Reviewer’s report

Title: Maternal Associated Factors of Low Birth weight: A Hospital Based Cross-sectional mixed Study in Tigray, Northern Ethiopia

Version: 3
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Reviewer: Samson Gebremedhin

Reviewer’s report:

General remark:
The authors made a remarkable effort to measure the prevalence of low birth weight in public health institutions of Tigray region, Northern Ethiopia. The study is valuable as such undertaking has not been made in the region. Nevertheless, the manuscript needs some serious revision, before being considered for publication.

Major compulsory revisions:
1. I don’t think the objectives of the study – assessing the prevalence and correlates of birth weight – require a qualitative design. Further, the manuscript gives very little information on the way the qualitative study was designed, executed and analyzed. The qualitative findings presented in the results section are full of direct quotations of the respondents indicating that the data might have been inadequately analyzed. In the discussion section, no effort has been made to link the findings of the quantitative and qualitative elements of the study. Accordingly, I recommend the authors to remove the qualitative section from the manuscript. May be they can have in-depth analysis on the qualitative data and develop it into a separate manuscript.
2. The authors did not acknowledge that the study is liable to selection bias. The selection of respondents was only limited to governmental hospitals (i.e. births at private health institutions and lower level of the public health system like health centers and health posts are excluded). The authors should discuss the implication of such selection bias.
3. The authors claimed that all newborn-mother pairs at governmental hospitals of Ethiopia are the study population of the undertaking. I think, this category can be neither the source nor the study population for the study. The statement should be revised.
4. In the “inclusion and exclusion criteria” sub-section, it’s mentioned that mothers with severe medical or surgical condition and those who did not remember their LMP were excluded from the study. I fear the exclusion might have further introduced selection bias in the study. The authors should discuss the implication of their exclusion criteria.
5. Despite the study design had clustering nature; the sample size calculation does not consider design effect correction. The authors should discuss that their
sample size might not be adequate to estimate the prevalence of low birth weight precisely. Further they did not attempt to calculate power for identifying the correlates of LBW. Post-hoc power calculation should be considered for major predictors of LBW.

6. In different parts of the manuscript, the authors used 50 kg as a cutoff point to classify maternal weight. But it’s not clear how they end up in this cutoff point. If they have a reference it should be cited. Otherwise, especially in the multivariate model, more acceptable way of classification (like weight quartiles) should be used.

7. In the result section, the authors tried to describe the pattern of weight gain during pregnancy. But determining weight gain in pregnancy requires enrolling and weighing the mothers as early as possible in the first trimester. Considering the fact that mothers in Ethiopia start ANC late in the second trimester, the study will not be in the position to correctly determine the extent of weight gain. Hence such information should be removed.

8. In the multivariate logistic regression analysis, the authors described the variables that were found to be significant in the model. However, they should also tell the readers: (1) the list of variables considered in the bivariate model; and (2) how the variables were selected for the multivariate model.

9. In the discussion section the authors compared their finding with that of prevalence of LBW reported in the DHS report. However, the two studies are not totally comparable. To start with the DHS survey is a community based study (in contrast to the health institution based nature of the current study); further, the DHS survey estimated child weight based on the subjective reported of the mothers – i.e. not by having birth weight measurement.

10. The discussion is generally weak. The authors only tried to compare their findings with other studies. They did not try to acknowledge and discuss the limitations, potential biases and implications. The discussion section requires serious revision.

Minor essential revisions:

1. Recently, reasonable numbers of studies have tried to measure the prevalence and correlates of low birth weight in Ethiopia. However, in the background section, only few are cited. In the introduction section, the authors should summarize the findings of the available recent studies and indicate the existing knowledge gap.

2. As the study is mainly conducted in the central zone of Tigray region, the study setting description should be limited to the zone (i.e. not to the whole Tigray region).

3. In the methods section, no information is given on the personnel used for the data collection. Who collected the data? Did they get any training?

4. As the study used quota sampling technique, reporting the “response rate” does not give much sense. The information should be removed from the manuscript.
5. The manuscript presented the association between chronic maternal problems and low birth weight. But the operational definition for “chronic maternal problems” has not been described and the proportion of mothers with such problem has not been stated.

6. It's not clear if the authors had tried to check the assumption of the logistic regression analysis. Assumptions like absence of interaction and multicollinearity should be check. For instance, multicollinearity is likely between maternal weight and MUAC.

7. In developing countries, factors like parity, birth interval, economic status, household food insecurity status are commonly identified as determinants of LBW. But the authors did not say anything about these variables. Were they considered in their study? If yes, what were the findings?

8. As the study mainly intended to estimate prevalence of LBW, the figure should be presented with its 95% confidence interval.

Discretionary revisions:

1. Grammatical errors are not rare in the manuscript. In some cases, sentences are too long to understand. Language edition and proof reading may improve the readability of the document.

2. In the “sampling techniques and procedures” sub-section, the phrase “consecutive sampling technique” should be corrected as “quota sampling technique”.

3. In the ethical consideration section the phrase “letter of permission” should be corrected as “ethical clearance”.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:

I declare that I have no competing interests