

Reply to decision letter reviews: #162-RNR1

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/IqKckHyrnYnt>

**A track-changes manuscript is provided with the file:
"PCIRR-RNR2-Lerner & Keltner (2001)_ Replication and Extension main manuscript-track-changes.docx"**

Response to Editor: Prof. Chris Chambers

Three of the four reviewers kindly returned to evaluate your revised manuscript. The reviews are broadly promising, with a few outstanding issues concerning the rationale for the replication, the determination of the SESOI, and whether a pilot study may be required (although I am personally cautious about the use of pilots in establishing effect size estimates). I would like see your response to these points before issuing a final decision

Thank you for the reviews obtained, your feedback, and the invitation to again revise and resubmit. We tried to explain our position on the points raised, and we hope that you and the reviewers would find those reasonable. We did our best to address the points made and explain our position and decisions, given the feedback that was given.

Response to Reviewer #1: Dr. Kelly Wolfe

Thank you for the work you've put in to address my comments on the proposed project, and the changes that you have made to your manuscript. Of the 8 points I raised during the last review, I have 2 points remaining; 1 is advice, and 1 point is a remaining concern. Please see below for details.

Thank you for the positive opening note, your time in providing us with a second review, and the detailed feedback.

Comment 1. I have read the work you included in your reply. In the paper, the two were found to be similar. Other works that I have read that compared undergraduate student and MTurk samples have reported both differences and similarities, and it seems to me that the context of the study matters in terms of the similarity between these populations. I also noticed that reviewer 4 shared some concerns about the extent that the original sample and proposed sample might differ in their perception of event ambiguity. I am satisfied with the response to my comment, but I do advise to keep those concerns in mind when critically considering (potential) differences between the original and proposed sample when interpreting the results of the proposed study (as you communicated in your response to my comment).

Yes, thank you. We noted this as a planned discussion in the discussion section with a placeholder, and this was noted as a deviation throughout.

Comment 2. I welcome the added explanation in the manuscript concerning the change in the number of people affected in the adaptation of the Asian Disease Problem. However, I remain concerned about the change in the number of affected people (which I will from now on refer to as *need*). I have no doubt that the adaptation would result in a successful framing effect in the same direction. However, increasing the need may lead to a larger effect, which seems relevant for this study (compared to others) as it is a replication effort.

In [the paper you included in your response](#), the researchers examined whether participants took more risk in scenarios that included higher need (i.e., a larger number of people affected in the scenario). Though the direction of the framing effect is the same as when need was small, the results showed that need did matter:

“While small variation in numbers did not significantly affect risk-taking, we found that the factor need did. When need is high, that is when the overall

number of people in need is high, the subjects tend to be more risk-averse” (p. 538).

“The subjects produced more framing effects when more people were affected in a scenario. This became even more obvious in condition High. This interaction effect may suggest that losses loom larger than gains depending on the size of a needy population.” (p. 539).

This to me illustrates that need should be taken into consideration and indicates that the size of the effect found using the proposed scenario may differ from that of the original study, simply because of the change in need. As such, I am unconvinced that the change to the scenario wouldn’t affect the replication effort and agree with reviewer 3 (Dr Sobkow) that a pilot study would be worthwhile. In this pilot study, you could present the original Asian Disease Problem and the suggested adaptation and examine whether the two scenarios indeed yield similar responses (e.g., an effect size of $d \leq 0.2$).

This is important, and we appreciate the opportunity to clarify this further. We also realized we were not accurate enough in the way we described this change.

Our point was not that there are not going to be differences in impact, but rather that we made the adjustment hoping that the effect would be more likely to appear with the adjusted numbers. It would not make sense to make this deviation if we did not think this mattered, we could have easily included the original version and accept the likely possibility of a weak effect given the pandemic. As we explained, we are worried that given the current pandemic the original numbers would not be taken seriously, as our world has become tragically numb to risk of losses involving “small” numbers of hundreds of deaths. The purpose was to make the manipulation stronger and relevant.

The citation you provided is in support of the needed change, and is appreciated. In our revision we included the references and explained this in more detail.

