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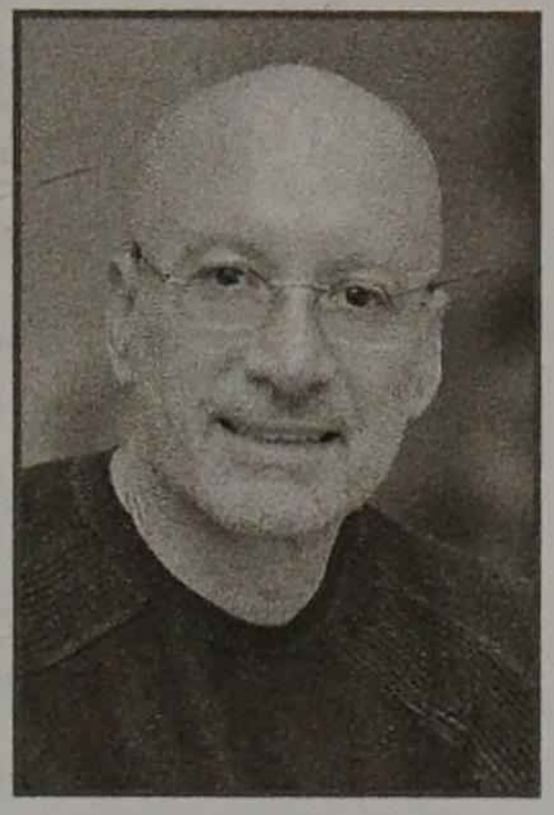
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EDITORIAL

JSE's First Retraction

STEPHEN E. BRAUDE

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This issue of the *JSE* includes a retraction of a paper by Alejandro Parra we published in 2017. As far as I can determine, it's the journal's first official retraction of a published paper. The reason for this action is the author's extensive plagiarism, both in that paper and in other published work (including a recent book whose publisher has since recalled all copies). It's a sad state of affairs, of course—and perhaps the first of its kind in this particular and admittedly minor scientific domain.

It reminds me that six years ago, in Volume 29(2), we published a paper on retractions in science, and in that issue I seized the opportunity to editorialize further on the subject. I recycle that Editorial below. But before that, I must note that careful examination has found no evidence of plagiarism in the one other research article (in 2018) and the one book review we've published by Parra—however, a recent Parra submission to *JSE* (that we rejected) was substantially plagiarized, except for an added abstract, from a paper published in Spanish by another author in another journal. I must also mention that, henceforth, the *JSE* will run routine plagiarism tests on papers accepted for publication. I thought this was a chore I'd left behind when I retired from teaching. However, I don't want the *JSE* to emulate the person who said "I've learned from my mistakes, and I'm certain I can repeat them exactly."

-------- EDITORIAL from *JSE* Volume 29(2) in 2015, pp. 189–194 --------

One of the Commentaries in this issue is something of a departure for the *JSE*. The paper by Jaime A. Teixeira da Silva (https://www.scientificexploration.org/docs/29/jse_29_2_TeixeiradaSilva.pdf) concerns

retractions in scientific publications, a topic that has been receiving increasing attention in recent years, apparently coinciding with an increasing number of retractions over the same period. Because the SSE and *JSE* focus not only on specific (usually controversial or neglected) domains of scientific investigation but also on broader issues concerning the practice of science itself, I figured that the cluster of issues surrounding retractions might be of both theoretical and practical interest to *JSE* readers.

The sheer number of retractions is enough to give one pause. A recent survey by R. Grant Steen of the PubMed database from 2000 to 2010 identified 788 retracted papers (Steen, 2011). For 46 of those papers, Steen was unable to find formal retraction notices. So, his survey dealt with the remaining 742 papers for which he could obtain such notices. The reasons for retraction were broadly identified as fraud and error. The former included data fabrication and data falsification, and the latter included (among other things) plagiarism,[1] scientific mistake, and ethical issues (violations of accepted publication practices—for example, IRB [Institutional Review Board] violations). Steen found that the reason for retraction was more often error than fraud—73.5% as compared to 26.5%.

A later survey (Steen et al., 2013) examined the interval between publication and retraction for what strikes me as an astounding 2,047 retracted articles indexed in PubMed.[2] And the number of papers that should be retracted may well be greater than that. As Cokol et al. (2007) observe, "Retracting a published scientific article is the academic counterpart of recalling a flawed industrial product" (p. 422). But

> . . . articles published in more prominent scientific journals receive increased attention and a concomitant increase in the level of scrutiny. This therefore raises the question of how many articles would have to be retracted if the highest standards of screening were universally applied to all journals. (Cokol et al., 2007)

Moreover, as Vedran Katavić noted, "the retracted articles do not die, but rather receive citations years and decades after their retraction, often by the authors themselves" (Katavić, 2014, p. 217). So one can easily see why da Silva is concerned about the consequences of all these

retractions for the downstream scientific literature. Katavić, in fact, supplies a stunning example of the extent to which retracted articles can infiltrate and leave traces in the media.

> On January 30, 2014, the scientific journal *Nature* published two papers by Haruko Obokata et al. detailing reprogramming of somatic into stem cells by an acidic bath. The journal's article metrics allow for some understanding of the impact these articles have attracted so far, before their inevitable retraction (at the time of writing this opinion piece, both papers are under investigation for fraud). Within approximately 50 days of publication, these two articles (taken together) have been tweeted about over 3,300 times, appeared on more than 100 Facebook pages, picked up by 130 news outlets, cited a total of 30 times (which puts them above the 90th percentile of tracked articles of similar age across journals or in *Nature*), blogged about on at least 50 scientific blogs, and their web pages at the source through the nature.com journal platform have been viewed (HTML views and PDF downloads) more than 1,300,000 times total! (Katavić, 2014, pp. 220–221)

Another piece of information I found especially startling was a presumably incomplete list of scientists with multiple retractions, some of them with truly amazing totals. Consider Table 1, provided by Katavić (2014, p. 219).

In addition to the specific concerns raised by da Silva, my own brief search of the relevant literature turned up the intriguing finding that "the probability that an article published in a higher-impact journal will be retracted is higher than that for an article published in a lower-impact journal" (Fang & Casadevall, 2011, p. 3856). The authors write,

> The correlation between a journal's retraction index and its impact factor suggests that there may be systemic aspects of the scientific publication process that can affect the likelihood of retraction. When considering various explanations, it is important to note that the economics and sociology of the current scientific enterprise dictate that publication in high-impact journals can confer a disproportionate benefit to authors relative to publication of the same material in a journal with a lower impact factor. For example, publication in journals with high impact factors can be associated with improved job opportunities, grant success, peer recognition,

TABLE 1
Some Authors with Multiple Retractions from the Last Decade

Name	Scientific field	Number of retracted publications
Yoshitaka Fujii	Anesthesiology	170
Joachim Boldt	Anesthesiology	90
Friedhelm Herrmann / Marion Brach	Neuroscience	94
Diderik Stapel	Psychology	50
Naoki Mori	Immunology	30
Jan Hendrik Schön	Physics	25
Shigeaki Kato	Biomedicine	20
Alirio Melendez	Immunology	20
Dipak K. Das (late)	Biomedicine	20
Silvia Bulfone-Paus	Biomedicine	13
Eric Poehlman	Biomedicine	10
Bengü Sezen	Biochemistry	9
Dirk Smeesters	Psychology	7

Source: Reprinted from Table 1 in Katavić, V. (2014). Retractions of scientific publications: Responsibility and accountability. *Biochemia Medica (Zagreb)*, 24(2), 217–222. https://www.biochemia-medica.com/en/journal/24/2/10.11613/BM.2014.024/fullArticle

and honorific rewards, despite widespread acknowledgment that impact factor is a flawed measure of scientific quality and importance. . . . Hence, one possibility is that fraud and scientific misconduct are higher in papers submitted and accepted to higher-impact journals. In this regard, the disproportionally high payoff associated with publishing in higher-impact journals could encourage risk-taking behavior by authors in study design, data presentation, data analysis, and interpretation that subsequently leads to the retraction of the work. Another possibility is that the desire of high-impact journals for clear and definitive reports may encourage authors to manipulate their data to meet this expecta-tion. In contradistinction to the crisp, orderly results of a typical manuscript

> in a high-impact journal, the reality of everyday science is often a messy affair littered with nonreproducible experiments, outlier data points, unexplained results, and observations that fail to fit into a neat story. In such situations, desperate authors may be enticed to take short cuts, withhold data from the review process, over-interpret results, manipulate images, and engage in behavior ranging from questionable practices to outright fraud. . . . Alternatively, publications in high-impact journals have increased visibility and may accordingly attract greater scrutiny that results in the discovery of problems eventually leading to retraction. It is possible that each of these explanations contributes to the correlation between retraction index and impact factor. Whatever the explanation, the phenomenon appears deserving of further study. The relationship between retraction index and impact factor is yet another reason to be wary of simple bibliometric measures of scientific performance, such as impact factor.[3] (Fang & Casadevall, 2011, pp. 3856–3857)

Furthermore, according to Shi V. Liu, the high impact factor (IF)

> for some journals is actually based—at least in part—on the high number of citations of their retracted papers. . . . Rather than removing these 'negative contributions' from the IF calculation, these journals have continued to use their inflated IFs to promote their publications. (Liu, 2007, p. 792)

I should add that, among the many interesting observations in the passage quoted above from Fang and Casadevall, I found it refreshing to see the authors acknowledge that "the reality of everyday science is often a messy affair littered with nonreproducible experiments, outlier data points, unexplained results, and observations that fail to fit into a neat story." No doubt *JSE* readers (and authors) are all too aware of this, although that grubby reality is often ignored by critics of the research to which this Journal is devoted. (Katavić also has some pertinent observations on this topic.)

Because I felt that this general topic of retractions would be of considerable interest to *JSE* readers, I thought I might be able to stimulate commentaries on the Commentary by reaching out to various SSE stalwarts and some others, to see if they wanted to offer reflections of their own. Here are some of those responses: Their authors have allowed me to submit them for your further consideration.

From psychiatrist and psychoanalyst (and dissociation researcher) John O'Neil (personal communication, February 3, 2015):

> With increasing digitization, there's more and more automatic registering of what gets cited, so I assume that at some point in the future there may be some automatic tag that goes on all papers citing a retracted paper, and then some derivative tag that goes on all papers citing a paper that cites a retracted paper, etc. Though at that point the carbon-based units would need to take over for a little interpretation.
>
> Automatic tagging wouldn't suffice, of course, as a review paper might cite a retracted paper as an example of a retracted paper, and cite the retraction as well, I would assume, so then there would need to be some mechanism to have the 'tag' removed from that paper; otherwise the tag would mislead, and be carried into all the 'progeny' of the paper concerned. So the idea that a retraction [can] cause the retracted paper to cease to exist is nonsense. What happens instead is a published retraction by an author (or publisher), and this compromises the credibility of the paper.
>
> And then, of course, there's the forced retraction. Like Galileo (or whoever) retracting solid science under social, political, or religious pressure. So a retraction may be done to save one's skin. Or to please others (e.g., retractions of accusations of incestuous sexual abuse). So, retractions arising from a lack of moral fibre (to use some dated expression).
>
> So I think the author is onto an important point, but I also think the devil is in the details.

From my editorial predecessor, Henry Bauer, some characteristically trenchant comments (February 14, 2015):

> That retracted material continues to be cited and the retraction known is unquestionably a bad thing. However, it is whistling in the wind to call for systemic solutions: There is no mechanism by which solutions could be enforced.
>
> The problem arises in part from "publish or perish," nowadays more aptly "get grants continually or perish." That has led to a spate of online commercial publishers putting out hordes of journals whose only purpose is to allow grant-seekers to publish anything at all merely by paying "publication costs" (more at "Fake, deceptive, predatory Science Journals and Conferences," http://wp.me/p2VG42-29).

If researchers were to be more scrupulous in checking what they cite, and peer reviewers were more conscientious, and editors, too, then the problem would not have reached its present proportions. That is water under the bridge. The issue da Silva addresses is simply one aspect of how science has become corrupted through excessive expectations and expansion, see "The Science Bubble" in *EdgeScience* #17, February 2014, http://www.scientificexploration.org/edgescience/17

Of greater concern to me and others who try to get minority views published is retraction as a form of censorship, the retraction of articles that had been accepted after appropriate review but whose publication meets storms of protest from vigilante defenders of mainstream orthodoxy. See, for example, the story of the demise of the journal *Medical Hypotheses* for transgressing HIV/AIDS theory, Chapter 3 in my *Dogmatism in Science and Medicine* (McFarland 2012). More recently a literature review of the controversy over HIV/AIDS by Patricia Goodson survived the call for retraction with the editors compromising by changing it to an "Opinion" piece from the original "Hypothesis and Theory," though apparently its abstract has been removed from PubMed (article and comments at http://journal.frontiersin.org/Journal/10.3389/fpubh.2014.00154/full; protest is at http://journal.frontiersin.org/Journal/10.3389/fpubh.2015.00030/full; and publisher's statement at http://journal.frontiersin.org/Journal/10.3389/fpubh.2015.00037/full).

Finally, Michael Ibison (personal communication, February 14, 2015) contributed this:

> I wonder if in the future the 'static' paper will be a special case, the more common being a dynamic version subject to continuous revision. The latter is already under way at arXiv and ResearchGate. For this reason, when I have an interest in a recent journal paper I check out arXiv and elsewhere on the Web, sometimes finding a 'new and improved' and/or extended version. The journal paper might function as an 'advertisement' in such cases.

I have no particular ax to grind (yet) with respect to this general topic of retractions and their aftermath. However, I look forward to seeing whether SSE members want to pursue the topic further, either with commentaries or correspondence submitted to the *JSE*, or perhaps at one of our conferences.

NOTES

1 One might wonder why plagiarism isn't considered fraud.
2 See also the figures cited recently by Gasparyan et al. (2014).
3 For further commentary, see, e.g., Cokol et al. (2007), Gasparyan et al. (2014), Gewin (2014), Katavić (2014), Liu (2007), Steen (2011), and Steen et al. (2013).

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---------- END of EDITORIAL from 2015 *JSE* 29(2) ----------

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RESEARCH ARTICLE

Associative Remote Viewing for Profit: Evaluating the Importance of the Judge and the Investment Instrument

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Abstract—This study examined the importance of the judge and the particular investment selection in the associative remote viewing (ARV) process. In Protocol 1, ARV was used to predict investments and to generate funds. Three viewers made weekly predictions on Sunday about an image they would be shown on Friday. Two images were selected to represent different states of a stock (value increase or value decrease), and a judge reviewed the images and the viewers' information. Based on the judge's evaluation, a coordinator informed an investor whether to invest for the stock to rise or fall during the weekly session. Though the sessions lost funds due to a complication in the investment process, this was not the focus of the study. A second judge (Protocol 2) and a mock investment instrument (Protocol 3) were included, blinded to all study participants. The second judge (J2) performed at a significantly less accurate level than the first judge ($p < .05$), and J2 also performed significantly lower than could be expected by chance ($p = .02$; effect size = –1.498; power > .80). Both judges performed significantly differently on the target investment than on a control investment. Although this is a pilot study with a small sample size and a limited number of sessions, conclusions are that the selection of a judge, even a very experienced judge, can have a significant effect on the success of an ARV project and that judges' decisions are more affected by the target investments than by a comparable control investment. Future ARV projects are advised to qualify judges for accuracy just as they qualify viewers for accuracy.

Keywords: associative remote viewing; ARV

INTRODUCTION

Upton Sinclair, the famous American author, watched his wife practice a form of telepathy over a period of three years. In his 1930 book, *Mental Radio*, Sinclair provides details of informal experiments he did where he and his wife would try to draw the same pictures when they couldn't see or communicate with each other. His wife's drawings included descriptive words or impressions of the information she was receiving. Sometimes, he would produce a number of images, seal them in paper, and randomly select one for his wife to reproduce. Though neither of them knew which drawing was selected, she was able to draw very similar images and sometimes provided accurate descriptions of the target drawing that was selected. Their early successes led them to try this with some of their friends, and they continued to have success, even at distances up to 30 miles.

Though Sinclair and his wife considered this a type of mind-to-mind communication or telepathy—later popularized as a form of extra-sensory perception (Rhine, 1934)—the practice of trying to draw information received through extrasensory means may have been the beginning of the practice of remote viewing. Warcollier (1948) explored the possibility of telepathically communicating images in his book *Mind to Mind*, but in contrast to the Sinclairs' informal sessions, Warcollier documented formal experiments that provided a foundation for many future studies. His work implicitly introduced the concepts of a model of mind, information transfer, and signal-to-noise ratio, and his methods may have been the foundation for later studies by the U.S. Government (Swann, 2001).

In 1972 the idea of remote perception of images was revived at the Stanford Research Institute (SRI) with Hal Puthoff and Russell Targ (Puthoff & Targ, 1976). Their work with viewers Ingo Swann and Pat Price caught the attention of the U.S. Government intelligence agencies and started a 20-year research program that is now commonly known as Project Star Gate. Research continued at SRI while a training program was developed for soldiers and intelligence officers to refine these techniques at Fort Meade, Maryland, USA (May & Marwaha, 2018).

The program at Fort Meade produced a number of actionable intelligence sessions including information that helped to locate a Soviet

aircraft that had gone down under a heavy jungle canopy in 1979. This information was later verified by former president Jimmy Carter in a 1995 speech (Reuters, 1995). Other information gathered by the Fort Meade viewers helped to locate hostages, and provided information valuable for new weapons systems, troop deployment, nuclear weapons testing, and anti-terrorist activities (Smith & Moddel, 2015).

Associative Remote Viewing

"Associative Remote Viewing (ARV) is not an RV method, but rather a mode of employing RV to predict the outcome of future events with limited outcome sets" (Smith & Moddel, 2015, p. 381).

ARV was developed to provide a practical way to examine events or activities that might be difficult to view directly or that might activate the logical processes that could repress the recognition of the psi information or result in analytical overlay. Analytical overlay (AOL) is a factor that is often recognized by remote viewers where their rational mind attempts to make sense of the information that is being received to construct a coherent image or perception. This rational process complicates the remote viewing procedure because the information being received by the viewer is being modified by a rational process rather than being perceived directly. In essence, AOL includes any rational but irrelevant activities that divert a viewer from the remote viewing task (Tart, 1979).

Often ARV projects have been used for investment purposes or to predict the outcome of a sporting event (e.g., Harary & Targ, 1985; Rosenblatt, 2000; Smith, Laham, & Moddel, 2014), but it can be applied in many different circumstances. In a typical ARV project, there are a limited number of possible outcomes, and one viewing target is selected to represent each outcome. The viewer attempts to view a target that they will be shown in the future, and based on the target they describe decisions that can be made about the associated activity or event.

In 1982, Harary and Targ conducted a well-publicized ARV project designed to predict changes in the silver futures commodities market. In an attempt to raise funds for future research projects, the researchers worked with an investor to generate income from investments using a modified RV protocol based on a design provided by Edwin

May. After raising more than $100,000 in 9 weeks using Harary as the viewer, they suspended the sessions and resumed a few months later using a slightly different protocol that prevented adequate feedback for the viewer. After just a few unsuccessful sessions, the second series was terminated (Harary, 1992).

Despite the inadequate results of the second series, this protocol has become iconic and a model for additional ARV sessions (e.g., Puthoff, 1984, pp. 121–122; Targ et al., 1995; Rosenblatt, 2000; Smith, Laham, & Moddel, 2014). The sessions use the following basic methodology.

Early in each week, the viewer is presented with a task to describe an object that will be shown to them on Friday. The viewer completes the viewing and provides it to the researcher. The investor is asked to select two targets, one to represent the price of silver futures to rise, and a second to represent the price of silver futures to go down. The description of the target provided by the viewer is compared with the actual targets and a judge determines which target more closely resembles the viewer's description. If the selected target represents the commodity going up, an investor invests in the value rising. If the target represents the commodity going down, the investor makes the appropriate investment. The investments are made on Monday in anticipation of the value of the commodity on Friday. On Friday, the investor evaluates the *actual* value of the commodity, and the viewer is shown the target that represents the *actual* state of the investment (up or down) (Harary & Targ, 1985).

In 2012, Kolodziejzyk published the results of a 13-year ARV investment study that he had conducted from 1998 to 2011 (Kolodziejzyk, 2012). Using a unique, computer-based approach enabled him to act as the viewer, judge, and investor for 5,677 trials. He correctly predicted his investments 52.65% of the time, which is a significant variance from chance ($z = 4.0$). Though the protocol was complicated because he combined his knowledge of the stock market with the ARV protocols he employed, the combination yielded a profit of more than $146,000 during this time period.

Using a process similar to the Harary and Targ silver futures investments, Smith, Laham, and Moddel (2014) trained inexperienced viewers to predict whether the value of the Dow Jones Industrial

Average would rise or fall on the day after the prediction. Seven out of seven predictions made using this protocol were correct and produced highly significant results ($p < .01$), earning a profit of $16,000 for two investors.

A meta-analysis by Bierman and Rabeyron (2013) combined the results of 17 ARV projects that they were able to verify as sufficiently designed and well-reported. In an evaluation of more than 550 trials, they identified a success rate of 63%. In a followup study of a casino-style ARV experiment, they found that they obtained a 56% success rate, attributing their lower success rate to their automated, machine-scoring technique rather than utilizing human scorers.

Recent ARV endeavors have explored ways to modify the protocols by integrating a larger number of viewers or using computerized, session-management tools to facilitate working with a large group. Katz, Grgic, and Fendley (2018) reported on a 14-month project that involved more than 60 viewers and 177 predictions. The predictions were focused on investments in the Foreign Exchange Currency Market (FOREX). The group sessions lost nearly all of their seed capital, but a contributing factor in this may have been their inexperience with FOREX investing and the selection of their investment instrument. Despite the significant losses in this project, the study examined the performance of different teams in an attempt to develop a model for best practices when using a large group for ARV sessions. The results indicated that the teams that kept their protocols simple produced better results, and also the addition of a large number of sessions over a short time frame added stress on the traders which may have negatively affected the investment outcome.

Prospective and Retrospective Studies

Studies such as those by Bierman and Rabeyron (2013) and Katz, Grgic, and Fendley (2018) implement a retrospective analysis of data gathered during an ARV session. Bierman and Rabeyron discussed the possibility that the differences in their judging process may have accounted for differing scores. Katz, Grgic, and Fendley indicated that their choice of an investment instrument (FOREX) could have affected the outcome of their investments due to the complexity of the instrument. Other retrospective analyses (e.g., Katz, Beem, & Bulgatz, 2014;

Grgic, Katz, & Tressoldi, 2018) have been presented related to ARV projects in an attempt to better understand the factors that can affect the success of a project. Retrospective studies are valuable to guide future research and identify factors that may be important to our understanding of ARV and other topics.

Prospective studies define all of the factors, variables, and analyses before the study begins. This approach increases the credibility of the study results and allows for the investigation of specific factors while limiting the influence of unanticipated activities. When a study is completely designed before it is conducted, there are fewer opportunities for bias, unconscious or conscious, to affect the analysis or interpretation of the results. Finally, prospective studies can be preregistered with a study registration database, like the Koestler Parapsychology Unit Study Registry, providing additional credibility to the scientific methodology and the resulting analysis.

Note: This study was not preregistered, but it was completely designed, specified, and reviewed before any data were collected.

Purpose and Study Design

The purpose of this study is to determine the impact of the judges' analyses and the choice of investment instrument on the success of an ARV series. Although all of the study participants recognized that the intent of the sessions was to generate revenue through investments, the scientific investigation was conducted using multiple judges and multiple investment instruments in order to analyze the results based on an evaluation of these factors rather than on the accumulation of wealth.

Many recent RV studies are focused on evaluating the performance of the viewers and exploring the factors that contribute to increasing their accuracy. This study does not examine these factors, and therefore will not report on any of the specific sessions provided by viewers or explore the success rates of the viewers. Those questions are left to other researchers who are more experienced with training methodologies and the environment that best supports viewers.

This is a prospective study that utilizes multiple judges and multiple investment instruments in order to determine if there is a difference between the results obtained when using different judges or different investing instruments.

Hypotheses

H1: In an ARV study, the judge's evaluation can have an impact on the accuracy of the predictions and affect the success of the investment process.

H2: In an ARV study, the predictions made will apply to the target investment instrument more than they will apply to a randomly selected investment instrument.

Design

This study involved three concurrent activities designed to explore the hypotheses. A single data collection protocol was used to gather information, but two additional protocols were implemented to test the main hypotheses. Details are provided in the methodology section below.

Protocol 1

The preliminary data collection protocol implemented traditional ARV sessions similar to the original silver futures study completed by Harary and Targ. There is one noted modification to the original protocol in that three viewers were used instead of one, and the judge reviewed information from all three viewers to determine which target was the best match for the data gathered from the viewers. After the viewers' data was gathered, it was sent to the judge for analysis. Finally, a single investment instrument was selected for the study, and investments were made based on the impressions of the viewers and the analyses of the judges throughout the study.

Protocol 2

When data were sent to the judge, an additional protocol was added, unknown to everyone except the primary investigator (PI). Two judges were included in the study though both judges believed they were the only judge involved. The PI took the information sent to the first judge and sent it to a second judge. The results of the judging sessions were compared to determine if the evaluations of the judges could affect the results of the ARV sessions. (See note about a potential experimenter effect in the **Methodology** section under ***Protocol 2***.) This protocol was designed to investigate H1.

Protocol 3

Unknown to all participants except the PI, a second instrument was selected for comparison purposes. No investments were made for this second instrument, but the activity of this instrument was tracked throughout the study. This protocol was designed to investigate H2.

Since the analysis of this study does not include an evaluation of the money lost or gained during the sessions, the values of the instruments are reported, but are not significant in evaluating the hypotheses. The hypotheses are evaluated strictly on a comparison of the judgments made by the two judges and the difference between the predicted results and the actual results of the two investment instruments.

Time Frame and Investments

The study included 13 viewing sessions, one week for each session, over a 15-week period. The first session was a test session to verify that the communication process would work correctly and to ensure that all participants understood their roles. There were 12 weeks of experimental sessions when there were plans to make investments (see Appendix).

The investment and the mock investment were selected before the study began, and the same instruments were used throughout the study to allow for the evaluation of the effects that the instrument selection had on the success of the process (H2). The original decision was to invest in commodities due to their high volatility over a short period of time. Since each session ran from Monday through Friday, it was important that each instrument have sufficient variability during the session to identify the effectiveness of the process and so that the change in value would give the study the best chance of producing profit during the week.

Due to investment limitations presented by the investment company selected for this study, commodity investment was not available. After investigations with the investment company, the instruments chosen were stocks with high volatility. By querying a site listing the 50 stocks with the highest volatility, two stocks with high volatility were chosen. The selection process investigated the extent of volatil-

ity within a 5-day period, the availability of the stock, and information about whether the stock was available to be *shorted*—a term used for investing in a stock when you believe its value will fall during a week. The exact same process was used for one stock that would be used for investments, and a second, *mock* stock that would just be monitored throughout the study for comparison purposes.

The selected stocks were used throughout the study regardless of whether they rose or fell or whether the investments were successful. The consistency of the investment instruments (i.e. using the same stocks throughout the study) was essential to evaluate the factors being examined for H2.

METHODOLOGY

All participants in this study were focused on the task of generating profit from the investments that were made during the 12 weeks of the study. There were five categories of participants included in this study.

Coordinator: The coordinator performed the project tasking, collected data, and passed information among the other members of the team. The coordinator tasked the viewers, collected the viewing data, requested the targets, passed the information to the judge, determined how the investment should be placed, informed the investor, and provided feedback to the viewers once the actual target was identified.

Viewer: There were three viewers. Each viewer completed the viewing with which they were tasked, one viewing per week. They would provide their viewing information to the coordinator via email when it was completed. None of the viewers knew any of the others, and they knew the identity of the coordinator only. The viewers were tasked on Saturday and returned their information to the coordinator on Sunday evening.

Target Selector: The target selector chose the two targets that would be used to represent changes in an investment for the week. In this study, the targets were selected randomly using the ARV Studio software which had a target pool of more than 1,000 targets and a random selection process (http://arv-studio.com/). The targets were electronic images specifically selected for ARV projects to be sufficiently different to facilitate the judging process.

Judge: The judge was provided with the viewers' information and the target images. The judge used a process that was familiar and comfortable. There were no limitations or instructions given to the judge, as they were highly experienced as a judge for ARV projects. The second judge (J2) included in this project to evaluate H2 was also highly experienced and received exactly the same instructions and information as the first judge (J1).

Investor: The investor received direction from the coordinator to invest as if the instrument was going up or down during the week. The investor would make the investment on Monday and resolve it on Friday. When the investor resolved the investment on Friday, the coordinator would be informed of the actual status of the investment at that time (up or down from the original value of the investment).

The viewers and judges in this study were very experienced in their roles and had demonstrated success in similar projects in the past. Only the coordinator and the investor had not been involved in a fully structured ARV project before, and the roles of these team members were carefully designed and structured to minimize their involvement in the portions of the process that directly involved psi processes (i.e. viewing and judging). The viewers used viewing methods that were most comfortable for them, and the judges followed a judging method that was familiar and comfortable.

Session Overview: Protocol 1

Each session began on a Saturday and was completed the following Friday (Figure 1). Investments for each week were made on Monday at 3 p.m. EST and resolved on Friday at 1 p.m. EST (if an investment was made for that week). The tasks included:

- The viewers were tasked on Saturday and completed viewing by Sunday evening when their session data were scheduled to be sent to the project coordinator.

The timing varied slightly for each viewer depending on the normal process used by the viewer. The viewers were not required to use a specific protocol or follow any standard procedure. They were encouraged to use a method for viewing that was familiar to them and that they felt would be most

likely to produce an accurate viewing. Each of the three viewers in this study chose to use a different approach to viewing.

After the viewing data were returned to the coordinator, the coordinator requested that the target selector (T.S.) select two target images for the week. The targets were selected after the viewing had been completed which implies a precognitive viewing process. Since the viewers were told to describe a target that would be seen on the following Friday, the viewing session was precognitive by definition, and the timing of the target selection was designed to facilitate maximum blinding rather than any consideration of when the viewers were targeted.

The two images were selected using ARV Studio software (http://arv-studio.com/) which is designed to select a pair of random targets specifically designed for use in ARV sessions. The T.S. sent the electronic images to the coordinator via email.

- The coordinator sent the targets and the viewers' descriptions/drawings to the judge for evaluation. After they were sent, the coordinator randomly assigned investment states to the two targets using a truly random process (implemented by random.org). Zero represented the investment going down during the week, and one represented the investment going up or staying the same during the week. The process for assigning meaning to the targets was designed to maintain maximum blinding for the participants.
- Judging of the viewing information and targets was completed before Monday at 3 p.m. and the results of the judging were sent to the Coordinator. The judge told the coordinator which target image was the best match for the viewers' information. If the judge determined that the viewers' data conflicted or did not match either image, the judge would tell the coordinator there was no choice for the week (NC).
- Based on the associations that had been determined earlier in the session, the coordinator contacted the investor on Monday and indicated whether the investor should make an investment for the stock to go up or to go down, or should not make an investment for the week in the case where the judge indicated no choice (NC).
- The investor made the appropriate investment on Monday at 3 p.m. and resolved the investment on Friday at 1 p.m.

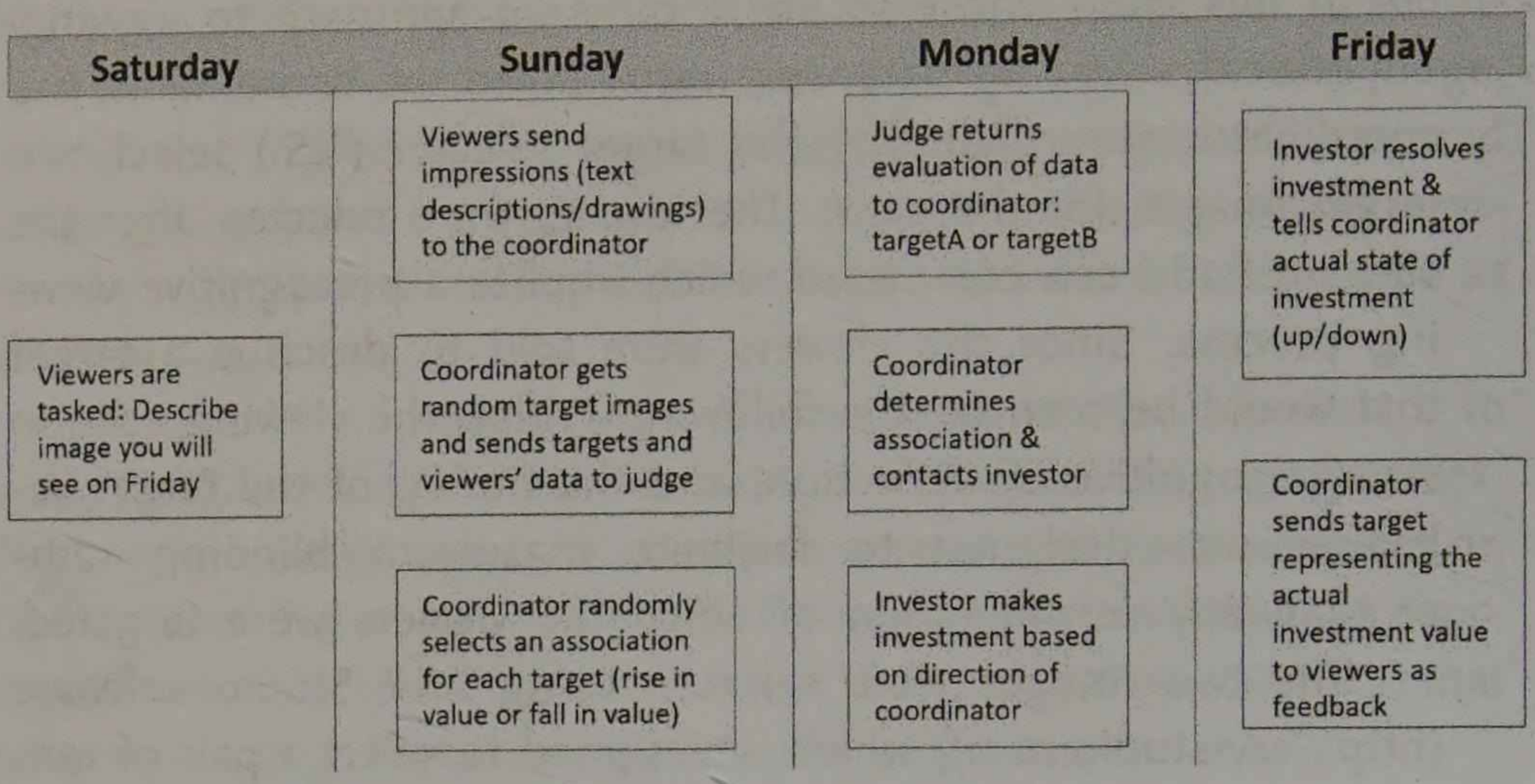


Figure 1. Standard ARV protocol.

- When resolving the investment on Friday, the investor would determine the actual status of the stock at that time and communicate the actual state to the coordinator. The three appropriate states were stock up, stock down, and no change.
- The coordinator would determine which image represented the actual state of the stock value, and that image would be provided to all of the viewers on Friday as feedback for the session that week.

The viewers and the judges were never informed of the investment instrument or the state of the investment each week. They also did not know whether or not the investment process was making money—i.e. the viewers and judges did not know whether their decisions resulted in gains or losses for the investments in the study. They remained blind to the entire process except for their specific tasks. The judges were never shown the feedback or told which targets were being used for investment purposes.

Results of the Investment Process

Nearly every recent ARV study that includes investments or betting evaluates the success of the project based on whether the project made a profit. When that is the primary goal of the project, this is a reasonable evaluation method. It is also a practical way to provide

an example of a viable application for ARV and an effective means of drawing attention to the value of psi research. In evidential terms, this evaluation method is flawed. While the overall goals of a study may be met, when the results are evaluated based on the overall earnings, the details of the individual trials may be minimized or dismissed.

For example, stock investments provide an uneven earning capacity from one session to another, and a study that successfully earns money investing in stocks could earn the majority of the funds in a single trial while losing money in every other trial. Using traditional statistical evaluations, significance would be evaluated based on the number of successful trials versus the number of unsuccessful trials, but in many ARV studies the results of individual trials are minimized when the study successfully earns a profit.

This study presented an intention to earn funds, but it was not related to the evidential goals of the study. In this study, the results of individual trials were used to evaluate the performance of the judges in the study and the impact of the choice of the investment instrument. The profits or losses from the investments are provided for informational purposes, but are not considered in the evaluation of H1 or H2.

Although the investment company used for this study indicated that the stock that was selected could be shorted (i.e. an investment could be made when the stock was predicted to fall), the stock was not available to be shorted with this company. This complication prevented investments for weeks when the stock was predicted to fall (Table 1). Because of this complication, the overall value of the investment fell approximately 7.5% during the study. If the stock could have been shorted at the appropriate time, the value of the investment would have risen 2.5% instead of falling.

Additional Explorations and Protocols

Many informal discussions about remote viewing protocols discuss the importance of the judge in the process, often indicating that the judge is more important than the viewers. Also, there are some discussions around ARV that emphasize the importance of selecting the correct investment target. This additional protocol is designed to evaluate these two claims about an ARV study to measure the impact of a judge (H1) and the selection of a target investment (H2).

TABLE 1
Down Weeks (Investments Could Not Be Made)

Weeks when investments were lower in value and an investment normally would have been placed. Due to complications in the investment process, no investments could be made during these weeks.

Week (hit/miss)	Start	End	Difference	Percent	Value
4 (miss)	11.68	11.28	.40	3.42	–684
5 (miss)	11.10	10.99	.11	0.99	–198
8 (hit)	11.26	11.20	.06	.53	106
9 (hit)	11.20	10.80	.40	3.57	714
10 (hit)	10.87	10.86	.01	.001	2
11 (hit)	10.54	10.25	.29	2.75	550

Additional Participants and Methodology: Protocol 2

Protocol 2 was designed to evaluate H1, and it involved one additional judge (J2) and an additional coordinator (C2)—the primary researcher.

The original protocol was as follows:

- Saturday: 3 viewers are tasked
- Sunday evening: viewers provide viewing information to coordinator
- Sunday evening: 2 targets are selected by target selector and sent to coordinator
- Sunday evening: coordinator sends all viewing data and 2 targets to the judge
- After judge is sent information, coordinator randomly assigns values to the targets (up/down) without informing the judge of these associations
- Monday morning: judge sends decision to the coordinator
- Monday 3 p.m.: investor makes investment based on coordinator recommendation
- Friday 1 p.m.: investment is resolved and feedback is sent to the viewers

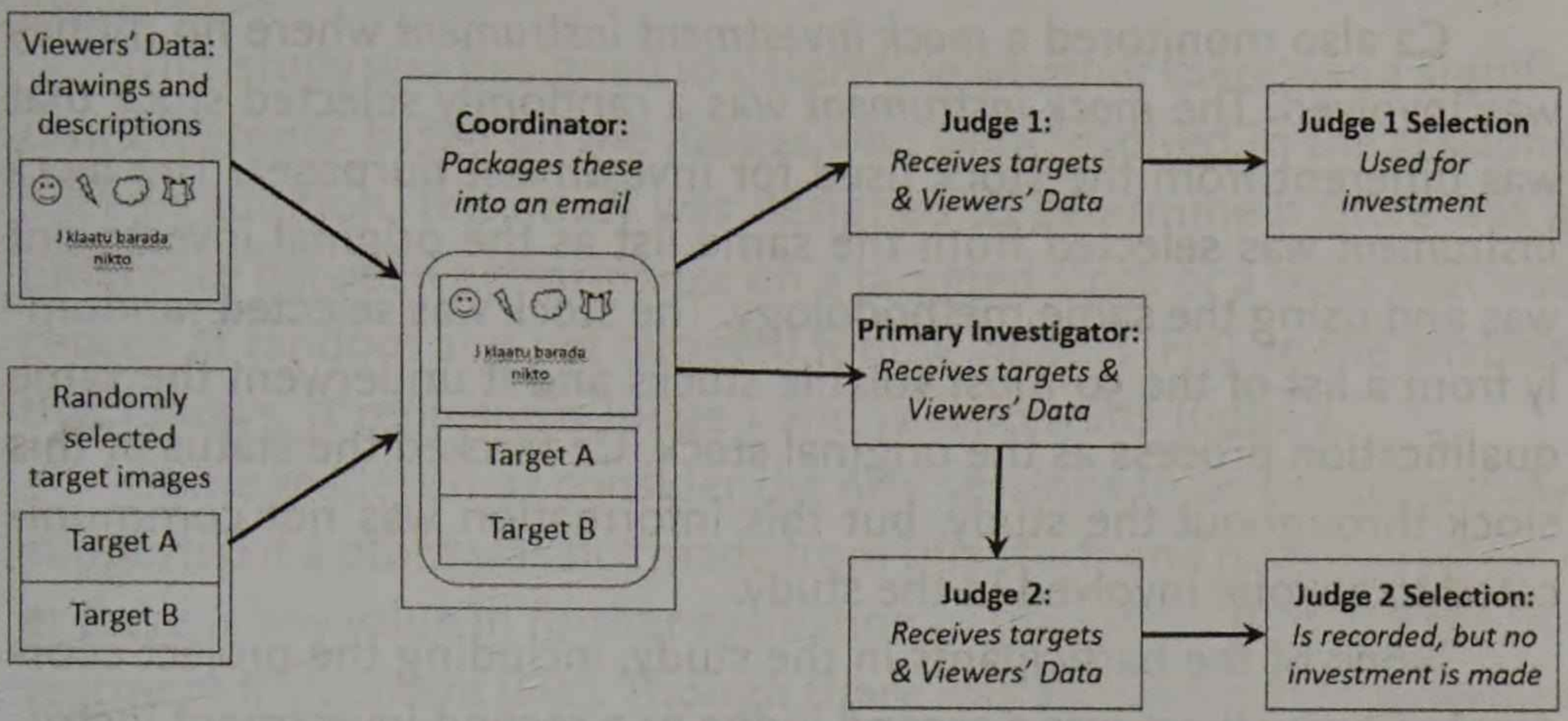


Figure 2. Protocol 2—Testing the judges.

Protocol 2 ran concurrently with Protocol 1 and for the same time frame (Figure 2). In addition to the original protocol, a second coordinator (C2) received the viewing and target information from the coordinator when it was sent to the first judge (J1). C2 took this information and sent it to a second judge (J2) who determined if the viewing material matched one of the targets. J2 returned the decision to C2 only. No investments were made based on the decisions of J2.

Note: Though C2 intervened as the second coordinator in the process and forwarded the information to J2, C2 sent exactly the same data to J2 as was sent to J1. Also, C2 had no more knowledge about the targets or data than the primary coordinator. Essentially, C2 stripped the email header from the information sent to J1 and sent the exact same email to J2 with no changes at all. There may have been a psi-enhanced experimenter effect introduced by this intervention, but every precaution was taken to ensure that the information sent to both judges was exactly the same and sent in exactly the same way.

Both judges involved in the study were informed that there might be an additional judge involved in the study as a backup for the primary judge, but both judges believed that they were the primary judge and that investments were being made based on their decisions.

Additional Investment Instrument: Protocol 3

C2 also monitored a *mock investment instrument* where no money was involved. The *mock instrument* was a randomly selected stock that was different from the stock used for investment purposes. The *mock instrument* was selected from the same list as the original investment was and using the same methodology. The stock was selected randomly from a list of the 50 most volatile stocks and it underwent the same qualification process as the original stock. C2 tracked the status of this stock throughout the study, but this information was not communicated to anyone involved in the study.

None of the participants in the study, including the project coordinator, knew there was a second judge or a second investment instrument. They also were not aware that there was an additional evaluation going on to determine the impact of the judge or the investment choice on ARV processes (Table 2).

TABLE 2
Judge's Performance for Each Investment Option and Total Correct Predictions for Judges and for Each Investment

The possible correct values for each judge (10 & 8) vary because both judges indicated that no choice (NC) should be made on some of the sessions. That is, the judges determined that the viewers' information was not sufficient to make a selection for some weeks, so no investment would be made in those weeks.

	Investment (Targeted)	**Investment (Control)**	**Totals for Judges**
Judge 1	5/10 correct	6/10 correct	11/20 correct
Judge 2	1/8 correct	5/8 correct	6/16 correct
Totals for Investments	6/18 correct	11/18 correct	

ANALYSIS

Many remote viewing studies focus on the value and accuracy of the viewers involved in the process. Viewers are considered to be the most important component of the process and most likely to be expressing psi. This study does not dispute the importance of having

good viewers or the value of the viewers in an Associative Remote Viewing process.

This study was designed to determine whether there was a significant difference based on the judges who were involved in the sessions (H1). In addition, this study was designed to determine if there was a difference between performance on a targeted stock or a stock that was chosen at random and not targeted for the viewers (H2). In the analysis that follows, J1 represents Judge 1 and J2 represents Judge 2.

Some readers may consider the ARV sessions to be unsuccessfully supported if a profit was not made from this study and question whether there is any value in further evaluation of the judges (H1) and the investment instrument (H2). **Though there was an intention to generate funds from this study, the primary goal of the study was the evaluation of H1 and H2**, and the primary goals are unaffected by the amount of profit generated. The profit generated from this study and the value of the investments are not used in the analysis of the hypotheses. For more information about why profits were not used in this analysis, see the section above on the **Results of the Investment Process.**

Initial Analysis Design

The initial design called for an analysis of the differences between the decisions made by J1 and J2. This analysis would be completed by doing a means comparison using an independent sample *t*-test to compare the number of correct decisions made by each judge (H1). This comparison would be completed for the experimental stock (SE) and the control stock (SC) in order to evaluate H2.

It was anticipated that there would be some sessions where the viewers' impressions would not match the selected targets well enough for the judges to make a clear distinction about which target matched the impressions. If this happened, it could be due to the viewers producing incomplete or inaccurate impressions, or it could be due to the judges misjudging the viewers' data. In these cases, the judges were given the option of indicating *no choice (NC)* indicating that the judge could not make a decision based on the data provided.

There are currently no set standards for evaluating no choice (NC) decisions by the judges. Some evaluators consider a NC decision by a

judge to be a miss, and others believe that NC should be left out of the evaluation (J. Lane, personal communication, 2018). For the purposes of this study, when the judge made an NC decision, it will be considered that this occurred because the viewers did not provide data that matched with either target, giving the benefit of the doubt to the judge. In these cases, the NC decisions will be excluded from the evaluations. Those cases will be dropped from the dataset, and only the cases where the judge made a specific decision will be included in the analysis.

Summary of Initial Analysis

The initial analysis revealed a significant difference between the judges, especially related to the targeted stock. Though J1 performed slightly higher than the expected mean across all of the sessions, the performance of J1 was not significantly different from chance. On the other hand, J2 performed at a level significantly lower than chance, but only on the targeted stock. Both judges performed at chance for the non-targeted or control stock (see Table 2).

RESULTS

COMPARING JUDGES AND INVESTMENT INSTRUMENTS

In an evaluation of H1, J1 outperformed J2 in predictions ($p = .05$ one-tailed in an independent sample *t*-test). This is due to the very low prediction scores for J2 (1/8 with 4 No Calls). This was only the case with the Experimental Stock, SE (J1 mean = .50, N = 10, SD = 0.527; J2 mean = .125, N = 8, SD = 0.354) (see Table 3).

TABLE 3
Judges' Performance and Comparison

The difference between the means is just at the threshold level for significance. Significance and equality of variance calculated using an independent sample *t*-test and adjusted due to unequal variances calculated using Levene's Test for Equality of Variance.

	Mean	N	SD	Variance	Significance
Judge 1	0.50	10	0.527	F = 11.43	*p* = .05
Judge 2	0.125	8	0.354	*p* = .004	(one-tailed)

The initial evaluation of H2 indicates that the number of correct predictions for the experimental stock was slightly lower than for the control stock (SE = 6/18; SC = 11/18). The difference was just outside the significance threshold ($p = .051$ one-tailed with an independent sample *t*-test).

