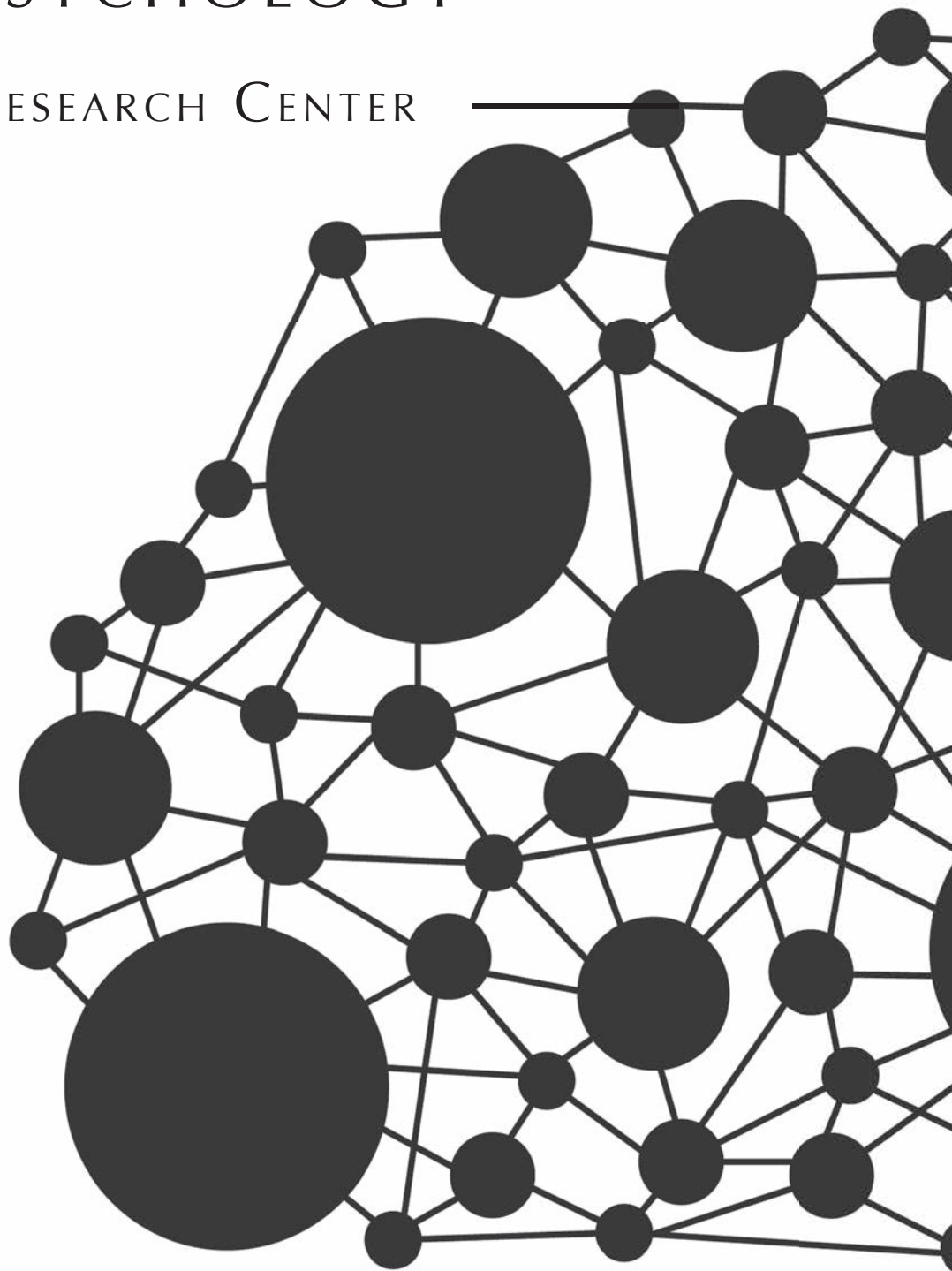


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EDITORIAL

HANSEL'S GHOST: RESURRECTION OF THE EXPERIMENTER FRAUD HYPOTHESIS IN PARAPSYCHOLOGY

By John Palmer

In his review of *Parapsychology: A Handbook for the 21st Century* in this issue of the *JP*, J. E. Kennedy (2016) comments favorably on a chapter by Douglas Stokes, a major theme of which is that a significant number of psi experiments were likely successful because of experimenter fraud. I had a far different reaction to Stokes' chapter, so much so that I decided to devote this editorial to the topic. It has turned out to be quite lengthy for an editorial, much longer than I thought it would be when I started writing it. To justify this loquacity, I cite the fact that it has been several issues since I wrote an editorial for the *JP*, so I'm just catching up.

Hansel (1966, 1980)

I was both surprised and troubled by Stokes' chapter. I was surprised because, as regular *JP* readers are aware, Stokes is a frequent critic of psi research, but this is the first time I had seen him make a push for the experimenter fraud hypothesis. Indeed, the hypothesis has been out of fashion at least since the early 1980s. Starting in the 1960s, fraud by either gifted psi subjects or the experimenters who tested them was the major line of attack by critics of parapsychology. This was due almost entirely to the psychologist (and magician) C. E. M. Hansel, who in effect (more on these two words later) accused one subject and two experimenters of using "tricks" to falsify the data in three ESP card guessing experiments back in the 1930s and 1940s (Hansel, 1966; 1980). At the time his first book came out, I was transitioning from undergraduate to graduate school and spent two summers at J. B. Rhine's parapsychology laboratory in North Carolina, where I became familiar with Hansel's attacks, and I was outraged by them. I was brought up to believe that a person is innocent until proven guilty, and that you don't accuse someone of a crime (which is what experimenter fraud is, whether or not one can be thrown in jail for it) without evidence that they did commit it, not just that they could have. To this day I have no idea why, but I was later given an opportunity to express my anger in a review of Hansel's second book for *Contemporary Psychology*, which at the time was the book review journal of the American Psychological Association (Palmer, 1981). Of greater importance (to me at least), my anger at Hansel was one of the reasons I decided to burn my bridges in mainstream academia and become a parapsychologist.

A noteworthy feature of Hansel's polemic—which in my view makes his conduct more despicable, not less—is that he does not explicitly accuse his targets of fraud. He claims only that fraud was possible, from which he further concludes that the experiments do not provide conclusive proof of psi. This is an extraordinarily weak claim, especially for someone like me (and I strongly suspect most scientists) who recognize that evidence is a matter of degree. (It reminds me of the claims made by the tobacco companies that smoking has not been conclusively shown to cause lung cancer.) So what was all the fuss about? The "trick" was to word the text in such a way that the qualifying remarks did not register with all but the most careful reader. I'd bet that if you removed the qualifying sentences from the text and asked readers what Hansel's conclusion was about a particular experiment, any reader would answer quite confidently that Hansel was claiming that the significant results were due to fraud. That clearly was the premise of both sides in the exchanges between Hansel and the defenders of the research in the journals, and Hansel never wrote anything to correct the "misperception." However, the fact remains that all Hansel can legitimately

be accused of is *insinuation* of fraud. Why did he do it this way? My guess is to avoid a possible lawsuit for defamation or libel.

In my opinion what ended the Hansel era of criticism was an article by psychologist Ray Hyman in *Skeptical Inquirer*, the main outlet for external psi-skeptics during that period. Hyman by this time had replaced Hansel as the leading critic of experimental parapsychology. Hansel had just come out with his second (and last) book on parapsychology (Hansel, 1980), which was essentially an update of Hansel (1966). The main new feature was an application of another hypothetical fraud scenario to physicist Helmut Schmidt, a prominent parapsychologist and the father of modern random number generator (RNG) research. Importantly, and unlike Hansel's previous targets, Schmidt was currently active in the field. That was too much for Hyman, who excoriated his fellow psi-skeptic for his use of experimenter fraud allegations: "The parapsychologists, of course, see Hansel's position for what it is—a dogmatism that is immune to falsification" (Hyman, 1981/1989, p. 294). Since then, what both sides generally have thought is needed to make the case for psi has shifted from Hansel's "fraud-proof" crucial experiments (FPCEs) with gifted participants to the collective significance of multiple experiments, mostly testing unselected participants, judged for methodological adequacy by the standards applied to comparable psychology experiments. Hyman has contributed to this kind of criticism himself, including in the paper I quoted above. This collective significance is determined by meta-analysis, which also demonstrates (badly, in my view) the parallel criterion of replicability. In short, the benchmark has changed from FPCEs to meta-analyses. Unfortunately, there is reason for concern that the Dark Ages are returning.

The Ghost(s)

Turning now to the resurrection, I focus on Stokes' chapter and a companion piece by Bierman, Spottiswoode, and Bijl (BSB; 2016), which provides a more technically sophisticated version of the kind of analysis around which Stokes builds his case for experimenter fraud. One notable difference between the first and second incarnations is that Hansel was a parapsychology outsider, whereas Stokes, Dick Bierman, and James Spottiswoode are all parapsychology insiders; I know nothing about Aron Bijl. Stokes' embrace of the experimenter fraud hypothesis is perhaps understandable, as he, along with Kennedy and a few others, were instrumental in uncovering the faking of data by Walter Levy at Rhine's laboratory (Rhine, 1974), one of only two adequately documented cases of this most serious type of fraud in the history of experimental parapsychology. I consider them heroes for detecting and verifying the fraud, bringing it to Rhine's attention, and ending Levy's career as a parapsychologist.

One thing I like about both the Stokes and BSB papers compared to Hansel's writings is that we are spared the latter's elaborate cheating scenarios. Second, in the Stokes paper no potentially guilty researchers are referred to by name. The situation is more complicated re the BSB paper, a matter I address in detail later. What troubles me about both papers is that they legitimate Hansel's practice of insinuating or accusing experimenters of fraud without evidence of fraud. It's a relatively small further step to start naming names.

Stokes (2015)

Stokes offers up what might be called a "thought experiment" in which he arbitrarily divides the results of 100 psi experiments into four produced by fraud (all significant at just the .01 level), 80 produced by investigators guilty of publication bias, that is, who fail to publish their nonsignificant experiments (all significant at just .05), and 16 single psi experiments with nonsignificant results (median psi score of 0). The mean expected z score of the 100 studies is ca. 0.757, which happens to be in the neighborhood of a corresponding value of 0.570 from a meta-analysis of 72 forced-choice ESP studies (Storm, Tressoldi, & Di Risio, 2012), which we are assured is typical of the values from numerous other meta-analyses that could have been chosen. We are apparently supposed to see this as evidence that the significance of the entire experimental parapsychology database can be attributed to the processes in the model, and fraud is clearly meant to be a necessary component.

In order to give Stokes' thought experiment more relevance to the real world, I will assume that the numbers of experiments in each category were at least inspired by the results of an anonymous survey emailed to 5,964 academic psychologists at major US universities by John, Lowenstein, and Prelec (2012). Based on only a 36% return rate, they estimated "that approximately 10% [precisely, 9%] of research psychologists have introduced false data into the scientific record [at least once] and that a majority ... have engaged in questionable [research] practices [QRPs]..." (Stokes, 2015, p. 45). The estimated *prevalence* of each QRP was the geometric mean of (a) the percentage of respondents who admitted they had engaged in the QRP, (b) the average of the percentage of other psychologists they thought had engaged in the QRP, and (c) the average of the percentage of these other psychologists they thought would admit to the QRP. We are offered no explanation of why we should consider the respondents' last two estimates anything other than wild guesses, or to put it another way, why we should take John et al.'s prevalence estimates seriously.

In any event, Stokes' 4% of *experiments* is a reasonable equivalent to John et al.'s 9% of *experimenters*, because it is likely that the fraudulent experimenters published at least as many successful nonfraudulent experiments as fraudulent ones. To be fair, it is this and related studies, rather than his own analysis, that Stokes says converted him to psi-skeptic. I will go further and suggest that it is the John et al. study, combined with the difficulty psi-skeptics have had in explaining away by more conventional means the successful psi experiments published in a prominent mainstream psychology journal by the prominent social psychologist Daryl Bem (2011), that is responsible for the recent resurrection of the experimenter fraud hypothesis in parapsychology.

So we now have argument by analogy, which immediately raises the question, how good is the analogy? Chris Roe, current president of the Parapsychological Association, shares my alarm about the resurrection of the experimenter fraud hypothesis enough to publish an article on the subject in the Association's magazine (Roe, 2016). He argues that on a priori grounds one would expect fraud to be much less likely in parapsychology than in mainstream fields. Compared to the mainstream, successful research in parapsychology brings much less tangible reward in terms of salary and career advancement; in fact, in the U.S. at least, your chances of getting a job at a reputable university with parapsychology on your résumé are close to nil. Although you will have a good reputation among your fellow parapsychologists, it will be just the opposite where it counts, among mainstream scientists. Your research will be more carefully scrutinized, and if you are caught cheating, the psi-skeptics will make sure the whole world knows about it. BSB acknowledge the scrutiny point, but they single out parapsychologists as being driven to defend "a non-materialist or spiritualist worldview" (p. 14). Although most mainstream scientists aren't driven to defend the materialist worldview (in their mind there's no need to), many are driven to defend lower-level theories they identify with. One group of mainstream scientists that does see a need to defend their worldview is the psi-skeptics, and they do so at least as passionately as psi-proponents. In short, by this criterion it's at best a tie.

However, the most important point to make is that evidential claims based on analogy are inherently much weaker than claims based on empirical evidence. Indeed, Stokes' thought experiment is about as far from a "smoking gun" as one can get. Fraud is a serious charge, and if a prosecuting attorney presented anything close to Stokes' analysis as evidence of guilt it would be laughed out of court. But even if we dignify Stokes' thought experiment as a scientific test of a scientific hypothesis, it fails on those terms. For a finding to support a theory, it must not only follow from that theory, but it also must *not* follow from a straightforward application of the competing theory (i.e., without the addition of gratuitous corollaries). In this case, it is obvious that Stokes' result could be explained just as easily by genuine psi as by fraud and publication bias, so its evidential value is nil—unless one follows Hansel (1966) by arguing that any other hypothesis should be chosen over psi on a priori grounds, the logic that prompted Hyman (1981/1989) to declare Hansel's position unfalsifiable in the quote above.

Bierman et al. (2016)

BSB analyzed six methodological flaws, which, following John et al. (2012), they call "questionable

research practices.” Two of these, “fraud” (“deception”) and “publication bias,” are the ones Stokes had in his model. The four new ones all concern misclassification of participants as in or not in the formal sample: “confirmation to pilot,” “pilot to confirmation,” “optional stopping,” “optimal extension,” and “biased removal of Ss [participants].” The criterion sample was a combination of 102 ganzfeld studies from two meta-analyses (Storm & Ertel, 2001; Storm, Tressoldi, and Di Risio, 2010), cut to 78 by removing studies published prior to 1985. Whereas Stokes started with a single model and tried to find a criterion sample that fit it, BSB started with a single criterion sample and tried to find the model that fit it best. The goals of the analyses also were different. All Stokes asked of his model was to predict the average effect size in the criterion sample. BSB had the more ambitious goal of seeing if they could explain away all the statistical significance of the combined studies in their sample as due to the QRPs. They didn’t quite get there, but they came close.

The BSB analysis is an improvement over the Stokes analysis in that the inductions from John et al. (2012) are explicit and reasonably well-defined. But in contrast to those of Stokes, BSB’s conclusions about the proportions of QRP studies in their models were also influenced by their judgments of whether QRPs were committed in the individual experiments in the database. If these were straightforward evaluations, I would have endorsed this procedure as an unqualified improvement over Stokes’; but instead they give us another layer of inferences—inferences that are more invalid (and more unethical) than the inferences from John et al.

Are the QRPs fraudulent? The only QRP besides “fraud” that BSB explicitly label as fraudulent is biased removal of participants, but only in some cases: “Removing a larger percentage than 5% of subjects with misses ... will make the post hoc arguments for removal that an experimenter has to come up with increasingly artificial and will basically turn this practice into fraud ...” (p. 20).¹ It is unclear whether only the “fraudulent” cases were included in the model.

As for the other QRPs, it is important to note that what makes a QRP fraudulent is not its nature but its intent. Take for example an (incredibly bad) experimenter who has a (fake) psychic guess a sequence of cards exposed one by one on a table he is seated at, while wearing a blindfold. The psychic gets a high score. If the experimenter is totally naïve about magic tricks and thought that the psychic couldn’t see the cards (e.g., by looking through the small gaps along the sides of the nose) we would classify the artifact as “sensory cues.” If, on the other hand, the experimenter knew the psychic could see “through” the blindfold, went ahead anyway, and failed to mention the artifact in the report, we would call it experimenter “fraud.”

Thus, whether we can say that BSB’s claim that these other QRPs are fraudulent depends on whether they are claiming the experimenter knew it was wrong. I will argue that for at least one QRP, optional stopping, BSB assume that at least the vast majority of the investigators in their sample knew it is wrong, based on the statement that “...students are generally taught that this particular flexibility in experimental practice gives misleadingly inflated scores” (p. 12). Second, it is important to note that optional stopping can only bias the data if the investigator is keeping track of the results and stops because he/she knows that stopping at this point will produce a favorable outcome. That this was assumed to be the case is evidenced by the following statement, which was cited in reference to another QRP but in fact applies to all the misclassification QRPs: “... we choose in our model for this QRP that the researcher starts contemplating early stopping and checking whether the *p*-value is smaller than 0.05 at trial 10” (p. 32). This implies intent, hence fraud. But what if the investigator is not keeping track of the results? I purposely keep myself blinded to how well an experiment is going because I am afraid knowing this might affect how I interact with future participants. The point is that my stopping a study at a certain point absent knowledge of previous scores has equal chances of helping and hurting the final outcome. In fact, one of the reasons I keep myself blinded is so I can stop a study prematurely if I want to or need to for some legitimate reason. BSB recognize this: “If subject removal happens blindly before inspection of the data, then the practice will not introduce a bias” (p. 19). The problem is with the immediately following sentence, which appears to be the basis for

¹ The online article has no page numbers. This and subsequent page numbers are from the preprint Bierman sent me, which is 33 pages up to the references. The page numbers should give the reader a rough idea where to find the quotes in the online article. *PLoS ONE* is open access.

inferring that all the optional stopping cases in the database are genuine QRPs: “However in GF research experimenters are generally not blind to the outcome of a session” (p. 19).

Are the “QRPs” QRPs? What disturbs me most about the BSB paper are the unjustified inferences that a QRP was committed at all. If BSB were interested in whether the “QRPs” were *really* QRPs they would have (a) carefully read the Method sections of the reports, and (b) if it was still unclear whether the procedure was defensible, contact the author. Many of the authors in the list of studies are currently active and accessible. Finally, (c) they would have removed the QRP tag from the experiment if an adequate justification was presented. There is nothing in the article to suggest that BSB did either (b) or (c); if they did, they certainly would have mentioned it. Of course, following this path would have reduced the percentage of studies identified as QRP studies, and adjusting the parameters of their model accordingly would have reduced the likelihood of explaining away all the significance in the ganzfeld database.

Optional stopping. I was most troubled by BSB’s treatment of two of the QRPs. The first is optional stopping, the presence of which they infer from “publication of a round number of trials” (p. 13). This is doubly egregious: not only can’t you infer that the stopping was intentional (as discussed above); you can’t even infer that it was optional (i.e., not specified in advance). There are many “innocent” reasons for a non-round number of participants. Although this round number criterion was mentioned in a paragraph devoted to optional stopping, it applies equally to the other misclassification QRPs, so I assume it was used for these as well. My use of the list of studies was complicated by two other deficiencies in the report: (a) “Round number” is never adequately defined; some guidance is given by the statement “ $N = 10, 20, 30$ etc. with frequencies around 10” (p.13), but it’s still not clear if all numbers not ending in 0 are “nonround”; I think most people would consider 25 to be round in the sense BSB seem to mean. (b) The numbers of studies in each QRP category are not listed for any of the models.

Anyway, I went to the list to try to identify the misclassification studies based on whether the sample size looked odd to me, and to my astonishment I found one that had me listed as an author (Kanthamani & Palmer, 1993)!² Although Kanthamani was listed as first author, the study was primarily mine, and I was listed as first author in the earlier version presented at a Parapsychological Association convention (Palmer & Kanthamani, 1990). I was the one who made all the important decisions pertaining to the analyses. I was puzzled myself why N was 22, as that is not a number I would ordinarily prespecify, so I raced to grab a copy of the report, where I found the following paragraph in the “Subjects” section under “Method”:

We tested more than 20 Ps [percipients – they brought their own senders] because two pairs were able to complete both sessions after we had assumed they could not, and we thought it best to fulfill our commitment to test them. No other factors influenced our decision to test 22 Ps. None of the mentation reports were judged [outside judges only] or ESP scores computed until all subjects had been tested. (Kanthamani & Palmer, 1993, p. 244)

In retrospect it would have been better to use “test and analyze” in the penultimate sentence but I think it is clear from the context, especially the last sentence, that this is what was meant. More importantly, the paragraph makes it clear that I was aware that optional extension could be a QRP, and I took steps to do the procedure in a way that would not bias the results. I can’t be 100% certain that my study was classified as a QRP study, but I consider it extremely likely that it was. If so, I see two possibilities, both of which are damning to BSB: (a) They didn’t care if the procedure was justified so they didn’t read past the abstract, or (b) they read the passage quoted above but still classified the study as a QRP, which is tantamount to accusing me of lying. All this was inferred from an irregular sample size!

To see if I could get an example of an investigator specifying a nonround number of participants at the outset, I chose an experiment by Roe, as I was planning to send him a draft of the editorial and could at the same time ask him for an explanation of the nonround sample size if necessary. The example I chose was listed as “Roe...Sherwood...and Holt ... 2004” with an N of 17. When I looked at the list a bit more closely I discovered a second study with the same designation except that N was 23. I was able to locate the

²Technically, this study should not even have been in the database. When Storm et al. (2010) homogenized their database, reducing it from 108 studies to the 102 that BSB drew from, this was one of the studies they removed.

report (Roe, Sherwood, & Holt, 2004), where I found that the total N was in fact a nice round 40. What BSB had done was to treat the sender condition ($N = 23$) and the no-sender condition ($N = 17$) as separate experiments. Nowhere in the report is this apparent departure from standard procedure mentioned (as it should have been), nor is there any mention of such in the report of the source meta-analysis (Storm et al., 2010). The statistical test for the entire sample was given in the report of the experiment (Roe et al., 2004), so it was not necessary for BSB to split the sample. What BSB got from the split was two more studies to place in the QRP bin, whereas they would have gotten none had they treated the study the same as all the others in the database; I could find no other examples of a split in the list of studies. Actually, this splitting should have been applied to all studies in which the sample was divided into two subsamples. I very much doubt Roe et al. (2004) was the only one. If most of these other studies had round subsample N s, BSB introduced a major source of bias into their models.

The question, then, becomes why the condition N s weren't split 20-20. Roe explained that immediately before the session the computer made an open-deck random decision (which does not guarantee an even split) as to whether to assign the pair to the sender or no-sender condition. The purpose was to "ensure that no-one but the nominal sender themselves was aware of the condition type until all data were collected and recorded" (C. Roe, personal communication, June 12, 2016). He also noted that computer assignment of the condition designations was mentioned in the report. Finally, this exercise suggested to me that BSB were in fact perusing the Participants sections of the reports, which increases the likelihood that the "lying" interpretation of BSB's conclusion re the Kanthamani and Palmer (1993) experiment is the correct one (see above).

Fraud. The second QRP treatment that concerned me, unsurprisingly, is the one labeled "fraud."³ Here is what BSB say they did:

We decided on the basis of the arguments given above [presumably the proportion of fraudulent researchers in the John et al. (2012) survey and the documented cases of fraud in parapsychology] that *one senior researcher* in the 80 studies post-1985 database might be guilty of deception. In order to take into account the contribution of deceptive research to the database we thus removed the two studies of *one senior researcher*, the person who had been implicated in errors in the randomization procedures. These studies were quite significant with HRs [hit rates] of 35% and 41%. In the database there are 29 principal investigators, so removal of one of them (3.4%) is near identical to the prevalence of deception found by JLP (4.4%). (Bierman et al., 2016, p. 21; italics added)

It looks on the surface that they are accusing one researcher of fraud, but a careful reading of the above quote paints a more complicated (and confusing) picture. First, exactly how was the experimenter selected for this dubious honor? It appears that one criterion is that the person fit the requirement of the model (which is outrageous from the ethical standpoint). Assuming we are talking about first authors, (a) the researcher had to be someone who had at least two experiments in the database and (b) both of these experiments had to have high HRs (Why else would they cheat?) As I was able to access the list of experiments, which included their HRs, I was able to identify the target by matching the HRs in the list to those in the quote. (For obvious reasons, I will not name this person, who is still active in the field.) I noted two relevant items. First, this author had more than two studies in the database. Second, there were other researchers who also had two high effect size studies. So how were the two target studies chosen? This presumably is where the randomization comes in. It would be plausible to conclude that BSB read through the candidate reports and chose the two studies with the most questionable methodology, which in this case happened to involve randomization.

There are two details about this paragraph that raise questions about what is really going on. First, it's hard to understand why BSB removed a QRP study from the database. My understanding of the procedure was that inferences from the database would take precedence over inferences from John et al. (2016) if

³ The transgressions subsumed under this heading and those labeled as fraud by Hansel and Stokes seem more egregious than the others, so I suggest that they be called "hard fraud" and that those such as publication bias and the misclassifications be called "soft fraud."

the two estimated percentages differed. Also, as I noted above, elsewhere it seems that BSB were going out of their way to add QRP studies to their models. At least this move does make me feel a little better about their objectivity vis-à-vis the models (but not the insinuations/allegations of fraud). This is just one more example of how poorly written this paper is with respect to communicating what was done.

Second, how many researchers are we talking about? Note in the quote that the person referred to in the first sentence and the person referred to in the second (and third) sentences are both labeled “one senior researcher.” However, different things are being said about them, so they are not necessarily the same person. You would expect the authors to recognize the juxtaposition and to clarify things by, the second time, using “this researcher” if the researchers are the same and “a different researcher” if they are different. So either this is just another example of inept writing or the authors intended to create ambiguity in the mind of a reader careful enough to notice the problem.

Why might BSB want to obfuscate? At the beginning of this section, I wrote “*on the surface* they are accusing one researcher of fraud.” The qualifier is necessary, because the authors are very careful not to go that far. Note that they say senior researcher 1 “*might* be guilty of deception” and the transgressions of senior researcher 2 were “*errors* of randomization” even though they were *classified* as fraud. The authors are (apparently) covered regardless of whether there are two researchers or one, and the ambiguity about the number just adds another layer of confusion about who is being “accused” of what. So all I can claim is that the authors are *insinuating* fraud, following the lead of Hansel. I expressed the opinion in that earlier discussion that an insinuation is at least as reprehensible as an accusation, and BSB have even less grounds for it than Hansel did.

There were some curious goings-on that could possibly be interpreted as suggesting that someone was concerned about a lawsuit re the BSB article. We must assume that at the time the paper was accepted for publication nobody thought there was a problem because nowhere in the body of the paper are any proper names associated with the QRPs. But what if late in the game it dawned on someone, perhaps as the result of my inquiries, that there might be a problem if those names could be deduced from information available elsewhere, namely the list of studies in the supplementary information section? Of course, this is what I consulted to identify the “culprits” in my two examples. These remarks supply the context for what I present below. From here on I will stick to the facts and let readers draw or not draw their own conclusions from them. Perhaps Bierman will clarify the situation in his anticipated reply to this editorial in the next *JP*.

In late April I asked Bierman to e-mail me a copy of the full report of the analysis he had submitted for the Parapsychological Association convention. Instead, he sent me a copy of the paper that had been accepted for publication in the online open access journal *PLoS ONE*. This was fine, except for the fact that missing were the figures and three supplemental files that were identified but not included. One of the latter, labeled “database,” appeared to be the list I was particularly interested in. So I wrote him back asking him to send me the figures and supplements. He refused, without explanation, telling me instead that the article should soon appear in *PLoS ONE* and I should access the materials there (D. Bierman, personal communication, April 29, 2016). This struck me as odd, as it would seem to be a simple matter to just send the files as e-mail attachments. On or around May 6, I went to the *PLoS ONE* site and found that the paper had been uploaded just a couple days previously. However, when I clicked on the icons for the supplements I got an error message telling me they were not available. I assumed this was just a technical glitch due to the recency of the upload and it would soon be identified and resolved. Sure enough, when I tried a few days later I was able to download the supplements, and the database file was the list I expected. I assumed the fix was permanent so I didn’t print out the database file, as I was not ready to use it. But when I tried to access the file a week or so later I got the error message again. I wrote a letter of complaint to the journal office and promptly got a very nice reply including the files as attachments. But as of July 18, 2016 they were still not accessible on the website. The other curiosity is that the list, an Excel spreadsheet, listed the ganzfeld articles only by author last names and year. There were no journal names, nor volume and page numbers. The reason you supply such a list is so that readers can find and read the articles that were included in the analyses (cf., e.g., Storm et al., 2010). Why was this information omitted? I wrote back my contact at the journal office asking if there was a limit on the size of supplement files. She replied that there was, but it left ample room for BSB to supply complete references had they so desired.

Inferences from psychology. I will close this section with one additional observation about the BSB report. In the abstract, one finds the following statement: “Restricting the parameter space to ranges observed in studies of QRP occurrence, under the untested assumption that parapsychologists use comparable QRPs, the fit to the published Ganzfeld meta-analysis with no anomalous effect was poor” (p. 2). This seems to suggest that one of their models was an indirect test of how well the hit rate in the database could be predicted from the QRP prevalences in John et al. (2012), although I could find nothing in the body of the paper that either supports or refutes this interpretation. If my interpretation is correct, the quoted outcome is evidence against Stokes’ premise (granted by me) that the prevalence of QRPs should be the same in parapsychology as in psychology.

Conclusion

The core problem with both the Stokes and BSB analyses is quite simple: You can’t conclude anything reliable about the existence of fraud from inference in the absence of evidence. Insinuations or allegations of fraud are a serious matter and not something to play mathematical games with, especially when there is any chance that the target persons can be identified. Fraud must be detected, not inferred. This means that it has to be determined on a case-by-case basis, by reviewers looking for possible evidence of QRPs in the reports and following up with the author, and by lab associates who happen to notice irregularities in a colleague’s data (not by conducting a witch-hunt!) and following up on their observations, as Stokes and Kennedy did in the Levy affair (Rhine, 1974).

So far I have been concerned with how past fraud should and should not be determined. I turn next to how future fraud might be detected.

A Solution? The Heymans Project

Both Stokes and Bierman maintain that the way to address the experimenter fraud problem is to conduct experiments in a way that precludes it, partly by monitoring the activities of the experimenter. It is represented by an ambitious proposed series of multilab experiments to be arranged by the Heymans Anomalous Cognition Group (2016). Bierman is listed as one of three “core members” of the group, but he seems to be the de facto head of the operation (e.g., he is identified as the one to whom inquiries should be addressed). I first learned of the project when Bierman asked me to contribute to the development of a test (stage 1 of the project) to select participants that would maximize the likelihood of success for the stage 2 experiment, most likely a replication of one of Bem’s (2011) “notorious” precognition experiments, conducted in multiple labs all presumably headed by psi-proponents. If this experiment succeeded (provided evidence for psi) it would be repeated to see if the results would hold up if the labs were “adversarial,” which presumably means headed by psi-skeptics (stage 3).⁴

Will the Project Demonstrate Psi?

Before I explain why Bierman’s protocol cannot eliminate the possibility of fraud, I want to explain why I think it unlikely to demonstrate psi even if it could eliminate fraud. I have developed a crude formula that encapsulates what I believe is required for someone to demonstrate psi: $\text{psi strength (P)} = \text{psi ability (A)} \times \text{motivation to see psi demonstrated in the experiment (M)}$, or $P = AM$. There are four corollaries: (a) a P score below a certain threshold level means no psi, (b) the person must be psychologically involved in the experiment, (c) psi can manifest over long distances (see, e.g., McMahan & Bates, 1954), and (d) there need not be a conscious intention to use psi to influence the results; that is, it could be *implicit psi*, which has been demonstrated, for example, by Bem’s (2011) experiments. This model has led me to conclude that at least some of the psi in most psi experiments is caused by the investigators (“experimenter psi” or E-psi), because we have good reason to believe the successful ones have high scores on both A and M in my formula (Palmer & Millar, 2015).

⁴This description of the protocol is taken from Bierman’s letters to me, referenced below. A somewhat more detailed parallel description can be found on the website (Heymans Anomalous Cognition Group, 2016).

I believe this model also would hold true for Bierman's experiments. I think most psi-skeptics would agree that if there were to be genuine psi in Bierman's experiments, it most likely would be E-psi (see, e.g., Kennedy, 2016). But what about the psi-skeptics? I suspect that most of us think it unlikely that a psi-skeptic would have any psi ability. At least part of the reason, I suspect, is that we think psi-skeptics invariably haven't had psi experiences, because if they had they wouldn't be skeptics. Indeed, if you ask psi-skeptics, "Have you ever had a psi experience?" the answer is invariably "no." But that could be an artifact of the way the question is typically phrased. Psi-skeptics will interpret this question as asking whether they have had an experience that was caused by psi. Of course the answer is "no": they are skeptics. This is also likely to be the case for any phrasing that does not explicitly deny this meaning. But if you asked, "Have you ever had an experience that looks like psi on the surface, whether it is really psychic or not?" The honest answer to this question may well be "yes."

At this point, I will share my "theory" of what drives the skepticism of at least some of the strong psi-skeptics of the type appropriate for a role in stage 3 of the Heymans project. Here goes. They have had in the past one or more impressive "psi" experiences that they could not easily explain away. These experiences were and are deeply troubling to them, because they conflict with their view of the world, the validity of which is very important to them. Thus, they are strongly motivated to find a conventional interpretation of their experiences. Because they have scientific skills and training, and given the difficulty in explaining their experiences away on their own terms, they seek to fulfill their objective by applying their skills to prove that psi in general does not or cannot exist. Following this line of thought, we would expect such psi-skeptics to be fans of Bayesian statistics because it allows them to prove that the null hypothesis is false (no psi).

This "theory" is pure speculation; I have no evidence for it. However, I am not aware of any evidence against it, and personally I believe on plausibility grounds that it is true in some cases. I normally don't like to "psychoanalyze" other scientists in print, even if I don't name them. My reason for the exception is to make the point that, *if we assume that psi exists*, my speculation provides a plausible basis for suspecting that the psi-skeptics in the Heymans project, who would have high psi strength scores in my formula, would unconsciously and unintentionally exert a negative E-psi effect that would offset any positive psi exerted by the psi-proponent investigators and/or participants.

Will the Project Preclude Fraud?

The controls against fraud in the Heymans project are to automate the procedure as much as possible and "to have outside skeptics controlling our measures like real time uploading of data..." (D. Bierman, personal communication, February 3, 2016). I found it noteworthy that nowhere in his description did Bierman say anything about the need to control the psi-skeptics, who could cheat by embedding malware in the program during its creation (if they had a role in that) or manipulating the data in real time while they are doing the "controlling." So I sent Bierman an e-mail asking what he planned to do to control the psi-skeptics. He took my question seriously and politely responded that he did plan the controls I asked about, although he hadn't worked out the details (D. Bierman, personal communication, May 19, 2016). I find that encouraging, but we'll have to wait to see if he follows through.

Unfortunately, even if Bierman does follow through it wouldn't solve the problem, because you would need someone to monitor the person monitoring the controller to be sure that he/she did his/her job properly, but then you would need someone to monitor this person, and so on. In other words, there is an infinite regress. This is why it is impossible to eliminate the possibility of fraud entirely in the Heymans project. In theory you could come close by following the suggestion of Stokes (2015), that is, by at each stage of the process "having multiple parties observe the experiment" (p. 46), the idea presumably being the more pairs of eyes, the more likely it is that one of them will catch the fraud. The problem is that what's important is not quantity but quality, and whether the quality is adequate is hard to verify a priori. I am reminded of the debate parapsychologists had in the 1970s about whether magicians should be present when we test psychics such as Uri Geller. Being the rigorous sort I was firmly in the "yes" camp. However, when

it looked like we might actually do this, the deflating response we got from the (magician) psi-skeptics was “magicians fool each other all the time.” Good point, actually. Although for the Heymans project the relevant skills are computer skills rather than magic skills, the same argument applies.

This last alternative highlights my primary objection to the kind of solution Stokes and Bierman propose. It creates a climate of paranoia that can be highly toxic and adversely affect the conduct of research at multiple levels. No researchers are going to be comfortable conducting an experiment with people who probably think they are cheats “looking over their shoulder,” even from a distance. If I were an experimenter in the Heymans project, I would be scared that the psi-skeptic observer would implant malware into the software that would make me look like a cheat, and the psi-proponent chosen to monitor the psi-skeptic wouldn’t be skilled enough to detect the manipulation. If that sounds paranoid, that’s the point. The experimenter’s discomfort could be picked up by research participants, adversely affecting their task performance, and it could even degrade the experimenter’s ability to properly perform the steps of the procedure. However, the most damaging objection is broader in scope: Who would want to become a scientist if it means working in that kind of environment? Perhaps this is why we have not seen mainstream sciences adopt such procedures.

The Real Solution

Before presenting my solution, I must address the question of why we want a solution. I think most parapsychologists would agree that our goal is to be able to report successful experiments of such quality and integrity as to convince mainstream scientists that psi is real. I agree—with one caveat. I do not believe it is necessary to convince those within that population who are strong psi-skeptics, which most parapsychologists agree is a hopeless task. Although it may not seem this way because they are so “loud,” strong psi-skeptics are most likely a minority within the scientific community. If the “silent majority” of scientists with an open mind about psi’s existence were to read the evidence and thus be persuaded of psi’s reality, and that majority included a proportionate number of scientists of distinction, that would be sufficient to define the “corporate position” and get parapsychology the associated tangibles such as mainstream grants and faculty appointments. (The downside, ironically, is that mainstream scientists would likely rush into the field and sweep us old-timers aside.) In any case, this is why the solution need not incorporate procedures intended specifically to convince psi-skeptics.

There is no need for the fraud-proof crucial experiment (FPCE) approach to assure that the knowledge base is not contaminated by false data, because science for years has agreed on a different solution: independent replication. In accepting this solution, they implicitly reject the FPCE approach, because they recognize that there are simply too many unknowns in any single experiment to put much faith in it, even if it somehow could be shown to be fraud-proof. However, for replication to achieve the objective, the replicating experimenters must be the kind of people highly unlikely to commit fraud. This requires that they have no incentive to commit fraud, which in the case of parapsychology is best achieved by removing from consideration individuals biased either for or against the psi hypothesis. In other words, they should come from the same subpopulation of scientists whom I suggested above we need to convince that psi is real.

The idea has been kicking around for years in parapsychology that psi might be inherently unreplicable, and I heard Bierman endorse it in a workshop at the 2015 PA Convention. It is even “predicted” by a major physics-based theory in the field (Lucadou, 1995). I think there are too many examples of albeit insufficient replication in parapsychology for this to be at all likely, but if it’s true we’ll just have to face the fact that we will never be able to make a scientifically compelling case for psi. Trying to get around it by turning parapsychology into a paranoid science is a fool’s errand doomed to fail.

I want to end this negative essay on a relatively positive note by pointing out that the Heymans project actually fits the required mold rather well, because the multilab experiments are close-to-independent replications not only of one another but also of Bem’s (2011) original experiments.⁵ So, here is how I suggest Bierman modify the Heymans project if he would like to maximize the chances of providing evidence for psi convincing to the “silent majority” of mainstream scientists.

⁵ Given Bierman’s attitude toward replication, it is odd that he would propose a replication project.

1. Abandon stage 3. *Psi-skeptics should have absolutely nothing to do with the project.*
2. Select a minimum of five independent Principal Investigators (PIs) for the stage 2 experiments. They (along with the members of their research teams) would need to meet the following criteria: (a) the PIs should be senior psychologists who are widely respected by the community of scientists (probably, but not necessarily psychologists) for their scientific accomplishments and integrity; (b) they should believe that the evidence, insofar as they are aware of it, is insufficient to justify either a positive or negative conclusion about the reality of psi (although ideally they would have genuine curiosity about whether psi is real); (c) they should have no preference for a positive or negative outcome; (d) they should have no prior involvement with anything having to do with psi or related anomalies, including publications; and (e) the PIs should have no fear of the repercussions if they obtain positive results. If they do, they can expect to be publically excoriated by the psi-skeptics. Thus, they must be secure both internally (ego strength) and externally (job security). Perhaps the greatest challenge would be to assure that a researcher claiming neutrality really is neutral. That is clearly impossible in any strong sense, but the odds are better if the person chosen has an applied rather than a theoretical orientation; if someone with a theoretical orientation is chosen, the theory should not be grounded in physicalism. I'm not suggesting it would be easy to find these PIs, but it would help immensely if they could be offered grant money.
3. Arrange for a limited number of psi-conductive experimenters, and possibly carefully selected psychics with demonstrated psi ability, to be "psychologically involved" in the experiment in ways that can't compromise the experiment's integrity. The PIs should establish (long-distance) contact with them, keep them informed about the progress of the study, and even solicit advice about aspects of the procedure consistent with the protocol, such as how to interact with participants. They should definitely know when each session is to be conducted. In turn, these psi-proponents have the responsibility to create a good impression on the PIs and not be offended if their advice is not taken.
4. Last but not least, make sure that the protocol includes rigorous controls against all nonpsi artifacts except experimenter fraud.

Finally, the replications in this adaptation are more fully independent than in the Heymans project because the E-fraud monitors, which by necessity would have to be mostly the same for each experiment, are eliminated. It is true that the psi-proponents in the adaptation would also most likely not be independent, but their effective role would be more like that of a subject than an experimenter.

In short, mission accomplished.

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ABSTRACTS OF PRESENTED PAPERS FROM THE FIRST
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ASSOCIATION (59TH) AND THE SOCIETY FOR SCIENTIFIC
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JUNE 19–24, 2016—PA PAPERS ONLY

[*EDITOR'S NOTE*: ABSTRACTS ARE SLIGHTLY EDITED, MOSTLY TO CONFORM TO THE
JOURNAL'S STYLE.]

A REVIEW OF PSI ACTIVITY IN THE DNA

Bradley Y. Bartholomew

This paper presents a general summary of psi experiments conducted with the DNA in the latter decades of the 20th century, particularly at the HeartMath Institute (HMI) in Boulder Creek, CA, by Dr. Glen Rein, relating to the ability of “healers” to affect the conformation of the DNA molecule simply by concentrating their mental and emotional powers of the brain and heart. In particular it was found that the intentional states of healers could change the molecular structure of water which in turn could alter the conformation of the DNA. Further to these findings about the relationship between water and DNA, this paper discusses a journal article by a team led by 2008 Nobel Prize for Medicine recipient Dr. Luc Montagnier, which describes an electromagnetic field in the DNA that is capable of being projected into water and which contains all the information of its sequence of bases in memory and is capable of reconstructing itself into the identical chemical molecule, provided the ambient electromagnetic environment in the water is maintained at a frequency of 7 Hz, which is in the same frequency window as alpha rhythm brainwaves and the natural Schumann resonance of the earth. This paper goes on to discuss new research in optogenetics where human beings have been able to modify gene expression with their alpha rhythm and mu rhythm brainwaves in the same frequency window as the Schumann resonance, simply by adopting different mental states, and takes note of the fact that these same alpha and mu rhythms are the main conduit frequency for brain computer interfaces (BCIs). The paper presents a general summary of earlier research relating to psi phenomena resulting from interference of brainwaves with the Schumann resonance, and proposes a novel explanation for the recently discovered “mirror neurons” phenomena where the mu rhythm brainwaves emanating from the motor cortex are able to interfere constructively or destructively with the Schumann resonance of the same frequency and thus bring about mental telepathic as well as psychokinetic happenings.

EXPLORATORY STUDY OF PHYSIOLOGICAL CONNECTEDNESS
AMONG TWINS IN RELATION TO ATTACHMENT

Göran Brusewitz¹, Adrian Parker², David Luke¹, Annekatrin Puhle³, & Ross Friday¹

Parapsychological research into distant interactions between individuals seems to indicate that the bond between sender and receiver is important. The present study was designed to investigate the ostensible relationship between telepathy and attachment between twins. By applying the concept of attachment from developmental psychology, this becomes the second in our series of studies in parapsychology to explore the degree of attachment between twins as a potential dependent variable relating to their apparent telepathic connection. At the current stage of evaluating a new methodology, we report the design and findings

of this exploratory study. From an initial pool of 40 pairs of twins, six pairs of identical and one pair of non-identical twins were selected on the basis of their responses to the *Exceptional Experiences Questionnaire* and on the availability of both twins to take part in the study. The test procedure required each of them to alternate in the role of sender, in which they were exposed to a shock or surprise stimulus and in the role of receiver in which they were physiologically monitored for their electrodermal responses (EDR). Senders were presented with altogether five stimuli, one stimulus during each of the five trials per twin run. For each trial, the stimulus was presented during a 30 second period randomly chosen from eight possible such epochs within a four-minute trial. With a further 30 seconds added to establish baselines before and after each trial, this meant each trial would last five minutes. Graphs from 53 of the useable trials belonging to seven pairs of twins were analyzed by the lead researcher (GB), who was blind to the time epochs in which the stimuli had been presented by the researcher working with the senders (AP). The task for the lead researcher was to identify a peak of the graph in the receiver's reaction that might approximately correspond to the midpoint in the period that the stimulus had been presented to the sender. In 12 trials out of 53, these identifications corresponded to the actual exposure period for the shock or surprise stimulus—constituting so-called “hits,” compared to the MCE = 6.625 and was significant, $p = .043$ (one-tailed). Three out of these 12 correct placements were contributed by just one of the twins. The attachment data that twins contributed via the EEQ questionnaire indicated that all the twins in the current study seemed to experience similar high levels of attachment. This consistency in strong and close relationships meant of course there was a lack of variance as concerns the hypothesis that attachment would predict the scores. The results of the *Experiences in Close Relationships-Revised* (ECR) indicated that all the twins had very low scores on attachment-related anxiety and avoidance in their relationships. The profiles of twins having many hits in the telepathy experiment as regards any aspects of attachment were not significantly different from those of the others. The data provides justification for a major study using this methodology with selected pairs of twins. Some major improvements in the design were suggested. The synchronous monitoring of the electrodermal activity for the sender will later be carried out, giving possibilities to give a precise record of the timing and the effect of the shock stimuli.

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VISUAL CATEGORIZATION OF IMAGES OF FACIAL PHOTOGRAPHS BY INTUITIVE INDIVIDUALS

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Some individuals claim to be adept at gauging mortality based on a glance at a person's photograph. To test this claim, we invited 12 such individuals to see if they could determine if a person was alive or dead based solely on a brief examination of photos of faces. All photos used in the experiment were transformed into a uniform gray scale and then counterbalanced across eight categories: gender, age, gaze direction, glasses, head position, smile, hair color, and image resolution. Participants examined 404 photographs displayed on a computer monitor, one photo at a time, each shown for a maximum of 8 seconds. Half of the individuals in the photos were deceased, and half were alive at the time the experiment was conducted. Participants were asked to press a button if they thought the person in a photo was alive or deceased. Overall mean accuracy on this task was 53.8%, where 50% was expected by chance ($p < .004$, two-tailed). Statistically significant accuracy was independently obtained in five of the 12 participants. We also collected 32-channel electrophysiological recordings and observed a robust difference between images of deceased individuals correctly versus incorrectly classified in the early event related potential at 100 ms poststimulus onset. Our

results support claims of individuals who report that some as yet unknown features of the face predict mortality. The results are also compatible with claims about clairvoyance.

