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TARGET ARTICLE

Precognition as a Form of Prospection: A Review of the Evidence

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Prospection, the act of attempting to foresee one's future, is generally assumed to be based on conscious and nonconscious inferences from past experiences and anticipation of future possibilities. Most scientists consider the idea that prospection may also involve influences from the future to be flatly impossible due to violation of common sense or constraints based on one or more physical laws. We present several classes of empirical evidence challenging this common assumption. If this line of evidence can be successfully and independently replicated using preregistered designs and analyses, then the consequences for the interpretation of experimental results from any empirical domain would be profound.

Keywords: prospection, precognition, presentiment, anticipatory activity, retrocausality

In this review, we discuss a set of controlled experiments investigating what we will argue is an inherent human ability that allows for accurate prediction of future events without inferential means; in the vernacular this ability is known as *precognition*. While taking this line of research seriously may seem beyond the pale to some, it is worth remembering that advances in psychology and physics have repeatedly demonstrated that everyday intuitions about the nature of reality only partially reflect the nature of reality itself. It is possible that such imprecise intuitions include the concept of a fixed "arrow of time," which Einstein famously called a "stubbornly persistent illusion" (Einstein & Hawking, 2007).

Everyday Intuitions About Events in Time

Common sense intuitions about events unfolding in time include the following:

- Events in the physical world are mirrored by our almost immediate perceptual recreation of those events in essentially the same order that they occurred.
- The recreation of physical events, first in perception and later in memory, occurs in a linear order based upon our original perception of events. Thus, given two events and assuming perfect perception and memory, Event A is said to occur before Event B if at some point we remember Event A but we have not yet experienced Event B, and then later, after Event B occurs, we remember both events.
- Event A may only be said to "cause" Event B if Event A precedes Event B.
- What we remember has occurred in the past.

These everyday intuitions seem reasonable because they arise and are inculcated by innumerable conscious waking experiences (Mossbridge, 2015). However, as developments in psychology and physics have repeatedly shown, even reasonable assumptions should be thoroughly checked using multiple methods to determine if in fact they are correct.

What methods can allow us to double-check our intuitions about the nature of time? If our everyday intuitions are correct and we assume that certain future events cannot be inferred from present circumstances or extrapolations

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based on past circumstances, and if we also recognize that coincidences will occasionally occur by chance, then the following predictions should be borne out in carefully conducted experiments: (a) Dreams will bear no relationship to unpredictable future events beyond chance levels; (b) individuals cannot consciously outguess unpredictable future events at a rate greater than chance; (c) behavioral measures administered in the present will not be affected by future events; (d) physiological measures will not be affected by future events; (e) events in the future do not influence what occurs in the present, except in cases of prospective planning (e.g., "I need to prepare for rain tomorrow, so today I will purchase an umbrella").

Precognition is not required to guess that researchers have performed experiments that examine all of these predictions, nor that some of these studies suggest that widely held intuitions about time are empirically falsifiable. Here we will describe and evaluate the literature in domains known as precognitive dreaming, forced-choice precognition, free-response precognition, implicit precognition, and presentiment. We will build from what we believe to be the weakest to the strongest evidence for precognition. Along the way, and in a concentrated form at the end of this review, we will suggest methodological improvements and future directions, as well as present some speculations about potential mechanisms.

Tests of Precognitive Dreaming

Anecdotal claims of precognitive dreams are common, reported by ~17–38% of survey participants (Lange, Schredl, & Houran, 2000; Parra, 2013). Many such claims have been reported, including dreams of historical figures such as Abraham Lincoln and Mark Twain. In the former case, on each of 3 nights before he was assassinated, the bodyguard assigned to Abraham Lincoln reported that the president mentioned dreams of his death (Lewis, 1994). In the latter case, Mark Twain wrote that he dreamed of his brother's death before his brother was tragically killed in a steamboat accident (Zohar, 1982). These stories are intriguing, but because of known frailties of memory, including confabulation and elaboration, and because some events are predictable based on nonconscious associations drawn from sensory cues (e.g., Bechara, Tranel, Damasio, & Damasio, 1996), anecdotes alone cannot provide persuasive evidence that precognition actually occurs in dreams.

For scientifically valid evidence, we turn to controlled experiments. Researchers have published four well-controlled precognitive dream experiments in peer-reviewed journals. By "well-controlled," we mean the following: (a) Participants were asked to attempt to dream about a target they would see the next day (i.e., images, staged multisensory experiences, or video clips); (b) a random-number generator was used to select one target from a pool of at least four available targets (e.g., a video clip of a birthday party) and only that one target was later shown to the dreamer; (c) on each trial the experimenters selected the target *only after* dream reporting was complete and submitted to the experimenters, *and before* the experimenters read the dream reports; (d) independent judges naïve to the identity of the actual target judged the similarity between dream content and the target; (e) judges' responses were considered final. Researchers reported significant results using binomial statistics ($\alpha = 0.05$, two-tailed tests) in three out of four of these studies (Krippner, Honorton, & Ullman, 1972; Krippner, Ullman, & Honorton, 1971; Watt, 2014). The fourth study (Watt, Wiseman, & Vuillaume, 2015) did not show statistically significant results but the effect was in the predicted direction ($ES = 0.11$; $N = 20$ with one trial per person).[1]

Four other peer-reviewed experiments deserve consideration (Luke, 2002; Luke & Zychowicz, 2014; Luke, Zychowicz, Richterova, Tjurina, & Polonnikova, 2012; Sherwood, Roe, Simmons, & Biles, 2002), although they do not fit our methodological constraints. The difference is that in these additional studies, dreamers viewed *all* items in the target pool after their dreams had been recorded. This allowed each dreamer to rank the similarity of all of the items in the target pool against his or her own dream content, which in turn simplified the judging of

[1] Another study (Schredl, Götz, & Ehrhardt-Knutsen, 2010), which is sometimes mentioned alongside these other reports, did not make our list because although the specific target was selected after dreams were recorded, the preselected target pool contained only two targets, small enough that the resulting outcome, while significant, is of questionable importance.

dream accuracy. But it also violated requirement (a) above. This type of design may pose a problem if dreams can indeed gather information about a future event. This is because all potential targets, rather than just the actual target, might confuse underlying precognition mechanisms in that they might send information about multiple targets backward in time. This postulated "information confusion" could explain why none of these studies produced a significant result. It is worth reminding readers that the investigator's goal when exploring a hypothesis is to try to disprove that hypothesis. If the hypothesis is that people can precognize future events, then it is not useful for researchers to expose participants to several future events, because according to the hypothesis the researcher is trying to disprove, participants should be able to precognize any or all of those events.

Further description of Watt's (2014) study can illustrate the important elements of a well-controlled precognitive dream experiment, as well as a methodological concern that was addressed by the authors in a timely fashion. The targets in this study consisted of a group of 68 video clips the experimenters developed before they reviewed participants' dreams. The study was preplanned to include 200 trials, with four trials performed by each of the first 50 participants. Mörck (2015) raised the concern that individuals who did not complete the four trials in a timely manner might have been discouraged by their performance on the first few trials. Accordingly, they may have exited the study, thereby producing a self-selection bias in that the participants who completed all four trials may have believed their dreams best matched the targets. A follow-up analysis (Watt & Valásek, 2015) indicated that the results of that study were indeed skewed toward cases in which participants' dreams happened to match the targets, although after accounting for this self-selection bias the results trended toward significance.

Each dreamer submitted a report of the contents of any dreams that occurred over five nights in which they attempted to focus on the target video, which had at this point neither been viewed or even selected. After that report was submitted an independent judge ranked the similarity between that participant's dreams and four video clips selected for the target pool using a random number generator based on atmospheric noise as the random source. The judge then completed a preplanned outcome measure, which was a ranking of the four clips in the target pool ranging from 1 (*most similar to dream content*) to 4 (*least similar*). After this ranking process was completed and submitted, the experimenters, who did not know the judge's ranking results, used the random number generator to select one target video clip from the four ranked by the judge. The experimenters then sent a website link to this one video clip to the dreamer. The two earlier dream precognition studies were similarly well controlled, but those studies used only one dreamer who performed multiple trials (Krippner et al., 1972; Krippner, Ullman, & Honorton, 1971).

In sum, two well-controlled studies with statistically significant findings, two well-controlled studies with nonsignificant findings, and four controlled studies with potential for information confusion that yielded nonstatistically significant findings constitute too small a dataset from which to draw firm conclusions about whether dreams can reveal the content of upcoming unpredictable events. One mundane explanation for significant results is that they are drawn from a much larger pool of unpublished experiments with nonsignificant results (i.e., publication bias, a questionable research practice known to be pervasive across disciplines; e.g., Ioannidis, 2005; Pashler & Wagenmakers, 2012).

If the reader desires to further delve into the field, more detailed reviews of this topic are available elsewhere (Baptista, Derakhshani, & Tressoldi, 2015; Sherwood & Roe, 2013). Thus far, we may conclude that these data are insufficient for drawing conclusions. To better assess whether dreams can reveal veridical information about truly unpredictable future events, what is needed are repeated studies performed across multiple laboratories. Those studies should ideally use the same controlled group-study methods employed by Watt et al. (2015) with larger sample sizes and controls for self-selection bias, or with controlled single-participant methods (see Krippner et al., 1972; Krippner, Ullman, & Honorton, 1971).

A Note on Meta-Analysis

In many of the remaining sections, we report the results of meta-analyses. In the present dis-

cussion, we contend that meta-analysis is the best approach to determine whether a particular empirical effect replicates across studies, although it has been argued that meta-analysis is not an ideal tool for changing researchers' minds (Ferguson, 2014). The Association for Psychological Science has included meta-analysis in its standards for building a cumulative discipline, as meta-analysis is the basis for evidence-based medicine (Cumming, 2014). The premise of meta-analysis is that a p value for any experiment may be above or below significance, even if the underlying effect is meaningful. This is because many uncontrolled factors can influence the result, especially in psychological experiments. When the average trend of the effect sizes is in the same direction, and the trend is statistically validated via a well-conducted meta-analysis, then we can be reasonably sure that the effect under consideration is meaningful (for updated reviews, see Lakens, Hilgard, & Staaks, 2016; Shamseer et al., 2015). In contrast, a single trial in a multitrial experiment often provides a poor indicator of the overall average effect of interest. Similarly, a single successful or failed replication of any study should be regarded as tentative and with caution, providing a rationale for why (a) all attempted replications should be included in any meta-analysis and (b) we restrict our presentation of p values to those from meta-analyses and simply comment on the significance of only a few individual studies (assuming an alpha of 0.05 and two-tailed tests, with noted exceptions).

It important to note that although it is seldom discussed explicitly, the meta-analytic approach is based on an implicit assumption that the studies included report results that are not fraudulent, nor based on inappropriate multiple analyses (otherwise known as p-hacking, or fishing for a significant effect), and that all experimental results pertaining to the question under study were reported. Fraud, multiple analyses, and underreporting of negative results remain difficult to assess, but well conducted meta-analyses, such as those reviewed here, attempt to address these concerns.

Prospective meta-analyses are used in clinical medicine to further enhance the predictive value of meta-analysis. Caroline Watt of the University of Edinburgh is currently organizing such analyses for the parapsychological research community, based on her experience operating a study registry for precognition and other controversial experiments (Watt & Kennedy, 2015). In a prospective meta-analysis, the studies to be analyzed and the methods of analyses are preplanned. This approach offers a way to further avoid the biasing effects of potential fraud, multiple analyses, and underreporting of negative results. Prospective meta-analysis is an approach sympathetic to an argument made by Wagenmakers, Wetzels, Borsboom, van der Maas, and Kievit (2012), who suggest that all researchers should conduct confirmatory experiments with preregistered analyses and designs.

Forced-Choice Conscious Precognition Tasks

A second approach to studying precognition is to test whether people can consciously access future information using a simple forced-choice task. In a forced-choice precognition task, a participant is offered two or more choices, one of which will randomly occur in the future, like the outcome of a coin toss. Participants perform multiple trials of the task, and the proportion of correct trials is the dependent variable. A meta-analysis of such experiments based on reports of 309 experiments published between 1935 and 1987 (Honorton & Ferrari, 1989) yielded a small overall effect size (Rosenthal $ES = 0.02$). Nevertheless, due to the high statistical power afforded by the many studies considered, it was statistically significant (Stouffer $Z = 6.02$, $p < 1.1 \times 10^{-9}$). Using Rosenthal's failsafe estimate, the authors calculated that 14,268 unreported studies averaging a null effect would have been required to transform the database into one with an overall null effect. The size of that file-drawer estimate, in comparison with the number of laboratories studying precognition, suggested that selective reporting was an unlikely explanation for the observed effect. However, small effect sizes may also reflect consistent artifacts or methodological errors instead of a genuine effect.