This initial analysis, based on the predetermined analysis methods, supported H1, indicating that there is a difference in study results based on the judge that is selected and the evaluation produced by a particular judge ($p = .05$). H2 was marginally unsupported or nearly supported ($p = .051$), indicating that there may be a difference in results based on the instrument that is targeted versus a randomly selected investment instrument that is not targeted by the participants.

Post Hoc Analyses

During the study, J1 *correctly* predicted the state of the stock 4 weeks in a row. The probability of this is 1 in 16 or 0.0625. J2 *incorrectly* predicted the state of the stock 9 weeks in a row. The probability of this is 1 in 512 or 0.002. Though not specifically related to a means comparison used for the evaluations, these streaks of correct predications by J1 and incorrect predictions by J2 add evidence to indicate that there is a strong difference between the judges.

In post hoc analysis, J2 demonstrated a significant tendency to make incorrect judgments which could be interpreted as *psi missing* or an exceptional string of bad luck. Total: 1/8 correct; mean = 0.125; $p = 0.02$ for one sample *t*-test with an expected mean of 0.5; D = –1.498; power > .99 (see Table 4).

Additional post hoc analyses were evaluated during weeks when the stocks changed significantly, and two levels were se-

TABLE 4
Evaluation of J2's Predictions for the Target Investment
Significance level calculated using a one-sample *t*-test with an expected mean of 0.5

	Mean	N	SD	Significance	Effect Size	Power
Judge 2	0.125	8	0.354	$p = .02$	$d = -1.498$	>.95

lected: delta >2% and delta >1%. In both cases, J2 continued to demonstrate a significant tendency toward incorrect judgments regarding the state of the stock at the end of the week. Delta >2%: 0/5 correct; mean = 0.0; *p* is undefined because the mean is 0. Delta >1%: 1/7 correct; mean = 0.143; $p = .047$ with a one sample *t*-test with an expected mean of 0.5.

When examining the judges' results only during weeks when stocks made significant changes (delta >1% or delta >2%), there was no significant difference in the judges' performances and no significant differences between the predictions made for the experimental versus the control stocks ($p > .05$ in all cases).

CONCLUSIONS

This was a pilot study with a very small sample and a limited number of sessions. It should be replicated, and additional evaluations of ARV projects should be completed to confirm these findings. The methodology and analysis methods used in this study vary from many recent ARV studies that focus on the total amount of profit generated to determine if the sessions successfully met their goal. These modified design elements are provided as a guide for future prospective studies that explore the factors that may impact the results of an ARV session and the significance of those sessions.

All viewers and all judges involved in this study were very experienced and extremely well-respected for their knowledge and abilities with viewing and/or judging. Though this is a small sample size and a small number of sessions, it is clear that the choice of judges can have a significant impact on the results of an ARV study. The judging methods varied (J1 used an intuitive judging method which was completed quickly; J2 used a more procedure-based method based on the Targ Scale and evaluated the target selection based on significant differences between the ratings), but this study was not designed to evaluate one judging method against another. It would be incorrect to assume that one judging method is superior to another based on the results of this study.

By randomly selecting a control investment to compare with the target investment, this study demonstrates that the targeted investment had a marginally more significant impact on the results than the

control investment. When the judges' decisions were applied to the control investment, the results were nearly chance, but when they were applied to the target investment, the decisions made by J2 showed a very strong variation from chance although they were in the negative direction.

In summary, the decisions of the judges varied significantly with regard to the targeted investment, but were nearly at chance for the control investment. Also, when focusing on the targeted investment, J2 demonstrated a strong influence toward incorrectly predicting the market performance while J1 performed at chance. This study demonstrates that the choice of judges is extremely important when performing an ARV study and that judges appear to respond more strongly to a targeted investment than to a random investment that is not targeted.

This study appears to have identified an effect that is produced by different judges in an ARV project. Additional studies should be pursued to determine if certain judging protocols are more accurate than others and to what extent the judges play a role in the results of an ARV project. More importantly, future studies or projects using ARV should consider evaluating the performance of the judges who have been involved in previous ARV studies. It is important to be cautious in the selection of viewers for ARV projects, but the judges also can have a strong effect on the outcome of the project. Even judges who are well-trained and familiar with judging protocols may produce inaccurate results that could influence the results of the study.

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APPENDIX

ARV For Profit—Data Summary
August 7–11, 2017: Pilot Test to Adjust Protocol
Timeframe: Aug. 14–Nov. 10; skipped Sept. 4–8 due to holiday

Exp = Experimental Investment Instrument (used for investment)
Ctrl = Control Investment Instrument (not used for investment)
J1H = Judge 1 Hit (correct prediction)
J1M = Judge 1 Miss (incorrect prediction)
J2H = Judge 2 Hit
J2M = Judge 2 Miss
NC = No call by the judge (no investment made)

	Exp %Chng	**Ctrl %Chng**
Aug. 14–18: Week 1	0.74 J1M J2H	0.79 J1M J2H
Aug. 21–25: Week 2	0.74 J1& J2 NC	4.34 J1& J2 NC
Aug. 28–Sept 1: Week 3	1.67 J1H J2H	2.06 J1H J2H
Sept. 4–8: Skipped (Holiday)	—	—
Sept. 11–15: Week 4	3.43 J1M J2NC	1.15 J1H J2NC
Sept. 18–22: Week 5	0.99 J1M J2M	2.66 J1M J2M
Sept. 25–29: Week 6	2.60 J1 & J2 NC	46.87 J1& J2 NC
Oct. 2–6: Week 7	0.73 J1M J2NC	7.12 J1M J2NC
Oct. 9–13: Week 8	0.53 J1H J2M	2.35 J1M J2H
Oct. 16–20: Week 9	3.57 J1H J2M	3.70 J1H J2M
Oct. 23–27: Week 10	0.00 J1H J2M	1.41 J1H J2M
Oct. 30–Nov 3: Week 11	2.75 J1H J2M	5.84 J1M J2H
Nov. 6–10: Week 12	3.62 J1M J2M	9.41 1H J2H

Experimental Stock 1:

Judge	**Hits**	**Misses**	**No Call**	**Cumulative**
1	5	5	2	0
2	1	7	4	–6
CommonScores (both judges in one week)	1	3	2	–2

Control Stock 2:

Judge	**Hits**	**Misses**	**No Call**	**Cumulative**
1	6	4	2	2
2	5	3	4	2
Common Scores (both judges in one week)	3	1	2	2

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RESEARCH ARTICLE

The Psychology and Parapsychology of Spiritual Emergency

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Abstract—A defining aspect of Spiritual Emergency (SE) is a 'Psychic Opening', which may predict psi performance. This study tested paranormal (psi) performance of individuals who had or were having experiences of Spiritual Emergency (i.e. 'SE-experients'), and compared their performance against controls. The study also assessed psychological aspects of SE to differentiate it from psychosis and other proposed psi-inhibitive symptoms—namely, alogia (i.e. poverty of speech), depression, anxiety, and stress. Two groups of participants were formed: controls (mainly psychology students) and SE-experients. Participants either completed the study on a computer in the laboratory or online. Questionnaires on Spiritual Emergency (which includes a subscale on Psychic Opening), positive symptoms of psychosis, alogia, spiritual identity, paranormal belief, mysticism, depression, anxiety, and stress, were administered to participants, who then completed the Imagery Cultivation (IC) picture-identification psi task, which uses a shamanic-like journeying protocol (Storm & Rock, 2009a, 2009b). The differences between controls and SE-experients on the psi measures, Direct Hitting (as a percent hit-rate) and Mean Rank Scores, were not significant, but the Sum-of-Ranks difference was highly significant. Also, SE-experients had a marginally significant Mean Rank Score. Direct Hitting did not correlate significantly with any variable, except Rank Scores, which correlated significantly with Psychic Opening, spiritual identity, and paranormal belief, and marginally significantly with Spiritual Emergency. Direct Hitting, Rank Scores, and SE did not correlate significantly with alogia, depression, anxiety, or stress, but the psychosis measure did correlate significantly with alogia,

depression, anxiety, stress, and SE. The statistical evidence suggests that some proportion of SE-experients experience Psychic Opening. While SE and psychosis overlap, only SE was predicted by spiritual identity, extrovertive mysticism, and paranormal belief (but not alogia), whereas psychosis was predicted by alogia only.

Keywords: imagery cultivation; psychic ability; psychosis; sheep–goat effect; spiritual emergency

INTRODUCTION

There is no research on the psi-performance capabilities of individuals undergoing Spiritual Emergency (SE; aka transpersonal crisis). Grof and Grof (1991) defined SE as "critical and experientially difficult stages of a profound *psychological transformation* that involves one's entire being" (p. 31; our italics). Those who experience SE can find themselves in non-ordinary (altered) states of consciousness that "involve intense emotions, visions, and other sensory changes, and unusual thoughts, as well as physical manifestations" (p. 31). As implied in the definitions, emergence (transformation) due to SE seems largely inevitable, though it is an implied outcome; likewise, emergence can occur without the obvious signs of SE: "sometimes the process of spiritual awakening is so subtle and gradual that it is almost imperceptible" (p. 35). As a group, individuals who undergo SE (i.e. 'SE-experients') may be differentiated from the general population by not only having high rates of (self-reported) Psychic Opening, but they may also perform well on objective psi tests. Decades of research on SE and parapsychology give rise to this assumption. The research described in this paper is a step toward clarifying and understanding the suggested relationship among psi, Psychic Opening, and Spiritual Emergency.

Spiritual Emergency, Psychopathology, and Psi

Grof and Grof (1989, 1991) have found SE to be a multidisciplinary concept that assimilates research findings from fields that include experimental and clinical psychiatry and psychology, humanistic and existential psychotherapies, and consciousness research. During SE there can be a marked presence of allegedly 'psychic' (psi) abilities. Specifically, accurate precognition of future situations, clairvoyant

perception, and telepathic abilities are said to occur while experiencing Psychic Opening (Grof & Grof, 2017). The Grofs tell us that most of the psychic abilities that emerge during SE tend to be temporary, but for some individuals the successful resolution of the crisis is associated with the emergence of a new capacity or talent such as an increase in creativity, intuitive ability, or, in rare circumstances, the development of a genuine 'psychic gift'. Negative reactions to these experiences can parallel the "psychopathological crisis atmosphere" and "psychotic break" (e.g., delusions, hallucinations) that Ullman (1977, p. 563) mentions, and it is understandable that a clinician might mistake the crisis experience for psychosis.

Little has changed over the decades—indeed, in their comprehensive review, Harris et al. (2019) noted that the mounting evidence for SE as a construct independent of psychosis has helped justify the diagnostic v-code V62.89 in the *Diagnostic and Statistical Manual of Mental Disorders, 5th edition* (*DSM–5*; American Psychiatric Association, 2013), but "clinicians are failing to utilize the code due to a lack of understanding and knowledge" (p. 89). But even a casual inspection can reveal a difference: While the individual afflicted by a true psychosis lacks the insight to see that the condition has something to do with their own psyche, SE-experients are "lucid and have a sense of their own inner processes [and] realize that the changes in their experiential world are due to the changes they are experiencing within and are not the cause of external events" (Grof & Grof, 1991, p. 44). Also, while SE (insofar as it is a noticeable crisis state) might at first appear like psychosis, and vice versa, psychosis does not promise emergence: a "gradual and subtle unfolding of spirituality that leads to a profound shift in values and/or a more fulfilling way of life" (Cooper et al., 2015, p. 243).

Psychosis and SE do bear some similarities; they even correlate (see Bronn & McIlwain, 2015). However, the evidence shows that SE differs from psychosis (Bronn & McIlwain, 2015; Harris et al., 2015), and it is noted that conventional understandings about the latter overlook the spiritual experiences that can prevail during and after psychosis (Goretzki et al., 2009; Grof, 1985; Grof & Grof, 1989, 1991; Harris et al., 2019; Phillips et al., 2009). Bronn and McIlwain were among the first to take an empirical approach to the dichotomy, and they found

that SE is a distinct construct "and should be differentiated from psychopathology" (p. 367). While four related symptoms—namely alogia (disfluency of thought and speech), depression, anxiety, and stress—are often comorbid with psychosis (Buckley et al., 2008; Hales et al., 2014), SE differs from psychosis by its "divergent relationship with alogia, depression, anxiety, and stress" (Bronn & McIlwain, 2015, p. 363). Storm et al. (2017) have since confirmed that scores on the Spiritual Emergency Scale (SES; a 30-item scale constructed by Goretzki et al., 2013, to measure Spiritual Emergency) do not correlate significantly with depression as measured on Beck's Depression Inventory II (BDI-II; Beck et al., 1996).

Harris et al. (2015) have expressed their reservations about the SES, suggesting that crisis variables should correlate with Spiritual Emergency (it being a crisis). And since they showed that psychosis (but not SES) was predicted by dissociation and emotional instability, they defaulted to the conclusion, shared by Cooper et al. (2015), that "the SES is measuring spiritual emergence and not SE" (p. 277).[1] Of course, an alternative view suggested by such findings is that SE-experients present with a unique set of crises characterized by and related to their experiences (more on this issue in the **Discussion**).

While the correlations between the SES and various psychosis indicators are significant (Goretzki et al., 2009, 2013), items on Kundalini awakening, shamanic crisis, and Psychic Opening (among others) were included in the SES based on research by Grof and Grof (1989), and these may help demarcate SE from psychosis. We might even assume such factors (especially *psychic opening* as the term suggests) would logically be better predictors of psychic ability than clinical measures. However, while Storm and Goretzki (2020) found that SES scores correlated significantly and positively with outcome on a precognition task, Psychic Opening did not significantly predict the same outcome, possibly because the sample comprised mostly university students (not individuals undergoing SE), and variance on Psychic Opening was low. The present study aims to address the sampling problem by targeting SE-experients.

In addition, there are important parapsychological implications in the distinction between psychotics and SE-experients, whereby the former are burdened by the negative (depression, anxiety, and stress)

and cognitive (alogia) symptoms of psychosis, whereas the latter are not (Bronn & McIlwain, 2015). Specifically, these symptoms (especially depression and anxiety) are not likely to be psi-conducive, whereas paranormal belief (PB) is well-established as being psi-conducive; PB measures often correlate positively with psi performance, indicating the so-called 'sheep–goat effect', with 'sheep' tending to perform above chance on psi tests, and 'goats' not (e.g., see Storm & Tressoldi, 2017). Specifically, scores on one PB measure (namely the Australian Sheep–Goat Scale; Thalbourne, 1995) correlated significantly with SES and Psychic Opening scores, and the correlation between PB scores and psi-task outcomes was marginally significant for a subsample of paranormal believers (i.e. sheep; see Storm & Goretzki, 2020). We suggest that PB measures may have a discriminative capacity in that they may help distinguish SE from psychosis. To go further, Thalbourne and Storm (2019) stated that " . . . there is no burgeoning need to pathologize paranormal believers, even if measures suggest a tendency for characteristic symptoms" (p. 181).

A mix of findings exist regarding PB and depression—some studies have found a positive relationship (Thalbourne, 2005; Thalbourne & Delin, 1994; Thalbourne & French, 1995), whereas others have found no association between the two (Zebb & Moore, 2003). Also, an association between depression and anxiety has been partially supported in past research (Billows & Storm, 2015). It is therefore difficult to say whether depression and anxiety predict PB and/or psychic ability, but it is more likely that they are psi-inhibitive. For example, although Irwin and Watt (2007) report suggestive evidence that anxiety is related to PB, anxiety is linked to neurotic behavior (Palmer, 1978, 1982), with some evidence that highly neurotic participants perform poorly on ESP tests. In other words, positive correlates of PB, like anxiety and depression, are not necessarily psi-conducive even though PB is. Likewise, little or nothing is known about the degree to which two other psychosis correlates—namely, alogia and stress—affect psi performance (Thalbourne & Storm, 2019). Generally, therefore, it is not known whether correlates (or hypothesized correlates) of PB, such as alogia, depression, anxiety, and stress, predict psi performance.

The Study Design

We aim to test two groups (controls and SE-experients drawn from relevant populations) in a precognitive, picture-identification task that is based on the Imagery Cultivation Model (Storm & Rock, 2009a, 2009b). After a shamanic-like journeying protocol is administered, participants attempt to identify a future target picture in a random array that includes four decoys, with success in the task indicated by a 'Direct Hit' (where the target photo was ranked #1 by the participant). A target pool of 300 pictures compiled by May (2007; see also May et al., 2012) is used—these are presented on a computer monitor. We hypothesize that SE-experients are higher in Psychic Opening than controls (non-SE-experients). It is further hypothesized that SE-experients demonstrate superior psi performance compared with controls. We also aim to determine which of the scales and sub-scales listed below (see **Measures**) correlate with psychic ability.

A second aim of the study was to show that SE is a distinct and measurable construct, distinguishable from psychosis by its divergent relationship with alogia, depression, anxiety, and stress. To do that we planned to test the internal consistency, and convergent and divergent validity, of two related scales, the SES (already mentioned) and the Experiences of Psychotic Symptoms Scale (EPSS; Goretzki et al., 2009) to confirm the hypothesized differences. Multiple Regression Analyses were also to be conducted to test for predictors of psi ability, psychosis, and SE. This research will further our understanding about Spiritual Emergency, psychosis, and psi ability.

METHODS

Participants

Initially, first-year psychology students (n = 92), who signed up online, were tested, and they received credit for participation as part of their curriculum program (using the Research Participation System). There were a handful of participants who became aware of the project through flyers or online advertisements on various university websites (n = 8). They contacted the principal investigator Lance Storm (L.S.) via ballot-box slip or SMS, so that suitable days and times for testing could

be arranged. The target of 100 participants for the control sample was attained by Friday, May 3, 2019.

SE-experients had to have experienced (or were experiencing) Spiritual Emergency (SE). They were assessed and approved as suitable by the second author and experimenter (M.G.), who is clinically trained. Many participants self-identified as SE-experients, and they were recruited through websites dedicated to various phenomena experienced during SE.[2] These participants provided brief outlines of their experiences via email and, where necessary, further information was requested (via phone or email) to determine if the experience contained SE-type phenomena. Some participants were referred to the study by those who had already participated, and others were referred through professional networks dedicated to understanding and studying SE experiences. Through the correspondence, we were able to confirm that we did recruit participants of both types: those who *were having* and those who *had had* SE.

The target of 100 SE-experients was surpassed as online admissions were not regulated for strategic purposes, as now explained. As is the case with online studies, a small number of participants completed the questionnaires within too short a time period (as fast as 2½ minutes!), so these were deleted (prior testing showed that 10 to 15 minutes is required just to read all the questionnaires, which does not include audio-listening time of 9½ minutes, and time to write the mentation). The number was further reduced by deleting unapproved participants. As the study had to be capped at 100 to eliminate optional extension (a questionable research practice),[3] legitimate 'spill-over' participants are being held over for a follow-up study (assuming the final number reaches a sizeable proportion). The target for the SE-experient sample of 100 participants was attained by June 15, 2020.

The final sample consisted of 200 participants (as planned) with a mean age of 33 years (SD = 16 years); 48 males, 152 females. The mean age of the controls was 23 years (SD = 11 years); 24 males, 76 females (total n = 100). The mean age of the SE-experients was 44 years (SD = 15 years); 24 males, 76 females (total n = 100). The age difference was significant, $t(184.98) = 11.08$, $p < .001$ (two-tailed). (Of course, it is to be expected that SE-experients drawn from the wider community will be older than a student sample.)

Measures

(1) Spiritual Emergency Scale (SES; Goretzki et al., 2014): The SES consists of 30 items (see Appendix), each using a five-point Likert scale (1 = 'Never' to 5 = 'Very Often') to measure the experience of SE. Summed scores give an SES score, from 30 to 150. The SES has demonstrated good psychometric properties (Goretzki et al., 2009, 2014). On multiple occasions, the SES has been shown to have a single underlying dimension (Storm & Goretzki, 2016), it has good test-retest reliability (.84), and it has good convergent and concurrent validity. The SES correlates .70 to .73 with the EPSS, indicating that SE overlaps with psychosis (Bronn & McIlwain, 2015; Goretzki et al., 2009). Reliability on Cronbach's α ranges between 0.71 and 0.95 (Bronn & McIlwain, 2015).

(2) Experiences of Psychotic Symptoms Scale (EPSS; Goretzki et al., 2009): A 15-item scale, with 12 multiple-choice items measuring positive symptoms of psychosis (EPSS-POS), and a 3-item multiple-choice subscale to assess the negative psychosis symptom 'Alogia' (i.e. constraints in the production and fluency of thought and speech). Example item: "Have you ever experienced a time when your sentences were unclear or didn't make sense?" Scoring for all items is via five-point Likert-scale (1 = 'Never' to 5 = 'Very Often'). Cronbach's α for EPSS-POS = 0.87, and for Alogia = 0.78 (Bronn & McIlwain, 2015).

(3) Spiritual Identification Scale (SIS; Astin et al., 2011): A 13-item scale to assess the degree to which participants identify themselves as "spiritual" (full-scale theoretical range: 13 to 41). Sample item: "People can reach a higher spiritual plane of consciousness through meditation or prayer." Cronbach's α = 0.88 (Bronn & McIlwain, 2015).

(4) Rasch-Scaled Australian Sheep–Goat Scale (RASGS; Thalbourne, 1995): An 18-item scale measuring belief and alleged experience of paranormal phenomena. Each item scores: 0 points (False), or 1 point (Uncertain), or 2 points (True). Raw range is 0 to 36; Raw Mean = 18. The ASGS data are then top–down purified (Rasch-scaled) to eliminate age and gender bias from the scale (Lange & Thalbourne, 2002), and this procedure alters the scoring range and mean (standardized mean = 25, SD = 5). RASGS scores range from 8.13 to 43.39. Cronbach's α ranges between 0.91 and 0.95 (Billows & Storm, 2015; Storm & Thalbourne, 2005).

(5) Mysticism Scale (MS; Hood, 1975): A 32-item multiple-choice scale that assesses commonly reported mystical experiences that provide the "basic essence to human religious experience" (Hood, 1975, p. 29). The MS comprises three factors (Hood, et al., 1993): Introvertive Mysticism (e.g., an "experience of nothingness"; Hood et al., 2001, p. 692), Extrovertive Mysticism (e.g., "the self reaches a unity with the multiplicity of objects in the universe"; Hood et al., 2001, p. 692), and Religious Interpretation ("experience expressed in explicit religious language"; Hood & Francis, 2013, p. 36). Items are scored on five-point scales ranging from *definitely not true* (–2) to *definitely true* (+2). Cronbach's α for the subscales range between 0.77 and 0.92 (Bronn & McIlwain, 2015).

(6) Depression Anxiety Stress Scales (DASS-21; Lovibond & Lovibond, 1995): A 21-item multiple-choice scale that measures three state factors (Depression, Anxiety, Stress) with seven items allocated to each. Participants rate their depression, anxiety, and stress on a four-point Likert-type scale ranging from 0 ('Did not apply to me at all; i.e. NEVER') to 3 ('Applied to me very much, or most of the time; i.e. ALMOST ALWAYS'). Scores range theoretically from 0 to 21. Cronbach's α ranges between 0.76 and 0.91 (Bronn & McIlwain, 2015).

Materials

Materials include a computer program containing Information and Consent pages (i.e. screens), plus separate pages for each of the measures listed above. Also presented are a page of five photographs with rank-scoring boxes for each photo, an outcome page, and a feedback page.

Apparatus

(1) A gallery of 300 photographs compiled by May (2007) from the Corel Stock Photo Library of Professional Photographs. The picture set consists of 12 Groups × 5 Categories × 5 Photographs = 300 photographs (presented onscreen via desktop computer or laptop).

(2) A true-noise Random Number Generator (Schmidt 1970, 1973). The RNG was purpose-built by Helmut Schmidt (dimensions: 25 × 30 × 7.5 cm). On the face side are 12 green lamps in a circular array and a red LED score-display in the center.

(3) Imagery Cultivation Audio (.mp3) sound file (13 MB; duration: 9½ minutes) containing instructions in the form of guided imagery in conjunction with relaxing meditative music (administered through headphones; for details, see Step 2 in **Procedure** below).

Procedure

The following two types of participants were tested:

Controls. Control participants (mainly first-year psychology students) signed up on the School of Psychology website (Research Participation System) for testing in the experimenters' laboratory (one session to complete the measures and the psi test). Other students sought participation after seeing online advertisements or flyers advertised around the campus.

SE-Experients. Participants first had to meet the criteria for suitability, and this was done by email or telephone interview conducted by the second experimenter (M.G.). Suitable participants were given a unique identifying code to log in to the test website where they completed the measures and the psi test (data were time-stamped to confirm validated participation).

Step 1. Instructions outlining the experiment were presented onscreen, and if participants chose to participate, they moved to another page that listed a series of consent statements. Participants then provided demographics details, and completed the SES, EPSS, SIS, RASGS, MS, and DASS-21.

Step 2. Via onscreen message, all participants were informed that they would undergo the Imagery Cultivation (IC) procedure (duration: 9½ minutes). They were asked to relax in their chair, start the pre-recorded instructions, close their eyes, and listen via headphones to pre-recorded instructions (audio) adapted from Harner (1990): Excerpt: " . . . Now visualise the future target photograph before you. Study the photograph in all its detail. Remember this information for later." Participants could not proceed to Step 3 unless they listened to the audio. After the audio, they answered a test question, and were then instructed to type onscreen notes (mentation) about their impressions of the future target. At this stage, neither the participant nor the experimenters (L.S. or M.G.) knew what the target was since it had not yet been generated.

Step 3. Five photographs were selected using a randomization method in the computer application for SE-experients, or the RNG for controls. The selection procedure followed May et al.'s (2012) recommendation. First, the computer or RNG randomly selected one Group of twelve, followed by one photograph from each of five Categories in that Group, all taken from the fuzzy set, encoded target pool totaling 300 photographs. Target selection did not take place until Step 5.

Step 4. Ranking—Once the set of five photos appeared onscreen, the experimenter instructed the participant to rank the five photographs from 1 to 5 (#1 = 'most likely' photo that matches the mentation, to #5 = 'least likely' photo that matches the mentation). The participant was permitted to re-read his/her mentation, in order to prompt his/her memory, thereby assisting him/her in the ranking process. Participants had to type under each photo the respective rank number.

Step 5. The computer generated the target photograph from the five that were presented previoiusly onscreen (MCE = 20%). The computer automatically presented the target as feedback to the participant and gave the rank number that had been given by the participant for that photo (if the photo was ranked #1, it was a Direct Hit). The participant was debriefed, and confidential results were emailed to each participant at a later date.

Data Analyses

Part I of the analytical component of this study involved (i) testing the differences on paranormal performance (Direct Hitting, Rank Scores, and Sum-of-Ranks) between SE-experients and controls; and (ii) determining correlations between psi outcomes (Direct Hitting and Rank Scores) and the ten variables on six measures (SES, EPSS, SIS, RASGS, MS, and DASS-21), in accordance with the hypotheses given below. All statistical analyses were conducted using SPSS (Version 26.0).

Part II of the study included confirmatory and exploratory exercises aimed at assessing the psychometric properties of the SES and the EPSS. In particular, these two scales were tested to confirm convergent and divergent relationships between SE and psychosis. As part of these analyses, a correlation matrix was scrutinized to decide whether to conduct Multiple Regression Analyses (MRA) using suitable demographic details (gender, age, etc.) and scale data (EPSS, SIS, RASGS, SES, MS) as Independent Variables. The Dependent Variables

(DVs) would be a psi measure (Direct Hitting or Rank Score), the SES, and the EPSS.

Hypotheses

Hypothesis 1. There is a difference in psi-scoring between SE-experients and controls, with SE-experients scoring (i) higher on Direct Hitting, and (ii) lower on Mean Rank Score.

Hypothesis 2. Direct Hitting correlates positively (Rank Scores correlate negatively) with Spiritual Emergency (SE), Psychic Opening (a subscale of SES), spirituality (SIS), paranormal belief (RASGS), and mysticism (MS)—but neither of the two psi measures (Direct Hitting and Rank Score) correlate with psychosis (EPSS-POS), alogia (an EPSS subscale), depression, anxiety, or stress (DASS-21).

RESULTS

Preliminary Findings

Direct Hitting for the whole sample was 50 hits out of 200, or 25% (where MCE = 20%). The effect is significant, Binomial Exact $z = 1.68$, $p = .049$ (one-tailed). The Mean Rank Score for the whole sample was 2.84, where MCE = 3.00 (*Mdn* = 3.00). The effect is significant, $t(199) = -1.67$, $p = .049$ (one-tailed). Ranks are listed in Table 1.

TABLE 1
Rank Scores: Full Sample (N = 200)

Rank Score	Frequency	%
1	50	25.0
2	37	18.5
3	39	19.5
4	44	22.0
5	30	15.0
Total	200	100.0

The 'Sum-of-Ranks' test using the sum of ordinal weighted ranks formula was also calculated (see Solfvin et al., 1978, pp. 97–99). Solfvin et al. assign a score (or 'weight') to all ranks (e.g., rank

#1 scores 1, etc.), and then score counts are totaled.[4] An ordering of observed distributions is therefore induced. For the sample ($N = 200$), the Sum-of-Ranks statistic is significant, $z = 3.98$, $p = 3.40 \times 10^{-5}$ (one-tailed). This result seems generous given the p value is three orders of magnitude larger than the p values for the previous two results (i.e. for Direct Hitting and Mean Rank Score). However, Sum-of-Ranks conveys more information than the conservative Direct-Hit count (less so for Mean Rank Score)—for example, note the significant avoidance effect relevant to rank #5 (a mere 15%) in Table 1, and upward shifts toward better scoring ranks, $z = 1.68$, $p = .046$.

Statistics for the six measures (and relevant subscales) are given in Table 2, which also shows ANOVA results assessing the differences between the two groups, controls and SE-experients. All differences were significant except for Alogia, DASS-DEP, and DASS-AX (see the **Discussion** section below for comments).

TABLE 2
Descriptives: Means & *SD*s Full Sample ($N = 200$), Controls ($n = 100$) & SE-Experients ($n = 100$)

Variable	Full Sample (*SD*)	Controls (*SD*)	SE-Experients (*SD*)	*F* (1, 198)	*p* (2t)
1a. SES	66.01 (24.79)	49.90 (17.57)	82.12 (20.05)	146.10	< .001
1b. Psychic Opening	14.08 (5.29)	10.93 (4.29)	17.23 (4.22)	109.51	< .001
2a. EPSS-POS	27.00 (11.30)	24.36 (8.67)	29.63 (9.47)	16.86	.013
2b. Alogia	6.61 (2.72)	6.64 (2.79)	6.58 (2.65)	-0.02	.876
3. SIS	32.41 (6.57)	28.37 (6.06)	36.44 (4.15)	120.69	< .001
4. RASGS	26.33 (7.07)	22.11 (5.68)	30.55 (5.67)	110.51	< .001
5. MS	110.19 (31.38)	89.40 (23.86)	130.97 (23.19)	156.10	< .001
6a. DASS-DEP	5.97 (4.49)	6.32 (4.83)	5.62 (4.12)	-1.22	.272
6b. DASS-AX	6.04 (4.39)	6.43 (4.56)	5.64 (4.19)	-1.63	.203
6c. DASS-ST	8.07 (4.55)	8.71 (4.49)	7.42 (4.55)	-4.08	.045

SES = Spiritual Emergency Scale; EPSS-POS = Experiences of Psychotic Symptoms Scale (positive symptoms); SIS = Spiritual Identification Scale; RASGS = Rasch-Scaled Australian Sheep–Goat Scale; MS = Mysticism Scale; DASS = Depression Anxiety Stress Scale (DEP = Depression, AX = Anxiety, ST = Stress).

Planned Analyses—Part I

Hypothesis 1. There is a difference in psi-scoring between SE-experients and controls, with SE-experients scoring (i) higher on Direct Hitting, and (ii) lower on Mean Rank Score:

(i) Direct Hitting for SE-experients was 26% (26/100), which is higher than the hit rate of 24% for the controls (24/100). These hit rates are in the directions expected. The Binomial hit-rate for SE-experients could be considered marginally significant (Exact $z = 1.38$, $p = .087$; Effect Size [ES] = $z/\sqrt{n} = .14$), but controls did not produce a significant hit rate (Exact $z = 0.88$, $p = .189$; ES = .09). A Mann-Whitney test indicated that the difference was not significant, $U = 4900.00$, $p = .435$ (one-tailed); Cohen's $d = .04$.

(ii) The Mean Rank Score for SE-experients was 2.71 (where MCE = 3.00), which is better than the Mean Rank Score for the controls of 2.96. Ranks for both groups are listed in Table 3. A Mann-Whitney test indicated that the difference was not significant, $U = 4501.50$, $p = .108$ (one-tailed); Cohen's $d = .17$.

TABLE 3
Rank Scores: Controls and SE-Experients

Rank Score	Control Group Frequency	%
1	24	24.0
2	17	17.0
3	16	16.0
4	25	25.0
5	18	18.0
Total	100	100.0

Rank Score	SE-Experients Group Frequency	%
1	26	26.0
2	20	20.0
3	23	23.0
4	19	19.0
5	12	12.0
Total	100	100.0

For SE-experients, the Sum-of-Ranks statistic was $z = 4.94$ ($p < .001$; ES = .49), but for controls the statistic was only $z = 0.61$ ($p = .271$; ES = .06). Scoring for SE-experients was significant, and the z-score difference ([4.94 – 0.61] / √2 = 3.06) between the two groups was also significant ($p < .01$). Note, too, for SE-experients, the very low number of hits on rank #5 of only 12% (MCE = 20%), thus indicating psi-avoidance of a non-target ($z = 1.87$, $p = .030$), suggesting a preference for better ranks.

Hypothesis 2. Direct Hitting correlates positively (Rank Scores correlate negatively) with SES, Psychic Opening, SIS, RASGS, and MS—but neither of the two psi measures correlate with EPSS-POS, alogia, depression, anxiety, or stress.

As some scales were significantly skewed, and Direct Hitting is a dichotomous measure, and Rank-score is ordinal, Spearman's rho tests were conducted. Direct Hitting did not correlate significantly with any of the five variables, although all were positively correlated. However, as predicted, the other five variables (i.e., EPSS-POS, alogia, depression, anxiety, and stress) did *not* correlate significantly with Direct Hitting, although the correlations were negative.

Rank Scores correlated negatively and significantly with RASGS, $r_s(198) = -0.12$, $p = .048$, so that high paranormal belief scores tended to indicate better Rank Scores. Rank Scores also correlated negatively and significantly with Psychic Opening, $r_s(198) = -0.12$, $p = .049$; and SIS, $r_s(198) = -0.13$, $p = .036$; and marginally significantly with SES, $r_s(198) = -0.11$, $p = .061$. Given these results, we ran a multiple regression analysis (MRA), with Rank Scores as the criterion variable, but the MRA failed. Rank Scores did not correlate significantly with MS (but the correlation was negative). As expected, Rank Scores did *not* correlate significantly with EPSS-POS (psychosis), alogia, depression, anxiety, or stress (though the correlations were all positive).

Planned Analyses—Part II

Reliability assessments (internal consistency) for the two scales (SES and EPSS) are indicated using Cronbach's α. For the SES, Cronbach's $\alpha = .96$. For the EPSS, Cronbach's $\alpha = .92$. For the measure of positive psychosis symptoms (EPSS-POS), Cronbach's $\alpha = .90$. For alogia, Cronbach's $\alpha = .78$. Bronn and McIlwain (2015) conducted a

number of statistical analyses on the SES and EPSS-POS, to evaluate their relationships with each other and with a range of possible correlates; namely, alogia, depression, anxiety, stress, spiritual identity, and mysticism.

For the present study, an inspection of three correlation matrices (one each for full sample, controls, and SE-experients), featuring all relevant psychological variables for confirmatory purposes (10 in total), revealed that all 36 correlations of interest were significant. However, due to multiple testing, the critical alpha ($\alpha = .05$) was divided by 36 (the number of tests), rendering a new critical $\alpha = 1.39 \times 10^{-3}$. The correction left us with 35 (97%) significant correlations. Table 4 lists the relevant correlations, most of which confirm the theoretical suppositions and/or past findings (all tests are two-tailed).

TABLE 4
Correlations: Pearson's *r* for the Full Sample (*N* = 200), Controls (*n* = 100), SE-Experients (*n* = 100)

Variable	*r* (Full Sample)	*r* (Controls)	*r* (SE-Experients)
1. EPSS-POS × Alogia	.72*	.78*	.73*
2. EPSS-POS × Depression	.40*	.36*	.54*
3. EPSS-POS × Anxiety	.41*	.36*	.55*
4. EPSS-POS × Stress	.37*	.33	.51*
5. Alogia × Depression	.51*	.42*	.62*
6. Alogia × Anxiety	.51*	.43*	.60*
7. Alogia × Stress	.47*	.36*	.59*
8. SES × EPSS-POS	.68*	.80*	.60*
9. SES × SIS	.67*	.46*	.47*
10a. SES × MS-INT	.67*	.61*	.40*
10b. SES × MS-EXT	.73*	.54*	.54*
10c. SES × MS-REL	.70*	.46*	.50*

Degrees of freedom (*df*) = 198; EPSS-POS = Experiences of Psychotic Symptoms Scale (positive symptoms); SES = Spiritual Emergency Scale; SIS = Spiritual Identification Scale; MS-INT = Introvertive Mysticism; MS-EXT = Extrovertive Mysticism; MS-REL = Religious Interpretation; After correction: * $p < 1.39 \times 10^{-3}$.

Rows 1 to 7 in Table 4 generally replicate the findings in Bronn and McIlwain (2015, pp. 362–363) under their Hypothesis 5 for their 'student' sample and their 'spiritual' sample. In our sample, specifically, positive symptoms of psychosis (EPSS-POS) and alogia are positively correlated (full sample, SE-experients, and controls). Other Alogia correlates are listed in Table 4. Also, EPSS-POS is positively correlated with depression, anxiety, and stress for the full sample and the SE-experients (for controls, EPSS-POS is positively correlated with depression and anxiety).

Rows 8 through 10 in Table 4 replicate the findings in Bronn and McIlwain (2015, p. 363) under their Hypothesis 6 for their 'student' and 'spiritual' samples. In our sample specifically, SE positively correlates with EPSS-POS, spiritual identity (SIS), and the three mysticism factors for the full sample and the two subsamples.

Under their Hypothesis 7, Bronn and McIlwain (2015) ran regression analyses, and after controlling for a number of variables, they found that depression, anxiety, and stress did not predict SE for their 'student' and 'spiritual' samples. Bronn and McIlwain also found that alogia predicted SE in their 'spiritual' sample only (not their 'student' sample), whereas alogia predicted positive symptoms of psychosis in both samples. It is to be noted that 'comparisons' of two samples, requiring two separate statistical analyses (one for each sample), is only one approach to evaluating two different datasets. Statistical comparison of two groups (i.e. subsamples) can be achieved in a single hierarchical regression analysis by simply combining the samples into one dataset, and then entering the sample variable in the second of two blocks to determine the unique contribution to the model of the grouping (sampling) variable. We therefore present the results of two hierarchical MRAs; one with SES scores as the criterion variable (i.e. DV), and the other with EPSS-POS scores as the criterion variable.

Hierarchical Multiple Regression Analysis—Spiritual Emergency Scale (SES)

The significant relationships in our correlation matrix showed a number of intercorrelations justifying an MRA on SES as our criterion variable, which could build a model showing the influence of all relevant variables and also show, hierarchically, any additional contribution

based on known differences between the samples, as already indicated from the statistical differences listed in Table 2. This procedure also gives us an empirical advantage over the zero-order correlations presented in our matrix, in that the MRA Outputs in SPSS report semi-partial correlations (whereby shared variance between the predictor of interest and other predictors is removed while all variance in the criterion variable is left intact). In Block 1, we entered age, gender, EPSS-POS, Alogia, SIS, MS (i.e. the three subscales, MS-INT, MS-EXT, & MS-REL), RASGS, depression, anxiety, and stress, and in Block 2 we entered 'sample' (comprising two datasets, controls, and SE-experients, where controls are numerically coded as '1', and SE-experients are coded as '2').

The assumptions of normality and linearity, as determined by visual inspection of the histogram, PP-plot, and scatterplot, were not violated. Outliers were determined from the Mahalanobis Distance by which significant outliers are indicated when maximum observed values exceed the critical value given by the chi-square distribution, with degrees of freedom equal to the number of predictors in the model (i.e. 12). The critical p value (α) for this test was set at .001. After the removal of two outliers, the maximum observed distance of 29.80 did not exceed the critical chi-square value of 32.91. There was faint visual evidence of heteroscedasticity in the scatterplot, and the LOESS line was perhaps not ideal.[5] Specifically, the LOESS line had a slight U-shaped curved, suggesting a questionable structure in the model. This trend might indicate the possibility that the model is not necessarily best described as linear. Multicollinearity was not indicated, as Tolerance was greater than .2 (.37).

Age, gender, alogia, MS-INT, MS-REL, depression, anxiety, and stress were all excluded from Model 1, but EPSS-POS, SIS, RASGS, and MS-EXT were all positive predictors of SE as given by the significant t values (see Table 5). This model was significant, $F(12, 185) = 60.75$, $p < .001$ (two-tailed). The strongest semi-partial r is EPSS-POS (.29).

In Model 2, 'sample' (i.e. group membership) was a significant *positive* predictor ($\beta = .20$), corresponding to an adjusted R^2 of .812, up from .798 ($R = .893$), yielding a final $R^2 = .812$ ($R = .901$), so that the model explains about 81% of the variance in SES scores (though the adjusted value is slightly lower at 80%), *F-change* (1, 184) = 14.55, $p < .001$ (two-tailed). The strongest semi-partial r is EPSS-POS (.26), followed by 'sample' (.12). As Table 5 shows, all β values are significant,

TABLE 5
Hierarchical MRA: Predictors of SES Score

Block	ΔR^2	B	*Std. Error*	β	*t*	*p*	Semi-partial *r*
Step 1	.798						
EPSS-POS		1.27	.15	.49	8.64	< .001	.29
SIS		.63	.20	.17	3.17	.002	.11
RASGS		.42	.19	.12	2.19	.030	.07
MS-EXT		.41	.13	.22	3.20	.002	.11
Step 2	.015						
EPSS-POS		1.17	.15	.44	8.03	< .001	.26
SIS		.52	.19	.14	2.69	.008	.09
RASGS		.40	.18	.11	2.17	.031	.07
MS-EXT		.34	.12	.19	2.76	.006	.09
Sample		9.83	2.58	.20	3.81	< .001	.12

ΔR^2 = change in R^2 (between Block 1 and Block 2); *p* values are two-tailed; MS-EXT = Extrovertive Mysticism; Sample = controls + SE-experients.

and the model at Step 2 is significant overall, $F(13, 184) = 61.30$, $p < .001$ (two-tailed). Therefore, the variable 'sample' made a significant additional contribution to the model as a predictor of SE.

Hierarchical Multiple Regression Analysis—Experiences of Psychotic Symptoms Scale (EPSS)

We ran an MRA on EPSS-POS as our criterion variable. In Block 1, we entered age, gender, SES, Alogia, SIS, MS-INT, MS-EXT, MS-REL, RASGS, depression, anxiety, and stress, and in Block 2 we entered 'sample'. The assumptions of normality and linearity were not violated. After the removal of the same two outliers in the previous MRA, the maximum observed distance of 29.82 did not exceed the critical chi-square value of 32.91. Again, there was faint visual evidence

of heteroscedasticity in the scatterplot, though the LOESS line was relatively flat. Multicollinearity was not indicated, as Tolerance was greater than .2 (.35).

Virtually all variables were excluded from Model 1, except Alogia and SES, which were positive predictors of EPSS-POS (see Table 6). This model was significant, $F(12, 185) = 46.82$, $p < .001$ (two-tailed). The strongest of two semi-partial *r* values is SES (.32), followed by Alogia (.29).

In Model 2, 'sample' was not a significant positive predictor

TABLE 6
Hierarchical MRA: Predictors of EPSS-POS Score

Block	ΔR^2	B	Std. Error	β	*t*	*p*	Semi-partial *r*
Step 1	.752						
Alogia		1.39	.18	.40	7.79	< .001	.29
SES		.23	.03	.59	8.64	< .001	.32
Step 2	.000						
Alogia		1.40	.18	.40	7.76	< .001	.29
SES		.22	.03	.59	8.03	< .001	.30
Sample		.37	1.17	.02	.32	.751	.01

ΔR^2 = change in R^2 (between Block 1 and Block 2); *p* values are two-tailed; MS-EXT = Extrovertive Mysticism; Sample = controls + SE-experients.

(change in R^2 = 0.00), indicating no change from .752 (R = .867) in Model 1, so that the model explains about 75% of the variance in EPSS-POS scores (adjusted to 74%); *F-change* was not significant. The strongest semi-partial *r* is SES (.30), followed by Alogia (.29). The β values are significant, and the model at Step 2 is still significant overall, $F(13, 184) = 43.01$, $p < .001$ (two-tailed). However, 'sample' did not make a significant additional contribution to the model as a predictor of EPSS-POS.

DISCUSSION

The results for this study are generally favorable. Outcomes for Hypothesis 1, on group differences between SE-experients and controls, were all in the hypothesized directions for Direct Hitting, Mean Rank Score, and Sum-of-Ranks. While the first two group differences were not significant, the Sum-of-Ranks difference, which is a more sensitive measure, was significant. Also, SE-experients had a marginally significant Mean Rank Score compared with the chance baseline (MCE). Looked at another way, the SE-experients also showed significant avoidance of the poorest rank, #5. As we hypothesized, the SE-experients also reported more Psychic Opening than controls (see Table 2), which is phenomenologically associated with Spiritual Emergency (SE). It might be argued that Psychic Opening is rarely retained after crisis (see Grof & Grof, 1991), but we emphasize the point that the second author's (M.G.'s) interviews and correspondence with potential SE-experients were conducted to find participants who had or were having Spiritual Emergency (see **Procedure** above). Had a majority or all of our SE-experients *no longer been experiencing spiritual emergence*, the vast majority could be expected to have lost their psi ability (and even be on par with the controls), and we would not have found the differences reported here. It is *because* we found differences that the SES more likely measures emergency rather than emergence.

Results for Hypothesis 2 were mixed. The hypothesized directions were generally as expected, but none of the Direct-Hitting correlations were significant. The failure of Direct Hitting to correlate significantly with SES, Psychic Opening, spiritual identity (SIS), paranormal belief (RASGS), and mystical experience (MS) is somewhat surprising; it was noted, however, that effects were small ($r_s < 0.1$). However, Rank Score proved to be a better psi measure—it correlated significantly with RASGS, Psychic Opening, and SIS, and MS was a marginally significant correlate.

We also hypothesized that Direct Hitting would *not* correlate with the pathological measures, EPSS-POS (positive symptoms of psy-chosis), alogia, depression, anxiety, and stress—none of these were sig-nificant, but the relationships were negative as we would expect since we regard these pathological conditions as psi-inhibitive (again, effects were small, $r_s < .1$). As expected, Rank Scores did *not* correlate significantly with EPSS-

POS, alogia, depression, anxiety, or stress (though the correlations were all positive).

It is noted in Table 2 that SE (including Psychic Opening), EPSS-POS, SIS, RASGS, and MS, are at significantly *higher* levels for the SE-experients (compared with controls), but SE-experients are significantly *lower* in stress (not significantly lower for alogia, depression, and anxiety). It is possible, even likely, that there are psychological advantages in having high levels of spirituality, Psychic Opening, paranormal belief, and mystical disposition, alongside low levels of stress. Indeed, they may act as resilience factors that help combat positive symptoms of psychosis and SE, neither of which can be considered a desired state of mental health, though we do take issue with the claim that the latter indicates psychopathology. As stated by Bronn and McIlwain (2015) "SEs have the potential to be classified as a spiritual problem under the nonpathological V code of the *DSM-V*" (p. 348; see also, Lukoff & Lu, 1998; Turner et al., 1995).

