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REPLY

Testing the Storm et al. (2010) Meta-Analysis Using Bayesian and Frequentist Approaches: Reply to Rouder et al. (2013)

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Rouder, Morey, and Province (2013) stated that (a) the evidence-based case for psi in Storm, Tressoldi, and Di Risio's (2010) meta-analysis is supported only by a number of studies that used manual randomization, and (b) when these studies are excluded so that only investigations using automatic randomization are evaluated (and some additional studies previously omitted by Storm et al., 2010, are included), the evidence for psi is "unpersuasive." Rouder et al. used a Bayesian approach, and we adopted the same methodology, finding that our case is upheld. Because of recent updates and corrections, we reassessed the free-response databases of Storm et al. using a frequentist approach. We discuss and critique the assumptions and findings of Rouder et al.

Keywords: Bayesian analysis, ESP, ganzfeld, meta-analysis, null hypothesis significance testing, parapsychology

We welcome the thought-provoking comment from Rouder, Morey, and Province (2013). We consider this article an attempt at unearthing some ostensible misconceptions about the psi construct, and the appropriate means by which one should go about testing the so-called psi hypothesis. Rouder et al. imply that support for the psi hypothesis is largely dependent upon the statistical procedures one adopts in testing that hypothesis. To a lesser degree, and independent of the statistical approach, care also needs to be taken in how data or studies are compiled and categorized. We agree. Rouder et al.'s article focuses mainly on the findings of the meta-analysis by Storm, Tressoldi, and Di Risio (2010). Fortunately, given the often controversial nature of psi, Rouder et al. confined their critique to the empirical evidence rather than opinion (see, e.g., Hyman, 2010). Instead, Rouder et al.'s contribution was facilitated by an open exchange of data and information.

In attempting, however, to bring to light certain flaws in the meta-analysis by Storm et al. (2010), and the alleged procedural errors in other studies (see Rouder et al.'s, 2013, criticisms of studies by Dalton, 1997; May, 2007; and Targ & Katra, 2000), we occasionally encounter some erroneous statements and arguable procedures using the Bayesian approach. Although the Bayesian alternative to the "frequentist" approach is now proving popular in parapsychology (see Bem, Utts, & Johnson, 2011; Tressoldi, 2011; Utts, Norris, Suess, & Johnson, 2010; Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011),[1] it should nevertheless be noted that "Bayesian methods utilize [a] 'degree of belief' interpretation of probability to model all uncertainty" (Utts et al., 2010, p. 2). In spite of this caveat, we respond to Rouder et al. by conducting a Bayesian analysis of our own. Before doing so, we address other issues raised by Rouder et al. regarding three specific studies. We subsequently reassess three subsets of studies (referred to as Categories 1, 2, and 3),[2] which were originally compiled by Storm et al. We believe that it is imperative to conduct this reassessment given recent updates and corrections that either came to our attention after publication or were erroneously omitted at the time of writing.

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[1] As an aside, Rouder et al. (2013) claimed that they "critiqued Bem's demonstration on statistical grounds and showed that the provided evidence was not convincing" (p. 241). Rouder et al. cited Rouder and Morey (2011) and Wagenmakers et al. (2011) in evidence, but we refer the reader to Bem et al. (2011) for a rebuttal of those claims. Indeed, Rouder and Morey were critical of the Wagenmakers et al. analysis.

[2] Category 1 = ganzfeld; Category 2 = non-Gz noise reduction (non-ganzfeld noise reduction techniques that alter the normal waking cognitive state through hypnosis, meditation, dreaming, or relaxation); and Category 3 = standard free response (normal waking cognitive state; no hypnosis, meditation, dreaming, or relaxation; see Storm, 2010, p. 474).

Suitability of Studies

The May (2007) Study

Rouder et al. (2013) suggested that May's (2007) study lacks internal validity. Although May provided "seemingly strong evidence for psi" (p. 242), his statistical procedures are regarded as "opaque" because he apparently constructed an idiosyncratic and difficult-to-interpret statistic that he called "the figure of merit." Rouder et al. stated that May presented "no theoretical sampling distribution of the figure-of-merit statistic under the null" (p. 242), resulting in a distribution under the null hypothesis that has unexplained variability not suited as a means of standardizing psi performance. In fact, May stated:

> The primary measure (*a priori*) for evidence of anomalous cognition was the number of direct hits. We observed 32 hits out of 50 trials (binomial $p = 2.4 \times 10^{-6}$, $z = 4.57$, ES = 0.647). (p. 62)

Thus, the primary analysis in May's article was based on a binomial random variable with $n = 50$ trials and the probability of a direct hit under the null hypothesis of $p = 1/3$, because there were three possible target choices for each session. The figure of merit was a secondary measure, used to assess whether the response was likely to be correct before the correct answer was known.

The Dalton (1997) Study

On advisement from Hyman and Honorton (1986), who recommended "proper randomization" in the interests of ruling out systematic errors that might yield false positives, Rouder et al. (2013) critiqued the randomization processes of some of the studies in Storm et al.'s (2010) meta-analysis. In particular, they stated that Dalton (1997) did not clearly indicate whether automatic randomization (AR) or manual randomization (MR) was used. In fact, Dalton stated:

> The target generating system . . . consisted of extracting the target generating instructions from the controlling program and embedding them in a program that generated a large number of autoganzfeld targets in the range of 1 to 100. (p. 128)

We read this as AR; indeed, the selection was certainly not a manual process.[3] Storm et al.'s (p. 475) original meta-analysis excluded Dalton's study as a statistical outlier because of its extremely high scoring ($z = 5.20$, effect size [ES] = 0.46), whereas Rouder et al.'s only reason for exclusion was the study's apparent ambiguity, which is clearly an unwarranted assumption.

The Targ and Katra (2000) Study

The Targ and Katra (2000) study met with Rouder et al.'s (2013) disapproval for discarding atypical sequences where randomly selected pictures were altered to provide a representative mixture of possible targets in order to avoid any accidental stacking. Rouder et al. argued that "such shaping can only have negative consequences, as it disrupts the randomization that lies at the heart of the experimental method" (p. 242). Although we do not agree that the consequences would necessarily be negative, we do agree that using this form of restricted randomization makes it more difficult to interpret the statistics. In essence, by altering the results of simple randomization, Targ and Katra added a form of statistical dependence to sessions that would otherwise be independent. Thus, we agree that it is reasonable to remove this study from further analysis.

