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Date: Sun, 22 Apr 2007 07:50:38 -0700

From: "Julio Siqueira" <juliocbdsiqueira@YAHOO.COM> [Add to Address Book](#) [Add Mobile Alert](#)
Subject: Re: Update on Dean Radin - New DATA !

To: AVOID-L@HAWAII.EDU

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Hi Brent,

I hope this message will reach you in your hideout... :-))

 My comments will be preceded by **[Julio]**, in bold blue.

Meeker wrote:

Date: Wed, 18 Apr 2007 09:49:33 -0700

From: "Brent Meeker" <meeckerdb@DSLEXTREME.COM> Subject: Re: Update on Dean Radin - New DATA !

To: AVOID-L@HAWAII.EDU

> I have placed on my site the book "/ESP After 60 Years/" (Pratt et
 all,
 > 1940). This is the source Radin claims to have used to reach the p
 value
 > of about 10^{-2000} , so now I believe it will be possible to tell fact
 > from fraud

The usual false dichotomy: fact or fraud. As pointed out in the paper
 on ganzefeld experiments the problem can be unintended bias. In any
 case it is unlikely that fraud can be discovered from published reports.
 The fraud and bias take place in the testing, not in the reporting or
 the analysis.

[Julio] I was not talking about the database from 1880 to 1940. I was talking about Radin's p value of 10^{-2000} and about your criticism to him...

> *Radin's Answer, exactly the way he presented it in his email to me:*

>

> "Is it possible that Radin got that huge z-score by not realizing
 that
 > the z-scores should be signed? "

>

> No, it is not possible. Z scores are distributed normally around a
 mean
 > of 0.

This response is self-contradictory! A variable that is normally
 distributed around a mean of zero is as likely to be negative as positive.
 If that is the case the answer would be "Yes, it is possible."
 Averaging a large number of such variable values would produce zero. I wonder
 what Julio thinks he's doing if he can't even understand the answers he
 gets?

[Julio] Let's undress, then, the self appointed "Emperor." First, Bill Jefferys himself disagrees with you (as I understood him). He said: "I read Radin's response differently, to wit: It is not possible that I (Radin) did not realize that z-scores should be signed, because I (Radin) know that they [should be] distributed normally around a mean of 0. To be sure, it is a bit difficult to parse Radin's reply, and I may not have done so correctly. I don't know what Julio thinks, though." (by the way, what "Julio thinks" is that he - i.e. I - has to "listen" carefully to what you two say and try to contrast it to what Radin says, to try to get some sense out of it. I have almost no expertise in statistics though I do know some basic statistic concepts of relevance to this discussion. Therefore, humbleness, from me, is a must.).

[Julio] But your "question" was a different one. You said "I wonder what Julio think he is doing"... Let's ther answer the question.

[Julio] Julio (I) thinks he is bringing information (in this case, Radin's answers) to people that will be able to see the validity (or the absence of validity) of what Radin is saying. Julio (I) also thinks he is acting very much as was (let's put it this way) "requested" by some here. Also, Julio thinks the people of AVOID-L (i.e. Brent Meeker) will live up to their word and to their responsibilities. Let's then refresh list members' memories...

Brent Meeker said on March 31, 2007: "Interesting that data in a sixty year old book which is supposedly at the foundation of a whole field of research is not available in the public domain. Why don't you just buy the book and tell the list exactly what these results are that Radin says imply odds of 10^{21} or 10^{2000} ?"

[Julio] Well, Julio did just that... He bought the book (U\$\$\$\$ - and it was Dean Radin himself that indicated to Julio where he could find it!). He copied and pasted page by page, 466 pages, often correcting the tables to correctly match MS Word format and pdf final format. He also copied image by image, adjusting to the final documents. All this very time consuming and tiring (and he had rather severe flu then).

[Julio] Now that Mr. Meeker has, for free and easily, what he requested, what will he do next? Julio still thinks that Mr. Meeker will live up to his word and to his social-scientific responsibilities, and will follow the data wherever it leads. Could it be that Julio is just being a naive believer? Perhaps...

Brent also said (Mar 31, 2007): "You should also remember that the question is not simply one of doing the statistics correctly. There were many procedural problems with Rhine's experiments and as he responded to criticisms and tightened controls the effect steadily declined. One wonders how Radin factored this into his odds."

[Julio] The question was never anything more than "doing the statistics correctly," Brent. This (i.e. the question being more than doing the statistics correctly), sadly enough, is a red herring that both you and Jefferys have been trying to sell to this list (I hope that the two of you are doing this unconsciously...). The question has always been only one, and a simple one: Radin's assertion in his book, Good's challenge, and the final answer from Radin. The two parties (Radin and Good) seem to have acted in good faith. The same cannot be said of you or of Jefferys (though you two may be acting due to unconscious emotional strong bias).

