Author's response to reviews

**Title:** How explicable are differences between reviews that appear to address a similar research question? A review of reviews of physical activity interventions

**Authors:**

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**Author's response to reviews:** see over
We are grateful for the points raised by the peer reviewers and trust that the changes we have made in response both clarify and strengthen the paper. In particular, it was clear that the perspective of the paper needed some clarification: the aim was to think through the utility and validity of a set of reviews in terms of their potential use in policymaking. We have now clarified this central point, from which a number of other implications have followed.

Our responses to specific items from reviewers are as follows.

**Reviewer: Lumaan Sheikh**

<table>
<thead>
<tr>
<th>Minor Essential Revisions</th>
<th>Corrected</th>
</tr>
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<tbody>
<tr>
<td>Repitition of results mentioned on page 6 last paragraph.</td>
<td>The conclusion has been substantially re-written and structured.</td>
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<tr>
<td>Conclusion needs more clarity,</td>
<td></td>
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<tr>
<td>Addition of a brief and precise summary pointwise about the implications of this review</td>
<td>The results of the analysis are summarised at the beginning of the discussion.</td>
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**Reviewer: Philip Baker**

<table>
<thead>
<tr>
<th>Reviewer comment</th>
<th>Authors’ response</th>
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<tbody>
<tr>
<td>1. Is the question posed by the authors new and well defined?</td>
<td>In the ‘limitations’ section of the paper, we now acknowledge explicitly that other reviews will have been published since our searches were carried out; and also state the date of searching explicitly.</td>
</tr>
<tr>
<td>The research question is interesting and seeks to answer their questions about important issues pertaining to reviews which promote physical activity.</td>
<td></td>
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<tr>
<td>Given the approach to identify the systematic reviews uses an earlier study that is a systematic map of reviews (published in 2008), the search is likely to be at least 4 years old. Hence this manuscript is a description of earlier, rather contemporary systematic reviews. (Please note that the search date is not stated in the present manuscript).</td>
<td></td>
</tr>
<tr>
<td>The research questions stated in Background Para 8 are helpful.</td>
<td></td>
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<tr>
<td>Minor Essential Revisions</td>
<td>As specified below, we have clarified the purpose of the paper and added additional content to the ‘limitations’ section.</td>
</tr>
<tr>
<td>1.1. The manuscript could benefit by explaining more clearly limits to generalise the findings.</td>
<td></td>
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<tr>
<td>2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? Minor Essential Revisions 2.1. The authors may wish to consider providing further details of the inclusion criteria in Methods section.</td>
<td>We have added more detail on this: &quot;This map included reviews about physical activity (or sedentary...&quot;</td>
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</table>
2.2. The method using an earlier mapping review in theory seems to be a practical approach, building upon earlier work. Although the approach is convenient, using an earlier search strategy has resulted in a set of reviews that is limited in currency and comprehensiveness. For example, it is unclear why the authors omitted a well-known Community Guide review, Khan 2002, Community based campaigns for physical activity (http://www.thecommunityguide.org/pa/campaigns/community.html).

Furthermore, basing the current paper upon an earlier review strategy omits more recent reviews on the topic such as Baker 2011 (Baker PRA, Francis DP, Soares J, Weightman AL, Foster C. Community wide interventions for increasing physical activity. Cochrane Database of Systematic Reviews 2011, Issue 4. Art. No.: CD008366. DOI: 10.1002/14651858.CD008366.pub2.).

It would be helpful for the authors to address the limitations of their approach in the methods.

In the ‘limitations’ section of the paper, we know acknowledge explicitly that other reviews will have been published since our searches were carried out; and also state the date of searching explicitly.

This review was in our original map, but its scope goes well beyond ‘community interventions for physical activity’, and so we did not include it in this analysis. We now recommend the more recent review for those interested in the substantive topic.

We have added the following to the ‘limitations’ section: “We also acknowledge that since the searches for the original review of reviews were carried out in November / December 2007, other reviews on this topic have been published. These may reflect developments in review method that overcome some of the weaknesses in the reviewed evidence base; however, the general messages contained in this paper about understanding how different reviews on the same subject relate to one another will remain valid.”

