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SSE NEWS

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EDITORIAL

One of the most valuable features of the early years of both the *Proceedings* and *Journal* of the Society for Psychical Research was the frequent publication of intriguing (and often scrupulously investigated) anecdotal reports. Indeed, the enterprising early SPR researchers produced some mammoth reports based on such material, including its 400-page "Report on the Census of Hallucinations" (Society for Psychical Research 1894) and the monumental *Phantasms of the Living* (Gurney, Myers, & Podmore 1886).

The pioneers of psychical research were shrewd enough to realize that apparent spontaneous occurrences of ESP and PK, and phenomena suggestive of postmortem survival, could provide valuable clues as to the nature of psychic functioning, and that the collection and careful study of this material was an essential precursor to doing serious theoretical work in the area. Sadly, that lesson seems not to have entirely survived the gnawing tooth of time, as more and more would-be psi researchers, in a misguided attempt to appear conventionally scientific and curry favor with mainstream science, confined their activities and attention strictly to the laboratory, having little familiarity with or comprehension of the day-to-day apparent eruptions of phenomena that drove earlier researchers into the lab in the first place.

I mention this now because this issue of the *JSE* features a contribution by Russell Targ to the material suggestive of postmortem survival—two incidents pointing to the survival of his daughter Elisabeth. The two incidents are considerably more intriguing than most, and they highlight what is probably the most recalcitrant issue in the debate over survival—namely, the apparent standoff between the survivalist and living-agent psi (LAP) interpretations of the evidence.

Because *JSE* readership has a wide range of interests beyond topics in parapsychology, I hope psi veterans will indulge me if I review briefly, primarily for the benefit of others, just what the relevant issues are here.

We should observe first that the type of postmortem survival at issue is more interesting and personal than the scenario envisioned by some Eastern religions: a kind of merging with the infinite or being-in-general. That might count as a kind of *life after death*, but that form of continuance would obliterate whatever is distinctive about us. By contrast, *survival* of death is typically considered to preserve some kind of *identity* between an ante-mortem individual and a postmortem individual. That's why those

who wonder about survival also wonder about such things as whether *they* will be able to meet up with their deceased relatives, communicate with the still-living members of their families, or enjoy a postmortem existence in which they simply get their hair back.[1] In general, they wonder whether *they* will continue to exist in some form or another after bodily death. And they wonder whether that future individual bears something like the same relationship to their present self that their present self bears to their physically and psychologically remote infant self. And it's why those who consult mediums or study reincarnation cases look for evidence that some deceased person's knowledge, traits, or skills continue to manifest.

Now as I'm sure all *JSE* readers are aware, there are many types of cases at least superficially suggesting postmortem survival. But for all of these there are non-survivalist explanatory options which any clear-headed appraisal of the evidence must consider seriously and which those favoring survivalist explanations must strive to rule out. The first wave of non-survivalist explanations would be in terms of what I've called the "Usual Suspects"—namely, malobservation, misreporting, hidden memories (cryptomnesia), and (of course) fraud (see Braude 2003, 2014). These can be easily ruled out in the most interesting survival cases, and so the debate over the evidence naturally turns to the next wave of non-survivalist explanations, in terms of what I called the "Unusual Suspects"—namely, rare or abnormal processes, such as a combination of dissociation and latent creative capacities, or exceptional (e.g., "photographic") memory, or something analogous to extreme or rare forms of savantism. Although these at least seem to be ruled out fairly easily in the strongest cases, some argue that they are more difficult to reject than many writers on survival have appreciated. Moreover, as I mention below, there are purely logical reasons why these Unusual Suspects may be difficult to dismiss.

But even when the Unusual Suspects seem unable to account normally for the evidence, a more intractable non-survivalist explanation remains—what most misleadingly call "super psi" but which Michael Sudduth more accurately dubbed "living-agent psi" (see Sudduth 2009, 2014, 2016). The reason why survivalists must take this seriously is easily illustrated in terms of a typical good case of mediumship. No matter how obscure the information provided by a medium, if that information can subsequently be verified normally, then in principle it can also be explained in terms of the medium's ESP. One of the earliest and most significant writers to take this issue seriously was E. R. Dodds (Dodds 1934). Similarly, in reincarnation cases one can appeal to ESP on the part of either the subject or relevant interested parties (such as family members), or to the paranormal influence they exert (presumably telepathic or psychokinetic).

Some survivalists reject these explanatory strategies because (they say) the LAP hypothesis posits psychic functioning of an implausible degree, and more than that for which we have evidence outside of survival cases (see, e.g., Almeder 1992, Fontana 2005, Lund 2009). However, others counter that survivalist line of argument by claiming that it's confused on two grounds: first, because there's no clear standard for evaluating the magnitude of psychic functioning, and second (and most important), because the argument overlooks a crucial (and ironic) logical entailment of the survivalist position—namely, that survivalists are committed to positing comparably impressive psi on the part of the deceased or the living, simply in order to explain how evidence suggesting survival was manifested in the first place.

For example, suppose a medium channels, without prompting and without normal access to the information, the message "Uncle Harry knows you're seriously thinking about quitting your job and becoming a circus clown." Even for survivalists, some kind of ESP must be posited merely to explain how the medium knows what deceased Uncle Harry is thinking, and how deceased Uncle Harry knows what the sitter is thinking. In each case, those would be examples of direct mind-to-mind interaction—or, in other words, telepathy. Or suppose the medium reports, "Uncle Harry says he's glad you're wearing the necktie he gave you." In this case, if the medium doesn't know normally who gifted the necktie, survivalists must posit psi involving deceased Uncle Harry to explain how he can be aware clairvoyantly of what the sitter is wearing, and how that information was exchanged telepathically between the medium and Uncle Harry.

We must also note one additional, and very important, introductory point concerning the logic of explanation. Survivalists often maintain that the LAP explanation of cases compares unfavorably to that of the survivalist. They usually support that claim by arguing that the survivalist explanation is simpler, or that it has greater explanatory power, or that it does a better job of predicting the data, than the living-agent psi alternative, or else that the LAP explanation of the data is indefensibly ad hoc. But Sudduth (2016) has noted that this type of comparison of the LAP and survivalist hypotheses seems plausible only in virtue of a kind of logical sleight of hand. As noted above, survivalists typically claim that the survival hypothesis explains (or predicts) various strands of evidence. But such explanation or prediction is possible only if one makes a number of *auxiliary assumptions* about the nature and character of the afterlife. For example, in cases of mediumship we find that communications are often trite, confused, or have a dreamy quality, and that at other times they seem quite clear and coherent. We also find that only some deceased people seem to communicate, and then only

for a short time. Why is that, and how do survivalists account for it (and the many other observed features of mediumship)?

On this issue, the literature on survival is too often discouragingly shabby. The problem is this. In order to explain both why the evidence from mediumship has the features we find and why it lacks certain others is to make numerous, *independently unverified* assumptions about (say) whether deceased persons would want to, or be able to, communicate with the living, the means by which that communication is achieved, and whether that communication is difficult or easy (e.g., whether there's "noise" in the "channel"). By contrast, a *simple* survival hypothesis—that is, a mere assertion that consciousness or personality can survive, *in the absence of further assumptions specifying conditions allowing the evidence to take the forms noted in the literature*—can make no specific (much less fine-grained) predictions at all about what the data of survival should actually look like. The same is true, obviously, about the living-agent psi hypothesis, which, in its more robust and sophisticated forms, makes numerous assumptions about (say) dissociative creativity, and the needs and interests of the living, in order to explain why the evidence has certain characteristics rather than others.

However, as Sudduth (2016) has noted, when survivalists try to claim that the survival hypothesis explains (or predicts) the evidence *better* than the LAP hypothesis, they usually compare robust versions of LAP (allegedly laden with implausible assumptions) only to a *simple* survival hypothesis—minus the sorts of assumptions required for that hypothesis to do any explanatory work at all. The proper comparison, however, must be between *robust* survival and *robust* LAP hypotheses, where each is bulked up by assumptions that permit the prediction of the observed, fine-grained features of the data. But in that case, the empirical argument for survival may amount merely to a comparison of the auxiliary assumptions attaching to both the LAP and survivalist hypotheses.

Now that's not an easy task, and a shoot-out between competing sets of auxiliary assumptions is likely to lead nowhere, at least not conclusively. That's why many feel that a slam-dunk *actual* (as opposed to theoretically ideal) case of survival will leave the survival vs. LAP debate at a standoff (but see Braude [2003, 2005] for a discussion of the significance of ideal cases). Nevertheless, good actual cases (like those described by Targ) still provide the raw material for getting clear on what kinds of auxiliary assumptions are needed (on either side of the debate) to accommodate the data.

So it may well turn out, especially in the absence of anything seriously approaching the ideal cases we can imagine, that our conviction (or

lack thereof) about the prospects of survival will rest on how personally compelling we find the evidence before us. An honest appraisal of the best cases requires that we be clear what we're assuming to be the case for the evidence to have taken the form(s) that it does, and how those assumptions fare against the most reasonable contrary assumptions of others. At that point, it may be that the best we can do is to make an educated guess. Indeed, I'd go so far as to say that those who think we have air-tight scientific grounds for believing in postmortem survival are simply flaunting their ignorance. Still, we can have *defensible* grounds for believing many things which don't admit of compelling scientific demonstration, and survival may simply turn out to be one of them. My book *Immortal Remains* documents in gory detail why a careful survey of the evidence and relevant conceptual issues makes it difficult to reach a confident decision on the matter.[2] Nevertheless, I concluded that book by asserting that the scales seemed to tilt in favor of survival. Now although my views don't change as frequently as Bertrand Russell's did, that was 14 years ago. So, for the record: As of today at least (I still sometimes waver, I'm embarrassed to say), this chronologically challenged Editor continues to stand tentatively on the side of the survivalist.

Notes

[1] Many years ago the famous medium and healer, Olga Worrall, told me she saw my deceased maternal grandfather standing behind me, and she described him as having a beard. I told Olga that my grandfather never had a beard, and Olga replied, "He does now; he can have a beard if he wants."

[2] Sudduth (2016) should also be required reading.

Stephen E. Braude

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RESEARCH ARTICLE

Laboratory Research on a Presumably PK-Gifted Subject

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Darío Burgo

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Abstract—Between June 2014 and December 2015, a PK laboratory was organized in Buenos Aires. Up to five video cameras were installed to record the events. Various devices were assembled to measure physical, physiological, and environmental variables. 23 meetings were held with a presumptive PK subject, identified in previous research. The subject was apparently able to move a table at will, through an alleged "PK force," and the phenomena were documented and recorded on several occasions. Although contactless movement of the table or other objects could not be achieved, muscular effort was ruled out as the cause of the observed movements. One experiment developed by William Crawford was repeated, although Crawford's results were not replicated. EEG studies were performed with the subject at rest and also during the production of the phenomena. Unexplained anomalies were observed in the EEG data obtained during the production of the phenomena, and the normal curve of a random number generator also deviated significantly ($p = 0.008$) during the trials. No variations of electric and magnetic fields were found to be associated with the phenomena. Stephen Braude visited the laboratory and attended 3 meetings. He offers his observations and commentary in the Appendix.

Background

People who could apparently produce ostensible psi phenomena more or less at will were the essential raw matter for the first psychical researchers of the nineteenth century. However, their prominence declined somewhat when J. B. Rhine and others changed strategy and conducted psi experiments using more ordinary people as subjects. Nevertheless, the search for and the investigation of psychically gifted subjects still occupies a strategic place within the parapsychological community. Moreover, although ostensibly gifted PK subjects have been identified regularly since the late nineteenth century, the investigation of those subjects has been, and remains, a challenge.

One of the emblematic cases is that of Nina Kulagina, a Russian woman visited by several prominent investigators (Benson 1972, 1973, Pratt &

Keil 1973, Cassirer 1974, Keil & Fahler 1976, Keil, Benson, Ullman, & Pratt 1976) who reported observing distant movements of tiny objects and the deviation of a compass needle up to 70°. Cold War tensions prevented additional, and closer, examination of Kulagina's phenomena. Another promising case (unfortunately failed in the experimental stage) was that of Felicia Parise, a co-worker of Charles Honorton. After watching some Kulagina films, Parise found she could repeat some of her feats working in informal conditions (Honorton 1993). However, although in a later visit to The Foundation for Research on the Nature of Man, Parise could deviate a compass needle and change the signals of a metal detector device (Watkins & Watkins 1974), she refused to go on, claiming she felt uncomfortable with the proposed method of work—as she revealed in a recent interview (Pilkington 2015).

Other subjects have collaborated more enthusiastically. Eusapia Palladino could levitate tables putting her hands over them, move distant objects, and produce apparent materializations; she was studied thoroughly by many researchers (see, for example, Feilding, Baggally, & Carrington 1909, Bottazzi 2011, Morselli 1908, Carrington 1913, Courtier 1908, and the discussion in Braude 1997). One of the first special subjects who gladly agreed to be investigated was D. D. Home. Home was investigated meticulously for nearly 25 years by many researchers. These included William Crookes, who designed several devices to register and certify the reduction in weight of bodies and the displacement of objects, and who concluded,

> These experiments appear conclusively to establish the existence of a new force, in some unknown manner connected with the human organization, which for convenience may be called the Psychic Force. (Crookes 1874:9)

For a recent survey of Home's case, see Braude (1997).

A long series of systematic experiments to study the mechanisms of PK was that of the engineer William Crawford (Crawford 1916, 1921), who organized a sitter group through a spiritualist circle focused on an exceptional subject, the teenager Kathleen Goligher. Beginning in 1915, Crawford conducted more than one hundred sessions with Miss Goligher, the results of which led him to postulate that "psychic rods" of ectoplasm exiting the body of the subject stick to objects and move them. Crawford even succeeded in photographing some of those apparent ectoplasmic extrusions. It should be noted, however, that these "rods" were also discussed critically, and that some commentators accused Miss Goligher of fraud. (For further discussion, see Braude 1997 and Nahm 2014a.)

In any case, although some of the physical phenomena attributed to Miss Goligher have been reported by other investigators, Crawford's exact tests have never been replicated (or apparently even attempted), and no other investigators have photographed ectoplasmic rods such as those provided by Crawford. Another impressive case was that of Rudy Schneider, studied by Eugene and Marcel Osty (1931) at the Institut Métapsychique International of Paris, where Schneider not only moved objects at a distance but also interfered with the path of an infrared beam. (These cases are also surveyed in Braude 1997.)

The Red Lights Group

Nowadays, there is not much interest in searching for and investigating promising macro-PK subjects. Most such subjects emerge, as in the past, through the activities of so-called sitter groups. Kenneth Batcheldor (1966, 1984) helped to reinvigorate interest in this activity and to demonstrate its suitability for parapsychological research and theory construction. The Red Lights Group in Buenos Aires was founded on Batcheldor's ideas as well as those taken from other similar and successful projects of the last decades (Owen & Sparrow 1976, Williams & Lang 2002, Storm & Mitchell 2003, Wilson, Williams, Harte, & Roll 2012). These various studies have much in common, including a shared belief that PK-induced table movements are possible.

The Red Lights group began to work in April 2013 (Gimeno 2015). The plan was that the nine group members would meet once a week for at least three months, to sit around a table with hands on top, with good illumination, and with a coordinator urging *"If there is someone present, able to move the table, or produce raps or other physical phenomena, we invite you to try, as we are here for that."* From the very first meeting, the table exhibited anomalous movements, and these increased in number and magnitude as weeks passed, occasionally becoming quite intense and uncontrollable. For example, in one meeting the table began to rotate right and left (like a compass), 40° or 50° to each side violently and rapidly (approximately twice per second). In one of the last meetings a strategy was designed to identify which sitter(s) were responsible for the movements. To do that, the coordinator asked each attendee to leave the table, one after the other. That procedure indicated quite clearly that Ariel Farias was the only sitter whose presence at the table seemed necessary for the table's movements. Because Ariel was willing to collaborate in a long-term investigation, the authors organized a formal Psychokinesis Laboratory for that purpose. The present report describes the work done during this research.

The participants in the Red Lights Group meetings were strongly

motivated to obtain results, and that motivation increased further when some participants attributed the table movements—although without any evidence—to actions by a recently deceased relative of one of the attendees. Those sitters believed that the spirit of the deceased relative had agreed to help the investigators move the table in the way they requested. In the ensuing enthusiasm for their apparent contact with the deceased, many felt that the phenomena should escalate and perhaps lead to the total levitation of the table. But the identification of Ariel as the probable sole and indispensable causal agent demoralized most other attendees who had also wanted to be the psychically gifted subject. From that moment on, the psychological atmosphere of the sittings deteriorated, and various attendees began to miss the regularly scheduled meetings.

We had considered organizing a new sitter group to accompany Ariel, modeled after the earlier Red Lights Group. However, it became clear that the complex interpersonal relationships of the former group participants had complicated not only the documentation of the phenomena but also the attempt to rule out the hypothesis of fraud. As a result, we adopted an alternative plan of working only with Ariel, on the assumption that he was indeed the sole (or at least the principal) causal agent responsible for the table movements. We knew there was a risk that the phenomena would decline in magnitude and frequency, as they had for many former PK subjects, especially since the investigators could not duplicate the motivations and excitement of discovery that characterized the activity of the Red Lights Group. Another concern was that Ariel had some fear of developing weird or unpleasant phenomena which some Red Lights Group members had assigned to the activity of discarnate spirits (a point of view for which Ariel had little sympathy).

The Laboratory

The working group was managed by Alejandro Parra, in collaboration with the investigators Juan Gimeno and Darío Burgo. The place selected to install the laboratory was the Instituto de Psicología Paranormal de Buenos Aires (The Institute for Paranormal Psychology of Buenos Aires). The Institute allowed us to use two rooms, one for general work (24 m^2) and another to store equipment and hold some special meetings (16 m^2). The main source of funding for this work was from a Gilbert Roller 2014 grant, devoted to fund research projects in the field of macro-PK, awarded by the Parapsychological Association.

To facilitate the measurement and recording of phenomena, the authors built a large wooden frame, similar to a cube with sides of 2 meters. The meetings were recorded with a video device PCBOX model PCB-

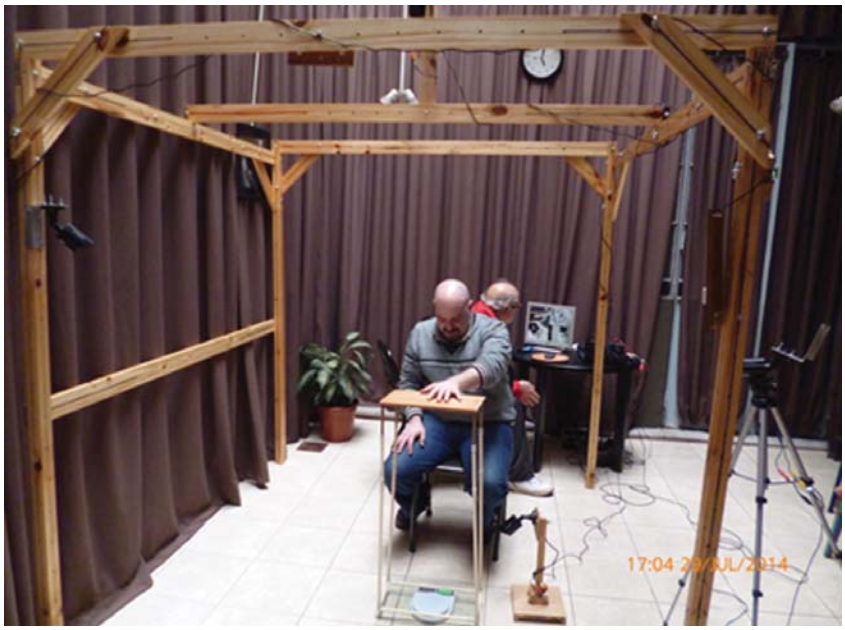


Photo 1. Panoramic view of the PK laboratory showing the wooden frame for mounting video cameras, with Ariel Farías at the center. There are cameras in the left column, above Ariel and near his left leg. To the right, there is another camera mounted in a tripod. At the right column, the microphone is mounted. Behind Ariel, one of the investigators is monitoring the work through the screen of the DVR. The photo shows Ariel trying to raise a little wooden table (weighing less than 400 g), mounted on a plastic structure, over a piece of flat glass, this last supported by a scale. This setup allowed Ariel to experiment with variations of the main phenomena, and to get feedback from the scale's display variations, during concentration and work.

DVR9004K, with 4 standard-definition security cameras, each of these equipped with infrared illumination. A 500-GB disk gave us the chance to store all the audio and video of the meetings for further scrutiny. The audio track was recorded by the same device, via a high-sensitivity microphone specially adapted for environmental sounds. Two independent cameras and an audio recorder were also used, to take photos, videos, and audio. To help with data collection and data correlation with video and audio records, we also built a multivariable recorder with 16 independent channels, the main core of which is a PC. The primary purpose of this device is to translate the electrical signals of different sensors into values that can be processed with standard software. Five electronic scales were modified, one of them to

measure the weight of the subject and the others to measure the weight and forces developed in the table and other objects. Among other sensors, two for temperature and one for estimating breathing rhythm were developed and built, all them pluggable to the recorder. Not to be plugged in to the recorder, two indirect indicators were designed to detect magnetic and electric fields. Also available were a random number generator (RNG), a laser light emitter, and a device to conduct and record electroencephalography tests.

Between July 1, 2014, and December 18, 2015, we held a total of 26 meetings (see Photo 1). As required by the investigators, Ariel attended 23 of those meetings to try to psychokinetically produce diverse table movements. The other 3 meetings (without Ariel) were conducted to measure the possibility of producing the same movements via normal muscular force. Several external observers were invited to attend: Sergio Matteucci (one meeting), Aníbal Melgar, Andrea Romano, and Naum Kliksberg (2 meetings), Alejandro Parra and Stephen Braude (3 meetings).

Description of the Main Phenomenon

The main device was a round wooden table, with three legs, and with an approximate weight of 14 kg and a diameter of 1.05 m. The table was almost identical to the table used in the decisive meetings held by The Red Lights Group, and Ariel felt comfortable and safe working with it.

During the three meetings held without Ariel, the investigators observed (testing the table's movements by themselves) that all the horizontal movements were easy to reproduce with muscular force. It was also easy to raise any leg by pressing the table downward, near the opposite border (e.g., to raise leg 2, press downward near the border between legs 1 and 3, and so on). Excluding complete table levitation (which we never achieved), the only movement the investigators were unable to reproduce through muscular force was raising the table leg closer to the subject (leg 1), with the table leg between the subject's legs (but without any physical contact with the table leg), and with only the subject's palms touching the table. Another important issue was to minimize the friction force exerted by the table legs 2 and 3 against the ceramic tiles of the floor. Those tiles were already rather smooth and slippery, but we also had to make sure that the legs were not jammed in the tile junctions (in the following paragraphs the reader will understand the importance of this last sentence).

In these conditions and with normal illumination, Ariel would try to will leg 1 to rise. That result was achieved from the very beginning of the research and then repeated several times (Video 1). In the best meetings, Ariel needed 5 or 6 minutes to achieve it; in others he had to try for one hour or even more before succeeding. There were also a few meetings (6 out

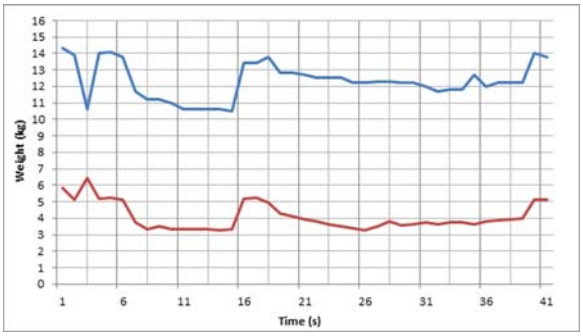
of 23) where Ariel could not raise any table leg, despite trying for several hours with periods of rest.

Video 1. Ariel Farías raising the table leg. A camera is located above and at his left and another under the table. The microphone is installed on the right column. The left screen shows the multivariable recorder data, the right screen shows the 4 camera images taken and recorded by the DVR. Meeting 21 (12-14-2015). Watch at https://youtu.be/Stoi27PugKI

As the meetings progressed, new elements were added to the Lab. At first, a scale was installed under table leg 1 (the scale was called b1), allowing Ariel to have an easy view of the display so that he could monitor the way the weight decreased from 4.8 kg approximately to zero, when the leg rose. This scale (b1) also allowed the detection of leg 1's weight reduction even when the leg had not risen. Moreover, we located that display within easy view of Ariel so that he could see how the weight started to decrease when he touched the table. Ariel claimed that he found this form of feedback very helpful. Later on, three new scales were added, two under legs 2 and 3 (called b2 and b3, respectively) and the other under Ariel (called Scale B). At the beginning, the scale values were recorded with two cameras of the DVR. The values were later recovered watching the videos and taking note of the values manually, at a rate of one record per second. Once the multivariable recorder was operational, this task was automatically performed, recording two values per second of each channel, leaving those values in a *.dat file, easily processed by any standard software. After that, the four cameras were used exclusively to take images of the meetings: one overhead for monitoring Ariel's hands, another in close range to leg 1, and the others in long-range views, taking in the whole scene from opposite angles (Video 2).

Video 2. Two cameras simultaneously capture the raising of the table leg. One is panoramic and the other takes a close range view of Ariel's hands and arms. Meeting 13 (12/02/2014). Watch at https://youtu.be/rdTBwRkypqo.

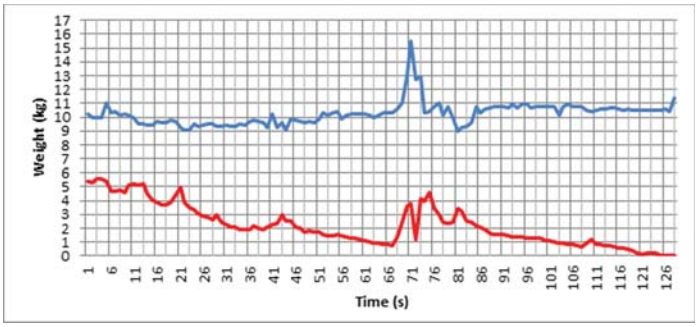
The B scale, used to measure the subject's weight, was at first intended to replicate some experiments developed by William Crawford. However, we also found an unexpected and valuable use for it: It could detect indirectly when leg 1 was raised by muscular force and when it was raised by an unknown force, presumably PK. During the simulation sessions (i.e. those which Ariel did not attend), we observed that leg 1 could be raised by muscular force, taking advantage of the high friction of the hands against the table, especially with sweaty hands. We also observed that, with the sitter's hands on the table and exerting muscular force with arms



Graph 1. The blue (upper) line shows the weight of the subject trying to raise leg 1 with muscular force. (The graphic scale was modified to appreciate the details. Add 85 kg to obtain the real values for the blue line.) The red (lower) line shows the weight measured by scale b1, located under the table leg 1. During the first 15 seconds, the seated investigator tried to raise leg 1 by exerting a horizontal force with the hand palms toward the center of the table. Scale b1 shows a weight loss of up to 2 kg, but scale B (measuring the weight of the subject) loses up to 4 kg, making the maneuver evident. Then, between seconds 15 and 17, the table finally moves ostensibly in the force direction, which makes the experiment come back to the start. At second 39, the table moves again in the force direction. With this maneuver, table leg 1 was never separated from the floor or scale b1. Meeting 11 (11/04/2014).

positioned horizontally toward the center of the table, the weight measured in b1 decreased from 4.5 to 2kg approximately.[1] However, this muscular maneuver could be detected by scale B, which registered a decrease in sitter-weight of 4 kg or even more (see Graph 1). Anyway, whether the sitter went on exerting a higher force with the arms, before b1 decreased to zero, the table eventually began to move, slipping on the floor as the friction force was defeated by the horizontal component of the muscular force, leaving visual evidence of the maneuver. So, that is why it was important to reduce and keep to a minimum the friction between the table legs and the floor, and also to keep an eye on legs 2 and 3 to ensure that they were not stopped or held in place by anything on the floor.

On the other hand, when Ariel (seated) put the palms of his hands on the table without exerting a muscular force, while b1 progressively went down to zero, his weight decreased only 2 kg, equivalent values to the mass of Ariel's body when he gently leaned forward (see Graph 2), while his hands and the table stayed immobile. These elements suggest strongly that leg 1 would not have been raised due to muscular force, but to an unknown force, presumably PK.



Graph 2. The blue (upper) line shows Ariel's weight while he was trying to raise leg 1. (The graphic scale was modified to appreciate the details. Add 85 kg to obtain the real values for the blue line.) The red (lower) line shows the weight measured by scale b1, located under table leg 1, and which registered the continuous decrease in weight until the value reaches zero at second 126 and leg 1 rises. In the meantime, Ariel's weight varies in the range of 2 kg. The perturbation shown in both lines between seconds 66 and 86 is due to secondary movements of Ariel, mainly to sit comfortably. Meeting 8 (09/16/2014).

Some Notes about Ariel

An all too familiar mistake in psi research is to treat the official subject as the only essential factor in eliciting the desired phenomena. In this case, the group comprising Ariel and the investigators functioned from the start as a unified group of friends who extensively talked about diverse subjects before starting the work prepared for each meeting. Also, the group gathered outside of the laboratory, not only to organize some tasks, but also to enhance this relationship. In addition, all decisions about tests, devices, schedules, etc., were arranged with, and previously sanctioned by, Ariel. This comfortable and relaxed climate can easily be seen in the videos of the meetings, where one can observe the harmonious and easygoing group dynamics.

As far as psi-conducive conditions are concerned, Ariel has proven to be a very cooperative subject, and someone who is not easily perturbed. He can work with several levels of illumination and is not seriously distracted or disturbed either by ambient noises, the movement of people surrounding him, or interruptions by the investigators to rearrange the cameras or adjust some other device. Moreover, he does not require any elaborate ritual to produce the phenomena. To feel more comfortable, he removes his ring, wrist chain, and watch. He can start working from a standing position or

sitting in a chair, touching the table's surface with the palms of one or two hands. He concentrates by keeping silent and closing his eyes from time to time. Once the table leg rises, he can start talking and laughing without any problem, and he can usually maintain that state of affairs for several minutes.

However, it must be admitted that we never solved the problem of motivating the subject to the degree present in the sitter group of 2013. We tried, no doubt with some exaggeration, to stress the importance of the ongoing investigation, both for parapsychology specifically and for science generally. Some of the motivational activities were publishing articles in magazines or journals (including Gimeno 2015), and organizing a conference in which we presented our work and at which Ariel would answer questions from the attendees. We also prepared a documentary posted on YouTube: https://www.youtube.com/watch?v=99hpf2ryQ-w. However, occasionally during our conversations questions arose that betrayed our lack of a clear direction—e.g., What are we going to do with this? or What is the purpose for such efforts? In fact, the frequency of very successful meetings began to fall off with time, as did the intensity of the phenomena. Finally, the news that Ariel would be a father for the first time, in February 2015, made the investigators re-evaluate the schedule of tests and consider ending the research. Nevertheless, testing was extended until December 2015, due to the visit of Dr. Stephen Braude.

The only thing that seemed to reverse the decline in Ariel's phenomena was the occasional visit from a "VIP," or at least from certain of them. Ariel could clearly anticipate how the attitude of the visitor would influence his will and temper. We had requests from professional magicians, orthodox scientists, and professed skeptics (actually psi-deniers) certain from the start either that Ariel's phenomena were fraudulent or that his investigators had committed some kind of error which they were determined to uncover. Previous encounters with members of that latter group had been unpleasant and inhibiting for Ariel. So further requests from that group were indefinitely delayed. On the other hand, when the visitor showed respect for and knowledge of the evidence for macro-PK and arrived with an open but critical mind, Ariel considered the situation to be a positive challenge. Indeed, these occasions often yielded some of his best results in terms of intensity and duration of the phenomena.

Attempting to Confirm the Hypothesis of Crawford

When W. J. Crawford tried to confirm the hypothesis that subjects moved objects psychokinetically by means of a "psychic rod" emerging from the body, one of his methods was to use *markers*:

> In order to obtain data concerning the shape of the ends of the structures and also of their methods of gripping the table, I often covered the undersurface and legs of the table with soot obtained from a turpentine lamp. In this way, wherever the structures touched, marks were left on the soot. It was soon found that there were two chief methods of levitating the table, viz. from the undersurface and by the legs. (Crawford 1921:167)

During meeting 12, 11/18/2014, an experiment based on the same ideas was developed, using methods and materials available nowadays. The undersurface of the usual wooden table was covered with a piece of fabric and then that fabric was covered with soft foam, as can be seen in Photos 2, 3, and 4. Before the work began, several photos of the table were taken of the irregularities of the foam (some at close range), in order to compare them with the possibly different shapes they would have at the end of the experiment in case the material had been disturbed by something like a psychic rod. Three cameras were arranged to take direct images of the surface covered by the foam; another camera had a general view; and a fifth camera monitored Ariel's hands (see Video 3).

Video 3. Ariel can be seen raising the table leg covered with foam. Note that he tried to raise the leg with other parts of his body, not only with his hands. Meeting 12 (11/18/2014). Watch at https://youtu.be/UoeoIznGjvk.

Photo 2. The piece of fabric had been nailed to the undersurface of the table, ready for the foam.

Photo 3. **The table with the foam spread over the surface of fabric and the leg.**

Photo 4. **The table in the work position.** Ariel sat close to the leg covered with foam.

Ariel concentrated and worked for more than an hour, and on several occasions raised the foam-covered table leg. At the end, several photos of the whole undersurface covered with foam were taken. After a detailed scrutiny in situ and further analysis of the video and photos taken before and after the experiment, not a single spot or tiny mark was found. Moreover, the cameras focused on the foam did not reveal the presence of any psychic protrusions from Ariel. Thus, although the session produced more evidence of Ariel's PK, it failed to replicate Crawford's result and provide evidence of a psychic rod.

Influence on a Random Number Generator

Since the appearance of modern random number generators (RNGs) based on subatomic processes seemingly impossible to influence normally, psi researchers have tried to find correlations between certain types of human behavior and low-probability deviations in the output of an RNG (see, for example, Schmidt 1973, 1974, 1976, Jahn et al. 1997, Radin & Nelson 1989, Radin et al. 2006, Bösch, Steinkamp, & Boller 2006, Bierman 1996, Bierman & Houtkooper 1975, Wilson et al. 2012).

In the present study, a Psyleron RNG version 1.64d was used during 14 sessions to collect data. In some of them, the differences between "non-activity" and "supposed PK activity" were easily apparent, as can be observed from Graphs 3 and 4.

However, as in most meetings, the periods of activity and rest were alternating, and a thorough analysis was necessary. To do that, 7 segments from 5 to 26 minutes each, of supposed "PK activity," were selected. Then, segments identical in extension and quantity were selected randomly, to be used as a control group, with two distinctions: without Ariel in the lab and with Ariel in the lab (at rest). So there were 21 segments in total, with 7 for each of the three conditions. With the Z values of each segment, the adding was obtained applying the Z (Stouffer) to each condition. See Table 1.

But, having in mind that any variation, negative or positive, would have the same meaning, the value of Z (Stouffer) was not representative. When doing the math, the sign of each individual Z value is preserved, so eventually the plus and minus signs would neutralize instead of add. To avoid this slant, the value of χ^2 was calculated in order to obtain the variance of each condition, giving Table 2. It can be observed that the A condition, which picks the variance of the RNG segments during the "supposed PK" moments, has a probability of occurrence far from the values of chance, while the other two groups (without Ariel and with Ariel but at rest), have a variance inside the values expected in an RNG.

Graph 3. RNG values taken before Ariel Farías's arrival. All values are clearly inside the chance area. Meeting 7 (09/02/2014), from 4:12 p.m. to 6:12 p.m.

Graph 4. RNG values taken during Ariel Farías's work trying to move the table. Values go in and out of the boundary line of chance. Meeting 7 (09/02/2014), from 6:13 p.m. to 7:11 p.m.

TABLE 1
RNG during Supposed PK Activity (A),
RNG without Ariel Farías (B), RNG with Ariel Farías at Rest (C)

Condition	Z (Stouffer)	p (1 tail)
A	–1.87	–0.03
B	1.25	0.11
C	–0.66	–0.25

TABLE 2
RNG during Supposed PK Activity (A),
RNG without Ariel Farias (B), RNG with Ariel Farias at Rest

Condition	χ^2	Degrees of Freedom	p (1 tail)
A	17.64	6	0.008
B	6.95	6	0.22
C	4.50	6	0.47

Electroencephalogram (EEG)

There have been previous attempts to study EEGs of psi subjects during the production of phenomena. In Argentina, Dr. Orlando Canavesio (1951) detected an original pattern which he called the "meta-psychic state," similar to the alpha state. Also Motoyama (1964) observed the so-called "ramp function," characteristic of the deep dream, in waking subjects while ostensibly demonstrating ESP, something also detected in Matthew Manning during informal tests of metal bending (Owen 1974). Similarly, Targ and Puthoff (1974) reportedly studied the EEGs of Uri Geller during ESP tests, but without specifying the results.

In Ariel's case, we conducted two EEG studies. The first one was on 09/02/2014, called "Base," with Ariel at rest, and subjected to sensorial excitation with light, sound, and touch, as well as during hyperventilation and recovery from it (see Photo 5). The second was on 09/16/2014, with Ariel producing apparent PK (see Photo 6 and Video 4).

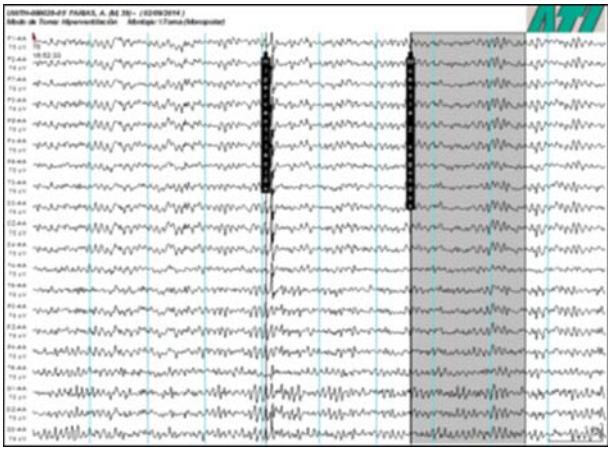
Video 4. Ariel Farías raising the table leg while the EEG is running. Watch at https://youtu.be/KjfAmN-_9iI.

The first study did not reveal any clinical abnormality, according to the medical report we solicited from Dr. Lucio Huayhua, Neurosurgery, Registration Number 88351. The report states: "Normal voltage plot with

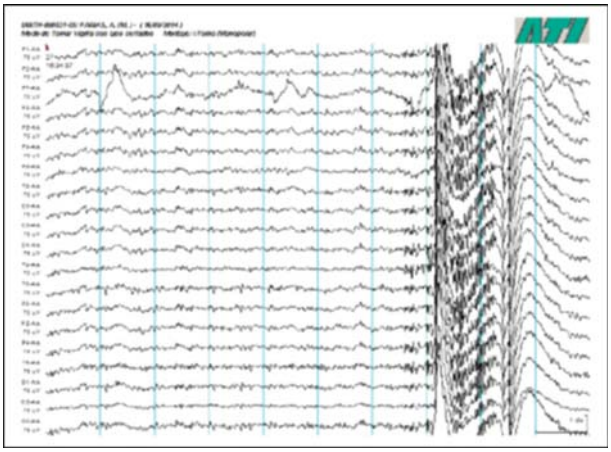
Photo 5. Ariel at rest during the EEG "Base," with the electrodes already connected, following the indications of Aníbal Melgar. Behind, Andrea Romano takes videos to document the test.

Photo 6. Ariel seen from his back, concentrating on trying to raise the table leg, with the electrodes already attached to his head by means of a cap especially designed for this test by Andrea Romano and Aníbal Melgar.

good reactivity. With Alpha activity to 8 cycles / second in posterior areas. During plotting, tip wave activity is evidenced in bifrontal areas, with left frontopolar focus" (see Graph 5). The second EEG, during Ariel's ostensible PK, detected anomalous curves and values that seem not to be explicable as "artefacts" (see Graph 6).



Graph 5. Part of the EEG "Base" plot. During this period, Ariel was still, with eyes closed and hyperventilating. The values and shapes seen are normal.



Graph 6. Part of the EEG plot during apparent PK. Ariel was still, with eyes closed, hands on the table, and hyperventilating. A big electric perturbation is observed at the end of the plot, while the scale under the table leg indicated a weight reduction of 2.6 kg (the initial weight was 4.8 kg). The table leg rose nineteen seconds after the perturbation, by which time the EEG appeared to be normal.[2] See Graph 8 for a zoom-in.

So far it has not been possible to recruit another neurologist to perform a more detailed analysis.

Variations of the Main Phenomenon

After Ariel reached the point where he could raise the table leg almost at will, and having the movements carefully measured and recorded, as well as differentiated from superficially similar movements produced by muscular force, we proposed several tests to improve on the already achieved results. As a result, Ariel tried several times to levitate the whole table, but at best he was only able to raise a second leg for a few seconds. Ariel also tried to levitate a much lighter table, weighing 5 kg, of the same size and shape as the usual table, and looking like wood but made from expanded polyethylene. The reduction in table weight did not seem to matter. Although Ariel did not manage a full levitation in those trials, he again raised one or two legs. He also tried, unsuccessfully, lifting other wooden devices placed on the table as well as lifting the table when it was hung from ropes. On the other hand, when sheets of paper were placed between Ariel's hands and the table, as in some meetings of the Red Lights Group, the table continued moving despite the inclusion of this barrier. However, the results were not consistent for any of the former conditions, though reductions of the table weight—less than 300g—were achieved in all these conditions.

In an effort to study the possibility of movement of an object without contact, a test setup was arranged with a container full of water and an object floating within it. With calm water, no wind or vibrations, Ariel tried to move the object, bringing his hands to less than 20 cm from the container, but without any detectable result.

Although Ariel enthusiastically accepted our invitations to try our variations in protocol, he became bored in a few minutes if the hoped-for results were not achieved, asking then to return to the well-known movement with the wooden table. It was evident that he felt much more comfortable repeating the main phenomenon than trying new ones. When we asked Ariel about this, he mentioned something he had also said in a meeting of the Red Lights Group—namely, that he is enthusiastic with tiny movements of the table, but if those movements grow in magnitude or become weird, he starts to fear that he will become part of an uncontrollable situation, similar to the poltergeist events that troubled him so much when he was a teenager. In response to that admission, we arranged a series of interviews between Ariel and Dr. Alejandro Parra. Parra is a psychologist specializing in the treatment of symptoms originating from the observation of diverse psi phenomena. Unfortunately, this activity could not be completed.

Other Variables Associated with the Main Phenomenon

As far as Ariel's general will, temper, and mood are concerned, it was evident, though not surprising, that his production was best on days when he appeared to be free from personal and work-related concerns. He was also stimulated by the progress of the research and receiving favorable feedback from invited observers. In fact, the most effective stimulus to success was the occasional visit from a VIP. Meeting 14, held on 12/18/2014, combined both of these positive stimuli. The objective of this session was to see if Ariel could improve his performance under hypnosis, a strategy that appealed greatly to Ariel, not only because of its novelty but also because Dr. Alejandro Parra planned to attend the meeting in order to hypnotize Ariel. Although it appeared that Ariel could not be hypnotized, he was nevertheless still able to raise the table as he had done in previous meetings. Finally, the best meeting of all happened on 12/14/2015 during the visit of Stephen Braude, recognized by the group as a leading authority on macro-PK, who travelled from the United States to witness Ariel at work (see Photo 7).

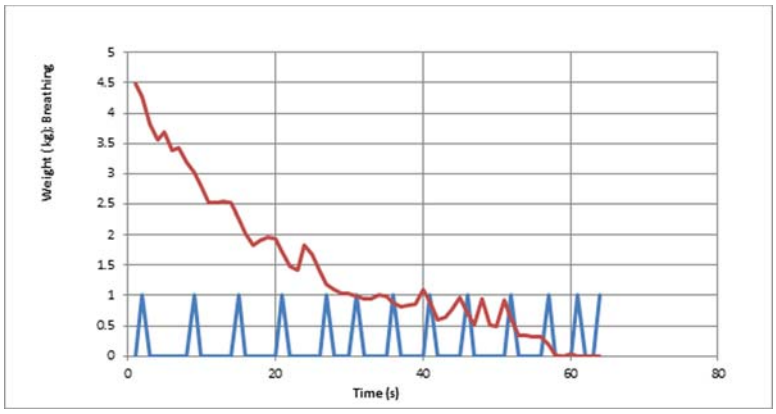
Photo 7. Stephen Braude, Ariel Farías, Juan Gimeno, and Darío Burgo in the psychokinesis laboratory.

