Received: 7 August 2022 | Revised: 30 August 2022 | Accepted: 31 August 2022
DOI: 10.1111/cobi.14007

Conservation Biology

COMMENT

Quantifying uncertainty about how interventions are assigned would improve impact evaluation in conservation: reply to Rasolofoson 2022

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Article Impact statement: We agree with Rasolofoson and encourage exploration of the biases introduced due to incomplete understanding of treatment assignment.

KEYWORDS
causal inference, counterfactual, hidden bias, impact evaluation, statistical matching

We fully endorse Rasolofoson's (2022) main point in his comment on our essay "Statistical Matching for Conservation Science" (Schleicher et al., 2020): scientists and practitioners using observational data for conservation impact evaluation need to pay careful attention to the process by which some units came to be exposed to the intervention and others did not (i.e., "the treatment assignment mechanism"). We also appreciate his excellent illustrative examples of this main point. We build on his argument by highlighting that the treatment assignment mechanism is rarely known with certainty and that additional analyses are needed to quantify the potential effect of this uncertainty on conclusions.

Conservation science as a discipline has been somewhat slow to embrace robust impact evaluations to advance effective policy and practice (Baylis et al., 2016). However, a shift is now underway. There has been an explosion of recent studies explicitly designed to quantify the effect of a conservation intervention by comparing outcomes with unobservable counterfactual outcomes (what would have happened in the absence of the intervention) (Ferraro, 2009). There is a range of methods for estimating these counterfactual outcomes when random assignment is not possible (Schleicher et al., 2020), of which statistical matching—the focus of our original essay—is the most common (Börner et al., 2020).

Statistical matching algorithms help researchers select control units that are as similar as possible to the treatment units with respect to observable attributes (Stuart, 2010). The most important observable attributes (known as confounders) are factors that affect both the outcome of interest and exposure (selection) to the treatment. Thus, as correctly highlighted by Rasolofoson, choosing appropriate control units requires a clear understanding of the mechanisms by which treatment units came to be exposed to the intervention. Although the key role of the treatment assignment mechanism is implicit in the text of our article, we realize that we were not as explicit as we should have been on this point. We fully agree with Rasolofoson that understanding the treatment assignment mechanism is a critical element when using statistical matching designs for causal inference.

The problem is that while matching on observable attributes is relatively straightforward, there will often be unobservable confounders that influence both selection to the treatment and outcomes of interest (Ferraro & Hanauer, 2014).

Rasolofoson emphasizes that researchers using matching can never be entirely certain that they fully understand the treatment assignment mechanism and have correctly measured and controlled for all possible confounders. We go further by stressing that uncertainty over the treatment assignment mechanism necessitates explicit analyses of the potential influence of hidden bias due to unobservable confounders. We did mention this issue in our original article, where we recommend that researchers assess the sensitivity of the postmatching estimate to the presence of an unobserved confounder and provide an example (see Figure 1 and check 3 on hidden bias in Table 2). Yet given how few articles in conservation science explore the sensitivity of results to hidden bias (including articles by some of us), we believe the topic deserves further attention.

When researchers use a statistical matching design for impact evaluation, they are assuming that once they achieve an acceptable balance in observable covariates, treatment assignment is "as-if random" (Cinelli & Hazlett, 2020). However, if unobserved confounders are unbalanced, any causal inference based on the difference in outcomes between control and treatment groups could be invalid. Thus, researchers ought to explore how fragile their results are to the potential presence of unobserved confounders (Ferraro & Hanauer, 2014). Such explorations are dramatically underutilized across scientific disciplines (Cinelli & Hazlett, 2020). In our original article, we provided one example of an approach to quantifying how results may change in the presence of a potential unobserved confounder (Rosenbaum bounds) (Rosenbaum, 2007). Yet, there are other approaches.

These approaches to assessing the sensitivity of the results to potential violations in the key causal assumptions used to infer causation from correlations proceed in one of two ways. They may start with the estimated impact and ask what happens to that estimate as the causal assumptions are relaxed (called "sensitivity tests to hidden bias" [Cinelli & Hazlett, 2020]). Or, they may start with the most unrestrictive causal assumptions that place bounds on the impact and then ask what happens to the bounds as assumptions are strengthened (called "partial identification" [Morgan & Winship, 2011]). McConnachie et al. (2016) provide a nice example of this in their use of the partial identification approach. Surprisingly, their article, published in this journal, has been seldom cited. A recent statistics article lays out some useful suggestions for how a such exploration of hidden bias can be done (Cinelli & Hazlett, 2020), but it is not very accessible to nonspecialists. Those looking for example conservation science articles that specifically explore the potential effects of hidden bias could refer to Ferraro and Simorangkir (2020).

Rasolofoson makes two other points. The first of these arises from a simple mistake in Schleicher et al. (2020). As Rasolofoson suggests, our text should have read "variables included in the matching process should not be affected by the intervention of interest".

In his final point, Rasolofoson suggests that our recommendation to select confounders and conduct quality checks before the outcome is explored contradicts our recommendation to run regression or principal component analyses involving the outcome variable to inform the selection of confounding variables to use in matching. Rasolofoson is correct that one should absolutely avoid the temptation to select confounding variables based on the sign, magnitude, or precision of the estimated impact. However, exploring relationships among potential confounders (e.g., to assess whether they are intercorrelated) and between confounders and outcome (e.g., to explore which variables appear to be most influential in affecting the outcome) can help improve the understanding of the study system and, used alongside theory and field experience, can inform a more robust study design. However, we should have clarified that such analyses should only use outcome data from the pretreatment period. We completely agree that estimation of the treatment effect should only be done after the matching analysis is complete and its quality is assessed on criteria that have nothing to do with the estimated treatment effect. We appreciate that Rasolofoson made this point salient in his comment.

Methods for impact evaluation from observational data are rapidly evolving and thus keeping up with the literature can be overwhelming. However, for conservation science to move effectively from a discipline that simply describes the causes and consequences of biodiversity loss to one that identifies solutions to nature and climate emergencies (Williams et al., 2020), conservation scientists and practitioners will need to become more familiar with counterfactual thinking and methods for impact evaluation. We appreciate Rasolofoson's engagement with our essay, which aimed to make statistical matching more accessible for those interested in conservation impact evaluation. We hope his comment and our response to it further accelerate the use of robust methods for answering the vital question: What works in conservation?

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How to cite this article: Gordon Jones, J. P., Barnes, M., Eklund, J., Ferraro, P. J., Geldmann, J., Oldekop, J. A., & Schleicher, J. (2022). Quantifying uncertainty about how interventions are assigned would improve impact evaluation in conservation: reply to Rasolofoson 2022. *Conservation Biology*, e14007. https://doi.org/10.1111/cobi.14007