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SPIRITUALLY TRANSFORMATIVE EXPERIENCES OF CLAIMANT MEDIUMS

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This qualitative study was designed to establish a comprehensive understanding of the initial experience associated with the spiritual transformation process of *inexperienced claimant mediums*, commonly described as individuals who allegedly have regular communications with the deceased. Spiritually transformative experiences are commonly thought to be a type of transformation and expansion of consciousness. Often referred to as *psychic openings*, these experiences have occasionally been described as being startling or traumatic, sometimes creating a loss of contact with consensual reality that may possibly lead to psychiatric misdiagnosis in the individual's search for an understanding of the experience. Consequently, the desired outcome of this study was to establish a better understanding of the initial psychic opening and propose a more reasoned approach to its acknowledgement and development by the scientific community. A five-part review of the background literature in the field of study focused on a history of the practice of mediumship, the proposed psycho-spiritual emergence process, the child's perspective of his or her spiritual development process via an archival biographical analysis of the psychic opening, the subsequent development of the medium's purported abilities, and a review of recent process-oriented mediumship studies. Six participants for this study were selected from a group of certified research mediums prescreened for their abilities by the Windbridge Institute for Applied Research in Human Potential in Tucson, Arizona. A semistructured questionnaire in compliance with Saybrook Institutional Review Board oral history specifications was utilized as the primary research instrument to provide biographical accountability. However, additional written documentation of the experience was included when appropriate to further clarify the psychological impact of the psychic opening. A thematic analysis of the data revealed that the participants' transformative experiences consisted of a sequence of developmental experiences that included an encounter with a single or multiple spiritual entities that one may or may not consider as spirit guides. Depending upon the age of the participant and the existing social support system at the time of the initial experience, the spiritual encounter can be either fearfully traumatic or merely an anomalistic variation of the individual's concept of reality.

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THE DIALOGUED CAPTURE: ANALYSIS AND APPLICATION OF ROE & ROXBURGH'S HIERARCHICAL MODEL OF COLD READING STRATEGIES DURING PSYCHIC READINGS

Renaud Evrard

A surprisingly high proportion of the population has attended readings by professional "psychics" or "mediums," but this situation has received little attention from psychologists (Roe, 1995; Wooffitt, 2006). Some authors have considered parallels between the psychic reading and more orthodox forms of psychotherapy, even calling it a "poor man's psychoanalysis" (Miller, 1995). What could these situations have in common? An implicit consensus reduced the dialogue between the pseudopsychic and its client to a "cold reading" or a "dialogued capture," as coined by the French sociologist Bertrand Méheust (2004), which didn't involve any paranormal process of information acquisition. But these allusions to "cold reading" tend to be

vague and inconsistent, and such an application of the term will cause it to lose any explanatory power it has. According to psychologist Ray Hyman (1981), “The cold reading employs the dynamics of the dyadic relationship between psychic and client to develop a sketch that is tailored to the client. The reader employs shrewd observation, nonverbal and verbal feedback from the client, and the client’s active cooperation to create a description that the client is sure penetrates to the core of his or her psyche.” There are also clear indications that the cold reading “process” actually consists of a number of discrete and independent strategies. Psychologists Chris Roe and Elizabeth Roxburgh (2013) have developed a hierarchical model of all reading strategies based on the amount of interaction between the psychic and the sitter and the generality of the information. Their model gathers both declarative (Barnum statements, specific trivia, specific generalisations, pigeon holing, warm reading, fishings, “true” cold reading) and manipulative strategies (setting the stage, try-ons, Procrustean effect reinforcements, hot reading, misrecall induced by diversion, back-channel behaviors). The aim of our paper is to describe this model and its limits, to suggest some refinements, and to apply it through the dialogue analysis of a psychic reading. Hyman (1981) notes that although it is unlikely that the pseudopsychic reading will generate information that is truly new to the client, it may still have utility for him or her as a therapeutic, quasi-counselling event. As such, its analysis can help us better understand the development of an alliance in verbal psychotherapies.

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FIGHT AND FLIGHT: TOWARD A PSYCHODYNAMIC MODEL OF NEAR-DEATH EXPERIENCES

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Historically, NDEs have been the subject of several psychological or psychoanalytic interpretations. However, they are now in competition with neuroscientific and medical approaches. Since Heim’s first observations, NDEs were discussed in the general framework of clinical practice and trauma by philosophers such as Victor Egger and by psychoanalysts such as Oskar Pfister and Sandor Ferenczi. Surprisingly, until the work of Russell Noyes and his collaborators in the 1970s, the findings focused on elements that were no longer phenomenologically in line with Moody’s work. OBE, panoramic memory, elation, and transcendental elements were in fact narrated by a portion of the experiments; yet, a majority claimed to have first felt a form of hyperalertness and automatism and to have been capable of acrobatic and intellectual performances thereby facilitating their own life-saving rescue efforts. Heim’s own testimony describes such efforts. In an attempt to build a psychodynamic model to reconcile these different aspects without falling into a form of reductionism, we begin with a diachronic exploration of the literature regarding the rescue actions during NDE. This process indicates both the traditional description of passing into a disembodied consciousness while, simultaneously, encountering a new concept we call hyperembodied consciousness. Noyes et al. interpreted this process as an almost universal reaction to life-threatening danger and a basic adaptive pattern of the nervous system akin to Cannon’s fight-or-flight reactions. To integrate their ideas in a new psychodynamic model, the second part of this article discusses on a synchronic axis both old and new literature on NDEs, focusing on several conceptual issues: the psychological versus biological triggers of NDEs, the notion of *fear-death experiences*, the description of the disjunction between a disembodied and a hyperembodied consciousness; the complementarity of the processes of *focalization* (fight) and *distanciation* (light) through the contribution of the Bergsonian theory of the body-mind relationship; and the psychodynamic functions and nature of the survival scenario experienced through the distanciation. We propose to reconsider the NDEs as part of a more general process: an adaptive psychosomatic response to the perception of imminent death, while discussing a case of drowning.

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ANOMALOUS MECHANICAL EFFECT ON AN EXPERIMENTAL TABLE-TIPPING SÉANCE: A MULTIMODAL APPROACH

Juan Gimeno, Darío Burgo, & Alejandro Parra

Between June 2014 to December 2015, a number of table-tipping séances were organized in (city name). Up to five video cameras were installed to record the events. Various devices were assembled to measure physical, physiological, psychological, and environmental variables. Twenty-three meetings were held with a subject about whom anomalous movements (vertical and horizontal) of a table incidentally had been discovered in previous observations. The subject (AF) was presumably capable of moving a table at will through an alleged "PK force." The phenomena were documented and recorded on several occasions. Muscle effort was ruled out as the cause of the movements (unconscious or conscious fraud). A multimodal approach was used, including EEGs records, anomalous effects on the RNG, and other data analysis with the subject at rest and also during experimental table-tipping séances. The normal curve of an RNG deviated significantly ($p = .008$). No variations of electric and magnetic fields were found associated with the phenomena. Contactless movement of the table or other objects could not be achieved.

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MULTIPLE-ANALYSIS CORRELATION STUDY BETWEEN HUMAN PSYCHOLOGICAL VARIABLES AND BINARY RANDOM EVENTS

Hartmut Grote

Mind-matter interaction experiments have been progressing from targeting simple bias of random number generators to correlation studies between psychological and physical variables, carried out over multiple combinations of these.

This paper reports on a new correlation study between human intention and the output of a binary random number generator. The study comprises a total of 720,000 bits from 20 equal sessions each on a different human participant. Each participant spent 1 hour of time attempting to "influence" the outcome of the random number generator according to a preselected intention. During this time the participant was provided feedback on his/her performance by an analog mechanical display, with the needle of a galvanometric instrument moving to the left or right hand side of its current position, according to the instantaneous output of the random number generator. Psychological variables were obtained from the participants by a hardware dial before each individual run and by a questionnaire before the participant's first experimental session.

Three types of data analysis were defined and tested before looking at the data. The first analysis looks at the distribution of results from the participants. A former study of this kind had found a significant result for this type of analysis. The second analysis tests for correlations between psychological variables obtained before each run and physical variables of the corresponding subsequent run. The third analysis is a conceptual replication of von Lucadou's correlation matrix method. It consists of multiple correlation tests between psychological and physical variables arranged in a matrix, which also can be interpreted as a multiple-analysis technique.

The results of these analyses will be presented, putting this study into context with other experiments of this kind.

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EXPLORATIONS INTO USING BRAIN SIGNALS TO PREDICT RANDOM FUTURE CONDITIONS

Jacob Jolij¹ & Dick Bierman²

Three studies are reported using a face-detection task where faces are presented (or not) with a dynamic noise mask, while the participant's EEG was measured. Each participant was exposed to 240 stimuli either "a face buried in noise" or "just noise, no face."

Four templates were created by averaging the EEG before and after stimulus presentation for face and no-face conditions. These templates we called anticipatory-before-face template (AF), anticipatory-before-no-face template (ANF), similarly response-to-face (RF) and response-to-no-face (RNF) templates. These templates were used to classify a specific EEG signal as being associated with the face or no-face (random) condition.

When using the templates constructed from the EEG signals measured after stimulus presentation (RF and RNF) this classification had an accuracy of 80%. Using the EEG patterns before the stimulus condition (AF and ANF) resulted in an accuracy of ~53%, where 50% is expected by chance ($p < .001$).

In order to explain this apparent anomalous effect where we can use anticipatory signals to predict the random future stimulus condition, randomness tests were performed on the sequence of conditions that the subject had been exposed to. Only one subject had been exposed to a significantly nonrandom condition sequence. Also the digital filter that had been used was excluded to be the source of the anomaly. When using the trial number as a factor in the analyses there are no interactions with that factor and hence there are no significant inclines in accuracy within a subject. This suggests that no learning (of patterns) occurred.

In all three studies there is a positive correlation between the anticipatory accuracy and the response accuracy, suggesting that there is some form of symmetry. Further analyses of these symmetries are in progress in order to differentiate between retrocausal and forward causal triggered correlations.

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DEVELOPING AN EXPERIMENTAL METHODOLOGY FOR APPARENT EXCEPTIONAL PK PARTICIPANTS

John G. Kruth

Two PK study participants have been studied at the Rhine Research Center at different times over the past four years. Both participants had demonstrated effects in other research situations, and they were invited to the Rhine to explore the apparent effects in a controlled situation. In tightly controlled environments, these participants apparently demonstrated consistent PK activity. The methods used to examine these phenomena are described as well as some new observations about PK activity.

One participant was examined using an Egely Wheel in a very controlled situation. The Egely Wheel was tested in detail to determine the effects of physical forces including heat, magnetism, static, light, and air flow, and with the necessary shielding the Egely Wheel was determined to be a valid instrument for PK testing.

Using the Egely Wheel, an apparent consistent effect was observed in sessions where the participant was less than 12 inches from the wheel, but no effect was measured from a greater distance. This distance limitation may be a limitation of the effect, but it is also possible that the limits are due to the beliefs and expectations of this specific participant. The discovery of a rebound effect in these sessions appears to

be an extension of the linger effect observed in earlier healing studies.

Participant number two apparently produced large effects on electronic devices from a distance. These effects were verified in a number of sessions with different devices. One series involving the Psyleron random event generator showed inconsistent results. Another series using a customized electronic device designed to detect electrical fields showed a clear effect on the device from distances of up to 4 meters. A third series utilized a photomultiplier tube that is a very sensitive instrument used to detect low level light, and when the light shutter was closed, the instrument continued to register large variations indicating an electronic disturbance in the equipment or the recording devices.

Some attempts were made to explore the mechanisms behind these effects including integrating into the process electrical shielding, distance, visual cues, electrical grounding, and location changes. The results of these process-oriented sessions show that the effects are not affected by wooden barriers, but diminishing effects observed due to distance and electrical shielding may have been more the result of the beliefs and expectations of the participants than an actual limitation of the observed effect. Further research is necessary to explore the mechanisms behind the PK process in more detail and to mitigate the influence of the participants' beliefs on the measured effects.

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THE PARADIGM OF REFLEXIVE ANOMALISTICS

Gerhard A. Mayer & Michael T. Schetsche

Scientific anomalistics is a content-determined, and delimited, area of science committed to the application of appropriate scientific methodology as well as generally accepted, and necessary, scientific control mechanisms. The specification of research subjects is not the result of assignment to groups of phenomena of specific scientific (sub)disciplines but of the ascription of an anomalistic nature (at first), which makes these phenomena, or experiences, a subject of anomalistic research. Accordingly, anomalistics is not characterized by its own specific methodology, but it is oriented by the requirements of the respectively concerned discipline(s) : physics, chemistry, biology, psychology, sociology, science of history, and so forth. For a long time, (natural) science approaches have been considered paradigmatic for anomalistic research, and for parapsychological research in particular, but during the last decades, social-scientific approaches and qualitative research methods have increasingly gained in importance as supplementary and alternative methods. With that, single case studies and the investigation of ostensible spontaneous psi phenomena lost its often premature reputation of being unscientific. Qualitative research methodology, which is predominantly used in social and cultural sciences as well as in anthropology, represents a useful supplement to quantitative approaches; in some cases, and for several research questions, it proves to be ultimately superior because one can go without a considerable reduction of complexity which is obligatory for quantitative methods and, therefore, the research as well as the researchers can come much closer to living-worldly manifestations of anomalistic phenomena and experiences than is the case with the relatively artificial context situation in laboratory experiments. With this paper we present an analysis of the specific conditions of anomalistic research that we systematize under the paradigm of "reflexive anomalistics." The term signifies a social scientific informed approach to anomalistic phenomena that is aware of: (a) the epistemic particularities of the phenomena under research; (b) the precarious cultural (media, science policy) framework conditions of this research; and (c) the areas of tension between subjective evidence, scientific proof, and social discourse. These factors have systematically to be taken into consideration in developing the scientific research questions as well as the methodological approaches.

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SEEKING GENES GOVERNING ANOMALOUS EXPERIENCE: CORRELATIONAL MAPPING OF COMMUNITY SURVEY DATA

James McClenon

A pilot study was designed to locate genes governing anomalous experience. The study uses a variation of Pearlson and Folley's (2008) strategy for locating alleles (alternative forms of a gene governing hereditary variation) based on evolutionary theory. The pilot study was guided by two theories: (a) A sheep theory hypothesizes that ESP alleles provide sufficient benefits to overcome the costs of schizophrenia. (b) A ritual healing theory argues that genes governing absorption and dissociation provided evolutionary benefits to archaic humans exposed to childhood trauma and shamanic healing. The ritual healing theory hypothesizes that shamanic healing, practiced by Paleolithic hunter-gatherers over many millennia, selected for alleles shaping anomalous experience, hypnotic suggestion, shamanism, and spirituality (McClenon, 1997, 2002a). Pearlson and Folley (2008) propose correlational mapping (multidimensional analysis) of community survey data to analyze hypothesized allele markers. The pilot study discusses the sheep theory and ritual healing theory with regard to allele markers. These theories predict existence of alleles governing ESP, absorption, dissociation, transliminality, boundary scales, and other anomalous experiences. A questionnaire was designed to measure frequency of anomalous experience, psychological symptoms, shamanic variables, childhood and adolescent difficulty, and other variables thought correlated with psychological symptoms (McClenon, 2012, 2013). Between 2001 and 2006, the questionnaire was administered to a non-random community sample in northeastern North Carolina ($N = 965$). The hypothesized allele markers were evaluated through correlational mapping of the survey data. Findings are based on five correlational maps (previously unpublished): (a) Anomalous experience variables were highly correlated with each other. (b) Particular correlational clusters, which suggest possible underlying alleles, include "waking ESP, OBE, apparitions," "waking ESP, apparitions," and "waking ESP, paranormal dreams, apparitions." (c) Waking ESP and apparitions were highly correlated with shamanic variables but were generally not within shamanic variable correlational clouds. (d) Although waking ESP is highly correlated with schizophrenia symptoms, researchers have not uncovered major schizophrenia alleles. Failure to locate these alleles calls into question the sheep theory. (e) Cluster patterns suggest that the search for alleles associated with shamanic ritual will be fruitful. (f) Correlational mapping provides no evidence of a psi allele (waking ESP, paranormal dreams, PK). (g) Analysis provides a list of 16 variables most correlated with a "waking ESP, paranormal dreams, apparition" cluster. Although correlational mapping does not provide clear evidence for an ESP allele, these variables are possible allele markers. Pilot study results suggest theoretical revisions. A revised ritual healing theory hypothesizes that random genetic mutations are the source of anomalous experiences.

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PERSONALITY AND PERCEPTUAL VARIABLES ASSOCIATED WITH MEDIUMISTIC EXPERIENCES: EXAMINING TWO SAMPLES

Alejandro Parra

Spontaneous mediumistic experiences have not previously been tested in the general population or in specific groups. The aim of this study, therefore, was to evaluate two samples: paranormal believers ($N = 239$, 74% females and 26% males) and undergraduate students ($N = 554$, 77% females and 22% males) in terms of psychological variables related to mediumistic experiences such as a sense of presence, an apparitional experience, and spirit possession. A number of scales were used in both studies. For S1, the results showed that a sense of presence is the most frequent experience (75%), followed by an apparitional experience (24%) and spirit possession (19%). Only 41% had a type of mediumistic experience, of which 31% had two and 6% had at least three types of experience. Individuals with a higher frequency of mediumistic ex-

periences tended to be more extraverted, have a propensity to schizotypy (with an emphasis on the positive dimension rather than negative schizotypy), have more intense visual vividness, and score higher on thin boundary and transliminality compared to others within the same group that had psychic experiences but not mediumistic experiences. For S2, the results showed that a sense of presence was the most frequent experience (33%), followed by apparitional experience (5%), and spirit possession (4.7%). Individuals with a higher frequency of mediumistic experiences tended to be emotionally unstable, were prone to schizotypy (both positive and negative), and had a greater capacity to dissociate, compared to others within the same group who had psychic experiences but not mediumistic experiences.

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EXPLORATORY STUDY OF THE TEMPERAMENT THEORY AND PARANORMAL EXPERIENCES

Alejandro Parra¹ & Juan Carlos Argibay²

Few studies explored traits of personality to explain paranormal experiences, so it's not surprising that connection has also been made between such simple experiences and a person's temperament. The aim is to compare four groups of temperaments in terms of frequency of paranormal experiences. From the usable questionnaires (1860, 21%), 403 undergraduate students (Psychology Department) were used for the categorization procedure, based on a mean score split of the sample, combined to produce four groups, Phlegmatic ($n = 30$), Sanguine ($n = 54$), Melancholic ($n = 159$), and Choleric ($n = 160$), using the standardized Spanish version of the *Eysenck Personality Inventory* (Form A). The sample filled out a second 10-item self-report inventory designed to collect spontaneous paranormal/anomalous experiences. Cholerics scored highest on frequency of out-of-body experiences (OOBEs), ESP dreams, aura, déjà-vu, and number of experiences; Sanguines scored highest on telepathy, and Melancholics scored highest on frequency of a sense of presence. Certain anomalous experiences, such as OOBE, dream recall, and seeing auras could be facilitated by using extravert subjects characterized as touchy, restless, excitable, changeable, and impulsive.

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EXPERIMENTER EFFECT AND REPLICATION IN PSI RESEARCH

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Sky Nelson³, Serena Roney-Dougal⁵, Garret Moddel¹⁰,
Patrizio E. Tressoldi⁶, & Arnaud Delorme^{1,2,3}

This study addressed the replication problem in parapsychology through the examination of experimenter and participant belief in psi and their impact on the outcome of a psi task. The meta-study involved an international collaboration of teachers, student experimenters, and experimental volunteers, who made use of a standardized psi protocol that has been the focus of a number of replication attempts and that allows for a systematic collection of data under well-controlled conditions (Bem, 2011). It included 12 different laboratories across 32 experimenters and 512 participants. While the preregistered hypothesis that was assessed on a participant basis did not show a significant psi effect, when the statistical power was increased by using a single trial analysis, the primary hypothesis was highly significant. The results did not support a correlation between study outcome and experimenter expectancy. Overall, these results support the feasibility of a multilaboratory collaboration and show that single trial analysis might be more appropriate and powerful to process these types of data.

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BEM'S "FEELING THE FUTURE" (2011), FIVE YEARS LATER: ITS IMPACT ON SCIENTIFIC LITERATURE

Bruno A. Silva & Gabrielle Poeschl

The study analyses the impact on scientific literature of the controversial article by Bem "Feeling the Future" (2011). Texts ($N = 162$) that cite Bem's article were identified from the Elsevier Scopus database for the years 2011 to 2015. Aiming to complement more in-depth approaches to the controversies in parapsychology, we analyzed the summaries of these texts using the Iramuteq program for textual data. The analysis extracted four classes of vocabulary suggesting that the impact can be grouped in four different areas: (a) the role of replication in psychology research; (b) Bayesian statistical inference; (c) experimental studies in anomalous experiences; and (d) the quantum phenomena and theories. The "Replication" vocabulary class is characterized by a vocabulary that addresses the role of replication in psychology research. The lack of replication of some studies, along with questionable research practices, may have contributed to a crisis of credibility regarding psychological science. The "Bayesian" class points mainly to concepts related with the Bayesian approach. The text segments mainly reflected the perceived merits of this approach when compared with more traditional inferential statistics, namely statistics relying on p values. The "Experimental Studies in Anomalous Experiences" class includes frequent terms related to experimental studies and psi phenomena (e.g., remote viewing, precognition, prestimulus), but also terms related to specific methodologies (e.g., nonintentional precognition task, Go/NoGo task), and theory (PMIR: psi-mediated instrumental response). The citation of Bem's article (2011) is applied in a context of process-oriented research on anomalous experiences (experimental work, designed to find evidence about the characteristics of processes that underlie anomalous experiences), in opposition to proof-oriented research. Finally, the "Quantum Phenomena and Theories" set of terms suggests that quantum theories of brain/consciousness function may leave the door open to the possibility of the existence of psi phenomena. The fact that the Replication and Bayesian classes were more represented in psychology literature confirms our hypothesis that psychology sources would have a more critical position. Data still suggest that the replication path is currently the one that is mainly followed in the attempt to reach closure for this controversy.

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OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH ABSTRACTS

Sergii Vakal & Igor Bombushkar

Background. At the moment, only a few initial issues of the *Journal of the American Society for Psychical Research* (JASPR) and *Proceedings of the American Society for Psychical Research* (PASPR) that were published from 1907 to the beginning of the 21st century are available in a digital format over the Internet. To the best of our knowledge, JASPR and PASPR are among the few remaining parapsychological journals

that are not available to the community through Lexscien, Scopus, Informat, or other databases. Given that full JASPR archive comprises more than 662 issues with more than 3,500 papers over nearly 100 years, it goes without saying that such a mass of information should be accessible to the community. The availability of comprehensive digital archives and bibliographic databases is one of the most important factors determining the rate of information turnover within a specific field of study. A need for creation of a joint digital archive of all parapsychological publications has been claimed at the 58th Annual Convention of Parapsychological Association that was held in London last year. A year ago we obtained a large bundle of original issues of JASPR and PASPR from Dr. Hoyt Edge, and under the impression of efforts of Tremmel et al. (2015) to digitize the *Journal of Parapsychology*, we decided to make our own contribution to the community by digitizing abstracts from the available JASPR and PASPR issues. The digitization is being done on a voluntary basis.

Purpose. To digitize abstracts from all available issues of JASPR and PASPR, and provide them in a searchable form in order to increase the rate of information turnover within the parapsychological community.

Methods. First of all, we performed a careful search for digitized JASPR issues on the Internet in order to confirm the idea that a large portion of the issues of this journal is not available in digital format. Secondly, a list of all available original issues, and issues found on the Web, was created and saved as a table in csv format. Scanning of all abstracts was performed manually with the Samsung SCX-4100 multifunctional printer with Smarthru v.4 software on a Windows 7 operating system. We selected a black-and-white scanning setting in order to obtain pictures with high contrast—sharp black text on a white and clear background, with a resolution of 300 pixels per inch. Abstracts both from original papers and book reviews were scanned manually one by one by the staff of the UCPR from January to March 2016. Each abstract was saved as a separate jpeg file in the highest available quality and put in the folder of the corresponding issue. In the next stage we performed manual horizontal alignment and edging of each picture with Adobe Photoshop CS5 v.12.1 in order to obtain files suitable for optical character recognition. The digital images of markings, inscriptions, and coarse dirt were erased manually. Since most of the pictures were of a good quality, further processing was not needed. Each picture was processed in ABBYY FineReader 12 Professional in order to recognize the characters. Each abstract was extracted and pasted to the same plain text file with the following fields: Title of the Paper, Authors, Journal Title, Year of Publication, Volume, Issue, Pages, Abstract Text, Key Words. As a result, we will get a single text file containing more than a thousand abstracts from JASPR issues in fully searchable form.

Results. All abstracts from 114 issues of JASPR and 3 volumes of PASPR were digitized during the project. Besides, 192 issues of JASPR and 16 volumes (19 issues) of PASPR were found on the Internet and were subjected to optical character recognition. Each abstract was categorized into one of the following types: Original Paper, Review, Book Review, Letter to Editor, Bibliography, Recollections, and Discussion. At the moment, we have processed all abstracts in digital format and 60% of them are fully processed. As a result, by the end of May, we will get more than 1,700 abstracts from 130 JASPR issues and 19 PASPR volumes in easily accessible and searchable form.

Conclusions. The electronic searchable archive with more than 1,700 abstracts from 306 issues of JASPR and 22 issues of PASPR was constructed and archived on the server of the Ukrainian Center for Parapsychological Research.

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THERE IS NO GATE: ON THE PA AND THE AAAS

Annalisa Ventola

The 1979 national meeting of the American Association for the Advancement of Science (AAAS) played host to one of the most retold stories in the history of field of parapsychology—when John Archibald

Wheeler, a theoretical physicist, veered off the stated program of his symposia and used part of his presentation time to launch an attack on the Parapsychological Association (PA), calling for its dismissal from the AAAS. His inappropriate outburst did not succeed in achieving this aim; it only strengthened the PA's affiliation with the AAAS. However, it did succeed in obscuring the successes of the dozens of other parapsychology papers presented at AAAS national meetings for years prior, the encouraging interactions that they stimulated, and the positive trajectory of the field up until that point.

This paper summarizes the history of parapsychological topics presented in *Science* magazine and AAAS meeting symposia from the first mention of the word *parapsychology* in *Science* (1937) to last psi-related symposia to take place at a national meeting (1993). Looking through these materials, the integration of parapsychology into the wider scientific community appears to have proceeded—at least for a time—in a linear and continuous way. In the decades prior to the PA's formal affiliation with the AAAS, *Science* magazine was already serving as a mouthpiece for the developing field of parapsychology by reprinting its most important announcements, such as the start of the *Journal of Parapsychology* and the formation of the PA. This was followed by a decade of symposia, which launched with an orientation to the very basics of psi research and increased in complexity year-by-year. The majority of these presentations were followed by question-and-answer periods that conveyed the interest of a curious and open-minded audience.

Examining the AAAS' mission and criteria for disaffiliation, the author concludes that the PA's affiliation with the AAAS is likely to remain untested. A member-facing association organized very similarly to the PA, the AAAS is more occupied by its mission of service than the gate-keeping image that some have put upon it. On occasions where they were thrust into the role of gate-keeper, the governance of the AAAS behaved fairly and dispassionately, which in most cases worked out in favor of parapsychology as a science. Rather than asking if parapsychology is inside or outside the gates of science, it may be more productive to question the very metaphor, and look at how we can support the AAAS's mission of service.

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STIMULATING PROGRESS IN PARAPSYCHOLOGY: PROSPECTIVE META-ANALYSIS

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In 2012, the authors launched the Koestler Parapsychology Unit (KPU) Registry for Parapsychological Experiments. Study registration is a valuable tool that helps to eliminate or identify many questionable research practices in individual studies and thereby enhances the methodological quality of an area of research. Researchers employ meta-analysis to quantitatively combine methodologically similar studies. When conducting a meta-analysis, they must make decisions about what data to include and what statistical methods to use. If researchers do this after knowing the outcomes of the studies, the decisions may be biased. Preregistration of individual studies does not eliminate potential biases that emerge from decisions during a meta-analysis. Retrospective meta-analyses are similar to exploratory research because methodological decisions are made after the study outcomes are known. Prospective meta-analyses are a form of preregistered confirmatory research because the analyses and the data that will be included are specified and ideally publicly registered before the studies are conducted.

The present paper uses ganzfeld ESP research to illustrate the limitations of retrospective meta-analysis, and to highlight how registration-based prospective meta-analysis can help to resolve debates over the evidence for psi and stimulate progress in parapsychology. For the first time, we present a summary of the KPU ganzfeld ESP studies, and note the decisions facing anyone seeking to evaluate their combined outcome. We then discuss the wider ganzfeld ESP database, which has been evaluated in a series of meta-analyses from 1985 to 2010. In each case, discussion has ensued over how to interpret the findings, as occurred for example following the 1999 meta-analysis by Milton and Wiseman. Here, discussion included

the definition of standard ganzfeld, statistical methods, and the meta-analysis cutoff date. The discussion of the Milton and Wiseman meta-analysis demonstrates the latitude in making decisions for a retrospective meta-analysis and, most importantly, the potential for bias either pro or con the psi hypothesis.

We then introduce a prospective meta-analysis of ganzfeld ESP studies that will be placed on the KPU Study Registry following review by parapsychologists and critics. The ganzfeld prospective meta-analysis protocol is preregistered and will make use of future study registrations to prospectively decide which studies will be included and what statistical test will be employed to assess the combined outcome. Any qualifications or modifications for the use of a study will also be specified prospectively on the list of included studies. This approach to meta-analysis does not limit process-oriented research or innovation, but simply specifies what studies will be included in a subsequent meta-analysis before the results of the studies are known.

Finally, we consider how prospective meta-analysis could be applied to other lines of parapsychological research, and summarize the many benefits of prospective meta-analysis for parapsychology.

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PANEL: THE TRICKSTER COMPANION OF PARAPSYCHOLOGY AND ANOMALISTICS

Renaud Evrard (Chair)

The relationship between paranormal phenomena and the trickster figure of mythology has been long recognized (e.g., Radin et al., 1956; Combs & Holland, 1990). *The Trickster and the Paranormal* (2001) by Hansen gave an extended overview of applications of the concept to the fields of parapsychology and anomalistics. It addressed their institutions, history, research, theories, paradoxes, and personnel. The paranormal seems to have affinities with certain social characteristics (e.g., liminality, antistructure, marginality, communitas, transgressiveness, reflexivity, reversibility) and aversions to others (e.g., centrality, structure, hierarchy, bureaucratic power). In sum, trickster theory describes a recurrent socioanthropological pattern that makes intelligible some ordinary observations in our fields, when taken in hindsight. But does this fruitful theory make specific predictions that would make it testable? What is its empirical basis? And what are its limits?

With this panel discussion, we offer an opportunity to address an often neglected theory that illustrates how the humanities may shed light on paranormal research.

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University of Lorraine, Nancy, France

THE PARANORMAL, THE TRICKSTER, AND STRUCTURALIST CONCEPTS

George P. Hansen

For thousands of years, humans have used religious rituals to influence, channel, and hedge off paranormal/supernatural forces. They have used myths to describe, explain, and understand those forces. Half a century ago, ritual and myth were illuminated by anthropologists (e.g., Claude Lévi-Strauss, Edmund Leach, Victor

Turner, Rodney Needham) using structuralist ideas. Concepts developed by them and later poststructuralists (e.g., Jacques Derrida) apply to the paranormal. Structuralist approaches often used comparative analyses, which are commonly employed in the humanities but less often in the sciences. They facilitate development of nonreductionistic theoretical perspectives.

The trickster is a character type found worldwide in myth. He embodies a collection of abstract qualities, including disruption, deception, marginality, supernatural powers, transgression, boundary crossing, and violation of sexual taboos. These qualities may manifest in individuals, small groups, even entire cultures. Structuralism's ideas gave insight into the trickster; the works of Barbara Babcock and Laura Makarius are noteworthy in this regard.

Pertinent concepts include binary oppositions, liminality, antistructure, *communitas*, *betwixt* and *between*, interstitiality, and status reversal—all are directly related to *classification*. These ideas illumine marginality, outsiderhood, stigma, and magic. All help explain the nature of psi as well as the position of the paranormal in Western culture.

Psi subverts commonly accepted categories. It blurs the boundaries between the binaries of self and other (telepathy), present and future (precognition), present and absent (clairvoyance), mind and matter (PK). But psi categories themselves break down; clear distinctions among them cannot be made. The labels can designate experimental procedures, but they do not identify different psi mechanisms. Likewise, the century-long debates on super psi versus spirit communication reflect similar problems of classification.

Earlier cultures used rituals to mark transitions such as child to adult or living to dead; the rituals typically included an in-between (or liminal) period that highlighted transitional conditions. The liminal realm is an area of indeterminacy and ambiguity (and is sometimes sacred); within it, customary rules and social roles are suspended. Characteristics of liminality include social instability, change, flux, transition, fluidity; it thus has parallels with William Braud's model of lability and inertia in psi processes.

Liminality and its governing archetype, the trickster, tend to subvert or dissolve hierarchical social structures. The term *anti-structure* expresses that quality. Psi is frequently accompanied by antistructural effects. Ghost research groups rarely achieve long-term existence and almost never own buildings or employ fulltime staff. Parapsychology laboratories never became securely integrated into mainstream bureaucratic institutions of government, industry, or academe.

Social marginality is a type of liminality. Parapsychology is marginalized and stigmatized. Marilyn Schlitz, in her Parapsychological Association presidential address in year 2000, explicitly disavowed the use of the word *parapsychology*. There may be no better illustration of the stigma and marginality of the field. For more than a century, parapsychologists have striven for respectability and general acceptance by cultural elites. They failed. Any comprehensive theory of psi must explain this plight.

East Windsor, NJ, USA

COMING TO TERMS WITH THE TRICKSTER

James E. Kennedy

The failure to produce convincingly reliable psi effects after 80 years of experimental research indicates that fundamental factors are not yet understood for the operation of psi. George Hansen's characterization of the paranormal as a trickster includes the property that psi effects can be striking and reliable for a period of time, but then seem to actively avoid sustained or useful effects.

The working assumptions for most parapsychologists are that psi is an unconscious process that is directed by human motivation and generally operates without conscious awareness and without conscious intention. The trickster properties of psi indicate that more is involved than just the motivations of the participants and experimenters. Anyone who has motivation about the existence or occurrence of psi could unconsciously use psi to influence the world to conform to their motivations. The outcome of psi experiments may be influenced by the social and cultural background of motivations and associated psi. Competition

among different motivations could produce unreliable psi effects.

Another hypothesis for the trickster nature of psi is that psi effects are due to influences from people in the future acting backwards in time or from entities in additional dimensions such as spirits of deceased persons, angels, god(s), karma, or some type of dualistic higher consciousness. These ideas are not scientifically parsimonious, but they are by far the most common explanations throughout human history. They reflect the fact that people typically experience paranormal phenomena as something that happens to them, not something that they do, as is assumed for experiments. The lack of scientific progress is a good indication that the past parsimonious parapsychological assumptions may not be applicable.

Experimental research with good methodological standards should in the next few years resolve the debates about the validity of the trickster ideas. The weaker methodological practices in the past may have obscured the trickster characteristics of psi. If the trickster ideas prove to be correct, those whose interest in psi is based on control and application—the masculine approach as described by Rhea White—will probably abandon psi research.

The striking differences among people in the occurrence of and attitudes toward paranormal phenomena need to be recognized and investigated. People appear to be living in different worlds with regard to the paranormal and often seem to have little ability to comprehend and accept other worldviews. Researchers need to recognize and to compensate for their own biases. This includes skeptics who are biased against the possibility of psi and parapsychologists who are biased against the possibility of supernatural entities. Psychological, sociological, religious, cultural, and life-event factors all need to be considered in understanding the differences in worldviews about the paranormal.

I also think that it is important to distinguish between experiences that appear to be truly paranormal versus experiences that are likely wishful thinking or other mistaken interpretations. Based on my experiences, actual paranormal experiences may be a component of a larger supernatural factor that creates a destiny for a person to have certain opportunities, experiences, and challenges in life.

Colorado, USA

WHY YOU ARE SO SCARY: UNDERSTANDING THE DEMONIZATION OF PARAPSYCHOLOGY AMONG EVANGELICAL AND FUNDAMENTALIST COMMUNITIES

Jeffrey J. Kripal

The present paper is a development of the last section of my J. B. Rhine Lecture in 2014 entitled “Authors of the Impossible: What the Humanities Have to Offer Parapsychology.” I want to pick up here where I left off there and address some of the contemporary Evangelical and fundamentalist readings of parapsychology as “demonic.” More specifically, I want to read these as a distorted insight into the Trickster, transgressive or antistructural nature of paranormal phenomena. I will engage the work of George Hansen on the antistructural nature of psi phenomena and put this in conversation with the work of the historian of religions Rudolf Otto on the left-handed sacred, the philosopher Georges Bataille on the mystical dimensions of transgression, some recent work on the mythical figure of Satan as a Trickster figure, and the scholarly literature on the paranormal in Roman Catholic hagiography and theology (where it tends to be much more appreciated).

My paper will engage the question of why parapsychology is still the object of various marginalizing, misinformation, and maligning strategies from both cultural elites and religious leaders. As these strange bedfellows suggest, parapsychology occupies a most interesting liminal or both-and position in the broader culture, somehow managing to offend both ends of the ideological spectrum.

On the secular side, I think the primary reason parapsychology is rejected is because its basic theoretical impulses around the nature of mind represent an implicit challenge to the base metaphysics of modernity and its instrumental reason, namely, materialism and mechanism. This is hardly news to you. So let me proceed immediately to the religious reasons, which I think are much deeper historically, trickier to

understand, and so much more difficult to get a handle on and answer.

I think there are at least three religious reasons that parapsychology is rejected and psi is feared. None of these are necessarily conscious reasons. Indeed, I suspect they are usually operating partly or even entirely unconsciously.

The first is what I will call the *problem of deification*. It is a direct function or result of Christian theology and is particularly prominent in American culture. I would not universalize this problem and suspect that the dynamics are very different in other theological contexts, say, in Jewish or Islamic contexts, and I know they are very different in Hindu, Buddhist, and Daoist ones. The second reason is what I will call the *problem of religious authority*. This problem can be found in different theological contexts and in different degrees. Again, I would not universalize it. The third is what I will call the *problem of black magic*. Historically speaking, it is the deepest of the three. It is also, I think, the one most resistant to an adequate response or resolution. Alas, it can probably be universalized.

I fully realize that none of these theological, institutional, and deep historical backgrounds easily translates into a conscious strategy of response or defense in our present. Indeed, I think some of it, if made fully conscious and public, might well make the backlash worse. Still, there is also a part of me that thinks that this deep religious background to the resistance does indeed help. For one thing, it makes sense of the otherwise senseless. For another, it makes the unconscious conscious. For still another, it can help us to better understand and appreciate our own intellectual radicalism.

Rice University, Houston, TX, USA

PA INVITED ADDRESSES

FOR THE 2014 OUTSTANDING CONTRIBUTION AWARD:

THE CASE FOR DREAM ESP RESEARCH: A CONTEXTUALISED REVIEW OF STUDY OUTCOMES FROM THE UNIVERSITY OF NORTHAMPTON

Chris A. Roe

Much recent work in parapsychology has concentrated on measuring unconscious responses to target stimuli or involve implicit psi tasks masked by conventional cognitive tasks. While these approaches are certainly worthwhile, they have little to say about the kinds of macroscopic spontaneous experience that prompted the foundation of the Society for Psychical Research or which preoccupy the general public today—if parapsychologists aspire to be employed in the university sector and paid from the public purse then it can reasonably be argued that their research must reflect that public's concerns. Many spontaneous psychic experiences involve altered states of consciousness (ASCs) in one form or another, and I will argue that this should again become a primary focus for parapsychology. Ganzfeld and dream ESP research seem to have fallen out of favour but continue to produce significant results (Storm, Sherwood, Roe, Tressoldi, Rock, & Di Risio, submitted). Indeed, it could be argued that these studies have been much more successful than we have any right to expect (cf. Roe, 2009), given a general presumption that “one-size-fits-all” when it comes to ASC induction (Rex Stanford refers to this as the “delusion of operational omnipotence”), which is exacerbated by the tendency for researchers not to monitor whether participants have actually experienced an altered state at all. It is therefore surprising that the number of studies utilising ASCs has dwindled in recent times when they seem to provide the most reliable method of capturing psi effects. In this presentation I will argue for a renewed interest in dream ESP research by summarizing those studies (published and unpublished) that have been undertaken at the University of Northampton, with a particular emphasis on process research that was intended to explore underlying patterns or mechanisms.

*Centre for the Study of Anomalous Psychological Processes
University of Northampton, Northampton, UK*

FOR THE 2015 OUTSTANDING CONTRIBUTION AWARD:

FINDING YOUR TEACHER: PUTTING TOGETHER A SHARED
EDUCATIONAL EXPERIENCE IN PARAPSYCHOLOGY

Nancy L. Zingrone

First, let me say I am grateful for the PA's recent recognition of my work. Thank you for the opportunity to talk about what online education can do for us, and why it's not just students and lab assistants who need to look online.

Here's the context: We all know there are individuals in the field who believe there is no point in scientifically studying parapsychology because all this is intrinsically unknowable, who know they already understand it all and we just need to accept that fact, or who argue that an academic degree inhibits scientific progress because they—who usually have less education—feel sorry for all of us who are confused by having too much. We all have a point of view. Whether congenial or uncongenial, the push and pull of scientific discourse really does enrich our field. But how productive is the conversation when we lack shared educational experiences?

When you look around a convention hall do you assume we're all on a similar page? We aren't. There are people here who don't know who Frederic Myers was or how his work inspired Ed Kelly and his colleagues to write *Irreducible Mind* or *Beyond Physicalism*. Many of your colleagues can't name a single important researcher outside their own language group. We are a well-educated community and we aren't. Because we usually master only the specific area we have chosen within a program dedicated to something else, we can be fundamentally uninformed about our field as a whole.

If the solution to this dilemma is the establishment of accredited programs of study that are *not* confined to a single course, are *not* an undergraduate minor with a wider purview, are *not* a year of modules on selected aspects of the field, are *not* a higher degree in a different discipline altogether, we are likely to have to wait 50 years for a shared educational experience that occurs in an on-site context.

Many of us are unwilling or unable to wait that long.

Recently Carlos and I have become involved with the International University of Graduate Studies on Dominica. Our goal is to develop eight to 10 deep courses on the breadth of the field as part of a postgraduate diploma, a postgraduate certificate, a masters or a Ph.D. that will not, however, transfer easily to the accredited schools of the world. IUGS is accredited only by the local government and the tuition is standard for online degree completion schools. Not a perfect solution, we admit, but at least they have supported our intention to provide IUGS students with a high-quality systematic program on the field. We're hoping that, some day, intellectual animosity towards parapsychology will lessen enough to permit a more transferable form of accreditation or a parapsychology-specific program in a more conventionally accredited school. We know it's a gamble but we think it's worthwhile.

So unfortunately, as always, a deep understanding of the field is most likely to rest on individual effort. What we want to emphasize strongly is, just because you are already conducting research or teaching students, do *not* presume the new online opportunities are *not* for you. There is value in the varied courses and certificates of the Rhine Education Center, and in Caroline Watt's KPU-based online course, especially if you have entered the field from another discipline. The content of an increasing number of YouTube channels such as those of the PF, the PA, our own Parapsychology Online channel, Jeff Mishlove's New Thinking Allowed, the IONS and SSE channels add significantly to the serious material that's available and useful for all of us.

So let's talk about the ParaMOOC series. First, MOOC means Massively Open Online Course. In 2008 when the first one of these online courses was taught by Canadian educators Stephen Downes and Dave Cormier on connectivist education, 700+ students was considered a massive course. Now that there's Coursera out there with hundreds of thousands of students per course, the ParaMOOC series is sort of a mini-Mooc. Carlos and I started the first one as a spare time activity when we were just starting to work for

the Parapsychology Foundation again in 2015 because I was familiar with the social media teaching platform WiziQ and our limited internet service could handle it. Now the series is part of our work for the PF.

Over 1,200 students participated in the ParaMOOCs over the last 2 years. Our 2015 backlog of ParaMOOC presentations to be uploaded this year to Parapsychology Online on YouTube include talks by Richard Broughton, Thomas Rabeyron, Roger Nelson, Dean Radin, and Peter Bancel, with many others recorded. Also to be uploaded to the Parapsychology Foundation channel is the 2016 backlog which includes talks by Patrizio Tressoldi, Dick Bierman, Renaud Evrard, Bernard Carr, and Stefan Schmidt, among others. But the PF also has an enormous backlog of audio and video interviews, courses, and conference presentations from the Foundation's 65 years of operation. So there is a lot more to come there to the PF channel. With more than 5,800 views on our 2-year-old channel and over 1,200 views on the PF 13-month-old channel, we can say confidently that our efforts have reduced the field's invisibility at least somewhat.

Make no mistake: The affordances of online education for shared education are massive and powerful. Take Coursera for example. Free education and paid certificate programs are available from top universities around the world in scores of languages. Not only has this changed the shape of modern education for conventional college students but also for individuals for whom university-based education was impossible prior to online education. Small institutions, individual educators, and universities are expanding their virtual footprints even into massively multiplayer online role playing games (MMORPGs) such as *Lord of the Rings Online*. Kahn Academy, SciShow and Minute Earth on YouTube, the International Society of Technology in Education's Virtual Environments Network in Second Life, the Camelot multiplatform online language learning projects, all these have revolutionized instruction all over the world. These and other projects—even our own The AZIRE Library and Learning Center in Second Life—are transforming the landscape for learners of all kinds. Preliminary research from a variety of online teaching and practice journals show that retention of material is not only better in online environments but the learning transfers to the real world more effectively than conventional lecture hall learning.

For a field like ours—situated unhappily in a contested political space—these efforts are extremely important, we think. And so convenient! Where else but online can you sit at your desk or on the Tube heading home and enjoy an hour or more of colleagues describing their work on your iPhone or Surface Pro? If you watch an online offering live and participate in the Q&A, the experience builds community as well. And ParaMOOC? One speaker per day—time limited only by how long the speaker and students want to keep on talking—is so much better than the 30 minutes and done convention model for an in-depth understanding of the topic at hand.

There is no need for us to wait 50 years for a shared educational experience. It's here, and we can take advantage of it as learners and contribute to it as teachers.

Parapsychology Foundation, New York, NY, USA

FOR THE 2015 CHARLES HONORTON INTEGRATIVE CONTRIBUTIONS AWARD:

CONSCIOUSNESS AND THE DOUBLE-SLIT

Dean Radin

If the path that photons take through a double-slit interferometer is known by any means, then the photons will behave like particles, otherwise they will behave like waves. There are numerous interpretations of this effect, known as the “quantum measurement problem.” One of the earliest proposed solutions, by John von Neumann, was based on characterization of the measurement process as a chain of interactions between physical entities—e.g., physical system, detector, eye, brain—with the process ending only when knowledge of the measurement is registered by what von Neumann called an “extra-physical” factor, that is, the observer's mind. In a series of 17 experiments with various optical systems, we put von Neumann's proposal to the test. Participants focused their attention toward or away from an optical system while interference patterns were measured. The hypothesis was that the act of conscious observation would cause a

change in interference. Some experiments were conducted in the lab, others over the Internet to rigorously isolate the observers from the optical apparatus. Overall the evidence strongly supported the hypothesis, but with a surprise. Observation did not always “collapse” the wavefunction, as we had expected. Rather, the interference pattern became sharper or more diffuse depending on the observers’ intentions, where intention was operationally defined by the nature of the feedback used to link the observers to the optical system. This outcome is consistent with consciousness as an active “steering force” rather than as a passive observer. It is also consistent with a half century of prior research using other targets of mental intention. These studies therefore suggest that the observer is an active participant in the formation of physical reality. I will suggest a framework for understanding these effects as predictable rather than anomalous. That framework requires no changes to existing scientific models, just a modification to the metaphysical assumptions underlying science itself.

