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ARE DIFFERENT STANDARDS WARRANTED TO EVALUATE PSI?[1]

By George Williams

ABSTRACT: Throughout the debate on psi, skeptics have almost universally insisted on different standards for evaluating the evidence, claiming that psi represents a radical departure from our current scientific understanding. Thus, there is considerable ambiguity about what standard of evaluation psi must meet. Little attention has been paid to the possible harm to the integrity of scientific investigation from this resulting inconsistency in testing standards. Some have proposed using a Bayesian framework as an improvement on this dilemma in order to more explicitly model beliefs, assumptions, and background scientific knowledge, especially when evaluating a controversial hypothesis. Recently, Kuhn's notion of paradigms, which constrains scientific research within boundaries believed to be most productive, has been incorporated into a Bayesian framework. Within this framework, I explore a likely paradigm or meta-theory used by skeptics that typically constrains research and makes it difficult for psi evidence to be accepted. It appears that such a paradigm would in many respects have difficulty accounting for consciousness, which is fundamental to an understanding of psi. I discuss why psi data are likely to play a key role in making progress in solving the problem of consciousness. Thus, applying different standards of evaluation to psi data is likely counterproductive.

Keywords: psi debate, testing standards, Bayesian, Kuhn, paradigm

Psi remains highly controversial. This umbrella term, which includes telepathy, clairvoyance, precognition, and mind-matter interaction remains a taboo subject in most quarters of academia. Nevertheless, a growing body of empirical literature, including meta-analyses in some cases, appears to support some modes of psi (Radin, 1997, 2006; Utts, 1996). These anomalous findings could be important in shedding light on the mystery of consciousness, as well as other aspects of reality. However, these results have not yet triggered substantially new inquiry or acceptance from more mainstream researchers in psychology or stirred much interest in the popular press.

Psi has been greeted with considerable skepticism, if not ridicule, throughout its history. However, evidence has continued to accumulate, perhaps shifting the debate to some (small) degree. Some skeptics have acknowledged that at least by the standards used to assess more conventional claims, evidence supports some modes of psi. But in general, psi skeptics maintain that the unconventional nature of psi requires different measures for assessment. For example, Wiseman, a well-known skeptic, has recently stated in an interview, "I agree that by the standards of any other area of science that remote viewing is proven, but begs the question: do we need higher standards of evidence when we study the paranormal? I think so" (Penman, 2008).

Perhaps relatively few see anything controversial about assessing psi evidence differently from more conventional claims. The maxim "extraordinary claims require extraordinary evidence" and Randi's million dollar challenge may have encouraged many to think that standard methods of statistical testing are insufficient to assess unconventional behavior such as psi. On the other hand, arguments that disparage using standard methods in the light of surprising or counter intuitive data could be viewed as attempts to "move the goal posts" in ways that compromise the integrity of scientific investigation.

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This debate raises interesting questions and problems for areas of research that are unconventional, surprising, or counter to our preconceived assumptions. Under what circumstances should we assess results differently? Given unusual or unconventional data, how should standards for assessing the evidence change? Also, in a world where scientists posit multiple parallel universes or sentient computers (with little or no evidence), what exactly do we mean by "extraordinary claims"? And what sort of "extraordinary evidence" should be required?

The focus of this paper is on whether additional standards for evaluating laboratory generated psi phenomena are warranted. I'll begin with a brief survey of psi research, with a primary focus on telepathy. This literature includes meta-analyses across several different research methods: forced-choice card guessing, dream telepathy, and the ganzfeld method. I focus on telepathy in order to analyze the strongest data in the least space. However, I should note that many have argued that parapsychological experiments cannot separate modes of psi such as telepathy and clairvoyance. Thus, my use of the word "telepathy" reflects the term best associated with the research method I discuss rather than a strict interpretation that precludes the possibility of clairvoyance or precognition. In this discussion, I will also focus on the arguments against accepting this data, paying close attention to how current arguments offered by psi skeptics have a tendency to require that we apply a different standard than we do for more conventional research cases.

I next consider the various arguments that appear to discourage wider acceptance of psi. The common thread behind most of these arguments is that telepathy (as well as other types of psi) conflicts with the consensus set of facts obtained by scientific means. Therefore, the reasoning usually goes, different standards should apply. While many (including perhaps some psi advocates) might agree, there is very little discussion in this debate on the possible problems that arise, with the resulting ambiguity of what appropriate standard to apply. For example, one might wonder how more widespread investigation might emerge to investigate psi, even if such phenomena were real, under conditions where the standards of evaluation are ambiguous.

On the one hand, as I will discuss below, unstable standards for evaluating evidence may impair our capacity for genuine scientific inquiry. On the other hand, as Kuhn (1962) has argued, stable scientific research must to some degree be constrained from investigating anomalies excessively. Kuhn persuasively argued that as paradigms constrain investigation, they allow research to be more organized and in some ways more effective. Of course, ignoring some routes of investigation or anomalies may have its cost; however, accumulation of anomalies may ultimately lead to a shift in the paradigm that allows research that sees anomalies as necessary data. Below, I follow Henderson, Goodman, Tenenbaum, and Woodward (2010) in incorporating Kuhn's notion of a paradigm within a Bayesian framework in order to capture this element of using different standards within the psi debate.

Within such a framework, I consider what paradigms, or what features within a paradigm, might currently constrain "normal" science away from such topics as psi. Drawing on various arguments and statements from skeptics, I attempt to articulate such a guiding principle for research that would exclude psi. I then consider how such a guiding principal or statement serves us in our inquiry into another area that remains mysterious: consciousness.

Consciousness, because of its intimate relationship with psi, is obviously important. My brief survey of the literature on consciousness suggests that it very much remains a mystery. The problem of explaining consciousness, especially what Chalmers (1995) has termed the hard problem, appears at this point formidable. A wide range of mainstream researchers concede at this time that no solution to explain subjective experience is on the horizon. Perhaps most important of all, there is nothing in the current laws of physics, chemistry, and biology that remotely hints at how consciousness emerges from collections of nonconscious particles. Given that consciousness is fundamental to any understanding of psi, I'll discuss the implications of dismissing or using a separate set of standards to judge psi data.

History and Evidence

In the early 1930s, J. B. Rhine began to explore telepathy and other forms of psi through a forced-choice method of card guessing, where participants draw from a deck containing a fixed number of symbols

(usually five). Rhine was a pioneer, not only in conducting such experiments under laboratory conditions, but also in using statistics to test for the presence of psi. Critics attempted numerous lines of attack, which included citing statistical errors, sensory cues, recording errors, and file drawer type problems. Honorton (1975) reports that, despite the uneven nature of his test conditions, Rhine's results demonstrated an astronomically significant psi effect (p. 105). Honorton also notes that while non-Duke laboratories did not quite match the replication rate of Rhine and his colleagues at Duke, there was no statistical difference. The results were persuasive to some but ultimately failed to convince the field of psychology that psi is real. While Rhine was able to answer his critics regarding experimental design and statistical methodology, some argued that replication of Rhine's results remained a significant issue (Crumbaugh, 1969).

Nevertheless, even at this relatively early stage, psi critics acknowledged that by normal standards, Rhine's results appeared to be conclusive. Consider this statement by Hebb (1951, p. 45): "Rhine has offered enough evidence to have convinced us on almost any other issue." However, he continues:

> Personally, I do not accept ESP for a moment, because it does not make sense. My external criteria, both of physics and physiology, say that ESP is not a fact despite the behavioral evidence that has been reported. I cannot see what other basis my colleagues have for rejecting it . . . Rhine may still turn out to be right, improbable as I think that is, and my own rejection of his view is—in the literal sense—prejudice.

