Reviewer's report

Title: Interventions Encouraging The Use of Systematic Reviews by Health Policymakers and Managers: A Systematic Review

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Reviewer: Atle Fretheim

Reviewer's report:

The authors address a very timely question: What can be done to effectively encourage the use of systematic review by health policymakers and managers?

Major compulsory revision

1. There is one major problem I have with this manuscript, and that is related to the authors’ choice of inclusion criteria. It does not make sense, in my view, to include any type of quantitative study if the aim is “to determine the impact” of interventions. This problem is clearly demonstrated in the findings: in addition to a single randomized trial, the authors have included cross-sectional studies where, as far as I understand, no attempt was even made to estimate the effectiveness of the interventions that were introduced. The findings that are being reported in the current manuscript (I have not read the primary studies) are all descriptive: How many decision-makers reported using systematic reviews, and what characterized those who used systematic reviews (“predictors”). No information is given that provides insight into the effectiveness of the interventions that were being employed (i.e. “being offered the opportunity to receive five relevant systematic reviews”). The authors briefly mention that lack of control group is a weakness. In my mind this is an understatement. When not even an attempt is made to provide some sort of effect-estimate, I do not see how the study is relevant to the question that is being addressed, namely “Did the intervention work?”

If the researchers behind the three surveys (it is not clear from the manuscript whether there were two or three, but my impression is that there were three) did for example compare the reported use of systematic reviews before and after the intervention, this could – perhaps – be seen as an effect-estimate. But this type of information is not mentioned in the current manuscript so I assume that it is not available.

The inclusions criteria is a fundamental problem. I see two possible solutions to this:

a) The authors can adjust their inclusion criteria and limit them to studies where the investigators, as a minimum, provided some form of effect-estimate (even this would be extremely lax criteria compared just about all other effectiveness reviews that I have heard of!)
b) The authors can adjust their stated aim, and elect to focus on both effect-estimates and determinants of use e.g. “a systematic review of quantitative studies” or something of that sort.

I would prefer the former since I am not sure what kind of research questions the latter would actually be addressing.

Minor Essential Revisions

2. On page 13 you state that “the online registry of systematic reviews improved significantly from baseline to follow-up”, but you don’t specify which outcome this is for. I assume it is some sort of measure of use of systematic reviews, but please make this explicit. I also find it confusing that this statement comes just after you have written that in the same study the authors “were not able to show a significant effect of any of the interventions on evidence-informed decision making”. A contradiction, seemingly.

3. On page 6, I did not understand what type of information you meant by “know effectiveness of the intervention for changing behaviours”. I think maybe I know what you are trying to say, but I think this needs to be better explained.

4. “Quality-check” is not easy, especially of studies in a “messy” area like this. The STROBE-statement is not intended for quality-assessment of studies, but as a check-list for reporting of studies. I realize that you could use the STROBE-statement as a tool to guide your quality-assessment, but there is a difference then between intended use and your use of the instrument. I think you should make this clear.

5. You should move the “Quality Assessment Results” backwards so that the sections describing participants and interventions come first.

Discretionary revisions

6. The mention at the beginning of a “scoping review” is, to me, a bit confusing. Although it is brought up again in the discussion, I propose either expanding a bit on this (in itself the term “scoping review” does not mean a lot to most people, and you might explain more of what you did) or maybe not mention it at all in this manuscript. A third alternative - perhaps the best - could be to only mention it in the discussion.

7. The claim that “systematic reviews are seen as helpful knowledge support for policymakers and managers” is supported by three references, where two of them are by some of the current authors themselves. This is close to circle argument! I suggest finding other authors that agree with you, and point to them.

8. I propose deleting “Three articles, reporting on two cross-sectional surveys, and one article describing a randomized controlled trial met the full inclusion criteria” as this is more or less repeated in the next para, as well as in the figure.

9. You don’t need to state that meta-analysis was not possible – that is obvious, in my mind, and serves only to confuse other readers than those who regularly write systematic reviews.
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

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

Declaration of competing interests:

I know one of the authors professionally and personally (Lavis), but do not consider this a potential conflict of interest. Thus:
I declare that I have no competing interests.