Author's response to reviews

Title: The Effect of Health Facility Delivery on Neonatal Mortality: Systematic Review And Meta-Analysis

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Author's response to reviews:

Dear Editors,

BMC Pregnancy and Childbirth,

First of all, we are grateful to all the editors and reviewers for their concern and effort by devoting their time to review and improve the quality of our manuscript.

With this, all the forwarded comments are well taken and addressed point by point as follows.

Comments of Reviewer-1: Xing Lin Feng

Introduction section:

1. The paragraph describing the situation in Ethiopia should be deleted or replaced by that in Africa for international readership

Response: this comment is well taken and the situation of Ethiopia was deleted and that of sub-Saharan Africa is included (indicated under introduction section: page #1, parag. 2)

2. Important references should be added. For example, in last paragraph, page 3, the reference that the WHO recommends hospital delivery should be carefully cited. Since as I know, more formal statement made by the WHO is “skilled birth attendance”

Response: accepted and the paragraph is modified to refer to skilled care.

3. Some jargons in this area should be used, for example, “Skilled support during labor” ( the same paragraph as point 2) should be changed to “Skilled birth attendance”

Response: accepted and the paragraph is modified to refer to skilled care.
Methods section:

1. It is not clear why the authors exclude studies on the effects of home delivery on neonatal mortality (page 7, exclusion criteria).

Response: effects of home delivery were not excluded. The problem was the way it was stated. Here, what was excluded was that studies in which planned home delivery (low risk) were compared with planned hospital delivery (high risk). This is rephrased and corrected.

Results:

1. By simple calculation, the coverage of hospital delivery for the pooled samples in the 19 studies is $1,504,450/ 1,606,805=94\%$. 94\% is a very high rate of hospital delivery coverage for the developing countries investigated. It means that the sample is extremely biased towards the study population-“all deliveries” rather than deliveries “in hospitals”.

Response: The facility delivery coverage is not only for hospitals but also include health centers. Moreover, this figure is higher, because, as seen in table 1, the first 2 studies (1 in China and 1 in Italy) have very big sample size. So, Italy is among the developed countries and China is also almost closer as compared to African and other developing Asian countries.

To avoid this confusion, stratified analysis was done based on the sample size and level of health facility delivery coverage, indicated in Table 2.

2. The stratification analysis shows that the fixed effects are different from random effects in CROSS SECTIONAL and COHORT studies, which mean heterogeneity DOES exist in the two study categories. Therefore it is unfair to say”With this, the difference in study design is less likely to be the cause of heterogeneity [Table 3].”

Response: This comment is accepted and corrected, as indicated on page #10, parag. 2 & 3.

3. Due to the above 2 comments, I suggest the authors to stratify their analysis by the coverage level of hospital delivery in the various countries to make their analysis more persuasive.

Response: accepted and stratified analysis was done. Indicated on page #10, parag. 2 & 3 and Table 2.

Discussion Sections:

1. Remove the paragraph regarding Ethiopia.

Response: removed

2. The bias as mentioned in the 1st comment of results should be carefully
discussed.

Response: This comment is incorporated under discussion section page #12, parag. 2.

Comments of Reviewer-2: Mats Malqvist

1. The search strategy is still a bit confusing. When I enter the search string described in the ms I get only 15 hits in pubmed. Without the " " I get 4008. Neither of these corresponds to the stated 2218, even if the time frame was different. Please revise and be sure to get all the " and boolean words right.

Response: the words were not used within the contention mark (" "). The contention mark was used while describing the combination key terms in the text. Now, this is removed to avoid confusions. Concerning the way it was searched, first of all, we used EndNote software to access records from the PUBMED. The combination key terms (Place of birth AND neonatal mortality, place of delivery AND neonatal mortality, health facility delivery AND neonatal mortality, home delivery AND neonatal mortality) were entered one by one in the dialog box of keyterms(MeSH) in the EndNote software. Then all the identified records were imported to a single library and articles published before 1980 were excluded, this ended up with 1802 records. The rest were searched from Advanced Google scholar and other data basis as stated in the methodology. In addition descendent and ancestor search strategies were used, that is why you couldn’t come to the final 2218 records. This paragraph is rephrased and stated in the methodology section.

2. The second paragraph in the introduction is about the situation in Ethiopia. Even if the authors are from Ethiopia it is a bit unclear why this section is here. I would suggest instead widening the introduction and further elaborate on the international discourse regarding home delivery. May be through a historical review of how strategies have changed over the years.

Response: well taken and addressed in the introduction section paragraph 2.

3. It could be made clearer in the abstract and the conclusions that the pooled effect size is for health facility deliveries in resource-poor settings, so not to confuse with the debate about planned home deliveries in the European and American settings

Response: accepted and addressed in the discussion and conclusion section as well as the abstract.

Comments of Reviewer-5: Victoria Nankabirwa

1. Consider limiting the studies to low and middle income countries (LMICS) because:
a. There are huge differences in this field between high income countries and LMICs

b. There is only one high income country that is included in the review. Conclusions from this paper may still not be generalizable to high income countries.

c. The authors can focus & strengthen their discussion & introduction sections based on LMICs

Response: the comment is well taken and addressed in the abstract, introduction, discussion and conclusion sections.

2. Secondary data analyses or retrospective data analyses are not in & of themselves study designs. These analyses can be done with data from cohort, cross-sectional or case-control studies. I suggest that these analyses be distributed in their appropriate study design categories.

Response: though the authors of the original studies stated the design as secondary data analysis, by looking in detail into the methodology, this is corrected as cross-sectional design.

3. Several limitations are explained in the methods section. I suggest that these are transferred to the limitations section in the discussion.

Response: accepted with appreciation and moved to limitation section. However, the limitations of the original studies under the exclusion criteria section are still retained.

4. Consider discussing the possibility that your overall effect could be an underestimate of the true association between your exposure and outcome variables. This is because in areas with low facility delivery uptake (which is the case in many LMICs), the likelihood that only very high risk deliveries end up at health facilities is quite high. As such, deliveries at health facilities could represent to a great extent those women that initially attempted to deliver at home and then resorted to a facility when the delivery failed to progress (information bias/misclassification). Because these are extremely high risk, the associated mortality in hospital deliveries could be exaggerated, while that happening among home deliveries could be underestimated.

Response: This comment is well taken and stratified analysis was done based on the coverage level of health facility delivery and included in the results and discussion sections. This is indicated on page #2 and figure 2 of result section and discussion section page # 12.

5. The discussion section generally needs tightening

Response: by accepting the comment, we have looked at the discussion again and tried to make as tight as possible.
Minor Essential Revisions

6. Are there strengths to this analysis? Perhaps these could be added to the discussion.

Responses: there are many strengths in the design, comprehensive search, analysis and involvement of multi profession in the review and analysis. But, we left these thinking of their importance as one can get the strength while reading the article.

7. Table 2 and figure 2 seem to provide more or less the same information, which is redundant.

Response: taking this comment positively, we removed Table 2 and instead added figure 3, the filed funnel plot.