

Response: Pollet - [016.Vidi.175.343](#)

I am thankful and honoured that all 3 reviewers (R's) praised my academic achievements. Moreover, the outlined research was positively reviewed (A+/A+/A-B). Having said this, the reviewers, in particular R3, also raised some relevant issues and I am grateful for the opportunity to address these points in more detail here. As at least two of the R's themselves indicate, none of the issues raised affect the core of the outlined research programme. Given that more substantial points were raised by R3, and to a lesser extent R2, this response is structured accordingly. Needless to say, I appreciate all suggestions, and thank the reviewers for their constructive comments.

R1.

R1 is unequivocally positive, but indicates a potential improvement under '[Final assessment](#)'. Specifically, R1 suggests that, apart from investigating structural factors, the project can be expanded to examining neighbourhood social factors (e.g., [social capital](#)). This is an excellent suggestion and, where available, I will include these.

R2.

R2 argues that the proposal would benefit from further [theorising](#) and lists two suggestions: one under '[quality of the proposal](#)' and one under '[knowledge utilisation](#)'. Specifically, R2 suggests: **(1)** extending the theoretical underpinnings for the mechanisms at multiple levels (in particular salience) and **(2)** considering different forms of prosociality in different contexts.

1. I fully agree with R2 that one can further theorise on the **mechanisms** at play. In particular, he/she points to the role of salience (also covered in ref.[20](#) in the proposal). Some theory on (psychological) mechanisms was already considered. For example, a cultural learning mechanism, lies at the heart of Study 5, where the role of acculturation as a mechanism is explicitly tested. Moreover, the proposed set of studies presents a necessary first step to uncovering the mechanisms proposed by R2. Having said this, to fully examine all possible mechanisms at play, one ideally needs large-scale, longitudinal data over an entire life course. Within the scope of a VIDI, it would be very hard to collect such data. Yet, relevant secondary data on potential mechanisms are available from various sources (e.g., the [National Child Development Study](#) / [Millennium Cohort Study](#)), but these do not offer the Dictator (DG) measure. Thus, after establishing childhood and neighbourhood Socio-Economic Position (SEP) effects with the DG, I argue that the project can be expanded in such a direction. However, before closely examining the full range of potential mechanisms via which childhood SEP and neighbourhood SEP effects operate we need to first establish that neighbourhood and childhood SEP do **actually** influence prosociality. It is this particular knowledge gap that my proposal addresses.

2. This comment is interrelated to the above point, with R2 arguing for a finer-grained situational approach, and considering other forms of prosociality. As R2 suggested, the project can be easily expanded to investigate other forms of prosocial behaviour (for example via use of the lost letter method, ref.[114](#)).

In conclusion, the project can be logically expanded in the theoretical directions suggested.

R3.

R3 raises [three methodological points](#): (1) sample size (statistical power), (2) selectivity, and (3) the use of the DG, in particular with online samples. Interestingly, R3 acknowledges that these issues are [addressable](#). Furthermore, these issues were actually considered in the proposal.

1. In the **Bayesian** framework I proposed, (frequentist) statistical power is rarely presented, as it hinges on p -values, which is [problematic](#). Traditional power analyses are further complicated by the [non-normal distributions](#) and the multilevel designs. Therefore, I explicitly chose not to present traditional power analyses. However, contrary to R's claim, the sample sizes have been argued for throughout the [proposal](#). For example, the number of countries and cities R3 refers to is based on a simulation study, cited in the original proposal (ref.[117](#)). Throughout, the samples are based on previously published studies and/or this simulation study (ref.[117](#)). Where doubt exists, I proposed piloting the procedure (via [simR](#) crude estimations can be obtained to plan the studies). Moreover, apart from power, other factors, such as cost/feasibility, were considered. Finally, I argue that the proposed samples are well in line with the literature (e.g., ref.[114](#)). In summary, the proposed minimum sample sizes are, contrary to the R3's claim, both logical and well-argued for.

2. R3 raises the issue of selectivity. In particular, he/she points to the **mechanism** of mobility between neighbourhoods accounting for neighbourhood effects. I actually explicitly discussed [this issue](#), as well as potential solutions. Perhaps surprisingly, some novel methods could address the **plausibility of causality**, even when faced with this issue. These methods include '[ecological inference](#)', and [Bayesian SEM/ networks / directed acyclic graphs](#). Therefore, I am optimistic that, even if such selectivity occurs, some scenarios can be shown to be more plausible than others. For example, one could model how large mobility effects would have to be to account for the observed effects. Finally, and most importantly, R3's claim, that selectivity would affect the results, is tacitly **assuming** that there is a neighbourhood effect to begin with. Note that the current research gap is not what the key mechanism (e.g., mobility) is via which neighbourhood effects occur, but rather whether there is **any neighbourhood effect at all**. As a whole, this set of studies will provide invaluable insights on the degree to which different levels can account for variation in prosocial behaviour, as acknowledged by R1-R2. In summary, while I do fully share the concern about potential mechanisms, as discussed under R2, the project can be further expanded in that direction. However, it is necessary to examine the degree to which different levels can account for the observed patterns, rather than assuming they do so and are affected by selectivity.

3. R3 questions the use of the DG measure and particularly the use of this measure with online, crowdsourced, samples. Firstly, the DG measure is amply used and the use of this measure **ensures comparability** to the established literature (e.g., ref.[28](#)). As discussed under the reply to R2, the project can be further extended to accommodate other forms of prosocial behaviour. However, the research gap I aim to fill centres on the DG measure. Hence, this is why this measure was chosen. Secondly, it is important to highlight that R3's

comment regarding the online samples **only** applies to Studies 1-3, rather than the entire proposal. My primary reason for using such samples is that these are commonly used in prosocial behaviour research (e.g., refs. [92,100-101](#)), thus ensuring comparability between prior work and my own results. Thus, in contrast to what R3 suggests, such samples have been widely used and form an established method (also see refs. [37-38,91](#)). Moreover, the costs of these samples are substantially lower compared to non-crowdsourced alternatives, such as [Flycatcher](#). However, I am able to apply to my university's equipment fund to pay for the use of Flycatcher, as I do feel the crowdsourced samples could be complemented in this way. Nonetheless, I consider the use of crowdsourced populations to be actively beneficial in this case, as it ensures comparability with existing datasets, and the ability to make such comparisons is an explicit goal of the project. In summary, the methodological choices in terms of both the measure used and the samples are motivated by the established literature in this research area. Moreover, I reiterate that the comment about online crowdsourced samples **only** applies to a subset of studies and these samples can be complemented.

Conclusion

All 3 R's recognised the existing knowledge gap. R1 and R2 offered suggestions, which form logical extensions of the project. While R3 indicated potential methodological issues, these are issues I have considered. Moreover, as R3 suggests, these design issues are addressable. Moreover, at best, R3's concerns deal with specific studies, which do not affect the proposal as a whole. Therefore, this proposal will provide invaluable insights on this research gap in the study of prosociality.