The Rouder et al. (2013) Bayesian Analysis

Constructing the Databases: Exclusion Criteria

Upon reading the article by Rouder et al. (2013), one is struck by an apparent mistrust or dislike of the frequentist approach—a statistical methodology that depends on null-hypothesis significance testing (NHST). Rouder et al. argued that (a) conventional proof of psi simply requires a failure to retain the null hypothesis, whereas the null "corresponds to the plausible and reasonable position that there is no psi" (p. 241), and (b) conventional NHST does not allow for the conclusion that the null hypothesis is true. Their problem with NHST seems to be that such a reasonable position can be (and often is) too easily rejected in parapsychological studies, as if the null were a kind of "straw man." However, we would argue that in a situation such as testing for psi, where there is a single parameter of interest (the true probability of a success), confidence intervals can be constructed that estimate the true magnitude of the effect with whatever confidence is desired. This has been done in studies of psi, and the lower endpoints of the confidence intervals are meaningfully larger than the null value (see, e.g., Utts, 1999).

Surely, the real problem for any empiricist should be surmounting the importance attached to one's *belief* about what is possible in the universe—or better, marginalizing it—and focusing on the bigger issue of what one can *conclude* statistically, which is exactly what is done in NHST. In short, we see the various statistical approaches available to researchers as being akin to tools in a toolbox, with each performing a specific function or limited range of functions, except that the "appropriate" application, where one method may be superior to another, can be more art than science, meaning that differences of opinion can, and often do, arise between investigators.

Turning to the analysis by Rouder et al. (2013), we appreciate their efforts to calculate Bayes factors (H_1/H_0) in order to quantify the odds of evidence for the mutually exclusive hypotheses H_1 = Psi and H_0 = Non-Psi. They created a database that they labeled "Revised Set 1" ($N = 47$), which is Storm et al.'s (2010) complete database of 67 studies minus 20 studies (i.e., 19 studies that used MR and the single study by May, 2007). This major exclusion criterion left Rouder et al. with an arguably "pure" set of studies that used only AR. However, May (2007) should not have been excluded, as explained above. Next, Simmonds-Moore and Holt (2007) should not have been excluded either, as it is an AR study (as they stated [p. 203], "The computer used the pseudo random function for target selection and to randomise the order of presentation of decoy and target clips at the judging stage"). Third, Dalton (1997) should not have been excluded because it too is an AR study, as explained above.

[3] In fact, one of us (Utts) was a visiting scholar in the psychology department of the University of Edinburgh when the study was in progress and can confirm that automated randomization was used.

Rouder et al. (2013) also constructed "Revised Set 2" ($N = 49$), which is Revised Set 1 plus data from Del Prete and Tressoldi (2005), and Tressoldi and Del Prete (2007), which were erroneously omitted in the Storm et al. (2010) database.

A major problem we have with the Rouder et al. (2013) analysis is the dubious justification of excluding studies merely because they are not considered AR studies. This means that the MR studies, which mostly used random number tables, were not considered "valid" according to an arbitrary criterion that takes exception to the processes by which random numbers are generated for random number tables. Random number tables were the gold standard used by statisticians for randomization before computer algorithms were widely available. No argument is presented as to (a) why the use of random number tables is problematic or (b) why AR means both "true" randomization caused by radioactive decay, as in state-of-the-art random number generators (see Stanford, 1977), and pseudorandomization with computer algorithms, but not one or the other. Furthermore, Rouder et al. did not test the difference between the AR and MR databases to see whether there is any statistical evidence to justify their claims of an evidentially real dichotomy. We intend to do exactly that.

In addition, regarding Rouder et al.'s (2013) Figure 1, Rouder et al. stated that it "shows the distribution of accuracy across the 63 studies *where the judge had four choices*" (p. 243, emphasis added). But it is misleading to illustrate the data this way because the figure excludes four studies (i.e., May, 2007; Roe & Flint, 2007; Storm, 2003; and Watt & Wiseman, 2002), simply because they are studies where k did not equal 4 (k is the number of choices, which is a count of the number of decoys plus the target). Hence, Rouder et al.'s set of studies has a total N of 63 (i.e., 67 minus 4). The four studies are listed in Table 1, which includes Dalton (1997) to show how strong the effects are—especially for the two excluded AR studies.

Altogether, these dubious exclusions comprise five very high-scoring studies, all with significant z scores ranging from 1.61 to 5.20, and ES values ranging from 0.21 to 0.65. Note that we have included Dalton (1997), Simmonds-Moore and Holt (2007), and May (2007) as AR studies. By including these studies (but excluding Targ & Katra, 2000), we regard our MR database ($N = 16$; $z = 1.27$, ES = 0.22) and our AR database ($N = 51$; $z = 0.65$, ES = 0.08) as more accurate than those of Rouder et al. (2013).

We tested the differences between the MR and AR mean ES values, and the MR and AR mean z scores, and found a significant ES difference, $t(65) = 2.40$, $p = .019$ (two-tailed), but the z score difference was not significant, $t(65) = 1.56$, $p = .124$ (two-tailed). In other words, due to the ambiguous test results, we cannot say with certainty that the MR and AR databases are heterogeneous, and we see no well-grounded justification for conducting a Bayesian analysis exclusively on the AR studies as if the set of MR studies were somehow tainted and had no validity. As an exercise, however, we pursue a Bayesian approach with quite a different approach and purpose in mind.

Constructing the Databases: Apples and Oranges

Glass, McGaw, and Smith (1981) once defended the meta-analytic approach of mixing "apples and oranges" (p. 218) if one's more general hypothesis was about fruit. However, we acknowledge the importance of the so-called process-oriented approach in parapsychology, which aims at revealing the sources of the psi construct, whether it ultimately proves to be an artifact of methodology or something other. Accordingly, we appreciate Rouder et al.'s (2013) attempts at drawing a distinction between the MR and AR studies. Storm et al. (2010) constructed three categories of studies for the same reason. Similarly, Rouder et al. effectively modeled a threefold categorical difference defined by state of consciousness. They claimed that the three-effect priors yielded the strongest support for psi: about 330 to 1 for Revised Set 2. However, Rouder et al.'s main conclusion was as follows:

> Psi is the quintessential extraordinary claim because there is a pronounced lack of any plausible mechanism. Accordingly, it is appropriate to hold very low prior odds of a psi effect, and appropriate odds may be as extreme as millions, billions, or even higher against psi. Against such odds, a Bayes factor of even 330 to 1 seems small and inconsequential in practical terms. Of course for the unskeptical reader who may believe a priori that psi is as likely to exist as not to exist, a Bayes factor of 330 to 1 is considerable. (p. 246)