Forced-choice, repeated-guessing precognition experiments began to fall out of favor in the 1990s for two reasons: (a) The tests were boring, encouraging participants to use conscious deliberation to try to outguess the next target, and (b) the tests did not resemble individuals' spontaneous experiences of apparent precognition (Storm, Tressoldi, & DiRisio, 2010). So,

investigators began to gravitate toward two new types of precognition tasks: free-response conscious precognition tasks and implicit precognition tasks.

Free-Response Conscious Precognition Tasks

In a free-response conscious precognition task, the participant is asked to describe the contents of a randomly selected visual target that will be shown to the participant in the future. In a properly controlled precognition test, at the time the participant reports her or his impressions, no one (not even a computer) knows what the target will be. Only after the participant has recorded and submitted her or his impressions is a target randomly selected and presented to the participant.

Beginning in 1976, researchers from the Princeton Engineering Anomalies Research Laboratory performed multiple free-response precognition studies using this protocol. Dean of the School of Engineering and head of the Princeton Engineering Anomalies Research Lab, Robert Jahn, summarized those studies in a publication in the *Proceedings of the IEEE* (Jahn, 1982). He presented multiple "precognitive remote perception" experiments that resulted in highly significant results. A later analysis suggested that methodological weaknesses, including allowing investigators in one condition to freely choose targets according to their preferences, might have provided percipients with clues about the target in both precognitive trials and those in which the targets were chosen at the same time or before the percipients' descriptions were recorded (Hansen, Utts, & Markwick, 1992). Thus, the overall findings were considered suspect until responses from the original experimenters made it clear that even after dropping the volitionally selected target trials, the data were still statistically significant (Dobyns, Dunne, Jahn, & Nelson, 1992; Nelson, Dunne, Dobyns, & Jahn, 1996). Further, taking into account only precognitive trials, data from a series using randomly selective targets located in the Chicago, Illinois, area as well as data from a series in which the percipient was located alone on a boat in the ocean could not be considered suspect because the percipient would have had no clues about the target (see "Chicago" and "Ocean" series; Jahn, 1982). Each of these precognitive remote viewing series were accurate significantly beyond chance, and neither suffered from the problems discussed by Hansen et al. (1992).

In support of the idea of using free-response instead of forced-choice tasks to examine precognition, Dunne and Jahn (2003) pointed out that trials using a truly free-response method produced larger effect sizes than those employing a partial-free-response (multiple-choice rating) approach (called feature importance discrimination option analysis). However, they performed this analysis as a post hoc attempt to explain what appeared to be a decline effect over time, and it is worth noting that some of the later trials that did not use feature importance discrimination option analysis still had generally smaller effect sizes than the original free-response trials.

May (2014) performed an experiment that exemplified a free-response precognition experiment with methods following the recommendations of Hansen et al. (1992). Three participants who had produced above-chance results in previous free-response tasks administered in the context of a classified government program, participated in a nonclassified experiment in which they were tasked with performing a total of 50 preplanned free-response conscious precognition trials. First, researchers asked a participant to describe a future target; this description was recorded. Then the experimenter made a "fuzzy-logic" judgment about that description *prior* to the random selection of a target image. To allow this judgment to be made without knowing the eventual target, researchers used a coding method that matched the elements in a participant's description against a prearranged set of elements describing aspects of the target stimulus set (e.g., each potential target image had already been coded to indicate if the image depicted water, buildings, trees, etc.). After the judgment was completed, the target image and two additional decoy targets were randomly selected from a stimulus pool consisting of 300 images.[2] Then, based on the number of matching elements between the previously coded de-

[2] Comparison images were not selected uniformly at random—they were instead selected randomly within two categories of images that were not in the same category as the target image. All images were selected from the following categories: canyons, waterfalls, bridges, cities, or Asian structures.

scriptions of each of the three target images, researchers calculated a "figure of merit" for the target image. After these calculations, the software randomly selected a target image, and the trial was judged to be a "hit" if the figure of merit for the target image was the highest figure of merit out of the three images. Finally, the target image was shown to the participant as feedback. This experiment produced 32 hits out of the 50 planned trials, a statistically significant hit rate based on a binomial test with a success rate of 33% expected by chance.

A significant result based on three participants is not worth mentioning if the experimenter's goal is to determine whether an effect generalizes to the population at large. However, we understand that May's (2014) experiment was simply a proof-of-principle demonstration that certain individuals can successfully predict future events that are specifically designed to be unpredictable by any ordinary means. Assuming that May's results were not due to chance and that his report was accurate, his results disproved the null hypothesis. Independent attempts to replicate May's experiment using the same participants would be both constructive and informative.

Implicit Precognition Tasks

The methods used in implicit precognition experiments may at first appear to be quite different from the methods already discussed, but they are similar in that implicit precognition tasks can be used to examine whether present *actions* are related to unpredictable future events. For example, we know that practicing or studying material that one desires to remember generally enhances recall of the studied material in the future. Daryl Bem (2011) conducted a clever experiment in which he reversed this practice effect in time to examine whether *future* practice might influence *present* performance on a recall task. Bem reported that the results of the first two of such tests revealed that participants were better at recall for words that they were *going to* practice in the future (Bem, 2011). This experiment and attempts to replicate its findings will be discussed in more detail below.

In recent decades, implicit precognition designs have become increasingly popular because they involve simple time-reversals of conventional tasks commonly used in experimental and social psychology. As such, these experiments are relatively easy to implement and they avoid the necessity of requiring participants to *consciously* guess future events, which researchers have long suspected impedes accessing future information (Carpenter, 2004, 2005).

Bem's (2011) report on a series of nine implicit precognition experiments, all showing statistically significant or near-significant results, is perhaps the most widely discussed recent paper on the topic of implicit precognition. Bem was criticized for using 1-tail statistical tests, lack of clarity in reporting his methods, and potentially using multiple analyses to get the desired results (e.g., Alcock, 2011). Critics also suggested that a Bayesian approach should be used for all data analyses in psychology, instead of null-hypothesis significance-testing, with Bem's report acting as the primary motivation for this suggestion (Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011). A response to that critique showed that after applying a Bayesian analysis with an appropriate prior distribution, the results were highly significant (Bem, Utts, & Johnson, 2011). Other authors also suggested that the analysis by Wagenmakers and others was flawed (Rouder & Morey, 2011). They reanalyzed Bem's combined data using a Bayesian approach and found the results to be personally unconvincing, though it is clear that in a less controversial field the Bayes factor that they had calculated for Bem's experiments with nonerotic stimuli would have been considered "moderate" to "strong" evidence (Goodman, 1999). Rouder and Morey (2011) stated at the end of the abstract of their report,

> There is some evidence. . .for the hypothesis that people can feel the future with emotionally valenced nonerotic stimuli, with a Bayes factor of about 40. Although this value is certainly noteworthy, we believe it is orders of magnitude lower than what is required to overcome appropriate skepticism of ESP. (p. 682)

Others have been critical of the approach of combining different studies in this way, and they do not accept that there is strong enough evidence in favor of precognition based on these data (Wagenmakers, Wetzels, Borsboom, Kievit, & van der Maas, 2015).

In general, consensus exists among sceptics that many of the concerns raised about Bem's research could have been avoided if Bem had

preregistered his experiments as confirmatory studies (see methodological recommendations, below). When there is too much freedom in the analytical steps that can be taken to confirm a hypothesized outcome, the probability increases that researchers will obtain their desired outcome. Preregistration of a confirmatory experiment, which includes specifying in advance the design and analytical strategies that will be adopted, constrains the experimenter to use only preplanned methodology, thereby mitigating or precluding confirmation bias in this or in any empirical field (Brandt et al., 2014).

Taken at face value, one of the most impressive findings from the Bem (2011) series of experiments was that practice on a list of words *after* a word-memory task was correlated with significant improvements in recall for the words that would subsequently be practiced. This was an implicit precognition test in that participants were not explicitly asked which words would be studied after they performed their word-memory test, but the test did use consciously deliberated responses (words typed by the participants) as the dependent variable. In contrast to these slower, more deliberate responses, other experiments in Bem's report included designs that required relatively fast response times as the dependent variable.

The distinction between slow and fast responses is pertinent. The most statistically impressive results in Bem's original report were from the two experiments investigating the time-reversed word-practice effect (Bem, 2011; Wagenmakers et al., 2011), but similar implicit precognition experiments, and some exact replications, using deliberate, slower responses (using so-called slow-thinking cognitive systems; e.g., Kahneman, 2011; Evans & Stanovich, 2013) have not produced reliably repeatable effects. It appears that experiments requiring fast responses (using so-called fast-thinking cognitive systems) have produced remarkably reliable and repeatable effects. This distinction was noted in a post hoc evaluation within a meta-analysis of 90 implicit precognition experiments conducted between 2000 and 2013, which included all known published and unpublished attempts to replicate Bem's experiments (Bem, Tressoldi, Rabeyron, & Duggan, 2016). The authors used dual-process theory, as described by Kahneman (2011) as well as Evans and Stanovich (2013), to divide the experiments into fast versus slow-thinking—although, again, it is worth noting that this division was made by the authors after the authors were aware of each experiments' results. According to that division, of the studies reported 61 were categorized as fast-thinking, including five categories: precognitive detection of reinforcement, precognitive avoidance of negative stimuli, retroactive priming, retroactive habituation, retroactive practice. The remaining 29 studies were categorized as slow-thinking, including the categories of retroactive facilitation of practice on recall and retroactive facilitation of practice on text reading speed.

Taken together, based on the original (not post hoc) analysis, the effect size for all 90 experiments combined was small but highly significant (Hedges' $g = 0.09$; $p < 1.2 \times 10^{-10}$). The possibility of significant decline effects, that is, a reduction in the effect size of a given phenomenon over time, has been pointed out as a problem in parapsychology research (Hyman, 2010). If an effect declines to null over time, it is possible that the original effect was spurious, and as the quality of the experimental design and analyses improved, the effect would be understood as a fluke or an artifact. Analysis of the effect sizes for all 90 Bem-style experiments over time showed no significant decline effect (year vs. effect size: Pearson's product-moment correlation: -0.149, $p > .161$). There was also little evidence that those results were due to multiple unreported analyses (p-hacking) or selective reporting (Bem et al., 2016). One concern that has been raised is that this meta-analysis included studies published prior to Bem's (2011) study, but as pointed out in the meta-analysis, Bem started providing software for replications in 2000, the year of the first replication report included in this meta-analysis, so that concern seems unwarranted. In terms of fast- versus slow-thinking paradigms, the authors observed that all five categories of fast-thinking tasks independently produced significant results within each category, and the overall fast-thinking effect was highly significant ($z = 7.11$, $p < 6 \times 10^{-13}$). By contrast, slow-thinking tasks produced nonsignificant results in both experimental categories as well as overall ($z = 1.38$, $p > .15$).

Bem et al. (2016) suggested that the disparity between fast- and slow-thinking outcomes is

due to the former not suffering as much as the latter from conscious judgment or from alteration of the information obtained from nonconscious sources. An alternative explanation could be that many of the experiments requiring fast responses also used emotionally charged stimuli, which are more engaging than the stimuli used in the experiments requiring slower responses (Barušs & Rabier, 2014). If attention to precognitive information depends on motivation and engagement, which seems reasonable, then this explanation would both make intuitive sense and be empirically testable. A third possibility may be that implicit precognition follows a decay time-course similar to that obtained in studies of forced-choice conscious precognition (Honorton & Ferrari, 1989), such that tasks providing immediate feedback produce more accurate precognition than tasks providing delayed feedback.

A useful next step in a research program focusing on implicit precognition would be to continue to examine moderators of the effect. The analysis of moderators such as age, gender, and Big Five personality type (McCrae & John, 1992; Tupes & Christal, 1961) could provide useful information about potential psychological, physiological and personality variables associated with precognitive effects, which might further shed light on whether precognition effects are due to subtle artifacts. In terms of potential mechanisms underlying purported precognitive effects, most fast-thinking paradigms currently conflate fast responses with immediate feedback. By teasing apart these two factors, researchers could determine if it is the speed of the response, the speed of the feedback, or both, contribute most to the precognitive effect. If the speed of the response matters most, this would suggest that deliberation over which response to select is what must be eliminated to access accurate precognition about future events. If instead the speed of the feedback matters most, this might suggest that precognition decays with a time course similar to decay of ordinary memory for past events. In general, variations of the implicit precognition paradigm offer many opportunities to study variables that interact with and modulate perception of future information, which in turn may offer insights into underlying physical mechanisms.

Presentiment: A Physiological Measure of Implicit Precognition

Outstanding predictive abilities, for example among elite athletes, may be based on highly efficient unconscious processing of sensory cues as well as exceptionally accurate predictive algorithms (e.g., Aglioti, Cesari, Romani, & Urgesi, 2008). This idea probably explains at least some aspects of superior performance. However, several decades of physiological evidence demonstrate that in humans and some animals, unconscious or semiconscious processes also seem to correctly anticipate unpredictable future events. This phenomenon has been called *predictive anticipatory activity* (Mossbridge, Tressoldi, & Utts, 2012; Radin, 2011), but a simpler moniker is *presentiment* (Radin, 1997; Radin & Pierce, 2015). As we describe below, presentiment may underlie superior performance in any sort of task that requires anticipation of noninferable future events.