We should also note that we often verified that Ariel's hands were not sticky (although, as we mention below, we found them to be cooler than expected). Moreover, given the proximity of observers to Ariel throughout the trials, as well as the recording of the sessions from multiple angles, there was no opportunity for Ariel to gain access furtively to any previously hidden sticky substance.

As far as other physiological parameters are concerned, Ariel usually mentioned feeling an intense heat in his hands before and during the production of the phenomenon. This is especially curious in view of the fact that observers consistently found Ariel's hands and forearms to be noticeably colder than their own. To examine the matter further, two temperature sensors of tiny mass were developed (one for each hand) and attached to the multivariable recorder. With an ambient temperature of 19.5 °C, the temperature of the hand palms became stabilized during the test at 32.7 °C for the left hand and 33.1 °C for the right (normal body temperature in Celsius varies between 36° and 37°). Thus, it appears that Ariel's experience of intense heat in his hands is a purely subjective sensation. We also observed some perspiration in Ariel's hands, which we attributed to the tension experienced or the effort expended during the tests. Other, more diffuse, sensations expressed by Ariel at the end of some meetings, were a bit of confusion lasting for a few minutes and a contracture in his shoulders which sometimes lasted until the next day.

It is also worth mentioning how Ariel described his experience of producing the table movements. In addition to anticipating by a few seconds when the phenomenon would begin, he said that in the very moment the table began to move, he felt sensations similar to those felt by an airline passenger when the plane takes off. He also said that when the table leg rises, it's "similar to when you put on your shoes; at the beginning you feel the difference between bare and covered skin, but in a few seconds you forget this difference and begin to feel the shoes as a part of you." He described the inverse sensation (removing one's shoes) when the force disappears and the table leg falls down to the floor. Regarding those and other biographical aspects, Ariel is preparing a more extensive and detailed paper.

The physiological process apparently most clearly related causally to the phenomenon was hyperventilation, which Ariel had spontaneously begun to practice, and which he continued to utilize in key moments once he realized that it speeded the weight reduction of table leg 1. To quantify this variable, a microphone was used to identify the breathing rhythm. We also designed an instrument to be attached to Ariel's chest. That instrument is based on a pump sphygmomanometer (the hand instrument designed to



Graph 7. The red (top) curve expresses the weight measured by the scale under table leg 1, closest to Ariel. The peaks of the blue (lower) line show the moments of breathing. Meeting 6 (08/26/2014).

measure blood pressure), but modified to measure the pressure of an air bag through an electronic sensor. The signal produced by this sensor is then amplified and sent to the multivariable recorder, digitalized, and presented in an Excel sheet, along with other signals. The air bag is attached to the chest with flexible strips. The idea is not to measure a value, but the variation among values, discriminating peaks and valleys of the variations, which are directly related to the breath pulses. Thus, the signals (a pulse with each breath) were sent to the multivariable recorder in order to correlate them with the rate of weight loss of leg 1, as can be seen in Graph 7.

As far as physical variables are concerned, it did not matter whether the room was darkened or filled with bright light. Moreover, the possible existence or generation of very low frequency electric fields was tested by two procedures. First, we built an "ad hoc electroscope," composed of a rod of isolating material from which hung several thin pieces of cotton thread, similar to a hairbrush. Putting this device close to Ariel's body, hands, and the table during his work, no disturbance of the threads was observed. Second, hundreds of tiny circular pieces of paper (5 mm in diameter) were spread close to Ariel's hands while he was working, without the detection of any movement in the pieces of paper. To detect very-low-frequency magnetic fields, two compasses were used, one close to Ariel's hands and the other near his head during his work, without any observable disturbance of the needles. Also, during several meetings, an audiocassette was stuck in the table's undersurface, close to Ariel's hands. The music prerecorded there remained unaltered, which presumably would not have been the case had

a magnetic field influenced the tape. The negative results in these informal tests did not encourage the researchers to try more accurate measurements, which in any case would have probably exceeded the group's modest budget.

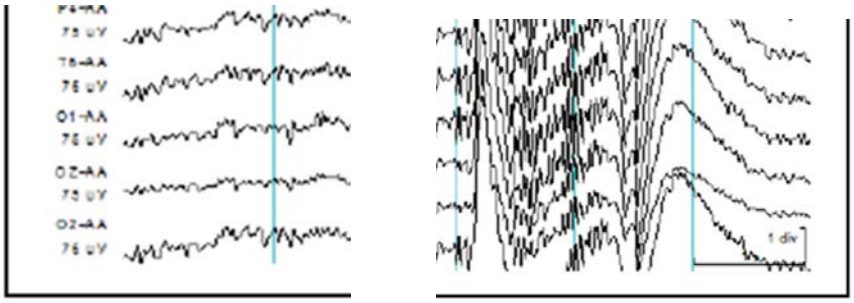
Conclusions

The research reported in this article concluded on December 18, 2015, after eighteen months of work. Meanwhile a psychokinesis laboratory had been organized, with a small budget and few human resources, though with great enthusiasm and dedication. Although we were unable to try all the experimental protocols we considered during the course of our investigation, we nevertheless consider it to be an achievement that a presumably gifted PK subject agreed to work with us for such an extended period, and under conditions that were often either taxing or simply boring. We think it is quite clear from the material we have compiled that Ariel has, non-fraudulently and almost at will, succeeded in psychokinetically producing (admittedly unspectacular) table movements, and that Ariel's abilities merit further, and better-funded, investigation. Moreover, although we tried to replicate W. J. Crawford's strategy for detecting the presence of a "psychic rod" that produced object movements, our results were negative.

Notes

[1] The term "approximately" is due to the not-very-refined method used to register the weights and the accuracy of the scales. These measurements were taken in the early days of the laboratory, reading the scale displays on the videos, and jotting down the values. Then, in 2015, we built the PC-based multivariable recorder.

[2] Regrettably, this trace does not permit a clear view of the time scale. For Graphs 5 and 6, they are (see Graph 8 for zoom-in of Graph 6):
- 75 μV/division (vertical) (lower left corner on Graph 6).
- 1 second/division (horizontal) (lower right corner on Graph 6).



Graph 8. Zooming in on Graph 6.

Acknowledgments

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Devices and Equipment

1 video recording system, PCBOX model PCB-DVR9004k
1 camera, Panasonic, model DMC-FH6 Lumix, 14 Mpx
1 camera, Sony, model Cyber-shot, 6 Mpx
1 MP3 recorder, Philips, model SA1105/55
1 random number generator (RNG), Psyleron, version 1.64d
1 PC laptop, Lenovo, model G550
1 microphone, NYH, model NYH301
4 scales, SWAN, model SF-400 (0 to 7 kg)
1 scale, GAMA, model SCG430 (0 to 180 kg)
1 compass, Recta, model DS-40
1 compass, no brand, no model
1 laser pointer ($P < 50$mW, wavelength 650 ± 10 nm)
1 multivariable recorder, 16 analog channels, PC-based, Acer-Mate, model 433S

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Appendix by Stephen Braude

The first thing to note about Ariel as a subject is how unpretentious and cooperative he is. Needless to say, in that respect he stands in marked contrast to many other ostensibly gifted PK subjects (e.g., Kai Mügge—see Braude 2014, 2016, Nahm 2014b). Ariel has no reservations at all about working under bright light and under very close scrutiny, including close-up video monitoring from multiple angles. I'm very impressed by him personally, and it's clear that Ariel is not driven by a desire to be a PK superstar or guru. I found him to be a down-to-earth and humble family man, reasonably content with a steady day-job, naturally curious about the PK abilities he has discovered (as well as psychic abilities generally), and with no religious or metaphysical axe to grind.

During my visit to Buenos Aires, I was able to schedule three sessions with Ariel, with a break of one day between sessions. This was a more intensive schedule than Ariel was accustomed to, and that was probably one of several factors resulting in getting our best results on the first scheduled

day. Before commenting on that day's results, let me mention briefly what happened in the second and third sessions. Session number 2 was a group sitting. In retrospect, I regret having agreed to this, although it seemed like a sensible arrangement at the time. Ariel had been very productive in session 1, and since we knew that the most dramatic table movements observed in the earlier Red Lights Group had occurred during group sittings, we wondered (in the spirit of Batcheldor) whether a group sitting would relieve Ariel of some of the responsibility for the phenomena and lead to even more impressive effects. Indeed, the table glided rapidly and dramatically around the room, but there were no partial levitations, which is really the phenomenon we had hoped to record. Although the videos (showing fingertips lightly touching the table) suggest strongly that unconscious muscular movement can't account for the table's trajectories, the recorded phenomena are clearly less interesting than the partial levitations recorded two days earlier.

Session number 3 yielded almost nothing, but that seemed clearly to be the result of Ariel's preoccupation with the aftermath of a motorcycle accident the day before, in which his bike was totaled and he only barely escaped serious injury. Ariel did his best to focus on the matter at hand, but he was still rattled from the previous day's events and worried about insurance and financial issues.

During session number 1, we got results from Ariel right from the beginning, and I made high-definition video recordings of six partial levitations. The levitations were also documented with four standard-resolution security video cameras located beneath the table and above and behind Ariel. It was clear that Ariel was not engaged in trickery or inadvertently using friction from the table legs on the relatively slick tile floor to lift the table. Indeed, Ariel was able to achieve this result using only one hand on the nearby edge of the table (see Photo 8).

Ariel also managed to raise the table when (at my suggestion) he placed his forearms on the table. Clearly, the weight of Ariel's arms on the nearby part of table would have tended to weigh *down* that side of the table. Ariel was in no position in that case to place his hands sufficiently forward on the table to make the side close to him rise (see Photo 9).

I'll just note for the record that I suggested to Ariel that he try cabeza-PK—that is, trying to lift the table by placing his head on it. Ariel, as usual, complied cooperatively, but he found this arrangement both amusing and uncomfortable, and it didn't succeed.

I should also note that there was nothing unusual or suspicious (e.g., hidden magnets or hooks) about the construction of the table (see Photo 10). Moreover, because I examined Ariel's hands and forearms between

Photo 8. Ariel raises the table with only one hand.

Photo 9. Ariel raises the table using his forearms.

Photo 10. Nothing special about the table.

levitations, I can confirm that they were never sticky and also that they were unusually cool to the touch.

It was especially interesting to see the readouts from the strain gauge placed under the nearby leg of the table. Ariel liked to watch this feedback, because he could see when his efforts were beginning to work, even before the table leg had risen from the floor. Even though the table didn't rise, the default weight of the table on the strain gauge changed continually and became *lower*, rather than higher as would ordinarily happen when the weight of fingers or hands is added to the table.

Although I consider the work conducted with Ariel so far to be impressive, I believe we must still regard it as preliminary. If Ariel can remain interested enough to continue with this line of investigation (something that can't be taken for granted—after all, the phenomena get pretty boring after a while), there is more we can do to document the levitations more clearly, and probe more thoroughly into what's going on. I'm currently pursuing options for bringing Ariel to the U.S. and recording the pressure on the table in a more fine-grained way.

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RESEARCH ARTICLE

A Question of Belief: An Analysis of Item Content in Paranormal Belief Questionnaires

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Abstract—This study examined the degree to which paranormal believers, who profess 'strong' belief in the popular expression of a topic known as the *primary item* (e.g., *Psychics possess a mysterious ability to know things about a person's past and future*), disagree with related items and/or the putative 'cause' of the topic, known as *secondary items* (e.g., *Some people have a mysterious ability to accurately predict such things as natural disasters, election results, political assassinations, etc.*). It was theorized that scoring differences between primary and secondary items might indicate certain kinds of paranormal believer, which might then allow us to conduct deeper analyses of paranormal belief (PB) and its putative relationships with deficits and dysfunctions. A complete set of items drawn from ten established PB scales was administered to a sample of 343 respondents. Using Factor Analysis, we developed the Paranormal Belief Informedness Scale (PBIS), consisting of 10 primary items, and 10 secondary items, scores of which were used to identify three major PB types: 'primary believers' (who believe in *all* 10 primary items, and thus exhibit 'strong' PB), 'primary non-believers' (who believe in none of the 10 primary items), and 'mixed believers' (who believe in only some primary items). We found significant response-rate differences between primary and secondary items across believer types, and across psi categories (i.e. extra-sensory perception, psychokinesis, and life after death). For the full sample, it was shown that there is a significant

relationship between PB and reality testing deficits as measured on the reality testing subscale of the *Inventory of Personality Organisation* (IPO-RT) (Lenzenweger et al. 2001). However, this relationship tended not to be significant across believer types. Also, there was no evidence in the full sample, or in any believer type, that PB was correlated with depression as measured on the Beck Depression Inventory (BDI-II). We suggest that paranormal believers have differences that may be reflected in their responses to predictor variables, and/or in how informed their paranormal belief is.

Keywords: depression—informed belief—paranormal belief—quasi-belief —primary belief—reality testing

Introduction

The literature suggests that people who believe in, and/or claim, paranormal experiences, as measured on a range of paranormal belief (PB) scales, can be seen as potentially 'deficient' or 'dysfunctional' (see Irwin 2009 for a thorough review). These conditions can be characterized under two clinically oriented hypotheses: (i) the cognitive deficits hypothesis—believers have uncritical, naïve, or irrational thought processes based on deficits in intelligence and/or reasoning skills, and (ii) the psychodynamic functions hypothesis, whereby believers are psychologically disadvantaged or maladapted (Irwin 2009, Irwin & Watt 2007). While some concessions have been made that the findings for cognitive deficits in paranormal believers are "mixed," "ambiguous," or "unequivocal" (Irwin & Watt 2007:229–231), or even "not encouraging" (Irwin 2009:90), *and* it has been proposed that PB might logically stem from "the data of parapsychological research" (Irwin & Watt 2007:232), there is, however, "general support" (Irwin & Watt 2007:234) that believers tend to be psychologically or socially deviant (dysfunctional). We would argue that the pathologization of paranormal believers, although warranted in some cases, has somehow become overextended to *all* paranormal believers, and much of the past research in anomalistic psychology is a primary influence in this assumption. On the psychodynamic aspects of PB, Jinks (2012a) has identified some specific sources of this assumption:

> . . . it is the associating of paranormal belief formation and maintenance with schizotypal ideation (Brugger et al. 1993, Brugger & Graves 1997, Hergovich, Schott, & Arendasy 2008, Irwin & Green 1998, Pizzagalli et al. 2000, Pizzagalli, Lehmann, & Brugger 2001, Thalbourne, Dunbar, & Delin 1995, Windholz & Diamant 1974), delusion, psychosis, and schizophrenia (e.g., Cella, Vellante, & Preti 2012, Houran & Lange 2004, Thalbourne 1994) that most successfully creates an impression that paranormal believers are psychologically dysfunctional. (Jinks 2012a:128)

We do not entirely dispute the insights drawn from some correlates and functions of PB (see Irwin & Watt 2007, Kumar & Pekala 2001, Lange & Houran 1997, 1998), and it is clear that many psi-researchers place great emphasis and importance on PB scales for scientific reasons—for example, due to the often-supported psi and sheep–goat hypotheses,[1] sheep are usually regarded as probable high-scorers on tests of psychic ability whereas goats are not (see Lawrence 1993, Palmer 1971, 1977). PB scores are therefore good predictors of a range of psychological and parapsychological responses. However, a number of PB scales have been criticized (see Irwin's 2009 review), and only in the last few decades have psychometric procedures reached the level of sophistication where PB has shifted from a unitary construct to a multi-factorial construct. It is now taken for granted that PB can refer *not only* to beliefs in (a) psychic abilities such as extra-sensory perception and psychokinesis, *but also* beliefs in (b) supernatural, occult, and crypto-morphic phenomena. These advances are noteworthy, but they do not resolve a critical problem—namely, that participants in PB studies merely tend to accept at face value most belief-scale items as either 'correct' or 'incorrect' (Walton 2010). Such acceptance fails to address the possibility that some believers per se might 'believe' in concepts they do not actually understand, even though those beliefs do not correspond to *informed beliefs*, or the type of beliefs PB researchers believe they are measuring. Rather, these 'believers' may hold *quasi-beliefs*—semi-propositional representations of the world superficially believed to be true prior to any truth evaluation (Recanati 1997).[2] Individuals may often hold quasi-beliefs indefinitely, never migrating them to the status of an informed belief, casually expressing agreement with a given proposition in such a way that their answer is indistinguishable from another individual who is better informed (see Jinks 2012a for details).[3] We note the fact that a person's level of paranormal belief is underpinned by how informed their belief is as much as how informed their non-belief, or disbelief, is. However, for the purposes of this study (and for operational reasons), we are mainly interested in informed belief; not informed non-belief (i.e. informed skepticism).

Without a clearer understanding of the nature and diversity of PB (i.e. the qualitative, not just quantitative, degrees to which these beliefs are held and maintained), the proposition that PB indicates deficits and/or dysfunctions may likely be unwarranted, or at the very least may not apply to some *subsets* of paranormal believers.

Primary and Secondary Paranormal Belief Items

A viable means of investigating quasi-belief, and other forms of belief, is to develop question sets with two classes of item. Items in the first class would

replicate most of the familiar propositions found in common paranormal belief questionnaires referring to anomalous processes, occurrences, locations, entities, or personalities. Such items could be labelled *primary items*. Items in the second class would variously (a) represent the standard anomalous explanations for the primary items; (b) offer an alternative example of the primary item; or (c) re-word the primary item to exclude specific reference to any anomalous process, occurrence, location, entity, or associated personality. Such items could be labeled *secondary items*.

Jinks (2012a) developed a set of primary and secondary items about a number of paranormal and related beliefs, and then administered them to more than 400 participants. He confirmed, for example, that those who held 'strong' beliefs in *primary* items (e.g., "Some places are haunted by the ghosts of dead people") actually displayed erratic patterns of approval toward related *secondary* items (e.g., "When people die, part of them still remains on earth in another form") (Jinks 2012a:141). Those participants who responded affirmatively to a given primary item, *and* their related secondary item(s) because they possess greater knowledge of the topic (i.e. they are better informed) were referred to as 'informed believers,' and those who responded affirmatively to the primary item only were referred to as 'quasi-believers.' Jinks concluded that the latter group might hold only superficial understanding of what they claim to believe. It may even be the case that the associations between disorders/dysfunctions and paranormal belief might be more a function of a propensity to hold quasi-beliefs, rather than the fact that these beliefs refer to extraordinary content.

Houran and Lange (2012) argued that Jinks' method had merit and could be applied to PB "scales that have been validated" (p. 161), a recommendation supported by Jinks (2012b). We argue that none of the established PB scales, as currently used, differentiate between informed believers and quasi-believers, though opportunity may be there in the pools of approved items to discern the hypothesized difference. From that perspective, it is possible that in any sample of believers, there are quasi-believers who have not fully considered the implications of their beliefs, and informed believers who have (which is not to ignore the fact that there are non-believers who can be classed as informed skeptics but, as mentioned, the present study is focused on belief, not non-belief or disbelief). If 'quasi-believer' and 'informed believer' are two legitimate types, as well as other possible types on a continuum of paranormal belief, scoring differences on primary items and related secondary items should help us to identify these types. Critically, these types may even display different psychological and behavioral traits than those that generally characterize the typical paranormal believer. In this paper, we will attempt to demonstrate such differences in

three areas: (i) the primary-item/secondary-item scoring dichotomy, (ii) reality testing, and (iii) depression.

Reality Testing

Reality testing comprises "a set of perceptual, cognitive, and sensorimotor acts that enables one to determine one's relationship with the external physical and social environments" (Reber 1995:640). Irwin points out that hypotheses may be scrutinized "in the light of prior personal experience, general knowledge, and the input of authoritative others and similar sociocultural sources" (Irwin 2003:15), and he explains that the evaluative process of logical testing and probing translates as *reality testing*. For the purposes of this paper, we classify reality testing deficits as cognitive deficits.

While the concept of reality testing has been arguably associated with paranormal belief for some decades (Alcock 1981, Zusne & Jones 1982), empirical testing of the relationship is "meagre" or "artifactual" (Irwin, 2004:144). Irwin (2003) used three subscales from the *Bell Object Relations and Reality Testing Inventory* (BORRTI) (Bell 1995) to test against various subscales of PB as measured on Tobacyk's Revised Paranormal Belief Scale (R-PBS), which include New Age Philosophy (NAP), Traditional Paranormal Beliefs (TPB), Extraordinary Life Forms, Precognition, Psi, Spiritualism, Superstition, and Witchcraft. The measures of reality testing from the BORRTI were 'Reality Distortion' (RD), 'Uncertainty of Perception' (UP), and 'Hallucinations and Delusions' (HD). Significant effects (Spearman's r_s) ranged from weak to moderate (.15 to .58), in a sample drawn from a "general community of Australian adults" (Irwin 2003:17)—only the correlations between Precognition and RD, and TRB and UP, were not significant. After transformation of NAP, TPB, and UP data, regression analyses showed that (a) NAP was predicted by RD, UP, and HD, and (b) TPB was predicted by RD and HD.

Using the Reality Testing subscale of the *Inventory of Personality Organisation* (IPO-RT) (Lenzenweger et al. 2001), Irwin (2004) found reality testing deficits (IPO-RT) correlated positively (Spearman's r_s) with the same measures of R-PBS, although TPB was replaced by Traditional Religious Beliefs (TRB). Effects ranged from weak to moderate (.32 to .63), in a sample drawn once again from a "general community of Australian adults" (Irwin 2004:147). Irwin concluded that some people, "when faced with an anomalous experience, jump to a paranormal interpretation without due critical testing of the logical plausibility of this belief" (Irwin 2004:149). Irwin could not demonstrate 'clinical' levels of reality testing deficits in his sample because the IPO-RT measures these deficits in terms of

an "informational processing style" rather than as "psychotic phenomena" (Irwin 2004:145).

Dagnall et al. (2010) found significant correlations (Pearson's *r*) of moderate strength between reality testing deficits (IPO-RT) and the two PB measures (NAP and TPB). They concluded that reality testing deficits were "fundamentally involved in the formation and maintenance of some paranormal beliefs" (Dagnall et al. 2010:25). Drinkwater, Dagnall, and Parker (2012) replicated these two results, and a significant correlation between Australian Sheep–Goat Scale (ASGS) (Thalbourne 1995) and IPO-RT was also found.

Irwin and Marks (2013) have also found a significant correlation of IPO-RT with their new subscale, New Age Belief (NAB), but not for their other subscale, Traditional Religious Belief (TRB), both of which make up their Survey of Scientifically Unaccepted Beliefs (a.k.a. Survey of Popular Beliefs). Recently, Irwin, Dagnall, and Drinkwater (2015) performed two bootstrapping regressions (since they had significantly skewed data) and found that the IPO-RT (their criterion variable) was again a predictor of NAB and, on this occasion, the TRB.

These findings indicate a consistent effect, but they may prove only how conventional approaches fail to draw out other dimensions of paranormal belief.

Depression

Depression (including the depression dimension of bipolar disorder) may be related to paranormal belief (Irwin 2009, Thalbourne & Houran 2004). Depression is a low-mood state indicated by aversion to activity, with possible negative effects on a person's thoughts, behavior, feelings, and well-being (DSM-IV-TR 2000). Given that depression is also described as a state of sadness, anxiety, emptiness, hopelessness, helplessness, worthlessness, guilt, irritability, shame, and restlessness, we class depression as a psychodynamic dysfunction. Depression per se and the depression dimension of bipolar disorder (i.e. manic-depression) are essentially the same. If a given paranormal belief measure correlates positively with a measure of manic-depression, it can be assumed that paranormal belief scores predict both mania and depression and not likely one or the other. For example, Thalbourne and Delin (1994) found that both Depressive Experience (measured on the Depression subscale of the Manic-Depression Scale, MDS) *and* Manic Experience (measured on the Mania subscale of the MDS) were both "significantly and positively related to belief in, and alleged experience of, the paranormal" (cited in Thalbourne & Houran 2004:140).

Focusing on depression only, a combination of findings exist with regard to the relationship between PB and *trait* depression (an ongoing, stable element of an individual, rather than a temporary state—see Spielberger et al. 2003). While some studies have found a positive relationship (Thalbourne & Delin 1994, Thalbourne & French 1995), others have found no association between the two (Zebb & Moore 2003). Tobacyk found a correlation between scores on the R-PBS and "depressive attributional style" (p. 864), the latter measured on the Attributional Style Questionnaire (Peterson et al. 1982). Though mixed findings exist, they do generally suggest that trait depression may enhance susceptibility to PB.

The relationship between *state* depression and PB is also implied where 'helplessness' (a cognitive aspect of depression, see Abramson, Seligman, & Teasdale 1978) correlated with scores on the R-PBS (Dudley 1999). However, Irwin (2009) suggests that depression is not necessarily indicated if a given situation was merely perceived to be uncontrollable.

From his own (sometimes co-authored) studies from 1994 to 2004, Thalbourne (2005) reports 10 out of 19 positive and significant Depression/PB correlations, but his review is confined to the same few researchers, and the effects are generally small. Overall, findings by other researchers are mixed (see Irwin 2009:94 for details). More recently, in a study by Billows and Storm (2015a), depression measured on the Beck Depression Inventory (BDI-II) did not correlate significantly with either the ASGS (Thalbourne 1995) or the Basic Limiting Principles Questionnaire (Thalbourne 2010).

Rationale for the Study

It is clear that further research is needed in the above areas of deficits and dysfunction, but we stress that it is crucial that any such research also involves critical approaches to PB that question current definitions and understandings. Measures of PB may be shown to be psychometrically sound, but we must also ask if those measures detect an internally consistent subset of propositions regarding an anomalous topic, its putative causes, and/or alternative examples of the topic. And, if not, we might ask how the presence of these apparent quasi-beliefs impacts on current conceptions of paranormal belief.

The over-arching aim of the present study is to develop and administer a question-set of PB items drawn from established PB measures to determine whether participants who self-report strong belief in primary items maintain this level of belief when responding to secondary items. To do this, we aim to construct a paranormal belief instrument (the so-called Paranormal Belief Informedness Scale) by which we shall endeavour to identify subsets of believers who may or may not exhibit varying levels of reality-testing

deficits and depression. It is therefore crucial that we first demonstrate the existence of, and differences between, primary and secondary items, and subsequently show that response differences between the two types of items help identify various believer types whose responses to particular measures of deficits and dysfunction are not necessarily consistent.

Methods

Participants

The original study was divided into two parts. For Part 1, the initial sample (N = 387) comprised: (i) first-year psychology students from the School of Psychology, University of Adelaide (Adelaide, South Australia), who received credit for laboratory participation (n = 71); (ii) students and staff from various disciplines from the University of Adelaide, including non-credited School of Psychology students (n = 36); and (iii) online respondents who were informed of the study by word of mouth or via various websites, including the Australian Institute of Parapsychological Research, Inc. (AIPR), a number of Facebook pages, and APD Performance Pty Ltd, a market research service (n = 280). All of the student-participants in (i) above, and most of the participants in (ii) above, completed the questionnaires in Lance Storm's (L.S.) laboratory. No remuneration was offered for participation. The research was approved by the School of Psychology Ethics Sub-Committee.

Of 387 participants, 59% were females (n = 227), and 41% were male (n = 160). Age ranged from 18 to 81 years (M = 42 years, SD = 18 years, n = 386—one participant did not give age). Age distributed normally.

For Part 2, which was a qualitative study, we sought to screen and interview ten gamblers from various gambling establishments in the Manchester area, UK. Five gambling categories would be covered: horses, bingo, slots, cards, and sports-betting. For comparative purposes, interviewees were either 'high-scoring' or 'low-scoring' on the so-called Paranormal Belief Informed Scale (PBIS; details about the PBIS are given in the Results section; a full report of Part 2 of this two-part study is planned as a separate article).

Measures

The study was administered via computer monitor and started with an information page (providing a plain language description of the aim and nature of the study and contact details for the principal experimenter and for counseling services in case of adverse reactions to the study). The information

page was followed by a consent form and a demographics inventory on one page each, followed by a test instrument labeled "Paranormal Belief Questionnaire" (PBQ). The latter included 244 anomalous and paranormal belief items drawn from ten established belief questionnaires. Six of these ten scales are regarded as "historically significant" (Irwin 2009:177). Primarily, the three major parapsychological categories—extra-sensory perception (ESP, including telepathy, clairvoyance, and precognition), psychokinesis (PK), and life after death (LAD)—were covered, as were other paranormal and anomalous categories such as supernatural and religious beings, witchcraft and occult practices, superstition, spiritualism, and extraordinary life forms (i.e. 'crypto-morphs'). The PBQ comprises all these categories as captured in the ten scales, which are here described:

1. **Anomalous Experiences Inventory (AEI)** (Gallagher, Kumar, & Pekala 1994). The full-scale AEI contains 70 true/false items that form five subscales concerning anomalous/paranormal experiences, beliefs, abilities, fear of the paranormal/anomalous, and use of drugs and alcohol. Example item #9: "I have lived before." The subscales have shown good convergent validity when correlated with selected personality measures. Specifically for the present study, the AEI acronym refers only to the *Anomalous/ Paranormal Belief Subscale*, consisting of 12 true/false items. The sub-scale theoretical mean score is 6 (min. = 0; max. = 12).

2. **Australian Sheep–Goat Scale (ASGS)** (Thalbourne 1995, 2010). The ASGS is an 18-item self-report measure of belief in and alleged experience of the paranormal (ESP, psychokinesis, and life after death). Items are each scored 0 (false), 1 (uncertain), and 2 (true), along a visual analogue scale (e.g., "I am completely convinced that: ESP does *not* exist . . . . . ESP exists"). Theoretical (raw) mean score = 18 (min. = 0; max. = 36). The ASGS data are 'top-down purified' using Rasch-scaling techniques (Rasch 1980),[4] thus yielding a measure that has *interval-level* properties (Lange & Thalbourne 2002). This procedure alters the scoring range and mean. Higher total scores indicate stronger beliefs in the facets of paranormal phenomena mentioned. For a total score on the Rasch-scaled ASGS (RASGS), only 16 of the 18 item-Rasch-scores are summed (the scores on the two afterlife items are not included). The RASGS has been standardized with a mean of 25 (SD = 5). RASGS scores range from 8.13 to 43.39. In a good-sized sample (N = 131, Storm & Thalbourne 2005), the ASGS gave a high reliability coefficient, Cronbach's $\alpha = 0.91$ (Billows & Storm 2015a, report Cronbach's $\alpha = 0.95$).

3. **Basic Limiting Principles Questionnaire (BLPQ)** (Thalbourne 2010). The BLPQ is a 26-item self-report measure of belief in and alleged experience of the paranormal. It is an attempted improvement by Thalbourne (2010) on the ASGS, professing superior wording, alternate positive and negative wording to avoid acquiescence response bias, and additional items (four on mind–body dualism, three on paranormal healing, and two on clairvoyance; see Thalbourne 2010). Example item #2: "I believe I have had personal experience of ESP." Each item includes a five-point Likert scale ranging from 1 = *Strongly Disagree*, to 5 = *Strongly Agree*. Theoretical (raw) mean score = 78 (min. = 26; max. = 130). Higher scores indicate stronger belief in the paranormal. Thus far, this measure has been used only once in a thesis by Billows (Billows 2014, see Billows & Storm 2015a, 2015b). Billows and Storm (2015a) report Cronbach's $\alpha = 0.96$. The BLPQ has since been Rasch-scaled (RBLPQ) (Lange 2016) to remove age and gender bias, resulting in a 23-item version. The RBLPQ has been standardized with a mean of 50 ($SD = 15$), scores range from 6.35 to 106.25.

4. **Belief in the Paranormal Scale (BPS)** (Jones, Russell, & Nickel 1977). The BPS is a 25-item scale measuring psychic, supernatural, and occult phenomena, as well as "divination and prophecy, legendary creatures and civilizations, and other scientifically unattested phenomena" (Irwin 2009:41). Example item #4: "I firmly believe that ghosts or spirits do exist." Five items are negatively worded to discourage acquiescence. Responses are recorded on a five-point Likert scale, ranging from 1 = *Strongly Disagree*, to 5 = *Strongly Agree*. Scores range from 25 to 125. As far as the scale's reliability (test–retest) and validity (predictive, concurrent, and construct) are concerned, Irwin reports that the scale has "psychometric adequacy" (Irwin 2007:42).

5. **Extraordinary Beliefs Inventory (EBI)** (Otis & Alcock 1982). The EBI is a 30-item scale measuring extraordinary beliefs such as "luck, spirits, religion, psychic phenomena, creatures, and fortune-telling" (Otis & Alcock 1982:81). Example item #19: "There is such a thing as *extrasensory perception* (ESP)." Responses are recorded on a seven-point Likert scale, ranging from 1 = *Strongly Disagree* to 7 = *Strongly Agree*. Scores range from 30 to 210. Jones and Alcock reported that the consistency of responses was high, with alpha values ranging from .68 (creatures) to .92 (religion).

6. **Jinks' Belief Questionnaire (JBQ)** (Jinks 2012a). The JBQ is an 89-item paranormal and anomalous belief scale containing 14 primary items and 30 secondary items about ESP, PK, LAD, the Bermuda triangle,

extraterrestrials, mysterious hominids, lake monsters, ghosts, astrology, and the prophesies of Nostradamus, as well as: (a) a limited range of complementary and alternative medical categories (CAM), including feng shui, homeopathy, iridology, and acupuncture for the purpose of hypothesis testing; (b) four control items to determine the extent of participant cooperation, and (c) 41 cover items (decoys) to disguise the explicit association between primary items and similar but non-specific secondary items. The four control items included three patently false propositions: (i) "*UFOs have landed in broad daylight near the Sydney Opera House*"; (ii) "*Some newborn babies can speak as well as adults*"; (iii) "*Some gifted people don't need to eat or drink but can live on sunlight alone*"; and one statement assuming agreement ("*Some people have bad nightmares*"). The order of presentation was randomized, with no question from the same category adjacent to another. Participants were required to answer items using a six-point Likert scale (Leung 2011) for the likelihood of the item's content being true, with responses ranging from 1 = *Definitely Not*, to 5 = *Definitely*. Each topic consisted of between two and six items, with one primary item and the remainder being secondary items. The secondary items, referring to potential anomalous *explanations* for the concepts, events, entities, or personalities referred to in the primary item, were derived from the relevant literature sources (e.g., Berlitz 1974 for a sympathetic, paranormal explanation of disappearances in the "Bermuda Triangle," or Barnes 2012 for an account of the shared ancestry of humans with mystery hominids).

7. **Magical Ideation Scale (MIS)** (Eckblad & Chapman 1983). The MIS, which consists of 30 true/false items, is a measure of paranormal aspects of magical ideation (i.e. "belief in forms of causation that by conventional standards are invalid"—Eckblad & Chapman 1983:215). The MIS has been used to predict symptoms of schizotypy and schizophrenia proneness. Example item #30. "I have sometimes felt that strangers were reading my mind." Twenty-three items score 1 point for a 'True' response; seven items score 1 point for a 'False' response. Internal consistency reliability values are good: .82 (males); .85 (females).

8. **Survey of Scientifically Unaccepted Beliefs (SSUB)** (Irwin & Marks 2013)—also labeled the *Survey of Popular Beliefs* (SPB) for general use. The SSUB is a 20-item self-report survey that measures the "intensity of scientifically unaccepted beliefs" (Irwin & Marks 2013:150). There are two sub-scales in the SSUB: *New Age Beliefs* (NAB), 15 items, example item #9: "Fortune tellers can accurately sense the future using a crystal

ball"; and *Traditional Religious Beliefs* (TRB), 5 items, example item #1: "The Devil (Satan) is a real entity." Responses range on a five-point Likert scale from 1 = *Strongly Disagree*, to 5 = *Strongly Agree*. Irwin and Marks explain that scores on each scale are "computed as the sum of responses to the items in the respective scale and then converted to scores with interval-level measurement" (Irwin & Marks 2013:150) based on Rasch-scaling techniques. The Rasch measures for both scales have been standardized with a mean of 25 (*SD* = 5). NAB scores range from 13.37 to 36.53; TRB scores range from 15.62 to 34.12. Cronbach's α range across studies from .89 to .93 (Irwin 2015, Irwin, Dagnall, & Drinkwater 2015).

9. **Paranormal Short Inventory (PSI)** (Randall 1997). The PSI is a 13-item measure of paranormal belief. Example item #4: "Contrary to scientific opinion, there is some validity to fortune telling." Seven items are reverse-scored (example item #2: "For the most part, people who claim to be psychics are in reality very good actors"). Responses are measured on a six-point Likert scale ranging from 1 = *Strongly Disagree*, to 6 = *Agree Strongly*. The full-scale theoretical mean score is 45.5 (min. = 13; max. = 78).

10. **Paranormal Belief Scale-Revised (RPBS)** (Tobacyk 2004). The RPBS (for convenience, PBS-R) is a 26-item scale that measures degree of belief in each of seven dimensions: Extraordinary Life Forms, Precognition, Psi, Spiritualism, Superstition, Traditional Religious Belief, and Witchcraft. Example item #21: "Some psychics can accurately predict the future." Tobacyk (2004) notes improvements from the original 25-item PBS (see Tobacyk & Milford 1983), including the adoption of a seven-point Likert scale, and item changes for three subscales (Extraordinary Life Forms, Precognition, and Witchcraft). The PBS-R boasts "greater reliability and validity, less restriction of range, and greater cross-cultural validity" (Tobacyk 2004:94). The full-scale theoretical mean score is 104 (min. = 26; max. = 182). Four-week test–retest reliabilities for the PBS-R subscales range from .60 to .95.

Also administered were[5]:

(a) the **Reality Testing subscale of the Inventory of Personality Organization (IPO-RT)** (Lenzenweger et al. 2001)—a 20-item unidimensional, self-report measure, which assesses aspects of reality testing. Responses are recorded on a five-point Likert scale (1 = *Never True*, to 5 = *Always True*). Total scores can range from 20 to 100, with high scores

indicating reality testing deficits—example item #3: "When I'm nervous or confused, it seems like things in the outside world don't make sense either." Internal consistencies ranging from .85 to .87, and test–retest reliability correlated highly at $r = .80$ (Lenzenweger et al. 2001:579).

(b) **Beck Depression Inventory II (BDI-II)** (Beck, Steer, & Brown 1996)—a 21-item self-report measure of depression, designed to align with characteristics of depression as dictated by the *Diagnostic and Statistical Manual of Mental Disorders (DSM-IV-TR)*. The items focus on symptoms of depression such as hopelessness, irritability, and changes in sleep and appetite. The BDI-II contains 21 questions, each answer being scored on a scale value of 0 to 3. Higher total scores indicate more severe depressive symptoms. Standardized cutoffs: 0 to 13 = "minimal depression" 14 to 19 = "mild depression"; 20 to 28 = "moderate depression"; and 29 to 63 = "severe depression" (Beck, Steer, & Garbin 1988). The BDI displays high internal consistency ($\alpha = 0.91$), and one-week test–retest reliability, $r = .93$ (Beck, Steer, & Brown 1996). Recent analysis of the instrument showed similar results with internal consistency of around $\alpha = .91$ and test–retest ranging from $r = .73$ to .96 (Wang & Gorenstein 2013).

(c) **Conformity Scale (CS)** (Mehrabian & Stefl 1995)—an 11-item scale with each item scored on a nine-point scale (four items are reverse-scored), ranging from −4 (*very strong disagreement*) to +4 (*very strong agreement*). Mehrabian (2005) defines conformity as "a characteristic willingness to identify with others and emulate them, to give in to others so as to avoid negative interactions, and generally to be a follower rather than a leader in terms of ideas, values, and behaviors" (p. 2)—example item #1: "I often rely on, and act upon, the advice of others." The CS was administered as a safeguard since some participants may be either conformist (or nonconformist) in their responses, and therefore indiscriminately agree (or disagree) to any or all items.

Procedure

Information and questionnaire material was presented on a computer monitor. Each stage of the experiment was time-stamped (i.e. after completion of each scale). All participants read the Information page and then confirmed their acknowledgment on the Consent page—clicking onto the next screen automatically registered consent of the participant, who then provided some demographic details. Participants then completed the PBQ, the IPO-RT, the BDI-II, and the CS. For the PBQ, participants were required to respond to items using the original Likert, visual analogue, multiple-

choice, and dichotomous (i.e. True/False) options unique to each of the ten PB instruments. The design was not counterbalanced. In studies where there are manipulations of the independent variables, and the researcher wishes to eliminate order effects and demand characteristics, counterbalancing can be helpful (for examples, see Reis & Judd 2000). However, since the PBQ consists of ten paranormal belief scales, all of which measure the same construct, and since there were no treatments, order effects are likely to be small. The IPO-RT and the BDI-II are administered after the PBQ. The studies reviewed above also show that paranormal belief scales were administered before the reality testing scale (Irwin 2003, 2004), or the depression scales (Thalbourne & Delin 1994, Thalbourne & Houran 2004).

Prior to analysis of the PBQ data, we independently determined the primary and secondary items within standard scales using our criteria above (see first paragraph in the section *Primary and Secondary Paranormal Belief Items*; see also the section *Preliminary Item Assessment* below). Jinks' *control* items were used to screen out unsuitable participants. These items are not specifically associated with any of the paranormal categories. We then:

(i) ran an exploratory factor analysis, the highest factor loadings of which were used to construct a Paranormal Belief Informedness Scale (PBIS) comprising 10 primary items, and 10 secondary items;

(ii) identified subsets of believers in the sample: (a) 'primary believers' who responded at any level of agreement to *all* 10 primary belief items of the PBIS (note that 'agree' was qualified as 'slightly,' 'somewhat,' 'moderately,' or 'strongly' depending on the Likert scale); (b) 'primary non-believers' who responded at any level of disagreement to *all* 10 primary belief items of the PBIS; (c) 'mixed believers'; a heterogeneous group who responded at any level of agreement with *some* of the 10 primary items in the PBIS; and (d) two smaller groups (i.e. quasi-believers and informed believers) derived from the primary believer group;[6]

(iii) compared response rates on primary and secondary items; and

(iv) differentiated mean-scoring and correlational differences between various belief types on reality testing deficits and depression.

For details regarding (i), see the section *Factor Analysis and Creation of the PBIS*. Regarding (ii), (iii), and (iv), see the section *Planned Analyses*.

Hypotheses

Hypothesis 1 (H1): Mean scores for primary belief items (by belief categories ESP, PK, and LAD, and paranormal belief generally) are higher than mean scores for secondary belief items for the three believer groups (primary non-believers, mixed-believers, and primary believers).

Hypothesis 2 (H2): Mean scores for secondary belief items (by belief categories ESP, PK, and LAD, and paranormal belief generally) are higher for informed believers than quasi-believers.

Hypothesis 3 (H3). There are rank-order relationships between paranormal belief and reality testing deficits and depression, both between and within believer groups.

Results

Preliminary Item Assessment

Prior to participant testing, there were two independent assessments of items from nine of the ten PB scales (the JBQ was not included in this assessment as that scale had been created with those subdivisions already established—see Jinks 2012a). Item assessment was by the first and third authors (L.S. and A.L.J.), requiring subdivision of all items into two types (namely, primary and secondary). For the most part, there was mutual agreement on item assessment, although the wording of some items was ambiguous (these items were never used in the Factor Analysis). Table 1 shows counts of primary and secondary items for all nine scales. Eckblad and Chapman's (1983) MIS was a particular challenge for L.S. and A.L.J., who concluded that 18 of the 30 items (60%), although arguably secondary, were ambiguous. The difficulty with the MIS may have stemmed from the fact that magical ideation tends to refer to specific ideas or situations and not to generalized notions.

Data from a total of 387 respondents (cases) were collected. All data were checked for scores of 5 (*very probably*) or 6 (*definitely*) to *at least one* of the three patently false control items, and scores of 1 (*definitely not*) to the "*nightmare*" item. These participants may have been non-cooperative or acquiescent (Krosnick 1999). In total, 35 cases were removed due to extreme scores on these items as just indicated. Another nine cases were removed for completing the entire online task in less than 10 minutes (prior testing of the time taken to read through the entire survey as fast as reasonably possible, not including time to ponder answers, and without actually using the mouse to select answers, took about 20 minutes; the median time taken

TABLE 1
Primary and Secondary Items for the Nine Paranormal Belief Scales

PB Scale	Primary Items	Secondary Items	Ambiguous Items	Totals
1. AEI	4	8	n/a	12
2. ASGS	10	7	1	18
3. BLPQ	16	10	n/a	26
4. BPS	18	6	1	25
5. EBI	24	6	n/a	30
6. MIS	7	5	18	30
7. PBS-R	17	9	n/a	26
8. PSI	9	4	n/a	13
9a. SPB-NAB	9	6	n/a	15
9b. SPB-TRB	4	1	n/a	5
Total Items	118	62	20	200

AEI = Anomalous Experiences Inventory; ASGS = Australian Sheep–Goat Scale; BLPQ = Basic Limiting Principles Questionnaire; BPS = Belief in the Paranormal Scale; EBI = Extraordinary Beliefs Inventory; MIS = Magical Ideation Scale; PBS-R = Paranormal Belief Scale–Revised; PSI = Paranormal Short Inventory; SPB-NAB = Survey of Popular Beliefs (New Age Belief); SPB-TRB = Survey of Popular Beliefs (Traditional Religious Belief); n/a = not applicable

to complete the survey was 55 minutes, which is close to the advertised time of 50 minutes based on legitimate pre-testing). Ultimately, the data of 44 respondents (11%) were excluded from the analysis on account of these two screenings.

