what Frederick Atwater was told when he visited SRI International in 1978 (Atwater 2001).

The reason for Atwater's visit was that the U.S. Army Intelligence and Security Command (INSCOM) in 1977 had initiated a program (Gondola

Wish). The Soviets' interest in parapsychology still worried intelligence agencies. In 1978, the basic idea was that the best way to assess the threat was to teach Americans to RV. Eventually, six subjects were selected and their RV abilities assessed at SRI International. One of the reproduced documents concerns this evaluation. Part of this report was published by Targ (1994) in the open literature, and more recently Targ (2015) has shared some further details. It is noteworthy how little the researchers wrote about the subjects (cf. Schnabel 1997). Joseph McMoneagle (1997), now one of the best-known RVers, has revealed that he was subject 372. The reproduced drawings are certainly thought-provoking. The RVers went on to become psychic spies for the U.S. intelligence agencies, using RV to gather information about various sites and people.

In order to learn what characterizes good RVers, the Personality Assessment System (PAS) was administered. The PAS is ". . . a comprehensive interpretive framework for profiles of subtest performances that have been generated by the Wechsler Adult Intelligence Scale (WAIS)" (p. 422). The sample of RVers was small, so to bolster the sample additional participants were recruited, "persons who had spontaneously reported psi phenomena" and "people with demonstrated psi skills" (p. 423), but it is unclear how many of them were RVers. The researchers concluded:

> It appears that potentially good viewers appear in about five to seven personality categories and collectively represent about 10 percent of the general population. (p. 427)

The involvement of the military brought along the need to enhance RV and to develop a training program. The latter task mainly fell to Ingo Swann. He had already tried for years to understand RV, and gradually after extensive self-testing he developed what came to be known as Coordinate Remote Viewing, also known as Controlled Remote Viewing (CRV). The Editors have reproduced several documents about this. One of Swann's ideas was that during training the RVers should get feedback immediately—if what the RVer said was wrong Swann remained silent. Swann was hence not blind to the target during training sessions. Elsewhere, one of the Editors, Edwin May, has provided some frank critical commentary about this fact (May, Rubel, & Auerbach 2014). The researchers understood that Swann's training method was problematic, but he was allowed to continue. Swann's own RV appears to have improved, but the efficacy of the training for his trainees was in fact not always properly assessed at SRI International (due to lack of time and funds). There is no consensus about the value of CRV training in the RV community.

Summary

The Editors have put together an informative anthology that gives readers a good idea about the kind of research that went on at SRI International. However, since the book is dedicated to present and future generations of researchers, the lack of information about methodological problems is unfortunate. The methodological problems alone, however, are insufficient to dismiss the evidence for RV. That said, the reproduced documents ". . . were written as responses to explicit statements of work and, therefore, do not generally follow an academic model of scientific reporting" (p. 6).

It is not easy to say whether intelligence agencies should task RVers. The researchers often stressed that ". . . the information is fragmentary and imperfect, and therefore should not be relied on alone . . . " (p. 361), but the anthology offers enough evidence to make it clear why intelligence agencies tasked RVers. However, few operational RV sessions ". . . are carried out under the same conditions. Feedback in operational contexts is often limited, making evaluation difficult" (p. 399). The reproduced documents do not contain detailed information about the sponsors' evaluations, and many remain classified (though two examples are presented, one target was a chemical warfare storage facility in Germany and the other target was a biological warfare facility in the Soviet Union). The evaluations conducted while the program was active make it clear that the RVers' accuracy was highly variable. Some data are undeniably interesting, but much erroneous data were also generated by RVers (e.g., Lenahan 1981).

For various reasons, the focus early on had to be on operational utility and practical applications rather than on the basic research that was needed (Kress 1977/1999). In hindsight, it seems as if the RVers became operational too early and that CRV was taught before it had been properly evaluated; in fact, it was not even fully developed when Swann accepted his first trainees. There were mitigating circumstances. Because of the Iran hostage crisis, the military RVers became operational just months after their RV sessions at SRI International. The need for funding and the involvement of the military produced the need to offer a training program fast. Now that the Cold War is over, it is possible to step back and assess the evidence for RV and its operational utility—reading the *Star Gate Archives* is a good starting point.

—Nemo C. Mörck
nemomorck@hotmail.com

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BOOK REVIEW

This House is Haunted: The Amazing Inside Story of the Enfield Poltergeist by Guy Lyon Playfair. Hove, UK: White Crow Books, 2011. 280 pp. $17.95 (paperback). ISBN 978-1-907661-78-5. [Originally published in the United Kingdom in 1980 by Souvenir Press, 2007 edition published by The History Press]

DOI: https://doi.org/10.31275/2018/1372

This House is Haunted is probably the most popular of the late Guy Lyon Playfair's books. This third edition contains the original text, with minor alterations, a new Preface, and three short Appendixes in which he provides some afterthoughts, suggests further reading, and lastly gives a few suggestions as to "What to Do with your Poltergeist."