Major Compulsory Revisions

2.3. The authors have used a tool by DARE which seems to classify the reviews as systematic or non systematic. The references provided for this tool is inadequate. Importantly, it is unclear how this tool is a quality assessment tool as several places in the manuscript the authors refer to the reviews as “high quality” (I can not find a description on the DARE website indicating it is a quality assessment tool, although I haven’t looked extensively).

2.4. A much better and useful approach for the authors to consider would be to quality score the included reviews with a recognised tool such as AMSTAR, Health-evidence.ca or CASP for appraising systematic reviews (e.g.

We agree that there are better tools to be used if the methodological quality of a review is to be assessed. What we were interested in getting at in this case was whether a review ‘looked like’ a systematic review; whether it was likely to be held up as being systematic for the purposes of decision-making or not.

We have redone the QA using AMSTAR.
2.5. Background para 4; the authors state “Since all the reviews are about the same issue”. This is difficult to ascertain from Table 2 and it is unclear whether the definitions of “community intervention” are similar enough to compare and draw conclusions as it is so loosely defined.

2.6. Methods, para 1 states that the systematic map was about “interventions to reduce childhood obesity”. Could it be possible that this aim is different than programs aimed at increasing populations levels of physical activity. An explanation is necessary.

We have amended the wording to say “about the same broad issue” and made a number of adjustments to the background to be clearer about the rationale for the study.

We have clarified (on page 3) our starting point now. “In essence, we placed ourselves in the hypothetical position of wanting to identify evidence about “what community interventions work” to promote physical activity among children to inform our decision-making.” Thus, both interventions that raise physical activity at the population level and those aimed specifically at children would be relevant. (What we couldn’t use this set of reviews for of course would be to make judgements about the evidence base re adult physical activity.)

3. Are the data sound and well controlled
Methods section, para 8, describes methods for data collection. No statistical analysis was undertaken, although scores of agreement between studies could be explored.

Discretionary Revisions
3.1. As stated above, it would be helpful if each review could be scored with one of the suggested tools and then be analysed further by their score.

We have rescored the reviews using AMSTAR; this has not changed the results, but we agree that this is the more appropriate tool to use.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes the manuscript adheres to relevant standard for reporting and data deposition.
The tables (in their present form) could be improved for greater clarity.
Minor Essential Revisions (not for publication once the authors address)
(The tables all need better labeling to help the reader)
4.1. Table 1: Consider restructuring and including more information as it is difficult to grasp what the authors wish to communicate.

As requested by the third reviewer, we have now deleted the difficult to decipher Table 1.

4.2. Table 2; the author could reformat the table as it is difficult to interpret. Table 2 does not indicate which reviews were Cochrane, although Results section, para 8 states two reviews were Cochrane Reviews.

We have now indicated in the text which reviews are the Cochrane reviews. The formatting in our copy looks ok, but we will check this on resubmission.

