

CORRESPONDENCE

[*Editor's Note:* The following Letter is longer and more substantial than I normally accept for the Correspondence section. I am making an exception in this one case because I think the content will be of particular interest to *JP* readers and it doesn't fit well in other categories.]

To the Editor:

The cover story written by Mr. Jim Popkin in the November 12, 2015 issue of *Newsweek* was about my colleagues and me (Popkin, 2015). This Letter is my response to that article. Jim Popkin is an award-winning investigative journalist and writes for a variety of publications. From 1995 through 2008, Popkin was a reporter and senior producer of the NBC News Investigative Unit, and he was granted an Emmy Award for his *Washington Post* story on Ana Montes, which now has been optioned as a feature film by Identity Films. In association with this work, Popkin worked with and eventually wrote about Scott Carmichael—a Defense Intelligence Agency (DIA) counter intelligence specialist—in *The Washington Post Magazine* entitled, “Ana Montes Did Much Harm Spying for Cuba. Chances Are, You Haven't Heard of Her” (Popkin, 2013).

One of my associates is Angela D. Ford, who worked for over 30 years as an intelligence analyst for a number of agencies, including DIA. Working for DIA, she was one of the remote viewers assigned to the Ft. Meade, Maryland, secret special unit to use remote viewing to collect intelligence during the Cold War. Because she knew Carmichael—as did I much later on while working with him on a special project—she had met Popkin; later Angela suggested I talk with him regarding a potential article he would like to write.

Only because of that reference, I agreed to meet Popkin, which I did on February 21st, 2015, upon my arrival directly from India. Washington was experiencing a massive snow storm, but I struggled to meet Popkin at Tysons Corner in Virginia for a late breakfast. I no longer interact with media people because barring very few exceptions I have been treated terribly, not paid, and made to look the fool by unscrupulous media people and unethical editing. It is beyond the scope of this commentary to document the shabby state of American media with regard to psi, but it suffices to say that I have been collecting names and associations to add to my “Wall of Shame,” which may appear either in print or on the web at some future date.

I did, however, inform Popkin of my previous media treatment in some detail and asked him directly (a) why I should agree to be interviewed and (b) why I should trust him. Over three hours, Popkin provided answers that made sense and began building some trust. He asked why I was still conducting research 20 years after the closing of the US Government's Star Gate program. That, he said, would be the focus of his article to be submitted to *The Washington Post Magazine*. The implication of and response to this question were the scientific advances made in the field, with particular reference to remote viewing. I looked forward to his approach.

We met in person two more times and engaged in countless email exchanges and phone calls as Popkin conducted his research. Like many investigative reporters he is aggressive and does not easily take “no” for an answer. He knew of my Bial bursary to study the degree to which expending liquid nitrogen at a randomly selected, remote site would affect the quality of the remote viewing. He wanted to film a real session and the three viewers (Angela D. Ford, Nevin D. Lantz, and Joseph W. McMoneagle) but I thought this would not be a good idea and denied Popkin access to our on-going study.

One of the in-person meetings occurred in Maryland *after* Angela and I had completed the final series of six sessions in the Bial study. We demonstrated part of the protocol. Angela and I pressed Popkin to allow us access to his article before publication; he did not agree. However, he *did* agree to allow us to fact check what he had written by reading the article to us on the phone. Angela and I were impressed. No other media person had ever been so open.

The last in-person meeting I had with Popkin was at Joe McMoneagle's house in Virginia. Again,

after we had completed six sessions in our on-going study, he agreed to be filmed while he and I demonstrated a binary associative remote viewing (ARV) session—based upon a future coin flip.

The Article

During October 2015, Popkin became more aggressive in his questioning and wanted access to my family and high school class mates—all of whom I denied access to. Besides, they knew nothing of my 45-year career. Popkin informed me that it was likely his article would appear in *Newsweek* by the end of October or in November and not, as previously thought, in *The Washington Post Magazine*.

I had agreed to give the Bill Roll memorial lecture on November 5, 2015 at the University of West Georgia. While boarding my flight on the 4th, Popkin called to ask if I was available for photographs. In Georgia, I received a call from someone at *Newsweek* asking if I would mind if they sent a photographer from Atlanta to photograph my lecture. Sure. No one showed up.

The Good

Popkin (2015) provided an entertaining—sometimes even amusing—portrayal as to why I still am conducting research. In addition, he was able to obtain interviews with important people in the history of the project: Former Senator and former Secretary of Defense William S. Cohen, LTC Brian Buzby (a former commander of the Government's in-house psychic spying program), and the director and president of the Bial Foundation in Porto, Portugal, Dr. Luís Portela. Gaining access to Secretary Cohen and Lieutenant Colonel Buzby speaks to Popkin's research skill. All these people were on record as being in support of psi research in general and in support of me and my research in particular. However, these few good points in Popkin's article are offset by a host of half-stories, outright inaccuracies, apparent sole-sourcing (to use a professional intelligence term) of material (no fact checking), and a number of ad hominem statements. Popkin reneged on his promise to allow us to fact check his writing.