The Enfield case is one of the longest-lasting and comprehensive visitations of what I prefer to call Recurrent Spontaneous Psychokinesis (RSPK) in the literature. I say comprehensive because it involved almost all the known phenomena associated with these outbreaks. These included the usual furniture disruptions, "flying" objects, raps and other sounds, levitations of things and people, pools of water appearing out of nowhere, stones falling from the ceilings and flung at others outside from "many directions," written messages (on paper, but at least one, spelling "shit" on a wall, in excrement!), dematerialization/rematerialization of objects, spontaneous fires, direct voices, and so on. Objects as light as LEGOs were thrown, but a very heavy gas fire iron frame cemented into the brickwork was wrenched from a wall as well.

The phenomena were influenced by the comments, interests, and even thoughts of participants and visitors. For instance, when a student physicist who'd been working on metal bending came to stay, metal objects began bending. Soon after the news that another RSPK outbreak had occurred nearby in which fires were spontaneously started and extinguished, fire, similar to those observed by Playfair in the other house, broke out at Enfield. At the very moment Maurice Grosse, actually the primary investigator of the case, mentally wondered what "it would think of next," a loud crash announced that a framed sports certificate, which had been securely fastened to the wall, had been dislodged and knocked over a tape recorder as it fell.

Oh yes, and as in many investigations of psychokinetic subjects, recorders and other devices often malfunctioned.

Maurice Grosse, an inventor and design engineer, had recently tragically lost his daughter, Janet, in an accident, and he and his family had experienced some odd occurrences at the time that caused him to wonder if she somehow had survived physical death. He had a lifelong interest in psychical phenomena, and, in my opinion from what I've gleaned from the information in the book, had some, perhaps latent, psychic/psychokinetic ability himself. He applied for membership in and was admitted to the Society for Psychical Research.

When the call came in to the SPR asking for an investigator to look into the Enfield case, Grosse eagerly and quickly accepted it. The house was occupied by a Mrs. Harper and her four children: Jimmy, 7, Peter, 10, who was often away at boarding school, Janet, 11, and Rose, 13. It was suspected at first that Janet, who had the same name as Grosse's deceased daughter, was the focus of the activity, but as the phenomena progressed it seemed more likely that Rose and Mrs. Harper were also involved. Gross spent nights in the house and was kept busy by a dizzying variety of incidents including Janet's chair being balanced atop her bedroom door. When Playfair realized what a difficult and demanding situation Grosse was up against, he volunteered to help in the investigation.

Over the next two years, the phenomena increased in scope and intensity. Although both investigators expressed an awareness that the mayhem might be caused by repressed anger in this rather dysfunctional family, whose father had left them and whose periodic visits to deliver alimony or child support seemed to arouse only resentment, they, especially Grosse, maintained and pursued the belief that these anomalies were being perpetrated by spirits. (For example, Grosse says, "The thing knows they're coming . . ." or "It threw Janet off the chair" rather than "Janet was thrown from the chair.") Of course, the family, it seems, adopted this attitude as well. This belief conveyed by the researchers may have contributed to the unusual length of the outbreak. But with the mindset on spirits causing the PK rather than on living agents, both researchers seemed reluctant, especially Grosse, to look for psychological explanations, and seemed to be more interested in proving the reality of the phenomena than in uncovering the underlying psychological cause. Perhaps if it had been recognized and dealt with and explained to the family, the siege could have ended much sooner. Attributing the phenomena to spirits may have only given permission for it to go on.

At one point Janet was taken to a hospital for evaluation, and under hypnosis when asked who might be causing the trouble, she responded "me and my sister" because of "an increase in unhappiness." She related

that she and Rose were frightened by her estranged father and it was always worse after his Saturday visits. This might be Janet unconsciously feeding back under hypnosis what the psychiatrist wanted to hear, of course, but it seems a lot closer to the truth than the silly and unlikely spirit "messages" they "received."

That said, the fact that it did go on and evolved in many interesting ways gives us a little more insight into the psychic/physical connections at work in such cases of RSPK. And to be fair, no pun intended, Playfair seems to waver between the "living agent" and "spirit" theories. He welcomed John Beloff and Anita Gregory in the hope that they'd be able to help on the "psychological side," for he recognized that "poltergeist cases offer rare opportunities to study the interaction of mind and matter" (p. 113). Unfortunately, they spent only a short time at the house and they were able to contribute very little.

