Author’s response to reviews

Title: Perceived Stress and Diet Quality in Women of Reproductive Age: A Systematic Review and Meta-Analysis

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Author’s response to reviews:

Point by point response to the Editor and Reviewers

Dear Dr Naude,

We would like to thank you and the reviewers for taking the time to evaluate our paper. We felt very grateful for the positive comments and the invitation to resubmit. Please find attached the resubmission of our manuscript entitled “Perceived Stress and Diet Quality in Women of Reproductive Age: A Systematic Review and Meta-Analysis”. Below we detail our responses to Associate Editor, Reviewer 1, and Reviewer 2.
Changes are highlighted in yellow in the main manuscript.

Reviewers comment

Associate Editor

1. PRISMA is a reporting guideline for systematic reviews (SR) and meta-analysis of randomised controlled trials and not a methodological guide for SRs of observational studies. Reporting guidelines for meta-analysis of observational studies exist (MOOSE). Reporting guidelines should be used to guide reporting and not be referenced as a methods guide.
Response:
We thank the Associate Editor for raising this valuable point. We have now revised the flow chart and included both the MOOSE flowchart (figure 1 on page 8) and MOOSE checklist (Appendix 2).
2. The Methods section needs a lot more detail
- Some methods are reported briefly in the Results section instead of the Methods section (e.g. meta-analysis methods)
  Response: We agree with the associate editor and have now provided additional details in the text. The paper now has a meta-analysis section in the methods part (line 177) and a meta-analysis section in the results part as recommended (line 270).

- Results are reported in the Methods section (e.g. quality evaluation)
  Response: We thank the associate editor for highlighting this and this data have been moved to the results section (line 308).

- Eligibility criteria: please include the criteria used for study designs that were eligible for inclusion
  Response: We thank the associate editor for raising this point and have now made this clearer in the manuscript (line 133).

- This section does not contain any information on measures of association, data synthesis, statistical approaches (assessment of heterogeneity, publication bias etc.) and other important components of the methods. A detailed description of all methods used is needed to drive decisions around pooling of data and to underpin the validity of the results.
  Response:
  We agree with the associate editor about the importance of reporting this information and have now provided additional details (such as assessment of heterogeneity, publication bias, risk of bias, sensitivity analysis, etc.) in the manuscript.
  A detailed analysis was added in the methods section to explain the data synthesis (line 178). The statistical methods that have been used within the meta-analysis such as assessment of heterogeneity, and publication bias have been included in the methods section for the meta-analysis (lines 181 and 186, respectively). We have explained the methods such as using the transformation to Fisher z scale, identifying the outlying studies, and we have calculated the prediction interval of the pooled effect to underpin the validity of the results along with the corrected pooled effect (line 303).

- The inclusion of 16 studies that did not measure diet quality but rather food intake and frequency of consumption is not aligned with the title and the research question (PEO), as no measure of diet quality or pattern were estimated or reported from this data.
  Response:
  The primary focus of this study is perceived stress and diet quality. This why the title matches the PEO. You are correct that we included the 16 studies on food intake and frequency of consumption, whereby this approach broadens the evidence. All 16 studies in Tables 1 and 3 were included because they met the inclusion criteria (line 131). You have made a valid point that these studies did not measure diet quality, but rather measured the consumption of specific dietary nutrients and food groups. These observations above highlight our inferences made in the discussion section of the manuscript where we mentioned that diet quality/patterns have been heterogeneously reported in the literature, thus making it difficult to draw a solid conclusion about the association between stress and diet quality/patterns (line 485).
Our points are also corroborated by reviewer’s 1 comments. We have now added some text to the results section (line 200) and discussion section (lines 456) to enhance clarity.

3. The quality evaluation should be reported in detail for each included study, and included in the recommended GRADE assessment
   Response: We thank the associate editor for raising this important point. We have sought advice from systematic review experts in the field and following your recommendation, we have now used the Cochrane domain-based risk of assessment tool; ROBINS-I tool for non-randomised studies as a quality evaluation tool. Cochrane Collaboration has adopted the principles of the GRADE system for evaluating the quality of evidence for outcomes reported in systematic reviews, and a recommended tool for assessing risk of bias is always domain-based evaluation.

4. Prospective registration (for example on PROSPERO) should be described and if not done, should be stated as a limitation.
   Response: We agree with the associate editor about the importance of clarifying this point. We have now added that prospective registration was not undertaken for this systematic review in the limitations section (line 475).