The Bad

One of the people Popkin interviewed by phone was Professor Ray Hyman. I have known Hyman since the late 1970s and privately we consider each other as colleagues, and he is convinced that I am not a fraud and that my strict adherence to the methods of good science are worthy of respect. Hyman's critique in the American Institutes of Research (AIR) evaluation of the Science Applications International Corporation remote viewing investigations is that I was the only judge of that data. In the AIR report, Hyman offers no explanation as to why that is a problem, beyond the implicit suggestion that I may have cheated somehow or my "obvious" bias toward the existence of remote viewing could have biased my analyses. Privately, at least to me, he acknowledges that he does not think I am a fraud and that double-blind protocols were strictly followed. Given that this is true, then there can be no problem with my being the only judge. Perhaps, as one question common in experimental psychology is how some effect is distributed in the population, it is an obvious requirement to use both unselected participants and unselected analysts (a.k.a., judges). But in studying any exceptional human performance, adhering to both these requirements is a mistake. For example, if one wants to study Olympic-level ice skating, he or she would use only the best possible skaters available and use as judges people who know the ins-and-outs of the sport, that is, all the technical details of Olympic competitive skating. Most likely I am the most knowledgeable and experienced remote viewing judge anywhere so far.

While reviewing the AIR report it is important to understand that they examined only 10 remote viewing studies, all of which were conducted at Science Applications International Corporation; none of the work conducted at SRI International from 1972–1989 was included in the analysis. I will now quote from Hyman's contribution to the AIR report:

- ◆ "We [Professor Utts and I] agree that the effect sizes reported in the SAIC experiments are too large and consistent to be dismissed as statistical flukes" (Mumford, Rose & Goslin, 1995, 3-42).

- ♦ “I agree with Jessica Utts that the effect sizes reported in the SAIC experiments and in the recent ganzfeld studies probably cannot be dismissed as due to chance. Nor do they appear to be accounted for by multiple testing, file-drawer distortions, inappropriate statistical testing or other misuse of statistical inference. I do not rule out the possibility that some of this apparent departure from the null hypothesis might simply reflect the failure of the underlying model to be a truly adequate model of the experimental situation. However, I am willing to assume that the effect sizes represent true effects beyond inadequacies in the underlying model. Statistical effects, by themselves, do not justify claiming that anomalous cognition has been demonstrated or, for that matter, that an anomaly of any kind has occurred. So, I accept Professor Utts’ assertion that the statistical results of the SAIC and other parapsychological experiments ‘are far beyond what is expected by chance’” (Mumford et al., 1995, 3-50).
- ♦ “Utts has concluded that ‘arguments that these results could be due to methodological flaws are soundly refuted.’ If she is correct, then I would have to agree with her bottom line that “psychic functioning has been well established” (Mumford et al., 1995, 3-51).
- ♦ “The statistical departures from chance appear to be too large and consistent to attribute to statistical flukes of any sort. Although I cannot dismiss the possibility that these rejections of the null hypothesis might reflect limitations in the statistical model as an approximation of the experimental situation, I tend to agree with Professor Utts that real effects are occurring in these experiments. *Something* other than chance departures from the null hypothesis has occurred in these experiments” (Mumford et al., 1995, 3-73).

A point by point rebuttal of Hyman’s remarks by Utts (Mumford et al. 1995) in the AIR report can be found beginning on page 3-2 and ending on page 3-40.

The unbalanced nature of Popkin’s article suggests that he had not read the AIR report and trusted Hyman as his sole source.

I go into this detail only because Popkin never afforded me the opportunity to respond as I have with some of the points above. In my view, this apparent sole-sourcing of his information reveals a possible editorial bias against the field.

However, the most egregious problem arises from Popkin’s (2015, p. 12) quoting of Hyman as saying “. . . a known ESP proponent” when commenting upon the CIA-funded American Institutes for Research review of Star Gate—the government’s psychic spying program. If Popkin had read the AIR report, he would have learned that the “known ESP proponent” was none other than Professor Jessica Utts—one of the nation’s most respected statisticians, who in January 2016 will be the president of the American Statistical Association. She currently is the chair of the statistics department at the University of California, Irvine. Utts was a major contributor to the AIR report and in doing so mounted a cogent, statistically-based counterargument to the skeptic Hyman (Mumford et al., 1995, p. 3-2).