Price (1955) also cites the impressive amount of evidence presented by Rhine:

> Believers in psychic phenomena . . . appear to have won a decisive victory and virtually silenced opposition. . . . This victory is the results of careful experimentation and intelligent argumentation. . . . Dozens of experimenters have obtained positive results in ESP experiments, and the mathematical procedures have been approved by leading statisticians. . . . Against all this evidence, almost the only defense remaining to the skeptical scientist is ignorance. (p. 359)

However, Price proceeds to dismiss all this evidence as "incompatible with current scientific theory," based primarily on Hume's argument against miracles:

> If, then, parapsychology and modern science are incompatible, why not reject parapsychology? . . . The choice is between believing in something "truly revolutionary" and "radically contradictory to contemporary thought" and believing in the occurrence of fraud and self-delusion. Which is more reasonable? (p. 361)

Nevertheless, a parallel investigation into telepathy was explored in dreams. From 1966 to 1972, Ullman and Krippner conducted a total of 450 dream-telepathy sessions at the Maimonides Medical Center in Brooklyn, NY. A meta-analysis conducted by Radin (1997) found an overall success rate of 63% (where chance was 50%), with odds at 1 in 75 million that the results could be attributable to chance. Sherwood and Roe (2003) examined 21 dream telepathy studies published between 1977 and 2002 and compared them with the Maimonides studies. They found significant results overall, however, with smaller effect sizes that they attributed to slightly different methods and protocols. These differences included using homes rather than a facility such as Maimonides and eschewing EEG monitoring. Overall, the authors concluded that a small or modest dream telepathy effect appears to be robust across a wide range of laboratories and variations in methods.

These results from dream telepathy, as well as other significant results associated with hypnosis (Schechter, 1984) and meditation (Braud & Braud, 1973, 1974; Dukhan & Rao, 1973) have suggested that putting participants into a sensitive state of awareness could be more conducive to anomalous transmissions. In addition, experience with forced-choice methods suggested that the repetitive nature of the experiment tired participants after a period of time, leading to declines in the hit rate. Also, it was believed that freeing

the allowable choices would result in images with greater emotional resonance, which might also lead to greater efficacy. The ganzfeld method was an attempt to employ a "noise reduction" model that suggested sensory deprivation might heighten areas of consciousness more sensitive to psi. The ganzfeld method was developed as a mild altered state by inhibiting external stimulation; ping-pong balls were placed over the eyes and soft static noise filled the ears.

The ganzfeld is also important for studying telepathy because many details of the procedure were influenced by the collaboration between Charles Honorton, a psi advocate, and Ray Hyman, a skeptic. The two debated the overall results of 28 ganzfeld studies, with Hyman (1985) arguing that all significant findings could be accounted for by flaws in the methodology, and Honorton (1985) finding strongly significant results ($p < 10^{-9}$), robust in consideration of study quality. Shortly after their exchange, Hyman and Honorton (1986) agreed to collaborate and generate a set of experimental protocols designed to alleviate the potential flaws identified in the previous collection of studies. In their joint paper, they argued:

> [T]he best way to resolve the controversy between us is to await the outcome of future ganzfeld psi experiments. These experiments, ideally, will be carried out in such a way as to circumvent the file-drawer problem, problems of multiple analysis, and the various defects in randomization, statistical application, and documentation pointed out by Hyman. If a variety of parapsychologists and other investigators continue to obtain significant results under these conditions, then the existence of a genuine communications anomaly will have been demonstrated. (pp. 353–354)

A few years later Honorton and his colleagues began to conduct new ganzfeld studies based on the new protocols established in the joint communiqué. Hyman (1991) appeared to acknowledge that confirmation of psi might soon come after all:

> Honorton's experiments have produced intriguing results. If … independent laboratories can produce similar results with the same relationships and with the same attention to rigorous methodology, then parapsychology may indeed have captured its elusive quarry. (p. 392)

Toward that end, Bem and Honorton (1994) conducted a meta-analysis of ganzfeld studies that used the new protocol and found an overall hit rate of 32.20%, significantly above the 25% expected by chance, with a p value of .002. Utts (1996) reported three other studies that replicated the results of Bem and Honorton, finding comparable hit rates. Milton and Wiseman (1999) challenged these results with a follow up meta-analysis of 30 more recent ganzfeld studies and concluded that these studies did not provide significant effects. Bem, Palmer, and Broughton (2001) then found that when 10 new studies were added to the database, the overall test results were significant, although with a lower average effect size than their original meta-analysis. Bem et al. also found that the smaller effect size could be accounted for by some ganzfeld studies deviating from the protocols stated in their original meta-analysis.

Most recently, Tressoldi, Storm, and Radin (2010) examined all the ganzfeld evidence reported in 108 publications from studies conducted during the years 1974 through 2008 by laboratories in six countries. Subsets of this evidence have been analyzed in six meta-analyses, including a meta-analysis by skeptics Milton and Wiseman (1999). Hit rates that exceeded chance with statistical significance were found in each study. The overall hit rate across all of the data was 31.50%, above the chance expectation of 25%, with a p value of 1.0×10^{-11}. Tressoldi, Storm, and Radin report that the "overall results now provide unambiguous evidence for an independently repeatable ESP effect." Overall, the ganzfeld results appear to robustly demonstrate support for the telepathy hypothesis.

However, Hyman ceased any effort at joint collaboration. He not only backtracked from his earlier claim that the ganzfeld data was showing signs of hope but appeared to cast doubt on the value of statistical evaluation altogether: "No other science, so far as I know, would draw conclusions about the existence of phenomena solely on the basis of statistical findings" (Hyman, 1996, p. 48). This claim of course would be strongly challenged by most biologists and psychologists. More recently, Hyman (2010a) hedged on the

value of meta-analysis for psi: "meta-analysis is basically an exploratory rather than a confirmatory procedure. . . . The parapsychologists who try to justify the replicability of psi results with meta-analysis are using a retrospective notion" (p. 44).

Hyman represents an especially interesting case in the history of this debate. Apparently, the protocols that he and Honorton developed to ensure a strong footing for ganzfeld research failed to suffice for him. Obviously, one is always free to reassesses one's opinions about methodology and evidence for a large number of reasons. However, this case highlights a couple of important problems for both psi critics and advocates: (a) how can progress be made if the criteria of evaluation are ambiguous or unstable? and (b) how do psi advocates know whether tests or the standards for evaluation offered by critics are made in good faith?

Nevertheless, the overall evidence for at least telepathy appears compelling. The ganzfeld literature evolved from the earlier modes of card guessing and dream telepathy as a way to explore the conjecture of how psi might be more conducive to more sensitive, altered states. The ganzfeld research then yielded further insights, such as the importance of emotional bonds between participants. Perhaps most important, meta-analysis appears to support the existence of some modest degree of telepathy across three different modalities (forced-choice card guessing, dream telepathy, and ganzfeld) in diverse laboratories and environmental conditions.

Should Different Rules Apply?

Overall, the evidence suggests that a serious look is warranted as to what explanations could account for this anomalous phenomenon. However, it's far from clear that this evidence has motivated many in academia to take such possibilities seriously. There is also little reporting and discussion in mass media and popular science magazines. In short, despite statistical significance in meta-analyses across a number of different methodologies, various categories of psi such as telepathy appear to be nowhere close to achieving mainstream respectability.

There is perhaps an institutional bias against this subject matter, where no matter what the evidence might be, many or most will prefer to disbelieve it. A good example of this is a quote from Professor von Helmholtz, according to Sir W. F. Barrett (1904) in an S.P.R. Presidential Address, on the question of telepathy:

> Neither the testimony of all the Fellows of the Royal Society, nor even the evidence of my own senses would lead me to believe in the transmission of thought from one person to another independently of the recognized channels of sense. It is clearly impossible. (p. 323)

Discerning the degree to which those who would be willing to forgo any evidence and instead cling to their prior beliefs on this matter may be difficult, due to the inherently unscientific (albeit honest) character of such a statement.

