

COMMENTARY

**Reassessing the “Impossible”:  
A Critical Commentary on Reber and Alcock’s  
“Why Parapsychological Claims Cannot Be True”**

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**Abstract**—A critical commentary is offered on a skeptical rebuttal made by Arthur Reber and James Alcock in the July/August 2019 issue of *Skeptical Inquirer*, which came in response to an article by Etzel Cardeña (published in the mainstream journal *American Psychologist* in 2018) that reviewed the extensive evidence from parapsychological experiments which collectively seems to offer support for the existence of psychic (or psi) phenomena. At the heart of their rebuttal, Reber and Alcock seek to make the counterargument that this evidence cannot be meaningful because psi phenomena are “impossible,” appearing to violate four fundamental principles of physics. It is shown here that rather than being based on any kind of substantial evidence, the criticisms that Reber and Alcock put forth in support of this counterargument are instead based on a combination of narrow personal opinion, unfounded assumption, and superficial rhetoric, leaving their claims unsound and ultimately unconvincing.

**Keywords:** parapsychology—psychic phenomena—psi—skepticism

In the July/August 2019 issue of *Skeptical Inquirer* (the magazine of the Committee for Skeptical Inquiry, an organization previously known as the Committee for the Scientific Investigation of Claims of the Paranormal), a special report appeared in which Arthur S. Reber and James E. Alcock attempted to offer a critical rebuttal to a 2018 review article published in *American Psychologist* (the flagship journal of the American Psychological Association—APA) which examined the various claims of ostensible psychic (or psi) phenomena that have long been the subject of empirical study within the field of parapsychology. In that particular article, Etzel Cardeña (2018) had summarized the extensive amount of data that

parapsychologists have managed to gather over the course of roughly eight decades in experiments designed to test for extrasensory perception (ESP) and psychokinesis (PK, or ostensible mind–matter interaction). His review ultimately led to the conclusion that when these data are collectively evaluated by meta-analysis, they seem to offer considerable support for the existence of psi, resulting in statistical outcomes that are highly significant.

Reber and Alcock (2019) take issue with Cardeña's conclusion, arguing that claims of psychic phenomena should not be given any serious consideration, regardless of what these experimental data would seem to indicate. The reason, they maintain, is that such phenomena are "impossible," and thus any claims made about them simply "cannot be true" (p. 8; see also Alcock, 2010a). This naturally raises the question: Is the rebuttal offered by Reber and Alcock (2019) a sound one? The objective of this paper is to show that when one examines it carefully, it is very likely not to be. To maintain sequential consistency, the critical arguments that Reber and Alcock raise in their rebuttal will be summarized and addressed here in the order that they are presented in their *Skeptical Inquirer* article.

### **Previous Coverage of Parapsychology in APA Journals**

Reber and Alcock (2019) open their rebuttal by initially pointing out that Cardeña's review was not "the first time the APA had entered this controversial domain of psychology" (p. 8). In 2011, the editors of the *Journal of Personality and Social Psychology* (another of the APA's journals) had decided to publish a study by Daryl Bem (2011a) that seemed to offer experimental evidence favoring the possible existence of precognition (psychic perception of future events). Apart from the high-profile controversy that it drew in both academic and public media circles, Reber and Alcock (2019) noted that Bem's study ". . . was immediately subjected to efforts at replication by other labs (which almost uniformly failed) and well-honed criticisms" (p. 8), which included a critique by Alcock (2011) that also appeared in *Skeptical Inquirer*.

While it is indeed true that various attempts were made by a number of laboratories to independently replicate the findings of Bem's (2011a) nine original experiments, is it also true that these replication attempts "almost uniformly failed?" By 2016, a total of 33 laboratories had conducted 90 experiments related to Bem's work; 69 of these experiments were replications conducted by independent researchers. As is typical with a series of psychological studies on a given topic, their outcomes did vary across the individual experiments, with some successfully replicating Bem's findings, and others not doing so. But one particularly important thing to recognize, which Reber and Alcock (2019) fail to do, is that when the data

from these 69 independent replication attempts are statistically evaluated by meta-analysis, they collectively result in a highly significant outcome, with a Stouffer's  $Z$  of 4.16 and a  $p$ -value of  $1.2 \times 10^{-5}$  (Bem, Tressoldi, Rabeyron, & Duggan, 2016). This would seem to indicate that Bem's results were in fact successfully replicated on the whole, contrary to what Reber and Alcock (2019) claim. Careful consideration should also be taken of Bem's (2011b) outlining of the various ways in which Alcock (2011) seemed to misconstrue some of the procedural details of the nine original experiments, in judging whether Alcock's criticisms of those experiments were particularly "well-honed."