It is important to note here that despite their disagreements in the AIR report, Hyman and Utts agreed on some key aspects. In discussing their reviews, Mumford et al. (1995) state:

At the outset, it should be noted that the two reviewers agree far more than they disagree. One central point of agreement concerns the existence of a statistically significant effect: Both reviewers note that the evidence accrued to date in the experimental laboratory studies of remote viewing indicate that a statistically significant effect has been obtained. Likewise, they agree that the current (e.g., post-NRC review) experimental procedures contain significant improvements in methodology and experimental control. (p. 3-80)

In the *Newsweek* article, there are a significant number of inaccurate quotes attributed to me; Popkin never enlisted my aid in determining whether his sole-source informants were telling the truth. Just one example. The skeptic, Hyman, told Popkin, “They [Hyman & May] agreed that the early SRI research was ‘crap,’ Hyman says, providing way too many clues to the psychics and fudging the results” (Popkin, 2015, p. 12). Although there were some difficulties in the early work as described now in the

published literature, I never agreed with Hyman as reported by Popkin. In my view, once again Popkin failed by (a) using sole-sources when they fit the apparently *a priori* bias of the piece, and (b) violating his agreement to allow me to fact check his writing before he submitted anywhere.

Popkin was fascinated with our, then, on-going entropy experiment, but because he failed to fact check with me he had many of the details wrong. He even obtained permission from Dr. Portela, to allow me to send him a copy of my final report to the BIAL Foundation. Yet, because Popkin is a journalist and not a scientist he apparently did not understand what he was reading, nor, as I said above, did he fact check the technical material with me. Examples of his mistakes include:

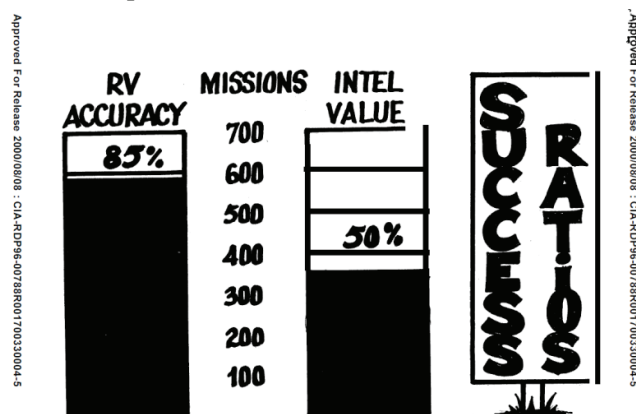
- ♦ “In his final report to Bial, May declared victory finding ‘a significant effect supporting the study hypothesis ($z_{diff} = 1.80, p = .036, ES = 0.425 \pm 0.236$) (Popkin, 2015, p. 11).” Had he understood the final report he would have seen that the above statistic was for the first half of the study only and the correct statistic should have been ($z_{diff} = 1.503, p = .066, ES = 0.251 \pm 0.167$).
- ♦ In referring to the liquid nitrogen pouring Popkin (2015, p. 1) says “. . . she’s released nitrogen gas clouds . . .” This is technically incorrect again. The cloud one sees is water vapor. Gaseous nitrogen is colorless—after all, 78% of what we breathe is nitrogen gas.

A List of Factual Errors in Order of Appearance

- ♦ Title: “Meet the Former Pentagon Scientist Who Says Psychics Can Help American Spies.” (Popkin, 2015, p. 1). I have never been a Pentagon scientist and my salary came from two defense contractors. Some of their resources came from the military but other sources included nonmilitary intelligence organizations and in-house funds supplied by the contractors.
- ♦ In the beginning of the article, Popkin (2015, p. 2) says: “That wasn’t good enough, however. Already embarrassed and under pressure for the disclosure that one of their own, Aldrich Ames, had been spying for the Russians for a decade, the CIA officially shut down the psychic spies program. Star Gate had fizzled out.”

There are a number of problems with this sentence besides the nonsequitur of Aldrich Ames. That case had nothing to do with the CIA closing Star Gate. All Popkin had to do was simply read my commentary on the AIR report (May, 1996). In this report, I was able to document a large number of problems with the CIA review of our program, too numerous to list here. However, I will give a single example. The CIA was obligated by the US Congress to conduct a review of the 20-year program. They only reviewed one year.

- ♦ Popkin (2015, p. 6) quotes me as referring to an “obscure presentation” and proceeds to give statistics from it. I never said anything like that, especially because the presentation was anything but obscure. Rather, it was from a briefing by LTC Brian Buzy, the commander of the Ft. Meade section of the Star Gate program. Popkin does not get his numbers correct. The graph in question, circa 1984, is reproduced below.



