**Peer Review and Communication History**

**MS Title:** Discrimination, Life Stress, and Mental Health among Muslims: A Preregistered Systematic Review and Meta-Analysis

**Author Names:** Ummul-Kiram Kathawalla and Moin Syed

**Submitted:** April 13, 2021

**Editor First Decision: Revise & Resubmit**

June 21, 2021

Dear Ummul-Kiram Kathawalla,

As you now, I have taken over handling of your Stage 2 Registered Report submission, “Discrimination, Life Stress, and Mental Health among Muslims: A Preregistered Systematic Review and Meta-analysis”, because the previous handling editor is no longer available. This is not ideal, especially for a Registered Report, but I have done my best to bring myself up to speed with the manuscript’s history and the commitments made by the previous editor. That said, I want to begin by saying that if any points I raise contradict a decision made by the previous editor, please notify me and do not make the change I requested. I also want to apologize for the unusual length of time it took to make a decision on this submission.

I have now received a review from one of the original reviewers of your Stage 1 RR submission. I am very grateful to this reviewer for contributing even more of their time to this process, and for doing such a thorough job in their review. I also independently read the manuscript before consulting this review. The reviewer and I agree that your manuscript is quite close to being publishable as is, but we both identified some issues that need to be addressed. I therefore encourage you to submit a revised version to Collabra: Psychology. Assuming you can address the issues raised here and in the review, I will not send the new version out for review, and I will do my best to make a quick decision on it.

In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewer’s comments, outlining each change made in your manuscript or providing a suitable rebuttal.

There were three somewhat significant issues with the discussion, to my mind.

First, the concern that the associations you were studying were likely inflated by shared (self-report) method variance only got one sentence of discussion, pretty far down in the limitations section. To my mind, this is one of the most important limitations, and probably should be discussed more than just in the limitations section. In my view, this is crucial context for interpreting the effect sizes of your meta-analyses, and should be a part of the main discussion of your findings.

Second, you also briefly raise the issue of causal inference in the limitation section, but I think this issue also needs to feature more prominently throughout the discussion. For example, in the section titled “Theoretical considerations” beginning on page 98, you discuss some important possible interpretations of your results. Many of these implicitly or explicitly rely on assumptions about causal effects of discrimination or life stressors on mental health. You use some causal language in this section (e.g., “may impact” “effects of stressors”) but you do not make these assumptions explicit. I would like you to open this section with an explicit discussion of what assumptions you need to make about causal effects for these theoretical implications to hold, and how likely you think these assumptions are to be valid. I would also like you to include an explicit discussion of alternative plausible causal mechanisms. In other words, pleaes make sure the reader is given as much information as possible regarding the assumptions you are making about the causal nature of the effects, and reasonable challenges to those assumptions, before discussing the theoretical or practical implications of your findings.

Third, I am a bit concerned that your claims about the lack of evidence for publication bias, in both the results and discussion sections, may be overstated. Here my ability to request changes is limited by the agreed-upon protocol for examining publication bias. You use funnel plots and another technique that I am not familiar with. My uneasiness comes from the fact that my understanding of the literature on the detection of and correction for publication bias makes me wary of any strong conclusion ruling out publication bias. Any two approaches would seem inadequate to me as the basis for a clear and strong conclusion that results are not affected by, or are robust to, publication bias. I would strongly prefer if you qualify these claims (e.g., on pages 47, 64, 76, 87, and 102) to make it clear that you only used two tools to detect and correct for publication bias, and that the literature on techniques for detecting and correcting for publication bias suggests that many of the tools available may not fully account for all sources of publication bias. In short, I think it is premature to conclude that there is nothing at all to worry about with publication bias.

I agee with the reviewer that there were quite a few grammatical errors in the new parts of the manuscript. Some of these are quite important as they change the meaning of the text. For example, on page 26 you write “studies were excluded when zero-order correlation coefficients could be calculate” - I assume you mean “could not be calculated”. There were quite a few other instances like this throughout the new text. I won’t list all of the ones I found here, but I would like you to have at least two people carefully proofread the manuscript and look for grammatical errors. I will, however, list a few of the ones that I am most anxious to see fixed. First, in the sentence that the reviewer pointed out was missing a word on page 102 “publication bias a significant moderator only…”, in addition to the missing word, the variable “publication bias” should actually be “publication status”. In addition, later on that page, you write “However, publication bias did not appear to affect the meta-analytic estimate, likely because 83% of the samples were from published studies.” This sentence only makes sense if you change “publication bias” to “publication status” - you could in principle detect serious publication bias problems even if 100% of samples were from published studies, but publication status could not be a moderator in that case. Second, in the section on limitations to causal inference on page 103, you write that “due to the causational nature of the data” - this should be “correlational nature”. (Also, “causational” in other places (e.g., “causational claims”) should be “causal”.)