I found particularly interesting the role of Mrs. Harper in this case. She would often know when an incident was going to happen, even to predict it. She also seemed physically affected by the phenomena, e.g., when Janet bends a spoon her mother claims she feels a headache come and go "just as it bent." Another time, after a cardboard box filled with soft cushions was flung at Grosse hitting him on the forehead after he asked "Are you having a game with me?", Mrs. Harper told Playfair of a strange headache she almost always felt just before something like that happened. "It varies," she said. "If the 'thing' is hanging about, I get a throbbing sensation, and if it's going to be bad there's a sort of tight band across the front of my head. And then it will sort of go" (p. 72).

Late in the case when Janet was sent away to stay in a hospital, Janet opined that nothing would happen there because she was "on her own." "The power can't build up 'cause there's no one else to help build it up . . ." (p. 246). Playfair admits: "She and her mother seemed to understand instinctively that the overall family situation was what had caused, or at least helped, the trouble to start" (p. 247).

There was so much obvious anger, aggression, and violence demonstrated, not only in the flinging of objects and excrement, but in the deaths of a fish and a bird, as well as self-punishment especially directed at Janet, that I would have thought that more could have been done to examine

the mental–emotional problems of the family. Again, I think progress in this respect was hampered by the underlying resistance of both investigators to abandon their spirit theory.

As in several cases in the literature (see, e.g., Owen 1976, Owen & Sparrow 1977, Pilkington 2006) in which, following experience with macro-PK, phenomena begin to occur to participants outside of the usual setting, strange things started to happen to Grosse in or near his own home. His car engine revved up and down for no apparent reason, he heard footsteps and other sounds in his house, and finally he experienced what Mary Rose Barrington has termed "JOTT" or "jottles," i.e. the phenomenon of objects disappearing and later reappearing, or just vanishing for good (Barrington 2018). The day after Grosse checked his wife's jewels for insurance purposes, particularly noting a valuable diamond ring especially precious to her because it had been her mother's, the ring disappeared. It was always kept in a dressing table drawer. Of course they searched for it extensively and after six weeks Grosse reluctantly reported it missing to his insurance company. The following morning the ring reappeared in the same drawer from which it had vanished.

Grosse speculated that his late daughter ("*my* Janet") could be responsible not only for the ring episode but also for drawing his, and Playfair's, attention to Enfield. This search for a connection to his daughter and his desire to have some "proof" of an afterlife I believe strongly influenced his attitude toward the case.

Having had no success with the psychologists they had consulted, who unfortunately seemed to know nothing about the psychological dynamics involved in psychokinetic phenomena, they brought in still another medium who, with the help of his guide "White Cloud" (in the best traditions of British mediums, a so-called "red Indian"), put on quite a show of talking to resident spirits and so on. But he told Mrs. Harper and the girls that they had the ability "within themselves to stop all these things." Before he left he gave them some contact healing, placing his hands on their heads and spines. They said they felt the heat coming from his hands, and following his visit the family actually experienced some peace for a short time, but despite Playfair's and Grosse's suggestions that the troubles were over, the phenomena started again after a few weeks.

The last medium who was brought in was interesting in that he strongly sensed Grosse's daughter Janet, who would have been 24 years old.

The "revelation" put the two investigators back onto the spirit explanation, which I think fogged their thinking despite the fact that this medium, when asked how to stop these outbreaks, replied, "Cure the people in the house" (p. 249).

This bit of advice reminded me of Scott Rogo, whose untimely death was a great loss to the field. Scott said that when he investigated a "poltergeist" outbreak his first thought and obligation was to help the family, rather than to just go in to study the phenomena and verify their reality.

In the "Afterthoughts" section written in 2011, Playfair debunks claims made by some journalists that the girls had "confessed" to causing the phenomena and adds some acoustic charts showing, as other researchers have found, that raps heard in the Enfield house and those made by normal means have different sound patterns. He also provides a bibliography for further reading.

This is a very readable, eye-opening book for anyone who is not familiar with the extent and variety of phenomena that have been witnessed in RSPK cases, and even experienced researchers might glean some new insights into the psychology and family dynamics that can underlie these outbreaks.

