Author's response to reviews

Title: Public involvement in the priority setting activities of a wait time management initiative: a qualitative case study and evaluation

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Author's response to reviews: see over
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To the editor

BioMed Central Health Services Research

Dear Editor

This is to re-submit a revised version of our paper (#1379717633131687): Public involvement in the priority setting activities of a wait time management initiative: a qualitative case study and evaluation.

We appreciate your interest in considering our paper for publication in BioMed Central, and herewith re-submit the revised version.

We are grateful to the two reviewers for their highly constructive comments. Their comments have been very instructive in revising and improving the manuscript.

Please find herewith attached a detailed response to all the reviewers’ comments.

Thank you again for the opportunity to publish this paper in BMC.

Waiting for a favourable response

Sincerely,

Rebecca Greenberg
Response to Reviewers' comments

Reviewer #1:

Comment 1:
It is not clear to me whether Part 1 (the description) is based on any qualitative analysis of the interview data or documentary evidence or whether it is simply based on a review of the literature (different to documentary analysis). On page 11 (last paragraph of this section) the use of "in interviews, some expert panel members ..." suggests that this section is based on interview analysis and documentary analysis. However, if this were the case it should be made more clear - for example do the headings that are used to structure the descriptions correspond to the main themes from the analysis, do you have any verbatim quotes that you can use to back up some of the statements made (either from interviewees or references from documents?) A statement such as that on page 9 first sentence, last paragraph: “the OWTS made a deliberate effort …” has to be substantially backed up. They may have made an effort – how ‘deliberate’ it was is interpretable from your analysis so the reader needs to see this. It may be for example that you need to revise some of the statements that you make to say something like: ‘from the documentary analysis it was evident that the OWTS depicted themselves as making a deliberate effort to … This can be demonstrated by … (then show the evidence of this)’. If this part is not actually based on the qualitative analysis then I would suggest that it is background information and would be better placed elsewhere (perhaps upfront in the paper) not in the results section.

Response 1:
Thank you for identifying this confusion. On page 9 we clarified that the Results section describes our study findings.

In addition, we have added stronger language about the evidence for our claims. For example, p. 11 par 1. “As shown in the expert panel reports the panels identified strategies to…”; p. 11 par 2. “The OWTS made an effort to disseminate their actions to the public, as evidenced by their construction of a wait times website and associated advertisement campaign, as well as their use of media briefings.”; and p. 11 par 2. “From the analysis of the interviews it was evident that the media has been instrumental in informing the public about the OWTS”

The description of the Wait Time Strategy belongs in the Results section as this information was obtained from the case study.

Comment 2: There is no reference to Figure 1 – it just simply appears at the end of Part 1. Looking at the figure, it is not clear to me what the labels A through to G or what Box1 and Box2 represent.

Response 2:
Figure 1 was referenced on p. 9 par 2. We have simplified Fig 1, p. 38.
Comment 3:
The first part of the evaluation (Part 2 i) needs some introduction. For example, how many themes did you find in total, are you presenting the results under these themes? Furthermore, there should be explicit reference made to the quotes you use. How do these fit in with the overall story that you are telling?

Response 3:
We have revised the introduction to the Evaluation section Part 1 (p. 14 par 3.): “This section provides an evaluation of the public involvement, according to participants’ perspectives. First is a brief analysis of the public involvement in the National Wait Times Reduction Strategy. Second is an in-depth evaluation of public involvement in the OWTS, which includes: a) were the public involved? b) should the public be involved?, and c) how should the public be involved?”

The verbatim quotes have been incorporated in the text in a standard way.

[Editor: Please see Reviewer 2 Comment 11.]

Comment 4:
I particularly liked the section on benefits and concerns but thought that this should be expanded more. In particular I would also have liked to see the counter view if there was one for some of these points. For example if some participants expressed one thing did the others not express any view on this or did they express an opposing view?

Response 4:
Please see benefits and concerns (pages 19-25). As suggested, we have added a few quotes to bolster this section. Please note that the counter views to benefits are concerns and vice versa.

Comment 5:
However, I think this section could also benefit from some substantial rewriting. In qualitative analysis it is not usual to be able to identify how participant “felt” (a word repeatedly used) or indeed what they “believed” – you can only interpret what they said they felt or believed (if respondents even used these words). Similarly you cannot express how strongly or weakly they felt or believed something – you don’t know this.

Response 5:
Thank you for pointing this out. We have eliminated ‘felt’ and ‘believed’ where appropriate.

Comment 6:
The evaluation of the process, using the conceptual framework A4R, is again not extensive. Whilst I agree that this is a good framework to evaluate the OWTS process against, as a reader I can neither agree or disagree with observations made by the authors because they do not offer the evidence (either from the interview accounts or documentary evidence) to back up the claims that they make.

