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REVIEWS

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Letter

Evidence Ranking Needs to Reflect Causality

Anne-Christine Mupepele[1,2,*] and Carsten F. Dormann[3]

In the article 'Evidence types and trends in tropical forest conservation literature', Burivalova *et al.* [1] present a classification of evidence and perform a literature review to assess whether the evidence in tropical forest conservation has changed over time. We argue that the evidence types presented by Burivalova *et al.* do not result in studies being classified according to their evidence, and their review does not conform to established criteria in evidence-based practice.

Evidence-based practice was originally introduced to improve health care outcomes by using the best available scientific evidence. From there, it spread to other disciplines, including conservation [2,3]. Fundamental to evidence-based practice is a hierarchy of study designs based on their strength to establish causality between an intervention and a measured effect [4,5]. Experience and correlation from unreplicated case–control studies and case reports simply provide less reliable evidence than replicated and randomised case–control experiments (RCT). This was also shown by simulating the predictive power of several study designs, including RCTs and uncontrolled case reports [6]. Burivalova *et al.* provide a classification of evidence types, distinguishing according to 'methodological rigor', and hence implicitly rely on the idea of evidence hierarchies. They broadly draw on existing evidence types and hierarchies (e.g., [5]). However, their evidence types do not respect differences in the capacity of studies to establish causality.

For example, Burivalova *et al.* distinguish two types of case–control studies. Both measure control and impact either by spatial (two different sites) or temporal (in a before–after design) separation and neither requires replication. They differ in so far as case–control II, but not case–control I, is meant to account for confounding variables. Accounting for confounding variables is crucial to elucidate causal relationships, but simply impossible when only one case and one control site are available, as allowed by Burivalova *et al.* Causality can be ascertained neither in unreplicated nor in confounded case–control studies. Quasi-experimental studies, (i.e., nonrandomised pre-post-intervention studies [7]), equally fail to rule out that unseen confounding variables led to the observed dissimilarity between control and treatment group (known as selection bias). Hence causality can only be established by sufficiently replicated RCTs. The authors further state that most quasi-experimental studies found in their review evaluate changes in deforestation as a result of a conservation intervention. We argue that these studies are purely correlative as they merely relate two variables that have changed over time.

Similarly, Burivalova *et al.* list meta-analysis as an evidence type, when in fact it is a statistical method to quantitatively synthesise effects across studies [8]. Using the term 'meta-analysis' in a wider sense, without strict reference to the definition, dilutes its quality and leads to conflicting overlap with systematic reviews. Systematic reviews, in contrast, are a description for a study design, which describes formalised stages taken in a review, such as publishing a protocol, conducting a systematic search, and selecting the eligible studies [9]. As such, meta-analysis should not be listed in the ranking of evidence, the same way as other statistical approaches are not ranked.

It is crucial to realise that the strength of evidence can not solely be identified by the underlying study design: even an RCT does not provide strong evidence if it is poorly implemented. Hence, evidence types must necessarily be accompanied by a quality assessment of the specific study, considering, among other things, the appropriate sample size and an adequate statistical analysis (as demonstrated in [5]). As a consequence, trends in the evidence types of the literature review of Burivalova *et al.* are tenuous because they do not account for the quality of study execution. Burivalova *et al.* combine RCTs (strongly causal) and quasi-experimental studies (largely correlational) and hence conflate evidence types with distinct power to detect causality. This may result in a seeming increase in strong-evidence studies, when in fact only correlational evidence has been amassed. The claimed increase in RCT and quasi-experimental studies is further based solely on a peak in 2015 (their Figure 1), and on the authors' eyeballing of the data, rather than a statistical analysis. It is ironic that the authors' interpretation that rigorous evidence types are replacing less rigorous ones is based on very weak evidence and falls short of the rigorous criteria the authors themselves seek to establish.

As we have argued elsewhere [10], there is no shortcut from individual studies to a reliable synthesis: study design, commonly listed in evidence hierarchies, and execution (i.e., the quality of the implementation; e.g., [5,11]) determine the strength of evidence. Communication with practitioners can be facilitated by identifying the strength of evidence (e.g., in four categories, as in [5]), ideally integrated in guidelines based on the best available evidence, similar to medicine [12]. All evidence should be used, but we need to be aware of its strength before acting.

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Letter

Reply to Mupepele and Dormann 'Evidence Ranking Needs to Reflect Causality'

Z. Burivalova,[1,*] D. Miteva,[2] N. Salafsky,[3] R.A. Butler,[4] and D.S. Wilcove[5]

In their letter, Mupepele and Dormann [1] argue that causality, in conservation effectiveness studies, can be established only by replicated randomized controlled trials (RCTs), whereas quasi-experimental studies, and studies where observable confounding variables are statistically considered, cannot. The authors suggest using an evidence classification that they had authored instead [2] and further argue that there is not enough evidence to show that, in tropical forest conservation studies, rigorous evidence is becoming more common over time.

Contrary to what the authors suggest, our evidence typology is not meant to be hierarchical [3]. Rather, we present it as a working typology of existing common types of studies in tropical forest conservation (with the exception of RCTs, which are as yet very uncommon, but are expected to become common for certain types of interventions). Our typology is based on those frequently presented and discussed in medicine, public health, and epidemiology, which