Reviewer's report

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

Version: 2 Date: 21 November 2012

Reviewer: Xing Lin Feng

Reviewer's report:

Re-review report for the manuscript entitled “the effect of health facility delivery on neonatal mortality: systematic review and meta-analysis”

The authors have made appropriate revision regarding my comments. Before its acceptance, I would like to raise some minor comments for consummation.

Introduction

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

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”

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”

Methods

1. It is not clear why the authors exclude studies on the effects of home delivery on neonatal mortality (page 7, exclusion criteria). Since it is obviously that “home delivery” is contrary to “hospital delivery”, i.e. number of deliveries “in hospitals” + that “at home”= all delivery. Therefore the relative risk for neonatal mortality “in hospitals” = 1 divided by that “at home”. I bet the authors’ original consideration is to exclude “planned home birth” in developed countries because they are comparing risks among low risk pregnancies.

Results

1. Page 11, paragraph 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”. This comment should be considered as major.

2. Page 12, paragraph 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]."

3. Due to the above 2 comment, 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.

Discussion

1. Remove the paragraph regarding Ethiopia.

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

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

None