Levels of conformity as measured on the Conformity Scale (CS) (Mehrabian & Stefl, 1995) were then assessed with the aim of excluding low- or high-scoring participants from further analysis. CS scores are shown in Table 2. To test the distributions for normality, the skewness and kurtosis values were divided by their respective *SE* values (if the statistics

fall between ±1.96 they are not significant, and are regarded as normal; see George & Mallery 2010). Test results showed that the data curve was significantly left-skewed ($p = .027$), suggesting that some number of low-scoring non-conformists accounted for the curve's deviation from normality. Although the Shapiro-Wilk test result was significant ($p < .001$), the lowest scores came from only two cases, each of whom had non-significantly low scores of −22 ($z = 1.28$, $p = .100$), and, given that the theoretical lowest score is considerably lower at −44, we did not deem these two cases significantly nonconformist. This assumption was supported by inspection of a box-and-whiskers graph which showed no outliers.

Descriptive statistics of all paranormal belief scales using data from the final reduced database ($N = 343$) are given in Table 2, including Cronbach's α values for each scale. Even though all efforts were made to produce an acceptable database for analyses, all PB scales were significantly skewed and/or kurtotic. The same was the case for the IPO-RT and BDI-II scales. It was decided that all hypotheses would be tested using nonparametric statistical tests where possible since the measures are ordinal or interval (e.g., Likert scales), and the relationships are monotonic (inspections of the distributions showed this to be so). In Hypothesis 1, we conducted Repeated Measures ANOVA, but we made appropriate tests on group variance beforehand.

A series of non-hypothesized Spearman's r_s correlations on PB scale scores (ten scales) for comparative purposes are given in Table 3. It can be seen that all scales (and subscales) correlate significantly, which is generally to be expected. The strengths of the correlations (moderate to high) are fairly consistent across scales. Weaker rank correlation coefficients are found to be consistent across belief scales for the three subscales, Superstition (from the PBS-R), Traditional Religious Belief (PBS-TRB), and Traditional Religious Belief (SPB-TRB), although the two TRB subscales correlate very highly, as is to be expected, $r_s(341) = .91$.

We then produced a correlation matrix of the 114 items to assess the strength of relationships since a matrix that is factorable should include sizeable correlations (we required all correlations to be over .30). Overall, the matrix was a positive manifold, but seven items were removed for having values <.30. This final count of 107 items was submitted for factor analysis (FA).

Factor Analysis and Creation of the PBIS

The final database was ready for the next stage of assessment. Given $N = 343$, our sample size was considered acceptable for FA. Cattell (1978:508, see also Arrindell & van der Ende 1985:166) recommends an absolute

TABLE 2
Descriptive Statistics (*N* = 343): Twelve Paranormal Belief Scales, Conformity, Depression, and Reality Testing Deficits

Variable	*M*	*SD*	Min.	Max.	Cronbach's α
1. AEI	6.56	3.60	0.00	12.00	.87
2. ASGS (R)	23.50	8.32	8.13	43.39	.95
3. BLPQ (R)	52.17	11.94	6.35	88.70	.96
4. BPS	70.70	22.59	25.00	116.00	.96
5. EBI	107.38	40.95	30.00	191.00	.97
6. JBQ	117.86	41.30	44.00	245.00	.98
7. MIS	7.85	5.78	0.00	24.00	.87
8. PBIS	27.92	7.36	20.00	40.00	.96
9. PBS-R (Full Scale)	92.90	32.94	29.00	157.00	.95
PBS-R Subscales					
Extraor. Life Forms	3.64	1.23	1.00	7.00	.60
Precognition	3.51	1.68	1.00	7.00	.90
Psi	3.72	1.63	1.00	7.00	.87
Spiritualism	3.96	1.89	1.00	7.00	.92
Superstition	2.09	1.37	1.00	7.00	.91
Trad. Religious Belief	4.01	1.82	1.00	7.00	.85
Witchcraft	3.74	1.80	1.00	7.00	.91
10. PSI	40.45	13.68	14.00	72.00	.89
11. SPB-NAB	24.12	3.37	13.37	31.94	.93
12. SPB-TRB	24.28	4.42	15.62	34.12	.87
13. Conformity	7.21	11.53	−22.00	36.00	.71
14. Depression (BDI-II)	11.36	11.30	0.00	59.00	.95
15. Reality Testing (IPO-RT)	45.22	15.12	20.00	90.00	.92

AEI = Anomalous Experiences Inventory; ASGS (R) = Rasch-scaled Australian Sheep–Goat Scale; BLPQ (R) = Rasch-scaled Basic Limiting Principles Questionnaire; BPS = Belief in the Paranormal Scale; EBI = Extraordinary Beliefs Inventory; JBQ = Jinks' Belief Questionnaire; MIS = Magical Ideation Scale; PBIS = Paranormal Belief Informedness Scale; PBS-R = Paranormal Belief Scale–Revised; PSI = Paranormal Short Inventory; SPB-NAB = Survey of Popular Beliefs (New Age Belief); SPB-TRB = Survey of Popular Beliefs (Traditional Religious Belief).

TABLE 3
Correlation Matrix (Spearman's r_s): Twelve Paranormal Belief Scales (*df* = 341)

Scale	1.	2.	3.	4.	5.	6.	7.	8.	9a.	9b.	9c.	9d.	9e.	9f.	9g.	9h.	10.	11.
1. AEI	—																	
2. ASGS (R)	.77																	
3. BLPQ (R)	.80	.92																
4. BPS	.81	.83	.88															
5. EBI	.76	.78	.83	.92														
6. JBQ	.78	.77	.81	.88	.87													
7. MIS	.69	.69	.72	.70	.69	.73												
8. PBIS	.83	.83	.85	.90	.85	.81	.67											
9. a. PBS-R Full	.76	.78	.83	.92	.95	.87	.70	.83										
b. Extraor. Life Form	.47	.47	.49	.64	.64	.71	.47	.52	.68									
c. Precognition	.71	.75	.79	.83	.82	.81	.64	.82	.85	.51								
d. Psi	.74	.81	.83	.86	.82	.79	.63	.84	.85	.57	.74							
e. Spiritualism	.83	.82	.86	.90	.85	.82	.65	.87	.87	.53	.81	.80						
f. Superstition	.14[a]	.21	.24	.28	.40	.39	.35	.14[b]	.45	.38	.37	.23	.21					
g. Trad. Religious Belief	.36	.33	.35	.41	.61	.38	.33	.35	.60	.28	.37	.34	.39	.27				
h. Witchcraft	.61	.63	.66	.79	.78	.69	.55	.69	.84	.54	.64	.70	.67	.33	.50			
10. PSI	.74	.79	.84	.88	.84	.83	.69	.83	.83	.53	.86	.78	.81	.31	.33	.67		
11. SPB-NAB	.77	.80	.84	.88	.86	.84	.69	.83	.86	.55	.85	.78	.85	.32	.37	.69	.89	
12. SPB-TRB	.27	.26	.28	.32	.53	.29	.25	.26	.48	.18[c]	.27	.26	.27	.22	.91	.42	.26	.29

AEI = Anomalous Experiences Inventory; ASGS (R) = Rasch-scaled Australian Sheep–Goat Scale; BLPQ (R) = Rasch-scaled Basic Limiting Principles Questionnaire; BPS = Belief in the Paranormal Scale; EBI = Extraordinary Beliefs Inventory; JBQ = Jinks' Belief Questionnaire; MIS = Magical Ideation Scale; PBIS = Paranormal Belief Informedness Scale; PBS-R Full = Paranormal Belief Scale–Revised (Full Scale); PSI = Paranormal Short Inventory; SPB-NAB = Survey of Popular Beliefs (New Age Belief); SPB-TRB = Survey of Popular Beliefs (Traditional Religious Belief). All p values significant at $p < .001$ except: [a] $p = .011$; [b] $p = .012$; [c] $p = .001$.

minimum sample size of no less than 250, but others advise larger samples—see Comrey & Lee 1992, who consider 300 to be 'good'). Also, we could have opted for a subject-to-variable (STV) ratio as low as 2:1, as suggested by Kline (1979:40), provided there is a minimum of 100 participants, and the lower limit of variables-to-factors ratio is 3 to 6, which was not an issue since we ran our FA for a single PB factor. However, we followed the rule that the STV ratio should be at least 3:1 (for pilot and theoretical studies), giving us an upper limit of 114 items that we could confidently enter into the FA.

Prior to this consideration (as we could not foresee how many participants we would get, or would be left with after screening), we had already decided in advance to create a Paranormal Belief Informedness Scale (PBIS) comprising only items that speak to conventional PB phenomena (namely, ESP, PK, and LAD items). Although there were nine PB scales with a total of 200 items (JBQ data were not entered into the FA, as we plan to analyze those data in a followup study), we reduced this number to 107 items that describe only these three conventional PB phenomena (65 primary belief items, and 42 secondary belief items).

Factor Analysis (principal axis factoring) was conducted, given that we are interested only in common variance, or, put another way, we only wished to analyze covariation among items, without intrusion of the specific variance associated with particular items. Only one factor was extracted, as we were preparing a single PB scale and not interested in factors per se as the items are all from previously published and validated scales with no purpose served in factor-wise reassessment of the items. Furthermore, for the purposes of hypothesis testing, we are only interested in primary/secondary differences. As a failsafe measure, we conducted another Factor Analysis, and a Principal Components Analysis, both allowing for multiple factors as discerned from Eigenvalues over 1.00. As it happened, in both cases, only one factor proved viable, with other factors producing loadings that were considerably smaller.

The Kaiser–Meyer–Olkin Measure of Sampling Adequacy was 0.98, which Kaiser (1974) characterizes as "marvelous." Moreover, Bartlett's test of sphericity gave a value that was large and significant: 42253.40, $df = 5671$, $p < .001$, so it appears unlikely that the correlation matrix is an identity matrix. Once again, the data were appropriate for factor analysis. The Factor Matrix is described next.

Our single factor has an Extraction Sum of 57.62, with 53.85% of the variance explained. As there are too many items to list, factor loadings ranged from .412 to .904, and communalities ranged from .170 to .802. Factor loadings (starting from the highest and working downward) were

used to select the items for a 20-item PBIS scale comprising 10 primary belief items and 10 secondary belief items. These 20 items are listed in the Appendix. It can be seen that they come from five scales: BLPQ (2 items), EBI (8 items), PBS-R (3 items), BPS (4 items), and PSI (3 items).

The next step in PBIS scale development was to standardize the scoring of the 20 items for hypothesis testing in the next section. In fact, due to further testing requirements, this standardization was necessary for all items since the PB measures use scoring methods ranging from true/false scales, to five-, six-, and seven-point Likert scales. Since responses for all scales are essentially binary (i.e. agreement vs. disagreement), we recoded all responses (disagreement = 1; agreement = 2), thus yielding dichotomous items. The PBIS theoretical mean score would be 30.00, but actual mean score was 27.92 (*SD* = 7.36; see Table 2 for other statistics). The distribution was significantly skewed and kurtotic, but there were no outliers.

Planned Analyses

H1: Mean scores for primary belief items (by belief categories ESP, PK, and LAD, and paranormal belief generally) are higher than mean scores for secondary belief items for the three believer groups (primary non-believers, mixed-believers, and primary believers). Testing this hypothesis involved assessing whether there was a scoring differential between mean scores on primary items and secondary items for each paranormal category: ESP, PK, and LAD, and for paranormal belief generally. Testing would also necessarily involve discerning differences between types of believer (i.e. we expect scoring to increase across the groups primary non-believers, mixed believers, and primary believers, in that order). We applied only one strict criterion for selection as a primary believer; respondents had to *agree* with *all* 10 primary items in the PBIS.[7] This criterion is necessary because if disagreement with even one primary item is allowed, other belief types could not be labeled as distinct types due to category overlap and statistical test results would be ambiguous. Primary non-believers were respondents who *disagreed* with *all* 10 primary items in the PBIS. The remainder were a heterogeneous (mixed) group of believers (they *agreed* or *disagreed* with any number of the 10 primary items in the PBIS). Scores on secondary items were included to make up the full PBIS score. There were 115 primary non-believers, 180 mixed believers, and 48 primary believers. Table 4 lists the PBIS mean scores for the three believer types.

A series of four Repeated Measures ANOVA tests were conducted to determine scoring differences: items (primary vs. secondary) × group (three primary believer types), where ‘items’ is our within-subjects variable, and

TABLE 4
Descriptive Statistics: Paranormal Belief Informedness Scale (PBIS), Reality Testing Deficits (IPO-RT), and Depression (BDI-II) for Believer Types

	M	*SD*
PBIS		
1. Primary Non-Believers (*n* = 115)	20.40	0.83
2. Mixed Believers (*n* = 180)	29.65	5.31
3. Primary Believers (*n* = 48)	39.35	1.18
• Quasi-Believers (*n* = 17)	38.18	1.33
• Informed Believers (*n* = 31)	40.00	0.00
IPO-RT		
1. Primary Non-Believers (*n* = 115)	37.03	12.58
2. Mixed Believers (*n* = 180)	47.69	14.47
3. Primary Believers (*n* = 48)	55.56	13.61
• Quasi-Believers (*n* = 17)	54.59	14.48
• Informed Believers (*n* = 31)	56.10	13.32
BDI-II		
1. Primary Non-Believers (*n* = 115)	11.77	13.00
2. Mixed Believers (*n* = 180)	11.22	10.56
3. Primary Believers (*n* = 48)	10.92	9.69
• Quasi-Believers (*n* = 17)	11.94	9.38
• Informed Believers (*n* = 31)	10.35	9.96

'group' is our between-subjects factor. It is emphasized that the primary and secondary item scores are separate variables and therefore the relatedness of their means cannot be tested other than by Repeated Measures ANOVA and, albeit routine, it is mandatory (and certainly not a perfunctory exercise) to test group differences, again justifying Repeated Measures ANOVA.

For the ANOVA to be valid, and ensure full variance in the item measures across types, we used the 87 remaining (or so-called 'excluded') non-PBIS items from the established scales after their original scores had been standardized via conversion to binary scores (i.e. agree/disagree). These 87 were also divided into primary and secondary for the comparisons to be run in the ANOVA tests. We were aware that some scales contained ambiguous items that might contaminate the results due to category overlap, so we excluded these items from the analyses. There were six primary items that could be about ESP or PK, and one that could be about ESP, or PK, or LAD, and one that could be about PK or LAD; there were three secondary items that could be about ESP, or PK, or LAD; and one secondary item that could describe either ESP or LAD. A total of 12 items were removed leaving 75 items: 47 primary items (ESP: 24 items; PK: 8 items; and LAD: 15 items); and 28 secondary items (ESP: 17 items; PK: 3 items; and LAD: 8 items). We are skeptical about the test validity on PK items with so few primary and secondary items (especially as there are only three secondary PK items—we comment further on this problem in the Discussion).

Levene's tests showed that the three belief groups failed to meet the assumption of homogeneity of variance, but pre-testing on the data using Welch's F test and Brown-Forsythe test indicated that the groups are nevertheless significantly different in spite of the violation (Tomarken & Serlin 1986).

Repeated measures ANOVA on ESP items. Table 5 lists mean scores on the 75-item scale by believer type and psi category. Results were significant, and in the directions expected: Items, $F(1, 340) = 5.90$, $p = .02$, partial eta-squared (η^2) = .017; Believer type, $F(2, 340) = 293.91$, $p < .001$, partial $\eta^2 = .63$ (primary/secondary item scoring was significantly different, and all groups were significantly different from each other). There was also a significant interaction effect, Item × Type, $F(2, 340) = 3.20$, $p = .042$, partial $\eta^2 = .02$. The interaction effect means the primary/secondary item scoring gap was not constant across levels of belief, but widened disproportionately. In other words, the scoring gap was determined in part by group membership and not by the primary–secondary difference alone. Figure 1 illustrates these effects.

Repeated measures ANOVA on PK items. Results were significant, but only two effects (group and interaction) were in the directions expected—the primary/secondary item difference was not in the direction hypothesized: Items, $F(1, 340) = 103.05$, $p < .001$, partial $\eta^2 = .23$; Believer type, $F(2, 340) = 199.88$, $p < .001$, partial $\eta^2 = .54$ (all groups were significantly different from each other). There was also a significant interaction effect, Item × Type, $F(2, 340) = 4.68$, $p = .010$, partial $\eta^2 = .03$. Figure 2 illustrates these

TABLE 5
Descriptive Statistics: Mean Item Score for Psi Categories (ESP, PK, and LAD) by Believer Type (75 Items)

Item Type	Believer*	Mean	*SD*
Primary ESP Items	1	1.08	0.08
	2	1.41	0.23
	3	1.82	0.13
	Total	1.36	0.30
Secondary ESP Items	1	1.09	0.10
	2	1.39	0.25
	3	1.79	0.15
	Total	1.31	0.30
Primary PK Items	1	1.03	0.08
	2	1.21	0.22
	3	1.59	0.25
	Total	1.20	0.26
Secondary PK Items	1	1.12	0.21
	2	1.41	0.32
	3	1.85	0.21
	Total	1.38	0.36
Primary LAD Items	1	1.15	0.19
	2	1.54	0.31
	3	1.90	0.12
	Total	1.46	0.36
Secondary LAD Items	1	1.11	0.12
	2	1.42	0.26
	3	1.81	0.19
	Total	1.37	0.31
All Primary Psi Items	1	3.25	0.24
	2	4.16	0.61
	3	5.31	0.40
	Total	4.02	0.82
All Secondary Psi Items	1	3.32	0.29
	2	4.21	0.66
	3	5.44	0.41
	Total	4.08	0.86

* 1 = primary non-believers ($n = 115$); 2 = mixed-believers ($n = 180$); 3 = primary believers ($n = 48$)

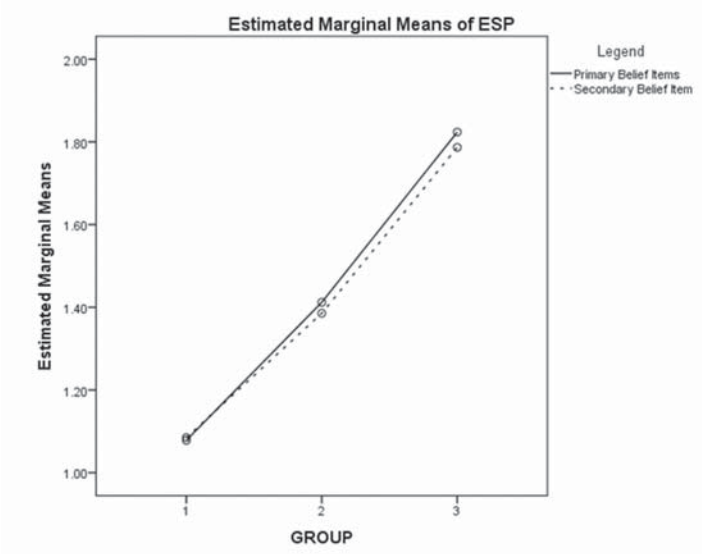

Figure 1. Belief in ESP: Three significant effects: (i) item (primary vs. secondary); (ii) group (1 = primary non-believers; 2 = mixed believers; 3 = primary believers); and (iii) interaction. All effects are in the directions hypothesized.

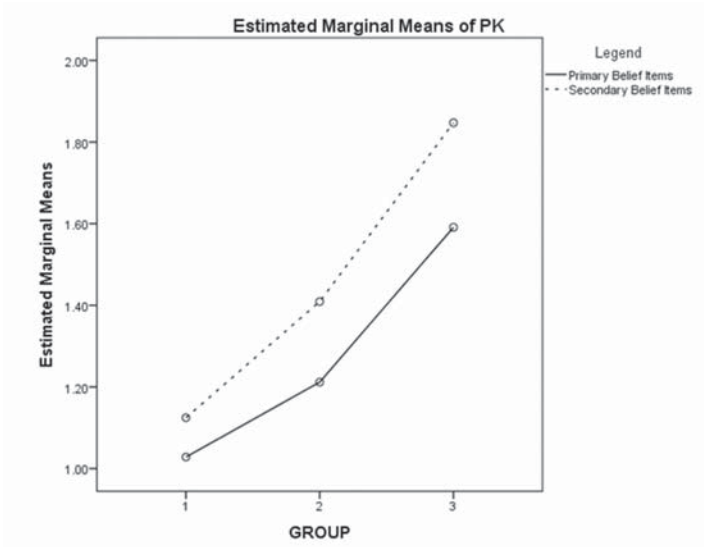

Figure 2. Belief in PK: Three significant effects: (i) item (primary vs. secondary); (ii) believer group (1 = primary non-believers; 2 = mixed believers; 3 = primary believers), and (iii) interaction.

effects. We will discuss the unexpected 'Items' result in the Discussion section.

Repeated measures ANOVA on LAD items. Results were significant, and in the directions expected: Items, $F(1, 340) = 27.08$, $p < .001$, partial $\eta^2 = .07$; Believer type, $F(2, 340) = 245.93$, $p < .001$, partial $\eta^2 = .59$ (all groups were significantly different from each other). There was also a significant interaction effect, Item × Group, $F(2, 340) = 3.30$, $p = .038$, partial = .02. Figure 3 illustrates these effects.

Repeated measures ANOVA on paranormal belief (all psi items). Two results were significant, but only one effect (group) was in the direction expected: Believer type, $F(2, 340) = 336.68$, $p < .001$, partial $\eta^2 = .66$ (all groups were significantly different from each other). The primary/secondary item difference was not in the direction hypothesized: Items, $F(1, 340) = 14.62$, $p < .001$, partial $\eta^2 = .04$. There was no significant interaction effect, Item × Type, $F(2, 340) = 0.98$, $p = .378$, partial $\eta^2 = .01$. Figure 4 illustrates these effects. Hypothesis 1 was partially supported. We will discuss the unexpected 'Items' result in the Discussion section.

H2: Mean scores for secondary belief items (by belief categories ESP, PK, and LAD, and paranormal belief generally) are higher for informed believers than quasi-believers. A major aim of the present study was to differentiate informed believers from quasi-believers on secondary item scoring as Jinks (2012a) had done. Informed believers respond affirmatively to *all* primary and *all* secondary items in the PBIS, whereas quasi-believers respond affirmatively to *all* primary items only in the PBIS, but they respond negatively to *all* secondary items in the PBIS. We found 31 informed believers among the 48 primary believers. Thirty-one informed believers in a sample of 343 participants is about 9%. There were 17 quasi-believers. Data from the 75-items scale were analyzed using Mann-Whitney U tests.

For ESP, secondary item scoring was significantly higher for informed believers ($Mdn = 1.82$) than for quasi-believers ($Mdn = 1.76$), $U = 157.50$, $p = .011$ (one-tailed), $r = .33$.

For PK, secondary item scoring was significantly higher for informed believers ($Mdn = 2.00$) than for quasi-believers ($Mdn = 1.67$), $U = 194.00$, $p = .041$ (one-tailed), $r = .43$.

For LAD, secondary item scoring was significantly higher for informed believers ($Mdn = 1.88$) than for quasi-believers ($Mdn = 1.75$), $U = 141.00$, $p = .004$ (two-tailed), $r = .39$.

For psi generally, secondary item scoring was significantly higher for informed believers ($Mdn = 5.64$) than for quasi-believers ($Mdn = 5.27$), $U = 125.50$, $p = .002$ (one-tailed), $r = .43$. The four-part hypothesis was supported.

Figure 3. Belief in Life After Death: Three significant effects: (i) item (primary vs. secondary); (ii) believer group (1 = primary non-believers; 2 = mixed believers; 3 = primary believers); and (iii) interaction. All effects are in the directions hypothesized.

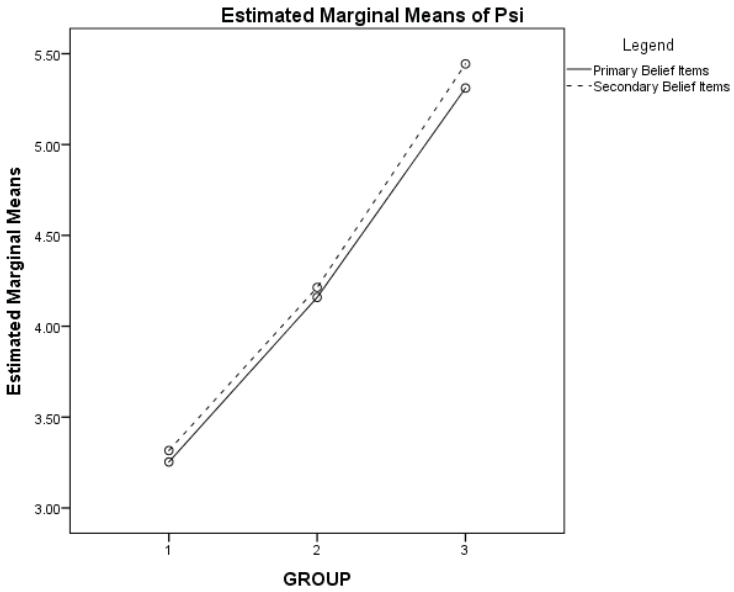

Figure 4. Paranormal Belief: Two significant effects: (i) item (primary vs. secondary); and (ii) group (1 = primary non-believers; 2 = mixed believers; 3 = primary believers).

To illustrate the kind of thinking that underpins quasi-belief, we take three examples from the PBIS (one from each psi category ESP, PK, and LAD) to show the response inconsistencies of quasi-believers. In the ESP category, informed believers endorsed the primary item ebi10 ("Psychics possess a mysterious ability to know things about a person's past and future") and the secondary item ebi23 ("Some people have a mysterious ability to accurately predict such things as natural disasters, election results, political assassinations, etc."), whereas quasi-believers endorsed only the primary item. The difference between the two types of believer may lie in the failure of quasi-believers to extend their belief about psychics' knowing "things about a person's . . . future" to 'predictions' about specific personal events ("election results, political assassinations"), and/or impersonal events ("natural disasters").

In the PK category, informed believers endorsed the primary item ebi1 ("There is a real phenomenon known as *psychokinesis* (the ability to move objects by the power of the mind)"), and the secondary item pbs16 ("A person's thoughts can influence the movement of a physical object"), whereas quasi-believers endorsed only the primary item. The difference between the two types of believer may lie in the failure of quasi-believers to equate "the power of the mind" with "the influence" of a "person's thoughts," both of which (mind power and thoughts) are generally seen as the underlying mechanism of psychokinesis.

For LAD, informed believers endorsed the primary item bps8 ("Through psychic individuals it is possible to communicate with the dead") and the secondary item ebi12 ("There is such a thing as astral projection (where the body remains behind while the spirit travels)"), whereas quasi-believers endorsed only the primary item. The difference between the two types of believer would seem to lie in the assumption that quasi-believers tend not equate "the dead" with the "spirit." Inspection of the PBIS (Appendix A) shows that similar assumptions about quasi-believers can be drawn from other item dichotomies.

H3: There are rank-order relationships between paranormal belief and reality testing deficits and depression, both between and within believer groups.

Reality testing deficits. Table 4 above lists IPO-RT mean scores by believer types. It can be seen that reality testing (RT) deficits generally increase across believer groups as PB increases. A Jonckheere-Terpstra test for ordered alternatives showed that there was a statistically significant trend of higher median RT scores with higher levels of PB (from primary non-believer, mixed believer, to primary believer), T_{JT} = 24,822.50, $z = 7.72$, $p < .001$ (two-tailed). A significant Kendall's tau-b

shows a weak-to-moderate effect (RT tended to increase with PB group), $r_\tau(341) = .33$, $p < .001$ (two-tailed).

Even though this significant trend lends support to the literature (see Introduction), these findings do not tell the whole story about the relationships between PB and RT deficits within groups. We ran Spearman's r_s tests for the groups, including one new group—a sub-group of the primary non-believers which we call informed skeptics ($n = 87$) because they respond negatively to *all* primary and *all* secondary items in the PBIS. This sub-group can be justifiably classed as 'informed' following our definition of consistent responding to primary and secondary items.

We need to point out that we cannot validly use PBIS scores in these tests because primary believers effectively have the highest PBIS scores on all 10 primary items (i.e. only the 'agreement' levels are included since agreement defines that type). Similarly, primary non-believers effectively have the lowest PBIS scores because their scores on all 10 primary items are the lowest (i.e. 'disagreement' defines that type). Therefore, variance on the 10 primary items would be reduced for primary believers and primary non-believers, though not for the full sample or mixed-believers. It would therefore be the case that significant correlations, with the PBIS as one variable, would probably be artifacts caused by the reduced variance in the PBIS. We therefore ran our tests on the same 75-item dataset (i.e. the excluded-items dataset) that was used in the series of tests on Hypotheses 1 and 2, and not only did we form one generalized paranormal belief set, we also preserved believer category, item category, and psi category, to see if the RT correlates would vary across these categories. We thus ran nine category tests for the full sample, and nine category tests for each of the six groups derived thereof.

Table 6 lists the correlations. As can be seen, all nine of nine correlations are significant for the full sample, across psi (ESP, PK, and LAD) and item (Primary, Secondary) categories, but the trend tends to dissolve across believer groups (i.e. as paranormal belief increases). Primary non-believers showed eight significant correlations out of nine; informed skeptics showed seven significant correlations out of nine; mixed-believers showed four significant correlations out of nine (note that mixed believers is the biggest group; $n = 180$); and finally primary believers, and the subgroups thereof (quasi-believers and informed believers) have no significant correlations between them (note, too, that generally the correlations are very weak for those subgroups).

Bonferroni correction was made by dividing the critical p value ($\alpha \leq .05$) by the number of correlations, which was 63: The new critical $p = .05/63 = .0008$. The nine full-sample correlations remain significant, as do

TABLE 6
Correlations: Reality Testing Deficits (IPO-RT) by Paranormal Belief, Item, and Psi Category

Variable (75 items)	Full Sample (N = 343)	Primary Non-Believers (n = 115)	Informed Skeptics (n = 87)	Mixed-Believers (n = 180)	Primary Believers (n = 48)	Quasi-Believers (n = 17)	Informed Believers (n = 31)
Primary Items							
ESP	.40***	.29**	.33**	.01	.05	.01	.02
PK	.41***	.29**	.30**	.20*	–.09	–.07	–.14
LAD	.34***	.18*	.22*	.03	.26	.35	.18
All Psi (P)	.41***	.32***	.34**	.09	.02	–.02	–.08
Secondary Items							
ESP	.40***	.24**	.23*	.12	.20	.20	.14
PK	.35***	.22*	.19	.12	.09	–.06	.14
LAD	.43***	.14	.10	.26***	.10	–.10	.10
All Psi (S)	.45***	.29**	.25*	.20**	.16	.01	.15
All Psi + (P + S)	.44***	.32**	.31**	.15*	.07	–.02	.01

All Psi = ESP + PK + LAD; P = Primary; S = Secondary; *** $p < .001$ (two-tailed); ** $p < .01$ (two-tailed); * $p < .05$ (two-tailed)

two others: (i) 'All Psi' (primary items only) for primary non-believers, and (ii) LAD (secondary items only) for mixed believers.

We then tested the difference between the correlation coefficients using the r_s values for the full sample as test statistics for comparisons against the corresponding r_s values for each group. There were 32 significant *z*-score differences out of 54 (59%). A Bonferroni correction was made by dividing the critical *p* value ($\alpha = .05$) by the number of tests. The new critical $p = .05/54 = .0009$. The count was reduced to 6/54, or 11%, which is more than twice the 5% we might expect by chance.

The hypothesis of relationships between paranormal belief and reality testing deficits was supported for the full sample across psi categories and item types, but not generally across believer types.

Depression. Table 2 above shows descriptive statistics for the BDI-II (Depression) scale ($N = 343$)—we note the mean BDI score is 11.36 ($SD = 11.30$), which is in the "minimal depression" (i.e. lowest) range of

0 to 13 (Beck, Steer, & Garbin 1988). Table 4 above lists BDI mean scores by believer types. It can be seen that mean scores for depression generally decrease across the groups and are lowest for primary believers. However, a Jonckheere-Terpstra test for ordered alternatives showed that the trend of higher median BDI-II scores with lower levels of PB was not significant, $T_{JT} = 17{,}891.00$, $z = 0.48$, $p = .629$ (two-tailed). Kendall's tau-b shows no effect (BDI-II is effectively constant across groups), $r_{\tau}(341) = .02$, $p = .629$ (two-tailed).

We ran tests on the 75-item dataset, using believer category, item category, and psi category, as before. For the full sample, correlations ranged between −.001 and .03 (none were significant); primary non-believers correlations ranged between −.02 and .16 (none were significant); mixed believers correlations ranged between −.01 and .06 (none were significant); and primary believers of which correlations were all negative, ranged between −.01 to −.44 (the latter being significant, $p = .002$). This one significant correlation out of 24 tests can be attributed to chance, and the general picture is that depression is constant whatever the level of PB.

Discriminant functions analysis. It must be borne in mind that the PBIS has primary and secondary subscales, scores of which are used to construct the three different groups: primary non-believers, mixed believers, and primary believers. Since this grouping factor cannot be taken into account in a regression analysis, we reversed our aims and conducted a discriminant functions analysis to find a model that might predict membership in the three groups based on scores on reality testing deficits (IPO-RT) and depression (BDI-II). If the literature is correct, this model should show that reality testing deficits and depression predict membership in the paranormal belief groups, but we propose that these relationships are tenuous and do not apply across all believer groups.

The assumption of equal group variance was met, as Box's M test was not significant at the critical level $\alpha < .001$, $F(6, 179044.94) = 2.48$, $p = .021$. As there were three groups, two functions were extracted. Only the first discriminant function was significant, Wilks' $\lambda = 0.80$, $\chi(4, N = 343) = 76.47$, $p < .001$ (Canonical correlation = .45). Function 1 had an Eigenvalue of 0.25, accounting for 100% of the explained variance between groups. Function 2 had an Eigenvalue of zero, explaining 0% of variance. We conclude that the reality testing measure (IPO-RT) has some predictive capacity, but depression (BDI-II) does not.

From Table 7 it can be seen that membership of mixed believers was predicted with the greatest accuracy (80.0%), followed by primary non-believers (53.9%). However, primary believers were predicted with least accuracy (2.1%). Although 60.3% of the original grouped cases have been

TABLE 7
Group Classification Matrix Using Reality Testing Deficits (IPO-RT) and Depression (BDI-II) as Predictors of Believer Group Membership

Group	Predicted Group Membership			
	Primary Non-Believers	Mixed Believers	Primary Believers	Total Count (%)
Primary Non-Believers	62 (53.9%)	53 (46.1%)	0 (3.0%)	115 (100%)
Mixed Believers	34 (18.9%)	144 (80.0%)	2 (1.1%)	180 (100%)
Primary Believers	4 (08.3%)	43 (89.6%)	1 (2.1%)	48 (100%)

correctly classified, the model fails for primary believers. In essence, reality testing did not predict for primary believers (see Discussion for more details).

Post Hoc Analyses

A reconsideration of reality testing. As a defense of the PBIS, we conducted Spearman's r_s tests on the reality testing variable with the nine PB scales and nine PB subscales (as well as the BDI-II, Conformity, and the SES). These correlations are presented in Table 8. Focusing on the established PB scales only, all 18 of 18 correlations are significant for the full sample, primary believers, skeptics. It seems an insurmountable fact that there are weak-to-moderate relationships between paranormal belief (in some of its various forms) and reality testing deficits. However, the trend starts to fragment as we move through the remaining types: Fourteen are significant for mixed believers; five are significant for primary believers; only three for quasi-believers; and only two for informed believers.

If we correct for multiple analysis (Bonferroni adjusted to $p = .05/126 = .0004$), our findings do not change for counts at the level of $p < .001$ (full sample, and most correlations for primary non-believers, informed skeptics, and mixed believers). The only significant correlation for primary believers (MIS; $r = .64$) maintains significance after adjustment, and bootstrapping revealed that the 95% CI [.39, .80] does not include zero. As we may expect, no correlations maintained significance for quasi-believers and informed believers.

We then tested the difference between the correlation coefficients using the r_s values for the full sample as test statistics for comparisons against the

TABLE 8
Reality Testing (IPO-RT) Correlations (Spearman's r_s): Ten PB Scales, Depression (BDI-II), Conformity, and Spiritual Emergency

	Reality Testing Deficits						
Scale	**Full Sample ($N = 343$)**	**Primary Non-Believers ($n = 115$)**	**Informed Skeptics ($n = 87$)**	**Mixed Believers ($n = 180$)**	**Primary Believers ($n = 48$)**	**Quasi-Believers ($n = 17$)**	**Informed Believers ($n = 31$)**
1. AEI	.44***	.27**	.22*	.20**	.09	−.02	.11
2. ASGS (R)	.54***	.59***	.52***	.27***	.09	−.01	.11
3. BLPQ (R)	.55***	.57***	.52***	.33**	−.04	−.17	.02
4. BPS	.49***	.51***	.53***	.18*	.27	.19	.31
5. EBI	.51***	.48***	.50***	.24**	.40**	.50*	.33
6. JBQ	.55***	.50***	.45***	.34***	.38**	.58*	.23
7. MIS	.66***	.43***	.34**	.61***	.64***	.70**	.55**
8. PBS (R) Full	.51***	.45***	.44***	.26***	.30*	.40	.22
Extraor. Life Form	.39***	.24**	.26*	.21**	.17	.39	.07
Precognition	.47***	.48***	.44***	.19*	.07	.26	−.09
Psi	.41***	.38***	.41***	.06	−.01	−.29	.27
Spiritualism	.46***	.41***	.41***	.18*	.07	−.07	.06
Superstition	.43***	.42***	.41***	.35***	.43**	.40	.49**
Trad. Religious Belief	.23***	.23*	.25*	.05	.08	.18	.05
Witchcraft	.40***	.40***	.43***	.13	.19	.34	.08
9. PSI	.49***	.42***	.42***	.25**	.09	.20	.02
10a. SPB-NAB (R)	.48***	.50***	.46***	.20**	.07	.05	.04
10b. SPB-TRB (R)	.18**	.26**	.29*	.01	.11	.24	.07
11. BDI-II	.31***	.33***	.34**	.34***	.41**	.43	.45*
12. Conformity	.20***	.40***	.45***	.21**	.13	.34	.10
13. SES	.61***	.47***	.40***	.49***	.31*	.18	.37*

Note that 'Quasi-Believers' and 'Informed Believers' are subsets of 'Primary Believers'. AEI = Anomalous Experiences Inventory; ASGS (R) = Rasch-scaled Australian Sheep–Goat Scale; BLPQ (R) = Rasch-scaled Basic Limiting Principles Questionnaire; BPS = Belief in the Paranormal Scale; EBI = Extraordinary Beliefs Inventory; JBQ = Jinks' Belief Questionnaire; MIS = Magical Ideation Scale; PBS-R Full = Paranormal Belief Scale-Revised (Full Scale); PSI = Paranormal Short Inventory; SPB-NAB = Rasch-scaled Survey of Popular Beliefs (New Age Belief); Rasch-scaled SPB-TRB = Survey of Popular Beliefs (Traditional Religious Belief); BDI-II (Beck Depression Inventory II); SES = Spiritual Emergency Scale (Likert scale version); *** $p < .001$ (two-tailed); ** $p < .01$; * $p < .05$ (two-tailed).

corresponding r_s values for each group. There were 45 significant *z*-score differences out of 108 (42%). A Bonferroni correction was made by dividing the critical *p* value (α = .05) by the number of tests. The new critical *p* = .05/108 = .0005. The count was reduced to 10/108, or 9%, which is greater than the 5% we might expect by chance.

Even if we regard the correlations for quasi-believers and informed believers as spurious due to small *n*, revised statistics are even more supportive of a decline. There would be 28 significant *z*-score differences out of 72 (39%). A Bonferroni correction gives a new critical *p* = .05/72 = .0007. The corrected count is now 11/72, or 15%, which is three times greater than that 5% expected by chance. We cannot dismiss the evidence of a decline in the number of significant relationships between reality testing and paranormal belief as we move through the groups from primary non-believers to primary believers.

Sample size. It is well-noted that small samples are best tested using nonparametric tests (Corder & Foreman 2014). We used Spearman's r_s, and argue that low-*n* and reduced variance are not likely to explain the decline in numbers of significant correlations across believer types (see Tables 6 and 8): First, most groups were of a suitable size, though the sub-groups are small (with the exception of informed skeptics; *n* = 87). The smallest group (primary believers; *n* = 48) has a maximum margin of error at 95% CI of about ±14% (i.e. .98/√*n*; Mallard 2011), and the 95% CIs drop to ±7% for the largest group (mixed believers; *n* = 180), and assuming the rank correlation coefficients should maintain their magnitude across types (under the assumption that the alternative hypothesis is true) only the *p* values should change (i.e. increase) as size of *n* changes (i.e. decreases).

Second, primary non-believers not only have the smallest *SD* (12.58) for IPO-RT (see Table 4), they also have the smallest *SDs* on all paranormal belief categories except for Primary LAD (see Table 5), yet primary non-believers produced eight significant correlations out of nine in Table 6.

Third, the mixed believers (the biggest *n*) not only have the largest *SD* (14.47) for IPO-RT (see Table 4), they also have the largest *SDs* on all paranormal belief categories except Primary PK (see Table 5), yet they produced only four significant correlations out of nine in Table 6.

Other correlates of reality testing. Looking at other correlations, we note that IPO-RT correlates positively and significantly with depression (BDI-II) across all believer types except quasi-believers due to low *n* (but even then, we can regard the correlation of .43 for quasi-believers as a replication). If there are RT deficits, there tends to be evidence of depression no matter what the type.

Finally, conformity correlates significantly and positively with reality

testing deficits four times out of seven (though not for primary believers, quasi-believers, or informed believers), and spiritual emergency (SES) correlates significantly and positively with reality testing deficits 6 times out of 7.[8] (These findings are discussed further in the Discussion section.)

Discussion

Jinks (2012a) claimed that his findings demonstrated that "most participants identified as strong believers in a select range of anomalous topics ([i.e. primary] items) were *less* likely to support the legitimacy of equivalent [secondary] items, or items expressing a widely held (anomalous) explanation for the topic" (p. 143). Items in the first class (primary items) would replicate the familiar propositions found in common PB questionnaires referring to, for example, anomalous occurrences like ESP, PK, and LAD. Items in the second class (secondary items) variously represent the standard anomalous explanations for the primary items, or they offer an alternative example of the primary item, or they are the primary items reworded so as to exclude specific reference to (in our example) any anomalous occurrences like ESP, PK, and LAD. For pragmatic (research) purposes, we aimed to investigate these differences, our first step being to explore whether, and which, paranormal belief items from standard (validated) scales could be designated 'primary' and 'secondary,' followed by the systematic development of a questionnaire set containing those two classes of item. Our preliminary evaluations of the ASGS and the BLPQ (Thalbourne 2010), the RPBS (Tobacyk 2004), and the SPB (Irwin & Marks 2013) indicated that such an undertaking was possible.

We stress that if the theoretical underpinnings of this qualitative process amounted to nothing more than random assignation, we could not expect significant scoring differences between the two sets of items as proposed in our hypotheses. On the contrary, we did demonstrate differences between primary and secondary items; and these differences support our theory. Namely, that there are such things as primary and secondary items *in extant scales*, and these two types of items *elicit different responses*. We add that the results of *H1* and *H2* show that the PBIS can be used as a predictor of responses to primary and secondary items in the range of extant belief scales tested in the present study. Also, we were able to show that belief in secondary items is not as strong as belief in primary items, but only for two types of paranormal phenomena, ESP and LAD, with no strong evidence that the claim is true for PK due to the low item count in that category. We found that the primary/secondary effect occurs across believer types,[9] and is therefore not confined to so-called 'strong' believers. We also found that paranormal belief was (a) not always predicted by reality testing

deficits, and (b) not predicted *at all* by depression. We will now discuss these findings in detail.