Institute of Noetic Sciences, Petaluma, CA, USA

PA PRESIDENTIAL ADDRESS

AS IT OCCURRED TO ME: LESSONS LEARNED IN RESEARCHING PARAPSYCHOLOGICAL CLAIMS

Chris A. Roe

Deciding what to discuss in a presidential address provides a welcome opportunity for reflection—after being actively involved in parapsychological research for 25 years, how has my perspective changed? What do I know now that I didn’t know when I began? I do not mean simply in terms of which experimental outcomes proved statistically significant or which themes emerged from qualitative data analysis, but what did I discover about the phenomena, my participants, and also about myself as a consequence of my active participation as a student, researcher, collaborator, and mentor? What painful lessons could I share with those at the start of their career that might save them some time, effort, and trouble? What insights presented themselves to me in the margins of research, from conversations with participants during debrief, from encounters with psychic practitioners in situ, and from interactions with researcher colleagues both within and without parapsychology? My presentation will inevitably have a biographical flavour, but will use that structure to provide a foundation for considering wider issues, such as the tension between the need for clarity and control, and for authenticity and ecological validity, when designing studies; the relative merits of qualitative and quantitative approaches, and the value of a transpersonal orientation to understanding psi; the dynamic interpersonal nature of research with a sentient subject and the effect this has on outcome consistency; the negative impact of our funding model upon the way in which parapsychology is organised; and the need to invest in professional development at undergraduate and postgraduate levels to ensure the long term viability of the discipline.

*Centre for the Study of Anomalous Psychological Processes
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J. B. RHINE BANQUET ADDRESS

THE SECRETS OF THE HEART: EMPATHY & ANOMALOUS/TRANSPERSONAL EXPERIENCES

Ian Wickramasekera

Dr. Ian Wickramasekera II will discuss the powerful transpersonal nature of empathy that sometimes gives rise to anomalous experiences. The keynote address will discuss how anomalous and transpersonal expe-

riences have often been reported to arise from deep experiences of empathy through practicing techniques such as hypnosis and compassion related meditation practices (Chod and Tonglen). The presentation will cover perspectives and practices from ancient spiritual traditions, classic psychological theorists, as well as modern psychological and psychophysiological research conducted by Dr. Wickramasekera and others. The presentation will end with a consideration of what might be gained through studying empathy and compassion to further understand “the secrets of the heart.”

Naropa University, Boulder, CO, USA

MICRO-PSYCHOKINESIS: EXCEPTIONAL OR UNIVERSAL?

By Mario Varvoglis and Peter A. Bancel

ABSTRACT: Most psychokinesis studies fall within either an elitist research tradition, involving exceptional participants and focusing on directly perceptible, macro-PK effects; or a universalist approach, exploring subtle micro-PK effects through massive data collection from unselected participants. However, Helmut Schmidt's highly significant body of micro-PK research was mostly elitist, involving intensive work with small numbers of selected individuals. We contrast his approach to that of the PEAR laboratory, with nearly 100 unselected participants and a highly standardized protocol. Although PEAR's 12-year benchmark study did produce significant cumulative results, a carefully designed replication, involving three laboratories, was nonsignificant. We argue that this apparent failure to replicate was due to the erroneous assumption that the original PEAR data were homogeneous across participants, when in fact they were dominated by two extreme outliers who contributed nearly a quarter of the total data. By ignoring this, the replication employed an overestimation of the effect size for the original study and underestimated the power needed to replicate. We conclude that research generally supports the view that micro-PK is not widely distributed, but exceptional, and that it is unproductive to attempt to tease extremely weak effects out of unselected volunteers. Like Schmidt, investigators should focus on optimizing testing conditions and work intensely with selected participants.

Keywords: micro-PK, RNG research, Schmidt, PEAR, experimenter effects

The term psychokinesis (PK) covers a wide spectrum of putative psi phenomena, ranging from metal bending, poltergeists, and table levitations, to distant influence on human physiology, animal activity, or plant growth. Psychokinesis also refers to situations where statistical deviations from chance in probabilistic systems, such as tumbling dice or coin-tosses, are observed to correlate with participants' wish or intention for a particular outcome. PK studies on probabilistic systems were carried out in various laboratories during the 1940s and 1950s, typically employing mechanical devices with tumbling dice or coin tosses. In the early 1970s hardware random number generators (RNGs) began to replace mechanical systems and quickly became widespread in parapsychology, first as self-standing units and later in computer controlled configurations. Compared to the earlier mechanical systems, RNGs greatly simplified the testing process and thus attracted a wide range of researchers who began exploring a spectrum of hypotheses and variables potentially associated with PK.

Effects reported in association with RNGs are generally qualified as micro-psychokinetic, suggesting a distinction between large-scale or directly perceptible PK and extremely subtle effects that can be inferred only through statistical methods. It is unclear whether the term should apply to all probabilistic systems, even when clearly macroscopic; nor is it certain whether the distinction between macro- and micro-PK points to fundamental differences or merely operational ones. Nevertheless, the fact is that this distinction tends to introduce a split in research strategies—in particular, with respect to the presumed agent or source of the effect. Historically, researchers interested in macro-PK have largely employed an elitist approach, seeking out either exceptional individuals who might produce PK in the laboratory, or exceptional circumstances, as in the case of field research on poltergeists. By contrast, insofar as it seems plausible that weaker PK effects may be more widespread or smoothly distributed in the general population, micro-PK research seems far more amenable to a universalist approach, employing massive data collection from a large number of unselected participants.

The issue we would like to examine here is whether the experimental literature indeed points to the idea that a weak form of micro-PK skill is distributed throughout the population at a detectable level, thus justifying a universalist research strategy, or whether it suggests instead a highly acentric distribution,

with micro-PK being a strong but rare or exceptional skill. This latter conclusion would encourage a return to highly selective, elitist strategies, rather reminiscent of what has been practiced in macro-PK research.

One approach for evaluating this question would be to conduct a meta-analysis comparing effect sizes in studies leaning toward elitist strategies with those that seem to adopt more universalist methods. The problem is that the two main meta-analyses of the RNG micro-PK literature (Bösch, Steinkamp, & Boller, 2006; Radin & Nelson, 1989) arrive at strikingly divergent conclusions, with the most recent one asserting that the small meta-analytic effect size found across studies can be explained by publication bias. As we have argued elsewhere (Varvoglīs & Bancel, 2015), although we agree that there is evidence for substantial publication bias, a subset of the micro-PK literature, involving a large number of high-*z* studies, renders implausible arguments that attribute the effect solely to publication bias and other methodological problems. The literature in fact is highly heterogeneous and meta-analyses cannot currently provide reliable estimates of micro-PK effect sizes.

As an alternative to a broadly-based meta-analytic approach, we focus here upon two major contributors to the experimental micro-PK literature that adopted opposite approaches on the elitist-universalist spectrum. The universalist approach is expressed in the PEAR lab's 12-year "benchmark" micro-PK study involving a highly standardized protocol and nearly 100 unselected participants. The elitist approach manifests quite clearly in the equally impressive and long-term body of research by Helmut Schmidt, who adopted a far more selective and personalized approach in his work with participants. Together these two bodies of research constitute a substantial portion of the micro-PK literature and thus afford a good approximation to the issue examined: Does experimental evidence for micro-PK reflect the minute contributions of the participant population as a whole, or is it rather the result of a few gifted individuals?

Schmidt's Research

Helmut Schmidt is rightfully considered the "father" of micro-PK RNG research: He was the first to introduce a practical hardware RNG for psi studies, was a highly prolific investigator over the course of three decades, played a major role in conceptualizing and modeling the phenomena, and produced by far the strongest and most consistent results in the field. Though a number of other researchers have had considerable success with RNG-PK studies, Schmidt's contribution was clearly exceptional. In our review of his work we found 22 experimental publications containing 50 independent studies, three quarters of which were significant ($p < .05$) and nearly half of which had *z*s above 3 (Varvoglīs & Bancel, 2015). Even if we admit some ambiguity in determining the number of independent studies in experiments that used different devices or participant groups, any tally of the combined significance of Schmidt's work leads to astronomical odds against the null hypothesis.

Why was Schmidt so phenomenally successful with RNG studies? To begin with, a close reading of his reports reveals a highly intuitive approach to the psychological facets of micro-PK research and a keen sense of how best to work with individuals. Indeed, his stance with regard to testing for micro-PK was straightforward and practical: Psi is neither egalitarian nor available on demand, and experiments should be run to proactively track it down and encourage its emergence. Among the strategies employed, foremost was the selection of people with established success in micro-PK tests. He sought out and then tested mediums, psychics, and people who reported extraordinary experiences. When working with larger participant pools, selection was frequently based on systematic preliminary tests. Schmidt was capable of investing months of his time preparing for a single experiment, testing many dozens of people before settling on a handful for the experiment:

For my own experiments, I found it inefficient to gather data from a very large number of people, because poor scores of the majority tend to dilute the effect of the successful performers. Therefore I pre-selected promising subjects, and then used these subjects immediately in a subsequent formal experiment with a specified number of trials. Unfortunately, the process of locating and pre-selecting promising subjects is time consuming and often frustrating. (Schmidt, 1987, p. 105)

Besides adopting this selection strategy, Schmidt was particularly careful to provide an inviting and friendly environment for participants. In some cases he would arrange to do experiments in people's homes and make himself available on short notice should volunteers find themselves well-disposed for a session. Participants could also postpone a session if they did not feel ready and were also given latitude in deciding on a preferred feedback mode. In some instances, a session would only be initiated after a preliminary "warm-up" test was successful, and volunteers were generally encouraged to set their own pace and take breaks or chat with an experimenter if they felt tired or bored. Schmidt indeed allowed for variable contributions from individual participants, for the interruption of sessions, and even for participants to be dropped from an experiment if performance lagged. This stopping was a methodologically sound procedure since Schmidt set the total number of trials (as opposed to the total number of participants) in advance (Schmidt, 1973).

Thus, besides his general tendency to select promising participants, a second explanation of his results is that he was simply a very good experimenter. Whether tacitly or explicitly, Schmidt understood the psychology of getting results, which he applied through skillful creation of good psychological conditions and flexibility in hypothesis testing (e.g., favoring psi-missing rather than psi-hitting when circumstances seemed to call for this). His personal investment in RNG research, his creativity in hypothesis testing, and his sheer perseverance over the course of three decades may have honed his ability to tease out effects that are subtle and difficult to reproduce, but quite real.

This brings us to a third way in which his research was elitist. Besides selecting for talented participants and seeking to create psi-conducive testing conditions for them, Schmidt was a highly gifted micro-PK subject himself. His basic interest was to study the underlying principles of micro-PK and address questions of temporality, causality, and the goal-oriented nature of psi. To do so, he needed strong effects—and he occasionally used himself as a subject, having discovered that he was often as reliable in obtaining positive results as his other subjects. But of course, if this was the case, there is little reason to suppose that his psi skills emerged only when he intentionally evoked them. Parapsychologists (including Schmidt himself) have suggested different channels through which experimenter psi may manifest in micro-PK experiments: direct (albeit unintentional) action on the RNGs during testing, retroactive effects during data analysis (Weiner & Zingrone, 1986, 1989), or numerous intuitive decisions that tacitly guide the experimenter's sampling of the RNG (May, Utts, & Spottiswoode, 1995). Whatever the potential channel used, it seems likely that Schmidt's striking success as experimenter was partly related to his talent as psi subject—a fact that may considerably challenge the generalizability of his results, as well as their replicability across laboratories.

PEAR and the Consortium

The Princeton Engineering Anomalies Research (PEAR) laboratory, founded in 1979 by Robert Jahn, Dean of the engineering school at Princeton University, assembled a staff of physicists, psychologists, and technicians who worked in a basement lab in the campus engineering building for nearly 30 years. Although PEAR explored numerous PK target systems, including macroscopic probabilistic systems such as the random mechanical cascade (Dunne, Nelson, & Jahn, 1988), their primary focus was on RNG research. In stark contrast to Schmidt's strongly personal approach, the laboratory was committed to a strict universalist approach, involving volunteers whose participation depended essentially on their own availability and willingness, and a patient, progressive accumulation of data using the same protocol over many years.

PEAR used a tripolar protocol for each experimental run, which consisted of three separate PK efforts of equal length. These were termed HI, LO, and BL (baseline) and indicated the direction of the participant's intention: Bias the output to go high, to go low, or to remain even. The experimental hypothesis was that the HI runs would give a positive deviation from the mean and the LO runs a negative deviation. The statistical test was based on the difference between the two directional runs. We focus here on the benchmark experiment, which was a 12-year study that collected over 2.5 million experimental trials from 91 participants, equally distributed across HI, LO, and BL conditions. At its termination, the experiment

had attained high significance, yielding a z of 3.8 (Jahn, Dunne, Nelson, Dobyns, & Bradish, 1997). The result is particularly noteworthy, insofar as PEAR had a firm policy of publishing all its experimental results in either refereed journals or publicly accessible internal reports, thus ensuring that the huge volume of research produced over the years can be considered free of publication bias and file-drawer problems.

In 1996 PEAR formed a research consortium with two German groups, the Institut für Grenzgebiete der Psychologie und Psychohygiene in Freiburg, and the Center for Psychobiology and Behavioral Medicine at Justus-Liebig Universität in Giessen. The Consortium undertook an extensive replication of the PEAR benchmark experiment, using the same protocol, uniform RNGs among the groups, and stipulation of an equal contribution of trials from each laboratory. The replication collected roughly the same amount of data as the original benchmark experiment, with a total of 750,000 trials per condition (HI, LO, and BL) from 227 volunteer participants. The primary hypothesis was retained, with a prediction for RNG deviations consistent with volunteers' intentions, and the difference between HI and LO scores as the test statistic. Given effect size estimations from the PEAR experiment, this gave the replication about an 85% chance of succeeding at $p < .01$.

Following three years of intensive data collection, and a period of careful analysis, the Consortium published its much-awaited report (Jahn et al., 2000), but the results were disappointing. All three research groups found positive deviations, but the effect size was nearly an order of magnitude smaller than expected and the overall z came in at a nonsignificant 0.6. The combined PEAR and consortium results were still significant, with a z of 3.2, yet the apparent failure to replicate a solid and well-founded prediction, despite a well-planned collaborative study that included PEAR staff, remained quite surprising.

Why would a well-powered, rigorous replication fail to reproduce the previous results? Was there an essential difference between the experiments that had been overlooked? The Consortium examined a number of possibilities, but found none that was compelling, other than a suggestion that psi may be intrinsically elusive (see Atmanspacher & Jahn, 2003). However, we believe that the explanation is far simpler: The replication attempt underestimated the power needed to obtain results comparable to the PEAR benchmark study.

The replication's validity depended on two assumptions. The first assumption is that the original participant pool was representative of the general population, and the second is that the benchmark effect size is a valid estimate of an average participant's PK effect size. Under these assumptions, any new group of participants should provide about the same effect size, and the appropriate size for a replication can be determined by power analysis.

However, a close look at the PEAR benchmark study shows that there were two extreme outliers in the participant pool, with highly significant personal databases yielding z s of 5.6 and 3.4. The outliers had large individual effect sizes, and they contributed nearly a quarter of the data, with individual contributions far exceeding those of any other participants. This resulted in their contributing over 80% of the total HI–LO deviation. It is easy to see that they are not representative of the 89 other participants, as the overall z of the remaining three quarters of the database is only 0.8, whereas the combined z for the two outliers is 6.5. This difference is significant at the 5-sigma level. Indeed, if we exclude these two outliers, and focus on the database of the 89 remaining participants, we obtain nearly the same effect size and z score as in the nonsignificant consortium replication (see Appendix).

In short, we suggest that the consortium's apparent failure to replicate was due to an overestimation of the true population effect size due an inclusion of the outlier participants, and a consequent underestimation of the power needed to replicate. Had the replication design been based on an effect size without the outliers, then the power needed to replicate would have had to be nearly quadrupled. This means that the apparent replication failure does not call into question the original evidence seen in the benchmark PEAR databases, but only the assumption of homogeneity of its effects across participants.

Optimizing Micro-PK Research

What, then, is there to make of the universalist claim that positive results with unselected participants should be a straightforward matter given sufficient data? Insofar as the PEAR and consortium

results, with outliers removed, did produce some indication of a small effect, and that they are clearly free from any file drawer problem, the cumulative PEAR/Consortium results might justify pursuing an approach based on unselected participants, massive data collection, and analytical tools to tease out effects in the data. However, our analysis shows that studies would need to be significantly larger than those of the benchmark and Consortium experiments merely to provide statistical evidence for an effect. Given the enormous resource investment this approach has represented, involving several laboratories over the course of several years in the case of the Consortium replication, the returns obtained seem meager indeed—and the universalist strategy far from optimal.

We emphasize that the original benchmark result was entirely dominated by a disproportionate contribution from just 2% of the participant population. This basic observation challenges the idea that “anybody can do it”—at least from a pragmatic viewpoint—and points to the benefits of participant selection. From this perspective, the PEAR/Consortium studies, in which we can trace an overall significant effect to the large contributions of two exceptional participants, essentially validate the wisdom of Schmidt’s approach, which was to work intensively with a few participants rather than teasing extremely weak effects out of unselected volunteers.

It should be emphasized that these conclusions are, for now, limited to micro-PK research; they do not necessarily carry over to other parapsychology paradigms. Unselected participants may well perceive ganzfeld, presentiment, or DMILS protocols as more relevant and motivating than micro-PK protocols, and therefore produce far better results. Also, even if the universalist approach is unsatisfactory for proof- or process-oriented hypothesis testing in micro-PK research, it can still be useful for participant selection. Following the lead of Tart (1976), who found that preselection for high scorers in ESP tests seemed to pay off in terms of subsequent ESP training, it may be worth undertaking large-scale (e.g., internet-based) testing to locate promising individuals, and then progressively focus on the few who show the highest potential.

Of course, participant selection alone is hardly sufficient. The testing conditions, the meaningfulness of the task for the participant, and most importantly the investigator-participant relationship, have repeatedly been acknowledged as critical, even with the most gifted macro-PK participants. Why should this be any different with micro-PK? Taking our cue from Schmidt, we suggest working with participants in a highly personalized manner, with a strong focus on motivational conditions and a readiness to adapt testing conditions to the participant (rather than rigidly imposing compliance to a predefined protocol). In this context, it is worth recognizing a rather substantial body of process-oriented research, by a broad spectrum of investigators, exploring factors that enhance micro-PK performance—somewhat in the way that “noise reduction” procedures seem to enhance ESP performance. This research has been amply documented elsewhere (see Gissurarsson, 1997; Varvoglis & Bancel, 2015); some of the more promising optimization factors include a passive-volition set, goal-oriented visualization techniques, and meditation practice.

Any discussion of laboratory psi research is incomplete without addressing the issue of experimenter psi, as has been discussed by a number of authors (Kennedy & Taddonio, 1976; Palmer & Millar, 2015; Parker, 2013). Some argue strongly that most psi researchers who obtain consistent results—whether for micro-PK or other experimental paradigms—are themselves good psi participants. Kennedy and Taddonio (1976) remark:

The case for experimenter PK seems clearly drawn when one considers that experimenters are typically more motivated than their subjects to achieve good results, that PK need not involve a conscious intent, and that most successful PK experimenters are themselves successful PK subjects. (p. 17)

This is not to suggest that experimenters are the only source of psi in the lab; it seems reasonable to assume that micro-PK effects are associated with strong performers—be they participants or experimenters—in conjunction with favorable testing conditions. But to the extent that experimental results potentially reflect the PK input of investigators as “hidden subjects,” we are confronted with an inherent ambiguity in how to interpret results. How do we distinguish participant effects, assumed to be representative of a

larger population and lawful phenomena, from effects that may be due to the experimenters themselves, and potentially dependent upon the very hypotheses they pose? If this issue cannot be resolved, parapsychology may need to reconsider the classical experimental paradigm altogether and turn to radically different epistemological approaches (Atmanspacher & Jahn, 2003; Lucadou, 2001).

The complex issue of experimenter psi notwithstanding, we should not lose sight of the role that experimenter skill may play in obtaining results. It may be that successful investigators such as Schmidt simply know how to facilitate participants' talents. If so, we need to understand in a far more detailed way just how they do it. The number of researchers who systematically succeed in psi research is limited and as Parker (2013) has pointed out, an important body of tacit knowledge risks being lost. Perhaps, in addition to mastery of all the analytical tools that go with the territory, upcoming parapsychologists should train or be mentored by psi-conducive experimenters, studying and modeling their state of mind, mental set, expectations, rituals, and so forth, so they can ensure the longevity of their subtle craft.

In summary, rather than assuming "anybody can do it," we recommend that micro-PK, like macro-PK, be approached as a rare event, one that emerges under exceptional circumstances or as a result of exceptional ability. From this perspective, its investigation demands that experimenters have a special skill set, a process for participant selection, flexible protocols that can adapt to participants' state, mood, or performance, and proactive optimization procedures that may enhance participant scoring.

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Appendix

Table 1 lists the relevant data from the PEAR benchmark experiment, showing the N , effect sizes, and z statistics for the full database, the outlier participant-operators (operator IDs: Op10 and Op78) and the outlier-removed 89-operator subset. Data for operators 10 and 78 can be read from Figure 4 in Jahn et al., 1997.

Table 1
Relative Effect Sizes of the Outlier Participants of the PEAR Benchmark Experiment

	N	μ	σ	z	Δ
All data	837,000	0.042	0.011	3.81	34,850
Operator 10	120,000	0.162	0.029	5.60	19,400
Operator 78	67,000	0.132	0.039	3.42	8,850
Ops 10 & 78	187,000	0.151	0.023	6.54	28,250
89 Operators	650,000	0.010	0.012	0.82	6,600

Note. N = number of trials per intention; μ = effect size of the mean HI-LO deviation; σ = theoretical standard deviation of μ ; z = z statistic for μ ; Δ = absolute HI-LO mean shift; $\sigma = \sqrt{100/N}$ = standard deviation of the HI-LO mean shift.

Abstracts in Other Languages

Spanish

LA MICRO-PSICOKINESIS: EXCEPCIONAL O UNIVERSAL?

RESUMEN: La mayoría de los estudios sobre psicoquinesis caen ya sea dentro de una tradición de investigación elitista, con participantes excepcionales y centrada en efectos directamente perceptibles, o sea macro-PK, o un enfoque universalista que explora efectos de micro-PK sutiles a través de la recolección masiva de datos con participantes no selectos. Sin embargo, la mayoría del trabajo muy significativo de Helmut Schmidt en la investigación de micro-PK fue en su mayoría elitista, trabajando intensamente con un pequeño número de individuos selectos. Contrastamos su acercamiento con el del laboratorio PEAR, con cerca de 100 participantes no selectos y un protocolo muy estandarizado. Aunque el estudio básico de 12 años de PEAR produjo resultados acumulados significativos, un estudio de replicación cuidadosamente diseñado con la participación de tres laboratorios no fue significativo. Argumentamos que este fracaso evidente en replicar se debió a la suposición errónea de que los datos PEAR originales eran homogéneos para todos los participantes, cuando en realidad estaban dominados por dos participantes con valores extremos que contribuyeron casi una cuarta parte del total de datos. Al hacer caso omiso de esto, la replicación sobreestimó el tamaño del efecto del estudio original y subestimó el poder necesario para replicar. Concluimos que la investigación en general apoya la opinión de que la habilidad micro-PK no está distribuida global sino excepcionalmente, y que es improductivo tratar de obtener efectos extremadamente pequeños con voluntarios no selectos. Al igual que Schmidt, los investigadores deben centrarse en la optimización de las condiciones de prueba y en trabajar intensamente con participantes selectos.

French

MICRO-PSYCHOCINÈSE : EXCEPTIONNELLE OU UNIVERSELLE ?

RESUMÉ : La plupart des études de la psychocinèse entrent dans une tradition de recherche élitiste, impliquant des participants exceptionnels et se focalisant sur des effets de macro-PK directement perceptibles ; ou une approche universaliste explorant des effets subtils de micro-PK à travers une collecte massive de données en provenance de participants non-sélectionnés. Toutefois, le corpus de recherche sur la micro-PK par Helmut Schmidt était principalement élitiste, impliquant un travail intensif avec un petit nombre d'individus sélectionnés. Nous comparons son approche à celle du laboratoire du PEAR, avec près de 100 participants non-sélectionnés et un protocole hautement standardisé. Bien que l'étude phare du PEAR, conduite durant 12 ans, ait produit des résultats cumulés statistiquement significatifs, une réplication conçue de façon minutieuse, impliquant trois laboratoires, a obtenu des résultats non-significatifs. Nous affirmons que cet apparent échec de réplication était dû à l'hypothèse erronée que les données originales du PEAR étaient homogènes, alors qu'en réalité elles furent largement influencées par deux participants aux résultats extrêmes qui ont contribué à près d'un quart du total des données. En ignorant ce fait, la réplication a employé une surestimation de la taille d'effet pour l'étude originale et une sous-estimation de la puissance statistique nécessaire pour la réplication. Nous concluons que la recherche supporte généralement la perspective selon laquelle la micro-PK n'est pas largement distribuée, mais reste une exception. Dès lors, il semble improductif d'essayer de détecter des effets faibles à partir de volontaires non-sélectionnés. Tout comme Schmidt, les chercheurs devraient plutôt se focaliser sur l'optimisation des conditions de test et travailler intensivement avec des participants sélectionnés.

German

MIKRO-PSYCHOKINESE: AUSSERGEWOEHNLICH ODER UNIVERSAL?

ZUSAMMENFASSUNG: Die meisten Untersuchungen zur Psychokinese fallen entweder in eine elitistische Forschungstradition mit ausgewählten Teilnehmern und Beschränkung auf direkt beobachtbare Makro-PK-Effekte oder in einen universalistischen Zugang, der schwache Mikro-PK-Effekte mittels umfangreicher Datensammlungen mit unausgewählten Teilnehmern erforscht. Helmut Schmidts hochsignifikantes Korpus von Mikro-PK-Forschungen war hauptsächlich elitistisch und resultierte aus einer intensiven Zusammenarbeit mit nur wenigen, zuvor ausgewählten Personen. Wir kontrastieren seinen Zugang mit jenem des PEAR-Labors, das aus beinahe 100 unausgewählten Teilnehmern und einem hochstandardisierten Protokoll bestand. Obwohl die 12 Jahre umfassende Benchmark-Studie von PEAR kumulativ ein signifikantes Ergebnis erbrachte, fiel eine sorgfältig geplante Replikationsstudie unter Einschluss von drei Laboratorien nichtsignifikant aus. Wir vertreten den Standpunkt, dass dieser offensichtliche Replikationsfehlschlag auf der irrtümlichen Annahme beruhte, die PEAR-Daten seien über alle Teilnehmer hinweg homogen verteilt, während sie in Wirklichkeit von zwei extremen Ausreißern bestimmt wurden, die nahezu ein Viertel der Gesamtdaten ausmachten. Durch fehlende Berücksichtigung dieses Aspekts wurde die Effektstärke der Originalstudie überschätzt und die Teststärke, die zur Replikation benötigt wurde, unterschätzt. Unsere Schlussfolgerung lautet, dass die bisherige Forschung allgemein die Anschauung unterstützt, dass Mikro-PK nicht weit verbreitet, sondern eher die Ausnahme ist, und dass es unproduktiv ist, extrem schwache Effekte aus unausgewählten Freiwilligen herauszukitzeln. Dem Vorbild Schmidts folgend, sollten sich Untersucher darauf fokussieren, die Testbedingungen zu optimieren, und mit ausgewählten Teilnehmern intensiv zusammenarbeiten.

THE EFFECTS OF EXPERIMENTER-PARTICIPANT INTERACTION QUALITIES IN A GOAL-ORIENTED NONINTENTIONAL PRECOGNITION TASK

By Glenn A. M. Hitchman, Christina U. Pfeuffer, Chris A. Roe, and Simon J. Sherwood

ABSTRACT: Several recent studies, inspired by psi theories such as Stanford's psi-mediated instrumental response (PMIR) model, have employed a tacit precognition protocol to test the notion that extrasensory perception may be nonintentional. After remarkable initial success, outcomes have been more inconsistent. One possible reason for the observed variability in results is that the studies were conducted by different experimenters. The current study therefore addressed a number of dimensions regarding participants' interaction with either a male or female experimenter. 52 participants took part in 12 nonintentional precognition trials and a positive or negative outcome task contingent on their performance. The total number of precognitive hits was marginally above mean chance expectation but failed to reach statistical significance. There were significant positive correlations between participants' precognition scores and their ratings of the positivity of their interaction with the experimenter, their rapport with the experimenter, and their level of relaxation. There were also notable differences between the two experimenters with respect to the relationships between their participant-experimenter interaction ratings and participants' tacit precognition scores; all correlations were in the predicted direction for the female experimenter, but in the opposite direction for the male experimenter.

Keywords: extrasensory perception, nonintentional precognition, experimenter-participant interaction

A number of recent studies (Hitchman, Roe, & Sherwood, 2012, 2015; Hitchman, Sherwood, & Roe, 2015; Luke, Delanoy, & Sherwood, 2008; Luke & Morin, 2014; Luke, Roe, & Davison, 2008) have focused on the idea that psi may function unconsciously. Such a notion is consistent with Stanford's (1974, 1977, 1982, 1990) psi-mediated instrumental response model, which frames psi as a process that can serve the best interests of an organism by triggering pre-existing mechanisms in response to threats or opportunities in the environment. Stanford has noted that psi-mediated instrumental responses can occur nonintentionally, and he has claimed that an individual becoming aware of certain need-relevant circumstances or exerting a will to manifest an extrasensory effect may in fact be counterproductive to the psi process. The basic paradigm developed by Luke and colleagues, and modified in subsequent studies to assess further predictions of the PMIR model, involves a picture preference task that serves as a test of nonintentional precognition. In this task, participants are presented with a set of four similar images and asked to choose the one they most prefer. Unbeknown to them, immediately after they make their selection, the computer chooses at random one of the images as a target. Trials in which the participant's selection matches the computer's random choice are scored as hits, whereas all other trials are scored as misses. To capture the goal-oriented nature of psi as proposed by Stanford, the protocol includes a subsequent outcome task that is contingent on the participant's precognitive performance. Those whose total number of hits exceeds mean chance expectation (MCE) are rewarded with a pleasant outcome task that involves viewing pleasant images, whereas those who score below MCE are punished with a negative outcome task that involves either presentation of negative images or a boring number-vigilance exercise. Studies employing this paradigm also have given the researchers opportunities to investigate some of the individual difference factors that are predicted by Stanford to influence the sensitivity of an individual to extrasensory information, and, in turn, their propensity to respond in a need-serving manner.

The results of the studies in which Luke served as the principal investigator were very encouraging. Participants in all four of the studies exhibited hit rates that were above MCE, with three of the four studies producing independently significant evidence of nonintentional psi. Compared with an MCE of 2.50 hits, the combined mean hit rate of participants across all four studies was 2.92 ($SD = 1.46$). This corresponds to an effect size of $ES(r) = .28$, which is highly significant, $t(197) = 4.04$, $p = .00008$, two-tailed. Please note that throughout this paper, effect sizes for t tests are calculated according to the following formula:

$$ES(r) = \frac{\sqrt{t^2}}{t^2 + df}$$

However, three subsequent studies by Hitchman and colleagues have been more inconsistent, with none yielding significant evidence of a tacit precognition effect. In their first study, intended primarily as a proof of principle replication, participants achieved a hit rate that was in the predicted direction but did not deviate significantly from MCE, giving an effect size that was somewhat smaller than those observed by Luke and colleagues, $ES(r) = .16$, $t(49) = 1.14$, $p = .13$, one-tailed (Hitchman et al., 2012). In their second study, which included more substantial methodological refinements, participants scored slightly fewer hits than the mean chance level on the nonintentional precognition task, with a corresponding effect size of $ES(r) = -.05$ (Hitchman, Roe, et al., 2015). Participants' performance in a third study was suggestive of a tacit psi effect, with an effect size approaching the combined score reported by Luke and colleagues, $ES(r) = .23$ vs. $ES(r) = .28$, just failing to reach a statistically significant level, $t(48) = 1.62$, $p = .06$, one-tailed (Hitchman, Sherwood, et al., 2015). As the core of the experimental protocol was generally consistent throughout all these studies, we speculated that the experimenter may be a key variable to be explored in an effort to explain the differing results obtained by the two primary investigators in Luke's and Hitchman's series.

Evidence of the so-called "experimenter effect" has been widely reported in the parapsychological literature (Kennedy & Taddonio, 1976; White, 1976). Early examples of the contrasting results that have been obtained by different researchers working under equivalent conditions were reported by Nicol and Humphrey (1953), Anderson and White (1956, 1957), Van Busschbach (1956), and Bednarz and Verrier (1969). More recently, a series of collaborative studies by Wiseman and Schlitz (1997, 1999; Watt, Schlitz, Wiseman, & Radin, 2005) provided one of the most striking examples of how different parapsychological researchers have obtained divergent results whilst employing exactly the same experimental protocol. Such findings have led to the view that some experimenters appear to be psi conducive and often obtain significant psi effects in their research, whereas others appear to be psi inhibitory and typically fail to find any evidence of psi (Irwin, 1999; Smith, 2003a). Indeed, Rhine and Pratt stated that the experimenter's role is critical to providing "the psychological conditions under which psi can operate" (Rhine & Pratt, 1957, p. 131, cited in White, 1977, p. 274), whilst Murphy (1949) went so far as to say that there never has been a gifted psi subject; rather, he accredited the success or otherwise of participants in psi research to the experimenter and the way he/she sets up the experimental conditions.

Although a number of factors have been proposed to account for the apparent experimenter effect, including experimenter beliefs (see Parker, 1975; Sharp & Clark, 1937; Smith, 2003b; Watt & Baker, 2002; Watt & Brady, 2002; Watt & Ramakers, 2003) and experimenter psi (e.g., Palmer, 1997; Schmeidler, 1997), it is possible that the effect of the experimenter is psychosocial in nature. According to Woodruff and Dale (1950), a key dimension of this psychosocial influence is the interaction style of the experimenter. Building upon this notion, Harris and Rosenthal (1985) and Rosenthal (1966) suggested that the personality and behaviour of the experimenter may play a role in motivating participants or providing subtle clues about the expected outcomes of the experiment. Evidence for this in a nonexperimental setting was provided by Schmeidler and Maher (1981), who found that when presenting papers at a conference, the body language of researchers considered to be psi conducive was rated by independent judges as being more flexible, enthusiastic, friendly, likeable, and warm and less tense, irritable, and cold than the body language of researchers without a track record of psi-indicative results. Edge and Farkash (1981) conducted a replica-

tion of this study using a larger number of judges and found that, compared to psi-inhibitory researchers, psi-conductive researchers were rated as significantly more active, nervous, and enthusiastic, whilst being considered less poised, egoistic, and cold. Whilst it is possible that researchers may naturally display more positive bodily motions and gestures when presenting the results of successful studies, this report provides an interesting insight into the interpersonal characteristics of successful psi researchers.

To provide a more direct test of this notion in an experimental context, Honorton, Ramsey, and Cabibbo (1975) asked two experimenters to control their interaction style when briefing participants. When dealing with some participants, they were asked to act in a positive, friendly, casual, and supportive manner and take time to establish rapport, whereas when dealing with other participants, they were told to behave in a negative, abrupt, formal, and unfriendly manner. Honorton et al. reported a significant effect of interaction style, with the more positive style being associated with more positive psi scoring. However, this finding was not replicated by Schneider, Binder, and Walach (2000), who manipulated the experimenter's interactions as being "personal" or "neutral."

Two recent studies that assessed more specific properties of the interactions between experimenters and participants as predictors of success in psi tasks were conducted by Roe, Davey, and Stevens (2006) and Roe, Sherwood, Farrell, Savva, and Baker (2007). Drawing on the aforementioned psi-conductive traits identified by Schmeidler and Maher (1981), they developed questionnaires measuring various dimensions of the qualities of experimenter-participant interactions (mood, feeling, optimism, confidence, rapport, warmth, spontaneity, and positivity) that were completed by both experimenters and participants. In each experiment, trials were conducted by two different experimenters (both male in the former study, and one male and one female in the latter).

Roe et al. (2006) reported a number of significant correlations which suggest that the experimenters' mood and levels of relaxation as well as their expectations regarding how the participant would perform are related to participants' actual performance on both ESP and PK tasks. Furthermore, Roe et al. (2007) reported three significant correlates of psi scoring that all related to the mood of the experimenters and participants. A similar study by Sherwood, Roe, Holt, and Wilson (2005) found evidence of consistent patterns in the data, with results varying depending on the experimenter. Whilst the majority of effects reported were small and nonsignificant, there were a few significant correlations between success in a ganzfeld task and sender mood ($r_s = -.34$), sender optimism ($r_s = .43$) and confidence of success ($r_s = .40$). Taken together, the research that has assessed the nature of interactions between experimenters and participants suggests that the style of experimenter-participant interactions and the resulting mood, relaxation, and expectancy levels may play a key role in determining the outcomes of psi studies. However, the inconsistent findings and the number of analyses carried out across these studies raise some concerns about the potential for Type I errors, and it is therefore necessary to collect further data in order to draw more reliable conclusions about these proposed relationships.

In discussing experimenter effects, researchers typically devote little attention to the gender of the experimenter. It is possible that the gender effects reported by Hitchman et al. (2012) were, in part, influenced by the exclusive use of a male experimenter. There is precedent for this in the ganzfeld literature, namely that cross-sex experimenter-participant pairings as well as cross-sex sender-receiver pairings sometimes yield stronger psi effects than same-sex pairings, although the findings are far from conclusive (Dalton, 1994; Dalton & Utts, 1995; Roberts & Hume, 2010). Whether these gender-pairing effects are due to general underlying properties associated with gender roles, or to the rapport and situational tensions or feelings of ease that result from certain qualities and styles of interaction, has not been thoroughly studied. Consequently, we were particularly interested in the present study to explore whether gender may be a mediating factor in experimenter effects.

The present study provided an opportunity to explore gender- and interaction-based psychosocial dimensions of experimenter effects in the context of a nonintentional psi paradigm. One of the only studies to have previously considered the role of the experimenter in a tacit psi scenario was carried out by Rao and Davis (1978), who asked a limited sample of 11 female participants to complete both intentional and nonintentional psi tasks. The intentional psi task consisted of a word-based ESP test in which participants

were asked to explicitly guess a series of English and Telugu (an unfamiliar language to the participants) target words that were concealed from their conventional sensory faculties. The nonintentional psi task required participants to rank 40 items from a mood adjective check list on a 4-point scale, with responses being scored by comparing them against a list of randomly generated target numbers. The results revealed a differential language effect in the intentional psi task, with participants scoring significantly higher on English words than on Telugu words, but only for one of the experimenters. For the nonintentional psi task, although the main nonintentional psi hypothesis was not supported, it was found that participants who gave different mood ranks in the second of two experimental sessions scored significantly higher than those who gave the same ranks. Furthermore, the number of mood items checked differently across the two nonintentional psi task sessions was found to correlate significantly with the differences between scores for the two languages on the intentional psi task. These findings seem to indicate not only a potential experimenter effect, but also a relationship between participants' performance in intentional and nonintentional psi tasks.

The present study was based on the protocol used by Hitchman, Sherwood, et al. (2015) with the following refinements. Firstly, rather than presenting two mirrored images (similar to those employed by Bem, 2011) to ensure the aesthetic equivalence of stimuli in the image preference task, the present study employed images that could be rotated 90, 180, and 270 degrees without appearing in any way to be incorrectly or unusually oriented. This meant that participants were highly unlikely to have a clear visual preference for any of the stimuli in each array, whilst having the probability of scoring a hit on each trial reduced from .50 to .25. Secondly, following the hypothesis that some participants may be primarily motivated to avoid the negative outcome task, the contingent reward criteria were set more stringently than in the Hitchman, Sherwood, et al. (2015) study, such that participants needed to score at least one hit above MCE to avoid the negative contingent task and enter the positive outcome condition.

Thirdly, in order to overcome potential response biases associated with the position of the mouse on the pad, the use of the mouse was eliminated during nonintentional precognition trials. Instead, participants indicated their preferred target images by pressing one of four correspondingly numbered keys on a keyboard. This also ensured that measures of reaction times were more reliable. We predicted that those exhibiting faster responses would perform better on the nonintentional precognition task than those whose reaction times were slower, on the assumption that a delayed response may be representative of conscious cognitive activities that are potentially counterproductive to the PMIR process. However, with respect to response times, Stanford (1974) specified an unconscious timing mechanism as a means by which psi-mediated instrumental responses could manifest, and this notion was supported by one of his own studies (Stanford & Thompson, 1973). However, more recently Anderson (2010) failed to obtain evidence that the timing of a behaviour can be instrumental in determining the favourability of its outcome. This raises the issue that there are at least two possible mechanisms through which the timing of a response could influence the outcome of a trial. Firstly, it could be that internal ruminations (indicated by a slower response) could overpower a psi-mediated bias to select a specific target. Alternatively, the timing of the response could be related to the system's selection of the target in a manner that coincides with the participant's selection. The latter possibility, that the exquisite timing of button presses could account for psi-mediated instrumental responses, fits well with decision augmentation theory (DAT; May, Utts, & Spottiswoode, 1995). This theory was supported by May, Spottiswoode, Utts, and James (1995), who found that the timing mechanism accounts better for an experimental database than an alternative psychokinetic explanation that the data were originally thought to support. Further evidence for the concept behind DAT has been provided by Palmer (2009) who reported that a subset of participants scored significantly more hits than chance in a temporally dependent computer-based ESP task. The new preference indication method developed for the present study is thought to be useful in helping to provide a more reliable means by which to further test these predictions.

In summary, the primary goal of this study was to consider the roles of properties of the experimenter-participant interaction. The primary hypothesis predicted that performance on the implicit precognition task would exceed mean chance expectation. On an exploratory basis, it was hypothesised that participants

working with an experimenter of the opposite sex and maintaining a positive mood and interaction style would achieve more hits on the precognition task. Note that this study also explored the relationships between participants' sensitivities to rewards and punishments and their precognitive performance. However, a discussion of this element of the study is beyond the scope of this article. Details are available from the authors on request.

Method

Design

This study employed a quasi-experimental design in which participants completed a 12-trial, forced-choice, nonintentional precognition task. The dependent variable was the number of direct hits they scored on the task. A contingent reward manipulation was subsequently administered, with participants who scored four hits or more receiving a positive reward of seeing images from sets they had previously rated as being preferred, whereas those who scored less than four hits were given a negative reward of seeing pictures from sets they had rated as their least preferred. In order to explore potential covariates of precognitive performance, questionnaire and performance measures were used to collect data on individual differences in sensitivity to rewards and punishments, openness to experience, emotional reactivity, and decision lability. To explore the influence of participant-experimenter interaction, participants' and experimenters' genders were also recorded, along with their ratings of their mood and level of relaxation, the warmth, spontaneity, and positivity of their interaction with their experimental counterpart, their rapport with their experimental counterpart, and their confidence in the participant's ability to contribute to the success of the experiment. For exploratory analysis, participants' reaction times during the covert precognition task were also measured.

Participants

Twenty-six male and 26 female participants (mean age = 35.38 years; $SD = 19.63$) were recruited by opportunity sampling from friends, colleagues, associates, and students from the University of Northampton. Participant numbers were prespecified in order to avoid optional stopping. The sample was divided equally by gender between a male and a female experimenter. Each experimenter was responsible for recruiting their own participants, but they used equivalent recruitment methods and materials. The experimenters recruited the majority of participants from the same source. However, for logistical reasons, 15 of the female experimenter's participants were recruited in Würzburg, Germany. Participants were not offered any incentives for taking part.

The principal experimenter in this study, GH, was a 28-year-old doctoral candidate at the University of Northampton. The co-experimenter, CP, was a 22-year-old female undergraduate psychology student from the University of Würzburg, Germany. Both experimenters were open to the psi hypothesis, very enthusiastic about parapsychological research, and considered themselves not to have frequent psi experiences. Efforts were made to ensure that CP was involved in the design of the study from an early stage, and several of her ideas were incorporated into the protocol to encourage a degree of co-ownership of the project.

Individual Difference Measures

The following questionnaires were administered:

Behavioural Inhibition System/Behavioural Activation System scales (BIS/BAS; Carver & White, 1994). A four-scale questionnaire consisting of 20 items designed to assess individual differences in people's BIS and BAS systems was administered (Gray, 1981, 1982). The BIS scale consists of seven items that address an individual's concerns relating to potential bad occurrences and their sensitivity to negative

outcomes (e.g., “Even if something bad is about to happen to me, I rarely experience fear or nervousness”). The BAS is assessed via three intercorrelated subscales: Reward Responsiveness (five items), Drive (four items) and Fun Seeking (four items). Participants respond to a series of statements (e.g., “I go out of my way to get things I want”) on a 4-point scale ranging from “very true for me” to “very false for me.” Scores on the BAS Drive and BAS Fun Seeking scales can range from 4 to 16, whereas scores on the BAS Reward Responsiveness scale can range from 5 to 20 and on the BIS scale from 7 to 28. In a scale development study, each subscale was found to have an acceptable level of internal reliability, with coefficient alphas ranging from .66 to .76. Carver and White’s (1994) factor analysis suggested that items from the four respective scales load on the appropriate factors, although Cogswell, Alloy, van Dulmen, and Fresco’s (2006) confirmatory factor analysis raised some concerns. However, Beck, Smits, Claes, Vandereycken, and Bijttebier (2009) reported an adequate fit.

Sensitivity to Punishment and Sensitivity to Reward Questionnaire (SPSRQ; Torrubia et al., 2001). A 48-item questionnaire designed to assess individuals’ sensitivities to punishments (SP) and rewards (SR) based on the BIS and BAS systems, respectively, was administered. Twenty-four items relate to SP and 24 relate to SR. Each item is scored on a yes/no basis, yielding a total score that can range from 0 to 24 for both SP and SR. Initial validation studies of the questionnaire divided respondents by gender, and acceptable levels of reliability were found in each case, with coefficient alphas ranging from .75 to .83. Test-retest reliabilities after three months were .89 and .87 for males and females, respectively, and declined to .57 and .61 after three years. In a similar analysis to that used for the BIS/BAS scales, Cogswell et al. (2006) found a relatively poor fit of the items to the factor structure in a confirmatory factor analysis, whereas Beck et al. (2009) reported no such problems.

Demographic questionnaire. This two-item questionnaire asked about participants’ age and gender.

Participant/Experimenter Interaction Questionnaire. A seven-item questionnaire completed by both experimenters (see Appendix) and participants addressed respondents’ mood and level of relaxation as well as the properties (warmth, spontaneity, positivity, rapport) of their interaction with their experimental counterpart. A final item was used to assess the respondent’s confidence in the participant’s ability to contribute to the success of the experiment. Each item is scored on a 7-point scale and scores can therefore range from 1 to 7. These questions were based largely upon those developed by Roe et al. (2006) and were adapted only slightly to avoid compromising the nonintentional nature of the psi task.

Openness to Experience scale (OE; Goldberg, 1999). This 20-item questionnaire addresses an individual’s openness to new experiences. Participants respond to statements such as “believe in the importance of art” and “have a rich vocabulary” by indicating the extent to which each statement is an accurate description of themselves. Each item is rated on a 5-point Likert scale from “very inaccurate” to “very accurate,” yielding a score that can range from 0 to 80. Coefficient alphas for the subscales range from .77 to .86 (Goldberg, 1999), and scores have been found to correlate with scores on the equivalent scale of the NEO personality inventory ($r = .56$; Gow, Whiteman, Pattie, & Deary, 2005).

