

Reply to decision letter reviews: #181 R&R

We would like to thank the editor and the reviewers for their useful suggestions and below we provide a detailed response as well as a tally of all the changes that were made in the manuscript. For an easier overview of all the changes made, we also provide a summary of changes.

Please note that the editor's and reviewers' comments are in bold while our answers are underneath in normal script.

A track-changes comparison of the previous submission and the revised submission can be found on: <https://draftable.com/compare/PWlpEtwZxXxU>

A track-changes manuscript is provided with the file: "PCIRR-RNR2-Epley-et-al-rep-ext-manuscript-v3-QX-trackchanges.docx"

Summary of changes

Below we provide a table with a summary of the main changes to the manuscript and our response to the editor and reviewers:

Section	Actions taken in the current manuscript
General	-
Introduction	R2: We removed the statistics from previous literature.
Methods	R2: We included sample size justification as laid out in our last response letter. We also corrected the mistake in our hypothesis section.
Results	-
Discussion	-
Reporting	R1: We included the exploratory analyses as suggested.
Supplementary materials	-

Note. Ed = Editor, R1/R2 = Reviewer 1/2

Response to Editor: Prof. Chris Chambers

The two reviewers from the first round kindly returned to evaluate your revised manuscript. Both are broadly satisfied with your revision and response, and there are now only a few minor matters to address. There remains a point of disagreement noted in John Protzko's review, for which he suggests a simple resolution that I think is sensible, and will help reach Stage 1 acceptance without further in-depth reiew. I look forward to receiving a final minor revision to address these remaining points from both reviewers.

Thank you for the reviews obtained and the invitation to revise and resubmit. We amended according to the received feedback, and answer item by item below.

Response to Reviewer #1: Dr./Prof. John Protzko

I am fine with the changes the authors propose except one. Their respectful disagreement about running a supplementary analysis removing the ‘free will’ item from the anthropomorphism scale. I, likewise, disagree (respectfully) with their response. It is not just a possibility of autocorrelation, but people who believe strongly in free will may be likely to give high ratings when asked about free will in general (a common method bias). All I ask is a supplementary analysis removing that one item. I would be happy even if the authors gave it a caveat such as: “At the insistence of one reviewer, we run a completely exploratory and supplementary analysis where we eliminated the free will item from this anthropomorphism scale, for this lone reviewer's concern. This additional analysis shows...” and then fill in the rest.

Thank you for the suggestion and further elaboration on your concern. We followed your advice and included the suggested analysis as part of the exploration:

“One reviewer suggested we examine whether, and to what extent, free will belief is related to anthropomorphism when items tapping free will are removed from those anthropomorphism measures. We conducted these exploratory analyses. (Report results)”

Otherwise, I look forward to seeing the results of this manuscript.

Thank you for your time and effort reviewing our submission. We look forward to your feedback at Stage 2.

Response to Reviewer #2: Dr./Prof. Marieke Wieringa

I would like to thank the authors for taking my comments into consideration for the new version of their manuscript and their elaborate answers to my concerns in the response letter. The new version of the manuscript provides a much clearer argument for the replication of the original study by Epley et al. (2008), though I do in fact also agree with the point that the authors raise in the response letter that “replications are worthy on their own so long as they are methodologically rigorous” (p. 12, response letter). Additionally, the authors now frame the FWB extension as exploratory and will only include correlations between FWB and anthropomorphism. Thereby, they have addressed my concern about the claim of the unique predictive power of FWB. Furthermore, in my previous review, I raised the concern of oversampling. The authors explain in the response letter their decision not to follow the suggestion. I understand the desire to err on the side of caution given that published effect sizes are generally overestimated. Given the large discrepancy between the sample size that was suggested by G*Power and the chosen sample size, I would, however, suggest to include the explanation given in the response letter on the rationale behind the n=1000 in the manuscript (i.e. “Effect sizes in the literature are generally overestimated. Since our budget allows us to collect the number of participants that we originally planned, we prefer to err on the side of caution to ensure that we have sufficient power to detect the actual and potentially small effect sizes and provide a more accurate effect size estimate. A large planned sample size can also help compensate for failed attention checks and make it possible to conduct well-powered moderator analyses (e.g., whether the order of completing the measures has an effect”, p. 15, response letter).

Thank you also for your valuable feedback. have included the rationale in our Participants section:

“Since effect sizes in the literature are mostly overestimates (Button et al., 2013), we decided to err on the side of caution and collect a much larger sample of 1,000 participants, the maximum allowed by our budget, which ensures that we have good power to detect potentially small effect sizes and provide a more accurate effect size estimate. Meanwhile, a large sample size can also help compensate for failed attention checks and make well-powered moderator analyses possible (e.g., examining whether the order of measures has an effect).”

Finally, I have two minor points that I noticed in the current version of the manuscript:

On page 3, I am not sure whether it is necessary to state the t-values and p-values reported in the Open Science Collaboration (2015). They do not add much more information that is not already mentioned in text (that they provided insufficient evidence that participants assigned to a social disconnection condition had stronger belief in supernatural beings) and they look a little out of place in the introduction. On page 13, the authors accidentally wrote ‘loneliness’ twice: “In line with the original study, we hypothesized that loneliness is positively associated with loneliness.”

Thank you for the suggestion and for catching the error. We have removed the statistics from the introduction and corrected the typo.

In conclusion, I am happy with the way in which the authors addressed my review and I think that the overall manuscript is even more clear than its previous version. I only have minor suggestions this time around. I wish the authors good luck in the data-collection, data-analysis and write-up phase.

Thanks for the constructive feedback. Much appreciated.