I will instead focus here simply on various arguments presented by skeptics. Once questions of statistical bias and file drawer biases have been dealt with, some continued along the lines already noted from Price (1955) and Hansel (1966): that psi is extremely unlikely, based on our existing understanding of science. Thus, experiments confirming psi must be connected with massive cases of fraud or error. However, such arguments have been rarely accompanied by evidence of massive fraud or error. For example, while Hansel (1966) reported no evidence of error or fraud, he nevertheless argued that psi should still be dismissed because of the possibility of error and fraud. Utts (1999) notes that many skeptics would agree with the sentiment, "If there is any potential explanation other than psychic functioning, no matter how remote, it should be accepted" (p. 636). As she notes, such a criterion would eliminate the findings of most scientific endeavors.

One recurring and popular argument among skeptics is that psi is not replicable (Alcock, 2010). The persistence of such claims would appear to be at odds with the meta-analyses cited earlier. Sometimes this appears to result from questionable use of the word "replicable." Consider for example this recent ar-

gument from Hyman (2010a): "Replicability [in the case of a meta-analysis] implies the ability to predict successfully from the results of a meta-analysis to a new set of independent data. This is where parapsychology evidence falls woefully short" (p. 44). This claim appears to ignore the evolution of ganzfeld research, where meta-analyses of autoganzfeld studies, using protocols Hyman helped establish, confirm effect sizes from the earlier studies.

Utts (1999) has argued that the debate regarding replicability has suffered from insufficient attention to statistical power. A phenomenon that is relatively weak or has a small effect size requires substantial statistical power. Braude (1979) notes that psi, if it exists, is likely to depend on a host of subtle factors such as the mood of participants and the demeanor of the experimenter. Thus, studies that use a relatively small sample size and find a nonsignificant statistical effect should not necessarily be counted as evidence against psi. Statistical power can generally be improved through meta-analysis, which combines data from numerous studies. Utts (1999) demonstrates this by comparing the ganzfeld meta-analysis with a meta-analysis for the effects of antiplatelets (such as aspirin) on vascular disease (such as heart attacks and strokes). She notes that both meta-analyses included some individual studies that were not statistically significant. However, in each case combining all the studies leads to overall statistical significance with 95% confidence.

Perhaps there has been some shift in the debate accompanying the accumulation of meta-analyses on the ganzfeld method. Honorton (1993) argues that while critics such as Hyman have not been won over, they agree that a statistically significant effect exists that cannot be explained by error. In the quote cited earlier (Penman, 2008), Wiseman acknowledges that by normal standards we would conclude a real psi effect, but he hastens to add that higher standards should apply to such a controversial effect. The authors of another evaluation of the ganzfeld evidence, including the psi critic Christopher French, conclude: "In summary, on balance the ganzfeld meta-analyses seem to support a psi effect and by extension the hypothesis that reported paranormal experiences may sometimes be veridical, rather than based on cognitive deficits" (Holt, Simmonds-Moore, Luke, & French, 2012, p. 111). In another source, however, French argues that he is not inclined to accept such results because this would require a "radical revision of our existing scientific world" (French, 2010, p. 58). He does argue that in light of this, the results should be taken more seriously if the effects sizes are larger and some practical applications could be developed (French, 2010, p. 59).

Kennedy (2013) argues that meta-analysis may not resolve debates on "controversial" issues such as parapsychology. Because of the many choices and decisions that have to be made when combining studies, Kennedy argues, meta-analysis remains a form of post hoc analysis and is unsuited to resolving "controversial" questions. For such subjects as psi, Kennedy proposes more stringent practices such as substantially increasing the sample size so that sufficient statistical power is obtained to avoid meta-analysis altogether. This practice has obvious virtues. Of course, researchers that follow this approach might be criticized for not utilizing data across different laboratories and experimenters, which meta-analyses typically do. Another relevant factor here is that parapsychologists typically have fewer resources than researchers in more conventional fields. In this case, insisting that psi research meets higher standards that require substantially more resources has the practical effect of raising the bar against achieving wider acceptance. Kennedy and others might argue that this is a price that must be paid for "controversial" areas of research such as psi.

The history of this debate has shown evolving lines of criticism accompanying the accumulated empirical results. In the presence of empirical results across various modes of telepathy yielding astronomically low p values, the common response is the declaration that psi evidence cannot be treated with the same standards of testing and evaluation as more conventional subjects. Such criticisms are perhaps different manifestations of the well-known maxim "extraordinary claims require extraordinary evidence." Of course, terms such as "extraordinary" and "controversial" are never defined in such a way that can advance our ability to evaluate the evidence. Adding to the confusion, perhaps, is that mainstream science entertains some theories that are at least arguably substantially more radical than psi. Examples that come to mind include Everett's many-world (or multi-universe) theory of quantum mechanics, claims that artificial intelligence will soon generate consciousness, and epiphenomenal theories of consciousness that exclude free will. Further, some of these claims come unaccompanied by evidence. This state of affairs must be

perplexing to psi researchers who can provide evidence for some categories of psi that meets conventional standards.

Arguments that move the bar from standard norms have also included proposals that accepting psi must be accompanied by larger effect sizes or practical applications of some kind (French, 2010; Kennedy, 2013). Although no doubt such findings would win psi more advocates, this may be another way that the burden of proof for psi is raised relative to other sorts of scientific claims. Usually, the question of whether there is a real effect is separate from the question of practical applications or effect size. In scientific development, practical applications generally follow at some point in time after a phenomenon is accepted as legitimate. For examples, we might cite applications within global positioning systems for general relativity and quantum tunneling for quantum mechanics. In this light, bundling together these two questions will likely hinder the deeper understanding that is required to develop practical applications.

Creating ambiguities in the standards for acceptance is not without costs. Widely accepted testing conventions are necessary for scientific progress. However, the current situation likely leads many researchers, especially those outside of the field of parapsychology, to question whether psi evidence is substantial at all. Such ambiguities might also discourage investigations into psi altogether; pursuing irresolvable questions unlikely provides a boost to one's career.

A psi skeptic might reply that that the costs of incorrectly accepting the evidence on telepathy or some other psi category are unusually high. It is unclear, however, that this exceeds the cost of incorrectly rejecting psi. Perhaps our current lack of understanding for any application makes the cost of this error appear small. On the other hand, acceptance of the evidence at hand might encourage research that would eventually lead to useful applications. In any case, given the astronomically low *p* values for the meta-analyses reviewed above, it is worrisome how little consideration skeptics typically give to type II errors. Naturally, legitimate criticisms on methods for evaluating psi need to be encouraged just as in any other subject under scrutiny. My main point, however, is that shifting the standard of evaluation or creating ambiguity about the appropriate standard has substantial costs that are rarely considered.

Arguments used by skeptics against treating the results of psi experiments like conventional ones usually claim that phenomena such as telepathy and clairvoyance are radical with respect to our currently accepted scientific understanding. As I have noted, some have argued further that the facts and theories accumulated from centuries of scientific investigation are completely inconsistent with psi. It is, of course, inadvisable to accept experimental results at face value that conflict with much more extensive data across many other areas. Recently such arguments have been incorporated within Bayesian methods, which might provide a framework for capturing the background knowledge and underlying assumptions that perhaps guide researchers away from seriously considering psi.

Kuhn's Paradigm Within a Bayesian Framework

Bayesian methods provide tools for statistical evaluation while also incorporating priors reflecting other sources of information that may conflict with the new data. Bayes' rule captures this feature and is generally expressed as:

$$p(H_1 \mid D) = \frac{p(D \mid H_1)p(H_1)}{p(D \mid H_0)p(H_0) + p(D \mid H_1)p(H_1)} \qquad (1)$$

Here, $p(H_1 \mid D)$ represents the posterior probability of hypothesis H_1, or the probability assigned to H_1 after taking into account a new piece of evidence or data, D. The expression $p(H_1)$ is the prior probability of H_1, the probability it is assigned before taking D into account. The likelihood of data D, if hypothesis H_1 is true, is expressed as $p(D \mid H_1)$. Of course, there are corresponding expressions for the null hypothesis, H_0, as well.

