Action-Effect Meta-Analysis

Response Letter to the Invitation to Second Round Minor Revise and Resubmit

We appreciate the helpful feedback and we are glad that reviewer were satisfied with our previous resubmission. We provide a summary of changes table below addressing the additional feedback provided. The editor's and reviewers' comments are in bold whereas our responses are in normal fonts.

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

A track-changes manuscript is provided with the file: PCIRR-RNR2-Action-Inaction and Emotions-meta analysis-Main Manuscript V9-G-TC.docx

Summary of Changes

Section	Actions taken based on comments from the reviewers
Method and Results	DQ: Added statements regarding assumptions of sunset plot analyses EC: Added a statement regarding sensitivity analyses in Method EC: Described simulated results of sensitivity analyses (excluding d_z effects) EC: Replaced "multivariate three-level model" with "three-level model" and revised statements about moderator analyses
Supplementary	EC: Added tables of sensitivity analyses results
Code	EC: Added files (code and outputs) for sensitivity analyses

Note. Editor: CC = Prof. Chris Chambers, Reviewers: DQ = Prof./Dr. Dan Quintana, EC = Prof./Dr. Emiel Cracco

In addition to the above changes based on reviewers' comments, we made some minor changes throughout the manuscript – e.g. updated some parts of the manuscript based on recent findings, improved clarity of the introduction, revised some numbers in the (simulated) results section as it appears that we forgot to update some numbers when we revised the code and analyses last time, change in the first author's current affiliation, and grammar changes.

Response to Editor Prof. Chris Chambers

> **The three reviewers from the previous round kindly returned to evaluate your revised submission, and I'm happy to report that all are broadly positive. There remain some minor matters to resolve concerning the potential inclusion of sensitivity analyses, details of analysis plans regarding moderators, and clarification of assumptions. These should be straightforward to address in a final Stage 1 revision.**
>
> **Following discussion among the Managing Board, I can now also report the bias control level that has been determined for your submission under the PCI RR taxonomy. In reaching this decision we considered carefully the arguments you put forward for Level 6 based on your correspondence of 16 July 2021. The consensus view among the Managing Board is that meta-analyses, systematic reviews, scoping reviews, and systematic maps can never achieve Level 6 under the PCI RR taxonomony because, unlike studies that will generate new data, the data that furnish these article types must already exist, even if not fully observed, analysed and interpreted. Most such submissions will achieve Level 3, 2 or 1 because at least some of the included data are likely to be in the public domain and will have been at least partially accessed by authors. In your case, because your meta-analysis includes some of your own authored work, for which you have not only accessed but necessarily observed the data at least partially, we have determined that your submission achieves Level 2 (keeping in mind that where a study includes elements at multiple levels, as your study does, it is PCI RR policy to assign the lowest level of applicable bias control). Because of the already rigorous methodological requirements for meta-analyses, systematic reviews, scoping reviews, and systematic maps at PCI RR, we are, however, waiving the usual requirement for additional stringent analytic corrections for potential bias that normally apply at Level 2. This means that you can proceed with your study as proposed and it will achieve a Level 2 designation. This decision from the Managing Board is final, but if you have any questions then feel free to contact me.**
>
> **Provided you are able to address the reviewers' points in a revised manuscript and response, in-principle acceptance should be forthcoming without requiring further in-depth Stage 1 review.**

Thank you very much for the positive comments and we are happy that you and the three reviewers are satisfied with our revision. We have addressed the reviewers’ suggestions in this revision.

Response to Prof./Dr. Dan Quintana

> **The authors have provided a comprehensive response to my intial queries, for which I am satisfied.**

Thank you for your helpful comments and suggestions.

> **I only have one more very minor suggestion. Regarding the sunset plot analyses (page 49 and figures 3 and 4), it should be noted in both the text and the figure captions that these power analyses assume that their respective effect sizes used in the sunset plot power analysis (i.e., -0.13 and 0.15) are indeed the true effect sizes.**

Thank you. We added this point in both the main text and the figure notes.

