Author's response to reviews

Title: Transfers to hospital in planned home births - a systematic review

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Author's response to reviews: see over
Re: Manuscript reference No. 6919737361162398

We want to thank the reviewers for their useful comments which enabled us to improve the quality of our manuscript.

Changes in the manuscript are highlighted in yellow. In accordance with additional editorial request, we had a professional editing service (Edanz). We have added the legend and title of the figure after the references in the manuscript file. The tables are now included in the manuscript file.

We hope that the revisions in the manuscript and our accompanying responses will be sufficient to make our manuscript suitable for publication in BMC Pregnancy & Childbirth. We regret that we exceeded the time limit (April 6th), the delay is according to that the manuscript needed an extra round of editing.

Responses to the comments of Reviewer #1

1. Page 5, Line 6; is the figure 16,848 correct? In line 4 you have 168,618 as a figure for one study, but the 16,848 suggests another figure of this magnitude for another study which does not add up with what you say in Line 4.

Response: We believe our text is correct:

“The 15 studies included totally 215,257 women with a planned home birth upon onset of labour. The Dutch study [24] included 168,618 women representing 78% of all women included in the review. The other 14 studies contributed with 46,639 women, the study populations varied from 70 to 16,848 women (Table 1).”

168,618+46,639=215,257

2. Page 5, Line 20; you state that one of the studies was assessed to be of good quality, yet you indicate 2 references, 1 and 19; which one is the correct one?

Response: 1, this is corrected

3. Page 7, Line 22; this sentence is posed as a question which does not read so well. Perhaps you might consider rephrasing to read; ‘this poses the question as to whether independent midwives are more willing to accept such women, or whether the women themselves are exerting pressure on midwives to accept
them for home birth’

Response: We have corrected according to the recommendations from reviewer 1.

Responses to the comments of Reviewer #2

1. In the background – could the authors indicate their rational for focusing on the three reasons for transfer that are outlined, i.e. fetal distress, hemorrhage, and respiratory problems in the infant. Why do the authors look back as far as 1985 (almost 30 years ago) and why do they limit studies to Western countries?

Response: At page 4, we have added:
“The review was limited to include studies from Western countries were in order to achieve some homogeneity across study populations and health care systems. Since the late 1970s, women with increased risk for adverse outcomes have not been recommended and usually not accepted, for home birth or birth in other midwifery-led settings.”

2. Under the assessment of methodological quality, how did the authors combine the various factors assessed to decide whether to keep a study or not? If any of the evaluation criteria were not met, was the study excluded?

Response: We have clarified the assessment of study quality and added (p 4):
“Studies scored as poor were excluded from the review.”

3. The authors note that it is difficult to assess what transfer rates should be to give the best outcomes. While the recommendations for uniform definitions for emergency transfers, and consistent methods or defining rates of transfers are important, the actual rates of transfers are not indicators of either quality of care or potential for adverse outcomes. In some settings, for example, high rates of transfer may be due to adverse weather conditions in winter seasons with the need for anticipatory planning in case of further weather deterioration. The ultimate relevant outcome is how the overall outcomes from planned home birth compare with planned hospital birth, with transfer protocols being part of the continuum of care for planned home births. I strongly disagree with the statement that outcomes should be assessed after transfers. Of course outcomes for this group are likely worse. The relevant question for evaluation of programs of care and policy making – is what the outcomes are based on the woman’s decision to plan home or hospital birth overall. The transfer is one component of home birth. If there are negative outcomes associated with transfer, home birth can still have better outcomes overall if care in the hospital setting is less than ideal. Also, what do the authors mean by “too strict criteria for transfers” and how would they
recommend that this be evaluated?

Response: We agree with Janssen that the transfer rates are not indicators of quality of care. We have extended the discussion according to her advice and added:

“Rates of transfer are not necessarily indicators of quality of care or potential for adverse outcomes. High rates may be due to weather or traffic conditions with the need for anticipatory planning. However, a low transfer rate may lead to cases of death and serious morbidity that could have been avoided. A high transfer rate may lead to unnecessary interventions and less patient satisfaction. “ (p 7)

We agree with Janssen that transfer is one component of home birth and that outcomes are based on the birthplace at onset of labour. We have removed the statement that that outcomes should be assessed after transfers.
At p 8, we have added:

“Performing audits to evaluate adverse outcomes during or after transfer to hospital would probably be useful. Audits may lead to improvements in health services (e.g., better information between the home birth midwife and hospital, preventing delay in decisions, and transport plans).”

We also agree that the term “too strict criteria for transfers” is unclear. The term is removed. The term “transfer” is defined in the abstract.

We also agree that the importance of women’s satisfaction is outside the scope for the paper. The paragraph is removed from the conclusion.

Responses to the comments of Reviewer #3

1. I suggest «Conclusion» changed to «Implications for future studies», as I think this extremely well performed systematic review deserves a more clinical conclusion, not just guidance for renewed review, being the largest systematic review ever on homebirth.

Response: We have changed “Conclusion” to “Implications for future studies”.

2. I would appreciate some formulations on the clinical impacts or extrapolations of their findings, perhaps also in a historical light, or in light of the global challenges in the developing part of the World. Just to lift the paper a bit more.

Response: This is a very important topic, but since we have restricted our study to Western countries it is outside the scope of this study. In many developing countries, all women do not have access to medical care and transfer to hospital is not always an option.


Response: It is corrected.

4. Page 7, Highlight (Yellow):
Content: "In one study, slow progress was one of the definitions for an emergency transfer [22]. This is usually not regarded as an emergency situation."
Comment: Would it be possible to adjust for this and reanalyze?

Response: No, it is not possible.