Finally, we ran two MRAs: First, our results support those of Bronn and McIlwain (2015, p. 363), in that depression, anxiety, and stress did not predict SE. However, we found that alogia did not predict SE, which partly undermines the finding by Bronn and McIlwain who reported that "alogia significantly predicted SE in the spiritual sample . . . , but not in the student sample" (p. 363). Also, EPSS-POS, SIS, extrovertive mysticism (MS-EXT), and RASGS all predicted SE. We note too that the grouping variable 'sample' (which refers to participant source, SE-experients and controls, the latter of which were mainly first-year psychology students) made a statistically significant (albeit minor) additional contribution to the model as a predictor of SE.

In the second MRA, alogia predicted EPSS-POS, but the 'sample' variable did not make an additional contribution above and beyond alogia and SES scores, which confirms the SE overlap mentioned by Bronn and McIlwain, who note, "SE emerges as a distinct measurable construct, overlapping with positive symptoms of psychosis, distinguishable from the negative dimension of psychosis by its divergent relationship with alogia" (p. 346; see also Harris et al., submitted). Our findings bear this out—alogia does *not* predict SE, but both alogia and SES scores *do* predict positive symptoms of psychosis.

A final word on emergency vs. emergence: As noted above, Harris

et al. (2015) and Cooper et al. (2015) suggested that the SES is a measure of emergence rather than SE. A reviewer of this paper allowed for the possibility that the SES does measure SE, but it mainly picks up emergence. In spite of all the research addressing this dichotomy, we argue that a successful resolution of the issue will depend critically on the appropriateness of the distress/crisis measures chosen as possible predictors of SE. This assumption naturally arises from the fact that the experiences associated with SE (especially if traumatic) constitute, or can lead to, psychological crises *unlike other psychosocial stressors or crises*. We recommend that research now focus on using the SES that features modified response devices that screen for SE-experients in *current* emergency and *past* emergency where *only emergence* is evident in the latter, and proceed from there to test appropriate crisis variables. As Harris et al. (2019) have advised, these variables would be "spiritual, or transpersonal, in nature," but certainly not be "attributable to a mental disorder" (p. 91; see also Turner et al., 1995). Past research shows deficiencies in this approach and therefore remains largely inconclusive.

In conclusion, the statistical evidence reported in the present paper indicates that SE experients experience more psi and report more Psychic Opening than controls. Rank Score correlated significantly with three measures (Psychic Opening, paranormal belief, and spiritual identity), and correlated marginally significantly with Spiritual Emergency. Direct Hitting, Rank Scores, and SE did not correlate significantly with measures often regarded as comorbid with psychosis (i.e., depression, anxiety, and stress), but the psychosis measure did correlate significantly with alogia, depression, anxiety, stress, and SE. While SE overlaps with psychosis (they predict each other), SE was differentiated from psychosis by not having alogia as a predictor, but does have spiritual identity, paranormal belief, and extrovertive mysticism as predictors (which was not the case for the psychosis measure).

NOTES

[1] For critiques on the SES, see Cooper et al. (2015) and Harris et al. (2015). We have addressed their concerns in Storm and Goretzki (2016).

[2] American Center for the Integration of Spiritually Transformative Experiences (https://aciste.org/), IKON (past students; Adelaide and

Brisbane campuses), Spiritual Emergence Network Australia, Yoga, mindfulness, professional contacts, and online groups (e.g., www.psychforums.com; www.shalomplace.org, forums.psychcentral.com, www.actualized.org/, ozvoices.org, imhu.org/coaching/directory, www.spiritualforums.com/vb/), Facebook groups (e.g., Osho, Spirituality, Mental Health, Positive Psychology, Spiritual Emergence Network, Psychology).

[3] Optional extension refers to the practice of continually testing participants past a designated *N* until the data yields a significant result in the higher *N*. This practice has been critiqued and tested by Bierman et al. (2016).

[4] Level of scoring is determined from the sum-of-ranks score and the corresponding *Z* score. $Z = (M - U_M \pm 0.5) / \sigma_M$, "where *M* is the observed sum-of-ranks, $U_M = N(R + 1) / 2$, and $\sigma_M = N(R - 1) / 12$. The 0.5 is the usual continuity correction and has sign opposite to that of $(M - U_M)$" (Solfvin, Kelly, & Burdick, 1978, p. 99). Psi hitting is indicated by a significant sum-of-ranks score that is lower (better) than MCE = 3.00.

[5] The LOESS ('local regression') line fits a smooth line to so-called 'residuals' (i.e. the difference between the observed values of the dependent variable and the predicted values). Patterns in the scatter of residuals may indicate other relationships not detected in the model.

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APPENDIX
THE SPIRITUAL EMERGENCY SCALE (SES)

Introduction: This research is seeking information about extraordinary experiences that occur in the natural, un-intoxicated state, so it is important that you do not include those instances when you may have been under the influence of drugs.
Instructions: Circle one answer only for each item: 'Never', or 'Not Often', or 'Sometimes', or 'Often', or 'Very Often'. [**Scoring**: 5-point Likert scale, Never = 1, Not Often = 2, Sometimes = 3, Often = 4, Very Often = 5]

1.	Have you ever lost your sense of reference as your outer and inner worlds dissolved? Never ● Not Often ● Sometimes ● Often ● Very Often ●
2.	Have you ever experienced the spontaneous production of complex visual geometrical images or chants inside your head? Never ● Not Often ● Sometimes ● Often ● Very Often ●
3.	Have you ever heard voices, music or the repetition of mantras, without knowing where they're coming from? Never ● Not Often ● Sometimes ● Often ● Very Often ●
4.	Have you ever experienced intense sensations of energy and/or heat streaming along your spine? Never ● Not Often ● Sometimes ● Often ● Very Often ●
5.	Have you ever experienced the spontaneous desire to create rituals? Never ● Not Often ● Sometimes ● Often ● Very Often ●
6.	Have you ever undertaken a powerful inner experience that involved a journey into another world? Never ● Not Often ● Sometimes ● Often ● Very Often ●
7.	Have you ever had the ability to move into and out of non-ordinary states of consciousness at will? Never ● Not Often ● Sometimes ● Often ● Very Often ●
8.	Have you ever developed a deep change in consciousness during which you lost contact with everyday reality? Never ● Not Often ● Sometimes ● Often ● Very Often ●
9.	Have you ever experienced insights and/or visions, in which you received secret or sacred teachings and healing powers to take back to the "ordinary" world? Never ● Not Often ● Sometimes ● Often ● Very Often ●
10.	Have you ever experienced an increased connection with animals and plants and the elemental forces of nature? Never ● Not Often ● Sometimes ● Often ● Very Often ●
11.	Have you ever had the experience of dealing with something that has a divine nature and is radically different from your ordinary perception of the everyday world? Never ● Not Often ● Sometimes ● Often ● Very Often ●
12.	Have you ever experienced the sense of becoming one with humanity, nature, the creative energy of the universe and/or God? Never ● Not Often ● Sometimes ● Often ● Very Often ●
13.	Have you ever spontaneously attained profound insights into the nature of reality? Never ● Not Often ● Sometimes ● Often ● Very Often ●
14.	Have you ever felt a sense of overcoming the usual divisions of the body and mind and reaching a state of complete inner unity and wholeness? Never ● Not Often ● Sometimes ● Often ● Very Often ●
15.	Have you ever experienced going beyond your normal understanding of time and space and entered a timeless realm where these categories no longer apply? Never ● Not Often ● Sometimes ● Often ● Very Often ●

16.	Have you ever been aware of the presence of spiritual entities? Never ● Not Often ● Sometimes ● Often ● Very Often ●
17.	Have you ever spontaneously received accurate information about things in the past, present or future, by extra-sensory means? Never ● Not Often ● Sometimes ● Often ● Very Often ●
18.	Have you ever spontaneously gained a greater understanding of the cosmos? Never ● Not Often ● Sometimes ● Often ● Very Often ●
19.	Have you ever spontaneously lost your sense of identity? Never ● Not Often ● Sometimes ● Often ● Very Often ●
20.	Have you ever been able to see auras around people, animals, plants or other living things? Never ● Not Often ● Sometimes ● Often ● Very Often ●
21.	Have you ever experienced a greater awareness of the interconnectedness of all things? Never ● Not Often ● Sometimes ● Often ● Very Often ●
22.	Have you ever been overwhelmed by powerful emotions and physical sensations, concerning yourself and others in various circumstances and historical settings? Never ● Not Often ● Sometimes ● Often ● Very Often ●
23.	Have you ever experienced living what seemed to be another life, in another time and place, in great detail? Never ● Not Often ● Sometimes ● Often ● Very Often ●
24.	Have you ever felt like you have personally witnessed detailed sequences of events taking place in other historical periods and/or cultures that you have had no previous exposure to? Never ● Not Often ● Sometimes ● Often ● Very Often ●
25.	Have you ever had the need to fight off or try to control the actions of a negative being or entity? Never ● Not Often ● Sometimes ● Often ● Very Often ●
26.	Have you ever experienced rich connections with mythological symbols from ancient history? Never ● Not Often ● Sometimes ● Often ● Very Often ●
27.	Have you ever felt that you were in the centre of huge events that had cosmic relevance and were important for the future of the world? Never ● Not Often ● Sometimes ● Often ● Very Often ●
28.	Have you ever experienced a visionary state taking you back through your own history and that of mankind to creation? Never ● Not Often ● Sometimes ● Often ● Very Often ●
29.	Have you ever been aware of a cosmic battle being played out between the forces of good and evil or light and darkness? Never ● Not Often ● Sometimes ● Often ● Very Often ●
30.	Have you ever experienced the destruction of an old sense of identity followed by rebirth and a renewed purpose for living? Never ● Not Often ● Sometimes ● Often ● Very Often ●

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RESEARCH ARTICLE

Two Replication Studies of a Time-Reversed (Psi) Priming Task and the Role of Expectancy in Reaction Times

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Abstract—Two experiments involving an international collaboration of experimenters sought to replicate and extend a previously published psi experiment on precognition by Daryl Bem that has been the focus of extensive research. The experiment reverses the usual cause–effect sequence of a standard psychology experiment using priming and reaction times. The preregistered confirmatory hypothesis is that response times to incongruent stimuli will be longer than response times to congruent stimuli even though the prime has not yet appeared when the participant records their judgments. The confirmatory hypothesis for Experiment 1 was not supported. Exploratory analyses indicated that those participants who completed the English-language version rather than a translation showed a significant effect, as was the case in the original study; no significant departure from chance was found in data involving non-English translations. Experiment 2 sought to enhance the predicted effect by having each participant read either a pro-psi or an anti-psi statement at the beginning of the experiment to test the hypothesis that a pro-psi statement would produce a larger effect than an anti-psi statement. The results did not support the primary psi hypothesis and there was no effect in the English-language sample. However, there was mixed support for the effect of the psi statement on performance; those participants who received the pro-psi statement had a greater psi score than those who received the anti-psi statement. As in the original experiment, neither the experimenters' nor participants' beliefs were consistently associated with the dependent measure. In sum, the pre-registered confirmatory hypotheses were not supported. The importance of the personality variable Sensation Seeking, a component of extraversion, as a correlate of psi performance is discussed as are the challenges and implications for international collaborations and replication in controversial science.

Keywords: priming; expectancy effect; retrocausation; consciousness; sociology; precognition; psi; replication

Psi research involves the study of purported anomalous mental phenomena, including telepathy, clairvoyance, precognition, and psychokinesis (mind over matter). It is an area of controversial science that began in the late 1800s and continues today. Although several meta-analytic reviews demonstrate an overall psi effect (for a review, see Cardeña, 2018), meta-analyses may suffer from publication biases and selective reporting. For instance, a recent comparison of effect sizes in meta-analyses and pre-registered replication attempts across

15 domains of psychology in general indicated that the former showed almost three times as large effects (Kvarven et al., 2020). Thus, this project sought to address these challenges by examining whether independent investigators can replicate reportedly successful psi experiments using pre-registered analyses.

In 2011, Bem published results from a series of precognition experiments in the *Journal of Personality and Social Psychology*. Using a variety of protocols, his nine experiments tested for possible retroactive (i.e. "precognitive") influences of well-established psychological effects by "time-reversing" the stimulus and response: On each trial, a participant's response was recorded *before* the purportedly causal stimulus was presented. Bem reported statistically significant results in eight of the nine experiments, with a statistically significant mean effect size (d) of 0.22 (Stouffer's $z = 6.66$, $p = 1.34 \times 10^{-11}$). Critics argued that the analyses were partly exploratory (Wagenmakers et al., 2011) and low-powered (Schimmack, 2012), which may result in false-positives. To encourage independent replications, Bem made all his experimental materials and instructions available to other investigators. By 2016, Bem et al. were able to report a meta-analysis of 90 such experiments from 33 laboratories in 14 countries. This yielded an overall effect size greater than 6 sigma, with a Bayes Factor of 3.8×10^9, greatly exceeding the criterion value of 100 for "decisive evidence" in support of the experimental hypothesis. With Bem's original studies excluded, the effect remained significant albeit small, ES = 0.06, $z = 4.16$, $p = 1.1 \times 10^{-5}$, BF = 3.85.

An important variable in determining the success or failure of experimental hypotheses is the experimenter's orientation toward the phenomenon under investigation (Collins & Pinch, 1979). In mainstream psychology, Rosenthal has demonstrated experimenter expectation effects in more than 300 studies, including studies in classroom and clinical settings (Rosenthal, 1978). Experimenter effects have also been observed in psi research for more than 70 years (Pratt et al., 1940), with Palmer and Millar (2015) suggesting that experimenter effects are important or even crucial determinants of outcomes in psi research. Krippner (1978) has summarized findings showing differences among experimenters (Honorton et al., 1975), data collectors (Johnson et al., 1972), reciprocal attitudes between experimenter and participant

(Nash, 1968), and differences across time by the same experimenter (Rivers, 1950). Although experimenter effects are usually attributed to sensory cues, some researchers have suggested that some may be psi-mediated (e.g., Honorton, 1978; Kennedy & Taddonio, 1976; Thouless, 1976; White, 1977). For example, participants did better at guessing psi targets prepared by a psi proponent than on those prepared by a psi skeptic (West & Fisk, 1953).

There is also evidence suggesting that an experimenter can remotely influence a participant's responses through the mediation of psi. For example, Schlitz and Braud (1997) reported that experimenters influenced a participant's electrodermal activity from a distance. Using this protocol, Schlitz and psi-skeptic Richard Wiseman collaborated in three attempted replications using the same participant pool and procedures. Schlitz obtained significant psi effects in two of the three experiments, but Wiseman failed to generate results that allowed for the rejection of the null hypothesis (Watt et al., 2005). Roe et al. (2006) also studied the effect of the experimenter on outcomes of two psi experiments and found that the more experienced experimenter obtained better results.

Each of the two experiments reported here sought to replicate Bem's (2011) two experiments on retroactive priming and to examine the possible effects of the experimenters' and participants' beliefs about psi on the outcome of the experiment in which they were participating. On each trial of a standard (i.e. non-psi) priming task, a pleasant or unpleasant word (the "prime") is briefly shown on a computer screen followed immediately by a pleasant or unpleasant picture drawn from the standard International Affective Picture System (IAPS) (Lang et al., 1993). Trials on which the image and the priming word have different valences (one pleasant and one unpleasant) are termed "Incongruent Trials"; trials on which the picture and the priming word share a common valence (both pleasant or both unpleasant) are termed "Congruent Trials." The typical finding is that participants respond more slowly on Incongruent trials than on Congruent ones.

In Bem's "time-reversed" psi version of the experiment, the presumed cause–effect sequence is reversed so that the prime is not flashed until *after* the participant has already recorded his or her judgment of the picture's valence. The experimental hypothesis

remains the same as in the standard non–time-reversed experiment: Response times will be longer on trials with Incongruent prime/picture pairs than on trials with Congruent prime/picture pairs. Both of Bem's time-reversed priming experiments were successful (Bem, 2011), and the followup meta-analysis of 15 precognitive priming experiments supported the hypothesis with an effect size (*d*) of 0.11, $p = 0.003$ (Bem et al., 2016).

On each trial of the procedure, two potential primes are predesignated for the picture, one pleasant and one unpleasant. Immediately after the participant records his or her judgment of the picture's valence, the computer randomly selects one of the two words to serve as the priming word and flashes it briefly on the screen. This procedure thus provides a genuine sampling-with-replacement or "open deck" procedure for determining whether a trial will be congruent or incongruent. Accordingly, the probability that the trial will be congruent or incongruent remains constant at 0.5 across all trials. As a result, there is no (non-psi) way for a participant to anticipate the kind of trial currently on the screen.

In his original psi article, Bem (2011) noted that the personality trait of extraversion has been frequently reported over the years to be an individual-difference correlate of psi performance, with extraverts achieving higher psi scores than introverts. An analysis of 60 independent psi experiments published between 1945 and 1983 revealed a small but reliable correlation between extraversion and psi performances, $r = 0.09$, $z = 4.63$, $p = 0.000004$ (Honorton et al., 1992). And the correlation was observed again in a later set of telepathy studies conducted in Honorton's own laboratory, $r = 0.18$, $t(216) = 2.67$, $p = 0.004$ (Bem & Honorton, 1994).

The component of extraversion that appears to underlie this correlation is the extravert's susceptibility to boredom and a tendency to seek out stimulation. Specifically, Eysenck (1966) attributed the positive correlation between extraversion and psi to the observation that extraverts "are more susceptible to monotony . . . [and] respond more favourably to novel stimuli" (p. 59). Sensation Seeking is one of the six factors of extraversion on the Revised NEO Personality Inventory (Costa & McCrae, 1992), and Zuckerman's Sensation Seeking Scale (1974), which contains a subscale of Boredom Susceptibility, is

moderately correlated with overall extraversion, $r = 0.47$, $p < 0.01$ (Farley & Farley, 1967).

To assess Stimulus Seeking as a correlate of psi performance in seven of his nine "Feeling the Future" experiments, Bem constructed a scale comprising the two statements: (a) "I am easily bored," and (b) "I often enjoy seeing movies I've seen before" (reverse-scored). Responses were recorded on 5-point scales that ranged from *Very Untrue* to *Very True* and averaged into a single score ranging from 1 to 5. Stimulus Seeking was significantly correlated with psi performance in five of the seven experiments. The mean effect size was 0.43. Both experiments reported here involved three levels of participants: (a) Professors and other Investigators who recruited student experimenters and were invited to serve as participants themselves, (b) Student experimenters who received standardized training in the experimental procedure, and (c) Participants who engaged in the psi task. Investigators who conducted the experiment in a university setting and obtained their own Institutional Review Board (IRB) approvals were offered the option of co-authorship on the final report.

The two experiments were pre-registered with the Koestler Parapsychology Unit at:
http://www.koestler-parapsychology.psy.ed.ac.uk/Documents/KPU_Registry_1007.pdf
and
http://www.koestler-parapsychology.psy.ed.ac.uk/Documents/KPU_registry_1016.pdf
The pre-registered study design of Experiment 1 called for 32 experimenters who would test 16 participants each for a total of 512 participants. Drawing on a global professional network of teachers and other colleagues, 16 professors and other investigators agreed to participate. Of these, four dropped out because of time constraints and other issues. The remaining 12 recruited a total of 34 experimenters. The experimenters were selected based on their interest in the studies, but not on their beliefs in psi. As planned, the first 32 experimenters who submitted complete datasets for each study were included in the analysis (the two other experimenters did not return all the necessary datasets). The script for both studies is included in the **Appendix**.

Experiment 1: Retroactive Priming as a Function of Psi Experiences and Beliefs in Psi

EXPERIMENT 1 METHODS

The procedure was identical to Bem's (2011) original experiments for retroactive priming. Both experimenters and participants were assessed for their beliefs in psi phenomena, and experimenters were also assessed for their belief that the experiment would "produce evidence for precognition." As in the original studies, participants were informed ahead of time that the experiment would test for extrasensory perception (ESP). After they responded to the belief questions, participants went through a 3-minute relaxation procedure and then began the retroactive priming task.

Experiment 1 investigated three pre-registered hypotheses: one confirmatory and two exploratory. The confirmatory hypothesis was that (a) The previous effect reported by Bem (2011) would be successfully replicated: Response times (RT) on trials with Incongruent picture/prime pairs would be greater than RT on trials with Congruent picture/prime pairs. The two exploratory hypotheses were: (b) The anomalous RT effects would be greater for experimenters with positive beliefs about psi and more psi experiences than for experimenters with negative beliefs about psi and fewer psi experiences, and (c) The anomalous RT effects would be greater for participants with positive beliefs about psi and more psi experiences than for those with negative beliefs about psi and fewer psi experiences. The study was not powered for significance on the two secondary hypotheses but sought to identify a trend.

The main dependent variable of analysis in both Experiments was a participant's RT score, defined as their mean response time on Incongruent trials minus their mean response time on Congruent trials—with the following arithmetic modification: RT measurements are not normally distributed but are positively skewed with a lower bound of 0. Accordingly, it is routine practice in experiments using RT as the dependent variable to transform each raw RT measurement using either a reciprocal transformation (1/RT) or a log transformation log(RT), and to define outliers that are too short or too long using the

same transformation. To examine the robustness of the results we examined two data transformations (log and inverse) and two cutoffs for maximum RTs (2,500 ms and 1,500 ms). All trials in which RTs were below 250 ms or above the maximum (i.e. 2,500 ms or 1,500 ms) were excluded.

Mean RT scores greater than 0 imply that a participant's RT on Incongruent trials were longer than RT on Congruent trials, a confirmation of the psi hypothesis; RT scores equal to or less than 0 denote disconfirming instances of the hypothesis. In total, 32 experimenters and 512 participants completed the test. In contrast to the pre-registration, we analyzed experimenter and participant effects with a two-level mixed model with random intercept to account for the multilevel structure of the data as participants were nested within experimenters (but similar results were obtained with the ANOVA). In addition, the mixed model analysis allows for examining the proportion of variance in the retroactive priming effect at the experimenter level. Experimenters' and participants' beliefs and experiences of psi were categorized as high, medium, or low according to 33rd percentiles, as pre-registered. The analyses were performed in JAMOVI 1.2.27. The pre-registration included bootstrapping in Experiment 2, but because bootstrapping and regular parametric methods yielded similar results and JAMOVI did not include bootstrapping for these analyses, we report the parametric analyses.

EXPERIMENT 1 RESULTS

Summary of Data

In Experiment 1, we analyzed the data of 32 experimenters and 511 participants (languages; $n_{Dutch} = 233$, $n_{English} = 189$, $n_{French} = 48$, $n_{Italian} = 25$, $n_{Bulgarian} = 16$). We first describe their self-report data. In both groups, the typical response to the question of whether ESP exists was that it "probably does," but there was a wide range of beliefs. Specifically, among the experimenters, 23% reported that they believe ESP definitely exists, 45% that it probably exists, 16% that they do not know, 13% that it probably does not exist, and 3% that it definitely does not exist (one experimenter did not answer the questionnaire). Among the participants, 18% reported believing that ESP definitely exists, 41%

that it probably does, 17% that they do not know, 14% that it probably does not exist, and 9% that it definitely does not exist. On average, experimenters and participants did not significantly differ in ESP belief, $t(540) = 1.15$, $p = .25$.

As for the practice of mental discipline (e.g., meditation, hypnosis), 29% of the experimenters reported having had regular practice, 29% reported occasional practice, and 42% reported practicing it only a few times or never. Among the participants, 18% reported having had regular practice, 18% reported occasional practice, and 64% reported having practiced it only a few times or never. On average, experimenters reported greater practice of mental discipline than participants, $t(540) = 2.47$, $p = .01$.

We next describe the behavioral data from the retroactive priming task. Participants accurately identified the images as "pleasant" or "unpleasant" in 92% of the trials. Seventeen participants (3%) had an error rate at 25% or higher. The data from these individuals were excluded from further analysis, as pre-registered. Figure 1 shows the raw and log transformed RT averaged for each participant and congruency condition using a 2,500 ms cutoff. As shown, skewness was moderately positive for *raw* data (0.89 for the congruent condition and 0.82 for the incongruent), but small for the *log* transformed data (0.34 for congruent and 0.30 for incongruent, respectively). The *inverse* transformed data also showed small skewness (0.22 and 0.21 for congruent and incongruent, respectively). Analogously, the data based on the 1,500 ms cutoff had absolute skewness values below 0.47. To summarize, the data transformations resulted in largely symmetric distributions as illustrated in Figure 1.

Confirmatory Analyses

The psi hypothesis states that RT will be longer for trials with incongruent than for congruent stimuli. The primary tests of this hypothesis are shown in Table 1. Neither of the four *t*-tests of the primary hypothesis yielded significant results (i.e. no significant retroactive priming effect). Thus, the null hypothesis could not be rejected. To supplement the primary analyses, we also performed binomial tests to examine whether the proportions of participants who scored positively

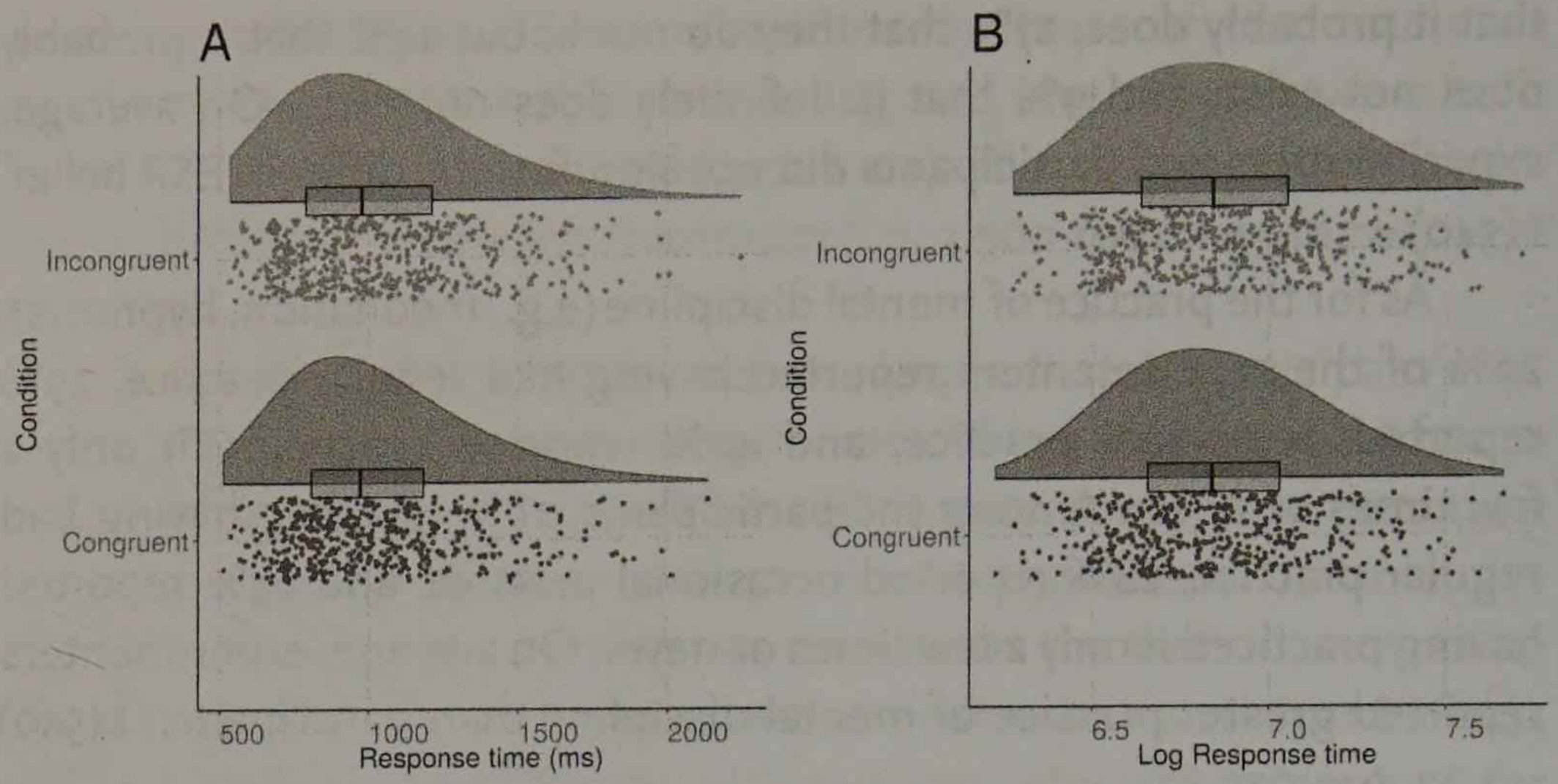


Figure 1. Illustration of RT distributions for (A) raw and (B) log transformed data in the retroactive priming task (Experiment 1). The psi hypothesis states that RT should be slower in the incongruent condition. Each dot represents the average score of a participant for that condition (jittered). The lower and upper hinge on the boxplots indicate the first and third quartile with the median in between. The whisker lines extend to the most extreme values but no further than 1.5 × the interquartile range from the hinge. The raincloud shapes represent distribution estimates based on Kernel density probability functions. Individuals with greater accuracy than .75 and trials with RT above 250 ms and below 2,500 ms are included in this analysis.

were greater than chance (mean chance expectation = 50%). This was done for each data transformation and cutoff. Consistent with the primary analyses, there was no support for the retroactive priming hypothesis in the full sample (*p*s > .30 for all four outcomes).

Exploratory Analyses

The two pre-registered exploratory hypotheses were not supported though they were not powered for significance. That is, RT differences for experimenters and participants did not depend on whether they had low, medium, or high psi beliefs/experiences (*F*s < 1.2, *p*s > .30 across the four psi outcomes). It should be noted that Bem's (2011) original experiments also failed to find such associations. In addition, our mixed model analysis indicated that there was almost o variance at the experimenter level (*ICC*s [intraclass correlation coefficients] < 1.88×10^{-15} across the four outcomes), indicating that there was virtually no systematic between-experimenter variance in retroactive priming scores.

TABLE 1
One-Sample *t*-Tests from Experiment 1 (one-tailed, positive *t*-scores reflect greater than 0 retroactive priming difference scores)

Data	Cutoff	Confirmatory			Exploratory					
		All languages			English Language			Non-English Language		
		t (*df*)	*p*	*d*	*t* (*df*)	*p*	*d*	*t* (*df*)	*p*	*d*
Log(RT)	2,500	*t*(492) = 0.76	.22	0.03	*t*(177) = 2.08	.02*	0.16	*t*(314) = −0.66	.74	−0.04
	1,500	*t*(489) = 0.24	.40	0.01	*t*(177) = 1.23	.11	0.09	*t*(311) = −0.60	.73	−0.03
1/RT [a]	2,500	*t*(492) = 0.49	.31	0.02	*t*(177) = 1.99	.02*	0.15	*t*(314) = −0.98	.84	−0.06
	1,500	*t*(489) = 0.25	.40	0.01	*t*(177) = 1.49	.07	0.11	*t*(311) = −0.80	.79	−0.05

[a] The *t*-test and *d*-statistics for the inverse transformation were reversed back so that the psi hypothesis would have the mean be *greater than* 0.
* $p < .05$

An exploratory, not pre-registered, analysis indicated a significant retroactive priming effect for those completing the English-language version (*t*-tests, see Table 1). This effect was observed with both log and inverse transformations of the data, but only with the more liberal 2,500 ms cutoff. Those completing the non-English versions scored non-significantly in the opposite direction (see Table 1). As for the supplementary binomial analyses, 56.2% of the English-language sample had a positive log transformed difference score regardless of whether 2,500 or 1,500 ms cutoffs were used (*p*s = .06), whereas for the inverse transformation the proportions of positive psi scores were 54.5% (*p* = .13) and 53.9% (*p* = .17) for the 2,500 ms and 1,500 ms, respectively. For the non-English samples the proportions of positive scores were lower than 48% across all data transformations (*p*s > .80). To summarize, the exploratory analyses indicated an effect among those performing English-language versions using the 2,500 ms cutoff and continuous scores.

As the exploratory psi analyses indicated an effect only in the English-language sample, we compared that sample to the non-

English samples on the five predictor variables. The English-language sample reported greater practice of *mental discipline*, $t(509) = 3.34$ $p < .001$, $d = 0.31$; *belief in ESP*, $t(509) = 4.26$, $p < .001$, $d = 0.39$; *experience of ESP*, $t(509) = 4.06$, $p < .001$, $d = 0.37$; and *easily getting bored*, $t(509) = 4.38$ $p < .001$, $d = 0.40$, but the samples did not differ in the extent they *enjoy watching the same movies again*, $t(509) = -1.48$ $p = .14$, $d = 0.14$.

We correlated these five predictors, as well as Bem's (2011) two-item Stimulus-Seeking scale, with the four retroactive priming outcomes (log and inverse data with 2,500 and 1,500 ms cutoff), that is, performing 24 analyses (see Table 2). With unadjusted *p*-values and one-tailed tests, the only predictors that significantly predicted retroactive priming outcome was the variable *getting easily bored* (significant for both data transformations but only the 1,500 ms cutoff) and the Stimulus-Seeking scale (but only for the 1,500 ms log score). However, those *p*-values would not remain significant with a Bonferroni correction (e.g., dividing the alpha value by 6 because of the six predictors). Nevertheless, for exploratory purposes a followup analysis was performed on those completing the English-language version. This analysis indicated that easily getting bored significantly correlated with the retroactive priming effect across all four outcomes (*r*s between .19 and .22, *p*-values < .005). This variable did not correlate with the retroactive priming effect among those completing the non-English version (*p*s > .42). Similar patterns were obtained with the Stimulus-Seeking scale (*r*s between .16 and .22, *p*s below .02 in the English sample).

In summary, those completing the English-language version reported greater ESP belief and experience, practice of mental discipline, and more easily getting bored. With adjustments of alpha value for multiple analyses, none of the six predictors correlated with the retroactive priming effect in the full sample, although the English-language sample exhibited a significant correlation between the retroactive priming effect and easily getting bored (and Stimulus Seeking more broadly).

TABLE 2
Pearson *r* Coefficients (*p*-values in parentheses) from Exploratory Correlational Analyses with Self-Reported Predictor Variables and Retroactive Priming Outcome (one-tailed, unadjusted *p*-values)

	Log RT		1/RT (reversed)	
Predictor	**2500 ms**	**1500 ms**	**2500 ms**	**1500 ms**
ESP belief	.05 (.13)	.00 (.49)	.04 (.20)	.00 (.50)
Practice mental discipline	.04 (.19)	−.00 (.52)	.03 (.24)	.00 (.50)
ESP experience	.02 (.35)	−.02 (.65)	.01 (.44)	−.02 (.66)
Easily getting bored	.05 (.16)	.10 (.02*)	.06 (.09)	.09 (.02*)
Enjoy watching the same movies again	−.04 (.21)	−0.01 (.44)	−.02 (.33)	−.01 (.45)
Stimulus Seeking (2 items) [a]	.06 (.10)	.07 (.05*)	0.06 (.10)	.07 (.06)

* $p < .05$

[a] Stimulus seeking is the mean of easily getting bored and enjoy watching the same movies again (reversed). The correlation coefficient is multiplied with −1 for the inverse transformation so that greater positive scores reflect greater retroactive priming effect across all four outcomes. Only individuals with mean accuracy > .75 are included (n = 493 for 2,500 ms cutoff and n = 490 for 1,500 ms cutoff).

Experiment 2: Retroactive Priming Effects as a Function of Reading Pro-Psi or Anti-Psi Arguments

Experiment 2 was identical to Experiment 1 except that participants read a (genuine) pro-psi or anti-psi statement before beginning the experimental trials. The pre-registered study design of Experiment 2 called for 32 experimenters who would test 20 participants each for a total of 640 participants. The confirmatory hypothesis was that mean RT would be longer for trials with Incongruent prime/picture pairs than for trials with Congruent pairs. The exploratory hypotheses were that the predicted RT effects would depend on: (a) experimenters' psi beliefs and experiences, (b) participants' psi beliefs and experiences, and (c) the interaction between experimenter's and participant's psi beliefs and experiences. In addition to these pre-registered hypotheses, we also explored if sessions with a positive prompt regarding psi phenomena would have greater psi effects than sessions with a negative prompt.

EXPERIMENT 2 METHODS

Prior to each session, subjects were randomly exposed to one of two prompts: pro-psi or anti-psi. These prompts were as follows.

Pro-Psi Introduction

Comment on psi (ESP) by Rupert Sheldrake, Ph.D., a biologist and author of more than 80 scientific papers and ten books. He was among the top 100 Global Thought Leaders for 2013, as ranked by the Duttweiler Institute in Zurich, Switzerland's leading think tank.

> Telepathy, ESP, and psychic/psi phenomena in general are real and backed up by convincing evidence; their investigation deserves to be part of science . . . I take seriously research within parapsychology. I think there is good evidence for precognitive dreams, and also for presentiment, whereby an emotional arousal can have a physiological arousing effect five or six seconds in advance.

Anti-Psi Introduction

Comment on psi (ESP) by Michael Shermer, Ph.D., the Founding Publisher of *Skeptic Magazine*, a monthly columnist for *Scientific American*, a regular contributor to Time.com, and Presidential Fellow at Chapman University.

> . . . a meta-analysis of . . . [psi] experiments found no evidence for psi, concluding that psi data are non-replicable, a fatal flaw in scientific research. In general, over the course of a century of research on psi, the tighter the controls on the experimental conditions, the weaker the psi effects seem to become, until they disappear entirely. This is a very strong indicator that ESP is not real.

EXPERIMENT 2 RESULTS

Summary of Data

In Experiment 2, we analyzed the data of 30 experimenters and 586 participants (languages: $n_{Dutch} = 409$, $n_{English} = 117$, $n_{German} = 42$, $n_{Swedish} = 18$). Mean accuracy on judging unpleasant and pleasant images was 92% (as in Experiment 1). Twenty-two participants (4%) were excluded

TABLE 3
One-Sample *t*-Tests from Experiment 2 (one-tailed, positive *t*-scores reflect greater than 0 retroactive priming difference scores)

Data	Cutoff	Confirmatory			Exploratory					
		All Languages			English Language			Non-English Language		
		t (*df*)	*p*	*d*	*t* (*df*)	*p*	*d*	*t* (*df*)	*p*	*d*
Log(RT)	2,500	$t(563) = -1.16$	.88	−0.05	$t(110) = -0.31$	.62	−0.03	$t(452) = -1.14$	.87	−0.05
	1,500	$t(560) = -1.28$	.90	−0.05	$t(109) = -0.66$	.75	−0.06	$t(450) = -1.09$	.86	−0.05
1/RT [a]	2,500	$t(563) = -1.31$	.91	−0.06	$t(110) = -0.51$	.69	−0.05	$t(452) = -1.21$	.89	−0.06
	1,500	$t(560) = -1.25$	.89	−0.05	$t(109) = -0.58$	.72	−0.06	$t(450) = -1.10$	.86	−0.05

[a] The *t*-test and *d*-statistics for the inverse transformation were reversed back so that the psi hypothesis would have the mean be *greater than* 0.

because their mean accuracy was not above 75%. We applied the same data transformations as in Experiment 1 to approximate normal distributions.

Confirmatory Analyses

To reiterate, the retroactive priming hypothesis states that participants have greater RT to incongruent than congruent trials, which we primarily tested with one-sample *t*-tests on difference scores. Contrary to our prediction, there was no significant retroactive priming effect for any of the four outcome variables (log and inverse transformation with 2,500 or 1,500 ms cutoff). As shown in Table 3, *p*-values were .88 or greater for the *t*-tests on the total sample. Unlike in Experiment 1, there was no significant retroactive priming effect observed in the English-language sample (*p*s > .62).

Exploratory Analyses

The second and third hypotheses state that experimenters and participants with greater experiences and beliefs would obtain greater

TABLE 4

Results from 4 Mixed Models Analyzing Whether the Four Retroactive Priming Outcomes Depend on Experiment Psi Belief/Experience (Hypothesis 2), Participant Psi Belief/Experience (Hypothesis 3), or Their Interaction (Hypothesis 4)

	Log Data				Inverse Data			
	2,500		1,500		2,500		1,500	
Predictor	*F*	*p*	*F*	*P*	*F*	*p*	*F*	*p*
Experimenter (E)	0.63	.54	3.02	.05*	1.50	.22	3.47	.03*
Participant (P)	0.79	.46	0.12	.89	0.39	.68	0.10	.91
E × P	0.67	.61	0.87	.48	0.97	.42	0.99	.41

The four models included random intercepts and fixed slopes, and maximum likelihood estimation method. Degrees of freedom (*df*) were calculated with the Satterthwaite method. Numerator df was 2 for main factors and 4 for the interaction. Denominator *df* was 525 for 2,500 ms cutoffs and 522 for 1,500 ms cutoffs. * $p < .05$

retroactive priming effects, whereas the fourth hypothesis states that there is an interaction between experimenters and participants. The two group factors were used as predictors in four mixed models (one for each of the four outcomes; log and inverse data with 2,500 ms or 1,500 ms cutoffs). Across all four outcomes, there were two significant main effects of experimenters' belief/experience, but these effects would not remain significant after a Bonferroni correction for four analyses. Nevertheless, experimenters with medium belief/experience had greater scores than those with low belief/experience ($p = .04$ for inverse and $p = .06$ for log outcome after Bonferroni correction). The overall results are shown in Table 4. In summary, we did not find clear support for a relation between self-reported belief and experience of ESP and behavioral outcome.

We then tested the hypothesis that the retroactive priming effect would be greater in sessions with a pro-psi than with an anti-psi introduction. For the log-transformed data with a 2,500 ms cutoff, there was a significant difference, $t(562) = 1.68$, $p = .05$, $d = 0.14$. Specifically, those 293 individuals who read the pro-psi statement had a mean difference score of 0.002 ($SD = 0.082$), whereas those 271 individuals who read the anti-psi statement had a mean difference score of −0.010

(*SD* = 0.088). This statement effect was also significant for the 1,500 ms cutoff (log transform), $t(559) = 1.78$, $p = .04$, $d = 0.15$. In contrast, this effect was not significant for the inverse transformations, $t(562) = 1.24$, $p = .11$, $d = 0.10$ for the 2,500 ms cutoff, and $t(559) = 1.23$, $p = .11$, $d = 0.10$ for the 1,500 ms cutoff. Thus, there was mixed support for an effect of pro- vs. anti-psi statement on retroactive priming outcome according to these exploratory analyses.

As there was a small positive correlation in Experiment 1 between the variable *getting bored easily* and the log and inverse outcomes with the 1,500 ms cutoff, we examined these associations in Experiment 2. The correlation observed was close to zero and not significant for log, $r(559) = .00$, $p = .50$, and inverse, $r(559) = -.01$, $p = .62$, outcomes. We also followed up on the correlation between Stimulus Seeking and the retroactive priming effect from Experiment 1 (log transformed, 1,500 ms cutoff). The correlation between these two variables in Experiment 2 was not significant, $r(559) = .05$, $p = .14$. Thus, we could not replicate the association between getting bored easily (and Stimulus Seeking, more broadly) and the retroactive priming effect observed in Experiment 1.

DISCUSSION

Overall, the two studies did not replicate the original Bem findings for time-reverse priming on RT. Both failed to reach significance in the preplanned confirmatory hypotheses. Exploratory analyses indicated significant effects in Study 1 for the English-only condition, which is consistent with Bem's initial work. Study 2 was successful in producing a greater effect on time-reversed RT for those who received a pro-psi prompt as compared with the negative psi prompt (although this effect may be more driven by the anti-psi than the pro-psi statement). These results indicate that the brief comments of the two genuine-but-disagreeing experts in this experiment held greater sway over the participants' psi performance than did their own initial beliefs and experiences. Study 2 did not yield significant psi effect in the English-language sample.

These studies build upon previous research by exploring whether the observations about beliefs in psi may play a role in the replication of anomalous results under controlled conditions. One limitation of

these two studies is that expectancies and beliefs were evaluated using self-report questionnaires. In a future study, the role of unconscious beliefs will be assessed to further understand the role of beliefs in psi performance. The implicit association test originally developed by Greenwald et al. (1998) has shown that overt responses of participants do not necessarily reflect their unconscious beliefs. This will be the approach in Study 3 of the series (Schlitz & Delorme, 2021).

At a meta-level, the studies strongly support the feasibility of a multi-laboratory collaboration involving researchers representing different worldviews and beliefs about psi phenomena. With the aid of technology and the spirit of goodwill, these studies speak to mutual support for common interests in the empirical study of psi phenomena.

As we aim to assimilate these results, we suggest that reported findings are open to two main competing interpretations. First, initial studies reported by Bem and colleagues may have been caused by a genuine psi effect and the current experiments failed to fully replicate this finding because some aspect of the current studies disrupted the production of that effect. Although it is impossible to falsify this position, it is difficult to identify any obvious factors that might have prevented a psi effect from operating. The issue of language (and culture) reveals an important dimension; did participants who were working in other languages lack the depth of understanding about the study and the goals enjoyed by native English speakers? Perhaps the interpretations and meta-cultural dimensions of the experimental exchanges were unexpected variables. It is also possible that a more subtle, unanticipated, and uncontrolled factor may have disrupted the production of an overall effect on the main pre-registered hypothesis. For example, the study took place in diverse settings with no consistent environment, set, or setting across sub-experiments. The background and experiences of the experimenters were uncontrolled, with the exception of the interventions. A much larger study would be needed to find statistical significance across experimenters. Future studies might aim to select participants and experimenters who have shown talent at performing this task and to find ways to increase statistical power.

Second, it is possible that the results from earlier studies represented chance findings or undetected subtle artifacts and that the results obtained in the present studies accurately reflect the absence of

a psi effect based on the preplanned analyses. This is consistent with the null results reported in another recent multi-laboratory, pre-registered replication attempt with large *N* (Maier et al., 2020), which also tested retroactive influence (but without informing participants prior to testing that they would be tested for ESP). Our results are also consistent with the broader observation of finding smaller psychological effects in pre-registered replication attempts than in retrospective meta-analytic estimates (Kvarven et al., 2020). On the other hand, there may be hidden moderators that influence the outcomes of these replication attempts (c.f., pro- vs. anti-psi priming). It is certainly the case that the methodology employed in the current studies was more ambitious than the original studies in scope, and sophisticated in terms of the use of preregistration. This may be driven by the development of a field of study more than the techniques and procedures used in previous work—for example, rather than being driven by any concern that the previous findings were the result of any obvious artifacts. The results of Experiment 2 also speak to the importance of process-oriented work that is not driven by proof of concept.

Further, these studies provide rich fodder for sociological investigations of replication in science. This series of experiments demonstrates that it is possible to conduct fruitful collaborative research involving both skeptics and proponents, and it offers the potential of a more productive route than more traditional forms of skeptic–proponent debate (e.g., Honorton, 1985; Hyman, 1985; Schlitz et al., 2006). The collaborative project described here reduces the likelihood of perpetuating nonconstructive rhetoric because skeptics and proponents are actively engaged in the same study, and the procedures employed should minimize methodological flaws and maximize the procedures that proponents believe to be conducive to psi functioning. In addition, opportunities for explaining away the results post hoc are limited since the experiments made use of preregistered protocols. The interpretation of the data remains in the eyes of the beholder.

There are, however, several barriers that may hinder this type of collaborative venture. In many controversial areas of psychology, communities of researchers with opposing views tend not to attend the same conferences, publish in the same journals, or even read the same type of academic articles and books (Blackmore, 1989). Additional

barriers include an inherent distrust of one another fueled by ideological differences, personal beliefs, and past involvement in acrimonious debates. Our experience suggests that there is considerable value in trying to overcome these barriers and carry out systematic and collaborative ventures. It is hoped that the studies described here will encourage researchers working in other controversial areas (e.g., the role of "trance" in hypnosis, false memory syndrome, unorthodox forms of psychotherapy, and complementary and alternative medicine) to engage in similar joint projects and that such work will help advance our understanding of the phenomena underlying these controversies. Advancing such collegial endeavors in the pursuit of truth is ultimately dependent on the degree to which researchers engage with goodwill, an open mind, and an active sense of curiosity.