Given the lack of agreed criteria for defining the level of evidence necessary to consider a phenomenon "real" or "plausible," we acknowledge the claim of Rouder et al. that appropriate odds may be extreme. But it is interesting to observe that Rouder et al. required a level of evidence well above that suggested by Wagenmakers et al. (2011), suggesting that Rouder et al.'s statement derives from an incapacity to accept psi, so that it may not be a matter of evidence but of belief. It is curious to note that not only in medicine but also in clinical psychology (the latter being a field that deals directly with human health and well-being), the criteria that define the level of evidence for declaring whether clinical intervention can be considered empirically supported, are well defined and applied worldwide (see Chambless & Ollendich, 2001). In principle, it should be possible to arrive at a consensus

Table 1
Rouder et al.'s (2013) Excluded Studies

Study	Category	k	Z score	Effect size	p (one-tailed)	Randomization
Dalton (1997)	1	4	5.20	0.46	<.001	AR
May (2007)	3	3	4.57	0.65	<.001	AR
Roe & Flint (2007)	1	8	1.81	0.48	.035	MR
Storm (2003)	3	5	1.84	0.58	.033	MR
Watt & Wiseman (2002)	3	5	1.61	0.21	.053	AR

Note. Data drawn from Storm et al. (2010, Appendix A). AR = automatic randomization; MR = manual randomization.

Table 2
Three Homogeneous Free-Response Databases by Category

		Z		Effect size				
Category	*N*	*M*	*SD*	*M*	*SD*	Sum of *Z* (ΣZ)	Stouffer *Z*	*p* (one-tailed)
1[a]	29	1.01	1.37	0.14	0.20	29.18	5.42	2.98×10^{-8}
2[b]	16	0.78	1.19	0.10	0.19	12.55	3.14	8.45×10^{-4}
3[c]	15	−0.20	0.49	−0.03	0.07	−3.01	−0.78	7.82×10^{-1}

[a] Ganzfeld. [b] Nonganzfeld noise reduction. [c] Standard free response.

about how much evidence is sufficient to declare a phenomenon real or very probable. It may seem puzzling to many, therefore, that such extreme odds ratios need to be posited in the case of psi.

It is in appreciation of a fundamental polarization in human beings that we find we must speak to—or better, appeal to—a broader issue when arguing the case for psi. Storm et al. (2010) already raised this issue when they implied that many of our 20th-century discoveries and breakthroughs (e.g., the relative properties of "spacetime," or "nonlocal" effects posited in quantum mechanics) would have been rejected as ludicrous in bygone days, yet these phenomena are now met with very little resistance. Many phenomena may be regarded as "quintessentially extraordinary" (to use Rouder et al.'s, 2013, words), and indeed are often considered marvels even when evidence abounds as to their existence. Consider that Nobel Prize–winning physicist Niels Bohr said, "Anyone who is not shocked by quantum theory has not understood it" (cited in Barad, 2007, p. 254).

Given that proofs of such physical phenomena are heavily driven by the application of NHST, we proceed with the following frequentist analysis for two reasons: First, it enables us to update our database and adjust our findings; and second, it allows us to present an alternative interpretation of the data. It is important to mention too that two corrections had to be made to our earlier database as given in Appendix A in Storm et al. (2010, pp. 483–484). Specifically, slight adjustments were made to the total number of trials and hits in Studies 7 and 11 as follows: For Study 7 (i.e., Parker, 2006) there were 28 trials and 10 hits (as reported in Parker, 2010), and for Study 11 (i.e., Parker & Westerlund, 1998, Study 5) there were 30 trials and 12 hits (as reported in Parker, 2000).[4]

The three databases were tested for outliers. Dalton (1997) was found to be an outlier again (see Storm et al., 2010, p. 475), so that study was removed. Once again, we have a 29-study database of ganzfeld studies (Category 1). Again, there were no outliers in the nonganzfeld noise reduction set of studies (Category 2; $N = 16$). Having removed Targ and Katra (2000; as explained above), we note, not surprisingly (see Storm et al., 2010, p. 476), that Category 3 (the standard free-response studies; $N = 21$) was not rendered homogeneous until six studies were removed (two by Holt, 2007, plus four others: May, 2007; Simmonds & Fox, 2004; Storm, 2003; and Watt & Wiseman, 2002), yielding a nonsignificant 15-study database. For other descriptive statistics, see Table 2.

An analysis of variance test of the three databases produced a significant test result, $F(2, 60) = 5.07$, $p = .009$ (one-tailed), but only Categories 1 and 3 were significantly different from each other: mean difference = 0.18 ($SE = 0.06$), $p = .007$ (two-tailed). These findings are comparable to those of Storm et al. (2010), except for one finding: Category 3 in Storm et al. produced a significant Stouffer *Z*.

An Alternative Bayesian Analysis

For the 63 four-choice studies, and for two revised sets of studies, Rouder et al. (2013) found Bayes factors using both uniform and informed priors. They conducted three sets of analyses. In the first set they assumed that the true effect was the same for all studies, in the second set they assumed that each study had its own unique true effect, and in the third set they allowed for a different true effect for each of the three categories of studies identified by Storm et al. (2010).

In the following Bayesian analyses, we dispute Rouder et al.'s (2013) decision over choice of databases from which they calculated the Bayes factor, introducing frequentist and Bayesian parameter estimation to demonstrate the robustness of the evidence supporting the case for psi. Even if we were to accept Rouder et al.'s conservative approach of excluding all studies that used MR,[5] we could not reasonably accept that those remaining studies constituted a homogeneous database. We are more supportive of their "three effects" model, in which they allowed for different underlying effect sizes for each of the three categories identified by Storm et al. (2010). However, the studies by Tressoldi (2011) and Storm et al. were precisely devised to contrast homogeneous sets of studies that tested psi in different conditions of noise reduction. Of the three categories—ganzfeld (Category 1), nonganzfeld noise reduction (Category 2), and standard free response (Category 3)—Categories 1 and 2 were not significantly different from each other, possibly justifying a merging of the two categories, arguably for the reason that they both describe studies using altered states of consciousness, whereas Category 3 studies do not. As was done in the Storm et al. article, and therefore for the following Bayesian parameter estimates, we believe that it is appropriate to contrast two databases.

Our first procedure for Bayesian analysis of the separate databases was adopted by Kruschke (2011b), who analyzed the three categories of studies and the combined 63 four-choice studies using a Bayesian parameter estimation approach. Bayesian estimation provides information about the possible true effect sizes

[4] We thank Bryan J. Williams for bringing these corrections to our attention (see Williams, 2011).

