Author’s response to reviews

Title: Maternal obstetric and socio-demographic determinants of low birth weight: A retrospective cross-sectional study in Ghana

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Author’s response to reviews:

Dear editor,

Please, find herein point-by-point responses to the critique by the reviewers of our manuscript entitled “Maternal obstetric and socio-demographic determinants of low birth weight: A retrospective cross-sectional study in Ghana”.

Our responses are organised and presented here, addressing each reviewer’s comments. Reviewer #1 comments are addressed before reviewer #2 then reviewer #3. Comments of the reviewers are dubbed “critique” and authors’ responses are dubbed “response.” Reviewers’ comments are made bold whiles authors’ responses are italicized, highlighted yellow and presented below every critique.

Reviewer #1

1. Critique: The first sentence of the Plain English Summary 'At birth, most newborns weigh between 2.5kg and 4.0kg', would be clearly and more accurate if edited to 'At birth, most term newborns weigh between 2.5kg and 4.0kg.

Response: We edited the sentence as suggested by the first reviewer in the manuscript.

2. Critique: 'low birth weight babies are exposed to birth asphyxia' could be rephrased to 'are more susceptible to birth asphyxia'. This sentence should also be referenced.

Response: We edited and referenced the sentence as suggested by the reviewer in the manuscript.
3. Critique: Again further referencing would be useful for the statements 'Notwithstanding these immediate health consequences, low birth weight has long-term consequences in the form of growth inhibition, impairment of cognitive development, and increased incidence of chronic diseases such as type 2 diabetes, hypertension and cardiovascular diseases.

Response: We edited and referenced the sentence as suggested by the reviewer in the manuscript.

4. Critique: The sentence 'The 2014 Ghana Demographic and Health Survey reported a low birth weight of 10% among infant with recorded birth weight in Ghana' is confusing and needs restructuring for example 'The 2014 Ghana Demographic and Health Survey reported that of those newborns weighed at birth, 10% had a low birth weight'

Response: The authors edited the sentence as suggested by the first reviewer in the manuscript.

5. The sentence 'However, most LBW in developing countries is due to intrauterine growth retardation whereas in developed countries prematurity is the common factor in LBW' (4) This reference is from 1987, over 30 years old. I would recommend the authors reference more recent literature and check this accuracy of this statement.

Response: the sentence has been rephrased and a more relevant and recent literature reference in the manuscript.

6. Critique: I would also suggest that referring to 'developing' and 'developed' is outdated and should be replace with low or high income countries.

Response: the words ‘developing’ and 'developed' have been replaced with low or high-income countries in the manuscript.

7. Critique: I would also suggest the intrauterine growth 'restriction' is used instead of 'retardation'

Response: the word ‘retardation’ in ‘intrauterine growth retardation’ have been replaced with ‘restriction’ in the manuscript.

8. Critique: Later in the Background section the sentence 'In many low-income countries, low birth weight is associated with socioeconomic conditions, infant sex, parity, maternal age...' should be more specific i.e. 'is associated with lower socioeconomic status, female sex etc.."
Response: the authors have edited the sentence to make the words more specific as suggested by the reviewer, such as ‘lower socioeconomic status’, ‘young maternal age’, ‘low level of maternal education’, ‘female sex’, etc.

9. Critique: Can the authors elucidate the reasons for not including stillbirths in this study as this may have provided useful information and highlighted important risk factors?

Response: Low birth weight for most of the stillbirths were missing, therefore the authors decided to exclude it.

10. Critique: In the Data extraction and sample size section, the authors report that 'Data collectors were supervised on a daily basis...' , please specify by whom?

Response: The second author supervised the data collectors. We included this statement in the revised manuscript

11. Critique: In the Explanatory (Independent) variables section 'Maternal age is defined as the age of a mother at the time of delivery.' This sentence is self-explanatory and I would recommend removing it.

Response: We have deleted the definition of maternal age as suggested by the reviewers.

12. Critique: 'Maternal age at birth was categorised into four groups: >20' the first group should be corrected to <20.

Response: the greater than (>) sign has been corrected to less than (<).

13. Critique: 'Obstetric factors that were extracted are gravida, parity..' should be corrected to 'Maternal factors that were extracted included gravidity, parity...'

Response: 'Obstetric factors’ were replace with 'Maternal factors as suggested by the reviewers.

14. Critique: It would be useful for the authors to define gestational age of viability used in this study/setting.

Response: literature search did not reveal standardised definition of gestational age of viability in Ghana. However, the authors considered W.H.O’s survival rates of babies born after 32weeks of pregnancy.
15. Critique: Did the authors take into account the gestation at which the haemoglobin levels of mothers were measured/recorded, it would be useful to provide the rationale for why this was or was not taken into account.

Response: We did not categorise mothers haemoglobin levels based on gestational age the data were extracted from Hospital records and the Hospital does not categorise haemoglobin based on gestational age.

16. Critique: In the Discussion section 'Surprisingly, in this study...'. Can the authors elucidate why this is surprising given they've recognised that this is consistent with other studies in Ethiopia and Bangladesh?

Response: the authors have rephrased the sentence by removing the word ‘surprisingly’.

17. Critique: Also, 'It seems possible that these results are due to urban women attitude towards antenatal care, interpregnancy interval...'. Can the authors be more specific about what they mean by 'urban women attitude towards antenatal care' and reference relevant findings in the literature.

Response: we have revised this to include. For instance, In Zambia, Banda et al. found that urban women did not believe that early commencement of ANC offers any benefits, they had inadequate knowledge about ANC, and negative cultural beliefs about early ANC attendance (Banda et al. 2012). Evidence from a case-control study in Brazil revealed an association between poor ANC attendance and increased risk of LBW (Regina et al. 2014). Given the relevance of early ANC attendance to maternal and neonatal health, these negative attitudes may account for the LBW among urban women in this study. However, unlike this study, an earlier study in Ghana reported an increased risk of LBW among women from rural settings.

18. Critique: Later in the discussion, 'The most important limitation of the current study lies in the fact that the data used for the analyses were primarily collected for healthcare services and not for research purposes.' Please expand on this, this could potentially be seen as a strength of the study as if this is routinely collected data is may allow for ongoing monitoring and future studies following implementation of public health interventions.

Response: we have revised the paragraph as “The most important limitation of the current study lies in the fact that the data used for the analyses were primarily collected for routine healthcare services and not for research purposes or for a specific intervention. Errors may have occurred during the documentation of the records. Notwithstanding this limitation, routine collection of health data may allow for the monitoring and evaluation of public interventions. Finally, the study analysed data from one hospital and findings may not be generalizable to mothers who attended other hospitals and those who delivered at home.”
19. Critique: 'The current findings add substantially to our understanding of the determinants of birth weight and provide empirical data for clinical and public health interventions aimed at reducing low birth weight and associated complications.' Can the authors be more specific about the how or what has been added to the current understanding? This is a very general statement.

Response: The evidence from this study suggests that maternal educational level, residence, haemoglobin level, parity, number of ANC visits, and gestational age are independent predictors of LBW. These findings contribute to the growing literature on the influence of maternal socio-economic and obstetric factors on LBW in resource-constrained settings. This could guide the development of clinical and public health interventions aimed at reducing LBW and its associated complications.

Reviewer 2:

1. Critique: This study used retrospective hospital based data to examine determinants of low birth weight babies in the Sunyani Municipality of Ghana. This could be a useful contribution to the literature and help Ghana identify opportunities for maternal health interventions to prevent low birth weight. It will need some proof reading and editing before publication (ie: p 10, Line 2 "Majority" should not be capitalized).

Response: We have done some proof reading and some corrections done.

2. Critique: The abstract has lines directly copied and pasted from the main paper. It may be best to rework it so that sentences don't repeat themselves.

