Reviewer’s report

Title: Task shifting in active management of the third stage of labor: a systematic review

Version: 0 Date: 19 Dec 2016

Reviewer: Jill Durocher

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Task-shifting in active management of the third stage of labor: a systematic review

Overall comments:

In light of WHO’s Optimizing health worker roles guidelines to improve access to key MNH interventions (2012) and other efforts around task-sharing, this is an interesting paper and topic worth exploring further. The role that women and lower level care providers can play in home-based PPH care, including self-administered uterotonics, is very critical to examine and understand in terms of safety, acceptability, and effectiveness. However, I have some reservations about the aim of this analysis as stated in the abstract and background and elsewhere in the paper in reference to ‘task-shifting AMTSL to unskilled birth attendants.’ For one, some of the AMTSL components are no longer recommended as routine components (i.e. controlled cord traction, uterine massage) and there are recommendations against their practice if the provider lacks appropriate skills. Thus, the goal really is not to task-shift at all some of the AMTSL components.

I think the paper and analysis would be much clearer if the analysis was focused on the task-sharing of uterotonic administration as opposed to task-shifting of AMTSL (since this acronym tends to refer to all components). Indeed the authors did end up focusing on the uterotonic in their analysis, but I think the background and stated aims for this paper/analysis may result in some confusion up front about what the goals/recommendations are around AMTSL and who should be doing what.

I also feel that task-shifting interventions and access to care needs to be accompanied by more contextual information. Successful effect of task-shifting cannot be evaluated alone by clinical outcomes and I think the feasibility/acceptability factors are the most important piece.

Specific comments for Background section:

Page 3, lines 10-18: According to WHO's 2012 guidelines on PPH, controlled cord traction and uterine massage are NOT recommended practices for all women (now optional). I think describing these components as cornerstone interventions of AMTSL is slightly misleading in this opening paragraph.

Page 3, lines 33-39: Similar to above comments, it says 'Task-shifting AMTSL (components) to CHWs and TBS who attend home deliveries...has been explored as a community-based
strategy,' but I don't think this statement accurately represents the original goals of the trials included in this systematic review. Most of the community-based trials focused on the uterotonic administration and did not look at 'task shifting AMTSL components' (i.e. whole package).

I would strongly recommend re-writing the background section to avoid general reference to 'AMTSL' (consider deleting from the title as well?), since this description and its acronym has several different meanings.

Specific comments for Methods section:

Page 4, lines 8-10: Refers to inclusion of 'trials evaluating effect of task shifting of components of AMTSL', however, I think it needs to be clarified that the original trials did not set for to measure the effect of task-sharing. The 'task shifting' phrase/discourse is being applied after the fact and used by the authors of this systematic review as new lens for analysis of the results. Thus, I would recommend modifying how you describe the trials selected and their goals.

Suggest defining skilled, unskilled, and task-shifting in the Methods section.

Page 4, line 25: I would strongly recommend analyzing findings from trials where women self-administered separate from TBA-administered medicine. Some of the TBAs who participated in these studies were 'trained TBAs' with a lot of skills (more than CHWs perhaps). This comment is in reference to the Mobeen et al trial from Pakistan in which I participated.

Page 4, line 30: If the primary outcome is incidence of PPH, I would suggest that you specify which trials objectively measured blood loss and which ones did not (i.e. in the tables). However, in my opinion, this analysis would be sufficient if it only focused on safety, acceptability, and feasibility as opposed to PPH incidence. There have been other systematic reviews of these same community-based articles reporting on the effect of interventions on PPH incidence.

What about acceptability among providers? Could this be analyzed?

Specific comments for Results section:

Page 6, line 34: The statement that 'controlled cord traction, uterine massage…were not reported in any of the studies' is not accurate. In the Mobeen et al trial, these components were documented and reported on. That said, I don't think this needs to be the focus of this paper.

Overall, the results section is very long and I would put forth the suggestion to reduce the level of detail, especially if the results are contained in the tables.

Specific comments for the Discussion section:

Page 9, lines 28-36: Please revisit WHO's PPH guidelines and update this paragraph. WHO recommends administration of uterotonic by skilled and unskilled birth attendant (anyone trained
in its administration including TBAs). Their current guidelines do not recommend self-
administration by women, however.

Page 9, line 42: I think it would be helpful to clarify/edit the term 'misuse' to describe it as
'mistimed administration' as opposed to misuse.

Page 9, lines 51-55: I question whether the sentence on recovered/unused miso is important for
this paper and for PPH programs promoting task-shifting.

Page 10, lines 38-40: I sort of disagree with the first cited strength of this review (inclusion of
all AMTSL components). The following sentence refers to 'earlier review's' which requires
citations.

Page 10, lines 42-44: I don't understand the point about heterogeneity of interventions,
especially since the focus of the review/analysis ended up being on uterotonics.

Specific comments for the Tables:

As per above comments, I would strongly recommend analyzing findings from trials where
women self-administered separate from TBA-administered medicine. Some of the TBAs who
participated in these studies were 'trained TBAs' with a lot of skills (more than CHWs perhaps).

I am not sure I understand the point of Tables 1A and 1B. What do these tables show that's not
already known in the literature or from other systematic reviews of community-based PPH
studies? Table 2 seems to be the most critical one for this paper/analysis.

Re Table 3 on side effects, I would suggest to the authors that they consider this table. Side
effects rates by comparison arms have been presented previously in other analyses and are well-
known. I'm not sure what this table adds in the context of task-shifting, especially since there is
no additional information on how the side effects were managed and by whom. I also would
strongly suggest deleting the column "severe side effects (abortion, pre-term)," especially since
almost every row says n/a and one includes data on livebirths. The data seems incomplete and
calling abortions 'severe side effects' sounds strange. If the authors want to discuss mistimed
use, why not include any available info in the column on correct dose/time in Table 2?

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

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