Emotional reactivity items (Bem, 2003). Two items addressed participants’ awareness of their emotional reactivity to violent, scary, or gruesome content in photographs, movies, and videos. Participants respond on a scale from 1 (“not at all intensely aware”) to 5 (“very intensely aware”). Bem (2003) advises the use of mean scores for correlational analysis, which can range from 1 to 5.

Research impressions items. These two items were included to assess whether participants were aware of the researcher’s interests in psi phenomena and whether the study involved a covert precognition task. The first item read “Do you have any idea what the experimenter’s main research interests are? If yes, please describe them briefly below.” The second item read “Do you have any ideas of the predictions being explored in this experiment? If yes, please describe them briefly below.”

Materials for Test Session

PMIR Visual Basic program. A software program was developed specifically for this experiment

by the first author. The program was based largely on the software used in the Hitchman, Sherwood, et al., (2015) study but adapted to reflect new design elements for the present study. The program was used to present images from the following set.

International Affective Picture System (IAPS; Lang & Greenwald, 1993). The IAPS is a large set of emotive colour photographs, the contents of which span numerous semantic categories including awe, excitement, contentment, amusement, fear, sadness, disgust, and anger. During the development of the IAPS, all images were rated by a large number of independent judges using self-assessment manikins (SAMs) for their perceived valence, arousal, and dominance, enabling them to be categorised according to a number of criteria. These ratings were used to sort the images into five categories: “very pleasant” ($9 \geq \text{pleasantness} > 6.5$), “mildly pleasant” ($6.5 \geq \text{pleasantness} > 5.5$), “neutral” ($5.5 \geq \text{pleasantness} > 4.5$), “mildly unpleasant” ($4.5 \geq \text{pleasantness} > 3.5$) and “very unpleasant” ($3.5 \geq \text{pleasantness} > 0.0$). For each of these categories, eight pictures were then selected that, in the opinion of the authors, best conform to similar semantic themes. The final picture sets, each consisting of eight images, thus consisted of the following: very pleasant pictures—animals; mildly pleasant pictures—relaxed city scenes; neutral pictures—household tools/utensils; mildly unpleasant pictures—broken/decaying items/landscapes; very unpleasant pictures—dead/injured/mutilated human bodies. Each set also met the following criteria: (a) their mean arousal ratings were closely matched, and (b) the sum of the standard deviations of the pleasantness and arousal ratings did not exceed 3.5 units (implying that the majority of individuals have similar emotional responses to the images). These five image sets defined the contingent reward conditions.

Target stimuli. Seventeen royalty-free images that could be rotated such that each orientation appears equally natural were identified specifically for the present study. Some pictures reflect arrangements of natural or everyday objects such as fruit, flowers, and buttons, whereas others reflect abstract artistic patterns. Each image was then rotated through 90, 180 and 270 degrees to yield sets of 4 equivalent images that differed only in their orientation (see Figure 1). Five of these sets were used in practice trials, whereas the remaining 12 were used as target stimuli in the covert precognition task.

Procedure

Participants were briefed either by a male (GH) or a female (CP) experimenter. The majority of briefings took place in a private office at the University of Northampton. However, for logistical reasons, 15 of the female experimenter’s participants were briefed and tested in a private room on the campus of the University of Würzburg, Germany. Each experimenter provided a standardised briefing but inevitably differed in their natural personal interaction style. Both experimenters maintained their ordinary demeanour with no unnatural efforts to appear more warm or friendly than they would under normal circumstances. During the briefing, participants were informed that the study was exploring individuals’ preferences for specific images and how these relate to some of their individual characteristics. They were fully informed about what they would be required to do in each part of the experiment, but at no point was it mentioned to them that the experiment had anything to do with a test of precognition or psychic ability. Both experimenters allowed ample opportunity for participants to ask any questions, and those who were willing to take part were directed toward the automated experimental program on a laptop computer.

Participants were left alone to complete all of the tasks, but the experimenter remained available in a nearby room in case participants had any problems or questions. During this time, experimenters completed a participant-experimenter interaction questionnaire, providing ratings of their mood, level of relaxation, and various aspects of the quality of their interaction with the participant. Finally, they indicated how confident they were that the participant would contribute towards the success of the experiment.

The computer program displayed instructions to guide participants through the first image rating task (Figure 2). Participants were asked to look briefly at a series of pictures and indicate how pleasant they found each one on a scale ranging from 1 (“extremely unpleasant”) to 10 (“extremely pleasant”). Participants rated two randomly selected images from each of five subsets, the order of presentation of all 10 images also being randomised.

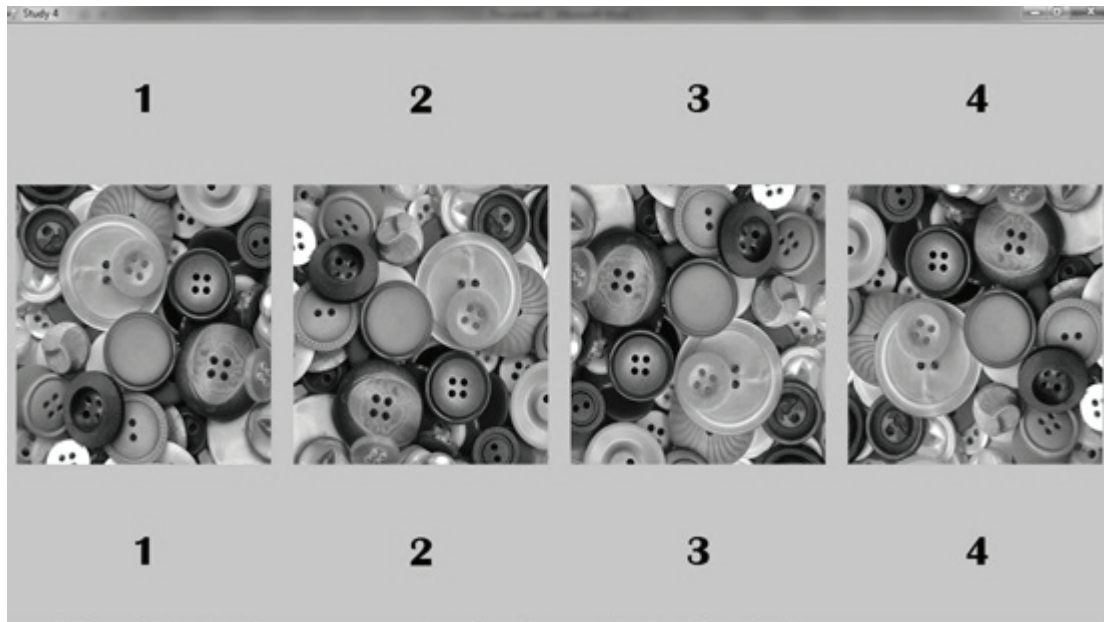


Figure 1. Image preference task 1 (tacit psi task)

The pictures within each of the subsets were the same as those employed by Hitchman, Sherwood, et al. (2015). Each subset was internally homogeneous, containing pictures of similar content, pleasantness, and arousal according to the IAPS rating data, whilst being superficially heterogeneous, with each subset representing distinctly different semantic categories and ranging in pleasantness from very pleasant animal images to very unpleasant pictures showing dead/injured/mutilated human bodies. The participants' ratings were used to calibrate the image sets so that the subsequent positive or negative contingent reward stimuli could be tailored to each participant's idiosyncratic preferences.

Participants then proceeded to the tacit psi task. The on-screen instructions for this task explained to them that they would be shown a series of sets of four images that would appear to look identical but differ in their orientation. Each image was numbered from 1 (leftmost) to 4 (rightmost). Numbered stickers were placed on the Q (1), W (2), O (3) and P (4) keys on the computer's keyboard, and participants were asked to indicate which of the four images they most preferred by pressing the correspondingly numbered button. The instructions reminded participants to be spontaneous in making their choices.

Participants were then shown 17 sets of four pictures displayed in a horizontal line across the screen (Figure 1). The pictures within each set were identical but rotated 90, 180, or 270 degrees. The order in which the pictures in each set were displayed on the screen was randomised for each trial. The first five trials were dummy trials intended to allow participants to familiarise themselves with the procedure, in order to ensure that the measures of response time were not distorted by an initial learning curve. The following 12 trials constituted the nonintentional precognition task. For each trial, immediately after the participants chose their favourite image from the target set, the computer chose one of the images as the target image. Trials in which participants' selection matched the computer's selection were scored as hits, otherwise they were scored as misses. Participants' button presses also initiated the display of the next set of stimuli, with no temporal spacing between trials. The order in which the picture sets were presented across trials was randomised separately for each participant.

Randomisation of the image array positions and computer target selections was achieved using the random number generation function of VB.NET, which is seeded by the CPU timer. A 1×4 chi-square analysis indicated there were no systematic patterns in the computer's selection of the targets, $\chi^2(3, N = 624) = 4.74, p = .19$. Furthermore, a 1×24 chi-square test did not reveal a bias in the positioning of targets in the arrays presented to participants, $\chi^2(23, N = 624) = 29.15, p = .18$.



Figure 2. Image rating task 1 (pre-rating task)¹

After the 17th trial (12th nonintentional precognition trial), the program administered a second image rating task that was contingent upon participants' tacit precognition scores. This task was identical to the initial image rating task, but this time with 10 images being selected from the remaining unrated images of the five aforementioned subsets. Participants who scored four hits or more ($MCE = 3$) were rewarded by being able to rate images from amongst their three most preferred subsets, whereas participants who scored three hits or fewer were negatively rewarded by being asked to rate images from their three least preferred subsets. The exact composition of images for the final task was determined according to the criteria specified in Table 1.

Table 1
Composition of Contingent Reward Pictures by Nonintentional Precognition Score

Psi score	Number of images from each subset rank				
	1st	2nd	3rd	4th	5th
≥ 10	6	4	0	0	0
8 or 9	6	2	2	0	0
6 or 7	2	6	2	0	0
4 or 5	2	2	6	0	0
2 or 3	0	0	2	6	2
0 or 1	0	0	2	2	6

After the final experimental task had been completed, the program informed participants that they had completed all the experimental tasks and were ready to move on to the questionnaire battery. Participants were then guided through a battery of questionnaires in the following order: demographic question-

¹For contractual reasons, IAPS images are not permitted to be displayed in journal publications. Indicative images are therefore displayed in all the figures rather than the genuine IAPS pictures that were used in the present study.

naire, emotional reactivity items, BIS/BAS scales, SPSRQ, and Openness to Experience questionnaire. Next came the Participant/Experimenter Interaction Questionnaire. Given that participants may have been reluctant to provide sincere answers if they felt they would be seen by the experimenter, in addition to being told so during the briefing, they were reminded by the on-screen instructions that their answers would be analysed by an independent researcher and not seen by anyone else. Participants were then requested to answer the two research impression items regarding their awareness of the experimenter's research interests and the experimental hypotheses, in order to verify that they were not aware of the true nature of the tacit psi task.

The final screen displayed the participant's cumulative score on the nonintentional psi task, which aided the researcher in providing a full debrief. This included an explanation that the image preference task was, in fact, a covert psi task and the reasons for the mild deception. During this time, participants were asked not to discuss the nature of the experiment with other potential participants.

Ethics

The project was designed to adhere to the British Psychological Society's Code of Ethics and Conduct (BPS, 2009) and received ethical approval from the University of Northampton Research Ethics Committee. Participants were briefed prior to giving their informed consent as part of the program. In particular, participants were forewarned that they may see images of a violent, gruesome, or scary nature during the experiment. All data were collected anonymously and participants were made aware of their right to withdraw from the experiment at any time without having to provide a reason. To ensure that participants did not feel uncomfortable when rating the properties of their interaction with the experimenters, neither experimenter saw the raw data. Instead, these data were written to a separate data file that was stored in an alternate location on the computer's disk drive. Instructions that described to an independent researcher how to retrieve and delete the data and how to conduct the planned analyses were prepared in advance.

Results

The total number of hits on the nonintentional psi task was recorded along with scores on the performance- and questionnaire-based individual difference measures. Seven of the 52 participants chose not to answer one or more of the questionnaire items. In instances where the omitted items amounted to less than 10% of all the items on the respective measure, a median substitution was applied; otherwise, all the data for that questionnaire were excluded from the analyses.

Manipulation Checks

Regarding the efficacy of the experimental manipulation of assigning participants to positive and negative contingent reward conditions, Table 2 shows that, as expected, those in the positive reward condition generally rated the contingent task images as significantly more pleasant than those in the negative reward condition (positive condition mean = 5.97, negative condition mean = 3.60; $t(50) = 9.08$, $p = 2 \times 10^{-12}$, one-tailed).

Table 2
Means and Standard Deviations of Participants' Subjective Ratings of the Pleasantness of the Contingent Task by Task Type

Reward condition	<i>N</i>	<i>M</i>	<i>SD</i>
Positive	15	5.97	0.96
Negative	37	3.60	0.81

In order to validate the sensitivity of the ordinally scaled contingent rewards, Spearman correlations were calculated between the severity rank of punishment or reward and participants' subjective ratings of the pleasantness of the outcome task. Because of Hitchman et al.'s (2012) concern about the relationship potentially being nonlinear, participants were classified by reward type (positive or negative). For those who received a positive reward, there was a medium-sized positive correlation between their psi scores and their ratings of the contingent reward images, but the relationship just failed to reach statistical significance, $r_s(14) = .41, p = .06$, one-tailed. For those who received a negative reward, there was also a medium-sized positive correlation between their psi scores and their ratings of the contingent reward images, and the relationship was statistically significant, $r_s(36) = .36, p = .01$, one-tailed. Given that the effect sizes of both relationships are similar, it is likely that the sample size determined the significance of the observed effects.

Furthermore, in response to the research impression items, none of the participants indicated any awareness of the experimenters' main research interests or the predictions being explored in the experiment. Before being debriefed, participants therefore appeared to be unaware that they had taken part in a psi study that involved a covert precognition task. Similarly, after being informed of the nature of the study during the debriefing, none of the participants indicated that they had surmised the nature of the study.

Nonintentional Precognition

The primary hypothesis predicted a nonintentional precognition effect. Fifty-two participants each completed 12 tacit precognition trials with an associated probability of correctly selecting the target image of .25. Thus, with a total of 624 trials, MCE was 156, whereas the actual number of hits was 160. The mean number of hits was 3.08 hits ($SD = 1.72$) per participant, marginally above the MCE of 3.00 ($SD = 1.50$). Figure 3 shows that the distribution of individual scores was positively skewed, and the extent of the skewness was found to be statistically significant ($IS = 1.09, z = 3.23, p = .001$, two-tailed). For consistency and comparison with previous studies, results of untransformed parametric tests are included in this report for reference only. Given the distribution of the data, readers are advised to interpret the nonparametric test results reported where appropriate as the more reliable. The results of a one-sample t test indicate that participants did not score significantly more hits during the nonintentional precognition task than would be expected by chance, $t(51) = .32, p = .37$, one-tailed, $ES(r) = .04$. A nonparametric trial-by-trial binomial analysis yielded a similar result, $z = .32, p = .37, ES(r) = .03$.

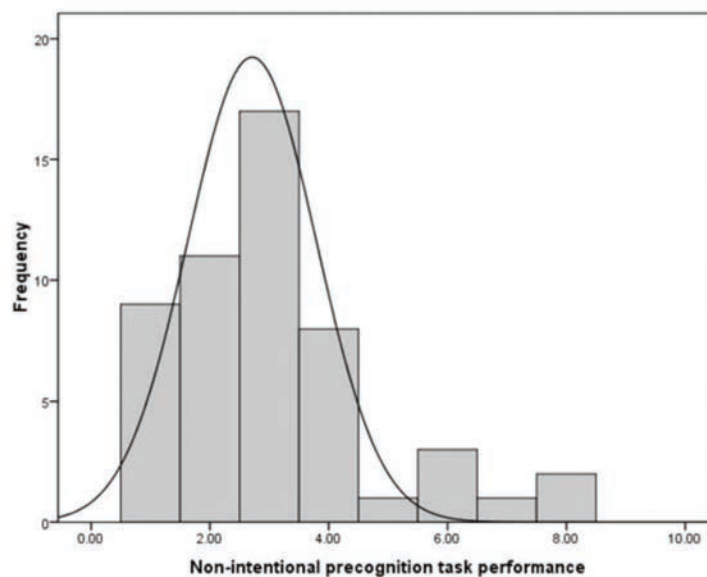


Figure 3. Histogram showing the frequency distribution of scores on the nonintentional precognition task ($N = 52$)

Experimenter Effects: Sex Pairing

In order to test whether there was a classical experimenter effect and/or effects of gender and experimenter-participant sex pairing, a 2 x 2 ANOVA with experimenter (GH vs. CP) and gender (male vs. female) as between-subjects factors was carried out. The results reveal that although CP's participants generally performed better than GH's (mean precognition score = 3.38 vs. 2.77), there was no significant main effect of experimenter, $F(1,48) = 1.74$, $p = .19$, or of gender, $F(1,48) = 3.92$, $p = .05$, and no significant interaction between experimenter and gender, $F(1,48) = 0.00$, $p = 1.00$. Table 3 presents the means, medians, and standard deviations of participants' scores by experimenter and gender, and Table 4 shows the means, medians, and standard deviations of hit rates by sex pairing. The mean hit rate for opposite sex gender pairings ($M = 3.08$, $SD = 1.62$) was similar to that of same sex gender pairings ($M = 3.08$, $SD = 1.85$), with the median scores being the same in each case (3.00). The results of a Mann-Whitney U test indicate that there was no statistically significant difference in precognitive performance between gender pairings, $U = 331.00$, $p = .45$, one-tailed.

Table 3
*Means, Medians, and Standard Deviations of Participants' Nonintentional
Precognition Task Scores by Experimenter and Gender*

		Overall	Male	Female
<i>N</i>		26	13	13
GH	Mean precognition score	2.77	2.31	3.23
	Median	2.50	2.00	3.00
	Standard deviation	1.73	0.95	2.20
CP	Mean precognition score	3.38	2.92	3.85
	Median	3.00	3.00	3.00
	Standard deviation	1.80	1.50	1.82

Table 4
*Means, Medians, and Standard Deviations of Hit Rates for the Precognition Task When
Participants Were Paired With an Experimenter of the Same or Opposite Gender*

Gender pairing	<i>N</i>	<i>M</i>	<i>Mdn</i>	<i>SD</i>
Same gender	26	3.08	3.00	1.62
Opposite gender	26	3.08	3.00	1.85

Experimenter Effects: Participant-Experimenter Interactions

Hypotheses in relation to the experimenter-participant interaction variables predicted that the number of hits participants would score in the covert precognition task would be positively related to their rating of: (a) their mood, (b) their level of relaxation, (c) the warmth of their interaction with the experimenter, (d) the spontaneity of their interaction with the experimenter, (e) the positivity of their interaction with the experimenter, (f) their rapport with the experimenter, and (g) their confidence in their ability to contribute to

the success of the experiment. Both the participants and the experimenters rated these dimensions on scales ranging from 1 to 7. Table 5 shows that participant ratings of their rapport with the experimenter were well above the midpoint for both experimenters (GH mean rapport = 5.68, $SD = 1.17$; CP mean rapport = 5.58, $SD = 1.03$).

Table 5
Means and Standard Deviations of Participants' and Experimenters' Experimenter-Participant Interaction Ratings

Question	Participant ratings				Experimenter ratings			
	GH ($N = 26$)		CP ($N = 26$)		GH ($N = 26$)		CP ($N = 26$)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Mood	4.46	1.30	6.00	0.89	5.46	1.03	4.31	1.12
Relaxation	3.69	1.46	2.38	1.75	4.69	1.44	3.92	1.13
Warmth	5.58	0.76	5.46	1.17	5.62	0.98	4.85	0.92
Spontaneity	5.31	1.09	5.19	1.55	5.35	1.02	4.50	1.39
Positivity	5.24	0.88	5.46	1.07	5.69	1.05	4.88	1.03
Rapport	5.68	1.03	5.58	1.17	5.38	1.10	4.88	1.07
Confidence	4.80	1.08	5.35	1.02	4.50	1.36	4.62	1.27

Correlation analysis was used here, as the data do not meet a number of the criteria recommended by Tabachnick and Fidell (2001) for binary logistic regression analysis. Table 6 shows that there were significant positive correlations between participants' nonintentional precognition task scores and their ratings of the positivity of their interaction with the experimenter, $r_s(51) = .25, p = .04$, one-tailed, their rapport with the experimenter, $r_s(51) = .27, p = .03$, one-tailed, and their level of relaxation, $r_s(51) = .27, p = .03$, one-tailed. There was also a suggestive positive correlation between participants' nonintentional precognition task scores and their ratings of the warmth of their interaction with the experimenter, $r_s(51) = .22, p = .06$, one-tailed.

Table 6 shows that when results were classified by experimenter, there were small-to-medium sized positive correlations between GH's participants' nonintentional precognition scores and their ratings of their level of relaxation, $r_s(25) = .31, p = .06$, one-tailed, the warmth of their interaction with GH, $r_s(25) = .22, p = .14$, one-tailed, the positivity of their interaction with GH, $r_s(25) = .31, p = .06$, one-tailed, and their confidence in their ability to perform well in the experiment, $r_s(25) = .20, p = .16$, one-tailed. However, none of these relationships achieved statistical significance, although the relatively small sample size may have precluded the detection of possibly genuine effects. For the female experimenter (CP), there was a significant positive correlation between her participants' psi scores and their ratings of the experimenter-participant rapport, $r_s(25) = .51, p = .01$, one-tailed. There were also small-to-medium-sized positive correlations between CP's participants' tacit precognition scores and their ratings of the warmth and positivity of their interaction with CP: warmth: $r_s(25) = .21, p = .16$; positivity: $r_s(25) = .27, p = .10$, both one-tailed, as well as their confidence in their ability to perform well in the experiment, $r_s(25) = .23, p = .13$, one-tailed. However, possibly due to the relatively small sample size, none of these relationships achieved statistical significance. All other correlations were small and nonsignificant.

Table 6
Spearman Correlations Between Psi Task Scores and Participants' Experimenter-Participant Interaction Ratings and One-Tailed Significance Values

	Overall ($N = 52$)		GH ($N = 26$)		CP ($N = 26$)	
	r_s	p	r_s	p	r_s	p
Mood	-.16	-	-.08	-	.11	.29
Relaxation	.27	.03	.31	.06	.05	.40
Warmth	.22	.06	.22	.14	.21	.16
Spontaneity	-.01	-	.04	.42	-.14	-
Positivity	.25	.04	.31	.06	.27	.10
Rapport	.27	.03	.07	.37	.51	.01
Confidence	.13	.18	.20	.16	.23	.13

Similar predictions were made in regard to the experimenters' ratings of the experimenter-participant interaction variables. Table 7 shows that in the overall database, none of the relationships was statistically significant. It is interesting to note that when the results were divided between the two experimenters, the majority of GH's correlations were negative, whereas CP's correlations were all in the predicted direction. However, it should be noted that there were large intercorrelations between both experimenters' interaction ratings (mean GH $r_s = .52$; mean CP $r_s = .56$). Among these correlations, there were small-to-medium-sized negative correlations between GH's participants' nonintentional precognition scores and his ratings of the spontaneity and positivity of his interactions with them: spontaneity: $r_s(25) = -.33$; positivity: $r_s(25) = -.25$, and a small-to-medium sized correlation between CP's evaluations of her mood and her participants' nonintentional precognition scores, $r_s(25) = .28$, $p = .08$, one-tailed. All other relationships were small and nonsignificant. These findings may suggest a subtle underlying difference between the two experimenters with respect to the predictive power of their ratings of the participant-experimenter interaction variables that was not sufficiently highlighted by the limited sample size.

Table 7
Spearman Correlations Between Psi Task Scores and Experimenters' Experimenter-Participant Interaction Ratings and One-Tailed Significance Values

	Overall ($N = 52$)		GH ($N = 26$)		CP ($N = 26$)	
	r_s	p	r_s	p	r_s	p
Mood	.16	.13	-.04	-	.28	.08
Relaxation	.07	.31	-.11	-	.09	.34
Warmth	.03	.42	-.17	-	.07	.38
Spontaneity	-.07	-	-.33	-	.10	.32
Positivity	.04	.39	-.25	-	.16	.21
Rapport	.11	.23	-.04	-	.18	.19
Confidence	.02	.44	.00	.50	.07	.38

Reaction Times

Turning to participants' reaction times, it was predicted that their reactions would be faster on trials in which they scored a hit relative to those in which they scored a miss. Table 8 shows that mean response times were shorter on trials in which participants scored a hit compared with those on which they scored a miss (mean response time = 5.18 s vs. 5.31 s). The distribution of reaction times for hit trials was found to be positively skewed, $IS = 1.67$, $z = 4.93$, $p = 4 \times 10^{-7}$, so consequently a nonparametric statistical test was employed. The median reaction times were 4.44 s for hit trials and 5.31 s for miss trials. The result of a Wilcoxon signed ranks test indicated that reaction times for trials in which participants scored a hit were significantly lower than those for trials in which participants scored a miss, $z = 2.14$, $p = .02$, one-tailed.

Table 8
Means, Medians, and Standard Deviations of Participants' Reaction Times (Seconds) by Outcome of Trial (N = 52)

	<i>M</i>	<i>SD</i>	<i>Mdn</i>
Hit	5.18	3.49	4.44
Miss	5.31	2.80	5.31

Openness to Experience and Emotional Reactivity

Previous studies have indicated interesting relationships between tacit precognition scores and scores on the Openness to Experience scale (Goldberg, 1999) and emotional reactivity items (Bem, 2003). Table 9 displays the means and standard deviations for participants' scores on each measure in the present study. Participants' Openness to Experience scores were marginally lower than those in previous studies ($M = 53.90$, $SD = 9.46$); Hitchman, Roe, et al., (2012): $M = 62.00$, $SD = 7.70$; Hitchman, Roe, et al. (2015): $M = 54.54$, $SD = 12.50$; Hitchman, Sherwood, et al. (2015): $M = 55.18$, $SD = 8.85$. On the other hand, participants generally rated their emotional reactivity to violent, scary, or gruesome material slightly higher than those in previous samples, with a mean score of 3.75 ($SD = 0.88$) compared with a mean of 3.44 ($SD = 0.87$) in Hitchman, Roe, et al., (2015) and 3.55 ($SD = 0.90$) in Hitchman, Sherwood, et al., (2015). Table 10 indicates that the correlation between participants' Openness to Experience scores and their scores on the nonintentional precognition task was close to zero, $r(51) = .06$, $p = .34$, one-tailed. Spearman nonparametric analysis yielded a similar result, $r_s(51) = .08$, $p = .28$, one-tailed. Meanwhile, as expected, there was a significant positive correlation between scores on the tacit precognition task and emotional reactivity ratings, $r(51) = .29$, $p = .02$, one-tailed. Spearman analysis yielded a similar result, $r_s(51) = .30$, $p = .02$, one-tailed.

Table 9
Means and Standard Deviations of Individual Difference Measures

Measure	<i>N</i>	<i>M</i>	<i>SD</i>
Openness to Experience	52	53.90	9.46
Emotional Reactivity	52	3.75	0.88
BAS Drive	51	10.61	1.84
BAS Fun Seeking	52	11.54	2.26
BAS Reward Responsiveness	52	16.44	2.19
BIS	50	20.44	3.28
Sensitivity to Punishment	51	13.08	4.89

Table 10
*Spearman and Pearson Correlations Between Psi Task Scores and Individual Difference Measures
 and One-Tailed Significance Values*

	<i>N</i>	<i>r_s</i>	<i>p</i>	<i>r</i>	<i>p</i>
Openness to Experience	52	.08	.28	.06	.34
Emotional Reactivity	52	.30	.02	.29	.02
BAS Drive	51	.01	.48	-.04	-
BAS Fun Seeking	52	.01	.48	-.05	-
BAS Reward Responsiveness	52	.11	.21	.14	.16
BIS	50	.19	.09	.11	.22
Sensitivity to Punishment	51	-.19	-	-.21	-
Sensitivity to Reward	51	-.05	-	.00	.49

Consistent with previous studies, multiple statistical tests are reported without a correction applied to the alpha levels for multiple analyses. Milton and Wiseman (1997) have noted that the standard Bonferroni adjustment should be considered conservative, whilst Abdi (2007) claims the Bonferroni correction is not appropriate when the inferential tests conducted are not entirely independent. Readers are advised to treat the results reported with caution, as the chance of a Type 1 error is increased as a consequence of the multiple analyses carried out.

Discussion

In an effort to demonstrate a reliable nonintentional precognition effect, this study aimed to further refine the protocol developed by Luke and colleagues (Luke, Delanoy, et al., 2008) which had been demonstrated and successfully replicated by Luke, Roe, et al. (2008), but the outcomes of three attempted replications by Hitchman and colleagues (Hitchman et al. 2012; Hitchman, Roe, et al., 2015; Hitchman, Sherwood, et al., 2015) have been more inconsistent. To explore the reasons for this inconsistency, this study had a further focus of evaluating experimenter-participant interaction qualities, which were expected to be predictive of participants' success.

Overall, participants in this study scored only marginally more hits (3.08) than MCE (3.00). However, the distribution of these scores was positively skewed, indicating that the majority of participants (71%) scored either at or below MCE. Only 29% managed to avoid the negative reward, whereas 35% would be expected to by chance. The overall effect size of $ES(r) = .04$ was the second lowest observed across the four studies by the present research team: Study 1: $ES(r) = .16$; Study 2: $ES(r) = -.02$; Study 3: $ES(r) = .23$.

Accounting for participants' poorer performance is not easy, as the methodological changes introduced for the present study relative to the most recent Hitchman et al. study (Hitchman, Sherwood, et al., 2015)—the most successful of the four—were mostly minor refinements of adaptations that had previously appeared to be effective. The most notable change was the shift from using binary mirrored images as targets/decoys to an array of four equivalent images rotated through different angles. Arrays of four images (albeit fractals, arranged in a square pattern rather than a horizontal line) was also the format employed in Luke and colleagues' successful studies. It therefore seems unlikely that the diminished performance is due to this minor adjustment, and it is possible that the observed differences in results across studies may largely be due to measurement error. Nevertheless, future researchers may wish to systematically manipulate the layout format of images to test whether or not this is a significant issue.

Given that the other elements of the method were broadly the same as those employed in the Hitchman, Sherwood, et al., (2015) study, the explanation for the divergent results may have to do with the

participants. The sample in the present study was considerably more diverse than in the Hitchman, Sherwood, et al. (2015) study. The mean age of participants in this study was 35.38 ($SD = 19.63$), compared with 23.06 ($SD = 7.00$) in the previous study, the difference being statistically significant, $t(99) = 4.15$, $p = .00007$. Furthermore, the majority (67%) of participants in the present study were either complete strangers or people with whom the experimenters had only short prior interactions, whereas in the previous study the principal investigator relied more heavily upon personal contacts. Nevertheless, participant ratings of their rapport with the experimenter were well above the midpoint for both experimenters (GH mean rapport = 5.58, $SD = 1.17$; CP mean rapport = 5.68, $SD = 1.03$).

As for the other individual difference characteristics of the participants, the Openness to Experience scale was one of the stronger predictors of precognitive success in the Luke and colleagues studies and the original study of the present team. However, this relationship has proven to be more inconsistent in subsequent studies. The present study yielded no overall relationship between nonintentional psi scores and Openness to Experience scores, raising further doubt over the reliability of the relationship reported in earlier studies. The authors are not aware of any reasons why the minor methodological deviations from the original studies should have caused the apparent lack of similar relationships in more recent studies.

From Hitchman et al.'s second study (Hitchman, Roe, et al., 2015) onwards, emotive images were used in negative reward conditions with the aim of providing a stronger aversive effect from the negative outcome task. Participants with higher levels of emotional reactivity were expected to be more averse to the negative rewards and consequently demonstrate higher precognition scores. This study is broadly supportive of the results of the Hitchman, Roe, et al. (2015) study, in which there was a significant positive correlation between participants' responses to the emotional reactivity items and their nonintentional precognition scores. Results of both studies differed, though, from those of the Hitchman, Sherwood, et al. (2015) study, in which a suggestive *negative* correlation was found between the two variables. Taken together with Bem's (2003) finding of a significant positive correlation between emotional reactivity scores and precognitive avoidance of negative stimuli, it may be that the result of the Hitchman, Sherwood, et al. (2015) study was an anomaly. It is concluded that the measures relating to participants' sensitivities to rewards and punishments did not convey the desired information in relation to the PMIR model and results relating to these metrics will therefore not be discussed.

A main focus of this study was the effect of the interaction between the experimenter and the participant. A female research placement student (CP) ran half of the participants through the experiment, whilst the other half of the sample was tested by the first author (GH). Both experimenters and participants rated their own psychological and physiological states, as well as several qualities (mood, relaxation, warmth, spontaneity, positivity, rapport) of their interaction with their experimental counterpart, and their confidence that the participant would be successful.

It was found that, with the exception of their ratings of their mood and the spontaneity of the interaction, all participants' ratings were positively correlated with their performance on the precognition task, with their ratings of their level of relaxation and the positivity of their interaction and rapport with the experimenter being significant. The direction of the effect was generally consistent for both experimenters. However, only their rapport with CP was significantly related to their tacit psi scores. This correlation was medium sized and was amongst the strongest of all the correlations observed in the study ($r_s = .51$). The predicted relationships involving experimenter ratings were generally modest and nonsignificant.

It is important to note the possibility that participants' responses to all of the questionnaire-based measures were influenced by the fact that they provided them after the experimental tasks. Because each participant encountered a different positive or negative reward that was contingent on their performance on the tacit psi task, and the task itself may have seemed rather puzzling to some participants, the reliability and construct validity of the questionnaire measures may have been compromised. Although this could easily have been avoided by having participants respond to the questionnaires before taking part in the nonintentional psi task, it was felt that this would have been just as likely to influence the outcome of the experimental tasks by causing fatigue, boredom, or some other context effect associated with completing a battery of individual difference measures. Although Krishna and Rao (1991) found no effect of ESP test

feedback on a self-report personality measure, the results reported above should be interpreted with caution as the order effects noted have the potential to cause both Type I and Type II errors.

Furthermore, it should be noted that the manipulation check employed in this study assumed that participants' enjoyment of the experience of completing the contingent positive or negative reward task was consistent with how they rated the images in the task. However, it is possible that participants enjoyed looking at the images they considered to be unpleasant, or did not enjoy looking at the images they found to be intrinsically pleasant, and hence did not necessarily find completing the negative reward task to be a negative experience (and vice versa). Therefore, in addition to participants rating the pleasantness of the images, a more reliable manipulation check would have been to ask participants to rate how pleasant they found the overall experience of completing the contingent task.

In addition, a reviewer pointed out a potential limitation of the procedure used to derive contingent positive or negative reward stimuli that are specifically tailored to the visual preferences of participants. As participants rated sample images from the contingent image subsets before the main psi task, they were necessarily exposed to the type of affect-laden stimuli that they would see in the positive or negative reward task. This therefore has the potential to either sensitise or desensitise participants and consequently alter the nature or magnitude of the effect of the contingent task. In future studies, the differential effect this may have on participants may be explored using the Miller Behavioral Style Scale (MBSS), a personality measure that has been used to predict the extent to which individuals continue to monitor an unpleasant or threatening situation rather than find a way to distract themselves (e.g., Miller, 1987).

Generally, then, the results observed in the present study are not well aligned with those discussed by Sherwood et al. (2005) and Roe et al. (2006, 2007). Sherwood et al.'s findings emphasised the roles of mood, optimism, and confidence and were partially supported by Roe et al.'s two studies, which suggested effects of mood and expectancy on psi task success. However, the results of the present study indicate that other properties of participants' disposition, such as their level of relaxation, as well as their rapport with the experimenter and spontaneity of their interaction with the experimenter, might have had some bearing on psi scoring rates. Overall, these analyses add further to the argument that the experimenter may play a critical role in the success of an experiment, with the mixed pattern of results across different studies implying that experimenters' influence might differ by task type and personal characteristics.

With regard to this experimenter and task specificity, based on results from previous ganzfeld studies (Dalton, 1994; Dalton & Utts, 1995; Roberts & Hume, 2010), a final prediction in the present study was that participants would perform better when they were recruited and briefed by an experimenter of the opposite sex. It was found that participants did perform slightly better when working with an experimenter of the opposite sex, however not to a statistically significant extent. At face value, the lack of effect would seem to imply that the findings from the previous ganzfeld studies, which utilised a very overt form of ESP in conjunction with a mild altered state of consciousness induction, do not necessarily apply to the nonintentional psi paradigm. However, in light of the lack of a clear psi effect in this study overall, this is another finding which remains difficult to evaluate. Only with a more extensive database including more overall successful studies can hypotheses relating to these effects be evaluated with any degree of conviction. Future researchers are therefore encouraged to assemble experimental teams consisting of both male and female investigators and consider mixed-gender experimenter-participant pairings as a potential predictor of success.

Finally, the methodological development implemented for the present study of allowing participants to indicate their image preferences by means of a keyboard button press rather than a mouse click enabled a more reliable measure of their response times to be recorded. It was found that the response times of trials in which participants scored a hit were significantly lower than those in which they scored a miss. This supported the hypothesis that those employing genuinely spontaneous selection strategies would perform better at the task. Relating this finding back to cases of spontaneous psi on which the PMIR model is largely based, this result would seem to support the idea that in many cases, the need for instinctual choices and time for cognitive deliberation would obviate the gains from the psi-mediated instrumental response. However, it is worthwhile to consider that rapid responses do not necessarily directly imply spontaneity,

and there may also be occasions in which rapid responses do not favour positive psi-mediated outcomes. Indeed, Honorton's (1987) work with a purportedly psi-gifted individual, Malcolm Bessent, revealed that psi-hitting was associated with longer response times than psi-missing in a forced-choice precognition task. Selections on such successful trials were more commonly labelled by Bessent as driven by cognitive impressions rather than by feelings or just being guesses. It can be speculated that in trials with longer response times, Bessent was able to achieve a relatively neutral state of mind that better allowed for psi-mediated impressions to be formed. A similar observation was reported by White (1964) with experienced psi experimenters. However, these findings relate to intentional forms of psi and, unlike the present study, were from experienced or gifted individuals rather than relative novices. Nevertheless, future researchers may wish to consider including a more direct measure of response spontaneity in conjunction with response times as a more comprehensive basis for predicting success in a nonintentional context.

This study is the fourth of its kind in this series that, in combination with other nonintentional precognition studies, provide an evidential basis against which the claims of theories such as Stanford's PMIR model can be evaluated. Despite incorporating a number of further refinements to the successful nonintentional psi protocol developed by Luke, Delanoy, et al. (2008), this study failed to demonstrate an overall nonintentional precognition effect. Nevertheless, a meta-analysis of the eight Luke and colleagues and Hitchman and colleagues studies combined that used this paradigm demonstrates that the overall paradigm still presents significant evidence of tacit psi, with Stouffer $Z = 3.94$, $p = .00004$ (cf. Rosenthal, 1991, p. 93) and a mean effect size of $ES(r) = .28$.

In turn, the lack of an overall psi effect makes interpreting findings in relation to the predictors somewhat problematic. A number of significant relationships were detected between participants' precognition scores and scores on various individual difference measures. In particular, there were significant positive correlations between participants' precognitive performance and their ratings of the positivity of their interaction with the experimenter, their rapport with the experimenter, and their level of relaxation, whilst emotional reactivity scores were once again found to be predictive of precognitive performance. Given the number of comparisons conducted, and the absence of adjustment of alpha levels in light of this, the reader is advised to interpret these effects with a sizeable amount of caution. Nevertheless, they all contribute to the growing database of relationships between psychological variables and nonintentional precognition scores that can be used in subsequent combined analyses.

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Appendix
Interaction Questionnaire—Experimenter

Participant Number:	
Date and Time of Session:	
Experimenter:	
Relationship with Participant:	<div style="display: flex; justify-content: space-around; padding: 5px;"> complete stranger short prior interaction acquaintance friend </div> <div style="margin-top: 10px;">further explanation:</div>

1. How would you rate your current mood?

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

negative

positive

2. How do you feel at this moment? / How relaxed are you at this moment?

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

tense

relaxed

3. How would you rate the quality of the interaction between you and the participant?

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

very cold

very warm

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

“rehearsed”

spontaneous

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

very negative

very positive

4. How would you describe the quality of rapport that you have with the participant?

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

extremely poor

extremely good

5. How confident are you that the participant will contribute towards the success of this experiment?

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
1	2	3	4	5	6	7

not at all confident

extremely confident

Abstracts in Other Languages*Spanish***LOS EFECTOS DE LAS CUALIDADES DE INTERACCIÓN EXPERIMENTADOR-PARTICIPANTE EN UNA TAREA DE PRECOGNICIÓN NO INTENCIONAL ORIENTADA A UN OBJETIVO**

RESUMEN: Varios estudios recientes inspirados en teorías de psi como la de Respuesta Instrumental Mediada por Psi (PMIR) de Stanford han empleado un protocolo de precognición tácito para poner a prueba la idea de que la percepción extrasensorial puede ser no intencional. Después de un notable éxito inicial, los resultados han sido más inconsistentes. Una posible razón de la variabilidad observada en los resultados es que diferentes experimentadores llevaron a cabo los estudios. Por lo tanto, el presente estudio evaluó varias dimensiones sobre la interacción de los participantes con experimentadores hombre o mujer. 52 personas participaron en 12 pruebas no intencionales de precognición y un contingente a la tarea positivo o negativo dependiente del rendimiento. El número total de aciertos precognitivos estuvo ligeramente por encima del azar, pero no alcanzó significación estadística. Hubo correlaciones positivas significativas entre las puntuaciones de los participantes en precognición y sus clasificaciones del agrado de la interacción con el experimentador y su rapport con él/ella, y el nivel de relajación del participante. También hubieron diferencias notables entre los dos experimentadores con respecto a sus puntuaciones sobre la interacción participante-experimentador y las puntuaciones tácitas de precognición de los participantes. Todas las correlaciones fueron en la dirección prevista para la experimentadora, pero en dirección contraria para el experimentador.

*French***LES EFFETS DES QUALITÉS DE L'INTERACTION EXPERIMENTATEUR-PARTICIPANT DANS UNE TÂCHE DE PRÉCOGNITION NON-INTENTIONNELLE ORIENTÉE PAR UN BUT**

RÉSUMÉ : Plusieurs études récentes, inspirées par des théories du psi telles que le modèle de la réaction instrumentale médiatisée par le psi (modèle PMIR de Stanford), ont employé un protocole de précognition tacite pour tester la notion d'une non-intentionnalité de la perception extra-sensorielle. Après un succès initial remarquable, les résultats ont été plus inconsistants. Une des possibles raisons pour la variabilité observée dans les résultats est que ces études furent conduites par des expérimentateurs différents. La présente étude interroge donc de nombreuses dimensions relatives à l'interaction des participants avec soit un expérimentateur mâle ou femelle. 52 participants ont pris part à 12 essais de précognition non-intentionnels et une tâche soit positive, soit négative, contingente à leur performance à la première tâche. Le nombre total de succès précognitifs était marginalement au-dessus de ce qui était attendu du hasard, mais insuffisant pour atteindre la significativité statistique. Il y a eu des corrélations positives significatives entre les scores de précognition des participants et leurs évaluations positives de l'interaction avec l'expérimentateur, de leur rapport avec l'expérimentateur, et de leur niveau de relaxation. Il y a eu également des différences notables entre les deux expérimentateurs par rapport à leurs relations entre leur interaction participant-expérimentateur et les scores de précognition tacite des participants : toutes les corrélations furent dans la direction prédite pour l'expérimentateur femelle, mais dans la direction opposée pour l'expérimentateur mâle.

*German***DIE AUSWIRKUNGEN DER BESCHAFFENHEIT DER EXPERIMENTATOR-TEILNEHMER-INTERAKTION IN EINER ZIELORIENTIERTEN NICHTINTENTIONALEN PRÄKOGNITIONSAUFGABE**

ZUSAMMENFASSUNG: Mehrere neuere Studien, die durch Psi-Theorien wie Stanfords Psi-vermittelte instrumentelle Reaktion (PVIR) inspiriert wurden, haben das Protokoll einer impliziten Präkognitionsauf-

gabe verwendet, um die Annahme zu testen, daß außersinnliche Wahrnehmung nicht intentionaler Natur sei. Nach anfänglich bemerkenswertem Erfolg, wurden die Ergebnisse uneinheitlicher. Eine mögliche Ursache für die beobachtete Variabilität der Ergebnisse könnte darin liegen, daß die Studien von verschiedenen Experimentatoren durchgeführt wurden. Die vorliegende Studie berücksichtigt eine Anzahl von Dimensionen bezüglich der Interaktion der Versuchsteilnehmer mit einem männlichen oder weiblichen Experimentator. 52 Teilnehmer nahmen an 12 nicht intentionalen Präkognitionsdurchgängen teil und an einer positiven oder negativen Abschlussaufgabe in Abhängigkeit von ihrer Trefferleistung. Die Gesamtzahl der präkognitiven Treffer lag knapp über der mittleren Zufallserwartung, erreichte aber keine statistische Signifikanz. Es zeigten sich signifikante positive Korrelationen zwischen den Präkognitionstreffern der Teilnehmer und der Einschätzung ihrer positiven Interaktion mit dem Experimentator, ihrer Einstellung zum Experimentator und dem Grad ihrer Entspannung. Es zeigten sich auch deutliche Unterschiede zwischen den beiden Experimentatoren in Bezug auf die Zusammenhänge zwischen ihrer Einschätzung der Teilnehmer-Experimentator-Interaktion und den impliziten Präkognitionstreffern der Teilnehmer; beim weiblichen Experimentator fielen alle Korrelationen in der vorausgesagten Richtung aus, beim männlichen Experimentator war es allerdings umgekehrt.

TAMING THE GHOST WITHIN: AN APPROACH TOWARD ADDRESSING APPARENT ELECTRONIC POLTERGEIST ACTIVITY

By John G. Kruth and William T. Joines

ABSTRACT: A report of electrical disturbances in a family home prompted a physician to contact the Rhine Research Center to explore a potential case of RSPK. This investigation provided much anecdotal and some strong evidence for an electronic poltergeist disturbance (EPD), a form of RSPK characterized by the disturbance of electronics and the signals from electronic devices. Strong evidence for EPD activity was observed by a computer systems expert in the behavior of a computer/printer system on a home network. A person-centered research approach was developed based on the advice of RSPK experts and experiencers. Meditation, mindful awareness, and targeted focus are introduced as activities that may help to manage or eliminate the unwanted activity by reducing the anxiety often observed in RSPK agents. In this situation, the EPD events dissipated over a 3-month period. Though family dynamics and other contributing factors complicated the evaluation of the effectiveness of mindfulness and relaxation techniques, the RSPK agent reported a calm and confident attitude after using these techniques regularly. Researchers are encouraged to explore the effects of relaxation and mindful awareness to determine if they are helpful to other RSPK agents in their quest to manage and control unwanted activity.

Keywords: poltergeist, RSPK, meditation, electronic disturbance, EPD

Poltergeist activity has always been a disturbing and frightening experience for the people involved in the cases. From stones that rain from the sky (Holder, 2012) to broken crockery and glassware (Pratt & Roll, 1958) to bite marks and scratches (Roll, 1976), the poltergeist has terrorized children, parents, and visitors to “haunted locations.” The popular media representation of these mischievous and noisy events has been couched in a terrorizing cloak that emphasizes the vengeful and damaging nature of the poltergeist (Craven, 1976; Spielberg, 1982) and stigmatizes any locations or people that are experiencing poltergeist-type effects.

