**Reviewer's report**

**Title:** A systematic review and meta-analysis of randomized controlled trials of palliative care for pain among Chinese adults with cancer

**Version:** 1  **Date:** 10 Jul 2019

**Reviewer:** Research Square

**Reviewer's report:**

"STATISTICAL REVIEWER ASSESSMENT:

Is the study design appropriate for the research question (considering whether the analyzed population accurately reflects the design and whether you see any problems with control/comparison groups, e.g., likely confounders)?

No - there are major issues

Are methodologies adequate and well implemented (considering whether assumptions are addressed and whether analyses are robust)?

No - there are major issues

Are the analyses adequately communicated (considering whether reporting details are adequate and whether figures and tables are well labeled and described)?

No - there are minor issues

Does the interpretation accurately reflect the analyses without overstatement (considering whether limitations/bias are acknowledged and whether accurate descriptors, e.g., 'significant', are used)?

No - there are minor issues

Could an appropriately REVISED version of this work represent a statistically sound contribution?

Maybe - with major revisions
STATISTICAL REVIEWER COMMENTS:

General comments

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This paper addresses an interesting topic of palliative care for pain among Chinese cancer patients.

This systematic review seems to have been done well. I will mention a few comments on methodological aspects below. But to give the investigators credit, the methods plan, screening, and most of the analyses seem to have been done well and in a sound manner. I'm glad they did a grey literature search as well using Google.

Specific comments

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Major comments

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1. It is not clear if a librarian was involved in conducting the literature search, please clarify. Also, the included databases might not be enough to capture the relevant research on this topic. Including more databases might offer a more comprehensive coverage of the literature. This includes databases such as Scopus and Medline. Additionally, the search keywords used for the Google search mentioned was not reported. Neither were the search strategies for the four Chinese databases mentioned. It is also not mentioned from what specific date (not only the year) until what specific date the databases were searched. The search strategy and methods are questionable and the number of identified studies is way too low.

It is also important to note that many studies could have been published since the end date of the search done. To avoid making this review outdated by the time it is published, it would be helpful to update the search done (preferably using the help of a librarian experienced in searching electronic medical databases).

It's great that they crossed checked the included studies' references for potentially eligible studies.
2. Did the reviewers use any particular tool or platform to do the screening and later the data extraction? What software did the authors use to conduct the analysis?

3. The analysis description makes sense and seems well done. However, why is an effect size of SMD of 0.80 considered large, 0.50 moderate, ...etc? It would be great if the authors would add a rationale for this or cite relevant supporting literature.

4. Did the reviewers attempt to contact the authors of studies where relevant pieces of data were not reported?

5- How did the authors conclude that certain factors were indeed behind the heterogeneity observed? Please clarify in more detail as part of the analysis section of the Methods. Similarly, please mention and explain the trim and fill method in the publication bias section of the Methods.

6. Please list first the total number of included studies and then go in detail as to where these studies came from (which database).

7- The first statement as to why certain approaches were used (particularly the choice of random effects model) is not valid. Using random effects model does not reduce heterogeneity in the pool of studies. Heterogeneity is an independent factor from what analysis method used to pool the group of included studies. Please rewrite.

8- The statement after the word ""additionally"" on line 16 of page 15 is not valid. You cannot draw such a conclusion from the available and presented data. No direct comparison was made to draw such a conclusion. Please rectify.
Minor comments

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9. Under Limitations, please change the word 'flows'. If this meta-analysis has any flaws, then these flaws must be corrected. Instead the authors might have wanted to mean limitations/drawbacks. Please reword.

10. In the limitations section of the review, it would be a good idea to mention the time since this search was conducted (unless an update is possible, which would certainly be preferable).

11- The statement on line 16 of page 16 is unclear. What is meant by "'descent degree was small'"? Please reword.

12- The statement on line 5 of page 17 is self-contradictory. Please rephrase. If there was high risk of bias, then conclusions would certainly change.

13- The evaluation of publication bias generally is not useful when less than 20 studies are included in a meta-analysis for any single outcome. In this meta-analysis, the total number of included studies is only 14. So I would recommend not evaluating the publication bias neither graphically nor using a statistical test in this context due to the small number of included studies. This should be included in the limitations as well.

14- Please add to all the forest plots a column of N (number of subjects in each study).

REQUESTED REVISIONS:

Included in the comments above."
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Acceptable

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

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5. Do you have any other financial competing interests?

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If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

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