In my opinion, large-scale psychokinesis, whether spontaneous or produced consciously by gifted individuals, is the most important area of parapsychology. As Grosse remarked to a group of mental health practitioners at Janet's hospital:

> Cases of this type, whatever their true nature, are of great interest because they involve unmistakable interactions between mind and matter. They would seem therefore to be the province of both physicist and psychiatrist. (p. 234)

And as Playfair adds:

> It now seems fairly certain that poltergeists need an atmosphere of group tension in which to operate, and that psychiatrists can help a good deal by dissolving that tension. But it must be up to the physicist to identify the force that turns tables over . . . (p. 248)

—Rosemarie Pilkington
rpilkin603@aol.com

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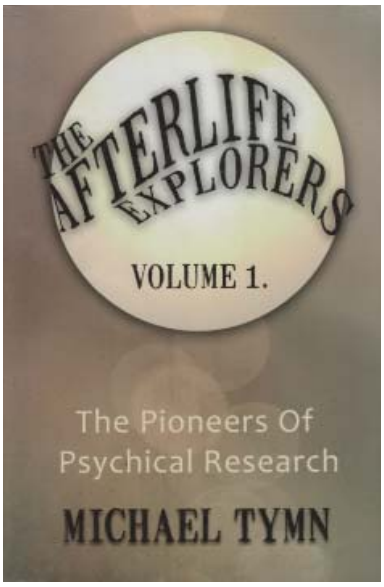
BOOK REVIEW

The Afterlife Explorers, Volume 1: The Pioneers of Psychical Research by Michael Tymn. Hove, UK: White Crow Books, 2011. xix + 148 pp. £9.99 (paperback) ISBN: 978-1-908733-00-9.

DOI: https://doi.org/10.31275/2018/1379

Afterlife explorers are, for Michael Tymn, people who actively sought and 'found' "evidence of man's survival after death" (p. ix) during the period from the mid-1800s to the 1930s. After that period, psychical research "changed its focus (. . .) to research in extra-sensory perception (ESP) and related psychic phenomena" (p. x), especially because scientists and scholars "began to realize that they . . . would never succeed in producing evidence to satisfy either the scientific fundamentalists or the religious fundamentalists" (p. ix). So, to write an overview of what he calls "exploration" of the afterlife, Tymn organizes the topic into twelve portraits, in which, in addition to brief hints about the individuals he intends to analyze, he offers quotes from works they published or that have been published about them.

The book opens with a Preface and an Introduction, where the author explains that the present volume is the first of four, which solely concerns the epoch before 1882. That date was chosen as a "turning point" because it was marked by the founding of the Society for Psychical Research (SPR), which began the modern way of exploring the realm of the occult. The author is convinced that to trace back the origin of psychical research, admittedly under other names, we can move far back in time to medieval "investigations" by the Church of alleged miracles. For the later part of the period, he refers to the works of Swedenborg (second half of 18th century) and Kerner (early 19th century), true precursors, according to him, of the Rochester knockings connected to the Fox sisters, in turn a "seminal event that gave rise to the advent of 'modern' psychical research" (p. xii). Subsequently, other individuals took care of, and studied, mediumship, often moving from skeptical positions to become "believer(s) in the reality of mediumship and in spirit communication" (p. xv). One of these, the physicist William Barrett, was the driving force for the establishment of the SPR and the ASPR (American Society for Psychical Research). A timeline, starting from Emanuel Swedenborg's personal investigation of afterlife



realms in 1741, until the "organization of the SPR" in 1882, delimits the timespan covered in this volume.

Each of the twelve chapters is dedicated to one person: E. Swedenborg, A. J. Davis, J. W. Edmonds, G. T. Dexter, N. P. Tallmage, R. Hare, V. Hugo, A. Kardec, A. R. Wallace, D. D. Home, W. Crookes, and W. S. Moses. Most of the biographical sketches include some quotes from the person's works, and a detailed description of an event of "close contact" with spirits. Each chapter ends with quotes of statements proposed by those authors or their "spirits" regarding a few matters considered important by Tymn. In this repertoire, the choice is usually different from one author to another, so that we have, for example, for George Dexter, quotes on "Purpose," "Liberation," "Benefits," "Positive Influence"; for Victor Hugo, "Spirit Presence," "Doubt Necessary," "Spirit Awareness"; for William Stainton Moses, "Conflicts with Christianity," "Spirit Language," "Subconscious Explanation"; and so on.

Lastly, four Appendices contain "teachings" believed to have come from prominent spirits, excerpted from the published books of A. J. Davis, J. W. Edmonds and G. T. Dexter, Allan Kardec, and W. Stainton Moses.