Despite the terrifying and disturbing nature of these events, with very few exceptions (Carpenter, 1993; Carrington & Fodor, 1951; Roll, 1977; Roll & Storey, 2004), very little research has been done into the effects that these activities have on the agents and their families. The majority of parapsychological research has been focused on the nature of the phenomena and the observation of poltergeist activity. With a keen scientific eye, researchers have watched for fraud and examined the psyche of individuals expressing RSPK in an attempt to uncover the origins and nature of the poltergeist activities (e.g., Palmer, 1974; Roll, 1976). When unexplained RSPK is directly observed by a researcher with an inquiring scientific mind, it is natural for him or her to search for a physical explanation of the phenomena, and when one cannot be found, it is also natural for her or him to try to understand how the phenomena occurred. Though there is no clear explanation of how the phenomena occur, poltergeist activity has been well documented by numerous researchers in the past, and further scientific evidence of its existence is necessary only for those among us who are most skeptical (Palmer, 1974). It is important for researchers to understand these events and how they occur, but the overriding theme in nearly every poltergeist story is the terror, disturbances, and discomfort experienced by the people closest to the poltergeist activity.

In the context of an investigation of a reported poltergeist situation, the nature of a possible electronic poltergeist disturbance (EPD) is presented as a subtle phenomenon that affects electronic devices. More importantly, this study is focused on the effects of the events on the agent and the family of the agent, and a general approach is proposed for assisting the poltergeist agents and their families in alleviating the poltergeist activities or managing the reactions to the unexpected and potentially unwanted phenomena. This methodology is presented as a starting point for future investigators who wish to address the needs of the agent and the people most affected by this type of unwanted experience.

A Very Brief History of the Poltergeist

The modern image of the poltergeist is derived from ancient reports of mischievous and harmful spirits who chased families from their homes and caused significant disturbances in the lives of parents and children. Reports as early as 530 A.D. discuss stone-throwing demons and demonic possession (Holder, 2012), and reports from the 17th century describe the movement of household objects and banging noises (Gauld & Cornell, 1979). Little has done more to influence the popular image of this noisy spirit phenomenon (Shepard, 1978) than the *Poltergeist* films that were first produced in 1982 depicting the horrors of hauntings and possessions that originated in homes that were built on top of cemeteries (Spielberg, 1982). In many recent film depictions, an enraged or disturbed spirit plagues an individual or a number of people who are recognized as the victims of the tormenting discarnate spirit.

Despite the popular depiction of these events, poltergeist activity is elusive and typically difficult to observe (Carrington & Fodor, 1951). Researchers have had some success in observing poltergeist activities, and the famous Seaford Case, which was documented by J. G. Pratt and W. G. Roll in 1958, captured the attention of the nation and was well covered in the popular media of the day. It was because of this case that the concept of recurrent spontaneous psychokinesis (RSPK) gained popularity and the activity was identified not as ghostly mischief, but as the unconscious psychokinetic response of an emotional child (Pratt & Roll, 1958).

Roll continued to investigate and observe numerous examples of macro-PK events centered on disturbed teenagers and young adults, including a 1961 case in New Jersey, a “biting poltergeist” in Indianapolis in 1962, and the Miami Poltergeist in 1967. In each case investigated by Roll, there seemed to be a person at the center of the activity. The activity would only occur when that person was around, and when that person, or agent, left, the activity would stop (Roll, 1976).

Contrary to the popular image of a spirit haunting a location, the poltergeists observed by Roll and Pratt appeared to be associated with a person. Typically this person was a teenager who was either going through puberty or was involved in an emotional family or personal situation (Roll, 1976). This representation of a disturbed teenager producing macro-PK activity was publicized in the Hollywood film *Carrie* (Craven, 1976), but the images presented in this film were more like the previous depictions of an angry spirit than they were like the mischievous behavior that was observed in the field investigations completed by trained scientists.

Each of these cases is mentioned along with the specific references in popular culture to help reveal not only the nature of the phenomena, but also how the phenomena are viewed by the public. People who are not initiated in the field of parapsychology will gather most of their knowledge of unusual phenomena such as poltergeist activity from the media sources that are most available to them, and the two motion pictures that were mentioned above are the iconic images of the poltergeists that have been used as the basis for numerous additional films, television shows, books, and even comedy sketches, for example, Saturday Night Live (Michaels, 1986). Each of these media channels contributes not only to the popular and terrifying image of poltergeists, but also to the anxiety and stress experienced by poltergeist agents and their families. Due to the extraordinary depictions of these events in popular culture, agents who demonstrate poltergeist effects are often singled out, avoided, and even ridiculed. This type of treatment can contribute to the stress and anxiety of the poltergeist experience and cause the activity to increase, beginning an escalating cycle of activity for the RSPK agent in a poltergeist case.

Theoretical Interpretations

At this point, it is valuable to note the theoretical proposals for poltergeist activities. In fact, “poltergeist” is a theory-laden term that implies the activity is the result of a disturbing spirit (from the German *Poltern Geist*). The theory that a spirit is responsible for the activities has been traditionally applied to location-based cases (Irwin & Watt, 2007), and it implies an underlying intelligence that may result in activity that is an attempt to communicate or the purposeful movement of objects in a controlled manner (Gauld & Cornell, 1979). In support of this theory are the cases where apparitions are observed in the location of the activity, but these cases have been reported in only 23% of the cases studied by Roll, 12% of the cases reported by the Institut für Grenzgebiete der Psychologie und Psychohygiene (IGPP) in Freiburg, Germany, and fewer than 38% of the cases gathered by Gauld and Cornell (Williams & Ventola, 2011). Because the majority of the collected cases are not tied to specific locations, and there are no indications of a specific intention or intelligence and no reports of apparitions, there are reasons to explore other explanations for this typically unwanted and mostly unexpected activity.

The RSPK theory, which is described in detail above, is a person-centered model which postulates that psychological factors and stresses induce unconscious and unintentional psychokinesis that manifests as poltergeist-type activity. This theory accounts for nearly every piece of evidence that is favorable to the spirit-centered model of poltergeist activity, and it has been strongly supported in investigated cases since it was first popularized by Pratt and Roll (1958) with the Seaford case. This model was later refined by Roll to include a neurological mechanism by which the unconscious PK is produced (Montago & Roll, 1983; Roll & Persinger, 1998). Though it was supported by Solfvín and Roll (1976) in their identification of patterns of epilepsy in the EEG of a few poltergeist agents, the neurological abnormalities that have been presented as indicative of this type of activity are not rare in growing teenagers, and so the evidence for this specific neurological mechanism is weak (Martinez-Taboas & Alvarado, 1981).

Despite the lack of physical evidence for abnormal brain activity in people who experience these phenomena, the general RSPK model remains a strong candidate for being the basis of the observed phenomena. In the present investigation, the RSPK model was assumed, and the report includes reference to this model to the exclusion of other theoretical explanations.

The Evolution of the Poltergeist

A review of the professional publications in the field of parapsychology reveals a strong interest in poltergeist phenomena from ancient times through the decade of the 1980's. The earliest literature from the first millennium focused on demonology and religious spirit possession (Gauld & Cornell, 1979), while literature from the 19th and early 20th centuries revolved around the materializations and auditory phenomena typical of physical mediums, the effects produced by spirits during séances (Podmore, 1963), and spontaneous activities demonstrated by spirits in relaying a message to a living person (see Gurney, Myers, & Podmore, 1886). The literature from the mid to late 20th century revolved around RSPK and the macro-PK effects that were generated by RSPK agents, resulting in objects moving through the air, furniture turning over, glassware spilling its contents, pebbles raining from the sky, and so forth (e.g., Auerbach, 2004; Pratt & Roll, 1958; Roll, 1976; Roll & Joines, 2013; Roll & Storey, 2004). With a few exceptions (e.g., Auerbach, 1993, 1996, 2004), the poltergeist has not been seriously studied by parapsychologists since 1990.

In the 21st century, people have developed a stronger dependence on electronic equipment and wireless communication networks. Before 1990, the major electronic appliances in the home were typical kitchen appliances, the wired telephone, and the television set. Today, the majority of the population in most developed countries regularly uses computer systems, wireless network devices, and, of course, nearly everyone carries a cellular telephone. Electronics and mini-computers are embedded in television remote controls, microwave ovens, washing machines, cars, and even keys and some light bulbs. The wireless revolution has exploded and the world around us is filled with microelectronics.

Where the RSPK activity of the past was macro-PK, including the movement of large objects, today's RSPK is likely to be more subtle. It is common to have trouble with a computer only to find that the

problem is fixed after a brief coffee break and calmer nerves. We typically attribute this to *user error* or a *glitch* in a computer program. Cell phone malfunctions are attributed to thunderstorms, a bad connection, or a low battery. Of course, these explanations all have validity and are often the culprit behind the malfunction, but in some cases there may be other factors that contribute to the interference or signal failure. Studies by Morris (1984, 1986) and Radin (1990) examined the effects that humans have on electronic devices with results indicative of an unintentional influence. As the use of electronic devices becomes more prevalent and as the devices become more integrated with the physical body, there are more opportunities for RSPK to be expressed as micro-PK effects that cause the malfunction of electronic equipment that we use every day.

Defining Electronic Poltergeist Disturbance

Whereas macro-PK has been defined as the movement of large objects such as bottles, stones, furniture, and other household objects without any obvious physical interaction, micro-PK is defined in the glossary of the *Journal of Parapsychology* as PK effects on quantum events or systems. Some events are clearly macro in nature (e.g., moving a statue across a room) whereas some are clearly micro in nature (e.g., effects on random number generators). Between these extremes, there is a continuum of phenomena that does not easily fit in either the macro-PK or micro-PK category. Are the small effects needed to affect a roll of dice micro- or macro-PK? Is the disruption of a beam of light or a radio signal the same as affecting a quantum system? These questions and questions about similar events lead to the conclusion that there is a continuum of PK phenomena, but it is beyond the scope of this paper to closely examine the labeling of all PK phenomena.

Helmut Schmidt (1970) demonstrated that electronic systems governed by quantum events could be affected by an observer, and subsequent studies at the PEAR Labs in Princeton, NJ, provided evidence that the intention of an observer could cause statistically significant effects on the randomness of events including the fall of balls, the movement of a pendulum, the patterns of a water fountain, and random events created by monitoring quantum processes (Jahn, Dunne, Nelson, Dobyns, & Bradish, 1997). All of these events have been characterized as micro-PK in different forums. For the purposes of this paper, the interference of electronic systems, electronic devices, and the disruption of signals produced by electronic systems will all be considered micro-PK effects.

Electronic poltergeist disturbances (EPDs) are unconscious micro-PK events that affect electronic devices or signals and occur near an RSPK agent who is likely unaware of the origin of the event. Some examples of possible EPDs are the disruption of the electronics of a computer, a household appliance, an alarm system, or a vehicle that uses electronics or computers as a control system. EPDs could be seen in cell phones or in the disruption of the signals sent from cell phones or on wireless networks. Many things could disrupt these devices or signals, but the defining characteristics of EPDs are that they are unintentional, often unnoticed, and as is the case in other RSPK cases (e.g., Carpenter, 1993; Roll & Pratt, 1971; Roll & Storey, 2004), these disruptions could originate from an individual who is experiencing stresses, pressures, or anxiety that could lead to repressed aggression or anger.

A Possible Case of Electronic Poltergeist Disturbances

In October of 2013, JGK was contacted by a medical doctor who indicated that he had an 11-year-old male patient who was experiencing unusual electrical activity. This activity was disturbing to his family, and they approached this physician to help to eliminate the problem. As it was described to the doctor, the boy could not use a cell phone due to interference that was produced when he was speaking on the phone. Electrical devices malfunctioned or behaved erratically when he was near them. The doctor reported that most disturbing to the family were the problems that developed with computers at the boy's school. Malfunctions were being interpreted by his teachers as pranks or electronic sabotage, and he was being punished by his teachers for these disturbances. The doctor examined the boy and found him to be in perfect health, and he was looking for assistance in addressing this phenomenon.

The initial research concern in this case was that the doctor, the young boy, or his family were looking for attention and publicity. There was skepticism about the reported activity, and this situation called for extreme circumspection to avoid any obvious fraud or malicious intent on the part of the parties that reported the activity. Before beginning our investigation or exploration of the phenomena, a preliminary interview was arranged with the doctor, the young boy, and his family.

The interview included the young boy, his mother, his grandmother, and the doctor. The boy lived with his mother and grandmother and he had never known his father. Prior to the interview, JGK met with the doctor privately and reviewed his reports to insure they were consistent with the original discussions of the activity. Subsequently, the family joined JGK and the doctor and they discussed why they were meeting.

The family expressed anxiety and concern about the activity, the expense related to the events, and possible health hazards that they thought might be related to the events. They described electrical appliances that were turning on and off without any physical contact. They mentioned that the grandmother's computer would crash when the boy came near. They described the TV and the computer-controlled washing machine breaking when he was near. Telephones would act in strange ways, car doors would lock and unlock, car windows would go up and down, and the electrical gauges in the car would not operate correctly when he was in the front seat.

At school, computers would crash when the boy tried to use them, printers would act very strangely, cell phones would sometimes make sounds when he was upset, and the scores on standardized tests (scored automatically by computer) would not register when he took the tests. The teacher did not believe that the boy was affecting these machines without touching them, and she often embarrassed him when things like this happened in class.

Besides the inconvenience of these events, the family expressed concerns about the expenses to replace many electronic items. The grandmother provided a list of 31 separate unusual happenings that had occurred near the boy since November of 2012, and the boy expressed a concern that he was going to be the cause of health issues with his family if these activities continued.

The concern and sincerity of the family seemed genuine in the personal judgement of JGK, so he agreed to further discuss these issues with the family at a later date.

Developing an Approach to the Investigation

Poltergeist activity is rare in our culture, and rarer still is the opportunity to explore poltergeist agents in the carefully controlled environment of a parapsychology laboratory under the watchful eyes of trained observers and experts in the phenomena. In rare situations, researchers trained in observation and critical thinking have observed apparent poltergeist events in uncontrolled environments such as private homes and public locations (e.g., Pratt & Roll, 1958; Roll, 1976; Roll & Pratt, 1971), and at times these same researchers have been able to examine the physiology and psychology of a possible poltergeist agent by bringing the potential agent into a controlled laboratory environment (Roll & Storey, 2004).

The case of the Miami Poltergeist (Roll & Pratt, 1971) provided an opportunity for the researchers to explore the psychology of Julio Vasquez, who was present during all reported cases of object movement. In this case, psychologists determined that the employee displayed signs of dismay and anger towards the owner of a public warehouse due to his phony demeanor. He also showed a number of specific psychological characteristics including high moral standards, feelings of guilt and rejection, passivity and inaction, detachment, and dissociative tendencies in his expression of aggression.

In the case of Tina Resch (Roll & Storey, 2004) feelings of shame and guilt were interpreted to have contributed to Tina's rich fantasy life, and she exhibited tendencies to use aggression to release the anxiety produced by situations that brought on the guilt (Carpenter, 1993). Despite her predisposition towards producing grand displays to distract people in social situations, the observed RSPK activities were interpreted as an expression of her dissatisfaction and stress related to her family life (Carpenter, 1993). In an additional review of 10 specific cases, Gauld and Cornell (1979) indicate that the most common theme in analyses of RSPK agents is repressed aggression and tension within the family (p. 341). Further investigations of

potential RSPK agents by Roll in the chapter Poltergeists in the *Handbook of Parapsychology* (Roll, 1977) indicated that 49 of 92 person-centered cases involved people with medical or psychological problems.

If RSPK were to be discovered in this investigation, there might be psychological issues related to repression of emotions and the tendency to avoid the expression of aggressive feelings. In addition, family pressures and tensions may be present that would complicate the observation of the events. In order to ameliorate the potential risks and ethical issues related to this visit, the researcher took precautions to insure that the young boy and his family would be protected while he was conducting the investigation and assisting the family in fulfilling their desire to eliminate the electrical disturbances or adapt to them.

Preparing for the Investigation

In preparation for this investigation, JGK recruited the assistance of a number of professionals who had previous experience working with children or in cases of possible RSPK.

1. A poltergeist agent who had been studied by scientists in three laboratories was contacted to provide counsel on her approach to the anxiety and inconvenience of managing unintentional movement and electrical disruptions in her life. In addition, she discussed her experiences with her family while she was trying to eliminate these activities from her life.
2. Loyd Auerbach, a professional field investigator and mentalist, provided advice on how best to observe the situation and what types of activities had helped other people who experienced other types of RSPK.
3. Dr. Athena Drewes, a psychologist who works with children with exceptional experiences, was contacted to discuss how best to approach a child in this type of situation while maintaining a critical eye towards the reality of the phenomena.
4. Dr. Sally Rhine Feather, a professional psychologist, was contacted to be available during the investigation to assist in any potential counselling needs if they would arise. In addition, the family's doctor was available via phone in the case that he may be needed.
5. Finally, Dr. William T. Joines, an experienced researcher and the second author, expert on electrical fields, and a veteran of numerous poltergeist investigations, accompanied JGK to provide a second objective perspective on the events and to help evaluate how best to manage ostensible RSPK events should any be observed.

The Research Approach

An investigation of this type requires careful planning and caution. The first step in developing an approach is to determine the goals of the investigation. In this case, the goals were two-fold. First, determine whether the reported events truly occurred, and second, since the family expressed a specific desire to eliminate electrical disturbances, provide the family with some tools that may help them to reduce the frequency of the occurrences or refer them to a professional if necessary.

Based on the advice of researchers, counsellors, and a person who had previously experienced RSPK, the following three-part approach was developed to assist the family in reducing the frequency of the electrical disturbances and to assist the family in managing the activity. None of this was designed to address family dynamics, provide guidance or counselling to the family, or advise the family on psychological or medical issues.

Calming the mind through anxiety reduction. One of the major psychological factors described in previous RSPK cases involved anxiety and the avoidance of expressing aggressive feelings (e.g., Gauld & Cornell, 1979; Roll & Pratt, 1971). Following the assumption that anxiety may play a part in the RSPK activity, it is reasonable to postulate that a reduction in anxiety may in turn reduce the unintentional PK activity. Numerous studies have demonstrated the benefits of simple relaxation and meditation techniques in reducing anxiety (e.g., Chen et al., 2012; Kabat-Zinn et al., 1992; Koszycki, Bengner, Shlik, & Bradwejn,

2007), so this information was communicated to the potential RSPK agent and family.

Mindful awareness. Mindfulness meditation techniques have been demonstrated to be effective in alleviating generalized anxiety disorder (Evans et al., 2008), but it also is a way to bring one's attention to the activities and the experience of the current moment (Baer, 2003). Since RSPK type events are spontaneous and unpredictable in nature, there may be consistent events that trigger the activities. By becoming aware of these triggers, an RSPK agent may be able to reduce the impact of the events or even prevent them completely.

If information was requested to reduce the frequency of the events, mindful awareness was to be presented as a technique that may help to increase awareness of activities and feelings just before and during an unintentional PK event. The agent may use this technique to develop an awareness of a possible trigger for the event. With this additional knowledge, agents can seek assistance in managing those feelings or find a method for handling those feelings themselves.

Rechanneling focus. Conversations with Auerbach and a person who had experienced RSPK activity personally indicated that unconscious or unintentional PK activity might be calmed if agents focus their attention on performing purposeful PK. It is not clear whether this would be a method to release some of the potential PK activities that have not been expressed or whether the act of focusing on intentional PK provides a type of psychological benefit to the agent. In any case, the recommended focus item was a "psi wheel"—a handmade device with a light pinwheel balanced on top of a needle. Since this type of device is easily affected by normal physical activity, it was recommended that the wheel be properly shielded to ameliorate the influence of heat or air flow. This was accomplished by placing the device inside a large jar with a sealing lid.

Additional information. One of the main recommendations from the person who had experienced RSPK activity and had successfully reduced the frequency of the events was related to the reactions of her family. She recommended that the family attempt to react casually to unexpected electronic occurrences rather than respond with surprise or anger to the events.

Summary. If the participant and/or the family requested information on how to reduce the frequency of the events, they were to be provided with information about relaxation and meditation, mindful awareness, and provided with an example of how one person rechanneled their unintentional activity to an intentional event. In addition, the family was to be advised to attempt to respond to an unexpected electronic event nonchalantly in order to reduce the focus on the unusual nature of the event.

The Complete Plan

The following plan was developed based upon the advice of researchers, counsellors, and a person who had previously experienced RSPK. The major theme of this approach is to maintain the comfort and confidence of the people reporting the activity while verifying the validity of the reported events. In addition, once the claimants are verified as serious (i.e., not seeking publicity), attempt to address their needs before completing tasks for scientific research. In other words, for ethical reasons, always put the needs of the participants ahead of the scientific research.

For example, in this case, scientific rigor would require a detailed inspection of the premises, interviews with workmen, teachers, and friends, and a review of documentation confirming the events reported by the family. A dispassionate scientist who is attempting to validate the phenomena and examine the mechanisms of the events may take these steps and include psychological testing and additional physical examinations of brain activity. These steps would be required in a pure scientific approach to the reports, but they do not consider the needs of the people experiencing these events.

The anxiety and concern expressed by the family indicated a focus on eliminating the activity, illustrated by their initial contact with a medical professional to help treat the child for the problems they were observing. When considering the needs of the family and the apparent PK agent, this approach recommends that validation of the activity is necessary, but not the primary goal of the first session. Once apparent activity is observed by the researchers, the researchers should respect the family's requests for help in eliminating

the phenomena if possible. If the problem persists, further investigation, including collecting detailed documentation and conducting psychological testing, would be necessary to further validate the phenomena and to provide information that may help to address the needs of the family.

The steps of the approach are as follows:

1. When a claim of unusual activity is initially reported, acknowledge it sincerely and seriously to assure the persons reporting the claim that they will not be ridiculed or made to feel uncomfortable about their experiences.
2. Interview the claimant and those close to the claimant to determine their intentions and goals. If their intentions do not include eliminating the activity or learning more about the mechanisms behind the activities, consider rejecting the opportunity to investigate the phenomena.
3. Evaluate the desirability of continuing the investigation based on the desires of the claimant. A desire for publicity or a lack of concern about the phenomena may be a sign of fraud. Proceed with caution.
4. Design and communicate a plan to address the activity, insuring that the goals of the claimant are addressed in the plan. This provides a level of comfort and confidence in the researcher and the approach to the research. It is essential that the claimant be comfortable throughout the process and that his or her needs be acknowledged and addressed.
5. Validate the claim of activity using controlled methodologies in an environment that is comfortable for the claimant. Respect the claim but protect against deception or misinterpretations of the events. When performing investigations outside of the laboratory, be sure to account for natural and environmental factors that might contribute to the activity.
6. Express the results of the validation clearly to formalize the recognition of the activities, and if there is an observed event, reassure the claimant that there is a real problem that has also been observed with other people. If there is no observed event, communicate that also. Sincerity and honesty are essential in communicating scientific observations.
7. Determine whether the activity is intentional or unintentional through interviews and observations.
8. Provide guidance for controlling unintentional activities. This includes a three-step process of anxiety reduction, mindfulness, and a redirection of energies using a focus object such as the psi wheel.
9. Follow up to address additional issues that may arise, such as a change in the expression of the phenomena (e.g., knocks occurring or larger objects being affected), an increase in activity, or signs that further psychological counselling may be warranted.
10. Follow up on the progress in alleviating the unintentional activities.

Investigating Possible EPD Phenomena

After the initial conversation and meeting with the family, in the judgement of the primary researcher, the family was seeking relief from the disturbances in their home and assurance that the activity would not result in health problems for the family. The family doctor provided the requested medical and health assurances. Before the investigation could proceed, the researchers attempted to verify that the reported events were not based on fraud, imagination, or a misunderstanding. Consultations with a number of professionals (see above) resulted in the development of a plan to approach the situation, and the approach was reviewed for ethical concerns by the Rhine Institutional Review Board and approved.

JGK and WTJ visited the family's home. The purpose of the visit was to observe the reported phenomena in an environment where it had been previously observed. The researchers purposely presented a friendly and approachable demeanor in order to make the family members comfortable with the situation and encourage their honesty and cooperation with the investigation. Before the researchers entered the

house, they inspected the outside of the house and did not notice any possible sources of electrical interference such as high tension lines or a power station or antenna nearby. The house appeared to have a normal electrical and phone wire running from a pole on the street to the house.

Numerous events were reported to the researchers by the mother and grandmother of the young boy in the family. A complete listing is provided in the appendix, but since most of these events occurred before the researchers' visit, there was no way to verify them. Some of these events occurred at the boy's school or at a friend's house, but the majority of the events were reported to have occurred inside the home. Since the activity was reported to be occurring nearly every day in the home or in the car, the researchers saw this as an opportunity to attempt to reproduce the most frequently reported events.

Though this investigation was carefully performed, there were some situations where the boy was out of sight of the researchers or when he may have used trickery to produce the effects. One reported event involved activity with the automatic garage door opener, but this proved to be a mechanical flaw that was mistakenly attributed to interference by the young boy. A second set of events was reported to involve the electrical system of the grandmother's car. Electronic gauges would fluctuate in unusual and unexpected ways, and the windows and door locks would open and close as someone drove down the road. With the researchers in the car with the grandmother and the boy, no electrical interference was observed, but the door locks and the car windows did behave in an unusual manner when the boy was in the front seat. Unfortunately, his hands and the hands of the grandmother were not visible to the researchers, so there was no way to rule out trickery in this situation.

Phone Interference

The first reports of activity in the home were related to telephones. The wired telephones in the home began making noises when the boy walked by them. A short time later, the phones began to make repeated calls to the neighbors when nobody was dialing them. Also, the phone base would consistently make a sound like it was paging the handset even though the handset was securely in the phone base. These events prompted the family to contact a repairman from the phone company, and, according to their report of the situation, the repair man made many visits to the home but could not locate any problems that would cause the phone to call the neighbors repeatedly or make noises when none of the buttons were being touched. Despite efforts to replace the phone battery, move the phone to different outlets or locations, or replace the phone itself, the events continued to occur. The researchers did not verify the service calls made by the repairman, but instead asked if the boy could try to make a phone call with the phone in question.

The mother and grandmother both reported that the boy could not call a cell phone or use a cell phone because it would result in interference. They agreed to have the boy try to make phone calls while the researchers were present, using the phones that had produced problems in the past.

The boy first made a call from the wired phone in the house to the mother's cell phone, since this was a situation that had regularly produced interference. Within 15 s of the first call, the mother's cell phone began producing a loud sound from the speaker, which sounded like feedback. Both the grandmother and mother appeared undisturbed and familiar with this event, and the mother happily handed the phone to JGK, who heard the same sound. WTJ then took the cell phone and JGK went into another room to observe the boy and his behavior with the wired phone. While the boy was holding the wired phone, the interference sound was heard by WTJ on the cell phone, but when JGK took the phone from the boy, the sound stopped and the phone worked normally. The boy appeared to be very frustrated that he could not communicate through the phone, and he expressed his frustration by shaking the phone and yelling at it. Each time JGK gave the phone back to the boy, the interference pattern returned, and each time JGK took the phone, the phone once again acted normally. Even when JGK had the boy hold the phone away from his head, the interference continued.

This test was repeated with other phones including the cell phones of each researcher. In each case, when the boy held the phone while making a call, the phone would go dead and the phone that was being called emitted a loud sound similar to feedback. It did not matter if the phone was a cell phone or a wired

phone, which type of phone (iPhone or Android), or which carrier served the phone. In each case, the phone behaved the same way in the boy's hand.

The events were observed in different location around the house. The electromagnetic field near the boy was measured using an EMF meter from Walker Scientific, Inc. (model ELF-500), but no unusual readings were observed around the boy while he was at rest or while he was making an interrupted phone call.

Though these events were convincing and impressive, the researchers never asked the boy to empty his pockets or searched him for potential electrical interference devices. There are stories of gamblers using interference devices in their shoes to disrupt radio communication at a casino, so this is a feasible means of producing this type of effect. There was no obvious evidence of fraud, and since the goal of the investigation at this point was to address the needs of the family, the researchers did not explore every possible means of trickery related to the phone interference.

Printer and Computer

With respect to most of the events inside the house, neither of the researchers were specifically expert related to the devices or observed activity. One exception to this was interference related to the computer system and the printer. JGK has more than 20 years of experiences in the technical field including training in software development, hardware, and more than 17 years working as a professional for a large software company. This experience provides him with special insight into computer systems, specifically PCs and laptops using the Windows operating system, and how they interact with peripheral devices.

The grandmother reported that when the boy was near her desktop computer it would begin malfunctioning in different ways even when he was not touching the computer. The display monitor would go blank. The system would often display an error screen that would require a reboot to correct the problem. Sometimes the system would go into complete failure and it could not be restarted for a number of minutes. Eventually, the computer went into complete failure even though it was a fairly new system, and it had to be completely replaced. She reported that these events had occurred so often that she would not let him in the same room with her computer for fear that he would damage the new system.

The young boy had gotten a new laptop as a gift, and he was very excited but very cautious and focused when he used this system. He wanted to print a document from his laptop to the wireless printer to show it to the researchers. When he left the room to print the document from the laptop, the printer would become active and every piece of paper in the printer was fed through the printer without printing. The grandmother would casually turn the printer off, replace all of the paper, and turn it back on. The document was then printed normally. She explained that this happened every time the boy printed a document from any computer.

JGK, being knowledgeable about this computer system, recognized that there are specific commands that can be activated from a computer to cause a printer to feed paper through without printing, so he went to the laptop and carefully observed the boy as he rebooted the laptop, opened only the document to be printed, and pressed the print button. Again, all the paper was fed through the printer, and it had to be reset for the document to print. With the permission of the family, JGK took the computer and went through exactly the same process to print the same document. The document printed normally and the blank pages were not fed through the printer. This process was repeated by the boy and by JGK in different combinations of rebooting, opening the document, and printing. In each situation, when JGK printed the document the printer worked normally, but when the boy printed the document, the printer would feed all of the paper through before the document was printed.

Unlike the phone interference issues mentioned earlier, these events did not involve an uncontrolled signal interruption but rather a specific behavior activated in the printer when the print command was sent from the computer. This was a simple home network that consisted of one desktop computer, one laptop, and one networked printer. The fact that this behavior was only observed when the young boy issued the command, but never when JGK issued the command provides strong evidence that the events were not the result of fraud. Though a complex mirroring system in the home network could produce this effect and

enable fraud, an inspection of the devices on the network provided no evidence that such a system was in place and the family had no knowledge of additional computers or mirroring devices in the home.

As a side note, at one point in the process, JGK asked the boy to reboot his computer, and the system would not start. This was a computer that was less than one month old and it hadn't shown any signs of a problem besides the printer issue. When JGK saw the boy begin to get upset when the computer wouldn't start, he reassured the boy and asked him to relax and focus on their conversation for a moment. After just a minute, the computer restarted normally.

There are some hardware systems that are flawed when they are first purchased, and they may exhibit unusual behavior or failures. Some may even produce inconsistent behavior and operate normally one moment and fail the next moment. The same activities can produce failures one moment and work fine after they have been rebooted. In this situation, the request from the laptop was repeated multiple times by both the young boy and by JGK. In each situation using multiple procedures, the printer consistently worked flawlessly for JGK and consistently failed in the same manner for the young boy. These events are considered by the researchers to be the strongest evidence of an electrical disturbance that was centered on the young boy.

Other Activity

Other events occurred while the researchers were in the house, including the sounding of a smoke alarm and CO₂ detector when the boy walked past the device, unusual beeping sounds and partial rings from the wired telephone when the boy was near it, and unusual activity with kitchen appliances. Most of these were one time, isolated events that could not be verified because the researchers could not always see the boy or his hands while the events were taking place.

An exception was the activation of a toaster oven in the kitchen just before the boy had lunch. Lunch was slightly delayed, and the boy was excited that it was going to be prepared. He went into the kitchen and stood near the toaster oven waiting for his mother to make him a sandwich. He was obviously excited because he was jumping up and down slightly and saying "Oh boy! Oh boy! Oh boy!" over and over. The toaster oven was not turned on, but it got very hot to the touch. When the boy was asked to move into the other room, the toaster oven immediately began to cool down to normal temperature. The researchers were able to observe the boy and the toaster oven closely during this event, and the boy never touched the toaster oven to either turn it on or turn it off. After his lunch was made, he calmed down considerably, and fewer events were observed in the afternoon.

Post-Investigation Discussions and Education

After the observation of numerous events throughout the day, the researchers sat with the family members to discuss the results. They informed the family that some of the phenomena, such as those involving the garage door and the car windows, were likely due to mechanical issues that should be examined by a professional. The issues related to the phone and the computer system were unexplainable at that time and acknowledged as problematic activity that the family would like to stop. The other events were acknowledged, but it was communicated that they could not be scientifically recognized as unintentional electrical disturbances. The goal of this session was a truthful recap by the researchers to reassure the family that their experiences were being taken seriously and that some of the events were similar to events experienced by other RSPK agents.

At this point the young boy was asked whether he was purposely trying to make any of these events happen. He clearly indicated that he wasn't doing any of this on purpose, and when the family was asked as a group what they wanted from this investigation, they all agreed that their main goal was to put a stop to the undesired electrical disturbances. The mother wanted to avoid the expense of replacing electronic devices in the house. The grandmother was concerned about the perception of the boy at his school and the need to restrict him from certain activities or locations when electronic devices were in use. The boy himself want-

ed to be able to use a cell phone like other children his age, and he wanted to be able to use the computers at his school without breaking them.

Education

The researchers provided the family with information about the research results from other cases of RSPK. They assured them that similar activities had been observed with other people, and that some people had managed to control the unwanted activity by calming the mind with relaxation techniques, simple meditation, mindful awareness, and by redirecting their focus to attempt to produce purposeful PK events. The family had never done meditation or relaxation before, so JGK described the process to them and led them through a simple breathing exercise to help them to relax. They were also referred to websites and online search terms that might help them to learn more about relaxation methods.

JGK also taught the young boy how to create a simple psi wheel that could be used as a focus item for practicing purposeful PK. The family showed little interest in this item, and the young boy never ended up focusing his attention on this item.

Finally, when the young boy was out of the room, the mother and grandmother were advised that the activity may increase when the young boy is aggravated or anxious. In order to avoid increasing these feelings, the family was informed that other people had reported that they were able to decrease the activities when their family remained calm during the events and approached the situation in a light-hearted way.

Follow-up

In the following month, JGK kept in touch with the grandmother, and she reported a significant decrease in electrical disturbances in the house. The young boy was using his “breathing exercises,” and he was proud that he was able to avoid getting upset when he thought the events were going to occur. The RSPK events decreased significantly during this time, but they did not vanish completely.

Approximately two months after the visit, an unexpected family crisis caused fear and discomfort for all of the family members. As was reported to JGK by the family members, they were forced to leave their home for a period of time, and they cut all communication with their friends. During this time, knocking, tapping, and banging noises began to occur around the house and in the hotel room that was their temporary home. The effects were so loud that they thought somebody was trying to break into their room.

The family crises ended in about ten days after external pressures were removed, and with the end of the crises, the knocking stopped. Other RSPK activity also stopped, and for two years since the investigation, the young boy has continued to do relaxation exercises. No further RSPK events or electrical disturbances have been reported in the house, in the car, at the boy’s school, or at his friends’ houses.

Why Did These Events Stop?

Though there is a temptation to attribute the stop in RSPK activity to the relaxation methods and mindfulness exercises that started as soon as the researchers left, the psychology of the family dynamic is complex. There are many factors that may have contributed to the activity stopping, including the additional attention that this young boy got from the researchers who came to learn about him. The methods introduced in this investigation are a single isolated example, and they cannot be considered to be responsible for the cessation of RSPK events without further examination.

Summary

A report of electrical disturbances in a family household led a physician to contact the Rhine Research Center for an examination of the phenomena and for assistance in helping to stop the activity. Thanks to the advice and contributions of a number of professionals who were experienced with RSPK phenomena

and how to work with children, a research approach was developed to keep the focus on the people experiencing RSPK and the people close to them. In the course of this study, the investigation provided much anecdotal and some convincing evidence of electronic poltergeist disturbances (EPDs) which may become more prevalent with the closer integration of electronic devices in our lives.

The research method used in this investigation emphasizes the needs of people before the needs of the research project, so there were some gaps in the scientific controls in parts of the field investigation. The strongest evidence of a confirmed event is related to the unusual interaction between a laptop computer and a printer when being used by the 11-year-old boy. In addition, the heating of a toaster oven when the boy was anticipating lunch demonstrated a clear phenomenon that dissipated when the boy left the room. Though the lack of some scientific rigor may have caused the researchers to overlook potential trickery by the boy in some cases with the telephones, the car windows, and the smoke alarm, there is little doubt that the researchers observed some instances of electrical disturbances that are not the result of observable physical causes and that seem to be centered around the young boy in the home.

Since the main focus of this investigation was helping the person experiencing RSPK and his family to manage the events and potentially eliminate the unwanted activity, the major thrust of this study is the presentation of a methodology and the development of a simple process of relaxation and mindfulness that was developed to help to address the possible anxiety that has been present in so many other RSPK cases. The RSPK agent was also introduced to a method that had been used by another RSPK agent to focus attention on intentional PK in order to reduce the occurrence of unintentional PK activity. This focus method was never adopted in this situation, but it is still considered to be a valuable tool in helping RSPK agents to manage their activity.

Although the RSPK agent reported feeling more confident, calm, and in control after regularly using relaxation techniques, it is premature to attribute the reduced RSPK activity to relaxation and mindfulness. A volatile family situation increased the activity for a short time after the investigation, and after the family matters were resolved, the RSPK nearly disappeared completely. In addition, just the act of having researchers visit to investigate the phenomena may have produced a response in the young boy that reduced the RSPK events.

Nonetheless, the young boy felt better after using relaxation and mindfulness techniques, and shortly afterwards, the RSPK events stopped. Because of this, the person-centered methodology described in this case is presented as a basis for approaches to investigate other cases of RSPK activity, and the use of relaxation, meditation, and mindfulness should be further explored in future cases to determine if it is helpful to other potential RSPK agents.

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Appendix

The following is a list of 31 separate events observed by the family over a 12-month period. Some of these event occurred multiple times. These events are included to give a sense of the experience of the family, though almost none of these activities were observed and validated by the researchers. This information was reported by the family to their doctor and to the researchers but was not validated. All of these events refer to the young boy who was the apparent agent in this study, and they were submitted to the researchers on March 17, 2014.

1. Telephone rang and beeped when nobody was touching the phone and when nobody was calling. *(observed by researchers, but not thoroughly validated)*
2. Smoke alarm sounded when there was no sign of smoke. *(observed by researchers, but not thoroughly validated)*
3. Weather alert radio sounded an alarm when there was no sign of a weather emergency. *(observed by researchers, but not thoroughly validated)*
4. Sheets used for standardized testing at school didn't scan when boy completed them.
5. Panic alarm on car key fobs went off when boy held them even when he wasn't pressing any buttons.
6. Laptop stopped working, appearing to be due to electrical overload.
7. Cell phone displayed strange behavior, had issues with the screen turning upside down, and could not be repaired by the Apple Store.
8. Television changed channels and volume increased when nobody was touching the remote control. Eventually, the television, which was only a year old, stopped working completely and had to be replaced.
9. Washing machine which was operated with a computer touch screen lit up all the lights on the touch screen and changed cycles rapidly when boy was near it. Eventually, it stopped working completely. Now, the new washing machine is always unplugged, and boy is not permitted in the basement when it is plugged in or running.
10. Toaster heated up when boy was near it, even when it was not turned on. *(observed by researchers)*
11. Timer on microwave would not operate when boy was in the kitchen. The display went blank or all the lights lit up.
12. Accelerated Reader (AR) system at school that is used for computerized teaching and testing froze and stopped operating when boy was near.
13. Printers at school, at home, and at mother's work fed all the paper when boy was near. *(observed by researchers in the home)*
14. Hotel key card demagnetized when boy held it.
15. Check Engine light in car came on regularly when boy was in the front seat of the car. Mechanics couldn't find any sign of problems, and the light went off when boy was not riding in the car.
16. Bar code scanner at grocery store wouldn't scan items when boy was near.
17. Light bulb over the table at a restaurant went off when family was sitting at the table.
18. Cat responded to boy as if it was in pain when he was near. The cat howled loudly and ran from the room even though boy was not touching the cat. *(observed by researchers, but not validated)*
19. Metronome stopped working when boy was near it.
20. Two cassette tape recorders stopped working.
21. The thermostat for the house heating and air conditioning did not operate and the display panel became unreadable.
22. Multiple computers at school shut down when boy went near them. The teacher often made him use the oldest and worst computers, and he had to take his tests last so that he didn't interrupt the other students' ability to use their computers.

23. Phone connections on landline and cell phones were interrupted with a loud screeching sound. (*observed by researchers, partially validated*)
24. Telephones called back the numbers that had previously been called, sometimes over and over again, when nobody was touching the phone. The phone repairman could find no problems with the line or the phones that were in the house. The phones had been replaced a number of times.
25. Text messages showed up on a cell phone when no one had sent a text. Sometimes many text messages were sent repeatedly and at a very fast rate.
26. The electronic checkout system at the library did not operate for boy.
27. The bread machine stopped in mid-cycle.
28. Miles per gallon gauge in the car changed values very quickly and went to unbelievable values such as 99 miles per gallon or two miles per gallon. This happened on normal drives, and the mechanic could find no flaws in the electrical system. It only happened when boy was in the car.
29. Car door lock and electric windows went up and down when boy was in the front seat and not touching the controls. (*observed by researchers, but not validated*)
30. While holding the TV remote but not pressing buttons, the TV ordered pay-per-view events.
31. When boy was using a computer, it sent emails automatically without his selecting the email program or pressing buttons.

The phenomena were reported to have been observed by family members, neighbors, teachers, working companions, and a telephone technician.

Abstracts in Other Languages

Spanish

DOMANDO AL FANTASMA INTERIOR: UN ACERCAMIENTO A LA RESOLUCIÓN DE ACTIVIDAD POLTERGEIST APARENTE

RESUMEN: Un informe sobre perturbaciones eléctricas en una casa llevó a un médico a ponerse en contacto con el Centro de Investigación Rhine para explorar un posible caso de RSPK. La investigación proporcionó mucha evidencia anecdótica, alguna sólida, sobre perturbación electrónica poltergeist (EPD), una forma de RSPK caracterizada por la alteración de aparatos electrónicos y de las señales de los dispositivos electrónicos. Un experto en sistemas informáticos observó una evidencia fuerte de actividad EPD en el comportamiento de un sistema de computadora/impresora en una red doméstica. Un enfoque de investigación centrada en la persona fue desarrollado con base en el asesoramiento de personas con experiencia en y experimentadores RSPK. Meditación, atención consciente (mindfulness), y un enfoque orientado pueden utilizarse como actividades para ayudar a controlar o eliminar la actividad no deseada mediante la reducción de la ansiedad a menudo observada en los agentes RSPK. En esta situación, los eventos de EPD se disipan durante un período de 3 meses. A pesar de que la dinámica familiar y otros factores contribuyeron a complicar la evaluación de la eficacia de las técnicas de mindfulness y relajación, el agente RSPK mencionó tener una actitud tranquila y segura después de usar estas técnicas con regularidad. Se alenta a los investigadores a explorar los efectos de relajación y mindfulness para determinar si son de utilidad para otros agentes RSPK en su intento por controlar la actividad no deseada.

French

APPRIVOISER LE FANTÔME INTÉRIEUR : UNE APPROCHE DE L'APPARENTE ACTIVITÉ ÉLECTRIQUE DES POLTERGEISTS

RÉSUMÉ : Un témoignage de perturbations électriques dans une demeure familiale a poussé un médecin à contacter le Rhine Research Center pour explorer un cas potentiel de PKSR. L'investigation a fourni beaucoup de preuves anecdotiques et quelques preuves fortes d'une perturbation électronique associée à

un poltergeist (PEP), une forme de PKSR caractérisée par la perturbation des appareils électroniques et des signaux émis par les dispositifs électroniques. De fortes preuves d'activité PEP furent observées par un expert en systèmes informatiques dans le comportement d'un système d'ordinateur et d'imprimante dans un réseau local. Une approche de recherche centrée sur la personne fut développée sur la base des conseils d'experts et d'expérienceurs en PKSR. La méditation, la pleine conscience et la focalisation ciblée furent introduites comme des activités pour tenter de contrôler ou d'éliminer l'activité non-désirée en réduisant l'anxiété souvent observée chez les agents de PKSR. Dans cette situation, les événements PEP se sont dissipés dans une période de 3 mois. Bien que la dynamique familiale et d'autres facteurs contributeurs ont compliqué l'évaluation de l'efficacité des techniques de pleine conscience et de relaxation, l'agent de PKSR a relaté une attitude de calme et de confiance après avoir utilisé ces techniques régulièrement. Les chercheurs sont encouragés à explorer les effets de la relaxation et de la pleine conscience pour déterminer si elles sont utiles pour d'autres agents PKSR dans leur quête pour gérer et contrôler l'activité non-désirée.

German

DIE ZÄHMUNG DES GEISTES IM INNERN: EINE ANNÄHERUNG ZUM UMGANG MIT ANSCHEINENDEN ELEKTRONISCHEN SPUKPHÄNOMENEN

ZUSAMMENFASSUNG: Ein Bericht über elektrische Störungen in einem Einfamilienhaus veranlasste einen Arzt, sich an das Rhine Research Center zu wenden, um einen möglichen Spukfall untersuchen zu lassen. Die Untersuchung ergab viel anekdotische und einige starke Hinweise auf elektronische Poltergeist-Störungen (EPS) als eine RSPK-Form, die durch elektronische Störungen und Signale elektronischer Geräte charakterisiert wird. Ein Experte für Computersysteme konnte im Verhalten eines Computer-Drucker-Systems eines häuslichen Netzwerkes deutliche Hinweise auf EPS-Aktivitäten feststellen. Ein personen-zentrierter Forschungsansatz wurde entwickelt, der auf Ratschlägen von Spukexperten und spukerfahrenen Personen beruhte. Darin werden Meditation, achtsamkeitszentrierte Wahrnehmung und gezieltes Fokussieren angeführt, mit deren Hilfe sich die störenden Vorkommnisse besser handhaben oder eliminieren lassen, indem sie zur Reduktion von Ängsten beitragen, wie sie oft bei Spukauslösern beobachtet werden. In diesem Falle konnten die EPS-Vorfälle innerhalb dreier Monate zum Verschwinden gebracht werden. Obwohl die Beurteilung der Wirksamkeit von Achtsamkeit und Entspannungstechniken aufgrund der Familiendynamik und anderer beteiligter Faktoren schwierig ist, berichtete der Spukauslöser von einer entspannten und zuversichtlichen Einstellung als Folge der regelmäßigen Anwendung dieser Techniken. Spukforscher sollten die Wirkungen von Entspannung und achtsamkeitszentrierter Wahrnehmung erproben, um herauszufinden, ob sie auch anderen Spukauslösern bei deren Suche nach Handhabung und Kontrolle dieser störenden Vorkommnisse hilfreich sein können.

OPTIMAL LENGTH EXPLORATION FOR FIELD RNG OUTPUTS USING A HAAR WAVELET FILTER: TV AUDIENCE RATINGS FOR NEW YEAR'S 2012 IN JAPAN

By Takeshi Shimizu and Masato Ishikawa

ABSTRACT: The current study examined the optimal period length using wavelet analysis assuming that TV audience ratings are indicators of field intensity that affects field random number generator (RNG) outputs. As TV programs around the New Year often have high ratings, we focused on these programs in 2012 in Japan. Using Psyleron, Rpg102, and Orion as physical RNG devices, the sum of squares of RNG outputs during 288 selected programs were decomposed into multiple levels from periods of 250 ms to 256 s through a Haar wavelet filter. Unexpectedly, the wavelet filter could not find sensitive periods, whereas an ANCOVA suggested that Rpg102 might detect audience rating effects over almost the whole range of wavelengths. Psyleron and Orion devices showed null results. These results suggest that RNG behavior cannot be described by physical signal modeling. There is a possibility of canceling effects in RNG outputs, and this may be a subject for a future study.