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APPENDIX
SCRIPT FOR EXPERIMENTERS

Before the Participant Arrives

Turn on the computer, monitor, and speakers. Set the floor lamp to its lowest setting, turn off the overhead fluorescents, and put the "Experiment in Progress" sign on the laboratory door.

Start the program, fill out the opening screen, and leave the participant's opening screen up for the participant.

Put the date, time, the participant's name, and your own name on the Session Record Form. [A copy of this form will be found in the Supplementary Files folder.]

When the Participant Arrives

You and the participant should be seated in two comfortable chairs facing one another. Chat long enough to relax him or her. Don't rush. This is a very short experiment and there is plenty of time. You can tell the participant this, too, if he or she seems tense or rushed.

As soon as it feels right, explain the experimental procedure. You can paraphrase the following:

This is an experiment that tests for ESP (Extrasensory Perception). The experiment is run completely by a computer and takes about 15 minutes. **[Note: Most participants take less than 10 minutes to complete the experiment once it is under way.]**

First, you will be asked to answer a few questions about yourself. Then, on each trial of the experiment you will be shown a picture on the screen and asked to indicate as quickly as you can whether it is pleasant or unpleasant. A word will then flash on the screen very briefly. There will be 40 trials in all.

At the end of the session, I will explain to you how this procedure tests for ESP.

At this point, you can answer any questions they have. If they express any doubts about having ESP or worry that they won't do well, reassure them that we are primarily interested in testing the experimental procedure, not their own individual ESP ability. Participants should not feel pressured to perform, but neither should they feel that we are just playing around.

If your institution requires participants to sign a consent form to be in an experiment, now is the time to have them sign it. If they have the option of being paid money or receiving credit for participation in a course, now is the time to confirm that information and to record it on the Session Record Form (which will be found in the Supplementary Files Folder).

Seat the participant in the computer chair and, if necessary, help adjust the chair and the tilt of the computer screen to a comfortable position. Remember to ask if he or she prefers to have the mouse positioned to the left of the keyboard. **Check to make sure that cell phones—both yours and the participant's—are turned off.**

Show them the two keys on the keyboard with the frowning and smiling faces. Tell them that they will be using these to enter their responses. Show them how to rest the heels of their hands on the desk so that they can reach the two keys quickly and easily with their two index fingers.

Explain that the instructions will be shown on the screen but that you will be within hearing range if they have any questions or difficulties. Then move out of the experimental space.

During the Experiment

Fill out the questions in the middle section of the Session Form. These ask about the participant's demeanor and will be entered by hand into the database later. It is important that you respond to these questions now, before you know how well the participant did.

After the Experiment

The program provides feedback to participants on the final screen by informing them whether they responded more quickly or more slowly on congruent trials than on incongruent. Glance at the final screen so you can interpret their performance for them in your post-experiment debriefing.

Depending on how much detail feels appropriate for this participant, you can include some or all of the following points

— This experiment is designed to test for precognition, a form of ESP in which a person can anticipate the future.

— This experiment is a modified version of what is known in cognitive/social psychology as a **priming experiment**. In a typical priming experiment, the participant is asked to judge as quickly as possible on each trial whether a picture is pleasant or unpleasant, and the time it takes for him or her to make a response is measured. Just before each picture appears, a word is flashed very quickly on the screen. This word is called a prime. On some trials the word and the picture are matched; that is, they are either both pleasant or both unpleasant. On other trials they are mismatched. For example, a pleasant word like "beautiful" might be flashed just before a picture of a snarling dog appears on the screen. Typically people respond more quickly when the word and the picture are matched than when they are mismatched.

— In the modified version of the experiment that you just participated in, the sequence was reversed so that the word was flashed AFTER you had already made your response to the picture. This is how we tested for ESP. If people can be affected by the immediate future, then the priming word could affect their response time even though it occurs after they make their judgment about the picture. Accordingly, the ESP hypothesis is that people will respond more quickly when the priming word matches the picture than when it doesn't—even though the word has not yet been flashed. Your results showed that you did [in fact, show this ESP result] [not show this pattern, however]. We are finding that some participants show the effect and others do not, and we are attempting to discover what might produce such an effect as it is actually found in the experiment.

Thank them for their participation and reassure them that they have done just fine and given us what we needed. If they were getting paid money, pay them and have them sign the receipt form. If they express any complaints or reservations about the experiment, give them a contact number where they

can register their complaint or give them a blank copy of the consent form with a contact listed on it so they can follow up.

After the participant leaves, add any comments or observations you have about the session that might help interpret the results (e.g., participant was rushed and unfriendly, participant was about to take a final exam after the session, participant expressed suspicions that the experiment was not really about ESP). Terminate the program by pressing 'q' on the keyboard.

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COMMENTARY

Thought-Forms Gone Rogue: A Theory for Psi-Critics and Parapsychologists

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Abstract—It is argued that psi-critics Reber and Alcock have lifted the debate from the impasse concerning the evidence for the existence of psi phenomena, toward focusing on understanding the nature of the phenomena. This focus concerns the demand to show that statistical findings are not anomalies but reflect real cause-and-effect relationships and to find a common theoretical framework for what otherwise appear to be heterogeneous rogue phenomena. It is maintained here that the demand for showing causal relationships is already met by a methodology using real-time recordings of changing target imagery along with receiver mentation. The demand by critics for a theoretical understanding linking all or most of the rogue phenomena, led to the proposition advanced here concerning thought-forms and co-conscious states. According to this, the many "rogue phenomena" both in psychology and parapsychology (such as automatic writing, lucid dream characters, spirit possessions, and entity experiences in psychedelic states) are to be understood as representing dissociated thought-forms with varying degrees of co-consciousness and in some cases the development of a genuine degree of autonomy and identity.

Keywords: altered states, thought-forms, consciousness, psi, skepticism, automatic writing, co-consciousness, possession

A major issue impeding the acceptance of parapsychology is that there is no theoretical conceptual framework linking its diverse phenomena. This paper will argue that if consciousness is seen as

primary in nature and if dissociated states are a normal characteristic of consciousness, then most of the phenomena do form a meaningful relationship. Many of the apparently heterogeneous phenomena in psychology as well as in parapsychology such as automatic writing, lucid dream characters, spirit possessions, and entity experiences can then be understood as representing dissociated thought-forms with varying degrees of co-consciousness and in some cases with the development of a genuine degree of autonomy.

The importance of establishing such a theoretical framework is highlighted by the current critique of parapsychology as a field of science by Reber & Alcock (2020) (R & A), although this critique by R & A was actually based on three grounds. These are 1) that the phenomena contradict the limiting principles on which science is based, 2) that the claimed psi (paranormal) effects have small effect sizes without any shown causal relationship to reality, and 3) that there is no theoretical conceptual framework that brings order to the chaos in the range of alleged phenomena. The first objection from R & A was met in the 2019 issue 4 of the *Journal of Scientific Exploration* by an Editorial and five individual author responses, all making counterarguments to what seemed to be R & A's assertion that psi phenomena are "impossible" because they contradict the limiting principles of science, which have served science so well that they could almost be written in stone.

The second objection by R & A claiming that there were only small effect sizes in parapsychology was partly met by showing their equivalence to those of other psychological findings (Williams, 2019), but the final thrust of R & A's second argument—that the "Statistical departures from chance expectation cannot and do not identify the causes of those departures" (Reber & Alcock, 2020, p. 397)—remains unparried. Since this is an issue that has been of some concern in our work at Gothenburg, some space will be given to this before proceeding to the related issue of the theoretical framework for understanding the phenomena.

This challenge assumes that all the significant experimental findings concerning ESP may only represent *unknown* artifacts or "Error Some Place" rather than causal relationships. This challenge is met if it can be shown that highly significant scores relate closely and apparently causally to the content of the experiences behind the

scores. The first attempt to deal with this occurred in some of the early ganzfeld work where the descriptions from successes in the telepathy setup were presented as voice-overs and were shown to closely match the target film clips (Honorton et al., 1990). However, there remained a possible bias in making this selection since there was no check on whether or not the recording of the receivers' mentation reports actually matched *in real time* the content of senders' film clips. Without this check, the matching could be seen as rather arbitrary. The series of real-time experiments we carried out at Gothenburg rectified this, and in doing so they deal directly with the critique of R & A concerning the lack of connection to reality.

Indeed, the very purpose of our real-time recordings with the digital ganzfeld was to produce the laboratory analogue of spontaneous psychic experiences where one person (the receiver) has a psychic experience in an altered state, usually a dream—which corresponds in content and in time with another crisis experience of another person (the sender). In the laboratory setup this meant that we would eavesdrop on the person in the altered state (the ganzfeld state) describing in the successful cases the sender's experience of an emotionally loaded film clip. The arrangement enabled us to record both experiences precisely as they were happening and to highlight the apparent causal relationship (Parker et al., 2000). These hits occurred when the participants not only chose the correct clip but their descriptions seemed to closely and causally follow unpredictable changes in the film content. About one in six of our real-time (first rank) hits were of this type. Coincidental false matches did occasionally occur but these matches did not seem to show continuity in following the unexpected changes. This was a distinctive feature of genuine matches, but because of a flaw in the choice of data used for correctly accessing this, the final evidence is still lacking (see Parker, 2020, for a further discussion.)

An ideal opportunity for dealing with R & A's challenge, however, did arise during the ganzfeld testing of a consistently successful mother–daughter pair. I was curious to know if an unexpected intervention in the "sender" room, where the daughter was situated and observing a film clip, would intervene in the flow of ganzfeld mentation of the mother located in the "receiver" room. Without informing any of the participants prior to the session of my intention, I asked the daughter to leave the room and a

close friend of the mother to come into the room and take over the role of sender. At precisely the point in time when the friend entered the room, the mother's flow of mentation images was interrupted and her voice was heard from the receiver room and recorded on tape saying "Where have you been?" This was the only occasion when I attempted such an intervention that fits with R & A's search for the *causal effects*, but others occurred spontaneously such as when the tape ran out unexpectedly or the film went into slow motion when the mentation responses were the words "change tape" and "slow motion."

The real-time digital ganzfeld does not stand alone in relating statistical findings to reality events. The work of James Carpenter (2012) has been outstanding in this respect, and on one occasion even used majority voting to successfully "transmit" the code-word peace (Carpenter, 1991).

I suspect that R & A would prefer to call these examples merely weird synchronous events, in which case I would agree with the terminology. Rather than extrasensory perception, a better term for ESP may indeed be *Extraordinary Synchronous Phenomena*. (The alert reader may ask about precognition, but this would involve a discussion of the specious presence. Although this lies beyond the scope of this article, it forms a part of Carr's theory [Carr, 2019]).

The accumulation of more evidence for psi has become a Sisyphean boulder for those wishing to resolve this issue. If the boulder is now to be moved forward, we still need to deal with the third objection.

The third objection concerns the maelstrom of phenomena, or as R & A prefer to call it "the farrago." They write: "In contemporary parapsychology one finds a crazy quilt set of effects that have no conceivable underlying foundation. It is as if actors from a dozen different plays have appeared on the same stage in a discordant farrago" (Reber & Alcock, 2020, p. 393.)

I agree with R & A. This issue is in dire need of being resolved. To do so, we need to state not only how the phenomena relate to each other but also how they relate to other known natural phenomena. Low-level hypotheses do already exist, as pointed out in two of the responses to R & A (Roe, 2019; Williams, 2019). These hypotheses include the relationship to noise-reduction (Honorton, 1977; Storm et al., 2010), to top-down processes (Parker, 2000), to change-in-state (Murphy, 1966;

Honorton, 1977; Parker, 1975, 1994), to unconscious expectancies and belief systems (Carpenter, 2012; Parker et al., 1997), and to morphic fields (Sheldrake, 2020). Although I have been active in formulating and testing some of these hypotheses, I freely admit these do not provide any commonality that would link the various paranormal phenomena, and they give neither a proper theory nor a deep understanding of what we are dealing with.

The demand to relate paranormal phenomena to known natural processes can be understood when the role of science from the Renaissance onward is seen as being "the candle in the dark." Science has created a secure, and largely predictable, orderly world, and followers of reductionistic science have little or zero tolerance for ambiguity or any retreat to the darkness of the occult. Rogue and occult phenomena can be seen as a threat to this Weltanschauung.

Perhaps this is why some parapsychologists with a background in physics disavow the existence of any phenomena other than ESP or even prefer "anomalous cognition," since this allows them to pin their hopes on the nonlocal effects discovered in quantum physics, where a connectedness, analogous to telepathy, occurs between particles at various distances. In addition, the duality of light waves and particles seems to demand an observer, that is consciousness, in determining the final outcome of these processes (Marwaha & May, 2015a; Millar in Parker & Millar, 2014; Stapp, 2015). Others would go further and argue that quantum theory requires the presence of consciousness in order to explain the transition to reality. In making these assertions, parapsychologists often cite the writings of eminent physicists such as Max Planck, Eugene Wigner, and Werner Heisenberg, who all endorsed the view that consciousness is a primary mover in the universe (Radin, 2013, 2018; van Lommel, 2020).

Unfortunately, this argument is seldom well-received given that the majority of modern physicists appear not to share the view that consciousness is involved in quantum outcomes (Carr 2019; Mroczkowski & Malozemoff, 2019), although it may be more accurate to say that such a view is rather controversial (Kastrup, 2019). What seems clear is that most physicists agree the bridge has not been found that would link phenomena occurring at the quantum level to those occurring in everyday human reality—the so-called measurement problem (Carr, 2019).

Against this background, appeals to the various interpretations of quantum theory can only strengthen any natural aversion the skeptic might have against ambiguity. Arguments about the similarity of nonlocal effects between particles and telepathy between humans, are seen as simply attempting to explain one unknown in terms of another unknown.

Here I will argue that the opposite tactic is more forthright and effective. What is surely needed is an alternative view of psi that enables us not only to explain the findings of ESP experiments but also the range of rogue phenomena in parapsychology from poltergeist cases to alleged spirit communications of mediums. I will further argue that we need to relate these rogue phenomena to an even wider range of phenomena such as automatic writing, and to savant abilities, which are a perennial challenge to clinical psychology and psychiatry. The situation is particularly favorable for such an approach given that consciousness is now a legitimate subject for scientific study. Support for such a starting point is found not only in the writings of the exceptional physicists named above but also in those of a few exceptional psychologist–philosophers such as William James and Frederic Myers who also insisted that consciousness is primary in nature (Kelly et al., 2007).

The insistence that consciousness is primary, is of course the idealist version of the panpsychism equivalent of the "one-free miracle" required by reductionism as parodied by Terence McKenna: "Give us one free miracle and we'll explain the rest. The one free miracle is the appearance of all the mass and energy in the universe and all the laws that govern it in a single instant from nothing" (McKenna, in a personal communication with Rupert Sheldrake in 1985). The difference here with panpsychism is that empirical work in parapsychology indicates that this one miracle, in this case consciousness with its attribute of psi, does occur.

Seen from this perspective, psi is merely a sign of the fundamental connectedness of consciousness, and physics would then only need to enter the picture as a way for consciousness to explain nature. This connectedness is most noticeable in altered states of consciousness as illustrated by the cases reported in the spontaneous literature from real life and confirmed in the laboratory by ganzfeld and altered-state research (Storm et al., 2010).

Of course, if we are to escape the chaos of R & A's "farrago," then we need to find some further commonality between laboratory ESP and the diversity of experiences such as spirit communications through mediums, mystical experiences, apparitional experiences, and poltergeist phenomena. In seeking this commonality by referring to the concept of "thought-forms," it is not being claimed that this provides the ultimate sought-after deeper explanation. In order to seek this, it may well be necessary to integrate the concept further into the framework of a theory from modern physics but one that gives primacy to a dimension of consciousness in describing the natural world (e.g., Carr, 2019; Pilotti, 2011).

THOUGHT-FORMS

A thought-form has been defined as "any perceivable form, which has been created directly and exclusively by the mind, unconsciously or consciously, and which in some cases develops autonomy" (Parker & Puhle, 2018). If we follow this definition, it is described in many different cultural contexts; the most well-known examples being the Celtic and Germanic "pooka," the Arabian "djinn," and the Tibetan "tulpa." The pooka is most well-known through the works of Shakespeare, while the tulpa is attributed to Tibetan beliefs popularized by Alexandra David-Néel. David-Néel, while travelling in the Himalayas, claimed to have produced a *tulpa*, with its own will. However, according to a recent paper, this concept is a combination of theosophical beliefs and a somewhat misappropriated Tibetan concept (Mikles & Laycock, 2015).

Nevertheless, what the concept of thought-forms brings to the debate forum, is to view consciousness as *non-unitary and potentially dissociative* and to recognize that this characteristic has various cultural expressions. There is in fact a commonality to all this if we presuppose that there is a transpersonal or 'extended level to consciousness,' which is capable of creating entities in response to extreme personal needs. Assuming this occurs, these thought-forms could in some states of consciousness eventually develop their own sense of identity, interact with other persons in a meaningful way, and in some extreme cases express their own will.

As preposterous as this may seem, there is actually consistency in modern research concerning these aspects. As I shall now present in detail, these apparently autonomous entities are reported as occurring in a variety of altered states such as lucid dreaming (Johnson, 2017; Puhle & Parker, 2017; Tholey, 1989; Waggoner & McCready, 2015); psychedelic experiences (Luke, 2012); and near-death-experiences (E. W. Kelly, 2001). In addition, it is being proposed here that the communicating spirits of mediums can be explained as autonomous thought-forms gaining the identity of deceased individuals. This means that in some cases they are not secondary personalities or appended "alters" but have become independently conscious alters.

Lucid Dreaming

Lucid dreams are dreams in which the dreamer retains the critical ability to realize that what is being experienced is a dream world and continues dreaming while maintaining this awareness. About 60–80% of individuals report having these experiences and about 20% have them on a regular basis—a least once a month (Schredl & Erlacher, 2004). Some individuals become adept at this and develop the ability to maintain the states for longer periods and to steer the content and direction of their dreams. Although lacking exact figures, it appears clear that the majority of the adepts report experiencing so-called dream characters.

A way of understanding dream characters is to relate them to the concept of dissociation. Dissociation is defined as "one or more parallel paths or systems operating outside of awareness and influencing cognition, affect, or behavior" (Kirsch & Council, 1992, p. 275). The work of Harvard clinical psychologist Deirdre Barrett (1994a; 1994b; 1995; 1996) is crucial here because it links dissociation to dreaming with the starting point that "each night when we dream we manufacture others out of parts of ourselves" (1994a, p. 123). In principle, this is the same process of dissociation that individuals with "Dissociative Identity Disorder" use in creating their "alters." A confusion can occur in such cases between what occurred in dream life and in waking life as well as in sorting out the experiences that belonged to the alter and to the host personality. To clarify this, Barrett carried out a questionnaire study of the dream experiences reported by such individuals, which indicated that 13 of the 23 patients experienced their "alters" not only in real life

but also as dream characters. In addition, eight of these patients had personalities reporting different dream memories. This led Barrett to conclude that dream characters are "normal dissociative cognitive and personality processes which operate largely outside consciousness." In waking life these characters can occasionally break into consciousness as dissociations (Barrett, 1995, p. 66). According to this perspective, lucid dream characters represent unexpressed parts of individuals that gained, so to speak, their own consciousness and are a normal analog of alters.

Indeed, many of the entities or characters appearing in lucid dream states do assert that they have their own independent memories or identities. Paul Tholey, a pioneer researcher of lucid dreaming, reported that when one of the characters was questioned in the lucid dream about his true identity, the character responded with: "I am sure that I have a consciousness, but I doubt if you have one, because you ask me such stupid questions!" (Tholey, 1989, p. 574). In another account, a dream character asserted "if I am a dream character then how come I can recall a whole other life before this moment. I have a husband and child and a whole other life" (quoted in Waggoner & McCready, 2015, p. 105).

Since Tholey's time onward, most lucid dream researchers fall into two groups. The first group are university-based researchers who publish in mainstream journals and focus on the normal characteristics and the psychophysiology of lucid dreamers rather than on the extraordinary experiences of the so-called adepts. The second group are themselves adepts, sometimes with an academic education, who have taken it upon themselves to document and explore their own experiences and those of others. It is of course easy to dismiss the accounts from this latter group as uncritical and biased, but perhaps a fairer critique is that many authors support their personal records with those taken from Internet sources. Nevertheless, one assertion in these accounts is currently being confirmed by our ongoing study of adept lucid dreamers: The adepts in our study do report a remarkable ability to exert control over the induction of lucid dreaming, to explore its content in a critical way, and to experience dream characters.

An example of these authors is Robert Waggoner. Although concluding that most dream characters represent previously unexpressed sides of oneself, Waggoner asserts that this is not always

so since his efforts to dismiss the characters occasionally result in the attitude described above by Tholey: "Some dream figures remain and look at me with a sense of disbelief that I did not recognize their independence from my thought processes" (Waggoner & McCready, 2015, p. 110). Indeed, this does seem to be the consensus opinion among the adepts. Clare Johnson, another adept who is an academic writer, suggests that lucid dream characters can vary from passive wooden characters to those expressing psychological needs, and ultimately take the form of what appears to be a spiritual guide. In agreement with Waggoner and Johnson, David Jay Brown, who has a neuroscience background, sees characters as creations of his own mind, but also believes that some characters may belong to other minds, and others may be even be "higher beings" (Brown, 2016).

One of the few research psychologists to study dream characters is Tadas Stumbrys. On the basis of his online survey, it would seem that about half of dream characters are friendly while one fifth are hostile (Stumbrys & Erlacher, 2017). As compared to those suffering from nightmares, the adept lucid dreamers were able in some way to resolve the conflicts with the hostile figures, suggesting some dream characters may express intra-psychic conflicts. Another study by Stumbrys and co-workers (Stumbrys & Daniels, 2010) indicated that dream characters show a high degree of creativity and cognitive functioning, but, interestingly, yet another study (Stumbrys et al., 2011) confirmed Tholey's observation that their mathematical ability was limited—to that of primary school children.

Adding to the view that lucid dream characters are dissociated parts of cognition and personality, are the accounts of the lucid dream figures indicating that they have their own visual perspective, and even their own thinking (Tholey translated in Johnson, 2017, pp. 117, 124). Among the anecdotal reports from lucid dreamers who described meeting deceased dream characters, were also about ten percent claiming to have received information they could not have known (Puhle & Parker, 2017).

Besides the need for more objective independent research, what is obviously lacking in the current research into these experiences is an attempt to distinguish between reports relating the different types of lucid dream figures as categorized by Waggoner, Brown, and Johnson.

Near Death Experiences (NDEs)

A major survey of cases of NDEs from the USA, Canada, and Australia found that 69% of the collected cases reported the presence of someone, and in 10% of cases they saw and recognized the deceased person (E. W. Kelly, 2001). Only 4%, eleven of the 274 cases, reported seeing *living* persons whom they recognized. There was no clear relationship of seeing deceased persons with the medical condition or closeness to death of the person concerned. The perception of these figures seems to be a core feature of the NDE since those who saw deceased persons were more likely to report the other major features of NDEs—enveloping light, an OBE, darkness, or a tunnel-like experience. In seeking "normal" explanations, it remains an unanswered question as to how many of these figures are created by dissociation and how many by mere expectancy. Whatever the case is, they do also fit with "End of Life Experiences" where caretakers and relatives frequently report dying persons having experiences with apparitions coming to "take them away" and deathbed visions (Fenwick et al., 2009).

Because NDEs are often dismissed as hallucinations caused by expectancies and dissociation, the many claims of veridical perception of near–death experiences have become contentious (see Holden et al., 2009, and Rivas et al., 2016, for reviews of these cases). Each of these cases has been subjected to careful scrutiny in attempts to identify alternative explanations—often without much respect for their plausibility. However, if it is conceded that ESP in the laboratory has been established and that it occurs most often during altered states of consciousness, then it is quite conceivable that ESP is an integral part of consciousness in NDEs. This still begs the question as to what ESP actually is, and what is the nature of the experience as a whole?

Michael Nahm (2011) has provided one answer to this in his impressively comprehensive survey of NDEs where he focuses on cases that can be described as "reciprocally confirmed." By reciprocally confirmed, he means the information encountered during the NDEs has been independently confirmed by information found in related coincidental cases. An example of this would be from medium communications where the dead person announces a birth and the born child later recalls the previous existence. Another example would

be where the person during an NDE sees a living person and that living person experiences an apparition of the other person having the NDE. Other apparently confirmatory cases concern shared NDEs, and intermediate states between death and reincarnation.

In finding such albeit extraordinary cases, Nahm argues that a full circle seems to have been created in tracing the continuation of consciousness after death from NDEs, intermission, and reincarnation. However, the cases Nahm reviews vary greatly in evidential value and he makes no attempt to evaluate their quality.

Automatic Writing and Savant Abilities

Automatic writing is an enigmatic form of dissociation cases where self-proclaimed entities assert their own independent identity and sometimes demonstrate skills that the normal personality has never, as far as can be determined, had the opportunity to acquire. One of the most outstanding cases is that of Patience Worth who claimed to be a 17th-century Englishwoman who had emigrated to and died in the United States. A proficient knowledge of the Anglo-Saxon language and its culture was demonstrated by the host personality, Pearl Curran, a thirty-year-old and a school dropout who had never left the Midwest. What was equally astounding was the incredibly rapid production of words and images, which spontaneously entered Curran's mind without any conscious effort or control. This suggests Curran had developed a form of co-consciousness. Co-consciousness is in terms of modern psychology represented as multitasking, but the form and content expressed by Pearl Curran is clearly exceptional. Moreover, the actual source of the information produced by Curran remains unexplained.

The case was investigated by W. F. Prince (1927/1964). Reputed to be critical-minded, Prince rejected the spiritualistic claims made by Patience Worth but was unable to offer any plausible explanation for her behavior. In recent years Stephen Braude (2003) devoted considerable time and effort to the case and even conducted further searches, which put to rest any notion that a real Patience Worth had ever existed. Braude was then left with the theory that Patience Worth was an "alter" whose presence allowed the latent literary skills of Pearl Curran to gain expression. Yet, there was never any evidence of Pearl Curran in earlier life having practiced these skills and nor had she shown any

dissociative states prior to the entry of Patience. On the contrary, Pearl had regarded the Ouija board as producing "silly chatter" and did not seem motivated to acquire any skills. A more recent review of the case by Gioia Diliberto (2010) concluded much the same as Braude: "The 17th-century spinster gave Pearl's life shape and meaning and allowed her to project herself beyond the confines of domestic womanhood to become a writer." The difference was that Braude did not hesitate to credit Pearl Curran via Patience with a psychic ability to access archaic forms of English and history.

It is of course easy to try to make the Patience Worth case vanish with the magic word "anomaly," but several other cases exist such as Hélène Smith and Rosemary Brown (Braude, 2003). Not only do these cases exist but there is another set of "anomalies": the congenital and acquired savants. In terms of latent abilities, these cases merge with the cases of automatic writing except that their remarkable abilities are not attributed to spirits or secondary personalities but are left hanging in the air, largely unexplained. The majority are congenital cases where severe brain damage has occurred. The genius-like abilities that characterize savants are assumed to have occurred in a compensatory manner following the release of an area of the brain, usually the right hemisphere, from the inhibition which would have otherwise been exerted had it not been for the damage. Yet the challenge, which mainstream psychology has never even attempted to deal with, concerns the savants who know things they never have learned. Darold Treffert, who is generally regarded as the world's leading expert on savant cases, attempts to deal with this by assuming this knowledge must somehow have been "factory installed" into the genetic software of our brains. It would seem to be almost in desperation that Treffert further defines this as a genetic form of the Jungian unconscious or brain plasticity bringing this about (Treffert, 2000, 2017). Yet genetic theories and brain plasticity become rather untenable when we consider the *acquired savant cases.*

The acquired savant cases generally occur as an outcome of concussion, and so-called "calendar calculators" (who can give the day of the week for a date in a given year apparently without calculating) are the most well-known examples of these. There are, however, cases with other skills such as Derek Amalo, described by Treffert, who had never played the piano but who could spontaneously play at a professional

level. Most challenging are the cases that can occur entirely devoid of any injury. The skill then occurs almost as a form of epiphany where a detailed knowledge is revealed of, say, the underlying principles of music, art, or math. Treffert even includes in this category some cases of *foreign language syndrome* in which there was little or no opportunity to have learned the language now fluently spoken (Treffert, 2010, 2017).

As for the explanation in the form of a genetically installed memory, Larry Dossey rightly asks concerning some of these skills (such as calendar calculating) "what is the survival value of knowing, as do savants, vast information in a narrow field that is utterly trivial?" Savants are surely then an example of further "rogue phenomena," but in this case belonging primarily to psychology rather than parapsychology. Nevertheless, they too are shunted aside as anomalies from mainstream clinical psychology and psychiatry. Dossey dares then to express the challenge that savants pose: "Savants are earthquakes that shove sacrosanct theories of the mind off their foundations and reduce them to rubble" (Dossey, 2012).

Perhaps the person who is at least potentially gifted with the insight to resolve some of the enigma, is Daniel Tammet. Tammet is a savant in mathematical and language areas with a diagnosis of Asperger syndrome, although he has become a socially skilled and insightful person. In contributing to understanding just how his abilities work, Tammet describes how his own synesthesia is an integral part of the ability that enables him to see symbols depicted as part of landscapes. Tammet does not calculate in conventional ways; instead the answers come spontaneously and take the form of landscapes that he can identify. His language ability seems facilitated also by synesthesia and by his ability to see connections between words (Parker, 2011).

The logical hypothesis, even if unpalatable for the skeptic, is that the savant phenomena might indicate that consciousness has access to some form of reservoir of knowledge beyond what is registered by normal brains. Indeed, Treffert and others describe that the savant is not learning knowledge but discovering it. The question naturally arises then if such savants show any evidence of psi in specific terms. There are a few anecdotal accounts (Rimland cited in Treffert, 2009) suggesting this might be so, but to my knowledge no controlled research has ever examined this possibility.

Psychedelic States

As is the case with psi experiences, there exists a stark polarization of opinion concerning the reality behind psychedelic experiences, which has inevitably led to a similar entrenchment of positions and unjustified stagnation of research on this issue. Those who have had profound experiences under DMT or LSD tend to be emphatic that they are "more real than anything ever experienced" (Luke, 2017; Cott & Rock, 2008). Those who have not had these experiences tend to be dismissive of them as chemically caused hallucinations. Rick Strassman (2001), who carried out the first studies following the era of the prohibition of psychedelic research, documented how many of his research participants unexpectedly reported the spiritual value of DMT. The spiritual aspects led Cott and Rock (2008) to find points of commonality between psychedelic experiences and NDEs, and David Jay Brown (2016) listed twenty correspondences between lucid dreaming and psychedelic experiences. What does, however, distinguish psychedelic experiences, especially those of DMT, from other altered states, is not just the spiritual beings and guides that are more frequently encountered, but the vast diversity of alien entities and mythological creatures that are reported.

For those experiencing these weird beings and events, the crucial persuasive factor, reminiscent of the claims made in other altered states, is that these entities, whether they be spirits, elves, dwarves, or reptile-like creatures, give to the perceiver every indication of being sentient beings. For those not having this type of experience, this will naturally just seem ludicrous. Whatever the case, it is still claimed that the "beings" communicate insightful information to those perceiving them (Luke, 2017). It should also be said that David Luke, who is the indisputable expert on this topic, did carry out a review of the research attempting to explain psychedelic experiences in neurochemical terms (as DMT-metabolites, ketamine, and dopamine), but he found no definite causal link. There is considerable evidence, also reviewed by Luke (2017), suggesting there is a link between psychedelics and psi experiences—with the caveat that there are special methodological issues in working with psychedelics—issues which so far preclude any firm conclusions.

In terms of explanations, the choice still remains between attributing these experiences to some form of Jungian archetypal unconsciousness released by the specific nature of the psychedelic substance or to a delusionary drug-induced state (or perhaps some combination of both). One can only agree with Luke that only further parapsychological experimentation, carefully adapted to the demands of the psychedelic situation, can take this area forward. Such a study could determine if there occurs an overlap in experiences of suitably chosen individuals who are having simultaneous psychedelic experiences in different locations. In addition to supportive guidance, if a shared phenomenological world were to be experienced, a parapsychological aspect could then be introduced by giving one of the individuals a code or gesture to convey to the other. A shared phenomenological world can apparently also be experienced in shared lucid dreams (Waggoner & McCready, 2015). At Gothenburg with the collaboration of Engelbart Winkler, we used two Lucia stroboscopes to induce dream-like states in a small group of selected volunteers, but to our chagrin the only success we obtained were from the experimenters when they were in the role of participants (Parker, 2017).

Mediumship

It is clearly beyond the scope of this review to evaluate the evidence for a postmortem continuation of consciousness obtained through mediums. What is being suggested here is that some of the most convincing or challenging communications from mediums can be understood in the same manner as that for entities appearing during lucid dreaming and psychedelic states, that is as expressions of dissociated and autonomous states. This conceptualization differs in a subtle but important way from the more dismissive hypothesis claiming that these communicators are mere secondary personalities of mediums. The entity is re-conceptualized here, not so much as being a repressed part of the medium personality, but as part of the medium's consciousness that has gained its own integrity and independence.

Indeed, it is striking how in a manner similar to what has been encountered with lucid dream characters, the communicating entity asserts it has the integrity and willpower of a real person. A much-quoted example is the following message from the entity claiming

to be the post-mortem Frederic Myers communicating through the mediumship of Trix Fleming (aka Mrs Holland), the sister of Rudyard Kipling, and one of the principal mediums involved in the "cross correspondences." These were a series of apparently interlocking messages that some contemporaries came to believe Myers and other deceased leaders of the Society for Psychical Research had provided as evidence for survival after death. They were given largely independently by different mediums in various parts of the world as evidence for their postmortem existence. The often-quoted message from the deceased "Myers" reads:

> It is impossible for me to know how much of what I send reaches you. I feel as if I had presented my credentials—reiterated the proofs of my identity in a wearisomely repetitive manner. The nearest simile I can find to express the difficulty of sending a message is that I appear to be standing behind a sheet of frosted glass, which blurs sight and deadens sound, dictating feebly to a reluctant and somewhat obtuse secretary. A feeling of terrible impotence burdens me. (Johnson, 1908–1909, p. 208)

The cross-correspondences continued until 1930 but partly because of their complexity, critical evaluation is very divided as to their interpretation (Carter, 2012; Hamilton, 2017; Parker, 2010a). Nevertheless, irrespective of this lack of agreement, the emotional tone and appeal is very evident throughout many of the accounts and is reminiscent of similar appeals by entities occurring in lucid dreams and other states. Previously, the skeptical position has simply dismissed the content of such messages as hallucinations, and that any meaning linking the messages would be due to chance and confirmation bias (subjective validation). That aside, the idea being advanced here is that communicating entities or parts of the original consciousness have an integrity in the sense of being convinced themselves of their own existence and independence.

Just how independent these entities were, is of course part of the perennial debate, but it seems as far as the Fleming records and those of the other mediums involved go, it would be naïve skepticism to blindly deny the many incidences of "extraordinary synchronous phenomena" (Hamilton, 2017).

Another of the aristocratic women who formed the core mediums involved in the cross-correspondences was Winifred Coombe-Tennant (then writing under the pseudonym Mrs Willet). She also described her state when automatic writing in terms that are congruent with the above conceptualization. She experienced this state as a division in the conscious self: One part of herself became the communicators but at the same time some form of unity was maintained in the form of a double state of consciousness. Some of this is evident in her words:

> Don't you ever walk out of yourself? Aren't you tired of always being yourself? It's so heavenly to be out of myself—when I am everything, and everything else is me . . . . so strange to be someone else, to feel somebody's heart beating inside you, someone else's mind inside your mind, And there isn't any time or place and either you're loosed or they have entered, and you all of you sudden know everything there ever was. (Quoted in Tyrell 1960, p. 160)

The eminent philosopher C. D. Broad writes in his assessment of the Fleming mediumship that even with a skeptical assessment: "We shall then have to postulate in some stratum of Mrs Willet's mind rather remarkable powers of acquiring information from unread books or the minds of living persons or both" (Broad, 1962, p. 313).

The third major medium, Leonora Piper, involved in the cross-correspondences showed in contrast a "trance state." This state seemed to be more than role-play, as some modern theories of hypnotic trance attempt to explain this, since she did not respond to various unexpected painful stimuli. She showed signs of a sleep-onset or hypnoidal state merging into what was described as a genuine "possession state" (Sidgwick, 1915), which will be discussed later.

Apparitional and Poltergeist Phenomena

Apparitional cases are of several different types. The most commonly reported are crisis apparitions occurring synchronously with death or some major emotional event. Given the evidence that some of these are non-chance events and not explicable by simple expectancy (Haraldsson, 2012), they can be understood as having a psi component in the sense of expressing the connectedness between the consciousnesses of individuals who have an emotional bond. There is

nothing new here and neither is there anything new in the long list of normal explanations for many of these experiences, such as sleep deprivation, stress, fantasy proneness, hypersensitivity, the effect of prior beliefs, and expectancies. Often "normal cases" are used as straw men to dismiss the whole area and to avoid the challenging cases.

One of the classical challenging cases is the Rosenheim case where phones repeatedly dialed by themselves, lamps swung by themselves, electric bulbs burst, pictures and calendars fell off walls, and large objects moved of their own accord (Resch, 1968; Bender, 1971). Normal explanations were excluded as far as humanly possible through the full resources of German meticulousness. These resources included those of the Criminal Investigation Department of the Police, the Municipal Fire Service, the Building Works, and the Electricity Department—as well as physicists from the Max Planck Institute in Munich. There were at least forty persons who were first-hand witnesses to the events. R & A can perhaps be consoled by how, despite all these efforts, nothing was learned about the causal aspects (other than that a young woman was the focus person).

One of the early documented historical accounts and which despite its age is more revealing concerns the Rerrick case in Scotland from 1695. The case was a mixture of a haunting and poltergeist witnessed by fourteen individuals; five of whom were clergymen (Gauld & Cornell, 1979). Several witnesses saw apparitions and phantasms along with typical poltergeist phenomena such as stone throwing, raps, fire setting, the inexplicable movement of furniture, and the disturbance of animals. Because poltergeist phenomena are framed in terms of the culture and context of the time, the Rerrick case was readily attributed to demonic crafts. What is revealing in all the well-attested cases is that the phenomena often involved the hurling of heavy objects through the air and the setting of fires, many of which were potentially lethal to those in the vicinity. This does not just apply to the Rerrick case but seems to be a recurrent objective feature of virtually all cases. The skeptic, who obstinately attributes poltergeist phenomena to fraud, would then be forced to concede that the perpetrators have a seemingly extraordinary ability, bordering on the magical, to consistently just miss hurting or killing the persons involved. Research psychologist Alan Gauld writes: "These intelligences, which so clearly reflect prevailing folklore and

religious beliefs, must simply be aspects of living people . . . acting in ways we do not understand" (Gauld, in Gauld & Cornell, 1979, p. 171).

A recent case which may be further revealing as to the nature of the phenomena, is the South Shields poltergeist (Hallowell & Ritson, 2009) reviewed by Alan Murdie (2010). The phenomena here included not only classical poltergeists but also more conventional expressions in the form of toys coming to life and threatening text messages being left on mobile phones. The poltergeist was similar to the one in the Rerrick case, responsive and interactive, showing it was not just a set of anomalous phenomena but with its own developed identity. Ironically, despite the modernity, there occurred later a dramatic throwback to medieval times when the amateur investigators became completely convinced that demons, in particular those of jinns, were responsible. Clearly, the case shows how cultural coloring and biased interpretation of the basic phenomena take place. Even cultural repainting can occur. Demon from Greek "daimon" meant originally a godlike being, a lower god, a being between gods and humans, who can be good or bad, and also a spirit of a deceased person (Puhle, 2019).

This sinister aspect of this case shows a resemblance to another contemporary case (Fisher, 1990). Fisher obtained through group séances with a medium the exact details of the former lives the medium's communicators were claiming to have had. Extensive investigation in England and Greece confirmed many of these improbable and virtually inaccessible details. In one such case, that of "Ernest," obscure and personal information concerning his bomber squadron was given and confirmed by still-living members of the squadron, and yet Ernest himself almost certainly never existed. This led Fisher to be fully convinced that demonic spirits which we called "Hungry Ghosts" were deceiving him.

The case known in the psychical research literature as "Conjuring up Philip" is one of the few innovative efforts, which may well give further insight into the sought-after "causal relationships." The case concerned a series of séances in Toronto under the direction of the former Cambridge University geneticist George Owen, and his wife Iris Owen (Owen & Sparrow, 1976). The Owens had the idea to invent a ghost story for the members of a séance group to use in the manner of a psychodrama. The fictional biography was dramatic enough to immerse the group in play-like behavior, and after about a year raps

occurred that responded to questions. The pattern of responses seemed to indicate the presence of a communicator who had developed its own will. Although the Owen group was able to record audio and video, their attempts at table levitation and producing raps, the paranormal sources for these, were not fully convincing, at least not to the current writer. However, what supports this approach, as potentially illustrating a principle, are the independent and yet similar findings reported by several others (Batcheldor, 1984; Björkhem, 1994; Pilkington, 2006; Türck, 1945). In particular, the Türck case presents a more (although not entirely) convincing form of photographic documentation. The unifying psi-conducive principle they illustrate is to create a group consensus reality in which the normal constraints over what reality is and what is possible are temporarily suspended (Parker, 2010b).

Creativity

There is considerable evidence to link creativity and altered states, especially dream states (Barrett, 1993; Zink & Pietrowsky, 2013). There may well be important links between dissociated states and creativity. Recently, John Foxwell and co-workers (2020) at the University of Durham interviewed writers attending a book festival concerning the autonomy of their characters. Close to two-thirds had heard their characters as voices and one-third experienced the characters as separate from their own thoughts and inner voices. More than half of the respondents had experienced some form of visual presence, and 61% thought their characters had their own agency, often determining how the plot would work out. In this investigation, no questions were apparently included about ESP or transpersonal experiences.

The psychical researcher Hereward Carrington (1920/1996) appeared convinced that such characters as thought-forms could gain an autonomy, and he illustrated this claim with a case of a medium apparently responding to a fictional person that a writer was engaged in producing. There is some experimental and anecdotal evidence from various studies that writers and artists are more prone to psychic experiences (e.g., Dalton, 1997; Dinnage, 2008; Holt, 2007; Prince, 1963).

Adam Crabtree, who has worked extensively with psychodrama, presented case histories suggesting:

> There seems to be within people a natural ability to take on a personality other than their own and to act from within that assumed personality. They appear actually to become that personality and to some extent leave their own personality behind. (Crabtree, 1985, p. 337)

Crabtree goes on to suppose in a similar manner to what is being proposed here that "one's many selves may retain a certain type of consciousness of their own when not operative in the world" (Crabtree, 1985, p. 348).

What is also highly relevant here concerns the little-known work of the Finnish psychiatrist Remi Kampman (1976), who discovered, contrary to his own expectations, that school pupils capable of producing secondary personalities during hypnosis were actually clinically healthier and more adaptive than those without secondary personalities.

EXPERIMENTAL PARAPSYCHOLOGY

The skeptic might now ask, supposing forms of dissociation are a unifying concept in spontaneous phenomena, then how does this relate to ESP performance in the laboratory?

There is considerable evidence that the most successful and efficient experimental designs are those using the so-called free-response methods: Among these, the ones using altered states, especially the ganzfeld, have the highest effect size (Storm et al., 2010). Yet, even if we go back to the card-guessing paradigm of J. B. Rhine, his star subjects were described as having been in a "state of 'detachment', 'abstraction', 'relaxation', and the like." Rhine's account described in detail the trance-like appearance of his star scorers during testing (Rhine, 1934/1964, p. 131). Remarkably, what is left un-researched are the strategies used by the sender in ESP experiments. A common, perhaps the most usual, instruction is "to feel in some way at one with the receiver," which would be in keeping with what is being proposed here. What does not apparently fit with the notion that ESP is a form of connectedness, are the occasions in which it does seem to be a perceptual ability. Contrary to the portrayal by R & A of there being only marginal effects, there are occasions of perfect or near-perfect

performances with Zener cards and the ganzfeld procedure. Perhaps these spectacular successes are somehow due to the involvement of the experimenter maximizing performance. What I agree with R & A about is that such maximum effects are the best opportunities for revealing errors or causal relationships.

The wish to keep science squeaky clean from experimenter effects may be a forlorn one both in psychology and parapsychology. Even the experimenter's own psychic experiences may well be "a well-kept secret" that determines the track record of success in psi research and, dare I say this, in other areas (Parker & Millar, 2014).

One the few important findings from the laboratory concerns the misnomer "psi-missing," which rather than missing is the ability to *significantly* avoid choosing the target. Although often dismissed by skeptics as an ad hoc finding, there seems to be sufficient consistency and conceptual meaning to regard it as a discovery and one having a reality-import. In other words, it would be the laboratory equivalent of the jinxed ability to consistently make the wrong choice in life situations. Rather than a jinx or even a trickster archetype (Hansen, 2001), it would be argued here that it is sufficient to conclude that this is an expression of the self-defeating needs or self-doubt of the person running amok.

Finally, it may be asked how psychokinesis would fit into this conception? For this it would be necessary to give a central role to *willpower*, which regrettably is largely today forgotten in psychology. The "Will" was a chapter in William James' classic book *Principles of Psychology* and formed the cognitive psychology of his successor William McDougal, but the monopoly of contemporary cognitive psychology has led to a will-less motivational psychology. For a while it seemed that Libet's experiments in the 1980s, showing that a preparatory or readiness potential preceded decision-making in the brain and suggesting all behavior was pre-determined by neurology, would ban even motivation as an illusory concept. As is often the case for extreme claims, later findings have questioned this radical conclusion so that a belief in free will is still at least defendable (Fifel, 2018). "The Will" is also a concept central to Schopenhauer's philosophy which is being revitalized by the philosopher–physicist Bernard Kastrup (2020).

THE FAILURE OF CONVENTIONAL THEORIES OF ALTERED STATES

The question is how far can the currently accepted theories be stretched to explain rogue phenomena: Does dissociation itself suffice as an explanation? Dissociation was until recently a diagnostic concept applied mainly within the clinical area for diagnosing multiple personality and for explaining hypnotic phenomena (Kihlstrom, 2013). The re-defining of multiple personality as "dissociated identity disorder," has allowed the concept to find a broader application so that now several scales exist for assessing dissociation as a dimension among the normal population (see Parker, 2015a; 2015b for reviews). For instance, such common occurrences as multi-tasking or engaging in conversation with oneself can be seen as a normal form of dissociative behavior. The theory is that information processing can occur outside the awareness of our central executive self—and can have its own memory registration.

An alternative to the concept of dissociation is "social–cognitive theory," which conceives that behavior, which is inconsistent with the person's self-concept or role, is given socially prescribed explanations. A current illustration of this is found in the way that members of the White House have become masters at explaining away presidential indiscretions in more socially approved terms, for example as "locker-room banter" rather than un-presidential behavior.

The theory gained much credibility due to an experiment in role-playing carried out by Nicholas Spanos and his co-workers. Students took on the role of having committed murder and were asked to get in touch with "a hidden part of themselves." This worked to the extent that most of them enacted this role and showed signs of what would be clinically regarded as multiple personality. Spanos et al. (1985, 1994) has applied this model to some of the above phenomena ranging from demonic possession and spirit possession, to past life regressions. Spanos does not, however, even attempt to deal with any of the above-challenging cases or indeed any challenging cases. In circumventing the concept of "dissociation," Spanos also avoids any discussion of the nature of consciousness that lies behind the various forms of role-playing and delegation, especially ones when the roles become incongruent.