Although useful to quickly get an idea of the way in which concepts and doctrine developed in Spiritism in the period under examination, the book possesses some weaknesses. First of all, from a historical point of view it risks giving an incorrect idea of what psychical research really has been and is now: a discipline, started in the last two decades of the nineteenth century, that tries to clarify through cautious and strict investigations the occurrence, true nature, and governing rules of anomalous psychic phenomena. By including in psychical research attempts to find evidence of "life beyond death," confusion is created. And by ascribing authors to the field, as Tymn does, who are certainly unrelated to that discipline, as are seers and mediums (E. Swedenborg, D. D. Home, etc.), literate men who were not researchers (e.g., Victor Hugo), spiritual leaders (Allan Kardec, W. S. Moses), and others, it obfuscates the fact that for decades psychical researchers have primarily studied psychic phenomena such as (seeming) telepathic, precognitive, and clairvoyant experiences, poltergeists, hauntings, and much less so

mediumship. Not to say that the very first activity of the SPR was marked by the decision to not assume a corporate position on the afterlife and mediumistic phenomena (as well on other phenomena) until clear evidence had emerged in this regard—and even today has not emerged.

Even from a fideistic perspective, however, Tymn's work shows flaws, because it is completely silent on questions one could ask while reading the book. In the first place, the author explicitly affirms, or refers to, the importance of becoming convinced *during the present life* of the survival of bodily death: but he never says why this would be important. Perhaps to believe in survival guarantees survival or a *better survival* more than if one does not believe? Or could that faith provide us (better) guidance on how to behave now in a certain way? (Which?) Moreover, considering that the vast majority of human beings already believe in survival (as followers of one of the world religions), it is not clear how faith in survival announced by a few mediums should or could have relevance.

On the other hand, it seems risky to include among the scouts of the afterlife Sir William Crookes, a scientist who has performed studies on the physical phenomena of Daniel Dunglas Home, and this renowned medium, too, who has produced very scarce "communications," of either ethical or philosophical value, apparently coming from the spirit world.

Finally, it should be noted that at no point in the volume does Tymn state the reasons for his belief in the *reliability* of the quotes he reports. It must be specified that he does not alter any of the materials he takes into consideration and uses; but to solely refer to the professional honesty or to a vague "authority" for the people he chooses is not enough to make them acceptable.

In other words, how do we know that the assertions for example of Nathaniel Tallmage or of Robert Hare on the afterlife and the meaning of life correspond to any reality? Why are the claims of Allan Kardec or Stainton Moses, or those of the alleged spirits communicating through them, important or *more important* than those of any other person who may have said otherwise? The lack of a clear discussion on the reliability of all that is presented represents a main weakness of this work.

—Massimo Biondi
mbiondi10@libero.it

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CALL FOR PAPERS

The Call for Papers for the 62nd Annual Convention of the Parapsychological Association in Paris, France (July 4–6, 2019) is out! Note the earlier-than-usual deadline of February 4th. We look forward to seeing what you might contribute to this event!

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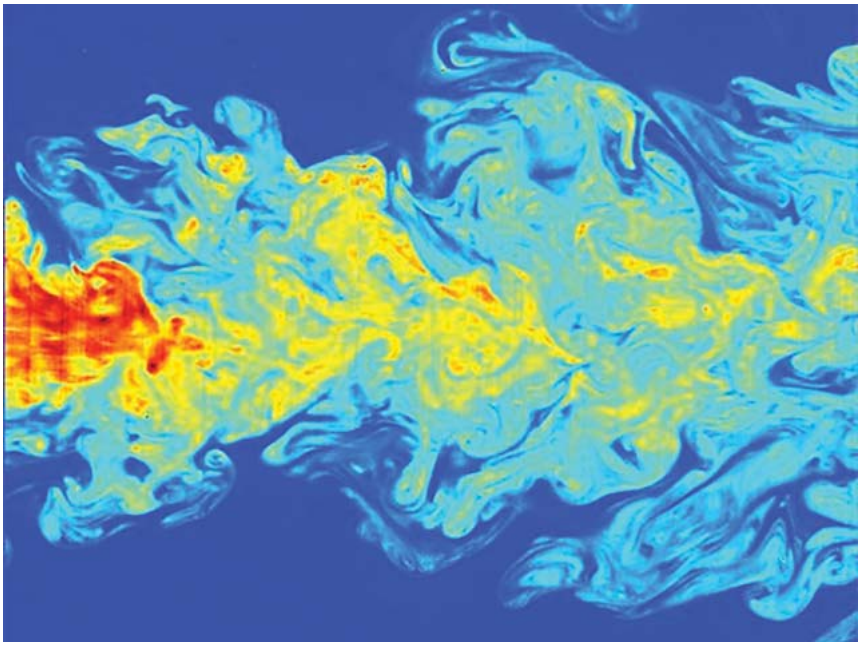

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