[Julio] What Radin said in his book was: (pg 97 the conscious universe 1996) "If we consider all the ESP card tests conducted from 1882 to 1939, reported in 186 publications by dozens of investigators around the world, the combined results of this four-million trial database translate into tremendous odds against chance—more than a billion trillion to one. If we assume that there is a selective-reporting problem in this database and calculate the number of unpublished, unsuccessful studies required to nullify these astronomical odds, we find that the file drawer would need to contain more than 626,000 reports. That's more than 3,300 unpublished, unsuccessful reports for each published report. This again demonstrates that chance results and selective reporting cannot reasonably explain these results."

[Julio] Radin is only talking in this paragraph about the problem of selective reporting. Therefore, the p value 10^{-2000} is associated ONLY with this issue of selective reporting. And it was to that that Good replied, claiming that the p value he came to was about 10^{-24} or something like it. Good did that by backward reasoning (calculating), changing the odds into p value (something Jefferys stressed that cannot be done, but that he later acknowledged that is one of the areas where Good himself has some works on). To say that "Radin first threw one number and then threw another" is unacceptable (it was not you who said it. You said worse things though...). It is easy to track down Radin's original calculation by using what he wrote. Not the informal "more than a billion trillion to one", but the more precise "That's more than 3,300 unpublished, unsuccessful reports for each published report." It was this number (3,300) that Good tried to demolish. And it was this number (3,000) that Radin further supported in his reply to Good.

[Julio] Now, we all know that both you and Jefferys have all that it takes to check very carefully (and perhaps very easily and fast...) all this issue. You have the expertise, the tables, etc. Now, Brent, it is your turn...

> This is point of Good's comment in /Nature/, and my response. There are
> simple ways of calculating the number of supposedly missing studies
> required to neutralize reported results.

The simple ways require assuming that the studies give null results, but what if the file-drawer bias is to suppress negative, not just null, results?

[Julio] This above has to do with Jeffrey Scargle's year 2000 brilliant article in the Journal of Scientific Exploration. I reproduce now just a very brief passage of it... : Acknowledgements: ... "I thank" (...) (among others, including Bill Jefferys) "its author," (i.e. author of "The Conscious Universe") "Dean Radin, for helpful discussions."

[Julio] But I will still talk about it later on...

Best Wishes,
Julio

P.S.: just a final afterthought... The article on Bayesian analysis of Ganzfeld that you indicated to me led me to conclude that Ganzfeld withstands the Bayesian challenge. This is in line with Utts assertion (1999) that she put Ganzfeld to the Bayesian scrutiny and it came out sound and safe...

Below, I include the relevant extracts from the thesis that you indicated to me:

THE PSI PHENOMENA: A BAYESIAN APPROACH TO THE GANZFELD PROCEDURE

A Thesis

Submitted to the Graduate School of the [University of Notre Dame](#) in Partial Fulfillment of the Requirements for the Degree of Master of Arts

by Michael Yan-Kiat Lau, B.A., M.A.

[Notre Dame, Indiana](#) - November 2004

[below are selected extracts from the work above]

I would like to thank the Parapsychology Foundation, Inc. for supporting this study by partially funding it through a research grant.

These inconsistencies in both parapsychology and other areas of psychology are due in part to complex but interrelated factors that include the file drawer problem and the discipline's reliance on a null hypothesis significance testing (NHST) as the dominant statistical analysis framework.

Limitations of the Null-Hypothesis Significance Testing Framework The dominance of null-hypothesis significance testing (NHST; also known as the frequentist framework; Gigerenzer, 1993) as a primary statistical analysis framework in the social sciences has resulted in some researchers characterizing it as an "overadoption" of a methodology by psychologists (Hubbard, Parsa, & Luthy, 1997). Meehl (1978), for example, argued that,

Howard and his colleagues (2000) have argued that meta-analytic and Bayesian approaches are viable alternatives to traditional NHST.

Milton and Wiseman (1999) subsequently attempted to replicate this meta-analysis by only including studies that presumably met the methodological guidelines set forth by Hyman and Honorton (1986). Milton and Wiseman argued that the exclusion criterion was implemented because studies conducted prior to the joint communiqué are contaminated by experiments with inadequate methodological controls.

[Milton & Wiseman (1999): Meta-analysis of studies that "began in 1987 or later and published by February 1997" (p. 388), but curiously excluding Honorton et al.'s (1990) autoganzfeld experiments.]

[Julio's comment] They also excluded the highly significant experiment from Dalton, claiming it was outside their deadline (though only for a few months, I think...). Other suspicious procedures were also used by Mr. Foxy Wiseman in this event...

NHST Interpretation. In six separate exact binomial tests, only one of the studies was statistically significant (Study 1). It is plausible that after the significant finding in the first study, researchers examining the data in a strict NHST approach might conclude that the psi phenomenon exists. Although this is uncommon in the psi literature, the larger psychological literature is peppered with examples of single experiment studies whose results are used to definitively support or refute the effect of interest.