[5] We remind readers that in Storm et al. (2010), type of randomization was considered in the assessment of methodological quality of the studies and that the correlation between effect size and study quality was nonsignificant and extremely weak, $r_s(65) = .08$, $p = .114$ (two-tailed).

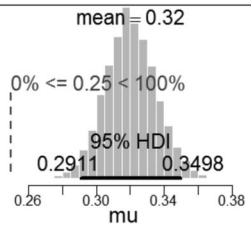
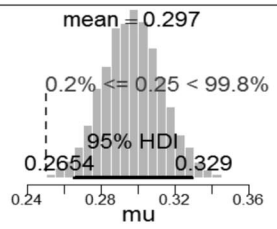
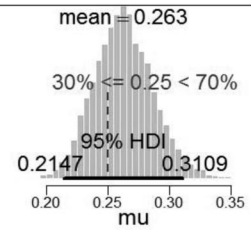
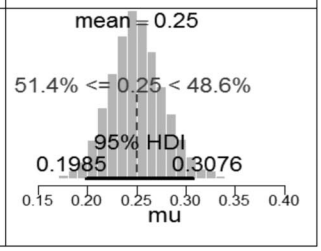
Database	Storm et al. (2010)	Mean Hit Rate (95%) High Density Interval (HDI)	*Revised Set II (AR studies; k = 4)*	Mean Hit Rate (95%) High Density Interval (HDI)
Ganzfeld + Non-Gz/Noise-Reduction	29 + 16 studies		20 + 14 studies	
Standard Free-Response	15 studies		14 studies	

Figure 1. All values inside an interval (indicated by the heavy black horizontal line) have higher credibility than values outside the interval, where each interval includes 95% of its respective distribution (note that May, 2007, and Watt & Wiseman, 2002, are omitted from standard free response in the Revised Set 2 column because $k = 3$ and 5, respectively). AR = automatic randomization; non-Gz = nonganzfeld.

that is not available from examining Bayes factors, much like frequentist confidence intervals provide information that is not available from hypothesis testing. In this analysis the parameter of interest is the true probability of success. Following Kruschke, we use a Bayesian hierarchical model in which the number of successes in study j is a binomial random variable with success probability θj, possibly different for each study. The values of θj are sampled from a beta distribution with mean μ, and we want to estimate μ. It represents the average of all the possible success probabilities. To be conservative, we used a noninformative beta distribution on μ, with $a = 1$ and $b = 1$ (i.e., a uniform distribution), and a gamma distribution on the dispersion. For a general discussion of this type of model, see Christensen, Johnson, Branscum, and Hanson (2011, Section 4.12). For technical details about this particular application of the statistical approach, see Kruschke (2011a, 2011b).

Figure 1 shows the results of the Bayesian parameter estimation of μ (recall that chance = 25%, or 0.25). Looking at the 95% highest posterior density interval (labeled HDI for high density interval)[6] for μ for the two databases, one can see that a clear superiority of the combined ganzfeld and nonganzfeld noise reduction studies emerges, with an HDI ranging from 0.26 to 0.32, followed by the standard free-response studies conducted with a normal (waking) state of consciousness, for which the HDI range includes the chance value of 0.25.

For our second Bayesian analysis, we recalculated the Bayes factors, contrasting ganzfeld and nonganzfeld noise reduction (i.e., altered-state-of-consciousness studies) with the normal-consciousness (standard free response) database, using Rouder's et al. (2013) one-model informed prior. We then added a frequentist estimation of hit rate parameters, for both the corrected database and the Revised Set 2 databases. Results are given in Table 3 together with a frequentist estimate of the average hit score parameter.

Conclusion

Analyzing the reduced Storm et al. (2010) databases, using a Bayesian model comparison and parameter estimation, results in support of the initial findings for the full database in Storm et al. Specifically, psi appears to be facilitated or enhanced with noise reduction techniques (supporting evidence is provided in Tressoldi, 2011). This evidence points to the advantages of the so-called process-oriented approach, as it yields important clues about how to go about investigating psi phenomena. Those concerned about whether psi (i.e., nonlocal perception) violates well-established physical laws need not be overly concerned. Although such a preoccupation may be de rigueur for laypersons (and especially skeptics), for aficionados psi is merely one of the many unsolved problems physicists are currently studying.[7] For those interested in an empirical approach aimed at modeling psi with ganzfeld procedures that follow a quantum mechanical information-processing protocol, see Tressoldi and Khrennikov (in press).

In closing, we must bear in mind, as Bem (2011) said in his milestone article:

> If one holds low Bayesian a priori probabilities about the existence of psi—as most academic psychologists do—it might actually be more logical from a Bayesian perspective to believe that some unknown flaw or artifact is hiding in the weeds of . . . an unfamiliar statistical analysis than to believe that genuine psi has been demonstrated. (p. 420)

[6] The HDI indicates the most plausible 95% of the values in the posterior distribution.

[7] For examples, see *Wikipedia* (http://en.wikipedia.org/wiki/List_of_unsolved_problems_in_physics).

Table 3
Bayes Factors (H_1/H_0) for Two Databases Related to Three Noise Reduction Conditions: Comparisons of Hit Rate Estimations Between Studies With Automatic Randomization

Database	Revised Set 2 (adjusted and split)	Bayes factor (H_1/H_0) (one-model informed priors)	Hit rate[a]	
			M	95% CI
Ganzfeld and nonganzfeld noise reduction[b]	20 + 14 studies	14,708	0.29	[0.26, 0.31]
Standard free response (non-ASC)[c]	16 studies	0.10	0.25	[0.21, 0.30]

Note. CI = confidence interval; ASC = altered state of consciousness.
[a] Obtained by a bootstrap procedure with 5,000 resamplings. [b] The original 37.5% hit rate reported in Tressoldi and Del Prete (2007) is corrected to an overall hit rate of 28.8% by including the results of both sessions; to be conservative, we have excluded Dalton (1997) in this analysis. [c] Includes the normal state-of-consciousness condition in Del Prete and Tressoldi (2005).

We may agree with Bem, but agree too that rejecting the null should not be so negatively viewed as a case of easily knocking down a straw man. Indeed, if the history of parapsychology shows us anything, it clearly indicates that whatever gains parapsychology has made, the hearts and minds of those who believe in the reality of psi phenomena are not being won purely on the strength of a handful of oftentimes ambiguous statistical findings. For the psi hypothesis to attract real interest from the relevant disciplines, a deliberated and considered use of both frequentist and Bayesian approaches must surely be superior to the exclusive use of one over the other.