In the first experiment that explicitly tested the presentiment hypothesis (Levin & Kennedy, 1975), contingent negative variation (CNV), a negative slow cortical potential that correlates with sensorimotor expectancy (Walter, Cooper, Aldridge, McCallum, & Winter, 1964), was used as a nonconscious physiological measure of precognition. Participants were asked to press a button as quickly as possible, but only after they saw a green light. After a red light, they were asked to withhold their button press. Electroencephalographic data showed a significant difference in CNV before the green light (when a motor action would be required), but not before a red light. Subsequent attempts to use CNV to measure presentiment failed to successfully replicate the effect (Hartwell, 1978, 1979).

Radin (1997) investigated the idea that emotional stimuli might be more likely to produce presentiment effects. He used heart rate, blood volume, and electrodermal activity as physiological measures. These measures were simultaneously recorded while participants viewed a randomized series of images. The protocol for these experiments included features that eventually became standard for most presentiment replication attempts, including the following: (a) Each trial consisted of an event that was either calm or emotional (e.g., viewing a picture

of a tree vs. a plane crash), (b) the order of these events was completely randomized, (c) the experimental procedure provided no known sensory or statistical cues about the content or nature of the next event, and (d) the values of the physiological measures in a predefined period preceding each trial type were the dependent variables. This protocol revealed significantly different physiological changes preceding upcoming emotional events, as compared with calm events, with the difference emerging between 2 and 4 s, on average, prior to an event (Radin, 1997).

This basic result has been successfully replicated more than 20 times after Radin's original study (e.g., McCraty, Atkinson, & Bradley, 2004; Radin, 2004; Radin & Lobach, 2007; Radin & Borges, 2009; Spottiswoode & May, 2003; Tressoldi, Martinelli, Semenzato, & Cappato, 2011). A statistically conservative meta-analysis of all known presentiment experiments following the above-described protocol concluded that presentiment is a repeatable effect ($N = 26$ studies; random effects: overall $ES = 0.21$, $z = 5.3$, $p < 5.7 \times 10^{-8}$; fixed effects: overall $ES = 0.21$, $z = 6.9$, $p < 2.7 \times 10^{-12}$), and that it occurs over a range of durations that appear to depend on the physiological system used to measure the effect (Mossbridge et al., 2012). An analysis of the meta-analytic data in Mossbridge et al. (2012) indicates that there is no significant decline effect in the findings over time (year vs. effect size: Pearson's product–moment correlation $= -0.14$, $p > .49$). In this meta-analysis and also in a later review, potential problems were assessed including multiple unreported analyses (*p*-hacking), publication bias, and fraud. All three possibilities were rejected as viable explanations (Mossbridge et al., 2012; Mossbridge et al., 2014), at least partly because this meta-analysis tested a slightly different hypothesis than the original experimenters did. That is, almost all of the original experimenters tested a nondirectional hypothesis that physiological variables will differ consistently (in any direction) prior to emotional versus calm stimuli, whereas the authors of the meta-analysis tested a directional hypothesis that physiological variables will change in a consistent direction prior to emotional versus calm stimuli. The fact remains, of course, that *p*-hacking, publication bias, or fraud might not be detected in the data examined in any meta-analysis.

Of the various conventional explanations proposed to explain presentiment effects, the most plausible is the gambler's fallacy. In this context the gambler's fallacy would manifest as participants gradually becoming more anxious after a series of calm events because they would increasingly expect that an emotional event *ought* to occur soon. The fact that the underlying sequence of events is randomly determined, and independent of one another, is overpowered by inaccurate expectations about sequential randomness (e.g., Laplace, 1796/1951; Tversky & Kahneman, 1971). In fact, simulations have shown that a small presentiment-type outcome could potentially be explained by increases in physiological arousal due to a series of calm events that, by chance, just happened to precede an emotional event (Dalkvist, Westerlund, & Bierman, 2002; Wackermann, 2002). However, of the 26 experiments examined in the 2012 presentiment meta-analysis, 19 of the studies were performed by researchers who were aware of this potential confound and performed analyses to determine if the gambler's fallacy bias could have reasonably explained the observed results. None of the researchers found convincing evidence for such an anticipatory bias (Mossbridge et al., 2012). In addition, a simulation of presentiment using a common expectation-bias test, a linear regression on the number of calm trials preceding an emotional one versus the physiological response preceding the emotional trial, found that 92% of the modeled presentiment effect remained unexplained even after removing experiments for which expectation bias detected with this test could potentially explain the results (Mossbridge et al., 2015).

Based on the results of the meta-analysis, the authors concluded that, barring widespread collusion among independent investigators, it appears that nonconscious access of future, unpredictable information is possible (Mossbridge et al., 2012). Of course, like the other experimental categories, research on presentiment effects has evoked debate (e.g., Mossbridge et al., 2015; Schwarzkopf, 2014). Schwarzkopf (2014) raised six points of concern, which were responded to in an exchange with Mossbridge and colleagues (2015). First, any meta-analysis is only as good as the data it considers. Meta-analyses, like any analysis, can be biased both in terms of which

articles are included and how the statistical analysis is performed (Franklin, Baumgart, & Schooler, 2014). Although Mossbridge and others performed a quality check and adopted a statistically conservative approach against their hypothesis whenever there was a choice of approaches, their meta-analysis might have been biased in some unspecified way. Schwarzkopf contended that the meta-analysis should have been broadened to include data from experiments not designed to test presentiment. In fact, Mossbridge et al. (2012) did analyze relevant psychophysiological data, but they did not include those results in their meta-analysis because the protocols did not use randomization of stimuli *with replacement*, an important feature of presentiment experiments intended to help reduce expectation bias. Schwarzkopf also expressed concern that because many of the presentiment experiments included in the meta-analysis used an uneven ratio of calm to arousing stimuli (often 2:1 or 3:1), which might have generated a response bias if participants figured out the stimulus ratio. Mossbridge and others pointed out that if this were the case, it would bias participants toward expecting calm stimuli, making presentiment more difficult to detect, not easier. Schwarzkopf then noted that clamping of the physiological trace to zero at a time prior to the stimulus could have produced the effects, but it was not clear how that could be the case given the analytical methods employed. Schwarzkopf further suggested that testing for expectation bias is generally performed by assuming that such biases would manifest linearly, but expectation biases might be nonlinear. Mossbridge et al. (2015) agreed. And finally, Schwarzkopf (2014) questioned if presentiment effects were biologically plausible. Mossbridge and others (2015) responded that if a presentiment effect were observed in these experiments, then there *must* be a natural explanation, even if one were not yet identified. Assessments of plausibility depend on current theoretical assumptions about what is or is not believed to be possible. Declaring a repeatable empirical effect to be implausible should arouse both caution and celebration; caution because expert intuitions about what is possible can be useful in identifying subtle confounds, artifacts, or experimenter biases, but also celebration because implausible effects can, at times, reveal entirely new, previously unimagined realms of knowledge.

Aside from these results in humans, a recent presentiment experiment in planaria (Alvarez, 2016) followed up a previous report of presentiment in birds by the same researcher (Alvarez, 2010). Experiments using lower level animals are useful in that they rule out common human forms of bias, such as the gambler's fallacy. In the experiment with planaria, the worms were observed prior to a random number generator's decision about whether a loud sound would be played. The experimenter measuring the behavior of the planaria was blind to whether the loud stimulus would be played. The planaria showed significantly more head movement (indicating stress or exploration) prior to when the loud stimulus was played as compared to times when it was not played. If researchers conduct a preregistered confirmatory experiment based on this finding, and if that experiment replicated the original effect, this would go a long way toward suggesting that biological mechanisms can use information about future events to influence present-time behavior.

Ideally, future presentiment experiments will continue to address what we believe to be the most outstanding concern: How do we know that the physiological effect we observe on any given trial is not caused by a delayed or latent response to the previous trial or trials? Most researchers performing psychophysiological experiments assume that trial randomization takes care of any such order effects and generally remain unconcerned about them, but those involved in presentiment research have been motivated to develop both analytical and experimental methods to determine whether expectation bias can explain presentiment (e.g., Dalkvist, Mossbridge, & Westerlund, 2014). As far as we know, the only guaranteed way to rule out order or expectation effects as an explanation for presentiment is to perform experiments in which each participant only experiences a single event, and in which comparisons between physiological measures preceding emotionally contrasting events are made across participants. The results of such a presentiment experiment, if they revealed a statistically significant presentiment effect, could strongly argue against any order- or expectation-based explanations for presentiment.

Recommendations for Future Investigations of Precognition

For decades, researchers interested in precognition and related phenomena routinely prespecified the numbers of participants and trials to eliminate "optional stopping." This strategy can be especially effective when it is incorporated into a preregistered design in a prospective meta-analysis. More recently, researchers have recommended preregistration of experimental methods and analyses, especially for confirmatory experiments, to help ensure that researchers cannot change their planned experimental or analytical approaches after the study is underway (see http://www.koestler-parapsychology.psy.ed.ac.uk/TrialRegistry.html for several recently registered precognition experiments). Replications across multiple laboratories with data analyses performed by individuals who are unaware of either the hypothesis or the experimental manipulation(s) are particularly valuable. We also recommend that researchers attempting to perform a meta-analysis of precognition studies collaborate with a statistician who is unaware of the underlying hypothesis of the meta-analysis. This extra step would eliminate concerns about researcher biases affecting data analysis.

As for recommended next steps in precognition research, we contend that beginning to understand the psychological and physiological mechanisms underlying free-response precognition, implicit precognition, and physiological precognitive effects like presentiment should be the top research priority. Such a research program is especially important in light of the argument that potentially all other purportedly anomalous psychological phenomena, such as telepathy, clairvoyance, and psychokinesis, may be explainable via precognition (Marwaha & May, 2016). Another fruitful area is the examination of potential moderators of precognition such as age, personality type, mood, gender, and belief regarding the phenomenon. We may note that the use of prescreening to find participants skilled at the precognitive task of interest remains an important step for researchers attempting to gain insight into the mechanisms underlying precognition. Assuming that precognition is distributed unevenly in the population, like any other cognitive or physical talent, then any attempt to gain a deeper understanding of the mechanisms underlying these skills without studying participants who exhibit some skill is unlikely to be fruitful (Barušs & Mossbridge, 2016). Following these recommendations, future precognition research should work toward (a) estimating the robustness and repeatability of precognitive effects; (b) gaining a clearer understanding of the relationships, if any, among different types of precognitive tasks; and (c) shedding light on underlying mechanisms. More mechanistically inspired recommendations for future work are offered below (see Summary).

Potential Psychological Mechanisms of Precognition

Based on the experiments discussed above, it appears that nonconscious mental processes are largely responsible for precognition (Stanford, 2015). Historically, scientists and the lay public have resisted the idea that a part of the mind functions in a way that is completely beyond conscious awareness. Subliminal perception is now openly discussed in most psychology texts and courses, but for many decades the idea of nonconscious cognitive processes was regarded as ridiculous (Kihlstrom, Barnhardt, & Tataryn, 1992; Hassin, 2013). Indeed, the idea that nonconscious processing associated with dreaming has any utility has only recently gained scientific respectability after robust evidence showed that dreaming influences learning (e.g., Bob & Louchakova, 2015; Hobson & Pace-Schott, 2002; Karni, Tanne, Rubenstein, Askenasy, & Sagi, 1994; Llewellyn & Hobson, 2015; Stickgold, Hobson, Fosse, & Fosse, 2001). Further, only in the last 15 years have most researchers in psychology and neuroscience acknowledged that nonconscious or implicitly accessed cognitive processes can sometimes assist in recalling memories, making choices, and solving complex problems better than conscious cognitive processes (Bowden, Jung-Beeman, Fleck, & Kounios, 2005; Dijksterhuis, Bos, Nordgren, & van Baaren, 2006; Voss, Baym, & Paller, 2008; Voss, Lucas, & Paller, 2012). In other words, evidence continues to accumulate that our intuitions about the nature, scope, and abilities of our conscious and unconscious awareness are not necessarily accurate.

The frailty of our intuitions becomes especially clear when we are confronted by data suggesting unconscious awareness of future

events. Not only is a persuasive theoretical explanation for precognition unavailable at this point, but we do not even know whether we should be attempting to identify one mechanism or many. Perhaps the time span between the precognition and the event is important; perhaps there is one mechanism for behavioral precognition (like precognitive remote viewing and precognitive dreaming, with time frames on the order of days) and a different mechanism for physiological precognition (like presentiment, with time frames on the order of seconds). Another unresolved question is whether precognition is achieved via accessing information about probable or actual future events, or by events in the future "influencing" or constraining events in the past.