We revised/added the following in the “Risk preference” section (changes underlined):

The adapted version of Asian Disease Problem has been used in other studies and was found to yield framing effect in the same direction as the original version (e.g., Dylman & Champoux-Larsson, 2020; Feldman et al., 2016; Miozzo et al., 2020). Larger framing effects were reported when the more people were affected in the problem (Diederich et al., 2018). Thus, our modification of the problem was meant to compensate for the potential weak effect of framing due to the likely impact of the pandemic in the decreased sensitivity to the loss of hundreds of lives.

Response to Reviewer #2: Max Primbs

I thank the authors for the responses to the reviews. I'm mostly satisfied with the changes the authors made. Some minor remaining points below:

Thank you for the positive opening note, your time in providing us with a second review, and the feedback pushing us to try and do better or clarify our arguments.

1) I can follow your reasoning for the choice of replication target. Although I still don't think that the section is strong enough (basically the same reasons as in my earlier review). You still do not describe why replicating this is e.g., important for theory.

Likewise, the section on practical implications (p.8) still is written on a very general level and does not spell out the practical implications of their work. I would not recommend rejection based on this, but I do believe that the authors should very carefully consider their choices of replication targets and build a strong argument for their choice.

Apologies, we very much appreciate your feedback, and I know this is meant in good spirits to help us. We would have like to do better here, we really would, but we are again not sure how to satisfy this comment/request. We need concrete examples and guidance to improve, otherwise we risk doing much worse. We are not sure what you and/or the editor would consider stronger and important for theory, especially in the context of replications, as this is a very subjective matter.

In his previous decision letter Recommender/editor Prof. Chris Chambers wrote:

“Replications require no additional justification over and above any other kind of research, and unlike some journals, we do not evaluate Stage 1 RRs on the basis of the perceived importance or value of a research question (but rather, the scientific validity of that question).”

In our previous reply we asked for clear editorial guidelines and that we were hoping for “citations and examples from the literature, preferably from published Replication Registered Reports, of how this was applied for replications.”. We cannot address broad generalized statements about something as grand as “theory” and a general request to make a claim stronger without any specifics.

Our subjective view on the issue of theory in replications is that we would very much rather not try and overclaim implications for theory with a single replication. Our attempted replication may help gain a more accurate estimate of the effect size, informing our priors about the phenomenon in a certain context, and adding insights and nuances from our extension.

What we wrote in the previous reply to the decision letter still applies and is relevant for the issue of theory: “We are happy to address this comment further given clear editorial guidelines, and we would ask for citations and examples from the literature, preferably from published Replication Registered Reports, of how this was applied for replications.”

2) "We found it difficult to understand whether the bottom line we should take from this comment is that you think we are too well-powered or not powered enough, or maybe a completely different point entirely. Clearer guidelines and constructive examples would have been helpful. We looked at the citations and we did not find a way to tie those into our replication effort, nor were we able to find any instances of replications making use of this paradigm."

Let me try and rephrase: If you argue that a particular effect size is your smallest effect of interest, then in my view you need to answer the question why that is the case. Merely saying that this corresponds to a small effect in social psychology does not do the trick. If your argument is that it is the SESOI because it allows for a fair test to detect the original findings (paraphrased from your response letter), then add that to this section and make it explicit.

We happily added a sentence indicating that we believe this is a conservative test of the target phenomenon. The link with weak effects in the literature was exactly about that point. We believe that this is a stronger argument than our subjective claim of what we may feel is a fair test (in the “Power analysis” section):

Therefore, we set these very conservative effect estimates as our Smallest Effect Size of Interest (SESOI) that we considered would serve as a fair test for detecting the target phenomenon.

We also understand and appreciate you wanting to push us to do better given that you care about everyone doing better on this point. As we explained, and in the passage you quoted, we asked for your help in providing us citations and examples from other replications or literature that implemented this, or if those are not available then more concrete advice from you or the editor on how to implement this kind of change and what is expected here, especially given that this is a replication.

Response to Reviewer #4: Dr. Karolina Scigala

The authors addressed my comments very well and I do not have any further comments. I am looking forward to seeing the results.

Thank you for the very positive opening note, and for your comments and suggestions.