Apart from Cardeña (2018) and Bem (2011a), Reber and Alcock (2019) fail to mention other notable occasions of fairly recent memory in which the APA addressed the topic of psi in the pages of its journals: In the mid-1980s, another review article was published in *American Psychologist* by Irvin Child (1985), which reexamined the statistical results from a series of dream ESP experiments that were conducted at the Maimonides Medical Center in Brooklyn, New York, during the late 1960s and early 1970s (Ullman, Krippner, with Vaughan, 1973). From his reexamination, Child (1985) found that:

Several segments of the data, considered separately, yield significant evidence that dreams (and associations to them) tended to resemble the picture chosen randomly as target more than they resembled other pictures in the pool (p. 1223),

with those rated for correspondence by independent judges having  $p$ -values of less than 0.005. Child's review article drew a small round of additional comments and critiques (Child, 1986; Clemmer, 1986; Hill, 1986; Vitulli, 1986), only one of which was clearly oriented toward skepticism (Clemmer, 1986).

Bem had also co-authored an earlier psi-related article with the late Charles Honorton that appeared in the APA journal *Psychological Bulletin* in the mid-1990s. This article (Bem & Honorton, 1994) reported a meta-analysis of 11 experiments on telepathy using the ganzfeld technique that were conducted at the Psychophysical Research Laboratories in New Jersey throughout the 1980s. When collectively analyzed, these 11 experiments produced a significant overall result ( $z = 2.89, p = 0.002$ ). This meta-analysis did stir further debate (Bem, 1994; Hyman, 1994) and encouraged additional analyses and critiques of the ganzfeld telepathy database (Hyman, 2010; Milton & Wiseman, 1999, 2001; Storm & Ertel, 2001; Storm, Tressoldi, & Di Risio, 2010a, 2010b) in later issues of the journal, as well.

The ganzfeld telepathy database also ties into the two most recent

meta-analytical evaluations of ESP experiments to appear in *Psychological Bulletin* (Rouder, Morey, & Province, 2013; Storm, Tressoldi, & Utts, 2013). While the results of these two recent meta-analyses were each interpreted in different ways by the two teams of collaborating researchers involved, perhaps the most important thing to note about them is that each of these teams had ultimately found a significant positive result in their respective analysis of the ESP database, with Bayes factors ranging from 330 to 1 (Rouder et al., 2013) to more than 14,000 to 1 (Storm et al., 2013). At the very least, these latest results would seem to indicate that something of interest is occurring in these ESP experiments.

Why do Reber and Alcock not mention these other occasions? While it could have perhaps been a simple oversight, one might also wonder whether it was because most of the results were not in line with their preferred viewpoint that there is nothing to psi.

### Effect Size in Psi Experiments

Reber and Alcock (2019) go on to minimally acknowledge Cardeña's (2018) review as being "an impressive effort," but then it seems that they try to subtly downplay the findings it presents by stating that the review was focused "... mainly on meta-analyses of published papers that showed small or marginal effects" (p. 8). This statement is notable because it is reminiscent of a common skeptical tactic in which descriptive terms like *small* and *marginal* are often used in a manner that is meant to subtly imply that the mean effect sizes observed in psi experiments are, in essence, "too minuscule to be worthy of notice." Although they do tend to be relatively small, are psi effects really so incredibly minute that they do not deserve any serious attention?