Reviewer #1
The authors of the present study conducted a review and meta-analysis examining the relation between perceived stress and diet quality in women of reproductive age. I very much enjoyed reading this thoroughly conducted review and meta-analysis focusing on this special sample. However, in the end there were only quiet few studies that were included in the meta-analysis or were reviewed because they did not fulfil the criteria for a meta-analysis. The authors did make the right decision regarding the inclusion of studies, but especially the measurement of diet quality is very heterogeneous. Thus, it is not surprising that there is no clear and powerful finding. Nevertheless, this review and meta-analysis underlines the importance to find a powerful way to measure diet quality which should then be implemented as standard measure in appropriate studies. Furthermore, this review and meta-analysis focuses on a very specific sample - women in reproductive age. This is in my opinion a very important sample. As the authors state because of the role of stress in this age group and the effects stress might have on pregnancy and the unborn child. But also because women in this age often gain weight and maybe the role of stress is very important. As such, I recommend publishing the manuscript and I hope that it might stimulate interesting studies.
   Response: We thank the reviewer for raising these valuable points regarding the importance of the topic and the population addressed in this paper. Indeed, we acknowledge the importance of the topic and the population addressed in this paper. We believe this paper will surely stimulate interesting research around the topic filling in the gaps found in the literature.
Importantly, we have now run outliers and influential analysis and found an improved pooled effect; the pooled correlation between stress and diet quality has been found to be statistically significant (line 277).

Reviewer #2

1. Background: Poorly written, contains some irrelevant information that does not relate to the study aim and topic, difficult to follow certain messages - not sure where it is heading.

   Response: We thank the reviewer for highlighting this and have adapted the background to enhance clarity and focus (pages 2-4). Detailed information about how stress affects the body physiologically was taken out since this was irrelevant to the study. In addition, some sentences about obesity and diet were removed and more information from the literature regarding the association between stress and diet was added instead in order to make the background more focused and to the point as recommended by the reviewer.

2. Although the authors report critically appraising studies using CASP, the results of this assessment are not reported. Not only should this be reported transparently, but study quality is an important consideration in meta-analysis and should be used to explore heterogeneity/inconsistency using sensitivity analysis.

   Response: We thank the reviewer for raising this important point. After this discussing this with systematic review field experts and following the associate editor’s recommendation, we have now decided to use a Cochrane domain-based risk of assessment tool; ROBINS-I tool for non-randomised studies as an evaluation assessment tool. Cochrane Collaboration has adopted the principles of the GRADE system for evaluating the quality of evidence for outcomes reported in systematic reviews. We have now run the risk of bias assessment for all the studies that were included in the systematic review (lines 170 and 308). Additionally, we ran Outlier and influential analysis, publication bias, and sensitivity analysis using trim-and-fill method in the meta-analysis to explore whether heterogeneity could be improved by taking out these outlying studies (line 184-187).

3. Increasingly, systematic reviews are expected to apply GRADE so we can determine the confidence in pooled results/estimated effects (meta-analysis). This takes into account risk of bias in each study as well as other considerations which help the reader to really understand how confident we can be in the conclusions. I strongly encourage the authors to add a GRADE assessment of confidence in the body of evidence.

   Response: We have followed your advice and applied the recommended tool for assessing the risk of bias in the domain. Because Cochrane Collaboration has adopted the principles of the GRADE system for evaluating the quality of evidence for outcomes reported in systematic reviews. We have used Cochrane domain-based risk of assessment tool; ROBINS-I tool; for non-randomised studies to assess the quality of the included studies and the results. For more details, please check our response to comment number 3 of the Associate editor.
4. Method: Did not provide meta-analysis process/methods.
Response: We agree with the reviewer about the importance of including a meta-analysis paragraph in the methods section. We have now provided additional details in the text (line 177).

5. Abstract and background: Does not clearly discuss the literature on the association/link between psychological stress and its impact on poor diet quality (with evidence) in the beginning until line 69.
Response: We thank the reviewer for these valuable comments. We have structured the background obesity specific to reproductive aged women (target population) is discussed at the outset, before going into the details of the association between stress and diet. We have now made some amendments in the background drawing upon the reviewer’s comments to enhance focus and clarity (please refer to pages 2-4).

6. Abstract: What types of papers were searched?
Response: We apologise for this lack of clarity, and a sentence has been added to the abstract to address regarding the reviewer’s comment (page 1).

7. Background: Authors have provided the health consequences of being obese and overweight in line 43-44 but have not linked it to women of reproductive age in particular.
Response: We thank the reviewers for highlighting this and have now added a sentence to link obesity with reproductive aged women (line 47).

