Author’s response to reviews

Title: Pregnancy and early motherhood among adolescents in five East African countries: a multi-level analysis of risk and protective factors

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Response to Reviewers Comments

Thank you for allowing us to resubmit a revision of our manuscript now entitled, “Pregnancy and early motherhood among adolescents in five East African countries: a multilevel analysis of risk factors” for publication consideration in your Journal (BMC Pregnancy and Childbirth).

We appreciate the reviewer comments, which we have considered carefully. An outline of the reviewers’ comments and our changes and/or rebuttal (in indented blue text) is described below. Our responses to the reviewer’s comments appear as highlighted text in the manuscript.

We address the reviewers’ comments below.

REVIEWER 1

Major comments are as follows:

1) Despite DHS data has been updated every five years, why you opened more than 20 years from 1989 to 2016.
Thank you for the feedback. The study used all DHS data collected since 1989/1990 in all the five countries for the trend analysis on adolescent pregnancy and early motherhood. As indicated by the reviewer, the DHS data is collected every five years, and this provided an opportunity to assess trends overtime (nearly 20 years).

2) I think it is clear from existing research that over 50% of adolescent women are unintended pregnancies, but have any new findings in this study?

We would like to point out that the focus of this paper is not actually on unintended pregnancy, but rather about adolescent childbearing and pregnancy experience. However, we did acknowledge in the background section that over 50% of adolescent pregnancies are unintended. Our intention is to use the trend analysis to show that high levels of adolescent pregnancy exists and continues to be a serious challenge to adolescent’s health and successful transition to adulthood, across many African Countries. Not all adolescent pregnancies are unintended, and our analysis focuses on the factors that are associated with childbearing and pregnancy, not just unintended pregnancy. More efforts are required to reduce adolescent pregnancy and early motherhood among the five countries.

3) The conclusion requires work to better summarize the paper and highlight the importance of its findings. I think that consistency will be lost.

We agree with the reviewer for this comment, we have summarized the discussion more concisely highlighting the important findings.

4) Descriptions about multi sectoral approaches and decision making suddenly came out of discussion. Please give a description based on the results.

Descriptions about the multi-sectoral approach comes from our findings that showed improved education, higher household wealth status, and exposure to media are significantly associated with lower risk of early pregnancy and motherhood among adolescents. Based on these findings, we therefore recommend that broader development programs that have positive impacts on girls educational and employment opportunities, may potentially influence their agency and decision-making around if and when to have children. We believe that the findings of the paper merit the discussion of these approaches, and therefore their inclusion in the Discussion section is appropriate.
Here are the minor comments.

1) There is no description of research design

We thank the reviewer for this great observation. We have now included a description of the study design in the manuscript. The study design for the DHS is clear - a cross-sectional survey that uses nationally representative sample size. The study design for the study is cross-sectional.

2) Please explain why you chose these five countries.

The five countries included in the study all fall under the East Africa region geographically and some have a coalition called East Africa community (Kenya, Uganda and Tanzania). These five countries also have conducted five or more rounds of DHS for trend analysis and comparability, and have relatively high level of adolescent pregnancy in the region.

3) P6. Line 51, in table 1, it is Malawi 2015, but the text is Malawi 2016, please check –

Thank you for picking this error – we have corrected this to Malawi 2015

4) Table1. Please adjust the format and the decimal point, and add the standard deviation to the mean

The table is formatted and standard deviation is added to mean values

5) P10.Line48. Moreover, the proportion of adolescent girls with secondary and above education doubled in Kenya and Zambia and almost tripled in Uganda, Tanzania and Malawi between 1990 and 2016. In addition, higher proportion of adolescents resided in urban areas during the later DHSs (Table 1). Secondary education in Tanzania is 6.4% → 35.1%, Malawi 4.5% → 27.2%. I think that it is three times or more.

This is noted, we have rephrased the statement to indicate the changes between the two times points (1989/1990 to 2017) but not quantatively due to the concern that confidence intervals are not shown.
Please check the fiscal year: 1990→1989.

Thank you for this observation. We have corrected this to 1989.

6) P11.Line25. 「A higher proportion of the adolescent pregnancies and births occurred among women in-union, ranging from 67% in Malawi and Uganda to 54% in Zambia (result not shown).」 →Please describe the year of this data.

This statement refers to data from their recent DHS: Kenya, 2014; Tanzania, 2015; Uganda, 2016; Malawi, 2015; Zambia, 2014(See the revised manuscript, page 11, line 231).