- Popkin quotes 760 missions whereas the chart shows 700. This may seem like a minor point but in my view, good investigative reporting usually prides itself on detail and accuracy.
- ◆ Under the heading, “I Believed It Then, and I Believe It Now” Popkin (2015, p. 5) attributes to me: “And May has even less time for all the former Star Gate psychics who peddle mood-ring junk science online, some warning paying customers about flying saucers and the coming apocalypse.” Although colorful, I never said such things. Yes, I am critical of many of the former Star Gate psychics, but for scientific protocol reasons only.
 - ◆ In the same section Popkin (2015, p. 6) writes, “‘It is clear to this author that [ESP] is possible and has been demonstrated,’ she wrote in the agency’s report. This conclusion is not based on belief, but rather on commonly accepted scientific criteria.” Yes, this is an accurate statement, but Popkin does not acknowledge the author, Professor (statistics) Jessica Utts. Acknowledging her clearly would have lent more credence to the positive outcome of Star Gate (Mumford et al., 1995, 3-2 to 3-40).

Ad Hominem Remarks and Other Disrespectful Statements

Working in a controversial arena, I have to have “thick skin.” Ad hominem and irrational attacks are relatively common. However, in a supposedly quality investigative journalistic piece such things detract from the message, lower respect for the journalist and, in this case, *Newsweek*, itself. Here I list in temporal order (in the piece) what I find to be unnecessary remarks, without further comment from me.

- ◆ In the section, “I believed it then, and I believe it now,” Popkin (2015, p. 5) refers to me as “a paranormal prophet.”
- ◆ In the same section Popkin (2015, p. 5) refers to me as, “Thick of waist now with a shiny pate and white beard . . .”
- ◆ Popkin (2015, p. 10) refers to Angela Ford: “Ford was one of only a half-dozen women who worked as psychics for the government’s program. Some of her military colleagues derided her because three ‘spirit guides’ would possess her mind during Star Gate remote-viewing sessions and guide her observations. One was a fat cherub, another a boy-like angel, and the last a 17th-century British professor who spoke through her, Ford says. In an interview, she also says she once saw a UFO outside her suburban home in 2010. ‘It reminded me of something like they call the mother ship,’ she says. ‘It was not moving. It was hovering . . . and then it sort of disappeared.’”

I must comment on this: Angela Ford has been an intelligence analyst for over 30 years. And some of her work has been briefed to the President of the United States. In my view, the paragraph makes her appear at a minimum as weird, and at the maximum a total whack-job. This is clearly unnecessary, especially in that the anomalous cognition data she obtained by whatever method was among the best ever produced in the Star Gate program (Jack Vorona, personal communication, July, 1998).¹

In my view, the belittling of Joe McMoneagle’s demonstration was totally unnecessary. Although it is true that earlier in the piece Popkin emphasized Joe’s Legion of Merit award for his remote viewing intelligence collection skill, he leaves the distinct impression of questioning his ability because of a failed single RV demonstration, and he supplies no context at all. I am not suggesting that Popkin provide a list of excuses for failure—Joe, himself would balk even at that suggestion. Rather, Popkin could have been kinder and far more informative by noting that (a) remote viewing does not happen on demand, and (b) like any other human performance, remote viewing depends significantly on what psychologists call set-and-setting. That is, states of mind like being tired, medical conditions, and feeling hungry, thirsty, or angry about something all matter and contribute in mostly unknown ways to moderate human behavior of any kind.

¹At the time of Angela’s contributions Dr. Vorona was the Deputy Director for Scientific & Technical Intelligence at the Defense Intelligence Agency.

Conclusion

In some respects, having any article, even one riddled with errors but suggesting positive results in psi research, appear in a national news magazine can be considered a positive outcome. What I find immensely disappointing is to know how much better and significant this article could have been if Popkin had done even a modicum of fact checking as promised but reneged.

Why am I still conducting research? The answer is rather straightforward. Psi research has made substantial progress since the closing of the Star Gate program. In addition to the models put forth by some of our colleagues, the Laboratories for Fundamental Research (LFR) has recently published the multiphasic model of precognition (Marwaha & May, 2015). While Popkin was in the know about the published model, including the recent publications from LFR (May & Marwaha, 2014, 2015a, 2015b; May, Rubel, McMoneagle, & Auerbach, 2014), he apparently failed to look at these reports on the scientific output from the Star Gate program and the theoretical advances made based on that. These would have answered the question that he first came to me with: why I was still conducting research 20 years after the closing of the US Government's Star Gate program.

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To the Editor:

Our organization recently read *Parapsychology: A Handbook for the 21st Century*. In Chapter 25, Dr. Michaelen Maher (2015) provides a comprehensive literature review entitled "Ghosts and Poltergeists: An Eternal Enigma." On examination, we noticed that our research (Laythe & Owen, 2013) was referenced and discussed. Although we were pleased to be included in her review of research related to ghosts and

paranormal phenomena, we regret to say that Dr. Maher's commentary describing our research contained several inaccuracies.

To begin, Dr. Maher stated that "it was not clear from their report how electromagnetism from their multiple data recorders was effectively shielded, or if the house could have been penetrated by magnetic fields from high tension wires nearby, or from a generator they installed outside to power their equipment" (Maher, 2015, p. 334). These are relevant confounds regarding the aberrant nature of the EMF/GMF data we collected. Nevertheless, we addressed these issues extensively in our paper.

With regards to penetration from "high tension wires nearby" (Maher, 2015, p. 334), we clearly stated that "there were no wires leading from the electricity pole to the building or fuse box, nor any wire leading from the pole to the main electricity poles along the road" (Laythe & Owen, 2013, p. 217). On the same page we stated that "the house is situated approximately one quarter mile from a traveled road" (Laythe & Owen, p. 217). Thus, active electrical wiring that could produce EMF was approximately one quarter mile from the electricity-free location we were studying. We also stated that "field tests of the meters demonstrate that both types have an approximate range of 8 feet in diameter in terms of detecting a 100 mg field" (Laythe & Owen, 2013, p. 218), and "EMF . . . practically decreases at a factor of about 10 to 1 foot of distance between the source of the field and the meter" (Laythe & Owen, 2013, p. 215).

We emphasized these points to the reader because we ourselves found this EMF/GMF data so vexing in our previous work. Correlations between five sets of immobile meters radically changed on an hour-to-hour basis. These coefficients ranged from $-.70$ to $.64$, and in some instances increased or decreased by a correlation coefficient of $.4$ or greater (Laythe & Owen, 2013). As explained in the discussion section, changes between meters in different locations of the house would represent either: (a) change in the location of the meters, (b) a change in reflective surfaces in the house, or (c) a change of the origin point of EMF fields (Laythe & Owen, 2013). As the meters did not move (see Protocol section, Laythe & Owen, 2013), and the house did not suffer an earthquake, the only likely explanation is that the source of the EMF/GMF fields themselves changed.

To further support the above point, we tested our meters with active power lines. The typical active power line, conducting many *kilowatts* of electricity through its cables, only registered a 1 milligauss field on the meters that were used in our study when set approximately 15 ft away from the cables. An 800 watt generator "placed 25 ft. from the investigation site" (Laythe & Owen, 2013, p. 218) would not create a sufficient field to affect our meters. Thus, our conclusion was that EM fields detected in the home varied from room to room, and appeared to generate and disappear within the house while lacking an obvious means to generate them.

We are unclear as to Dr. Maher's meaning with regards to her statement that "it was not clear from their report how electromagnetism from their multiple data recorders was effectively shielded" (Maher, 2015, p. 334). Our lines to our meters were shielded, but we are confused as to why the meters themselves would need to be shielded. The meters themselves could not be shielded, or they would not be able to detect EM fields. If Dr. Maher is alluding to some type of artifact in our data due to the data-logging system we employed, we had already addressed this in our work. Any sort of interference in EMF from our data-logging system itself still would not explain the crucial point of the paper. We obtained highly significant relationships with time synced spikes of EMF/GMF and events captured on audio/video (Laythe & Owen, p. 232).

Whereas the above issues focus on technical concerns clearly addressed in our manuscript, we are more concerned with the following statement drawn directly from the above referenced book chapter: "Moreover, a sizable crowd of ghost enthusiasts had apparently gathered in front of Black Moon Manor to observe the investigation—a situation hardly conducive to controlling potential artifacts when conducting a formal study" (Maher, 2015, p. 334). We can only infer that Dr. Maher is claiming that we either allowed crowds to gather during our investigation or that our members themselves have been denigrated to "paranormal enthusiasts." This inference is a great disservice to our organization's character and experience with field research design.

Perhaps this misstatement was the consequence of some confusion in interpretation, as we had

stated in our manuscript that two organizations (ITPO and ASAFP) were present (Laythe & Owen, 2013, p. 217). Hence we wish to clarify that the same 10 participants reported in our methods were members of both organizations (one public, one an approved college organization). Nevertheless, we clearly stated that “all members were briefed in protocols for the current study, and have had previous training on data collections in previous investigations” (Laythe & Owen, 2013, p. 217) and “Only investigators were present on the site during data collection” (Laythe & Owen, 2013, p. 219). To be perfectly clear, we did not allow any individuals that were untrained on the site, nor is it ever a practice for us to do so. If that were the case and we did allow onlookers, we would certainly have noted this potential confound in the body of the discussion section of our manuscript.

