

Reply to: Controlling for non-independence of nations should NOT be the default choice in
cross-cultural research

Scott Claessens^{1*}, Thanos Kyritsis¹, & Quentin D. Atkinson^{1,2*}

¹ School of Psychology, University of Auckland, Auckland, New Zealand

² School of Anthropology and Museum Ethnography, University of Oxford, Oxford, United Kingdom

*email: scott.claessens@gmail.com; q.atkinson@auckland.ac.nz

1 It is widely acknowledged that failing to account for non-independence in data can
2 bias statistical estimates and that national-level data often exhibit such non-independence.
3 In our recent paper¹, we show that, despite this awareness, most analyses of cross-national
4 variation in psychological values or economic indicators do not make any attempt to control
5 for non-independence, and for those that do, most methods deployed do little to reduce bias
6 in simulated datasets with non-independence due to proximity or shared cultural ancestry.
7 We further show that reanalysing a small sample of published datasets using the best
8 performing methods from our simulations can appreciably change parameter estimates.

9 In a *Matters Arising*, Akaliyski highlights potential issues with our study design,
10 distinguishes between different sources of confounding that can arise from
11 non-independence, and proposes causal models where controlling for non-independence
12 might actually bias, rather than help, inference. For these reasons, Akaliyski argues that
13 controlling for non-independence should not be the default analytic choice in cross-national
14 research.

15 We appreciate this commentary on our work. It is useful to discuss these
16 methodological issues in more detail to improve the rigour of cross-national research. That
17 said, we feel that the commentary misinterprets our argument in places and overstates
18 potential concerns.

19 It is worth clarifying from the outset that nowhere in the paper do we argue that
20 controlling for non-independence should be the default analytic choice in cross-national
21 analyses. Instead of proposing a one-size-fits-all solution, we explicitly recommend that
22 “individual studies must outline their own particular causal assumptions, which... can
23 then be used to design tailored statistical estimation strategies”¹ (p. 8). If controls for
24 non-independence are likely to bias inference, then they should not be included. However,
25 as we will see, we believe these scenarios to be much rarer in the real world than Akaliyski
26 claims.

The commentary raises concerns with our particular non-independence controls, our simulations, and our reanalyses. First, while it is interesting that Akaliyski² finds a reduced effect of geographic and linguistic distances in some regressions, our study and a large body of work^{3–10} shows that geography and language are important explanatory factors for a range of cross-cultural variables. Second, in our simulations we generated data with levels of spatial and cultural non-independence that were comparable to the strength of non-independence found in real-world datasets. We grant this does not prove that the controls for non-independence will work in real-world scenarios, but such a proof seems an impossibly high bar given that we can never know the true causal model in the real world. Third, we acknowledge in the paper that our reanalyses do not include all controls from the original studies, meaning that “we are unable to outright reject the claims from these studies”¹ (p. 9). Our more modest goal was to show that adding controls for non-independence in a stepwise fashion can have an appreciable impact on reported relationships in real-world data.

Table 1 in the commentary distinguishes between two sources of confounding that can arise from non-independence: confounding via diffusion (the eponymous Galton’s Problem) and confounding via third variables. This is a useful contribution, as in hindsight we did not clearly delineate these two sources of confounding. In the former scenario, we must control for non-independence to account for local and historical diffusion of outcome and predictor variables. In the latter scenario, we would like to control for confounds such as climate, physical topography, cultural norms, and institutions. If these variables are unobserved, then, to the extent that they are autocorrelated in space or down cultural genealogies, we can potentially use geographic and cultural phylogenetic distances to account for their influence. Of course, if these third variables are observed, then we should just include them directly: nowhere do we propose using controls for non-independence as stand-ins for available direct measures. But if we allow that unobserved confounds likely exist, then exploring the effect of including controls for non-independence seems prudent,

at the very least.

