Reviewer's report

Title: Priority Setting, Wait Lists, and Public Involvement: A Qualitative Study

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Reviewer: Wilfreda E Thurston

General

BMC Review: Priority Setting, Wait Times, and Public Involvement: A Qualitative Study

Specific BMC Questions:

1 - Is the question posed by the authors new and well defined?
There is some confusion in this paper as to the goal. The authors state that no “empirical study describing and evaluating a formal system-wide wait time strategy” has been identified. This is not the goal of this paper which only talks about the use of participation in the priority setting process. There is considerable literature on the issue of public participation and the authors have used some of this; however, what was this particular study meant to add to that literature? The authors need to be more clear about what public participation is and what one would expect when “typically the public do not have a s direct role I priority setting.” Is a “consultation” (p. 3) limiting? Since the accountability for reasonableness framework is their conceptual framework, it could guide their definition of public participation. In addition, the authors use public engagement in the abstract and not again until page 11 – are these two concepts the same?

In fact, no research questions are put forward and the purpose is worded vaguely. More accurately, the paper assesses the priority setting process according to the accountability for reasonableness framework. Furthermore, we do not know that the question is new within the context of other wait time strategies that may have been employed in other provinces and/or countries. It is unclear whether and how this particular study fit into a larger study and what it contributes in terms of new information about processes in the Ontario health policy system.

2 – Are the methods appropriate and well described, and are sufficient details provided to replicate work?
The authors seem confused about design and methods. The design may be a case study using qualitative methods, but there really are no specific “case study methods.” They are very correct that a qualitative design is appropriate when little is known and the situation is complex. I see both a lack of sophistication with qualitative methods and an apologetic approach. I am not sure why the setting is inserted here and it really does not provide us with enough detail about the case. The case (OWTS) is first described in the results. I would prefer this to precede the Methods section, to set the stage, then the reader can move from that to expectations about what she/he will read in the methods. In addition, move the conceptual framework before the methods so the reader is not looking for it.

The methods are not described well. What is described and implied is entirely congruent with the purpose of studying the process of public participation in the OWTS. If sampling was theoretical, what was the theoretical basis for the decisions? Rather it seems that sampling was purposive (appropriate to gathering the best information about what happened), and using snowball sampling. We need to know what comprised “key documents” (p.5), not so much the naming of specific reports, but what makes them key to understanding public participation in the priority setting process. Similarly, who were “key informants”, what were their roles in relation to the strategy and decision making around public engagement? I am sorry to say that it is unlikely that data saturation actually drove sampling in a case study (p. 5). The authors only need to justify what “key” means, and there are only a limited number of Key documents and key informants available. If they could not access some, that can be discussed in results and conclusion and whether it limits the study; for instance, if they were unable to talk to the one person that could make a decision about public participation that might be a limitation, but potentially not serious to this paper.

3 – Are the data sound and well controlled?
It is of interest to someone trying to replicate the study what previous research was used to develop interview guides so this should be cited. I would like to see more referencing of their methodological sources, as I have never seen “evaluation” used as a descriptor of data analysis. Secondly, having two
researchers code raw data to “ensure consistency and accuracy” is very unusual. A benefit of qualitative methodology is that the lens brought to the table by each researcher is valuable and consistency is not necessary. Next, associating the term “reflexivity” with “veracity of the data” is new to me. Reflexivity refers to the researchers’ abilities to examine their own biases, assumptions, and reasons for coding and interpretation. Also, I think the involvement of a team of colleagues was excellent but given their similar backgrounds, would they actually “check preconceived assumptions” or reinforce them? Was the feedback treated as new data by the authors? Finally, with whom was member checking done and why do the researchers privilege their opinions about “accuracy and verisimilitude”?

4 – Does the manuscript adhere to the relevant standards for reporting and data deposition?
It is always a challenge in a case study to decide where to include the description of the case. At least some of the information on pages 8-10 can be assumed to be public and the official representation of OWTS. Perhaps this is what should be moved to the introduction and the remainder critical analysis of what happened as revealed by the study put in the results. Results seem to start on the bottom of page 10 after “[Insert Table 2 here].”

The use of quotation is well done.

I wondered at the need for tables 2 through 4. Do they really add to the text and understanding the text?

5 – Are the discussion and conclusions well balanced and adequately supported by the data?
The discussion addresses some key information gained from the study, but the authors fail to elaborate on certain key points. For example, the authors make the claim that “These findings will almost surely be helpful to similar wait time initiatives elsewhere”, but fail to identify how this study will be helpful or what about the study will be helpful to other initiatives. This may be due to a lack of familiarity with the literature on public participation and what can be expected as outcomes of participation over the lifetime of a particular policy initiative. On the other hand, their suggestions for operationalization of public involvement are well articulated. Additionally, while the authors have clearly explained how this evaluation will improve public involvement in priority setting, we are still left wondering at the end of the article how public involvement in the OWTS is going to improve wait times and/or improve the effectiveness of OWTS implementation. Although the authors have articulated that public involvement will essentially act as another lens in the operationalization of the OWTS, they fail to address how this will help or improve OWTS implementation and sustainability and/or ultimately improve patient care by reducing wait times. Perhaps more explanation how the eight context-specific factors used in making recommendations were developed might be useful for the reader to expand on the thought process behind evaluating the OWTS.

The conclusion lacks flow and needs to be improved as a finisher of the article.

6 - Do the title and abstract accurately convey what has been found?
The title does not. Please see above comments.
The abstract is accurate except I would ask if the authors mean consequences “for” the OWTS in last line of results.

7 – Is the writing acceptable?
Overall, the writing is very good. Grammar is good and the few minor flow problems can be easily adjusted with a bit of editing. I find the use of single quotation marks around accountability for reasonableness inappropriate.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Tighten up all parts of the methods section with more reference to methodological texts.
Clarify the purpose of the project and place it in the context of literature on public participation.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
• The title
Review use of the term reflexivity
• There are some wording issues throughout that could be adjusted to improve flow throughout the article: (i.e. p3: add “However” to the beginning of sentence that is in the second paragraph and starts with “Public involvement…”)

Discretionary Revisions (which the author can choose to ignore)
• Perhaps more explanation how the eight context-specific factors used in making recommendations were
developed might be useful for the reader to expand on the thought process behind evaluating the OWTS.
• Integrate tables into the text

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major
compulsory revisions

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.