Reviewer's report

Title: How comprehensive are literature reviews investigating community interventions to promote physical activity? A review of reviews

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Reviewer: Erica James

Reviewer's report:

This manuscript describes an analysis of existing reviews with the explicit aim of determining the suitability of reviews to make evidence informed decision making in the area of preventing and reducing obesity.

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 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.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? 3. Are the data sound and well controlled?

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.

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.

The authors explain how they accounted for year of publication in Table 5 (extent to which reviews cited each 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.

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

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.

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.

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?
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.

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.

Major Compulsory Revisions
• Revise title, aim stated in abstract and stated research questions at the end of the discussion (and revise methods accordingly)
• Include more detail on the search strategy used in the original mapping exercise
• Update searches to 2012
• Contact authors in the case of missing data on methodological quality assessment
• Consider splitting the included reviews in to more similar groups based on review aim/scope
• Revise discussion as described above

Minor Essential Revisions
• Reduce the number of tables
• Ensure all cited references are included in the reference list
• Ensure consistent use of the terms ‘efficacy’ and ‘effectiveness’.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests’