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LETTER TO THE EDITOR

Quality in Parapsychological Meta-Analyses

Skeptical wisdom holds that parapsychological studies that produce significant results must have a low methodological quality. In most meta-analyses of parapsychological paradigms such as 'Ganzfeld telepathy' and so-called 'presentiment', the meta-analyzers, who are generally proponents of the psi hypothesis, also present a quality-effect size relation. These relations generally are at odds with the 'skeptical wisdom', i.e. they produce a positive or nonsignificant relation.

One can and should wonder what the quality is of the assessment of these relations. I became aware of this question when I found that a so-called PK–RNG (or Mind-over-Matter) study of mine got a low quality rating in one of the first large-scale meta-analyses (Radin & Nelson 2000). With my pride hurt, I delved a bit deeper into this and soon found that quality is assessed on the basis of the written reports. These reports generally have the standard structure of scientific writings, but often there is a special paragraph dealing with 'alternative explanations' where the authors go to some length to discuss sensory leakage, randomization problems, and other potential alternative explanations of their anomalous results. Never will one find here remarks such as: the sensory shielding was inadequate or the randomization was done by hand-shuffling. For the vast majority, these paragraphs carry information to persuade the reader that there are no alternative explanations.

I had no paragraph on alternative explanations simply because I didn't need one, because the results were nonsignificant. So my nonsignificant study got a low quality rating even though of course I did extensive randomization tests before even starting the study.

One doesn't need to be a mathematical genius to infer what this way of scoring quality (on the basis of a report rather than asking the author "did you check your RNG") is doing to the assessment of quality-effect size relations. Most if not all nonsignificant outcomes will get a low quality rating while they actually had a good quality. Even if in reality there is no relation, this approach will result in a positive relation between quality and effect size. And of course run against 'skeptical wisdom.'

I communicated this misuse of quality-effect size relations to the parapsychological community (in their discussion list) several times asking the culprits to stop using these relations as a 'proof' that results were more

significant with better quality. Nothing happened. Actually the practice just continued, most notably by some of the researchers with academic affiliations. That was the most shocking aspect of this. Could these "psi proponents" really not understand this obvious error?

The incorrect use of the report-based study quality ratings does of course not prove the skeptic wisdom. Therefore, real quality should be assessed by independent assessors inspecting the actual experiments, preferably onsite and preferably by raters blinded to the study outcome. This probably will never happen because it is a lot of work, and diehard skeptics don't need this because they have their wisdom unshakeably set already.

For the moment the best thing for the meta-analyzers of psi studies is to at least mention the caveat of using the current method of quality-study size asessment and for interested readers to consider the results published so far with a grain of salt. This effect introduces a bias into the quality–effect size relationship reported by the meta-analysis.

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Reference Cited

Radin, D., & Nelson, R. (2000). Meta-Analysis of Mind–Matter Interaction Experiments: 1959 to 2000. Los Altos, CA: Boundary Institute, and Princeton, NJ: Princeton Engineering Anomalies Research Laboratory.