In addition to being a concern in and of themselves, because they can lead to misleading sentences or claims and can make the paper harder to read, these kinds of errors in the text also make me nervous about how accurate the numerical reporting - I understand that grammatical errors slip in even if authors pay a great deal of attention to detail, but I would still strongly recommend that you also have at least one person who was not responsible for making the tables and figures reproduce and check the numbers in the tables and figures as well as the numbers reported in the text. I realize this is a big ask.

In summary, I think this is a very strong Stage 2 Registered Report manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to resubmission.

If you have any questions or difficulties during this process, please contact the editorial office [editorialoffice@collabra.org](mailto:editorialoffice@collabra.org).

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Simine Vazire  
Editor in Chief  
Collabra: Psychology

# Reviewer 1

##### Open response questions

### Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

After some friendly reminding by the editor, I am happy to provide my third review of this registered report in Stage 2. This manuscript is a formidable achievement and I appreciate the work the authors have put in! I am more than happy to recommend this for publication given the current version and its history. I’ve done a meta-analysis before, but a RR meta-analysis takes the meticulousness to the next level. Well done!

I evaluate the manuscript from a methodological point of view and am not an expert on the specific content topic. I did my best to provide a thorough review given the time available to me and the length of the manuscript.

All my comments are for consideration, but none of them should be considered blocking - although I strongly encourage the first point that follows below.

1. The dataset on OSF is currently password protected. This is not reported in the manuscript, and it reads as if the data are freely available on the OSF. Given that it’s archival research and (parts of?) the data are included in Table 1, it may be worth considering removing the password protection? I have been unable to attempt to reproduce the presented results, which I would’ve liked to do as part of my review.
2. Some of the references to files on the OSF throughout the manuscript could be supplemented with direct links to the relevant files - currently the links refer to the project or provide no link at all. This may help readers find your results more easily (e.g., “see supplemental Table S2 on OSF for RVE results” without a link)

## Other (minor) points

* There are a few crossed out words spread throughout the manuscript, some formatting issues, and some sentences missing words (e.g., p102 “publication bias a significant moderator only in the life stressors dataset”. May be worth proofreading the manuscript one last time for these minor things?
* Throughout the tables there are p-values of 0 included. For those that are reference groups, it can be eliminated altogether and for those that are rounded, I would suggest to use “p<.001” instead
* Table 12 denotes the RVE setting as p, whereas earlier in the manuscript it is denoted as ⍴ (rho), which confused me for a second.

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  |  | ✔ |

**Author Response**  
Sept 7, 2021

Dear Dr. Vazire,

Thank you for your thoughtful review of our manuscript titled, “Discrimination, Life Stress, and Mental Health among Muslims: A Preregistered Systematic Review and Meta-Analysis.” We appreciated the comments provided by you and the previous reviewer and believe they have further improved the manuscript. We are also grateful for your flexibility in the timeline for returning the revision. We address each comment below, in turn. We hope you will agree that the manuscript is ready for publication and will be a solid contribution to the field.

Thank you for your time and consideration.

Sincerely,

Ummul Kathawalla and Moin Syed

1. *First, the concern that the associations you were studying were likely inflated by shared (self-report) method variance only got one sentence of discussion, pretty far down in the limitations section. To my mind, this is one of the most important limitations, and probably should be discussed more than just in the limitations section. In my view, this is crucial context for interpreting the effect sizes of your meta-analyses, and should be a part of the main discussion of your findings.*

Thank you for bringing up this concern. We agree that this is a major limitation of the study and is worth more attention. We have now added a full paragraph that discusses this issue, including some reasons why we think the concerns could be mitigated. Including this discussion has made the paper much stronger.

2. *Second, you also briefly raise the issue of causal inference in the limitation section, but I think this issue also needs to feature more prominently throughout the discussion. For example, in the section titled “Theoretical considerations” beginning on page 98, you discuss some important possible interpretations of your results. Many of these implicitly or explicitly rely on assumptions about causal effects of discrimination or life stressors on mental health. You use some causal language in this section (e.g., “may impact” “effects of stressors”) but you do not make these assumptions explicit. I would like you to open this section with an explicit discussion of what assumptions you need to make about causal effects for these theoretical implications to hold, and how likely you think these assumptions are to be valid. I would also like you to include an explicit discussion of alternative plausible causal mechanisms. In other words, please make sure the reader is given as much information as possible regarding the assumptions you are making about the causal nature of the effects, and reasonable challenges to those assumptions, before discussing the theoretical or practical implications of your findings.*

Thank you for mentioning this concern. It is helpful to have an outside perspective as we had thought we had appropriately discussed this problem. We want to be sure that readers are clear that we cannot establish a causal association. We have gone through the full manuscript to eliminate or soften causal language, and have included a much more in-depth discussion of the different causal possibilities. Again, we feel that inclusion of this discussion has greatly strengthened the paper.

