Author’s response to reviews

Title: Obstetric Determinants of Preterm Delivery in a Regional Hospital, Accra, Ghana 2016

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Author’s response to reviews:

Comments from BMC pregnancy and child birth – PRCH-D-18-00057

Obstetric Determinants of Preterm Delivery in a Regional Hospital, Accra, Ghana 2016

Dear Editor and Reviewers,

We would like to express our sincere appreciation for your insightful comments made to improve our manuscript.

Below is a point by point response of the comments made.

Thank you very much for your assistance.

Kind regards

Dr. Ernest Kenu
Editor Comments:

BMC Pregnancy and Childbirth operates a policy of open peer review, which means that you will be able to see the names of the reviewers who provided the reports via the online peer review system. We encourage you to also view the reports there, via the action links on the left-hand side of the page, to see the names of the reviewers.

Reviewer reports:

Luke C. Mullany (Reviewer 1): The researchers have described a straightforward analysis of factors associated with preterm birth among deliveries in a referral hospital in Accra, Ghana. The topic is important given that complications arising from preterm birth is the leading cause of child death globally. The extent to which this contribution adds new knowledge, however, is questionable, given that risk factors for preterm birth are largely well characterized. Rather it is the absence of a broad set of tools to prevent these outcomes that remains the leading gap in our knowledge preventing substantive reductions in preterm delivery in low resource settings.

Overall, the manuscript covers the basics, but the writing and presentation needs substantive improvements in spelling, grammar, and presentation/style prior to being ready for publication. I have summarized some comments, below, not in order of importance.

1. The statement in the results section of the abstract about the proportion of preterm seems out of place. Later we learn that this is the hospital-based prevalence of preterm during the study period; however, since these data come from outside the participants in the study, and readers readily understand that case/control studies do not in general provide burden of "caseness" estimates, the information should be removed from the results of the abstract.

Response: Thank you very much for your comment, this has been removed

2. The presentation of results in the abstract is problematic. The authors are discussing odds ratios (which is relative parameter estimate (i.e. it is relative to those unexposed), but they simply indicate odds. An improvement in language might follow something like this: "The odds of preterm delivery were increased 2.1 (95% CI 1.1-4.5), 1.7 (1.1-2.6) and 1.4 (1.2 - 4.6) times among those exposed to antepartum hemorrhage, premature rupture of membranes, or c-section, respectively."

Response: Thank you very much for your comment. This statement has been rewritten. (Page 1; Results section of abstract)

3. The ANC result has a CI with different values than the CI reported later in the text.

Response: thank you very much for your observation. This has been corrected. (page 13, first line under table 2).
4. The Conclusion statement of the abstract repeats what is already known prior to this study; the researchers should try to focus on what is new or learned because of this particular study.

Response: The conclusion has been rewritten to reflect the findings of the study

5. The recommendation for screening approach to high risk pregnancy outcomes should be rethought. Screening in this manner was abandoned as a realistic strategy many years ago, given its poor predictive values. The presence of known and quantifiable risk factors does not equate to good discriminatory power.

Response: The recommendation has been rewritten to reflect the findings of the study

Introduction

1. rupture, not "rapture"

Response

Thank you very much for your comment. This word has been corrected throughout the manuscript.

2. all babies are "precious", what do the authors mean?

Response

Thank you very much for your comment. This statement has been corrected throughout the manuscript.

2. The 2nd and 3rd last paragraphs before the final paragraph of the introduction do not provide a strong justification for this study. Those paragraphs appear to focus on the burden of preterm and contribution to mortality (something already stated in the first paragraph) and the lack of good data on preterm rates. Neither of these are particularly well suited for arguing for the need for this study, which is focusing on risk factors.

Response: Thank you very much for your comment. The introduction has been rewritten to clearly reflect the problem and the justification for the study.

Methods

1. One of the biggest weaknesses of this study is the use of a referral hospital to collect cases and controls; Shouldn't the controls be selected from among those women that were not referred to the hospital because of some complication? Later in the discussion section, the authors are left numerous times to explain that this or that factor could not be
detected as risk factor for preterm because of the fact that both term and preterm delivering women were referred.

Response: Thank you very much for your comment. However, though the facility is a referral facility, there are ANC attendees who are not referrals and these were the group of women controls were selected from. This has been well explained under the definition of controls, in the methods section (page 5)

2. Through the methods there appear to be repeated descriptions of the definition and selection of cases and controls. This needs to be written in a better flowing narrative.

Response: Thank you very much for your comment. The case and control definitions and selection procedure have been written with clarity. Methods section, page 6

3. It is hard to understand the various categories of women that were included as cases, especially as they seem to have been sourced from multiples wards (i.e. labor and delivery, NICU, and even postpartum visits?). Or was it controls that came to postnatal clinic? This seems to conflict with another description of the controls as been the prior and next woman delivering at term? Very confusing.

Response: Thank you very much for your comment. Cases were selected from the labour ward delivery registers and followed to the respective wards so that interviews could be conducted with the mothers. The sampling procedure has been rewritten with clarity. Methods section, page 6

4. Why were complicated neonatal cases excluded?

Response: Thank you very much for your comment. The complicated neonatal cases were not excluded but as their condition was critical, and admitted to the NICU, their mothers were not psychologically stable to to be interviewed. The case and control definitions and selection procedure has been well written with clarity. Methods section, page 6

5. Sample Size description is very confusing.
   a. "of the control were exposed" Exposed to what?
   b. what does it mean to "adopt" at 95% Confidence interval?
   c. the paragraph starting with "This assumption was ..." is particularly opaque. I was not able to understand this at all.

Response: Thank you very much for your comments, the sample size calculation sub-section has been well written explaining the various steps and parameters used in determining the sample size. (Methods section, sampling sub-heading, pages 6-7)

6. sought not "sort"
Response: thank you very much for your comment. However, this word could not be found after the manuscript was revised

Results

1. What is the second classification used by Ridge Hospital?

Response: Thank you for your question. Authors have decided to stick to the WHO classification since it is a globally accepted standard.

2. I don't understand the analysis with number of ANC. By definition the preterm cases will have less ANC since some ANC visits are scheduled for later in pregnancy when early deliveries have already occurred. The discussion about ANC should be limited to time-appropriate receipt of ANC content; the focus on ANC visits is too non-specific and subject to "survival bias" in this case.

Response: Thank you for your comment. We agree that by definition, cases would have less ANC visits than controls, however, based on the new WHO recommendations of a minimum of eight antenatal contacts before the birth of a child. It is expected that both cases and controls should have been for ANC visits at least four times before delivery irrespective of the gestational weeks at birth.

The point we are trying to bring out is the need to begin ANC visits in the early stages of pregnancy and continual attendance till delivery irrespective of the gestational age of the baby. That way, the pregnant woman can be monitored throughout the pregnancy till delivery.


3. The various tables should have full spelling of the abbreviations, as readers may only look at the abstract and the tables.

Response: Thank you for your comments. All abbreviations have been written in full. (Results section, pages 10-14)

4. The bivariate ANC result appears incorrectly described as the estimate (0.8) is outside the accompanying confidence interval (0.1-0.3).

Response: Thank you for your comment. The interpretation has been written out correctly. In the bivariate analysis of the obstetric determinants, ANC 0.2 (95%CI 0.1-0.4), (Results section, page 10)

5. I don't understand why previous LUSCS is protective?
Response: previous LUSCS was not found to be protective. Perhaps, this is because those with a previous LUSCS attended ANC at the hospital where doctors reviewed them at each visit with additional care. This has been written clearly in the revised document. (Results table 4, page 14)

6. The expression of odds ratios needs work. For example the adjusted ANC odds ratio (which has more substantive problems - see my note above) is expressed as 0.2 "times reduced". I think what you mean is that the odds of preterm delivery were 80% lower among those with 4 or more ANC visits.

Response: Thank you very much for your comment. The interpretations of the odds ratios have been reviewed. (Results, pages 10-14)

Conclusions

1. Overall the discussion section needs to be much more clearer about preterm delivery because of clinical indications. Did the authors not consider restricting the definition of preterm to spontaneous delivery? For example, what is the implication of including in the cases those women that were delivered/induced early because of accelerating complications due to hypertensive conditions?

Response: Thank you very much for your comment, however, this was not considered. We did not restrict preterm to spontaneous delivery.

2. Minor comment about multiple births: the text indicates that all multiple births occurred in the preterm group, but this does not seem to be the actual case in the table.

Response: thank you very much for your comment. This has been addressed in the revision document. (Discussion page 16, paragraph 2)

3. The authors appear to indicate that parity was the same across preterm and term because nearly all the mothers "have similar parity across term and preterm", which is not an explanation, but simply a restatement.

Response: thank you very much for your comment. This has been addressed in the revision document. (Discussion page 17, paragraph 1)

Reviewer 2 (Reviewer 2): PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?

Yes - there is a clear objective
DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?

No - there are major issues

EXECUTION - Are the experiments and analyses performed with technical rigor to allow confidence in the results?

Yes - experiments and analyses were performed appropriately

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?

No - there are major issues

OVERALL MANUSCRIPT POTENTIAL - Could an appropriately REVISED version of this work represent a technically sound contribution?

Probably - with minor revisions

Response:

PEER REVIEWER COMMENTS:

Response: Thank you very much for your comments. The entire manuscript has been revised and clearly written out.

GENERAL COMMENTS: This is a well planned and well conducted study.

The peculiarity of the study population limit the external validity of the study findings.

Introduction and Discussion could be significantly shortened.

Response: Thank you very much for your comments. The discussion and introduction sections of the manuscript has been revised and clearly written out.

REQUESTED REVISIONS:

The current study dervived data from a secondary hospital. Even in term controls there was a very high caesarean section rate (54%) as well as incidence of pre-eclampsia (13.9%). It is not
clear if this is peculiar of Ghana in general, or just applies to selected cases attending a secondary hospital, but it greatly limits the generalizability of the findings in the current study. Authors should explain this high prevalence of complications, and list them as limitations in the Discussion section.

ADDITIONAL REQUESTS/SUGGESTIONS:

Introduction and discussion need to be shortened.

Response: Thank you very much for your comments. The discussion and introduction sections of the manuscript have been shortened, revised and clearly written out as suggested.

Note: This reviewer report can be downloaded - see attached pdf file.

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- Competing interests
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