Primary vs. Secondary Items

In testing *H1*, we found primary/secondary differences for ESP and LAD, but we found that the difference between primary and secondary PK items was not in the direction hypothesized—i.e. the mean score for primary items was *not* higher than the mean score for secondary items. We pointed out that we were skeptical about the test validity on PK items with so few primary and secondary items (especially as there are only three secondary PK items in the whole set of 75 items), and it is clear that this bias adversely affected the outcomes for the test on paranormal belief. Ironically, it is not so much a failure of the present paper to deliver the kind of result we hypothesized regarding PK, as much as it is a shortcoming in scale designs over the past four decades for not having more PK items—after all, we can only work with what we have available to us. A count of the items in the scales used in this study (not including the JBQ) shows an overwhelming obsession with ESP and mental states, and a corresponding lack of interest in PK and physical states—we counted 72 ESP items (52% of the total), but only 32 PK items (23% of the total). There were 35 LAD items (25%; note that due to concept overlap, some items were counted more than once so that 107 items becomes 139 counts). This bias probably has disciplinary roots—it is mostly psychologists who do parapsychology (their focus tends to be on the 'mental,' and it is they who design PB scales), whereas we see lesser numbers of physicists and biologists doing parapsychology (their focus tends to be on the 'physical,' but they do not design PB scales). It is an oversight (for want of a better word) that may or may not have gone unnoticed by other researchers, but it is certainly not a talking point among parapsychologists as far as we know. Perhaps the present paper will bring some attention to this bias.

Informed Believers vs. Quasi-Believers

Notwithstanding the issues just raised, we can say that designation of PB items as either primary or secondary aided us in identifying two other types of paranormal believer—we have shown that there is evidence that respondents in our sample can hold *quasi-beliefs* (semi-propositional representations of the world superficially believed to be true prior to any truth evaluation), or they can hold *informed beliefs* (which indicate greater knowledge of the topic). For *H2*, we showed that quasi-believers and informed believers respond differently to the large pool of secondary items

with quasi-believers endorsing secondary items significantly less often than informed believers.

In *H2*, we also looked at some inconsistent responses among quasi-believers, confining our comparisons to ESP, PK, and LAD items in the PBIS. It would appear that quasi-belief implies no deep understanding. Responses are either fashioned during the test session itself, or they are outwardly believed in, and have existed as part of an unchallenged belief structure for some time. Thus, we cannot assume with certainty that beliefs quantified by items in a paranormal questionnaire are stable constructs, or are well-formed and logically consistent, yet these are the very assumptions that are often being made by researchers.

Reality Testing Deficits and Depression

In *H3*, we showed that scoring on the reality testing deficits measure (IPO-RT) is related to paranormal belief. Table 8 shows that nine PB scales, and nine PB sub-scales, correlate significantly with IPO-RT. While these results support (and even replicate) those of Dagnall et al. (2010), Drinkwater et al. (2012), Irwin (2004), Irwin, Dagnall, and Drinkwater (2015), etc., we argue that the results may be misleading because they have been generalized to all paranormal believers. We cannot assume that what appears true for a type is true for a sub-type, and we showed this to be the case when we ran tests on the three believer types: primary believers, quasi-believers, and informed believers. As Table 8 also shows, while scoring on conventional PB scales does not reliably predict reality testing deficits for primary believers, and even mixed belief on a few occasions, it does for primary non-believers, including informed skeptics.

In *H3*, we also showed that depression was not related to paranormal belief, and there were no significant differences between believer types. These results, supported by the recent findings of Billows and Storm (2015a, 2016), are a move away from the mixed results of the past, toward the likelihood that PB does not predict depression, and vice versa.

The discriminant functions analysis produced a model that successfully identified membership for primary non-believers and mixed believers, but it also revealed that group membership for primary believers is not predicted by scores on the reality testing measure. While the model would prefer to classify this type as mixed believers (see Table 7), it seems not to distinguish how the two groups are constructed—primary believers have to believe in *all* 10 primary items in the PBIS; mixed believers do not. We suggest this is the kind of problem that arises in conventional paranormal belief testing—special cases are not discerned, yet these may be the very cases to whom past trends and correlations reported in the literature do not apply.

Finally, we note (post hoc) that RT deficits correlated positively and significantly with depression (BDI-II) across all believer types—where there are RT deficits there tends to be evidence of depression. Space does not permit an attempted explanation of the causality underlying this relationship but, given our other findings, we cannot suggest at this stage that PB is instrumental in this relationship.

Though also post hoc, conformity and spiritual emergency appear to correlate significantly and positively with reality testing deficits across most PB groups (though for conformity, we exclude all primary believers). For the SES, we do note that it is a predictor of a number of psychosis symptoms and indicators, but not depression, and the SES does contain a number of paranormal items (Goretzki, Storm, & Thalbourne 2014). Therefore, while we suggest some findings of PB research may be misleading, or misrepresent subsets of paranormal believers, we naturally have reservations about applying that assumption to all PB research as far as some deficits and dysfunctions are concerned. We plan to investigate the PB/SES relationship in more detail in a later study.

Conclusions

In the present study, we demonstrated the tendency for the number of significant PB/IPO-RT correlations to decrease across believer types. We found no evidence of significant correlations of PB with depression (as measured on the BDI-II). Some paranormal beliefs may not be mere expressions of a cultural trend, or fanciful or popular notions that embody contradictions evident in, for example, scoring differences between primary and secondary items. It may be the case that an informed or sufficiently informed subset of paranormal believers, albeit small, has a genuine understanding of the phenomena not entirely (if at all) governed or brought about by some number of deficits, dysfunctions, or disorders. Conventional procedures do not identify this type, possibly because the designers do not concede its likelihood. We do not argue that there is anything fundamentally at fault with the basic constructs investigated in the present study—it is merely our suggestion that there may be subtle differences among paranormal believers, especially those who are high-scoring, but investigators do not seek out those differences. As Jinks (2012a) has said ". . . items in paranormal and anomalous belief questionnaires are not necessarily homogenous devices successfully extracting 'informed' beliefs possessing a rational basis" (p. 148). Our findings suggest that if researchers continue to make generalizations from samples and measures that are clearly heterogeneous, gains will be slow in our understanding of paranormal belief.

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Notes

[1] PB scales, such as Thalbourne's (1995) Australian Sheep–Goat Scale, and Tobacyk's (2004) Revised Paranormal Belief Scale, are often used to identify paranormal believers and disbelievers (i.e. 'sheep' and 'goats,' respectively—see Schmeidler 1945).

[2] A person's informed belief itself does not necessarily have to be legitimate. For example, an individual might possess an informed belief, gleaned through intensive research, regarding the legitimacy of psi. Nevertheless, the veracity of psi effects remains controversial, and might not be legitimate. Similarly, another person's belief in psi could be a quasi-belief if it was accepted blindly without question.

[3] Irwin (2015), citing an example from Quine and Ullian (1970), has recently made a similar argument about belief formation and maintenance.

[4] Top-down purification refers to a set of Rasch-scaling procedures that identify and remedy differential item functioning in questionnaires (i.e. response biases related to extraneous variables such as respondents' age, gender, or even culture).

[5] The Spiritual Emergency Scale (SES) (Goretzki, Storm, & Thalbourne 2014) was also administered, for purposes to be explained in a future article.

[6] Jinks (2012a) does not use the term *primary believer*. His term 'quasi-believer' refers to a respondent who holds a 'strong' belief (i.e. "*very probably*" or "*definitely*") in a given primary item, but does not endorse the relevant secondary item(s). Our term 'primary believer' is provisional and refers to a respondent who expresses agreement to *all* ten primary items—he/she is either a quasi-believer or an informed believer depending on his/her responses to the secondary items in the PBIS.

[7] Note that by Jinks' (2012a:134) criteria, assignment to primary belief status was less restricted, in accordance with categories consisting of no more than a few items. For example, in the "Nostradamus" category, there were 49 "quasi-believers" and only 2 "informed believers" (i.e. 51 primary believers), whereas in the "Ghosts" category there were 27 "quasi-believers" and 7 "informed believers" (i.e. 34 primary believers).

[8] Two versions of the 30-item Spiritual Emergency Scale were

administered—the forced-choice ('Yes'/'No') version, and a five-point Likert-scale version ('*Never*' to '*Very Often*'). These two scales correlate significantly and the relationship is strong, $r(341) = .76$, $p < .001$. The Likert scale version is used in Table 8. We note that both versions do not correlate with depression, suggesting that spiritual emergency is independent of depression.

[9] Of course, it was always expected that PB mean scores on the 75 remaining items would increase across believer types which were defined by scores on the PBIS, items of which were drawn from the same pool as the 75 items. Therefore, we might logically expect mean PB scores to increase significantly across believer categories. Nevertheless, these between-group differences must be proven statistically—we cannot make scientific statements prior to testing, which is the only way to determine the sizes of the main effects and interaction effect, and confirm (or not) the theoretical premise we are trying to demonstrate.

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APPENDIX A
Paranormal Belief Informedness Scale (20 Items): Items, Factor Loadings, and Extraction Communalities

Item	Code	Item wording (ESP, PK, and/or LAD)	P/R *	Loadings	Communalities
1	ebi10	Psychics possess a mysterious ability to know things about a person's past and future. (ESP)	P	.904	.817
2	ebi19	There is such a thing as *extrasensory perception.* (ESP)	P	.895	.802
3	ebi27	There is such a thing as *telepathy* (communication directly from mind to mind). (ESP)	P	.892	.795
4	bps1	I believe psychic phenomena are real and should become a part of psychology and be studied scientifically. (ESP/PK/LAD)	S	.874	.763
5	psi13	Contrary to scientific belief, some people can make contact with dead people. (PK/LAD)	S	.872	.760
6	psi1	It is probably true that some people can predict the future quite accurately. (ESP)	P	.870	.757
7	ebi23	Some people have a mysterious ability to accurately predict such things as natural disasters, election results, political assassinations, etc. (ESP)	S	.865	.748
8	pbs21	Some psychics can accurately predict the future. (ESP)	P	.864	.747
9	ebi11	The spirits of people who have died can sometimes communicate with the living. (LAD)	S	.863	.744
10	bps11	Some individuals are able to levitate (lift objects) through mysterious mental forces. (PK)	S	.862	.743
11	bps8	Through psychic individuals it is possible to communicate with the dead. (LAD)	P	.860	.739
12	pbs26	Some people have an unexplained ability to predict the future. (ESP)	S	.858	.737
13	bps12	I believe that many special persons throughout the world have the ability to predict the future. (ESP)	P	.858	.737
14	ebi1	There is a real phenomenon known as *psychokinesis* (the ability to move objects by the power of the mind). (PK)	P	.858	.736
15	blpq24	I believe that *psychic healing* occurs. (PK)	P	.852	.725
16	psi5	In spite of the laws of science, some people can use their psychic powers to make objects move. (PK)	P	.851	.724
17	pbs16	A person's thoughts can influence the movement of a physical object. (PK)	S	.848	.720
18	ebi12	There is such a thing as astral projection (where the body remains behind while the spirit travels). (LAD)	S	.847	.717
19	ebi25	There is such a thing as *levitation* (raising the body through mental power). (PK)	S	.841	.708
20	blpq14	I believe some people can contact spirits of the dead. (LAD)	S	.828	.685

* P = Primary item, S = Secondary item

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RESEARCH ARTICLE

Multiple-Analysis Correlation Study between Human Psychological Variables and Binary Random Events

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Abstract—Mind–matter interaction experiments have been progressing from targeting simple bias of random number generators to correlation studies between psychological and physical variables, carried out over multiple combinations of these. This paper reports on a new correlation study between human intention and the output of a binary random number generator. The study comprises a total of 720,000 bits from 20 equal sessions, each with a different human participant. Each participant spent one hour attempting to 'influence' the outcome of the random number generator according to a pre-selected intention. During this time the participant was provided feedback on his/her performance by an analog mechanical display, with the needle of a galvanometric instrument moving to the left or right of its initial position, according to the instantaneous output of the random number generator. Psychological variables were obtained from the participants by a hardware dial ahead of each individual run and by a questionnaire before the participant's first experimental session. Three types of data analysis were defined and tested before looking at the data, resembling a blind analysis technique. The first analysis looks at the distribution of hit rates from the 20 participants. A former study of this kind had found a significant result for this type of analysis (Grote 2015). The second analysis tests for correlations between psychological variables obtained before each run and the hit rate of the corresponding subsequent run. The third analysis is a conceptual replication of von Lucadou's correlation matrix method. It consists of multiple correlation tests between psychological and physical variables, which also can be interpreted as a multiple-analysis technique. The results of the study are p-values of $p = 0.438$, $p = 0.703$, and $p = 0.0949$ for the three analysis' results to have occurred by chance under a null hypothesis. The combined p-value for these results is $p = 0.315$. While none of the pre-defined analysis results is significant, a post hoc variant of Analysis 3 that includes the control data is significant with $p = 0.012$ to have occurred by chance, under a null hypothesis.

Introduction

The debate on the existence or non-existence of mind–matter interaction (MMI) is a topic at the fringes of mainstream science, with sometimes strong opinions held by individual researchers defending either view. While for some researchers in the field of anomalous psychology, the existence of mind–matter interaction seems beyond doubt (see, e.g., Radin & Nelson 1989, 2003, Jahn & Dunne 1986), this is not the case at all for the majority of the scientific audience (Odling-Smee 2007, Bösch, Steinkamp, & Boller 2006). Experimental evidence is often a matter of the interpretation of the studies, which makes it difficult for new researchers to form an opinion on the research performed to date, as is visibly exemplified in the dispute on the interpretation and validity of meta-analysis of existing mind–matter experiments (Bösch, Steinkamp, & Boller 2006, Radin et al. 2006, Pallikari 2015). See also the references in Bösch, Steinkamp, and Boller (2006) for an overview of existing research.

Also, the more cautious label of mind–matter correlation (i.e. correlation between human intention and the output of a physical system), which may not postulate direct causality, seems largely neglected by most scientists, even though attempts at explanations of a putative correlation effect, like for example the interpretation as entanglement correlations in a Generalized Quantum Theory (Atmanspacher, Römer, & Walach 2002, Filk & Römer 2011) do exist (von Lucadou, Römer, & Walach 2007, Walach, von Lucadou, & Römer 2014).

For these reasons, it seems of some value to the field if new mind–matter experiments are performed from time to time, in particular if new researchers are involved in conducting such experiments and possibly new aspects are introduced in the experimental approach. The latter should also serve to prevent strict replications of earlier MMI-like experiments, which may suffer from a possible decline of a putative effect, found by a number of replication studies in this field, and discussed for example in Kennedy (2003), von Lucadou, Römer, and Walach (2007), Walach, von Lucadou, & Römer (2014), and references therein.

The study described in this paper is the second study by this author. The first study is described in Grote (2015), and the experimental setting of that study has been modified in the following ways:

1. The rate of random bits produced has been reduced from 1,000 bits/s to 10 bits/s.
2. The random bit generation process has been modified from a Schmidt process to a 1-step Markov process.[1]
3. The feedback has been extended to include a color-lighting

scheme in the background of the galvanometer needle.
4. Feedback has also been extended by the sound of a gong, which is played during a run, if the participant is successful.
5. The sequence of left/right intentions is recorded in the new experiment.
6. The duration of a single run has been reduced from 60 s to 30 s.
7. Before each participant starts the first run, psychological variables categorized into 6 items have been obtained by questionnaire.
8. Before the start of each run, psychological variables are obtained from the participant.
9. The number of participants is 20.

Items 1 and 2 have been introduced based on a suggestion by W. von Lucadou. Items 3 and 4 have been introduced to potentially increase the focus of the participants, and items 5 to 8 allow for different types of analysis, mostly searching for correlations between psychological and physical variables.

While the outcome of the first study (Grote 2015) was not significant overall, one out of four individual analyses was found significant. That analysis is also carried out in this study (Analysis 1), testing the distribution of the basic results (z-scores) of the 20 participants. Analysis 2 in this study tests for correlations between three psychological variables obtained *before each run*, and the basic outcome (number of hits above chance expectation) of the corresponding runs. Analysis 3 is a conceptual replication of the correlation matrix technique that has been used by von Lucadou and others (von Lucadou 2006, von Lucadou, Römer, & Walach 2007), though with fewer variables and fewer participants. This technique uses multiple correlation tests between physical variables (properties of the data) and psychological variables (properties of the participants). No predictions are made about which of the correlations would be significant, but rather the combined significance of all correlations is assessed. This is further detailed in the section ***Pre-planned Data Analysis***.

The analysis of the data was defined and tested before any of the data were actually analyzed, which is also referred to as a *blind analysis* method (Klein & Roodman 2005). Blind analysis is a strict form of a pre-specified analysis in which the analysis code is fully implemented and tested before the data are looked at. Blind analysis is particularly useful in looking for small effects in noise and, in the opinion of the current author, is well-suited to address criticisms of data analysis (Wagenmakers et al. 2015) and of questionable research practices (Bierman, Spottiswoode, & Bijl 2016) in this domain of research.

It was decided to attempt to publish the result of this study regardless of the outcome of the analysis, in order to not contribute to publication bias.

In the section ***Experimental Design***, the experimental setup is described, followed by the section ***Pre-planned Data Analysis*** on the pre-defined data analysis plan. The results of the analysis are presented in the **Results** section. Finally, the **Discussion** section contains a brief discussion of the analysis and results.

Methods

Experimental Design

The experiment described in this paper was designed and conducted by the author. Participants were 20 people (including the author) in different relationships with the author (i.e. friends, friends of friends, work colleagues, etc.) who were interested in the topic, and willing to spend one hour each on actual experimentation time. The participants' age spanned from 21 to 76 years old with a mean age of 46 and a standard deviation of 13 years. The participants included both genders, 11 female and 9 male.

Each participant had agreed to carry out 120 "runs" of the experiment, with each run lasting 30 seconds. A single run would always begin by the participant selecting whether he/she would try to influence the motion of the needle of a galvanometer display to the left side or to the right side during that run. This choice had to be executed by the participant by pushing a switch either to the left or to the right, respectively. The chosen direction would then be displayed to the participant throughout the following (30-s long) run, in order to remind the participant of the chosen direction.

Next, the participant had to turn a dial in order to choose on a scale from 0 to 10 his/her actual mood (0 meaning 'very bad mood', 10 'very good mood'). This dial consisted of a rotary knob that could be rotated by about 270 degrees, in order to choose a number between 0 and 10, which would be displayed to the participant while the knob was rotated. Then the participant would press the 'start' button to begin the 30-s long run. While the run was active, a colored light was lit in the background of the display needle, to signal to the participant that the run was in progress. Figure 1 shows a photograph of the galvanometer display with the background lit during a run.

During each 30-s long run, random binary events would be generated at a rate of 10 per second. A Markov chain with a memory length of one was used to generate the random numbers, as described below. The draw (from the Markov chain) of a logical '0' would result in a step of the display needle to the left side of its current position, while a logical '1' would result

Figure 1. The galvanometer display with lit background during a run.

in a step of the needle to the right side of its initial position. In this way, 300 binary random draws were accumulated during each 30-s run, resulting in a corresponding random walk of the needle. The maximal range of the needle was 11 steps in either direction, with one standard deviation equal to 5 steps ($N = 300$ for Equation 2 below). The color of the light in the background of the display needle was made to change in correspondence with the position of the needle. Additional feedback was given to the participant by playback of a gong sound when the participant exceeded a threshold of 6 steps in the intended direction over the expectation value (zero steps), during the ongoing run.

The participants operated the device (almost exclusively) at their homes and at times convenient to them, according to their own choice. They were instructed to if possible be alone in the room when operating the device, and to finish the assigned 120 runs within one to two weeks if possible.

An individual run of 30 s could not be interrupted by any means, by an internal mechanism that inhibited switching the device off while a run was proceeding. An internal battery in the device assured that the device would run independent from the main power and thus also independent from any possible interruption of the main power during a run. The participants were free to distribute the time to perform the runs at their choice and could choose for any run between left or right intention, but had to respect the constraint that over the 120 runs both left and right intention had to be picked the same number of times, 60 each, respectively. For example, it would have been possible to do all 60 left-intention runs first, followed by the 60 right-intention runs, but the device would not allow for either intention to be chosen more than 60 times, to assure the balancing of

intentions. Therefore, each participant conducted 60 runs with left intention and 60 runs with right intention, accumulating one hour of data in total. Each participant committed to collect this one hour of experimental data, and each participant fulfilled this goal. The total timespan used by the participants to complete the 120 runs varied from less than 1 day to about 4 weeks. The experimental data-taking started in the spring of 2014 and concluded in the summer of 2015, when the number of 20 participants had been reached. Up to four participants could share the device (e.g., members of a family) by freely distributing experimentation time among themselves. Each participant simply had to choose his/her name on the display ahead of a run, in order to allow the data to be associated with the correct participant.

The data of the experiments were stored in two different formats in the device as a safeguard against data errors. No such errors occurred. The data were transmitted to a personal computer after 1 to 4 participants had completed their runs. This data transmission used check-sums to safeguard against transmission errors, and no such errors occurred.[2] The device was then prepared for the next participant(s) by resetting the data memory of the device and programming the names of one or more new participants.

In addition to participant data, a set of control data was taken, which was not explicitly subject to any interaction with the intention of any participant.

Between participants (i.e. when the device was in the hands of the conductor of the study for transferring data and preparing the device for new participants), a number of complete datasets for 'dummy participants' were automatically generated. For this purpose, dummy persons with names '01' to '20' were generated by the conductor, and when the device would recognize a dummy participant name (by the fact that such a name would start with a *number* rather than with a *letter*), it would automatically start an individual run after a random time interval of order 1 minute length. The 'intention' for each such run was chosen randomly by the internal hardware random number generator (RNG) (see the section ***The Binary Random Number Generator***) but satisfying the required equal total number of left and right intentions as for the real runs. This way a complete set of 20 dummy participants was created, spread throughout the time of acquisition of the participants' data, which is taken as a complete control dataset for the study.

As a particular feature of this study, the participants carried the experimental device to their homes, where they could 'work' on the experiment, at the time and in the environment of their choice. While this may appear to be giving up control over the conductance of the experiment compared with a laboratory setting, it has the advantage that the participants might feel more at ease in environments of their choice, and thus might get

more involved in their effort to 'influence' the needle. Ultimately, even in the laboratory, the conductor of the experiment has no control of whether the participant would assert 'influence' on the device according to the pre-stated intention or not. Although no fraud on the participants side was to be expected whatsoever, principal measures to detect physical manipulation or malfunctioning of the binary random number generator were taken, as detailed below.

The author preferred to choose a real physical system (the needle of a galvanometer display) over a computer screen, which is often used in other experiments of this kind. Computer screens are so common in our modern life, that a mechanical display carries the element of 'being different'.

A description of the random number generator is given in the Appendix section ***The Binary Random Number Generator***.

Pre-Planned Data Analysis

To avoid bias, the data analysis procedure was defined and tested before any of the data were actually looked at. Three different investigations (named Analysis 1, Analysis 2, Analysis 3) were carried out, as described in the following subsections. The principal outcome of each of the three analyses is a number describing the probability that the obtained result would have occurred by chance under the null hypothesis, i.e. assuming no correlation between the data and experimenters' intention.[3] The chance probability for the combined results of the three investigations is also given.

Each of the 3 analyses uses simulated (Monte Carlo) data, in order to estimate likelihoods of test results from the participants (and control) data. Using simulated data is a standard technique when the background cannot be easily modeled analytically and in low–signal-to-noise experiments. The null-hypothesis distributions against which the measured scores are evaluated are generated using software random number generators, simulating trials like the ones that the participants in the experiment undertake. However, there is actually no participant providing an intention and so we take the results from these fake-trials as expressions of the statistical scores under the null hypothesis.[4] The simulated (Monte-Carlo) data consist of 10,000 complete sets of data, each resembling data of a full study comprising 20 'participants'.

Another feature of the analysis is that in particular Analysis 2 and Analysis 3 have several degrees of freedom, which is equivalent to applying several tests to a set of data. However, no predictions are made about the outcome of individual tests, but the results of a number of tests are combined into one 'figure-of-merit' (FOM), which can also be called a 'test statistic'. This FOM can, for example, be the product of the estimated likelihoods

of individual test results. This principle was inspired by the correlation matrix technique used by von Lucadou and others, as mentioned in the **Introduction**. In the form used here in Analysis 2, it mainly consists of a method to perform multiple analysis. Analysis 3 is a conceptual replication of the correlation matrix technique as detailed below.

The control dataset, as defined in the above section ***Experimental Design***, will be subject to the same Analyses (1, 2, and 3) as the main dataset. However, the control data play no role in the pre-defined analysis, and can be viewed as a consistency check or can be used in post hoc analysis. Since for the control data there exists no separate set of psychological variables, the psychological variables of the 20 participants are used to be correlated with the control data (this applies to Analysis 2 and Analysis 3, where psychological data are used for correlation with physical data).

All three analyses have been tested with fake datasets, which have been generated by an independent (independent from the algorithm used to generate the simulated/Monte Carlo data) algorithm. No deviation from the expected uniform distribution was found in the 100 datasets used for testing.[5]

Analysis 2 and Analysis 3 have also been tested with dedicated fake datasets that included intentional biases tailored to the specific analysis. This way the proper functioning of the analysis was confirmed, i.e. the ability to detect what the analysis is supposed to detect.

Finally, we point out that the description of the experiment, the definition of the pre-planned data analysis, as well as the analysis code and the complete experimental data, have been uploaded to the website *opensciencefframework* (https://osf.io/) prior to the actual analysis of the data. Also prior to the actual analysis, the data on said website were marked as a read-only representation of the project (i.e. it cannot be modified anymore), and can be made accessible upon request to the author. In particular, this procedure is a *blind analysis* procedure. The pre-defined analysis of the experimental data is performed only after the analysis code has been frozen. In principle, it can then be performed by a single button press. This process is called the *unblinding* or *opening of the box* in other fields. Blind analysis has been successfully applied in nuclear physics and particle physics (Klein & Roodman 2005) and is the standard method to analyze data in these fields today.

Analysis 1. We define a hit to be a high bit when the participant's intention was to move the needle to the right, and to be a low bit when the participant's intention was to move the needle to the left. The total number of hits n_{hits} is the sum of hits scored under *right* intention plus the hits acquired under *left* intention. The z-value over a total number of trials N is then defined as

$$z = \frac{n_{hits} - N/2}{SD} \tag{1}$$

The standard deviation SD is estimated as

$$SD = \sqrt{N/12} \tag{2}$$

Note that the factor 12 under the square root comes from the fact that we obey the statistic of a 1-step Markov chain (von Lucadou 2006), where each random bit depends on the last random bit, as a result of the bit-generating procedure described in the Appendix section ***The Binary Random Number Generator***. The z-score is a useful quantity because it provides an immediate sense of the deviation of the results from expectation.[6]

For Analysis 1, the data as detailed above (z-scores for the number of obtained hits) are calculated for each of the 20 participants separately, such that 20 z-scores are generated. These 20 z-scores are then sorted and (frequentist) p-values are generated for the highest ranking, second-highest ranking, third-highest ranking, and so forth down to the lowest ranking, by comparison with the distribution of the same ranking values determined from a simulated (null hypothesis) dataset. These p-values are two-sided, with $p = 1$ if a data point is exactly in the middle of the compared distribution. The resulting 20 p-values are combined (by summing over the inverse squares of p-values) and result in the figure of merit (FOM) for this test. The chance probability for the value of this FOM is measured against the distribution for the same FOM derived from the Monte Carlo dataset. A one-sided probability will mean that the FOM of the test data (or a lower one) has occurred by chance. This is the result of Analysis 1.

Notes on Analysis 1. This analysis is sensitive to the *distribution* of results among the participants. It is also sensitive to deviations from randomness in directions opposite to a participants' intention. No prediction is made on how in particular the individual results would deviate from the expected distribution. However, a one-sided probability is chosen as the main result, under the hypothesis that deviations would more likely show up in the direction of deviations of individual results from their reference class. A probability of this analysis that is close to unity would indicate that the participants' data are closer to the expected distribution than expected by chance.

The total hit rate over all participants, which is the classical type of analysis for this kind of experiment, is not foreseen as a test, but can be considered as post hoc analysis, while explicitly not counting in the final statistical evidence of the study at hand.

Analysis 2. This analysis comprises three correlation tests between three different psychological variables obtained before each run, and the hit rates of individual run results.

The three psychological variables used are:

- The variable *mood*, obtained before each individual run on a scale of 0 to 10.
- The variable *time*, also obtained before each individual run, which is the time the participant needed from starting to choose the mood parameter (by turning the mood dial) to the actual start of the run (by pressing the start button).
- The variable *sequence*, which is a measure of how many runs in the past the direction of the intention (left or right) was chosen to be the same as for the actual run.

For the calculation of the correlations, Spearman's rho is used. The correlations are split between right and left intention, such that there are two correlations calculated for each psychological variable (and for each participant). Each correlation uses the 60 hit rates (as defined in the subsection **Analysis 1** of the ***Pre-Planned Data Analysis*** section) for each run of either left or right intention. The p-values of the two resulting correlation factors pertaining to one psychological variable are multiplied and yield the test result for one correlation test. This procedure is performed for all 20 participants, and the 20 test results are multiplied to yield one combined result for each psychological variable.

Each of the three combined results is then compared to the equivalent test results of a large number of simulated data (again by a ranking). By this comparison, a two-sided (frequentist) probability is estimated for each test, that the acquired result (or a lower/higher one) would have occurred by chance. In a second step, all of these probabilities (one for each statistical test) are combined (by summing over the inverse squares of p-values) to yield a single figure of merit (FOM) of the acquired data. Finally, this FOM is compared to the distribution of the same FOMs of the simulated data, and a one-sided (frequentist) likelihood results, that the actual FOM (or a lower one) of the data under test would have occurred by chance. This likelihood is the result of Analysis 2.

Notes on Analysis 2. While basically a test of 3 correlations, this analysis can also be interpreted as a correlation matrix technique as described for example in von Lucadou (2006). A correlation matrix (as used in these references) shows the number (and strength) of correlations between several physical and psychological variables of the experiment as a whole. In terms of Analysis 2 defined here, there are 3 psychological variables, and one

physical variable, such that this 'matrix' has only three entries. However, one could also argue that three correlations are actually calculated for each participant, which are then combined for all 20 participants. In this sense we have 60 correlations.

Analysis 3. This analysis is a conceptual replication of the correlation matrix technique used by von Lucadou and others.

Psychological variables of each participant have been obtained by questionnaires before the start of the first run of that participant. The questionnaires are summarized into the following categories, to form 6 psychological variables:

- TAS: Tellegen absorption scale with 34 items
- SG: Sheep–Goat scale with 9 items
- SENS: reduced sensitivity person scale with 9 items
- TRANS: reduced transcendental scale with 6 items
- EX: Extraversion scale with 12 items
- MED: Experience with a meditation technique

Five physical variables are formed for each participant, resulting from the 120 runs that each participant conducted:

- HIT: Total hit rate
- ACR: Autocorrelation of the time series data, shifted by 1 and 2 s
- RUN: Runtest of time series data, testing the hypothesis that the data are randomly distributed in time
- EXC: Number of excursions in intended direction
- GNG: Number of audio feedbacks (gongs) obtained

For each psychological variable, the correlation with each physical variable is calculated using Spearman's rho. The resulting 30 values are then compared to the equivalent test results of a large number of simulated data (again by ranking). By this comparison, a two-sided (frequentist) probability is estimated for each test, that the acquired result (or a lower/higher one) would have occurred by chance. In a second step, all of these probabilities (one for each correlation) are combined (by summing over the inverse squares of p-values) to yield a single figure of merit (FOM) of Analysis 3. Finally, this FOM is compared with the distribution of the same FOMs of the simulated data, and a one-sided (frequentist) likelihood results, that the actual FOM (or a lower one) of the data under test would have occurred by chance. This likelihood is the result of Analysis 3.

Notes on Analysis 3. This is the first independent conceptual replication of the *correlation matrix method* (CMM) using multiple participants. A brief explanation of the CMM method can be found in the Appendix section ***Notes on the Correlation Matrix Method***.

A strange anecdotal occurrence: Trickster at play? The author (*I*, for this section) would like to share an anecdotal occurrence here, which happened during the testing of the data-analysis procedures. There are 3 types of analysis defined, as described above. As far as my best memory goes, on all three occasions of *first* testing each analysis (but certainly for 2 of them), the very first statistical outcome for a single test dataset was rather on the edge of the distribution of possible outcomes (of order 1% or lower), which initially raised my concern with the validity of the analysis. However, after applying more than 100 test datasets, the statistics of the outcome resolved to the expected normal distribution, for all 3 analyses, as stated above. For all of these tests, new test data were generated and the system timer value was set as seed number to the pseudo-random algorithm before generating each test dataset.

One other similar instance happened with an auxiliary analysis for Analysis 3 which was testing the counting method of matrix elements above a threshold, rather than using the pre-planned method of combining all matrix elements. When first testing the counting of correlations above a threshold, again with a fresh set of simulated data, on the very first instance this number was found to be 8. According to the test with many hundred simulated datasets afterward, the likelihood for obtaining 8 significant results is about 0.1%. Just to be clear, the generation of the matrix correlation factors was not changed on this occasion, just their evaluation via the threshold method was tested as an auxiliary investigation of the analysis procedure.

Taking at least 2 instances with 1% chance and one with 0.1%, this gives a combined chance of about 10^{-5} using Fisher's method for combining p-values uniform on the interval [0,1] (Fisher 1970). Of course, this was not predicted, and is a spontaneous observation, which, however, I found quite curious and which reminded me of G. Hansen's book *The Trickster and the Paranormal* (Hansen 2001) as well as J. Kennedy's paper "The capricious, actively evasive, unsustainable nature of psi" (Kennedy 2014).

It is obvious that the testing of the pre-defined analysis with a single set of test data can be viewed as a PK-like experiment on its own. The Trickster quality of this occurrence is interesting to contemplate.

Results

Analysis 1

Figure 2 shows the result of Analysis 1. The probability of the participants' results to have occurred by chance (null hypothesis) is $p = 0.438$, which is not significant. This probability is obtained by the fraction of more extreme results (more negative FOM) divided by the number of all results of the

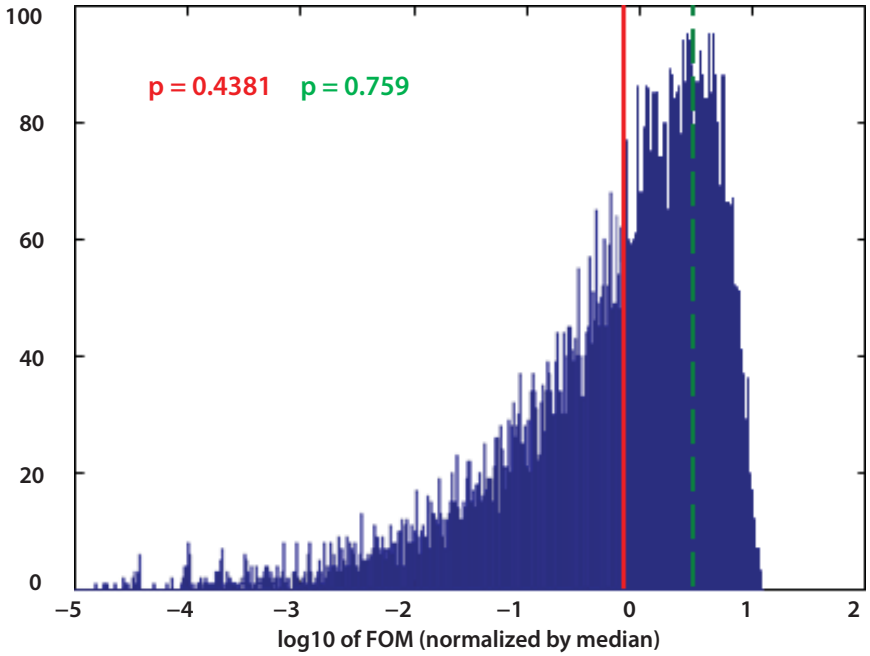

Figure 2. Result of Analysis 1 for the participants' dataset and the control dataset compared with simulated data. The horizontal axis denotes a normalized logarithmic representation of the figure of merit (FOM) as described in the subsection **Analysis 1**. The vertical axis denotes the counts per bin of the simulated dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the control dataset (green/dashed).

simulated data. As implicit in the description of this analysis in the subsection **Analysis 2**, this result means that the distribution of the 20 participants' results (regarding their individual hit rates) does not significantly deviate from the expected distribution under a null hypothesis.

The probability for the result of the control dataset to have occurred by chance (null hypothesis) is $p = 0.759$, and thus also not significant. Table 4 with the individual participant results can be found in the Appendix section ***Individual Participant Results from Analysis 1***.

Analysis 2

Figure 3 shows the results of Analysis 2. The probability for the participants' results to have occurred by chance (null hypothesis) is $p = 0.703$, which is not significant. The probability for the result of the control dataset to have occurred by chance (null hypothesis) is $p = 0.512$, and thus also not significant.

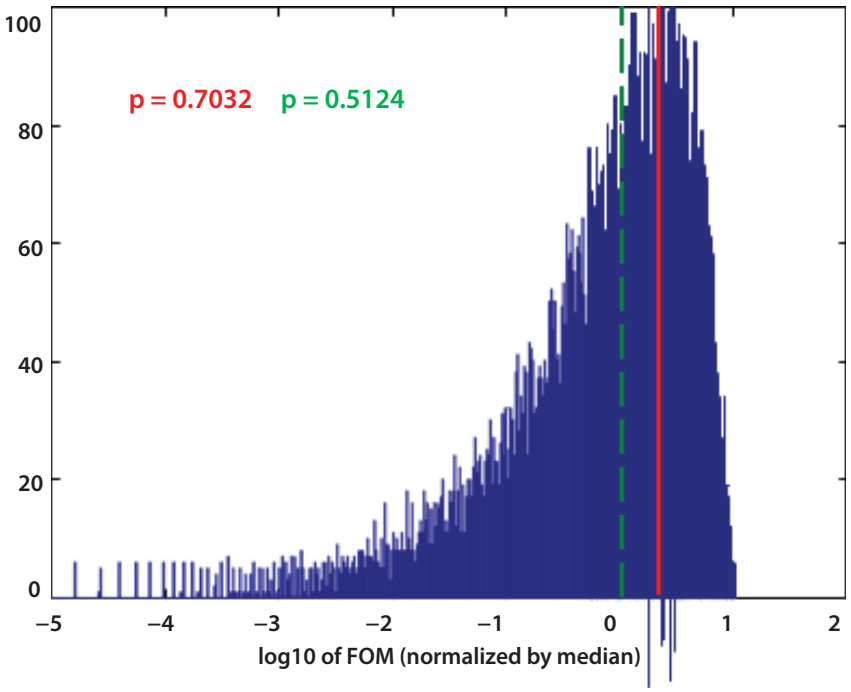

Figure 3. Result of Analysis 2 for the participants' dataset and the control dataset compared to simulated data. The horizontal axis denotes a normalized logarithmic representation of the figure of merit (FOM) as described in the subsection **Analysis 2**. The vertical axis denotes the counts per bin of the simulated dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the control dataset (green/dashed).

Analysis 3

Figure 4 shows the result of Analysis 3. The probability for the participants' result to have occurred by chance (null hypothesis) is $p = 0.0949$, which is not significant using a significance threshold of $p = 0.05$. The probability for the result of the control dataset to have occurred by chance (null hypothesis) is $p = 0.983$, and thus also not significant, given that a one-sided probability had been specified. The observation that the control data are located on the right side of the distribution led to the post hoc analysis described in the next section.

As an additional illustration of the result of Analysis 3, we show here the two correlation matrices for the participants (Table 1) and control data (Table 2), respectively. For the 6 psychological and 5 physical variables as described in the subsection **Analysis 3** in the section ***Pre-Planned Data***

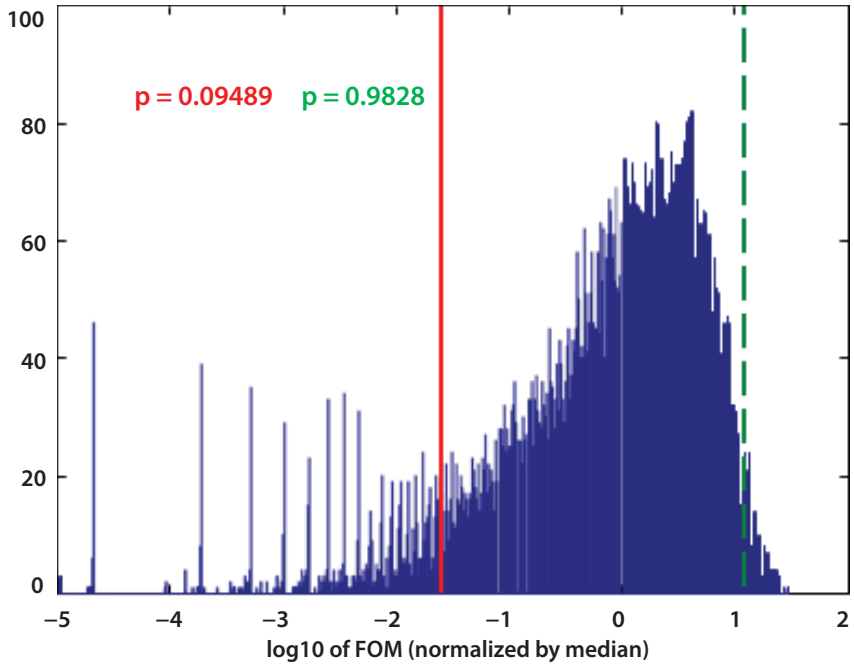

Figure 4. Result of Analysis 3 for the participants' dataset and the control dataset compared with simulated data. The horizontal axis denotes a normalized logarithmic representation of the figure of merit (FOM) as described in the subsection **Analysis 3**. The vertical axis denotes the counts per bin of the simulated dataset, with a total of 10,000 simulated datasets being used. The two vertical lines denote the FOM of the participants' data (red/solid) and the control dataset (green/dashed).

Analysis, we have 30 correlation factors, which are converted to p-values here to be more illustrative.

It can be observed that Table 1 contains two significant correaltions with $p < 0.05$. Another element (EX correlated with HIT) comes close to $p = 0.05$. On the other hand, the matrix for the control data, Table 2, shows no element with $p < 0.16$, which indicates why the control data are on the other side of the distribution of possible results, i.e. showing particularly low correlations between psychological and physical variables.

Post Hoc Analysis

Analysis reported in this section has been performed post hoc and as such does not contribute to the statistical outcome of the pre-planned analysis.

As a post hoc analysis for Analysis 3, one can combine the results for the participants' and the control data and evaluate their combined

TABLE 1
Matrix Arrangement of p-Values for the 30 Correlations of Participant Data

Participant	HIT	ACR	RUN	EXC	GNG
TAS	0.7761	0.9046	0.2075	0.1703	0.8942
SG	0.5045	0.6657	0.7270	0.3037	0.6783
SENS	0.4911	0.9697	0.7347	**0.0032**	0.4220
TRANS	0.8221	0.8719	0.4940	**0.0285**	0.6046
EX	0.0538	0.7237	0.6399	0.2527	0.1794
MED	0.4638	0.2055	0.3934	0.1236	0.8099

p-Values smaller than $p = 0.05$ are shown in bold.

TABLE 2
Matrix Arrangement of p-Values for the 30 Correlations of Control Data

Control	HIT	ACR	RUN	EXC	GNG
TAS	0.3136	0.2461	0.3898	0.1607	0.6942
SG	0.8878	0.6176	0.6335	0.8773	0.3304
SENS	0.9924	0.9899	0.6944	0.8277	0.4205
TRANS	0.8063	0.5807	0.8969	0.4840	0.3003
EX	0.4275	0.2052	0.6907	0.7598	0.8595
MED	0.6755	0.6218	0.9421	0.5488	0.9242

significance, using a one-sided probability. In this case the prediction is that the participants' data are in the direction of high correlations, and the control data in the direction of low correlations (i.e. $p = 1 - 0.983 = 0.017$ for the control data, as pertaining to the right hand side of the distribution).

The combined probability of $p = 0.0949$ and $p = 0.017$ for uniform distributions on [0,1] is $p = 0.012$. However, while this can be called significant, even if this analysis had been pre-specified as Analysis 3, when combined with the results of Analysis 1 and Analysis 2, the combined p-value would still only be $p = 0.082$.

