Reviewer’s report

Title: Reproducible and Transparent Research Practices in Published Neurology Research

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Reviewer: Ulrich Dirnagl

Reviewer's report:

Rauh et al. have 'cloned' the article and methods of Hardwicke et al. (referenced) to systematically review and meta-analyze transparency and reproducibility practices of articles published in neurology journals from 2014 until 2018. The methodology of Hardwicke et al. (which to my knowledge only exists as a preprint, not a peer reviewed article) as applied in the current study appears to be sound. This article seems to be part of a large scale investigation which applies the same methodology (and uses much of the same text) on at least 17 fields of medicine (from Addiction Medicine to Urology). The authors do not mention this approach in the article. The reference to the study materials is misspelled (BIOARKIV/2019/763730, should be BIORXIV/2019/763730). The random sample of 300 studies was drawn from an NLM catalog query (Neurology[ST]). Using this query, I retrieved 492 publications with an extreme bandwidth of types of study, audience, quality, etc. Talking a random sample of 300 publications from those journals (even if a portion of them is excluded due to language etc.) in my view is a gross undersampling, and may almost certainly lead to erroneous results. I doubt that from such an approach one can claim to present results that accurately and quantitatively reflect transparency and reproducibility practices in neurology in general. How stable would the results be if replicated on a different sample? And how relevant is it to lump clinical journals, in which opening up raw data is limited by data protection laws, and journals which almost exclusively use animals, where no such restrictions exist. In fact I doubt that such an omnibus approach to a huge and diverse field is very useful, even if done on a much larger sample. I agree with the authors that Neurology (clinical, experimental, computational, imaging, cognition...) has a transparency and reproducibility problem, but to expose it in such a diffuse manner will be rightfully criticized by many who don't feel that there is an issue. I therefore believe that the paper is potentially harmful to the quest to improve transparency. At a minimum, the article would require a much longer and more complete list of limitations. But taking those limitations seriously, I am afraid that what can be concluded safely from the article is rather minute.

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