When one compares them to the effect sizes found in experiments conducted in more conventional areas of psychology, it would appear that they actually are not. For instance, a large-scale meta-analysis of one hundred years of experiments in social psychology by Richard, Bond, and Stokes-Zoota (2003) had yielded an overall mean effect size of 0.21. Similarly, a large-scale attempt by the Open Science Collaboration (2015) to directly replicate one hundred original experiments reported in three psychology journals had resulted in a mean effect size of 0.197 for the data from these experimental replications. A slightly higher (but still relatively comparable) mean effect size of 0.249 was found in a second, large-scale attempt by Camerer et al. (2018) to directly replicate 21 original social psychology experiments that were published in *Nature* and *Science*. When these three values are combined (simply for purposes of relative comparison), they would seem to indicate that the mean overall effect size across these

**TABLE 1**  
**Mean Effect Sizes Observed in Various ESP Experiments**

Experimental Area	Source	Effect Size
Ganzfeld Telepathy	Storm, Tressoldi, & Di Risio (2010a)	0.14
SRI Remote Viewing	Utts (1996)	0.21
SAIC Remote Viewing	Utts (1996)	0.23
PEAR Remote Viewing	Dunne & Jahn (2003)	0.21
General Free-Response ESP	Milton (1997)	0.16
Forced-Choice Precognition	Honorton & Ferrari (1989)	0.02
Implicit Precognition—D. Bem	Bem et al. (2016)	0.09
Presentiment (1978–2010)	Mossbridge, Tressoldi, & Utts (2012)	0.21
Presentiment (2008–2018 Update)	Duggan & Tressoldi (2018)	0.28
Dream ESP (1966–2016)	Storm et al. (2017)	0.18

conventional psychological experiments is around **0.22**. When this value is compared with the mean effect sizes obtained in various experiments designed to test for ESP (Table 1), it can be seen that several of the ESP effect sizes either tend to be quite near, or come fairly close to approaching, that value.

This would seem to suggest that: 1) effect sizes in conventional psychological research tend to be relatively small, as well; and 2) the effect sizes in parapsychological experiments tend to be nearly on par with them. Thus, psi effects should not be discounted or ignored on the basis of their relatively small size.

### **Are Psi Phenomena Incompatible with the Established Laws and Principles of Science?**

Reber and Alcock (2019) criticize Cardeña's (2018) efforts to explore some of the ways in which one might begin to conceptualize psi phenomena in terms of what is currently known in physics, and in doing so they reveal the crux of their rebuttal:

[P]arapsychology's claims cannot be true. The entire field is bankrupt—and has been from the beginning. Each and every claim made by psi researchers violates fundamental principles of science, and, hence, can have no ontological status. (p. 8)

With having made such a bold claim, one might figure that Reber and Alcock would be able to cite ample amounts of empirical evidence from parapsychological studies to support it. But further examination reveals that their argument is not based on any such evidence; instead, it is based purely upon their own personal opinions and unfounded assumptions about psi. *And that is the ultimate flaw in their rebuttal.*

In formulating their argument, Reber and Alcock (2019) openly admit: “We did not examine the data for psi. . . . Our reason was simple: the data are irrelevant” (p. 8).<sup>1</sup> They then try to justify this by further adding that: “Examining the data may be useful if the goal is to challenge the veracity of the findings but has no role in the kinds of criticism we were mounting” (p. 8). But then one may reasonably ask: If Reber and Alcock did not examine any part of the vast parapsychological database that is currently available, then how might they know whether the conceptualizations they have formed about psi are correct? Arguably, without actually testing their concepts against the existing data, they *would not* have any way of knowing this. (And as will be discussed below, it is even likely that they are not correct.)

Instead of evidence, Reber and Alcock (2019) simply choose to base their argument upon a piece of superficial rhetoric (“pigs cannot fly,” p. 8) and make the sweeping generalization that “[any] data that show they can are the result of flawed methodology, weak controls, inappropriate data analysis, or fraud” (p. 8). Yet, they cite no general findings or outline any empirically identified deficiencies that would serve to substantiate this claim on a wide scale across the field of parapsychology. Moreover, a careful examination of the studies contained in the parapsychological database would reveal that parapsychological experiments are specifically intended (by the way in which they are methodologically designed) to address and exclude ordinary factors such as those listed by Reber and Alcock. But since they admit that they did not actually examine the database, one can only figure that Reber and Alcock are simply making a totally unsubstantiated assumption that such factors are indeed present and confounding the data.

Reber and Alcock (2019) then proceed to identify “four fundamental principles of science that psi effects, were they true, would violate: causality, time’s arrow, thermodynamics, and the inverse square law” (p. 8). One thing that is particularly important to realize about the arguments that Reber and Alcock make with regard to each of these principles is that they would seem to be reasonable *only if the assumptions they make about psi are valid* (in the sense that they have been formulated based on what has been learned about psi functioning from empirical results and observations). However, there is reason to think in each instance that their assumptions are not valid.

## **Causality**

Reber and Alcock (2019) firstly argue:

Within the study of psi, there are no causal mechanisms, and none have been hypothesized. Worse, there is virtually no discussion over whether the claimed effects have singular or multiple causal mechanisms or why the purported findings lack coherency. (p. 9)

But without having consulted the parapsychological database, how can Reber and Alcock know whether this claim has any merit? Had they actually consulted the database, they would have learned that it is without merit: Consideration of these issues would largely fall into the area of theoretical development within parapsychology, and even a casual survey of the parapsychological literature would reveal that there has most certainly been an ongoing effort to consider them and subsequently develop theories with testable hypotheses (see, e.g., Evrard & Ventola, 2018, 2019; May & Marwaha, 2015; Stokes, 1987).

And as Cardeña (2018) and Tressoldi (2012) both point out, one possible reason why the findings seem to lack coherency is because some of the experiments may not have initially had sufficient statistical power to be able to adequately detect the fairly small effects associated with psi (considering the relatively low magnitude of the effect sizes listed in Table 1).

Being mindful of the low magnitude of psi effects is especially pertinent in the case of psychokinesis (Bösch, Steinkamp, & Boller, 2006; Radin & Ferrari, 1991), where the effects are typically found to reflect only small fractions of a meanshift, on average. Consideration of this finding would offer an answer to a question that Reber and Alcock (2019, p. 9) ask about this phenomenon: "If psychokinesis affects the roll of dice in a psi lab, why not at craps tables?" Given how small PK effects tend to be, it is likely that they would be too weak and intermittent to be able to regularly overcome the much larger odds that are shifted in favor of the house (which is what keeps casinos in business). Thus, a casino craps table would likely be a poor testing ground for PK; such an illustrative display of "psi in everyday life" would only seem plausible if one does not take the findings of the parapsychological database into adequate account.

Reber and Alcock (2019) further claim that "[t]here are no patterns" in parapsychological findings (p. 9). But again, how can they know whether this claim has merit, without consulting the parapsychological database? Upon consulting it, they would have again learned that this also has no merit: Parapsychologists have actually found a fair number of significant correlations between psi and certain psychological variables, which have

exhibited enough consistency so far to suggest that an underlying pattern may be involved. One of them (often referred to as the “sheep–goat” effect; Schmeidler & McConnell, 1958) involves the apparent correlation between an individual’s own personal beliefs regarding ESP and their subsequent performance on an ESP test (Lawrence, 1993; Palmer, 1977, pp. 193–195; Storm & Tressoldi, 2017). Another involves the correlation between extroversion and ESP test performance (Honorton, Ferrari, & Bem, 1998; Palmer, 1977, pp. 185–188; Palmer & Carpenter, 1998; Zdrenka & Wilson, 2017).

One aspect of psi performance also seems to exhibit a notable parallel with the serial position effect, a pattern observed in psychological studies of memory and recall (Reed, 2004, pp.102 –103; Thompson, 1994). When tasked with reciting words from a memorized list, participants in these studies have often been found to accurately recall many of the words at the beginning and at the end of the list, with relatively few in the middle—a pattern that takes the form of a widened U-shaped parabola. In a similar fashion, psi test performance has been found in several studies to initially start off high, exhibit a decline, and then modestly increase again at the end; this pattern also follows a widened U-shaped parabolic trend (Bierman, 2001; Dunne, Dobyns, Jahn, & Nelson, 1994; Pratt, 1949; Rhine, 1969; Storm et al., 2010a, p. 478). This similarity is one of the things that seems to suggest that psi may not be so different from more ordinary forms of human behavior.