Keywords: Psyleron, Rpg102, Orion, Kohaku, TV audience rating

Field random number generator (RNG) studies have found that RNG outputs are significantly biased during periods in which many people focus on the same event at the same time, such as during the 911 terror attacks (Bancel, 2001; Nelson 2001; Nelson, Radin, Shoup, & Bancel, 2002). Devices seem to have the sensitivity to detect field signals even though the audience does not have any intention or knowledge of the existence of the RNG.

The most sensitive time scale or wavelength for RNG outputs during such events remains unclear. At present, 1 s trial generation is the methodological standard in field RNG studies. Bit sequences are generated by an RNG per trial t , creating a bit array (e.g., 0, 1, 0, 0, 1, . . . 0), whereas the sum of the 1 bits (ignoring 0s) is binomially distributed (e.g., 251, 267, 248, . . . 256, when a trial has a total of 512 bits), and the standardized z score for chance expectation is calculated as:

$$z_t = (X_t - N\pi) / \sqrt{N\pi(1-\pi)} = (X_t - \mu) / \sigma, \quad (1)$$

where π is .50, the probability of obtaining a 1, and N is the total number of bits per trial generated by the RNG, although trial quantities differ in field RNG studies. If N is large enough, the bit sequence can be converted into a variance measure reflecting the unsigned deviation of the bit sum from chance. The chi square values obtained for one trial t are given as $\chi^2_t = z^2_t$, and cumulative statistics are calculated as:

$$\chi^2_T = \sum_{t=1}^T z^2_t, df = T, \quad (2)$$

where T is the total number of trials based on event length.

Compatible Period-Length Exploration

The method for computing RNG outputs is somewhat arbitrary because a 1 s interval is not

necessarily natural for most people. Discovery of an optimal length would be advantageous for future micro-PK studies. Such micro-PK phenomena might not approximate physical modeling, but an assumption that RNGs can detect physical signals as waves would be helpful for understanding the characteristics of RNG behavior. In fact, several findings seem to support the possibility that RNGs have an optimally sensitive time scale, because RNG outputs show a significant autocorrelation during highly focused events such as 911 calls (Bancel, 2001; Nelson et al., 2002; Radin, 2006) or events such as workshops that have many participants (Radin & Atwater, 2009). These positive autocorrelations, or persistence effects, suggest that processing using a longer interval (e.g., 2 or 4 s) would show larger variances for these sequences than for sequences with an interval of 1 s.

For this purpose, in the current study we focused on effects of audience rating, because RNG biases may have positive correlations with audience rating during worldwide events and also during local events, such as sports games (Shimizu & Ishikawa, 2012a) and movies played in theaters (Shimizu & Ishikawa, 2010, 2013). These findings support the hypothesis that repeated events with variable audience ratings can reveal the most sensitive period length for detecting field consciousness. We focused on TV programs as variable audience-size events to examine associations between TV ratings and particular wavelengths (i.e., intervals) as time scales that show biases more clearly.

High variability in TV audience ratings was obtained by using New Year's Eve ratings. Many people in Japan return to their family home just before the New Year to celebrate the event with their family, and many people watch TV programs on New Year's Eve. The TV ratings represent the size of the audience watching the programs. As expected, the highest rated program in 2011 was a music program produced by NHK (a major broadcasting company in Japan) with a rating of more than 40%. Such a high rating is rarely observed throughout the year.

One reason why this optimal wavelength issue has not been widely discussed is the methodology of data processing. We use calculation of a wavelet as a method to decompose a signal wave into multiple levels of energy, which is defined as variance, allowing examination of the duration (length) of the signal from field consciousness. Wavelet transformation can decompose original chi squares from RNG outputs into comparable statistics for periods of different lengths (Shimizu, Kokubo, & Ishikawa, 2013).

Method

TV Ratings

We predicted that higher TV ratings should produce more bias in RNG outputs of an optimal length. We sought the optimal wavelength to detect signals from field consciousness.

Information on TV audience ratings was provided by Video Research Corporation in Japan, covering the Kanto Area from 5:00 a.m. (in Japan) on December 28, 2011 to 5:00 a.m. January 4, 2012; that is, 3 days before and 3 days after New Year's Eve. This period corresponds to December 27, 2011 at 15:00 to January 3, 2012 at 9:00 UTC. Some long programs were divided into subprograms with a short break for a weathercast (Table 1). The TV audience ratings were based on the population of about 30 million people who live in the Kanto area. However, as most TV programs were broadcast on a national network and ratings in other areas were similar to those in the Kanto Area, the TV audience population is estimated at about 120 million nationwide. Seven major broadcasters provided a total of 1,618 programs. Audience TV ratings ranged from 0.0 to 41.6%, and the lengths of the programs ranged from 1 to 388 min.

To identify the higher rated programs, all 1,618 programs were sorted by audience ratings before being registered as events, unless the program's time range overlapped that of a previously registered program. When a higher rated TV program overlapped another program, only the higher rated program was analyzed (Table 1). A total of 228 programs were finally selected. The analyzed week had a total of 9,468 minutes of program time, which accounted for 93.93% of the week (10,080 min = 1,440 min \times 7 days).

Table 1
Examples of TV Programs Around New Year's 2012

	Ratings	Start	End	TV	Programs
1	41.6	12/31/2011 21:00	12/31/2011 23:45	NHK	62th Kohaku Music Festival (2nd)
2	35.2	12/31/2011 19:15	12/31/2011 20:55	NHK	62th Kohaku Music Festival (1st)
3	33.6	12/31/2011 20:55	12/31/2011 21:00	NHK	News Weather Bulletin
4	28.5	1/3/2012 07:50	1/3/2012 14:18	NipponTV	The 88th Ekiden Race Backhaul (2nd)
5	27.9	1/2/2012 07:50	1/2/2012 14:05	NipponTV	The 88th Ekiden Race Approach route (2nd)
6	25.4	12/31/2011 23:45	1/1/2012 00:15	NHK	Old year and new year
7	21.0	12/29/2011 19:00	12/29/2011 20:54	NipponTV	Gurunai Final (2nd)
excluded	18.7	12/31/2011 18:30	12/31/2011 21:00	NipponTV	DownTown's Gakitsuka New Year's Eve Special (1st)
8	18.6	12/28/2011 21:00	12/28/2011 23:30	NipponTV	Documentary of Big Family Ishida 2011
9	17.7	1/2/2012 09:00	1/2/2012 23:30	TV Asahi	Tonnels Sports King - 5 hour special (2nd)
228	0.6	12/29/2011 04:25	12/29/2011 04:45	TBS	Kaimono Lab

Note. The time zone is based on Tokyo. Orion device data were analyzed after translation into UTC. One program produced by Nippon TV was excluded because it partly overlapped with a higher rated program (2nd).

Random Number Generation

A total of 11 devices were used as true RNGs (four Psyleron, four Rpg102, and three Orion devices). The Psyleron and Rpg102 devices were connected to four personal computers via a USB port (Acer Aspire One, VAIO Type-G, Type-X, and Dell Dimension 4600). All three Orion devices were registered in the Global Consciousness Project (GCP) and were associated with the following identification numbers: Tokyo (1101), Tsukuba (2201), and Tokorozawa (2202). The four Psyleron, four Rpg102, and one Orion (2201) devices were located within a 1 m circle of the first author's home in Tsukuba before and after the 2012 New Year. They were run continuously in parallel for about one week.

All the RNG devices, except Orion, collected 64 random bits per trial, which consumed 125 ms. Then, they collected random bits at a sampling rate of 512 bits/s. A software application was developed using Visual Studio 2010 to control the Psyleron and Rpg102 devices simultaneously. All the RNG outputs were recorded in a CSV file. GCP RNGs produce more than 8,000 (maximally 16,000) bits/s, and the GCP trials were sums of 200 bits. Data files for Orion were downloaded from the GCP website.

Procedure

The current analysis used two procedures: (a) sum of squares decomposition of the standard deviation of the bit sum from chance expectation into wavelets and (b) regression analysis using audience TV ratings.

Wavelet decomposition. Discrete wavelet transformation is a method to process real data in which the signal is known only at sampling points, with the spacing dependent on the sampling rates of outputs (Capilla, 2006) such as those from a field RNG. We used the Haar filter (Haar, 1910) to decompose the sum of squares of RNG outputs into multiple levels, each level representing a period length. The procedure is described mathematically in the Appendix. Two of the results of these analyses, dZ and Stouffer's Z , were incorporated in subsequent analyses; dZ represents the output variance for each period length. Stouffer's Z is a single value representing the output variance for all the period lengths combined.

Note that the lengths of the analyzed events (programs) ranged from 1 to 388 min, resulting in different maximum levels for different programs. The longer periods have fewer degrees of freedom, resulting in a worse approximation to the normal distribution. Thus, sums of squares with $df < 60$ were excluded.

Table 2 shows the number of samples for each RNG in the analysis of the programs.

Table 2
Number of Samples for Each RNG Device Based on the Wavelet Decomposition

	Psyleron		Rpg102		Orion	
	Analyzed	Base	Analyzed	Base	Analyzed	Base
Stouffer's Z	912	912	912	912	684	684
250 ms	912	912	912	912		
500 ms	912	912	912	912		
1 s	912	912				
2 s	908	912	908	912	681	684
4 s	863	912	863	912	647	684
8 s	636	912	636	912	477	684
16 s	408	912	408	912	306	684
32 s	264	912	264	912	198	684
64 s	168	912	168	912	126	684
128 s	92	908	92	908	69	681
256 s	16	876	16	876	12	657
512 s		840		840		630
1024 s		636		636		477
2048 s		408		408		306
4096 s		264		264		198
8192 s		164		164		123
16834 s		88		88		66
32768 s		12		12		9
N	7003	13316	7003	13316	3200	7935

Note. There were a total of 228 TV programs. Four machines were used, giving a simultaneous sample of 912 (2287 x 4) for the Psyleron and Rpg102 devices, and 684 (228 x 3) for the Orion device. Sample sizes are smaller for longer periods because of the short TV program length. Very short programs did not provide any samples for longer periods. Analyzed samples have sufficient $df (< 60)$ for calculating the standardized dZ scores.

ANCOVA. Using dZ values as the dependent variable, we conducted an ANCOVA with fixed factors of Period Length (12 levels) and RNG Type (three levels: Psyleron, Rpg102, and Orion), and Audience Rating (continuous variable, $N = 228$) as a covariate. There also were two interaction terms involving Audience Rating: Period Length \times Audience Rating, and RNG Type \times Audience Rating. There was no Period Length \times RNG Type interaction because the outputs of the Orion device didn't have periods < 1 s. The intercept in the ANCOVA model was excluded because of parameter redundancy. Second, after the ANCOVA model yielded significance, ANCOVAs were conducted for each RNG device separately, including Period Length, Audience Rating, and their interaction. The ANCOVAs were analyzed using JMP 11.0 (SAS Institute).

A significant main effect for Audience Rating would mean that audience size had an influence across all periods. A significant interaction would suggest that the RNGs were more sensitive at some period lengths than at others. A main effect for Period Length would simply reflect bias in the RNGs.

Results

Using 228 TV programs with audience ratings, we tested the hypothesis that peaks would be found for highly sensitive wavelengths, if these exist. There were no undue outliers. Figure 1 shows the Pearson correlations between audience ratings and dZ .

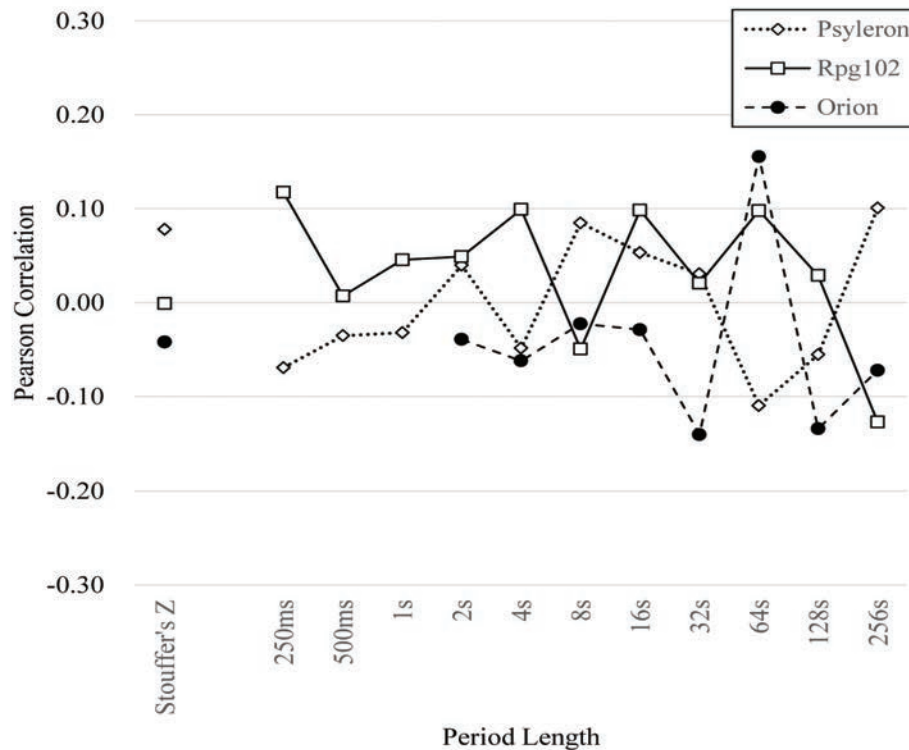


Figure 1. Pearson correlations between audience ratings and dZ as a function of period length and RNG type. Stouffer's Z has no relation to period length. It is included in the figure because it is a "scale" value needed for application of the Haar filter (see Appendix).

The global ANCOVA yielded a significant main effect for RNG Type, $F(2, 17179) = 4.68$, $p = .0009$, and significant interaction of RNG Type with Audience Rating, $F(2, 17179) = 8.72$, $p = .0002$. Table 3 shows results from three ANCOVAs conducted independently for each RNG. Only the ANCOVA using Rpg102 was significant after adjusting the significance level ($\alpha = .05 / 3$), $F(23, 6980) = 2.16$, $p = .001$, permitting us to examine the Rpg102 ANCOVA model in more detail. There was a significant main effect for Audience Rating, $F(1, 6980) = 13.62$, $p = .0002$, meaning that the variance (i.e., bias) in the output of

the Rpg102 devices was positively correlated with audience size. The main effect for Period Length was nonsignificant, but the interaction between Period Length and Audience Rating was significant, $F(11,6980) = 1.92$, $p = .032$. Although the Psyleron device had a peak period length of 8 s and the ANCOVA gave a small p value, it was not significant. The Orion device results were also nonsignificant.

Table 3
ANCOVA Models and Effects on dZ Values Using Rpg102

1. Models		<i>SS</i>	<i>df</i>	<i>MS</i>	<i>F</i>	<i>p</i>
Psyleron	model	31.6	23	1.37	1.402	.096
	error	6842.1	6980	0.98		
Rpg102	model	48.9	23	2.12	2.164	.001
	error	6851.5	6980	0.98		
Orion	model	23.1	17	1.36	1.390	.131
	error	3112.3	3183	.98		
2. Detailed analysis for Rpg102						
		<i>SS</i>	<i>df</i>	<i>MS</i>	<i>F</i>	<i>p</i>
	Audience Rating	13.35	1	13.36	13.62	.0002
	Period Length	6.06	11	.55	.56	.862
	AR x Period Length	20.76	11	1.89	1.92	.032
	Error		6980			

Discussion

In this study, we examined the optimal wavelength as a time interval for RNG outputs, using information on TV programs and audience ratings around New Year's 2012. Outputs of 11 RNG devices were evaluated using Haar wavelet decomposed sums of squares (chi squares).

We hypothesized that a large audience size would increase the variance of X_i for a particular period length that would be optimal for picking up signals. The results showed that the output from the Rpg102 device might be sensitive enough to detect field consciousness. Audience rating effects were observed, as expected, but no optimal period lengths were identified. The significant results with Rpg102 for most period lengths suggest that Rpg102 is sensitive to audience size at all the sampled period lengths, from shortest to longest. This is a characteristic of fractal-shaped waves in bit sequences, as was also reported in a field RNG experiment using music that was listened to repeatedly (Shimizu et al., 2013). It is perhaps worthy of note that there was a marked dropoff of the correlation between audience ratings and dZ after 64 s for Rpg102 and the opposite for Psyleron (see Figure 1).

The results also showed device differences in susceptibility to influence by field consciousness. These tendencies have been found in previous reports showing that the Rpg102 device repeatedly shows high sensitivity (Shimizu & Ishikawa, 2010; 2012a, 2012b), whereas Psyleron does not (Shimizu & Ishika-

wa, 2013). Because these kinds of anomalies are not considered to be influenced by such physical factors, it might appear odd that only a particular RNG, Rpg102 in the current case, showed sensitivity to audience size. We cannot yet conclude that thermal noise, the source of Rpg102 output, was the cause of the sensitivity. Further exploration is needed to determine if this was a unique event or if Rpg102 always has high sensitivity. For this purpose, there is a need to examine the measurement reliability of RNG devices. Good reliability is needed to differentiate individual true (universal) scores from measurement errors. This reliability issue is often discussed in relation to generalizability theory (Cronbach, Gleser, Nanda, & Rajaratnam, 1972).

The results we obtained for wavelets depended on the application of the Haar filter. The current Haar filter is the simplest to use and the best suited for the short period lengths used in this study, but it has relatively low resolution. In contrast, the general filters developed by Daubechies (1992) are better suited for longer lengths, which probably explains their better resolution. Bit generation speed defines the maximum resolution in wavelet analysis, and more bits with a higher resolution can be obtained than was the case in the present study.

It is unlikely that the anomalous RNG behavior we found can be explained exclusively by physical wave modeling in the framework of signal detection, because it appears that the bit stream continues to be generated between trials. This means that the RNG biases were not derived only from wave-like signals, but also from some kind of entanglement of quantum particles.

Cancellation Effects

We had hypothesized that audience size would increase bias in the RNG outputs, but we didn't predict that different RNG devices would have different effects. However, we found that waves coming from one kind of device interfered with waves coming from the other kinds of devices. Thus, as a post hoc test, we evaluated the reliability of the regression coefficients using intraclass correlation (ICC). Excluding the three missing short periods of data for the Orion ($N = 9 = 12 - 3$), the ICC (1, 3) was estimated to be -2.24 , $F(8, 18) = 0.31$, $p = .95$, which is significantly low reliability by a one-tailed test, suggesting that the coefficients for these devices canceled each other out for all period lengths. Such low reliability may keep results from being statistically significant in an analysis that combines data from different types of RNGs.

Cancellation effects between devices have been reported for baseball games (Shimizu & Ishikawa, 2012a) and in reliability analyses of control conditions in field RNG experiments (Shimizu & Ishikawa, 2012b). Fundamental "cancellation pressure" could explain large biases in the variance of RNG outputs during events. For instance, if for a given period length an RNG generates the strongly biased bit array 0, 0, 0, 0, 0, . . . 0 following the strongly biased bit array 1, 1, 1, 1, 1, . . . 1, the mean of the bit sequence would be the null expected value of 0, increasing the value of the corresponding chi square. Then, to keep the variance at the null expected value of 1, the RNGs would work cooperatively with one another as well as independently. It is premature to discuss such mechanisms as a new hypothesis, and a future task is to examine the possibilities of such cancellation mechanisms.

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Appendix

A wavelet is a function $\psi(t) \in L^2(\mathbb{R})$, which is the space of square integral functions and t is time, with the following properties:

$$\int_{-\infty}^{\infty} \psi(t) dt = 0, \quad (3)$$

as well as $\|\psi\| = 1$, where $\|\cdot\|$ is the L^2 norm. The mother wavelet function $\psi(t) \in L^2(\mathbb{R})$, which is dilated/scaled by a , and translated by b , and denoted by $\psi_{a,b}(t)$, is given as

$$\psi_{a,b}(t) = \frac{1}{\sqrt{a}} \psi\left(\frac{t-b}{a}\right), \quad (4)$$

where a is the scale factor determining the extent the wavelet is stretched or compressed, and b is the extent of the shift with which the wavelet is moved along the time or space scale (Dong & Li, 2008); $1/\sqrt{a}$ is the normalization factor. The continuous wavelet transform (CWT) of a function

$f(t) \in L^2(\mathbb{R})$ is given as

$$W_{\psi}f(a, b) = \int_{-\infty}^{\infty} \frac{1}{\sqrt{a}} \overline{\psi\left(\frac{t-b}{a}\right)} f(t) dt \quad (5)$$

where the ψ term represents the complex conjugate (Daubechies, 1992).

To analyze these one dimensional outputs we again assume that time series X_t is the sum of the 1 (not 0) bits in trial t (e.g., 38, 27, 36, 35, 29, 27, . . . 34 when a trial has a total of 64 bits generated), and its standardized score (z_t), where $t = 1, 2, \dots, T$, with $T = 2^L$ = the number of trials with some positive integer. Wavelet transforms are defined under the restriction that $a = 2^j$ and $b = ak$ ($j, k \in \mathbb{Z}$), where k is the index ranging from 1 to $T/2^j$ (the number of trials within a level j), and j is the scale parameter or transform level ranging from 1 to L ($1 \leq j \leq L$).

The Haar filter decomposes a sequence of length 2^L (raw sequence) into coefficients of *details* (information as differences between outputs at a given level) and *scales* (information as averages, approximations of the level outputs), as shown below. The mother wavelet of the Haar filter is expressed simply as

$$\psi(t) = \begin{cases} 1 & 0 \leq t < 0.5, \\ -1 & 0.5 \leq t < 1, \\ 0 & \text{otherwise.} \end{cases} \quad (6)$$

Then, the computation of the wavelet (or detail) coefficients using the Haar basis is performed from the scaling coefficients c_j at scale level

$$d_{j+1,k} = \frac{1}{\sqrt{2}} (c_{j,2k-1} - c_{j,2k}). \quad (7)$$

This means that a detail coefficient consists of the difference between two neighbor scales. The value is the deviation and its sums of squares becomes the wavelet variance.

On the other hand, its father wavelet or scaling function is defined as

$$\phi(t) = \begin{cases} 1 & 0 \leq t < 1, \\ 0 & \text{otherwise.} \end{cases} \quad (8)$$

The purpose of the scaling is to approximate the sequences. Note that scaling (approximation) coefficients can be expressed

$$c_{j+1,k} = \frac{1}{\sqrt{2}} (c_{j,2k-1} + c_{j,2k}) \quad (9)$$

Because these scales become the inputs at the next level, the Haar filter process allows recursive calculation of the “differences” and “sums” of the scale coefficients.

Therefore, this analysis can break down original sequences into scale levels from 1 to L , creating $2^L - 1$ detail coefficients and one approximation.

These structures become very simple when it is assumed that all original time series (X_i) are standardized (z_i) from RNG outputs. Both wavelets and scaling coefficients become standardized, that is, Equation (8) and Equation (10) standardize values in the same way.

The original sequences are available when $j = 0$ and $a = 2^j$, whereas the coarsest scale $c_{L,k}$ corresponds to a single data point $c_{L,1}$, representing the signal average, which is

$$\begin{aligned} c_{L,1} &= \frac{1}{\sqrt{2}}(c_{L-1,1} + c_{L-1,2}) = \frac{1}{\sqrt{4}}(c_{L-2,1} + c_{L-2,2} + c_{L-2,3} + c_{L-2,4}) \\ &= \dots = \frac{1}{\sqrt{2^L}}(c_{0,1} + c_{0,2} + \dots + c_{0,T}) = \frac{1}{\sqrt{T}} \sum_{t=1}^T c_{0,t}, \end{aligned} \quad (10)$$

showing that the final approximation value is actually equal to Stouffer's Z , which is

$$\text{Stouffer's } Z = \frac{1}{\sqrt{T}} \sum_{t=1}^T z_t \quad (11)$$

suggesting that using Haar wavelets fits well with previous RNG methodology.

According to Percival (1995), the energy preservation characteristic of the wavelet transform can be expressed in discrete cases as

$$\sum_{t=1}^T f(t)^2 = \sum_{j=1}^L \sum_{k=1}^{T/2^j} d_{j,k}^2 \quad (12)$$

suggesting that the sum of squared wavelet coefficients over scales provides an orthogonal decomposition of the total sample sum of squares. The energy contained in scale j can be computed from the wavelet coefficient as

$$\sum_{k=1}^{T/2^j} d_{j,k}^2 \quad (13)$$

Energies at different levels are theoretically independent of one another.

Note that whole energy defined by the above equation is based on the sample mean:

$$\bar{c}_0 = \sum c_{0,k}/T, \sum_{j=1}^L \sum_{k=1}^{T/2^j} d_{j,k}^2 = \sum_{k=1}^T (c_{0,k} - \bar{c}_0)^2 \quad (14)$$

whereas field RNG studies usually compute sum of squares from expectation $\mu = 0$, as

$$\sum_{k=1}^T (c_{0,k} - \mu)^2 = \sum_{k=1}^T c_{0,k}^2 \quad (15)$$

Then, the decomposition is given as

$$\sum_{k=1}^T c_{0,k}^2 = \sum_{k=1}^{T/2^j} d_{j,k}^2 + c_{L,1}^2 = \sum_{k=1}^{T/2^j} d_{j,k}^2 + \text{Stouffer's } Z^2 \quad (16)$$

Zero Padding

One unsolved issue is the restriction of event lengths, because the wavelet decomposition (Shimizu et al., 2013) assumes a dyadic time series with sample size $T = 2^L$, where L is a positive integer. To moderate it, we used zero padding (Shimizu & Ishikawa, 2014). Suppose that we have an original time series of

length T , and a minimum positive integer L , which fulfills $T \leq 2^L$, where all the points in which the number is more than T are filled with 0 values. The degrees of freedom (df) of the wavelet variance correspond to the number of coefficients, defined as

$$df_{0,k} = \begin{cases} 1 & k \leq T \\ 0 & k > T, \end{cases} \quad (17)$$

At level $j = 0$ (original); df at the next level, $j + 1$, then becomes

$$df_{j+1,k} = (df_{j,2k-1} + df_{j,2k})/2 \quad (18)$$

As an example, assume an original data set with the values 1.0, 2.0, 3.0, 4.0, and 5.0 ($T = 5$). The corresponding zero padded values are 1.0, 2.0, 3.0, 4.0, 5.0, 0.0, 0.0, and 0.0 ($T = 8, L = 3$). For the next level, $df_{1,k} = 1.0, 1.0, 0.5$, and 0.0 ($T/2 = 4$), next level $df_{2,k} = 1.0, 0.25$ ($T/4 = 2$), and finally $df_{3,1} = 0.625$ ($T/8 = 1$). The sum of squares for all the levels are calculated as

$$SS_j = \sum d_{j,k}^2 \quad (19)$$

And standardized as

$$dZ_j = \sqrt{2 \times \sum d_{jk}^2} - \sqrt{2df_j - 1} \quad (20)$$

Abstracts in Other Languages

Spanish

EXPLORACIÓN LONGITUDINAL ÓPTIMA PARA PRODUCTOS DE RNG DE CAMPO
UTILIZANDO UN FILTRO WAVELET HAAR: ÍNDICES DE AUDIENCIA DE TELEVISIÓN
PARA EL AÑO NUEVO 2012 EN JAPÓN

RESUMEN: Este estudio examinó la duración óptima de longitud de periodo usando un análisis wavelet suponiendo que los índices de audiencia de televisión son indicadores de la intensidad de campo que afecta al producto de un generador de números aleatorios de campo (RNG). Ya que los programas de televisión de Año Nuevo a menudo tienen altos índices de audiencia, nos centramos en estos programas en 2012 en Japón. Usando Psyleron, Rpg102, y Orión como dispositivos RNG físicos, la suma de los cuadrados de los resultados de RNG durante 288 programas seleccionados fueron descompuestos en múltiples niveles de periodos de 250 ms a 256 s a través de un filtro de wavelets Haar. Inesperadamente, el filtro de wavelets no pudo encontrar periodos sensibles, mientras que un ANCOVA sugirió que el Rpg102 pudo detectar los efectos de la audiencia en casi toda la gama de longitudes de onda. Los dispositivos Psyleron y Orion mostraron resultados nulos. Estos resultados sugieren que el comportamiento RNG no puede ser descrito como modelamiento de la señal física. Hay la posibilidad de efectos de cancelación en resultados RNG, lo que puede ser un tema para un estudio futuro.

French

EXPLORATION DE LA LONGUEUR OPTIMALE DES DONNÉES SORTANTES POUR DES GNA
DE CHAMP EN UTILISANT UN FILTRE D'ONDELETTE DE HAAR : LES TAUX D'AUDIENCE
TÉLÉVISUELLE POUR LE NOUVEL AN 2012 AU JAPON

RÉSUMÉ : La présente étude examine la longueur de la période optimale en utilisant l'analyse par ondelette en faisant l'hypothèse que les taux d'audience télévisuelle sont des indicateurs de l'intensité du champ qui

affecte les données sortantes d'un générateur de champ aléatoire de champ (GNA). Comme les programmes télé autour du Nouvel an engrangent souvent des audiences élevées, nous nous sommes focalisés sur ces programmes en 2012 au Japon. En employant un Psyléron, un Rpg102, et un Orion en tant que dispositifs GNA, la somme des carrés des données GNA durant les 288 programmes sélectionnés fut décomposée en de multiples niveaux pour des périodes de 250 ms à 256 s, à travers un filtre d'ondelette de Haar. De façon inattendue, le filtre à ondelette ne parvint pas à trouver des périodes sensibles, tandis qu'une ANCOVA a suggéré que le Rpg102 pourrait avoir détecté des effets de taux d'audience sur presque toute la gamme des longueurs d'onde. Les dispositifs Psyléron et Orion montrèrent des résultats nuls. Ces résultats suggèrent que le comportement du GNA ne peut pas être décrit par une modélisation en signal physique. Il y a une possibilité d'effets d'annulation dans les données GNA, et cela pourrait être le sujet d'une étude future.

German

DIE VERWENDUNG EINES HAAR-WAVELET-FILTERS: DIE EINSCHALTQUOTEN JAPANISCHER FERNSEHZUSCHAUER FUER DAS NEUE JAHR 2012

ZUSAMMENFASSUNG: Die vorliegende Studie untersuchte mit Hilfe einer Wavelet-Analyse die optimale Periodenlänge unter der Annahme, dass TV-Einschaltquoten Indikatoren für die Intensität eines Feldes sind, das sich auf den Output tragbarer Zufallszahlengeneratoren (RNG) auswirkt. Da TV-Programme zum Jahreswechsel oft hohe Einschaltquoten haben, haben wir uns 2012 auf diese japanischen Programme konzentriert. Unter Verwendung von Psyleron, Rpg102 und Orion als physikalische RNG-Geräte wurden die Quadratsummen der RNG-Daten während 288 ausgewählter Programme in mehreren Stufen in Perioden von 250 ms bis 256 sec mittels eines Haar-Wavelet-Filters zerlegt. Wider Erwarten konnte der Wavelet-Filter keine sensitiven Perioden entdecken, wohingegen eine ANCOVA anzeigte, dass der Rpg 102 Effekte bei Publikums-Einschaltquoten praktisch über das gesamte Spektrum der Wellenlängen hinweg entdeckt haben könnte. Psyleron und Orion zeigten keine Resultate. Diese Ergebnisse deuten darauf hin, dass sich das RNG-Verhalten nicht mittels physikalischer Signalmodellierung beschreiben lässt. Es besteht die Möglichkeit auslöschender Effekte bei RNG-Outputs, und dies könnte Gegenstand einer zukünftigen Studie sein.

BOOK REVIEWS

PARAPSYCHOLOGY: A HANDBOOK FOR THE 21ST CENTURY edited by Etzel Cardeña, John Palmer, and David Marcusson-Clavertz. Jefferson, NC: McFarland, 2015. Pp. ix + 414 (paperback). \$65.00. ISBN 978-0786479160.

Parapsychology: A Handbook for the 21st Century is intended to update the Wolman (1977) *Handbook of Parapsychology*. That is a major undertaking because the Wolman *Handbook* was a milestone for parapsychology and one of the most important books in the history of the field.

The new handbook was published during a period of dramatic improvements to the methodological standards for research in psychology and parapsychology. Many of the chapters have comments and recommendations in line with the new standards, notably preregistration of well-powered confirmatory research. These new methodological standards present a dilemma when dealing with previous research. How much weight should be given to studies that were conducted with methodology that is now recognized as providing many opportunities for potential bias? This question has no clear answer.

Due to space limitations for the *Journal*, this review is an abbreviated version of a longer review that provided additional comments and references. The longer review is available online (Kennedy, 2016).

The new handbook has 31 chapters that are divided into five parts or sections. Each of these is discussed separately below. The preface by the editors includes an introduction that discusses “what is parapsychology” and associated definitions and terminology. The preface also includes a comparison of the topics in the previous and new handbooks.

Part One: Basic Concepts

In Chapter 1, Nancy Zingrone, Carlos Alvarado, and Gerd Hövelmann point out key developments since 1977 for a wide variety of topics related to parapsychology. These topics include not only various lines of research but also developments pertaining to sociology, religion, anthropology, philosophy, education, institutions, journals, and funding sources. These discussions are more historical than attempts to present the current state of knowledge.

In Chapter 2, Ed Kelly identifies psi phenomena as one of several mental phenomena that cannot be explained with current physicalist assumptions about the human mind. Some of the other phenomena include genius-level creativity, mystical experiences, secondary centers of consciousness, and the unity of conscious experience. Kelly advocates that applying a philosophy of dualism and associated models as originally proposed by Frederic Myers and William James will be a fruitful approach. He concludes the chapter by suggesting that research on mystical experiences and the various means to facilitate mystical experience are the highest priority.

In Chapter 3, Doug Stokes presents a skeptical view of parapsychological research findings. This is a significant chapter that should not be ignored. After decades of involvement in parapsychology with an open mind, Stokes has come to the conclusion that the research findings are more consistent with bias and fraud than with paranormal phenomena. Stokes does not consider parapsychology unique in having many ostensible findings that may actually be due to bias and fraud. Rather, he points out the need for improved research standards throughout science and the “recent avalanche of exposed fraud in mainstream biological and psychological science.” He concludes the chapter by noting that evidence for psi could be established by thorough investigation of spontaneous cases and by experiments using good research methodology that can be repeated by the vast majority of competent scientists.

As discussed in the longer review (Kennedy, 2016), I have reached similar conclusions about the experimental research given past methodology. However, because of several striking personal paranormal experiences, I am absolutely certain that paranormal phenomena sometimes occur.

For purposes of this review, the term *methodological optimist* refers to those who believe that undetected bias and fraud are negligible in previous psi research and that recent meta-analyses provide compelling evidence for psi. Similarly, *methodological pessimist* refers to those who like Stokes and myself believe that the small, sporadic, unreliable effects and the differences among experimenters in producing the effects in parapsychology could easily be a result of undetected bias and fraud given past research methodology.

Part Two: Research Methods and Statistical Approaches

In Chapter 4, John Palmer provides an introduction to parapsychological experimental methods for those who are not familiar with them. This chapter is suitable for undergraduates and the interested general public.

In Chapter 5, Emily Williams Kelly and Jim Tucker discuss spontaneous cases. Although this chapter is in the methodology section of the handbook, it presents findings from research on spontaneous cases as well as descriptions of the methods. The first part of the chapter describes the authors' approach to spontaneous cases and critiques some other approaches. One point the authors emphasize is that the investigation of spontaneous cases has a fundamental and essential role in understanding psi and consciousness. They challenge the view held by some that spontaneous cases are secondary or peripheral to experimental research. I strongly agree with their perspective.

Their primary recommendations for research are to conduct surveys that are followed up with systematic investigation of cases. They also think the investigations should include a broad range of experiences, including mystical, bereavement, and dying.

In Chapter 6, Graham Watkins discusses methodology for investigating macro-PK effects, which are effects that do not require statistical analyses because there is no possibility that they would occur by chance or from random fluctuations. Levitation of objects is the classic example. He notes that it is a good idea to have a trained magician involved in investigations of those who claim to perform macro-PK. One noteworthy conclusion is that "it should be understood that there probably are no demonstrations that will not be dismissed as fraudulent by skeptics and stage magicians."

In Chapter 7, Patrizio Tressoldi and Jessica Utts discuss statistical methods for psi research. This starts with a basic introduction to statistics that would be suitable for undergraduates or the interested general public. Their final list of recommendations is a useful summary of the new methodological standards for research.

Part Three: Psychology and Psi

In Chapter 8, Rex Stanford describes his psi-mediated instrumental response model (PMIR) and Jim Carpenter's first-sight model. Both of these psychological models are basically elaborations of the working assumptions about psi at J.B. Rhine's Duke Parapsychology Laboratory in the 1960s when Stanford and Carpenter were there. The working assumptions can be found in the book *Parapsychology From Duke to FRNM*: "[P]si seems to function as a normal, healthy ability, responsive to motivational drives, and as part of the general unconscious system of the individual" (Rhine and Associates, 1965, p. 109). The key points are that everyone can be assumed to have potential psi ability that is guided by motivation and that operates unconsciously and can occur without conscious intention and usually without awareness that psi is occurring. It was noted that physiological measures such as blood volume, EEG, galvanic skin response, or other physiological conditions that do not depend on a conscious response, may be the best way to detect the occurrence of the unconscious psi process. The capricious unreliability of psi was assumed to be due to unconscious psychological factors.

Researchers increasingly recognize that noticeable scientific progress has not occurred with these Rhinean assumptions and that the obstacles to reliable psi may be a fundamental aspect of psi. Ideas such as that psi is actively evasive (Beloff, 1994), manifests as a trickster (Hansen, 2001), is unsustainable (Kennedy, 2003), and is constrained to be useless (Millar, Chapter 13 in the *Handbook*) are increasingly recognized

as worthy of investigation. In fact, the field of parapsychology is becoming divided into two camps. One camp, composed largely of researchers with degrees in psychology, focuses on hypotheses consistent with the Rhinean assumptions. Those in the other camp, composed largely of researchers with degrees in areas other than psychology, focus on nonpsychological constraints to the operation of psi. Brian Millar (Ph.D. in chemistry) describes one of those models in his chapter in this handbook.

Stanford's discussion of the first-sight model is divided into two sections. The first part attempts to present Carpenter's key ideas in a way that is easier to understand than in Carpenter's admittedly difficult writings. The second part corrects various mistakes Carpenter made in discussing Stanford's research. Although Stanford does not explicitly address this point, the mistakes he discusses do raise the possibility that similar mistakes may be present in Carpenter's discussions of other research.

In Chapter 9, Etzel Cardeña and David Marcusson-Clavertz review studies on states, traits, and cognitive variables. The topics include personality, attitude, previous psi experiences, creativity, altered states of consciousness, dissociation, and need for control. They conclude that their review "does not reveal one or more psychological variables that can be used to consistently and strongly predict performance in psi tests, yet there is some regularity to the findings of various areas of research." They note that some areas, particularly research on personality and psi, are at a stage where preregistered confirmatory studies are needed.

In Chapter 10, Serena Roney-Dougal reviews research on meditation and psi. She notes that in many of the early studies psi-missing in the control condition contributed as much to the results as high scoring in the meditation conditions—which may be fulfilling the wishes of the experimenter more than revealing the relationship between meditation and psi. She concludes that the research has shown that meditation is a psi-conducive condition. She also adds that most meditation research has been "with relatively untrained people who show a minimal level of very unreliable psi" and that "these initial tentative explorations into this area with Western practitioners, who by Eastern standards are just beginners, does show potential." This chapter does not inspire attempts to confirm any particular previous study.

Part Four: Biology and Psi

In Chapter 11, Richard Broughton discusses biological aspects of psi research, including evolution. He first briefly reviews research pertaining to possible psi by animals and various studies involving psi and physiological and brain measures. He then proposes an evolutionary framework for psi. His idea is that receptive psi (ESP) is linked to the human ability to imagine the future. He discusses several factors that are related to this ability and that appear similar to findings about psi. He recognizes that unknown factors apparently limit psi ability but suggests that sufficient benefit is obtained for evolution to have developed weak psi ability.

Efforts to place psi in an evolutionary framework are much needed. However, Broughton's ideas appear to emerge from the Rhinean assumptions. To a great extent, the Rhinean assumptions are ad hoc speculations that psi has some benefit even though the capriciously unreliable results for experimental research do not demonstrate useful benefits. Thus, psi is assumed to be an unconscious process that produces benefits that are not apparent. As discussed in the longer review (Kennedy, 2016), research on spontaneous cases provides a more direct way to investigate the benefits of psi and overall does not provide support for the Rhinean assumptions.

In Chapter 12, David Luke discusses psychedelic drugs and psi. He describes various neurochemical models that could be related to psi ability and summarizes the extensive anecdotal reports that associate psychedelics and psi. He found published reports of 23 experiments but notes that these were exploratory and are difficult to assess. The chapter concludes with many useful recommendations and ideas for further research.

Part Five: Physics and Psi

In Chapter 13, Brian Millar discusses quantum theory and psi. Millar describes an updated version of an observational theory for psi that has testable predictions. According to this model, some type of feed-

back is required for psi to occur. The model can be tested by manipulating feedback.

The model is based on quantum entanglement, which is a precarious state that breaks down if efforts are made to transmit a signal or to make use of psi. Like other manifestations of entanglement, correlations can be observed, but signals cannot be sent—which means that repeatable experiments and useful applications of psi cannot be expected. He points out that those who propose that psi can be explained by nonlocal entanglement and also propose that useful applications of psi will be developed are missing the point of entanglement.

In Chapter 14, Adrian Ryan discusses physical factors that could affect psi performance. The most extensively investigated factor is geomagnetic activity. He concludes that there is strong evidence that telepathy and clairvoyance are stronger during periods of low geomagnetic activity, but precognition and PK may be stronger during periods of high activity. However, these studies appear to be post hoc correlational analyses of previously collected data. His recommendations for research appear to be exploratory. Preregistered prospective confirmatory research is needed for geomagnetic effects.

Part Six: Psi Phenomena: Anomalous Cognition, Perturbation, and Force

In Chapter 15, Johann Baptista, Max Derakhshani, and Patrizio Tressoldi provide meta-analyses and recommendations for research methodology in ganzfeld, forced-choice, remote viewing, and dream studies. Those who are seeking a technical review of the current state of knowledge about psi research and/or recommendations about methodology for future research will find this chapter useful.

The authors are methodological optimists and their meta-analyses are conducted and interpreted accordingly. Although methodological pessimists will not find their meta-analyses and associated extensive post hoc data explorations as convincing evidence for psi, all will agree that their analyses will be highly useful in planning the confirmatory research and prospective meta-analyses that are now needed. Their methodological recommendations include efforts to prevent fraud in addition to well-powered, preregistered confirmatory experiments.

One topic that I think they do not treat optimally is the evaluation of experimenter effects. One strategy for evaluating experimenter effects is to do an ANOVA with each experimenter as a factor level. This strategy has low power and does not utilize available information about experimenter effects. Another strategy is to categorize experimenters as generally successful, sometimes successful, or generally unsuccessful based on their history with other research. This more powerful strategy is more likely to find evidence for experimenter effects. In general, statistical power must be considered when drawing conclusions about the absence of an effect.

In Chapter 16, John Palmer reviews studies of implicit or non-intentional ESP (anomalous cognition). These are studies that contain an ESP task that is not consciously known or intended by the participant. This type of research was primarily inspired by Stanford's PMIR model. Palmer also included studies in which precognition is assumed to facilitate success on a psychological task as in the studies by Bem.

The available studies are diverse and generally not appropriate for a meta-analysis. Palmer concludes that the PMIR studies have impressive results, but the Bem studies did not replicate as consistently.

Palmer notes but does not fully develop the substantial likelihood of experimenter effects in the PMIR studies. The possibility of experimenter effects is particularly high in studies that have contrived artificial assumptions. For example, some of the studies were academic exams for students with the answers to some questions in a sealed envelope behind each student's exam sheet. The questions with answers varied among the students. The assumption was that the students would do better on the questions with the answers in their envelope. This assumes that ESP works better if the answer is closer to the student than if the answer is in the envelope of a nearby student. In fact, the answers for all questions presumably are readily available by ESP. A higher success rate for questions with answers in the student's envelope almost certainly reflects the motivations of the experimenter more than the operation of psi by the students.

In Chapter 17, Dean Radin and Alan Pierce review studies of presentiment, studies of correlations among brain waves, and studies of brain states associated with successful psi performance. They note that

the first presentiment study using physiological measures of precognitive anticipation was a study by Levin and Kennedy (1975) that reported successful results. Unfortunately, they do not discuss the later summary of the research (Kennedy, 1979) that noted methodological problems that raise doubts about the earlier report. That summary also reported findings from three subsequent studies. The overall conclusion was that these exploratory research efforts were inconclusive. As Radin and Pierce note, this line of research was subsequently pursued by Hartwell who reported two more formal, but unsuccessful, studies. It is notable that Hartwell's (1978) research was preregistered in accordance with the policy of the *European Journal of Parapsychology* at that time and is one of the first preregistered studies in parapsychology.

This research utilized the contingent negative variation (CNV), which is a slow brainwave that indicates anticipation. We chose this measure because it captures a relatively high level of cerebral processing. We thought that a high level of cerebral functioning might be more likely to show psi effects than lower level physiological processes. However, the presentiment line of research initiated by Radin in 1997 utilized skin conductance with emotional stimuli and has been reported to be more successful.

Unfortunately, this area of research is confounded by methodological issues. Most of the research used physiological measures as the dependent or outcome variable. However, human responses, whether conscious or physiological measures, cannot be assumed to be independent, as is required for the dependent variable in standard statistical analysis. This has long been recognized in parapsychology and is why the traditional analysis uses the human responses to predict the random events rather than the random events to predict the human responses (Burdick & Kelly, 1977; Kennedy, 2014b). The optimal analysis for presentiment studies would be to develop prediction criteria from previous data and then apply the criteria to predict the random stimulus on new trials (Kennedy, 2013, 2014b). The early CNV studies did not fully employ this methodology but generally came closer than the later presentiment studies.

Radin and Pierce claim that permutation tests adjust for dependencies among trials, but I do not see how that is possible for these data. Permutation tests are based on the assumption that the observations are exchangeable (Good, 2005). Permutation tests can be used with dependent data if the data can be placed in blocks that are exchangeable. However, when the participants' responses likely incorporate reactions to feedback on previous trials, the sequence of responses can be expected to depend on the specific sequence of random stimuli to which the participant was exposed, which makes the responses not exchangeable.

Attempts to correct for dependence with simple statistical adjustments or to do post hoc analyses to argue that certain types of dependence did not occur are controversial (Dalkvist, Mossbridge, and West-erlund, 2014; Kennedy, 2013, 2014b). As the early statisticians for parapsychology recognized, dependence problems are better avoided than debated.

Preregistration of presentiment studies is very important because these studies have significant potential for bias (Kennedy, 2013). The physiological values in the analysis are typically derived from complex data processing after the researchers know the type of stimulus on each trial. This gives opportunity for bias to enter into the data processing decisions and likely has greater potential for bias than the dependence problems.

In Chapter 18, Stefan Schmidt reviews research on distant intention. In these experiments, an agent attempts to activate or calm the physiology of a remote person or the state of the remote person as evaluated by physiological measures. Meta-analyses of the three most common types of studies (electro-dermal activity, remote staring, and attention focusing) found small but significant effects for each type of study. The primary obstacle to conducting convincing experiments in this area of research is that with the observed effect sizes, 650 sessions are needed to obtain a power of .80 for confirmatory research. This is not considered feasible. Schmidt concludes that the top priority for the research is to explore factors that might enhance effect size.