Summary

The full epistemological and ontological consequences of time-reversed influences are not yet clear, but one implication is that the experimental sciences may soon be faced with a troubling dilemma: Time-reversed effects, if they exist, cannot be prevented by any currently known experimental controls. As we have seen in this review, several classes of experiments have demonstrated time-reversed anomalies under tightly controlled protocols. Accordingly, our most cherished epistemologies may be unavoidably influenced by future outcomes. We may take comfort in the likelihood that the magnitudes of these influences are probably small, but in some disciplines, especially domains like the life sciences and experimental psychology in which thousands of variables influence the observed effects, time-reversed effects may fundamentally affect the interpretation of results.

Such speculative implications, of course, can be considered scientific heresies of the first order. But if positive empirical evidence continues to accumulate, especially if the methodological recommendations suggested by ourselves and others are followed, then a time may come when we are forced to think the unthinkable. Indeed, the implications of retrocausation are so remote from engrained ways of thinking that the first reaction to this line of research is that it must be flawed. The second reaction may be horror that it represents a previously unaccepted fact about reality.

To better understand the nature of precognition, we need to study the relationships between nonconscious processes, conscious processes, and how time unfolds in the physical world. Several avenues of inquiry that may lead to greater understanding include examining the circumstances under which nonconscious processes share information with conscious processes about imminent events, determining how a conscious decision to receive information about a future event influences one's ability to accurately perceive that information, collaborating with physicists to study how nonconscious and conscious processes might interact with events as they unfold in time, and examining how time is perceived during alterations in consciousness.

A recent "taxonomy of prospection" delineates four broad categories in which most skills related to prospection seem to fall: simulation, prediction, intention, and planning (Szpunar, Spreng, & Schacter, 2014). Based on the data reviewed here, it seems to us that precognition may eventually be considered just one of several forms of prediction that have evolved to enhance our survival. A handful of neuroscientists, psychologists, and physicists are examining precognition with this idea in mind, and some have published their results (e.g., Bem, 2011; Franklin et al., 2014; Mossbridge, Tressoldi, & Utts, 2012). However, due to a lack of awareness of this line of research in mainstream academia, such efforts are vastly underfunded. That is a pity because the ability to gain information about future events could potentially lead to major advances in both psychological and physical theories, as well as a host of pragmatic applications. We join others (Franklin et al., 2014) in supporting efforts to increase funding for precognition research.

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COMMENTARY

Cross-Examining the Case for Precognition: Comment on Mossbridge and Radin (2018)

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Based on a review and meta-analyses of empirical literature in parapsychology, Mossbridge and Radin (2018) argued for anomalous replicable effects that suggest the possibility of precognitive ability or retrocausal phenomena. However, these conclusions are refuted on statistical and theoretical grounds—the touted effects are neither meaningful, interpretable, nor even convincingly replicable. Moreover, contrary to assertions otherwise, the possibility of authentic retrocausation is discredited by modern theories in physics. Accordingly, Mossbridge and Radin's interpretations are discussed in terms of misattribution biases that serve anxiolytic functions when individuals confront ambiguity, with potential reinforcement from perceptual–personality variables such as paranormal belief. Finally, we argue that research in human consciousness should be multidisciplinary, and notably, leverage informed investigators in the physical sciences to advance truly valid and cumulative theory building.

Keywords: transliminality, precognition, effect size, meta-analysis, physics

> For generalization, psychologists must finally rely, as has been done in all the older sciences, on replication. Cohen (1994, p. 997)

The conceptual foundation above sets the stage to confront Mossbridge and Radin's (2018) case for "precognition" built on a review and synthesis of empirical literature in parapsychology. Anomalistic and clinical psychologists are well acquainted with esoteric experiences of "advance knowing" that can occur in many contexts, for example, waking visions, déjà vu experiences, "crisis apparitions" and intuitive thinking. But beyond issues of phenomenology or epidemiology—and working from a misunderstanding of Albert Einstein's description of time as a "stubbornly persistent illusion (Einstein & Hawking, 2007)"—Mossbridge and Radin (2018) argued that the evidence suggests a replicable anomaly that denotes precognitive ability or retrocausation that is implicitly rooted in *psi*[1]. We credit this *Journal* for its willingness to publish controversial data that can have enormous import in model or theory building in consciousness studies, but skeptical readers scrutinizing the reasoning for those authors' provocative conclusions might regard belief in the paranormal as another stubbornly persistent

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[1] Psi is a collective term used by parapsychologists for extrasensory perception (ESP, or paranormal cognition) and psychokinesis (PK, or paranormal action); see Irwin and Watts (2007). Mossbridge and Radin did not explicitly use this term, but it will be obvious to informed readers that their case for "precognition" is implicitly synonymous with positive evidence for ψ.

illusion—one that evokes the familiar pun by Sam Walton (1992/1993, p. 68), who wrote that ESP stood for "error some place."

Unfortunately, we did indeed find errors all over the place in Mossbridge and Radin's (2018) presentation. These shortcomings rendered the paper fundamentally and fatally flawed, so we would have rejected it as reviewers. There were too many misconceptions or misstatements to address comprehensively in a rebuttal limited to several thousand words; therefore, our present commentary focuses on two major issues that must be clarified in any argument for precognition. In particular, we seek empirical and robust answers to the questions, "Is there a replicable and meaningful signal within the statistical noise?" If so, "How does any such finding square with well-validated physics theory?"

We further note that this commentary, perhaps uniquely, represents a multidisciplinary effort that draws on specific expertise in anomalistic psychology, tests, measurement and statistics, and theoretical physics. This is not mentioned as an appeal to authority, but rather to emphasize that one of our goals here is to encourage investigators to pursue, and grant-giving agencies to prioritize in funding, quantitative or experimental approaches that leverage collaborations across scientific disciplines. In this way, as Lange (2017) discussed at length, we can achieve truly cumulative and valid theory building in consciousness studies, and ultimately throughout all of science.

Statistics and Replication

> Statistical models for data are *never true*. The question of whether a model is true is irrelevant. A more appropriate question is whether we obtain the correct scientific conclusion if we pretend that the process under study behaves according to a particular statistical model. (Zeger, 1991, p. 1064)

The authors' basic arguments in favor of precognition are problematic for statistical reasons, as their presentation in places harks back to times when lower p values (i.e., the probability of incorrectly rejecting the null hypothesis [H_0]) were interpreted as reflecting stronger effects than do larger values. This approach is likely to reinforce in the readers' minds the classic "odds against chance fantasy" (Carver, 1978), which confuses the probability (P) of observing the data (D) given the H_0, i.e., P(D|H_0), with the probability of the H_0 given the data, i.e., P(H_0|D). This is clearly erroneous. Most (standard) statistical tests explicitly assume H_0 to be true (i.e., with *fixed* probability at 1.0 and *not p*, the probability value estimated by the test). As well, P(H_0|D) bears no relation to P(D|H_0): For example, although it is highly likely that a person is dead (D) after being lynched (H_0), it does not follow, that most dead people must have been lynched.

Mossbridge and Radin (2018) reported extremely small, but highly significant, effect sizes across meta-analyses for precognitive dreaming studies, forced-choice conscious precognition tasks, implicit precognition tasks, and presentient studies. These studies, however, have the conceptual flaw that two-sided hypotheses of the form H_0: $\mu = 0$ and H_1: $\mu \neq 0$ were apparently used, and these hypotheses were tested by standard statistical tests. In this context, we first note that it has long been recognized (e.g., Edwards, Lindman, & Savage, 1963) that rejection of such null hypotheses may result from an exaggeration of the evidence for the postulated effect, which occurs because standard statistical tests are consistent only if the H_0 is false. That is, if we increase sample size, we are guaranteed to reject H_0, no mistakes will occur, as any deviation from 0 will eventually be flagged as having a probability less than α (i.e., the preselected Type-I error rate concerning H_0). On the other hand, mistakes *do* occur when H_0 is true because H_0 will be rejected with probability α, and Rouder, Speckman, Sun, Morey, and Iverson (2009) noted that this rejection bias already starts occurring for realistically small sample sizes.

Also, it is no secret to most researchers that psychological experiments are inherently noisy and their results are potentially distorted by many factors, not all of which are random and not all of which can be controlled. Bluntly speaking, the tests and measurements community call this the crap factor to remind us that small effects, regardless of their statistical significance, are best interpreted as artifacts. For instance, should we really expect human behavior to be described precisely by theoretical coin flips when the behavior of real coins is known to deviate from theory (Diaconis, Holmes, & Montgomery, 2007)? We suspect that Mossbridge and Radin might well agree, as they

noted that small experimental effects could reflect ". . . consistent artifacts or methodological errors instead of a genuine effect" (p. 10). It is somewhat surprising that they advocate the use of meta-analysis, as well as propose or endorse interpretations of small experimental effects as viable examples of precognition, retrocausation, or paranormal presentiment.

In our opinion, meta-analysis is ill-advised in the present context, and our objections agree substantially with those already voiced by Wagenmakers, Wetzels, Borsboom, and van der Maas (2011) in a similar context. First, in the best case (i.e., when no included study is distorted by any artifacts), a meta-analysis is just another experiment, and therefore the objection that (standard) statistical tests artificially favor H_1 applies here as well. Second, in reality, all experiments are flawed because of the crap factor referred to earlier. The question arises, therefore, do meta-analyses not simply combine multiple irrelevant findings into a single equally irrelevant pile? Third, note that two-sided (null) hypotheses may be rejected because of opposite empirical findings. That is, provided that the estimated probability $P(D|H_0)$ falls below α, H_0: $\mu = 0$ can be rejected with $M > 0$ as well as with $M < 0$ (where M is a statistic computed over the data, e.g., a mean). But, is it reasonable to say that two experiments showing opposite (and likely artificial) outcomes both support the same kind of precognition? We do not think so.

It is possible to avoid some of the statistical objections listed above by adopting a Baysian approach (Rouder et al., 2009) that takes into account specific H_0 values, so as not to exaggerate the evidence favoring the H_0 (i.e., it is consistent). Simultaneously, it allows researchers to express their preferences for either H_0 or the alternative. This approach will decrease the putative effect size when H_0 is seen as less likely to be true (for examples, see, Wagenmakers et al., 2011). Applied to the research reviewed here, this would further undermine Mossbridge and Radin's (2018) conclusions. In the end, however, their basic logical argument relies on the finding of unlikely data patterns that seemingly support the existence of precognition. We already noted that this very argument is not logically tenable, and that the support for this hypothesis derives from ambiguous data that were gathered in a noisy context, and analyzed using questionable assumptions. We thus agree with Wagenmakers et al. (2011) that "the field of psychology currently uses methodological and statistical strategies that are too weak, too malleable, and offer far too many opportunities for researchers to befuddle themselves and their peers" (p. 426).

Yet, even if Mossbridge and Radin's (2018) statistical arguments were valid, this alone would not establish precognition or retrocausation as a workable theory. As we discuss next, retrocausation disagrees with all established knowledge of the natural world, and to determine whether we should even take this explanation seriously, it is important to know whether the proposed phenomena are theoretically possible.

Perception, Time, and Causality

First, and admittedly it sounds nit-picky, but we balk at the four "everyday intuitions about time" set forth by Mossbridge and Radin (2018). We suspect they meant to say "beliefs" or "assumptions," not "intuitions," as the latter term refers to a very specific, and possibly distinct, psychological phenomenon. Nevertheless, their source for these "straw-man" assumptions is unclear. To assist readers, they are repeated here: (a) "Events in the physical world are mirrored by our almost immediate perceptual recreation of those events in essentially the same order that they occurred;" (b) "The re-creation of physical events, first in perception and later in memory, occurs in a linear order based upon our original perception of events;" (c) "Event A may only be said to 'cause' event B if event A precedes event B;" and (d) "What we remember has occurred in the past."

We are unaware of any evidence that people hold some or all of these beliefs, and the scientific community understands these four premises to be misguided. To be sure, even though lay people often confuse correlation and causation, many can still appreciate and grasp the notion that effects can apparently precede their causes under certain circumstances. In particular, roosters crowing at sunrise is an everyday example of reverse causation, and in other daily contexts, such as economics (e.g., Stock, & Trebbi, 2003; Heckman, 2008), causes and effects are routinely observed to coincide in time.

Furthermore, a multitude of studies, for example, the classic, replicable work of Elizabeth

Loftus (see, e.g., Loftus, Doyle, & Dysert, 2008), upend the above assumptions speaking to sensation, perception and the encoding, retrieval and accuracy of memory in relation to events in the physical world. It is well-known that perception and memory are so malleable that people even vividly remember things that never happened. For example, discussing events can influence memories of the events or increase the rate of false memories of the events, as exemplified in studies involving (a) discussions among romantic partners (French, Garry, & Mori, 2008), (b) conversational encounters at a beach (Carlucci, Kieckhaefer, Schwartz, Villalba, & Wright, 2011), and (c) discussions of a film (Gabbert, Memon, & Wright, 2006). In addition, not only is eyewitness testimony not necessarily reliable, but witnesses can even voice strong confidence in their inaccurate remembrances (Smalarz & Wells, 2015).