Using Bayes' rule, weights are generated and applied to the evidence based on stated assumptions

and beliefs that are captured in the prior and likelihood. However, pitfalls may occur if care isn't given to these distributions. Bayesian methods have been criticized for allowing excessive subjectivity in the role of selecting priors, without sufficient grounding in data and background information. This was recently illustrated in a debate triggered by Bem's (2011) presentation of results concerning precognition (a different mode of psi from what has been discussed so far). Wagenmakers, Wetzels, Borsboom, and van der Maas (2011) criticized Bem's results by using a Bayesian framework. Bem, Johnson, and Utts (2011) responded that Wagenmakers et al.'s statistics could be attributable to an excessively low prior and a posterior distribution that exaggerated the effect size. Bem et al. (2011) found results consistent with those reported by Bem (2011) within a Bayesian framework using priors and posterior distributions consistent with values found in the psychology and parapsychology literature.

Palmer (2011) has explored the problematic nature of Bayesian analysis. He notes that because there are no ground rules for establishing the prior probabilities, subjective and perhaps arbitrary arguments often play a significant role. Palmer also argues that biases can emerge simply as a result of a given sequence of experimental findings and theory development. He illustrates this by noting how a Bayesian might deal with a particular psychological effect that has been subject to a "decline effect" (Lehrer, 2010). In such a case, a theory that is initially supported by evidence that eventually diminishes over time can nevertheless be insulated from refutation using a prior based on the earlier findings. This opens the door to a sort of circular reasoning that provides "an excellent way for psychologists to protect their pet theories from refutation" (Palmer, 2011, p.180).

Perhaps a more common way that Bayesian frameworks can support circular reasoning is through the underlying beliefs and metaphysical assumptions employed within priors. While various metaphysical assumptions are generally required to ground any theory, they are of course generally not testable. Arguably, they are perceived to be successful—not through explicit testing—but via their relationship with theories that have achieved some degree of empirical success. However, what happens when some metaphysical assumption or guiding research principle, useful for a certain class of theories, is incorporated in a prior for a very different area of inquiry? Thus, a Bayesian framework can help ensure that any evidence that conflicts with previously "successful" assumptions or underlying beliefs will go unchallenged.

These concerns raise the question of whether Bayesian frameworks can be useful at all for resolving debates that are rooted in fundamental differences in assumptions and worldviews, with one or both sides simply confirming their initial prior, no matter the data. The exchange between Wagenmakers et al. (2011) and Bem et al. (2011) indeed illustrates a clash in fundamental beliefs that characterize most debates on psi. Bem et al. do not state beliefs that might constrain what results are reasonable, but they do argue that the effect size of precognition (should it exist) is likely within a range of other known psychological parameters. They also cite other evidence of precognition. Wagenmakers et al. state: "Psi has no clear grounding in known biological or physical mechanisms" and "lack . . . a plausible mechanistic account" (p. 2). Many psi advocates might agree with these statements in Wagenmakers et al.; however, they usually appear open to the possibility that some forms of psi exist even if we currently have no well-understood mechanism to account for various forms of it.

Thus a Bayesian framework may be inherently problematic for resolving issues involving deeply rooted assumptions and differing worldviews. However, an advantage of a Bayesian framework is that it does offer a formal framework for making such differing fundamental views more explicit. Despite its limitations, perhaps a Bayesian framework can help us clarify the clashing views between the two sides.

Toward this end, we might also incorporate Thomas Kuhn's notions of how paradigms guide and constrain research within science. For Kuhn (1962), a paradigm consists of a collection of key theories, methods of testing, values, and metaphysical assumptions that together guide and constrain what he terms "normal science." Kuhn argues that research on anomalies or experimental findings (and their theories) that do not fit into the current paradigm are generally not encouraged. He also believed that most scientific work could be characterized as "puzzle solving" and that true revolutions within science, where one paradigm replaces another, are rare. For Kuhn, this institutional feature has the advantage of encouraging coordination, organization, and efficiency across a large number of scientists. However, anomalies sometimes do

accumulate sufficiently that the paradigm of that time becomes untenable. Yet Kuhn isn't clear on exactly how this transition occurs.

Recently, Henderson, Goodman, Tenenbaum, and Woodward (2010) have developed a framework incorporating Kuhn's notion of the structure of evolution of scientific theories within a Bayesian context. They capture within this framework the hierarchical structure of Kuhn's ideas: higher-level theories or paradigms frame the inquiry and evaluation of lower-level theories. What we've considered previously as background knowledge and metaphysical assumptions can be replaced by Kuhn's notion of a paradigm or higher-level theories. Henderson et al. (2010) explore a hierarchical framework in which lower level theories are generated from higher level theories. Of course, for the case at hand our current mainstream paradigm or set of high-level theories discourages the acceptance of psi evidence, which most would consider outside the range of what Kuhn terms "normal science."

Using a simplified version of Henderson et al. (2010), we might rewrite (1) incorporating Kuhn's notion of paradigm:

$$p(H_1 \mid D,T) = \frac{p(D \mid H_1)\, p(H_1 \mid T)}{p(D \mid T)} \qquad (2)$$

In this version of Bayes' rule, T represents a paradigm or collection of higher-level theories (which also include underlying assumptions and background knowledge) that constrains research to remain within what Kuhn calls "normal science." Thus the prior $p(H_1|T)$, depending on paradigm T, may be sufficiently low to prevent hypothesis H_1 from being confirmed, no matter the data D, as long as the restrictive paradigm prevails in the scientist's judgment. This corresponds well with psi skeptics' unwillingness to accept a meta-analysis that would ordinarily be sufficient for more conventional research questions. These skeptics will either dismiss the data or demand higher standards. On the other hand, researchers more sympathetic to the possible existence of psi will not incorporate such a restrictive paradigm into their prior and will be more willing to let the data confirm the hypothesis at hand.

Next, we might consider what beliefs, metaphysical assumptions, or principles on method might lead us to dismiss or heavily discount psi evidence. We can certainly note that in a number of ways psi phenomena do depart from some well-established principles in physics. For the psi research we reviewed in the first section of this paper, these include the lack of attenuation over distances and the ability to penetrate electromagnetic shielding. Broad (1949) composed a longer list of ways that psi appears to clash with fundamental principles, which he terms "basic limiting principles."

Other statements in this debate provide some key points. Both Alcock (2010) and Hyman (2010a) compare the anomalies of psi unfavorably with those found in astronomy, such as the precession of Mercury and the perceived orbit of Uranus, both of which led to new discoveries. As we noted earlier, Wagenmakers (2011) argues that "there are no mechanisms" for psi phenomena. Elsewhere, Hyman (2010b) argues that "a materialistic framework is necessary to do scientific work" (p. 134).

Alcock (2010) argues in general that psi simply doesn't jibe with other areas of science. He claims:

> There is nothing in physics or neurology that would allow for processes such as extrasensory perception or psychokinesis or other putative paranormal phenomenon. This is unlike any area in normal science: biochemical knowledge does not violate the basic principles of physics; chemistry and genetics do not produce grossly conflicting results; biological data is in line with all three of these sciences; geology and astronomy work happily together. While there may be disputes between disciplines at the leading edge of scientific discovery, the diverse areas of science basically jibe with one another. (p. 39)

Alcock paraphrases neuropsychologist Donald Hebb as follows: "…were parapsychology's claims to prove

to be true, then there is something horribly and fundamentally wrong in physics and in biology and in neuroscience" (p. 39).

