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Invited Commentaries

Research credibility: the devil is in the details: a comment on Ihle et al

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Ihle et al. (2017) is a timely and important paper. Because the field of behavioral ecology expects and celebrates variability, variation should not necessarily be deemed wrong or irreproducible. Yet, to have faith in our results we must ensure best practices exist and are followed. Thus, arguing against a series of concrete suggestions, some of which seem to work in other fields, seems like arguing against (as an American) baseball, hotdogs, and apple pie! The devil, however, is in the details as well as the considerable inertia currently in the system. Cultures change when people benefit from changing their behavior; top down control rarely has the desired effects. I see some things that Ihle et al. (2017) suggest as in alignment with researcher needs (version control documentation, better archiving practices) and possibly more likely to “take”, while I see other things (pre-registration) a bigger up-hill challenge for widespread adoption.

Pre-registering future analyses and future experiments in a publicly accessible repository may provide novel ideas to others who can test them. Sometimes it takes a long time to properly evaluate a question (we are slow, unforeseen problems crop up, etc.). I see no mechanism to guard against this. Once published, I cannot imagine an editor retracting a paper because the author got the idea from a pre-registered study and was able to complete the work before the idea's originators. Science is productively competitive and this competition itself ensures quality. Long-term studies create additional difficulties; we collect data for questions we had not imagined at the start. Of course one could register the study right before actually conducting the analyses, but concerns about getting scooped remain.

The ideas of archiving data, documenting the workflow, and archiving scripts and results are important. As a co-author of the Mills et al. (2015) paper, and as a director of a long-term study, I have concerns with posting data that can be combined with other datasets to answer novel questions while we are still using the data. Embargo policies help address this concern.

Archiving analysis scripts and maintaining version control is fine but different people use different programs (SPSS, SAS, R, and bespoke software) software evolves, and it is sometimes difficult to get analyses running in subsequent years. While many of these issues can be addressed by working in the R environment, I do not believe we should be all forced to become programmers and work in any specific environment. It is an illusion that these files will be accessible forever; to my dismay I am now having problems opening old EXCEL files. The focus should be on documenting workflow and clearly explaining what specific analyses were planned and what analyses were follow-up.

I have had the unfortunate experience of a reviewer using our data and re-analyzing it in an obviously incorrect way and then using that as evidence the paper should be rejected. Our rebuttal was unsuccessful. Despite such unfortunate experiences, sharing data, scripts, and results should help improve the quality of the science and might even create new collaborations.

Blindness has been written about in the behavioral literature a lot! We know we should do it but often cannot. We should be nudged to report what, precisely, we did. The 21-word statement is an excellent idea, as are the pre-publication checklists that some journals have adopted.

Of course there are other things that happen in good behavioral ecology—instruments and people are calibrated, trained, and evaluated and this is discussed. A clear separation is made between original questions and follow-up questions that were stimulated by analyses.

We should all be concerned with increasing the quality and reproducibility of our work. Creating ways to incentivize people beyond tokens or badges is essential. Ultimately, even the best-documented and pre-registered study is a single study that is conducted in a particular place and at a certain time. I believe that comparative and meta-analyses help us explain behavioral diversity. Better data going into these will help identify robust patterns.

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Practical models for publishing replications in behavioral ecology: a comment on Ihle et al.

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