Second, and most important, credible information about time and causality needs to be injected into any serious discussion of precognition, because Mossbridge and Radin only cited two questionable sources referencing physics. In particular, the case for precognition began with a misunderstanding of Einstein's comment about the nature of time. They are not alone, as evidenced in a lay article published on *redOrbit* (Savage & Penman, 2007) that used Einstein's same words to help promote the unsubstantiated idea that precognition had increasing acceptance among mainstream academics, and especially informed physicists. But the description of time as a "stubbornly persistent illusion" actually reflects Einstein's view of the universe and its laws as objective and deterministic. What he was referring to here is the impossibility, according to his theory of relativity, of any objective determination of "now," and the lack of a unique, objective distinction between past and future, as these depend on a reference frame (more on this below). For Einstein, reality is what is true, not what is illusory, as exemplified by his further clarification.

> If, then, it is true that the axiomatic basis of theoretical physics cannot be extracted from experience but must be freely invented, can we ever hope to find the right way? Nay, more, has this right way any existence outside illusions? . . . I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it. . . . In a certain sense, therefore, I hold that pure thought can grasp reality, as the ancients dreamed. (Einstein & Seelig 1954, p. 274)

To be clear, nothing about Einstein's expressed views or body of work suggest that he considered retrocausal mechanisms or behaviors as possibilities. Any claim to the contrary betrays an erroneous understanding of modern physics theory.

Of course, some parapsychologists with reported backgrounds in physics have published proposals that seem to be the antithesis of Einstein's thinking. For example, consider Dick Bierman's (2010) thesis grandiloquently titled, "Consciousness-Induced Restoration of Time-Symmetry (CIRTS): a Psychophysical Theoretical Perspective." Upon a review of Bierman's assertions in that paper by the third author (Dan Hooper), a professional physicist, it is difficult to know how to respond to proposals like these. In particular, Bierman's sentences about quantum physics often appear to be poorly informed, and carry little or no actual meaning or substance. We therefore wonder whether "physics-oriented" parapsychologists like Bierman are sufficiently informed on modern theories in physics or are possibly being intentionally disingenuous.

From a physical perspective, there are extremely compelling reasons to discard the possibility of retrocausal phenomena. In contrast to Mossbridge and Radin's (2018) suggestions, physicists do not disregard the possibility of such phenomena on the grounds that they violate their "everyday intuitions" about the nature of reality. Generally speaking, contemporary physicists are very comfortable with, and have accepted, the highly counterintuitive nature of our universe, in particular, within the realms of the subatomic and the cosmological. The physics community has instead rejected retrocausal mechanisms on well-substantiated, empirical, and logical grounds.

In contrast, individuals ignorant of modern physics and its nuances sometimes reference some of the counterintuitive phenomena associated with special relativity and quantum mechanics in an effort to provide conceptual support for precognition, related retrocausal events, or other so-called paranormal phenomena. In special relativity, for example, it is the case that the order in which a series of events occurs can depend on the choice of reference frame. This may seem to suggest that retrocausal behavior is possible, but in fact this is not the case at all. All events that have the potential to influence an-

other (e.g., any event within another's light cone) are strictly causally, although not necessarily deterministically, related. There is simply nothing about special (or general) relativity that allows for retrocausal phenomena (see, e.g., Carroll, 2010).

Quantum mechanics is also often cited in support of retrocausality or other putative paranormal phenomena, but again this connection is misplaced. Quantum entanglement, for instance, has often been referenced in support of retrocausal behavior. In fact, however, entanglement does not in any way enable information to propagate in anything but a causal fashion. There is nothing about quantum mechanics, or its more modern incarnation, quantum field theory, that enables any authentically retrocausal behavior (see, e.g., Bub, 1999).

On more general grounds, any means by which information could be transferred from a future event to a past event would violate the second law of thermodynamics (i.e., the entropy of a large system always increases, or the reason why an egg cannot be unscrambled), allowing, among other things, for the possibility of perpetual motion machines, and leading to considerable and deep logical challenges, such as the class of problems known as the "grandfather paradox." In summary, any explanatory model for precognition, or any retrocausal or paranormal-presentiment-type effects, for that matter, must be reconciled against the overwhelmingly validated theories in modern physics.

Unconscious–Conscious Processing of Sensory Cues

Perhaps the most useful and interesting—but least developed—part of Mossbridge/Radin (2018) presentation was the section on predictive anticipatory activity (PAA), that is, presentiment effects. Setting aside these contrived terms, this is where the construct of intuition (or intuitive thinking) now becomes relevant, as well as the concept of *transliminality,* defined as the "hypothesized tendency for psychological material to cross (*trans*) thresholds (*limines*) into or out of consciousness" (Thalbourne & Houran, 2000, p. 853), which was inexplicably omitted from their references to unconscious–conscious processing of sensory (and other) cues. We submit that any discussions or models related to the issues in Mossbridge and Radin are incomplete without addressing the literature on these two important variables.

Transliminality, a perceptual–personality variable, is measured with the Revised Transliminality Scale (RTS: Lange, Thalbourne, Houran, & Storm, 2000; cf. Houran, Thalbourne, & Lange, 2003), which has considerable literature supporting its content and predictive validities (for reviews, see: Lange & Houran, 2000; Houran, Hughes, Thalbourne, & Delin, 2006). Over all, the available evidence suggests that a variety of highly unusual or anomalous experiences are a function or correlate of heightened transliminality (Cardeña, Lynn, & Krippner, 2014; Lange & Houran, 2000). In particular, its major correlates are *syncretic cognitions* (Houran et al., 2006; Lange & Houran, 2000), that is, the fusion of perceptual qualities in subjective experience such as *physiognomic perception* (fusion of perception and feeling), *synesthesia* (fusion of sensory modalities), and *eidetic imagery* (fusion of imagery and perception). Accordingly, transliminality is presently conceptualized as enhanced interconnectedness between brain hemispheres, as well as among frontal cortical loops, temporal-limbic structures, and primary or secondary sensory areas or sensory association cortices (Houran et al., 2006; Thalbourne, Crawley, & Houran, 2003; Thalbourne, Houran, Alias, & Brugger, 2001). Studies of perception, imagery, and memory all provide evidence for a threshold that mediates unconscious–conscious awareness, and findings from several psychophysiological experiments seem consistent with the neurological interconnectedness model in particular (cf. Crawley, French, & Yesson, 2002; Fleck et al., 2008; Houran et al., 2006).

Transliminality and intuitive thinking are related phenomena (Lange & Houran, 2010)—even to the extent that questionnaire items measuring both constructs can be subsumed within a common Rasch (1960/1980) scale (cf. Houran & Lange, 2010, Appendix, pp. 73–75). These findings suggest to us that presentiment-type effects are extremely complex, conventional phenomena grounded in established neurological mechanisms. In particular, the human brain has evolved over tens of thousands of years into a three-pound, organic computer, arguably with the sole task of "making things more certain." Gaining mastery and a sense of control over our physical and psychological environments is, therefore, a natural, inherent motivation. But beyond the

brain's amazing sophistication, there seems to be something especially remarkable happening in highly transliminal minds. These individuals can appear almost "superhuman" in the permeability and manifestations related to their mental boundaries.

For instance, intuition essentially appears to be an unconscious confluence of affect, imagery, ideation, and perception. Langley, Mintzberg, Pitcher, Posada, and Saint-Macary (1995) concluded that decision-making processes are partially driven by emotion, imagination, and memories all collectively crystallized into occasional insights. Eisenhardt and Zbaracki (1992) echoed this view in their multidimensional approach to decision making encompassing bounded rationality, heuristics, insight, and intuition. Moreover, most researchers acknowledge that (a) intuitive events originate beyond consciousness, (b) information is processed holistically, and (c) intuitive perceptions are frequently accompanied by emotion (Shapiro & Spence, 1997; Sinclair & Ashkanasy, 2005). Accordingly, Sinclair and Ashkanasy (2005) conceptualized intuition as "a nonsequential information processing mode, which comprises both cognitive and affective elements and results in direct knowing without any use of conscious reasoning" (p. 353; cf. Simon, 1987; Epstein, Pacini, Denes-Raj, & Heier, 1996; Shapiro & Spence, 1997).

This definition does not explicitly identify the source of the cognitive and affective contents of intuitions, and there are two competing views on this issue (Boucouvalas, 1997; Shirley & Langan-Fox, 1996). One view defines intuition as an experience-based phenomenon that draws on tacit knowledge accumulated through experience and retrieved through pattern recognition (e.g., Behling & Eckel, 1991; Brockmann & Anthony, 1998; Isenberg, 1984; Klein, 1998; Simon, 1987). The second view is that these experiences follow from a more spontaneous, natural psychophysiological ability that rely heavily on sensory and affective elements in the intuitive process (e.g., Bastick, 1982; Epstein, 1998; Parikh, Neubauer, & Lank, 1994; Petitmengin-Peugeot, 1999).

Sinclair and Ashkanasy (2005) proposed a general model that incorporates both mechanisms simultaneously. It is important to note that intuitive thinking often occurs in situations of significant ambiguity or uncertainty (Burke & Miller, 1999; Isenberg, 1984)—such as situations in which problems are poorly structured (Behling & Eckel, 1991) or involve nonroutine decisions (Simon, 1960), in which problems do not have existing precedents (Parikh et al., 1994), or when an individual is faced with conflicting facts or inadequate information (Agor, 1984). Other contributing factors include motivational issues like the perceived importance of the decision (Goodman, 1993) and its potential impact on the decision-maker (Kriger & Barnes, 1992). Intuitive thinking strongly resembles magical thinking and paranormal belief and experience, which we find interesting, as these phenomena also thrive during situations of marked ambiguity or uncertainty (e.g., Houran, Irwin, & Lange, 2001; Irwin, 1992; Lange & Houran, 1998, 1999, 2000). Moreover, the situational and motivational factors associated with intuitions arguably parallel the well-known experimenter effects documented in the parapsychological literature (e.g., Schmeidler, 1997; Storm & Thalbourne, 2005; Wiseman & Watt, 2002).

There is ample rationale to hypothesize that "flashes of genius," or intuitions about key decisions or future events are examples of transliminality manifesting in everyday contexts. First, the phenomenology of intuitions summarized above strongly parallels the neurological interconnectedness model of transliminality and suggests that intuition is either caused or moderated by transliminality. Second, and consistent with the neurological interconnectedness model, there is preliminary experimental evidence that intuitive processes involve interactions among the frontal, temporal, occipital and parietal brain areas, and perhaps even the cardiovascular system (McCraty, Atkinson, & Bradley, 2004a, 2004b). Finally, Lange and Houran (2010) found a moderate correlation, $r = .38$, $p < .001$, between transliminality and self-reported intuitions in the workplace in a sample of individuals at different management levels. However, self-reported intuitions increased with higher management level, independent of transliminality. These findings are consistent with a two-mechanism model of intuition (cf. Sinclair & Ashkanasy, 2005), whereby transliminality equates to intuitive predisposition subsequently honed or reinforced over time by tacit knowledge that comes from work experience or structured training. In other

words, intuitive ability might build upon transliminality, but then goes beyond it.

Next we discuss some important points of elaboration that were mistakenly excluded in the published version of Houran and Lange (2010). Although previous studies have found that transliminality correlates with characteristics that promote misattributions in reasoning (i.e., memory aberrations, impulsive thought and behavior; Lange & Houran, 2000; Thalbourne et al., 2003; Houran & Thalbourne, 2003), Lange and Houran's (2010; cf. Houran & Lange, 2010) sample showed no evidence consistent with the hypothesis that the workplace intuitions in their study were related to emotional or cognitive biases. Specifically, Houran and Lange (2010) reported that transliminality showed nonsignificant correlations with scores of emotional and cognitive biases, whereas scores on the Intuitive Decision-Making Profile (Andrews, 1999) showed a nonsignificant correlation with overcoming emotional biases and a moderate correlation with scores on overcoming cognitive biases (i.e., higher scores indicate a lack of confirmatory biases).

In other words, there is no evidence that the intuitive experiences analyzed by Lange and Houran (2010; cf. Houran & Lange, 2010) were illusory in nature. Assuming instead that the intuitions under study were accurate in their content and not the result of misattributional processes, this implies that phenomena like transliminality and intuitive thinking can collectively facilitate markedly keen, rich, and accurate anticipatory senses or instincts that might appear practically clairvoyant in nature, à la the famous investor and philanthropist Warren Buffet, nicknamed the "Oracle of Omaha."