Two types of statistical background estimation. For the pre-defined analysis, 10,000 complete sets of simulated data derived from a Mersenne twister algorithm were used (Matsumoto & Nishimura 1998). This method relies on the assumption that the generation algorithm is sufficiently random

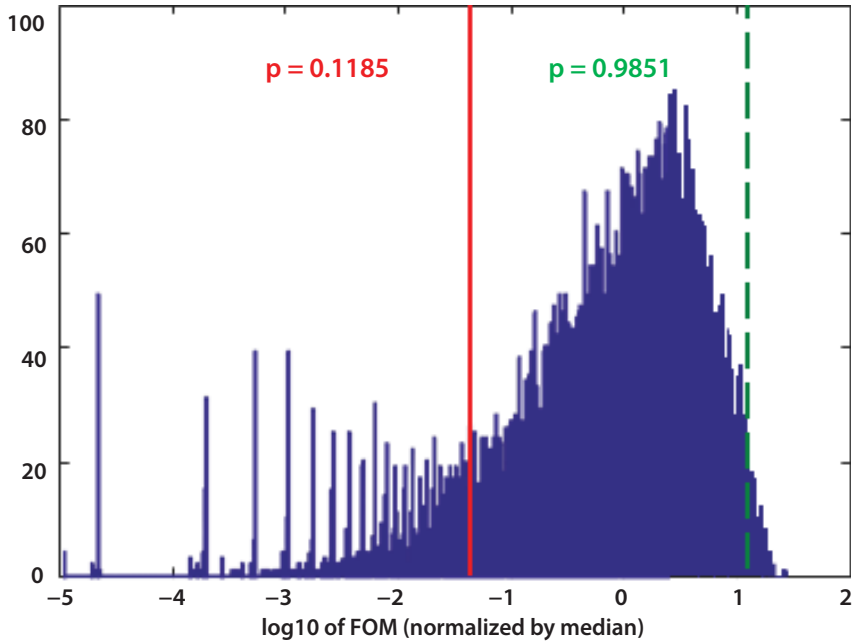

Figure 5. Result of Analysis 3 for the participants' dataset and the control dataset compared with random permutations of participant data. The two vertical lines denote the FOM of the participants' data (red/solid) and the control dataset (green/dashed) for the appropriate permutation. The background distribution is similar to the one derived from simulated data (compare with Figure 4). The combined probability of $p = 0.1185$ and $p = 0.0149$ for uniform distributions on [0,1] is $p = 0.013$.

for the purpose of the study. While data could also be generated with a hardware random number generator, the amount of required data (of order 10^{11} bits to feed the Markov chain) makes this slightly non-trivial, and a sufficiently fast hardware RNG was not at hand. Another way to estimate the background distribution is to use participants' or control data, but use many permutations of these with respect to the psychological data to which they are to be correlated. For Analysis 3 this means that the association of physical data (derived from the output of the RNG) is randomly permutated 10,000 times with respect to the psychological data. This type of background generation has been performed for Analysis 3, using participants' data.

The result of this permutation analysis is shown in Figure 5. The background distribution and the estimated probabilities are similar to the background distribution and probabilities from the simulated data in Figure 4, which corroborates the result derived from simulated data, and vice versa.

Other statistics. It may be interesting to look at the data in this experiment in a more familiar way, at the overall hit rate over all participants.

TABLE 3
Basic Statistics of Experimental Data

	N	$N_{1's}$	$N_{0's}$	z-score
Participant right intention	360,000	179,871	180,129	–0.745
Participant left intention	360,000	179,763	180,237	1.368
Control right intention	360,000	179,934	180,066	–0.381
Control left intention	360,000	179,790	180,210	1.212

N denotes total number of bits for each condition. $N_{1's}$ denotes the number of '1' bits. $N_{0's}$ denotes the number of '0' bits. z-Values have been calculated with Equation (1) and Equation (2).

Table 3 shows basic statistics for the participants' data and control data split by left and right intention. This is the classical way of analyzing data from this type of experiment. No z-score is significant for any of the 4 datasets. It may look slightly surprising that all numbers of $N_{1's}$ and $N_{0's}$ are in the same direction. However, the author would attribute this to chance, since the evaluation of the random event generator yielded no deviation from chance expectation, as described in the Appendix section ***The Binary Random Number Generator***.

Discussion

The result of Analysis 1 does not confirm the hypothesis that the distribution of individual results from the 20 participants would deviate significantly from the expected distribution under a null hypothesis. Even though the number of participants has been smaller by a factor of 2, compared with the study in Grote (2015), it seems that there is no hint of an anomalous distribution.

The results of Analysis 2 can hardly be further commented on. This analysis was exploratory in the sense of the hypotheses put forward. However, the analysis was strictly pre-specified.

The result of Analysis 3 is more interesting. Even though the main outcome is not significant with $p = 0.0949$, it is notable that the control data are located toward the right side of the distribution of the simulated data (see Figure 4). This means that the control data show significantly less correlation (between psychological and physical variables) than to be expected given the simulated data. While this could be interpreted as a

chance fluctuation, it is at least noteworthy that von Lucadou proposed that the control data might be part of the *operational closure of the system*, and thus be part of the experiment as a whole. Von Lucadou and others have used the *difference* between experimental data and control data to estimate the overall significance of experiments (von Lucadou 1986, 2006, Walach et al. 2016), thus including the control data in the analysis. However, Walach et al. (2016) find that their control data mostly conform to expectation values under a null hypothesis. In the study here, the author chose to use only the experimental data in comparison with the simulated data as a result of Analysis 3. This decision was made since it seemed more plausible to this author that an effect (if existent at all) would more likely show up in the main experimental data and not in the control data. Perhaps the fact that the control data of Analysis 3 is significantly shifted toward the low-correlated side of the distribution is yet another Trickster manifestation?

Similar to postulating a Trickster effect would be to speculate on experimenter-psi as a source of the observed result. See Parker and Millar (2014) for a more recent overview of experimenter-psi. It is interesting to note that in Analysis 3, a significant result only can be obtained by correlations across participants. There is no way an individual participant can 'score high' in this type of analysis, since each participant is only evaluated as part of an ensemble of participants. This fact may (or may not) make this type of analysis more prone to experimenter-psi.

We can note that the post hoc analysis using the difference between experimental and control data in Analysis 3 yields a probability to have occurred by chance of $p = 0.012$ under a null hypothesis. However, even when this type of analysis would have been pre-specified for Analysis 3, the combination of Analysis 1, Analysis 2, and Analysis 3 still only would yield a combined p-value of $p = 0.082$.

For future replications of correlation matrix experiments, the number of participants to be employed seems an open question for this author. While the study in Walach et al. (2016) employed about 300 participants, the study here employed 20. However the p-values for both experiments are comparable, of order $p = 0.01$ when looking for the difference between participant data and control data, and using simulated or permutated data to estimate the background. Based on this finding, one may wonder whether the number of participants plays an important role. Perhaps a useful measure for this kind of experiment could be the total interaction time between humans and the machine, which is different for the 2 experiments, but less so than the number of participants: The total interaction time for the CMM experiment in Walach et al. (2016) was 125 h while it was 20 h for the CMM experiment reported here.

Acknowledgments

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Notes

[1] A *Schmidt* process (terminology used by W. von Lucadou) is a process where generated random bits are statistically independent events. In a Markov process, the actual random event has a non-zero statistical dependence on the last internal state of the Markov process. This is further detailed in the Appendix section ***The Binary Random Number Generator***.

[2] The check-sums were generated by the device from the sum of all transmitted bytes modulo 256. The device would transmit this check-sum after a block of data had been transmitted, and the receiving computer compared this check-sum with the one it calculated from the received data.

[3] The author is aware of possible criticism of p-values for some domains of research and hypothesis testing. However, p-values as used in classical (frequentist) statistical analysis still have their merits and reasonable domains of application, as pointed out by an overview article on Bayesian and classical hypothesis testing (Kennedy 2014).

[4] Of course, in principle it may be possible to calculate the likelihood of the employed tests analytically; however, a Monte-Carlo approach was chosen here for simplicity and for better transparency of the data analysis. Further, the Monte-Carlo method makes it straightforward to combine different statistical tests and analyses that may be overlapping. The analytic approach would be exceedingly complex in this case. However, care has to be taken to assure that the random number generator used for the background distribution suffices for the intended usage. For the case here, different algorithms have been compared with no significant differences found in the resulting distributions relevant for this analysis. Another approach is to use the existing dataset with random incursion points (i.e. random permutations of the data) to generate the background distribution. This was performed for Analysis 3 and is described in the subsection **Two types of statistical background estimation**.

[5] See the subsection **A strange anecdotal occurrence: Trickster at play?**, though, for an anecdote about this testing.

[6] Equation 2 is an approximation. However, since simulated data with the same statistic as the experimental data are used to estimate the background, the exact statistic used does not matter. Just counting the number of hits

for each participant would thus yield the same result for this analysis.

[7] If both values are equal, an independent random bit is generated from the hardware random generator to resolve the tie. For consistency, the same Markov algorithm is used to generate the Monte Carlo data for the background distribution.

[8] Upon suggestion of the current author, this type of analysis has been incorporated in the most recent replication of the CMM experiment, as reported in Walach et al. (2016).

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Appendix

The Binary Random Number Generator

The random number generator (RNG) is a hardware RNG. Figure 6 shows a simplified schematic of the RNG components. The hardware RNG is based on the differential thermal noise of two resistors. The difference of the resistors thermal noise voltage is amplified and fed to the input of a comparator, comparing the noise voltage to its time average. This yields a random sequence of logic high and low levels at the output of the comparator with close to equal distribution, but which is still sensitive, for example, to offset voltage drifts of the involved amplifiers, etc. Therefore, in order to better equalize the distribution of the data, the bit stream is fed to a frequency divider which toggles its logical output on the transitions from high to low of the comparator output. This corresponds to a frequency division by a factor of two, and is a technique to equalize over time the high- to low-level ratio of a binary signal. On average, the divider registers 65 high-to-low transitions of the comparator per millisecond, corresponding to an average count frequency of 65 kHz.

This stream of randomly alternating logic high/low levels is fed to a microcontroller that controls the whole experiment. Within the microcontroller, the random bit stream from the hardware generator is sampled at a frequency of 200 Hz and fed to a 16-bit long shift register at this frequency, such that every 5 ms a new random bit is fed into the shift register.

To generate one random bit (we call this bit *b*) for the main experiment (i.e. a bit to be 'influenced' according to the participants' intentions), the software

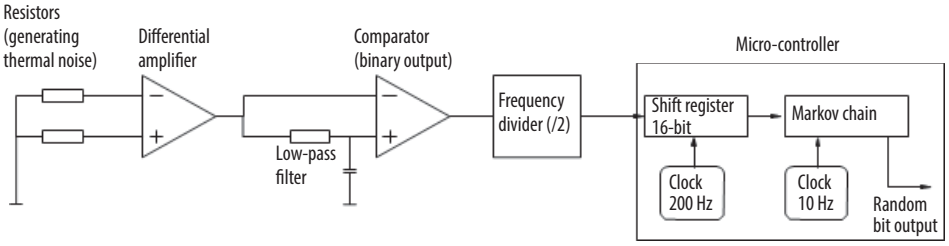

Figure 6. Schematic of the binary random number generator. See text for description.

of the microcontroller performs the following operations:

- The shift register is read out obtaining a 16-bit long word (we call it A). This word A is formed by the 16 random bits that have been fed into the shift register during the last 16 ∗ 5 ms = 80 ms. Since the individual 16 bits of A are random, A has a uniform distribution on the interval of integer numbers [0.65535].
- Word A is then compared to the 16-bit word that had been obtained in the previous sampling of the shift register (we call it $A-1$). If the actual word is larger than the previous one ($A > A-1$), a logical 1 is the output bit, such that $b = 1$. If it is smaller ($A < A-1$), the output bit is a logical 0, respectively, such that $b = 0$.[7] This procedure constitutes a 1-step Markov chain.
- In the last step, the value of word A is assigned to word $A-1$ to be used in the next iteration of these steps.

This procedure is executed 10 times per second, and thus, for the purpose of the main experiment, random bits b are generated with a rate of 10 Hz.

In the following, the bits "1" will be referred to as the "high bits" whereas the "0" bits will be referred to as the "low bits". A test run of this RNG comprising $N = 57{,}565{,}280$ (57 million) bits yielded $n_h = 324 + N/2$ high bits, corresponding to 50.0000056% of the cases. The corresponding z-value is $z = 0.148$, as calculated with Equations (1) and (2) above in the **Analysis 1** subsection of the ***Pre-Planned Data Analysis*** section.

The functioning of the hardware RNG was monitored automatically throughout the experiment. This monitoring was done by counting the number of high to low transitions of the random noise generator for each second, and requiring that a threshold number of transitions was passed. No error on the hardware RNG occurred during the regular experimental time of the participants.

Notes on the Correlation Matrix Method

A correlation matrix, as introduced by von Lucadou (1986), is simply the arrangement of all calculated correlation factors (or their respective p-values) in the form of a matrix, for the purpose of illustration. However, there are two questions arising about how to evaluate the matrix elements (i.e. the correlation factors) with respect to their combined statistical significance.

First, we need a method of how to combine the matrix elements into one figure of merit or combined statistic. The chosen method here, for the 30 correlation results, takes each correlation factor into account, forming one quantitative outcome of all matrix elements combined, as described above. In contrast, the method used by Von Lucadou uses only those correlation factors that are above a threshold value, and counts their number of occurrences as the combined statistic. Both methods are similar in principle, but here the first method was chosen on the hypothesis it would be more suitable for a small number of total correlations, and may also be more sensitive altogether, since no matrix elements are omitted from the analysis.

Secondly, after we have established a combined figure of merit of all matrix elements, we need to assess the statistical significance of this figure of merit (the participants' result) against an expectation value or against the control data. Due to the fact that at least the psychological variables, but perhaps also the physical variables, can be expected to correlate among each other, a comparison of the participants' data with a large set of simulated (Monte Carlo) data (i.e. the *correlations* of the simulated data with the participants' psychological data), or with a set of random permutations among psychological and physical data, seems the only way to establish a valid background distribution for this kind of analysis.[8]

Individual Participant Results from Analysis 1

TABLE 4
z-Scores of the 20 Participants for Analysis 1, Ranked by z-Score Value

This score (as defined in the subsection **Analysis 1**) is a measure of how well the participants succeeded in 'influencing' the galvanometer needle in the desired direction. Also shown are the expectation values for the z-scores.

Rank	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
z-Score	1.80	1.55	1.53	1.35	1.26	0.95	0.79	0.59	0.35	0.16
Exp.value	1.87	1.41	1.14	0.92	0.75	0.59	0.45	0.31	0.19	0.06
Rank	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**	**20**
z-Score	0.16	0.16	–0.02	–0.24	–0.27	–0.44	–1.24	–1.59	–1.95	–2.94
Exp.value	–0.06	–0.19	–0.31	–0.45	–0.59	–0.75	–0.92	–1.14	–1.41	–1.87

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HISTORICAL PERSPECTIVE

Telepathy, Mediumship, and Psychology: Psychical Research at the International Congresses of Psychology, 1889–1905

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Abstract—The development of psychology includes the rejection of concepts and movements some groups consider undesirable, such as psychical research. One such example was the way psychologists dealt with phenomena such as telepathy and mediumship in the first five international congresses of psychology held between 1889 and 1905. This included papers about telepathy and mediumship by individuals such as Gabriel Delanne, Léon Denis, Théodore Flournoy, Paul Joire, Léon Marillier, Frederic W. H. Myers, Julian Ochorowicz, Charles Richet, Eleanor M. Sidgwick, and Henry Sidgwick. These topics were eventually rejected from the congresses, and provide us with an example of the boundary-work psychologists were engaging in during that period to build their discipline. The height of such presentations took place at the 1900 congress, after which there was a marked decline in discussion on the topic which mirrored the rejection science at large showed for psychical research during the period in question.

Introduction

The purpose of this essay is to review the inclusion of psychical research in international congresses of psychology held from 1889 to 1905 as it appears in published conference proceedings. My aim is to give readers an idea of the topics presented at the time. This is particularly important because previous writings about psychic phenomena in the congresses have not given much attention to the content of the actual discussions about psychic phenomena (e.g., Benjamin & Baker 2012, Rosenzweig, Holtzman, Sabourin, & Bélanger 2000, Taves 2014). Furthermore, I will also comment about controversies, as discussed in the proceedings of the fourth conference, and about the eventual rejection of the topic from the congresses. The latter is related to the demarcation problem in discussions of science and so-called non-science or pseudoscience (Pigliucci & Boudry 2013) and to boundary work (Gieryn 1999).

Rejecting the Psychic

The story told in these congresses, particularly the 1900 meeting, is part of the rejection by the establishment of phenomena referred to in different times as spiritualistic, psychic, and supernormal. Bertrand Méheust (1999a) has argued that the French medical community stripped hypnosis of phenomena such as thought transmission without sensory means, assimilating in its canon only those features considered respectable. In his view, while "positivist savants reappropriated somnambulism, the mysterious phenomena described by the magnetizers after a century seemed to dissipate like a mirage" (Méheust 1999a:584; this and other translations are mine). Such tendency for psychology, medicine, and other fields to explain through conventional means, or to outright reject psychic phenomena, has been examined by others. Examples of this include examinations of the rejection of psychical research related to institutions (Dommeyer 1975), specific individuals (Le Maléfan 2002), research programs (Mauskopf & McVaugh 1980), and specific investigations (Parot 1993).[1]

The eventual rejection of psychical research from the international congresses of psychology is an example of the field's rejection and ambivalent position within psychology (on these issues see Alvarado 2014, Coon 1992, Marmin 2001, Sommer 2012, 2013). Psychologists' attempts at professionalization led them to separate themselves from other knowledge claims and perspectives that they felt threatened their status. They engaged in boundary-work, where there is an active defense of practice, methods, and concepts "for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science" (Gieryn 1999:4–5). Nineteenth-century psychologists, as argued by Burnham (1987:91) in the American context, were establishing themselves as scientists by combatting popular and spiritual claims about the mind. This also applied to psychical research work, as psychologists, who were struggling to get their discipline accepted in academia, felt its scientific status threatened by attention to psychic phenomena (Coon 1992, Sommer 2012, 2013, Taves 2014).

The eventual disappearance of psychical research from the psychology congresses represented what has been characterized as the "expulsion of intruders" (Paicheler 1992:248), as well as the separation "between the acceptable and the unacceptable in psychology" (Le Maléfan 1995:624). Such expulsion took place mainly in the 1900 congress (Taves 2014).

The Background to the Congresses and Psychical Research

The appearance of the international congresses during the late nineteenth century was part of the professionalization of psychology as seen in the

creation of various institutions and journals, and the development of many specialties in the field. By the beginning of the 1880s, a writer was able to list specialties such as criminal, general, mathematical, pathological, pedagogical, physiological, psychophysical, and zoological psychology (Ochorowicz 1881).

There was also a variety of investigative practices that defined psychology in different ways (e.g., Carroy & Plas 1996, Danzinger 1990). Laboratory studies of psychophysical processes, particularly important in Germany, were one of the main developments of the times (Ribot 1879). Another important current was the clinical specialty. This included French studies of hypnosis and its phenomena that were one of the main topics of the early congresses (Alvarado 2010). Examples of work in this area were the publications of Jean-Martin Charcot (1825–1893), Charles Richet (1850–1935) (Charcot 1882, Richet 1883), and several other scholars (e.g., Bernheim 1884, Gilles de la Tourette 1887).[2] This, and phenomena such as amnesia, somnambulism, double and multiple personality, and mental mediumship, contributed greatly to the development of ideas about the subconscious mind (for overviews, see Crabtree 1993 and Ellenberger 1970).

Julian Ochorowicz

These, and other developments, were to some extent behind the organization of the congresses.[3] Before the first congress, philosopher and psychologist Julian Ochorowicz (1850–1917; see photo) had suggested that the organization of psychology could be greatly assisted by a congress of psychology following the model of congresses from other disciplines (Ochorowicz 1881).[4] This first congress took place in Paris in 1889, the year of the universal exposition that featured the Eiffel Tower (*Les Merveilles de l'Exposition de 1889* no date). Furthermore, this year saw in France important developments related to psychic phenomena and to the idea of the subconscious mind, as seen in the field of hypnosis (e.g., Janet 1889, Liébeault 1889). But such developments in the study of subconscious activity were not limited to France (e.g., Dessoir 1889, James 1889b).

Another development of particular importance to the topic of this paper was the study of psychic or supernormal phenomena that came to be called in England, and other countries, "psychical research." Influenced by mesmeric phenomena such as magnetic healing and clairvoyance, a long tradition of tales about apparitions and haunted houses, and phenomena from Spiritualism such as mediumistic communications and materializations of

spirit forms, organized psychical research developed during the latter part of the nineteenth century. By this time the still new field had gone through important developments in many countries, such as the founding of the London-based Society for Psychical Research (SPR).[5]

The work of the SPR was particularly important in setting new evidential standards and a more systematic approach to the study of cases (such as apparitions), mediums, and the performance of experimental studies. A main focus of the SPR was telepathy, which included experiments (e.g., Sidgwick, Sidgwick, & Smith 1889) and case studies. The best known early example of the latter was *Phantasms of the Living* (Gurney, Myers, & Podmore 1886), of which the main thesis was that telepathic messages could be expressed through hallucinations representing different sensory modalities.

Another important aspect of the SPR was the influential work of classical scholar Frederic W. H. Myers (1843–1901), who by 1889 had published various papers on his ideas about the subliminal mind and the concept of motor and sensory automatisms as the means through which the subliminal could communicate with the supraliminal, or conscious mind (Myers 1884, 1885, 1887, 1889a). Such messages, which included flashes of creativity and telepathy, could "float up into superficial consciousness as deeds, visions, words, ready-made and full-blown, without any accompanying perception of the elaborative process which has made them what they are" (Myers 1889a:524; see photo).[6]

F. W. H. Myers

Many psychical researchers challenged the current scientific paradigm that assumed that sensory and motor functioning was confined to the workings of the human body. One writer stated about telepathy that "there is hardly any longer room for doubt that we have something here which no physical process at present known can adequately account for" (Podmore 1894:382).

If this was not enough, many psychical researchers were also interested in the ultimate challenge to the physicalistic paradigm, the question of survival of death. While some presented overviews of different types of phenomena and arguments supporting spirit agency (e.g., Aksakof 1890/no date), others focused on specific phenomena. For example, the SPR published studies of mental mediums (e.g., Lodge 1890), as well as systematic discussions of cases of apparitions of the dead (e.g., Myers 1889b).

In France, where the first congress took place, there was much interest

in the topic (Lachapelle 2011, Plas 2000). An internationally influential publication was Richet's (1884) pioneering study of mental suggestion, in which he analyzed his results statistically, and explored the effectiveness of a variety of targets and the use of motor automatisms as a vehicle of expression of the hypothesized mental transmission.[7] In 1885 a group of scholars founded the Sociétè de Psychologie Physiologique. Presided over by Charcot, and having Richet as Secretary, the Society included psychical research among its interests (Plas 2000:54–55). In addition, several members of this society—such as Pierre Janet (1859–1947), Ochorowicz, and Richet—were involved in the first psychology congress.

The activities of the Société were indicative of the interaction between psychology and psychical research during the nineteenth century, as were the studies of men such as Janet, Richet, and others who conducted both psychological and psychical research work (Plas 2000). Early lists of members of the SPR included eminent psychologists, philosophers, and physicians interested in different aspects of the mind and its manifestations (List of Members and Associates 1889). The fact that the SPR had contacts with all of these influential scientists and scholars showed that the Society was well connected to psychology, but this does not change the fact that the SPR, and psychical research at large, was not an established part of psychology.

While the contributions of the SPR about dissociation and the workings of the subconscious mind (Alvarado 2002) were welcome by many, probably most did not accept the rest of their work. Myers was cited by well-known psychologists (e.g., Binet 1892:299, Janet 1889:392, 394, 403), but his influence was limited to the psychology of automatic writing and the subconscious mind, and not to phenomena such as telepathy. For these authors, and for psychology at large, there was a difference between Myers as a psychologist and as a psychical researcher (or his emphasis on telepathy, veridical apparitions, and mediumistic communications). Myers, of course, was aware of this. In a unpublished letter he wrote to Richet, he stated that he was conscious of his own "psychological heterodoxy" (Myers 1891).

Psychical research had many enemies among psychologists. A prominent example was psychologist Joseph Jastrow (1863–1944), who stated that the study of psychic phenomena "has . . . contributed an interesting chapter to the natural history of error . . . " (Jastrow 1889:81).

Jastrow questioned the training of psychical researchers to conduct their work (see also Scripture 1897). Others referred to improper methodology or conventional explanations to justify doubts about the evidence for telepathy (e.g., Hall 1887, Titchener 1898).

Other phenomena were explained via conventional concepts. Several authors had psychological views about mediumship based on dissociation and subconscious activity (e.g., Binet 1892, Janet 1889). This literature, as well as that which pathologized the figure of the medium (Le Maléfan 1999), did much to eclipse the views of veridical mediumship supported by many psychical researchers.

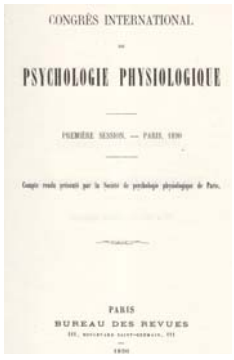
While I have emphasized the negative, neglecting the supportive comments and work of many others (e.g., James 1896, Ochorowicz 1887), the fact is that by the time the congresses started, psychical research was at best a controversial discipline far from being accepted as part of psychology by many of its professionals.

The International Congresses of Psychology

The 1889 Congress

The first congress was held in Paris August 6–10, 1889 (Congrès International de Psychologie Physiologique 1890; see photo).[8] Called originally Congrès International de Psychologie Physiologique (International Congress of Physiological Psychology), its title was changed during the congress to International Congress of Experimental Psychology. Nonetheless, the published proceedings kept the original name. While Charcot was the president, he did not attend the congress and his place was taken by Théodule Ribot (1839–1916). Richet acted as Secretary.

While many topics were discussed, some of the main ones were heredity, muscular sense, hallucinations, and hypnotism. Psychical research was discussed in sessions about the last two topics. As William James (1842–1910) wrote in his short report about the congress in the journal *Mind*: "The most striking feature of the discussions was, perhaps, their tendency to slope off to some or other of those shady horizons with which the name of 'psychic research' is now associated" (James 1889a:615). In this context, it is interesting to notice that several of the congress' attendees wrote about psychic phenomena in a positive way, either before or after the congress. Among them were Alexandre Baréty (1844–1918), Henri Bourru (1840–1914), Prosper Burot (1849–1888), Charles Richet, and Albert de Rochas (1837–1914), all from France, and William James (United States), Frederic W. H. Myers (England), Julian Ochorowicz (Poland), and Albert von Schrenck-Notzing (1862–1929, Germany).


CONGRÈS INTERNATIONAL

DE

PSYCHOLOGIE PHYSIOLOGIQUE

PREMIÈRE SESSION. — PARIS, 1889

Compte rendu présenté par le Secrétaire du psychologie physiologique de Paris.

PARIS

BUREAU DES REVUES

1890

Cover of Proceedings of First Congress 1889

Psychical research seems to have entered the congress thanks to Richet, who, at the time, was eminent both in physiological as well as in psychical research. He was a member of the above-mentioned Société de Psychologie Physiologique involved in the planning of the meeting. Henry Sidgwick (1838–1900), a well-known English philosopher who was President of the SPR, had the following entry in his diary for March 25, 1892:

> Prof. Richet, our friend and colleague in S.P.R. matters, got up a 'Congress of Physiological Psychology' in Paris and asked us to come to it. We came out of simple friendship; but when we arrived we found that the ingenious Richet designed to bring the SPR to glory at this Congress. And this, to some degree, came about.[9] (Sidgwick & Sidgwick 1906:515)

As seen in the proceedings, French researcher Léon Marillier (1862–1901), one of the congresses secretaries, announced a survey of hallucinations that the SPR was conducting (Marillier 1890b).[10] Marillier also discussed the subject in a different presentation. The data collected by the SPR, he said, "seem to establish that frequent coincidences exist between hallucinations and real facts" (Marillier 1890c:44).

One of the best aspects of this, and other proceedings, is the presentation of discussions between the attendees. An example is a session about the SPR's work on hallucinations. Richet stated that some members of the congress wanted to discuss telepathy, to which Janet suggested that Myers could speak about it. Myers spoke and summarized the SPR's thought-transference experiments. He stated his belief that there was good evidence for the existence of the phenomenon, while recognizing that it could not be produced at will. "If such mental transmission is true," Richet stated, "it will constitute . . . one of the greatest discoveries of the times" (Statistique des Hallucinations 1890:153).

However, and indicative of a general incredulity about such phenomena, Marillier (1889) stated in a report of the conference that members of the Congress had not yet reached the point to "allow for the formation of a definitive opinion" on the subject (p. 545). He further said that the officers of the congress decided to have "an international committee, charged with comparing the results of investigations made in various countries and to prepare a report for the next congress. Such commission is composed of . . . Sidgwick, Grote, W. James, von Schrenck-Notzing, and Marillier" (p. 544).

Marillier (1890a) was concerned with the control of sensory cues in experimental explorations of telepathy. He was particularly worried about preventing the possibility that experimenters could give "any sign that the subject may interpret, consciously or unconsciously" (p. 17).

In the discussion of another section in the congress about hypnosis,

Myers continued to present summaries of SPR work. This time he discussed Edmund Gurney's (1847–1888) experiments offering evidence for the existence of a mesmeric emanation capable of causing sensations on the hands of human subjects (e.g., Gurney 1884).[11] However, as seen in the proceedings, the implications of these studies to support the existence of physical effluvia met with skepticism. French physician Gilbert Ballet (1853–1916) and Belgian philosopher Joseph Delboeuf (1831–1896) prefered to interpret the results as the influence of heat from the mesmerizer's hands (De la Sensibilité Hypnotique 1890). Their reactions represent the skeptical tradition prevalent at the time about the existence of animal magnetism as a force projecting from the body of the magnetizer (Alvarado 2009b).

The 1892 Congress

This congress was held in London and it had two leading SPR members as important officers (International Congress of Experimental Psychology 1892a). Sidgwick was the president of both the SPR and the congress, while Myers was the congress' Secretary.

There is evidence that Sidgwick was somewhat worried about his and the SPR's involvement in the congress. Before the congress, he wrote to a friend that he was expecting to "have the delicate and difficult task of persuading the orthodox psychologists to regard 'Psychical Research' as a legitimate branch of experimental psychology!" (Sidgwick & Sidgwick 1906:513). Furthermore, he wrote in his diary:

> Behold me, then, President-elect of a Congress of experimental Psychologists—most of them stubborn materialists, interested solely in psychophysical experiments on the senses; whereas *I* have never experimented except in telepathy. Water and fire, oil and vinegar, are feeble to express our antagonism! (Sidgwick & Sidgwick 1906:516)

Sidgwick's strategy was to recruit psychologist James Sully (1843–1923) to manage the congress on the psychology side, while he, with Myers, would "provide the extraordinary element" (Sidgwick & Sidgwick 1906:516).[12]

In a summary of the meeting written for the SPR, H. Sidgwick (1892a) stated that "the representatives of our Society have claimed a place for their special investigations, as a recognized department of the scientific study of psychology, and have their claim admitted without opposition" (p. 284). Nonetheless, Sidgwick was careful to state that he did not think that telepathy was generally accepted by psychologists. Furthermore, he expressed anxiety at the possibility that anyone could believe that people attending the congress were "even in the most indirect way committed to

a view in favour of the conclusions which the workers of our Society have put forward" (p. 284).

In fact, German psychologist Wilhelm Wundt (1832–1920) expressed worries about Sidgwick's influence. Wundt (1892/2000:24) believed that under the disguise of statistics of hallucinations, clairvoyance would probably be the main topic at the congress. H. Sidgwick (1892b) answered Wundt in his opening address at the congress stating that Wundt was "rather wide of the mark" in giving opinions about "matters on which he is determined to seek no information" (p. 2). In fact, Sidgwick stated that he was interested in offering a balanced program, one representative of psychology at large (see also H. Sidgwick 1892a). But the incident is indicative of the worries one eminent psychologist had in having SPR influences at the congress.[13]

During the course of the first day of the meeting, Richet (1892) read a paper about the future of psychology. He identified several promising areas. The first three were brain physiology, the study of sensation, and the relationship of man to other beings, to the insane, and to criminals. The final area chosen by Richet was what he decided to call "transcendental psychology," or psychical research. This involved the supposition that "human intelligence has extraordinary resources" (p. 25) such as clairvoyance and thought-transference. Richet expressed his hope that future studies would show if this area was either a reality or an illusion.

Like the previous congresses, this one had discussions about the study of hallucinations. Henry Sidgwick (1892c) informed the congress attendees that out of 17,000 answers, 1, 272 replied affirmatively to the basic question about hallucinations. He mentioned the existence of collective hallucinations and recognized the possibility that some of them could have taken place due to verbal suggestions. But he believed there were "other cases in which no transference of ideas appears possible except one that takes place otherwise than through the ordinary channels of sense" (p. 61). Such cases suggested telepathy. Sidgwick further wrote:

> This hypothesis is, in the view of the Committee, supported by the results of the present collection. The . . . most important part of . . . [the evidence] consists in cases of human apparitions, coinciding with the death of the person whom they represent, under circumstances which exclude the supposition that they were due to anxiety or any similar emotion of the percipient. (Sidgwick 1892c:61)

Marillier (1892) reported on the survey of hallucinations conducted in France, as well as in Belgium and Switzerland, and some other countries. Out of 54 veridical hallucinations, 35 were reported first-hand and 19 were second-hand. But it was not possible to obtain independent confirmation of

the testimony. Marillier was not as positive of his data about the telepathic hypothesis as Sidgwick was of his.

Another presentation appearing in the proceedings was a short note about the hallucinations collected in the United States, in which out of 6,311 answers, 13.5% were positive (The Statistical Inquiry into Hallucinations in America 1892). Furthermore, there were interesting discussions on the topic. American psychologist Christine Ladd-Franklin (1847–1930)[14] stated: "That if the hallucinatory apparition was of a person known to be ill—even if the knowledge were not accompanied by anxiety—the chances against the coincidence of hallucination and death would be very much reduced" (Discussion Remarks 1892:68). Henry Sidgwick replied:

> That no doubt the chances would be somewhat reduced in this case: since, if the percipient's state of health at the time were such as to cause a hallucination, it would perhaps be more likely to take the form of a friend known to be ill than of one known to be well. But mere knowledge without anxiety could not be regarded as a *vera causa* of hallucinations: therefore, if—as was most frequently the case in his collection—the hallucination was the percipient's only experience of the kind, the chances would still be very much against its coinciding accidently with the death of a friend. (Discussion Remarks 1892:69)

During the congress, the issue of the possible pathology of hallucinations was discussed (Discussion Remarks 1892:67), a topic emphasized by Janet (1892:615) in a conference report. But telepathic hallucinations were not included in the discussion.

The concept of telepathy received further discussion in the congress in a paper by Myers (1892a) about sensory automatisms. Finally, Eleanor Sidgwick (1845–1936), an important early SPR researcher and wife of Henry Sidgwick, presented a report of SPR thought-transference experiments under hypnosis (Mrs. H. Sidgwick 1892).[15]

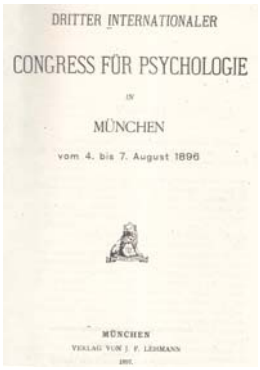
The 1896 Congress

The third congress was held in Munich under the presidency of Carl Stumpf (1848–1936), with Schrenck-Notzing as General Secretary (Dritter Internationaler Congress für Psychologie 1897).[16] The papers in the proceedings were grouped under topics, among them the psychology of normal individuals, physiological topics, comparative and educational psychology.

Although the official report of the SPR's study of hallucinations had already been published (Sidgwick et al. 1894), there were more discussions about it. Representing the SPR, Eleanor M. Sidgwick discussed the issue of chance to account for hallucinations coinciding with a distant death. After

discussing statistical issues, she stated (as recorded in the proceedings; see photo of cover):

> The number of coincidences cannot . . . be accounted for by chance; and, as in about a third of the cases, the percipient was unaware of the illness, and in another third was in no anxiety, we conclude that they cannot be accounted for by the mind of the percipient being specially occupied with the agent. Even in the cases where there was or may have been anxiety, the duration of the anxiety compared with the shortness of the interval between the hallucinations and the death makes it impossible to attribute the coincidences as a whole to anxiety only, though anxiety is to some extent a favouring condition for hallucination. We conclude, therefore, that the statistical inquiry supports the hypothesis of telepathy . . . . (Mrs. H. Sidgwick 1897:391–392)


DRITTER INTERNATIONALER
CONGRESS FÜR PSYCHOLOGIE
IN
MÜNCHEN
vom 4. bis 7. August 1896
MÜNCHEN

Cover of Proceedings of Third Congress 1896

In the discussion that followed, Mrs. Sidgwick read a letter sent by William James about the American statistics on hallucinations. James stated that "apparitions on the day of death are, according to our statistics, 487 times more numerous than pure chance ought to make them" (James 1897:393). However, regardless of this, there were 12 cases of veridical hallucinations, and only 5 of these had corroboration. James added that: "The veridical cases are not strong . . . Only five have any corroboration, and in no case it is first rate. Our best cases are not among these 12" (p. 394).

In critical comments printed in the proceedings, one author argued that the statistics were not convincing (Bager-Sjögren 1897), and that associations of thought could account for the cases, an objection presented by Edmund Parish (Discussion Remarks 1897:402–403).[17] Richet was of the opinion that the issue was not only about statistical analysis, but also about the details of the witnesses testimony. In his opinion both aspects corroborated each other and, together, suggested the existence of veridical hallucinations (Discussion Remarks 1897:402).

Finally, the proceedings includes discussions about unconscious whispering as an explanation of thought-transference experiments (H. Sidgwick 1897a), and about the subconscious imagination of mediums (Flournoy 1897), both of which were published in greater detail in places other than the conference proceedings (Flournoy 1899, Sidgwick 1897b). There is also a summary of a paper about mental suggestion by hypnosis pioneer Ambroise-Auguste Liébeault (1823–1904) that was not presented because he did not attend the congress (Liébeault 1897).[18]

Years later, it was revealed that there had been some inside opposition to psychic phenomena in the congress. Stumpf (1930), the congress' president, stated in an autobiographical essay: "I endeavored to prevent hypnotic and occult phenomena from occupying the foreground, as had been the case in former sessions" (p. 404).[19]

The 1900 Congress

This congress took place in Paris, and had more papers about psychical research topics than previous ones (Janet 1901). In the words of a commentator: "Psychical research was thoroughly ventilated at the Congress" (Woodworth 1900:606).

Ribot (see photo) was the President, Richet the Vice President, and Janet the General Secretary and the proceedings' editor.[20] As in previous meetings, the program reflected many areas of psychology, among them anatomical and physiological studies, and pathology. In addition, that year's program included reports on cases that interested both psychologists and psychical researchers. These were cases of a Spanish three-year-old child prodigy specializing in playing the piano and composing (Richet 1901), of a mathematical prodigy (Bryan 1901), and of multiple personality (Prince 1901).[21]

Théodule Ribot

Ribot's presidential address, while addressing the whole range of psychological specialties such as physiological studies, referred to the founding of a new organization which had in its program the "phenomena that the London Society [the SPR] proposes to call 'super-normal,'—a more appropriate term than supernatural,—that are the advanced, adventurous parties of experimental psychology, but not the less enticing" (Ribot 1901:46). This organization, the Institut Psychologique International (later named Institut Générale Psychologique), was also mentioned by others during the congress (Flournoy 1901, Ochorowicz 1901). While the history of this group remains to be written, its work brought together many prominent individuals to develop a psychological institution that paid attention to the supernormal. However, as time went by the supernormal became less frequent in the bulletin of the institute.[22]

Several interesting papers related to psychical research were presented in a session of the congress called "Studies About the Phenomena of Somnambulism." Swiss psychologist Théodore Flournoy (1854–1920) presented a summary of his work with medium Hélène Smith, with

emphasis on the production of a written Martian script.[23] He discussed the case to illustrate the existence of "subliminal imagination," or the creative potential of the subconscious mind, and its importance to psychology.

Other studies focused on English medium Rosalie Thompson (born 1868), who, according to an observer, "was present at the meetings, and certainly did not give one the impression of anything abnormal or uncanny" (Woodworth 1900:606). Dutch researcher and psychotherapist Frederik van Eeden (1860–1932, see photo) discussed many veridical communications obtained with this medium (van Eeden 1901). Mrs. Thompson was the subject of other papers appearing in the proceedings (Myers 1901, Verrall 1901), but according to Myers (1900) the last two papers were not read at the congress due to lack of time.[24] A conference attendee later expressed skepticism about the veridical material obtained with Thompson. In his view, it was "impossible to follow M. van Eden [sic] in his extraordinary explanation" (Vaschide 1900:801).

Frederick van Eeden

Myers (1901) stated that he had "good reason for ascribing many of these messages to definite surviving personalities" (p. 120). However, he was aware that his claim was controversial in the context of the congress. As he wrote: "These ideas are far removed from ordinary scientific experience. It may still seem, I fear, almost impertinent to offer them for the consideration of a Congress of *savants*" (p. 120).

The rest of the papers relevant to psychical research appeared as part of a section of the congress entitled "Psychology of Hypnotism, of Suggestion, and Related Matters." Hippolyte Bernheim (1840–1919), professor at the Faculty of Medicine at Nancy, and one of the main figures on the French hypnosis scene, was the President of the section.[25] Table 1 includes the topics of several of the papers I will not comment in the text.

In one of the papers in this section, physician and hypnotist Paul Joire (born 1856) argued that psychic phenomena, such as exteriorization of force from the body, had not been sufficiently investigated (Joire 1901b).[26] In his view there were three reasons supporting the existence of psychic phenomena. These were the fact that they have been recorded in different time periods, that there were recent observations in their support, and that there were studies by scientists on the subject.

In addition to forces believed to be projected from the human body, Joire (1901a) defended the idea that magnets could exert actions on human bodies by means "different from all suggestion" (p. 619). Similarly, Hippolyte Baraduc (1850–1909) discussed some of his ideas

TABLE 1
Additional Papers Related to Psychical Research in the 1900 Paris Congress

Reference	Topic
Dariex (1901)	Movement of objects without contact with a medium
Encausse (1901a)	Instruments for the study of mediums
Encausse (1901b)	Transfer of sensations and motor phenomena from one side of the body to the other using magnets
Ferrari (1901)	Thought-transference performances explained as the interpretation of muscular movements
Gibier (1901)	Mediumistic materialization of phenomena
Pascal (1901)	Astral body as vehicle of consciousness
Purdon (1901)	Transference of pulse pattern from one person to another in close proximity
Stannard (1901)	Evidence for survival of death

about emanating "vibrations" or forces from the human body that acted differently if they came from the right or from the left side of the body (Baraduc 1901). Such presentations led a commentator to say in a conference report that: "Baraduc and others expounded queer ideas and demonstrated queerer-seeming facts relating to 'psychic exteriorisation,' etc." (Woodworth 1900:606).

French spiritists were represented at the congress by two leaders of the movement, Gabriel Delanne (1857–1926) and Léon Denis (1846–1927).[27] Both presented papers in which they used the expression "experimental psychology" in their titles. Delanne (1901) argued for the expansion of experimental psychology to phenomena such as telepathy that showed the existence of "extra-corporeal manifestations of man" (p. 610). He cited the work of psychical researchers as evidence that had established that thought could be exteriorized from mind to mind without the use of the senses. In addition, Delanne believed that studies of Italian medium Eusapia Palladino (1854–1918) proved the existence of materialized forms. He referred in particular to tests in which imprints of faces and hands were obtained when the medium's spirit control was asked to affect soft plaster placed at a distance.[28] Denis (1901) expressed similar views to Delanne's in a paper appearing in the proceedings. The "psychic being," he stated, "is not confined to the limits of the body, but it is susceptible to exteriorization and release" (p. 614).

In her book *Naissance d'une Science Humaine*, historian Régina Plas (2000) commented that Delanne's and Dennis's use of the expression "experimental psychology" at the congress was a strategy to combat psychologists on their own turf and an attempt to obtain legitimation by association. In her view, spiritists were trying during this congress to "occupy part of the territory in constant expansion of this positive psychology" (p. 36).[29] This is consistent with Delanne's (1902) statement that, at the congress, spiritists faced "materialism right in its own temple" (p. 40).

This brings us to the topic of opposition to psychic phenomena during the congress. Writing in the *Proceedings of the Society for Psychical Research*, Frederik van Eeden (1900) stated that the work of the SPR was not opposed during the congress, and that, in fact, it found a "more general acknowledgement and approbation than at any of the three previous congresses" (p. 445). In his view psychical research had gained wide acceptance and its researchers were "no longer considered . . . cranks, or scientific outlaws . . . ." (p. 447).[30] Van Eeden was arguing this from personal experience, since he attended the congress, and had conversations with many of the congress's attendees. However, there are reasons to believe his view was, at best, incomplete.