After defining different sources of confounding, Akaliyski proposes two causal models where controlling for non-independence might actually bias inference. Akaliyski's Models 4 and 5 illustrate scenarios where the predictor variable (X) is non-independent, but the outcome variable (Y) is not. Under such models, controlling for non-independence (Z) could either harm precision or induce bias. Akaliyski argues that models like these may be more common in reality than models where both variables are non-independent (Model 1) because processes of diffusion are more likely to influence predictor variables than outcome variables. If this is true, controls for non-independence may harm inference more often than they will help.

However, compared to Model 1, Models 4 and 5 make much stronger and more unrealistic assumptions about the causal relationships between national-level variables. These models assume that spatial proximity and cultural ancestry influence the outcome variable solely through the predictor variable ($Z \rightarrow X \rightarrow Y$) and not through any other causal paths. This seems to us an unlikely scenario. It is very likely that outcome variables will also be affected by spatial proximity and cultural ancestry, either directly or indirectly through their effects on unobserved variables.

We can illustrate this by expanding Akaliyski's example of collectivism predicting mask wearing, using generative simulations to inform our intuitions (Figure 1). If shared ancestry only influences mask wearing through collectivism as a mediator, as Akaliyski claims, then controlling for shared ancestry does slightly harm the precision of estimates (Model 4a in Figure 1). But shared ancestry also likely influences many other national-level variables that have been shown to be related to COVID-19 preventative behaviours, including but not limited to cultural tightness¹¹, long-term orientation¹², uncertainty avoidance¹³, religiosity¹⁴, and trust in science and government¹⁵. If we have relevant data on all of these variables, then we can control for them directly, but if any are unobserved

then it is necessary to control for shared ancestry to reduce bias (Model 4b in Figure 1).