Schmidt does not discuss the striking declines in effect size that occurred in the first studies by Braud and Schlitz (1991) and the associated implications for experimenter effects. The first three studies each had 10 sessions and obtained significant results—which is extremely unlikely if the effect size from the meta-analysis was applicable. Meta-analyses that focus on overall average effect size without considering factors such as experimenter effects and the initial declines for a new line of research, which have

been common in parapsychology, may obscure more than enhance understanding of psi. Other challenging methodological issues for this type of research are discussed in the longer review (Kennedy, 2016).

In Chapter 19, Stephen Braude discusses macro-PK. This chapter starts with a very interesting discussion of the controversial nature of macro-PK, the common criticisms, and the historical background. However, the discussion of recent developments is not nearly as detailed or thorough as the introduction. The emphasis in this chapter on the introductory discussions of extremely rare individuals in the past is unlikely to inspire new research.

In Chapter 20, Mario Varvoglis and Peter Bancel discuss PK experiments with electronic RNGs. This appears to be an unbiased effort on a very difficult topic. The inconsistent nature of their conclusions reflects the complexity of the topic. They conclude that the data provide evidence for psi and that theory-driven research is needed, but also that the available data cannot be used to estimate effect sizes for properly planning research and that the high likelihood of experimenter effects in PK studies “may be pointing to the need for reconsidering the experimental paradigm of parapsychology altogether.” The conclusion I get is that research with RNGs remains at an exploratory stage and that any hope for confirmatory research must be predicated upon selected experimenters and/or selected subjects.

One possibility that the authors do not discuss is goal-oriented psi experimenter effects. As discussed in the longer review (Kennedy, 2016), the pattern of results with this model is the same as for methodological bias. This pattern has been found in the RNG PK meta-analyses and has been interpreted as evidence for bias. Preregistered experiments that eliminate methodological biases will allow this model to be evaluated.

In Chapter 21, Roger Nelson discusses the Global Consciousness Project that investigates deviations in the output of a network of electronic RNGs during times of major world events. The RNGs are located around the world. The idea is that a large number of people concentrating on the same world event creates some type of field consciousness that causes deviations in the RNGs. The project has been running for 15 years and investigated over 450 events.

Of course, for those who adhere to a Rhinean or other motivation-driven model of psi, any psi effects in this research are assumed to be experimenter effects. This is just another study of nonintentional psi. The intrinsically post hoc nature of the analyses also raises substantial methodological challenges.

In Chapter 22, John Palmer and Brian Millar discuss experimenter effects in parapsychological research. Palmer reviews the main studies that investigated experimenter differences. Millar notes that experimenters can be expected to influence the experimental outcome with either Rhinean or non-Rhinean models of psi. He also discusses cases of experimenters who admitted that they attempt to use psi to influence the outcomes of their experiments.

The authors do not discuss one of the best strategies for investigating experimenter effects. That is to conduct experiments in which the participants have motivation for a certain outcome and the experimenter has motivation for a different outcome. The results for such an experiment indicate the relative roles of participant and experimenter psi. This type of situation occurs more frequently than is generally realized as experimenters investigate their pet hypotheses. The studies of psi in academic exams noted above are an example. Studies of majority-vote procedures are another example (Kennedy, 1995).

Part Seven: Psi Phenomena: Research on Survival

In Chapter 23, Julie Beischel and Nancy Zingrone review research and associated controversies involving mediumship. They focus on the findings of research with mediums and put aside the unresolvable debate about whether information from mediums is due to psi by the medium or due to a discarnate being. I found this approach more productive than the usual rehash of the debate about the source of psi. They provide summaries of recent studies investigating the validity of readings by mediums and the associated methodological criticisms. They also summarize research on dissociative and other characteristics of mediums and on the experiences of mediums. They conclude that a standard proof-oriented protocol would be valuable and make recommendations for that.

In Chapter 24, Antonia Mills and Jim Tucker provide a summary and update of the research and writings on reincarnation by Ian Stevenson. Stevenson's in-depth case studies provide very interesting results but also have the well-known limitations of nonexperimental research.

In Chapter 25, Michaelleen Maher provides a useful review of diverse research and proposed explanations related to ghosts and poltergeists. She concludes that there is no irrefutable evidence that can support the paranormal validity of ghosts and poltergeists, and that the proposed explanations, both normal and paranormal, have not been productive. The matter remains a mystery. She notes that poltergeists, in particular, exemplify the "trickster-liminality connection" described by Hansen (2001), who proposes that ostensible paranormal phenomena often include a mixture of bogus effects and truly paranormal effects that are disruptive and cannot be captured and controlled.

In Chapter 26, Mark Leary and Tom Butler review the findings and proposed explanations for electronic voice phenomena (EVP), which are voice-like sounds in the background noise of magnetic tape and digital audio recordings. Instances of ostensible EVP are usually no longer than three seconds and one to five words. The quality and characteristics of the voice-like sounds vary greatly. Leary and Butler note that most of the research on EVP has been by amateur investigators and that scientific research has been rare. They conclude that some EVP effects are difficult to explain by normal processes and that the current research is inadequate to justify conclusions one way or the other.

Part Eight: Practical Applications

In Chapter 27, Rupert Sheldrake discusses psi in everyday life by animals and humans. Most of the discussion is about reports of anecdotal experiences, but experiments are reported for three effects. The first is experiments investigating the possibility that dogs precognitively know when their owners will return home. However, as discussed in the longer review (Kennedy, 2016), these experiments had a poor design that invites controversy and could be avoided with a simple alternative design. Sheldrake also reports experiments with people knowing or guessing who is calling them. These experiments did not have obvious methodological problems. Other experiments investigated a person sensing that they were being started at. Unfortunately, many of these experiments apparently had unacceptable designs, such as the two people in the same room and/or the same random sequence used for all participants. Well-designed preregistered confirmatory experiments are needed in these research areas.

In Chapter 28, Martina Belz and Wolfgang Fach discuss clinical counseling for people who believe they are having paranormal or other exceptional experiences and seek understanding of the experiences, or find them distressing, or have underlying psychopathology. Much of the chapter is devoted to describing various classification models for the experiences and associated psychology. The section on intervention notes that counselors need to have an understanding of parapsychology and that each case typically has unique features that prevent applying a standard formula for counseling.

In Chapter 29, Paul Smith and Garret Model review technological approaches for practical applications of psi. The topics include remote viewing in intelligence collection, associative remote viewing for financial gain, precognitive dreams, dowsing, ESP in police investigations, ESP in archeology, and divination. Their review is highly optimistic with little discussion of critical evaluations. They appear to assume that the reversals and loss of effects that have sometimes occurred with these investigations are minor problems that will soon be overcome. As noted in the comments above for Chapters 8 and 13, the lack of reliable results after a century of psi research has resulted in increasing recognition that some fundamental property of psi may prevent reliable, useful effects. A balanced review of the current state of knowledge for applications of psi would include these alternative views.

Part Nine: To Sum It Up

In Chapter 30, Gerd Hövelmann argues that parapsychology has made important methodological contributions to scientific research. He also provides philosophical discussions about the relationship between established science and parapsychology as a borderline area of research.

In Chapter 31, Etzel Cardeña summarizes his thoughts about the status of parapsychological research in light of this handbook. He believes that the evidence indicates that psi effects do occur, but he also remains adrift in uncertainty and mystery about the nature of the phenomena and about consciousness. He considers the new methodological standards to be an important and necessary step but suspects that they will result in scientists being forced to confront how little they know more often than providing clear resolution of scientific questions. Cardeña ends the chapter (and book) with a list of recommendations for further research. Notable recommendations include to investigate whether “psi phenomena in the lab [are] of a different nature than those in real life,” and to “investigate systematically the social ecology and psychology of the most successful researchers.”

Final Thoughts

For those who are methodological pessimists, the primary value of this new handbook is to guide future research and to provide a foundation for designing studies that have better methodology than in the past. Many chapters do this well. For those who are methodological optimists, the handbook summarizes compelling evidence that psi occurs in addition to providing a foundation for future research. Unfortunately, given past research practices, this handbook cannot resolve the different perspectives of methodological optimists and methodological pessimists. Hopefully, future research will narrow the gap and the next version of a handbook of parapsychology will have a greater emphasis on what is known compared to what needs to be done.

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A PHILOSOPHICAL CRITIQUE OF EMPIRICAL ARGUMENTS FOR POSTMORTEM SURVIVAL by Michael Sudduth. New York, NY: Palgrave Macmillan, 2016. Pp. xv + 336. \$100 (hardback). ISBN 978-137-44093-8.

Between the 1920s and early 1960s, several major philosophers (H. H. Price, C. D. Broad, and C. J. Ducasse, in particular) directed their attention to the material amassed by the Society for Psychical Research and similar organizations on problems related to the survival of consciousness and personality after death. The most significant philosophical treatments of the survival issue in recent years have been those of Griffin (1997) and Braude (2003), both of whom end up concluding that the data point toward the reality of postmortem survival. They have been joined by several others (e.g., Almeder, 1992; Lund, 2009; Weiss, 2012) who have been impressed by the evidence for reincarnation above all.

In this book, Michael Sudduth offers a dissenting view. He does it, however, not by addressing the strengths and flaws of select cases, but by analyzing the structure of the arguments that have been made by survival proponents, using formal techniques from confirmation theory. This is a useful exercise and if that were all Sudduth set out to do, he could have made a real contribution to the survival literature by exposing weaknesses and suggesting improvements in the way the debate has been carried out. But that is not all he set out to do. He moves beyond analysis of the existing positions to argue that even if survival is a fact, it cannot be proven to be so, in part because it is impossible to rule out challenges from its nearest competitor, living-agent psi.

The book has eleven chapters. In Chapter 1, Sudduth introduces the “classical empirical survival debate.” In Chapter 2, he outlines what he understands to be “the hypothesis of personal survival.” Chapters 3 through 5 are data-centered. Chapter 3 is on out-of-body and near-death experiences, Chapter 4 is on mediumistic communications, and Chapter 5 is on past-life memories. Chapter 6 analyzes “classical explanatory arguments for survival.” Chapter 7 tackles “Bayesian explanatory arguments,” which Sudduth associates with Broad and E. R. Dodds. Chapter 8 continues with “Bayesian defenses of the survival hypothesis,” especially those of Ducasse and R. W. K. Paterson. The last three chapters build toward Sudduth’s conclusions. Chapter 9 deals with “the problem of auxiliary assumptions” and Chapter 10 with “exotic counter explanations.” Chapter 11 is entitled, “The Classical Arguments Defeated.”

Sudduth observes that to date the survival debate has been essentially explanatory. Proponents seek to show that survival is a more compelling interpretation of the evidence than are rival possibilities. The “survival hypothesis” becomes more refined with the inclusion of Boolean constraints such as antecedent probability. Another way of looking at hypotheses is in terms of their predictive power, which Sudduth terms “likelihood.” Sudduth holds that all three sorts of survival arguments have fallen short—the explanatory arguments have not been able to prove that the survival hypothesis provides the best account of the evidence; Bayesian arguments have not established that survival is more probable than not; and likelihood arguments have not demonstrated that survival has greater predictive power than living-agent psi.

The antecedent probability and predictive power of the survival and living-agent psi hypotheses are a major consideration in the later chapters. Before I get to those topics, though, I want to say something about Sudduth’s notion of “hypothesis.” He uses “hypothesis” in the sense that psychology and parapsychology use “theory” and doesn’t have a way of talking about how theories are developed and deployed in these fields. Thus he says things such as, “survival is treated as an empirically testable hypothesis that . . . may be tested against the facts of experience, which in principle can confirm or disconfirm the hypothesis” (pp. 1–2). “Theory” and “hypothesis” are conflated in this statement. Later on, Sudduth introduces “auxiliary assumptions,” which at times sound like research hypotheses, at other times like theoretical premises, but mostly seem to be ad hoc propositions brought in to support the argument. Sudduth’s approach and terminology come from philosophy, not psychology, and that may be confusing to readers with backgrounds in the latter and related fields.

Another preliminary comment concerns the meaning of embodied vs. disembodied survival. This is a big issue for those (both proponents and detractors) trying to conceive of postmortem survival. In his classic treatment of the topic, Flew (1972) held that survival is intelligible only in the form of a subtle or

astral body, which would maintain form, contain personality and memory, and allow for socializing in the afterlife. Flew and others distinguish survival in an astral body from disembodied survival, but this is not the way that Sudduth conceives of the matter. For him, embodied survival means survival in a resurrection body (pp. 27–28), a concept absent from the survival literature in psychical research and parapsychology.

Then there is the question of “living-agent psi.” Sudduth dislikes the term “super-psi,” which he considers pejorative. His proposed “living-agent psi” is hardly an improvement, though. It is, in fact, a step backward, because it lumps together simple, regular psi, for which there is a great deal of evidence, with complex super-psi, for which there is little if any evidence. The issue here is the nature of super-psi. If super-psi were merely an extensive psi, Sudduth might have a point, but the psi that would be required to explain much of the reincarnation data, in particular, is often quite complex. It would involve not only the paranormal acquisition of information but the blending of this with psychokinetic activity. If “living-agent psi” is not intended to make super-psi seem more credible than it is, it will have that effect on the reader unaware of the paucity of evidence for complex super-psi. Apart from this problem, there is a conceptual distinction to be drawn between simple and complex psi operations. Toward the end of the book, Sudduth acknowledges this by introducing the term “robust living-agent psi” in the sense of super-psi, but if we need this concept, why not simply call it by its old and familiar name, rather than introducing an awkward new one?

Again and again Sudduth reveals that he does not appreciate what is asked of super-psi as an explanatory principle, especially in relation to reincarnation cases. His living-agent psi hypothesis, he tells us, “does not postulate unlimited psi, only psi sufficiently potent to accommodate the veridical features of the data” (p. 285). Fine, but in addition to veridical memory claims, there are emotional, behavioral, and physical aspects to the reincarnation cases. Children identify with the previous persons they speak about, they behave like those persons, they act in appropriate ways toward people known to those persons, and they may bear physical resemblance to or have physical traits in common with those persons. Physical features include birthmarks corresponding to fatal wounds but are by no means confined to them, as Sudduth seems to think (pp. 132–133). Nor do birthmarks invariably follow fatal wounds, as he implies. The behavioral and physical features of the reincarnation cases are a good deal more varied and extensive than Sudduth appears to realize (see Stevenson, 1997).

Another example of Sudduth’s limited appreciation of the facts to be explained is his statement that “the phenomena under discussion relate to ostensible evidence for survival that derives from patterns exhibited solely in embodied persons, none of which involve ostensible communications with the living that originate from discarnate persons” (p. 226). In a good number of cases there are what are called announcing dreams, which purport to be just this—communications from discarnate entities regarding their intentions to be reborn to the dreamer or a close relative of the dreamer. Apparitions are sometimes seen in the same role and there are instances of communications through mediums, where a similar intent is expressed. Sudduth asserts that “none of the data requires attributing to survivors any knowledge of this world acquired during a period of discarnate existence” (p. 226). In a footnote, he acknowledges that intermission memories—memories of the period between death and birth—present exceptions, but these are important exceptions, so why are they relegated to a footnote? He affirms that “ E_{CORT} (his summary of reincarnation case features) does not presuppose any period of conscious discarnate existence for living persons” (p. 226). True, but that is a characteristic of E_{CORT} , not of the data.

Sudduth picks up from Braude (e.g., Braude, 2003) the idea that reincarnation cases reveal greater motivation on the part of the subject than the previous person, and this becomes a piece of his closing argument (pp. 270–280). If this really were so, it indeed would suggest greater plausibility of a psi interpretation of the evidence relative to a survival interpretation. However, Stevenson (2001, p. 212) noted that in almost all of the cases he studied, the previous person died prematurely, often leaving some sort of unfinished business. There may be a desire to return to young children left behind, to collect or repay debts, to show widows where money is hidden, and so forth. There are several cases in which people stated before their deaths where they wished to be reborn, then apparently succeeded in carrying out their intentions. Intention and the prosecution of intention are apparent also in announcing dreams and intermission memories in which

children claim to have selected their parents. A motive for reincarnation—and control over where to reincarnate—on the part of the previous person or his postmortem counterpart is apparent in all 14 of the solved international cases and all 10 of the solved suicide cases known to me (Haraldsson & Matlock, in press).

Sudduth's response would undoubtedly be that allowing for these factors adds to the number of "auxiliary assumptions" that the "survival hypothesis" must bear. Each auxiliary assumption carries its own antecedent probability, which Sudduth presumes to be low, and the more auxiliaries that are accumulated, the more unlikely the survival hypothesis becomes. Against this tendency he thinks living-agent psi fares well, but how can it fare well when it becomes more and more challenged as the complexity of the phenomena for which it is asked to account increases? Braude (2003) calls this "crippling complexity," and the crippling complexity faced by super-psi explanations is the main reason he ended up favoring a survival interpretation of at least some of the data (2003, p. 306).

Moreover, is the probability of survival really so low? The existence and attributes of psi (regular psi) suggest what many parapsychologists have come to call "nonlocal consciousness." To the extent that this phrase has meaning, it implies that consciousness exists independently of the body; and if consciousness exists independently of the body, the door is wide open to its survival after the body's demise. I am not contending that the opened door presumes the persistence of memory, personality, and other psychological features after death, but it does make it conceivable. It is risky for Sudduth to use living-agent psi as a counter explanation for the survival data, because psi and survival share a fundamental auxiliary assumption, namely, that consciousness is not generated by the brain but rather exists independently of it. But perhaps Sudduth is on the side of the materialists here. He acknowledges that his arguments do not include "the alleged force of considerations from philosophy of mind and cognitive neuroscience that imply that consciousness or certain mental functions depend on a functioning brain" (p. 295). If his sympathies lie with the materialists, he has a completely different, and entirely unexplored, obligation to show how living-agent psi is a product of neural activity.

An important line of independent support for the survival hypothesis and several of its key auxiliaries comes from the quantum theory of Henry Stapp. In Stapp's view, quantum mechanics requires attention, intention, and will to be intrinsic properties of consciousness (Stapp, 1999). Stapp has shown that the survival of personality and identity is not incompatible with quantum mechanics (Stapp, 2009) and he has said that reincarnation would require only minor tweaking of its mathematical formalisms (Stapp, 2015). Stapp's theory is built on the standard or "orthodox" version of quantum mechanics endorsed by a plurality of physicists and is not to be dismissed lightly. Nothing of the kind exists for robust living-agent psi (super-psi), so the survival hypothesis has an advantage over the living-agent psi hypothesis in this respect. The antecedent probability of survival is relatively high and the problem of increasingly crippling complexity means that living-agent psi will never be its match, quite the opposite of what Sudduth contends.

Now, Sudduth claims not only that he has shown that the survival hypothesis cannot win the game, but that "*the classical arguments are unsuccessful in showing that there is good evidence for personal survival*" (p. 281, emphasis his). He says this because he thinks that the classical arguments depend on auxiliary assumptions that are not well supported. "To account for any of the strands of evidence, the survival hypothesis must enlist auxiliaries that are lacking independent support/testability, whereas . . . at least some of the psychological auxiliaries required by the [living-agent psi] hypothesis are independently plausible" (p. 295). I can't agree. The same psychological auxiliaries utilized by the living-agent psi hypothesis are drawn upon by the survival hypothesis. Because he is not as familiar with the data as he might be, Sudduth overlooks their psychological dimension and underestimates the psychological consistency of the survival hypothesis. Reincarnation and past-life memory are nothing if not psychological processes (Haraldsson & Matlock, in press).

I do not see how one can fairly charge that the classical survival arguments have failed to show that survival is the best interpretation of the evidence as a whole, but Sudduth is correct when he says that explanatory arguments are not enough. We have a considerable amount of data, and many ideas about what these data mean. We need to move beyond the exploratory stage in survival research and begin to construct proper theories of survival, reincarnation, and past-life memory, producing testable hypotheses that can

confirm or disconfirm the theories. Properly constructed theories, with well substantiated premises, will do much to address Sudduth's concerns about unsupported and ad hoc auxiliary assumptions and they may help us finally to settle the question of whether survival or super-psi provides the better accounting of the evidence.

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PARACOUSTICS: SOUND AND THE PARANORMAL edited by Steven T. Parsons and Callum E. Cooper. Hove, United Kingdom: White Crow Books, 2015. Pp. 309. \$17.99 (paperback); \$8.99 (Kindle). ISBN 978-1-910121-32-0.

Within the ranks of parapsychologists, researchers who study phenomena that are associated with claims of ghosts and hauntings are sometimes viewed as particularly controversial denizens of an already controversial field. In this environment, *Paracoustics: Sound and the Paranormal* should reassure readers that those who study anomalous sounds are serious in their intent and critical in their scholarship (notwithstanding the image of a ghost wearing headphones on the book's front cover).

Reports of paranormal activity are often associated with anomalous sounds that seem to have no physical source—unexplained rapping and banging, footsteps, breaths, shrieks, disembodied voices, whistles, and music. In *Paracoustics*, Parsons and Cooper have drawn together a collection of chapters that

review what is known about these kinds of anomalous sounds, as well as phenomena such as infrasound and electronic voice phenomena that cannot be heard by the unaided ear yet can be studied using electronic equipment. The book has an atypical structure, consisting of seven chapters by the editors, five chapters by other scholars, and appendices that consist of reprints of scholarly articles and brief methodological essays. In general, the book is exceptionally readable without being “dumbed-down.” Some of the published articles in the appendices may be difficult for readers without a research background, but even then the gist is clear.

In Section 1, Parsons leads off by providing readers with a brief primer on the physics of sound, followed by a chapter by Cooper on the psychology of auditory perception. Together, these chapters provide a useful introduction to acoustical concepts and the nature of hearing that provides a foundation for the rest of the book. After two chapters on the role of sound in hauntings (Parsons) and séances (Cooper), the editors collaborate on a history of research on electronic voice phenomena (EVP). Parsons then reviews research on the relationship between infrasound and subjective paranormal experiences, including his work with Ciaran O’Keeffe on the Acoustic Research Infrasound Detector and experimental studies of the effects of infrasound on subjective experience. Cooper concludes Section 1 with a chapter on cases in which people ostensibly receive telephone calls from individuals who could not have possibly made the call, either because they were deceased or were known to be otherwise occupied at the time.

In Section 2 of the book, Ann Winsper provides a chapter that focuses on the psychological characteristics of people who are more versus less likely to hear EVP voices in ambiguous recordings. Three chapters then examine three varieties of paranormal music—spontaneous music and melodious sounds (C. R. Foley), music that is heard at the time of death, either by the dying person or others who are present (Melvyn Willin), and music in shamanism, trance states, and spirit possession (Jack Hunter). The section concludes with a chapter by Barrie Colvin on the acoustic properties of anomalous rapping sounds. Colvin’s chapter (my personal favorite) shows that the waveforms of unexplained rapping sounds are quite different from raps that are produced in a conventional manner by striking two hard objects together. Whereas the waveform of an everyday knock shows maximum amplitude at the moment of initial impact that then quickly decays, the waveform of inexplicable rapping sounds shows an increase in amplitude to its maximum level, followed by a longer decay as well.

Section 3 presents reprints of five journal articles on the relationship between infrasound and hauntings (Parsons, 2012; Parsons & O’Keeffe, 2008; Tandy, 2000, 2002; Tandy & Lawrence, 1998), and three brief appendices in which Parsons offers practical advice for measuring ambient infrasound, recording and analyzing sound during paranormal investigations, and recording EVP. The book concludes with an extensive list of references and suggested readings.

By drawing together all of these topics within a single volume, the book establishes paracoustics as a distinct domain of study, which should attract the interest of both parapsychologists and amateur paranormal investigators. Every weekend “ghost hunter” would benefit from reading this book, both in terms of knowledge gained and exposure to the careful, critical, and measured approach that the authors generally take in discussing their topics. Although each of these phenomena has been discussed previously in its own right, much can be gained by considering them together.

Unfortunately, owing partly to its edited nature, the book does not delve as deeply into the possible connections among these topics as it might have, lacking an integrative, “big picture” analysis that draws connections among the various phenomena and poses questions for future work. Some of the individual authors speculate regarding possible processes—both mundane and parapsychological—that might produce the phenomena they discuss, but I finished the book wishing for more in the way of an integrative framework for thinking about anomalous sounds. In particular, little is said about whether the authors believe that various paracoustic effects reflect different processes or are mostly manifestations of the same thing. Of course, no one knows whether these experiences reflect similar or different processes, but readers would have benefitted from hearing the authors speculate on such questions.

By and large, the authors seemed to remain carefully agnostic regarding the causes of the phenomena they address, explicitly invoking “ghosts” or “spirits” as explanations on only a few occasions. When

dealing with such puzzling phenomena and so little data, I applaud authors who consider the merits of various explanations without giving the unwarranted impression that the issue has in any way been decided. At the most general level, the paracoustic phenomena examined in the book might arise from one or more of three broad categories of influences—mundane physical and psychological processes (such as the effects of infrasound on subjective experience or the effects of pareidolia on interpretations of EVP), parapsychological processes (ESP and PK), or disembodied consciousness. None of these explanations provides a parsimonious account for the full range of experiences described in the book, so readers need guidance about how to weigh the evidence and should be encouraged to suspend judgment until more is known. Certain authors seemed to accept evidence of paranormality at face value, while others seemed to dismiss it entirely because mundane explanations can explain some occurrences. From a scientific and logical standpoint, either conclusion seems premature.

Paracoustics succeeds on two different levels. In the hands of a parapsychologist, it provides a scholarly introduction to the role of sound in paranormal phenomena and fodder for theory and research on these topics. For the paranormal enthusiast and weekend investigator, the book provides an overview of interesting studies and cases that should prompt an informed, critical assessment of the auditory aspects of paranormal experience.

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THE MAN WHO COULD FLY: ST. JOSEPH OF COPERTINO AND THE MYSTERY OF LEVITATION
 by Michael Grosso. Lanham, MD: Rowman & Littlefield, 2016. Pp. xi + 251. \$38.00 (hardback). ISBN
 978-1-4422-5672-9.

In this effort, Michael Grosso has written an engaging, entertaining, and interesting book about not only the life story of one of the most fascinating Catholic saints, St. Joseph of Copertino, but about how we might account for the alleged levitations for which he is best known as genuine physical phenomena. Grosso, for his part, opens the text claiming that “[t]his book is about the possibility of transcendence” (p. 1), but, despite his dedication of the book to Pope Francis, he eschews religious interpretations in favor of a range of familiar paranormal hypotheses. The book is about wonderment and belief, to be certain, most explicitly that of Joseph and his contemporaries, but also about the author’s own perspective on the phenomena and their possibility.

I want to begin my review by stating unequivocally that I enjoyed reading this book, which I see divided into two distinct sets of material. On one hand, the book relates the life story of Joseph in a manner that is both highly informative and engaging. It is in this area that the book achieves its greatest success and would be a valuable resource for any reader interested in an accessible hagiography of this most intriguing of saints. The other material Grosso presents can be summarized as an attempt to defend the purported levitational phenomena as genuine and then to offer a potential explanation for how these miracles might be explained. Unfortunately, the arguments presented therein are altogether weak, relying more on a sense of wonderment than on a logic compelling enough to convince the skeptic.

The book itself is divided into three parts: Part I: The Man and His Marvels; Part II: Steps Toward Understanding; and Part III: Concluding Reflections. It is in the first part that the life story of Joseph largely appears, most of it contained within the first chapter. It is important to note that Grosso returns to details of the story of Joseph's life and miracles throughout the book, such that one interested only in the hagiographic elements will still benefit from reading the book in its entirety. Grosso does an excellent job of summarizing the details of Joseph's life, at times both wondrous and ugly, in a very readable style that encourages interest. Religious experts and readers who have no prior knowledge of St. Joseph of Copertino will be equally entertained by the presentation here.

Most impressive is Grosso's use of original sources in constructing the story. For this project, Grosso, with funding from Cedar Creek Institute and the Esalen Center, commissioned a translation of Domenico Bernini's 18th-Century hagiography, *Vita Fr. Giuseppe da Copertino*, representing the first English translation of the text yet to be made. One hopes that Grosso will make the entire translation available in the future. It is this text that provides the bulk of the accounts from first-hand witnesses. But Grosso does not stop there, as he also includes material from a number of other texts about the levitating saint, all of which are detailed in a useful Appendix that allows interested readers to seek out the material for more in-depth reading on their own. Of particular value in Grosso's endeavor are the number of reports from various trials from that time, which are included in several of these old Italian sources, as they often include further witness testimony of Joseph's miracles.

The rest of this first section moves into a defense of Joseph's levitation as a real possibility, one which would seemingly contravene materialistic science and its laws. As such, Grosso moves to show the shaky ground underlying a strictly materialistic view of the universe. Beyond this early point, I became increasingly unconvinced by his argument, and in the rest of my review I offer a critique less of the merits of the book as a whole than of particular core aspects of the argument presented therein.

To begin, Grosso spends an entire chapter (Chapter 2: A New Force) preparing what is essentially a kind of slippery-slope argument whereby he offers a number of "mind-body interactions" in which context Joseph's levitations, "at first so incredible, might begin to appear more intelligible" (p. 37). On offer are a series of strange phenomena that build in their capacity to amaze from relatively common flying dreams and the placebo effect to ever-increasingly amazing claims, including mediumistic materializations and examples of levitations from other religious traditions. The suggestion is that if one were to accept one strange phenomenon as real, why not the next more strange, and the next, until we arrive at levitation itself? Accepting strangeness in stages is no way to arrive at comfort with the possibility of levitation.

In fact, Grosso betrays an early comfort with the possibility of levitation even as he explains an instance of what he calls "quasi-levitation" as a "partial answer" to why he became interested in the phenomena in the first place. Grosso describes an instance of the relatively common finger-lift trick, sometimes called "light-as-a-feather, stiff-as-a-board," in which a group of five female students picked up a male classmate using nothing but their fingers. Although the physics of this party trick are fairly simple, and examples of how to do it can easily be found on YouTube, Grosso admits to an astonishment that he would never forget, concluding: "If a group of students could manage this little marvel, think what a seasoned religious community, under intense psychic pressure, might be able to accomplish" (p. 42). Grosso's reaction to this event may well speak to religious wonder, but it is also an example of how belief not only can leave one open to certain possibilities, but potentially close one off to otherwise obvious normal explanations.

Another problem with Grosso's overall argument is apparent in this chapter, as his list of astonishing mind-body phenomena moves all too fluidly between examples of a mind's interacting with its own body and a mind's interacting outside of its body. The placebo effect certainly illustrates a great power of the mind to influence one's own bodily health through belief, but that is an entirely different thing than the mind's being able to reach out into the world beyond. Grosso's mistake is a simple one that occurs early in his effort. While describing the psychokinesis (PK) that would seemingly be required for levitation, he offers an example of what he considers "a very normal type of PK [...] which occurs whenever our minds affect our own bodies" (p. 39). He suggests that when I mentally will my hand to write, I thereby exert psychic influence on my hand to cause it to write, which he then equates to "the more unusual mind-body

effects,” such as “man flying” (p. 40). His equation moves through the fact that materialists take issue with the mind in the first place, but ends up at a point that only muddies the water, since Grosso ignores the fact that my mind is never thought to directly affect the movement of my hand but rather, at best, my mind influences my brain, which in turn activates my hand to carry out my will. Materialists certainly take issue with the mind, but only insofar as it exists to affect the brain. If the nerves from my brain to my hand were severed, then we would not normally expect to be able to mentally will the hand to write. The problem that materialists have with the mind is not one of how I make my hand move but rather how I make my brain make my hand move. That Grosso grounds much of his argument on this unjustified leap seriously weakens any further attempt to discuss levitation effectively.

In his third chapter, *The Case for Joseph’s Levitations*, Grosso seeks to address the skeptical response to the mere possibility of levitation by, essentially, asserting the strength of the eye-witness testimony involved in the saint’s life story. Again, the story itself is fascinating, and the dozens of testimonies given over many years, and by those both positively and negatively inclined towards Joseph, are intriguing. But since Grosso asserts that we ought to trust in fantastic claims of the miraculous on witness testimony alone, then he must also defend not only sightings of such things as Bigfoot or the Jersey Devil, but all claims that appear in any of the religions of the world, from the veracity of the assertions of St. Paul to the reality of Joseph Smith’s golden plates, and thereby the religions based thereupon. Grosso asserts that the number of witness accounts is important, but where is the objective threshold beyond which we ought to accept a thing as definitively real? He says “one or two scenes that so-and-so mentioned” (p. 86) might be insufficient. What about the 11 first-hand written testimonies to the Book of Mormon? Of course, Grosso is inclined to some skepticism of first-hand reports, as, for instance, when comparing Joseph with the Buddhist monk, Milarepa, who was described as flying while walking, meditating, and even sleeping; here Grosso reaches his limit, stating, without further argument, “Sleeping in a levitated state sounds like a tall tale” (p. 59). In the end, the evidence in support of St. Joseph’s levitations remains a collection of centuries-old witness testimonies of a phenomenon that, when reported in other cultures, even Grosso maintains some skepticism toward.

Still, Grosso offers, throughout Part II, an explanation for how Joseph’s levitations might have occurred. It is entirely reasonable to offer speculation on how a phenomenon might occur if it occurs, so this theorizing has a useful place even if one disregards the evidence for St. Joseph’s levitation. Here I find an altogether different problem, and one again of perspective and preconceived belief. “To make sense of Joseph the mystic and thaumaturge,” Gross insists, “we need to expand the prevailing concept of mind” (p. 131). “The first step,” he says, “toward coming to grips with Joseph’s wild talents, then, is to question the dominant concept of mind. Otherwise, his story will strike you as beyond belief. We must stipulate an expanded concept of mind” (p. 150). Grosso clearly endorses an expanded-mind conception, and presents it as the best, in fact only, means of explaining the phenomena under consideration. Why we must focus on the powers of the mind, specifically, Grosso does not explain.

As a possibility for an alternate explanation, Grosso takes up materialism throughout the book, but never does he argue against a supernaturalist explanation for the phenomena. In fact, Grosso dismisses the supernatural explanation by asserting early on that it is “unhelpful,” questioning why something that is unexplained should ever be considered supernatural rather than a thing not yet understood (pp. 7–8). If God is involved, though, thus making the occurrence supernatural, then it is, by definition, outside the bounds of normal understanding and so requires no further explanation. Given Joseph’s status as a saint, established in large part on the acceptance of his phenomena as miraculous, a great many people have already come to believe in their genuineness without an “expanded concept of mind,” but rather by a belief that, crudely put, God made him fly. Accepting this possibility would seem to remove the need for any of the rest of Grosso’s argument, which delves into quantum indeterminism in a search for a “physics of levitation” (Chapter 8).

In the end, Grosso has succeeded in writing an entertaining and informative book based on some sources that have not previously been made available to an English readership. Grosso thus does a great service in bringing to light more details of the life of St. Joseph. I believe that the hagiographic detail provided is expressed in a lively and engaging manner that would be of interest to many readers, general and academic. Although Grosso’s arguments rest on the shaky grounds of an overreliance on eye-witness

testimony and unnecessary leaps of faith and reason, collectively their presentation makes the book useful for an examination of biases, which would be of value in courses in critical thinking and to those interested in the role of wonder in belief and experience. Grosso's arguments in favor of Joseph's levitation and any possible explanations for them ultimately rest more on belief and might find favor among those readers similarly inclined.

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ILLUMINATIONS. THE UFO EXPERIENCE AS A PARAPSYCHOLOGICAL EVENT by Eric Ouellet. San Antonio, TX : Anomalist Books, 2015. Pp. 234 (paperback). ISBN 193-839853X

Eric Ouellet has a Ph.D. in sociology and is professor in the Department of Defence Studies at the Royal Military College of Canada, located in the Canadian Forces College in Toronto. He is the Parapsychological Association's liaison officer for Canada. In this book, he has gathered together papers published in parapsychological journals and several years' worth of blogging reflections on and discussions of the links between UFO phenomena and parapsychological research. Having heard about a UFO observation by a family member (not seeing anything himself) when he was a child (p. 5) and having a UFO experience himself in 2010 (pp. 6–7), the sources of his interest are obvious. But the perspective he adopts has never been discussed so extensively.

In his literature review, Ouellet points out (Chapter 2, pp. 25–32) that the boundary between parapsychology and ufology has already been crossed by several authors, such as Carl G. Jung, Allen F. Hynek, Jacques Vallée, Bertrand Méheust, Pierre Viéroudy (aka Pierre Berthault), John Keel, Jerome Clark, Loren Coleman, David Scott Rogo, Jenny Randles, and Berthold Schwartz. The chapter is very interesting but far from exhaustive. Several authors from France that he does not mention have also contributed to this topic: The physician François Favre (1973, 1996) developed Jung's comparison between UFO and ectoplasm; the physicist René Hardy created a research group on paradynamics at the Institut Métapsychique International, and he gathered information on UFO and macro-PK phenomena such as levitation (Evrard, 2010); the chemist Michel Granger (2010) discussed in several articles application of the psi hypothesis to UFOs. Using a different approach, the sociologist Pierre Lagrange (2009) compared, in his doctoral thesis, the sociology of the parasciences with that of ufology, parapsychology, and cryptozoology as main examples.

Thinking that the paranormal is actually the core of the UFO phenomenon (p. 161), Ouellet calls this approach the "parapsychological hypothesis" (Chapter 3, 33–50) because "it is essentially based on ideas, knowledge, models, and findings that are found in the field of scientific parapsychology" (p. 2). But he mainly uses as scientific support the model of pragmatic information (MPI) applied by Walter von Lucadou and Frauke Zahradnik (2004) to RSPK (i.e., poltergeist) phenomena. Paradoxically, this link is motivated by a skeptical stance toward UFO evidence: "Although UFOs are sometimes said to leave traces, no conclusive physical evidence is ever found" (p. 14). Ouellet is mainly interested in the framing of UFO experiences as a mixture of physical and psychological phenomena, of which the "most important material characteristic" (p. 14) is their elusive character.

The MPI application to RSPK phenomena has led to some predictions, but it has hardly been tested since its formulation. The 2004 article is mainly a phenomenological description of the development of RSPK cases in four phases: surprise, displacement, decline, and suppression. The descriptions are based on empirical observations but are mainly assertions based on a weak formalism; that is to say, there are still huge gaps between the mathematical and the literary language used to describe RSPK dynamics. Some aspects

of this article are still very equivocal, and it seems perilous to take it as representative of psi research (and even as representative of this model) which follows multiple lines of inquiry, some of which lead to relevant interpretations of experimental outcomes (Walach, von Lucadou, & Römer, 2014).

Ouellet, however, uses this model as an entry point, as it is based on a systems theory approach and integrates the social dynamics with the whole system under study. He then uses the term “social psi” proposed by parapsychologist Dean Radin (2006, p. 295) to label the collective effects and aftereffects of psi. Social psi also allows the possibility that many people are involved in a spontaneous event, but no particular individual is responsible for it, as is the case for some parapsychological and UFO events. It helps to be able to interpret situations with many witnesses without having to involve any nonhuman entities.

The other chapters are dedicated to the application of the parapsychological hypothesis to many UFO sightings, and especially UFO waves (increases in local UFO sighting reports over a certain period of time): the 1954 French UFO wave (pp. 48–49), the 1952 Washington D.C. UFO wave (Chapter 4, pp. 53–65), the Belgian UFO wave of 1989–1991 (Chapter 5, pp. 67–89), the Rendlesham UFO incident of 1980 (Chapter 6, pp. 91–105), the Canadian UFO wave of 1966–1967 (Chapter 7, pp. 109–132), and the Barney and Betty Hill story (Chapter 8, pp. 133–148).

As parapsychologists do in RSPK cases, Ouellet always attempts to identify the focal person(s), defined as “someone going through some sort of turmoil but who cannot express it for some reason except through psi effects” (p. 45). He has his own parameters for applying this definition to groups and social psi effects (pp. 46–47): (a) The observation system is social; (b) it has geographical proximity; (c) it has chronological proximity; (d) there is a “symbolic relationship” between the system and the events. But these criteria are so loosely defined that they can be applied to almost anything. Then Ouellet only has to find an event that creates social tension, even if it will be felt only many years later. He then uses a hermeneutical argument to link the social event with the anomalous event. Sometimes the argument seems relevant, sometimes less so, but the main problem is that this retrospective method is not falsifiable. The percentage of social events that fit these criteria is just too large to differentiate a synchronicity from a mere coincidence. For example, Ouellet argues that the 1954 French UFO wave, with sighting peaks on October 3 and October 15, occurred simultaneously with the formation of a small group of anticolonial insurgents in Algeria (*Front de Libération Nationale*), a development that was unknown to the French public in October and was not seen as threatening even after the terrorist attacks in November 1954. But Ouellet doesn’t need perfect timing; only “chronological proximity” is required. Finally, he concludes that an unconscious “collective premonition” was the source of the UFO wave (p. 49). This is not an isolated example: Many times Ouellet associates anomalous events with political or social events, which fly under the radar at the time of their occurrence but become parts of the historical record later. This “political sociology” of UFO experiences and alien encounters (Chapter 9, pp. 149–159) is not really convincing. The symbolic perspective, although it remains attractive, gives him too much freedom to approximate a scientific analysis. As he puts it in his conclusion (p. 162):

This approach frees us from the false choice of having to decide if the phenomenon is either subjective or objective, because it appears to actually be both at the same time. It also provides more flexibility in understanding the phenomenon because it allows for the subjective and the objective aspects to influence one another while existing in parallel. By removing this artificial barrier, this false either/or category, a wider perspective can be developed to embrace the full complexity of the phenomenon.

Perhaps adding more constraints to the model and offering testable predictions would improve this methodology.

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To the Editor:

The papers (Parker, 2015a, 2015b) to which Dr. Cardeña refers in his letter published in the Fall 2015 *JP* (Cardeña, 2015) were intended to be constructive critiques of all the recent work on the hypnotic state and on hypnotic psi. It is rather against the spirit of these papers that Cardeña dismisses the review as being “slanted,” having “innuendos,” and being “inaccurate.” I also regret that Dr. Cardeña refers to a debate over the loss of the Freiburg chair and its funding for psi research, but he does not inform the reader of my detailed reply to this rather different issue (Parker, 2013).

It is for me not meaningful to spend time on snide remarks and diversions to other issues. Nor is it so meaningful to spend time on who first coined specific terms within the hypnosis area, but it has to be said categorical statements are not in order. For instance, concerning the coining of the word “hypnotism,” it is true that “D’Henin de Cuvillers used various terms beginning with ‘hypno,’ as for example hypnoboté, hypnoscopie, hypnotique, hypnologie but while he may have been getting close, none of his terms caught on” (A. Gauld, personal communication, October 21, 2015). I should however thank Dr. Cardeña for spending valuable time on finding amongst 30 pages of text that “dissociation” in DES should have actually been written “dissociative” and what was actually a “he” should indeed have been written as “she.” I also thank Dr. Cardeña for making clear his use of “wolverine” which I had misunderstood.

Nevertheless, I take the above tone of animosity to indicate Dr. Cardeña thinks he was unfairly treated. I wish therefore to look at the relevant complaints and see if any of them have substance and can be resolved fairly. I did say I thought the study had a “handful of participants,” and it was “overloaded with variables and hypotheses” that were tested on small subgroups. The “handful of participants” refers of course not to the starting number, which was 26, but rather to the fact that some of the main hypotheses were evaluated using five to nine participants. Frankly, I would find it difficult to get any support from statisticians for drawing conclusions on *F* tests with cell frequencies as low as five. I confess that I cannot understand why Dr. Cardeña does not agree with me. Instead, Dr. Cardeña countered by saying that our ganzfeld studies had only a few more participants than his starting quota of 26. He will find on reading the relevant studies concerning psychological variables that these were based on 50 participants (Parker, 2000; Parker, Grams, & Pettersson, 1998).

The overall outcome of the Marcusson-Clavertz and Cardeña (2011) ganzfeld study with the 26 participants gave 27% as the hit rate, which is of course very near mean chance expectancy. It is never stated (beyond saying it was process research) why significant overall scoring was not a main hypothesis and why there was a failure to replicate previous findings. Instead of this, the psi scores were analyzed in relationship to numerous variables: hypnotizability, dissociation, eleven dimensions of the PCI, and the dimensions of belief in psi. The description “overloaded with variables and hypotheses” is then surely not too “inaccurate.”

In his defense, Dr. Cardeña advertises that he has one study that used even more variables than were “accepted into the very rigorous *Journal of Experimental Psychology*.” A recent evaluation of its replication rate (Open Science Collaboration, 2015) suggests it might not be so rigorous. Whatever the case, surely the malpractice of others is not a good argument—and certainly not something for parapsychology to emulate.

In the review of the Marcusson-Clavertz and Cardeña study, it was stated that the “sheep-goat effect” was the one positive finding. This is conventional shorthand for what Dr. Cardeña means when he says two of their hypotheses were confirmed: Hypotheses 1 and 2, namely, those concerning “believing that one would be ‘successful’ in the experiment” and “having previous psi experiences.” If so we are in agreement. Since we also agree Hypothesis 3 (concerning psi scores of those with high versus low hypnotizability) was not confirmed, this leaves Hypothesis 4.

Hypothesis 4 was that dissociation would modify the effect of hypnotizability on psi scores. Cardeña still claims this was confirmed. Other than a negative relationship between hypnotizability taken alone and

the psi score, I cannot find anything to support this claim.

Hypothesis 5 concerned predicting a relationship between psi scores and the reporting of altered states (in the ganzfeld). However, the only significant finding, as interesting as it is, occurred only *within the highly hypnotizables* and is clearly post hoc. Moreover, the questionnaire used to evaluate the altered state in the ganzfeld session was assessed in another (mind-wandering) experiment, which seems a little problematic.

Dr. Cardeña further maintains the choices of statistics used for testing of all the hypotheses were all specified in advance. Maybe they were, but where? What is written on page 244 does not appear to have been prespecified. Likewise, when it concerns the nonparametric tests, Cardeña refers us again to page 244 of his original text and such tests are certainly mentioned there, but here again, where are the results of these tests? The only exception I can find concerns the above finding that I do not dispute, namely the sheep-goat test.

Dr. Cardeña rejects my suggestion that they did not identify which were the post hoc findings: He replies this is “false since we described our hypotheses in the paper and the reader can then precisely determine which are the post-hoc findings.” However given the vagueness of some of the hypotheses and the multiplicity of the tests used, to insist on this surely presupposes some form of extraordinary ability on the part of the reader in order to discern what is what. Magnus Fontes, professor of statistics at Lund University, totally dismissed the whole study, expressing a radically opposite opinion on the claims that were then made and which Dr. Cardeña still continues to make (Fontes, 2013). The same was true of Georg Lindgren, the statistician included amongst the nine professors who published a critical article titled “Pseudoscience is Spread Uncritically” (Halle et al., 2012), which might easily have led to the loss of the Lund chair.

It is of course true, as Dr. Cardeña says, that it is now possible to register the means of testing hypotheses in advance via the Edinburgh KPU study registry, but the debated study was carried out prior to the registry and is not to be found there.

Finally, Dr. Cardeña asks for a reference concerning the possible bimodality of the Dissociative Experiences Scale (DES). The term bimodality can be, on reflection, misleading here, but I was wanting to summarize the Wright and Loftus (1999) review, which concluded there are two different populations with their own means—those with dissociative disorders and those without. Given that Cardeña’s reply suggests that we agree that the DES is a highly skewed scale and is aimed at identifying pathology, then why use it on students when there are plenty of other good tests of dissociation aimed at measuring healthy experiences?