In his paper, Flournoy (1901) referred to the attitude of individuals who did not like psychic phenomena considered at the congress. Some, he said, considered the topic "compromising" and did not welcome it. Flournoy noticed that the papers on the subject were "prudently hidden under the ingenious rubric of *related matters* . . . ." (p. 102). But, he continued, "you will forgive me when I call a spade a spade, and admit that underneath 'related matters' are actually hidden spiritism, occultism, and other pet peeves of contemporary scientific psychology" (pp. 102–103). Flournoy was aware that some would be worried that the interest of members of the above-mentioned newly founded Institute on psychic phenomena would be perceived as "horrible things," and as the "way to perdition" (p. 103). But he did not share such negative feelings, believing in the importance of empirical studies of psychic phenomena. After the congress, Flournoy wrote to William James in a letter dated August 27, 1900, about aspects of conference presentations related to psychical research. In his opinion, the presentations on the subject "very much scandalized the narrow-minded anatomophysiological group" (Le Clair 1966:103).[31]

That things were not as positive as van Eeden reported can be seen from the reactions of several congress attendees. Romanian physician Nicolas Vaschide (1874–1907)[32] characterized Delanne and Denis's papers as mere words. In his view, those dealing with "occult sciences" seemed to him

to lack "real scientific knowledge, and their observations, made in really unscientific conditions, are based on their feelings or on the phenomenon of belief" (Vaschide 1901:617). Delanne and Denis, he continued, merely presented "literary impressions, confessions, some opinions of faith, mixed with a regrettable ignorance of scientific documents . . . ." (p. 617). Vaschide (1900) was also critical of the spiritists in a conference report published in the *Revue de Métaphysique et de Morale*. As he wrote: "The different chapels of occult science ostensibly connected to experimental psychology will sound its bells in vain from the beyond . . . ." (p. 816).

German physician Oskar Vogt (1870–1959) presented a paper criticizing Spiritism (Vogt 1901).[33] He was clearly unhappy about the attempts of spiritists to get recognition by using the "name of science and psychology in general" (p. 656). He felt that psychology had much to suffer from its association with Spiritism because psychology had just obtained recognition regarding hypnotism and other topics. Talking about the section "Psychology of Hypnotism, of Suggestion, and Related Matters," Vogt stated that "spiritists invaded our section and compromised it with their anti-scientific communications" (p. 656). Such view was shared by a French psychologist who wrote a few years later referring to the "invasion of the 1900 psychology Congress by the spiritists" (Piéron 1905:42).

Other congress attendees were equally negative, as seen from the following discussion remarks:

> Dr. P. Valentin (**Paris**) If the spiritists rested on science, they would, to avoid a regrettable confusion, define exactly the words *psychism* and *psychic*.
> M. Ebbinghaus (**Breslau**) sincerely deplores that the foreign savants came from afar to a Congress of scientific psychology to assist in those discussions . . . . [The spiritists'] theories do not deserve the honor of discussion, for the time spent is lost for useful studies.
> M. Tokarsky (**Moscow**) protests in the name of science against ideas that pretend to be scientific . . . . [Spiritists need to] provide facts in place of their imaginary theories . . . .
> M. Hartenberg (**Paris**) . . . . The principal object of our section consists in the study of the psychological mechanism of hypnotism, of suggestion, of psychotherapy. It would be fine if our sessions were devoted to such issues, that are more useful in practice than theoretical dissertations about spiritism. I request that issues of spiritism, telepathy, super-normal phenomena, are placed apart during the next Congress.
> (Discussion 1901:662–663)

Bernheim (1901:645, all quotes) presented additional comments about the "issue of psychic or paranormal phenomena." Like others

in the congress, he asked for facts and not theories. Bernheim was not convinced of the reality of psychic phenomena even after having made his own observations of "subjects" and mediums because there were always "causes or error that impeded certitude." Furthermore, he believed that the human mind could suffer from "illusion of the senses . . . deformations of recollections . . . [and] errors of interpretation" that caused doubts about the adequacy of human testimony.

Interestingly, papers about psychic phenomena in the program of this congress came from different groups having different evidential standards. The papers of Myers (1901), van Eeden (1901), and Verrall (1901) were more empirical than the papers by Delanne (1901), Denis (1901), and Pascal (1901). This brings us to consider the existence of different layers of belief and methodological emphasis within those concerned with psychic phenomena. As Hess (1993:145) has argued, ideas of boundary-work can be expanded to include differences within particular groups.[34] In our case, there was also boundary-work between believers in psychic phenomena. An illustration of this in the fourth congress was Myers'views. Perhaps Myers (1900) had Delanne, Denis, and some of the papers listed in Table 1 in mind when he wrote about separating SPR work from other approaches:

> We must learn to submit to hearing our own achievements exaggerated,—and at the same time mixed up with narratives and opinions for which we have no intention whatever of making ourselves responsible . . . . and to insist that our object is still to stimulate inquiry far more than to propagate beliefs. We are not missionaries, but researchers.[35] (Myers 1900:448)

Myers' view was not represented in the conference proceedings. But I doubt that Bernheim and the like agreed with him. It is unlikely that many psychologists, who were already skeptical on the topic, distinguished SPR work from the writings of Delanne, Denis, Baraduc, and others. In fact, it is likely that they were as embarrassed by SPR work, as Myers was bothered by the above-mentioned papers. After all, probably few psychologists at the time would have felt sympathy or respect for Myers's (1901) statement in the proceedings:

> I claim that a spirit exists in man . . . itself the enjoying an increased freedom and vision, and also thereby allowing some departed spirit to make use of the partially vacated organism for the sake of communication with other spirits still incarnate on earth. (Myers 1901:114)

The 1905 Congress

The fifth congress took place at Rome under the presidency of Giuseppe Sergi (1841–1936) (De Sanctis, 1906).[36] While some papers touched on

psychic topics, there were considerably fewer than in the previous meeting.

As in the second congress (Richet 1892), Richet (1906) had a paper about the future of psychology, but his paper, while printed in the proceedings, was not read. Richet used the expression "occult psychology," but he stated in the address that he preferred the term "metapsychics," which he suggested in his presidential address to the SPR in February of 1905 (Richet 1905), and popularized after the congress in his celebrated *Traité de Métapsychique* (Richet 1922). Richet argued that there were no contradictions between scientific facts and metapsychic phenomena, there was only lack of knowledge. He wrote that "the facts of metapsychics, if they are real, should be studied honestly, methodologically, without hostility . . . ." (Richet 1906:172). Nonetheless, Richet was well aware that many individuals considered the topic to be a strange one.[37]

Other papers focused on the concept of vital forces capable of being projected from the body. One author defended the existence of a "vital electro-magnetism" (Gasc Desfossés 1906), while another discussed thought-transference as a function of a vital field projecting "psychoneurotic energy" (Del Torto 1906). The force was said to be particularly strong in the hypnotized and in the hysteric. In the discussion, the idea was strongly criticized for being based on imagination as opposed to empirical evidence (Tamburini 1906).

Following tests such as those of Gurney (1884), the effects of magnetic passes were explored, and the report included successful effects (presumably after suggestion was controlled for) for the production of a variety of sensations on human subjects, such as feelings of tingling and of coldness (Courtier 1906). In another paper the research question was inspired by the possibility of establishing if the hand of a hypnotist had an effect due to a force independent of suggestion (Favre 1906). Tests were done to affect microbes and seeds, finding that the right hand accelerated the growth of grains while the left hand hindered the growth of microbes.[38]

Only two other papers included material relevant to psychic phenomena. One of them was about instrumental tests of involuntary movements possibly related to thought-transference performances (d'Allonnes 1906), while the other considered supernormal phenomena in relation to the origin and development of religious belief (Marzorati 1906).

The congress was criticized later for its materialistic stance (Carreras 1905). It was suggested that spiritists did not attend the congress because they felt antagonism. Furthermore, it was argued that there were attempts to "recur to all kinds of ruses in order to prevent the few spiritists present at the Congress from reading their communications" (p. 654).

Concluding Remarks

The content of the first five congresses discussed here show the presence of psychical research in their programs. I have attempted to present a more detailed summary of some of these papers than that found in previous accounts in order to inform current readers about the actual discussions and presentations that took place at the time. Nonetheless, not all papers were summarized due to space limitations.

The fact that some papers on topics such as veridical hallucinations and mediumship were admitted to the congresses, and that the 1892 congress had Sidgwick and Myers as its President and Secretary, shows some level of acceptance, or tolerance, by the establishment. But it is clear that acceptance of papers in the congress did not mean acceptance of the reality of phenomena beyond conventional principles. The objections presented at the third and fourth congress are an example of this. These discussions show that psychical research was far from being accepted as a part of psychology during the nineteenth century and later, a topic discussed by others as well (e.g., Alvarado 2014, Coon 1992, Mauskopf & McVaugh 1980:Chapter 3, Sommer 2012, Wolffram 2009).

The situation was not as simple as Boring (1950) stated, who believed that emphasis on psychical research at the 1892 congress "led to a definite reaction away from the topic in the succeeding congresses" (p. 502). While there was a decline, this did not take place just after the 1892 meeting. Commenting on the "assault of all type of occultists, spiritists, theosophists, etc.," in the fourth congress, Nicolas (2002:152) believed that such presence led to the disappearance of the topic in later meetings. When we compare the fourth and the fifth congresses, it is evident that there was a decline of discussions of psychic phenomena, something particularly noticeable after the fifth meeting, which has been described as the "official evacuation" of the topic (Marmin 2001:157). There were, of course, some exceptions in later meetings. Among them were single papers about the claim to have shown "that a nervous radiation or effluence from the human body exists" (Alrutz 1924:260, at the 1923 congress), and a discussion of phenomena involving changes of personality, including mediumship (Oesterreich 1927, at the 1926 congress). But these papers stood alone in the congresses between many discussions of psychological topics. The days of having psychical research as an important part of the psychology congresses were gone, as seen in the absence of the topic in later congresses (e.g., Boring 1930).

As psychology became more organized as an academic field, it was easier, and desirable (according to one's perspective), to delimit the

content of the discipline. It was one thing to have occasional discussions of psychic phenomena in journals (e.g., James 1896, Richet 1884), and quite another to allow psychic phenomena to be part of the subject matter of psychology. Because the congresses represented a process of identity formation and professionalization for psychology, it was important to purge the content of the field of what was considered to have little respectability and scientific content. This content consisted of phenomena such as telepathy and mediumship, which were reminiscent of pre-nineteenth-century spiritual, occult, and supernatural traditions that were alien to the new psychology. Furthermore, as argued by Coon (1992), these topics were considered by most psychologists to be a "malevolent ghost preventing public confidence in scientific naturalism" (p. 149).

A later commentator, parapsychologist Joseph Banks Rhine (1895–1980), argued that because psychology was trying to get accepted into academia it needed to neglect difficult-to-measure phenomena. In his view the psychologist "needed to choose his ground with care and confine himself to research material that was manageable" (Rhine 1968:104).

Like Rhine, others have argued that part of the reason behind the rejection of psychic phenomena as processes more than conventional mechanisms was related to the professionalization of psychology. That is, by presenting themselves as the only group with the proper knowledge and training to handle such problems, they were justifying their existence and purpose in society and eliminating the competition in matters related to human experience and behavior (Brown 1983, Coon 1992, Parot 1994, Wolffram 2009). However, we cannot ignore the fact that psychic phenomena represented more than a threat to a professional image. Psychical research was, from the beginning, a problem for those psychologists who, convinced of the limits they had set on sensory-motor interaction, were not willing to consider that humans could interact in different ways. If telepathy implied that "the mind of the individual organism no longer appears as inevitably isolated from all other minds" (McDougall 1912:223), then this was a challenge to the idea that the mind was alive or active only within the confines of the nervous system. In a psychology where the brain and the rest of the nervous system reign supreme, such ideas were not only controversial, they were a challenge to the current physiological paradigm.

This paradigm was clearly in place in discussions of the topic of hallucinations that was so important during the first two congresses (for an overview see Berrios 1996:Chapter 3). Based on physiological and psychological factors, these concepts were hardly open to telepathic influences.

In addition to the issue of professionalization and the threat to the materialistic paradigm, which probably were the main reasons for the resistance of psychologists to psychical research, there may have been other factors influencing the decline of the topic in later congresses. One of them was the similar decline of hypnosis papers in later meetings. Because sessions devoted to this topic were, on occasion, one of the few protective coverings that provided a place for psychic phenomena in psychology when no other areas fulfilled that function (e.g., Crocq 1900:Chapters 11, 12, 18, 19, Liébeault 1889:Part 2, Chapters 3–6), such a decline could have eliminated one of the main contexts in which the psychic was discussed by psychologists.

Another factor may have been the death of two important SPR figures, Henry Sidgwick (in 1900) and Frederic W. H. Myers (in 1901). They were both moving forces, one in organization of work (Sidgwick) and the other in theory development (Myers). As Gauld (1968) has argued, the loss of these men affected the course of the SPR, and of psychical research. While this affected the development of psychical research in England, and probably changed the influence of the SPR in other places, enough has been said in this paper to make clear that the SPR's work concerned with the supernormal was never completely accepted by psychologists. Consequently, it is doubtful that the situation would have been different if the life of the early SPR leaders would have been longer.[39] Their work was continued by others such as James H. Hyslop (1854–1920), Oliver J. Lodge (1851–1940), Enrico Morselli (1852–1929) and others mentioned already (e.g., Flournoy, James, Ochorowicz, Richet, Schrenck-Notzing). These, and other individuals, kept psychical research alive after the 1905 congress but were not successful in integrating it into psychology, or into science at large.[40]

While discussions of psychical research did not disappear completely from forums of psychological discussion such as conferences and journals, its presence diminished considerably after the first five international congresses. Psychology journals still carried some discussions on the topic, but most of them were negative toward psychical research (e.g., Troland 1914), as seen as well in reviews of the many books (e.g., Janet 1923, Jones 1910). Eventually the field became more separated from psychology, developing its own journals and congresses.[41]

In reality, the presence of psychic topics in the congresses was never a seal of approval from the growing field of psychology during the period of the above-mentioned meetings. Instead, the congresses represented the struggles of psychical researchers for recognition and, as Parot (1994) has argued, the separation of psychical research and psychology. Similarly, in

his discussion of the congresses, Nuttin (1992) referred to the "separation of scientific psychology from elements that risked to contaminate it" (p. 8).

Some modern psychologists have discussed psychic phenomena in the congresses, seemingly adopting the perspective that the disappearance of psychical research was a desirable outcome leading to the formation of scientific psychology (e.g., Nicolas 2002, Nuttin 1992). But such view is at odds with aspects of the modern historiography of psychology.

Ellenberger's influential study *The Discovery of the Unconscious* (1970) alerted us to the importance of theorization and research on the phenomena discussed in this paper—as well as to the movements of mesmerism and Spiritism—as factors contributing to the development of the concept of the subconscious mind. Later studies have presented a similar perspective, one that places interest in topics such as telepathy and mediumship as agents of influence, as opposed to simple obstacles that had to be eliminated for the development of psychology as a science (e.g., Alvarado 2002, Crabtree 1993, Plas 2000, Shamdasani 1993).

Furthermore, while the disappearance of psychical research from the congresses is related to an attempt to take psychology into specific directions devoid of spiritualistic conceptions of human nature, and thus is a historical example of rejection and depuration of a field, we need to remember that the topics discussed at the congress were influential in other ways. For one, they contributed to the database of phenomena that contributed to the construction of the concept of dissociation (e.g., Alvarado 2002, Alvarado & Krippner 2010). The SPR study of hallucinations, as recognized by skeptic Moll (1889/1890), was a significant contribution to the furthering of empirical knowledge on the prevalence and phenomenology of hallucinations, regardless of the rejection of the telepathic component (see Le Maléfan & Sommer 2015). Other contributions to psychology and psychiatry came from the study of mediumship, as seen in Flournoy's studies of subliminal imagination, and from other observations leading to specific diagnoses and the concept of automatisms (Alvarado, Maraldi, Machado, & Zangari 2014, Le Maléfan 1999). This is instructive in that it illustrates how marginal movements, the periphery, or what has been rejected, can have an impact on the mainstream, or the core of a field such as psychology.

Notes

[1] Further examples are discussed by other authors, among them Alvarado (2014), Coon (1992), Le Maléfan (1999), and Sommer (2012, 2013).

[2] Nineteenth-century French work on hypnosis has been discussed by Carroy (1991), Crabtree (1993), and Gauld (1992). Nicolas (2004)

focuses on the controversies between the Salpêtrière and Nancy schools of hypnosis. On French interest in personality and dissociation, see Foschi (2003) and Nicolas (2002).

[3] There are several discussions of the psychology congresses (Benjamin, Jr., & Baker 2012, Claparède 1930, Evrard 2016, Montoro, Carpintero & Tortosa 1983, Montoro, Tortosa, & Carpintero 1992, Nicolas 2002, Nuttin 1992, Piéron 1954, Rosenzweig, Holtzman, Sabourin, & Bélanger 2000, Taves 2014). It has been suggested that the congresses were affected as well by the influence of scientific societies and the meetings of other disciplines and by the impetus provided by the universal expositions (Nicolas 2002). Shore (2001) argues that these, and other congresses, developed in the context of the values and concerns of modernity.

[4] Ochorowicz has been discussed by Domanski (2003). He made several contributions to the psychical research literature (e.g., Ochorowicz 1887, 1909). His 1881 paper has been discussed by Nicolas and Söderlund (2005).

[5] There are studies of developments in Italy (Biondi 1988), France (Lachapelle 2011), the United States (Moore 1977), England (Oppenheim 1985), and Germany (Wolffram 2009). The history of the early SPR is chronicled by Gauld (1968).

[6] Myers' important work on the subliminal mind, and on psychical research, was discussed in detail for the first time by Gauld (1968:38–44, 89–114, 116–136, Chapters 12–13). See also Crabtree (1993:Chapter 16), and Kelly (2007). On Myers in general, see Hamilton (2009).

[7] Aspects of Richet's physiological and medical career are reviewed by Wolf (1993). For his psychical research, see my overview (Alvarado 2016), as well as Brower (2010:Chapter 3), Evrard (2016:Chapter 5), and Le Maléfan (1999:85–88, 2002).

[8] In addition to the congress proceedings, see various other reports (James 1889a, Marillier 1889, A. T. Myers 1889).

[9] Marmin (2001:150–155) refers to Richet as the "main artisan" for the rapprochement between psychology and psychical research. This was possible due to the mediating influence of Richet's high social, intellectual, and scientific prestige. Richet was well-known in psychical research circles before 1889 (e.g., Richet 1884, 1888). On Sidgwick, see Schultz (2004).

[10] On Marillier, see Le Maléfan and Sommer (2015). Eventually the SPR published a detailed report of the study conducted in England (Sidgwick et al. 1894; see also Denning 1994). Somewhat later, James (e.g., 1890a, 1890b) was publishing letters in the United States asking for cooperation for the American part of the project.

[11] On Gurney, see Epperson's (1997) biography. The experiments by Gurney mentioned by Myers were part of the late Nineteenth-century magnetic movement (Alvarado 2009b).

[12] For information on this congress, see, in addition to its proceedings, several other reports (International Congress of Experimental Psychology 1892b, Macdonald 1892, H. Sidgwick 1892a, Sidgwick & Myers 1892). Sully (1918:230) wrote that he represented the "orthodox branch," while Myers "was to look after the Psychical Research Department."

[13] On Wundt, see Bringmann and Tweney (1980), and Rieber (1980). He was by no means a friend of psychical research nor of Spiritualism (Wundt 1879, 2000/1892, see also Kohls & Sommer 2006 and Marshall & Went 1980; I owe the 2006 reference to Andreas Sommer). Carroy and Schmidgen (2006) suggest that Wundt was defensive because he may have felt that his approach to psychology could be marginalized. Janet (1892:611) actually stated that SPR members kept a low profile in the conference program. Nonetheless, Nuttin (1992) has argued that psychical research came to "dominate the scene and the personal orientation of the organizers" (p. 51). Considering the overall program of the congress, this assertion seems an exaggeration.

[14] On Franklin, see Scarborough and Furumoto (1987:Chapter 5). She presented a paper at the 1892 congress (Franklin 1892).

[15] For longer discussions published in the SPR *Proceedings*, see Myers (1892b) and Sidgwick, Sidgwick, and Smith (1889). On Mrs. Sidgwick's life and psychical research, see E. Sidgwick (1938). Before 1892 the SPR had conducted and published many experiments on the subject (see the overviews of Luckhurst, 2002:Chapter 2, and Podmore 1894).

[16] The congress was discussed in various reports (Buchner 1896, Franz 1896, H. Sidgwick 1896, Titchener 1896). Stumpf is discussed by Boring (1950:362–371).

[17] Parish (1894/1897:Chapters 3, 9) discussed the SPR's work with hallucinations and was skeptical of telepathy. For discussions of his views, see James (1897).

[18] H. Sidgwick's (1897a; see also H. Sidgwick, 1897b) paper was an analysis and a reply to Hansen and Lehmann's (1895) reduction of telepathy to unconscious whispering. On Liébeault's career in hypnosis, see Carrer (2002). Liébeault wrote about psychic phenomena in several publications (Alvarado 2009a)

[19] I am grateful to Niko Kohls for this reference.

[20] The proceedings of the 1900 congress were the first ones to have an editor's name in its title page. They were edited by Janet. Reports of the congress include Quatrième Congrès International de Psychologie

(1900), van Eeden (1900), Warren (1900), and Woodworth (1900).

[21] Richet investigated Pepito Rodríguez Arriola, a Spanish boy 3½ years old who could play the piano and compose music without formal instruction (see also A Musical Prodigy 1901). Prince's report was about the famous Beauchamp multiple personality case (Prince 1906).

[22] There are brief discussions of this organization in the works of Brower (2010:Chapter 3), Méheust (1999b:146–147), and Plas (2000:137–138, 147–150).

[23] Hélène Smith was the pseudonym of Élise Catherine Müller (1861–1929). Her mediumship was discussed in more detail by Flournoy (1900). The case, and Flournoy's psychical research work, has been discussed by Alvarado, Maraldi, Machado, and Zangari (2014), and by Shamdasani (1994).

[24] The SPR *Proceeedings* reprinted Myers's paper about Thompson, and published longer versions of the other two papers (Myers 1902, van Eeden 1902, Verrall 1902). Myers had died by the time the papers appeared in the *Proceedings*.

[25] Bernheim, the leader of the Nancy school of hypnosis, published many important works (e.g., Bernheim 1884). He is discussed by Gauld (1992:324–337) and by Nicolas (2004:Chapter 3). His skeptical attitude about psychic phenomena can be seen in publications that appeared before the congress (Bernheim 1884:56, 1888).

[26] This refers to the idea that forces related to the body's vital processes could be projected at a distance to cause phenomena such as movement of objects without contact, and materializations, and to late mesmeric ideas (Alvarado 2006, 2009b).

[27] The books of Delanne (1897) and Denis (1893) have longer discussions of the ideas these men presented during the congress. French Spiritism is discussed by Edelman (1995) and Sharp (2006).

[28] Example of these imprints are discussed by de Rochas (1898). On Palladino's career and her influence on psychical research, see Alvarado (1993).

[29] The expression "experimental psychology" was used in different ways during the nineteenth century. Janet (1889) used it in the title of one of his main works referring in part to hypnotically induced phenomena, as did Binet (1888), who also discussed hysterical writing. While some, like Wundt, defined the topic in terms of laboratories and measurement, Myers (1886) did not follow such limits and referred to empirical studies that did not depend on metaphysical speculation nor solely on introspection. Years later, Myers (1894) described psychical research as the "left wing of Experimental Psychology" (p. 731). In

addition to the use of "experimental psychology," we also need to be aware that French spiritists used the words psychology, psychological, and experimental frequently before the congress. Allan Kardec (the pseudonym of Hippolyte Léon Dénizard Rivail, 1804–1869) used the word experimental, as seen in the cover page of one of his main books (Kardec 1863). His journal, *Revue Spirite*, first published in 1858, was subtitled "Journal of Psychological Studies." Referring to spiritist phenomena, Denis (1893:215) used the expression "psychological studies" and Delanne (1897) used physiological psychology. Similar uses can be found in the English-language spiritualist literature (e.g., Barkas 1876). See Binet's (1894:495, footnote) complaint about what he perceived were the psychical researcher's attempts to use the term "psychic" as a synonym of psychological.

[30] Spiritists discussed the congress as a victory for their movement in spiritist publications. An example was Denis (1902:32, no date:30), who saw the 1900 congress as evidence that spiritism was starting to get into the "fortress " of science. In his view, "in spite of the hostility of the organizers," an unnamed member of the conference committee could not help but say that they were invaded by spiritism (Denis 1902:32). Delanne (1902), while aware of the opposition, believed the event to have been a "memorable date in the history of our doctrine" (p. 40), a day representing the entrance of spiritism into the official world of science. In a later commentary, probably written by Delanne, it was stated that Ribot, Janet, and their associates "announced contemptuously their intention to outlaw in later Congresses all communications which purpose was the study of psychic phenomena" (anonymous editorial note in Carreras 1905:654).

[31] In the same letter, dated August 7, 1900, Flournoy told James that while some individuals wanted to include psychic phenomena among the subject matter of the institute, Pierre Janet said he would be associated with the institute "only with the very fixed idea that it would not be concerned with occultism, spiritism, etc." (Le Clair 1966:104).

[32] On Vaschide, see Herseni (1965) and Nicolas (2002:173–174). Vaschide (1902) later expressed skepticism about the existence of telepathic hallucinations.

[33] For biographical information, see Klatzo with Zu Rhein (2002).

[34] Examples of modern sociological studies relevant to boundary work and parapsychology include Collins and Pinch (1982) and Pinch and Collins (1984). See also Sommer's (2012, 2013) historical work.

[35] The SPR continuously engaged in boundary-work through reviews of works by other students of psychic phenomena (e.g., Leaf 1893, Myers 1898).

[36] See also Carreras (1905) and Piéron's (1905) reports.
[37] On this paper, see Alvarado (2011).
[38] Gasc Defossés (1907) and Favre (1904, 1905) published longer accounts of their studies.
[39] Myers was an important loss due to his particular emphasis on integrating psychology and psychical research, as seen in his well-known posthumously published book *Human Personality and Its Survival of Bodily Death* (Myers 1903). With some exceptions (e.g., James 1903), most psychologists rejected Myers' ideas about the supernormal and survival of death (e.g., Review 1903, Riley 1903).
[40] For discussions of Twentieth-century developments, see Inglis (1984), Mauskopf and McVaugh (1980), Méheust (1999b), and Zingrone (2010). Some examples of later psychological studies include those of Coover (1917), Morselli (1908), Osty (1926), and Mrs. H. Sidgwick (1915).
[41] There were five psychical research congresses held in Copenhagen (1921), Warsaw (1923), Paris (1927), Athens (1930), and Oslo (1935). While there were attempts to standardize the field in some of them regarding things such as terminology, there were many differences and conflicts along national and conceptual lines that limited the usefulness of the meetings (Lachapelle 2005). Some of the journals created which helped the field to develop were the *Journal of the American Society for Psychical Research* (1907), the *Revue Métapsychique* (1920, first called *Bulletin de l'Institut Métapsychique International*), the *Zeitschrift für Parapsychologie* (1926, which continued the *Psychische Studien*), and the *Journal of Parapsychology* (1937) (Alvarado, Biondi, & Kramer 2006).

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ESSAY

Apparent Communications from an Eager Spirit

Russell Targ

Elisabeth Targ

My daughter, psychiatrist Elisabeth Targ, a pioneer in the field of mind/body medical research, died July 18th, 2002, of a brain tumor at the age of 40. She was recognized as a brilliant researcher, graduating from Stanford University at age 19, with a degree in biology and a Russian Translators Certificate. She then went on to do original research in distant healing at California Pacific Medical Center. In a double-blind study with her 60 AIDS patients, those who received distant healing had significantly better outcomes, fewer trips to the hospital, fewer days in the hospital, better self-report, etc., than the controls for whom no prayers were said (*Western Medical Journal*, December1998).

Elisabeth died in the living room of my home in Portola Valley, California, overlooking Stanford and San Francisco Bay to the east. That was her request. The following day, I sat with her husband Mark Comings and my friend Jane Katra, on a deck watching the lights of Palo Alto begin to come on through the fog. We were wondering if we would ever hear from Elisabeth. Moments later, all the lights in the three-story house flashed off, leaving us in the semi-darkness. "What was that," we all said at once. Could it be an answer to our question? At which the lights came on. A few seconds later the lights again flashed off and then on again. Such a thing never happened before or since in this five-year-old building.

A week later, Mark received a phone call from a nurse in Seattle who had taken part in Elisabeth's distant healing experiment. She wanted to send him a letter that was dictated to her by Elisabeth. In the nurse's dream the previous night, Elisabeth appeared to her, and strongly requested that she copy down a couple of sentences in Russian and send them to her husband Mark. The nurse spoke no Russian nor any language but English. Elisabeth told her in the dream, "I will give you the words one syllable at a time, and

you will remember them, and copy them when you wake up." Neither the nurse nor Mark had any idea what these two little "code groups" of four syllables meant. When I finally saw the letter in the nurse's blue envelope, in the kitchen of my house, I could phonetically say one line which was Russian for, "I see you." A few days later, I found a Russian speaker who told me that the second line was old-fashioned Russian for, "I adore you." If the nurse had simply spoken the English sentences to Mark on the phone, we would not be relating this story. It required the imagination of Elisabeth, who was a fluent Russian speaker, to find a unique way to communicate so as to send a message that would be understood as unambiguously from her.

A year later, Jane, who has a Ph.D. in Health Education, had an opportunity to have an interview for a teaching position at Duke University. As she relates it, she was sitting at a little round table with the department chairman and his nursing assistant, probably a Ph.D. in Nursing Education. After a few minutes of preliminaries, the nurse said that she had to interrupt for just a minute. She wanted to know if Jane knew a tall woman with long dark hair who died recently. The image of the woman was standing, very clear to her, behind Jane. Jane said yes. She knew such a person. The nurse than went on, "This spirit is very insistent. She says that she has an important message for her father, whom you know." Jane said yes. "The figure is saying that she had a message that would convince her father that she still survives. Tell my father that he should remember a time when I was a little child, and he and another person strongly forced me to wear a red dress that I didn't want to wear. It was traumatic for me. And he should remember it, too."

What had happened was that my wife Joan and I had received a very pretty red dress from my mother in New York. As a publicist, she had the idea of how sweet it would be if baby Elisabeth could meet grandma at the airport in the fabulous dress. Joan never wore dresses, and Elisabeth wasn't having any of it, and ripped it off as soon as we let her loose. Needless to say, neither Joan nor I have ever discussed this absurd moment in our early childrearing experience. Thus at the time that Jane related the story to me, a few days later, I was the only living person who had any knowledge of the embarrassing red-dress caper of forty years before. My wife Joan had died five years earlier. Notice that this case is like a nineteenth-century proxy sitting, in which a sitter goes to a medium on behalf of a third person who is entirely unknown to the medium.

I know that there are some readers who will scoff and say that this is just another case of living agent psi. But it seems to me that any instance of ostensible survival requires some knowledgeable person to verify that the utterances or observations are correct. It reminds me of one of Ian

Stevenson's cases in which a six-year-old child was taken to the schoolhouse in another city where he claimed to have been a student a decade previously. The child was able to identify and name almost all of the children in a dusty old class picture of that time. And I have heard people assert that the child was simply reading the mind of the schoolmaster who provided the photo, and later looked up the names of the children. I propose that the two cases that I offer here are on the far side of what Steve Braude considers to be a burden of crippling complexity.

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BOOK REVIEW

Phenomena: The Secret History of the U.S. Government's Investigations into Extrasensory Perception and Psychokinesis by Annie Jacobsen. Boston, MA: Little, Brown, 2017. 544 pp. $14.64 (hardcover). ISBN 978-0316349369.

When the book *Phenomena* by Annie Jacobsen first arrived, I expected it to be filled with all the facts that I know about the U.S. Army's involvement with ESP. Having been involved with the Army's use of remote viewing since the very beginning as Remote Viewer #001, and serving in that capacity for the entire 27 years of the Army's Star Gate program's existence, I have had direct access to all of the operational material, as well as the science. I also worked in the lab at Stanford Research Institute International (SRI) for a number of years, as well as the Science Applications International Corporation (SAIC) lab for the entire time of its existence. So, I expected a pleasurable and comfortable read. Annie Jacobsen's book is supposed to be the definitive history of the Government's use of psychics for intelligence purposes. Unfortunately, the book does not start out in this direction. Within the first 200 pages, she attempts to entertain the reader with a less-than-concise history of the paranormal and its tangencies to the government; her purported connections to intelligence services are all over the map. They include the Air Force, CIA, and sometimes the Army. However, the way she presents the material is confusing and fails to identify which service or agency she is talking about. The reader must repetitively go to the index or chapter references to know which. The 'primary' behind the Star Gate Program was the United States Army, which is why this is important. The CIA funded approximately $160,000 at the very beginning of the program, which established the possibility of remote viewing research (RV). The U.S. Army and the Defense Intelligence Agency (DIA) provided just short of $19,000,000, and established the RV threat as a collection methodology.

What should be established from the very beginning is the fact that the Central Intelligence Agency has always had an interest in whatever bizarre method might be used to shut down, interfere with, or eradicate problems the U. S. Government might be dealing with (such as specially treated cigars intended to make Castro's beard fall out, or psychedelics to lessen someone's resistance to interrogation). But there is no CIA connection to how or why U.S. Army Counter Intelligence decided to investigate RV as a threat.

Himmler's interest in chasing the occult to ensure Hitler's victories and world domination, or Andrija Puharich's interest in psychedelics and mushrooms as well as his difficulties in finding the source of the mushrooms (while interesting and quite normal for a medical doctor), have no bearing on what then follows in the book beyond page 200. Puharich's interest in psychedelics is clear, but what is not clear is whether Puharich was ever an agent or employee of the CIA, or the U.S. Army.

There are many reasons why this book shouldn't be thought of as a definitive history. A lot of what the author says is simply wrong, and many of her assumptions are based on false premises. Some of these errors are presented in this Review.

Phenomena opens with a concise statement in the Prologue, establishing what the book covers. It's about:

1. "The U.S. government's decades-long interest in anomalous mental phenomena, including extra-sensory perception [ESP], psychokinesis [PK], map dowsing, and other forms of divination, ..." *This is true*.
2. "And then, just a few years after the end of World War II, the U.S. government determined anomalous mental phenomena to be effective military and intelligence tools, and began to investigate their possible use in classified operations." *This is not true.*

Jacobsen then jumps to what is essentially the beginning of what she calls "the real action" which she says began in 1972, when a small group of promising young scientists was approached by the CIA to embark upon a research program involving psychics, or "sensitives." At "Stanford Research Institute . . ." *Which is only half true*.

The CIA did spend $50,000 to ascertain if ESP could be of value, and it was determined that it could be. Jacobsen states that because of this finding ". . . everybody wanted in on it—the Navy, the Air Force, the Army [including its Intelligence and Security Command (INSCOM) and the Development and Readiness Command], the Coast Guard . . .", and she goes on to list just about all the people who were eventually supported by Project Star Gate. She cleverly twists the truth here to imply that the entire U.S. Government wanted in on it from the beginning. *Which is not true*.

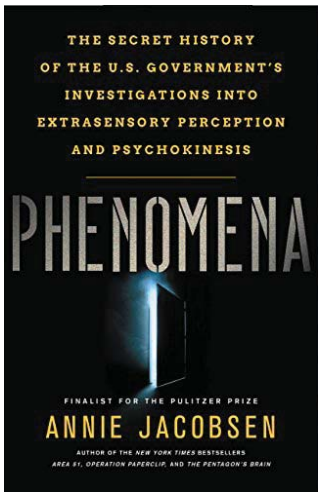
The reason the U.S. Army, specifically the 902nd Military Intelligence (MI) Group, Fort George G. Meade, Maryland, originally became involved, was based on the premise that if what was happening at SRI International was real, then perhaps what the Soviets were alledgedly doing in the field of the paranormal could also be true, and could be a direct threat to the security

of the United States. After nearly a year of investigating this issue, the 902nd MI made the decision to test this premise. They began by trying to locate three possible psychics within its own ranks, and then with the intention of spending a year training them to do what was being done at SRI they would use them to target our own facilities for a year, after which an independent analysis of the material would be made to verify what the actual threat in this area to the United States from the Soviet Union might actually be. It was originally called Project *Gondola Wish*.

These facts are a far cry from the sensationalistic novel being sold here as fact. Jacobsen goes further, implying there were many scientists, physicists, biologists, neurophysiologists, cyberneticists, astrophysicists, a general, an admiral, a Nobel Laureate, and an Apollo astronaut involved within this program effort. However, many of these people she investigated or declares she interviewed had either very little to do with the project, or had absolutely nothing to do with the project at all. Bringing them into the book simply added a huge and further complicating extension to what had really happened. Much of what these people had to say was either not pertinent to the story of what the government was doing or provided Jacobsen with detritus to fill in the paragraphs she needed to complete the outline she had already carved from her imagination.

The work done within what is now known as the Star Gate Project most certainly didn't begin with the Nazis, nor did it have anything to do with Colonel William Donovan, the father of Military Intelligence (MI), or with the Office of Strategic Services (OSS), which was a precursor to the CIA. It had nothing to do with De Wohl, or Himmler, Goudsmit, truth serums, or controlling human behavior. It was never connected to Bluebird, or Artichoke, or MKULTRA, even though Jacobsen says it was. While Andrija Puharich was certainly a colorful individual, he had nothing to do with SRI and the U.S. Army's interest in determining the Communist psi threat. And neither do the beginning chapters of this book.

In Chapter Three, the most notable error is misnaming the father of modern American ESP research as "James Bank Rhine" (p. 41). His real name is "Joseph Banks Rhine," which anyone truly interested in accurately reporting on American interests in the field of psi would know. Also, if one spent any time at all reviewing Rhine's depth of knowledge within the field, his research reports and papers, they would know that Martin Gardner's efforts at debunking Rhine's work, like that of many skeptics, completely ignores the data, instead preferring to attack Rhine's "beliefs" or "attitudes." But, more to the point, Jacobsen says that locating mines buried underwater using dogs or understanding the skills of homing pigeons are somehow linked to the continuing saga of the U.S. Army's interests in



ESP; but actually none of this material has anything to do with the use of psychics or ESP in the Star Gate Program.

It is no surprise that we suddenly find Puharich's re-entry into the picture trying to "locate a drug that might enhance ESP." But, Jacobsen then quickly leaps to a conclusion based on the CIA's quest for a truth serum, that this must also mean "the Army wanted a drug to turn ESP on and off like a light switch." While this may have been Puharich's goal, I've never seen nor heard of this goal being one the U.S. Army has pursued, and I was part of the effort for the entire length of the program, having worked both sides—collection as well as within the labs. I can say most emphatically it was never a part of the Army's ESP Program Star Gate.

Again, any investigation of the Star Gate Program would have to have uncovered the more than half a dozen oversight committees—Congressional, Scientific, as well as Human Use—that oversaw our Program. I am completely surprised that somehow Jacobsen missed this. Any one of these committees would have shut down the program at the merest hint of any interest in the use of drugs. To imply there even was an interest degrades the reputation of every person who honorably served within the Star Gate Project. If that isn't bad enough, Jacobsen goes further by saying "It is not known whether or not he [Puharich] was privy to the CIA's ESP programs." She still directly connects him to MKULTRA Subproject 58, which had nothing to do with ESP. MKULTRA was targeted toward interrogations. And, Puharich had no connection to Project Star Gate. Jacobsen knows this, but says it anyway (pp. 44, 45).

She drives the hammer home on these "alternate facts" with her statement; "The program Puharich's superior was likely referring to was the CIA's MKULTRA, Subproject 58 . . . ." In numerous instances throughout her book, Jacobsen treats us to her leaps of faith, which is a dishonest or deceitful method of including, connecting, or otherwise implying connection to something where no real connection exists. This is clearly shoddy reporting, and unfortunately it exists throughout the manuscript, so much so that all the errors would fill many pages in this Review to the point of reader boredom.

Part II, THE CIA YEARS, opens with Chapter Six, the Enigma of Uri Geller. Uri Geller's connection to the Army's use of psychics is threaded

throughout the book. While Geller was involved in a very short series of experiments over a period of less than 6 weeks in the 23-year project, his results were typically common to those of dozens of people tested over the years. He was brought into the lab at SRI at the specific request of the CIA, prior to the U.S. Army's decision to test the degree of threat. Geller had no impact on this decision.

While it was reported numerous times that Geller demonstrated an ability to bend metal by paranormal means, the lab noted in *Nature 252*:602–607 that lab personnel were unable to combine such observations with adequately controlled experiments to obtain sufficient data to support any paranormal hypothesis. He was not part of the Army program, yet Jacobsen talks of metal bending for nearly 15 pages, implying general CIA interest in his abilities. In fact, Geller was tested at the request of Director Helms for reasons known only to him. The 6 weeks of testing that took place at SRI were set up to satisfy Helm's request. There was no connection to the Army project, if for no other reason than that Geller and Puharich lacked valid security clearances and Star Gate was a Special Access Program (SAP) that didn't exist yet. In spite of these facts, Jacobsen makes it appear that all this was taking place at the same time. The testing of Uri Geller was done years before the Army's interest in pursuing ESP or the use of psychics to test the viability of the Soviet threat. Nevertheless, there is still an attempt to make a connection, to sell the continuity of her assumptions.

In the next chapter, we are treated to Edgar Mitchell's trip to the Moon, which is quite entertaining but has absolutely nothing to do with the Army Project Star Gate or the Army's interest in using psychics for intelligence collection purposes. The U.S. Army project began as a counter-intelligence operation to determine the effectiveness and threat of the Soviet use of psychics against the United States of America. This was generated by the early findings at SRI vis-à-vis the CIA-sponsored research begun circa 1972 (https://www.cia.gov/library/readingroom/docs/CIA-RDP96-00788R002000160011-2.pdf). Note: The date on this document is 1978, many years after the CIA's efforts at SRI.

Given the above, I would advise a reader who is interested in the Government's true efforts in using psychics for Intelligence purposes to skip the first 200 pages of *Phenomena* and go to where the action truly begins—with Dale Graff's efforts to translate the piles of military research material that had been obtained from the Soviet Union by the Intelligence community at large, and his preliminary uses of psi to locate a Russian bomber that had disappeared over Zaire. This success significantly demonstrated to the U.S. Army that the use of psychics for intelligence purposes might well be a viable path.

Chapter 14 introduces the beginning of the formal efforts by the U.S. Army 902nd Military Intelligence Group, previously noted as *Project Gondola Wish*. As an Operational Security (OPSEC) Officer, U.S. Army, Second Lieutenant Frederick Holmes Atwater identified the potential Soviet psi threat underscored by the early research done on what would eventually be called *Remote Viewing* by Dr. Hal Puthoff and Russell Targ at SRI International.

According to Jacobsen, Lieutenant Atwater made a proposal and recommendation to his boss, Major Robert E. Keenan, that OPSEC hire SRI and their "sensitives" to target U.S. Army classified facilities and operations to see if they could obtain any information of value, thus replicating possible Soviet capabilities that might highlight U.S. Army vulnerabilities. Keenan responded this would be "impossible" since the SRI sensitives didn't have the proper security clearances. Rather than stand down, Atwater suggested they might be able to find personnel within INSCOM with high levels of latent ability to do the same. Keenan kicked it to the top of his chain of command, where it was eventually approved by Major General Edmund R. Thompson, the assistant Chief of Staff for Army Intelligence (ACSI), and Project *Gondola Wish* was born.

Unfortunately, Jacobsen isn't three pages into the very beginning of Chapter Fourteen, when she severely undermines my own military history by saying: "A senior projects officer in Signals Intelligence and Electronic Warfare, he was thirty-two years old. His personal life was a mess, and he disliked the Army. From his perspective, he had given his employer everything, and it had given him back very little." She quotes page 59 of my book *Memoirs of a Psychic Spy*. But her statements are false. If she had taken the time to read my book in its entirety, she would have known this was referring to an in-the-moment argument I had with my Company Commander over approval of leave (from Germany) when my first wife deserted me and took my 2-year-old son with her back to the States. It was not referring to my feelings toward the United States Army.

Additionally, the period she is describing on page 59 wasn't about the above time period at all. It was the beginning of Project *Gondola Wish*, while I was assigned to the INSCOM Headquarters and was in charge of my Military Occupational Specialty (MOS) worldwide. At that point in time, I reflected on my overall feelings for my entire time in the Army on page 73 of *Memoirs*, where I said:

> Within my MOS or group of peers, I was sitting in the catbird seat. There just weren't any jobs really that were better, more demanding, or more respected than the one I was sitting in. I was working right next to the flagpole, putting in ten- to twelve-hour days with lots of weekend overtime, dealing with unbelievable challenges, *and loving every minute of it*. [Italics my own]

Reading my entire book would have been of great benefit to her for determining how I felt toward the Army. No one spends more than 12 straight years plus with an unbroken chain of overseas assignments, back-to-back, unless they truly do love it.

I **did**, because I loved every minute of it. And, no one resents the threat of being assigned to a stateside training unit, like Fort Bragg, more than when they've just completed a string of 14 years working at nothing but active operational missions in defense of their homeland and Nation.