Palmer (2011) notes that arguments that claim psi doesn't fit with our current scientific understanding imply a degree of unification between scientific theories that does not exist. Quantum physics, for example, is radically different from classical physics in a number of respects. Interpretations of quantum mechanics involving multiple universes or superposition of possible states hardly square with the rest of science. Also, we currently have no theory that unifies gravity with quantum mechanics. Therefore, perhaps psi requires laws or theoretical structures that are different from those of other phenomenon.

We might note that Alcock here avoids mentioning areas of science that remain mysterious to us. Two examples include consciousness and quantum mechanics. The argument made in Wagenmakers et al. (2011) that there are no known mechanisms for psi also applies to the measurement problem in quantum mechanics, which has to do with how the wave function "collapses" into the observed results of a quantum experiment. I'll address this issue of persistent gaps in our understanding later, focusing on the problem of consciousness, which is obviously fundamental to psi.

Based on these and similar claims, I'll formulate a statement about a guiding principle (within a paradigm that constrains our current "normal science") that I believe will represent most or all skeptics' thinking. Such an overarching meta-theory would govern research and evaluation throughout scientific enquiry. One example, based on the quotes above, could be: proper scientific theories must be consistent with currently well-understood laws of physics. The problem of course here is that as scientific understanding expands, what seems improbable today may be perfectly comprehensible in time. Indeed, psi researchers generally believe that their debate with skeptics corresponds with the dividing line between what we "know" and what we don't.

Nevertheless, perhaps something like this is close to a core guideline for most conventional research. The influential philosophers of science Ladyman, Ross, Spurrett, and Collier (2009), while acknowledging that science (and therefore metaphysics) is never complete, nevertheless argue: "our most careful science at time t . . . is the best metaphysics we can have at time t" (p. 2). The core argument of these authors is that the only kind of metaphysics that can contribute to our understanding of the objective reality is one based on contemporary science as it really is rather than on philosophers' a priori intuitions or common sense. I believe that this kind of attempt to naturalize metaphysics leads to an overarching meta-theory about science in general that most skeptics would agree with.

Another challenge, however, as I've expressed it, is the phrase "well-understood physics." Although the central equations behind quantum mechanics are well established, they are not well understood. Currently there is no consensus interpretation concerning the measurement problem within quantum mechanics. Further, some of these interpretations involve consciousness or observer dependence in ways that are controversial. Most physicists do not currently embrace such arguments. Thus, many skeptics frown upon explanations of psi that invoke quantum mechanics.

With this in mind, I will therefore propose that the following meta-theory or paradigm guideline be employed in skeptics' prior for evaluating scientific theories, which I label T_C: *Proper scientific theories that describe macroscopic events (i.e. outside of subatomic physics) should be consistent with currently well-understood laws of classical physics*. I believe this is consistent with the beliefs of most skeptics, and the arguments of Alcock (2010), Hyman (2010b), and Wagenmakers et al. (2011).

A skeptic's prior of the hypothesis that a psi effect exists, $H\psi$, using T_C can be expressed simply as $p\,(H\psi|T_C)$. This prior incorporates a paradigm that, for all lower-level theories (outside of quantum physics), assigns a low probability to evidence that is not consistent with well-known laws of classical physics. This of course fits well with such fields as chemistry, biology, and neuroscience, which are with rare exceptions based on classical physics. Although theories in the social sciences, such as economics, are not "hard" in the sense that they are not fundamentally based in physics or chemistry, they too are not expected to clash with classical physics. Thus, our modified version of Bayes' rule can be rewritten using our constraining paradigm, T_C.

$$p(H_\psi \mid D, T_C) = \frac{p(D \mid H_\psi)\, p(H_\psi \mid T_C)}{p(D \mid T_C)} \qquad (3)$$

As long as a scientist believes that psi is inconsistent with well known laws of classical physics, a scientist will maintain a low probability for prior p ($H\psi \mid T_C$), no matter the data D. However, a psi advocate will instead follow a version of (3) where T_C is absent. Despite whatever prior the psi researcher chooses, evaluation of the data will not be constrained so that only explanations that can be linked to well-established laws of physics are accepted. Thus (3) provides a formal expression to help us pinpoint the clash in world-views between psi advocates and skeptics.

Of course, there may be exceptions where some scientists pursue an explanation of psi that is consistent with classical physics. Others may attempt an explanation using quantum mechanics (Shoup, 2015). My expression (3) for Bayes' rule incorporating a paradigm ruling out psi will not apply to these groups of researchers. However, I believe (3) likely captures the views of most skeptics and mainstream scientists.

Psi advocates might question the appropriateness of T_C as an appropriate governing principle for scientific inquiry, especially given the accumulated evidence. As I noted, such an assumption for appropriate research would obviously not be adequate for subatomic physics, and this suggests an important challenge to arguments by skeptics. Palmer (1987) discusses the inherently inductive nature of arguments typically used to attack psi: theoretical frameworks and assumptions that have been found to be useful in shedding light on previous areas of science are assumed to be valid for future inquiries as well. However, borrowing from the parlance of Ladyman et al. (2009), we might ask how well the metaphysics obtained at time t for science at time t serve us for a very different area of inquiry at time t + 1? The history of science has abundant cases where theories and claims that were once useful had to be set aside or modified in order to make progress. Thus we entail some risk if we insist that future theories for areas of inquiry we don't presently understand must have the same features as present theories for areas that we do.

Palmer (2011) notes that given that a "theory of everything" remains elusive for physicists, the characteristics of our current set of theories may not necessarily dictate the features of things about which we remain ignorant. We might thus inquire how T_C fares against areas that have so far remained stubbornly mysterious. I believe two areas especially stand out: quantum mechanics and consciousness. Both of these areas overlap significantly with the psi literature. With respect to quantum mechanics, for example, there exists a class of mind-matter interaction experiments that utilizes random number generators based in quantum mechanics. However, in the interest of conserving space (and partly for this reason I have avoided discussion of mind-matter interaction modes of psi) I will focus on consciousness, which obviously is closely linked to psi phenomena. I will then discuss whether a paradigm that insists on consistency with classical physics can be applied to research on consciousness.

The Mystery of Consciousness

With this framework, one might see the debate between psi advocates and skeptics as one between those who argue that consciousness must be based on well-understood laws of physics versus those who believe we may need to look beyond such theories. As background knowledge, we can certainly place a great deal of confidence in the laws of physics, especially classical physics. And in a number of respects, this branch of physics does indeed appear to clash with findings of psi.

However, is confidence that consciousness can be explained completely by classical physics warranted? Today many neuroscientists and philosophers of mind would answer no. Although neuroscience has made great strides in understanding how we perceive, learn, and store memory, how our inner subjective states emerge from the moist matter in our brains remains a mystery. A wide range of scientists and philosophers acknowledge that we are not remotely close to solving what Chalmers (1995) has termed the hard problem of consciousness. Steve Pinker describes consciousness or sentience as "a riddle wrapped in a mystery inside an enigma" (Pinker, 1997, p. 60). Many materialists likely share Searle's (1994) view that

consciousness is likely to be an emergent property of certain kinds of biological processes. However, Searle acknowledges that we are very far from understanding this potential process. Nagel (1974) is much more pessimistic that physical explanations can ever capture the inherent subjectivity, or what he calls the "what it is like to be" of a conscious organism. Chalmers (1995) argues materialistic explanations of consciousness are consistent with zombies that look and behave like us yet are devoid of inner experience. Chalmers (1995) argues that we must take a radical step such as positing that consciousness is in some sense fundamental, rather than something that emerges from collections of nonconscious subatomic particles.