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COMMENT

A Bayes Factor Meta-Analysis of Recent Extrasensory Perception Experiments: Comment on Storm, Tressoldi, and Di Risio (2010)

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Psi phenomena, such as mental telepathy, precognition, and clairvoyance, have garnered much recent attention. We reassess the evidence for psi effects from Storm, Tressoldi, and Di Risio's (2010) meta-analysis. Our analysis differs from Storm et al.'s in that we rely on Bayes factors, a Bayesian approach for stating the evidence from data for competing theoretical positions. In contrast to more conventional analyses, inference by Bayes factors allows the analyst to state evidence for the no-psi-effect null as well as for a psi-effect alternative. We find that the evidence from Storm et al.'s presented data set favors the existence of psi by a factor of about 6 billion to 1, which is noteworthy even for a skeptical reader. Much of this effect, however, may reflect difficulties in randomization: Studies with computerized randomization have smaller psi effects than those with manual randomization. When the manually randomized studies are excluded and omitted studies included, the Bayes factor evidence is at most 330 to 1, a greatly attenuated value. We argue that this value is unpersuasive in the context of psi because there is no plausible mechanism and because there are almost certainly omitted replication failures.

Keywords: psi phenomena, ESP, Bayes factor, Bayesian meta-analysis

The term *psi* refers to a class of phenomena more colloquially known as extrasensory perception, and includes telepathy, clairvoyance, and precognition. Although psi has a long history at the fringes of psychology, it has recently become more prominent with Bem's (2011) claim that people may literally feel the future and Storm, Tressoldi, and Di Risio's (2010) meta-analytic conclusion that there is broad-based evidence for psi in a variety of domains. In previous work, we critiqued Bem's demonstration on statistical grounds and showed that the provided evidence was not convincing (Rouder & Morey, 2011; see also Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011). In this article, we assess the evidence in Storm et al.'s meta-analysis.

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Our main concern is that Bem (2011) and Storm et al. (2010) do not provide principled measures of the evidence from their data. Bem, for example, relies on conventional null hypothesis significance testing (NHST). NHST has a well-known and important asymmetry: The researcher can only accumulate evidence for the alternative, and the null serves as a straw-man hypothesis that may only be rejected. In assessments of psi, the null hypothesis corresponds to the plausible and reasonable position that there is no psi. It is problematic that such a reasonable position may only be rejected and never accepted in NHST. Storm et al. performed a conventional meta-analysis where the goal was to estimate the central tendency and dispersion of effect sizes across a sequence of studies, as well as to provide a summary statement about these effect sizes. They found a summary z score of about 6, which corresponds to an exceedingly low p value. Yet, the interpretation of this p value was conditional on never accepting the null, effectively ruling out the skeptical hypothesis a priori (see Hyman, 2010).

Problems with the interpretation of NHST are well known in the statistical community, and there are many authors who advocate Bayes factor as a principled approach for assessing evidence from data (Berger & Berry, 1988; Jeffreys, 1961; Kass, 1992). The Bayes factor, first proposed by Laplace (1986), is the probability of the data under one hypothesis relative to the probability of the data under another. These hypotheses may be null or alternatives, and in this manner, there is no asymmetry in the treatment of the null. The Bayes factor describes the degree to which researchers

and readers should update their beliefs about the relative plausibility of the two hypotheses in light of the data. Many authors, including Bem, Utts, and Johnson (2011); Edwards, Lindman, and Savage (1963); Gallistel (2009); Rouder, Speckman, Sun, Morey, and Iverson (2009); and Wagenmakers (2007), advocate inference by Bayes factors in psychological settings.

In our assessment of Bem's (2011) data, we found Bayes factor values ranging from 1.5 to 1 to 40 to 1 in favor of a psi effect, with the value dependent on the type of stimulus. Consider the largest value, 40 to 1, which is the evidence for a psi effect with emotionally evocative, nonerotic stimuli. Researchers who held beliefs that a psi effect was as likely to exist before observing the data, should hold beliefs that favor a psi effect by a factor of 40 after observing them. We, however, remain skeptical. Given the lack of mechanism for the feeling-the-future hypothesis, and its discordance with well-established principles in physics, we agree with Bem that it is prudent to hold a priori beliefs that favor the nonexistence of psi, perhaps by several orders of magnitude. Against this appropriate skepticism, the factor of 40 from the data is unimpressive. We emphasize here that a Bayes factor informs the community about how beliefs should change. Different researchers with different a priori beliefs may hold different a posterior beliefs while agreeing on the evidence from data. The goal in this article is to provide a Bayes factor assessment of the evidence for psi provided by Storm et al.'s (2010) large meta-analysis. A similar endeavor is undertaken by Tressoldi (2011), though our conclusions differ substantially from his.

A Reassessment of Storm et al. (2010)

Storm et al. (2010) provided a meta-analysis of 67 psi experiments conducted from 1992 to 2008. These experiments typically involve three people: a *sender,* a *receiver,* and a *judge.* The sender telepathically broadcasts an item to the receiver, who is isolated from the sender. The receiver then describes his or her thoughts about the item in a free-report format. The judge, who is also isolated from the sender, hears the free report from the receiver and decides which of several possible targets this free report best matches. One of these targets is the sent item, and the judge is said to be correct if he or she chooses this target as the best match.

Table 1 shows a Bayes factor analysis for a number of data sets and models. The rows of the table indicate the data set, and the columns indicate which models are compared. For now, we focus on the first row, for full set, and the first column, for one effect and informed prior. The full set includes all 67 studies analyzed by Storm et al. (2010), and the details of the one-effect informed prior model are discussed subsequently. The Bayes factor is about 6 billion to 1, which is a large degree of statistical support. These values indicate that readers should update their priors by at least nine orders of magnitude, which is highly noteworthy. The value we obtain is larger than the 19-million-to-1 Bayes factor reported by Tressoldi (2011) on an expanded set of 108 studies.[1] In summary, there is ample evidence in the data set as constituted to sway a skeptical but open-minded reader. As discussed next, however, there is reason to suspect that perhaps the data set is not well constituted.

Issues With Storm et al.'s (2010) Data Set

We carefully examined the nine studies that provide the highest degree of support for psi.[2] Some of these studies are documented thoroughly and appear to use standard and accepted experimental controls (e.g., Del Prete & Tressoldi, 2005; Smith & Savva, 2008; Tressoldi & Del Prete, 2007; Wezelman, Gerding, & Verhoeven, 1997). Nonetheless, the following key problems were evident either in the studies themselves or in their treatment in the Storm et al. meta-analysis.

