

Reply to PCIRR S2 decision letter reviews #772: Lerner & Keltner (2001) replication and extension

We would like to thank the editor and the reviewers for their useful suggestions and below we provide a detailed response to each item. We also provide a summary table of changes. Please note that the editor's and reviewers' comments are in bold with our reply underneath in normal script.

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

A track-changes manuscript is provided with the file:
PCIRR-S2-RNR-Lerner-Keltner-2001-replication-main-manuscript-trackchanges.docx

[We note that we are not familiar with the titles and ranks of the reviewers, and looking for that information proves tricky. To try and err on the side of caution, we refer to all reviewers with the rank Dr./Prof. . We apologize for any possible misalignments and are happy to amend that in future correspondence.]

Reply to Editor: Prof. Chris Chambers

Two of the reviewers from Stage 1 were available to evaluate your completed Stage 2 submission. The reviews are broadly very positive, which is much as I expected based on my own reading of your submission. Among the suggestions for revision you will find requests for clarification of terminology and interpretation of results, as well as a useful suggestion for a (light) additional analysis. I look forward to receiving your revised manuscript and response in due course.

Thank you for the reviews obtained, your feedback, and the invitation to revise and resubmit. We provide a detailed list with responses and actions below. We hope these will prove satisfactory.

Reply to Reviewer #1: Dr./Prof. Kelly Wolfe

Thank you for your submission of this Stage 2 Registered Report, in which you have conducted a close replication study (with extension) of Lerner and Keltner (2001). I reviewed your Stage 1 manuscript and am pleased to have reviewed your Stage 2 manuscript as well and see the outcomes of your efforts.

Overall, I think it is a very good study, well-executed with a very transparent approach to the materials and reporting of the results. I have some smaller comments that I believe could improve the readability of the paper, and some may clarify aspects of the manuscript, which you can find below.

Thank you for the positive and supportive opening note and for the constructive feedback.

Larger comments

.1. There are so many results that are discussed in the manuscript, which can be a bit confusing at times. I'd suggest including the hypothesis numbers with the outcomes to make it clearer to the reader which hypothesis the results refer to (e.g., H1).

Response: This is a good suggestion that will help with readability. Thank you.

Action: In parentheses, we now include the hypotheses that are related to the specific result as outlined in the results section. The hypothesis numbers can be found in Table 1. Two additional hypotheses (8 and 9) are identified explicitly in the "Extension: Dispositional hope" section.

Smaller comments

.2. (p. 14) The section on pre-registration and open science has missing information (currently in highlighted in red already).

Response: This is indeed the case but the reason is entirely practical. Namely, this information can only be updated *after* Stage 2 PCIRR endorsement. Upon such eventuality, we will update these sections.

Action: Currently, no action is possible due to practical considerations (see above). To minimize any further confusion we now clearly state, in red, that this info will be updated after Stage 2 endorsement. Specifically, we say:

[To be updated after Stage 2 endorsement:] It has then gone through peer review and officially endorsed by Peer Community in Registered Reports ([Endorsement citation will be provided after Stage 2 endorsement]; [Endorsement link will be provided after Stage 2 endorsement]).

.3. (p. 28) Missing word (or two) in the second line of the second paragraph (“Consistent with... $p < .001$).

Response: Good catch, thank you.

Action: We now say:

“Consistent with the expected valence of emotions, we found support for positive correlations between two of the the negative emotions, anger and fear ($r = .54, p < .001$), between two of the positive emotions, happiness and hope ($r = .69, p < .001$), and for negative correlations between each negative emotion and each positive emotion (anger and happiness: $r = -.47, p < .001$; anger and hope: $r = -.27, p < .001$; fear and happiness: $r = -.68, p < .001$; fear and hope: $r = -.52, p < .001$).

.4. (p.32) The results mention risk-taking and risk preference interchangeably a couple times (both on page 32). Risk preference and risk-taking are similar but separate concepts; risk preference can be defined as the propensity to engage in behaviours or activities that are rewarding yet involve some potential for loss, such as activities associated with physical and mental harm to individuals (Mata et al., 2018). Risk is assumed to underlie, or explain, risk-taking behaviour, as risk-taking is an expression of someone’s preference or attitude towards the risk itself. As such, I would remove the mention of risk-taking in this section and replace this with risk preference.

Response: Agreed. Thank you for the comment.

Action: We have now replaced all instances of “risk-taking” with “risk preference” in this paragraph. There are no other instances of “risk-taking” in the manuscript.