Of course, I resented the very idea of a training assignment. These are issues someone would understand after reading my book, or volunteering two decades of their life to the American people and defense of the United States Constitution. But, Jacobsen found it was apparently easier to trash my entire career and personal commitment to the love for my country. When I specifically asked her to correct this, she refused (email; Annie Jacobsen, 12:04 a.m., 4/18/2017).

In fairness, she did agree to correct three other errors: "Sometimes his father would hit him so hard, his ears would ring and his face would bleed" (p. 230). She is changing this to accurately reflect that it was my mother and not my father who did this (p. 7 in my *Memoirs*). What's more curious, is that Jacobsen felt the need to write about it as part of my Near Death Experience (NDE), which it wasn't. And—another error—my NDE actually occurred 6 years later in Europe, not in Miami where I had lived as a child.

Jacobsen must have also felt that including my twin-sister Margaret's problems in the description of my NDE might play better to her readers. Nevertheless, it didn't happen the way she stated. She agreed to correct the part where she says Margaret was "dependent on drugs" (p. 230), in other words, a *drug addict*. Of course, my sister wasn't. And the nuns didn't take her baby away when she was pregnant during her high school years; the aunt she was sent to live with in Baltimore did (p. 19 in my *Memoirs*), something Jacobsen has also refused to correct. Jacobsen refused to correct her statement that my sister was ". . . sedated" (p. 230). I never made that statement either.

"Back in America after the war, he worked at a series of unsatisfying Army Jobs" (p. 231); an interesting statement, but also *not true*. Following my tour of duty in Vietnam, I went directly to Europe and while there served in four different assignments in four different cities, over my three very satisfying years tour of duty in West Germany. All four of those assignments were real and active missions in Europe, every bit as serious within the drama of the Cold War, as were my assignments and activities served over 27 months of duty in Southeast Asia.

"Here, inside an electronically shielded room . . ." (p. 233); none of the remote viewing rooms on Fort Meade had electronic shielding. Like much of the book, this is pure invention.

"At Fort Meade, the stage was now set for a state of utter confusion and chaos" (p. 240). What was remote viewing? Unknown. How did it work? Unknown. Where does the information come from? Unknown. How does the remote viewer interpret it? Unknown. All of these questions, 'unknown' at the time. But, utter confusion and chaos?—that never happened!

Jacobsen's statement is demeaning and irresponsibly defames the professionalism and herculean efforts by the handful of professional Intelligence officers who stepped up when asked, even at the cost of their careers. Some gave their lives to the effort, in support of a program to explore a Soviet threat, doing something which was never expected to work. Jacobsen's statement is outrageous, but not surprising given the amount of abuse participants of Star Gate have suffered over many decades now. It's precisely this kind of shoddy reporting that the Star Gate personnel have been subjected to that is so offensive. An investigative reporter should at least spend the critical time required to ensure that their facts are correct.

The following are other errors noticed within the book *Phenomena*:

1. Many of the people identified by Jacobsen as having been interviewed by her regarding the U.S. Army psi Program, had nothing to do with Star Gate (following and pp. 488–492). The few she does list within her book who were aware of Star Gate were not cleared for and had no access to the program offices. This is especially true following the taking of the United States Embassy in Tehran, Iran, and the incarceration of American hostages. Information provided to Jacobsen by these people is hearsay and either invented or second-hand. I would list them here, but there are just too many.

An example is Lieutenant Colonel John Alexander. We were all under very specific orders from the Commander of INSCOM, General Burt Stubblebine, not to share information about Star Gate with John Alexander. The General told me in private that he was upset that John was becoming too involved with too many things, and he (John) had too much on his plate. The General wanted him focused on the job he had assigned to him. We complied, and to my knowledge this order was never rescinded.

I was friends with John, and knew at the time that John was chasing down many other leads for the General. However, as excited as things had become under Bert's command, the General himself was sometimes mixing apples and oranges, or he would forget the firewalls he had himself created and would bring things up in front of people who were not read in

for them—that is, had a specific need to know. Need-to-know is one of the most important concepts within intelligence for controlling those firewalls.

2. Jacobsen states: "This time, McMoneagle produced a very positive, ten-page classified report encouraging his commanding officers at INSCOM to allow other soldiers to learn how to expand their consciousness and have out-of-body experiences at the Monroe Institute" (p. 289). First and foremost, those were not **my** commanding officers. This is fiction and simply not true. What is true is that I wrote a *trip report* on how I benefited from my participation at The Monroe Institute (TMI) and how I felt it would support my efforts as a remote viewer in Star Gate. I wrote this report under a direct order from General Stubblebine. Jacobsen then also states: ". . . this is where a *red flag* should have been raised . . ." (italics my own). This implies my report was reckless at best and dangerous at worse. This is an assault on my integrity and intelligence as a U.S. Army Chief Warrant Officer. One of the major responsibilities of a Chief Warrant Officer in the United States Army is keeping his/her commander out of trouble. If the report is read in detail, end-to-end, it is obvious that it is a *warning* that such experiences, like those one might have at TMI, might not be well-received by many of the Officers within the INSCOM Command structure (italics my own). A primary reason I wrote the trip report in the first place.

In fact, as one of General Stubblebine's personal advisors, I sat in my car from midnight until almost 3:00 a.m. in the Monroe Institute parking lot, talking privately with the General, warning him that, in my perception, he had gone one step too far and his career was at great risk. He listened intently and then ignored my recommendation that he cease sending INSCOM people to TMI. This quickly resulted in his early retirement at the specific request of the ACSI, at the Pentagon—the very same person who saw great value coming from the Star Gate Program and approved its initiation.

3. Jacobsen says the following regarding why people were being sent to TMI; what they were supposed to be learning: ". . . how to expand their consciousness and have an out-of-body experience [OBE]" (p. 292). Neither TMI nor I have ever said that while attending the Institute one will have an OBE. It can and does happen, but it is not guaranteed. I mentioned this in my trip report, because I had had spontaneous OBEs since my NDE in Austria, in 1970. The fact that it occurred more than once while attending the Seminar at TMI, I felt was important. The way Jacobsen says it is not the way I said it in my trip report.

4. Most of what Jacobsen says about the focus levels and TMI's

program (pp. 302–303) is not only a single person's viewpoint, but inaccurate on many levels, as well as hearsay. It would have been more informative had Jacobsen interviewed someone from the TMI staff who worked there and asked these questions directly. This is especially true regarding the final exercise.

Most of what was done in the final exercise was written by General Stubblebine, and done at his specific direction. It is not philosophically consistent with what Mr. Robert Monroe would have done. This is something I should know, since he was my close friend and father-in-law for quite a few years. Nor was this an accurate representation or portrayal of an appropriate remote viewing protocol. It does reflect what was going on with the General inside INSCOM at that time and a primary example of why the General was asked to retire early.

The General was excited by what he was experiencing and felt it would be of great benefit to those within his command. His euphoria from what he was discovering may have gotten him into trouble with his immediate supervisor, and it would have been far more accurate to have addressed that issue in an appropriate way, rather than writing about it in the sensationalistic and manipulative fashion exemplified throughout Jacobsen's book. It's true that Jacobsen is entitled to her own opinion on such matters, however it denigrates and badly distorts what was actually happening at that time. These issues could have been addressed with a single phone call, but in my opinion this would have disrupted the message Jacobsen intended to express from the very beginning.

It is also my opinion that this entire book is just one more example of slanted journalism. It is far easier to ignore the vast collection of scientific research and facts amassed over the past 45 years that support what was going on back then, than it is to spend the time and effort it would take to read the supportive material. Of course, if you take the low road as a writer, instead of a balanced appraisal, you don't have to suffer the slings and arrows of ridicule that automatically come with investigating the paranormal.

5. I've spent considerable time addressing problems pertinent only to myself within Jacobsen's book for two reasons: 1) In many cases I have no idea the number of errors there are specific to others as far as details are concerned, and 2) I do not wish to put words in the mouths of others. However, I do know there are many errors within Jacobsen's book because a significant number of them have been identified and forwarded to me. Some of these comments are itemized below, beginning with those forwarded to me by Angela Dellafiora. I've known Angela for a long time. She is a Professional Intelligence Analyst who is highly respected and valued for

her work over many decades within the walls of numerous agencies in the Washington D.C. area. Her concerns are as follows:

a. On page 304, Jacobsen says that when Ms. Dellafiora heard that the Defense Intelligence Agency was hiring young civilians with degrees in political science, she leapt at the chance. But the truth is that she heard about intern programs that the Department of Army (INSCOM) had and that they were hiring people with political science degrees. She was hired by INSCOM, not the Defense Intelligence Agency.

b. On page 306 (top of page): Ms. Dellafiora did not ask for a meeting with General Stubblebine, as is stated. She was introduced to John Alexander by a young female captain. John Alexander made the introduction to General Stubblebine. She did not ask Stubblebine for a job. She would never have done that as it was not in her nature to do such a thing. Additionally, it would have been a terrible violation of military ethics to have done so; which is obviously something Jacobsen doesn't understand about the military.

c. On page 306 (bottom of page), Jacobsen claims that Ms. Dellafiora told her that she was scheduled to go to Monroe, but that her supervisor and his colleague went instead. Once again, this is not true. Ms. Dellafiora's supervisor did not go to Monroe. Ms. Dellafiora was taken off the list twice because higher-ranking military men wanted to attend. One traveled from Hawaii to do so. The third time, she voluntarily decided not to go because she knew two of the people going (Douglas Patt and Major Finch), and she did not want to be there with them. She had worked with Patt previously (but he was sent to the front office to work) and she was still working with Major Finch. Doug Patt was a replacement that was approved solely by General Stubbleinne just prior to the bus departing Arlington Hall Station. He had not been vetted by the Staff Psychologist prior to his participation, which ended up causing a major problem for TMI as well as for the General.

d. On page 308, an error. Paul Smith called Ms. Dellafiora (in reference to her recruitment for the Star Gate Program), so her first meeting was with Smith, at Arlington Hall Station. She was not recruited at the 902nd MI Group at Fort Meade, as Jacobsen says. But Dellafiora does remember two later interviews occurring at Fort Meade. She doesn't know how Jacobsen could have mixed this up.

e. On page 315, Jacobsen states that Dellafiora told her that on January 1, 1986, Dr. Jack Vorona made administrative changes he felt were

long overdue. He converted the job of branch chief to a civilian. This is not accurate. On January 1, 1986, Angela Dellafiora wasn't yet part of the unit. Dellafiora didn't show up until July of 1986, and then Fern Gauvin entered the unit as a contributing member at the end of 1986. In 1987, Bill Ray entered the unit as branch chief, followed by Colonel William Xenakis who took his place. After Xenakis left in early 1988, fully two years later, was when Dr. Vorona made the position civilian and gave it to Fern Gauvin. There are quite a few places within the book that Jacobsen mixes dates and events. In many cases, this leads a reader to believe things which occurred for other reasons and not the reasons stated. An example would be section 4. above.

f. On page 347, Jacobsen reports that the first Higgins session was conducted at the DIAC by Angela Dellafiora, with only Paul Smith and Ed Dames present. But she neglected to include Lyn Buchanan, Mel Riley, and Fern Gauvin who were also at the session. Fern served as Angela's monitor, and Dr. Vorona was also there. Dellafiora cannot remember if Dale Graff was there or not. This might sound like a small issue, but when one is discussing things happening of significance in reference to the Star Gate Program, nothing is trivial.

g. On page 366, an error. Dellafiora wonders where the name "Jim Marrs" comes from, introduced here by Jacobsen. Jacobsen reports that he is a newspaper reporter from Texas and that Dames and Morehouse were collaborating with him to write an expose about the still-classified RV program. This is an impossibility since transcripts from David Morehouse's Court-Martial at Fort Bragg four years after his abrupt termination from the Star Gate Program state that he and Dames were collaborating with their book agent over weekends in New York. Another example of inaccurate research and reporting.

h. On pages 369–370, Jacobsen reports that the CIA ordered an evaluation of Star Gate by an outside firm after the Agency was put in charge as the unit custodian. Jacobsen doesn't report on how the Star Gate Program was moved to the CIA. She jumped from Morehouse to the CIA without any clarification. She makes these jumps in time in numerous places throughout the book. This is sloppy when compiling an historical record, and leads a reader to make wrong assumptions. In this case, a significant error which follows in section 5.i. below.

i. On page 371, Jacobsen missed a major point. She said that Angela

Dellafiora traveled to Langley (CIA Headquarters) to box up the Star Gate materials for the National Archives. Angela Dellafiora says that she did not go to Langley to do this. The Star Gate boxes were packed at Fort Meade and shipped from there. Following the formal termination of Project Star Gate, Angela received a call from a Department of Defense (DoD) policymaker who followed Special Access Programs. Angela remembers this was in the winter because it was very cold outside when they first met and went to lunch. She states: "He wanted to see the documents and asked if I would accompany him since I would know what was important and what was not." He felt that she could save him time. It was at this time they noticed the boxes had *never been opened* (italics my own).

This meant the CIA NEVER REVIEWED ANY OF THE OPERATIONAL FILES during their reported study while deciding whether to accept managerial responsibility for the project as directed by Congress. This is an astounding statement. It points to one of the greatest *disservices* ever done to the Star Gate unit, its personnel, and the American people. It further sullied the reputations of those who had addressed and established a still extant threat to the United States of America and underscores the politicization of the Central Intelligence Agency. This is something that it seems would have been far more relevant and specific to the content of Jacobsen's investigative book. It shows a serious breach in the CIA's responsibility to Congress and their directives. Jacobsen never mentions this, and yet it is one of the most critical issues in the history of the Star Gate Program. Unbelievable!

The CIA was one of the heaviest users of Star Gate intelligence for the entire 27-year period of its existence, and, while they refused in many cases to provide feedback on how accurate or inaccurate the information provided by Star Gate was, they then lied to Congress concerning the efficacy of its evaluation of the Project in order not to assume managerial responsibility for it. This was a serious insult to the government and the Administration in charge at the time. It seems that any investigative reporter would have gone after this immediately, but Jacobsen leaves it untouched. She obviously either doesn't understand the subject she is writing about or it didn't fit into her already established outline.

6. On page 167, Jacobsen comments on one of Pat Price's most effective remote viewings, and states: ". . . but the spheres were not locatable, and this agitated [the] CIA." Kress then wrote in a now declassified report: "From experience, it was obvious that Price produced bad data as well as good." And this is where her investigation of RV and Price's accuracy stops.

The following is a comment from Russell Targ: "I think the true story

of the Russian spheres is too complicated for her [Jacobsen's] purposes. The sixty-foot gores for [the] spheres were being assembled underground, just as Price drew them. But, the CIA didn't learn about them until two years later, by satellite imaging. We got our confirming information from *Aviation Week* magazine, the size, location, and all. Kit [Christopher Green] confirms it in our film, which I believe she [Annie Jacobsen] saw in a PA [Parapsychological Association] screening, since some of our CIA interview conversations appear in her book" (per Russell Targ's iPhone message, 3/23/2017, 12:12 p.m.). If she saw that screening, it's surprising she failed to correct this in her manuscript, or she purposely left it out. Since Jacobsen knew that Kit was an agent of the CIA (p. 394, *Phenomena*), again the CIA lied, and the author failed to pick up on it.

7. An additional comment from Russell Targ, specifically regarding accuracy is: "Annie also repeats the absurd conjecture that a Russian confederate might have given the crane and sphere info to Price before he started his RV. But, I had the Geo coordinates in my wallet from Ken Kress. Price didn't see them until we were in our SRI second-floor shielded room for an hour. Absolutely no opportunity for Russian inputs. Russ" (per Russell Targ's iPhone message, 3/23/2017, 12:32 p.m.).

8. On page 311 and contrary to what Jacobsen asserts, Thomas McNear, while trained in CRV by Mr. Ingo Swann, never worked as a remote viewer in the unit. He chose instead to depart for health reasons. It is common knowledge that Ed Dames was also never a remote viewer within the unit. He took over Operational management from Frederick Atwater who put in his retirement papers. These are important errors because they show further ignorance of basic facts of the Program.

This points out the single greatest deficiency of the entire book: Jacobsen interviewed Dr. Hal Puthoff and Russell Targ, the co-founders of the paranormal lab at SRI International, but their work represents something less than 23–25% of the research and known information regarding the use of psychics to collect intelligence materials. She gives a single passing reference to Dr. Edwin C. May. Dr. May joined the research team at SRI in 1976. Dr. May then became the Research Director and head of the science side of the project at SRI in 1985 and remained so through the transfer of the lab from SRI to SAIC in 1990. He was responsible for all of the science support to the U.S. Army and DIA until the project was formally closed by the CIA in November of 1995. Under his watch, nearly 70% of the research money was not only raised by him, but he was also responsible for

approximately 85% of the research accomplished in the Star Gate Project.

He and our esteemed colleague, Dr. Sonali Marwaha (who isn't mentioned at all in Jacobsen's book), have spent about 4 years organizing and polishing the 1.3 million words of science supporting Remote Viewing. This work is planned for publication and release by McFarland Publishing Company of Jefferson, North Carolina, sometime this fall (2017). All this scientific research has been replicated numerous times in many labs, peer reviewed and published in a number of respected journals. It goes without saying, that just mentioning this material would have been critical to publishing a work that speaks to the U.S. Army Remote Viewing unit history. One must ask: Why wasn't this mentioned? Again, Jacobsen chose the low road as a journalist, the sensationalist path. Meanwhile, the CIA gets away with saying it was "of no value." Critical areas an investigative reporter would question in my opinion.

There are numerous references and comments made about Dr. Jack Vorona. He is listed as someone Jacobsen either interviewed or with whom she had written correspondence. When queried by Dr. Edwin May, Jack said he was never queried by email, but she did call and ask him for an interview. He said "She was a very stubborn sort and it took me a while to convince her that I wanted no part of it" (per email with Dr. Edwin C. May, regarding his personal conversation with Dr. Jack Vorona, 3/23/2017, 12:37 p.m.). I also know Jack very well and know that he will not consent to providing a statement to anyone about the Star Gate Program for any reason. That has been his modus operandi for all the years I have known him. I respect him for this.

The following list of people interviewed had no direct knowledge of the U.S. Army Star Gate Project or any of the Army's information collection effectiveness using remote viewing: Colonel John B. Alexander, Michael Bigelow, Deepak Chopra, Dr. Eric W. Davis, Don Eyles, Dr. Brian D. Josephson, Serge Kernbach, Lawrence M. Krauss, Louis J. Matacia, Richard Allen Miller, Captain Edgar Mitchell, Dr. Garry Nolan, Dr. Alvaro Pascual-Leone, James Randi, Caleb A. Scharf, Harrison Schmitt, Stephan A. Schwartz, Angela Thompson Smith, Winston Smith, Andrea Stocco, Dan Williams, Hanna Geller, Shipi Shtrang, Ginette Matacia Lucas, Stephanie Hurkos, Murleen Ryder, Andrew Puharich, Adrienne Puthoff. Forgive my ignorance if I have left the proper title off for anyone listed above. I took these names directly from the list of interviews in *Phenomena* which listed no proper titles. A further important note: According to Jacobsen, I gave her an interview, but I am NOT listed as a source in her book. Since all of this occurred following major surgery on my spine, which I was right in the middle of recovering from when we supposedly shared this phone call,

I told her that I could not remember that we had a phone call. While I can find nothing in my records attesting to this interview, Jacobsen claims we did the interview by phone, so I will give her the benefit of the doubt. I just find it interesting that with all the names listed that do not belong there, mine apparently should be there but is not.

There are dozens of names which should be there, but for some reason are not listed, who **did** have direct access to the *Star Gate* Project. I must also state that there are more errors regarding material Jacobsen attributes to me, but most are of no real consequence in comparison with the ones I've already pointed out. Once again, it just shows a terrible sloppiness.

In Summation

I eventually finished reading the book, and there were many more errors I'm just too fatigued to pursue in this report. I would never recommend this book to anyone as an historical record, as there are too many errors and false statements. What is sad and terribly disconcerting about this, is that a reader who knows absolutely nothing about the Star Gate Project, has no way of knowing what is true and what is not. Just about any reader coming from a background of little to no knowledge will walk away after reading the book even more ignorant than they were before they started.

Finally, the way it is written belies the value of remote viewing to the number of agencies supported for more than 20 years. Her story damages the reputations of those who dedicated their efforts and significant time to explore and understand the possible threat to American security. Informal interviews with some of the most notable people quoted in the book underplay the seriousness with which the U.S. Army approached its responsibilities in chasing down and understanding this new information collection capability.

Despite the truth, Annie Jacobsen chose to produce a more sensationalized report, shot through with seriously flawed material, much of which is completely disconnected from the reality of psi collection history in the U.S. Army. In the end, she deliberately chose to sensationalize and ridicule rather than present what could and should have been a far more accurate, fair, balanced, complete, and effective historical discovery of record. It is a very poorly written book with too many errors to recommend it to anyone seriously seeking information on the Army ESP Program Star Gate.

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BOOK REVIEW

More Corrections about the Book *Phenomena*

In mid-March of 2017, colleagues on private discussion lists for scientific studies of parapsychological phenomena began discussing the forthcoming publication of Annie Jacobsen's new book, *Phenomena: The Secret History of the U. S. Government's Investigations into Extrasensory Perception and Psychokinesis* (Jacobsen 2017). Expectations were understandably high, as the jacket of *Phenomena* bills it as "The definitive history of the military's decades-long investigation into mental powers and phenomena." Knowing a lot about that important area, since I spent a year as a consultant of the Stanford Research Institute's (SRI's) original program on remote viewing, as well as having done many independent studies of parapsychological phenomena and related areas like altered states of consciousness (ASCs) and transpersonal psychology, I was very interested. But my colleagues' main comments were about important distortions of the history in the book. McMoneagle's (2017) detailed refutation and correction of *Phenomena* in this *JSE* issue (Summer 2017) is the start of many detailed articles on this.

Annie Jacobsen's name rang a bell, and I recalled she did a pleasant interview with me a few years ago, although it was primarily about my work with ASCs, rather than parapsychology. She kindly sent me a copy of *Phenomena*, though apologizing for using so little of that material and only mentioning me twice in the book.

So I began reading with great interest, but caution. She's an excellent writer. The text flows nicely and I easily get caught up in the story lines. But a "definitive history" requires more than a smooth flow, it requires rigorous factuality. So I've concentrated here on her two mentions of me and my work, and, I'm sad to say, have had to question the "definitive history" categorization.

Her first mention of me (p. 69) notes, largely in passing, my attendance at a conference on human energy fields where Andrija Puharich described some of his research. She writes "Also present at the conference were several of Puharich's former colleagues from the Round Table Foundation, including Arthur Young and Charles T. Tart." Puharich is a controversial figure in scientific parapsychological research, although I believe some of his early research was very important. Describing me as a colleague from Puharich's Round Table research is a small departure from factuality

that perhaps honors me too much, I was just a college sophomore then. Under most circumstances, I would not bother to point this out, but it's that "definitive" adjective.

I worked for Puharich as a research assistant for the summer of 1957, between my sophomore year as an MIT student and transferring to Duke University as a junior. Duke was where J. B. Rhine's laboratory was located, and I chose it because of my interests in parapsychology. On the other hand, I am the only scientific parapsychologist I know of who carried out a high quality, double-blind study of one of Puharich's basic discoveries, confirming that the electrical condition of a Faraday cage could enhance ESP ability (Tart 1988), so I became a colleague many years later.

But the second mention is more seriously distorted. I regret that the publisher (Little Brown and Company) didn't fact check the manuscript before publishing if they were going to use that word "definitive" to describe it. Jacobsen writes:

> As head of the Electro-Optic Threat Assessment section, Graff was also involved in an array of brainstorming ideas, designed to beat the MX missile basing system as part of an official Air Force vulnerability assessment team. He wondered whether remote viewers using ESP could determine which transport vehicles were carrying the real missiles and which were carrying dummy warheads. He contracted with Hal Puthoff to conduct a study. Using a computer-generated shell game, Puthoff's colleague Charles Tart of the University of California, Davis collected data from a group of psychics tasked to try to beat the shell game. Random guesses would produce a correct guess 10% of the time. On the average, remote viewers trained in SRI protocols were correct 25% of the time. One "sensitive" individual in the group produced exceptional results, Graff learned. After 50 shell game trials times, she had guessed the location of a marble with an accuracy of 80%. Hal Puthoff's report for Graff indicated that remote viewers could significantly increase the odds in determining the location of the real ICBMs. This report was sent to the Pentagon. (pp. 218–219)

Really dramatic, yes? And mostly real and very important! Very briefly described: What was going on?

The "computer-generated shell game" was not a project developed or carried out at SRI, though, nor was it done with the MX missile system in mind. Many years before, I analyzed the way ESP was commonly tested with multiple-choice guessing (Tart 1966), usually with cards, and, although it could be described as a "shell game," there were no peas, no shells, nothing was physically manipulated. It struck me that doing multiple trials without immediate feedback as to whether you were right or wrong (that would have invalidated the statistics used then by allowing some form of card

counting to inflate scores) was what was standardly called in psychology an *extinction paradigm*, a way to confuse and discourage a person, even if they had some talent to begin with, until they showed only chance results, and that's what was commonly found, a *decline effect* as it was called, in ESP studies. This decline effect provided strong evidence for the reality of ESP. People get tired, bored, confused, but chance doesn't. The positive side of my analysis was that if you used a computer-like device to randomize targets/cards, you could give immediate feedback and you would expect declines to disappear and see the start of learning. That's what I found in my later studies at the University of California at Davis, reported in detail in Tart (1976), and also in this *Journal* (Tart 2017).

The year I was consulting full time on remote viewing at SRI (1978–1979) was when they were asked to see if the MX missile system could be defeated. The basic question was that the Soviets had a certain number of (very expensive!) ICBMs, as we did, and if they launched a first strike, could they wipe out most of our missiles before we could launch and then take over the world? Neither we nor the Soviets could afford to build several times as many missiles (and there was already enough nuclear weaponry to blow up the earth several times over in those insane times!), but we could afford to build (for many billions!) a lot of silos to hide missiles in and constantly shuttle them about in a concealed way. The Soviets would not know which silos were empty, which had the missiles they wanted to destroy, we could retaliate devastatingly if they struck first, so (hopefully!) they wouldn't.

But if you had some way of knowing better, not perfectly but better, where our missiles were, maybe a Soviet first strike would be worthwhile? That was the question SRI was tasked with: Could ESP, remote viewing by the Soviets, improve their odds of winning with a first strike?

Hal Puthoff did the sophisticated mathematical analyses, using both results from SRI remote viewing studies up until that time AND the data from my ESP training studies at UC Davis. I don't know the relative weights given these two kinds of data, but I think my data were particularly frightening.

Jacobsen writes that I ". . . collected data from a group of psychics," implying specially talented people, "psychics." Maybe there weren't too many "psychics" around in the Soviet Union so there wasn't too much danger?

But my data was from ordinary college students, roughly a couple of thousand to start with, who had no thoughts of being "psychics," they were ordinary students at UC Davis who were selected by taking a very simple and quick card-guessing test at the end of one of their regular classes. Details

on this screening are provided elsewhere (Palmer, Tart, & Redington 1976). The ones who scored high were invited to take half a dozen formal ESP tests in the laboratory with one of my several student apprentices. Those who continued to score high probably had some ESP ability to begin with, and they were then each able to take part in 20 formal tests, with immediate feedback. If you could end up with even half a dozen people quite talented at ESP, at a level practical enough to indicate, with far from perfect but better-than-chance accuracy, which silos had missiles in them, finding and training "psychics" to beat the MX system looked practical.

OK, I've set the record straighter on that part that I was intimately involved with, but examination of just this particular aspect of the book has certainly alerted me to be cautious and skeptical about how "definitive" *Phenomena* is . . .

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BOOK REVIEW

Annie Jacobsen's *Phenomena*: A Commentary

Annie Jacobsen's (2017) book *Phenomena* says it is a "definitive history of the military's decades-long investigation into mental powers and phenomena." The Star Gate program (last of several nicknames) is the *only* extensive U.S. government–sponsored psi program. However, there are far too many errors in Jacobsen's work for it to be considered as definitive or even representing the Star Gate program.

While there are many books available on the Star Gate program, none of them claim to be the "definitive" work, as most are first-person perspectives of the participants in the program. So why does Jacobsen's work deserve an open critique? The faux story narrated in her work has led to a concerted effort to bring to the reader the many errors in the book. These critiques counter the claim of *Phenomena* being a "definitive" work on the SRI–SAIC Star Gate program.

But first, let me introduce myself. I am Dr. Sonali Bhatt Marwaha, research associate with the Laboratories for Fundamental Research (LFR), Palo Alto, California. The LFR inherited the psi research begun at SRI, SRI-International (1972–1990), and SAIC (1991–1995), and is actively involved in continuing the research to date. One of our current projects is archiving the Star Gate research from 1972–1995. This four-volume effort is due to be released shortly as: *The Star Gate Archives: Reports of the United States Government Sponsored PSI Program* (*1972–1995*) (May & Marwaha 2017).

As the co-editor of the *Star Gate Archives,* along with Dr. Edwin C. May, and co-author on several theoretical and experimental papers in this area, I am in a unique position of having reviewed *all* the U.S. government documents in English, originally released in 2000 and thereafter, on their support and involvement in psi research. I can confidently state that I am the *only* person so far to have reviewed the *complete* set of documents released by the CIA, including those in the possession of LFR. This provides me a unique position to comment with authority on Annie Jacobsen's *Phenomena.*

Jacobsen may claim that this book is not *only* about the Star Gate program. As she states in her prologue (p. 5, Kindle Edition)

> . . . . the U.S. government determined anomalous mental phenomena to be effective military and intelligence tools, and began to investigate their

> possible use in classified operations. This book tells the story of this postwar endeavor and its continuation into the modern era.

However, it is important to note that the post-war story of psi involves several laboratories such as the Duke University parapsychology effort, Foundation for Research on the Nature of Man (FRNM, now known as the Rhine Research Center), the American Society for Psychical Research, the Maimonides Medical Center, and later the Princeton Engineering Anomalies Research Lab, and other independent researchers. Andrija Puharich and Edgar Mitchell—the protagonists in *Phenomena*—were not the movers and shakers of the era. Several of the others who are a focus in the book were either not key or marginal players, but were just bystanders. The bystander stories are just their personal interests and not part of any psi research nor were they funded by the government.

Jacobsen states that "To research and report this book I interviewed fifty-five scientists and psychics who worked on government programs, including the core members of the original group from Stanford Research Institute and the CIA, the core group on the military side, defense scientists, former military intelligence officers and government psychics, physicists, biologists, neurophysiologists, cyberneticists, astrophysicists, a general, an admiral, a Nobel Laureate, and an Apollo astronaut" (pp. 7–8). In the section on *List of Interviews and Written Correspondence*, Jacobsen provides a list of 49 persons whom she claims to have interviewed. Of these 49, only about 13 were associated with the program at various times for limited periods and involvement. The rest were probably not even aware of the existence of the program or the details of the program. Many names seem to have been included in the list for their name-dropping value. For instance, Noble Laurate Brian Josephson although associated with the psi research field, was not associated with the program in any manner nor aware of its details; others are not even associated with psi research in any manner: astrophysicist Eric W. Davis, NASA engineer Don Eyles, theoretical physicist Lawrence M. Krauss, physicists Richard Allen Miller, and well-known new-age guru Deepak Chopra. Deepak Chopra, for instance, was not even interviewed by Jacobsen (Personal email communication, 2017, quoted with permission). This casts a grave shadow on the authenticity of this book.

A critique against the critique of *Phenomena* is that the book does not refer to the Star Gate program alone. However, Jacobsen has stated:

> The real action began in 1972, when a small group of promising young scientists was approached by the Central Intelligence Agency to embark upon a research program involving psychics, or "sensitives." The work took place at Stanford Research Institute, the second-largest Defense Department–

> funded independent research facility in the nation. (p. 5)

Jacobsen's book is *clearly* about the Star Gate program— the only sustained government-funded applied and basic psi research program (1972–1995). Nevertheless, there are several prominent members of the psi research community who have sought to support this faux investigative work, despite not being associated with the program nor being aware of the full nature of the program. Some see this work by a "Pulitzer nominated" journalist as an impetus to bringing psi research into mainstream focus and to increasing its approval ratings and therefore they support the work, even though it is riddled with errors. However, as the book reveals, Jacobsen perpetuates the myth of psi research as a fringe "woo-woo" science, and does great disservice, to put it mildly, to the science of psi, and the serious psi researchers from a variety of academic disciplines who have made substantial progress in understanding the phenomena. The book is a travesty on the life's work of several scientists involved in serious psi research.

On the impassioned recommendation of the well-known anthropologist Margaret Mead, the Parapsychological Association became an affiliate member of the American Association for the Advancement of Science in 1969. This important aspect on research parapsychology is missing in Jacobsen's work.

I quite understand the reasons behind the lay public's excitement and enthusiasm over Jacobsen's work, after all it is well written in a breezy thriller format, it has a conspiracy element of hiding from public view "what the government knows but is hiding from us" manner.

In the following, I will address the shortcomings of Jacobsen's work in the same order as they appear in the book. These will discredit Jacobsen's claim of "research" and "investigation" in the making of this unfortunate book.

Part I: The Early Days

Chapter One: The Supernatural

To begin, the term "Supernatural" is a rather outmoded term to describe psi research. That said, the existence and study of psi phenomena can be traced back to ancient times from Egypt, to India, to Greece, to the setting up of the Society for Psychical Research in London in 1882, to the establishment of the Parapsychology Laboratory at Duke University in the early 1930s by Dr. Joseph Banks Rhine—widely acknowledged as the founder of modern scientific psi research. Incidentally, in her work she refers to J. B. Rhine as "James Bank "J. B." Rhine," (p. 41).

The military use of psi can be traced back to the shamans of yore, across the ancient world. A detailed account of this can be found in Chapters 1, 2, and 3 of *ESP Wars, East and West: An Account of the Military Use of Psychic Espionage as Narrated by the Key Russian and American Players* (May et al. 2015).

Jacobsen begins her work with the Hess–Hitler–Nazi interest in psi. While some aspects of this may be appropriate in a historical context, they are in *no way* related to the U.S. government–sponsored psi research program beginning in 1972 at Stanford Research Institute, and closing at Science Applications International Corporation (SAIC) in 1995. Jacobsen's references to sorcery, hallucinogenic drugs, and mushrooms, "God's flesh," MKULTRA, were simply *not a part of the SRI program.* Thus, Chapter 1 is a total error in a book that claims to be a "definitive history of the military's decades-long investigation into mental powers and phenomena."

Chapter Two: The Puharich Theory

Aside from being one of several psi researchers of his time, Andrija Puharich was in *no way related to or associated with the Star Gate program*. Moreover, his experimental and theoretical approaches were *not* followed by the SRI team. References to his work in the SRI documents occur in the same manner as would normally happen in the literature review of any scientific work; there is *no* focus on Puharich's approach. Thus Jacobsen's extensive focus on Puharich throughout the book is a gross error and misrepresentation of the SRI–SAIC work.

The theoretical approach of the SRI–SAIC work is distinctly different from that of other laboratories engaged in psi research. It followed a physicalist, signal-based approach to the investigation and understanding of psi. Telepathy, quantum mechanics, spirituality, consciousness, astral traveling, UFO, aliens, "staring at goats" have *never* been a part of the SRI–SAIC approach.

Chapter Three: Skeptics, Charlatans, and the U.S. Army

This chapter is a total waste. As the Star Gate program was a classified program, skeptics as well as researchers from the extended psi research community were not aware of the existence and/or details of the program. SRI papers published in *Nature* and in *Proceedings of the IEEE* presented a thin slice of the research effort, devoid of any classified material, or links to the sponsors. The reviewers of the program over the years were privy to only *some* of the unclassified documents.

Chapter Four: Quasi Science

In this chapter Jacobsen continues her focus on Puharich, the Round Table Foundation, "magic mushroom," MKULTRA. As noted above, these are *not* related to the Star Gate program. The only thing this chapter does is to paint all serious psi research with the pseudoscience brush. Considering the advances psi research has made in methodology, statistics, and theory, it is a shame that this representation of the field is encouraged.

Chapter Five: The Soviet Threat

Finally, in Chapter Five, Jacobsen addresses some parts of the reasons for the U.S. government interest in psi research. However, here too she focuses on peripheral issues rather than on the issues of Soviet psychology or parapsychological research. At best, this chapter gives a breezy overview of the Soviet effort.

Part II: The CIA Years

Chapter Six: The Enigma of Uri Geller

The less said about this subject the better. And, factually, there is not much to say about Uri Geller, as he was involved in *only one* series of experiments and visited the SRI laboratory for only 6 weeks in 1972. Jacobsen has grossly erred in focusing on Geller in this supposedly "definitive" work. To reiterate, Geller *was not a participant in the 22 years of the SRI–SAIC program aside from his six-week participation.* The only part that is true is the link between Puharich and Geller, and the CIA having introduced him to Targ and Puthoff. Moreover, there is documented evidence that the CIA did *not* want any further work with him other than the six weeks of work done, as they perceived him a security threat because he was a self-promoter and hence a security nightmare for a classified program. This view of Geller has been borne out with the extensive self-promotion and grossly exaggerated role that Geller has accorded to himself vis-à-vis the program, as is evident from his numerous writings, public presentations, media appearances, all of which can be found on his website (www.urigeller.com).

Chapter Seven: The Man on the Moon

While the story of Edgar Mitchell is interesting because of his many achievements, primarily as one of the astronauts to have walked on the moon, Mitchell was *not* associated with the Star Gate program. The psi

research community is a small one, and it is inevitable that researchers and interested parties know each other. But that does *not* imply that they were participants in each other's work. As far as psi research is concerned, Edgar Mitchell's contribution is the establishment of the Institute for Noetic Sciences (IONS) in California; the approach of IONS is primarily a consciousness-based spiritual science.

This is in sharp contrast to the SRI–SAIC approach. While Jacobsen may not realize and value these differences, they are of critical importance in the trajectory of research and theory in understanding psi phenomena. The SRI–SAIC approach has led to several theoretical advances in understanding psi.

Chapter Eight: The Physicist and the Psychic

Hal Puthoff was one of the founders of the SRI psi program, along with Russell Targ—who is quite ignored in this book. He initiated the program at SRI in 1972, and resigned in 1985 to pursue his interests in theoretical physics. Russ Targ was a part of the program from 1972–1982.

As a historical narrative, it may be interesting to mention the connection between Cleve Backster, Puthoff, and Ingo Swann, as it brought Puthoff and Swann together. However, it is important to note that Backster's research interests were *not* a part of the Star Gate program. Backster's work is not even a part of psi research in general.

This chapter focuses on just the initial meeting of Puthoff and Swann, ignoring Swann's contribution and participation until 1986, when he left the program. Moreover, the extensive work of Puthoff, from 1972 to 1985, is ignored.

Chapter Nine: Skeptics versus CIA

In the narrative approach taken in *Phenomena,* this chapter may be the only one, so far, that gives some indication of the beginnings of the 22-year research program. This is a very thin slice, but an interesting one.

As in the other chapters so far, the absence of links to the notes (in the Kindle version) makes it difficult to determine the sources, hence the authenticity.

Chapter Ten: Remote Viewing

Finally, in Chapter Ten, after having traveled over unnecessary territory, Jacobsen reaches some aspects of the SRI program. Since this is mostly a he said–she said narrative, one has to take at face value what is attributed

to the various players in the chapter. By and large, it is a brief narration of events in the early years of the program.

Chapter Eleven: The Unconscious

More on Uri Geller. I don't really understand the purpose of this chapter. Considering the content of this chapter, why it is called "The Unconscious" is a mystery to me.

Chapter Twelve: Submarines

In this chapter, too, Jacobsen goes over a mish-mash of ideas that are either unrelated to the program or formed one element/experiment of the program. While Puthoff and Targ participated along with Stephan Schwartz in Project Deep Quest, much of the SRI part of the program was classified. Schwartz was not privy to this aspect of the program which essentially was addressing a theoretical aspect of the phenomena. Other key members on the team, including Dr. Edwin C. May, are not covered in Jacobsen's narrative.

By 1977, the period to which this chapter refers, the SRI program was on its way, despite several ups and downs. Some interesting research was under way, with Dr. Edwin C. May having joined the program in 1976. Several key research areas in applied remote viewing were under way, and the upper echelons of the U.S. government and intelligence communities were read into the program.

The problem with Jacobsen's writing is that she has failed to independently source her material, with the standard journalistic practice of having multiple independent sources.

On page 200 (Kindle edition), Jacobsen states:

> Graff urged his superiors to fund a classified program with the SRI scientists that would focus on remote viewing research with special emphasis on locating lost airplanes. Several months later Graff got his answer: funding had been approved. Graff could not have foreseen that his initial effort would turn into a colossal, twenty-year effort by the Defense . . .

Dale Graff, of the Foreign Technology Division, was one of the members on the Grill Flame Committee (1980–1982), and later manager of the Fort Meade division of the applied program, (1990–1993). He was *not* a scientist on the program. The main scientists on board were Hal Puthoff, Russ Targ, and Ed May, in addition to other SRI and SAIC associates of the program. There were several others up the command chain instrumental in program funding and decision making, higher than Dale Graff, with whom the SRI team was in contact with and for whom they worked.

Part III: The Defense Department Years

Chapter Thirteen: Paraphysics

The term 'paraphysics' in the title is quite a misnomer as far as the subject matter and contents of this chapter are concerned. The role of Dale Graff in the overall scheme of affairs would be akin to the role of a copyeditor in the production process; higher above is the author, the publisher, the acquisitions editor, and the editorial group. Much of this chapter is related to Graff's personal story and his interpretation of the phenomena being studied. The SRI–SAIC work does not refer to consciousness, Jung's collective unconscious and such, as does Graff's interpretation of the phenomena as narrated by Jacobsen.

In this chapter she also mentions the MX missile system and remote viewing. While Graff may have participated in brainstorming sessions for this project, the scientists behind this effort were Ed May and Hal Puthoff. They are listed as the authors on the documents associated with this effort.

As an aside, it is important to note another oversight by Jacobsen: She uses the term "anomalous mental phenomenon" from page 5 onward. However, she fails to note that this new terminology (now widely accepted) for psi phenomena was developed by Drs. Ed May, Jessica Utts, and S. J. P. Spottiswoode at SAIC. Incidentally, Professor Jessica Utts, a visiting scientist at SRI, a key member of the Blue Ribbon Panel set up by the CIA to assess the program, and President of the American Statistical Association (in 2016), is not mentioned in this book. The absence of these three is rather strange since their work related to the Star Gate program is available online, in peer reviewed literature, and reproduced/referenced in several publications.

Chapter Fourteen: Psychic Soldiers

This chapter gives the impression that the Fort Meade effort was independent of the SRI effort, when in fact it was a concerted effort under the command of the higher ups and SRI. There are several errors in the story about Joe McMoneagle, which I leave to him to address. The rest of the chapter is a dramatization of some of the sessions at Fort Meade.

Chapter Fifteen: Qigong and the Mystery of H. S. Tsien

While this chapter starts off as a review of Chinese psi investigation, Jacobsen slips back to Geller and his spoon bending efforts, with reference to strangers such as Jack Houck, who had nothing to do with the program, aside from their personal interest in psychokinesis (PK) and participation in

the psi fads of the time in California. By 1981, the period referred to in this chapter, the SRI team, including Ed May, had started several psychokinesis experiments. Eventually, they rejected the PK hypothesis, based on the Intuitive Data Sorting Model; now known as Decision Augmentation Theory, it has been applied to several databases, rejecting the PK hypothesis.

Chapter Sixteen: Killers and Kidnappers

This chapter focuses on Dale Graff's experience at Fort Meade (1990–1993), along with some examples of operational remote viewing. It is mixed with either Jacobsen's own interpretations or those of the members at Fort Meade, along with other extraneous unrelated issues.

Chapter Seventeen: Consciousness

The term "consciousness" in a book on the Star Gate program is in itself a big error. As noted earlier, the program did *not* have consciousness as a basis for understanding psi phenomena.

In the opening paragraph of this chapter Jacobsen refers to a U.S. Army publication "The New Mental Battlefield: Beam Me Up Spock" by U.S. Army Lieutenant Colonel John B. Alexander. Neither the article nor the author are related to the program in any manner. As Jacobsen states:

> Alexander was not yet part of the Grill Flame program, nor did he have access to information about any of the CIA, DIA, or Army projects involving ESP and PK, now also being called remote action (RA) and remote perturbation (RP). Alexander's article was based on personal experience and open-source information, material found in books and articles in the public domain. . . . (p. 277, Kindle Edition)

Now why Jacobsen chose to include his perspective in this work is again a mystery to me.