4.3. Title of Table 2 has error “which met out inclusion Corrected.
<table>
<thead>
<tr>
<th><strong>criteria</strong></th>
<th><strong>The title has been amended to read: “How the results about physical activity from the seven studies that were included in more than one review were reported in each review.”</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>4.4. Table 4:</strong></td>
<td><strong>The table has now been given shaded rows and ordered by date of publication (rather than name of first author).</strong></td>
</tr>
<tr>
<td><strong>4.4.1. Title and labeling could be improved</strong></td>
<td><strong>The table has now been given shaded rows and ordered by date of publication (rather than name of first author).</strong></td>
</tr>
<tr>
<td><strong>4.4.2. Where there were conflicts between the studies,</strong></td>
<td><strong>The title and labels have been amended.</strong></td>
</tr>
<tr>
<td><strong>4.5. Table 5:</strong></td>
<td><strong>The title and labels have been amended.</strong></td>
</tr>
<tr>
<td><strong>Perhaps graded shading or reorganisation could help with interpretation of the table. Why is the first paper published in the centre of the table?</strong></td>
<td><strong>The title and labels have been amended.</strong></td>
</tr>
<tr>
<td><strong>4.6. Table 6:</strong></td>
<td><strong>Title and labels have been elaborated upon.</strong></td>
</tr>
<tr>
<td><strong>The adequacy of the labels on this table could be improved to aid readers</strong></td>
<td><strong>Title and labels have been elaborated upon.</strong></td>
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<tr>
<td><strong>4.7. Table 7:</strong></td>
<td><strong>Minor essential revisions</strong></td>
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<tr>
<td><strong>Readers would find improvement in titles and labels to be helpful. Difficult to follow in its current form.</strong></td>
<td><strong>We have assessed each section and attempted to improve flow and organisation; particularly in the discussion section.</strong></td>
</tr>
<tr>
<td><strong>5. Are the discussion and conclusions well balanced and adequately supported by the data?</strong></td>
<td><strong>We have assessed each section and attempted to improve flow and organisation; particularly in the discussion section.</strong></td>
</tr>
<tr>
<td><strong>The author’s discussion are supported by the data and are interesting.</strong></td>
<td><strong>We have assessed each section and attempted to improve flow and organisation; particularly in the discussion section.</strong></td>
</tr>
<tr>
<td><strong>5.1. The sections could benefit from better organisation and flow.</strong></td>
<td><strong>We have assessed each section and attempted to improve flow and organisation; particularly in the discussion section.</strong></td>
</tr>
<tr>
<td><strong>5.2. In Discussion, para 8, “Strengths of the study”, the authors state “First, our searches were far reaching and sensitive and our “community intervention” was broad”. It could be argued that a broad definition is problematic as the reviews have significant heterogeneity, and that this would result in different studies included. Heterogeneity makes it more difficult to compare reviews as there are answering different research questions (e.g. community wide vs. Community based). In the Cochrane review by Baker 2011, “community-wide” is specifically defined to avoid this problem and a logic model is presented. It would be helpful for the authors to explore the definition of community further.</strong></td>
<td><strong>We agree that heterogeneity makes it more difficult to compare reviews – but it also makes it more difficult to understand what they are saying as a whole; this is the issue that we highlight in the discussion. (We began writing a discussion about community, but felt that this moved us away somewhat from the main methodological points of the paper.)</strong></td>
</tr>
<tr>
<td><strong>5.3. The particular elements of each intervention are not stated with enough detail for comparison. I suggest the authors look at Table 3 in as Baker 2011 as idea to help the readers understand the heterogeneity in the included studies (both primary studies and in the reviews). Understanding the heterogeneity might help explain some of the differences observed or may help support the current explanations and the author’s conclusions.</strong></td>
<td><strong>We like this presentation of intervention characteristics (as shown in Table 3), but were concerned that this would take the study in a different direction, and not address our research questions directly.</strong></td>
</tr>
<tr>
<td><strong>5.4. The authors might consider identify that many methodological issues in public health reviews are have been addressed by the Cochrane Public Health Group, and how this work may result in improvements in future reviews.</strong></td>
<td><strong>We now conclude the paper with a recommendation to the Cochrane Public Health Group’s guidance for further reading.</strong></td>
</tr>
</tbody>
</table>
of public health. (It is interesting to note that earlier reviews score highest were Cochrane reviews. The authors may wish to elaborate on this).

6. Do the title and abstract accurately convey what has been found?
Minor essential revisions
6.1. The authors should reconsider the title of the paper which presently seems inconsistent with the contents of the heading “background”.

The title has been changed to be a better fit with the rest of the paper.

6.2. In the Abstract, the Background section could be enhanced as it is particularly brief and presents only the research question

The background section in the abstract has been rewritten.

7. Is the writing acceptable?
Improvements could be made in the clarity of the text in several sections:
Minor essential revisions (not for publication)
The authors may wish to consider improving the clarity in the following sections:
7.1. Background, para 1, sentence 1: change Theories to principles

Done (and good point!)

7.2. Results para 1

This has been clarified

7.3. Discussion para 5, the para beginning “There was little-cross citation...”

This has been amended

7.4. Conclusion para 2

This has been rephrased – we trust more clearly than before.