I am sure that Dr. Maher appreciates the importance of maintaining a strict protocol in such field research, including tracking of participants on video monitoring systems during an investigation. Specifically, these protocols included maintaining silence when not actively investigating, and keeping a large distance away from the investigation site. We also train members to verbally announce themselves when entering/exiting, and require members to announce any noise made by themselves to facilitate the minimization of such confounds.

Even if these protocols were not in place, our “scheme for classifying phenomena,” as per Dr. Maher (Maher, 2015, p. 334) further controlled for audio artifacts. We clearly described in our work that audio phenomena on a particular audio recorder was not considered *likely to be anomalous* unless comparison across four other audio recorders on location demonstrated that there was *no noise present whatsoever* at the time of the proposed EVP (Laythe & Owen, 2013, p. 220). As such, any noise present from any recorder during a potential EVP immediately disqualified the sound as contaminated. Although this method is not foolproof, it does greatly reduce the probability that the EVP collected in the current study was explainable by vocal or environmental contamination. We also engaged in vigorous recreation for several hours (which failed) with the video phenomena that we captured. Thus, we justifiably stated with regard to some of the apparition phenomena that we “were unable to re-create these shadows from any aspect of the room or house” (Laythe & Owen, 2013, p. 225).

The possibility that Dr. Maher deduced that our members were “a crowd of paranormal enthusiasts” (Maher, 2015, p. 334) is understandable given the host of paranormal television shows with ghost hunters, and (sadly) the generally unscientific reputation of the ghost hunting community. Our organization does include a variety of individuals from various walks of life and educational backgrounds. However, the goal of our organization has always been the empirical assessment of haunting phenomena and the publishing of such information in scientific journals. As such, all of our members are trained in recognizing threats to validity, basic research methods, and techniques for determining alternative nonparanormal explanations as well as psychological explanations (cognitive, neurological, and social) for purported anomalous phenomena.

In closing, we write this letter not to incite controversy, nor to disparage Dr. Maher, whom we admire, but rather to make it clear that our findings in Laythe and Owen (2013) were obtained using strict methodological protocols and analytic techniques. We employed the best laboratory conditions and analyses available. We are open to scientific criticism and debate but desire that said criticism involve an earnest analysis of our methods or attempts at replication. It is our hope that other parapsychologists might adopt our protocols, or a variant of them. We would happily welcome company in scientifically investigating these locations, regardless of our findings. However, we understandably take issue with criticisms that we obviously controlled for in our research design.

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To the Editor:

The interesting study by Laythe and Owen (2013) was included in my review (Maher, 2015) because it was a novel approach to examining magnetic fields at ostensibly haunted locations. Generally, the GMF/EMF approach in parapsychology has been focused on the hallucinatory potential of magnetic fields, whereas the authors were attempting to determine if such fields could be products of the “ghosts” themselves. Diverse approaches are needed to sort out the possible effects (and/or causes) of experience-inducing magnetic fields. However, in a critical review, it is incumbent upon the author to discuss weaknesses or potential confounds in the design or execution of the study. Let me address their objections to my remarks.

With regard to magnetic fields penetrating the house from “high tension wires nearby” (Maher, 2015, p. 334), the reference was to high-voltage transmission lines that transport the electricity generated at power plants to substations with step-down transformers, which in turn send smaller loads to densely populated areas. Such high-voltage lines (commonly called high-tension wires) are not found along roads but rather crisscross the countryside, typically on steel lattice towers, creating powerful transverse waves of EMF. These emissions have been implicated in various illnesses and have been a significant factor in reducing the value of nearby properties, leading in some cases to abandoned homes. The presence or absence of high-voltage transmission lines in the vicinity was not addressed in the report, and readers know only what they are told.

Electromagnetism is a complex force and its effects are not perfectly understood. Because scientists have learned to harness a force does not mean that all of its implications have been grasped. There are many possible confounds in an experimental setup such as the authors have described. (A floor plan of the premises, showing the placement and orientation of their equipment, and the configuration of the wiring, would have been helpful.) I cannot say to what extent their data collection procedures might have affected their results, but as they reported that “no readings or spikes were detected” (Laythe & Owen, p. 218) prior to turning on their equipment, the suggestion that EMFs from the equipment may have impacted the data they were collecting is certainly plausible. Video cameras, tape recorders, magnetometers, computers, electrical wiring, generators, and even people moving rapidly through space (as in the “interactive” sessions) can all create detectable EMF. Their finding that EMF readings inside the house were 50% to 100% greater compared to readings outside the house (p. 222) could also imply that their energized equipment was influencing the EMF data they were recording.