3. T*hird, I am a bit concerned that your claims about the lack of evidence for publication bias, in both the results and discussion sections, may be overstated. Here my ability to request changes is limited by the agreed-upon protocol for examining publication bias. You use funnel plots and another technique that I am not familiar with. My uneasiness comes from the fact that my understanding of the literature on the detection of and correction for publication bias makes me wary of any strong conclusion ruling out publication bias. Any two approaches would seem inadequate to me as the basis for a clear and strong conclusion that results are not affected by, or are robust to, publication bias. I would strongly prefer if you qualify these claims (e.g., on pages 47, 64, 76, 87, and 102) to make it clear that you only used two tools to detect and correct for publication bias, and that the literature on techniques for detecting and correcting for publication bias suggests that many of the tools available may not fully account for all sources of publication bias. In short, I think it is premature to conclude that there is nothing at all to worry about with publication bias.*

As we are both very aware of, and concerned by, publication bias, we appreciate your perspective that our claims may be overstated. We have gone through the results section at the pages listed to add a qualifier that the claims made are based upon the “current methods used to detect and correct for publication bias.” We also added in more discussion of these concerns in the discussion section. As a point of clarity on your comments, we did not only use two approaches to assess publication bias, rather we used four: 1) we examined differences between effect-size estimates of unpublished and published studies, 2) visually examined funnel plots and forest plots, 3) conducted Egger’s test, 4) and conducted selection models as sensitivity analyses using weighted correlations. That latter approach is currently the approach believed to be the strongest test for publications bias. We acknowledge that these methods have their limitations, but our goal was that by using a variety of approaches, we would be able to more accurately detect and correct for potential issues. All of these tests pointed in the direction of publication bias not being a major concern with the data that we analyzed. Nevertheless, we agree that we cannot rule it out completely, and have been clear about that in the revision.

4. *Grammar and Typos*

Thank you for bringing to our attention the specific grammatical errors, as well as pointing out your concerns. We have made all the edits for the specific errors you mentioned. We both carefully read through the manuscript, and also worked with a professional copy editor who edited the manuscript for grammatical errors and improved clarity. Overall, the changes were minimal, but we believe improved the quality of the manuscript. Finally, a member of our research lab who is familiar with meta-analysis output checked all of the numbers within the manuscript and tables to ensure accurate reporting. We appreciate your suggestions as these additional steps have greatly improved the manuscript.

5. *The dataset on OSF is currently password protected. This is not reported in the manuscript, and it reads as if the data are freely available on the OSF. Given that it’s archival research and (parts of?) the data are included in Table 1, it may be worth considering removing the password protection? I have been unable to attempt to reproduce the presented results, which I would’ve liked to do as part of my review.*

Thank you for this comment. We have removed the password from the spreadsheet.

*6. Some of the references to files on the OSF throughout the manuscript could be supplemented with direct links to the relevant files - currently the links refer to the project or provide no link at all. This may help readers find your results more easily (e.g., “see supplemental Table S2 on OSF for RVE results” without a link).*

We appreciate this great suggestion and have added in direct links to the OSF files for all supplemental tables and figures.

*7. Throughout the tables there are p-values of 0 included. For those that are reference groups, it can be eliminated altogether and for those that are rounded, I would suggest to use “p<.001” instead.*

Thank you for this great suggestion. We have removed the p-values from the reference groups and changed the p-values of 0 in the table to “<.001”.

*8. Table 12 denotes the RVE setting as p, whereas earlier in the manuscript it is denoted as ⍴ (rho), which confused me for a second.*

Thank you for noticing this typo. We have corrected the tables so that it is the correct symbol, ⍴.

**Editor Final Decision: Accept**

Sept 7, 2021

Dear Ummul,

I have now had a chance to read over your revised Stage 2 Registered Report manuscript. Thank you for your responsiveness to the points raised by me and the reviewer in the last round of peer review. I am happy to say that your paper is now officially accepted for publication in Collabra: Psychology. Congratulations on this excellent work, I think it will make an important contribution to the literature and I look forward to seeing it published! I hope your experiences with Collabra: Psychology have been positive and that you will continue to consider it as an outlet for your work.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point. Our managing editor will contact you in case there are any pre-prodution file related questions. You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely,  
Simine Vazire  
Editor in Chief  
Collabra: Psychology