It is also instructive to note, in these contemplations about permeability in mental boundaries, the classic anomaly of *déjà vu* (French for "already seen"), which the social and medical sciences have studied intensely for decades. In particular, Brown (2004) reviewed several physiological or psychological explanatory mechanisms that include (a) a brief change in normal neural transmission speed causing a slightly longer separation between identical messages received from two separate pathways, (b) a brief split in a continuous perceptual experience that is caused by distractions (external or internal) and gives the impression of two separate perceptual events, and (c) the activation of implicit familiarity for some portion (or all) of the present experience without an accompanying conscious recollection of the prior encounter.

Similar to intuitive thinking and transliminality, these general hypotheses for déjà vu all relate to unconscious–conscious processing. Investigators have even begun to map specific neurological mechanisms at play, which we find interesting. For example, Bartolomei et al. (2012) found that synchronized neural firing between the rhinal cortices and the hippocampus (involved in memory formation) or amygdala (involved in emotion) were increased during electrical stimulation that induced déjà vu experiences in epileptic patients. This suggests that some sort of coincident occurrence in medial temporal lobe structures may trigger activation of the recollection system. The role of mesiotemporal structures in the pathogenesis of induced déjà vu experiences has been corroborated in other studies as well (e.g., Kovacs et al., 2009).

Psychophysiological phenomena—for example, intuitive thinking, transliminality and déjà vu—that are salient to investigations of PAA and "advance knowing" are still under active investigation. As such, it seems reasonable to us that considerable research is still needed to gain comprehensive models of known perceptual and decision-making mechanisms like these before scientists have reason to speculate about unknown, esoteric ones like paranormal presentiment or precognition.

Discussion

In the court of public opinion, which is traditionally sympathetic to the paranormal (McClenon, 1984), Mossbridge and Radin's (2018) review might be readily accepted as compelling evidence. But scientifically speaking, Mossbridge and Radin's case for precognition or retrocausal phenomena arguably fails on statistical and theoretical grounds. Their information and arguments neither convince us that there are any meaningful, empirical anomalies that defy the ever-present crap factor in measurement and analysis, nor do they present a logical and internally consistent case, much less explanation, for precognitive phenomena. It is tantalizing and entertaining to ponder precognition, retrocasuality, or paranormal presentiment

as legitimate phenomena that reflect some intersection between consciousness and space–time, but a dispassionate analysis of their presentation does not give any scientific impetus to speculate that such phenomena could even theoretically exist. If anything, their review only reinforces to us the tremendous potential and opportunity for consciousness studies in exploring established constructs like transliminality, intuitive thinking, and déjà vu.

As it stands, we propose that principles in clinical and anomalistic psychology help to explain why some people might believe in precognition (or retrocausality) based on the type of studies reviewed by Mossbridge and Radin (2018). Along with other authorities (e.g., Irwin, 1992; Bentall, 1990, 2000), we have equated general paranormal belief and experience to fundamentally adaptive, nonpathological delusions (Houran & Lange, 2004; Lange & Houran, 1998, 1999, 2000, 2001), which in the absence of clear and convincing orthodox explanations, give people a sense of relief or control when facing stressful or ambiguous stimuli (cf. Kossowska, Szwed, Wronka, Czarnek, & Wyczesany, 2016). Part of that work specifically targeted self-reported experiences of precognitive dreams (Houran & Lange, 1998; Lange, Shredl, & Houran, 2000–2001), which we modeled as "meaningful coincidences" facilitated by dream recall, tolerance of ambiguity, and belief in the paranormal, as well as bolstered by misunderstanding the probability of coincidences (cf. Blackmore, 1997; Blackmore & Troscianko, 1985; Brugger, Regard, Landis, Krebs, & Niederberger, 1994). Of course, Mossbridge and Radin presented empirical data from controlled, experimental studies that appear immune to psychological explanations like misattribution biases or unconscious prospection revealed in dream content.

Nevertheless, Mossbridge and Radin's (2018) presentation is a workable case study of cognitive–emotional reaction to ambiguity, as predicted by Lange and Houran's (2000) misattribution model for delusional thinking. At best, their case seems only to show evidence for effects that amount to "replicable ambiguity"—statistical results that do not overcome the crap factor, and even if they did, the meaning and import of the results would still remain ambiguous and dubious in relation to the immense predictive power of modern physics. And yet, in the absence of a *meaningful*, *interpretable*, and *replicable* effect—much less an explanatory model—some people feel compelled or justified to levy speculations of disruptive, paranormal, or retrocausal mechanisms. In other words, Mossbridge and Radin are imposing order and meaning on chaos and ambiguity by using one unknown (precognition, or psi) to explain another unknown (trivial, ambiguous effects). This is the fundamental logical fallacy that Houran, Lynn, and Lange (2017) referred to in their discussion of the self-deceiving nature of otherwise well-meaning investigators who intentionally set out to prove the ontological reality of psi, or at least are comfortable with inferring its existence (or any retrocausal events) simply from a lack of other, readily available explanations.

Mossbridge and Radin (2018) do not overtly claim that their review proves precognition or psi exists; they instead position such hypotheses as rational alternatives that deserve legitimacy and continued study. They acknowledge that "such speculative implications, of course, can be considered scientific heresies of the first order" (p. 29), but immediately pivot with the following assertions.

> . . . If positive empirical evidence continues to accumulate, especially if the methodological recommendations suggested by ourselves and others are followed, then a time may come when we are forced to think the unthinkable. Indeed, the implications of retrocausation are so remote from engrained ways of thinking that the first reaction to this line of research is that it must be flawed. The second reaction may be horror that it represents a previously unaccepted fact about reality.

Their assertions are flatly incorrect, as considerably more would need to occur before the scientific community, and principally informed physicists, are compelled to "think the unthinkable." For instance, Bierman (2010) reasoned the following.

> Since psi phenomena are labeled anomalous because they appear to be in conflict with our present day physical worldview, any fundamental psi theory should be an extension or a modification of physics. Psychology is not in conflict with psi phenomena per se, so although psychological theories . . . are useful when speculating how to optimize effect size, they do not touch upon the apparent anomalous character of psi. (p. 274)

Similarly, we agree that psychology does not a priori conflict with psi experiences, because at

this point it seems to us that the latter derives from the former. Moreover, psychological principles should be able to help parapsychologists pinpoint precise reasons for low effect sizes in studies and then remedy them, assuming we are dealing with a natural ability grounded in established psychophysiology—a topic discussed at length by Houran et al. (2017). But contrary to Bierman's claim, skeptical scientists do not describe psi phenomena as anomalous because they appear to conflict with existing scientific theory. Rather, the moniker anomalous is synonymous with unexplained—it refers to the ambiguity surrounding apparent psi phenomena; they are experiences or outcomes susceptible to a myriad of interpretations and explanations.

So, yes, proving scientifically the case for precognition (retrocausation, or psi in general) is a formidable challenge on several levels: (a) replicable effects must be demonstrated, as per Cohen (1994); (b) the effect sizes should be meaningful, that is, large enough to exceed the crap factor in measurement and analysis; and (c) the effects should be interpretable, that is, they are unambiguous and grounded in a genuine explanatory model that is reconciled with modern physics theory. This third criterion will be the compelling factor in disrupting modern physics theory, or consciousness studies, for that matter. Time will tell whether there will ever be studies that clearly and convincingly satisfy these three criteria, but we submit that Mossbridge and Radin's (2018) case, at least, did not overcome any of them.

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COMMENTARY

On the Plausibility of Scientific Hypotheses: Commentary on Mossbridge and Radin (2018)

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Mossbridge and Radin reviewed psychological and physiological experiments that purportedly show time-reversed effects. I discuss why these claims are not plausible. I conclude that scientists should generally consider the plausibility of the hypotheses they test.

Keywords: plausibility, base rate fallacy, statistics, scientific inference

Science seeks to explain a chaotic world by formulating lawful relationships that permit causal predictions. When I flip a coin, it will land on either heads or tails. The exact outcome depends on a multitude of factors and is difficult to predict—but I am confident that after many flips, the number of heads and tails should be roughly equal. However, I watch as my friend Frank flips a coin, and it keeps landing on heads. Something is obviously amiss.

It is possible that Frank is a wizard. In fact, if you knew Frank, you could be forgiven for thinking that. He looks and acts like a wizard. So, it is theoretically possible that Frank can magically force the coin to land on heads every single time. However, in spite of his arcane aura, this would not be my first hypothesis.

My first step would probably be to inspect the coin. Perhaps it shows heads on both sides? If that is not the case, I could give Frank another coin, one I know to be fair. If this one also lands on heads all the time, my next guess will be that Frank uses some kind of trick. Perhaps he is throwing the coin in a particular way that ensures it will land on heads? I would carefully watch how he flips the coin to see if I can spot anything unusual. I could compare his coin flipping movements to those of other people. I could enlist the help of modern technology and record his coin flips with a high-speed camera and play them back in slow motion. When I get desperate to find a rational explanation, I might analyze magnetic fields and air pressure.

How long should I search for an explanation until I conclude that Frank is a wizard? Whatever the explanation for Frank's uncanny coin flipping ability, calling him a wizard is essentially admitting defeat. All we really know is that he can get coins to always land on heads. That is an interesting observation, but it is not an explanation.

The hypothesis that Frank is a wizard is very *implausible*. I do not know any other wizards. I may have watched or read about some fictional wizards, and I know magicians who can perform elaborate magic tricks. But to the best of my knowledge, I have never witnessed the casting of any actual magical spells. I also have no idea how magic could physically work. I accept that I do not know everything about the universe—but I choose to go with what I *do* know. Therefore, my prior belief in the existence of magic, and in Frank being a wizard, remains extremely weak. Whatever his coin flipping abilities, it cannot convince me that he is a wizard. I want more conclusive evidence than that. For instance, if lots of wizards suddenly revealed they are capable of similar feats, the interpretation that Frank is one of them would seem far more likely.

It is the same with the scientific study of precognition. In the current issue of this journal, Mossbridge and Radin (2018; henceforth,

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M&R) reviewed studies testing the hypothesis that future events can influence the past. This includes experiments on precognitive dreams, lab experiments in which participants correctly guess events before they occurred, and so-called presentiment effects, physiological responses that manifest only *before* emotional stimuli. I will not address all the points they raised. I previously commented (Schwarzkopf, 2014) on their earlier meta-analysis on presentiment (Mossbridge, Tressoldi, & Utts, 2012). In my view, M&R failed to address my earlier concerns, in particular with regards to expectation bias or randomization procedures, but I do not want to dwell on those smaller points. Instead, I only want to discuss one fundamental issue: Are presentiment or precognition effects *plausible*?

I previously raised the concern that the time-reversed physiological responses to emotional stimuli reviewed by M&R are not *biologically* plausible. In conventional thinking, an emotional stimulus generates neuronal responses within the first few hundred milliseconds after it is shown to the participant. Slower physiological responses, such as pupil dilation, galvanic skin responses, and changes in neural blood flow then follow this neuronal response.

According to M&R, presentiment effects show similar differences in the response latency, but they are time reversed: Electrophysiological responses are reported to occur hundreds of milliseconds *prior* to the stimulus, while galvanic skin responses or hemodynamic changes occur even several seconds *earlier* (Mossbridge et al., 2012). Does this mean that all such events are mirrored back in time relative to stimulus onset (Bierman, 2010)? Does blood flow increase because several seconds *later* neurons in the brain will fire, and in turn, they fire because even several hundred milliseconds *later* an emotional stimulus will appear? The main reason for hemodynamic responses in the brain is thought to be the metabolic demand caused by increased neuronal firing. Therefore, should these retrocausal electrophysiological responses not also *cause* hemodynamic consequences? If presentiment existed, the response to any stimulus would be a constant swamp of causal and retrocausal effects as well as their nonlinear interactions.

Similarly, it is implausible that participants can guess trials in a two-alternative forced-choice task correctly at a rate of 51–53%, the rate Daryl Bem's (2011) precognition experiments reported. I am not a betting man, but if this were true, I would start a coin-flip betting business. Even with such a miniscule winning margin, this would nevertheless soon turn a pretty healthy profit (see Figure 1). Even if we accept ideas about quantum entanglement or other subatomic time reversals as possible explanations, such effects should be *tiny*. Either Bem somehow amplified his participants' natural precognitive ability by several orders of magnitude, or his findings were the result of methodological flexibility and/or experimental artifacts instead. The latter is a far more plausible hypothesis.