Response: the above suggestion has been considered and some rephrasing was done

3. Critique: Source 3 should be checked - in one part of the introduction it's referenced as the DHS (p. 4 line 34) and in another part of the introduction source 3 sounds like it should be a study or peer-reviewed paper (p. 5 line 5).

Response: editing and appropriate citations were done in the revised manuscript.

4. Critique: Literature should be cited after it's summary intro sentence (p 5, line 8)

Response: the reference was appropriately cited as suggested

5. Critique: Include the full N in the top line of the table
Response: Total number of participants were quoted on the top line of the table as suggested

6. Critique: Expand the title so that the table can stand on its own (ie: Socio-demographic characteristics of women delivering at XXX hospital between XX 2017-2017)

Response: We expanded the title of all the tables to include study setting and the period as recommended by the reviewers.

7. Critique: I'm curious why blood type is presented - Is that associated with LBW?

Response: some current studies have reported an association between low birth weight and blood type. Therefore, we included blood type to test this finding in the current study.

8. Critique: Presumably, the authors don't have all the variables for every woman in the study, so it would be good to have some reference to that (maybe in footnotes) and, in the methods, describe how you handled messiness.

Response: the authors can confirm that we had all the variables for all the women in the study. This was double-checked during the revision and all variables for all the women summed up to the total sample size of 931 participants

9. Critique: Why is blood type necessary to include? Is it an important factor for LBW?

Response: this question has been responded to in critique number 8 for reviewer 2

10. Critique: Tables 1 and 2 could probably be combined as well as Tables 3 and 4

Response: Authors appreciate the suggestion of reviewers to combine tables 1 and 2 as well as tables 3 and 4. However, we believe that if the two tables are combined, it will appear excessively long and this may make it less reader friendly

11. Critique: Footnote what variables were controlled for in each model

Response: The authors have indicated in the footnote the variables, which were controlled for in each model

12. Critique: Study setting: It would be nice if the authors expand upon the context of the municipality in the 'study setting' section. They bring it up a lot in the introduction to justify the purpose of the study, but then don't give any context. How is Sunyani
Municipality different or similar to other parts of Ghana? Size, urbanicity, density, stability? Is Sunyani Municipal Hospital the only place women give birth? Or are the more complicated births referred there? A bit of context would be helpful.

Response: the study setting has been expanded and revised

13. Critique: Data extraction and sample size: Justify why you opted to exclude half the sample (multiples, still, congenital abnormalities, etc.) It could be clarified by just adding on a 'because….' clause.

Response: Stillbirths were excluded because the birth weights of the few cases of stillbirths that were recorded within the study period were missing. We excluded records of multiple births and congenital abnormalities because they have a different aetiology and risk profile for LBW

14. Critique: Cite the relevant studies being referred to in the first sentence.

Response: the relevant studies have been cited in the revised manuscript

15. Critique: Authors should consider including primigravidae as its own category, rather than lumping it in a 1-3 category.

Response: this suggestion has been included in the revised manuscript

16. Critique: If the authors have it, timing of first ANC would be good to include as it can reasonably be correlated with birth outcomes, particularly LBW.

Response: the birth records from which we extracted the data did not classify/ include timing of first ANC visit. Therefore, we could not include it in our analysis.

17. Critique: When segueing into table 3, it would be good to remind the reader that only XXX and XXX were retained because they were the only variables significant in the unadjusted models

Response: We appreciate this suggestion and we have revised this section as suggested.

18. Critique: Not critical to present the unadjusted results in the narrative; the adjusted results would be just fine and your reader can refer to the table.

Response: this critique is considered in the revised manuscript.
19. Beginning with the second sentence of the discussion (p 14, line 23), the entire first paragraph does not align with the result presented. Table 3 suggest ages is not a significant predictor of LBW in this population, using the significance testing limits outlined in the methods. Authors should revisit the discussion to ensure it aligns with their findings.

Response: We have rewriting the sentence to align with the results presented in the study.

