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Date: Sun, 22 Apr 2007 07:46:45 -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

Hello Bill,

My comments below, preceded by [Julio] (they will appear in blue, in case you can see colors in your system.

>What you claim (you and Vic) is that it is not valid to combine the >results Christ's Holy sawing. Is it Right?

That's Radin's claim.

Here's why I don't believe it.

I claim that there are limits to what you can accomplish by taking more and more data.

[Julio] Interesting point.

You have to remember that I have been dealing with data, real data, for over 40 years.

[Julio] "Real Data"... ?

I have learned that it is very difficult to pull a signal out of a large amount of noise.

[Julio] And this "signal" would be the "real data", I suppose.

If you have a signal that is swamped by the noise, there is only a

limited amount that statistics can do for you to pull it out.

[Julio] Ok.

Amateur scientists, and I would include most of the parapsychologists I am aware of, imagine that all you have to do is to take more and more data and eventually the signal will come out, due to the magic of "the square root of N". This is an extraordinarily naive belief, and in real life it will lead to grief if carried too far.

[Julio] I think Jessica Utts explained this very clearly in her paper (1999), by saying that meta-analysis can be well performed or poorly performed.

The reason is that there are ALWAYS

[Julio] "ALWAYS and NEVER are two words that you should always remember never to say"...

unknown experimental biases in any experiment that have nothing to do with the phenomenon you are trying to investigate. They are always there, they are unavoidable.

[Julio] You are exaggerating too much in this piece above.

It is virtually certain that if you have a per-data-point error of a certain amount, then at the level of 1/100 of that error, and sometimes greater, there will be some sort of bias of this sort.

[Julio] Ok, let's suppose so. I just cannot figure out how this "bias" would influence the result in, say, Ganzfeld experiments. This "bias" that you talk about (in light of the highly exquisite design of the Ganzfeld protocol) seems, to me, far more amazing than ESP itself. And I am not saying that this "bias" is impossible (for if I were, I would actually be using the word "never", that I never use...). But it would be highly indicative (perhaps proof itself) of something highly interesting and relevant. That is precisely what skeptics Brugger and Taylor pointed out in their paper to the Journal of Consciousness Studies in 2003 (in the special volume "Psi Wars").

I know of two ways to control such biases: (1) Identify their source and measure them accurately enough to eliminate them or (2) Do a better experiment that has a smaller per-data-point error.

Parapsychologists do none of this. Instead, they have a touching, but naive faith in the power of the square root of N to overcome all uncertainty.

[Julio] I can't help saying that this that you are saying above is contrary to everything that I know about the present-day state-of-the-art in parapsychology. Maybe many parapsychologists (and their research) fit this tragic scenario that you picture.

Perhaps most of them fit this scenario. But certainly not all of them. Many carry out very good experiments. Of course when I say this, it is from the point of view of someone who is outside of the parapsychological community. Maybe those who know it really well and from inside know that it is all fraud and pretence. But that is not what the papers of psi researchers lead us to believe. And more: when it comes to parapsychology, we have the help (or *supposedly* have the help...) of the organized skeptic movement (CSICOP et al). Checking the arguments from the skeptics (or better, from those who believe psi does not exist) against the arguments from the believers (i.e. psi researchers), the arguments from skeptics are very poor and weak. And I am talking about people like Hyman, Alcock and Blackmore.

[Julio] So, you said just above that parapsychologists (1) do not try to identify the source of biases and do not try to control them. I believe Honorton is shaking in his tomb right now (even if there is no afterlife!). His agreement with Hyman, it seems, meant nothing to some... "skeptics". As to (2) the "better experiment" with a smaller per-data-point error, I don't understand this concept. I would appreciate if you could explain it.

It is a good rule of thumb that you can't get a signal out of noise that is much greater than 100 times the signal.