> "(which assume g = -0.13 and g = 0.15 to be the true effect sizes for comparison studies and experimental studies respectively, as reminded by the reviewer Dan Quintana)" (Publication Bias section of Results)
>
> "Note that these power analyses assume that g = -0.13 is the true effect size." (Figure 3 caption)
>
> "*Note.* The above power analyses are based on the assumption that g = 0.15 is the true effect size." (Figure 4 caption)

Response to Prof./Dr. Priyali Rajagopal

> **I think that the authors have done an excellent job addressing the issues that I raised in the previous round. The revision is clearer with appropriate justification for their areas of focus and clearer boundary conditions.**

Thank you for your helpful feedback. Glad to hear.

Response to Prof./Dr. Emiel Cracco

Thank you for addressing my previous comments. I think the adjustments have improved the manuscript.

Thank you for your constructive and helpful comments.

I appreciate the authors' adjustments to the effect size calculation and see their point. Nevertheless, if d_z and d_{av} are mixed together in a single meta-analysis, then I think sensitivity analyses should be added that test if the results change if d_z effect sizes are excluded. The authors note that differences between d_z and d_{av} are usually quite small, but this depends strongly on the correlation between the paired measures. When this correlation is high, the difference between d_z and d_{av} can be quite substantial. I realize that discarding effect sizes is far from an ideal solution, but at least it would give some (even if flawed) indication of the extent to which differences in effect size computation contribute to the results of the meta-analysis. Again, I think this is especially important for H3, which I think will almost certainly show that within-subject designs produce larger effect sizes, simply because of differences between d_z and d_s.

Thank you very much for your reasonable and important suggestion. Reflecting more, we agree that sometimes there can be substantial differences in effects between d_z, d_{av}, and d_s. We added the following statement in the Method section:

> "As suggested by the reviewer Emiel Cracco, we plan to conduct sensitivity analyses excluding studies in which we are only able to calculate d_z, as sometimes there can be substantial differences between d_z and other types of d. We plan to report results before exclusion and after exclusion of studies with only t-statistics and sample size information."

Also, we have added sensitivity analyses (based on simulated data) of effects, moderator analyses, and publication bias tests results to both the main manuscript (descriptions of findings) and the supplementary (results tables – Table 9, Table 10, and Table 11). These changes are tracked and will be replaced by real data and interpretations based on real data at Stage 2.

Moreover, we added the following in the Discussion (Limitations and future directions) for addressing this potential issue:

> "[If there are meaningful differences in findings before and after excluding studies in which we are only able to calculate d_z, we will address and discuss the issue. In any case, we will emphasize that future studies should report both *M* and *SD*, with effect sizes, so that more accurate effect size estimates can be obtained]"

I might be missing something here, but it is not clear to me how multivariate three-level models can account for confounding between moderators (and I don't think they automatically do that). Unless multiple moderators are included together in a

> **single model, how could these models account for confounding between said moderators?**

Thank you for the comment.

Three-level models indeed do not account for potential confounding relationships between moderators (but take account into the possible relationships between effects of the same article, article as the third level variable, Cheung, 2019).

We removed “multivariate” from “multivariate three-level model” and corrected related statements regarding moderator analyses throughout the manuscript.

As you suggested, we had already included analyses to test the possible associations between moderators (Supplementary Table 7, Hofmann et al., 2010; Lipsey, 2003) to address this potential issue.

References

Cheung, M. W. L. (2019). A guide to conducting a meta-analysis with non-independent effect sizes. *Neuropsychology Review*, 1-10. https://doi.org/10.1007/s11065-019-09415-6

Feldman, G. (2022). *Response to reviewers/decision-letter Collaborative template*. [Work in Preparation]*

Hofmann, W., De Houwer, J., Perugini, M., Baeyens, F., & Crombez, G. (2010). Evaluative conditioning in humans: a meta-analysis. *Psychological Bulletin, 136*(3), 390. https://doi.org/10.1037/a0018916

Lipsey, M. W. (2003). Those confounded moderators in meta-analysis: Good, bad, and ugly. *The Annals of the American Academy of Political and Social Science, 587*(1), 69-81. https://doi.org/10.1177/0002716202250791

*We wrote this response letter with reference to this template.