### ***Time’s Arrow***

Reber and Alcock (2019) next point out: “Within parapsychology, time is turned upon itself, most glaringly in precognition” (p. 9). While a reversal of the arrow of time would seemingly pose a potential explanatory problem, two considerations should be made in this regard: First, as some physicists have previously pointed out (Feinberg, 1975; Sheehan & Ibison, 2011; Targ, 1974), a number of theoretical equations in physics are time-symmetric, allowing for both forward and backward temporal solutions. Thus, time reversal would seem to be posited mathematically, although it remains uncertain whether the backward solutions (which are typically disregarded) might have any meaningful significance apart from this.

This leads to the second consideration: The issue of retrocausality still remains an open issue of debate in physics, as evidenced by the fact that at least three conferences sponsored by the American Institute of Physics have recently been held on this topic within a ten-year period (Sheehan, 2006, 2011, 2017). Thus, the matter of whether some form of time reversal is possible or not would still seem to be open and unresolved as of yet.



### **Thermodynamics**

Again referring to precognition, Reber and Alcock (2019) then argue: "If the future affected the present, it would violate the thermodynamic principle that energy cannot be created or destroyed in an isolated system" (p. 9). This argument seems to be inherently based on an assumption that precognition necessarily operates by a mode involving the transfer of some type of energy-based "signal" through space and across time. But does the evidence tend to indicate that such a mode is operating in ESP? Quite to the contrary, it would seem that when the parapsychological database is examined, there actually is not much clear evidence for that possibility at all.

As one example, there have been studies that have found that remote viewing continues to function well even when the participants have been electrically shielded by sitting inside a Faraday cage (Puthoff & Targ, 1976), or have been taken down in a diving submersible to ocean depths of several hundred meters—depths that are quite effective at attenuating signals in the extremely-low frequency range of the electromagnetic spectrum (Puthoff, Targ, & May, 1981; Schwartz, 2015, pp. 189–194). Arguably, a corresponding attenuation in the accuracy of the participants' impressions of the ESP target would be expected under these conditions if the functioning of ESP was being mediated by some type of "signal," and the apparent lack of ESP attenuation observed in these results would suggest that such a "signal" process is not operating here. But since they did not consult the parapsychological database, Reber and Alcock have likely not taken this (or any other related findings and observations that seem to hint against a signal transfer process<sup>2</sup>) into account. From this, it can be argued that if precognition and the other forms of ESP do not operate via a signal transfer process, then there may not be a reason to necessarily assume that they involve the spontaneous generation and transfer of some type of energy, and thus, it is not readily apparent that they would necessarily violate the first law of thermodynamics.

### **Inverse Square Law**

Perhaps containing the one (and only) point they make about psi in their rebuttal that is actually consistent with the findings of the parapsychological database, Reber and Alcock (2019) lastly argue: "In telepathy, the distance between the two linked persons is never reported to be a factor, a claim that violates the principle that signal strength falls off with the square of the distance traveled" (p. 9). Here again, their use of the phrase "signal strength" would imply that Reber and Alcock are inherently assuming that ESP operates by a process involving the transfer of an information-

or energy-based “signal.” But as with the case for thermodynamics, this claimed violation of the inverse square law would pose a serious issue *only if the evidence did seem to clearly indicate that such a process is involved in telepathy and the other forms of ESP.*

One might also note that, on the surface, the lack of attenuation with spatial distance in telepathy would appear to be akin to the kind of nonlocal correlation that two entangled particles seem to exhibit, regardless of the spatial distance at which they are separated—an observation that would hint at a possible conceptual analogy with quantum mechanics (Atmanspacher, Römer, & Walach, 2002; Josephson & Pallikari-Viras, 1991; Radin, 2006; Tressoldi, Storm, & Radin, 2010). Reber and Alcock (2019) take issue with such a possibility, stating that such an analogy “. . . won’t work. In QM [quantum mechanics], there is no transmission of energy between the separated particles; it is only that they are ‘entangled’” (p. 9). They do make a valid point in one respect, in that physical findings tend to indicate that it is highly unlikely that nonlocal correlations can be accounted for by some form of information or energy transmission between entangled particles (Salart, Baas, Branchard, Gisin, & Zbinden, 2008). But if the parapsychological evidence also does not clearly indicate that such a transmission process is involved in telepathy and the other forms of ESP, then this would not necessarily rule out an analogy with quantum entanglement and nonlocality.

