

 [IP--the replication crisis debate by Louise Sundararajan](#) by Louise

S. [2016, Mar 15]

Dear All,

Debate over the replication crisis in psychology is raging on. For an update, see the note from Harris Friedman below. If you attend the APA Annual Convention in Denver, August 4th to 7th, there will be a Cross-Divisional symposium devoted to this topic, see attached program. Time to be decided.

Stay tuned,

Louise

From :

For those interested in psychology as a science, this has been one of the more entertaining, as well as sad, weeks in the history of the field. As I hope everyone on this listserv knows, there has been a widespread "replicability" crisis within psychology (see http://www.nytimes.com/2015/08/28/science/many-social-science-findings-not-as-strong-as-claimed-study-says.html?_r=0), as

well as in many other related fields (eg, economics, and even in the medical science, including those that involve psychology). It was found in a widely publicized study led by Brian Nosek that less than 40% of papers in top psychology journals could be replicated. It challenges the basic underpinnings of psychology's scientific core!

Now there is pushback against Nosek's study from Daniel Gilbert, a prominent Harvard psychology professor and proponent of positive psychology (eg, see <http://www.wired.com/2016/03/psychology-crisis-whether-crisis/> & http://www.slate.com/blogs/the_slatest/2016/03/03/psychology_study_that_induced_the_reproducibility_crisis_was_wrong.html).

He is also widely known as the pitchman for Prudential Insurance on TV commercials

(see <http://www.ispot.tv/topic/expert/k7f/daniel-gilbert>), so has a national media presence.

I'll share my assessment. Gilbert is mounting a challenge to defend psychology's honor (and the big dollars at stake in the psychology, especially positive psychology, business) based on a few minor statistical and methodological quibbles, essentially challenging about

30% and using that to taint the other 70% that is undisputedly solid. And, of course, the 30% he is challenging is not at all clearly wrong, as my (and many others') conclusions are that Gilbert is himself mostly wrong on the challenges he's raised. However, it shows how difficult it is to definitively demonstrate anything in our troubled field, even when using the best science we as psychologists were often taught to uncritically believe as valid while in graduate school.

Last, I might share that a major part of my professional involvements these last few years has been focused on debunking bad science stemming from positive psychology. I've been moderately successful at this, as several papers in prominent journals that I've helped critique have since been "corrected" or at least publicly rebutted, including in the 2 flagship psychology association journals (eg, American Psychologist & Psychological Science), as well as in some top general science journals (eg, PLoS One & PNAS) -- so perhaps this shows bias on my part, as well as possibly some source credibility. I am now working on publishing several follow-ups on what this all means, especially for humanistic psychology.

Harris Friedman, PhD

PS.

And, just when it looks like it could get no darker for so-called scientific psychology, look at the latest failure to replicate that was just published: http://www.slate.com/articles/health_and_science/cover_story/2016/03/ego_depletion_an_influential_theory_in_psychology_may_have_just_been_debunked.html

 [Replication collaborative APA proposal](#)

 [Comment by Jeanne Marecek](#) by Louise S. [2016, Mar 15]

Louise,


Here is a rejoinder to the Gilbert critique of Nosek. The authors argue that Gilbert et al.'s ideas about confidence intervals and effect sizes are in

error: <https://hardsci.wordpress.com/2016/03/03/evaluating-a-new-critique-of-the-reproducibility-project/>

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