

Comment: Is Reform Possible Without a Paradigm Shift?

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Looking back over the ASA Statement on Statistical Significance and P -values, which I think is an important and valuable contribution to a real and pressing problem, a striking feature is that so much effort appears to be needed to *counteract* the misuse and misinterpretation of what must have seemed to Fisher and other pioneer statisticians to be a simple set of ideas. Of course the originators of the concepts of the p -value and its far more invidious offshoot, the practice of reducing empirical comparisons to declarations of “statistical significance” (or otherwise), could not have anticipated how these ideas would become so embedded in the practice of nonstatistical researchers. As has been pointed out and will continue presumably to be analysed by the philosophers and historians of science, there seems to be an irresistible urge to encode scientific conclusion-making into a rule-based activity (Gigerenzer and Marewski 2015). This always seems curious to me because it appears obvious that conclusions about the empirical world can only be made tentatively (beware the black swan!). Thus inductive inference must always be couched in a language of uncertainty, in contrast with which the familiar phraseology of statistically based research (“an association was found; $P < 0.05 \dots$ ”, “no effect was observed”) just doesn’t make sense.

Can the general scientific usage of statistical inference methods be reformed? The outright ban on traditional tools such as the p -value and confidence interval by the journal *Basic and Applied Social Psychology* (Trafimow and Marks, 2015) has achieved some positive outcomes, by way of much broader recognition and discussion of the underlying problems (Ashworth 2015). It is less clear whether the quality of scientific inference within the pages of the journal has improved (Lakens 2016).

I believe that fundamental improvement will only be possible if and when we can agree on some broad principles about the inference task. In particular, we need to cultivate a viable language of uncertainty that is primarily focused on expressing uncertain knowledge conditional on observed data (Morey et al. 2016). To my mind the only general language that seems to have any reasonable chance of fulfilling this goal is the Bayesian use of probability. Genuine post-data conclusions seem to be possible only within a Bayesian or perhaps closely related paradigm.

However, I am well aware of the difficulties of this path. The overwhelming challenge is that as soon as we enter this paradigm we appear to require god-like knowledge of the “true” or at least “appropriate” model that should be specified, including prior distributions that will be needed to kick-start the uncer-

tainty calculus. The difficulty of creating broadly accepted conventions for how models should be specified and checked before any conclusions based on their application to the data at hand may be trusted often seems insuperable, despite some suggested strategies (Gelman and Shalizi 2013). To many, the danger of the rules becoming even more malleable—and so even more likely to allow researchers to put arbitrary stamps of statistical authority on the conclusions they would like to draw—under this paradigm than under the traditional muddled modes of p -value-based inference outweigh its compelling inherent logic.

Although I understand this point of view I just don’t see any real choice, as no one seems to be coming close to proposing a way of salvaging the traditional muddle. Indeed in a companion paper to this discussion (Greenland et al. 2016) we see a long list of misconceptions and misinterpretations, which it can be hoped scientists may start to avoid. Yet the length and complexity of the list itself suggests that the fundamental ideas that it seeks to clarify are so convoluted—and inherently unsuited to the task of uncertain inductive post-data inference—that a solution might only be possible with a more fundamental paradigm shift.

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