

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).

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.”

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.