This widespread avoidance of issues has meant that contemporary clinical psychology and psychiatry are ill-equipped to deal with some of the rare but traumatic effects of ego loss that can occur through psychedelic and shamanic experiences or indeed spontaneously. Recently Rachek Aviv (2018) documented the serial disappearances of Hannah Upp, who suffered from dissociated fugue. Hannah's consciousness lost all its sense of identity, which seemed to occur on entering what she described as a mystical state. Periodically, she would then go missing, which finally occurred without her being found. Conventional treatment had apparently nothing to offer her or similar such people.

ROGUE FINDINGS IN NEUROSCIENCE

This failure to deal with rogue phenomena occurs in other areas of neuroscience. There is the current case of Noah, who, as documented by his neurosurgeon Claire Nicholson, was born with hydrocephalus, leaving a mere 2% of brain volume. Yet by the age of 4, this had grown to 80% with little or no sign of intellectual impairment. Much of this success was attributed to high parental involvement, belief, and care (Noah, 2017).

Neurologist and vocal skeptic Steven Novella (2016) argued concerning a similar case (Feuillet et al., 2007), that when the basic structures are in place they can later become functional when the hydrocephalus is treated and the pressure relieved. This ignores how such pressure in radically squashing the fetal brain would not only compress but grossly deform the brain's delicate structures. To believe otherwise would be the Donald Duck character version of the brain in which Donald is flattened by a streamroller but emerges unscathed. There are further dramatic case accounts that current reductionist neuroscience has been found to be totally inadequate in dealing with (Bolte Taylor, 2008; E. F. Kelly, 2018; Pistorius, 2015). There is also a review of the discrepancies between cerebral structures and cognitive functioning published by Nahm, Rousseau, and Greyson (2017).

Finally, it should be added that the evidence indicates consciousness has a primary influence, does not just consist of clinical anecdotes but is prominent in two other as yet un-integrated areas of neuroscience,

namely hypnosis and psycho-immunology. Experimental hypnosis has existed for as long as experimental parapsychology, and although there is still a lack of consensus about what hypnosis actually is, no authority would seriously question the dramatic effects of hypnosis as a form of psychological intervention that can alter perception (for example creating hallucinations) and the autonomic nervous system (for example heart rate). Likewise, the effect of expectancy and belief on the strength of the immune system has been well-established for many years (Cloninger, 2004; Ornstein & Swencionis, 1990).

THE IMPLICATIONS FOR A THEORY OF CONSCIOUSNESS

It would seem clear from the above review that the current science of cognitive psychology has survived through default, in the absence of a better theory and through the choice to ignore or at best downgrade a multitude of phenomena as anomalies. The range and content of these experiences all are pointers to a radically more dynamic view of consciousness than that conceived by the contemporary theories of cognition. Our normal state of consciousness is focused on a temporary and transient social self while other states have potential access to transpersonally connected consciousness. This is not a new supposition but has featured in the works of several contemporary writers (Braude, 1995; Carter, 2012; Crabtree, 1985; Kelly et al., 2015; Rowan, 1990; Tart, 1986).

What is new in the above presentation is not the role of consciousness itself, but of *co-consciousness*. Co-consciousness can be defined as the simultaneous operation of two independent systems of mental activity. Stephen Braude (1995) has made an extensive discussion of the term with respect to multiple personality and fugue states. The problem, highlighted in Braude's discussion, is that the terms dissociation and co-consciousness are not mutually exclusive and are not absolute, watertight categories. There are varieties of dissociation where the barrier between the "alters" differs, with some "alters" being aware of each other and sharing memories, while for others this is not so. A degree of co-consciousness without much of a memory barrier can be said to occur in multi-tasking, but this contrasts with the more extreme form, which occurs in the many cases

of mediumship and automatic writing, some of which were mentioned earlier. In the case of thought-forms, it goes one stage further and it would seem that consciousness has been fully transformed into two or more autonomous self-aware entities.

However, thought-forms are insufficient to explain all the vagaries of the phenomena. For this we need to refer to the major contribution of Stanley Krippner who it can be said has more than anyone else produced order among R & A's "farrago" phenomena. What we experience in an altered state is according to Krippner decided by the different combinations of dissociation and flow with respect to the degree of control exerted over the altered state. Some of the states, such as those occurring in mediums, are instances of controlled dissociation in that they occur largely to order, whereas in multiple personality and possession the states are uncontrolled. Dissociation and flow are seen by Krippner to be opposing poles of the same dimension. If flow is controlled by rituals such as those in shamanism or in the prescribed use of Ayahuasca, then experience can be an ineffable or a mystical one—an encounter with what Krippner terms the "all-self." How these experiences are received depends of course on the prevailing zeitgeist, so that possession states in the context of paganism was seen by the Catholic Church from the fourteenth century onward as works of the devil (Krippner, 2000; Krippner & Powers, 1997).

Some fundamental changes will eventually be needed in psychology in order to accommodate these findings, and there are signs of this happening. An example is found in the work of the former CERN-physicist and now also philosopher Bernardo Kastrup, who is responsible for some of the blogs and exchanges in *Scientific American.* There, in collaboration with the modern interpreters of William James and Frederic Myers, Kastrup argues in strong philosophical terms for the function of consciousness in actively creating consensus reality (Kastrup, Crabtree, & Kelly, 2018). They write: "the one universal consciousness could, as a result, give rise to many alters with private inner lives like yours and ours. As such, we may all be alters—dissociated personalities—of universal consciousness." Years earlier in writing about channeling, John Klimo (1998) arrived at the identical conclusion:

> We are all sub- or alter-personalities within one Universal Mind . . . .

> The sub-personalities within an individual MPD/DID (or co-conscious) subject usually believe that they possess an identity separate from their host or parent mind. Similarly, . . . we sub-personalities of the Universal Mind maintain our dissociated states, relatively unconscious of our deeper identity. (Klimo, 1998, p. 357)

CONCLUSION

Even if consciousness is conceived of as primary in nature, which is the basic assumption in this theoretical reasoning, it seems to have from birth a biologically built-in organizing function (Stern, 1985; Trevarthen, 2011). It is this organizing function that creates a consensus reality. As such, the normal states of consciousness impose restrictions, the so-called basic limiting principles concerning the temporal and spatial relationships that R & A defer to. Yet these should not be regarded as holy principles since even these show some cultural influence. The Amondawa are an Amazonian tribe first discovered in 1986 who do not have a concept of time that can be "measured, counted, or talked about in the abstract." Individuals do not age but take on new names, which reflect their life stage and position (University of Portmouth, 2011). It could well be that our own concepts make it difficult to capture psi in a test tube since its presence would invalidate many of these principles organizing consensus reality. It is these limitations that sabotage replication efforts in experimental parapsychology, according to Walter von Lucadou (2017). Von Lucadou's way of luring in these limiting principles is to use correlation matrixes composed of alternative ways that psi can morph. The matrix, by allowing alternative outcomes and by applying statistical corrections for multiple analyses, may become a novel way of resolving the replication issue (Walach et al., 2020). Should one be willing to go further (which I doubt many skeptics or physicists would) and regard these effects as nonlocal correlations in quantum theory (Walach et al., 2014), then these effects can be seen as facilitated by dissociated states.

Leaving aside the controversy surrounding basic limiting principles and now focusing on the phenomena, the evidence from the above interaction between dissociation and co-consciousness then is that many of the entities (irrespective of whether they are occurring in lucid dreams, psychedelic states, NDEs, or mediumistic states) are *from*

their perspective making entirely authentic claims as to their identity. The entity version of Myers communicating through the medium Trix Fleming really does believe itself to be Myers and may have acquired access to some memories of being so. There is a certain irony in that some of the ideas expressed here about the consciousness of split-off entities were already beginning to be explored by Myers when he was in the living form (Myers, 1903/1975, pp. 36–39). Unfortunately, Myer's ideas never caught on, and psychology continued to operate on a theory of science from Isaac Newton and a theory of neuroscience that has changed little since Johannes Müller in the early nineteenth century.

Some signs of a remedy to this situation are found in the current work of cosmologist Bernard Carr (2015) who has developed a dimensional theory of physics that includes human consciousness, where time perception and mystical and ESP experiences are interrelated. However, it seems obvious that for a theory to give us deeper understanding, a more detailed and integrated explanation linked to mainstream psychological science as well as modern physics needs to be forthcoming.

There are, however, problems with the arguments being put forth above. In addition to the unfinished theoretical aspect, much of the case material is unashamedly anecdotal. There is for instance the case cited by Harvard psychologist George Estabrooks, who was an important contributor to what have become our modern ideas about hypnosis (Estabrooks, 1962, 1971). Estrabrooks claimed to have created the hypnotic equivalent of "the Tulpa." By means of auto-suggestion, he was able to conjure up an imaginary polar bear which apparently developed its own willpower and ran amok chasing nurses and became hard to get rid of (Estabrooks, 1957, pp. 93–94). Like the *Conjuring Up Philip* case, this one, too, lacks serious and extensive followups.

Skeptics seizing on this will of course ignore the insurmountable logistic problems in allocating the virtually nonexistent resources for studying taboo topics. The irony is that we simply do not know if the problems of parapsychology highlighted by skeptics are as they claim, insurmountable, or actually a consequence of skeptics' success in effecting a prohibition of research on the topic.

While it is almost a cosmic joke that both the Philip case and the earlier-mentioned Hungry Ghost case actually occurred in R &

A's backyard, it is nevertheless easy to understand the reluctance of skeptics to indulge in scrutinizing activities, which on the surface seem to be frivolous. The paradox is that we may need to accept, if only on heuristic grounds, that the suspension of the rational waking state seems to be a prerequisite for the phenomena to occur. This does not at all have to mean the abandonment of customary controls exerted by the lead experimenter. It is here that genuine skeptics (adhering to the Greek meaning of *skeptikoi*) have a vital role to play in promoting critical standards in parapsychology research in order to discover a way forward.

So, is there a way forward? Here recognition is also needed for the real experts on the psychology of deception: the professional illusionists. Contrary to what many skeptics might expect, there are surveys and reports indicating that the large majority of magicians both as a group (Truzzi, 1997) and as practitioners (Hansen, 1990) strongly endorse the existence of genuine paranormal phenomena. Since the surveys were not done for public consumption, mere promotion of the mystique in magic would not seem to explain this. The explanation may well be that the performances succeed in creating a magical atmosphere akin to the Philip case in which belief and reality are temporarily suspended and critical ability is so overwhelmed that the psi-conducive atmosphere that follows sometimes allows "real magic" to happen. This would offer an ideal situation for skeptics to cooperate with magicians *who have this conviction and these skills*.

The ramifications suggested above would not be complete without giving credit also to Shakespeare whose works are replete with this way of thinking (see Puhle & Parker-Reed, 2017). This is probably best illustrated in the play *Julius Caesar* when Brutus in a half-waken state before his final battle sees an apparition and demands "what thou art?" The answer, "Thy evil spirit, Brutus" implies this is not in the normal sense an external demon, but is a dramatic representation in the form of dissociation. While Brutus's own consciousness produced the apparition, what follows is that the apparition's consciousness gains an independence from Brutus's mind and has its own will, disturbs the sleep of others, and departs against Brutus' wish, leaving him only with a prophesy of being reunited at death on the battlefield. It is perhaps more hope than prophesy that the thoughts about the issues expressed

here will help skeptics and psi-researchers find a productive basis for collaboration in the future.

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LETTER TO THE EDITOR

Another Cryptogram in "Shakespeare's" Dedication of His Sonnets

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I read with great interest the paper by Peter Sturrock and Kathleen Erickson (Sturrock & Erickson, 2020) on the Dedication of Shakespeare's Sonnets. I am neither a scholar of literature, nor of Shakespeare, and do not want to enter the fray as to who was the author of Shakespeare's sonnets and plays. But I must confess that I found the arguments presented by Sturrock and Erickson intriguing. It is in that vein I would like to communicate an interesting finding.

On page 302, Figure 21, of their paper, they present the Dedication of the Sonnets as a grid of 12 x 12 letters. This was done under the assumption that cryptograms can be deciphered better if they are laid out in a certain format. They then present the message they assume is contained there: "PRO PARE VOTIS EMERITER" as a devotion of Edward de Vere, the Earl of Oxford, to his supposed friend, the Earl of Southampton, Henry Wriothesley.

I find this a possible meaning. My experience with Latin texts—based on a translation of a medieval mystical writer from Latin into German and the reading of many original Latin texts, mainly from the Middle Ages and beyond (Hugo de Balma, 2017; Walach, 1994, 2010)—lets another sequence jump out at me:

SI PATET PRO MIRE VERO RETIRO

The translation would read:

> "If it becomes miraculously obvious [who I am], I retire."

That this is a reference of the proposed author, Edward de Vere, to himself would become clear from the double use of "vero." Vero is a very frequent Latin word. Normally, as an adverb, it means "but." It is derived from the adjective "verus,-a,-um", meaning true, with the noun being "veritas—truth." "Pro" is a preposition that has a multitude of meanings and is necessarily followed by a case that is typical for Latin, called "ablative." "Pro vero" would mean "for true" or "as true." "Mire" is an adverbial construction derived from the adjective "mirus—miraculous, fabulous, splendid." "Patet" is a very frequent construction and means "It is clear, it is obvious." All scholastic disputations used this to make clear what does not need any argument. A typical scholastic argument would read, for instance, "Patet quod deus mundum creavit—It is clear that God has created the world."

"Si" is a conditional and means "if, in case." "Retiro" is clear. It has the same meaning as the English "retire," and retire is derived from it. It means "I retire." One would actually expect the future case "retirabo" to be used, but I think this little grammatical lapse is forgiveable and would be an instance of colloquial Latin.

If we read the "vero" in a double sense, and perhaps a double meaning, both as "for true" and as "for de Vere," we would read:

> "If de Vere becomes obvious as a truth, by some miraculous circumstance, I retire."

One could also parse the center words as "pro mi r. e. vero", which would be bad Latin for "as myself r. Edward de Vere," with the "r." possibly meaning "recte—right" or some honorary title such as "reverendus—the venerable," or something similar.

At any rate, it would be a clear threat to not reveal the identity of the author, else he would [have to] retire and stop writing, or withdraw from the relationship, however that is to be understood.

My guess is: If the Dedication contains a hidden reference to Wriothesley and the author of the sonnets (and plays), Edward de

Vere, and this threat jumps out at the reader at the same time, this strengthens the case for the Dedication being a cryptogram.

Let Peter Sturrock now take out his abacus and calculate the odds of there being not only one or two hidden meanings in the text, but even three, and another quite complex one at that. My guess is: The probability of detecting the Higg's particle or gravitation waves is trumped.

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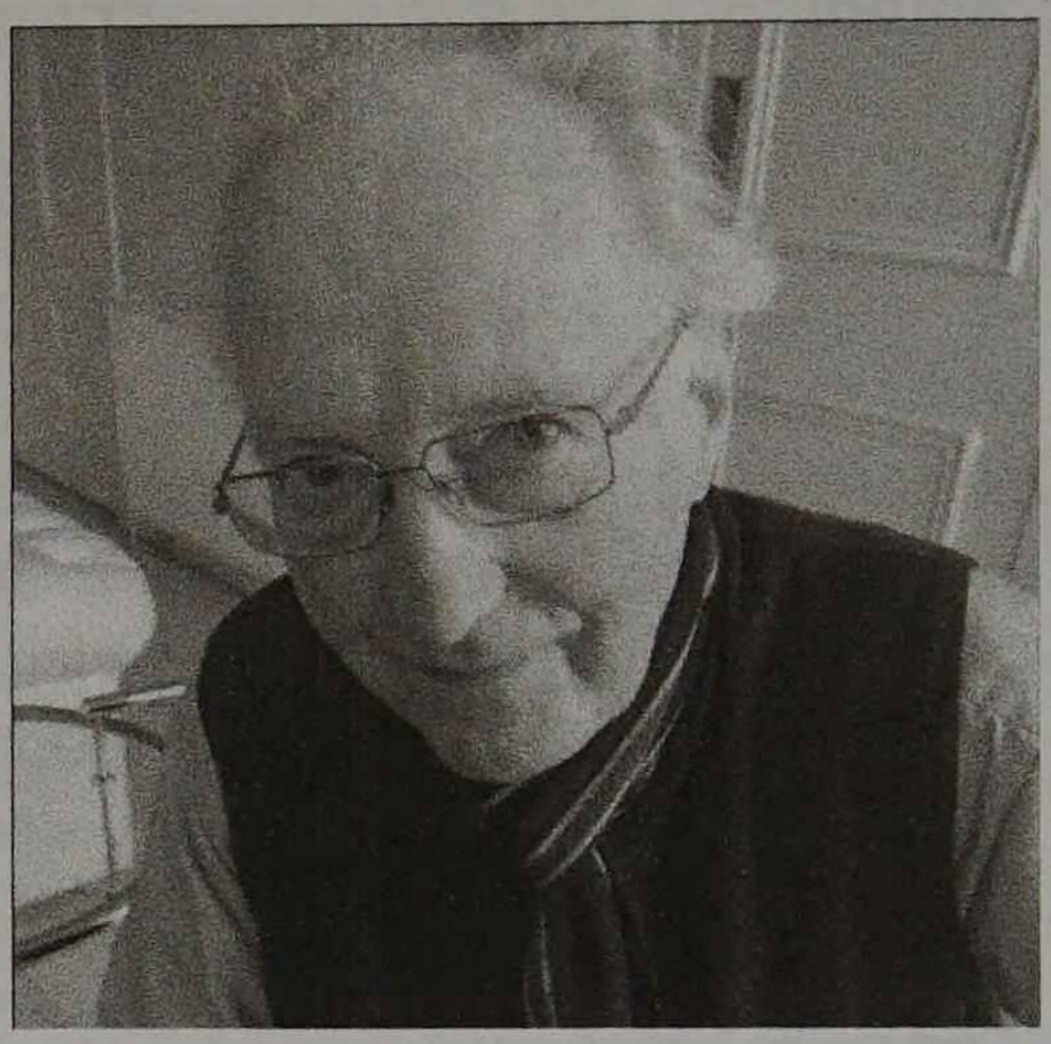
OBITUARY

Erlendur Haraldsson: An Appreciation

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Erlendur Haraldsson, a prolific researcher who made a number of major contributions in various areas of parapsychology and survival research, died in Reykjavik, Iceland, on November 22, 2020, at the age of 89.

Born near Reykjavik, Erlendur studied philosophy in college, but his interest in understanding more about the world began before that. When he was 15, he had an experience during a heavy storm when the sun suddenly shone through the clouds and lit up pebbles on the banks of the nearby shore. As the light reflected off the pebbles, Erlendur sensed being filled with light himself in a way that was immense and beyond words. In an interview with Michael Tymn (2015), he said that a vivid trace of that feeling stayed with him forever and that after that, he never doubted that there was a superior reality.

Following college, he worked for three years, mostly as a journalist, before returning to school to study psychology, eventually earning a Ph.D. under Hans Bender in Freiburg, Germany. After that, he spent a year working at J. B. Rhine's parapsychology center in Durham, North Carolina, followed by an internship in clinical psychology at the University of Virginia, where he met Ian Stevenson. He and Stevenson studied an Icelandic medium together, introducing Erlendur to the topic of mediumship to which he would return in subsequent decades.

Following his internship, he entered the field with a bang. Karlis Osis, the director of research of the American Society for Psychical Research, invited Erlendur to join him in a large study of deathbed visions. They surveyed hundreds of doctors and nurses in both the United States and India about events they had witnessed in their patients. What resulted was a landmark study, one that exemplified the best the field has to offer—detailed statistical analysis along with compelling individual reports. One striking example involved a two-and-a-half-year-old boy whose mother had died six months before. The respondent wrote, "He was lying there very quiet. He just sat himself up, and he put his arms out and said, 'Mama,' and fell back [dead]" (Osis & Haraldsson, 1977, p. 53). Osis and Haraldsson found that the data did not support known medical or psychological causes of hallucinations. Likewise, the influences of religious or other cultural factors could not be used to explain away the phenomena.

When Tymn asked for the highlights of his career, Erlendur started with that study and then discussed his survey of psychic experiences and apparitions in Iceland. He worked on the European Values Survey, in which half of the respondents in Nordic countries said they believed in life after death and 43 percent believed in reincarnation (Haraldsson, 2006). In addition, 41 percent of respondents in Iceland reported personal experiences of contact with a deceased individual. Erlendur then led interviews of 450 people in Iceland who reported such experiences, and he published his findings in the book *The Departed Among the Living* (Haraldsson, 2012).

Erlendur's work with Icelandic mediums included his remarkable investigation of a sitting that Indiri Indridason held in Reykjavik in 1905 (Haraldsson & Gerding, 2010). During the sitting, Indridason assumed a personality who said he was a Dane named Mr. Jensen. Jensen said he had just come from Copenhagen, which was 1300 miles away. (Indridason, who died when he was twenty-eight, had never been there.) He said he had seen a fire there and that a factory was burning. He gave various details that witnesses were only able to confirm when the next ship from Copenhagen brought newspapers that described the events surrounding the fire.

When Jensen appeared in subsequent sittings, he said his first name was Emil and that he was a manufacturer. No one tried to

confirm his existence—until a hundred years later when Erlendur did. He searched the state and city archives in Copenhagen and found only one Emil Jensen who was registered as a manufacturer. In the 1885 census, he was living at Store Kongensgade 68, in the immediate vicinity of Store Kongensgade 63 where the fire broke out. He was last registered in 1898, the year he died, at an address that was only some three hundred yards away.

Erlendur brought the same meticulousness to his study of children's purported past-life memories, which is where I intersected with him. We both followed in Ian Stevenson's footsteps in the systematic study of the phenomenon. Along with researching some cases with Ian, Erlendur independently investigated many more. Focusing on ones in Sri Lanka and Lebanon, he demonstrated the same dogged attention to detail that Ian had, producing some very strong case reports. One involved a little boy in Sri Lanka named Duminda, who made the unlikely claim to have been a senior Buddhist monk who nonetheless enjoyed a red car, a moneybag, and a radio. Rejecting a journalist's quick identification of a deceased man whose life was a very imprecise match for the boy's statements, Erlendur worked until he honed in on one particular monk from the 1920s. He had been at the temple that Duminda named, and he had in fact owned the items Duminda remembered (except that it was a gramophone he owned, not a radio) (Haraldsson & Samararatne, 1999). In another, a Sri Lankan girl gave details about an incense maker who had been killed nearly 150 miles away, including the names of the two incense brands he sold, ones that were not available in the area where she and her family lived (Haraldsson, 2000).

Erlendur also moved beyond the study of individual cases to explore aspects of the phenomenon that no one had previously investigated. He did extensive psychological testing of children in both Sri Lanka and Lebanon, comparing those who reported memories of past lives with others who did not (Haraldsson, 1997, 2003; Haraldsson et al., 2000). He was also the first to systematically interview the children when grown up, as he met with adults he or Stevenson had originally studied when they were children (Haraldsson, 2008; Haraldsson & Abu-Izzedin, 2012).

Erlendur recently reviewed his work with past-life memories in a book he wrote with James Matlock, *I Saw a Light and Came Here.*

He concluded that the reincarnation theory best fit the data and the various features of the cases. From there, he proposed a circular/spiral model of human life's progression: circular as life is followed by death is followed by life, but also, we might hope, a spiral, as humanity's development gradually moves along a slow, ever-widening path of progress (Haraldsson & Matlock, 2017).

I've only scratched the surface of Erlendur's efforts over his many years. You can learn more from Matlock (2020) and Tymn (2015). Or you can read Erlendur's publications themselves, as listed on his website at https://notendur.hi.is/erlendur/english/. But it may take you a while—there are more than 350 of them.

What a career. And what a contribution he made.

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Journal of Scientific Exploration, Vol. 35, No. 1, pp. 137–140, 2021 0892-3310/21

RETRACTION

Retraction of: Parra, A., & Giménez Amarilla, P. (2017). Anomalous/paranormal experiences reported by nurses in relation to their patients in hospitals. *Journal of Scientific Exploration*, 31(1), 11–28.

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The *Journal of Scientific Exploration* is retracting this article because of multiple instances of plagiarism, with no attribution (no quotation marks used, no text citations). The first source is not cited at all and does not appear in the References. The second source is cited once but not in relation to the plagiarized text, but it does appear in the References list. Here are some examples of the text taken from the two unattributed sources:

Text taken from:

Qaid, R. T. A. (2011, June). *Patients' and nurses' perspectives on patients' experience for coronary care unit stressors using a mixed method approach.* Ph.D. thesis, School of Health Sciences and Social Care, Brunel University.

* * * * *

p. 13 of Parra & Giménez Amarilla, 2017, paragraph 2
Stress is usually defined from a 'demand-perception-response' perspective (see Bartlett, 1998; Lazarus & Folkman, 1984; Lehrer & Woolfolk, 1993; Crandall & Perrewe, 1995).

p. xxvii of Qaid, 2011, paragraph 2
Stress is usually defined from a 'demand-perception-response' perspective (Bartlett, 1998). Lazarus and Folkman (1984) integrated this view into a cognitive theory of stress, that has become the most widely applied theory in the study of occupational stress and stress management (Lehrer & Woolfolk 1993; Rick & Perrewe 1995).

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p. 13 of Parra & Giménez Amarilla, 2017, paragraph 2
The transition to severe distress is likely to be most detrimental for nurses, closely linked to staff absenteeism, poor staff retention, and ill-health (Healy & McKay, 2000; McGowan, 2001; Shader, Broom, West, & Nash, 2001).

p. *lxx* of Qaid, 2011, paragraph 2
It is the transition to severe distress that is likely to be most detrimental for nurses, and is closely linked to staff absenteeism, poor staff retention, and ill-health (Healy & McKay, 1999; McGowan, 2001; Shader et al., 2001).

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p. 13 of Parra & Giménez Amarilla, 2017, paragraph 3
In fact, nursing provides a wide range of potential workplace stressors, as it is a profession requiring a high level of skill, teamwork in a variety of situations, provision of 24-hour delivery of care, and input of what is often referred to as 'emotional labour' (Phillips, 1996).

p. *lxx* of Qaid, 2011, paragraph 2
Nursing provides a wide range of potential workplace stressors, as it is a profession that requires a high level of skill, team working in a variety of situations, provision of 24-hour delivery of patient care, and input of what is often referred to as 'emotional labour' (Phillips and Pearson, 1996).

Text taken from:

Fenwick, P., Lovelace, H., & Brayne, S. (2009, October). Comfort for dying: Five year retrospective and one year prospective studies of end of life experiences [preprint]. September–October 2010 article in *Archives of Gerontology and Geriatrics*, 51(2), 173–179.

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p. 12 of Parra & Giménez Amarilla, 2017, paragraph 2
More recent anecdotal accounts from nurses and doctors suggest

that APEs consist of a much wider range of phenomena than purely deathbed visions (Barret, 1926; Osis & Haraldsson, 1997; Kubler Ross, 1971). They may include coincidences around the time of death involving the dying person appearing to a relative or close friend who is not present at the time of death, or a need to settle unfinished business such as reconciling with estranged family members or putting affairs in order before death (Baumrucker, 1996).

p. 1 of Fenwick, Lovelace, & Brayne, 2009, paragraph 1
More recent anecdotal accounts from nurses and doctors suggest that ELEs consist of a much wider range of phenomena than purely deathbed-visions (Barratt, 1926; Osis & Haraldsson, 1997). These phenomena include the ability to transition to and from other realities, usually involving love and light (Kubler Ross, 1971), coincidences around the time of death involving the dying person appearing to a relative or close friend who is not present at the time of death and a need to settle unfinished business such as reconciling with estranged family members, or putting affairs in order before death (Baumrucker, 1996).

*****

p. 12 of Parra & Giménez Amarilla, 2017, paragraph 3
O'Connor (2003) conducted research with care nurses suggesting that they find APEs neither rare nor surprising, which our own research has found corroborated even among palliative care professionals (Katz & Payne, 2003; Kellehear, 2003). Many people now die in hospitals, where, unfortunately, nurses have neither the time nor the training to deal adequately with this very important aspect of the dying and grieving process.

p. 1 of Fenwick, Lovelace, & Brayne, 2009, paragraph 3
Research conducted by O'Connor (2003) with end-of-life care nurses suggests that they find ELEs neither rare nor surprising. And yet our own research has found that even amongst palliative care professionals, ELE training is lacking and many palliative care nurses feel inadequate when dealing with such spiritual issues (Katz & Payne; 2003; Kellehear, 2003). Many people now die in hospital but unfortunately, nurses have neither the time nor the training to deal adequately with this very important aspect of the dying and grieving process.

*****

p. 12 of Parra & Giménez Amarilla, 2017, paragraph 3
Imhof (1996) points out that, since death is not taught as a medical subject, and 'dying right' is not part of medical studies, this special awareness of the dying process is often ignored by those who care for the dying.

p. 1 of Fenwick, Lovelace, & Brayne, 2009, paragraph 3
Imhof (1996) points out that since death is not taught as a medical subject, and 'dying right' is not part of medical studies, this special awareness of the dying process is often ignored by those who care for the dying.

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p. 12 of Parra & Giménez Amarilla, 2017, paragraph 3
Thus coincidences that occur around the time of death, involving the appearance of the dying person to a close relative or friend who is not physically present (Kubler Ross, 1971; Fenwick & Fenwick, 2008), may be missed. Phenomena occurring around the time of death such as clocks stopping, strange animal behavior, or lights and equipment turning on and off (O'Connor, 2003; Betty, 2006; for review see Fenwick, Lovelace, & Brayne, 2010), similarly may be overlooked.

p. 2 of Fenwick, Lovelace, & Brayne, 2009, paragraphs 7 & 8
Coincidences which occur around the time of death, involving the appearance of the dying person to a close relative or friend who is not physically present (Kubler Ross, 1971; Fenwick & Fenwick, 2008). Phenomena which occur around the time of death such as clocks stopping, strange animal behavior, or lights and equipment turning on and off (O'Connor, 2003; Betty, 2006).

For another current retraction of Parra's work, see the "Retraction Notice," *EdgeScience, 45*(March 2021), p. 5. Moreover, Parra's book *The Last Farewell Embrace* has been withdrawn by Nova Science Publishers.

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BOOK REVIEW

The Myth of Disenchantment: Magic, Modernity, and the Birth of the Human Sciences by Jason Ā. Josephson-Storm. University of Chicago Press, 2017, 411 pp. ISBN 978-0226403366.

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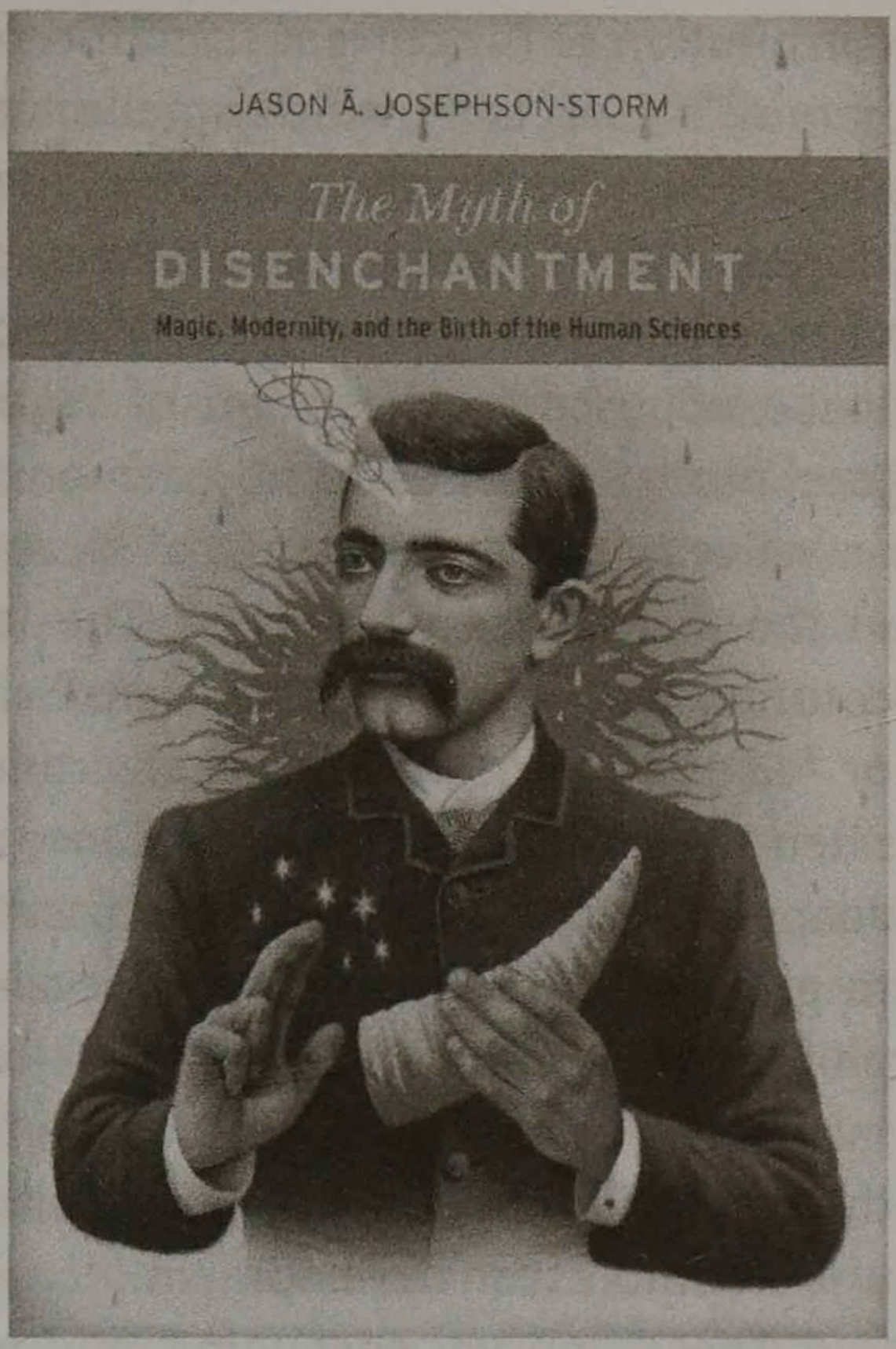
We live in a contradictory world. Self-proclaimed "skeptics," as the original meaning itself suggests, should first of all strive for scientific rationality, for reflective and objective distancing in the apprehension of reality, for methodological caution, and for an extensive ability to theoretically and philosophically understand intricate problems. In practice, too often there is entrenchment in dogmatic groups. Inquisitors endowed with an appearance of religious fanaticism, in the worst sense of the term, invest their energies in crusades of attacks against everyone to whom they attribute mistakes, naïvete, or even bad intentions—the universe of those who do not fit in their often restricted, idealized, and naïve views of scientific practice. In those cases, there is hardly a possibility of frank dialogue, or openness to research fields outside preconceptions of what science and philosophy can approach and how they should operate. Researchers who dare to go beyond the limits some people establish for science and rationality can be disqualified as charlatans, backward, true believers, or superstitious.

To substantiate their certainties, such self-proclaimed skeptics often claim to base their approach to science on examples given by highly regarded scientists and philosophers of the past. We speak here of scholars of the stature of Giordano Bruno, Francis Bacon, René

Descartes, Isaac Newton, the Encyclopedists, Immanuel Kant, Arthur Schopenhauer, Sigmund Freud, James Frazer, the Vienna Circle, Max Weber, etc. Despite their different approaches, we are talking about many of the very founders of modern Western knowledge. The self-proclaimed contemporary "skeptics" claim their inscriptions in the tradition inaugurated by these illustrious intellectual ancestors. They claim to defend with determination such a rationalist tradition against "pseudoscientists" and "mystic-religious" philosophers who, in their opinion, wish to corrupt it through insidious insertions into fields not rightfully belonging to them.

But what if we realized that the "founding fathers" of Western science and rationalism have never corresponded to what skeptics would have liked them to have been? Even worse, what if the methodological, epistemological, and theoretical developments of their discoveries were deeply embedded in the methodology inherited from magic, in activities such as alchemy, in the experiences of spiritualist séances, in mystical knowledge, and in all sorts of paranormal experiences which each of these would-be "disenchanters of the world" were interested in? This is precisely the task assumed by the brilliant, extensive, well-documented, and almost too-ambitious book *The Myth of Disenchantment: Magic, Modernity, and the Birth of the Human Sciences* by Jason Josephson-Storm: To demystify what he calls the "myth of disenchantment," that is, a truth regime that presupposes a self-representation (at least in Europe and North America) of fully "disenchanted" cultures.

Inscribed in a series of robust studies that emerged in the last decade (Harrison, 2015; Numbers, 2009; Sommer, 2014) are questions about the commonplaces established about the history of science—such as, for example, the supposed "eternal struggles" between faith and reason, religion and science, magic and rationality, myth and reality, etc. Josephson-Storm's doctoral dissertation, transformed into a book, brings us a vision that is at least disconcerting. The role played by the main heralds noted above with respect to the overlapping between "magic" and the process of Western rationalization is not even close to what we usually learn in college. The compelling demonstration, with abundant documentation (mainly from primary sources) of this fact, is perhaps its greatest merit. His demolition of the Myth of Disenchantment is in line with the provocative and highly cited paper "Secularization, R.I.P.,"



published two decades ago by the sociologist of religion Rodney Stark (1999).

Josephson-Storm brings to light many largely unknown facts about the intellectual biographies of many celebrated leaders of Western Enlightenment and scientific development. These biographical facts were often found in their own writings, but nevertheless were subject to misrepresentation or systematic cleaning by renowned interpreters. To give clarity to this mechanism, the concept of "occult disavowal" (p. 18) is coined by the author. This is a process that has given a predetermined direction to the ideas espoused by disenchanting interpreters: They projected their own narratives back into the works and lives of the great names of Western thought in a proselytism contrary to magic, paranormal phenomena, and the spiritual element. These interpreters also stressed that the contributions of these leading philosophers and scientists would be part of an explicitly secular and materialist framework and that these leading scientists would have actively contributed to a catechesis against what they believed to belong to the realm of superstition or the supernatural. However, recently found letters, updated information, and other materials have consistently reported the close contact of these respected intellectuals with the "forbidden" spheres of the sacred, spirituality, and the paranormal, revealing a reality and quite different history from that painted by the interpreters.

In addition to bringing these discoveries to light, Josephson-Storm recovers the role played by apparently secondary characters in canonical intellectual history, stressing their importance for the

constitution of the current scientific–philosophical universe. We speak here of "curses" in the official intellectual world, people of the Paracelsus strain, Madame Blavatsky, Aleister Crowley, Baron Karl von Prel, Ludwig Klages, Stefan George, and others commonly linked to the fields of mysticism, magic, religion, the occult, and thus usually thought to be opposed to the realm of legitimate science and knowledge. Josephson-Storm abundantly demonstrates how these figures played an active role in the exchange of ideas with the intellectuals celebrated in the academic environment. The forgotten or deliberately hidden contributions of these "magicians" shaped the supposedly "secular" or "disenchanted" intellectual environment that we live in today. They often were the formulators of concepts, findings, and theories that, adapted or concealed, served as a basis for the "legitimate" intellectuals to give rise to the creation and development of modern science and philosophy. Among these concepts, Josephson-Storm launches a bold hypothesis: that what we know as the "disenchantment of the world" is the paradoxical fruit of these same alleged "enchanters," although this was an unforeseen development.

These unusual encounters and intertwinings of knowledge and resulting experiences between two apparently disparate universes become the background of the pertinent—and ambitious—theoretical questions raised by Josephson-Storm. He builds his research based on three very general questions: 1) Was there really a pattern of development in history that could be called the disenchantment of the world? 2) Was there really a rupture between a time when magic predominated, on the one hand, and another time that saw the product of the world's disenchantment? 3) Does modernity define a singular period? (p. 17). The answers to these questions, which are not easy to solve, are sought through an evaluation of more than five hundred years of the history of culture and of ideas.

The inculcation of what he calls a "disciplinary norm," in other words the self-image that the affluent West was building of itself as a rational, disenchanted, modern territory is a long-term historical trend resulting from the participation of several agents. The straitjacket of a very limited and specific version of "rationalism," which wears well to many self-proclaimed "skeptics," has an embarrassing history to be told. And it is to its genealogy that Josephson-Storm embarks on his

long undertaking, divided into ten chapters grouped in two parts. In the first part he analyzes many founding fathers of the Enlightenment, followed by the German metaphysicians and the British evolutionary anthropologists of the 19th century. Magicians, alchemists, spiritualists, and esoterics of the same time period are presented and discussed. In the second part he discusses the articulations established by Freud and psychoanalysis, the Critical Theory of the Frankfurt School, the Vienna Circle, and the most famous user of the concept of disenchantment of the world, the sociologist Max Weber, with the "magic" and the "occult" through the hidden characters who shaped their thoughts in the background of history.

Josephson-Storm raises current data that cast doubt on the modern belief that we live in an era in which magic and the sacred have disintegrated amid the wonders of the advent of modernity and the increase in the education of peoples. Contrary to what the defenders of secularism preach, not only "backward" countries live with voodoo, possessions, black magic, spiritual healing, mystical experiences, etc. The most advanced capitalist countries in the world, including the United States, England, and Germany, maintain a high rate (usually the majority of their populations) of belief in spirits, extra-sensorial perception, and in the survival of the soul, with most of their population reporting having already had some form of paranormal experience in their lives. This evidence makes clear that the raising of educational levels does not mean the automatic fall in the belief in the existence of transcendence, as defenders of a vulgar version of the Enlightenment erroneously believe. The occult is present in television series of worldwide success; and literature on magic, angels, and near-death have increased exponentially in recent times (Kripal, 2010). A profusion of different types of "charms" flourishes in every corner.

These indications do not mean that there is no rise in atheism or a marked decline in attendance at churches and in traditional religions, at least in Europe and North America. These two factors combined, apparently proving the thesis of the growing secularization of the world, actually do not mean a conversion to a purely materialistic perspective of life and of the universe. Even in those regions, belief in the paranormal or in a transcendent aspect of reality is held by most people. If we take the entire world population, 84% report having a

religious affiliation (Center, 2012). Based on recent worldwide Gallup polls in 163 nations, Stark (2015) has argued that today "the world is more religious than it has ever been." Josephson-Storm proposes that secularization even seems to increase enchantment, or at least the belief in an enchanted, supernatural, world (p. 32), a view also somewhat endorsed by Stark (2015) and Kripal (2010). This would be because such beliefs are empirically based on experiences people actually have (p. 34). That is, although many no longer have a set of beliefs and practices guided by a conventional religion, to paraphrase Max Weber, they still have transcendental experiences and other types of relationships with the sacred that are independent of institutionalized religion.

The grand narratives of modernity that consider any belief in the transcendent as debris from past times and superstition, have been overthrown throughout the 20th century. They were replaced by theories that questioned the advent of a progressive reason capable of indefinitely disenchanting the world. Intellectuals such as Theodor Adorno, Max Horkheimer, Ernst Gellner, George Ritzer, and others kicked off a radical critique that did not spare the Enlightenment, modernity, and capitalism. Such institutions were said to be steeped in the enchanted and irrational artifice at their cores, even as they expressed theories of Cultural Industry, commodity fetishism, and cathedrals of consumption. Late capitalism was nothing more than a return to the realm of enchantment. On the other hand, the subsequent advent of postmodernity and the eruption of related movements, such as the New Age, gave rise to interpretations that framed them as correlated ways of rejecting the Enlightenment and its values. The death of God announced by Nietzsche may have been a valid way to further the escape from the coldness of the world through magical devices. All this converges to the thesis that both modernity and post-modernity formed enchanted periods. The interest in all the themes linked to the paranormal, the supernatural, or the reality of spirits and the survival of the soul after death has never ceased over the past centuries.

Starting his historical analysis with the so-called patriarchs of the Enlightenment—Giordano Bruno, René Descartes, Isaac Newton, Francis Bacon, and the Encyclopedists—Josephson-Storm demonstrates that, behind the development of the thinking for all of them, the same hidden principle reigned: that of magic. And magic not

understood in a restricted definition, but as dynamic and mutant, as defined by those who practiced it in their respective time periods:

> What follows will take precisely *not* as given the meaning of magic, religion, or science. This is necessary because the key terms of our analysis had different meanings in different historical moments, and their reoccurrence obscures breaks, discontinuities, and important shifts. Moreover, concepts are partially defined differentially, and current terminology often bears the legacy of lost oppositions. Accordingly, we must pay careful attention to the construction of putative antagonisms (e.g., between myth and enlightenment). (pp. 10–11)

The author shows that the philosophical and scientific elite before the 19th century was basically formed by mystics, religious devouts, and alchemists. The representation that the group of "heroes of the era of Reason" was composed of zealots of mechanistic and secular thought would be a reinterpretation initiated by influential science popularizers of the 19th century, an image that has been constantly nurtured to the present day. A similar analysis has also been recently proposed by the historian of science Andreas Sommer (2016).

Throughout the book, Storm presents his argument that a cleansed history concealed intellectual aspects linked to magic, spiritualism, mysticism, and the sacred in general, a denial operating successfully over time. An illustrative example is provided from Bacon, regarded as the "father" of experimental knowledge: "Knowledge is power" (from Bacon's 1597 *Meditationes Sacrae*), which is used by Horkheimer and Adorno to unveil the meaning that knowledge took in the early days of the Enlightenment (Horkheimer & Adorno, 2002). For them, the de-spiritualization of nature, the calculation, the mechanical and rationalizing model of a science serving the established power finds in Bacon one of its main sources. Josephson-Storm, using on Bacon's own writings, reveals to us that the original meaning of this phrase had little to do with the conclusions of Horkheimer and Adorno. For Bacon, it was a matter of equating the power of God with knowledge (p. 47). This is in keeping with the fact that Bacon saw himself much more as "as an alchemist with a prophetic mission" (p. 45) than as a disenchanter of the world ready to erect a mechanistic

model of explanation. Rather, it was a question of finding a method that would lead him to the creation of purified magic, which would be "a pragmatic, or instrumentalist, form of natural philosophy" (p. 46). Natural philosophy, distorted by scholasticism, in Bacon's view had to be restored to its beginnings for the authenticity of true magic to surface, giving rise to its subjection to public scrutiny in a methodical manner. Here are the principles of experimentalist philosophy at its hidden root: that of the foundation of rational and publicly controlled magic.

Interesting and noteworthy are also the genealogy and transformations in the use of the term "superstition," as a means of attacking and legitimizing specific groups. Throughout power struggles in history, the word superstition has assumed different (and often opposing) meanings as a target to be attacked and devalued. As traced by Josephson-Storm, it first appeared in the 13th century as opposite to true religion, as used by Thomas Aquinas in the sense of "[. . .] offering 'divine worship either to whom it ought not, or in a manner it ought not'" (p. 47). In the 16th century, Catholics still used it to refer to a "misdirected worship," especially witchcraft. Protestants, on the other hand, used the word "superstition" to attack Catholic beliefs and practices. In the 18th century, the oppositional structure of the true-religion-versus-superstition binary began to shift into that of science versus superstition. At that moment, according to Joseph-Storm, "Scientists inherited the theologians' list of superstitions, and indeed both groups often attacked the same paradigmatic superstitions, such as astrology, magic, and spirits" (p. 49). It was only in the 19th century that the binomial that opposed science versus religion would prevail, especially on the part of historians such as Jacob Burckhardt, thus relegating religion to the gray and illegitimate region of superstition. It is at this moment that the concept of science with its unitary meaning also emerges, close to what we know today, something linked to the progress of knowledge.

The major thesis of the book is that "modernity is a myth," first because "the term modernity is itself vague" (p. 306); and, second, because if modernity is understood as disenchantment of the world, as embracing a materialistic and mechanistic worldview, it has never happened—neither in the "developed" Western general population nor

among intellectuals. "The struggle between 'the Enlightenment' and 'counter-Enlightenment' is mainly a twentieth-century myth, projected backward" (p. 311).