8. Background: Explain why women of reproductive age may be at an increased risk of obesity related morbidity and mortality, why this target group? Slightly mentioned it in the conclusion.
Response: We agree with the reviewer about the importance of explaining this and have added a sentence to make this clearer (line 49).

9. Background: The mechanism of how fat builds up in line 65 may not be necessary. The mechanism of how psychological stress impacts on diet quality in line 76 would be more relevant and sufficient.
Response: We agree with the reviewer and the text about fat related mechanisms in the body has been removed from the background.

10. Background: Summarise in one statement the strength of current literature & why this research is important - coming back to what is the gap you are addressing?
Response: A statement has been added to the last paragraph of the background to highlight the gap being addressed in the current systematic review (lines 98-102).

11. Method: Authors did not provide study protocol registration with PROSPERO - this should be added or recognised as a limitation.
Response: We agree with the reviewer about the importance of clarifying this and have now added to the limitations section that prospective registration was not done for this systematic review (line 475).
12. Method: Authors did not provide key words & index terms used in search strategy in line 116. The full search strategy across all databases should be provided as an appendix or supplementary material.
Response: We apologise for this lack of clarity as this might had been unclear in the existing text. We have now clarified (lines 114-116) that Keywords and index terms used in the search strategy are provided in Appendix 1.

13. Method: Paragraph from line 123 requires its own subheading (i.e. Selection of Studies) and authors did not provide deduplication process.
Response: This has been amended and the paragraph has its own subheading now (Selection of Studies) as suggested by the reviewer (line 124).

Response: We have amended the manuscript and these tables and figures are now added to appendices as follows; we also indicate Figures that go inside the text:
- Table 1-Search strategy in Appendix 1
- Figure 1 inside the manuscript (MOOSE Flow Chart)
- MOOSE checklist in Appendix 2
- Figure 2 in the manuscript (Meta-analysis forest plot)
- Funnel Plot in Appendix 6
- Funnel Plot after applying trim-and-fill method in Appendix 7
- Figure 3 in the manuscript (Weighed bar plot for quality assessment)
- Tables 2, 3, and 4 in the manuscript in Appendices 3, 4, and 5 (respectively).

15. Method: Inclusion criteria did not address that only English language studies were included until discussion.
Response: We thank the reviewer for highlighting this point and we apologise for any confusion because we had mentioned this information already in paragraph “2.1 Search Strategy and Identification of Studies” in methods.
Response: We have now stated in paragraph “2.3 Inclusion and Exclusion Criteria” that only English language studies were included (line 133).

16. Method: Did not provide subheading for study outcomes (primary and secondary outcomes); outcomes are reported in data extraction.
Response: We agree with the reviewer and the subheading “2.4.1 Study outcomes” was added to the data extraction paragraph (line 164).

17. Method: data extraction checked for accuracy or review by?
Response: A sentence has been added to clarify this point (line 148).

18. Method: Fig 1 Flow diagram of studies selection process to specify records per database searched, number of duplicates removed, records screened to specify as title & abstract screening.
Response: We thank the reviewer for raising this important point and have now followed the MOOSE flow diagram that includes all this the information required (page 8).

19. Results: Line 236, where is reference 40? Is that a reference error?
Response: This error has been rectified and is now reference 10.
20. Results: Meta-analysis results to have subheading.
   Response: A new subheading “3.3 Meta-analysis” has been added to the results section (line 270).

21. Discussion: Line 346 basically states the gap for this research, which should be in the background.
   This sentence of the discussion has now been moved to the background (line 101).

22. Conclusion loses relevancy and focus - suggest deleting lines 444 to 446.
   We agree with the reviewer and these lines have now been deleted.

23. Conclusion and discussion - it is important to highlight that although some studies found an association; this study cannot determine causation - which came first the stress (chicken) or the poor diet quality (egg)?
   Response: We agree with the reviewer and recognise this valid point. As such, we have added a sentence to the last paragraph of the conclusion (line 490).
   This point was already mentioned in the limitation paragraph of the discussion (line 472) and has now been made much clearer.

   We hope that you are satisfied with the revised manuscript and will consider it suitable for publication in the Nutrition Journal. We look forward to receiving your views in due course.

Yours Sincerely,

Dr Fotini Tsofliou (corresponding author)
(On behalf of co-authors: Karim Khaled, Vanora Hundley, Rebecca Helmreich, and Orouba Almilaji).