[Julio] Supposing that telepathy exists, what would be, in the Ganzfeld protocol, the signal and the noise? Would the signal be the effect size of telepathy and the hit rate? Would the noise be the methodological flaws? (by the way... which ones? It has been more than ten years since any skeptic forwarded any putative methodological flaw in Ganzfeld...). Why do you believe (or has concluded) that the ratio signal/noise in this would be smaller than 1/100?

Maybe at the outside 1000, but that is very dicey, and would require a very successful and diligent application of method (1) above. Parapsychologists claim to be able to do much better than this.

[Julio] At this point, I would ask the same that I just asked above, but this time regarding the PEAR micro-PK experiments...

Because of my experience in working with real data, I just don't believe them.

[Julio] Bill, the score is this: it may very well be that you are right. You and the CSICOPers. But what really seems is that you are dead wrong. You report the status of the psi research unfaithfully. Just like CSICOPers like Hyman and Alcock. You say they (psi researchers) do not care to improving their methodology. Psi researchers claim just the opposite. And they prove it with articles and etc. Counter arguments from you (and Hyman-Alcock-

Blackmore, etc) are just nowhere to be found (regarding the improving in methodology). Maybe they will come out now... I am not forgetting the things that you said, regarding statistical flaws from the psi researchers, Radin include. Yes, that can be the link to the rotten core of psi research. But so far, honestly, it does not seem to be a real link. It just seems to be... noise.

Meta-analysis won't help you. All it does is to make N bigger without giving any understanding of the underlying problems that are certainly in the experiments.

[Julio] Again, Utts put it very clear in her paper (1999). Used correctly, meta-analyses may be of great help. Perhaps meta-analysis addresses the item 2 that you referred to (per-data-point error).

If Radin's meta-analysis had any merit, he could do something like the following, which is often done by real scientists: Divide his data set into a number of smaller sets of data. He's got 186 experiments, he can construct say 10 groups of 18 or 19 by randomly choosing them from the pool of 186 (or better, having someone with no axe to grind do the choosing, like a professional and neutral statistician). Then do his meta-analysis on each of the 10 groups. Since the sigma of 96.5 standard deviations is so huge, the sigmas of the individual experiments will be about 30, still huge. Therefore, if the different experiments are measuring the same thing, they all ought to produce the same effect in the same direction to quite high precision.

[Julio] He himself said that neither he nor his colleagues care that much about these experiments from 1940. Yes, this that you propose above is good indeed. I will forward it to Radin. If he wants to write books talking about this old data, then I think he must be ready to look at this data in the proper way. You may be surprised by what I will say, but I do not like meta-analyses. And more, I do think (feel) Radin exaggerates and misuses this tool often. Just as I criticized him in the way he used the term "consciousness" in his recent book (Entangled Minds; 2006), I feel that there is a high risk of "garbage in, garbage out" when one (Radin) uses meta-analysis in a rather lax way or ventures into meta-meta-analysis and etc. So, perhaps, I already agree with the core of your criticism to Radin (if this is the core). But I strongly disagree with you regarding the extent to which this flaw from Radin (if really a flaw) devastates the bulk of his works and the essence of his "findings."

I will bet that if he did this, the effects would be all over the map.

[Julio] Now both you and Brent Meeker can do it quite easily on your own... The book is online, and free. Anyway, I will pass to Radin your suggestion.

This would prove that they aren't measuring anything at all.

[Julio] Type II error now?

I know what Radin would say to this proposal. He will whine that psi is variable and unpredictable, that will be his excuse.

[Julio] Perhaps it is worth asking first and shooting later...

It's the same excuse that parapsychologists have been giving for 100 years. Look up "sheep and goat effect," "decline effect," etc. It is all excuses intended to paper over the fact that there is no there there.

[Julio] Come on, Bill, let's not fool ourselves. They do not claim that psi is THAT elusive. They claim that it is stable enough to be detected, and so far it seems that they are right. By the way, why did Hyman say, in 1999, that Radin and Utts were the two who made the most to establish the reality of ESP? Just courtesy? Perhaps CSICOPers should think over before saying certain things...