7) Figure 1 is hard to understand which country the chart shows.

We have tried to make the figure clearer by changing colors and increasing fonts for the key. The figure key placed below the figure has different colors for each country to help readers identify which country the chart shows. We hope to work with the production team to make this clearer for publication.

8) P13.Line17. In Zambia and Kenya, significant variation was observed in adolescent pregnancy and motherhood with the sex of the household head (Table 2).」 →Mistakes between Uganda and Kenya?

Thank you for identifying this error – we have corrected to Kenya and Uganda

9) P15.discussion. If you compare it in the events of 1990 and 2017, do not you need other results in figure1?

This comment is not very clear – our understanding is that you are referring to results for other years (no other indicators). This is based on availability of DHS data for these countries. DHS surveys are conducted at intervals of five years and we have used all DHS data available for the five countries during these periods (1989/90-2016).

Compared with 1990, pregnant women by age 19 are decreasing, but I thought why Tanzania and Malawi had an upward trend with the latest data.

Tanzania and Malawi have experienced an upward trend in teenage pregnancy during the latest surveys (In Tanzania it increased from 22.8% in 2010 to 26.7% (with non-overlapping confidence intervals), and in Malawi from 25.6% to 29.1% during the same period. This has
caused several policy level conversations and debates in the last few years. There are several reasons for the upward trend in adolescent pregnancy and motherhood in the two countries, but the poor attention given to adolescent friendly SRH information and services particularly in the hard to reach rural and remote areas of the two countries was identified an important factor. In Tanzania for instance, the disparity in adolescent pregnancy varies from 5% in Mijini in Zanzibar to 45% in Katavi in mainland Tanzania. The level of adolescent pregnancy and marriage in some of the districts and regions is very high. Similarly, there is a lack of school based comprehensive sex education in the two countries. In Tanzania for instance, the education policy does not support school re-entry for pregnant girls.

10) Table 1. Please describe FP.
What is the indicator of Knowledge of FP?
The abbreviation FP stands for family planning – but this has been changed to contraception
This indicator shows the percentage of adolescents who mentioned that they have heard of any contraceptive method asked in the DHS.

11) Table 3. Please check if the fiscal year is accurate. Is it difficult to describe not only% but the number of subjects?
We have checked and corrected the fiscal year. We have also added the number of subjects (denominators for the %)

12) Table 4. Please adjust the format.
Thank you for the observation. We have adjusted the format of the table

REVIEWER 2
Anna kubota (Reviewer 2): I think this article is significant outcomes in the field of pregnancy and childbirth. Please see my comments and respond. Thank you.

Methods:
(1) Please describe the sample size in the text.
A description of the sample size has been added to the manuscript (See Methods section, pp. 6, lines 125-129).

(2) When extracting this number of samples, did you have no exclusion criteria?

This paper is based on analysis of secondary data from the DHS, and as such we didn’t have any exclusion criteria for surveys in the selected countries. The only exclusion criteria applied was age, limited to adolescent’s age 15-19 in the DHS women individual recode data. No other exclusion criteria were applied within this age group. The current study focuses on a weighted sub-sample of adolescents age 15-19 in the most recent DHS for each of the five countries.

Table:

(3) Are the units in table 1 all %?

The units in table 1 are %, mean and median. For mean values, we have also added standard deviations

(4) Please add the each total number in table 2-4.

We have added the total number (n) for tables 2-4 including cluster numbers for table 4. Please see the revised tables.

(5) Please unify the uppercase and lowercase letters in table 2-4.

Thanks for the observation. We have unified the letters

(6) What is the RC of residence in table 4?

The RC for residence in table 4 is urban – this is indicated in table 4.

(7) Unify the Ref of "Comm." to "ref" in table 4.

Thank you for the observation, we have changed the reference category for the ‘Comm’ variables in table 4 to make similar ref categories.

(8) Are the fonts in table 4 all the same size?

Thank you for the observation. We have made the font size consistent on table 4.

REVIEWER 3
Ruth S Buzi (Reviewer 3): Please include all comments for the authors in this box rather than uploading your report as an attachment. Please only upload as attachments annotated versions of manuscripts, graphs, supporting materials or other aspects of your report which cannot be included in a text format.

