

# Why meta-analysis isn't the answer to the replication issue, a response to Dean Radin

Dick Bierman, June 2007

According to Dean Radin, there is no replication problem in parapsychology because meta-analysis shows consistent effects. However this is an incorrect position and causes a lot of problems with our main stream colleagues.

In any field of science the meta-analytic effect size is understood as an estimate of the true effect size with appropriate confidence intervals in the following way:

If a scientist is going to replicate the phenomenon then there will be a 95% probability that this scientist finds an effect size between two specified values say from 0.1 to 0.5. Sometime the interval includes the 0-effect but for most of the psi meta-analyses one is 'guaranteed' to get a positive effect size because even the 99.9% confidence interval excludes the 0-effect size. Note that I am talking effect sizes here not p-values.

We all know this claim is ridiculous so there seems to be a paradox. The explanation of the paradox, at least partly, is the experimenter effect.

Experimenter effect and the replication issue cannot be separated. Here is why. The field of parapsychology attracts say 5 newcomers each year. These people start doing experiments. Some are successful others are not. The probability that the unsuccessful experimenters do stay in the field and substantially contribute to the meta-analysis is small. We know a few like John Beloff but most of these unsuccessful experimenters disappear without leaving a trace.

Thus the meta-analytic database has to be interpreted different from what generally the interpretation is. The effect size and the confidence intervals that come out of this database are for SELECTED EXPERIMENTERS. Thus the replication efforts by unselected experimenters will not confirm to the figures from the database.

Note that I am not saying that this implies there is no psi. It implies that it is not as easy to replicate psi as is suggested by the meta-analysis. This results in much frustration.

But this is just the first level. Because it would suggest that, if we construct a protocol that prescribes how to select experimenters, we might be able to replicate the meta-analytic figures. And I doubt, I doubt it strongly. There are empirical reasons to doubt it.

The case of Joe McMoneagle is one. May & Spottiswoode claim that over years the mean effect size of Joe is over 0.3. Of course this will vary and there may be periods with 0-effects etc. But these periods never last very very long.

I have run simulations of Joe predicting the outcome of roulette by ARV and having a mean effect size of 0.3 with fluctuations that average out over two years. If one runs a ARV session each month with a conservative betting scheme like only putting at

stake half the amount you have and starting with 1000 dollars it turns out this is an almost 99% secure way to end up with a 100000 dollars in 2 years. Note that since this is ARV this scheme can easily be done in practice because the actual person playing roulette can be replaced each time the casino becomes suspicious. However the same persons that claim this long term replicability of Joe's performance are always in need of money. There is a paradox. And the paradox is not caused by Pecunia Olet, because Joe would be happy to supply money for psi research along these line.

Another indication is the multi-presentiment experiment by Spottiswoode. He argued on the basis of signal to noise arguments that if we have an effectsize of 0.1 based upon a single average participant in a presentiment experiment we can get a much larger effect size when we run the experiment with 100 participants in parallel.

Actually one could get to an experiment where the presentiment is so strong it could indicate the upcoming stimulus category with almost certainty.

The experiment failed totally. I predicted this failure not on the basis of some intuition with respect to the elusiveness of the phenomenon but based upon the argument that Spottiswoode set up was almost geared at creating a time paradox akin to the grandfather paradox. I do not believe nature will allow these paradoxes. In other words there is the possibility of an intrinsic theoretical reason against strong and replicable effects. If the effect is replicable one can USE it to create a time-paradox (this holds of course for precognition but if telepathy and clv are basically precognition of feedback, and PK is precognition of suitable button presses, it holds for all psi phenomena).

There are two possible strategies with respect to facing our main stream colleagues. The first one is the one advocated by Dean Radin and previously by Jessica Utts. It consists of basically overstating our case by not (or hardly) mentioning the experimenter effect and its influence on the meta-analytic figures. This will bring in main stream colleagues that will be intrigued by the meta-analytic figures but might become frustrated and turn against us the moment it turns out replication is practically and maybe even fundamentally difficult.

The second strategy is to be explicit about the replication problem and the role of the experimenter therein and ASK them how this could be solved. This might results in some main streamers arguing that psi is not an objective phenomenon (because it is so much dependent on the experimenter). They might turn away but possibly not become frustrated and hostile. And of course there might also be a few, especially those that encountered psi in their own life, that become even more intrigued by the phenomena.