The space given to Bob Monroe is also unfortunate, as he was involved in only one project (nickname Gondola Wish), in an effort that did not lead to scientific validity. The salacious references to Monroe are particularly disgusting, and their validity highly suspect.

Jacobsen again slips back to Geller and James Randi, who as stated, had nothing to do with the program. Randi has not even commented on the Star Gate program elsewhere.

This entire chapter is based primarily on people and events not associated with the program.

Chapter Eighteen: Psychic Training

This chapter refers to Dale Graff and Paul Smith. Smith was one of the remote viewers at Fort Meade from September 1983 to August 1990. Although he has written extensively about his experience, they were not associated with the higher levels of the program or involved in the SRI scientific effort. The issues that the Fort Meade group talks about are just their chatting. To put it differently, in any experiment or application, the participants in the program are the least aware of the nature of the scientific problem and the issues being examined. It is like asking persons in a drug trial their understanding of the chemical components and mode of action of a drug and what the scientists are looking for.

Chapter Nineteen: The Woman with the Third Eye

Jacobsen shifts to one of the most pathetic parts of the book: her story on Angela Dellafiora. Although she has interviewed Angela, much of the writing in this chapter and the previous one is taken from the work of others, who had an axe to grind.

The references to remote viewing are described with very poor protocols that will bring the skeptical community down on psi research. Having checked the examples that are referred to here, it was apparent to me that these were trial sessions. While this may not matter to Jacobsen, it is of crucial importance to the scientific effort behind the program.

There is much richness in the operational part of the program vis-à-vis the phenomenon, which Jacobsen has entirely missed. To be able to write it up effectively, she would first need to grasp the nature of the problem, which she clearly hasn't.

Chapter Twenty: The End of an Era

In this Chapter Jacobsen shifts back to Ed Mitchell and Uri Geller. Why? She also brings up Randi again. Why?

On pages 337–338, Jacobsen states:

> Declassified documents reveal that in the winter of 1987, Dames tasked remote viewers to dozens of sites of celebrated UFO encounters and alien abductions. Paul Smith reports that many in the unit were "fed up with Ed Dames' shenanigans and chafed at his parade of extraterrestrial targets," but official documents indicate that his folly seems to have had a Pied Piper effect on others in the unit, with many viewers following his lead. This is evident in hundreds of pages of declassified operations logs.

Why pay attention to what some low-level participants did in their spare time, when there is so much richness in the real effort? Having personally gone through the operations data logs, I don't find these items.

Chapter Twenty One: Hostages and Drugs

This chapter again refers to participants such as Ed Dames, Paul Smith, and David Morehouse. While some operational remote viewings are narrated, the real story is hidden in a few paragraphs.

Chapter Twenty Two: Downfall

The chapter begins with 1991, when the project shifted from SRI to Science Applications International Corporation (SAIC). That is a story to tell, which has been completely missed in favor of Ed Dames and Morehouse. Moreover, she ends the chapter with Puharich again.

Part IV: The Modern Era

Chapter Twenty Three: Intuition, Premonition, and Synthetic Telepathy

This chapter is a sorry representation of the Star Gate program. The data from this program has led to several advances in the field of psi research which Jacobsen has no clue about.

Chapters Twenty Four: The Scientists and the Skeptics

Here she again focuses on fringe aspects, including areas that are not related to the phenomena under study.

Chapter Twenty Five: The Psychic and the Skeptic

Uri Geller and Edgar Mitchell resurface in the final chapter.

This is a very poorly researched book, even in comparison with the wiki page on the Star Gate Project, which is largely inaccurate. This does not speak well for a Pulitzer nominated "investigative" reporter.

There is a fine story to tell about this 23-year program which the author has missed in its entirety. She has based her "definitive" work on a few people who say they were "there" or who are otherwise interesting characters to throw into a book, rather than on those intimately involved with and leading the program.

The number of inaccuracies in this book casts a grave shadow over her other works. A reading of the critical reviews by experts of her other books

clearly shows that Jacobsen's investigative skills are limited, and that she has made a habit of misrepresenting the life's work of serious researchers.

Some of the glaring omissions in this book include:

(1) ignoring the science behind the program—the successes, failures, and limitations of psi,
(2) not including an account of the program at SRI and later at SAIC,
(3) not taking into account, or even mentioning, Dr. Edwin C. May (SRI/SAIC 1976–1995, program director 1986–1995) and others, who were behind the bulk of the scientific research. Ironically, as mentioned earlier, she consistently uses the term "anomalous mental phenomena," but fails to mention those responsible for this terminology (Ed May, Jessica Utts, James Spottiswoode). Moreover, May was one of three people interviewed on the *Nightline* program in 1995—with former CIA director Robert Gates, and a CIA operative only identified as 'Norm.'

These points are enough to disqualify Jacobsen's work as a well-researched, "definitive work" on a program that has led to several advances in understanding precognition/remote viewing and micro-PK. Briefly, the Star Gate program—applied and basic research—concluded that psi is an inherent ability and cannot be developed, remote viewing is real and can be applied, and the evidence for psychokinesis is statistically weak. The program has led to several testable theories, some of which have been and are being put to the test, and this is a science in progress.

Sonali Bhatt Marwaha
Laboratories for Fundamental Research

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BOOK REVIEW

Letter to the Publisher of *Phenomena*

Editor's Note: The following is a letter Stephen Schwartz sent to the publisher of ***Phenomena****, prior to the book's release, after being sent a galley of the section of the manuscript in which his work was represented. The letter itself explains the context.*

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18 November 2016

Little Brown
Market Place Center
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enclosures

RE: PHENOMENA

Let me begin by telling you that I had hoped I would not have to write this letter, but I have no choice because as a scientist and historian I place accuracy, both factual and contextual, as a first priority.

Some months ago I had a brief telephone interview with one of your authors, Annie Jacobsen. I heard nothing further until I received an email on 16 November attached to which was a selection of pages for the upcoming book *Phenomena* covering my research with the admonishment my edits must be in her hands by "Sunday/Monday"—basically three days in the future. Since then I have exchanged 13 emails, and had three quite acrimonious telephone conversations, all very hurried because Ms. Jacobsen couldn't talk at length.

The first thing I noticed when I read pages 191–199 (which I enclose) that covered my research was that the narrative of what happened bore very little tangency to real events, or real context. Ms. Jacobsen made it explicitly clear that she was not interested "in your interpretation of events, only specific corrections." In response I pointed out multiple name misspellings, as well as incorrect institutional affiliations.

The funny thing about all of this was that her pages were describing events that occurred 40 years ago about which much has already been published. Indeed, Little Brown published my chapter on this entire event

in 1984, in *Stories From Omni*. I also wrote about these experiments in my book *Opening to the Infinite*, as well as in numerous research papers presented at science conferences in several disciplines. A high school sophomore in five minutes of Googling could have obtained the correct information. Apparently Ms. Jacobsen couldn't be bothered to do any of that. I will simply offer you one example. On page 199 she says, "This was the first time in history that psychics had located an archaeological site."

In fact in 1978 Grosset & Dunlap published my book, *The Secret Vaults of Time*, which recounted multiple uses of remote viewing in archaeology over the previous century prior to my doing the experiment described in *Phenomena*. The book is still in print and can be found on Amazon. At least a dozen other books, and several dozen papers, reports, and articles over the past 40 years have also cited this research and discussed it at length. Searching Google for "remote viewing and archaeology" produces 187,000 "hits" mostly relating to my work but also much else. How could one miss that? When I challenged Ms. Jacobsen, her response was to cite cyrons on a television program, and her interview with a man named Dale Graff who played no role of which I am or was aware in Project Deep Quest, and a once secret (now apparently declassified) document in the CIA files that he apparently wrote.

Ms. Jacobsen in pages 191–199 presents a narrative of a joint project carried out by SRI and myself, largely designed by Graff, and filmed by Leonard Nimoy for the old television show . . . *In Search Of*. In fact not one word of that [narrative] is correct.

I enclose the first paper on this study which I wrote in 1977, a few weeks after Project Deep Quest was carried out. I hate to ask you to read something, but if you will at least scan this, and read pages 191–199, and compare them you will see what I mean by tone and context. Let me add that I still retain all audio tapes, footage, letters, contracts, memoranda, field notes, and cancelled checks on the Deep Quest project in my files, and had I been contacted and asked about any of it, I would happily have provided the material, as I have frequently done in [the] past with many other journalists. A Google search on "Deep Quest and remote viewing" will produce hundreds of hits.

Ms. Jacobsen describes the project as having been funded in part by the Air Force. This is factually incorrect. I funded the project, along with the Institute for Marine and Coastal Studies of the University of Southern California, and the Canadian deep ocean technology company Hyco, Ltd. She describes part of the experiment as classified, in fact nothing I know about the experiment was classified.

Ms. Jacobsen, based on what she has written appears—I do not know

this to be correct, but her account suggests it—that SRI had lost their funding and used my experiment, claiming it in part as their own, and wrote a classified report to get new funding to save their program. Having spent a number of years as part of a senior government staff dealing with mostly classified material I am familiar with this ploy, and understand that they had no fear that any of this would be contradicted because it was classified and unknown to me or anyone else outside of that hermetically sealed world.

In this letter I have intentionally focused only on the part of *Phenomena* that covers my work, but I know from personal experience and direct first-hand accounts that the rest of the book is as inaccurate in both narrative and factual details as the part addressing my work. Most egregious of all, in my view, *Phenomena* does not even mention the contribution to Deep Quest of physicist Edwin May who joined the program in the 1970s, and was its director from 1985 until its end, and whose original and innovative work is responsible for much of the SRI program's reputation. Ms. Jacobsen also doesn't seem to mention that when the program ended at SRI, Dr. May was able to find it a new home at SAIC.

I have great regard for Little Brown. But I know every time a writer or editor lets something inaccurate slide the chances are it will be canonized through repetition. So I hope Ms. Jacobsen's manuscript can be corrected before publication.

Stephan A. Schwartz

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BOOK REVIEW

Letter to the Editor and Management Team of *Phenomena*

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21 April 2017

Review of Ms. Annie Jacobsen's book *Phenomena*

Please find attached my credentials for providing this review.

I originally began to provide a line-by-line critique; however, I found that I was rewriting a book that was inaccurate, self-serving, and a legally risky example of incompetent investigative reporting. One tell-tale indication of this is the lack of fact checking and providing independent second sources. Clearly this is devastating in the intelligence community and by implication undermines all investigative reporting.

Ms. Jacobsen has surrounded herself with excellent representation given the media exposure she has enjoyed. The book and this media frenzy continues to misrepresent not only the hard work and science conducted by the Star Gate team, but devalues the excellent operations carried out over the years.

A few examples of the most egregious errors follow:

• On page (Hardback) 492, Jacobsen claims she interviewed or otherwise corresponded with Dr. Jack Vorona, former Deputy Director of Science and Technical Intelligence for the Defense Intelligence Agency. I contacted him to verify this assertion. I was told in quite unambiguous language that he refused an interview despite Jacobsen's continued aggression. In my view, this is not an oversight or a typo—it is, in fact, an outright lie.

• Jacobsen devotes Chapter Two to "The Puharich Theory," but he was an infrequent bystander in the very early days of the SRI program in psi research. Neither his story about drugs nor any other aspect about him was involved in the government's effort whatsoever. The life and work of Puharich is a continuous theme throughout the book.

• Jacobsen devotes Chapter Six to "The Enigma of Uri Geller," a constant theme throughout the book. Geller was involved in only a short series of experiments lasting six weeks out of a 23-year long project. This

early SRI work was reported in the prestigious journal *Nature*—Targ, R. and Puthoff, H. E. (1974). Information transmission under conditions of sensory shielding. *Nature*, **252**, 602–607. This landmark publication in the history of ESP research and the government's involvement was ignored. Moreover, it is clearly stated on page 604 of the *Nature* paper:

> It has been widely reported that Geller has demonstrated the ability to bend metal by paranormal means. Although metal bending by Geller has been observed in our laboratory, we have not been able to combine such observations with adequately controlled experiments to obtain data sufficient to support the paranormal hypothesis. [Emphasis added]

Yet, Jacobsen devotes 15 separate pages to the general topic of metal bending which was hardly ever addressed in the remaining 20+ years of activity.

• There is substantial discussion about Edgar Mitchell in the book, which is totally unwarranted as he did not play any role in the history or activity of the U.S. Government's interest in psychic phenomena.

• James Randi is another name prominently displayed in 22 separate places implying active influence and/or valued critique of the program. The problem is he never appeared on site at SRI, nor was otherwise involved with the program.

• The defense contractor, Science Applications International Corporation, which provided a home for the Star Gate program for about five years (1991–1995) and accounted for approximately 25% of its total funding is totally absent from Jacobsen's writing.

• There are several other people and paranormal phenomena mentioned throughout the book which have not been part of the research program at SRI, SRI International, and SAIC during any stage of the 23-year research effort.

• Seemingly trivial, Jacobsen gets the name wrong of someone she correctly identifies as the "Father of modern American ESP experiments." His proper name is Joseph Banks Rhine not James Bank Rhine (page 41). I reported this lack of attention to detail to Sally Rhine Feather, his daughter, who posted a negative comment on Amazon describing this insulting error.

Conclusion: This brief collection of egregious errors clearly disqualifies *Phenomena* as being a definitive story of anything. It focuses on persons, topics, and theoretical assumptions that were not part of the program. Unfortunately, this book should appear more on a fiction shelf than a non-fiction story about a fascinating 23-year activity in applying and understanding psychic phenomena to problems of National Security.

Edwin C. May

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BOOK REVIEW

Mirrors and Mazes by Howard Thomas Brady. CreateSpace, 2016. 175 pp. $17.10 (paperback). ISBN 978-1522814689.

This book raises a number of the points that demonstrate flaws and downright errors in the theory that human-generated carbon dioxide is the chief driver of global warming and climate change. The author has a respectable academic record; he worked and published on the geological and climatic history of the Ross Sea and McMurdo Sound regions of Antarctica, using microscopic fossils as clues.

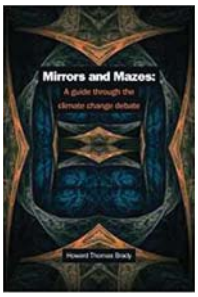
For most of Earth's history, global temperatures were higher than now by several degrees Centigrade while animal as well as plant life flourished. There has been much more carbon dioxide in the atmosphere than now during lengthy periods when global temperatures were much lower, including in some Ice Ages. Moreover, during roughly half of the last 150 years, temperatures were not rising while carbon dioxide levels were increasing.

A common assertion in the mass media and by climate catastrophists is that global warming has already resulted in more frequent and more extreme storms and the like. But the actual data show that extreme weather events have *not* increased in recent decades; not Atlantic storms, nor Australian cyclones, nor U.S. tornadoes, nor "global tropical cyclone accumulated energy," nor extremely dry periods in the USA, in the last 150 years during which atmospheric carbon dioxide increased by 40% (pp. 46–51).

Nor have sea levels been rising in any unusual manner (Chapter 6). An important point here is that the best data come from local gauges, not from measurements made from satellites, which incorporate several assumptions and inevitable uncertainties.

The climate-change alarmists call wolf also over melting glaciers and disappearing polar ice sheets. They ignore historical facts and cherry-pick contemporary evidence. Thus while the extent of Arctic pack ice has indeed been declining fairly steadily for the last three or four decades, the Antarctic has gained in extent by roughly the same amount (p. 71; cited as from "US National Sea Ice & Data Centre," but actually the National Snow & Ice Data Center[1]).

Alarmist warnings assert that recent changes are unprecedented in scale and rapidity, but this is simply not true. Antarctic ice cores reveal greater



changes in the 18th and 19th centuries than any since 1950 (p. 81[2]).

Climate is controlled by a number of natural cycles, whose periods range from decades through hundreds of years to tens of thousands of years and even longer: cycles of sun-spot activity, of wobbles of the Earth, of movement of the solar system through the Milky Way, and many more.[3] Climate is determined by an exceedingly complex system comprising innumerable interacting sub-systems: large-scale movements and regional currents in atmosphere and oceans, input of energy from the Sun, effects of clouds and greenhouse gases (water vapor being the predominant one by far), and much more. Some cycles suggest that the next century or so will see overall cooling rather than global warming.

Chapter 9 lays out the complexities of the greenhouse effect, which is quite different from the simplistic notion that it is all about carbon dioxide absorbing heat; water vapor is actually the major absorber of infrared radiation, i.e. heat. Perhaps the worst flaw in the computer models is the set of assumptions about how heat absorbed by carbon dioxide influences overall temperature: "There is empirical evidence that the equilibrium climate sensitivity index used by the IPCC [International Panel on Climate Change] for the past 30 years is far too high" (p. 93).

The notion that any computer model could accurately describe and predict the outcome of all these complexities is absurd.[4] Chapter 10 explains this in detail. Any model has to divide the atmosphere into bits ("cells"). In each cell all the influences have to be taken into account; *and* global conclusions require also taking into account the interactions between cells, which vary in both horizontal and vertical directions. So it is no surprise that all the models have been demonstrably wrong, predicting continually increasing temperatures while in reality there has been a pause, hiatus, or plateau of temperature since about the turn of the century (p. 99). Furthermore, the models predict a hot zone in the mid-troposphere that does not in fact exist (p. 105).

The disconnect between evidence and the alarmist propaganda has led a number of insiders to leave the ranks of "mainstream" climate science, for example Hans von Storch, professor of meteorology at the University of Hamburg and a leading author of the official 2001 IPCC Assessment Report (p. 41); a number of resignations from IPCC by prominent climate scientists are listed at pp. 150–151.

A paragraph on p. 157 refers to the harassment and persecution of researchers who question the primary role of human-generated carbon

dioxide in climate change. For readers not already familiar with those many stories, this mention is too skimpy and lacks references for further reading. It does mention, though (albeit again without source citation), the particularly egregious occasion when people holding positions as scientists petitioned the government to bring charges of conspiracy against those who disagreed with them.[5]

The material presented in this book seems quite sound, but the book suffers from the typical deficiencies of self-publication: typos; other glitches, for instance the lack of fact-checking that allows a National Snow & Ice Data Center to be cited as National Sea Ice & Data Centre; most seriously, an index is lacking, which for a book of this kind is surely unforgivable. And the Selected Bibliography is inadequate, missing for example the books by Fred Singer. A much more comprehensive bibliography is given in Ian Plimer's *Heaven and Earth*,[6] which is cited in the Selected Bibliography.

Notes

[1] The Arctic and Antarctic data are shown at http://nsidc.org/data/seaice_index\

[2] Citing (without page numbers) Thomas et al., A 308 year record of climate variability in West Antarctica, *Geophysical Research Letters*, *40*.

[3] David Dilley, *Natural Climate Pulse: Global Warming—Global Cooling—Carbon Dioxide.* http://media.wix.com/ugd/857cde_4e48a92c95df433ba869069b1dbcee7d.pdf

[4] For a more general discussion of the inability of computer models to handle genuinely complex systems, see Orrin H. Pilkey & Linda Pilkey-Jarvis, *Useless Arithmetic: Why Environmental Scientists Can't Predict the Future*, Columbia University Press, 2007.

[5] Letter to President Obama, Attorney General Lynch, and OSTP Director Holdren, 1 September 2015. http://scienceblogs.com/gregladen/2015/09/19/letter-to-president-obama-investigate-deniers-under-rico

[6] Ian Plimer, *Heaven and Earth. Global Warming: The Missing Science*, Connor Court Publishing, 2009.

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BOOK REVIEW

Prometheus and Atlas by Jason Reza Jorjani. Arktos Media, 2016. 468 pp. $36.59 (paperback). ISBN 978-1910524619.

A few years ago, I received a manuscript for *Prometheus and Atlas* from Jason Reza Jorjani, who had then recently received his doctoral degree in philosophy from Stony Brook University. Jorjani particularly wanted me to review his treatment of my book *The PK Man* in his final chapter. I was happy to do so; for while *The PK Man* has received many positive reviews, it was rare that another scholar would endeavor to incorporate my decade-long, field research project into their own body of work.

As I read through Jorjani's manuscript, I was consistently pleased with the scope of his scholarship and the brilliance of his insights. I found that the treatment of my work was both fair and accurate. I was nearly overwhelmed by both the breadth and depth of Jorjani's erudition. I sent him several emails with positive feedback on his manuscript. These were eventually collated into the following blurb—and, today, I still stand behind these comments:

> *Prometheus and Atlas* is the most brilliant treatise related to parapsychological material that I have ever encountered . . . it is also a very serious exploration of depth psychology and mythology. Jorjani's emphasis on what he terms "the spectral" affords us an opportunity to expand some of our existing models concerning psi. . . . Jorjani has written the definitive book regarding the proper place of psi phenomena in the history of philosophical ideas . . . . However, *Prometheus and Atlas* takes the argument much further and demonstrates that parapsychology and psi phenomena can be viewed, not only within the history of philosophy, but in the larger context of cultural history itself. Jorjani examines the mechanistic worldview [that] dominates science and has led to the marginalization of parapsychology (as well as many other cultural imbalances). The range of scholarship required to make this argument is, in my estimation, nothing short of awesome. . . . I don't think any other writer comes even close to tying things together the way Jorjani has done. The experience of reading it is rather like gazing out at a brilliant starry sky, with many interrelated constellations, stars, and planets. Each is beautiful and unique and, together, one senses a whole cosmos.

Another voice of praise for this book comes from Jeffrey J. Kripal, Chair of the Department of Religious Studies at Rice University, who served as an

outside faculty member on Jorjani's dissertation committee. Kripal wrote:

> Jason Jorjani's *Prometheus and Atlas* is what profound philosophical writing used to be but has long refused to be: visionary in its method and content, sweeping in its scope, literally mythical, and above all, positive. That is a gross understatement, though. His notions of the paranormal as normal, of a coming spectral revolution, of a future spectral technology, and of a still unrealized but very real superhuman potential come together to form a coherent but still emerging worldview that is neither modern nor postmodern but something other and more.

Because of my early enthusiasm for Jorjani's manuscript, I nominated the book for the 2016 Book Award from the Parapsychological Association. *Prometheus and Atlas* received that award from a panel of independent judges.

I am aware of few books that have endeavored to place parapsychology within a global philosophical and cultural context. So, while many of Jorjani's brilliant and penetrating insights may be muddled, or even wrong, there are almost no other scholars with whom his work can be meaningfully compared. Perhaps the only comparable philosopher and writer has been Colin Wilson.

I am far from qualified to provide a scholarly analysis of the myriad interwoven arguments in *Prometheus and Atlas*. But at minimum I can say that until there are more serious efforts within the genre of cultural critique from a parapsychological perspective it will be hard to evaluate Jorjani's work. Furthermore, since Jorjani is the first academic philosopher within this genre, *Prometheus & Atlas* is, for now, the standard by which all other comers can be evaluated. But, the project undertaken here, to provide a relatively complete philosophical and cultural account of the paranormal, strikes me as essential if parapsychology is ever to achieve its revolutionary potential for future human development.

Prometheus and Atlas takes its departure from Martin Heidegger's prophecy of a return of the gods. As such, it is clear that he is writing in the tradition of European, continental philosophy that goes back to Nietzsche. This is a tradition that resorts to poetic metaphor and is, I gather, rather antithetical to the analytic style of American philosophy. Suffice it to say that this style of writing has both strengths and weaknesses. On the positive side, it lends itself to a far-reaching penetration of thought. On the negative side, there is an inevitable tendency to over-reach. Also, one must be mindful that—for all his acclaim as a leading, twentieth-century, European philosopher—Heidegger remains a controversial figure as a result of his German National Socialist affiliation.

Jorjani, in fact, does not back away from this controversy. He makes a point of noting that—at the end of his life—Heidegger affirmed that German National Socialism, for all its many grievous faults, "represented the most profound reckoning hitherto with 'the situation of man in the world of planetary technology . . . '" Why is this important? Because, Jorjani agrees with Heidegger that technology itself is a spectral agency that acts upon the world through demonic possession.

This is a provocative idea. We normally think of demonic possession as a rare event to which only particular, unfortunate individuals are subject. Here, Jorjani proposes that entire cultures are subject to a form of possession of which they are almost completely unaware. When one observes the absurdity and madness of many aspects of the human project (in spite of the age-old, selfless wisdom traditions), the notion finds some resonance. I find that it is also akin to the idea presented by Charles Musès to the American Anthropological Association meeting in San Francisco in 1975 that all of human acculturation is a hypnotic process.

Jorjani maintains that possession by this demonic agency (whom Jorjani also equates with the Greek Titan, Prometheus, as well as the apocryphal figure, Lucifer) leads humans to project the mechanistic principles of technology on to nature herself. This distorted view of nature leads to many horrible consequences. Jorjani states that it "diabolically uproots man and renders him homeless in any and every land in which modern technology essentially takes root." I agree with Jorjani that a mechanistic picture of nature is woefully inadequate, and is largely responsible for the mainstream rejection of the important psi research data. However, he clearly goes so much further that, in my opinion his argument has become one-sided. After all, rationalistic and mechanistic thinking has also freed humankind from the perverse cruelty of many primitive superstitions. His bias here, as I see it, seems to reflect his felt necessity to have enemies against whom he can struggle. This, perhaps, is the major weakness of an otherwise wonderful book.

Another of Jorjani's key ideas is that humans, like all other life forms, battle one another to maintain access to that which is of vital concern. Jorjani sees this struggle as primary and claims that there is no objective Nature "prior to, or outside of, this historical struggle." This is a controversial idea and it strikes me as muddled, and actually unnecessary to Jorjani's main thesis as I see it (to which we shall come). It is, however, consistent with the main claim of post-modernism itself that, in effect, all our concepts of reality are socially constructed and there is no such thing as an objective truth apart from social reality. Of course, this notion is inconsistent with the thinking of the Seventeenth and Eighteenth century philosophers of

the European Enlightenment—who argued that Nature obeyed rational principles. It is also inconsistent with the thinking of Platonists who believe that mathematical and geometrical abstractions have an eternal existence, prior to and outside of our historical dramas.

I do not think it is logically possible to resolve these disputes among metaphysical postulates. In fact, by invoking the notion of the "spectral" as a core idea throughout his book, Jorjani is, in spite of himself, clearly imputing certain "irrational" propensities and attributes to Nature.

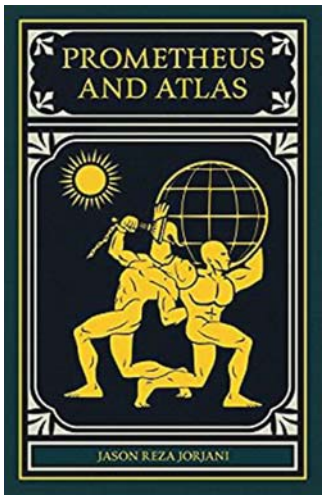
Nevertheless, Jorjani's description of this battle for survival strikes a resonant chord. After all, nature feeds upon itself. Every form of natural life requires the subjugation and digestion of other life forms for its survival. The only possible exceptions to this principle are the most primitive lichens, and perhaps the earliest life forms that survived only by ingesting minerals.

However, once again, I find this idea one-sided and incomplete. There is much to say about cooperation and symbiosis in nature. A worldview based upon the core notion of psychic battles is, in my view, narrow and unhealthy. Jorjani pushes the argument even further, maintaining that—in the struggle of ideas—"the spectral essence of Technology has a unique power to assimilate all others."

The opening chapter of *Prometheus and Atlas* focuses on scientific research in parapsychology. Wisely, Jorjani notes how many of our vital interests (such as the right to privacy) are threatened by a mainstream acknowledgment of the paranormal. He also makes the interesting observation in the second chapter that "mainstream scientific recognition of the paranormal could in itself amplify manifestations of it." I am in full agreement with these points. There are some who will maintain that Jorjani's review of this material is too uncritical and too prone to assume that greater magnitudes of psi are possible than are generally found in the laboratory. Yet, my own field research, and that of others, frequently exposes levels of psi—and psychokinesis in particular—that exceed laboratory observations.

Chapters III–VI provide the reader with a detailed history of philosophical encounters with various aspects of the paranormal, the spectral, and the irrational. He particularly covers the thinking of Descartes and Kant. But, this grand tour of philosophy also includes Schelling, Bergson, Heidegger, and William James. The exposition is fascinating and well worth reading. At the end of this journey, Jorjani concludes that all philosophical model-building concerning the paranormal "covers over or filters out certain 'irrational' aspects of Nature."

Chapter VII makes the important claim that "our task is to become consciously aware of our hitherto unconscious and unique historical relationship to the world-colonizing essence of Technology . . ." Who



could disagree? In my view, the over-arching project of humanity is to achieve greater consciousness of unconscious processes.

In Chapters VIII and IX, Jorjani explores the aesthetic ideas of Prometheus and Atlas. It is important to notes that, as Greek Titans, they were locked into conflict with the Olympian deities, and Zeus, in particular. By elevating these mythical figures as the tutelary deities of a new age, Jorjani is implicitly invoking teachings of the Iranian mystic, Zarathustra, who elevated an "ashura" or demon to the status of the highest deity—Ahura Mazda—the bringer of light. This is a deep idea, as it entails rebellion against the highest god of one's age.

Just as Hesiod portrays the Olympian gods overthrowing the era of the Titans, Jorjani is arguing for rebellion against the autocratic rule of the father-god, Zeus. Similarly, he argues that Jahweh, the father deity of the Abrahamic traditions, should also be overthrown, as his demand for absolute obedience is detrimental to humanity. Lucifer/Prometheus should, instead, be restored to his proper role as the god of light (just as Zoroaster/Zarathustra restored the "demon" Ahura Mazda).

By invoking Atlas, Jorjani is suggesting the necessity for a new world order that will, indeed, be worldwide and unified. In effect, technology and commerce have already been achieving this end—but doing so destructively in a manner that uproots traditional cultures. He sees the potential for a new, global civilization that will be less destructive and harmful—one that will be, in the words of the French writer Guillaume Faye, "archeo-futuristic"—free from the demonic possession by technology, and thereby able to consciously incorporate the parapsychological into a new understanding that is both highly technical and yet appreciative of the irrational in nature.

In Chapter IX, Jorjani also explores the legend of Atlantis and emphasizes his willingness to assume that it represents traces of an authentic, prehistorical civilization that achieved many advances and that passed its knowledge on to the ancient civilizations in Egypt, the Americas, and elsewhere. This proposition is highly speculative and likely incomplete or incorrect. One might as well cite the legend that the arts of civilization originated with a fishlike being, as was claimed by the ancient Sumerians according to the historian Berosus. These tales regarding the earliest history of humanity are fascinating. However tantalizing, they are far too tenuous

at this point in time to serve as the foundational ideas of a new human era.

Chapter X contains an exegesis of the relationship between the earliest fragments of Western philosophy (i.e. Heraclitus) and the comparable writings of the Chinese sage Lao Tzu. Jorjani, however, is highly critical of the "dangerous political naïveté" of the Taoists. So, his attention shifts to Japan as a unique culture, largely free from the stranglehold that he believes the revealed religions in the Abrahamic lineage have upon humanity. He also points out that in contemporary Japanese anime, "visionary artists have best crystallized transformative images of the coming metamorphosis of the merely human being into a more diabolically daring and dynamic superhuman race, destined to liberate a capriciously ruled cosmos and conquer the inner space of latent psychic powers." Here I disagree. I think that visionary artists in the West, such as Alejandro Jodorowsky, are producing work comparable to the best Japanese anime.

Ironically, Jorjani's focus on both Atlantis and on Japan suggest a thread in his thinking that is reflected in his political activities (for which he has achieved some notoriety). His writings suggest an inclination toward Aryan supremacy. His interest in Japanese Buddhism is, in part, due to his understanding that Mahayana Buddhism was influenced by the Persian (Aryan) Zoroastrian tradition. While I am fascinated by Jorjani's scholarship in this area, I find these hints of Aryan supremacy to be unnecessary, unwarranted, and potentially damaging to his larger and more important points (if not to the entire field of parapsychology). I also find them to be inconsistent with the humanitarian spirit that pervades his work. However, I support his celebration of his Aryan heritage and, in particular, his elucidation of how the ancient Persians influenced Greek philosophy.

Chapter XI reintroduces the philosophy of William James, with an emphasis on his pragmatism and on his radical empiricism. This is counterbalanced by a discussion of the Abrahamic religious traditions. Here Jorjani makes the daring move of suggesting that certain biblical accounts entail an encounter with a homicidal, spectral being who demands absolute obedience. He notes the striking similarity between various biblical accounts and reports in more recent times of UFOs. While I find this chapter to be both visionary and important, his treatment of the Abrahamic traditions is rather one-dimensional. There is much more to be said about the teachings and laws of Jahweh/Allah than merely the demand for obedience. In fact, as I view the Jewish tradition, the primary requirement is not to "obey" but to lovingly "wrestle" with god as did the patriarch Jacob, who wrestled with an angel all night long and was then given the name Israel.

Jorjani's final chapter introduces the important work of Jacques Vallee, linking the fields of parapsychology and UFOlogy. It is here that he also

draws upon my decade-long investigation of "The PK Man," Ted Owens, who demonstrated that he could produce UFO sightings. Jorjani cites Ted Owens himself, who equated his psychokinetic demonstrations with the feats of the Biblical Moses. Here Jorjani introduces the notion of "Mercurial hermeneutics," suggesting that the trickster god, Mercury, is the spectral agency behind both contemporary UFO appearances and Biblical accounts of Yahweh.

The importance of this final chapter is in its emphasis on our need to digest and come to understand material of high strangeness. Jorjani's analysis may be muddled and even wrong-headed, but it is an honest effort to come to terms with some of the most bizarre findings in the paranormal arena. For this alone, I highly recommend this book—even though the Mercury = Yahweh equation strikes me as a rough heuristic at best.

Jorjani concludes his epic by referencing William James' essay on ethics, wherein James invokes the necessity of a deity to inspire humans to shift into a strenuous mood. He brings up James' shamanic reference to the "alpine eagle" who calls us to a higher destiny. He then suggests that Prometheus and Atlas are the "finite gods" whose "infinite demands" can lead us to greatness, once we liberate ourselves from demonic possession by them and then join them in rebellion against the "one true god" of the Abrahamic faiths. In my view, this partial conclusion, requiring an enemy, epitomizes a weakness of Jorjani's otherwise magnificent book.

After all, the alpine eagle (a metaphor Jorjani favors) was also, in Greek legend, the torturer of Prometheus and, thus, "the minister" of Zeus. Perhaps, Prometheus and his torturer are like Yin and Yang. They are incomplete without each other.

To Jorjani's credit, he is a scholar with a subtle and sophisticated mind who enjoys exploring ideas that are antitheses of each other. His thinking may be more Taoist than even he suspects. In his concluding paragraph, he acknowledges that Prometheus and Atlas are postulates he has evoked in order to summon the strenuous mood that he believes is now required of humanity. Jorjani's most important concluding thought is that William James, through his shamanic metaphor of the alpine eagle, is echoing the Nietzschean call to develop a non-mechanistic science of the future and to cultivate the superhumans of tomorrow.

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BOOK REVIEW

Taking the Back off the Watch: A Personal Memoir (*Astrophysics and Space Science Library Series Book 381*) by Thomas Gold, edited by Simon Mitton. Berlin/Heidelberg: Springer, 2012. 234 pp. ISBN 978-3642275876.

Early in my career at Stanford University, in the course of a conversation with Leonard Schiff (then the Department Chair of the Physics Department), I asked him what he considered to be the most important characteristic of a successful scientist. He replied "strength of character." Thomas Gold (always known as Tommy) had that characteristic in spades. He was not a physicist, nor a biologist, nor a geologist, nor a space scientist, nor an astrophysicist, nor a cosmologist—he was all of the above.

My first encounter with Tommy was in 1953, at a conference on "Gas Dynamics of Cosmic Clouds" in Cambridge, UK. There was discussion about geomagnetic storms, and their various components—the Sudden Commencement, the Main Phase, etc. No one was offering any convincing theoretical interpretation of these phases, when up spoke someone with a clear and confident voice. He argued that the only way to understand how the "sudden commencement," with a timescale of minutes, could be initiated by a solar flare that had occurred perhaps a day earlier, was to attribute the sudden commencement to a shock wave that had traveled ahead of the material ejected by the flare (material that would subsequently initiate the main phase). He then went on to point out that it could not be a conventional hydrodynamic shock, because the mean-free path of the atoms, electrons, and ions was far too long to lead to a shock wave duration of just minutes. It had to involve the interaction of the electrically conducting gas with a magnetic field. Once stated, the interpretation was obvious! But it took someone with the intelligence, curiosity—and strength of character—of Tommy Gold to see it.

Some years later I was at a conference at the Villa Monastero at Varenna, Italy, on Lake Como. I had by then heard of Tommy's athleticism (he was a formidable skier), but I was still taken aback by what I saw. There was a stone staircase leading down to an underground vault, and of course there were railings around the staircase. I saw a crazy man, who obviously had no concern for his life or limb, calmly walking on top of the railings starting where the steps began, and walking to the other end, where there would

have been a ten foot drop to the stone steps below! That was Tommy—fearless as always.

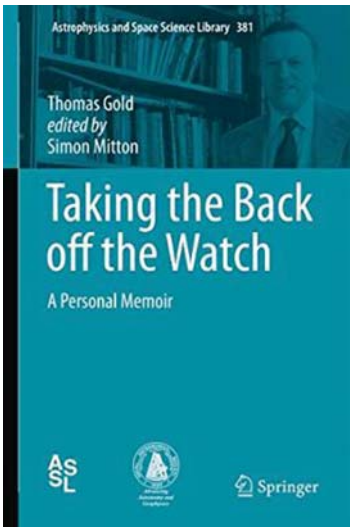
As a boy, I once asked permission to take an old alarm clock to pieces. Permission was granted, and I successfully took the clock apart and then reassembled it. Tommy, as a boy, achieved the far more impressive feat of opening up a Swiss watch, taking it to pieces, and then putting it back together! Hence the title of this memoir, *Taking the Back off the Watch*, which Tommy never completed in his lifetime (1920 to 2004) but which has been prepared for publication by Simon Mitton, who was himself once a young astrophysicist at Cambridge University. The life of Tommy Gold is a fascinating story, and we owe a great debt to Mitton for bringing it to publication.

Gold was born in Vienna in 1920 and lived there until 1930, when his family moved to Berlin. Tommy did not do well in the Berlin school, and the teachers suggested to his parents that Tommy might be mentally retarded! However, he excelled in gymnastics and learned to defend himself against the "ruffians and young thugs" a Jewish boy would encounter in Berlin in those days. In 1933, Tommy was sent to a school in Switzerland which—after Berlin—was a heavenly experience.

Tommy left Switzerland in 1938 to rejoin his parents, who had by then emigrated to England. He was admitted to Trinity College, Cambridge University, in 1939, but in May 1940 all persons in England with German or Austrian passports were interned. With other internees, Tommy was moved to an internment camp in Canada, where he had the great fortune of meeting Hermann Bondi, who would become a lifelong friend and collaborator. Tommy recounts an unpleasant experience at the hands of the commandant of an internment camp, and then advises us "Beware of humans when they are put in charge of other humans."

Tommy was returned to England in 1941, and was able to resume his studies (in Mechanical Engineering!) at Cambridge. Once he had completed his studies, Tommy was sent to the Admiralty radar establishment in Surrey to become part of the theory section where he rejoined Bondi, and where he met Fred Hoyle who was in charge of that section. It was in this way that the famous trio of Bondi, Gold, and Hoyle came together.

When World War II ended, Tommy was temporarily given the illustrious rank of Wing Commander and sent to Germany with a small delegation to inspect and report on various scientific and technological centers in Germany. One night in Berlin, he was accosted by an American soldier who demanded that they take him to an address in Berlin. Somehow the interaction became hostile and the soldier produced a knife and began to attack Tommy. Tommy had as a boy learned how to fight, and he was able



to break the soldier's wrist and so survive a very dangerous situation.

I met Tommy—well after the War had ended—at the conference on "Gas Dynamics of Cosmic Clouds" mentioned earlier. However, my principal contact with Tommy occurred (probably in 1969) after the discovery of pulsars by Jocelyn Bell and Tony Hewish in 1968 at the Mullard Radio Observatory at Cambridge. My first thought was that pulsars had to be pulsating white dwarfs, and I developed a theory along those lines. Tommy's immediate interpretation was that they had to be rotating neutron stars. When he first asked for time at an astrophysical conference to present this idea, the conference chairman refused, saying "If I give you time for that, Tommy, I'll have people asking to present all kinds of crazy ideas." Undeterred, Tommy gave his speech from the floor! Tommy got the basic idea correctly, but he did not go on to develop that idea into a theory, which is a problem I took on in 1970.

As one must expect of any creative scientist, Tommy had both hits and misses. The sudden-commencement model and his pulsar model were two of his hits. Another was his concept of the "magnetosphere" for the region far from the Earth's surface where the plasma "atmosphere" is dominated by the Earth's magnetic field. Another remarkable intellectual achievement was Tommy's theory that the ear is not a passive receiver of sound waves, but is a receiver that connects with an active transducer. Tommy proposed that theory in 1948, but it was not accepted by the biological community until 1998. Tommy could be years ahead of the establishment!

One theory that Tommy would have to abandon was his idea that the surface of the Moon would be covered by dust two or three feet thick. Most astrophysicists would also claim that he (and Hermann Bondi and Fred Hoyle) were off base in advocating the "Steady State" model of cosmology. Fred Hoyle had used the term "Big Bang" to denigrate the alternative idea of a sudden beginning of the universe, but the name has stuck!

Tommy took an active and creative interest in many other problems. One was the nature of radio sources being discovered by radio observatories, including the Mullard Radio Observatory under the direction of Martin Ryle in Cambridge in the 1960s. The distribution of sources appeared to be

almost isotropic, so they had to be either very close by (inside our galaxy) or extragalactic. Ryle initially went for the first option and Tommy for the second—which proved to be the correct choice.

Tommy took an interest in the intriguing problem of finding a mechanism that can explain the polarization of starlight—now believed to be due to asymmetric interstellar grains that are aligned either by magnetic field or by flow-fields in the interstellar gas.

Tommy was never afraid of controversy, once saying "For a theory to be useful, it should be wrong" (intended meaning debatable of course). One of Tommy's theories (which almost all scientists would consider to be wrong) is that oil has a geological origin, not a biochemical origin. He was successful in getting a Swedish oil company to drill through 500 meters of rock, an experiment that produced oil, but only a very small amount. That test argues against Tommy's theory, but it does not absolutely disprove it.

If I could bring back to Earth one of my erstwhile colleagues to talk over my current research (the influence of neutrinos on beta decays), it would be Tommy. It is unlikely that he would immediately accept the reality of the phenomenon—still less immediately accept my theory—but he would certainly bring to the discussion an open and inquiring mind—which is all one can ask of a true scientist.

Peter Sturrock

The Helene Reeder Memorial Fund for Research into Life after Death

Grant Announcement for 2017

The Helene Reeder Fund (HRF) is pleased to announce the availability of grants for small- and medium-sized scientific research projects concerning the question of Life after Death.

Grants will be awarded in the range of EURO 500–5,000 maximum.

The topic Research into Life after Death should constitute the main objective of the project.

Applications in English to be submitted by email to The Helene Reeder Memorial Fund, edg.muller@comhem.se, should include:

- detailed description of the project, including objectives
- methodology
- cost budget
- timetable
- plans to publish the results in scientific journals
- CV of the applicant
- how the applicant plans to report back to the HRF about progress and result
- any financing other than from HRF

Applications should be received not later than 1st of October 2017. It is the intention of the HRF to evaluate the applications and to make the decision regarding the grants before the end of December. Applicants will be notified by email after the decision and the grants will be payable during December.

For further information, please apply to edg.muller@comhem.se

Edgar E. Müller, Secretary of the Board, the Swedish Society for Parapsychological Research

Society for Scientific Exploration

SSE ASPIRING EXPLORERS PROGRAM

The SSE has established an Aspiring Explorers Prize for meritorious student research projects judged to be the most original and well-executed submission in subject areas of interest to the SSE. A committee is in place to review all entries and determine the winner, who will receive an award of $500 and have the opportunity to present a talk describing the project at the annual meeting, for which the Society will cover her/his registration fee. Submissions must be made per the guidelines and deadline as stated on the SSE website "Call for Papers" for the conference you are considering attending in order to be eligible for that year's prize.

If your paper is selected for the Aspiring Explorer Award, you will be either invited to present your talk at the meeting or able to submit your paper as a poster session. We are very excited about doing poster sessions now, so please let your fellow student colleagues and professors know about this.

In addition, the SSE is also offering a 50% discount on future meeting registrations for any student member who brings one student friend to our conferences (one discount per student). We are eager to see student clubs or SSE discussion groups established at various academic institutions or in local communities. Contact us at sseaspiringexplorers@gmail.com to start your own group!

C. M. Chantal Toporow, Ph.D., SSE Education Officer
sseaspiringexplorers@gmail.com

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