We can note that skeptics of psi generally skip over this void of a consensus theory of consciousness, grounded in well-established principles in science. For instance, Alcock (2010, p. 38) contrasts psi unfavorably with cosmology, which has developed a sophisticated mathematical model of black holes that in turn is consistent with data in other domains. Alcock notes that there is no comparable theory that explains psi. However, this could also be said for any current theory trying to explain the emergence of consciousness.

Increased attention to the problem of consciousness may have been influenced by the neurologist Francis Crick's (1994) attempts toward establishing some initial understanding of how vast assemblies of neurons and associated molecules lead toward conscious experience. His thoroughly materialistic and reductionist approach was laid out in his book, *The Astonishing Hypothesis*. Searle (1997), while admiring Crick's book, complains that he falls short in explaining the hard part of the mind-body problem, specifically when Crick questions how "does the brain get us over the hump from electro-chemistry to feeling" (p. 28). Crick spent the remainder of his life working with neurologist Christof Koch on developing a materialistic explanation of consciousness. Recently, however, Koch has abandoned this approach and has teamed up with Giulio Tononi to develop an information-based model of panpsychism (Koch, 2012).

A number of others are attempting approaches that radically deviate from more conventional, materialistic theories. Chalmers (1995) initially advocated what he termed naturalistic dualism, but he has also expressed sympathy for panpsychism. Seager (1999) has endorsed the view that solving the hard problem will likely require taking consciousness as fundamental and outlines another version of panpsychism. Neurophysiologist Stuart Hameroff and mathematician and physicist Roger Penrose (1996) have developed a framework through which quantum gravity interacts with quantum waveform reduction to generate a rudimentary level of consciousness. According to Hameroff and Penrose (1996), our actual experience requires a sophisticated orchestrated objective reduction of this process across clusters of microtubules within the brain's neurons.

While most scientists and philosophers agree that we do not currently possess an explanation of consciousness in terms of purely materialistic processes, they are split on the implications. Those such as Chalmers and Nagel who hold the view that the problem of consciousness requires a radical solution are probably in a minority, albeit an influential one. A majority probably share Searle's view that the problem is very real and difficult but consciousness will ultimately be revealed to be an emergent phenomenon of biological processes. Thus, for most, consciousness remains an important and difficult problem, but disagreement remains between those who believe that a purely materialistic account is possible and those who don't.

Clearly those who favor a nonmaterial explanation will not likely accept T_C as a constraining meta-theory or paradigm. How might they respond to the presence of psi data? They may be able to dismiss or discount it using another prior, but it's difficult to see what the basis for that might be. Again, recall that by standard norms, the empirical data for such psi categories as telepathy are strong. Although a scientist who accepts consciousness as fundamental may not initially hold beliefs congruent with psi data, the lack of a theoretical structure and evidence for a new theory of consciousness at this point would seem to make it difficult to dismiss or discount such data out of hand. Chalmers (2014) helped to drive home this point in a TED Talk presentation:

> I think consciousness right now is a kind of anomaly, one that we need to integrate into our view of the world, but we don't yet see how. Faced with an anomaly like this, radical ideas may be needed, and I think that we may need one or two ideas that initially seem crazy before we can come to grips with consciousness scientifically.

Within this context, there seems to be little justification for discounting the psi data that I reviewed above. Of course, Chalmers is speaking about the difficulty of accounting for subjective experience, not psi phenomena. However, once we acknowledge the anomalous nature of consciousness with respect to the laws of physics and chemistry, it's difficult to rule out other possible anomalies that are supported by data. And at this point, there is little consensus around a new theory of consciousness that could justify discounting psi.

However, most scientists, unlike Chalmers and Nagel, probably employ something like T_C, although we might legitimately wonder how strongly they hold to it. Although they may not possess an explanation of consciousness based on classical physics or other material processes, they may invoke an argument of promissory materialism. Such an argument might claim that while we are not currently able to explain consciousness via material processes, given the progress and usefulness of meta-theory T_C we are still justified in insisting that all explanations are most likely to remain within the confines of well-understood physics.

However, let's consider this more carefully. We can note that physics consists of sets of equations specifying precise, quantitative relationships between such entities as mass, energy, time, distance, and frequency. Important examples include Newton's laws of motion, Maxwell's equations of electromagnetism, and Einstein's theories of relativity. These sets of equations that comprise classical physics have been enormously successful in providing a foundation for physics, chemistry, biology, and neurophysiology, not to mention computer science and all types of engineering. Historically speaking, scientists who invoke classical physics as a touchstone for all lower level research have strong, practical justifications for doing so.

However, our subjective inner experience does not fit into this framework of mathematical relationships between purely physical constituents. Any attempt to do so leads to a category error. Nagel (1974) put it this way:

> If physicalism is to be defended, the phenomenological features must themselves be given a physical account. But when we examine their subjective character it seems that such a result is impossible. The reason is that every subjective phenomenon is essentially connected with a single point of view, and it seems inevitable that an objective, physical theory will abandon that point of view. (p. 437)

Thus, it seems we cannot hope to establish a theory of phenomenal experience in physics the way we can in other branches of science, such as chemistry and biology.

Nevertheless, advocates of T_C will likely insist that consciousness somehow emerges from complex, biological processes, such as dense networks of neurons in the brain (Searle, 1994). Although we may not now understand how this may arise, given the enormous progress under T_C (or similar versions), we still are better off focusing in this direction than considering an alternative meta-theory. Given the complexity of the human brain and the fact that we may have only scratched the surface, some argue we may need to wait several hundred years before we can hope for a complete theory of emergence (Simmons, 2006, p. 148).

However, the emergence of consciousness from purely material processes would be far more radical than any other process of emergence we know. Well known examples of emergence include the liquidity of water from H_2O molecules, complex traffic patterns from simpler movement of vehicles, the vorticular motions of a hurricane, and fluid movements displayed in Bénard convection cells from heated water. All of these examples involve relatively simple motions of substances or objects that lead to more complex, possibly unpredictable motion of the same substance or collection of objects, given various constraints across some defined system, as well as other inputs such as heat. But phenomenal experience is intrinsically different from nonconscious subatomic particles and molecules. The emergence of consciousness, unlike these examples, requires something in a completely different category from matter and energy arising from matter and energy. Given that we know nothing from physics upon which to base such a radical emergence, this sort of explanation appears to require something along the lines of a miracle.

These serious problems raise the question of whether research on consciousness should continue to be chained to something like T_C. Of course, dispensing with meta-theories such as T_C would unmistakably

signify a paradigm shift, at least with respect to consciousness. Kuhn does not provide clear guidance toward how to identify a paradigm in crisis preceding a revolution. However, Chalmers's plea for one or two "crazy" ideas is about as unequivocal a statement as one could hope that business as usual will not suffice.

Let's briefly consider what this sort of paradigm shift might entail. Establishing a true science of consciousness faces perhaps an insurmountable obstacle: how to observe something inherently subjective through objective methods. This centuries-old problem has plagued everyone who has sought to unravel the mystery of consciousness. The inability to "observe" consciousness in something else (whether an animal or brick) makes distinguishing between different possible explanations of consciousness virtually impossible. Thus, all explorations of consciousness capable of explaining our inner experience remain mired in purely philosophical arguments. Of course it's doubtful that philosophical arguments alone are sufficient to give us a truly scientific understanding of consciousness. Although there may be no perfect solution to this problem, perhaps we can still make progress if there are ways to acquire objective evidence, gathered under controlled laboratory conditions, that allow for anomalous features.

Here psi data make a crucial contribution. Psi data, gathered under strict controlled laboratory conditions, give us indications about how consciousness is in some respects anomalous with respect to our current understanding of physics. Although it may be anomalous in different ways than how Chalmers and Nagel describe subjective experience, it nevertheless provides scientists with crucial observations that allow them to explore theories of consciousness that deviate from more conventional ones. Thus, scientists wishing to explore a new paradigm beyond T_C will most likely need to carefully regard the present psi data, as well as consider new directions for psi research.