Response 6:
Thank you for this important comment. The evaluation evidence is derived from the empirical description. To clarify, we have revised the description on pages 11 and 12. We have added: “All panel reports have been published on the website within 10 working days of their receipt” (page 11 paragraph 2); “Although the OWTS made substantial efforts to disseminate the activities of the strategy the actual decision making (‘how’ and ‘why’) were not made accessible to the public” (page 12 paragraph 1); “The OWTS did not formally evaluate the legitimacy and fairness of its priority setting activities. There was no formal appeals mechanism for the OWTS priority setting. All feedback from advisory panel members was managed informally by the panel chairs. Any public feedback received from either the MoHLTC email address or media briefings was dealt with informally” (page 12 paragraph 2).

Comment 7:
I have similar problems with the section on the consequences of the OWTS. Are these consequences themes identified from the interview accounts? If so, they should show the evidence for these. Again expression of views as “felt” etc should be avoided. Also I would be interested to know if there were any participants that expressed contrary views.

Response 7:
As stated on p. 27 par 2., the consequences were identified from the interviews.

Again, we have deleted ‘felt’ and ‘believed’ where appropriate.

Comment 8:
I am not clear on where the plan for operationalising a public involvement strategy has come from. Although the authors state that it is “based on this analysis” (page 24 second paragraph) it is not clear whether the strategies have emerged from the participant interviews, the documentary analysis, or are the authors own views having reflected upon the data. All are legitimate but the section needs to be made clearer. You need to be clear about how you have arrived at these three strategies and show how you have drawn together all the evidence that you present in the results in order to make these assertions. Moreover it would be interesting to know whether you have fed these back to the participants that you interviewed in order to ascertain their views. As well as the above, the section would also need to be extended in terms of referencing work that other people have done the area. Are these strategies known to work well and under what conditions? I think that this section is far too short as it stands. Additionally, as a side, I was surprised that there was no reference to the work done by Julia Abelson and colleagues on enhancing the legitimacy of public involvement in Canada.
Response 8:
The recommendations appear in the Discussion section of the paper and, therefore appropriately, they are based on ideas from our findings (presented in the Results section) and ideas from the existing published literature.

We clarified this on pp. 30-31, “This plan is based on this data analysis, current public involvement literature, and guided by the ‘accountability for reasonableness’ framework, it includes: . . .”

Comment 9:
I feel a little uncomfortable about participants’ views on barriers to public involvement being presented as myths. This assumes that there is also an objective ‘truth’ or ‘reality’. It may be that participants discuss or describe barriers to public involvement in these ways for many different (not necessarily conscious) reasons. In particular they may construct the public (as biased, not interested etc…) because these are familiar narratives and they provide an answer to your question, or they may use them as an avoidance tactic - to avoid the difficult job of involving the public. Simply setting these up as myths that require debunking presumes that participants are not aware that they are telling you a myth – whereas they may actually be fully aware that it is a myth and are deliberately using it because it enables them to achieve some other end. It would be interesting to examine the participants transcripts in more detail and do more analysis of this sort on them – but that could be an entire paper in itself.

Response 9:
Thank you for this comment. To avoid this debate, we have replaced ‘myth’ with ‘concern’ on pp. 31-33.
Reviewer #2:

Comment 1:
There is some confusion in this paper as to the goal.

Response 1:
We respectfully disagree. We have stated the goal on p. 5 par 2., “The purpose of this study was to *describe* priority setting in the Ontario Wait Time Strategy (Ontario, Canada) and *evaluate* it with particular attention to public involvement.” That is exactly what we did.

Comment 2:
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?

Response 2:
Unfortunately, the reviewer has misquoted us. We wrote on p. 5 par 1., “To our knowledge there has not been an empirical study describing and evaluating a formal system-wide wait time strategy using “accountability for reasonableness” with particular attention to public involvement, nor is there guidance for how to involve the public in a wait time management priority setting initiative.” This statement is perfectly aligned with the goals of the paper.

Comment 3:
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 direct role in 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?

Response 3:
Thank you for identifying this confusion. We have added a definition of public participation and clarified its use on p. 4 par 2., “Public participation is “the practice of involving members of the public in the agenda-setting, decision-making, and policy-forming activities of organizations/institutions responsible for policy development.” [19] (For the purpose of this paper public participation, public involvement and public engagement will be used interchangeably.)”
Comment 4:
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 fits into a larger study and what it contributes in terms of new information about processes in the Ontario health policy system.