In their review, M&R casually dismissed my earlier concerns with the plausibility of time-reversed phenomena like presentiment and precognition. According to them, the fact that lab experiments found such effects directly demonstrates that they *are* plausible. This is a circular argument. A statistically significant observation does not prove that a hypothesis is true. The plausibility of a hypothesis depends on whether an observation is consistent with our current understanding of the world. I have little reason to believe that Frank is a wizard other than the

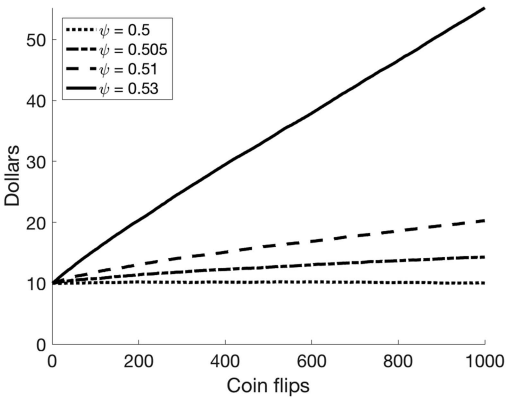

Figure 1. Simulated universes in which typical reported precognition effects exist. I start with $10. For every coin flip, I bet $1 that I can guess the outcome. If I guess correctly, I get back $2 and thus win $1. If I fail, I lose my bet. I keep flipping the coin 1,000 times or until I run out of money. The curves show the amount of money I have, plotted against the number of coin flips (averaged across 10,000 simulations). Different line styles denote different "natural precognition rates." Without any precognition ($\psi = 0.5$), I would not win any money. However, even with tiny precognition effects ($0.5 < \psi < 0.54$), I would turn a profit.

fact that his coin flipping is unusual and that he kind of looks like one. I have no reason to believe that precognition is possible but for some anecdotes and a handful of parapsychology experiments with effect sizes that are really small—but at the same time, far too large to be theoretically feasible.

M&R's argument is known as the base rate fallacy: No matter how strong the statistical evidence, if the hypothesis is impossible, it must necessarily be false. The *p* value is irrelevant when the observed effect size cannot be observed under the alternative hypothesis. I cannot confidently claim that precognition and presentiment are *impossible*. I simply do not know enough about the universe to know this for certain. I am, however, extremely skeptical that such retro-ausal effects exist. Critically, even if I accept that such effects are at least possible, the rate at which they can be observed in noisy psychology or physiology experiments must be nanoscopic, many orders of magnitude below those reported by these studies. The reported effects are not plausible under this hypothesis, and thus, alternative explanations are far more likely.

Therefore, I must disagree with M&R that we are dealing here with "scientific heresies of the first order." Rather, this statement betrays a fundamental misunderstanding: There are no heresies in science. Dogma is antithetical to science and any assumption can be challenged. Critically, however, nobody should take you seriously without compelling evidence. Frank may very well be a wizard, but unless you show me more conclusive evidence that wizards actually exist, I remain doubtful. I am skeptical that precognition is even possible, but I certainly will not be convinced of its existence by some implausible observations, no matter how significant the meta-analysis.

What evidence for precognition would I find compelling? The experimental test must be highly sensitive (much larger sample sizes and low-noise measurements) and provide rigorous control for methodological flexibility like p-hacking. In that regard, I applaud M&R's call for preregistered replications of these effects. Preregistration provides a clear delineation of the confirmatory and exploratory aspects of a study. Statistical significance is only meaningful for the former. I would go one step further and suggest that such replications should be Registered Reports (https://cos.io/rr), a format enjoying increasing popularity in several psychology journals including the recently launched *Nature Human Behavior*. Here, the methods are refined in an initial stage of peer-review and data collection only commences when the methods have been finalized. However, even that does not control adequately for some of the problems that could skew the findings. To ensure that the results are convincing even to skeptics, the experiment should be conducted as an *adversarial collaboration*, where skeptics and proponents of precognition effects work together to ensure the experiment is conducted in a way they both agree with. While such collaborations do not always end the disagreement between parties, both sides are given a chance to interpret the results—and the readers can make up their own mind about which hypothesis the evidence supports.

If all these steps have been taken and precognition findings nonetheless replicate in a set of homogenous replications, I will accept that there is a result worthy of an explanation. However, even such a finding still does not mean that precognition exists. If the effect size is similar to Bem's reports of 51–53% correct, then it is simply not *plausible* that this occurs in the general population and everyday situations. At most, this would imply that precognition can only be demonstrated in these particular experimental contexts, which seems rather unlikely. The onus then is on proponents of the precognition hypothesis to show experimentally what makes this effect so unstable. If they cannot do so, methodological artifacts or other uncontrolled flexibility remain a more plausible alternative explanation.

In general, the burden of proof must always lie with the one making a claim. Therefore, it falls on proponents of a novel hypothesis to provide compelling evidence for it. Moreover, a fundamental principle of scientific research is that a hypothesis should be *falsifiable*. Before setting out to test a new hypothesis, investigators should always ask themselves what evidence could *disprove* this hypothesis. If they cannot answer this question, the hypothesis is probably not worth testing. To my knowledge, proponents of precognition have yet to provide an answer to this question.

But let me be clear: My problem with the research on precognition is not with its fringe nature. Instead it is with the approach and the interpretation of these findings. This is not a problem limited to parapsychology but it plagues a lot of scientific research. The precognition effects reported in these studies are not plausible but neither are claims that unscrambling words related to old age can make participants walk down a corridor a second more slowly than controls (Bargh, Chen, & Burrows, 1996). Given the messy nature of human behavior, it seems very unlikely that a simple psychology experiment can have such a profound effect. It should therefore not surprise anyone when such findings fail to replicate (Doyen, Klein, Pichon, & Cleeremans, 2012). The same principle must apply to reports of gravitational waves, discoveries of arsenic microbes, brain-behavior correlations, and even simple psychophysical tests of visual perception.

We can all do a lot better. We should put our hypotheses to much greater scrutiny. If you observe an effect, you must ask whether it is plausible under the hypothesis you are testing. Extraordinary claims require extraordinary evidence. And always ask yourself what would convince you that you are wrong. Mossbridge and Radin (2018) clearly challenge our current science—just not in the way they seem to think.

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REPLY

Plausibility, Statistical Interpretations, Physical Mechanisms and a New Outlook: Response to Commentaries on a Precognition Review

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We address what we consider to be the main points of disagreement by showing that (a) scientific plausibility (or lack thereof) is a weak argument in the face of empirical data, (b) the statistical methods we used were sound according to at least one of several possible statistical positions, and (c) the potential physical mechanisms underlying precognition could include quantum biological phenomena. We close with a discussion of what we believe is an unfortunate but currently dominant tendency to focus on reducing Type-I statistical errors without balancing that approach by also paying attention to the potential for Type-II errors.

Keywords: precognition, retrocausality, statistical arguments, plausibility

We thank James Houran, Rense Lange, Dan Hooper, and Samuel Schwarzkopf for their commentaries on our review of experimental evidence for precognition (Schwarzkopf, 2018). Science advances not only when novel data are observed in rigorous experiments, but also as a result of serious debates about the interpretation of those data. Here we respond to the two commentaries by briefly stating our own positions on what we believe to be the major points of disagreement. We are grateful to the journal editors and the commentators for the opportunity to clarify these points.

Plausibility and the Scientific Venture

Both commentaries raise the question of the plausibility of our interpretation of results from experiments testing precognition and related effects. Schwarzkopf's commentary (Schwarzkopf, 2018) is almost entirely focused on this issue, so we first respond to his points here.

Schwarzkopf stated, "The plausibility of a hypothesis depends on whether an observation is consistent with our current understanding of the world. . . . No matter how strong the statistical evidence, if the hypothesis is impossible, it must necessarily be false (Schwarzkopf, 2018, p. 95)." This argument is invalid because it is circular. If our current understanding of the world is inexact, which is a core assumption in science, then we cannot be sure that any hypothesis is impossible. Thankfully, Schwarzkopf is aware of this problem, so he immediately followed that statement with this one: "I cannot confidently claim that precognition or presentiment are impossible. I simply do not know enough about the universe to know this for certain. I am however extremely skeptical that such retro-causal effects exist (Schwarzkopf, 2018, p. 96)."

Schwarzkopf's skepticism is understandable. Precognition challenges the commonsense notion that cause precedes effect. Our scientific skepticism remains intact as well, except unlike Schwarzkopf, we have the benefit of repeatedly observing reversals of the usual cause–effect sequence in our own laboratory studies. We invite Schwarzkopf and other scientifically

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minded skeptics to recall their first time reading about the results of the delayed-choice quantum-eraser experiment (Kim, Yu, Kulik, Shih, & Scully, 2000). That experiment provided highly repeatable data for a retrocausal phenomenon at the quantum scale. Because it seems to violently disagree with everyday experience, some scientists continue to struggle with that interpretation and they offer different interpretations of the data. The same is true, we believe, for the data from precognition experiments.

Schwarzkopf went on to say, "Critically, even if I accept that such effects are at least possible, the rate at which they can be observed in noisy psychology or physiology experiments must be nanoscopic, many orders of magnitude below those reported by these studies (Schwarzkopf, 2018, p. 96)." Schwarzkopf's reasoning is not clear. No reference or theoretical arguments are provided that would allow us to judge this claim, unless he is implying an effect operating at the quantum scale. In any case, we remind the reader that the supreme achievement of science has been to repeatedly disprove our cherished assumptions; for a recent summary of some seemingly impossible phenomena in the world of physics, consider the cover story in *New Scientist* from April 2016, in which the author states, "There's no shortage of hints that our current theories don't provide a full picture of reality" (Brooks, 2016, p. 28). It is abundantly clear that this is also the case for the fields of psychology and physiology.

Schwarzkopf continued, "If you observe an effect, you must ask whether it is plausible under the hypothesis you are testing (Schwarzkopf, 2018, p. 97)." This suggests that if a dataset challenges one's prior conceptions, then one should question the validity of the data. We agree. But if a methodologically sound experiment provides data that support a clearly stated hypothesis, even one that violates one's prior beliefs, then what? Reject the anomalous data because existing theory must be true? In our view such a position is antiscientific in that, if followed to its logical limits, Schwarzkopf's approach would collapse today's scientific worldview into unassailable dogma.

We have previously countered Schwarzkopf's methodological and analytical concerns about presentiment experiments (Mossbridge et al., 2015), and we and other investigators performing similar experiments have also countered other critiques (e.g., Bem, Utts, & Johnson, 2011; Dalkvist, Mossbridge, & Westerlund, 2014; Mossbridge et al., 2014; Radin, 2004; Utts, 1996). Independently repeatable empirical results continue to support the precognition hypothesis, so in principle, the weight of the accumulating evidence ought to eventually overcome skeptical objections. Unfortunately, the history of scientific discoveries clearly documents that when and even whether that happens depends more on sociopolitical factors, idiosyncratic temperaments, and maintenance of the status quo, rather than a neutral assessment of data (Kuhn, 1970).

We agree with Schwarzkopf (2018) that, even if rigorous meta-analyses continue to support the precognition hypothesis, that does not necessarily prove that some overlooked methodological artifact cannot explain the effect. But we would point out that over the past 40 years, there have been repeated attempts to find such artifacts, and when potential loopholes were identified, they were closed and the phenomena continued to be observed. It is thanks to critiques like Schwarzkopf's that this line of research has continuously tightened its methods and controls, and in the process, has introduced important methodological advancements, including the use of meta-analysis, study preregistration, and awareness about selective reporting and multiple analyses. It is always possible that other artifacts may someday be discovered that will explain what seems like precognition to instead be a result of a mundane, but probably subtle, mistake. Our best guess at this point is that this will not happen. But time will tell (pun intended).

Ultimately, critiques about the plausibility of precognition seem to rest far less on the available evidence, and far more on what is deemed to be already understood about the nature of consciousness and its role in the physical world. Assigning plausibility based on commonsense is obviously unsatisfactory, but so are assumptions based on the existing scientific worldview. That worldview, a hodge-podge collection of sometimes contradictory theories and data, continues to evolve, and it does not take a crystal ball to predict that future science will contain many surprises.

Is There Something Wrong With the Statistics?

We find Houran, Lange, and Hooper's (2018) concerns with the statistical results cited in our review partly reasonable and partly unreason-

able. First, they raised the concern that our interpretation of *p* values was likely to reinforce the idea that *p* values reflect the probability of the null hypothesis given the data. They are correct. The way we discussed some of our interpretations of *p* values might have reinforced this erroneous idea. Nevertheless, that interpretation does not change the *p* values themselves. Their second concern was stated as follows.

> These studies, however, have the conceptual flaw that two-sided hypotheses of the form H_0: $\mu = 0$ and H_1: $\mu \neq 0$ were apparently used, and these hypotheses were tested by standard statistical tests. In this context, we first note that it has long been recognized . . . that rejection of such null hypotheses may result from an exaggeration of the evidence for the postulated effect. This occurs because standard statistical tests are consistent only if the H_0 is false. (Houran et al., 2018, p. 99)

Yes, but the point of a well-conducted meta-analysis is to determine whether the effect sizes recorded in replications are in the *same direction*. They continued, "But, is it reasonable to say that two experiments showing opposite (and likely artificial) outcomes both support the same kind of precognition? We don't think so (Houran et al., 2018, p. 100)." And neither do we. That is why the meta-analyses applied to the precognition and presentiment experiments we discussed tested a *one-sided hypothesis*. That is, these meta-analyses could only end up with a significant outcome if the replications on average *produced outcomes in the same direction*.