Lack of Internal Validity

May (2007) provided seemingly strong evidence for psi; he reported 64% accuracy across 50 three-choice trials ($z = 4.57$, $p < .001$). May's statistical procedures, however, are opaque. He constructed an idiosyncratic and difficult-to-interpret statistic that he called "the figure of merit." Unfortunately, May presented no theoretical sampling distribution of the figure-of-merit statistic under the null. Instead, he constructed this null sampling distribution from the performance of three participants contributing 15 trials each. Hence, the distribution under the null has unaccounted-for variability, and cannot be used to standardize performance in psi conditions. We exclude this experiment because it lacks sufficient internal validity.

Shaping the Randomization Process

One of the key methodological components in exploring psi is proper randomization of trials (Hyman & Honorton, 1986). Storm et al. (2010) stated that they included only studies in which randomization was proper and was performed only by computer algorithm or with reference to random-number tables. Yet, we found examples of included studies that either did not mention how randomization was achieved (e.g., Dalton, 1997) or added an extra step of discarding "atypical" sequences. Consider, for example, Targ and Katra (2000), who stated: "These pictures were selected randomly, and then filtered to provide a representative mixture of possible targets to avoid any accidental stacking that could occur if, for example, we had an overrepresentation . . . of [a particular picture]" (p. 110). Clearly, such shaping can only have negative consequences, as it disrupts the randomization that lies at the heart of the experimental method (Hyman & Honorton, 1986).

Fortunately, Storm et al. (2010) indicated in their spreadsheet whether each study was *computer randomized* or *manually randomized.* Manual randomization is a heterogeneous class of studies including those where randomization is not mentioned (e.g., Dalton, 1997) or was shaped (e.g., Targ & Katra, 2000). If manual randomization is innocuous, then there should be no difference in

[1] Tressoldi (2011) used our meta-analytic Bayes factor (Rouder & Morey, 2011) in which it is assumed that the data are normally rather than binomial distributed. The normal model may be less efficient because it contains two base parameters (mean, variance) rather than one.

[2] We originally set out to survey the 12 studies referenced in Storm et al. (2010) that yielded z scores over 2.0. Unfortunately, it is difficult to obtain these studies as they are neither carried by many academic institutions nor available through interlibrary loan.

Table 1
Bayes Factor Assessment of Storm et al.'s (2010) Data Sets

	One effect		Multiple effects		Three effects	
Data set	Informed	Uniform	Informed	Uniform	Informed	Uniform
Full set	5.59×10^{9}	1.69×10^{9}	3.08×10^{11}	1.05×10^{-16}	2.40×10^{14}	7.30×10^{12}
Revised Set 1	63.3	17.7	1.25×10^{-6}	5.58×10^{-28}	2,973	76.3
Revised Set 2	31.7	8.77	5.45×10^{-8}	1.95×10^{-30}	328	7.85

performance across computerized and manual randomization procedures.

Before we assess whether performance varied across randomization strategies, the status of Lau (2004) needs consideration. In one of his experiments, Lau ran an unusually large number of number of trials, 937, which is more than 20% of the total number of trials in the data set and more than 7 times larger than the next largest experiment (128 trials). Storm et al. (2010) classified Lau's studies as manually randomized, and the study with 937 trials accounts for 49% of the total number of manually randomized trials. Yet, in the introduction to his studies, Lau discussed the importance of proper randomization. In the method section, however, he provided no further detail. We contacted Lau and learned through personal communication that he generated random number sequences via the Research Randomizer website (http://www.randomizer.org), which uses the Math.random JavaScript function. Hence, we have reclassified his studies as computer randomized.

Figure 1 shows the distribution of accuracy across the 63 studies where the judge had four choices. As can be seen, manual randomization leads to better psi performance than computerized randomization. We performed a Bayes factor analysis of all studies except May (2007) and found that the evidence for a difference in performance is about 6,350 to 1. We discuss the construction of this Bayes factor subsequently. A reasonable explanation for this difference is that there is a flaw in at least some of the manual randomization studies, leading to predictable dependencies between experimental trials. No psi is needed to explain higher-than-chance performance under these conditions.

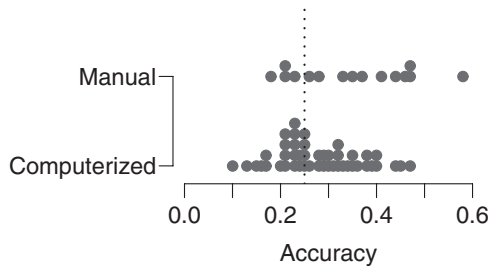

Figure 1. Distribution of accuracy across psi experiments as a function of the implementation of randomization. In computerized randomization, computers drew random numbers without any human filtering. In manual randomization, either there was filtering for atypical sequences or the method of randomization was not mentioned. The figure shows those studies with four choices, and chance performance corresponds to .25.

Selection of Studies

We noticed in our brief survey that not all the data in the reports were included in the Storm et al. (2010) meta-analysis. Consider the work of Del Prete and Tressoldi (2005), who ran two extrasensory perception conditions: one standard and one under hypnosis. In the hypnosis condition, Del Prete and Tressoldi observed 45 successes out of 120 trials (37.5%) in four-choice trials (chance baseline performance of 25%). In the condition with no hypnosis, there were 29 successes out of 120 trials (24.2%). Storm et al. included the first condition but not the second. This exclusion is surprising in the context of their meta-analysis because the no-hypnosis condition is similar to other included studies. Another example of selectivity comes from the treatment of Tressoldi and Del Prete (2007), who also ran psi experiments under hypnosis. These researchers used two sets of instructions, one to imagine an out-of-body experience and a second with more standard remote-viewing instructions. Instructions were manipulated within subjects in an AB design; half the participants had the out-of-body instructions first and the remote-viewing instructions second. The other half had the reverse. There was no effect of the instructions, but there was an unexpected effect of order. There was a psi effect for the first block of trials (a combined 40 successes out of 120 four-choice trials) but not for the second (a combined 29 successes out of 120 four-choice trials). Storm et al. included only the first block of trials but not the second. We see no basis for such an ad hoc exclusion given the criteria set out by Storm et al. These two omissions are examples of a selection artifact.

Analysis of Revised Data Sets

A prudent course is to analyze the set with the manual randomization studies excluded.[3] Of the original set of 67 studies, we excluded May (2007; insufficient internal validity) and 19 others that had manual randomization (see Appendix). We include two sets from Lau (2004), as these used computer randomization without any human filtering. We call this reduced set of 47 studies Revised Set 1. We also constructed a second revised set, Revised Set 2, by including the omitted conditions from Del Prete and Tressoldi (2005) and Tressoldi and Del Prete (2007). The additional rows in Table 1 provide Bayes factors for these two revised

[3] We do not wish to imply that Storm et al. (2010) are imprudent in their inclusion of the manual randomization studies. Claims of psi are sufficiently theoretically important and controversial that the community benefits from multiple analyses with these studies included and excluded, as we have done here.

sets. As can be seen, the Bayes factor in the first column is no longer a towering value of several orders of magnitude. Instead, it is around 63 to 1 and 32 to 1 for the two sets, respectively. Context for this value, as well as others in the table, is provided subsequently.