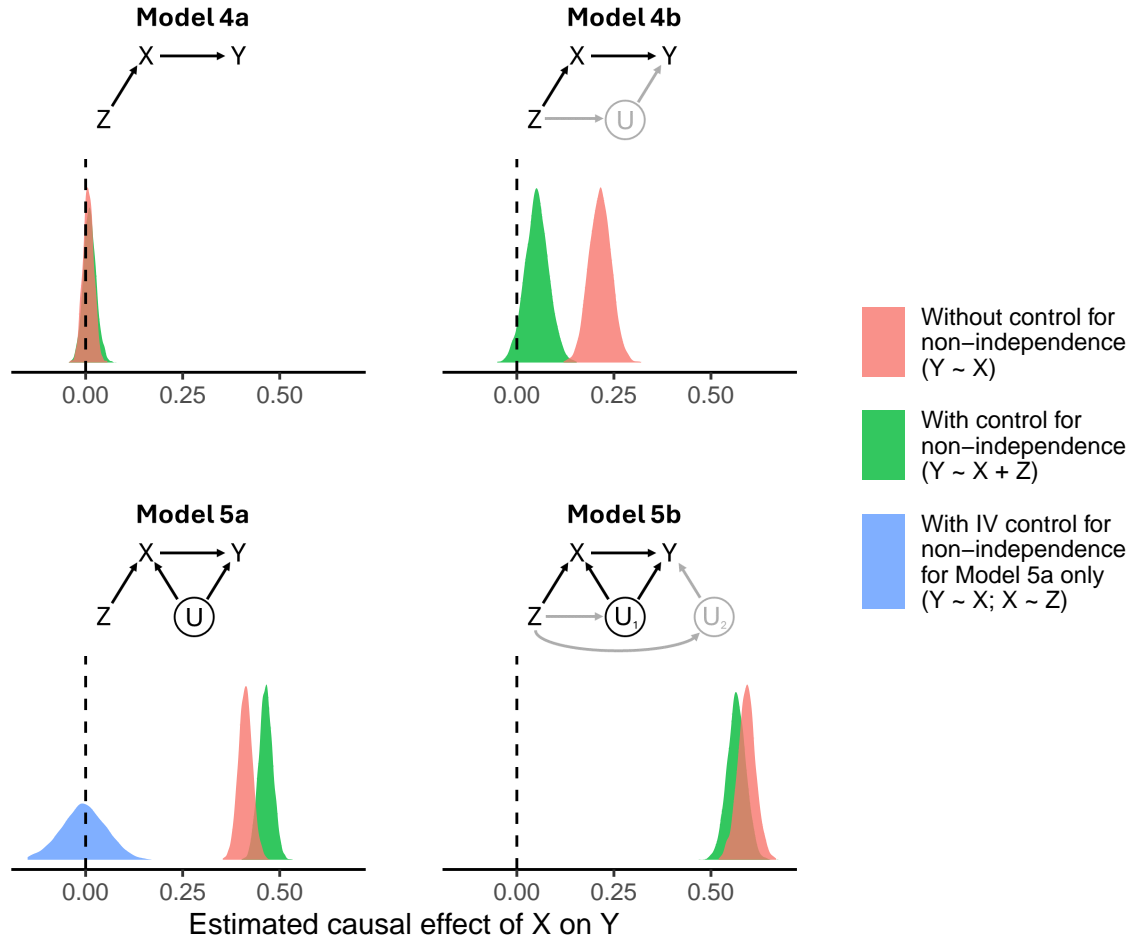


Figure 1. Results of simulations testing the implications of controlling for non-independence under different generative causal models. In all simulations, the true causal effect of X on Y is 0. All other causal paths are set to 1. Z represents spatial or cultural phylogenetic non-independence. All other variables are standard normal variables. $N = 2000$ in each simulated dataset. Densities show the posterior causal effect of X on Y from models fitted without controls for non-independence (red), with controls for non-independence (green), or with a control for non-independence included as an instrumental variable (blue). The IV control is only fitted to data generated from Model 5a, as Z only meets the criteria for an instrumental variable under this generative causal model.

Akaliyski then introduces an unobserved variable — perceived pathogen threat — which influences both collectivism and mask wearing. Indeed, under this generative model, controlling for shared ancestry results in bias amplification¹⁶ (Model 5a in Figure 1). But again, this causal model is unrealistic. If we allow that perceived pathogen threat could be influenced by shared ancestry¹⁷ and that shared ancestry could influence mask wearing

indirectly through any of the unobserved variables listed above, then it is necessary to control for shared ancestry to reduce bias (though bias still remains due to the unblocked backdoor path $X \leftarrow U_1 \rightarrow Y$; Model 5b in Figure 1).

Even in situations where Models 4a or 5a do hold, we can still use controls for non-independence to estimate unbiased causal effects. For example, in Model 5a, Z fits the criteria for an instrumental variable: Z is independent of unmeasured confounds and only influences Y through its effect on X ¹⁸. In this situation, including Z as an instrumental variable adjusts for unmeasured confounding, producing an unbiased estimate of the causal effect (Model 5a in Figure 1).

In sum, while Akaliyski's commentary illustrates several theoretical cases where controls for non-independence could hinder inferences, we contend that such cases are likely to be rare in practice. This is because national-level predictor and outcome variables (even contemporary variables) are likely to be influenced by a host of factors that are spatially and culturally patterned. Even in rare situations where such theoretical cases hold, controls for non-independence can still be useful to include if implemented in a manner consistent with the underlying causal assumptions (e.g., as instrumental variables). Thus, we still think it critically important researchers consider that cross-national analyses require additional controls to account for the non-independence of nations.

Data Availability

All simulated data in this manuscript can be reproduced using the code on GitHub:
<https://github.com/ScottClaessens/crossNationalCorrelationsReply>

Code Availability

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Author Contributions Statement

SC and QDA wrote the original draft of the manuscript. SC ran the simulations and created the figure. All authors reviewed and edited the final draft of the manuscript.

Competing Interests Statement

The authors declare no competing interests.

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