Conclusion

Psi of course continues to be controversial, and perhaps will remain so for some time. However, meta-analyses conducted on a number of psi categories, such as telepathy, clairvoyance, and precognition, appear to achieve the level of confirmation usually associated with a substantial degree of evidence. Given the failure of early criticisms such as fraud, error, or file-drawer bias to account for the data, psi advocates might understandably expect greater mainstream acceptance. However, most current responses from psi skeptics involve departing from the usual norms or standards of evaluation, given the controversial nature of psi. As I've argued, unstable or ambiguous standards of evaluation must adversely affect the integrity of scientific investigation, with the exception of cases where experimental findings are incongruent with relevant scientific theories for which a consensus exists, based on copious data.

Psi skeptics have generally assumed that results that conflict with currently well-developed theories of our world are sufficient grounds to dismiss or discount psi data. However, they have never grappled with the fact we have no theory of consciousness truly grounded in our understanding of physics. Hence, they have no foundation for rejecting psi data generated under controlled laboratory conditions. Further, philosophers of mind and scientists who wish to consider alternatives to materialist-based theories of consciousness will likely require data, generated under controlled, objective conditions, that are anomalous in at least some aspects. Thus, attempts to reject or limit the acceptance of psi data may inhibit our ability to make progress in understanding the nature of consciousness.

References

Alcock, J. (2010). Attributions about impossible things. In. S. Krippner & H. Friedman (Eds.), *Debating psychic experience: Human potential or human illusion.* (pp. 29–42). Santa Barbara, CA: Praeger.

Barrett, W. (1904). Address by the President. *Proceedings of the Society for Psychical Research*, *18*, 323.

Bem, D. J. (2011). Feeling the future: Experimental evidence for anomalous retroactive influences on cognition and affect. *Journal of Personality and Social Psychology*, *100*, 407–425.

Bem, D. J., & Honorton, C. (1994). Does PSI exist? Replicable evidence for an anomalous process of information transfer. *Psychological Bulletin*, *115*, 4–18.

Bem, D. J., Johnson, W. O., & Utts, J. (2011). Reply: Must psychologists change the way they analyze their data? *Journal of Personality and Social Psychology*, *101*, 716–719.

Bem, D. J., Palmer, J., & Broughton, R. S. (2001). Updating the ganzfeld database: A victim of its own success? *Journal of Parapsychology*, *65*, 207–218.
Braud, L. W., & Braud, W. G. (1974). Further studies of relaxation as a psi-conducive state. *Journal of the American Society for Psychical Research*, *68*, 229–245.
Braud, W. G., & Braud, L. W. (1973). Preliminary explorations of psi-conducive states. Progressive muscular relaxation. *Journal of the American Society for Psychical Research*, *67*, 27–46.
Braude, S. (1979). *ESP and psychokinesis: A philosophical examination*. Philadelphia, PA: Temple University Press.
Broad, C. D. (1949). The relevance of psychical research to philosophy. *Philosophy*, *24*, 291–309.
Chalmers, D. (1995). Facing up to the problem of consciousness. *Journal of Consciousness Studies*, *2*, 200–219.
Chalmers, D. (2014, March). How do you explain consciousness? [Transcript from *TED Talk*]. Retrieved from https://www.ted.com/talks/david_chalmers_how_do_you_explain_consciousness/transcript?language=en
Crick, F. (1994). *The astonishing hypothesis.* New York, NY: Simon & Schuster.
Crumbaugh, J. (1969). A scientific critique of parapsychology. In G. Schmeidler (Ed.) *Extrasensory perception* (pp. 58–72). New York, NY: Atherton.
Dukhan, H., & Rao, K. R. (1973). Meditation and ESP scoring. In W. G. Roll, R. L. Morris, & J. D. Morris (Eds.), *Research in parapsychology 1972* (pp. 148–151). Metuchen, NJ: Scarecrow Press.
French, C. (2010). Reflections of a (relatively) moderate skeptic, In S. Krippner & H. Friedman (Eds.), *Debating psychic experience: Human potential or human illusion*? (pp. 53–64). Santa Barbara, CA: Praeger.
Hameroff, S., & Penrose, R. (1996). Orchestrated reduction of quantum coherence in brain microtubules: A model for consciousness. In S. Hameroff, A. Kaszniak, & A. Scott (Eds.), *Toward a science of consciousness: The first Tucson discussions and debates*. (pp. 507–540). Cambridge, MA: MIT Press.
Hansel, C. E. M. (1966). *ESP and parapsychology: A critical re-evaluation*. Buffalo, NY: Prometheus.
Hebb, D. O. (1951). The role of neurological ideas in psychology. *Journal of Personality*, *20*, 39–55.
Henderson, L., Goodman, N., Tenenbaum, J., & Woodward, J. (2010). The structure and dynamics of scientific theories: A hierarchical Bayesian perspective. *Philosophy of Science*, 77, 172–200.
Holt, N., Simmonds-Moore, C., Luke, D., & French, C. (2012). Methodological issues related to the study of psi. In N. Holt & R. Lewis (Eds.), *Anomalistic psychology* (pp. 94–121). New York, NY: Palgrave Macmillan.
Honorton, C. (1975). Error some place! *Journal of Communication*, *25*, 103–116.
Honorton, C. (1985). Meta-analysis of psi ganzfeld research: A response to Hyman. *Journal of Parapsychology*, *49*, 51–91.
Honorton, C. (1993). Rhetoric over substance: The impoverished state of skepticism. *Journal of Parapsychology*, *57*, 191–214.
Hyman, R. (1985). The ganzfeld psi experiment: A critical appraisal. *Journal of Parapsychology*, *49*, 3–49.
Hyman, R. (1991). [Comment]. *Statistical Science*, *6*, 389–392.
Hyman, R. (1996). Evaluation of a program on anomalous mental phenomena. *Journal of Scientific Exploration*, *10*, 31–58.
Hyman, R. (2010a). Parapsychology's Achilles heel: Persistent inconsistency. In S. Krippner & H. Friedman (Eds.), *Debating psychic experience: Human potential or human illusion.* (pp. 43–52). Santa Barbara, CA: Praeger.
Hyman, R. (2010b). What's wrong with materialism? In. S. Krippner & H. Friedman (Eds.), *Debating psychic experience: Human potential or human illusion.* (pp. 133–148). Santa Barbara, CA: Praeger.
Hyman, R., & Honorton, C. (1986). A joint communique: The psi ganzfeld controversy. *Journal of Parapsychology*, *50*, 351–364.
Kennedy, J. E. (2013). Can parapsychology move beyond the controversies of retrospective meta-analysis? *Journal of Parapsychology*, *77*, 21–38.
Koch, C. (2012). *Consciousness: Confessions of a romantic reductionist*. Cambridge, MA: MIT Press.
Kuhn, T. (1962). *The structure of scientific revolutions* (4th ed.). Chicago, IL: University of Chicago Press.
Ladyman, J., Ross, D., Spurrett, D., & Collier, J. (2009). *Everything must go: Metaphysics naturalized*. Oxford, England: Oxford University Press.
Lehrer, J. (2010, December 13). The truth wears off: Is there something wrong with the scientific method? *New Yorker*. Retrieved from www.newyorker.com/magazine/2010/13/the-truth-wears-off
Milton, J., & Wiseman, R. (1999). Does PSI exist? Lack of replication of an anomalous process of information transfer. *Psychological Bulletin*, *125*, 387–391.
Nagel, T. (1974). What is it like to be a bat? *Philosophical Review, 4*, 435–450.
Palmer, J. (1987). Dulling Occam's razor: The role of coherence in assessing scientific knowledge claims. *European Journal of Parapsychology*, *7*, 73–82.