Joining threads of apparently disconnected aspects of the history of philosophy, Josephson-Storm unravels the tacit articulation between different moments and intellectual movements over time. In the German idealism of Mendelssohn, Fichte, Herder, Jacobi, Schiller, Schlegel, Kant, Hegel, Stirner, and Novalis, he finds the roots of the regret of the loss of myth, as well as the discussion that arises about pantheism and its ethical consequences (nihilism) and epistemology (the rise of mechanistic explanations), the disenchantment of the world, alienation, and, of course, the later death of God. In the elements that shaped what we know as modernity, the dawn of rationalism emerges amid this small circle of German rationalists. What almost no one says is that the works of mystics such as Jakob Böhme and Emanuel Swedenborg were commonly debated among them, serving as paths to be opened even when some were opposed to others in philosophical terms (p. 81). Schiller's vitalist philosophy, for example, which rejected the mechanistic model of clockwork in favor of a dynamic dialectic, which resulted in a superior synthesis, is indebted to debates promoted by the esoterics and spiritualists by which he and so many others were explicitly inspired.

Deepening his argument, Josephson-Storm presents a rich analysis of the development of the theories of 19th-century scientists, such as the evolutionary anthropologists Edward Tylor, James Frazer, and Andrew Lang, and the philologist Max Müller, who contributed to substantiating what was conventionally called the "science of religion" or comparative studies of religion, magic, science, and folklore. He reveals that such scholars have had an intense intellectual exchange with mystics and esoterics, such as Eliphas Levy, Aleister Crowley, and Madame Blavatsky, contributors whose theories and impact are usually erased by conventional historians of Western thought. The very notion of comparative studies of religion originated from the attempt to carry out a pioneering synthesis of the sacred by such spiritualists of the 19th century, who sought to reveal through the comparison between different religions, beliefs, and rites the same hidden essence within all manifestations of the sacred around the world.

We must remember that spiritualism was one of the largest transnational movements of the 19th century. Therefore, its importance and its discussions reverberated far beyond the specific terrain of the sacred, so much so that almost all these spiritualist and occult advances tried to serve as mediation, and often as a practical and theoretical resolution to eventual conflicts between religion, science, and philosophy. Along with the birth of sociology, psychology, psychoanalysis, research, and inquiries that dealt with spirits, ghosts and all kinds of paranormal experiences were often considered viable and pertinent. Such movements exchanged methods, language, themes, and problems with what was conventionally called institutionalized or "legitimate" science.

The second part of the book begins with the following question: When did scholars begin to suppress—or to repress—their interests in the occult? Josephson-Storm claims ". . . they did so much later and more sporadically than is conventionally supposed and that much of the cleanup has been retroactive" (p. 181). To address this question, he explores the example of the "father" of psychoanalysis and his socio–historical environment. Sigmund Freud acknowledges his debt to "that brilliant mystic du Prel" (p. 179) in the development of his theory of "the unconscious," a word used and analyzed by the spiritualist Baron Karl von Prel fifteen years before Freud. In addition to being an admirer of von Prel, Freud attended spiritualist sessions, believed in telepathy, was a member of the British Society for Psychical Research, and encouraged Carl Jung and Sándor Ferenczi to scrutinize the universe of the occult. However, in order to protect psychoanalysis' scientific respectability, and under the strong advice of his biographer and friend Ernst Jones, he concealed those interests. In this way, Freud became an engaged and normative defender of disenchantment. Provocatively, Josephson-Storm "psychoanalyzes" Freud, suggesting that the superego, represented by introjected society values, made him repress his own beliefs in favor of an identification with the authority that had been gestating: that of disenchantment as an episteme within the scientific milieu at the turn of the 19th to the 20th century.

Then Josephson-Storm brings us the case of the philosophers, artists, and mystical poets who orbited around Ludwig Klages, his Cosmic Circle. They maintained close contacts with the intellectuals

of the so-called Frankfurt School, especially with Walter Benjamin, whose works focused directly on the thoughts of Theodor Adorno and Max Horkheimer, according to a refined analysis of the work of the forgotten, but not unimportant, Ludwig Klages. The School's central theses, such as the radical critique of instrumental reason, and its inevitable consequences, such as the impulse for domination and the domestication of nature, find their source in the works of the referenced German mystics, long before they surfaced in the famous writings of Literary Theory and Criticism. Through the concept of logocentrism by Klages, the disenchantment of the world was not only explicitly thematized, but was also a consequence of his theory of commodity fetishism. From Benjamin, to Bataille, Habermas, and Derrida, these theses and contributions were adopted.

But perhaps it is in dealing with the most famous skeptical and materialistic philosophers of the 20th century, whom no one would ever imagine flirting with the occult, that Josephson-Storm's thesis surprises us: the logical positivists of the Vienna Circle. More specifically, Otto Neurath, Rudolf Carnap, and Hans Hahn, the most leftist members of the group. Nurturing the same contempt for metaphysics, theology, and religious thought that characterized the other members of the group, they tried to develop a scientifically "corrected" Marxism, which eliminated metaphysics—an element seen as an illusion in the service of the bourgeoisie by Neurath, for example. They were accused by Martin Heidegger of being directly responsible for the process of de-divinization of the world. This was not enough, however, to fully remove these philosophers from interest in the fields of magic, spiritualism, and parapsychology.

The immersion in areas of spiritualist and paranormal research or even in pagan circles marked the lives of some of them, such as the mathematician Hans Hahn and Rudolf Carnap, who joined in these endeavors with other famous scientists, such as the mathematician Kurt Gödel. Vienna was lavish in its interest in the paranormal—so says Freud! It is argued that the fixed demarcation of rigid boundaries between rational and irrational, science and magic, etc., are exceedingly difficult to defend.

Finally, Josephson-Storm, through scrutiny of the Max Weber case, crowns his argument and clarifies once and for all the question

that permeates the book: the concept of the disenchantment of the world. Once again it is surprising what he reveals in biographical terms: the deep involvement of Max Weber, the most famous user of the world's disenchantment concept, with the enchanted spheres of magic and mysticism. The preponderance of these aspects in the internal organization of Weber's theory is also shown by the author. The virtually unknown experience of Weber's in the community of the heterodox psychoanalyst Otto Gross, on Monte Veritá ("Mount Truth" in Switzerland with its many utopian communities during the 20th century), and his contacts with the mystic poet Stefan George yielded more than the reader might have imagined. On the one hand, his plunge into a world full of enchantments and magic in 1913 provided Weber with the elements for the development of its opposite: the concept of disenchantment of the world, glimpsed in his work shortly after his return from such an environment. On the other hand, Weber's well-known neurasthenia, which prevented him from writing and teaching for many years, endowed him with a new sensitivity, attracting his attention to the work of the charismatic poet Stefan George, with whom he became close—and from then on he developed the sociological concept of charisma, which became central to his work.

Weber's pessimism and his criticism of what would become alienated modernity may find its roots hidden in the mystic Ludwig Klages, much more than in the celebrated influence Nietzsche exercised over him. Weber confessed (in an unknown continuation of a letter he wrote to Ferdinand Tönnies, different from what appears in the biography written by his widow) that he has never been anti-religious or irreligious. On the contrary, the documentation said that he felt like a mystic, to the amazement of many. A new view, then, emerges not only of the concept of disenchantment of Weber's world, but also of all of his theory. Josephson-Storm defends Max Weber trying to suture the modern gap between magic and rationality, choosing mysticism as a kind of prophylaxis to this disenchanted world.

After all, Joseph-Storm demonstrates that Max Weber's concept of disenchantment of the world can live very well with the permanence of magic in this world. Rationalization does not necessarily imply an extinction of the sacred, the mystical, and spiritual experiences. Such practices would be endowed with relative autonomy, such as economic,

religious, legal, etc., and would continue to be perpetuated, especially at the individual level. The main consequence of this observation is that the myth of modernity, which encompasses the myth of the disenchantment of the world as one of its central products, cannot be sustained. The concept of modernity is broad, taking into account all the phenomena it intends to describe, and if that means a rational explanation that covers the domination of nature and the disappearance of magic, it is wrong-headed. And so Josephson-Storm has answered a clear no to the three questions posed at the beginning of this work, about whether there was a clear development of the disenchantment of the world, a set time when magic vanished, and a set time when modernity started.

Of course, a book of this intellectual size, with such ambition, would leave flanks open to several criticisms. From a methodological point of view, the fact that the author relied only on a kind of traditional history of ideas is noteworthy. That is to say, it left aside what a materialist analysis, carried out through a sociology of intellectuals in the manner of Pierre Bourdieu, for example, could render from the diverse unpublished biographical information brought to the fore by various intellectuals and their socio–historical contexts. An example would be the establishment of poles of force in the dispute for truths, which are clear in the book, but not theoretically worked out in this way.

It is also noteworthy that the author has made little use of the analysis of the paranormal events themselves, emphasizing more the narratives that have been raised around the events and their epoch. Perhaps by providing us stronger materiality for the phenomena behind the narratives, his own argument would become clearer. Some assertions, on the other hand, are generalized and not very defensible, such as "The tyranny of reason or instrumental rationality never occurred. We are not stranded in the 'desolate time of the world's night', forced to scan the horizon for glimmers of the messianic dawn. [. . .] We are already free." (p. 314). This statement is more the expressed will of the author, to which we may be bound, but which, unfortunately, is not a verifiable fact in our societies. And finally, a gap: The book misses the contributions of spiritualism and psychical research for the debate on science/rationality and the occult/spiritual in 19th-century France, England, Italy, and the US, which brought together several

well-known and influent intellectuals, such as William Crookes, Ernesto Bozzano, Gabriel Delanne, Camille Flammarion, George Sand, and Victor Hugo, among others. Of the few criticisms raised, however, we are sure that they do not in any way diminish Joseph-Storm's brilliance and vast contributions to several fields, including those of philosophy, sociology, anthropology, psychoanalysis, critical theory, studies on religion, etc. This is, without a doubt, a necessary book for anyone who wants to delve into any of these branches of knowledge.

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BOOK REVIEW

Matlock's Theoretical Offenses

Signs of Reincarnation: Exploring Beliefs, Cases, and Theory by James G. Matlock. Rowman & Littlefield, 2019. 408 pp. $39 (paper-back). ISBN 978-1538124796.

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James Matlock's book, *Signs of Reincarnation*, is a recent addition to a seemingly endless stream of confused or superficial works on the topic of survival. Admittedly (and as one would expect), the case material is often of genuine interest. But when Matlock tries to make sense of that material, he demonstrates little grasp of the current state of the debate. Even worse, he seems unaware of the intellectually responsible strategies for challenging and criticizing positions opposed to his own.

Since Matlock criticizes what he says are my views throughout his book, and because this issue of the *JSE* features two comprehensive reviews of that book, I'll focus only on the principal respects in which Matlock misdescribes my position and ignores the extended discussions I've provided, not only in *Immortal Remains* (Braude, 2003), but elsewhere (e.g., Braude, 2005a, 2005b, 2014a, 2014b, 2020), explaining the problems with the tired and flawed lines of reasoning he endorses.

Some of Matlock's significant lapses in understanding concern my discussions of so-called "super psi" and what I dubbed the Argument from Crippling Complexity (CC). The problems are captured nicely in two of Matlock's glossary entries:

> **crippling complexity** Complications so dense and convoluted that they defy all plausibility. The concept and term were introduced by philosopher Stephen Braude to describe the convoluted nature of

> many *super-psi* explanations of the evidence for discarnate survival, including *reincarnation cases.* (p. 292)

> **super-psi** In parapsychology, a hypothetical extrasensory ability that is either more extensive or more complex than regular *psi.* Because the limits of regular psi are unknown, it is impossible to rule out the possibility of an unusually extensive psi on theoretical grounds, but that is not true of complex super psi, for which there is no evidence. In this book, super psi denotes a hypothetical complex psi ability involving the acquisition of information in more than a single step, often requiring the integration of multiple sources, sometimes accompanied by psychokinetic (PK) actions on human bodies or on the material world in addition. (p. 305)

The errors here are egregious and painfully elementary, and so deeply intertwined it's difficult to know where to begin. Consider first Matlock's characterization of super psi, distinguished first of all by its obliviousness to several warnings I lodged in *Immortal Remains*, concerning the ease with which that term leads to conceptual confusions. Those pitfalls were the reason I've endorsed Michael Sudduth's much more satisfactory replacement in the context of the survival debate—*living-agent psi* (LAP).

I've noted, not only in *Immortal Remains* (Braude, 2003) but in many of my articles and books (e.g., Braude, 1997, 2017, 2020), several crucial and related points that Matlock simply ignores. That's all the more surprising because the relevant issues are actually straightforward and easy-to-understand. The most notable are (1) that we have no credible, general scale or measure of either psi-amazingness or psi-complexity. This effectively undermines the intelligibility of distinguishing "regular" psi from "more extensive or complex than regular psi"—not to mention between dandy psi and super psi; (2) that what impresses us about an ostensibly psychic achievement may not be the extent, magnitude, or seeming complexity of the effect, but its pinpoint refinement (e.g., accuracy or timeliness); (3) that impressive (and misleadingly labeled) super psi needn't be complex at all but may instead work like a magic wand (rather than through one or more series of transitive causal links); and [in the spirit of point #1] (4) Richet's sensible and oft-cited remark:

> it is as difficult to understand the materialization of a living hand, warm, articulated, and mobile, or even of a single finger, as to understand the materialization of an entire personality which comes and goes, speaks, and moves the veil that covers him. (Richet 1923/1975, p. 491)

We'll return to some of these points shortly (I deal with them at great length in Braude 2020). But consider, first, how Matlock totally misses the related point about Crippling Complexity. My discussion of CC was not even superficially about the "convoluted nature of many *super-psi* explanations of the evidence for discarnate survival," or about "complications so dense and convoluted that they defy all plausibility." And that's not simply because I left it open whether psi works like a magic wand, or because we have no clear standard for measuring the complexity or impressiveness of a psi phenomenon. On the contrary (and ironically), it's because the argument for CC points out a potential limiting factor in psi of *any magnitude, and no matter the source.* It's about the complexity, breadth, and density of the normal and paranormal causal activity—what I've called the *causal nexus*—presumably underlying *any* exercise of psi. It's not about the complexity of the psychic event itself.

Incidentally, I dismiss in advance any effort from Matlock to claim that my discussion of CC was obscure. That's not to say it couldn't have been clearer. Probably all writing can be endlessly polished, and, besides, complex views inevitably undergo refinement with time. But my text in *Immortal Remains* was clear enough for others to understand very well what my positions are, and without the need for my personal tutoring.

For example, Sharon Rawlette (2019) recognized that my argument was about the density of the underlying causal nexus and its potential for interfering with one's intentional psychic efforts. And she also knew exactly which quotes from me capture important steps in my argument—among them, "the super-psi hypothesis suggests that ESP faces *too many natural obstacles to be consistently successful*, at least to the degree required by the best cases of mediumship" (Braude, 2003, p. 90, emphasis added); and, "what makes the best cases so impressive is both the amount of correct material and the *consistency* with which subjects provide it" (ibid., p. 91).

Michael Sudduth (2009) also understands this very well. And unlike Matlock he clearly grasps the crucial point that "This [crippling complexity] will be a problem even if psi works like a magic wand, for the problem of crippling complexity is concerned with conditions that undercut psi functioning, whether psi functioning is simple or complex" (Sudduth, 2009, p. 182). That's why crippling complexity might seem to be as much a problem for survival psi as for living-agent psi. For both, Sudduth proposes that psi must be neither too weak nor too strong—i.e. neither impotent nor self-defeating—what he calls "goldilocks psi." Similarly, Sudduth (2014) notes that

> even if the information flow from discarnate persons to mediums is less subject to interference from the causal nexus, the information flow from the world/other minds to discarnate persons is just as fragile as a flow of information from the world/other minds to the medium. (Sudduth, 2014, p. 60)

Adam Rock and Lance Storm (Rock & Storm, 2015) also understand these issues. They write, "Braude argued that the crippling complexity of the *psychic traffic* produced by the totality of embodied minds might serve as an obstacle to LAP in the context of the medium–sitter interaction" (Rock & Storm, 2015, p. 570, emphasis in original). And like Sudduth, they note correctly that the "Argument from Crippling Complexity applies equally to the LAP and survival hypotheses" (ibid.). Moreover, they identified a passage in *Immortal Remains* where I state this clearly:

> . . . it should be as difficult for communicator and medium to create (say) a consistent, long-term impersonation as it would be for the medium to accomplish the same thing through clairvoyance and telepathy with the living. Both tasks would encounter inevitable obstacles from the bustling underlying nexus of psychic activity, and that underlying causal network would have to include attempts by the deceased to gather information and influence the living (Braude, 2003, p. 93). (Rock & Storm, 2015)

Likewise, Lee Irwin (2017) does a much better job than Matlock in grasping the point of CC. He notes,

> Braude identifies the problem of "crippling complexity" and conjectures that the omniscient use of ESP . . . is questionable if interference or blocking can occur due to unconscious resistance, or even hostile ESP from others, and argues that lines of psychic connection and communication are so entangled with the intention of other minds that it "might be more remarkable for it [accurate psychic perceptions] to succeed than to fail." *The more extensive the network of connections, the greater is the possibility of interference, blocking, or misperception.* The more sources required for information gathered, not just from living minds but from books, objects, places, or skill-based knowledge, the more likely the possibility of interference. *The more "super" the psi, the more difficult it may be to accurately attain the required information.* (Irwin, 2017, p. 376, emphasis added)

In any case, for those who found my discursive argument for crippling complexity in *Immortal Remains* too difficult to untangle, here is a stepwise, stripped-down version of the argument that I hope will clarify the essentials once and for all. (Readers can find detailed arguments for premises 1, 3, 5, and 6 in *Immortal Remains*.)

(1) Most (if not all) of our abilities or capacities are situation-sensitive—including ordinarily subconscious and involuntary capacities and even virtuosic abilities.

(2) *Therefore,* it's reasonable to think that the manifestation of psychic capacities would also be situation-sensitive.

(3) The parapsychological evidence supports that conjecture.

(4) *Therefore,* it's reasonable to think that no matter how extensive, refined, or virtuosic psychic capacities might be, like other capacities they will also be subject to actual case-by-case limitations.

(5) The hypothesis that humans have psychic capacities presupposes a vast underlying network of both normal causality and (typically covert) psi-processes initiated both consciously and unconsciously.

(6) The more dense and extensive that network is, the more obstacles any particular psychic inquiry or effort must navigate in order to succeed (e.g., the more likely it is that each effort will be caught in the crossfire of underlying causal activity).

(7) ***Therefore,*** **the greater the range, pervasiveness, and refinement of psychic functioning (i.e. the more "super" we take it to be), the more vulnerable one's psychic efforts will be to paranormal interference from within the causal nexus, and the less likely it becomes that any given psychic effort will succeed, much less that a series of such efforts will succeed.**

(8) ***Therefore,*** **the more potentially wide-ranging and virtuosic we take psi to be, the less likely it becomes that a person's psi could produce an extended and accurate trance persona, or provide all the detailed, intimate information found in the most astonishing survival cases—and even more so, to do these things consistently.**

One of the most disappointing features of Matlock's book is his frequent failure to engage in competent rational argumentation. As Sudduth also observes in his review in this journal issue, for Matlock's rejection of others' positions to have any teeth, it's not enough simply to deny the claim objected to, or to cite someone else who denies that claim. However, my views—or rather, what Matlock often erroneously claims are my views, consistently receive such shallow treatment. Clearly, though, the only meaningful way to proceed is to state the *reasons* and *arguments* given in support of the rejected claim, and to explain where exactly therein the errors lie.

For example, Matlock writes,

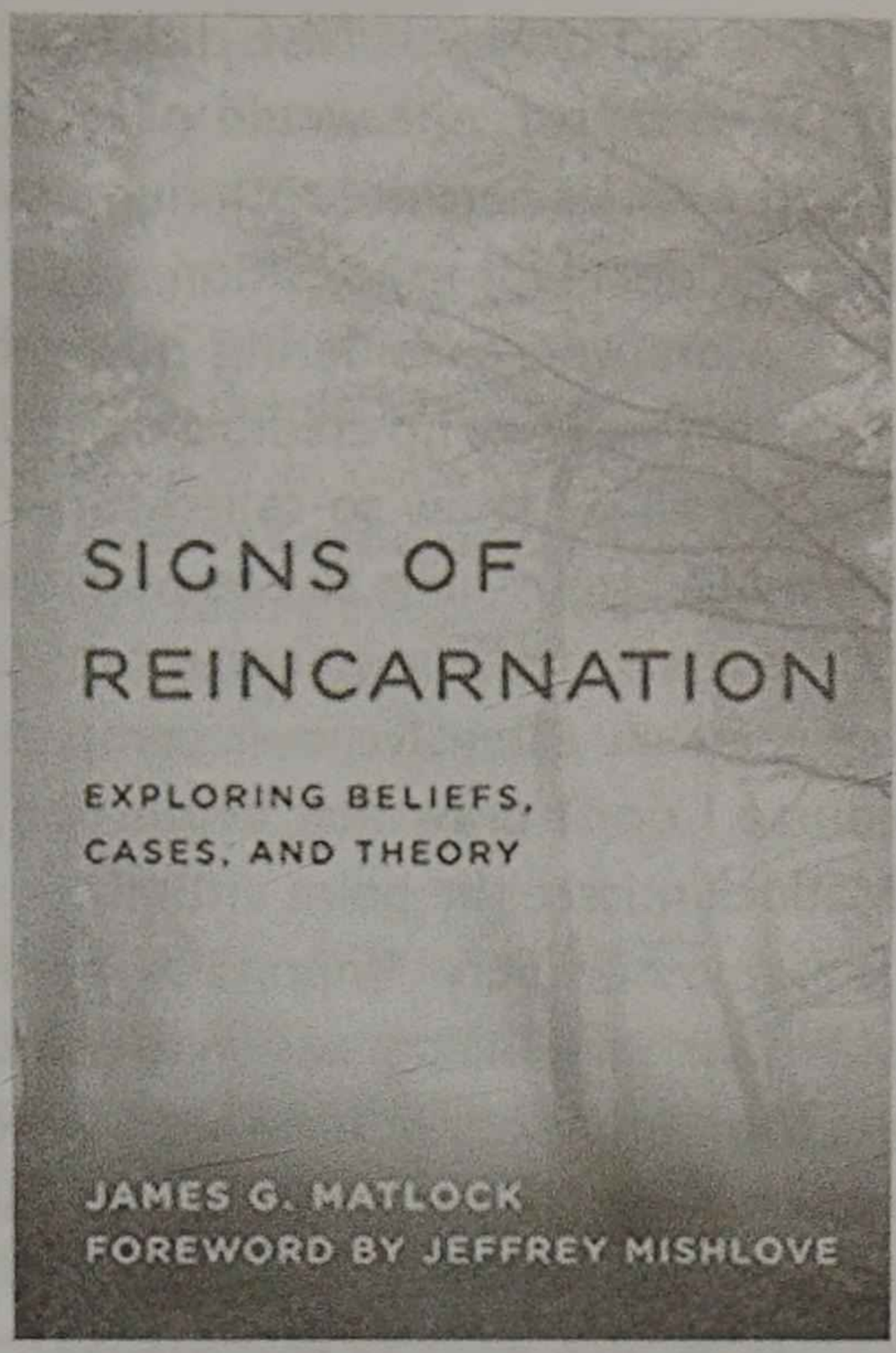
> Skeptical parapsychologists and parapsychologically sensitive philosophers downplay or ignore reported behavioral corres-pondences between a case subject and an identified previous person or else claim (with Braude, 2003) that the behaviors could have been shaped by psi impressions, when *there is no independent evidence that complex behaviors can be acquired via psi.* (p. 51, italics added)

Similarly, he claims,

> Braude argued that not only "knowledge that" but "knowledge how" (skills, including language skills) might be acquired by super-psi, although he could not explain how this would be possible, or muster any data in its support. (p. 213) [1]

Each of these single sen-tences contains an impressive number

of major mistakes. According to Matlock, I believe that complex behaviors (skills or abilities) can be acquired via psi or super psi. That's also a charge Ian Stevenson liked to make. But I've never said that. On the contrary, I've often challenged that entire way of framing the relevant issues. I've presented, in many places and over several decades, reasons for challenging Stev-enson's claim, "if skills are incommunicable normally . . ., they are also incommunicable paranormally" (see, e.g., Braude, 2003, pp. 114ff). And the reason I've taken that position, as I've argued in gory detail, is that *communicating* or *acquiring* skills is not the issue, and that it's question-begging to express the issue in such terms.



All we know is that some individuals *manifest* anomalous abilities, and that it's a mystery *how* they got them. It's one reason I've focused on the anomalous and poorly understood appearance of astonishing skills in savants and prodigies, and the sometimes equally startling performances of good hypnotic subjects. It's one reason I've examined at length what we don't understand about the nature of skills (see, e.g., Braude, 2014a). And it's why I noted in *Immortal Remains* four crucial topics demanding our attention: (1) the extent to which people can express and develop seemingly latent skills by sidestepping their customary and resistance-laden modes of cognition (e.g., as in hypnosis); (2) the situation-sensitivity of all human endowments—even the most rudimentary involuntary capacities; (3) the apparently non-lawlike relationship between skills and practice; and (4) the difficulty in generalizing about skills or abilities, including the ability to speak a language. In fact, in *Immortal Remains* I even have a long discussion just about the nature and varieties of linguistic proficiency, which doesn't rely at all on appeals to super, or any, psi.

So not only is it false that I claim that complex behaviors can be acquired by psi, one would never guess from Matlock's presentation that the issues here are both numerous and very deep and that I argued in great detail for my position.

Moreover, considering how often I've noted in my publications both that we have no credible objective scale of psi impressiveness and that for all we know so-called super psi may work like a magic wand, it's quite astonishing to see Matlock claim that I defend "a super-psi interpretation of survival phenomena, without drawing the distinction between an extensive and complex hypothetical ability" (p. 117). Of course I don't draw that distinction. I claim it's not even intelligible. Matlock misses the point entirely.

Another very important point I've made repeatedly is that survivalists are in no position to object that LAP explanations of the data are too complex or otherwise astounding to be taken seriously. And the reason I've said that is that survivalists are committed to a comparable level of survival-psi amazingness. This point, like others mentioned earlier, is also easy to comprehend. Yet Matlock fails to do so. Consider, for example, how he botches his discussion of the fascinating Maróczy chess case, in which a deceased Hungarian grandmaster, Géza Maróczy, ostensibly played a very high-level game with the thoroughly alive Russian grandmaster Victor Korchnoi, apparently transmitting his moves through a medium (Eisenbeiss & Hassler, 2006). Matlock describes my position as follows.

> [Braude] thought that the medium would have been able to achieve these things by the exercise of his psi alone, although as always with super-psi propositions, it is difficult to understand how the psi of a living agent (the medium, in this case) could account for all the elements on display. (p. 242)

Now first of all, I've taken no stand on whether the LAP explanation must be solely in terms of the medium's psi. It doesn't take much digging into my writings to see that I routinely acknowledge the potential paranormal contribution (and perhaps subconscious collaboration) of both central and peripheral persons in a mediumistic scenario. (See, for example, my [actually, Jule Eisenbud's] discussion

of Mrs. Chenoweth's Cagliostro persona [Braude, 2003, pp. 39ff]). Concerning the possibility in the Maróczy case of telepathic leakage from a sitter (or other interested parties), I've written,

> the subject was playing chess with an opponent who had grand-master skills; the idiosyncratic moves of the deceased were verifiable and therefore available through ESP to both players; and both the grand-master opponent and others were aware of the deceased's presumed ignorance of chess strategies developed after the deceased's death (in particular, a strategy used to counter an opening variation attempted by the medium). (Braude, 2014a, p. 172)

But the main error here, quite apart from Matlock's aforementioned anachronistic reliance on the defective expression "super psi" and his confusions about psi complexity and amazingness, is his blindness to what we can call *the parity of psi explanations* in survival cases. Matlock simply regurgitates the old and defective argument that a living-agent–psi interpretation presupposes an implausible degree or refinement of psychic functioning and (in particular) *more than would be required by the survivalist*. I've explained very clearly the flaw in this line of argument.

> [In the chess case] the survival hypothesis *requires* virtually the same degree of psychic functioning as is posited by the living-agent alternative, and this is not a difficult point to grasp. According to the survivalist, the persisting intelligence of the deceased communicator is causally responsible for the forty-seven chess moves in question. But for that to occur, the deceased would need extended, accurate ESP (either telepathy with the medium or an onlooker or else clairvoyance of the chessboard) to know what the state of play is and then ongoing and effective ESP (presumably telepathic influence on the medium) to convey the desired next move. (Braude, 2014a, p. 172)

For an impressive case like this chess case, or the consistently striking "hits" of Mrs. Piper over her long career, appeals to non-psi explanations in terms of what I've called the Usual and Unusual Suspects seem out of the question. Instead, there seems to be notable

psi happening no matter which side of the ontological divide you consider. So rather than whine about the apparently extreme psi posited by advocates of LAP interpretations of the data, survivalists would do well to heed Ben Franklin's imperative, "Clean your finger before you point at my spots!" Sadly, none of this is breaking news in the survival debate, and Matlock's failure to attend to these well-worn points is simply inexcusable.

But enough from me about the ways in which Matlock fails to engage in competent critical discourse and misrepresents both the state of the survival debate generally and my views in particular. For additional pertinent commentary, I encourage you to read this issue's other two reviews of Matlock's book.

NOTE

[1] This passage occurs in the midst of a shallow and question-begging discussion of the Uttara/Sharada reincarnation case, which also falsely attributes to me the claim that "[previous personality] Sharada's detailed knowledge of Bengali geography and customs was retrieved by Uttara through a 'motivated psi' or super-psi in her altered state of mind" (p. 212). My position on this case is much more nuanced than one would guess from Matlock's account. For one thing (and as Sudduth correctly observes in his review in this journal issue), I never argue for the superiority of my analysis over a survivalist interpretation. I claim merely that the survivalist accounts have been psychologically superficial and have done little or nothing to rule out my approach. Moreover, I never said Uttara needed psi of any kind to behave like a Bengali of times past. She already spoke some Bengali and was an ardent student and admirer of Bengali culture (and was disdainful of her Marathi culture). In light of what little we understand about savants, prodigies, the varieties of linguistic proficiency, and hypnotic and dissociative liberation of latent capacities, who knows how far Uttara could have run creatively with what she already knew, while also benefiting from the hopeful perceptions of others that Uttara was displaying signs of reincarnation rather than mental illness?

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BOOK REVIEW

Signs of Reincarnation: Exploring Beliefs, Cases, and Theory by James G. Matlock. Rowman & Littlefield, 2019. 408 pp. $39 (paper-back). ISBN 978-1538124796.

Reviewed by Peter Mulacz

Vienna

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At 1606:22, Clipper 759 informed the tower that it was ready for takeoff. At 1606:24, the local controller cleared the flight for takeoff, and at 1606:30, the first officer acknowledged the clearance. The acknowledgment was the last radio transmission received from Clipper 759.

On July 8, 1982, Pan American World Airways Flight 759 (Clipper 759), a Boeing 727-235, N4737, was a regularly scheduled passenger flight from Miami, Florida, to Las Vegas, Nevada, with an en route stop at New Orleans, Louisiana. About 1607:57 central daylight time, Clipper 759, with 7 crewmembers, 1 nonrevenue passenger on the cockpit jumpseat, and 137 passengers on board, began its takeoff from runway 10 at the New Orleans International Airport, Kenner, Louisiana.

At the time of Flight 759's takeoff, there were showers over the east end of the airport and to the east end of the airport along the airplane's intended takeoff path. The winds at the time were gusty, variable, and swirling. Clipper 759 lifted off the runway, climbed to an altitude of between 95 feet to about 150 feet above the ground, and then began to descend. At 1608:57, the Ground Proximity Warning System (GPWS) activated and "Whoop whoop pull up whoop. . . ." was recorded. The airplane struck a line of trees about 2,376 feet beyond the departure end of runway 10 at an altitude of about 50 feet above the ground. The airplane continued on an eastward track for another 2,234 feet, hitting

trees and houses, and then crashed into a residential area about 4,100 feet from the end of the runway.

The airplane was destroyed during the impact, explosion, and subsequent ground fire. One hundred forty-five persons on board the airplane and eight persons on the ground were killed in the crash. Six houses were destroyed; five houses were damaged substantially.[1,2] Moreover, nine people on the ground suffered severe injuries.

The aircraft hit the ground with a considerable left bank angle, firstly hitting an oak tree with the left wing, cutting the power and the telephone lines mounted on poles, then destroying the houses of the Schultz family, the neighboring house, and a few others, and eventually cartwheeled and broke into pieces. Kerosene spilled from the ruptured tanks and ignited although there was a thunderstorm with heavy rain; three members of the Schultz family staying in their house were badly burned, one of them died in hospital. Among those killed on the ground—actually the first victim along the swathe of destruction caused by the crashing/impacting aircraft—was Jennifer Schultz, then eleven years of age, who was in the carport (perhaps sitting on a swing there as she used to do) when disaster struck.

On March 11, 2008, in Bartlesville, Oklahoma, a girl, Rylann, was born to the O'Bannion family. Rylann appeared to be developing earlier than usual, but she showed some curious habits, e.g., for some time she kept sleepwalking. She started complaining that her hair touching her back hurt her back; she drew dramatic fits about putting on shirts. The clothing, she would complain, hurt her back, neck, and shoulders—it felt like her skin was burning.

Referring to a photograph she mentioned time and again, she said she had been "bigger" than on that picture, a statement that didn't make sense to her mother at that point in time. Eventually, at the age of three years and five months, again touching the topic of having been "bigger" before, she said: "Mommy, I died. I was in our backyard. It was raining. I was alone but I wasn't scared. Then the rain shocked me. It was raining a lot. There was a loud noise, then the rain shocked me. I floated up to the sky then."

As the O'Bannion family subscribed to the Catholic faith, reincarnation was not a subject to consider. Over time, Rylann added new bits of memory; at the age of five she started talking about what

happened to her "in heaven" after her death (meeting God and Jesus, and 'Grandy Sally' whom she had never met in reality), and that "you can choose to come back if you died before you were supposed to." Once, out of the blue, she said "I remember the name of Jennifer."

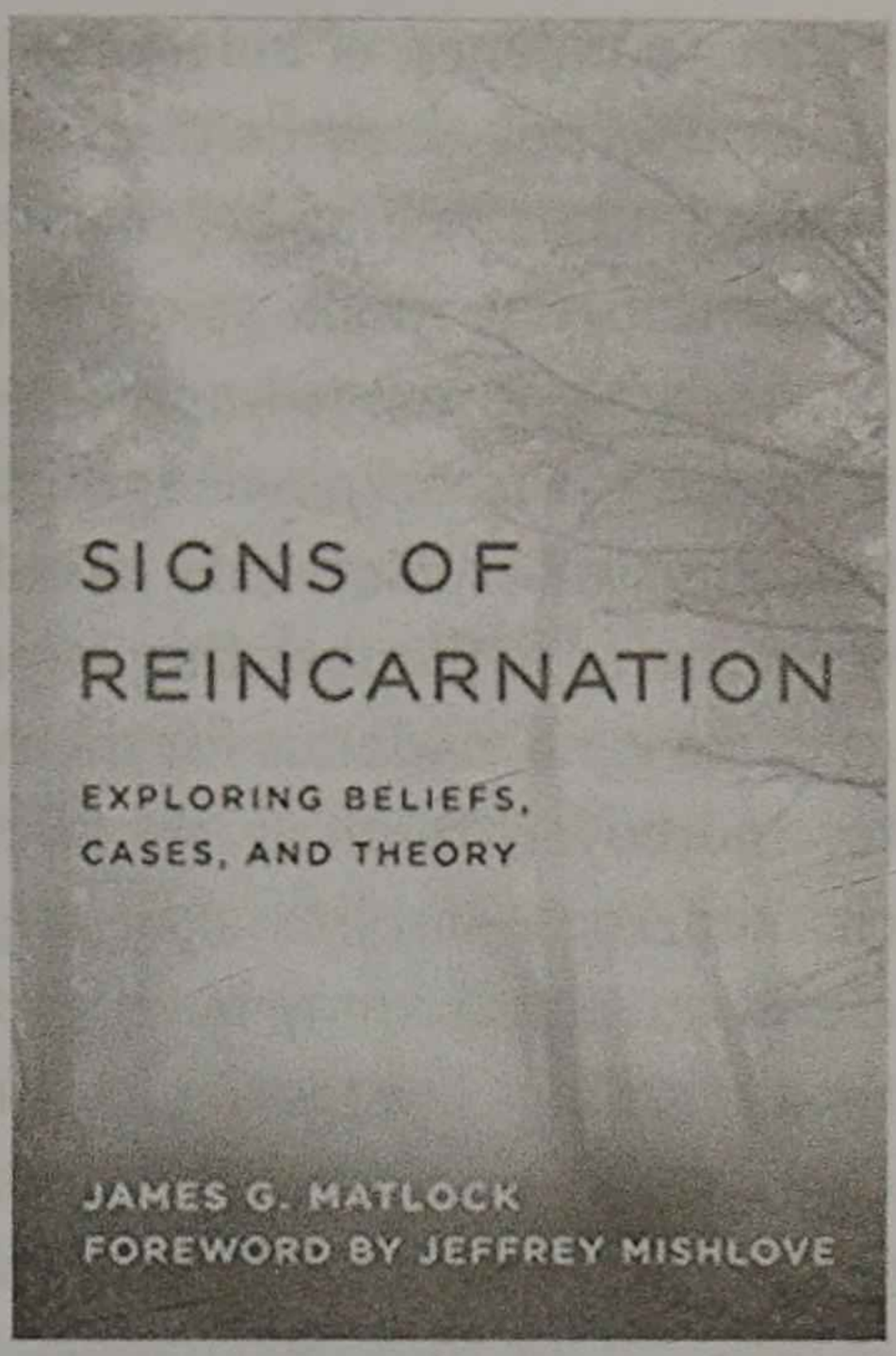
In 2013, Lifetime television aired a series *Ghost Inside My Child*. Rylann's mother resolved to watch this program together with Rylann and her brother, hoping that seeing other children claiming to recall previous lives would help Rylann with respect to pieces of her own memory, perhaps eliciting more. Rylann disliked that TV program as she felt it was creepy and overly dramatic; on the title of the series she commented, "It's not a ghost inside of you. It's you, just different."

The case evolved further when, in March 2014, she recalled a dream, "I was standing there in the yard and saw a plane crash." (While most fragments of memory came back in the waking state, some appeared in dreams, too.) Rylann's mother started a web search for plane crashes; there were numerous pages on Pan Am 759 crashing in Kenner, Louisiana, as this has been the flight accident with the then highest amount of compensation paid to various families affected by a crash.

Rylann's case appeared in a later episode of the *Ghost Inside My Child* series; the TV crew had taken her and her mother to the village of Kenner where Rylann showed some peculiar behavior such as finding the way to the toilet in a house she had never been in before in her life, opposite the former Schultz' house, etc. Unfortunately, the Rylann episode was heavily edited. Through the *Signs of Reincarnation* Facebook Group established by James Matlock in 2014, Rylann's mother came in contact with the author who started proper investigations (including interviewing witnesses in Kenner, procuring Jennifer's autopsy report, etc.).

At the end, there were thirty-two statements by Rylann referring to Jennifer and the plane crash; Matlock rates twenty-five of those "correct, substantially correct, or plausible," while eight are "incorrect or implausible, but of these only four are demonstrably false or highly unlikely." The latter refers to Jennifer's first (later corrected) statement she had died in the yard of their present house (probably a conflation with other impressions), the color of the family car, the number of dresses Jennifer owned, etc.; the false statements were made only once, never

repeated. Whether Jennifer was killed by a strong electric pulse (somehow by the broken telephone cables hanging close to the ground, by lightning during that thunderstorm, or by static discharge of the plane in proximity to the ground) or killed by the fire is discussed; the autopsy report states that the trachea contained no soot (indicating that the *exitus* occurred prior to the fire reaching the body, i.e. electrocution being a pos-sibility); however, the corpse had been badly charred and hence no definite answer on the girl's cause of death can be given.



This case, a recent one and meticulously investigated by Matlock, fills the major portion of the opening chapter of James G. Matlock's book *Signs of Reincarnation*, a book that developed from courses the author taught on reincarnation research and theory. This representative case study is followed by deliberations on "What Is Reincarnation?" and "Challenge to Materialism," the latter drawing heavily on ideological quotations by various thinkers, not all of them well-digested, while the former reflects on the idea of reincarnation in various societies, various religious systems, and during various periods in time; interwoven with that are considerations on the terminology to be used. While for most authors the word *reincarnation* indicates the top domain, Matlock prefers *rebirth* instead; where Stevenson talks of the *former personality*, Matlock prefers the *former person*, etc. Some of these terminological suggestions are not convincing; rather they might be confusing, particularly for newcomers to the field for whom after all this book is intended.

The next chapter, *The Belief in Reincarnation*, is broken down into three sections: *Signs, Beliefs, and Customs in Animistic Cultures*; *A Brief History of the Belief in Rebirth, West and East*; and *Karma, God, and the Individual in Rebirth Theory*. The anthropological and historical aspects, though mostly well-known, are nicely compiled and underpinned

by an abundance of references, yet the sequence in which they are covered is not clear, neither chronological nor regional, nor ordered by the importance of belief in reincarnation in the respective society. Unwarrantedly, much space is devoted to (Adyar-) Theosophy; while Blavatsky's enormous impact on modern occultism cannot be denied, it needs to be acknowledged that what she amalgamated into her teachings is based on older occult literature (which she plagiarized or at least quoted without stating any references) or stems from pure fantasy as there is no evidence for the real existence of the *Masters* she refers to; moreover, there is no evidence she ever entered Tibet as she claimed, thus—for the sake of argumentation—Theosophical teachings are worthless. As may be expected, classical Greek philosophers and their teachings are touched upon, as well as Neoplatonism; and so are Gnostic and early Christian doctrines as well as Judaism; Islamic sects (or peoples in their self-conception) are mentioned very briefly. The Hindu and the Buddhist persuasions are discussed as well as the pivotal notion of *Karma*. Different solutions to the *selection problem* (how souls choose their future parents) are discussed, including the teachings of Allan Kardec. Altogether, the wording *rebirth theory* as used by Matlock seems a bit high-pitched: There are several opinions, irreconcilable with one another, and not backed by any empirical evidence.

Talking about *Research Methods and Interpretative Frames*, Matlock stays with the format of three sections per chapter (and for the remainder of his book). The first section bears the heading *Accounts of Past-Life Memory Recorded before 1960*. Again we are brought into Greek antiquity (Pythagoras, Apollonius of Tyana), then the findings of a Dutch sinologist makes us jump to China in the 3rd century A.C., later we visit cases in Japan, Burma, and India. The first significant European case, that of Alessandrina Samonà of Palermo, Italy, was published by Charles Lancelin in 1922. Edgar Cayce and "Bridey Murphy" are mentioned in passing; concluding this period, Ian Stevenson published his epoch-making paper in 1960: The Evidence for Survival from Claimed Memories of Former Incarnations (in the *Journal of the American Society for Psychical Research*, in two parts). The next section of this chapter deals with *Ian Stevenson's Field Research and Its Critics*. The problem was the one of coming in to the case too late: Usually the *previous person* had been identified and the child had met the *previous family* before

Stevenson learned about the case, thus he could only establish what had already transpired. He interviewed as many firsthand witnesses as possible on "both sides" of the case, allowing them in the first stage to recall without being prompted, then going down a checklist of frequently occurring features; later he re-interviewed the interviewees over a period of time to check for consistency. His methodology reached far beyond that and was steadily improved; he used to cooperate with locals, not only for translation purposes but also for insight in customs and beliefs, etc. In 1961 he started his investigations in India, Ceylon (now Sri Lanka), later travelling to Lebanon, Brazil, and Alaska. These field studies were funded by the multimillionaire Chester Carlson (Xerox Corporation), famous for his more-than-generous financial support of the ASPR and their then Research Director Karlis Osis. Stevenson later extended his investigations to some European countries, Burma, Thailand, Turkey, and Nigeria. As Stevenson dominated the CORT (Cases of the Reincarnation Type) research for a few decades, all this is supposed to be already familiar to the reader.

Nonetheless, Stevenson's research work met with criticism by skeptics, mostly arguing that the patterns Stevenson found were mere coincidences; in particular, they criticized Stevenson's "backwards reasoning" from birthmarks to fatal wounds. Matlock quotes skeptical voices at length, providing many references within the critical literature.

Discussing *Interpretative Frames for Reincarnation Cases*, Matlock gets closer to the core of the reincarnation issue; however, in order to do so he again goes back to philosophers from classical antiquity already covered in previous chapters, and to the Vedanta, and to Theosophy, probing what they teach about the nature of the soul. From there, he jumps to cases of self-identification where a person is convinced they have been reborn after a former life . . . (usually as a person of historical importance). These cases are not rare. (I personally know two ladies living in Vienna, one of them of Danish nationality, who both claim to have been Marie Antoinette, the infelicitous wife of Louis XVI and who was beheaded during the French Revolution; each of them keeps telling me that she is *the real Marie Antoinette* and the other one is an imposter.) Matlock moves on to narrate a few cases where social constructions involved led to wrong conclusions and misinterpretations. More important, in my view, is discussing reincarnation vs. *super-psi* (the

extrapolation of psi beyond the empirical data) aka *living agent psi* or even *robust living agent psi*. Matlock enumerates the respective opinions of quite a number of researchers in the field; however, each one only rather briefly without going into details that would be desirable. Genetic memory, spirit possession, the psyche at death fragmenting into pieces, personal or local connections, psychometry, "thought bundles" . . . there are many ideas but no sound theory. Although these ideas are very speculative and not backed empirically it would be interesting to look into these more deeply. (It might be noted that the psychoanalyst/parapsychologist Alfred, Baron Winterstein used the notion of "surviving fragments" of the [composite] soul to explain hauntings.)

In the chapter *Child Studies*, Matlock supplies several examples of how everyday occurrences might trigger memories of a past life, particularly the first memory. One case, for instance, is of a three-year-old girl, riding with her father (the author himself) in their family car. They stop at a traffic light, with a motorcycle next to them, prompting the little girl to start a conversation with her father, "Daddy, do you like to ride on motos?" (Moto, in Spanish, is short for motorcycle.) To which the father replied, "no, I don't, they scare me," upon which the girl said, earnestly, "you have to hold on real tight." Surprised, the father asked, "honey, when did you ride on a motorcycle? Was it in Lima?" "No," was her answer, "it was a long time ago. Before I came to you and Mommy."

From recalling bits and pieces relating to the "previous life," and further from discussing various types of memory, the author arrives at the crucial question of how and where the memories are being stored. At first glance, the question as such appears to be a problematic one as the interrogative pronoun "where" demands an answer in relation to space, yet the memory is not an object with any spatial extension. Matlock shifts this problem as he pronounces memory as "registered in the subconscious part of the mind," yet not "exactly like bits on a computer hard drive." He thinks of memories "as imperfectly mirrored representations that are susceptible to psychological processes during their registration, storage, and retrieval in and from the subconscious." This, so he argues, is corroborated by NDEs (near-death experiences), during which memories are formed then and retrieved later, after recovery, or mediumistic communications

that often imply memory formation, storage, and retrieval in a discarnate state. While I readily admit that it makes sense to arrange these phenomena tentatively together and to examine them jointly, there are a few caveats. In the first place, one needs to be cautious not to explain one unknown phenomenon by another one. Secondly, as far as NDEs are concerned, Matlock's assertation that "the brain is shut down or effectively off-line" is questionable (critical sources, such as Gerard M. Woerlee on the famous Pam Reynolds case, are not referenced). Moreover, the "discarnate state" Matlock mentions in the context of mediumistic communications is kind of an interpretation, not an established fact. Indeed, we do not have any evidence of the very existence of discarnate minds. Thus, Matlock's conception of memory is therefore question-begging.

In the following chapter, *Behavioral Identification with the Previous Person*, Matlock re-narrates a number of cases, drawn from several sources. For the reader, it is one of the merits of this book—maybe *the* merit—to encounter a wealth of case studies scattered all over the ever-growing number of publications on the topic of reincarnation.