Bayes Factor Analysis

In this section, we describe the computation of the Bayes factor and the development of psi alternative hypotheses. The Bayes factor is the ratio of the probability of data under competing hypotheses H_1 and H_0:

$$B = \frac{\Pr(\text{Data}|H_1)}{\Pr(\text{Data}|H_0)}.$$

Let Y_i, N_i, and K_i denote the number of correct responses, the number of trials, and the number of choices per trial for the ith study, $i = 1, \ldots, I$. In this case, the binomial is a natural model of the data. One property of the Storm et al. (2010) data set is that the studies span a range of number of choices. Y_i is modeled as

$$Y_i \sim \text{Binomial}(N_i, p_i),$$

where

$$p_i = \frac{1}{K_i} + \left(1 - \frac{1}{K_i}\right)\eta_i.$$

The free parameter η_i denotes the performance on the ith study, with higher values of η_i corresponding to better true performance. Parameter η_i ranges from 0 to 1, and these anchors denote floor and ceiling levels of performance, respectively.

One key property of Bayes factors is that they are sensitive to prior assumptions about parameters. Although some critics consider this dependency may be problematic (e.g., Gelman, Carlin, Stern, & Rubin, 2004; Liu & Aitkin, 2008), we consider it an opportunity to explore several different types of prior assumptions about psi effects. This strategy of exploring a range of psi alternatives is also used by Bem et al. (2011) in their Bayes factor analysis.

Under the no-psi null hypothesis, the prior on η_i has all the mass at the point $\eta_i = 0$ for all studies. With this prior,

$$\Pr(\text{Data}|H_0) = \prod_{i=1}^{I} f(Y_i, N_i, K_i^{-1}),$$

where f is the probability mass function of the binomial distribution.[4]

Specifying priors that include psi effects is more complicated than specifying priors for the no-psi null. One could specify an alternative hypothesis by committing a priori to a specific known performance level, say, $\eta_i = .10$ for all studies. This commitment, however, is too constraining to be persuasive. Fortunately, in Bayesian statistics, one can specify an alternative that encompasses a range of prior values for η_i. We first develop priors for the case there is a single unknown performance parameter η for all studies, that is, $\eta_1 = \cdots = \eta_I = \eta$. Let $\pi(\eta)$ denote a prior density for η. Two examples of $\pi(\eta)$ are given in Figure 2A. The solid line, which is a uniform distribution, shows the case where η takes on values with equal density. The dashed line is a different prior that favors smaller values of η over larger ones. This is an informative prior that captures the belief that psi effects should be small. Both priors in Figure 2A are beta distributions, which is a flexible and convenient form when data are binomially distributed.[5] The corresponding priors on p, the probability of success, is shown for the four-choice studies ($k = 4$) in Figure 2B.

With these specifications:

$$\Pr(\text{Data}|H_1) = \int_0^1 \left[\prod_i f\left(Y_i, N_i, \frac{1}{K_i} + \left(1 - \frac{1}{K_i}\right)\eta\right)\right]\pi(\eta)d\eta,$$

where π is the probability density function of the uniform or informed beta distribution. The one-dimensional integral may be performed accurately and quickly by numeric methods such as Gaussian quadrature (Press, Teukolsky, Vetterling, & Flannery, 1992). The resulting Bayes factor for both priors is shown in Table 1 in the columns labeled "One effect." There is no penalty or correction needed for considering multiple alternative models with Bayes factor; one may consider as many priors as one desires without any loss. The resulting Bayes factor is always qualified by the reasonable or appropriateness of the prior. We believe in this case that the one-effect informed prior is perhaps the most appropriate of those we explore here.

In these one-effect priors, there is a single true-performance parameter for all studies. This degree of homogeneity, however, may be unwarranted. We constructed multiple-effect priors that allowed a separate parameter η_i for each study. The prior on each performance parameter η_i is an independent and identical beta distribution. We considered a uniform ($\alpha = \beta = 1$) and informed prior ($\alpha = 1$, $\beta = 4$) for each η_i. The resulting marginal probability is shown at the bottom of the page.

$$\Pr(\text{Data}|H_1) = \int_0^1 \cdots \int_0^1 \prod_i \left[f\left(Y_i, N_i, \frac{1}{K_i} + \left(1 - \frac{1}{K_i}\right)\eta_i\right)\pi(\eta_i)\right] d\eta_1 \cdots d\eta_I$$

$$= \prod_i \left[\int_0^1 f\left(Y_i, N_i, \frac{1}{K_i} + \left(1 - \frac{1}{K_i}\right)\eta_i\right)\pi(\eta_i)d\eta_i\right].$$

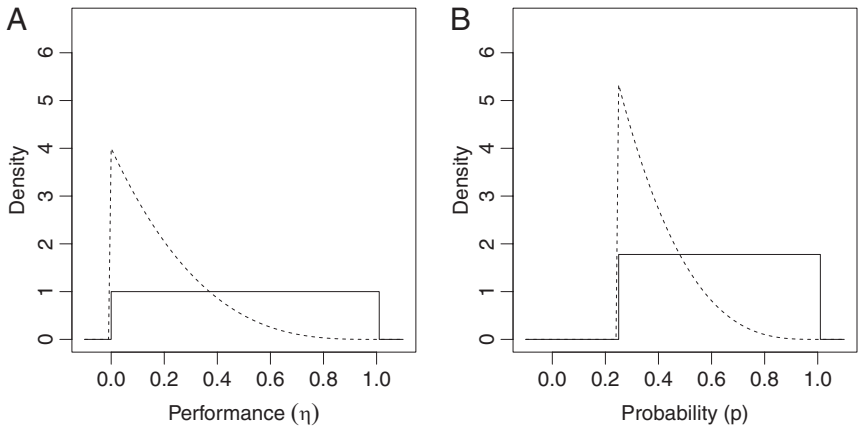

Figure 2. The informed prior (dashed lines) and uniform prior (solid lines) used in analysis: priors on performance parameter η (A) and priors on probability parameter p for a four-choice experiment (B).