At the end of his reply, Dr. Cardeña thinks it unfortunate that I had not discussed “integrative, multifactorial models,” but perhaps there is room for agreement here too. Although the model he seems to prefer, namely Shor’s model (and I do apologize for once making a typical Swedish typo in spelling Shor’s name), is of early 1970s vintage, it should be said that some of the more skeptical contemporary theorists are indeed now moving slowly towards this type of theory of hypnosis (see, e.g., Lynn, Laurence, & Kirsch, 2015). I had hoped that the very titles and themes of my two papers would communicate just that: Hypnosis is a jungle of variables—or if you will, multifactorial—which makes it exceedingly difficult to combine with psi research, as I believe Dr. Cardeña’s work illustrates. The papers I wrote focused finally on the specific ways in which research in this complex multifactorial area can go further.

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To the Editor:

Dr. Parker continues to attack our 5-year-old publication (Marcusson-Clavertz & Cardeña, 2011) and also my reply (Cardeña, 2016) to his criticisms. Regarding his remark that his papers were “intended to be constructive critiques,” anyone who is aware of his more than decade-long public and private history of attacks against me will be able to evaluate his likely intention.

Now to more substantial issues:

1. Dr. Parker maintains that it is not “so meaningful” to spend time pointing out the various typos, misinterpretations, and inaccuracies in his original papers, but besides correcting the record they provide a potential indicator of how careful and accurate may be the rest of the content.
2. His statement that “some of the main hypotheses were evaluated using five to nine participants,” is highly misleading. We had five main hypotheses (Marcusson-Clavertz & Cardeña, 2012). Hypothesis 1, belief in self-success in the psi task will be positively related to psi scoring, was confirmed with all 26 participants. The same was true of Hypothesis 2, which postulated that previous psi experiences (not the same as the “sheep-goat” effect, contrary to what Dr. Parker states) would also correlate with psi scoring. For Hypothesis 3, we conducted a *t* test between high and low hypnotizables (a correlational analysis would be inappropriate because the medium hypnotizables were excluded), comparing cells of 14 versus 12. For Hypothesis 4, contrary to Dr. Parker’s claim that it “was confirmed,” we actually wrote on page 246 that “Neither was there a significant interaction between hypnotizability and dissociation.” This is the *only* hypothesis-testing analysis for which we had cells of between five and nine. Incidentally, Table 6 of one of his papers shows that Dr. Parker (Parker, Grams, & Pettersson, 1998, p. 330) analyzed cells of $n = 8$ and $n = 10$, besides carrying out many analyses on multiple variables. Finally, for Hypothesis 5, which proposed that greater alterations in consciousness would relate to high psi performance and hypnotizability, the significant result with a very strong size effect was with the group of 14 high hypnotizables, not a cell of “five to nine.”
3. As for not predicting that participants overall would significantly psi hit, we wrote on p. 244, “Evaluating overall psi was not a target of this process-oriented research.” Choosing participants

that we thought would be psi-hitters along with those we thought would be chance-scorers or even psi-missers made it oxymoronic to predict overall success. As Bem, Palmer, and Broughton (2001, p. 215) wrote “genuine progress in understanding psi rests on investigators’ being willing to risk replication failures by modifying the procedure in any way that seems best suited for exploring new domains or answering new questions.”

Regarding other misleading statements by Dr. Parker:

1. He mentions that we did not specify “which were the post-hoc findings,” but on page 247, we stated that analyses with other PCI dimensions than the *altered state* one “can be considered exploratory.”
2. As for preregistering our study, at that time it was not common practice and the PKU registry was not even started until the fall of 2012, so if Dr. Parker wants to blame us for that lack of precognition, perhaps he could start listing his own previous studies with preregistered analyses. And, as I mentioned in my previous reply, the samples for each one of his studies (Parker et al., 1998) was 30. Whether he combined his data later for some analyses is irrelevant to my statement.
3. Contrary to what Dr. Parker seems to hint, an indication that we were aware of the strengths and limitations of the DES and other dissociation instruments is that the American Psychiatric Association tasked me some years ago to write a review chapter of dissociation measures (Cardena, 2008).
4. Dr. Parker questions that the *Journal of Experimental Psychology: Learning, Memory, and Cognition* is a rigorous journal because a recent Open Science Collaboration paper found problems with replicability of some studies (incidentally, these findings have also been disputed, see Gilbert, King, Pettigrew, & Wilson, 2016), despite its 78% rejection rate (<http://www.apa.org/pubs/journals/features/2013-statistics.pdf>) and 2.86 impact factor. Sadly Dr. Parker offers no references to journals in which he has published with better statistics than these.
5. Finally, Dr. Parker might cite and agree as much as he wants with the antipsi people here in Sweden, but his statement that their criticism “might easily have led to the loss of the Lund chair” goes against the public statements of support I received in the media by Lund University’s then-President Per Eriksson (Stiernstedt, 2012), the current Dean of my College Ann-Katrin Bäcklund (Anonymous, 2012), and the then-Chair of the Department, Per Johnsson (Fagerström, 2012; see also Cardena, 2015).

Is there anything I can agree with Dr. Parker on? Well, of course our study had a number of limitations, which we listed on page 252 and which Dr. Parker repeats, including a small *N* and using the measure of alteration of consciousness with the same people under ganzfeld but not from the psi session. As with most research in any field, it should not be considered definitive but as evidence supporting some hypotheses and not others, and offering some promising new leads. So I hope that he will agree with me that it will be more constructive for everyone concerned to spend time conducting their own preregistered new research with large *N*s rather than pursuing this debate.

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To the Editor:

Few recent parapsychological experiments have given rise to as much acrimony as a study in this journal by Dr. David Marcusson-Clavertz and Prof. Etzel Cardena (2011; hereafter DM/EC). They reported that, for high hypnotizables *only*, there was a significant correlation between psi z scores and being in an altered state of consciousness. Later in an interview, EC was quoted as having said that this correlation is "... a very strong indication that telepathy has really occurred" (Oredsson, 2012, p. 17). Vociferous exception was registered by a number of Swedish academics (Halle et al., 2012). They emphasized that the overall hit rate was in fact nonsignificant (27% direct hits, chance = 25%) and considered the correlation an artifact of multiple analyses: The study was profiled as pseudoscience. For most English-speaking readers this controversy lies largely behind the (Swedish) language barrier (but see Cardena, 2013a, 2013b).

More recently, in the course of a major review of psi and hypnosis research in this journal Prof. Adrian Parker (2015) characterized the DM/EC study as "overloaded with variables and hypotheses" (p. 41). Cardena (2015) has disputed aspects of Parker's criticism but did not really settle the underlying issue. Is the DM/EC inference from the data really justified?

The recent statement by the American Statistical Association (Wasserstein & Lazar, in press) highlights the gulf between "statisticians' statistics" and "researcher's statistics" and brings the vexing problem of multiple analyses into the limelight. Every additional analysis is basically an extra shot at the significance jackpot and the strict logic of statistical significance testing requires that a "family-wise" (or similar) analysis is performed per study; only then is the probability value calculated correct for the *set* of analyses, rather than for the individual analysis. This does not mean that only a single variable should be investigated per study; rather the *set* of *p* values must be corrected appropriately (e.g., Bretz, Hothorn, & Westfall, 2011).

Most researchers do not formally correct probabilities for multiple analyses. If a *large number* of analyses are performed this has unfortunate consequences: (Virtually) every such study contains sufficient (spurious) "significances" to be published as "evidence of some effect." It is unlikely that such a study will come up totally empty with "no evidence for an effect." Karl Popper (1959 /1992) proposed that the essential characteristic of scientific theories is that they are *falsifiable* (demarcation criterion). Excessive uncorrected analyses "immunize" against the possibility of falsification.

With 100 analyses per study the (binomial) probability of some "significances" is as high as 99.4% and the corresponding empty hands (no "sigs") is a negligible .6%. For 14 independent analyses per experiment just more than half of the studies are expected to be "publishable" merely by capitalizing on chance. Stacking the odds in the researcher's favour by multiplying analyses is decidedly not "playing the game."

In lieu of formal correction for multiplicity, researchers early devised a rough and ready work-around. Two broad classes are distinguished—confirmatory and exploratory studies. For confirmatory studies the number of analyses is typically restricted to a few and significance testing can be meaningful (if tak-

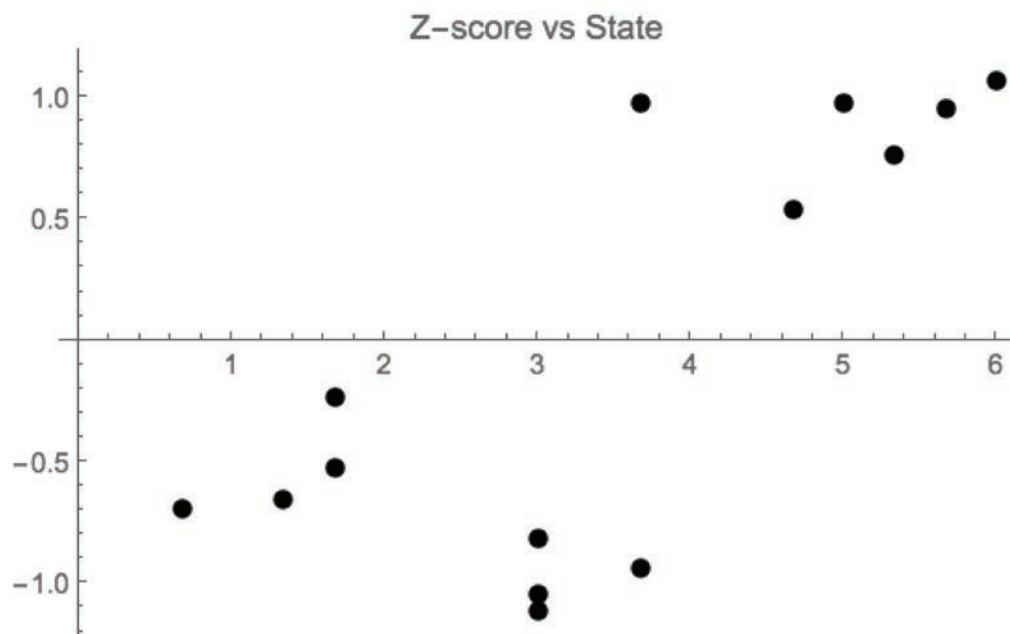
en with the appropriate amount of salt). In exploratory research, the net is spread wide, over every variable that might be relevant. Exploratory studies do not *prove* anything: the idea is simply to get some idea of what's what. For such studies statistical probabilities are essentially meaningless (unless the total number of analyses is known). For exploratory work “significant” findings are, at best, hints which have to be followed up by rigorous confirmatory studies. Some researchers, however, speciously regard “significance” ($p < .05$) as a “magic marker” for a real effect, *regardless* of how the analysis came to be carried out. And the mandatory publication ritual is to report such nugatory “significances” even for exploratory studies.

In the DM/EC paper there is no discussion of the distinction between exploratory and confirmatory studies. Is the study exploratory or confirmatory, or perhaps some mixture of the two? Were sufficient analyses actually performed to make cherry picking a plausible explanation for most (or all of) the results of the DM/EC experiment? In the following I prefer the term “selection” (with scare quotes) to avoid unintended negative connotations.

In reply to Parker (2015), Cardeña (2015) states: “As far as ‘overloading’ of hypotheses, we had a big total of five” (p. 253). There is, however, no mention of the statistics to be employed and each can be tested in many ways, limited only by the ingenuity of the analyst. Unless an *exact* specification is made, the researcher is free to select (unwittingly) whichever “equivalent” analysis is most significant. EC maintains that “... the reader can ... precisely determine which are the post-hoc findings” (p. 253). But this reader is not alone in being unable to identify them unambiguously. It would have saved a great deal of puzzling if each analysis had been clearly labelled (confirmatory or exploratory). Were there, as EC implies, only five preplanned analyses, with the remainder (about 95%) post hoc (exploratory)? In Tables 1 and 3 alone there are as many as 78 analyses reported and there are many others in the text. A minimum of a hundred analyses must have been carried out in all—substantially more (one hopes) than in a typical parapsychological study. Analyses outnumber participants (26) by something like 4 to 1. Although the study seems to be very much of the exploratory type, DM/EC do not so much as mention the impact of multiple analyses: A single Bonferroni correction is reported in passing (p. 244).

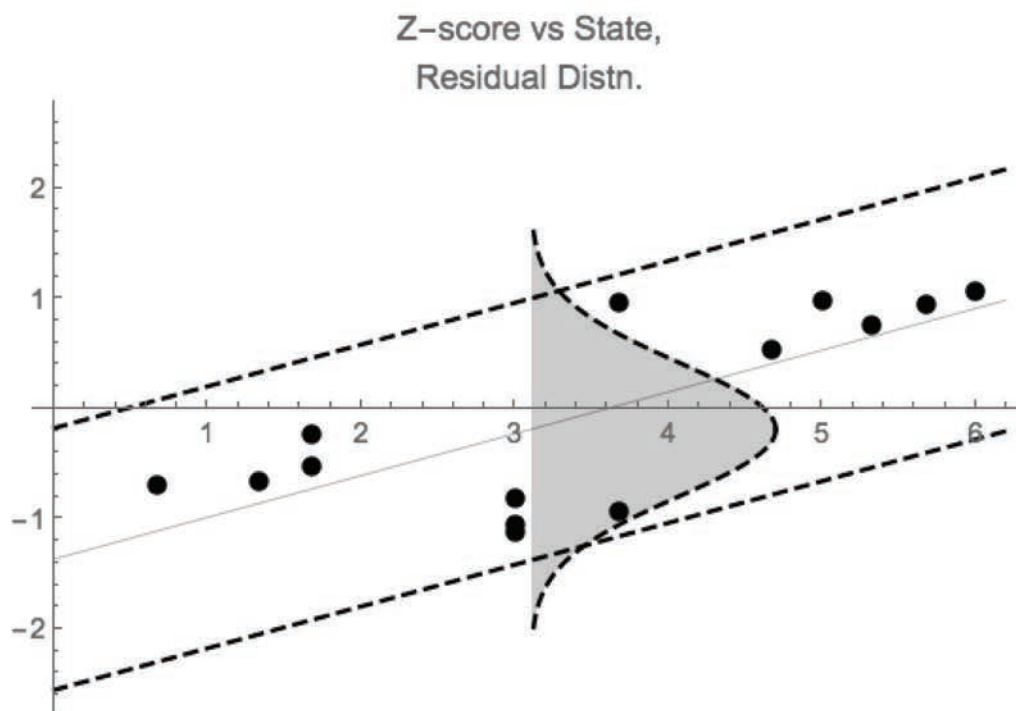
In the first column of Table 1 are a total of 15 correlations with psi z scores, six accompanied by “significance stars.” However, when corrected for multiple analyses using the Holm method (R package “p.adjust”) only one remains significant. This is the altered state/psi correlation, which DM/EC regard as their prime finding: “Generally, our results may be the clearest evidence until now of a relationship between experiencing an altered state of consciousness and psi-hitting . . .” (Marcusson-Clavertz & Cardeña, 2011, p. 251).

The raw data for high hypnotizables are regraphed as Figure 1.



Inspection reveals that “psi-hitting” palpably does NOT increase with increasing state change. The z score actually changes from *missing* for low state change to *hitting* for larger changes. This is difficult to interpret in terms of participant state change. Here the slope is significant ($p = .002$), whereas the mean z score for the DM/EC psi data is $-.05$, very near zero.

1. The most negative (regression) z score is (perversely) at zero state change: This intercept is significantly negative ($p = .003$). The most positive z score at maximum state change cancels this out (overall chance scoring).
2. A peculiarity less obvious to the eye is that many of the data points seem to be just “too close” to the regression line. This residual variance is 0.37 , only about a third of the expected value for a z score (1.0): This is significantly low ($p = .025$).



The pattern is very similar to what may arise due to experimenter effects. In the simplest case, the experimenter has a hypothesis that a given experimental (E) condition is associated with the occurrence of psi and compares this with a control (C) condition. A significant E/C difference is interpreted as supporting the hypothesis, but closer examination reveals C is about as much *below* chance as E is above and together they cancel out. For a continuous independent variable, as here, there is a significant correlation without any overall psi. There is actually a little evidence in parapsychology suggestive of an experimenter effect with hypnotized people. In one experiment (Fahler & Osis, 1966) confidence calls produced a highly significant positive deviation, whereas the remainder gave an almost equal negative deviation, the classic mirror pattern.

Although an experimenter effect is theoretically a possible explanation for the results of the DM/EC study, much more plausible is run-of-the-mill “selection.” To make things concrete, imagine that with everything else being the same as in DM/EC, random sampling from a standard normal replaces the empirical psi z score measures. A linear regression is done for each of a very large number of these thought experiments and those with nominally significant slopes are selected. The properties of these “selected” experiments will be systematically different from the great mass of pure random regressions and not just in that they have significant slopes.

It is easily seen that such “selection” accounts for the zero-straddling (change from missing to

hitting) nature of the claimed DM/EC regression, because “selection” for slope does not change the (zero) mean. The total variance too is not influenced by selection for slope; but this is the sum of regression and residual components, and since those with large regression are selected, there is systematically less residual variance. The “selection” hypothesis accounts neatly for all of the observations above, in particular the very peculiar deficit of residual variance.

I have implemented the above thought experiment for “selection” as a numerical computer simulation. Although details lie outside the scope proper for a Letter, I note here that the mean simulation regression line is virtually the same as that from the DM/EC study.

Many (perhaps most) parapsychologists “want to believe.” For a confirmatory experiment, statistical testing imposes some restraint. In an exploratory study, this check on “fooling oneself” is missing and uncorrected statistical probabilities actually seem to support belief. Did DM/EC deceive themselves into believing one thing they wanted to see in the extensive array of analyses they laboriously carried out, like tea-leaf reading?

Uncorrected multiple analyses is the one “questionable research practice” (QRP) to be found, to some degree, in nearly *every* published study. Parapsychology too has its fair share of such studies (a single putative example is Honorton, Davidson, & Bindler, 1971). The DM/EC study is no worse than many others but *no worse is not good enough*.

About 70% of psychological studies published in the premium journals have recently proven unrepeatable in other hands (Open Science Collaboration, 2015). While long suspected, it is still a blow to find that two thirds of psychology findings now appear to be spurious. For too long researchers have relaxed the rigour theoretically required for experiments and some QRPs have even become the norm: The chickens have come home to roost. Current methodological standards in the behavioral sciences are woefully inadequate for experimental psychology itself. Tighter procedural and statistical standards of evidence are required for more contentious areas, such as parapsychology.

Dr. John Palmer (2013) has set out his editorial policy with respect to multiple analyses and whether statistical corrections should be made. Like most journals this is regarded as the responsibility of the individual author. The general problem, however, cannot effectively be tackled on that level: Preregistration of experiments and planned analyses are the way to go. Will this make positive (parapsychological) findings vanish? This does seem to be the case for clinical trials testing heart disease treatments (Kaplan & Irvin, 2015).

In a forensic context, eyewitness testimony has been extensively studied (Loftus, 1996). In general, testimony is surprisingly unreliable: Factors which play an important role include reconstructive memory as well as confirmation bias. In theory, all analyses should be preplanned in full detail before an experiment is ever begun. But in practice (for other than very simple designs) this does not reflect the messy reality of actually carrying out a study: Updates are continually made based on earlier results. I have seen this taken to the extreme of taking the complete data of a finished experiment “upstairs” to the resident SPSS expert, who is expected to deliver publishable “significances.” Much more subtle (and common) is the distorting effect of reconstructive memory on slowly changing predictions: The prediction update is “written over” the original memory and the researcher is prepared to swear this is *the* “preplanned” analysis. Published analyses often bear little relationship to the original plan (cf. Millar, 1980; Tart, 1980).

Many a researcher feels his scientific integrity attacked if publicly suspected of cherry picking, and Cardeña (2015) characterizes Parker’s suggestion as innuendo. But it is increasingly recognized that researchers are not the infallible authority figures of the late 19th Century; rather they are subject to the same human frailties as the people they study. The DM/EC study was carried out over a considerable period (two years for participant selection alone) and was interwoven with other work. Long drawn out and segmented activity seems particularly conducive to the subversive effect of reconstructive memory. No special precautions are mentioned to ensure that any analyses planned did, in fact, remain constant over time. The bottom line is that only preregistered analyses should be taken at full face value. Happily, EC has largely preempted suggestions of where to go from here by planning a followup study, for which the critical analyses to be performed have already been preregistered.

In summary, the DM/EC report follows the style of an exploratory study: Confirmatory analyses are not clearly distinguished from exploratory ones. “Significant” results claimed are tenuous if they are considered against a background of considerable multiplicity of analyses. In particular, the flagship correlation of psi z scores with state of consciousness measures bears telltale traces of “selection” rather than the signs of a real effect of the participants’ state of consciousness.

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To the Editor:

I am flattered that one of our papers (Marcusson-Clavertz & Cardeña, 2011) has provoked not only one (Parker, 2015; Parker, this issue) but two reactions, currently the letter by Dr. Parker’s sometime collaborator Dr. Millar (Parker & Millar, 2014). I will first clarify some points and then describe our points of agreement and disagreement.

Regarding the quotation in the employee magazine *LUM* by Ms. Oredsson (2012), what I mentioned to her in our interview was that we had found a “large correlation” (a technical term for correlations greater than .50) between psi z scores and an altered-state scale. She thought that the general audience would not understand the technical term and chose the phrase of “a very strong indication of telepathy,” a phrase which cannot be found in our original study. As anyone who has interacted with the media knows, one may ask a reporter to use more precise language but they may choose a different route. In a response to the Halle et al. (2012) attacks, I mentioned to newspapers that the evidence for psi in the ganzfeld rested

in ganzfeld meta-analyses rather than in the findings of a single study such as ours (e.g., Cardeña, 2012).

As for the criticism by Halle et al (2012) about the overall close to chance hit rate (27%) in our study, they had reason to know of its lack of validity because before they published their letter I had sent them a copy of our paper, in which we specifically stated on page 244, "Evaluating overall psi was not a target of this process-oriented research." How could it be when we chose to include two groups (low hypnotizables and low dissociators) whom we thought might be random scorers or even psi-missers?

Now, with regard to Dr. Millar's own misinterpretation of our work, he stated that "In the DM/EC paper there is no explicit mention of the distinction between exploratory and confirmatory studies." Darned if on page 247 we did not write "We estimated the correlations between psi z scores and all of the PCI major dimensions. Except for the altered state dimension already discussed, these analyses can be considered *exploratory* (emphasis added)." Thus, the confirmatory analyses referring to the dependent variable (psi z scores) are reported only in Tables 1 and 2, not in 1 and 3 as he implies, and encompass 20 analyses for the dependent variable blocked according to the factors in our hypotheses. Millar writes rhetorically "Were there, in fact, only five pre-planned analyses...?" To which the answer is yes. We designed the experiment to have one dependent variable (z scores) and a few independent variables, mentioned in our hypotheses: hypnotizability, dissociation (as mediator or moderator of hypnotizability), belief in success in the experiment, previous ostensible psi experiences, and an overall experience of being in an altered state of consciousness. Our hypotheses were not post-hoc elucubrations but were based on previous research findings by others mentioned in our paper, including: (a) a meta-analysis showing a relation between success in a psi task and belief in one's own experimental success (Lawrence, 1993); (b) a report of a positive relation between reported ostensible psi experiences and experimental success (Honorton, 1997); and (c) a meta-analysis showing participants doing better in a hypnosis than in a nonhypnosis context (Stanford & Stein, 1994); and we had reason to predict that the former would be more likely to impact high hypnotizables. To our knowledge there had not been studies of dissociation and psi performance (as compared with reports of ostensible psi in everyday life), but there is previous research showing that hypnotizability and dissociation interact in important ways (e.g., Terhune & Cardeña, 2010), and thus it made sense to hypothesize that dissociation would mediate or moderate the effect of hypnotizability. Finally, the hypothesis that has seemed to most discombobulate the critics of our study, that experiencing an altered state of consciousness would correlate with psi scoring, was not picked out of thin air (or post-hoc findings), but was based on a number of previous studies, including one by Palmer, Khamashta, and Israelson (1979) with meditators (who are likely to perform similarly to high hypnotizables given their ability to focus) in which a scale representing experiencing an altered state of consciousness correlated positively with success in the ganzfeld psi task.

As far as Millar's remark about our not discussing the impact of multiple analyses (he curiously omits to mention the multiple analyses of his sometime-collaborator, e.g., Parker, Grams, & Pettersson, 1998), we were aware of it, which is why we reported only the hypothesis-driven analysis as confirmatory and did one Bonferroni correction to analyze a question that was similarly phrased to another. The usefulness of the Bonferroni and similar procedures is for unplanned, post-hoc comparisons, including those when the overall F is not significant but the researchers want to still carry out contrast analyses (Keppel, 1982; Rosenthal & Rosnow, 1985), which was not the case for our confirmatory analyses. And, of course, there is always the issue of balance between avoiding Type I (i.e., falsely reporting a significant difference) and Type II errors (i.e., falsely not reporting a significant difference). An exclusive regard with the former can lead to strangulating the generation and testing of alternative hypotheses (Fiedler, Kutzner, & Krueger, 2012). It is ironic that in his "corrected" Holm-method analysis, Dr. Millar continued to find support for the finding he most inveighs against, namely experiencing an altered state and psi scoring.

In any case, I believe that a number of statisticians would agree with me that statistical analyses, including possible corrections, should not be applied automatically and mindlessly, but Dr. Millar seems to have fallen prey to the worship of the ("corrected") p value, while ignoring calls for the "new statistics" that also or instead consider other indexes, including effect sizes such as the correlation values in our study (cf. Cumming, 2013). Dr. Millar states that "statisticians' statistics" would demand a correction of p values for more than one "family-wise" analysis per study, but there is not even consensus about what the ideal

correction may be, or even if it is always a mandatory step (cf. Fiedler et al., 2012; Keppel, 1982; Rosenthal & Rosnow, 1985). And as mentioned elsewhere in this Letter, we did conduct a Bonferroni correction when we thought it was justified; the result he objects to remains significant even after different corrections, and we specified which were hypothesis-testing and which were exploratory analyses. Statisticians and psychologists may prioritize different aspects, with the latter being more involved in the theoretical context and previous empirical support relevant to a particular area of research, as well as with the potential generation of valuable hypotheses.

Dr. Millar implies that our result concerning the large correlation ($r = .74, p < .01$), which remains significant after his correction and explains more than half of the variance between z scores and the altered-state scale, is artifactual, perhaps the product of “the subversive effect of reconstructive memory” about what analyses we had planned, or worse. Actually, as mentioned earlier, our hypotheses and corresponding analyses were based on previous research and theory; had we “cherry-picked” our significant analyses, we would have had a perfect set of matching hypotheses and significant results, which clearly was not the case.

Having said that, as we also mentioned in our paper, we had a small N , which demands a larger effect difference to reach significance but makes the latter less reliable than results obtained with a larger N (Button et al., 2013). I do agree with Dr. Millar that preregistering a study, particularly in such a controversial area as parapsychology, is a good idea and may reduce innuendoes and “analytical retrocognitions” and assist us in establishing more reliable relations. In fact, we preregistered our follow-up study (both the KPU and the Open Science Framework registries were started later than our study) once the KPU was created. After all, it is not only the original researchers but also their critics who are vulnerable to the “human frailties” that Dr. Millar mentions. Having said that, I disagree with his conclusion that a failure to replicate many psychology (and parapsychology) experiments necessarily shows that they are spurious (cf. Open Science Collaboration, 2015). Failures to replicate may be due to the impact of methodological, social, and other contextual issues in research (Barrett, 2015). For instance, when replications matched more closely the initial study according to the original investigator in the Open Science Collaboration, the replications were considerably more successful (Gilbert, King, Pettigrew, & Wilson, 2016); for the case of ganzfeld psi research, see Bem, Palmer, and Broughton (2001).

Finally, despite Dr. Millar’s statement that the positive correlation between psi z scores and the altered-state scale “bears characteristic traces of ‘selection’,” in the replication study that we preregistered, which we will soon submit for publication, we found again a significant relation between them.

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To the Editor:

I enjoyed reading Loyd Auerbach's review of William Hall's book, *The World's Most Haunted House: The True Story of the Bridgeport Poltergeist on Lindley Street* in the Fall 2015 *JP*. As one of the investigators involved in that study, I want to record my own thoughts about it and about the book. One of our cultural icons is a good ghost story. This has not gone unnoticed by the media, the entertainment industry, book publishers, and a growing list of "psychic investigators" and "ghostbusters" with varying credentials and intentions. And then there's the scientific field of parapsychology, as represented by the Parapsychological Association (PA), a composite of credentialed scientists with publications in peer-reviewed science journals who attempt to apply scientific methodologies to explain, model, or predict the phenomena at the heart of a good ghost story. These are two different worlds; they are superficially similar but in fact vastly different.

One major difference between these two worlds is how they go about their business—and, indeed, what their business is! Viewing the two approaches side-by-side in the Bridgeport case highlights the contrasts. More importantly, it opens a discussion to which various investigators of possible RSPK cases might contribute.

The book focuses upon a highly publicized RSPK case in Bridgeport, Connecticut in 1974, when William J. Hall was a child of 10 growing up in that city. He has a vague recollection of the case and recalls

that it was quickly “solved” by the local police as being a hoax. I was a research associate at Duke University’s Psychical Research Foundation (PRF) at that time. Boyce Batey, a friend of the PRF who lived near Bridgeport, called us and convinced Bill Roll, Keith Harary, and myself that the case was not a “hoax,” that it was worth investigating, and that the police chief (who’d publically closed the case) would welcome and even help facilitate our investigation, as would the focal family. Keith and I went there as soon as we could to make initial contact with Boyce, the police, and the family. I stayed on for another few weeks, interviewing the numerous people who’d been involved. Thus, the factual skeleton of data upon which William J. Hall’s book hangs was largely constructed by me.

Years later the adult Hall took an interest in “things that go bump in the night” and embedded with the “psychic investigator,” “ghostbuster” community. He created a syndicated news column, *Magic and the Unknown*, and searched for “proof” that ghosts are “real”; but these adventures generally ended in disappointment. When he was reminded of the Bridgeport case by a Facebook post, Hall took a closer look and found there was more than a “hoax” involved. He soon discovered a virtual treasure trove of readily accessible, high quality media accounts published between November 25 and 29, 1974, including detailed eye-witness reports of large scale events from police, a neighboring fireman, and journalists. He decided to reopen the investigation even though it was almost 40 years later. Hall soon discovered Boyce Batey, who had preserved the original investigative data from the case, including a lengthy reel-to-reel audio tape on which I had recorded many of my official interviews. Out of Hall’s investigative efforts came this book.

To set the tone for reading this book, keep in mind that the blockbuster movie, *The Exorcist*, had opened in December, 1973! A year later, in November of 1974, crowds were still lining up at theatres in Bridgeport (and elsewhere) to see it. So it was a no-brainer that on Sunday morning, November 24, when the largest local Bridgeport radio station (WNAB) announced on air that there were strange, unexplained, exorcist-like activities going on at a house at xxx Lindley Street, there would be a literal traffic jam of rubber-neckers lining Lindley Street before long. In addition, numerous police, firemen, neighbors, and friends of the family, as well as radio, TV, and newspaper journalists, descended upon the now public address where the little family of three opened the door to all of them. There were reportedly dozens of people swarming around inside the house for much of that day, even while TVs, large recliner chairs, kitchen shelves, tables, wall-hangings, lamps, mirrors, and other large furniture flipped, flopped, and flew mysteriously about in view of all.

This zoo-like atmosphere continued into the next day until the next night, when two officers caught the 11-year-old girl who lived there perpetrating one rather insignificant false “event.” The case was immediately declared solved and was closed. The Tuesday morning newspaper carried the headline, “FAMILY ‘HAUNTED’ NO LONGER; COPS SAY GIRL TELLS OF HOAX.” But, as I discovered in the following weeks by interviewing many of the numerous eyewitnesses (the tape-recordings that Hall obtained from Boyce Batey), this case was more than a simple hoax.

In retrospect, the book’s title—*The World’s Most Haunted House*—may in fact be well-deserved. The number of eye-witnesses alone sets it apart from other RSPK cases. Although spontaneous observations are typically the most questionable form of data, a number of the events were quite compelling, due to multiple witnesses observing from different perspectives while all potential perpetrators were absent or otherwise controlled. Some events were witnessed by people with no expectancy set, such as friends or neighbors dropping by without having heard the radio reports.

Although the scrutiny of events reported to occur in RSPK cases usually dominates the spotlight, it’s only a part of the story. The other part, the most important part from the family’s perspective, is the extraordinary disruption and emotional turmoil that are the poltergeist’s constant companions. Boyce Batey and I knew this part first-hand because we came to know the Goodin family. Hall had only second-hand knowledge of this disruptive factor. So I applaud him for his insistence on doing the best he could to capture this aspect of the situation in his book.

What Hall has done well is to take a simple chronological approach to the basic data in the first half of the book. This makes the sequence of events easy to follow. Whenever Hall drifts away from this chronology we move into relative chaos. Also, Hall is to be commended for being intent upon getting the

facts of the case straight. I can myself attest to that, both from the book and from my personal interactions with Hall.

The second half of the book is a disaster, in my view. This may reflect a key difference between ghostbusters and scientific parapsychologists. Hall appears to buy into the ghostbusters' fallacy—that they are (he is) doing scientific research. He himself does not see that the worlds of the ghostbuster and scientist are quite separate, and it shows here. Starting with Chapter 13, on The Scientific Investigation, the writing disintegrates into a hodge-podge of random ideas, quotes, and ramblings that are disconnected, irrelevant, and often just a distraction from the central theme. And what is the central theme of this book and investigation? Hall, the ghostbuster, has only *one single ill-defined research question*—"Is it real?" He states his answer early on:

...what happened in November 1974 at that now-iconic little house was "real" beyond any doubt. I found myself struggling to prove it a hoax. I reached a tipping point where the hoax story—what most people would call the most logical of stories, was the hardest—impossible, in fact, to prove at all. (p. 16)

This is where Hall departs (in my humble opinion) from science. This brings me back to my primary goal in writing this Letter. I will outline four specific issues this book highlights for me.

First of all, no single case with uncontrolled observations can be proven as "real"! There are always alternative explanations. (Think of a magic show where you were unable to explain a levitation or disappearance—did that "prove" levitation was "real"?). This is, at best, a single observation, an "*n*" of 1, in a nonexperimental study composed entirely of *post hoc* data.

Second, although this case has the earmarks of a valid RSPK case, that's the beginning of science, not the end. Hall has done a very creditable job of carefully presenting the facts of the case in the book and dealing with the hoax hypothesis. This is a prerequisite for the case to be entered into the database with other similar cases with credible witness reports. It is from such a database of hundreds of cases, spanning centuries in time, that we now recognize common features, such as rapping, object movements and disappearances, the concept of a "focal" person or "agent," the commonality of a tense family environment, and features suggestive of human involvement from which the RSPK rubric has emerged (Maher, 2016; Roll, 1976).

At this stage of the game, RSPK is of necessity "bottom-up" science in which we painstakingly add the details of carefully screened cases to the database in hopes of inching closer to an understanding of these fantastical phenomena. In contrast, many ghostbusters employ a "top-down" approach based upon an unfalsifiable hypothesis—that ghosts did it.

Third, in this scenario science is best served by the systematic collection of as much detail as possible concerning each event, not just the hoax negation (which can never be 100% eliminated, *post hoc*, for any individual event) but the circumstances, such as time of day, duration, number, and identity of all witnesses, location of the suspected agent and other family members, and even such things as temperature, weather, and activity in the neighborhood. Since RSPK cases often involve some faux events, each event needs to be considered independently. It would be ideal if each event could be rated by the investigating team for its degree of causal uncertainty.

Fourth, attempting to bring the RSPK events to a halt and following up afterwards may also contribute to science. The Bridgeport RSPK activity ended, in my opinion, when the family tension was relieved, which supports—but certainly does not "prove"—the RSPK model. At present, we have little data concerning the life trajectories of poltergeist agents after the phenomena have ceased, but I suspect this could be very useful for science to track.

So, for the scientist, eliminating the hoax theory is just and only that, and you are now left with nearly as much of a mystery as you started with. Thus, in science, concluding "it's not a hoax" is not the same as "it's real," *whatever that means!*

When I returned to Durham, PRF director W. G. Roll, Keith Harary, Boyce Batey, and I compiled a

short paper on the Bridgeport case, which we presented at a scientific conference (Solfvin, Harary, & Batey, 1976). That was the only report we ever published of the case, because we did not think there was anything in it which advanced our scientific understanding of this fascinating unexplained phenomena. That is, it was another “good” case—but just another—to add to our accumulating database.

Overall, I am delighted that Hall took the trouble to resurrect the Bridgeport poltergeist case. Interacting with Hall on the telephone prior to publication, and reading the book after publication, has reinvigorated my own interest in these fascinating family phenomena. And looking backward in time, despite the limitations of post hoc analysis, I find there are a few things that I would have done a bit differently had I known then what I know today. Perhaps someday I’ll write more on that topic!

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GLOSSARY

The definitions of most of the following terms have been borrowed or adapted from A Glossary of Terms Used in Parapsychology by Michael A. Thalbourne (republished by Puente Publications, Charlottesville, VA, USA, 2003). We highly recommend this book to those who seek a more complete glossary of parapsychological terms.

AGENT: In a test of GESP, the individual who looks at the information constituting the target and who is said to “send” or “transmit” that information to a percipient; in a test of telepathy and in cases of spontaneous ESP, the individual about whose mental states information is acquired by a percipient. The term is sometimes used to refer to the subject in a test of PK.

ANOMALOUS COGNITION (AC): A form of information transfer in which all known sensory stimuli are absent; that is, some individuals are able to gain access to information by an as yet unknown process; also known as remote viewing (RV) and clairvoyance.

ANOMALOUS PERTURBATION (AP): A form of interaction with matter in which all known physical mechanisms are absent; that is, some individuals are able to influence matter by an as yet unknown process; also known as psychokinesis (PK).

CALL: (As noun), the overt response made by the percipient in guessing the target in a test of ESP; (as verb), to make a response.

CLAIRVOYANCE: Paranormal acquisition of information about an object or contemporary physical event; in contrast to telepathy, the information is assumed to derive directly from an external physical source and not from the mind of another person.

CLOSED DECK: A procedure for generating the target order for each run, not by independent random selection of successive targets, but by randomization of a fixed set of targets (e.g., a deck of 25 ESP cards containing exactly five of each of the standard symbols).

CONFIDENCE CALL: A response the subject feels relatively certain is correct and indicates so before it is compared with its target.

CRITICAL RATIO (CR): A mathematical quantity used to decide whether the size of the observed deviation from chance in a psi test is significantly greater than the expected degree of random fluctuation about the average; it is obtained by dividing the observed deviation by the standard deviation; also called the z statistic.

Critical Ratio of Difference (CR_d): A critical ratio used to decide whether the numbers of hits obtained under two conditions (or by two groups of subjects) differ significantly from each other; it is obtained by dividing the difference between the two total-hits scores by the standard deviation of the difference.

DECLINE EFFECT: The tendency for high scores in a test of psi to decrease, either within a run, within a session, or over a longer period of time; may also be used in reference to the waning and disappearance of psi talent.

DIFFERENTIAL EFFECT: In an experiment where the subjects are tested under two different procedural conditions: (i) the tendency of subjects who score above chance in one condition to score below chance in the other, and vice versa; (ii) the tendency of one condition to elicit psi-hitting from the group of subjects as a whole and the other condition to elicit psi-missing.

DISPLACEMENT: A form of ESP shown by a percipient who consistently obtains information about a target that is one or more removed, spatially or temporally, from the actual target designated for that trial.

Backward Displacement: Displacement in which the target extrasensorially cognized precedes the intended target by one, two, or more steps (designated as -1, -2, etc.).

Forward Displacement: Displacement in which the target actually responded to occurs later than the intended target by one, two, or more steps (designated as +1, +2, etc.).

ESP CARDS: Special cards, introduced by J. B. Rhine, for use in tests of ESP; a standard pack contains 25 cards, each portraying one of five symbols, viz., circle, cross, square, star, and waves.

EXPERIMENTER EFFECT: An experimental outcome that results, not from manipulation of the variable of interest itself, but from some aspect of the experimenter’s behavior, such as unconscious communication to the subjects, or possibly even a psi-mediated effect working in accord with the experimenter’s desire or motivation.

EXTRASENSORY PERCEPTION (ESP): Paranormal cognition; the acquisition of information about an external event, object, or influence (mental or physical; past, present, or future) in some way other than through any of the known sensory channels.

FORCED-CHOICE TEST: Any test of ESP in which the percipient is required to make a response that is limited to a range of possibilities known in advance.

- FREE-RESPONSE TEST:** Any test of ESP in which the range of possible targets is relatively unlimited and is unknown to the percipient, thus permitting a free response to whatever impressions come to mind.
- GANZFELD:** Term for a special type of environment (or the technique for producing it) consisting of homogeneous, unpatterned sensory stimulation; an audiovisual ganzfeld may be accomplished by placing halved ping-pong balls over each eye of the subject, with diffused light (frequently red in hue) projected onto them from an external source, together with the playing of unstructured sounds (such as “pink noise”) into the ears.
- GENERAL EXTRASENSORY PERCEPTION (GESP):** A noncommittal technical term used to refer to instances of ESP in which the information paranormally acquired may have derived either from another person’s mind (i.e., as telepathy), or from a physical event or state of affairs (i.e., as clairvoyance), or even from both sources.
- GOAL-ORIENTED:** Term for the hypothesis that psi accomplishes a subject’s or experimenter’s objective as economically as possible, irrespective of the complexity of the physical system involved.
- MACRO-PK:** Any psychokinetic effect that does not require statistical analysis for its demonstration; sometimes used to refer to PK that has as its target a system larger than quantum mechanical processes, including microorganisms, dice, as well as larger objects.
- MAJORITY-VOTE TECHNIQUE (MV):** The so-called repeated or multipleguessing technique of testing for ESP. The symbol most frequently called by a subject (or a group of subjects) for a given target is used as the “majority-vote” response to that target on the theory that such a response is more likely to be correct than one obtained from a single call.
- MEAN CHANCE EXPECTATION (MCE):** The average (or “mean”) number of hits, or the most likely score to be expected in a test of psi on the null hypothesis that nothing apart from chance is involved in the production of the score.
- MICRO-PK:** Any psychokinetic effect that requires statistical analysis for its demonstration. Sometimes used to refer to PK that has as its target a quantum mechanical system.
- NEAR-DEATH EXPERIENCE (NDE):** A predominantly visual experience undergone by persons who either seem to be at the point of death but then recover, or who narrowly escape death (as in a motor car accident) without being seriously injured. NDEs often incorporate out-of-body experiences.
- OPEN DECK:** A procedure for generating a target order in which each successive target is chosen at random independently of all the others; thus, for example, in the case of a standard deck of ESP cards whose target order is “open deck,” each type of symbol is not necessarily represented an equal number of times.
- OUT-OF-THE-BODY EXPERIENCE (OBE):** An experience, either spontaneous or induced, in which one’s center of consciousness seems to be in a spatial location outside of one’s physical body.
- PARANORMAL:** Term for any phenomenon that in one or more respects exceeds the limits of what is deemed physically possible according to current scientific assumptions.
- PARAPSYCHOLOGY:** The scientific study of certain paranormal or ostensibly paranormal phenomena, in particular, ESP and PK.
- PERCIPIENT:** The individual who experiences or “receives” an extrasensory influence or impression; also, one who is tested for ESP ability.
- POLTERGEIST:** A disturbance characterized by physical effects of ostensibly paranormal origin, suggesting mischievous or destructive intent. These phenomena include such events as the unexplained movement or breakage of objects, loud raps, electrical disturbances, and the lighting of fires.
- POSITION EFFECT (PE):** The tendency of scores in a test of psi to vary systematically according to the location of the trial on the record sheet.
- PRECOGNITION:** A form of ESP involving awareness of some future event that cannot be deduced from normally known data in the present.
- PROCESS-ORIENTED:** Term for research whose main objective is to determine how the occurrence of psi is related to other factors and variables.
- PROOF-ORIENTED:** Term for research whose main objective is to gain evidence for the existence of psi.
- PSI:** A general term used either as a noun or adjective to identify ESP or PK.
- PSI-HITTING:** The use of psi in such a way that the target at which the subject is aiming is “hit” (correctly responded to in a test of ESP, or influenced in a test of PK) more frequently than would be expected if only chance were operating.
- PSI-MISSING:** The use of psi in such a way that the target at which the subject is aiming is “missed” (responded to incorrectly in a test of ESP, or influenced in a direction contrary to aim in a test of PK) more frequently than would be expected if only chance were operating.
- PSYCHOKINESIS (PK):** Paranormal action; the influence of mind on a physical system that cannot be entirely accounted for by the mediation of any known physical energy.

- RANDOM EVENT GENERATOR (REG):** An apparatus (typically electronic) incorporating an element capable of generating a random sequence of outputs; used in automated tests of psi for generating target sequences; in tests of PK, it may itself be the target system that the subject is required to influence; also called a random number generator (RNG).
- RECURRENT SPONTANEOUS PSYCHOKINESIS (RSPK):** Expression for paranormal physical effects that occur repeatedly over a period of time; used especially as a technical term for poltergeist disturbances.
- REMOTE VIEWING:** A term for ESP used especially in the context of an experimental design wherein a percipient attempts to describe the surroundings of a geographically distant agent.
- RESPONSE BIAS:** The tendency to respond or behave in predictable, nonrandom ways.
- RETROACTIVE PK:** PK producing an effect backward in time; to say that event A was caused by retroactive PK is to say that A would not have happened in the way that it did had it not been for a later PK effort exerted so as to influence it; sometimes abbreviated as *retroPK*; also referred to as *backward PK* or *time-displaced PK*.
- RUN:** A fixed group of successive trials in a test of psi.
- SHEEP-GOAT EFFECT (SGE):** The relationship between one's acceptance of the possibility of ESP's occurrence under the given experimental conditions and the level of scoring actually achieved on that ESP test; specifically, the tendency for those who do not reject this possibility ("sheep") to score above chance and those who do reject it ("goats") to score below chance.
- SPONTANEOUS CASE:** Any psychic occurrence that takes place naturally, and is often unanticipated—psi in a real-life situation, as opposed to the experimentally-elicited psi phenomena of the laboratory.
- STACKING EFFECT:** A spuriously high (or low) score in a test of ESP when two or more percipients make guesses in relation to the same sequence of targets; it is due to a fortuitous relationship occurring between the guessing biases of the percipients and the peculiarities of the target sequence.
- TARGET:** In a test of ESP, the object or event that the percipient attempts to identify through information paranormally acquired; in a test of PK, the physical system, or a prescribed outcome thereof, that the subject attempts to influence or bring about.
- TELEPATHY:** The paranormal acquisition of information about the thoughts, feelings, or activity of another conscious being.
- TRIAL:** An experimentally defined smallest unit of measurement in a test of psi: in a test of ESP, it is usually associated with the attempt to gain information paranormally about a single target; in a test of PK, it is usually defined in terms of the single events to be influenced.
- VARIANCE:** A statistic for the degree to which a group of scores are scattered or dispersed around their average; formally, it is the average of the squared deviations from the mean; in parapsychology, the term is often used somewhat idiosyncratically to refer to the variance around the theoretical mean of a group of scores (e.g., MCE) rather than around the actual, obtained mean.
- Run-Score Variance:* The variance around the mean of the scores obtained on individual runs.
- Subject Variance:* The variance around the mean of a subject's total score.

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Manuscripts must be submitted to the *Journal* electronically (either as an e-mail attachment or on disk) in Microsoft Word. The e-mail address is: john@rhine.org. All submissions must be single column. Preparation of the manuscript must generally follow the guidelines presented in the 6th edition of the *Publication Manual of the American Psychological Association*. However, there are exceptions, so authors should also consult the *Journal's* supplementary guidelines, available at <http://www.rhine.org/styleguidelines.html>. It is also helpful to consult recent back issues of the *Journal*.

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