This study utilizes data from five East African countries and examines factors affecting adolescent pregnancy and early motherhood. This is an important study in light of the high prevalence of early motherhood in these countries. The strength of the study is the use of a large data set from five countries and the examination of factors on many levels of influence. Better understanding the contextual factors that affect early sexual activity and pregnancy could guide prevention efforts. However, the study should clarify the following:

1. Title:

* The title of the study is long. I would like to suggest the authors consider shortening the title and only keeping the main aspects the study.

* The title and then later the paper discuss household factors. It appears the authors use a socio-ecological framework to guide their study. If this is the case, the authors should consider using the constructs addressed in this framework and replace household with interpersonal factors.

We have tried to shorten the title, keeping in the title both the subject as well as the general methodological approach. The list and factors used in this study followed the DHS approach although we used the ecological framework as a guide. This is a secondary analysis of DHS data and as such there are limitations in using the ecological framework – the survey does not collect data on many of the constructs in the framework.

2. Abstract:

* The authors refer to their methodology in the title as multi-level and then later as multi-sectoral. The authors should be consistent. I believe that the use of multi-level or socio-ecological methodology captures more effectively the constructs than multi-sectoral.

Apparently, the terms multi-level and multi-sectoral were used to refer to two different things. Multi-level refers to the analytical approach – the use of multi-level analysis methods. We used multi-level statistical analysis due to the hierarchical nature of the DHS data so that it captures covariance at the individual and cluster level. We used the term multi-sectoral to indicate the need for different types of interventions (education, wealth creation, media) required to reduce adolescent pregnancy and early motherhood in these countries.
* The size of the sample should be mentioned in the abstract in some way.

We have added the sample of adolescents used for this analysis.

3. Introduction:

* Can the authors provide a reference for their statement that few studies utilized multi-level approaches to studying teen pregnancy?

Thank you for the comment. We could not find this statement in our introduction. Please do mention the line number so it can be rectified. In our introduction, we mentioned that few studies examined the determinants of adolescent pregnancy and early motherhood in some of the countries where adolescent pregnancy is very high, such as those in Eastern Africa. But, given that there are very few studies on the determinants of adolescent pregnancy and early motherhood in these countries, it is possible to suggest that few studies utilized multi-level approaches to capture a combination of individual, household and community-level factors in Eastern Africa.

* The authors refer to multiple levels of influence but do not mention a theoretical framework they utilized to guide their study and the selection of variables. This should be included in the introduction as well as a rationale for such an approach.

On page 8, line 172-177, we have mentioned that previous research used the ecological framework of multidimensional factors at the individual, relational, familial and structural levels that influence adolescent pregnancy but we could not use this framework due to the limitations in our data. The data we used (a secondary data) is not collected to capture the constructs in the model comprehensively – and as a result we have focused on the influence of few constructs at the individual, household and community levels.

4. Methods:

* It is unclear what participants' age at the time of the survey was. Can the authors provide a mean age?

The age indicated in tables 2-4 all indicate respondent’s age at the time of the survey (ranging from 15-19).
The section of the explanatory variables is not well organized and difficult to follow. I recommend the authors use subheadings to identify the various variables they used. This could follow the theoretical framework.

The section on explanatory variables has been re-organized based on the hierarchy of the variables in an ecological perspective – from individual to household and community level variables. For ease of presentation and understanding we have bolded the variables groups (individual, household and community) instead of sub-headings (lines 159-189).

5. Results:

* When the authors organize the variables in the methods section, they should also follow the same structure in the results section. This will help with clarity.

* When the authors report results and CI, they should also include the P values.

Thank you for the comment: we have restructured the variables in the tables (mainly tables 3 and 4) in the following structure – individual, household and community levels.

However, P values are indicated – as asterisks (*) to show results that have significant associations with the outcome variable (when the p values are less than 0.05). Those without the asterisks (*) are associations that were not significant and indicate p values above 0.05. This is the most common format used in the BMC pregnancy and childbirth journal.

6. Discussion:

* The discussion is very long and repeats information already reported under results. This section should only highlight main findings and refer to the literature. Although this section does not require subheadings, referring to the main findings in the same order of the subheadings, will be helpful.

General comments: The authors should review the manuscript for grammatical inaccuracies. For example, the sentence on page 17 line 49, is incomplete.

Thank you for the comment. We have made the discussion section more concise highlighting main findings of the study and reducing repetition of information already reported under results section. We have also reviewed and edited the manuscript for grammatical errors (See the revised version of the section, pages 16-19.)