Response 4:
We believe this comment emerged from the misunderstandings expressed in Comments 1 and 2, which have been addressed.

Comment 5:
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.

Response 5:
We respectfully disagree. There are case study methods. We have used them and our research group alone has published 15 case studies in the past 7 years, which are easily accessible through a PubMed or similar search engine. Many other researchers have also published case studies using similar methods.

Comment 6:
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.

Response 6:
This may be a matter of taste rather than rigour. The description of the ‘case’ is part of the Results.

Comment 7:
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.

Response 7:
We respectfully disagree. The methods have been described according to standards of similar previously published studies.
Comment 8:
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.

Response 8:
Thank you for identifying this confusion. We have revised “data collection” and “sampling and sample size” paragraphs. Please see pages 6 for the new “sampling and sample size”: “We sought to interview all people in leadership positions at the OWTS. All but one agreed to be interviewed. These included OWTS staff, Ontario Ministry of Health and Long Term Care (MoHLTC) staff, expert panel members, and the like. In addition we asked these participants to suggest others who were relevant to the research. As a result we interviewed individuals at the MoHLTC, hospital CEOs, patient advocates, nongovernmental associations and medical associations. Sampling continued until we began to hear the same views repeatedly in consecutive interviews – sometimes called saturation. Sample size was not formally calculated, but sampling decisions were made concurrently with the data analysis and continued until no new concepts arose during the data analysis.” Please see page 7 for the additions to the “data collection” paragraph: “Documents were obtained between January 2006 and July 2007. Documents were selected based on their ability to provide an understanding of the public participation in the OWTS priority setting process and information on how decisions were made, what factors were considered, who was involved in decision-making and how the strategy was disseminated....... In total, 25 documents and 28 interviews were collected and analyzed. Those who were interviewed we selected on the basis that they would provide information and insight into the priority setting activities of the OWTS that was not available by public means (i.e. public documents, press releases, etc.)..... The final data set for this study consisted of 25 documents and 28 interviews.”

Comment 9:
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”?

Response 9:
Again, see comments above about methods. We have consistently and rigourously used familiar and standard analytic techniques.

However, to help resolve any confusion:

- We have added new references to previous research used to develop the interview guides, “Initial interview guides were developed based on previous research [9,21,22,27,31,32]”.

- The concept of “evaluation” using ‘accountability for reasonableness’ was has been used in a variety of studies and was developed by Martin and Singer. Please see Martin DK, Singer PA. A Strategy to Improve Priority Setting in Health Care Institutions. Health Care Analysis 2003; 11(1): 59-68. This is now sited on page 8.

- We have deleted the words “veracity of the data” as they created confusion.

- A member check was conducted with three leaders at the OWTS, please see page 8, “Fourth, a member check with three leaders at the OWTS verified the verisimilitude of the findings.”

Comment 10:
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].”

Response 10:
Again, this may be a matter of taste. It is true that some data sources were publicly available, however no one before has collected them and conducted the analysis as we did. No revision required.

Comment 11:
The use of quotation is well done.

Response 11:
We thank you.
[Editor: Please compare with Reviewer 1, Comment 3 and see our response.]

Comment 12:
I wondered at the need for tables 2 through 4. Do they really add to the text and understanding the text?

Response 12:
We believe that Tables 2 through 4 are helpful as they provide the reader with quick, easily identifiable summaries of key findings.

Comment 13:
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.

Response 13:
As stated, we believe that describing and evaluating a system-wide wait-time strategy (for the first time) would be helpful to others. We thank you for the compliment about the recommendations, which we believe are also helpful to similarly placed others.

Comment 14:
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.

Response 14:
As described in both the Introduction and Discussion, public involvement is a key element of achieving legitimacy and fairness in ANY priority setting initiative -- that is an important goal for the wait time strategy.
It remains unclear to us whether the OWTS is improving wait times -- that was not a goal of our study.

Comment 15:
The conclusion lacks flow and needs to be improved as a finisher of the article.

Response 15:
The conclusion has been revised (p. 34): “This study helps to address three gaps in the scientific literature. Firsts, we have provided an in-depth description of real world priority setting in wait time management with a focus on public engagement. Second, we have evaluated the description of priority setting in a wait time management initiative using ‘accountability for reasonableness’, and have identified both areas of good practice and opportunities for improvement which will be helpful to other decision-makers in comparable priority setting endeavors. Third, from this study we have developed practical guidance for when and how to engage the public in wait time management priority setting. Our approach – describe, evaluate, improve – can be used as a helpful learning platform for others engaged in priority setting.”

Comment 16:
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.

Response 16:
We appreciate this comment and have revised the title and abstract.