Palmer, J. (2011). On Bem and Bayes [Editorial]. *Journal of Parapsychology*, *75*, 179–184.
Penman, D. (2008, January 28). Could there be proof to the theory that we're all psychic? *Daily mail online*. Retrieved from http://www.dailymail.co.uk/news/article-510762/Could-proof-theory-ALL-psychic.html
Pinker, S. (1997). *How the mind works*. New York, NY: W. W. Norton.
Price, G. (1955). Science and the supernatural. *Science*, *122*, 359–367.
Radin, D. (1997). *The conscious universe: The scientific truth of psychic phenomena.* New York, NY*: Harper Collins*.
Radin, D. (2006) *Entangled minds: Extrasensory experiences in a quantum reality.* New York, NY: Paraview Pocket Books.
Schechter, E. I. (1984). Hypnotic induction vs. control conditions: Illustrating an approach to the evaluation of replicability in parapsychology. *Journal of the American Society for Psychical Research*, *78*, 1–27.
Seager, W. (1999). *Theories of consciousness*. London, England: Routledge.
Searle, J. (1994). *The rediscovery of mind*. Cambridge, MA: MIT Press.
Searle, J. R. (1997). *The mystery of consciousness*. New York, NY: New York Review of Books.
Sherwood, S., & Roe, C. (2003). A review of dream ESP studies conducted since the Maimonides dream ESP programme. *Journal of Consciousness Studies*, *10*, 85–109.
Shoup, R., (2015). Physics beyond causality: Making sense of quantum mechanics and certain experimental anomalies. In E. May & S. Marwaha (Eds.), *Extrasensory perception: Support, skepticism, and science*. (pp. 63–83). Santa Barbara, CA: Praeger.
Tressoldi, P., Storm, L., & Radin, D. (2010). Extrasensory perception and quantum models of cognition. *NeuroQuantology*, *8*, 581–587.
Utts, J. M. (1996). An assessment of the evidence for psychic functioning, *Journal of Scientific Exploration*, *10*, 3–30.
Utts, J. M. (1999). The significance of statistics in mind-matter research. *Journal of Scientific Exploration*, *13*, 615–638.
Wagenmakers, E.-J., Wetzels, R., Borsboom, D., & van der Maas, H. (2011). Why psychologists must change the way they analyze their data: The case of psi: Comment on Bem (2011). *Journal of Personality and Social Psychology*, *100*, 426–432.

8004 Maple Ave.
Takoma Park, MD 20912, USA
grwilliams@gmail.com

Abstracts in Other Languages

German

SIND UNTERSCHIEDLICHE MASSSTAEBE ZUR BEURTEILUNGVON PSI GERECHTFERTIGT?

ZUSAMMENFASSUNG: Seit es die Debatte um Psi gibt, haben Skeptiker nahezu geschlossen darauf beharrt, dass bei der Beurteilung des Beweismaterials unterschiedliche Maßstäbe anzulegen seien, da Psi eine radikale Abkehr von unserem gegenwärtigen wissenschaftlichen Verständnis bedeute. Von daher herrscht erhebliche Unsicherheit, welcher Maßstab bei der Bewertung von Psi anzulegen sei. Dem Umstand, dass die Integrität einer wissenschaftlichen Untersuchung aufgrund dieser Inkonsistenz bei der Überprüfung von Maßstäben möglicherweise leiden könnte, wurde nur wenig Aufmerksamkeit geschenkt. Von manchen wurde zur Verbesserung dieses Dilemmas ein Bayesscher Zugang vorgeschlagen, um Einstellungen, Annahmen und den Hintergrund wissenschaftlicher Erkenntnis mehr explizit zu machen, besonders wenn es um die Einschätzung einer kontroversen Hypothese geht. Kuhns Vorstellung von Paradigmen, die wissenschaftliche Forschung auf solche Randbedingungen einschränkt, von denen angenommen wird, sie seien die produktivsten, wurde unlängst in einen Bayesschen Konzeptrahmen überführt. Innerhalb dieses Rahmens erprobe ich ein mögliches, von Skeptikern verwendetes Paradigma oder eine Metatheorie, die typischerweise die Forschung einengt und die es schwierig macht, dass die Evidenz für Psi akzeptiert wird. Es hat den Anschein, dass ein solches Paradigma auch beim Versuch, Bewusstsein zu erklären, auf vielerlei Probleme stößt, was für ein Verständnis von Psi grundlegend ist. Ich lege dar, warum Psi-Daten wahrscheinlich eine Schlüsselrolle spielen werden, wenn es darum geht, bei der Lösung des Bewusstsein-

sproblems Fortschritte zu machen. Daher ist die Anwendung unterschiedlicher Bewertungmaßstäbe bei Psi-Daten wahrscheinlich kontraproduktiv.

Spanish

¿ESTÁ JUSTIFICADO USAR NORMAS DISTINTAS PARA EVALUAR A LOS FENÓMENOS PSI?

RESUMEN: A lo largo del debate sobre los fenómenos psi, los escépticos han insistido casi universalmente en usar diferentes estándares para evaluar la evidencia, alegando que psi representa un cambio radical de nuestra comprensión científica actual. Por lo tanto, hay considerable ambigüedad sobre qué estándar de evaluación debe cumplir la investigación de psi. Se ha prestado poca atención a los posibles daños a la integridad de la investigación científica que resultarían de esta incoherencia en las normas de investigación. Algunos han propuesto el uso de un modelo Bayesiano como una mejora a este dilema al modelar de forma más explícita creencias, suposiciones, y el marco de conocimiento científico, especialmente en la evaluación de una hipótesis controvertida. Recientemente, la noción de Kuhn de los paradigmas, que circunscribe la investigación científica dentro de los límites que se consideran más productivos, se ha incorporado a un marco Bayesiano. Dentro de este marco, exploro un paradigma probable, o meta-teoría, utilizado por los escépticos que típicamente limita la investigación y dificulta que la evidencia de psi sea aceptada. Tal paradigma podría dificultar en muchos aspectos la comprensión de la consciencia, que es fundamental para la comprensión de psi. Analizo por qué es probable que los resultados sobre psi desempeñen un papel clave para avanzar en la solución del problema de la consciencia. Por lo tanto, la aplicación de diferentes normas de evaluación a los datos psi es probablemente contraproducente.

French

EST-CE QUE L'EVALUATION DU PSI NECESSITE DES STANDARDS DIFFERENTS?

RESUME : Au cours du débat sur le psi, presque tous les sceptiques ont insisté sur les standards différents nécessaires à l'évaluation des preuves associées à ce processus, affirmant que le psi représentait un écart radical par rapport à notre compréhension scientifique actuelle. De ce fait, il y a une ambiguïté considérable quant aux standards d'évaluation susceptibles de s'appliquer pour le psi. Peu d'attention a été portée sur les possibles répercussions de cette inconsistance des modèles d'évaluation sur l'intégrité de la recherche scientifique. Certains ont proposé d'employer un modèle bayésien comme solution à ce dilemme afin de mesurer plus explicitement la part des croyances, des hypothèses implicites et des connaissances scientifiques antérieures, en particulier lorsqu'il s'agit d'évaluer une question controversée. Récemment, la notion kuhnienne de paradigmes, qui limite la recherche scientifique à certaines bornes afin de la rendre supposément plus productive, a été incorporée dans le modèle bayésien. Dans un tel cadre, j'explore un paradigme ou une méta-théorie possible employée par les sceptiques qui délimite de façon typique les zones de recherche et rend difficile l'acceptation des preuves du psi. Il apparaît qu'un tel paradigme aurait, à bien des égards, beaucoup de mal à rendre compte de la conscience, qui est fondamentale pour une compréhension du psi. Je discute des raisons qui me poussent à penser que le psi va probablement jouer un grand rôle dans la résolution du problème de la conscience. Dès lors, appliquer des standards différents lors de l'évaluation des preuves du psi me semble contreproductif.