.5. (p. 41) On page 41, you mention the following: “The subscales of the risk optimism reached higher reliability scores than in the analysis above...”. To me, it is unclear what is meant with above. I assume it means the approach to calculating (un)ambiguity in the main analysis. If so, I would refer to it in such words instead, so it is clearer to the reader.

Response: Agreed. Thank you for the comment.

Action: We now specifically say:

“The subscales of the risk optimism reached higher reliability scores than in the main analysis where ambiguity of events was based on the categorization from the target article (ambiguous events: Cronbach alpha = .76; unambiguous events: Cronbach alpha = .78).”

.6. (p. 46) You mention that the effect of framing in the Pandemic Problem scenario may have been affected by the COVID-19 pandemic, as this was a recent real-world scenario that involved large numbers of people dying. Is there any published work on whether people’s perception of numbers indicating losses, or framing of losses, has been affected?

Response: Thank you for this suggestion. Although our initial write-up was speculative we now looked for such published evidence and indeed there does appear to be some indicative evidence.

Action: We now say:

“This may indicate that the responses to the pandemic problem have been affected by the COVID-19 situation. Even after expanding the figures, the real world’s loss may have changed people’s perception and understanding of the pandemic problem. For instance, Hameleers (2021) replicated the predictions of prospect theory in times of a pandemic, but they did not find consistent support for the effects of gain versus loss framing on support for specific policies that aim to prevent negative outcomes.”

Reply to Reviewer #2: Dr./Prof. Max Primbs

Summary of the Article:

The conducted Registered Replication Report for the foundational Lerner & Keltner paper by 2001., which investigated associations between dispositional fear, anger, happiness and risk optimism or risk preference. The replication features an adapted design, improved analyses, and an extension (hope). The replication was partially successful.

Summary of the Review:

I recommend minor revisions. The authors report their results and conclusions faithful to the Stage 1 RR. Moreover, the track changed manuscript does not show that the authors unduly altered their introduction and methods sections. I have a few small comments:

Thank you for the positive and supportive opening note and for the constructive feedback.

Minor points:

1. I commend the authors for setting a SESOI. However, it would be even better if the SESOI was used more throughout the manuscript. Currently, the reporting of results very much focusses on statistical significance. I would suggest that the authors make use of their earlier work and discuss how their results relate to the SESOI.

2. Relatedly to 1, the authors present a series of null results for their hope extension. I would suggest that the authors make use of equivalence testing to increase the informativeness of these nulls. Equivalence testing rules out a few explanations for these null results.

Thank you for the suggestion. We were debating as to how to address that, and finally decided against further reference to the SESOI. We should note that we are not merely using significance testing, but reporting effect sizes and confidence intervals.

About your suggestion - Given that this is a replication and a registered report, we note that we registered and received an IPA of the aim to conduct the test using the same terms used by the target article, albeit with higher power to detect even smaller effects. The SESOI we set was for the power analysis to ensure that effects as small as the SESOI would be detectable when tested with null hypothesis significance testing against the null. Our understanding was that you were

suggesting that we use the same SESOI when evaluating lack of support for rejecting the null, to conduct an equivalence to see whether the effect is lower than the SESOI. This gives the SESOI a different meaning, and using it as such is a stricter test than was planned or intended in replicating the target's. However, this can be achieved by comparing the upper confidence interval of the effects that were not detectable by NHST to the set SESOI of $r = .12$. Given that we planned and registered the NHST approach we opted to keep the current use of NHST. Confidence intervals around the detected effect should aid readers in interpreting whether the effects (or their higher confidence interval) are below what they would consider SESOI in the sense of a meaningful effect that would render the effect no different than the null.

3. Related to 2, I feel like the discussion of the results of the hope extension falls flat. The authors write that there are many possible reasons: Discuss those reasons! Why does the framework not extend to hope? And why and to which other positive emotions does it extend (the negativity bias discussion is good!)? And what makes these other positive emotions different than hope? More broadly, I often miss a discussion of what the results mean for the ATF – for both the successful and the failed replications.

Response: This is a valuable comment and we understand that more could be said here. Therefore, we now expand on this discussion, specifically mentioning research that points to the multifaceted role of hope and how this may pertain to ATFs predictions. Please also note that we have added specifications, where possible, in the general discussion on the implications of the results for the ATF.