20. Critique: Second paragraph (p. 14, line 55) equates no schooling with being illiterate. Suggest changing illiterate to better 'those with no formal schooling.'

Response: We have changed the word ‘illiterate’ to ‘those with no formal education’

21. Critique: P. 15, line 18-32: I think this paragraph warrants more discussion. The authors posit that it may be related to antenatal care per Azimul et al. It's unclear from their findings what ANC utilization is by urban women.

Response: The paragraph has been revised and appropriately referenced. For instance, we stated that “In this study, the likelihood of delivering a LBW baby was significantly high among urban dwellers, which is consistent with studies in Ethiopia (Tema 2006) and Bangladesh (Azimul et al. 2009). It seems possible that these results may be due to some urban women attitude toward antenatal care, interpregnancy interval, and iron and vitamin supplementation during pregnancy (Azimul et al. 2009). For instance, In Zambia, Banda et al. found that urban women did not believe that early commencement of ANC offers any benefits, they had inadequate knowledge about ANC, and negative cultural beliefs about early ANC attendance (Banda et al. 2012). Evidence from a case-control study in Brazil revealed an association between poor ANC attendance and increased risk of LBW (Regina et al. 2014). Given the relevance of early ANC attendance to maternal and neonatal health, these negative attitudes may account for the LBW among urban women in this study. However, unlike this study, an earlier study in Ghana reported an increased risk of LBW among women from rural settings (Abubakari et al. 2015).”

22. Critique: Generally, the discussion needs strengthening and to be better grounded in the results. Additionally, the purpose of the paper was to identify drivers of LBW to better identify opportunities for maternal health interventions. It would be a stronger discussion if that were pulled back into it.

Response: We have done some revision in the discussion of the manuscript.

24. Critique: Why did you choose to do two separate models, one for socio-demographics and once for obstetric factors? It seems it would be important to analyze all the factors.
Response: we did that to make it reader friendly

25. Critique: How did the authors handle the collinearity between gestational age and LBW? I actually expect there is other collinearity too, so please clarify.

Response: Since collinearity becomes a concern in regression analysis when there is a high correlation or an association between two potential predictor (independent) variables, there was no collinearity between gestational age (independent variable) and low birth weight (dependent variable). Additionally, collinearity diagnosis using variance inflation factor (vif) did not reveal collinearity among the predictor (independent) variables that were included in the model. The image below shows the vif of the predictor variables.

26. Critique: The introduction and methods are clear and well organized, though the methods will need a bit more depth. The results and discussion need a bit more work to really highlight what the results mean in the broader context and within the specific purpose of this paper.

Response: some revision have been done to the methods, results and discussions of the manuscript

27. Critique: This paper will need some proofreading and editing before publication (ie: p 10, Line 2 "Majority" should not be capitalized).

Response: some proofreading was done in the revised manuscript

28. Critique: I suggest authors try to better incorporate the literature in the discussion, rather than simply say it aligns or doesn't align with other findings. A deeper story on what that may or may not mean is important.

Response: The methods and result sections have been revised and relevant literature incorporated in the discussion section.

29. Critique: Tables need appropriate formatting

Response: some formatting have been done to the tables

Reviewer 3
1. Critique: I would suggest a slow read-through by somebody familiar with the topic but who hasn't been involved in the study. Some of the introduction could be written a bit more succinctly.

Response: the authors have considered the suggestion and have engaged an independent person to vet the script

2. Critique: Throughout the manuscript you say Low birth weight; Suggest you alter to the well recognise abbreviation of LBW throughout this manuscript

Response: We have replaced low birth weight with LBW.

3. Critique: The references need to be checked for consistency (some in upper case). I think reference one needs to be updated with a more recent reference.

Response: reference one has been updated and the rest of the references have been double checked for consistency.


Response: we have seen the report and it has valuable information and has been incorporated into the revised manuscript

5. Critique: New-borns: Suggest change to newborn (as one word)

Response: the suggestion is accepted and applied.

