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Replication in behavioural ecology: a comment on Ihle et al.

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Ihle et al. (2017) have made a valuable contribution to a current debate on open science. The general sentiment that scientific disciplines should be reliable, reproducible and replicable should, of course, hold for behavioural ecology as much as it does for any other scientific discipline, and while the extent of some of the problems they describe in our field is a matter for debate, few would disagree that we should adopt practices that enhance the transparency and credibility of behavioural ecology.

One issue arising from Ihle et al.'s (2017) prospectus concerns their suggestions regarding replication. Behavioural ecologists study evolutionary adaptations of organisms to their environment, and variation is an inherent property of that dynamic process. This variation is observable at every level of biological organization, but we are particularly concerned by the causes and consequences of variation among individuals, populations and species. But, how replicable should we expect studies to be, and how do we interpret failure to replicate? For example, in studies of cooperatively breeding birds comparisons across populations have revealed drivers of cooperation and led to novel insights on benefits of helping (e.g. Reyer 1990; Koenig & Stacey 1990; Baglione *et al*. 2002; Sharp *et al*. 2011) because of population-level differences. Likewise, long-term studies of single populations have shown that while some effects may be stable, temporal variation in ecology or social environment may drive marked changes in helping behaviour (Koenig & Dickinson 2016). Thus, replicated analyses across populations or through time cannot be treated as validatory exercises. Inherent variation may be better understood and controlled in model systems, although even here replicated studies often produce inconsistent results in lab (Seguin & Forstmeier 2012) and field (Parker 2013), the basis for which may be uncertain. Therefore, while there is a strong argument to be made for greater replication, the motive for doing so should not necessarily be one of validation, as implied by Ihle et al. (2017). The strong theoretical basis to behavioural ecology makes it a robust, predictive science, but we should not lose sight of the fact that the dynamic response of organisms to their environment also makes it an essentially variable and often intriguingly complex one.

The other difficulty with replication is that virtually every new research programme, whether a grant application, PhD studentship proposal or new line of enquiry in an established study, starts with the question: is it novel or original and will it produce outputs that have some impact and advance the field? Rightly or wrongly, the incentive of funding agencies to direct resources towards replication is low, and for individual scientists (especially early career researchers trying to build a reputation for cutting-edge work) there may be little benefit from replicating previous studies; ironically, this may be particularly true if the same conclusion is drawn. This is not to deny the desirability of replication, but simply recognizes the realities of funding, career advancement and editorial practice.

The other recommendations of Ihle et al. (2017) largely formalize what should be regarded as good scientific practice, e.g. 'blinding' of experiments (even though this may not always be practical, especially in field studies), testing of *a priori* hypotheses, etc. The key change they advocate is in the transparency of the processes involved in the conduct of research; one could characterize this agenda as one of opening for scrutiny the lab and field notebooks in which are recorded the hypotheses we test, the predictions we make, the data we collect and use, and the analyses and interpretation of our results. One potential

benefit of this would be to improve the connection between data and results. I am probably not alone in sometimes being frustrated by the difficulty of relating the outcomes of statistical models in publications to the data they describe - the formal documenting of workflow and process of analysis would certainly increase transparency in this regard. Of course, this begs the question of where the onus lies to scrutinize preregistrations and workflows, and raises the concern that reviewer goodwill may be exhausted if their task is made more onerous by increasing journals' expectations of them. This will be a question at the forefront of editors' minds as moves to increase credibility in all scientific disciplines develop.

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