Reviewer: Erica James

1. Is the question posed by the authors new and well defined?
The question posed is useful however it has not been well defined by the authors. The authors appear to have been opportunistic; utilising data obtained from a previous exercise (a systematic map of reviews on social and environmental interventions to reduce childhood obesity) and using the reviews obtained to explore a different question. Whilst this is admirable, it has led to some confusion surrounding the actual questions being explored and raises some issues about the suitability of the available data. For example, by stating in the abstract that the aim of the manuscript was to determine “How suitable are existing reviews for evidence-informed decision making in the area of preventing and reducing obesity?”, implies that reviews would include both diet and physical activity (PA) interventions as well as those for treating obesity (for example gastric band surgery). In fact, the authors then go on to only include reviews of PA promotion (which is also what the title of the manuscript implies will be included). A review of reviews of PA interventions is a fine aim but at the moment it does not corresponded with their stated aim. The issue is further confused by the 6 research questions

The abstract has been amended to remove this detail.

We are not sure that the research
stated at the end of the introduction. The research questions overlap significantly (for example #6 overlaps with #2 and #4). I suggest that one primary aim and a series of secondary objectives would be more suitable.

<table>
<thead>
<tr>
<th>2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?</th>
<th>3. Are the data sound and well controlled?</th>
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<tbody>
<tr>
<td>The method is a ‘review of reviews’ based on the findings of a previous systematic mapping exercise (citation provided). The methodology would be more compelling if the authors provided more detail of the search strategy used to identify the reviews included in the mapping exercise as these reviews form the basis of the current analysis. The authors also chose not to update the search and include reviews published since 2007-8; this is a major limitation. Of the 16 reviews included in the original mapping exercise, the authors excluded 8 reviews and then judged that the remaining 8 reviews were similar enough in scope to be compared. I do not agree. The 8 reviews included in the current analysis had significant differences in aims and therefore in scope (for example, to summarise the efficacy of PA interventions for the whole community, compared to efficacy of one-off sporting events in increasing PA). It was therefore not surprising that there was not a lot of overlap in the original empirical studies included in the reviews.</td>
<td>We have referenced the original ‘map’ of reviews where the search strategy is freely available online. We have not updated the searches, but do not see that this is a major problem in a methodological piece such as this.</td>
</tr>
</tbody>
</table>

| The confusion about the focus of content of the analysis (PA alone vs. childhood obesity prevention) continues in the methods by inclusion of search terms such as “eating or food” in the methodological assessment on included reviews, and inclusion of “BMI or energy intake” in identification of included studies. It seems that some CVD prevention reviews were included (eg. Dobbins & Beyers, 1995) whilst others were excluded (eg. Dobbins, Thomas & Ploeg, 1996). The authors also failed to contact review authors in the case of no mention of methodological assessment (or it was unclear) in the published reviews. This seems a bit lazy given there were only 8 reviews and therefore 8 authors to contact. | We have attempted to clarify the scope of the map compared with the scope of this study. The quality assessment has been redone using the AMSTAR tool. When carrying out systematic reviews we contact authors as a matter of course. However, the focus of this study is the usability and coherence of published material. |

| The authors explain how they accounted for year of publication in Table 5 (extent to which reviews cited each | We have attempted to clarify the scope of the map compared with the scope of this study. The quality assessment has been redone using the AMSTAR tool. When carrying out systematic reviews we contact authors as a matter of course. However, the focus of this study is the usability and coherence of published material. |
other) but not how they accounted for year of publication in
which primary studies the reviews have in common. In the
discussion the authors state “Reviews of longitudinal and
multi-stage interventions were more likely to find larger
studies, but less likely to report their findings
comprehensively because these are dispersed across many
publications, not all of which were necessarily reported” but
they did not adequately deal with year of publication of the
multiple publications from these larger trials in relation to
year of publication of the reviews.

The year of publication was taken
into account in this analysis. (Table 6
has a column entitled ‘publication
date’ which summarises this.