Action: We now specifically say:

“While there could be many reasons on why we failed to demonstrate consistent effects between hope and our target decisions one potential reason could be that research is still nascent when it comes to this specific emotion and we may simply lack enough understanding on how best to measure the emotion, as well as how the emotion presents. For example, it has been suggested that hope carries mixed and complex feelings of distress and worry (Halevy, 2017) and that it is distinct from other positive emotions (Rosler et al., 2016). It may thus be that hope is a multifaceted emotion and particularly context dependent. This would imply that any outcomes as predicted by the appraisal tendency framework would not be straightforwardly related to hope. The target article discusses whether fear and anger only bias the perception of negative events, while leaving the option of whether only negative emotions bias the risk perception through their cognitive appraisal themes untested. We may thus also need further studies (e.g., including more distinct emotions) to test whether the appraisal-tendency framework only,

or more strongly, applies to negative emotions. One should note here the prevailing presence of a negativity bias in that negative, compared to positive, emotions may be more strongly felt, more easily induced, and thus may display stronger and more consistent downstream effects on decision making (cf., Joseph et al., 2020).”

Really minor points:

4. The authors report e.g., on page 32 and following results in both text and table form. The table contains the CI in addition. I feel like we can shorten the manuscript by moving the tables to an appendix and/or reporting the CIs in text.

Response: We tend to prefer tables and text over text, given that they allow readers (and researchers, such as meta-analyzers) to easily find all they need to understand the overall findings when those are summarized in one table. Given that there are no space limitations, we see no reason to shorten the article.

We are open to amending this given editorial guidelines.

5. The authors report Cronbach’s alpha (e.g. page 20). Current papers recommend McDonald’s Omega instead (<https://www.tandfonline.com/doi/full/10.1080/19312458.2020.1718629>). The authors might consider adding this to the manuscript.

Response: Thank you for the suggestion.

Action: We have now added information on McDonald’s Omega next to the Cronbach’s alpha calculations directly in the text. We hope that this gives the reader even more information on the reliability of the underlying scales.

6. I applaud the transparency and usefulness of Table 7.

Thank you for the kind words.

7. Page 49: You mention that whether the ATF has constraints on population generalisability still needs further investigation. I’d ask the authors to discuss this a bit more: Why would there be constraints theoretically?

Response: We now spell this out more clearly as, indeed, the previous version may have left things a bit too vague. Primarily, our idea was that given that the ATF relies on cognitive appraisals of emotions, such appraisals may differ across cultures meaning that the same emotions may entail different appraisal tendencies.

Action: We now specifically say:

“Whether the appraisal-tendency framework has constraints on population generalizability still needs further investigation among a larger population. As the ATF relies on cognitive appraisal of emotions, these could differ cross-culturally thereby impacting the appraisal tendencies associated with the emotions. For example, there is evidence that cultural factors influence appraisal biases, affecting emotional responses in similar situations across different cultures (Scherer & Brosch, 2009) with other evidence pointing to sizable differences between different regions when it comes to general appraisal tendencies (Scherer, 1997).”

8. Page 11: Your description of what got Ethics approval changed. Why?

You mean “The project ~~is part of a larger replications project that~~ received ethical approval from the University of Hong Kong Human Research Ethics Committee (EA210265).”?

I think perhaps at the time of revisions, we were trying to avoid the appearance that some replications might be missing from this report, given that the other replications referred to were completely unrelated to this one. They just happened to share the same umbrella ethical approval. This is a rather minor point, so we changed it back:

The project is part of a larger replications project that received ethical approval from the University of Hong Kong Human Research Ethics Committee (EA210265).

9. Make the number of total trials and the number of trials per ambiguity level clearer in the main text. The way it is presented in Table 4 was not super clear.

Response: Thank you for indicating this. We now make this clearer in the text. We do however hesitate to refer to these as “trials” as the answer DVs form part of questionnaires. We also now make it much clearer to the readers that the full details and exact question wordings are available in the supplementary material and where they can find it.

Action: Specifically, we now say:

“Participants then rated events optimism and indicated risk preferences in randomized order, with a follow-up section examining either their perceived controllability or certainty of the same events. Overall, each participant provided risk optimism estimates for each of the 23 events (7 ambiguous and 16 unambiguous as per the target article classification) and they provided risk preferences for both the gain and loss frame. Finally, there were funneling, demographics information, and debriefing sections. Please

see the details and all measures in the supplementary materials (section “Materials and scales used in the replication and extension experiment”).”

I congratulate the authors on their manuscript and regret that I cannot offer more content-related comments.

Thank you for the kind words.