Reviewer's report

Title: Evidence Summaries: The Evolution of a Rapid Review Approach

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Reviewer: Julia Littell

Reviewer's report:

The title and abstract are informative and the paper is generally well written.

The topic is new and well defined. The authors address important gaps in the transparency of rapid review methodologies. They provide useful descriptions of their experience in working with knowledge users and conducting rapid evidence summaries, along with important lessons learned in the process.

The evidence summary methods may be appropriate, but are not described in sufficient detail for readers to understand how these summaries were conducted. As a result, it is difficult to judge the trustworthiness of the evidence summaries. Since this paper aims to improve the transparency of rapid evidence summaries, some additional clarification and explication of methods is needed here. Specific suggestions for doing this are made below.

The discussion and conclusions sections could be developed to more fully address potential limitations as well as strengths of the authors’ work.

Major Compulsory Revisions
[none]

Minor Essential Revisions
1. It is not clear what methods (if any) were used to search for grey literature. If there were explicit attempts to find grey literature, these sources and strategies should be briefly described. If not, authors should consider potential effects of publication bias on the results and conclusions of evidence summaries.

2. Study inclusion criteria should be explained in a bit more detail. Authors note that only prospective studies are used in reviews on risk factors. What designs are considered to be eligible and ineligible in reviews on treatment effectiveness? The discussion on p. 8 would seem to suggest that there are no formal study design inclusion criteria (e.g., “recent” and “oft-cited” studies may be included). If that is so, the discussion section should include some consideration of problems encountered in the initial inclusion methodologically weak studies. For example, the authors note that only the “highest quality” studies are summarized, but one wonders whether the initial inclusion of a wide array of studies is inefficient.

3. It would be useful to say a bit more about how primary studies of effectiveness are judged to be “high quality” and how potential biases are identified. This would be very helpful for readers who are not familiar with the rubric developed by the
Cochrane Musculoskeletal Group. Although the authors provide a link to a completed evidence summary (so readers can find some of these details in that summary), overall issues of “quality” and risk of bias are important enough to merit more explanation in the text.

4. The discussion section mentions limitations and bias, but these issues should be addressed more explicitly. Specifically, authors should consider potential effects of publication bias in a truncated search. Authors should also note that there is some (albeit) limited evidence that meta-analysis provides more accurate summaries of quantitative data than narrative reviews.


Discretionary Revisions

1. The authors might note that review methodologies have very wide applications that are not limited to health care settings. Rapid reviews are currently used in the fields of social care (education, crime and justice, social welfare) and could be used in environmental sciences, economics, and other fields.

2. Authors could include the term “rapid evidence assessment,” perhaps as a key word, since this is more commonly used than “rapid review” in fields of social care, and there is some online guidance (however opaque) on how to conduct a REA.

3. It might be misleading to characterize rapid reviews as an emerging approach, in contrast to “traditional systematic reviews.” Proponents of systematic reviews (SRs) would argue that SRs should replace “traditional” (narrative, nonsystematic) reviews, and that rapid reviews actually represent a partial return to these older (quicker, less thorough) methods.

4. It is not clear to me why interest in risk factors is a topic that is “difficult to address” (p. 6), given the large literature on epidemiology and guidance on systematic reviews in this area (MOOSE). Perhaps this point could be clarified?

5. The proposal template (mentioned on p. 8) could be attached as an appendix or provided via a link. This might aid in increasing transparency.

6. Absent data on the extent of agreement between independent screeners/coders, how do we know whether criteria are “uniformly applied” in rapid reviews (Table 1)? The authors might deal with this (and other issues raised above) by acknowledging that gaps in transparency and in knowledge about the trustworthiness of rapid reviews remain. Overall this paper serves to narrow those gaps, however it does not (and cannot be expected to) eliminate them completely.

Level of interest: An article of importance in its field
Quality of written English: Acceptable

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.