Birthmarks and Other Physical Signs are what I would rate as perhaps the most interesting aspects of the entire "reincarnation syndrome." What Matlock presents to the reader in this section are rebuttals to critics, case reports with partly detailed descriptions, some statistics on the frequency of occurrence, experimental birthmarks, and various beliefs in tribal societies associated with birthmarks or birth defects. While one may think of birthmarks mimicking fatal wounds received in the previous life, the span of these phenomena is much wider, e.g., Hindu boys who recall previous lives as Muslim men who may be born without foreskins. Last not least, Matlock touches on the conceptional aspects of birthmarks, criticizing Stevenson's notion of the *psychophore* (thought as a carrier conveying memories, behavior, and form from one life to another).

The most interesting topic presented in the chapter *Child Studies: Secondary Signs of Reincarnation* refers to intermission memories, i.e. memories of the period after the death of the previous person and before the present incarnation. Basically, they can mainly be broken down into two groups, one belonging to an extramundane place (be it "heaven," be it an equivalent in a different tradition), the other one

referring to the earthly plane during choosing the parents-to-be. Encounters with Jesus or angels, or other religious figures depending on religious belief, welcoming or guiding the deceased, are reported, as well as encounters with deceased relatives. These intermission memories resemble the "heavenly" or transcendental stage of NDE reports, displaying a cross-culturally common structure, whereas the details vary from region to region, from one religious persuasion to the other, and, finally, from person to person.

One detail calls for being highlighted: Some children ascribe their ability of recalling elements from their previous lives to the fact that they didn't accept food or beverages offered to them during the intermission period. Matlock points to the fact that this corresponds to drinking water from the river *Lethe* (and thereby inducing forgetfulness, in contrast to those who drink water from the river *Mnemosyne* inducing omniscience). One might ask the questions whether this element of ancient Greek mythology is based on experiences of children talking about previous lives, or, conversely, these reports Matlock refers to are induced by (at least fragmentary) knowledge of the mythology of the Greek underworld. However, not all children talking about their experiences during the intermission point at the correlation of non-accepting food and the ability to retrieve memories.

As far as the selection problem (choosing parents for the next incarnation) is concerned, there is, again, based on what those children report, apparently a wide variety of possibilities, e.g., spirits assisting in some cases, etc. Obviously, cases where the intermission period is less than nine months raise general suspicion. Postnatal replacement reincarnation cases complicate the issue even further; Matlock distinguishes them from *walk-in cases* akin to possession.

For some *Universal, Near-Universal, and Culture-Linked Patterns,* Matlock provides some statistical data including a few tables that facilitate the overview of several countries regarding issues like the medium intermission length; percentage of family, acquaintance, and stranger cases; and percentage of sex change cases.

The chapter is closed by discussing *The Psychological Impacts of Past-Life Memory,* i.e. the socio-dynamic effect on the affected families and the environment, quoting at length studies by the late Erlendur Haraldsson and other investigators.

The first lines of the second-to-last chapter, *Past-Life Recall in Adulthood and Third-Party Reports*, state that reincarnation cases of adults are weaker than those of children and are so in various aspects. Adults seldom report unstimulated or uncued memories in the waking state.

(May I add my own experience, dating back some 35 years, during military service. There was one fellow officer of the same rank whom I always had a somewhat strained relationship with. Once I asked him what he thought was the reason we do not get that well along with one another relative to all the others. His explanation was that we had met in a previous life, during WWI, both being Air Force officers, but on opposite sides—he German, I British—and that he had sent me down to the ground during aerial combat. This could have been pure fantasy were it not for the fact I have a liking for flying which he could not have been aware of by normal means (leaving aside ESP). Indeed, I am fond of flying aerobatics still today; I love doing loops and rolls and spins and all the other figures, akin to the dogfights of WWI. Questioned about the basis for this assertion, he replied he simply knew it, neither did that knowledge come as the recollection of a particular scene nor in the form of a dream, it was simply a type of pure, not-concrete, not-vivid awareness that is not furtherly retraceable, and he felt certain about it.

There is a difference between psychological validity and factual validity, Matlock emphasizes (that also would apply to my narration just above), past-life memories may have good psychological validity but lack factual validity. While in agreement with the author on this, I think Matlock does not pursue to a desirable degree what a person's needs or gains are by remembering (or inventing) a previous life.

Several cases are open to interpretations of different kinds, e.g., reincarnation or multiple personalities/dissociation; while Matlock had touched on this issue in a previous chapter, he here goes more into the details of competing approaches in one particular case (Sharada). Based on the fact that the two personalities eventually merged (as has been possible in such extraordinary cases as Sally Beauchamp), there is not much space for interpreting the case as a CORT.

Fantasy and Fact in Past-Life Regression under Hypnosis. Much has been published on the famous Bridey Murphy case that Matlock analyses

in this section, arriving at the conclusion that Bridey Murphy never existed, which is reasonable. He reviews the activities of psychotherapists/hypnotherapists and the problem of them possibly planting their own prejudices onto their subjects. What he does not mention is the fact that psychotherapists—different from investigators—earn their living by applying their methods and have a vested interest in spectacular cases. In addition to hypnosis, G. M. Glaskin's *Christos Technique* to induce ASCs (altered states of consciousness) has been used for experimental regression into previous lives; that could have been mentioned in this context. While this method is very easy to apply, the questionableness of the veridicality of the retrieved memories (or fantasy productions) is the same as with hypnosis.

Altogether, Matlock maintains—and rightly so—that spontaneous cases have more to offer than hypnotic regression.

The same is true—mutatis mutandis—for *past life readings*, etc., as outlined in the last section of this next-to-last chapter we have been talking about (Chapter 6), *The Contribution of Shamans, Psychics, and Mediums*. Albeit belonging to a different category, Semkiw's approach that has become rather popular lately is briefly reviewed.

After all this tour d'horizon, Matlock opens his final chapter, *The Process of Reincarnation*, with this paragraph:

> Reincarnation cases do not stand alone in suggesting that the mind has an existence apart from the body. I begin this last chapter with an examination of other evidence of mind/body interaction and postmortem survival, then return to and refine my theory of the reincarnation process. In the final section, I summarize my "processual soul" theory, constructed from the case data, and compare it to the rebirth concepts promoted by animism, the world religions, Theosophy and New Age metaphysics.

In the first section Matlock compiles and summarizes all the (well-known) indicators for the mind working independently of the (e.g., impaired) body, sometimes relying on already existing synopses without going to the original sources. Matlock names this section *Beyond Materialism* and indeed all that is assembled here cannot be explained by a purely materialist–mechanic interpretation. The weakness, however, is that from this criticism of materialism no vision

is emerging at all of what kind of different approach could address the mind–body problem in a more appropriate way. Matlock sympathizes with Stapp's dualist interactionism, based on the probabilistic nature of quantum interactions, yet the problem (that Matlock seems to ignore) is the same as with the Popper–Eccles and any other kind of interactionism: How can two totally different elements, the material body and the non-material mind, interact upon one another?

Personal Identity and Postmortem Survival: In this section Matlock embarks on an examination of the philosophical debates about personal identity, personal survival, and the nature of postmortem states of consciousness and how reincarnation fits into these.

Matlock commences by quoting Atmanspacher, Stapp, and Chalmers. One would expect that he discusses dual-aspect monism (Atmansbacher/Fach is listed as a reference), yet this is not the case—not surprisingly as dual-aspect monism is hardly compatible with discarnate souls floating around somewhere on an extramundane plane and refusing to taste the fruits of forgetfulness. Likewise, Chalmers and his *hard problem*: Just naming it doesn't replace a proper philosophical debate; this is name-dropping rather than argumentation. Matlock returns to the notion of the unconscious, quoting Myers and Freud, and in a different context C. G. Jung, leaving all others aside. While Ellenberger's monumental work is listed within the references, no mention is made of Dessoir's *Double Ego* covered in Ellenberger, let alone other (earlier) similar concepts from H. B. Schindler (*Day and Night Pole of the Soul*), etc. Myers, Matlock writes, thought of subliminal and supraliminal levels of mind. Fine. Freud's *unconscious*, again following Matlock, is "the repository of forgotten memories and repressed conflicts." This representation by Matlock fails to take notice of the fact that Freud later replaced this layer or strata model by the one of different instances, the Id, the Ego, and the Superego. (Other psychoanalysts, e.g., H. J. Urban, followed with accepting the notion of a superego.) Now to C. G. Jung: Matlock portrays his *unconscious* as just like Freud's, just with the addition of the collective unconscious. That doesn't do justice to Jung. For Jung, the unconscious is the potentially expanding part of the mind, where creativity is located.

Matlock states that an individual's subconscious may be expected to maintain its memories and personality intact throughout the period

we call death. Apart from the toggling between *un*conscious and *sub*conscious, this statement just reflects Matlock's personal belief but is in no way compelling.

Next, Matlock asks, "What criteria do we use to identify a surviving individual with a deceased person?," and adds that philosophers are divided over whether memory or physical features are more important in recognizing someone we know. In my opinion, this discussion doesn't hit the mark. Back in 1976, in a book chapter (unfortunately only in German), I used the overarching notion of *information*. The memory of a certain event may be seen as a story which in turn may be seen as a certain amount of information, and the same holds for the physical appearance which can be described in some detail. The forty-five years that has elapsed since the publication of that book has brought the computer into every household. Hence, in today's wording I might rephrase what I wrote above: The memory of a certain event may translate into so-and-so many bits and bytes, and so does the physical appearance. A photo of someone taken by my smartphone may be displayed on my computer screen, may be sent by e-mail, may be burned on a CD or a DVD; however, in any case it is a certain amount of information. If we leave aside the concrete details (whether memory or physical appearance) and limit ourselves to viewing the problem of recognition, the recognition issue boils down to comparing two sets of information, one originating from a purported deceased person and retrieved by the assistance of a medium (or uttered by a child claiming past-life memories), and the other one available on the terrestrial plane—contained in the memories of surviving persons, in photo albums, in libraries, and in archives. Given that they match to a certain degree, the recognition (and further the identification) is successful, otherwise it fails.

If successful, the next problem arises: What is the source of the information purportedly coming from the deceased, is it really from the beyond or are there alternative explanations? Surely there are: psi—or even *super-psi*. As we don't know the limits of psi, it makes sense to tentatively extrapolate it beyond what has hitherto been experienced. In this view, super-psi could be the source of that information that claims to stem from beyond the grave. I am afraid I cannot see any possibility to distinguish between the two—similar to the problem of which came first: the chicken or the egg?

Henceforth, I rate the problem of purported otherworldly communications as proof of afterlife as irresolvable on logical grounds. Occam's razor would suggest staying with living-agent psi as there is no independent evidence for a non-physical entity (soul) existing without being linked to a physical body, thus this would be a new *ens*, while *entia non sunt numeranda praeter necessitate* [entities are not to be multiplied beyond necessity]. Ultimately, the only (practical) judge for this discrimination might be the degree of complexity, but this again is a very subjective measure, falling into the category of personal belief rather than scientific reasoning.

Back to Matlock: He speculates as to what degree a personality is fixed postmortem or able to change (to develop). The considerations he engages in are not convincing, neither this way nor that.

The next problem he tackles is the existence or otherwise of a quasi-material *subtle body* to which the mind is attached or not, kind of an *astral body*. Matlock does away with this notion, he assumes the reincarnating mind would (e.g., in cases of birthmarks) introduce alterations to the genetically engineered body directly, via PK (psychokinesis).

Matlock then proceeds to discuss *substance dualism*, which he, following Whitehead, rejects in favor of idealism; Whitehead believed, and so does Matlock, that an individual's experiential stream survives his death; Whitehead's process metaphysics would allow for the survival of personality, discarnate agency, and elective reincarnation.

Matlock's own idea on reincarnation, in short, is as follows:

> An experiential stream persists with its identity intact until its reincarnation. At that point, at the subliminal level the stream continues unimpeded, but at the supraliminal level there is a decisive break brought about by the engagement with the new body and brain. We begin each life with a tabula rasa, a blank slate, onto which the past impresses itself through involuntary memories and unconscious influence on our behavior. Autobiographical knowledge of the past is lost, or at least pushed deep into the subconscious mind, when the connection is made to the new brain. My [Matlock's] revised process model acknowledges the discontinuity of conscious awareness between lives while asserting the subconscious continuity of self over successive lives. I will name it the Processual Soul model or theory. The processual soul theory rec-

> ognizes a dualism of mind and body, but its dualism is a type of idealist property dualism rather than substance dualism. There is only one substance, and that is consciousness.

So far, Matlock's own theoretical approach is the processual soul theory—apparently not a theory in the Popperian sense that could be falsified, but perhaps that would mean asking too much. Moreover, the terminology is a bit questionable, as reincarnation might be called processual, but not the soul as such.

The final section deals with rather bizarre occurrences, reincarnation of animals in species lines, a boy having formerly been a python, reincarnation in groups, concurrent reincarnation, two or more spirits coexisting in one body, experiences of transplant recipients, and some more strange things . . .

The book has xxi plus 386 pages; Matlock's own text runs along 276 pages. The book is augmented with a Foreword by Jeffrey Mishlove (*Reincarnation versus Archetypal Synchronistic Resonance*) and an Afterword by Michael Nahm (*Implications of Reincarnation Cases for Biology*), a ten-page *Glossary of Specialized and Technical Terms* (containing a few rather dubious definitions), an extensive *References* section (49 pages), and a *very* useful Index (both names and topics).

Let me add a few words on Michael Nahm's Afterword. In the beginning he gives a quick historical rundown of the notion of evolution in biology, both mainstream and dissident (inheritance of acquired properties). An enhanced biological perspective would call to incorporate psi. Vitalistic models of evolution would comprise three modes: random variation/mutation corresponding with the mainstream, plus inheritance of acquired properties, plus non-mechanistic organizing principles.

Reincarnation cases involving bodily characteristics such as birthmarks are difficult to explain within the framework of conventional biology. Hans Driesch, founder of neo-vitalism, proposed an additional kind of causality, a consideration that may be applicable to these aspects of the formation of the body, too. Nahm maintains that dualistic and monistic position do not exclude each other; they merely apply to different reference systems.

Reincarnation cases signify that regarding the ontogenesis of their subjects, there is a third factor at work that supplements genetics and environmental influence in the formation of human personality and physical features—this aspect alone has the potential to contribute to the necessary paradigm shift in biology.

While I concur widely with Michael Nahm's position—his excellent representation of the present situation in biology vis-à-vis the challenge of integrating reincarnation into an expanded biology as well as his request for a paradigm shift that goes well beyond accommodating CORT—my opinion of James Matlock's text is a bit more reserved. The mastering of the topic is admirable, the numerous references he quotes permit a comprehensive picture both of the phenomena in question and the theoretical positions of leading researchers in the field; however, several of these representations are inaccurate. This book—rather small compared with the voluminous works of Stevenson—might replace an entire library. Nothing is new, yet it is well-compiled, and the index permits easy search and access of particular features or patterns.

My first point of critique is the arrangement of the material. There are many places where a certain topic is dealt with, and at a later occasion it is taken up again. I suspect the intention of the author might have been to demonstrate the interconnections, and, perhaps, to reinforce the contents by repetition (maybe a residuum of the fact that the book originated from courses for students). Anyway, an arrangement of the material where one topic is treated after the other might be preferable. Secondly, some aspects are dealt with rather superficially, as demonstrated above. Thirdly, I can't find Matlock's processual soul theory to be progress, let alone convincing. With stark exaggeration, one might say what is good in this book is not new, and what is new is not good. Nonetheless, for a certain segment of readers—those who want to get a general overview and are not keen to delve into philosophical details—Matlock's book might be quite recommendable.

NOTES

[1] U.S. National Transportation Safety Board. (1983, March 21). *Aircraft Accident Report, Pan American World Airways, Inc., Clipper 759, Boeing*

727-235, N4737, New Orleans International Airport, Kenner, Louisiana, July 9, 1982, NTSB/AAR-83/02. [Reprinted in 2006 as *Aircraft Accident Reports on DVD* by Flight Simulation Systems] https://www.fss.aero/accident-reports/dvdfiles/US/1982-07-09-US.pdf

[2] See also https://www.ntsb.gov/investigations/AccidentReports/Reports/AAR8302.pdf

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BOOK REVIEW

Signs of Reincarnation: Exploring Beliefs, Cases, and Theory by James G. Matlock. Rowman & Littlefield, 2019. 408 pp. $39.00 (paperback). ISBN 978-1538124796.

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James Matlock's *Signs of Reincarnation* discusses important issues related to the belief in reincarnation. These include the historical and social prominence of this belief in various cultures around the world, especially its place in spiritual and religious communities. Matlock also explores data seemly suggestive of reincarnation and attempts to develop a theory of reincarnation that can account for the data collected by parapsychological investigators and researchers. In this way, Matlock aims to show that belief in reincarnation is defensible as a conclusion drawn from what he calls "signs" of reincarnation.

Matlock does a good job mapping out the wide range of beliefs about reincarnation across time and culture. His description of various case studies and their salient features is highly informative. And his effort to develop a theory of reincarnation—what he calls a "processual soul theory"—is a laudable attempt at trying to accommodate the various details of interesting case studies and a core idea of reincarnation in the spiritual traditions of the world.

Unfortunately, this is where my praise ends. Like many other books on the topic, Matlock's book suffers from a variety of serious defects. The cavalcade of poor scholarship, conceptual confusion, and impoverished argumentation is particularly egregious given that *Signs* is allegedly based on the lecture notes for Matlock's course on reincarnation pitched at the advanced undergraduate or Masters-level graduate seminar. In what follows, I'll explain why Matlock's book is paradigmatic of nearly everything that's wrong with survival research over the past thirty years.

MARGINALIZING ARGUMENTATION

The first serious problem is Matlock's tendency to marginalize argumentation.

By *argumentation* I mean the giving of reasons (premises) to support claims (conclusions). Matlock marginalizes this enterprise in a few ways. First, he lacks adequate clarity about the structure of his own arguments. Second, when discussing positions opposed to his own, he doesn't provide the arguments given on behalf of the positions. He fails to do this even when discussing viewpoints with which he is in agreement. Third, he privileges assertions and a selective quoting of sources over the heavy lifting of argument analysis and critique. Consequently, his case for and in defense of reincarnation is illusory.

The Problem of Clarity in Matlock's Argumentation

Matlock says the following in the Preface to *Signs*:

> I am chiefly interested in the nature of the evidence for reincarnation, the question of how good the evidence is, and, if it is satisfactory, how to best interpret it. (p. xix)

On the content of Chapter 1, he says

> I supply operational definitions [of reincarnation] to assist with my appraisal of the evidence for reincarnation. (p. xix)

Throughout the Preface, Matlock tells us what he intends to *do* but not what he intends to *argue*. But the reader needs to know what *propositions* he intends to show are true, plausible, or probable, and Matlock needs to clearly lay out the structure of his intended reasoning. The activity or process of how he intends to explore the topic is secondary at best. A thin, programmatic topical outline isn't a suitable substitute for an analytical outline that shows the overall structure of his intended argument.

Being clear about the structure of one's intended argument should answer two important questions for the reader: (i) What is the main argument? (ii) What arguments are invoked to support the premises of the main argument? His answers should be clearly stated, at least in a general way, in either the Preface or the introductory chapter. And

he should clearly track the answers in an organized way throughout the book and succinctly summarize his argumentation in the book's conclusion. None of this happens.

There are, of course, various *attempts* at argument throughout the book. So, Matlock is clearly interested in making arguments. It's just poorly executed.

One example of this is Matlock's failure to connect what appear to be distinct conclusions at different points in the book. Portions of text attempt to refute materialist objections to reincarnation. The conclusion here seems to be that *materialism is false or at least highly implausible* (pp. 42–44, 235–246). Other parts of the text allegedly refute alternative explanations for the data by showing they are less plausible than reincarnation (pp. 44–51, 110–121, 192–200, 211–223, 248). The conclusion here seems to be that *reincarnation is the best interpretation of the data* (pp. 115, 120, 270). Matlock also attempts to construct a theory of reincarnation that fits the data (pp. 259–271). The conclusion here seems to be that *there is* a *theory of reincarnation—the processual soul theory—that predicts the data and exhibits other explanatory virtues necessary for a good theory* (pp. 270–271).

Below I'll address his apparent arguments for each of these three conclusions. Here I only want to point out that Matlock isn't clear about how these conclusions are supposed to be related, and most importantly how they're supposed to be related to whether and how good the evidence is in support of the truth of reincarnation. This is especially odd since he repeatedly says he's interested in and intends to explore whether there's evidence for reincarnation and how strong that evidence is (pp. xix, 42, 52, 86, 201, 235). We get no clear answer to this question, nor how it's related to what he takes himself to have shown about the implausibility of materialism and the explanatory deficiencies of non-reincarnation theories, or even the alleged explanatory virtues of his proposed theory of reincarnation.

Just to be clear, I'm not criticizing Matlock for not giving a *formal* presentation of his arguments. My criticism is that his argumentation suffers from remedial deficiencies with respect to presentation.[1] Argumentation should not be a scavenger hunt or analogous to working a jigsaw puzzle. The reader should not have to search for hidden clues to uncover the premises and conclusion of the main argument and then

search for further clues that distinguish it from supporting arguments. This is especially true for a book that's touted as a college-level text suitable for professionals (pp. xviii–xix).

Privileging Claims over Argument

Far from being a minor presentational problem, this dialectical defect is baked into Matlock's entire approach. Not only does Matlock fail to clearly state his own arguments, he neglects to present the arguments of others. He repeatedly tells his readers what people *claim*, but he doesn't cite, much less critically engage, the reasons they offer in support of their claims. Sadly, this isn't surprising. If someone doesn't see the value in clearly presenting the reasons for his own viewpoint, he's unlikely to see the merits of doing this when it comes to the perspective of others.

Matlock's discussion of materialism (pp. 42–43, 235–245) illustrates this. He refers to scientists and philosophers who have allegedly shown problems with materialism, but he does not give their arguments. He attributes claims to them but does not show their alleged support for these claims or how strongly the evidence supports their claims, much less how these opinions of scientists provide support for Matlock's claim that consciousness is independent of a physical substrate.

Matlock enlists the views of Henry Stapp (pp. 43, 236–246) ostensibly to support his own mind/brain independence thesis, but there's so little detail here that the only obvious connection between their views is a shared vocabulary and syntactically similar sentences. But this makes Matlock's discussion of consciousness no more credible than a Deepak Chopra lecture. He says physicist Stapp "espouses an interactionism that permits the mind to act directly on quantum processes in the brain and to play a key role in quantum biology" (pp. 236–237). What is this key role? What is the argument for it? How does it support the claim that consciousness can exist without a brain or any physical substrate? Matlock does not say. The reader does not know.

Matlock says, "A strong argument can be made for mind/brain interaction and the postmortem survival of consciousness independent of the reincarnation case data" (p. 237). But what is this argument? Matlock has not given it. Why is it strong? He does not say. The reader has no idea.

Several pages later we're told philosopher Alva Noe thinks conscious experience does not arise from neural activity, followed by references to other scientists and philosophers who have questioned the mind/brain identity thesis (p. 246). Again, Matlock does not state these arguments nor their wider context, much less how they would lend support to Matlock's more specific claim that consciousness doesn't depend on a functioning brain or suitable physical substrate. The reader's need to understand these relevant details remains ignored.

While the appeal to authority is salient, it must be judiciously handled. The testimony of a handful of scientists is not a strong argument for what most scientists *think*, much less the truth of what they think. We need to see the scientific arguments for the claims under discussion, together with a clear statement about the degree of credence these thinkers give to their claims. Just because a scientist proposes something does not mean he believes it, much less believes it firmly. And if Matlock only wishes to make an *argument from authority*, he needs to better calibrate his level of credence to fit the totality of opinions among all qualified scientists, not base his opinions solely on convenient outliers. Unless, of course, he can show that the majority of scientists are subject to a cognitive bias his preferred scientists are immune to.

Matlock fares no better when it comes to discussing positions that differ from his own.

Consider Matlock's perfunctory treatment of appeals to psychic functioning in living persons as a potential counter-explanation of past-life memories and other ostensible signs of reincarnation (pp. 116–120, 212–213, 248, 260). At no point does Matlock explain how thinkers who appeal to psi in this way construe its challenge to reincarnation explanations. He merely selects claims these thinkers have made, removes the claims from their context, then throws objections at them. Matlock's objections are not new, nor is his refusal to acknowledge, much less critically address, the obvious counterarguments against his assertions provided by the very authors he discusses.

Whether conscious or not, Matlock's strategy amounts to little more than rhetorical trickery. You don't refute a person's argument by merely denying their conclusion. You don't refute a person's argument by quoting an authority who denies their conclusion. You don't refute a person's argument even by marshalling evidence against their

conclusion. Refuting an argument requires showing that the person's *argument* fails. This requires stating their argument, demonstrating you understand it, and engaging in conscientious critique—for example, by providing reasons to deny one of their key premises, showing that other considerations weaken the force of their inference, or showing how the evidence against their conclusion outweighs the evidence for it. Had Matlock attempted any of this, he would've realized that his criticisms—for example, appealing to lack of independent support for the kind of psi allegedly needed to account for the phenomena under discussion (p. 117)—have been anticipated and answered by the very thinkers he's discussing.

Here's another illustration:

> Contrary to the claims of Braude (2009, 2013) and Sudduth (2009, 2016), I see no reason discarnate psi processes need involve super-psi, so granting psi capabilities to disembodied actors does not oblige survival theorists to credit the supposed super-psi of embodied actors. (p. 248)

Stephen Braude and I have argued that survivalists must postulate a kind or degree of psi indistinguishable from what would need to be postulated if we attempted to explain the same data solely in terms of living-agent psi. Matlock doesn't present our arguments, and yet he says he sees *no reason* to accept the *claims* he attributes to us. Does this mean he didn't read what we've written? Maybe he isn't persuaded by our reasoning and so sees no *good* reason to accept our claims. But this doesn't absolve Matlock of the intellectual responsibility of stating the reasons we've offered and critically engaging them. He should show why the arguments we've presented are not good arguments. Without this, the reader isn't adequately informed about what Braude and I think, why we think it, much less whether Matlock has a remotely plausible reason for rejecting it. Consequently, the reader isn't the least bit informed about the debate Matlock is allegedly discussing.

CONFUSIONS ABOUT MATERIALISM AND SURVIVAL

Marginalizing argument tends to be comorbid with a wide range of conceptual confusions. And this is exactly what we find in Matlock.

Matlock says that skeptics frequently use materialism to prematurely and unfairly dismiss the evidence for reincarnation, so he sees it as part of his task to challenge this position in philosophy of mind (pp. 42–44, 235–46). His definition of materialism is "The philosophical position that material (physical) reality is primary and the mind or consciousness secondary to it" (p. 296). Throughout the book the term is a catchall that includes a metaphysical claim about the foundations of reality (p. 254) and various claims about the nature of human persons and consciousness, including mind/brain identity (pp. 46, 246–247) and the claim that the mind depends on the brain or presumably any other surrogate physical substratum (pp. 45, 236–237, 239).

I'm not sure Matlock even sees a difference between mind/brain dependence and mind/brain identity. He casually switches between them (see above references), conflates the two when critiquing so-called materialists (see below), and the pages listed in his index under mind/brain identity thesis refer to pages where he's discussing mind/brain dependence. This confusion betrays an astonishing disconnect from the entire field of philosophy of mind. Not surprisingly, it has bizarre and implausible implications.

Here's one such implication: It entails that some forms of *substance dualism* will count as materialist philosophies of mind. Why? Because some contemporary forms of substance dualism affirm that consciousness depends on a functioning brain, and they do not entail that consciousness can exist without a physical substrate. I have elsewhere (Sudduth, 2016, pp. 26–27) discussed these versions of substance dualism, but here I'll note Lowe (2010), Hasker (2001), and Taliaferro (2001).

To quote Taliaferro:

> Substance dualists need not deny that the destruction of the body leads to the destruction or annihilation of consciousness and the person . . . [they may hold] that there is no conscious, personal life without certain configurations of physical states. (Taliaferro, 2001, pp. 66–67)

But Matlock's sloppy discussion of materialism also leads him to make patently false claims about the prospects for life after death from a physicalist viewpoint. After referring to "the materialist conception of

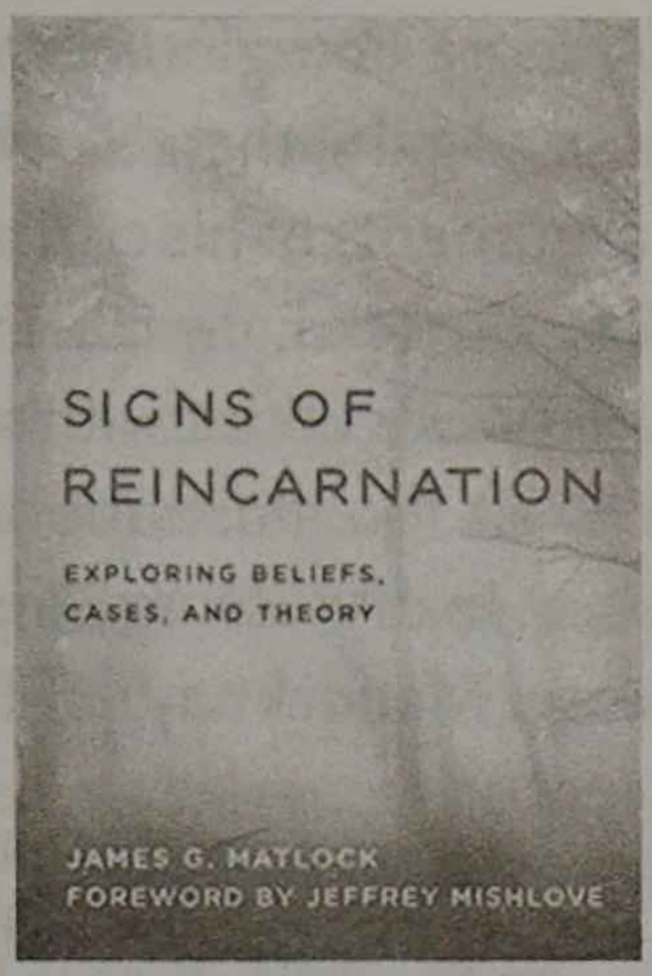
consciousness as a product of cerebral activity," he says "The materialist position rules out any possibility of the survival of consciousness after physical death" (p. xx, cf. pp. 42, 51, 245, 260).

Matlock's claim is false.

As just noted, some substance dualists affirm that the brain produces consciousness or that consciousness otherwise depends on a functioning brain. These substance dualists are materialists given Matlock's broad definition of materialism, but their position does not rule out the survival of consciousness, as the sources referenced above explicitly argue.

The thing to appreciate here is that even if the brain produces consciousness or mental states are dependent on the brain, it does not follow (even probabilistically) that consciousness is *essentially* connected to the brain and cannot exist without it. This would be true, for example, if mental states are properties of a soul (immaterial substance), but souls cannot have conscious episodes without an appropriate physical substrate. It would also be true if mind or consciousness is an information-processing structure, for the same information, form, or structure produced in or by one physical substrate can be transferred to different physical media. The mind could be *substrate independent* even if the brain produced it. In which case it does not follow that the mind is destined for cessation upon biological death. Some physicalists accept substrate independence—mind can supervene on any number of physical substrates (Bostrom, 2003).

It follows from the above that the postmortem persistence of consciousness does not require any commitment to disembodied minds or the possibility of consciousness existing without a physical substrate. And this is true, not just from the point of view of non-Cartesian substance dualists, but also from the viewpoint of prominent physicalists. There are various physicalist theories of the person—roughly, humans are wholly physical or material beings—that involve life after death in the form of divinely assisted supernatural physical resurrection (Baker, 2011; Corcoran, 2001, 2006; van Inwagen, 1978; Zimmerman, 1999). Alternatively, there is digitalism, a naturalistic view of immortality that rejects mind–body dualism, idealism, and mind–brain identity (Steinhart, 2014, 2015). Digitalists—for example, Moravec (2000), Tipler (1995), Kurzweil (2005), and Bostrom (2003)—hold that



the mind is entirely computable. As such, it is substrate inde-pendent. So, consciousness is capable of persistence beyond biological death—for example, through mind-uploading and simulation (Steinhart, 2012).

I suspect Matlock would find these physicalist accounts of life after death implausible or indicative of wishful thinking. But that's not relevant here. The point is not about the truth or falsity of these theories, but about their logical implications. Even if false, these physicalist approaches show that Matlock's claim (p. xx) is both false and confused. Nor is this an incidental error in Matlock's network of falsehoods and half-truths. It's a vital part of the rhetorical scaffolding of the entire book, for he would have the reader believe that scientists who reject reincarnation do so because they reject the very possibility of survival *on account of* their materialist commitment (pp. 42, 45, 51–52, 198, 235, 245, 260). No doubt, some do. But one can make that sensible observation without relying on obscurantism and falsehoods.

What's also bizarre is that given Matlock's definition of reincarnation, even reincarnation turns out to be compatible with some of the physicalist viewpoints above. According to Matlock's operational definition, what reincarnates is a "duplex stream of consciousness that carries forward memories, behavioral dispositions, and other aspects of personality through death to union with a new body" (p. 44). Matlock later explains "it is clear that reincarnation is not about the replication of a complete identity in a new person, but about the persistence of a conscious stream and the influence of a previous personality on the present personality" (p. 252). All this requires is substrate independence. So, for all we know, the so-called evidence for reincarnation is only evidence for substrate independence, not evidence against the dependence of consciousness on the brain or other physical substrates. This is why digitalism permits reincarnation (Steinhart, 2017, pp. 3–5).

BLATANT MISREPRESENTATIONS

In the first section of this review, **Marginalizing Argumentation**, I criticized Matlock for failing to present the arguments of his critics.

A more egregious error is his blatant *misrepresentation* of their views.

Again, there's nothing surprising here. If Matlock doesn't state the arguments of his critics, there's no context to constrain his interpretation of source material. Equally, there's no way for the reader to assess Matlock's critics without reading the authors he has mispresented.

A few illustrations will suffice.

In discussing Stephen Braude's analysis of the well-known Sharada case, Matlock says of Braude:

> He *supposed* that Uttara invented the Sharada personality in response to the rejection by her friend and as a compensation for an unhappy life. He *contended* that Sharada's detailed knowledge of Bengali geography and customs *was* retrieved by Uttara through a 'motivated psi' or super-psi in her altered state of mind. (p. 212, *italics* added)

But Braude does no such thing. He does not say or imply that this *is* what happened. He's not arguing for the truth of the motivated-psi hypothesis or even for its superior explanatory power. As Braude explains (Braude, 2003, pp. 101–102), he's arguing that survivalists have prematurely dismissed appeals to psi in this context because their analyses have been psychologically superficial and consequently survivalists have decided too quickly in favor of survival. His point is not that motivated psi *is* a superior explanation to survival, but that survivalists—not seeing how motivated psi poses a challenge to survival—haven't presented good enough reasons to rule out this counter-explanation.[2]

Another example. While clarifying that reincarnation should be understood as involving the persistence of a conscious stream, not the replication of a complete identity, Matlock says:

> Braude (2003, p. 298) considered "implausible" the idea that the personality of a deceased individual survived for a time in a discarnate state, then went through a gradual transformation after reincarnating. (p. 252)

Is this really what Braude said? No.

Braude didn't say it's implausible that the discarnate personality of a deceased person survives for a time, then goes through a transition after reincarnating. He spoke of the *seemingly* implausible scenario

of *one person becoming another person* as a potential implication *of Quinton's neo-Lockean view*. His comments are about an implication of someone else's views.

In some cases, the distortions pile up in a single passage and produce a cavalcade of falsehoods and rhetorically charged misdirection.

Prior to *Signs*, Matlock published a critical commentary on *The Myth of an Afterlife*, a collection of essays critical of survival and edited by Keith Augustine and Michael Martin (Matlock, 2016b). Matlock's review was a cacophony of recalcitrant distortions and misrepresentations, especially with respect to the Augustine and Fishman coauthored paper, "The Dualist's Dilemma," in that collection (Augustine & Fishman, 2015). Despite Augustine having corrected Matlock on crucial interpretive points (Augustine, 2016), Matlock ignored them in his reply to Augustine (Matlock, 2016c) and chose to reproduce several of the more egregious errors in *Signs*.[3]

> Augustine and Fishman (2015) maintain that the materialist position has so much going for it that it should be given the presumption of truth. They introduce a Bayesian analysis in which they assign much more weight to the brain/identity thesis than to the possibility of mind/brain interaction. The outcome of a Bayesian analysis is heavily dependent on how one weights the factors that go into it. By assigning the weights as they do, Augustine and Fishman ensure that the mind/brain identity thesis emerges the winner. However, the mere fact that there are serious questions about the mind/brain identity thesis reduces the weight that may in fairness be allotted to it, and if all the evidence in favor of mind/brain interaction is taken into account as well, the outcome of the Bayesian analysis looks very different (Matlock 2016b, 2016c). (p. 246)

Augustine and Fishman do offer a Bayesian analysis in the cited article, and Matlock is also correct that the outcome of Bayesian analysis depends on the values assigned to the components of Bayes' theorem. But everything else Matlock says here is false.

First, Augustine and Fishman are not discussing the mind/brain identity thesis or contrasting it with mind/brain interaction. They're comparing the mind/brain dependence and mind/brain independence theses. Their conclusion? "Using Bayesian confirmation theory and information theory, we find that the dependence thesis is vastly more

probable than the independence thesis" (Augustine & Fishman, 2015, p. 204). Matlock's error is doubly inexcusable since Augustine corrected Matlock on it (Augustine, 2016, pp. 216–218) long before the publication of *Signs*.

Second, in their Bayesian analysis (2015, pp. 259–271), Augustine and Fishman explicitly state that they assign the equiprobable weight of 0.5 to the prior probability of each of the contrasting theses (2015, pp. 259–260). This is the same prior probability prominent survivalists have assigned to survival in Bayesian-styled arguments for survival—for example, C. J. Ducasse (1961) and David Lund (2009). So, Augustine and Fishman do not "assign much more weight to the brain/identity thesis than to the possibility of mind/brain interaction." This isn't even true with respect to the mind/brain dependence thesis which they *are* discussing.

Consequently, it is false to say that Augustine and Fishman have rigged their analysis to ensure that the mind/brain dependence thesis—much less the mind/brain identity thesis—will emerge as the winner. They assign the value 0.5 to the mind/brain dependence thesis precisely *not* to prejudice the case against survival. They have, contrary to Matlock's unsupported assertion, given due consideration to the "serious questions" that should reduce "the weight that may in fairness be allotted to it." They do not grant mind/brain dependence a presumption of truth nor does the value they assign to the prior probability of mind/brain dependence predispose their Bayesian analysis to a conclusion against survival.

Two other things are worth noticing here. First, Matlock once again shirks the responsibility of giving an argument. He claims, "if all the evidence in favor of mind/brain interaction is taken into account as well, the outcome of the Bayesian analysis looks very different." But he does not give an argument showing this. Nor does he show what values Augustine and Fishman provide and how such values would yield the result he claims. Second, Matlock obscures the crux of the Augustine and Fishman argument, which is that the net plausibility or posterior probability of the independence thesis in its various incarnations is inferior to the mind/brain dependence thesis because of the former's lack of predictive power, not because of any particular assignment of prior probability.

It's hard to say whether stating the Augustine and Fishman argument would have prevented such egregious errors. But considering how important Matlock makes materialism to the reincarnation debate, you'd think he would have at least tried. His failure to do so is lazy and amateurish. And given that Augustine has corrected Matlock on these remedial interpretive errors, Matlock's insistence on reproducing them—as he does with others—is a form of cognitive intransigence that undermines the scholarly integrity essential to advancing healthy debate on any topic.

Matlock wishes to refute materialism. This is clear. But his reasoning at this juncture depends on a variety of false assumptions about what materialism is and how it's related to survival. He also fails to offer a single argument on behalf of materialism by those people branded with this rhetorically charged term. Much less does he give the reader a glimpse into how skeptics have proposed that materialism or mind/brain dependence challenges arguments for reincarnation. Consequently, we can't take Matlock's criticisms of materialism seriously. And to the extent that his case for reincarnation depends on refuting materialism, his case for reincarnation fails.

REINCARNATION—THE BEST EXPLANATION?

It's common for survivalists to claim that reincarnation provides the best explanation of the kind of data Matlock considers. Matlock too makes this claim (pp. 120, 270). He says his reincarnation theory exemplifies a variety of explanatory virtues (pp. 44, 86, 259–270), and even has "considerable explanatory power" (p. 270).

After arguing that at least five different non-reincarnation interpretations of the evidence are inadequate, he writes:

> I have now considered all the major interpretive frames for rebirth syndrome accounts and reincarnation cases alternative to personal survival and reincarnation and found all wanting as explanations of at least the better cases. This places us in the uncomfortable position of having either to denigrate the investigators who have concluded that reincarnation is the best interpretation of the cases they have studied or to agree with them. (p. 120)

The rhetoric here is unfortunate. Instead of either denigrating or agreeing, we might simply disagree. Does Matlock think there can't be reasonable disagreement with someone without denigrating them? I see no reason why disagreeing with how researchers have reasoned to a conclusion requires denigrating them.

But more importantly, does Matlock succeed in showing that reincarnation is the best explanation of the data he considers? No, not even close.

A good inference to best explanation must adequately rule out competing hypotheses or theories. This means showing that alternatives—especially nearby ones—cannot explain the total dataset as well as the preferred theory. To do this requires having a clear set of explanatory criteria, assigning weights to them, and applying them consistently to the alternate theories and one's preferred theory. Matlock does none of this.

Consider Matlock's perfunctory dismissal of the more recalcitrant counter-explanations of the data—for example, the living-agent psi explanation, especially in its robust psychological forms. As shown above, Matlock relies on a variety of transparent falsehoods and distortions of what this counter-explanation is and how it allegedly challenges reincarnation as an explanatory candidate. He can't even bring himself to state the arguments of those who have insisted that survivalists have been short-sighted at precisely this juncture.

Consider also that Matlock's reasons for dismissing counter-explanations often rest on the presumed lack of independent support for what these theories would need to commit themselves to. Case in point: the alleged lack of "independent evidence" for the kind of living-agent psi that would be required to account for the data (p. 117). And yet, Matlock's theory of reincarnation can be made to fit the data only given a wide range of assumptions for which he's provided no independent evidence—for example, it being unlikely that a conscious stream would lose its structure (i.e. memories, personality traits, and cognitive functioning, including psi capacities) when becoming discarnate (p. 248), and the expectation that we would see other evidence of postmortem activities such as "announcing dreams, apparitions, and mediumistic communications" (p. 259). Matlock presents no independent evidence for what the content of

consciousness and mental functioning will look like should it persist after death. If Matlock can help himself to all manner of assumptions that seem correct to him but for which he feels no obligation to present evidence, there's no reason why those sporting alternate explanations can't do the same.

But let's set aside the above criticism and grant that Matlock has given us good reasons to think that reincarnation—simpliciter or his processual theory—is the best explanation of the data. What follows? Or rather, what does *not* follow from this conclusion?

It does not follow that reincarnation is a *good* explanation, much less a very good one. It might be a very poor one. It's a truism of inference to best explanation that the best explanation for our data need not be a very good explanation. It might be a terrible one. And given that Matlock thinks alternate explanations are as bad as they are, it seems pretty clear that Matlock has, at best, only shown that reincarnation is the best explanation of a bad lot of explanations. This is an underwhelming conclusion.

I suspect that Matlock thinks the explanatory virtues he attributes to his processual soul theory show that his particular reincarnation theory is a good one, not merely better than the alternatives. After all, after listing some of its alleged explanatory virtues, he says his theory has "considerable explanatory power" (p. 270). But does he successfully show this? No. In fact, he seems utterly unaware of the bridge that must be built from *best* explanation to *good* explanation.

A glaring problem here is that the theoretical virtues he attributes to his theory are illusory.

Good theories, he tells us, can be confirmed or falsified (p. 44). True, but at no point does Matlock say what *observational* datum would falsify or disconfirm his hypothesis. He says his theory is incompatible with certain possibilities—for example, transmigrating across species or retributive karma (p. 270). But these possibilities are not observational data. To show that a theory is incompatible with certain (theoretical) *statements* is not to show that the theory is incompatible with statements that report observational facts. Hence, it's not an empirical prediction of this theory that it rules out certain possibilities.

Of course, Matlock does state some observational data, which he claims his theory predicts—for example, the facts surrounding children

who make veridical claims about a previous personality or who exhibit behavior or physical signs characteristic of a previous personality. But I find the reasoning here opaque at best.

It's not clear what predictions his theory makes with reference to the data he's discussing. Memories as such are not observational data, though a *claim* to have remembered a past life is. But as Matlock explains it, his reincarnation theory supposedly *explains* both the fact that some children claim to remember past lives and the fact that many don't make such claims or otherwise exhibit characteristics of a previous personality (pp. 124, 200, 251). So, what is the observational datum the theory predicts? What should we expect to observe if reincarnation is true? And more importantly, what should we not expect to observe if reincarnation is true? And why?

When a hypothesis or theory has a predictive consequence, it either entails or makes probable some observational datum D. "D" is an outcome with parameters that exclude other states of affairs that can, at least in principle, be observed. So if we expect D, we do not expect not-D, nor any other state of affairs incompatible with D. It's only because the prediction D is incompatible with other possible predictive outcomes that we say the hypothesis or theory can be disconfirmed. So, what observational datum does Matlock's reincarnation theory lead us to expect, and which if not observed would disconfirm his theory?

Presumably the prediction that allegedly confirms reincarnation has *something* to do with persons (especially children) claiming past-life memories and displaying behavioral resemblances and birthmarks. But what exactly is the prediction here? By Matlock's own admission, his theory is compatible with the majority of the race not displaying these features. So why does the presence of these features in some cases confirm reincarnation but their absence in other cases (apparently the vast majority of the race) not disconfirm reincarnation?

I understand why Matlock thinks his theory can accommodate the fact that some (most?) people don't have or don't claim to have past-life memories. In our present life, the subconscious is the repository of memories (p. 124), but because we have built-in defense mechanisms against consciously remembering (p. 200), material in the unconscious can be repressed or blocked (p. 251). But I fail to see why these facts, when incorporated into Matlock's theory of

reincarnation, would lead us to expect the world to look the way it does—*some children and adults having past-life memories and some not*. Why not some other way? For example, *no one having past-life memories, everyone having such memories, most people having such memories, most children having past-life memories but no adults having such memories, no children having such memories but most adults having such memories.*

Naturally, the absence of past-life memories across the world does not count against Matlock's reincarnation theory. But that's only because the theory is, as far as I can see, compatible with a very wide range of possible outcomes on this and other points of data. But this is hardly a theoretical virtue. Quite the opposite. The only reason I can see for his not making any of the alternate scenarios a prediction of his theory is that he already knows these scenarios do not obtain. So, Matlock's theory merely accommodates the data, rather than genuinely predicts it. He doesn't seem to appreciate that explanations that merely accommodate previously known facts are much weaker than explanations that predict novel facts.[4] The former easily creates an illusion of genuine explanation by way of post hoc theorizing.

Particularly illuminating in this regard is Matlock's explanation of how he built his reincarnation theory.

> [It] is grounded in data rather than in any a priori considerations. I did not begin with a theory of reincarnation and go looking for evidence to support it; rather, I let the evidence guide the development of the theory. The result is an empirically based statement about what reincarnation is and how it works that can serve as a starting point for further discussion and theory building. (p. 261)

This passage raises red flags.

First, it's unclear how the data could shape the details of his theory unless he antecedently assumed that the data were suggestive of reincarnation in *some* sense. But this begs the evidential question—why are we justified in taking the data as suggestive of reincarnation in the first place? *If* I assume that various seemingly unusual features of my garden are signs of an invisible gardener who's tending to my garden, it's only a lack of imagination that would prevent me from developing

a theory about this gardener and how he/she/it works that could easily accommodate anything I might observe. Similarly, if I assume that the various data Matlock discusses *are* indeed signs of reincarnation, I can flesh out a "theory" of reincarnation that develops the core idea of what reincarnation is and how it works.

Second, Matlock's theory incorporates a variety of auxiliary assumptions the independent evidence for which (or any other kind of justification) seems wholly lacking. This gives the impression that the only reason for adopting the auxiliaries is to make the theory fit the facts. What's the motivation for accepting these assumptions unless one is already committed—not to a theory of reincarnation but to a contentious assumption that reincarnation is true in some sense and the facts in question are connected to it?

Anyone can create a just-so story to accommodate facts, even otherwise recalcitrant ones. One should not confuse this with theories in an empirically robust sense. Matlock's theory of reincarnation leads us to expect nothing with stated parameters. Yes, it can accommodate pretty much any datum, it would seem, but only by relying on a large number of assumptions, limited only by one's imagination, but most of which are at least as contentious as reincarnation itself. And we have no reason to suppose that the data Matlock's theory accommodates are otherwise improbable (i.e. predicated by the theory but unexpected on alternative theories and not merely a part of our background knowledge). For these reasons, Matlock's processual soul theory is not a good theory of reincarnation.

THE EVIDENTIAL QUESTION

I'm baffled by another problem—the final problem—that vitiates Matlock's entire book. Matlock gives us every indication that he's interested in whether there is good evidence for reincarnation.

> I am chiefly interested in the nature of the evidence for reincarnation, the question of how good the evidence is, and, if it is satisfactory, how to best interpret it. (p. xix)

> I aim to focus attention on the evidential dimensions of the problem. Could reincarnation be not merely a belief of a good many of

> the world's people, but a reality for some or even all of us? Is there evidence to support this straightforward conclusion from cases like Rylann's? (p. 42)
>
> I aim to develop a theory that makes sense of the case data in the wider context of scientific knowledge, not merely to demonstrate the likelihood of reincarnation in a generalized sense. (p. 86)

In the first quote above, it's not clear what Matlock means. It might mean he intends to look at the quality of the data and investigative methodology behind the collection of the data. But assessing how trustworthy data are isn't the same thing as asking whether the data are evidence *for* reincarnation. That's a question about the logical relationship between the statements that express the data and the statement(s) that affirm reincarnation. We might have good reason to think that investigators have accurately described the facts at a murder scene. Whether these facts are good *evidence* that a particular person committed the murder requires a good inference from the facts.

That said, I think it's clear from the other two quotes above that Matlock aims also to consider whether there is evidence that would support the likelihood of reincarnation (cf. pp. 33–34, 52). And Matlock seems to think he's been successful in this regard.

First, after a detailed discussion of ostensible reincarnation cases (pp. 123–200), he says "reincarnation cases do not stand alone in suggesting that the mind has an existence apart from the body" (p. 235), and he goes on to claim to provide evidence of mind/body interaction and postmortem survival and to laud "all the evidence now available of the mind's ability to function in a discarnate state" (p. 245).

Second, in the wrap-up in the final chapter, he reintroduces the basic evidential question—is there evidence for reincarnation?—and says an affirmative answer depends on doing what he has done in the book. This at least suggests he thinks there's evidence for reincarnation. Very strong evidence, for he concludes: "I now feel no hesitancy in declaring I believe reincarnation is the only intellectually defensible interpretation of the data" (p. 270).

For all the expressed interest in addressing the evidential question, I don't see that Matlock has done anything to address it.

Matlock doesn't even state what it would mean for one statement to be evidence for another, much less do we find any account of criteria of evidence. And I see no argument anywhere in the book that shows that the data Matlock considers are evidence for reincarnation, much less good evidence or evidence that makes reincarnation likely.

Some survivalists who argue that reincarnation is the best explanation of the data infer from this that the data are therefore (good) evidence for reincarnation. In other words, they convert explanatory value into evidential value. This, of course, is an implausible inference, unless one appropriately bridges the gap between explanation and evidence.

Is Matlock making or suggesting this kind argument? His discussion is so lacking clarity it's hard to say. What we can say is that if he isn't making the bogus argument—illicitly inferring good evidence from explanatory power—he's made no argument at all for the truth of reincarnation. On the other hand, if he is making the bogus inference, his argument for reincarnation is poorly stated and monumentally bad.

Let me unpack this a bit.

The problem with attempting to cash in the explanatory power of reincarnation for hard evidential currency is justifying the conversion of one kind of value (explanatory value) into another (evidential value). There is no simple inference *to* good evidence or probably true (or any other such epistemic assessments of belief) *from* the mere fact that some theory better explains the facts than the explanatory rivals.[5] And Matlock doesn't help us bridge the gap here. Although he lists a handful of explanatory criteria, he doesn't inform the reader how he thinks the explanatory success of his theory justifies any kind of answer to the evidential question, much less how strong the evidence for reincarnation is supposed to be based on its alleged explanatory power, though he seems to think that it makes reincarnation likely (p. 86).

Just to be clear, I'm not claiming that the best explanation of some data does not get evidential support from the data. Nor that this cannot be argued. Elsewhere I've shown in detail exactly how this can and often is done using Bayesian probability (Sudduth, 2016), though other frameworks are available. The salient point is Matlock does not do it, but he should. And he doesn't even seem to be aware of this problem. So, he cannot justifiably claim that the data provide good evidence for reincarnation or that he's shown that reincarnation is likely. And if he's

not purporting to do this in the book, he should not claim or otherwise suggest that he is.

Matlock's own claims about the alternate non-reincarnation theories exacerbates the problem. He's highly critical of rival theories invoked to account for the evidence. He thinks they're bad explanations. But if these alternatives are so improbable, then it looks like the most we can conclude from reincarnation being the best explanation is that it's just *more* probable than other highly improbable theories. But this is consistent with reincarnation being improbable. Every space of improbable theories will be occupied by theories more or less improbable in relation to each other. None of them thereby merit our acceptance.

Much of this obscurantism could have been avoided had Matlock simply told us what it means for some fact or observational datum to count as evidence for the truth of some statement(s). This is a very basic epistemological question and central to the broader survival debate. Why should, for example, verified claims to past-life memories count as evidence for reincarnation? Why should behavioral resemblances between a current personality and a formerly living person count as evidence for reincarnation? Why should birthmarks be evidence for any kind of reincarnation? Matlock's book provides no answer to these crucial questions.

Matlock's frequent use of the phrase "evidence for reincarnation" only masks this problem. It allows him to conflate (i) facts *alleged* to be evidence for survival and (ii) facts *shown* to be evidence for survival. Whether intentional or not, it allows him to sidestep the crucial challenge of showing that the data *are* evidence for reincarnation, good evidence, or that reincarnation is likely.

I'm not saying the data Matlock is examining are not evidence for reincarnation. I'm saying Matlock has failed to show that they are. Why? Because he's not done what's required in point of logic to do this. Merely describing the data in meticulous detail and saying they're suggestive of reincarnation doesn't meet this demand because it either doesn't tell us enough or it begs the question.

Here is the recipe for at least partial success in this regard.

First, Matlock needs to be clear about what it *means* for the data to be (good) evidence for reincarnation. The base-level idea is

straightforward enough: Whatever is evidence for another statement h counts in favor of the truth of h, is an indicator of the truth of h, or gives us a (good) reason to think h is true. Survivalists and non-survivalists often parse the concept of evidence in terms of evidential probability. Here "data are evidence for h" means data-statements confer some favorable probability on h—for example, raising h's probability. Others parse evidence totally or at least partly in terms of explanation—for example data-statements are evidence for h only if h successfully explains the data-statements. I don't expect Matlock to develop a complete epistemology or theory of evidence, but he should at least be clear about what he's talking about.

Second, Matlock needs to state his *criteria* of evidence; he should state the conditions under which he thinks data-statements count as evidence for some other statement(s). And since evidence comes in degrees, his criteria of evidence should include principles that discriminate between different degrees of evidential support between statements. Such principles should tell us when data weakly support a hypothesis, when data offer modest support, and especially when data strongly support a hypothesis, as well as when and to what extent the data support one hypothesis more than they support another.[6]

Finally, Matlock needs to apply such principles or canons of evidence to the data he discusses, his reincarnation theory, and alternative theories.

Matlock does none of this. Consequently, he cannot justifiably claim that the detailed data he meticulously describes over two hundred pages are evidence for reincarnation, much less good evidence for reincarnation.

CONCLUDING THOUGHTS

I've given four substantive criticisms of Matlock's book.

First, he fails to adequately address a crucial evidential question—is there (good) evidence for reincarnation? But he claims his book will do this.

Second, he doesn't adequately address the explanatory question he flirts with—is reincarnation the best explanation of the data? But he claims the book will do this.

Third, as the result of the first two deficiencies, the connection between explanatory power and evidence is opaque. In this way, Matlock's book exhibits a more widespread deficiency in the literature on survival.

Finally, the analysis and argumentation in the book is badly impoverished, amateurish even in places. He fails to offer clearly stated arguments (of his own position or those of his critics), doesn't seem to understand remedial philosophical concepts, and misrepresents the claims and arguments of those who hold positions that differ from his own.

Like many other survivalists, Matlock gives us narrative, a just-so story, not a clearly stated argument with a coherent structure and command of the essential concepts he deploys, like evidence and explanation. He ends up reproducing a familiar pattern in survival literature: Present data, describe non-survival explanations, ignore the arguments for these counter-explanations and instead quote "experts" who seem to reject them or simply assert that they fail, then claim that survival—or in this case reincarnation—wins.

This is not how a case for reincarnation is made. This is not how arguments of any kind are made. And it's is not how one advances the debate on survival, a topic that is worthy of a more serious kind of critical engagement.[7]

NOTES

[1] For a striking contrast in survival literature over the past forty years, see Almeder (1992), Braude (2003), Gauld (1982), Lund (2009), Paterson (1995), and Augustine and Fishman (2015). Whatever else one might say about these books, one cannot accuse the authors of failing to be adequately clear about their conclusion(s) and premises.

[2] In the same context, Matlock accuses me of *adopting* "the alternate personality interpretation" (p. 213). This is false; I don't *adopt* this hypothesis. I argue that, in the reincarnation arguments I consider, *survivalists* cannot justify the explanatory power of reincarnation and simultaneously rule out the explanatory power of appeals to a robust psi hypothesis that includes psi and impersonation features. Matlock likely distorts my position for the same reason he distorts Braude's. He fails to understand what kinds of claims we're committed to because he fails to understand the kind of argument we're making.

[3] Matlock has similarly ignored my corrections and reproduced in *Signs* many of his distortions of my arguments which he first published in a review of my book on survival (Matlock, 2016a). Among these is his contention (pp. 51–52, 246) that my Bayesian analysis fails for the same reasons that Augustine and Fishman's analysis fails. While some of my arguments respond to Bayesian-style survival arguments, I do not leverage Bayes' theorem against survival by relying on reductive materialism, mind/brain dependence, or any other position in philosophy of mind to drive down the prior probability of survival. I argue that survivalists themselves drive down the prior probability of their own hypothesis/theory when they bulk it up with a wide range of auxiliary assumptions in order to ensure that their hypothesis/theory can properly accommodate the data (Sudduth, 2016, pp. 18–20, 245, 296). I also argue that survivalists have not succeeded in arguing that a bulked survival theory leads us to expect data which are otherwise improbable, in part because they've not been able to sufficiently rule out rival theories that would lead us to expect the data. And so we're not justified in concluding that the data make survival more probable than not, much less highly probable.

[4] The need for "novelty" does not seem to center around when facts were found or known relative to the development of the theory, but whether the theory was specifically adjusted to entail them. Thanks to Keith Augustine for bringing this to my attention in an earlier draft of this review.

[5] Bas van Fraassen has argued this point in detail (see van Fraassen, 1989, pp. 142–150).

[6] If evidence is parsed in terms of probability, one's criteria of evidence naturally include principles that distinguish between (i) evidence raising the probability of a hypothesis, (ii) evidence making some hypothesis more probable than not, and highly probable, and (iii) evidence favoring one hypothesis h* over another h. Bayesian analysis is often invoked to give a formal account of (i) and (ii), whereas Likelihoodism can give a formal account of (iii). For discussions on probability and explanatory approaches to evidence, see Achinstein (2001). On Bayesian and Likelihoodist approaches to probability and evidence, see Sober (2008, Chapter 1).

[7] Thanks to Stephen Braude and Keith Augustine for commenting on earlier drafts of this review. And to James Matlock (correspondence, January 1, 2021) for clarifying his interpretation of Augustine and Fishman (2015).

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ESSAY REVIEW

Mediumistic Materializations in France During the Early 1920s

Sessões de Ectoplasmia: Experimentos com Ectoplasma na França de 1920 no Instituto de Metapsíquica Internacional [Ectoplasm Séances: Experiments with Ectoplasm in France in the 1920s at the International Metapsychic Institute] by Antonio Leon. Epígrafe, 2019. 349 pp. ISBN 978-85-65307-32-1.

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As I have argued before in this journal, there is a rich tradition of psychical research studies of materialization mediums published before 1930 (Alvarado, 2019a). The phenomenon, associated with many well-known mediums such as Eva C., Florence Cook, William Eglinton, D. D. Home, Franek Kluski, and Eusapia Palladino, has been reviewed by many people during the nineteenth century and later (e.g., Moses, 1884–1886; Richet, 1922, Part 3). Opinions about it have been diverse. In a review of nineteenth-century evidence about it in his book *Modern Spiritualism*, Frank Podmore (1902, Vol. 2, Chapter 6) was rather dubious about the existence of the phenomenon. In his later concise history of psychical research, Rudolf Tischner (1924) argued that we cannot be sure if "strict proof of the reality of materialization has been provided," but there has been "circumstantial evidence of considerable strength" (p. 68; this, and other translations, are mine). More positively, Charles Richet (1922) wrote in his celebrated *Traité de métapsychique* that materializations could "take a definitive rank in science" even if "we understand absolutely nothing about it" (p. 690). Over the years these attitudes have been maintained by many writers and students of the

subject, some of which speculate about vital forces and spirit action. In addition, there have been various reports of fraud with materialization mediums (e.g., Sitwell & Von Buch, 1880; Wallace, 1906).

Students of the history of materialization phenomena are aware of the studies on the subject by French individuals such as Juliette Bisson and Gustave Geley. This is the main work reviewed by Antonio Leon, who has a doctorate in history from the Federal University of Rio de Janeiro. In *Sessões de Ectoplasmia*, which focuses on French developments during the 1920s, Leon analyzes materialization phenomena, some of which were studied at the Institut Métapsychique International (IMI) during the 1920s. This book appears at an appropriate time because IMI celebrated their centenary in 2019.

Leon states that in his work about IMI he set out to

> investigate how the experiments took place, their organization, the precautions taken to prevent fraud, their procedures of control, the phenomena, their description, and who the mediums were and the investigators involved . . . [The book] aims to verify the various aspects that pervaded the experiments during the decade of the 1920s, . . . the values and rules of the investigations of ectoplasm of this period. It will also focus on the research context in which the experiments were located. (p. 19)

Furthermore, Leon proposes that during the 1920s there were two approaches to psychical research. One was a French school mainly interested in physical phenomena such as materializations, and an English one that focused on mental phenomena, a topic I will discuss in more detail later.

The first chapter is about the founding of IMI in 1919, and its early development, a topic discussed by other authors such as Lachapelle (2011). The President of the organization was Italian physician Rocco Santoliquido, and its Director was French physician Gustave Geley (Figure 1). The financial support came from Jean Meyer, a rich wine merchant. But the actual research background came with Geley, who had shown belief in psychic phenomena which he connected to a non-material subconscious mind (Geley, 1899). His initial work with materialization was presented in 1918 in a lecture at the Collège de France entitled "La Physiologie Dite Supranormale et les Phénomènes

d'Idéoplastie" (Geley, 1918). In this work Geley showed a vitalistic biological and physiological approach to the phenomenon, arguing that supranormal physiology was not more mysterious than conventional physiology. Both depended on the same vital processes, both constructed biological matter, and both were affected by a directing idea that determined organic processes and ectoplasmic formations outside the body. They also had in common that they worked via an organic substance that manifested inside and outside the body. This substance, Geley stated, was shaped "by a superior dynamism that conditions it, and this dynamism is itself dependent on an Idea" (Geley, 1918, p. 22).



Figure 1. Gustave Geley.

Geley is described by Leon as "the soul of the great scientific movement of metapsychics" (p. 35), whose work nurtured the development of the movement. He became better known in metapsychic studies when he published articles in the *Bulletin de l'Institut Métapsychique International*, which later changed its title to the *Revue Métapsychique*. His fame also spread through articles in newspapers and via books that were translated into other languages, such as English: *From the Unconscious to the Conscious*, and *Clairvoyance and Materialisation* (Geley, 1919/1920, 1924/1927). Geley stated in a newspaper article that IMI's goal was to contribute to change occultism into science. In this work, he wrote, emphasis will be given to materializations because this phenomenon showed how ideas shaped matter, and "prove that thought is not a product of matter, but that, on the contrary, it is matter that depends on an idea" (Geley, 1919, p. 2).

The second chapter is a short overview about ectoplasm in the 1920s. Leon argued that several factors affected the development of research about ectoplasm in that decade. This includes opposition from different individuals, such as Catholic clergy, some spiritists, "metapsychic researchers from the English School," and traditional scientists (pp. 65–66). To this list I would add critics such as the famous magician Houdini (1924), and the journalist Paul Heuzé (1924).

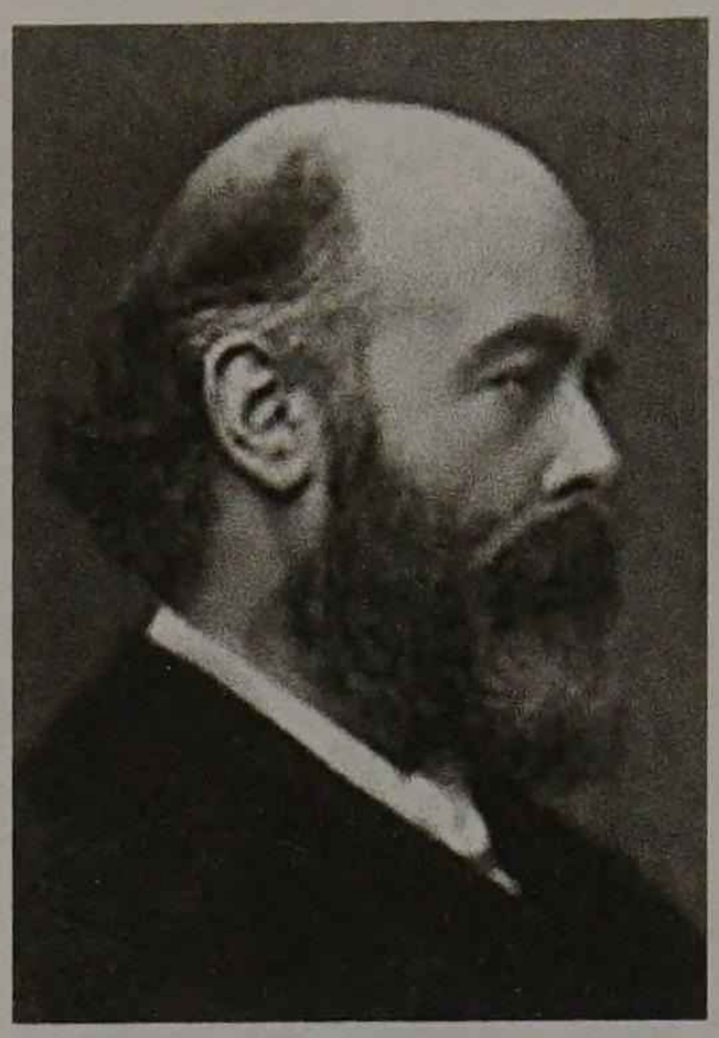


Figure 2. Oliver J. Lodge.

Of course, not everyone in England was negative about ectoplasm. For example, several short papers positive about the concept were published in 1921 in the London spiritualist publication *Light* by individuals such as William F. Barrett (1921), Stanley De Brath (1921), and Oliver J. Lodge (1921). The well-known physicist Oliver J. Lodge (Figure 2) stated in his article:

> All the evidence goes to show that it is an emanation from the medium, and that it returns to the medium's organism . . . The curious thing is that it not only gets moulded into organic semblances or temporary organic forms, it, or something associated with it, is able to exert considerable force. The material itself does not seem adapted to do this, and I conjecture that its main function is to nourish and maintain living connection with an ethereal projection of the organism, to which, and not to the superincumbent or permeating matter, the observed forces and energies are primarily due . . . (Lodge, 1921)[1]

The author does not give much detail about the researchers he mentions, several of whom are pre-1920s investigators. This includes short comments about William J. Crawford (p. 60) and Enrico Morselli (pp. 61–62), and others whom he only lists, such as William Crookes, Paul Gibier, Enrico Imoda, Pierre Lebiedzinski, and Albert F. von Schrenck-Notzing (p. 61). Many others could be mentioned as well.[2]

There are also three chapters about investigations conducted by Geley. One was devoted to Eva C. and includes the observations of Juliette Bisson, conducted before the founding of IMI. She stated in her book *Les phénomènes dits de materialisation* that: "The only certain fact seems to be this: The medium gives off a material substance that can be touched and seen" (Alexandre-Bisson, 1921, p. 308).

But the chapter also has sections about Geley's observations in 1917 and 1918 which include, in addition to amorphous ectoplasm, observations of small heads appearing in séances (Figure 3), and comments about the SPR's investigation of the medium. Leon also devotes various



Figure 3. Materialization of small heads in séances with Eva C. reported by Geley (1918).

pages to Geley's critique of the suspicion of fraud by the SPR committee, in particular the comments of Eric J. Dingwall. The fourth chapter is about the materializations of Franek Kluski. There is much here about the moulds of hands and feet produced in séances held in both Poland and France (Figure 4). It is important to add, something forgotten by some, that the use of moulds to document the existence of materializations was not new with Kluski. Less-sophisticated examples can be found in the nineteenth-century spiritualist literature (e.g., Adshead, 1879, pp. 34–39; Denton, 1875). Denton sat with a medium named Emma E. Weston and a cast of a hand was obtained that had fingers and half of the palm. It had a deformation that Denton believed identified it as coming from a deceased friend of his. He wrote that: "The lines of the skin are very perfect over nearly the entire surface; and even the impression of small hairs on the back of two of the fingers can be distinctly seen."

These moulds, considered by Geley to have no conventional explanation, became well-known not only in French psychical research and spiritualist publications, but elsewhere as well.[3] An article about the hands appeared in the *Scientific American*. Here Geley (1923) summarized for the American public the features of the phenomena,

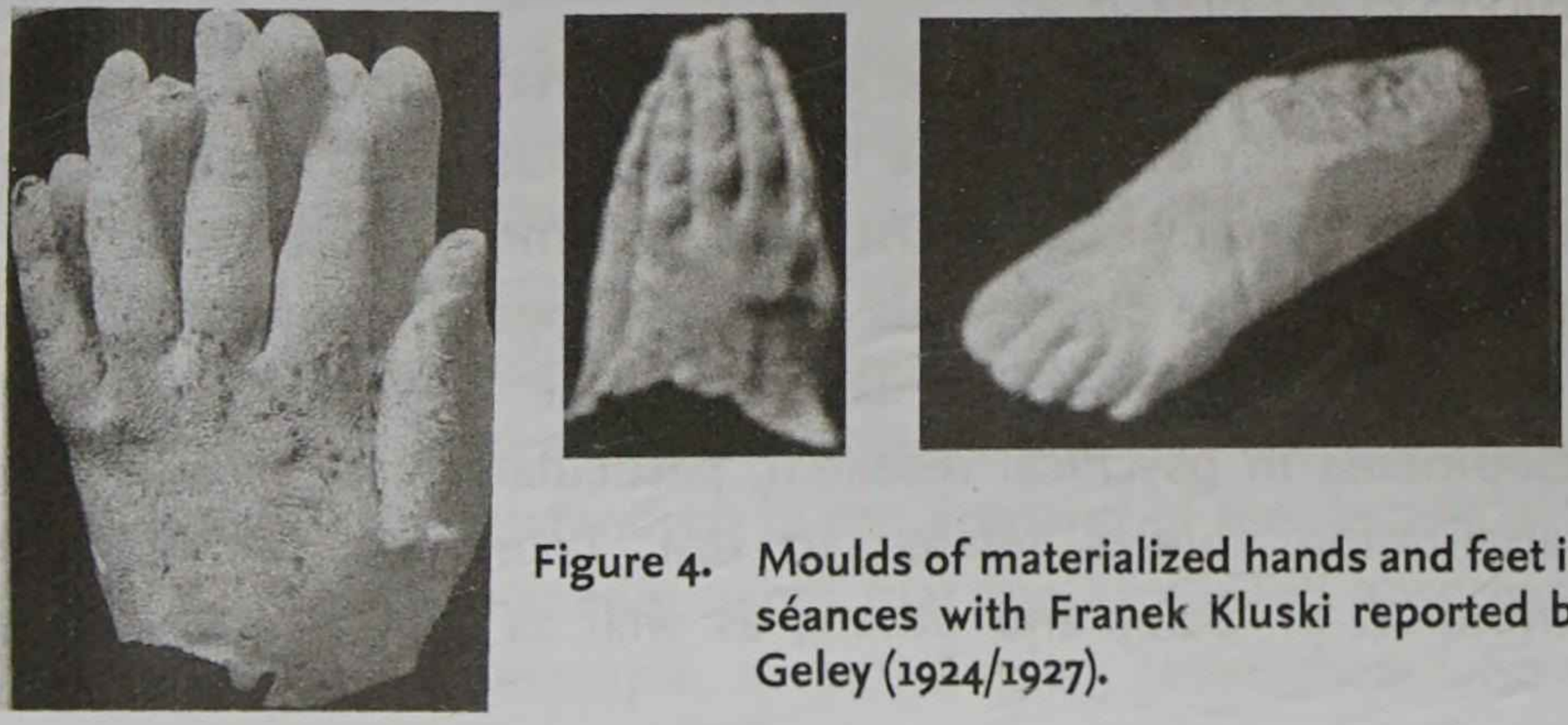


Figure 4. Moulds of materialized hands and feet in séances with Franek Kluski reported by Geley (1924/1927).

the precautions taken against fraud in the séances, and some theory. About the latter, also discussed elsewhere (e.g., Geley, 1919/1920, 1924/1927), Geley wrote:

> *What we know* as regards ectoplasmic forms is that the materialized organ is a temporary creation dependent upon the organism of the medium . . . *What we do not know* is from what directing idea the materialization proceeds . . . Logically and if we are faithful to scientific method, we ought to explain all ectoplasmic manifestations and materializations by *the exteriorization of a part of the organism of the medium in an amorphous state and by the subconscious ideo-plastic organization of the substance so externalized.* This is, of course, only a working hypothesis. It seems very narrow to cover the whole of the known facts, and possibly may very soon be found insufficient. But, of course, it is for the present the only hypothesis conformable to positive scientific method. (Geley, 1923, p. 374)

The fifth chapter is about medium Jean Guzik. Many pages are devoted to the phenomena observed with him. The discussion includes the famous Manifest of the 34, a report of individuals who got together to investigate Guzik using strict controls. The report transcended metapsychic circles because it was made public in a newspaper (Ageorges et al., 1923). In addition to Geley, there were several individuals signing the report who were not generally associated with metapsychics. But there were others who were involved with metapsychics, among them Camille Flammarion, Oliver Lodge, Eugène Osty, Charles Richet, Rocco Santoliquido, and René Sudre. The report affirmed the conviction of the signatories that the phenomena observed "are not explainable by illusions nor by individual or collective hallucination, nor by fraud" (Ageorges et al., 1923, p. 2).

The author points out that interest in materializations decreased in IMI after Geley died in 1924 in a plane crash, and the position of Director was filled by Eugène Osty. Osty was more interested in mental phenomena, and he directed his research program in that direction.[4]

Sessões de Ectoplasmia is a valuable contribution of past developments in psychical research, particularly those about French materialization studies related to IMI. Leon reminds us of the contribution of Geley and others, as well as of the existence of a

research specialty that has almost disappeared today, that of systematic studies of materialization like those conducted with Kluski. The author makes it clear that Geley and others had an empirical orientation, and that they took many measures to control for fraud.

In addition, and as mentioned above, Leon reminds us of differences in emphasis between English and French researchers, something that brings to mind interest in the history of science about the topic of national research traditions and styles (Kwa, 2005/2011). Leon argues for the existence of an English school focused on mental phenomena, and a French one focused on physical phenomena. To support this view, he analyzed unpublished correspondence, such as that between Santoliquido, Richet, and Lodge, and between Bisson and Schrenck-Notzing, as well as Geley's critiques of the SPR investigation of Eva C. The state of mind of SPR investigators, wrote the author, showed a tendency in the 1920s against physical mediumship, something the French could not understand. "For the French, ectoplasm was the main phenomenon of metapsychics" (p. 316), a phenomenon associated with the mystery of life.

This is consistent with Richet's critique of the SPR. He wrote in his *Traité de métapsychique* that while the SPR was more accepting of telepathy, when it came to physical phenomena they "demanded impossible proof, even when it is useless for demonstration" (Richet, 1922, p. 10). Such attitudes during the 1920s have been reviewed by Inglis (1984). But it is good to have Leon's reminder of this situation in connection with Geley and the IMI.

Some years ago, I analyzed the content of articles about mental and physical mediumship in the *Proceedings of the Society for Psychical Research* and in the *Revue Métapsychique* for the years 1920–1930 (Alvarado et al., 2006, p. 68). The first journal had a higher number of papers about mental instead of physical mediumship (75% vs. 25%), while mental mediumship was less covered in the French journal compared with physical mediumship (21% vs. 79%).

But we need to be careful about generalizing to all SPR-connected individuals, as well as to a whole country. There was not, as stated by Leon, a "common understanding in the England of the decade of the 1920s against objective mediumship that the . . . [SPR] absorbed at the time" (p. 147). For example, several English researchers who did



Figure 5. Felicia R. Scatcherd.

not represent the SPR—such as Barbara MacKenzie (1924), James Hewat McKenzie (1922), Harry Price (1925), and Felicia Scatcherd (1922)—investigated physical mediumship and defended the reality of the phenomena during the 1920s. Scatcherd (Figure 5), who deserves to be studied further, wrote in an essay:

> Ectoplasm was at first understood to indicate the substance exuding from a medium in visible form... It is now applied to any mode of mediumistic emanation which renders possible the various forms of physical phenomena (including psychic photography) from simple raps to partial or complete materialisations . . . (Scatcherd, 1924, p. 130) [5]

Of course, much more could be done to study approaches to mediumship (and other phenomena) in both countries, a topic beyond the scope of Leon's work. To study these tendencies of preferences for mental mediumship versus physical mediumship, it would be useful to follow the example of studies such as Hardwood's (1993) examinations of communities involved with genetics, and to focus on the institutional structure and goals of both the IMI and the SPR, as well as to conduct a prosopographical study of the researchers in both institutions that will illuminate the educational background of the active researchers in both institutions.[6]

Within the SPR, there were several figures deserving of attention during the 1920s, among them Theodore Besterman, Eric Dingwall, Oliver J. Lodge, J. G. Piddington, and Helen Salter. As argued in a recent study, Lodge's position about physical phenomena and physical ideas was well-known, and was not reductive solely to physical processes (Noakes, 2019). The attitude of the SPR, not necessarily a corporate one, may also be examined in relation to phenomena other than materializations. This could include other aspects of physical mediumship, among them spirit photography and telekinesis. In

addition, the issue may also be explored with psychical researchers in other countries such as Germany and the United States.

Leon wrote that Geley's procedure was "to observe the facts attentively, without bias, without preconceived ideas, without prejudice of any kind, observing them faithfully, recording them in such a way as to make all mistakes and distortions impossible" (p. 40). However, and not getting into the complex topic of objectivity in science (Padovani et al., 2015), we cannot ignore the fact that observations and data have to be interpreted by a person working within a network of needs, values, and beliefs. Before Geley started his work at IMI he had already committed himself to a non-material belief in the human mind, a mind independent of the nervous system, a belief that was inclusive of psychic phenomena (Geley, 1899). Furthermore, he had developed a biological view of materialization, and its dependence on the influence of an idea, thus helping to further develop the concept of ideoplasty with ectoplasmic forms (Geley, 1918; this is well-summarized by Leon, pp. 64–65).[7] Such a view certainly influenced his later work on the subject (e.g., Geley, 1924/1927) without diminishing his systematic empirical approach.

This book presents some bibliographical problems, among them incomplete references to articles in the *Revue Métapsychique*. Furthermore, there is practically no use of the secondary literature on the subject, something that would have helped the author to provide more background information, as well as relevant bibliography for his readers. This includes works such as Zofia Weaver's (2015) study of Franek Kluski. There are also works discussing French metapsychics that include research with materialization phenomena, and that show that there were many examples of negative views in France about IMI's work, particularly during Geley's time.[8] Some examples of reviews of French psychic studies are M. Brady Brower's (2010) *Unruly Spirits: The Science of Psychic Phenomena in Modern France*, Sophie Lachapelle's (2011) *Investigating the Supernatural: From Spiritism and Occultism to Psychical Research and Metapsychics in France, 1853–1931*, and Renaud Evrard's (2016) *Enquête sur 150 Ans de Parapsychologie: La Légende de L'esprit*. However, Leon made good use of archival materials from the IMI and reproduced some of them as appendices.

CONCLUDING REMARKS

The literature about past observations and ideas regarding materializations is sparse, especially when we consider books solely devoted to the topic, as opposed to works presenting only sections or chapters about it. For these reasons, as well as for the attention to detail and summaries of difficult-to-obtain literature, Antonio Leon's *Sessões de Ectoplasmia* will be of interest to current students of physical mediumship. It provides a useful window to past interest in these currently neglected mediumistic phenomena and will remind us about aspects of the social dynamics behind such research. One hopes that future studies of materializations will expand on this by including the important work of individuals from other countries, among them the work of German researcher Albert F. von Schrenck-Notzing, who argued that: "The telekinetic process and teleplastic phenomena are only different degrees of the same animistic process and ultimately depend on psychic processes in the subconscious sphere of the medium" (Schrenck-Notzing, 1920, p. 188).

NOTES

[1] See Lodge's earlier speculation (1894, pp. 326–327) about prolongations coming out of Eusapia Palladino's body.

[2] Other twentieth-century individuals who studied materializations include Eric Dingwall (1921, 1926), Thomas Glen Hamilton (1929), and Friedrich Schwab (1923). In the latter's view, teleplasm: (1) seems to come only from a medium; (2) emanates mainly from body orifices; (3) can disappear and is sensitive to light; (4) is white or gray, but sometimes appears in other colors; (5) "is fibrous and irregular, mostly one sees honeycomb-like very uneven large, often warped formations" (p. 59); and (6) its density varies.

[3] Geley's writings about Kluski attracted attention outside France, as seen in the United States (Geley, 1921/1922a) and Germany (Geley, 1921/1922b). There were also many summaries and comments in other languages, such as Italian (Il "Medium" Franek Kluski, 1922).

[4] Osty's career is reviewed by Evrard (2016, Chapter 8). I have briefly summarized some psychological aspects of Osty's ESP research (Alvarado, 2019b).

[5] Leon does not get into this, but we should be aware that negative SPR attitudes against physical phenomena preceded the 1920s, as seen in the writings of Frank Podmore and Eleanor Sidgwick (summarized by Gauld, 1968). Writing about the SPR before the 1920s, Rudolf Tischner stated that within "the English researchers' circles it was generally believed that, unless illusions played a role, everything must be attributed to fraud" (p. 223). We should also remember the strong pre-1920s interest in physical phenomena in France. This was evident in the strong magnetic tradition that came up to the twentieth century, as seen with figures such as Albert de Rochas (Alvarado, 2016), and in the interest in physical mediumship evident in the work of individuals such as Charles Richet and Joseph Maxwell (Evrard, 2016).

[6] On prosopography, see Clark (2003). Geley's training as a physician must have nurtured his biological and physiological approach, but perhaps most SPR figures had more philosophical–psychological education that promoted interest in mental phenomena. Although Geley was a leading figure, we need to explore the background of others in France, such as those serving in the committee overseeing the IMI (Leon presents information about Santoliquido and Meyer). Some, like Camille Flammarion and Charles Richet, had mixed interests (Evrard, 2016). It is important to remember that, for Geley, the implications of ectoplasm were not physical, but were about the supremacy of the power of the idea over matter, separating consciousness from physical limitations (Geley, 1919/1920), a topic well-summarized by Leon.

[7] On the influence of ideas on physical, biological, and psychological processes, see Bozzano's (1926–1927) discussion of the topic. In addition to materialization, he included hypnotic suggestion, thought-forms, psychic photography, and the way mediumship manifests in general. In Bozzano's view, depending much on Geley's ideas, thought and will were forces that shaped the physical and the mental worlds. This, he argued, combated materialism and promoted belief in both survival of death and a pantheistic conception of reality.

[8] A well-known example were the critiques of journalist Paul Heuzé (1924), who referred to metapsychics as a "science—if there is such a science—[that] has so far not deigned to consent to use scientific

methods" (Heuzé, 1924, p. 175). Perhaps in the future, Heuzé wrote, metapsychics will be able to prove its case, and then it would be considered "as one of the most interesting conquests of the human mind" (p. 175).

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BOOK REVIEW

Life Before Life: Is There Life Before Birth? 750 Cases of Hypnosis by Helen Wambach. White Crow Books, 2020. 182 pp. $17.99 (paperback). ISBN 978-1786771414.

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This book was originally published in 1979. This reviewer is not certain why it has been re-published in 2020—possibly because a very popular, relatively current, text by Dr. Jim Tucker (2008) of the same name has been on the market since 2008. Of course, a reprint of a past life research classic is always welcome. The current book by Helen Wambach being reviewed is also quite different from Tucker's and consists of a compilation of answers from individual questionnaires (750) filled out by participants who gathered for a group workshop in Chicago with Dr. Wambach. Nearly all of the data that Wambach analyzes were acquired though these questionnaires which were distributed to the participants of the workshop after they had undergone extensive hypnosis.

The book begins with an introduction where Wambach shares a bit of her history as a psychologist and researcher in the field of past life regression hypnosis. The reader must keep in mind she wrote this book in 1979 regardless of the 2020 copyright and the "new release" feel of the book. 1979 was more than 40 years ago, and quite a few advances have been made both in medical science—she mentions her interest in doing work in the area of biofeedback "so we can begin to relate specific EEG recordings with subjective phenomena experienced . . . " (Wambach, 2020, p. 7)—and in past life regression work. Although not a serious hindrance in assimilating the material she presents, it is something worth keeping in mind while reading *Life Before Life*.

Wambach then explains why she used Chicago as a source for her data. She wanted to see if the data she received from her previous work in California matched up in some way with the Chicago data, to dispel any possibility that the answers her participants gave her on her questionnaires were influenced by their geographical and cultural backgrounds. The first hypnotic induction she conducted was for the "birth trip." She describes how she also slips into trance while inducing her participants to follow her on the hypnogogic journey. Here is a bit of an example of the flavor of the experience:

> I would get a feeling that in a certain corner of the room someone was experiencing difficulty. I couldn't pinpoint exactly what this was, as I am not sure when I am experiencing telepathic communication from others. Like most of us, I require some kind of objective proof before I can accept telepathy as a fact. But still, I felt that in the right-hand corner of the room there was someone who was experiencing some anxiety. I sent a thought to this person that all would go well with them and they could trust me. (Wambach, 2020, p. 2)

This comment reminded me of Carl Jung's "transference grid," where not only do we convey information through our voice to the ears of whomever we are communicating with (conscious to conscious) but also in a variety of other ways, one being "unconscious to unconscious." Certainly we can apply this concept to any form of communication with any sentient being (or possibly non-sentient as well!). In hypnosis it would seem the "unconscious to unconscious" route would be more prominent, as well as conscious (hypnotist) to unconscious (subject) (Jung, 1946/1969, pp. 163–323).

After the hypnotic induction, Wambach asks her subjects a number of questions such as:

> Now I want you to go back into your memory and find a picture of yourself that was taken between the ages of six and twelve. Look closely at that picture. Where were you when that picture was taken? You will remember more and more details of that place where the picture was taken. (Wambach, 2020, p. 3)

Throughout this series of sessions with her subjects conducted in one long day, her own personal experience is recorded: different

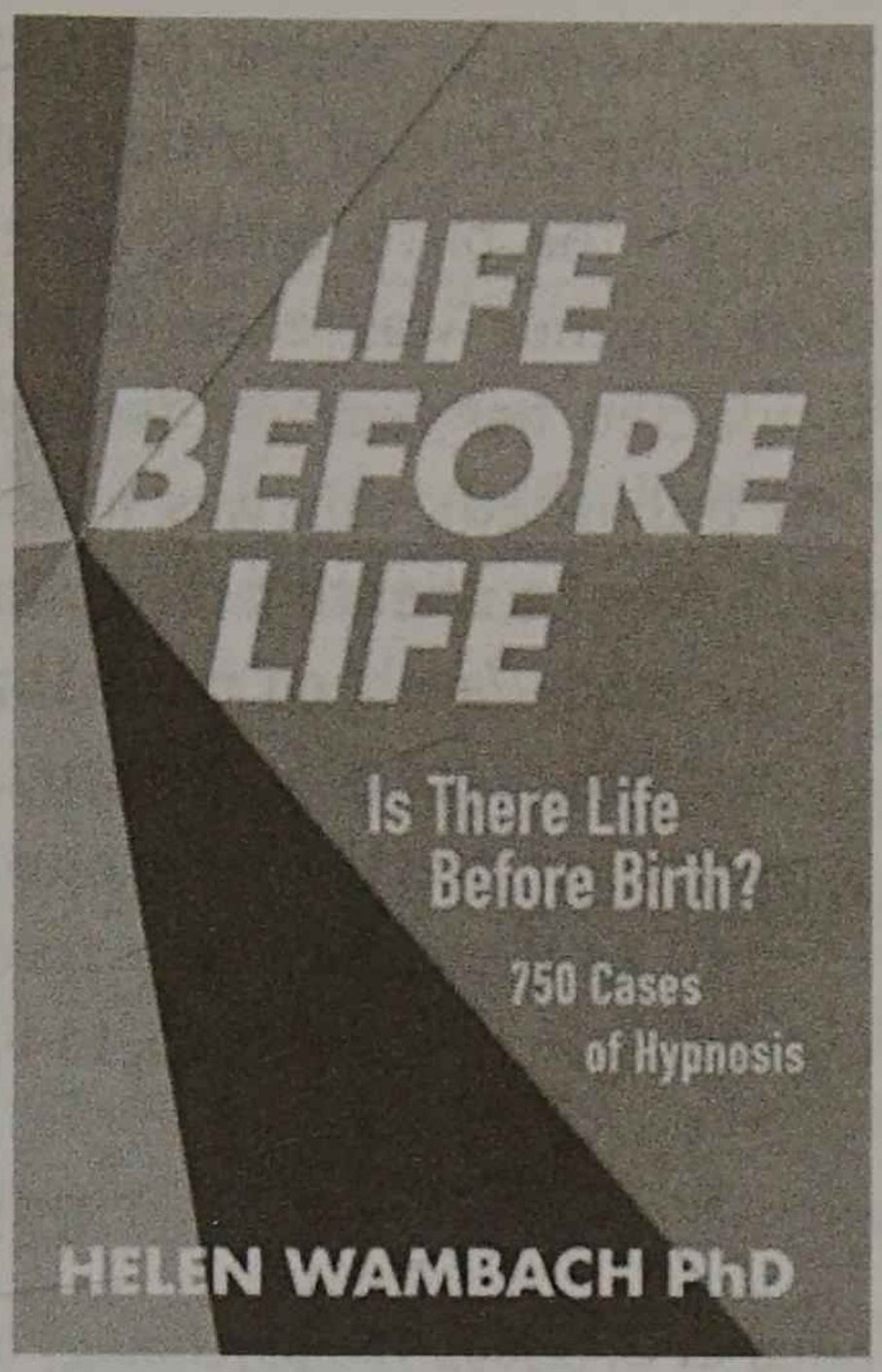
images that come up for her, some abstract and some relating to the questions she is asking, and they are interjected into the book. She ends up asking quite a few questions (dozens) including questions about her participants' choice of birth parents, partners, whether they wanted to be born or not, etc. When she is finished (after about four hours with her participants sitting on the floor the entire time), she takes them out of trance and has them all fill out a detailed questionnaire.

The next chapters detail the answers, her impressions of the answers, and an overall analysis of the session(s). It is again emphasized that her goal in conducting these sessions in the Midwest (Chicago) was to compare the results to her results in California where most of her previous work was performed. She says in the early part of this chapter that she did not see much difference between California and Chicago.

Essentially the rest of the book focuses on analyzing the answers in her questionnaire, moving from topic to topic, which are (as chapter headings): "Choosing to Live Again", "Choosing the Twentieth Century and Choosing One's Sex", "Why Are We Here on Earth? Have We Known Our Family and Friends in Other Lifetimes?", "When Does the Soul Enter the Fetus? Is the Soul of the Infant Aware of the Feelings of the Mother?", "Taking the Big Step. Getting Born", and "Adopted Children. Premature Births. Caesareans" (Wambach, 2020, p. v). These are fascinating questions, and the answers from her hundreds of participants are even more fascinating and intriguing.

Imagine a quantitative research article with many charts, with columns containing percentages of specific types of responses: sex of participant, age of participant, whether the participant has been under past life hypnosis before, etc. And then take all that data, and rather than keep it in "chart format" verbalize it all in a narrative. It is a qualitative presentation of a quantitative study. The book is not quite that sterile. Wambach does include her own personal interjections about her own life and other experiences she knows about from other participants that are relevant to a particular participant's personal response. She also allows for the subjective, heartfelt, responses to her questions, presented in the participant's own words, which a quantitative report would have little use for.

Wambach also makes much effort to straddle the fence between a serious quantitative analysis and a more qualitative approach by



inserting percentages throughout: "Only 11 percent of the sample reported being aware of being inside the fetus any time between conception and the six-month gestation period" (p. 92) and "When all the 750 cases were analyzed, 89 percent of all the subjects responding said that they did not become a part of the fetus or involved with the fetus until after six months of gestation" (p. 78). Although Wambach enjoyed giving percentages, she also, seemingly just as often, referred to ambiguous phrases such as *many participants* or *very few subjects* without any numerical indication as to what those descriptions meant.

The insight she gleans from her informal studies is quite fascinating. But the conveyance of information, essentially being in the same format throughout the book, does get a bit tedious (not, however, as tedious as pages and pages of charts!). If a reader is interested in Wambach's data and results from her participants' answers to her questionnaire (as described above), this book would be of some interest to them. Otherwise, to this reviewer, it consists of a lot of data, most of it rather subjective, without too much else to consolidate it into anything too compelling.

That said, Wambach's methodology was quite interesting. She framed her inquiry into a large number of participants' past lives by asking questions while they were under trance, and then seeking written answers from the participants in a questionnaire after they were out of trance. The questions were specific, and "active-imagination" focused, pertaining to specific times and personal actions—similar to: "I've asked you to visualize the house you lived in when you were four; if it is a brick house, what color is the brickwork?" "If you are making something, describe how you are creating what you are creating, and what tools you are using?" (These sorts of questions were asked while her subjects were hypnotized, but not, of course, answered verbally by

the participants at that time.) For another example, she asked details about the birth of participants, what they felt as they were going through the birth canal, were they frightened?, could they tell what their mother was experiencing? Considering the time period she conducted her research (sometime prior to 1979, when the book was first published), this was innovative research on a topic that was not considered a scientific discipline.

Helen Wambach more than likely was one of the first researchers in past life regression therapy to study large numbers of participants through her practice of group hypnosis. For that, her work should be revered as the results of a true pioneer attempting something that is exceedingly difficult to do, to link hard line science with metaphysics.

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