There are a variety of ways that electrical artifacts could have affected the authors’ EMF results. Inadequately shielded wiring, a generator producing uneven power (above or below 120v), harmonic distortion in the circuitry, the proximity effect (a redistribution of current in a conductor caused by another conductor), battery versus AC-powered equipment, and the use of different magnetometers for measurement are all potential confounds that might have influenced the EMF readings. Alternatively, the low EMF they recorded may indicate that there was ambient EMF already present in the environment, but it was too weak to be detected during their initial surveys. After the magnetometers were amplified by coils (p. 218), the EMF became apparent. Potential sources for such EMF would include not only passing cars and planes, as the authors suggested (p. 231–232), but also the high-voltage transmission lines mentioned earlier. Highly sensitive magnetometers might have registered additional EMF from the generator, or from distant lightning strikes. The warren of copper pipes (conductors) routinely found in old houses could be another contaminating factor. With so many potential confounds, and with so much energized equipment on the premises, it would be very difficult (if not impossible) to eliminate all of the unwanted sources of EMF and measure only what was left with any accuracy or consistency.

These speculations would not explain why a pattern of spikes occurred in conjunction with hard-to-interpret audio/visual effects unless both resulted from common (shared) artifacts. If the power quality from the generator was uneven, harmonic distortion (acting like static on a telephone line) could

have produced spikes or glitches in both cameras and computers. It would be instructive to learn what pattern of results would be found using the same equipment in a comparable house with no electricity and no history of ghosts. (EMPE evaluators, in such a study, would have to be kept “blind” as to the nonhaunted status of the house.)

It was difficult to make out from the report how many onlookers were present or what their purpose was. It was never made clear how many designated “investigators” as opposed to “noninvestigating members” (p. 219) there were, or that the 10 “participants” noted in the Method section (p. 217) included the “noninvestigating” members. (Perhaps the noninvestigating members began investigating at some juncture.) As no video or audio equipment was set up outside the building, I assume that no documentation of the activities of the onlookers was attempted. In my experience, it is never a good idea to have multiple persons—trained or untrained—milling about the site of an investigation. The more people there are, the harder it is to keep track of what each is doing.

The term “ghost enthusiasts” (Maher, p. 334) was meant to be descriptive rather than pejorative. Enthusiasm is a beautiful thing—implying interest, pleasure, and curiosity. Should I have described the noninvestigating participants as “members of more than one lay organization interested in the study of ghosts”? Although I never used the term “paranormal enthusiasts,” I would hope that all of the persons mentioned in the authors’ study are enthusiastic about the discipline of parapsychology, which attempts to extend knowledge of the world we inhabit beyond what is presently known. This pursuit poses challenges that are not always easy to surmount.

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To the Editor:

In his recent two papers on hypnosis and psi in this journal, Dr. Adrian Parker (2015a, 2015b) substantially misrepresents my work and that of my doctoral student, so I would like to set the record straight and point out some other inaccuracies:

- ◆ Dr. Parker (2015a) states on page 25 that “. . . the critics of parapsychology have been described as “wolverines” for their aggressiveness” (Cardeña, 2011). Not true at all. In that paper, which the reader can consult, the only being who is called a wolverine is, well, a wolverine in a zoo. I used it to exemplify that how a creature can be fierce and tender at different moments can serve as an analogy to the complexity found in the study of psi, not to call anybody names.
- ◆ On page 41, when commenting on Marcusson-Clavertz and Cardeña (2011), Dr. Parker (2015b) states that we only had “a handful of participants.” Although we ourselves mentioned as the “foremost” limitation of the study the small *N* and consequent low power (Marcusson-Clavertz & Cardeña, 2011, p. 252), we did not have “a handful” but 26 individuals, the result of our selection procedure that required more than 2 years to administer a hypnotizability test to 332 individuals and then recruit willing participants who fulfilled our selection criteria. Our tested

sample, furthermore, was only four individuals lower than the *Ns* that Dr. Parker used in his single ganzfeld studies (cf. Parker, 2000, p. 5).

Also on page 41, Dr. Parker (2015b) opines that we had “a study overloaded with variables and hypotheses.” He does not provide his criterion for what constitutes an “overloaded” study, but here are the facts: Besides the screening instruments, the only predictor variables we analyzed were the Phenomenology of Consciousness Inventory, a questionnaire asking about demographic variables, and three questions, two on whether the person thought that the experiment in general or him/her in particular would succeed, and one on whether s/he had had a prior psi experience. In other published studies, one of them accepted into the very rigorous *Journal of Experimental Psychology*, we have used more variables without the editors or reviewers objecting to it.