The last expression is the product of one-dimensional integrals and may be conveniently evaluated with standard numerical techniques. The resulting Bayes factors are shown in Table 1 under the columns labeled "Multiple effects." Multiple-effect priors with multiple performance parameters fare relatively poorly. They are too richly parameterized and too flexible for the simple structure and relatively small sample sizes of the studies in the data set. For this set, it is more appropriate to consider one-effect models than multiple-effect models.

We also considered priors in which there are three effects rather than many. The motivation for this choice comes from Storm et al. (2010), who divided the experiments in the meta-analysis into three categories based on the conscious state of the receiver in the experiment. In one category, the receivers were in their normal waking state of consciousness. In the other two categories, receivers were in altered state of consciousness. In the second category, consciousness was altered by the ganzfeld procedure; in the final category consciousness was altered by some other technique such as hypnosis or advanced relaxation. To model this difference in conscious state, we allowed all experiments within a category common performance parameter, but there were separate performance parameters across the three categories. As before, informed and uniform prior settings were used on performance parameters, and the results are shown in the last two columns labeled "Three effects." These three-effect priors yielded the strongest support for psi, about 330 to 1 for Revised Set 2. Interpretation and qualifications are provided in the Conclusion.

As discussed previously, we also performed a Bayes factor analysis to assess the difference in performance between the 47 studies with computer randomization and the 19 studies with manual randomization. This analysis was performed assuming one common performance parameter for computer-randomized studies and a different common performance parameter for manually randomized studies. The prior on each of these performance parameters was the informed prior in Figure 2A (dashed line). The resulting value of 6,350 to 1 provides evidence for the proposition that studies with manual randomization had higher performance than those with computerized randomization.

Conclusion

We agree with Storm et al. (2010) and Tressoldi (2011) that uncritical consideration of full set of recent psi experiment provides strong statistical evidence for a psi effect. The Bayes factor, the ratio of the probability of the data under competing hypotheses, is on the order of billions to one or higher in favor of an effect, and the magnitude of this factor implies that even skeptics would need to substantially revise their beliefs. Nonetheless, closer examination of the data set reveals that the method of randomization affects performance. Experiments with manual randomization resulted in higher performance than those with computerized randomization (Bayes factor of 6,350 to 1). When these manually randomized experiments are excluded, the evidence for psi is attenuated by at least eight orders of magnitude (hundred million). Moreover, this attenuation does not take into account the possibility of file-drawer selectivity artifacts. In our brief review of just eight notable psi experiments, we found two data sets from Del Prete and Tressoldi (2005) and Tressoldi and Del Prete (2007), that should have been included. When these two sets are included, the largest Bayes factor for psi is 330 to 1, and this value is conditional on psi differences across altered states of consciousness. Although this degree of support is greater than that provided in many routine studies in cognition (Wetzels et al., 2011), we nonetheless remain skeptical of the existence of psi for the following two reasons:

[4] The probability mass function of a binomial distribution for y successes in N trials with probability parameter p is

$$f(y, n; p) = \binom{n}{y} p^y (1-p)^{n-y} \quad 0 \le p \le 1.$$

[5] The probability density function of a beta distribution for probability p with parameters α and β is

$$f(p; \alpha, \beta) = \frac{p^{\alpha-1}(1-p)^{\beta-1}}{B(\alpha, \beta)}, \quad 0 \le p \le 1, \alpha, \beta > 0,$$

where B is the beta function (Press et al., 1992). For the uniform prior, $\alpha = \beta = 1$; for the informed prior, $\alpha = 1$ and $\beta = 4$.

1. The Bayes factor describes how researchers should update their prior beliefs. Bem (2011) and Tressoldi (2011) provided the appropriate context for setting these prior beliefs about psi. They recommended that researchers apply Laplace's maxim that extraordinary claims require extraordinary evidence. Psi is the quintessential extraordinary claim because there is a pronounced lack of any plausible mechanism. Accordingly, it is appropriate to hold very low prior odds of a psi effect, and appropriate odds may be as extreme as millions, billions, or even higher against psi. Against such odds, a Bayes factor of even 330 to 1 seems small and inconsequential in practical terms. Of course for the unskeptical reader who may believe a priori that psi is as likely to exist as not to exist, a Bayes factor of 330 to 1 is considerable.

2. Perhaps more importantly, the Bayes factors in Table 1 should be viewed as upper bounds on the evidence from Storm et al. (2010). We are struck in that reviewing only eight studies, we found a host of infelicities including missing data sets from Del Prete and Tressoldi (2005) and Tressoldi and Del Prete (2007). Including these two studies reduced the three-effect model Bayes factor by a factor of 9. In all likelihood, these are not the only two missing sets, and it is reasonable to worry about the existence of others. Our concern differs from Storm et al., who concluded there would have to be at least 86 null studies missing from the meta-analysis to account for their significant findings. This computation, however, rests on the full set, which is seemingly contaminated by studies without proper randomization. As an aside, we are not convinced that either the philosophical or distributional assumptions in Storm et al. are the most satisfying (see, e.g., Givens, Smith, & Tweedie, 1997, for a Bayesian approach to estimating the number of missing studies in a meta-analysis). We simply note here that the obtained Bayes factors are upper bounds and the true value may be less favorable for psi.

In summary, although Storm et al.'s (2010) meta-analysis seems to provide a large degree of support for psi, more critical evaluation reveals that it does not. In our view, the evidence from Storm et al. for psi is relatively equivocal and certainly not sufficient to sway an appropriately skeptical reader.

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Appendix

List of Studies Excluded From the Full Set to Form Revised Set 1

Study	No. of trials	No. correct	No. of choices
Dalton (1997)	128	60	4
Dalton et al. (1999)	32	15	4
Dalton et al. (2000)	16	7	4
da Silva et al. (2003), ganzfeld condition	54	18	4
da Silva et al. (2003), nonganzfeld condition	54	10	4
May (2007)	50	32	3
Parker & Westerlund (1998), serial study	30	7	4
Parker & Westerlund (1998), Study 4	30	14	4
Parker & Westerlund (1998), Study 5	30	11	4
Parra & Villanueva (2004), picture	54	25	4
Parra & Villanueva (2004), music clips	54	19	4
Parra & Villanueva (2006), ganzfeld condition	138	57	4
Parra & Villanueva (2006), nonganzfeld condition	138	57	4
Roe & Flint (2007)	14	4	8
Roe et al. (2001)	24	5	4
Simmonds & Holt (2007)	26	8	4
Storm (2003)	10	5	5
Storm & Barrett-Woodbridge (2007)	76	16	4
Storm & Thalbourne (2001)	84	22	4
Targ & Katra (2000)	24	14	4

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