Figure 1 represents the meta-analytic treatment of the long ganzfeld data presented in Table 1. The figure shows the percentage of correct hits as studies are meta-analytically combined. A 95% confidence interval (CI) was constructed around the hit rate Combined hit rate of the six studies 30%, 95% confidence interval from about 23% to about 38%

With the addition of the last study, the hit rate is further reduced to 30%. From the perspective of a meta-analyst, it appears that although we may be approximating the effect size of interest, we are at the same time unsure whether this estimate is significantly a chance occurrence⁵.

The meta-analytic approach is an improvement over the strict NHST approach in several ways. The meta-analytic focuses the analysis on the effect size of interest rather than dichotomous acceptance/rejection of the null hypothesis.

The non-believer is initially skeptical, but after the addition of the first study the hit rate estimate increases almost ten percentage points, although it still fails to reject the null hypothesis.

After six studies, the Bayesian analysis using a noninformative prior (also the meta-analysis) resulted in a failure to reject the null hypothesis. The non-believer also fails to reject the null hypothesis, whereas the believer rejects the null hypothesis.

The results so far further demonstrate the improvement of the interpretation of findings from the Bayesian approach over the meta-analytic approach. If interpreted from a strict NHST perspective, the meta-analysis described earlier yields a nonsignificant finding, failing to provide evidence for psi phenomena.

As discussed earlier, the extent to which any literature is biased is ultimately unknown. The psi ganzfeld community is arguably more sensitive to problems that contribute to bias than many other areas of psychology, and this may mean that the problem is less severe than in other areas.

Let's assume we can begin to do this by using a set of data whereby we know there is no file drawer effect, that is, the data presented above. Let's also assume that the psi effect is in reality zero. If we proceed as if a cumulative science is self-correctable, we can examine the impact of adding the six studies to the existing literature.

In Table 5, the Bayesian analysis of three recent meta-analytic results (Bem & Honorton, 1994; Milton & Wiseman, 1999; Storm & Ertel, 2001; see Appendix C for summary of the meta-analyses) with the six current studies is presented.

The graphical representation of this analysis with 95% confidence intervals is depicted in Figure 3.

Figure 3: the six studies are significant and have hit rates of relevance.

Both Bem and Honorton (1994) and the six studies conducted here are free of the publication bias problem, yet their conclusions are not in agreement. The removal of the selective publishing bias does not necessarily yield consistent and conclusive results.

The overarching questions that have occupied parapsychological research has been, "Does psi exist?" and "To what extent have decades of psi research served to support or refute the existence of it?" The series of six studies conducted here are an attempt to contribute to this discussion both on a substantial and methodological level.

Examining only the data collected in this study, one might argue that there is insufficient evidence to reliably conclude that a psi effect exists. The meta-analysis of the six studies yielded a final hit rate estimate of 30%, although this is not statistically significant from the null hypothesis.

A power analysis indicate that if Bem and Honorton's (1994) 32.2% estimate (or mean ES = .162) is an accurate assessment of the psi effect, 231 trials would be needed to achieve a power of .80, whereas if Storm and Ertel's (2001) 31% estimate (or mean ES = .138) is accurate, 320 trials would be needed to achieve the same level of power. In other words, this series of studies are underpowered to detect those levels of effect sizes. It is possible that if indeed psi exists at around the level proposed by these two metaanalyses that the six studies conducted here would not be able to reliably detect it. Interestingly though, the final estimate of 30% is relatively close to that of Bem and Honorton's (1994) 32.2% and Storm and Ertel's (2001) 31%. If more trials are conducted, the stability of the estimate can be better assessed as power is increased.

The results from the six studies conducted should be examined in concert with previously analyzed data to assess the psi effect. The meta-analytic treatment of the current data with three previously published meta-analyses is an attempt at synthesizing results across studies. Results indicate that with the addition of the six studies to the meta-analytic databases, the effect size estimates do not change drastically and that conclusions based on the rejection/acceptance of the null hypothesis also do not change as a result of the new findings.

One of the criticisms of the meta-analytic literature has been the problem associated with biased estimates as a result of the file drawer problem (Hyman, 1985; Kupfersmid, 1988; Rosenthal, 1979). I hope to have demonstrated that the removal of this bias is not in and of itself a panacea. Ruling out the problem of selective reporting does not preclude the need for studies to be conducted in a procedurally and methodologically sound manner. The necessitation for studies to have adequate power (Cohen, 1992) and consistently demonstrate replication of effect sizes (Krippner et al., 1993; Rosenthal, 1986; Utts, 1991) are two suggestions for developing a coherent and convincing literature.

Milton & Wiseman (1999): Meta-analysis of studies that "began in 1987 or later and published by February 1997" (p. 388), but curiously excluding Honorton et al.'s (1990) autoganzfeld experiments.

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