Their third concern was about what these commentators call a "crap factor," that is, noise in the data, and how that crap factor can be magnified by meta-analyses. There are several opinions on this topic among statisticians, and Houran et al. (2018) cite only one side of the ongoing debate. It is obvious that noise exists in all measurements, especially in the biological and social sciences. One of the most straightforward ways to see through the noise is to conduct a conservative meta-analysis. Otherwise, there is no way to judge if an effect has been independently replicated. And if effect sizes need to be of a certain magnitude to be taken seriously, then what is the appropriate threshold? Taking an aspirin each day is said to reduce the risk of a heart attack; the estimated effect size is a mere 0.03 (Rosnow & Rosenthal, 2003). And yet, aspirin is regularly prescribed for preventing heart attacks. By contrast, the meta-analytical estimate of the effect size for presentiment effects is nearly an order of magnitude larger (0.21; Mossbridge, Tressoldi, & Utts, 2012). Is that large enough? We suppose it depends on one's Bayesian priors about the plausibility of precognition.

Houran et al. (2018) did suggest Bayesian analyses, but they failed to point out that Bayesian analyses are unavoidably influenced by subjective bias—the setting of at least one of the two necessary priors, which is set according to the researcher's beliefs. An article discussing problems of multiple analyses pointed out that Bayesian analyses increase researchers' degrees of freedom by giving them the decision to set at least one prior (Simmons, Nelson, & Simonsohn, 2011). Because this degree of freedom is, by definition, tied to a researcher's prior beliefs, it is not clear how Bayesian methods can prevent a researcher from discovering something that she did not already expect to find, or not find something she preferred to avoid. Despite such real concerns, in our paper we turned to the medical Bayesian-analysis literature to assess if precognition effects reported by Daryl Bem were large enough to be of practical importance. The answer was clearly *yes*.

Overall, we take issue with the final comment on statistics made by Houran et al.

> In the end, however, their basic logical argument relies on the finding of unlikely data patterns that seemingly support the existence of precognition. . . . [This] very argument is not logically tenable, and that the support for this hypothesis derives from ambiguous data that were gathered in a noisy context, and analyzed using questionable assumptions. (Houran et al., 2018, p. 100)

This criticism is invalid because it implies that there is something unique to this line of research, when in reality the same criticism could be applied to any experiment in any domain. We are all striving to improve our methods, and we suggested several such improvements (e.g., preregistration, preregistered meta-analytic methods, and prospective meta-analyses). Schwarzkopf suggested an additional approach in his commentary, namely, collaborations between precognition researchers and skeptics. That might sound reasonable, but actually it perpetuates a false distinction. It wrongly implies that scientists who report successful precognition experiments are not prop-

erly skeptical. For example, a scientist with no prior involvement in precognition studies, like Daryl Bem, conducts several experiments and reports positive evidence. Skeptics then label him a "proponent," or worse, a "believer," and then he is no longer trusted as being sufficiently skeptical. For the rare skeptics who actually conduct an experiment and obtain positive evidence, they either have to accept that they will no longer be considered skeptical, or they must feel obligated to dismiss their own evidence based on the ever-convenient crap factor (e.g., Delgado-Romero & Howard, 2005, who, when faced with their own evidence supporting Ψ abilities, called it a "crud" factor).

Later in the Houran et al. (2018) commentary, we were presented with a review of the literature of transliminality and intuition. Although these phenomena are intriguing and possibly relevant to precognition, we did not comment on them within the limited space allotted to us in our original review. However, we find their discussion in the commentary more than a little peculiar in light of their concerns about the mysterious crap factor. Indeed, research on transliminality and intuition are perfect exemplars for the very problems they had already decried. And yet now the reader is expected to take *these* topics seriously?

They ended this part of their critique with the statement, "Considerable research is still needed in order to gain comprehensive models of known perceptual and decision-making mechanisms . . . before scientists have reason to speculate about unknown, esoteric ones like paranormal presentiment or precognition (Houran et al., 2018, p. 104)." It seems to us that if scientists felt compelled to wait until everything *known* were already understood, progress would come to a grinding halt. It is not only reasonable to use rigorous methods to ask well-formed questions of experiences suggestive of precognition and other commonly reported, if exotic, experiences, we feel it is absolutely *essential*. Intuition, transliminality, and déjà vu are experiences suggesting that something about our ordinary perception of time may not be correct. What better way to probe that unknown than by experimentally tackling our basic assumptions about the nature of time head-on about the nature of time?

Finally, suggesting an odd lack of familiarity with the literature on intuitive thinking, Houran et al. stated that, "Consistent with neurological [sic] interconnectedness model, there is preliminary experimental evidence that intuitive processes involve interactions among the frontal, temporal, occipital and parietal brain areas, and perhaps even the cardiovascular system (Houran et al., 2018, p. 102; McCraty, Atkinson, & Bradley, 2004a, 2004b)." The studies they cited explicitly provide evidence of presentiment and are noted as close replications of previously reported presentiment experiments (e.g., Radin, 1997). In fact, the key methodological factor in those studies was that the upcoming stimulus was selected at random, thus unknown at the time of the response. Even a cursory glance at the methods and the accompanying task-timeline figures in those citations (Figure 2 in 2004a, Figure 1 in 2004b) show that the period during which physiological preresponses are measured comes before the time at which the software selects the stimulus. This oversight suggests that Houran et al. (2018) have not differentiated between methods used to test for unconscious decision making or unconscious intuition and those used to test for precognition. If this is indeed the case, then Houran et al.'s concerns about our interpretations of the precognition data would make more sense. Of course, if any sensory information about an upcoming event is available at the time a response is measured, it is perfectly reasonable to assume that the results are not due to a result of precognition. Clearly, it is only by using methods that explicitly rule out such "sensory leakage" about the future event, as was the case in our reported experiments, that one can begin to think about precognition as a viable explanation. This apparent misunderstanding of the methodological details of precognition experiments perhaps explains Houran et al.'s concerns about our interpretations of the data.

Physical Mechanisms and Causality

We agree with Houran et al.'s (2018) comments that physical mechanisms and a discussion of causality should be discussed in a review about the possibility of precognition. We included that material in our original submission, but we were asked to remove it by an editor who felt that the physics potentially underlying precognition did not appear to be within our areas of expertise. We are grateful to have the oppor-

tunity here to include that discussion in its entirety.

> A common criticism of the experiments reviewed here is that reversed time violates common-sense assumptions about causality, and thus any positive effects found in such studies are impossible and can only be understood as flukes or flaws. Such complaints may seem reasonable, but science has repeatedly demonstrated that common-sense assumptions do not apply to the world beyond the reach of the ordinary senses. For example, Einstein demonstrated that matter, energy, space and time are not the separate entities suggested by commonsense, but rather they are intertwined relationships. Likewise, quantum theory tells us that quanta (i.e., elementary particles) do not have definite properties when no one is looking, at least not in the way we understand either "properties" or "looking" in common-sense terms.
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> But perhaps one of the most self-evident concepts questioned by modern science is the nature of causality. This topic has generated more uneasiness among scientists and philosophers than is commonly appreciated. As Bertrand Russell put it in 1913, "All philosophers imagine that causation is one of the fundamental axioms of science, yet oddly enough, in advanced sciences, the word 'cause' never occurs. . . . The law of causality, I believe, is a relic of bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm." (cited in Pearl, 2000, p. 337)
>
> Or as mathematician John von Neumann wrote in 1955, "We may say that there is at present no occasion and no reason to speak of causality in nature—because no [macroscopic] experiment indicates its presence . . . and ∵ quantum mechanics contradicts it." (cited in Rosen, 1999, p. 88)
>
> With such cautions in mind, it is worth noting that, within physics, it is well-known that on a quantum scale, present events can be influenced by future events, a clear violation of the commonsense expectation that cause must always precede effect. As described by physicist Brian Greene, "By any classical-common-sense-reckoning, that's, well, crazy. Of course, that's the point: classical reckoning is the wrong kind of reckoning to use in a quantum universe." (cited in Aharonov & Zubairy, 2005, p. 875)
>
> This retrocausal effect, first proposed as a thought experiment by Wheeler (1978), has been experimentally demonstrated to high degrees of confidence in physics labs around the world (Aharonov & Zubairy, 2005; Jacques et al., 2007; Peruzzo, Shadbolt, Brunner, Popescu, & O'Brien, 2012). A critic might respond by saying that time reversal might exist at microscopic levels, but that it is irrelevant for understanding precognition because the special, fragile state of quantum coherence—which is required to sustain these strange effects—is rapidly washed out within the hot, wet environment of the brain. This was the prevailing view for many years. But today, with rapid theoretical and experimental advancements in quantum biology (e.g., Vattay, Kauffman, & Niiranen, 2014), there are now cogent reasons to suspect that living systems, including the human brain, have very likely taken advantage of quantum effects in nontrivial ways, including "harnessing quantum coherence on physiologically important timescales" (Lambert et al., 2013, p. 10). In addition, with new evidence indicating that individual neurons are associated with memory, learning, and stimulus novelty (Rutishauser et al., 2015), it appears to be increasingly likely that quantum-level effects, which are present in neuronal synapses, may in fact influence the brain.
>
> These speculations do not fully explain retrocausal effects in human conscious experience, but they do strongly counter proposals that such effects are prohibited by known physics. Thus, until the "quantum brain" is better understood, the most prudent proposal we can offer is that a conceivable physical mechanism for precognition may be on the horizon. Whether a fleshed-out model based on this idea will lead to falsifiable theories will require further research.

From the above, it is clear that we disagree with the Houran et al.'s (2018) assertion that "The physics community has instead rejected retrocausal mechanisms on well-substantiated, empirical and logical grounds," or that, "There is nothing about quantum mechanics, or its more modern incarnation, quantum field theory, than enables any authentically retrocausal behavior (Houran et al., 2018, pp. 101–102)." As to the concern about Einstein, at no point did we assert that he championed retrocausality, nor that general relativity supports retrocausality. We simply quoted his "stubbornly persistent illusion" phrase because it emphasized that assessing the plausibility of any phenomenon must always take into account assumptions which may or may not be true.

Finally, we would like to point out that Houran et al.'s (2018) description of the second law of thermodynamics is questionable on two grounds. First, their reliance on the second law is odd because it contradicts their opening gambit, which quoted Zeger (1991), namely, "Statistical models for data are *never true* (p. 1064)." The second law of thermodynamics is, of course, a statistical model, thus, by their logic, complete reliance on its infallibility is questionable (Boltzmann, 1974/1886). Second, their interpretation of the second law is incorrect. They state, "On more general grounds, any means by which information could be transferred from a future event to a past event would violate the Second Law of Thermodynamics. . . (Houran et al., 2018, p. 102)." In fact, that statement is only true for a *closed system* (Spa-

kovszky, 2017). Here we are dealing with living organisms, that is, *open systems*. The second law does not apply to open systems, including humans, birds, and worms, the three living systems we discussed in our precognition review. Further, recent empirical results reveal a reversal of the arrow of time as a result of using a closed system consisting of particles with quantum correlations, which trade their correlations for a decrease in entropy, suggesting that the second law must be modified to take information correlations into account (Micadei et al., 2017).

The Crux of the Disagreements

The strength of today's scientific worldview rests upon theories that accurately explain observations, which, in turn, rest upon a host of experimental methodologies. Those very same methodologies were used in the experiments under discussion, and they reveal what appear to be precognitive effects. Thus, it is invalid to argue that precognition is implausible, especially on methodological grounds, because doing so would necessarily have to raise red flags about the very foundations of the scientific worldview. Thus, we suspect that plausibility arguments against precognition are really based on a commonsense or everyday view of reality, and not the worldview actually revealed by science, which abounds with counterintuitive discoveries.

It seems to us that the commentators have been primarily concerned about making a Type-I error, (i.e., accidentally declaring something to be real that isn't). All researchers are taught to be careful about finding meaning in data when meaning isn't there. To guard against this possibility, we advocated for further research using even more rigorous approaches than those already in place. At the same time, we trust it is clear that overconcern about Type-I error carries the risk of not seeing something in data that is in fact real. Concern about Type-II errors is less commonly emphasized, but it is just as serious a problem as Type I, because it reduces the rate at which genuine discoveries can be made. Another more serious risk of overlooking the possibility that precognition is real is that we may fail to make use of potentially life-saving applications. A speculative example that is not without precedent is that precognitive remote viewers could potentially determine the whereabouts or timing of imminent terrorist events using precognitive means (May & Marwaha, 2014). Even if a much less dramatic application of precognition could save lives on a smaller scale, it would be a pity to fail to explore such applications because of overconcerns about Type-I error. In summary, we feel it is likely that better balance between concerns about Type-I and Type-II errors will lead to a higher rate of useful discoveries without losing essential scientific rigor.

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