In addition, it could be argued that such an analogy would seem to provide a useful means of conceptualizing ESP in terms of known physics, thereby making it seem less “paranormal.” It would also offer a possible answer to a question that Reber and Alcock pose: “If telepathy exists, why are our brains not constantly abuzz with the thoughts of those around us?” (p. 9) This would indeed be an issue if it were clear that ESP operated by signal transmission, as one might expect in such a case that the supposed ESP “signal” would propagate outward from the telepathic agent in all directions, and could potentially be “received” by others in addition to the (presumably intended) percipient. But this is not what is often found in anecdotal accounts of telepathy (Feather & Schmicker, 2005; Rhine, 1981; Stevenson, 1970); instead, the telepathic connection often seems to be limited largely to the agent and the percipient. In a notable parallel, nonlocal correlations *extend only to particles that are entangled with each other.*

A potential issue that arises with such an analogy is that quantum nonlocality represents a known aspect of physics that is still not too well understood and seems to go against all common sense. As Reber and Alcock (2019) rightfully point out, “QM is a physical theory but not in the ordinary, Newtonian sense that we confront in daily life” (p. 9). Yet in spite of that, quantum mechanics has been extensively tested and found to be a valid

physical theory: There is now a good amount of experimental evidence to indicate that nonlocal correlations can (and do) take place between entangled particles at a distance (e.g., Giustina et al., 2015; Moreau et al., 2019; and references cited therein). Arguably, the very existence of such a seemingly strange phenomenon should offer reason as to why one should not always rely strictly and solely upon superficial rhetoric (as Reber and Alcock do) when it comes to trying to determine what is possible and what is not. Common sense (upon which this kind of rhetoric is often reliant) would dictate that this kind of phenomenon should not happen. But there is now ample physical evidence that it does. Perhaps a similar consideration can now be made with regard to psychic phenomena on this basis, in light of the considerable amount of evidence for psi that has accumulated (Cardeña, 2018).

### Implications

Is it then the case that “parapsychology cannot be true unless the rest of science isn’t,” as Reber and Alcock (2019, p. 9) contend? Perhaps not: An analogy with quantum mechanics would seem to offer a way to place psi within the conceptual realm of presently known physics, without grossly violating or relinquishing any of its established principles. There are also a number of parapsychological findings that seem to offer preliminary indication that psi exhibits correlations with known aspects of brain functioning, such as alpha wave activity, event-related potentials, and cerebral lateralization (Alexander, 2002; Broughton, 2015; Krippner & Friedman, 2010; Williams, 2015), suggesting that psi would not be inconsistent with what is known in neuroscience. These initial findings might eventually prove helpful in highlighting certain promising avenues that, with further development, could pave the way toward ultimately achieving an understanding of psi that is actually compatible with mainstream science.

And when one again considers how relatively small the effects tend to be (Table 1; Bösch et al., 2006; Radin & Ferrari, 1991), it may be realized that Reber and Alcock’s (2019) claim that “if psi effects were real, they would have already fatally disrupted the rest of the body of science” (p. 9) is not likely to be valid at all. To a large extent, psi effects simply are not as strong and pronounced as Reber and Alcock seem to think they are. In being relatively small, it is unlikely that psi effects would pose much of a serious problem (if any at all) in affecting the findings of scientists on a broad scale.

Ultimately, Reber and Alcock (2019) wonder “why parapsychology still exists as a field of study. Why are some scientists still focused on the impossible?” (p. 9) The answer they try to offer relates to an impression received by Alcock that parapsychologists are motivated by a predisposed

and persisting belief in psi, which

. . . is likely linked with a vague sense that science, hard-nosed and physicalist, lacks that mysterianist element found in religious or spiritual realms. The lure of the “para”-normal emerges, it seems, from the belief that there is more to our existence than can be accounted for in terms of flesh, blood, atoms, and molecules. A century and a half of parapsychological research has failed to yield evidence to support that belief. (p. 9)

This claim raises two questions: First, are the majority of parapsychologists really motivated by some type of religious or spiritual belief? One online survey conducted among the members of the Parapsychological Association in 2001 seems to indicate that the likely answer is “no” (Tart, 2003): Approximately half of the responding members had indicated that spiritual motivation *was not* a central influencing factor in their pursuit of the study of psi, and only about a third of them had indicated that spiritual interests were of some importance in their current parapsychological work. This would seem to suggest that Alcock’s impression is not backed by much clear evidence.