This is why I think the whole statistical-analysis-of-a-humungus-number-of-trials paradigm that has dominated parapsychology since Rhine will NEVER be convincing to those not already convinced.

[Julio] Science is much more than that. For example, Michael Shermer's insistant complaint is the absence of a theory for psi. It is a fact that psi is highly rejected by the academia. The reason for this is, to me, a deep mystery. I think it is greatly (but not solely) due to CSICOP's "bad fingers."

Parapsychologists need an experiment that passes Jimmie Savage's interocular traumatic test. I said as much in print over fifteen years ago, and suggested some ideas about how to go about it. The ideas that I suggested overcome the objection you suggest:

[Julio] Let me comment on your idea below:

By working with individual atoms at the microscopic level, it should be possible to devise clear and convincing experiments, for example telekenesis, by having people move individual atoms around on a substrate at will. Since the claims of the random event generator experiments are that people can control the decay of atomic particles (a much higher energy process), surely something simple like this would work.

[Julio] A good experiment. I myself, if I had the required expertise and apparatus, would put it to the test! By the way, why haven't you or any other skeptic ever attempted it? (Stanley Jeffreys tried similar protocols to PEAR ones, and ended up, some years ago, finding introductory evidence for micro-PK - He must have been severely pressured afterwards, for he even wrote an article recently in the Skeptical Inquiry where he seems to have completely wiped out of his mind any memory of his

positive finding...). However... I do not know very well the nature of this quantum mechanic event that you refer to (single atoms on the move). Using my high school physics terminology (and not quantum mechanics derived, but from classical mechanics), I would point out that if the system that you are talking about is either at static equilibrium or at dynamic equilibrium, it would not be a good comparison to what is tested in micro-PK. What they test is systems that are not in equilibrium (you may here point out a misunderstanding of mine in this regard...). So, first, why would one expect that a phenomenon that operates in systems not in equilibrium would also operate in systems that are in equilibrium? Second, if the "result" would be... random, then we would go back to the same situation that they have in PEAR. Merely a shift in the probability that the event would occur. There are also further problems. But for now, these will suffice.

So far, I've seen no interest from the parapsychologists in doing anything like this. I got a few excuses from people who basically said, there's no point in doing this because psi might not work that way. What kind of excuse is that?

[Julio] The actual replies would be of great interest... But I must say that I consider it very bad that, out of the whole of the parapsychology community, no one has stepped out to try this test.

Instead, the parapsychologists continue on the same old treadmill, mindlessly producing large numbers based on huge numbers of trials in meaningless experiments, with no significant attention to the problems I mention above.

And they wonder why they can't convince anyone but the true believers!

[Julio] Now it is you that are playing the true believer. You believe that they can only convince the true believers. That can hardly be true. I myself am someone that just couldn't care less for telepathy or PK (micro or macro) or clairvoyance and etc. Also God is very low in my priority list (and in my agenda...). Afterlife is topmost to me, but it receives almost no support from any of these oddities of psi research. And, after carefully weighing the evidence pro and con, and the arguments pro and con, I came to consider that, scientifically, ESP is already proved. (And so what...). If the next ten years bring to us a clear disproof of ESP (which, in the last 150 years, has never happened... despite Vic' failed propaganda), I would not care at all. It is like the micro spacial dimensions. 11 or 10 or 9, to me it is just the same. And the macro spacial dimensions? Are they three or four? (and some even claim that macro spacial dimensions are not three, but two. Ok with me, as long as I do not feel "flat" :-)). It seems really true that skeptics, too, have turned into non-skeptics. Sometimes it is said that Daryl Bem is one such example. Also, it is clear that the guys from the Journal of

Consciousness Studies are pro psi. So, Bill, be a psi foe and psi fighter if you will. But do not play ostrich thinking the impact of psi is too little in academia. Well, but actually you know that far better than me...

**Best Wishes,
Julio**

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