There is inconsistency throughout the manuscript in use of
the terms ‘efficacy’ and ‘effectiveness’.

We cannot find any occurrences of
the term ‘efficacy’; maybe these had
been removed before we looked to
address this point.

As a ‘review of reviews’ I was surprised that the authors
made up their own methods rather than using one of the
established approaches for meta-review. This was not
justified. ‘Risk of bias’ is more acceptable than ‘critical
appraisal’ in this context.

Redone using the AMSTAR tool.

4. Does the manuscript adhere to the relevant standards for
reporting and data deposition?

There are too many tables. E.g. Table 1 is not necessary.
Table 5 is very difficult to interpret (I could not work out
whether I was reading down the columns or across the
rows). Towards the end of the discussion, the format for in-
text referencing changes (at ref #48) and from there on the
cited references are not included in the reference list (refs
#48, #9, #6) I assume this is due to not picking them up in a
reference management software application.

We think the tables are an important
part of being transparent about our
analysis and would like to retain most
of them. However, we have deleted
Table 1 and made changes to their
structure and format (especially
Table 5 – now Table 4) and their
headings.

The references should all be there
now.

5. Are the discussion and conclusions well balanced and
adequately supported by the data?

I fundamentally don’t agree that the 8 included reviews
were similar enough in aims to be able to be compared. It
therefore came as no great surprise that there was little
overlap within the 8 reviews. I think the authors could have
discussed in more detail the dilemma facing policy makers:
if the aim of a systematic review is to synthesis the existing
evidence and it ends up ‘empty’ due to a lack of high quality
primary studies then do we accept non-systematic, low
quality reviews in their absence?

The reviewer is raising two issues
here: that of heterogeneity between
reviews and that of their quality. In
terms of similarity, we have stated
above that the reviews were
adjudged to be similar enough to
inform decision-making about the
promotion of physical activity; we
therefore expected that they would
have different scopes, but this did
not, we felt, mean that they were so
dissimilar as to make this analysis
impossible. With regard to the
second point, about the quality of
studies, we think the answer is
probably “no”, though the question
of “how bad is too bad?” is one that has not yet been addressed empirically.

6. Do the title and abstract accurately convey what has been found?
   No, as outlined above there is inconsistency between the title and the aim stated in the abstract and the research questions.

   The title has been changed.

7. Is the writing acceptable?
   I think once the research questions are tidied up it will make the presentation of methods and results more straightforward. At the moment it is unstructured and needs tightening.

   We trust the revised draft is more straightforward.

**Major Compulsory Revisions**
- Revise title, aim stated in abstract and stated research questions at the end of the discussion (and revise methods accordingly)
  - The title has been revised

- Include more detail on the search strategy used in the original mapping exercise
  - We have included more information about the inclusion criteria used in the mapping exercise. However, replicating the search strategy seems a little unnecessary given that the original map is freely available online for those interested in looking at the search in more depth.

- Update searches to 2012
  - We feel the benefit to a methodological paper of doing this is outweighed by the resource expenditure involved.

- Contact authors in the case of missing data on methodological quality assessment
  - This would not usually be done by someone using the reviews to inform their decision-making; the review reporting should be adequate.

- Consider splitting the included reviews in to more similar groups based on review aim/scope
  - We have looked at this, but this is quite difficult to do – especially as some studies would fall into several different groups. What we have done is to discuss issues of heterogeneity in more depth and outlined how more effort in terms of ‘mapping’ research domains would help review users to gain an overall understanding of research across a given area.

- Revise discussion as described above
  - Please see above for a description of the changes made.

**Minor Essential Revisions**
- Reduce the number of tables
  - The tables are an important part of ensuring the analysis is transparent so we would like to retain most of them, though we have deleted Table 1 and in line with feedback from
another reviewer as well, we have revised their headings and labelling to make them more user-friendly.

| • Ensure all cited references are included in the reference list | The references should all be in the list now. |
| • Ensure consistent use of the terms ‘efficacy’ and ‘effectiveness’. | We cannot find any occurrences of the term ‘efficacy’; maybe these had been removed before we looked to address this point. |