Second, after a century and a half of research, could parapsychology be considered a “failed” science? It would seem that such a claim can only be maintained if one completely ignores the accumulated data in the parapsychological database (Cardeña, 2018). Moreover, in pondering this very question just over a quarter of a century ago, the late Charles Honorton (1993) had made the following observation, which should be carefully considered even now:

If we were to apply the “century of failure” arguments . . . to academic psychology, we might as well conclude that psychology has failed in its mission: After a hundred years of relatively well-funded research, vigorous controversies continue over such basic phenomena as memory, learning, and perception. . . . And while it is widely assumed that consciousness is a by-product of brain activity, neither psychology nor physiology has produced, over the past 100 years, even an intelligible model of how biochemical processes could be transformed into conscious experience. Are psychology and physiology failed sciences? Of course not. The most successful sciences such as physics deal with relatively simple and invariant processes: electrons, for example, are interchangeable; they do not have individual personalities, intentions, emotional states, or motivations. The behavioral sciences must contend with extremely complex and variable biological systems that possess these and many other individual attributes. Nevertheless, these sciences have produced many achievements, and so has parapsychology, even though it has been forced to exist on the outskirts of established science with marginal resources. (p. 193)

Lastly, there is one answer to Reber and Alcock's question that they do not consider themselves, which is really quite simple: Perhaps parapsychology persists because there may actually be something to its empirical findings (Cardeña, 2018), and once their unfounded assumptions are set aside, maybe—just *maybe*—the phenomena seemingly implied by those findings are not as "impossible" as Reber and Alcock (2019; Alcock, 2010a) might think.

### Conclusion

All of the points made here should make it amply clear that, in the end, Reber and Alcock's (2019) rebuttal is not particularly sound, containing many of the same unsubstantiated (and ultimately flawed) arguments that staunch skeptics have continued to echo about parapsychology throughout the years (Roe, 2017; Schwartz, 2011; Zingrone, 2004). Rather than parapsychology (as Reber and Alcock maintain, p. 9), it would seem that it is staunchly closed-minded skepticism which has learned nothing over time and "shows little evidence of progress" (Roe, 2017, p. 143). In light of this, an observation made by Zingrone (2004) would seem to be quite apt in the case of critical arguments made by staunch skeptics such as Reber and Alcock:

Armchair criticism is not useful. Blind criticism—which sees neither its own flaws nor any consequences—is not useful. If critics wish to be heard they will have to progress methodologically, they will have to evolve. It is no longer enough merely to raise a dissenting voice. There must be substantive content in that dissent and a consciousness of the context in which that dissent is raised.

One should especially keep this in mind, when ultimately assessing the value of Reber and Alcock's (2019) rebuttal to Cardeña's (2018) article.

### Notes

- <sup>1</sup> This choice to not examine the parapsychological data carries a bit of irony in Alcock's case, because not so long ago he had actually urged others to focus on those data, as part of a claim he made that the data are weak and unreliable (Alcock, 2010b). Yet now, when Cardeña (2018) does focus on those data and science is seemingly responding to them (as one might infer from the publication of Cardeña's review in *American Psychologist*), Alcock chooses to willfully ignore his own advice.
- <sup>2</sup> Detailed review of the other findings that hint against a signal transfer process in ESP is beyond the scope of this paper, although additional

discussion can be found in a separate paper by the author that is available through the Academia online preprint repository: [https://www.academia.edu/37157173/Towards\\_Normalizing\\_the\\_Paranormal\\_On\\_the\\_Seeming\\_Incompatibility\\_of\\_Science\\_and\\_Psychic\\_Phenomena](https://www.academia.edu/37157173/Towards_Normalizing_the_Paranormal_On_the_Seeming_Incompatibility_of_Science_and_Psychic_Phenomena)

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