As far as “overloading” of hypotheses, we had a big total of five (Marcusson-Clavertz & Cardeña, 2011, p. 238), in which we predicted that the following would correlate with the dependent variable (psi *z* scores): (a) believing that one would be successful in the experiment; (b) previous psi experiences; (c) high hypnotizability; (d) alterations in consciousness, which would also correlate with high hypnotizability; and (e) that dissociation would mediate or moderate the effect of hypnotizability on psi scoring. Contrary to Dr. Parker’s (2015b) statement that “the only indisputable finding” was a sheep-goat effect, hypotheses a, b, and d (the latter one only for the subgroup of high hypnotizables, but we had prespecified that high hypnotizables would exhibit greater consciousness alterations) were supported.

Also on page 41 Dr. Parker (2015b) states that “the specific measure used to test [the first] hypothesis may [*sic*] not have been prespecified.” This is not true, as we prespecified our hypotheses and ways to test them. Furthermore, in the Results section (Marcusson-Clavertz & Cardeña, 2011, p. 244), we stated that when we used two similarly worded questions we had corrected alpha by dividing by 2. Also, Dr. Parker makes the groundless accusation on that same page that “The study appears to present many post hoc findings although they were not identified as post hoc.” This is false, as we described our hypotheses in the paper and the reader can then precisely determine which are the post-hoc findings. Incidentally, to avoid this kind of innuendo and as a matter of good research practice, we preregistered the analyses to be performed in our follow-up study in the KPU registry, something that we encourage all researchers to do.

Finally, also on page 41 Dr. Parker (2015b) states that “it is likely that some of the tests chosen by the authors violated the rule for the use of parametric statistics,” even though we specifically mentioned in the Analysis section that we had used the Mann-Whitney *U* test and rank correlations when parametric assumptions were violated (Marcusson-Clavertz & Cardeña, 2011, p. 244). The scores on the DES, which are not normally distributed (see below) were used only to classify participants, not to conduct other analyses. Incidentally, all of our parametric results are still significant even when using nonparametric tests. Specifically, of the three results that supported our hypotheses one had been already subjected to a nonparametric test in the original report (p. 245), the other two remain significantly associated with psi *z* scores when a Spearman rank correlation is applied: psi *z* scores correlated with belief in individual success, $r_s = .49$, $p = .012$, and altered state in the ganzfeld (for high hypnotizables), $r = .63$, $p = .015$. Overall, I believe that Dr. Parker’s descriptions are inaccurate and slanted (see also Bauer, Hövelmann, & Lucadou, 2013, in press).

Dr. Parker also makes inaccurate statements when referring to other authors’ work:

- ◆ The Dissociative Experiences Scale is wrongly labeled as the Dissociation Experiences Scale (Parker, 2015a, p. 26) and as the Dissociated Experience Scale (Parker, 2015b, p. 38). More importantly, its distribution is described as “being extremely skewed or even bimodal” (Parker, 2015a, p. 26). Although we are aware of the skewness of the DES, we are not aware of any study showing DES bimodality nor does Dr. Parker cite any. The DES was analyzed taxometrically by Waller, Putnam, and Carlson (1996), who commented on the bimodality of hypnotizability measures, not of the DES.
- ◆ On page 28 Dr. Parker (2015a) claims that Kallio and Revonsuo (2003) “coined the term ‘virtuoso’ [hypnotizable],” when in fact Ernest R. Hilgard was using it already in 1977 (p. 57), if not earlier.

- ◆ When discussing T. X. Barber's typology of high hypnotizables, Dr. Parker (2015a) mentions that "prone to expectancy" (p. 28) is one of these types, which is not quite right because Barber (1999, p. 27) called it the "positively-set person," because these individuals do not only have high expectancies but also high positive attitudes and motivations. Dr. Parker (2015b) also states that "support for this typology was found in a study of highly hypnotizable people" and then refers to a 2010 publication by Deirdre Barrett (a "she" not a "he" as Dr. Parker writes on p. 42). Actually, the original study by Barrett was first published in 1990 and it partly inspired Barber's theory. We found some corroboration for this theory using more sophisticated analyses than had been used previously (e.g., Terhune & Cardeña, 2010).
- ◆ Finally, and in this Dr. Parker (2015a) follows many other hypnosis authors, Braid did not coin the term "hypnotism" and related cognates: Etienne Félix d'Hénin de Cuvillers did (Gravitz & Gerton, 1984).

It also bears mentioning that although Dr. Parker lists various proposed components of hypnosis, unfortunately he does not discuss integrative, multifactorial models such as those of Ronald Shor, not "Schor" as Dr. Parker (2015a) writes (p. 28), and Daniel Brown and Erika Fromm (1986), who considered in their theoretical models individual-cognitive, social, and cultural variables.

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