6. Critique: In the background you talk of low-income countries and then developing countries. Change to the same, probably low-income or low-middle income

Response: the suggestion is accepted and applied.

7. Critique: P5, line 25: You say "livelong"; You mean "life-long"

Response: Yes, we meant life-long and we have changed it.

8. Critique: P7, line 12: You say 4 groups and describe 3: "Maternal age at birth was categorised into four groups: >20, 20-30, and <30". Also, think you mean <20 (not >20)
Response: it was a mistake; we have change it in the revised manuscript. We have also corrected the ‘greater than’ sign to the ‘less than’ sign.

9. Critique: P 7, line 17: JHS; Write in full the first time: junior high school.
Response: the suggestion is accepted and applied.

10. Critique: P 7, line 25: "Obstetric factors that were extracted are gravida, parity, number of ANC visits, gestational age"; Obstetric factors that were extracted were gravida, parity, number of ANC visits, gestational age,
Response: We have corrected the sentence.

11. Critique: P7, line 42: Intermittent Preventive Treatment in pregnancy with sulfadoxine-pyrimethamine (IPTp-SP) is taken by pregnant women during pregnancy to prevent malaria in pregnancy. You mention this at the start of this paragraph, so don't need full explanation again.
Response: the suggestion is accepted and applied.

12. Critique: P 7 Line 50-52: Haemoglobin level of mothers was binary coded based on WHO recommendation (11) as no-anaemia (≥11g/dl) and anaemia (<11g/dl). I am surprised that you used international estimates. Are Hb normally pretty good among pregnant women in Ghana? Or is there a national figure that is lower (given the amount of malaria there).
Response: National figure for anaemia in pregnancy is based on WHO guidelines.

13. Critique: P8, line 18,25,27: P<.05; Suggest write as <0.05
Response: the suggestion is accepted and applied.

14. Critique: Suggest: As shown in Table 2, the mean gestational age at birth was 37.95(SD=1.85) weeks. Most women (797; 85.6%) gave birth between 37 and 42 weeks gestation. Preterm birth (26-36 weeks) was prevalent in one hundred and thirty-four (14.4%) mothers. I am not sure you need to say this as it is the rest of women. Unless you want to highlight the number who gave birth prematurely. The majority of women (75.4%) were para 1-3; 15.4% (n=x) were primiparous.
Response: the suggestion is accepted and applied.
15. Critique: P10: three fourths; Change to three quarters.
Response: the suggestion is accepted and applied.

16. Critique: P9-10 I think a lot of these results don't need to be included, since they are in the tables. Maybe just highlight the key outcomes.
Response: the suggestion is accepted and applied.

17. Critique: P10 I don't think you need to keep including the (26-36) everytime you talk about PTB, just define it in your methods and re-iterate in intro to results
Response: the suggestion is accepted and applied.

18. Critique: Throughout the manuscript you say Low birth weight; Suggest you alter to the well recognise abbreviation of LBW throughout this manuscript
Response: the suggestion is accepted and applied.

19. Critique: After adjusting for the effect of other significant covariates, gestational age (OR 0.67 95%CI 0.59-0.76), parity (OR 1.43 95%CI 1.21-1.70), number of ANC visits (OR 0.78 95%CI 0.67-0.90), and maternal haemoglobin level (OR 0.78 95%CI 0.63-0.95) remained significant predictors of low birth weight. I am interested to know what the cut-offs were for the Hb levels having an impact on LBW. Are you using 11g/dl as the reference point??
Response: Yes, the authors used the 11g/dl as the reference point as recommended by WHO and adopted in Ghana.

20. Critique: I am interested to know the link between blood group and birth outcome. Did you have any literature to include in the introduction that links blood group to birth weight, otherwise I am not sure this adds anything to the paper.
Response: Evidence from several studies have demonstrated a link between ABO blood group and birth weight(Cozzoli et al. 2011; Beyazit et al. 2017). A sentence about this link is now added to the background of the study.