Author's response to reviews

Title: Prevalence and correlates of contraceptive use among female adolescents in Ghana

Authors:

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Version: 4 Date: 20 May 2015

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Reviewer #1: Katherine Tumlinson

Abstract

1. Lines 39-40: Please add a sentence or two with more concrete programmatic and policy implications/recommendations.

   Page 2: Lines 41-43. Reviewer’s comment has been noted and a sentence has been added to make the policy implications more concrete as recommended.

Introduction

1. Lines 52-53: Please provide a citation for this sentence.

   Page 2: Lines 48-50. The sentence has been modified and citation provided as recommended.

2. Line 52: I believe “pregnancy prevention” is part of “fertility regulation” and therefore it may be redundant to refer to both. I recommend using one term or the other but not both.

   Page 2: Lines 48-50. the sentence has been modified.

3. Line 56: It’s not necessary to say both “more evident” and “particularly”. You could say “These constraints are particularly evident in SSA…” or “These constraints are more evident in SSA…”

   Page 3. Lines 53, 54: sentence has been revised as suggested by the reviewer.

4. Lines 54-56: What are the “constraints” that prevent us from meeting the sexual and reproductive health needs of adolescents? Please be specific.

   Page 2. Lines 51, 52: some specific constraints have been cited as recommended.

5. Lines 56-57: What proportion of female adolescents in SSA has begun childbearing? You say it is the greatest (relative to the rest of the world?) but don’t state what it is. Additionally, you cite four articles to support this statement but they were all written 12-14 years ago. It is likely that the proportion of adolescents who has begun childbearing in
SSA has changed in the last decade. Please provide current statistics and corresponding citations.

Page 3. Lines 53, 54: the sentence has been recast and more current citations provided as recommended

6. Lines 64-65: Please cite this sentence.

Page 3. Line 63: These line has been cited as recommended.

7. Lines 75-77: The use of the term “manifold” suggests there are numerous (more than 3) studies that support your statement that FP reduces maternal, child, and infant mortality.
   Furthermore, the article by Kathryn Kost is not relevant to your statement (it refers to negative birth outcomes in the US such as low birth weight, not infant mortality in SSA).
   Please remove the Kost citation and add other citations. Look for Lancet articles by John Cleland as one article you could cite here, but please include others as well.

Page 3. Lines 73, 75: Manifold has been replaced with a more appropriate word and Kathryn Kost has been replaced with Cleland as recommended.


Page 4. Line 77: the recommended correction has been made.

9. Line 84: Please insert the words “sexually active” in front of “female adolescents” – this helps to distinguish your study from the previous similar study.

Dear editor, this is a secondary data in which women in the survey including female adolescents were selected based on probability sampling techniques and
therefore all may not necessarily be sexually active. So I do not think adding “sexually active” is appropriate. Also, to the best of my knowledge, no similar work has been done on Ghana for now.

Methods

1. Line 102: State the size of the sample of women ages 15 to 19 (n=1037).

Page 4. Line 98: the size of the sample of women ages 15 to 19 (n=1037) has been stated as recommended.

2. Lines 104-105: I’m confused by the coding of the outcome variable. You state that women using contraception were coded as ‘0’ and those not using were coded as ‘1’. Would it make more sense to code those NOT using any contraceptive method as ‘0’ and those who DO use as ‘1’? Actually, when I look at the results in Table 2, I’m certain you used the coding scheme I recommend, because it doesn’t make any sense that having higher education would make you more likely to NOT use any method of contraception. Therefore you don’t need to recode your outcome variable, you just need to correct the text on line 104 and 105.

Page 4. Lines 100, 101: the text has been corrected as recommended by Reviewer #3

3. Lines 105-109: Some of the variables need better explanation either in the methods section or in the body of Tables 1 and 2. For example, does “work” only refer to paid work? Which religious groups comprise “Other Christian”? How do you define “poor”, “average”, and “rich” – this is really important; did you combine certain wealth quintiles together? Be very explicit about this.
Page 5. Lines 102-108: the recommended variables were explained in the methods section. However, the exact definition of poor, average and rich is the sole responsibility of the DHS program and includes some country-specific criteria that cannot be included in this paper but can be referred from the DHS website.

4. Discretionary Revision: Lines 112-115: I think you can just say “logistic regression” rather than “binary logistic regression” as multinomial logistic regression is fairly uncommon and we know that your outcome is binary.

   Page 5. Lines 111: “binary logistic regression” has been replaced with “logistic regression” as recommended.

5. Lines 110-117: I’m concerned by your decision to use bivariate analysis to present your regression results. Please explain to your readers/audience why you decided to use bivariate rather than multivariate analysis. Please also explain why you are not concerned that your significant bivariate results could be potentially confounded by other variables. This is a major concern that must be addressed to the satisfaction of all reviewers and editors.

   Dear editor, I did NOT use bivariate analysis to present the regression results. In lines 109-112, I stated categorically that bivariate analysis was used to present prevalence of contraceptive use in the form of proportions while logistic regression analysis was done to examine the correlates of female adolescent contraceptive use and was therefore rather used to present the regression results. The regression results were actually multivariate analysis in the form of logistic regression.

6. Please also explain how you came up with your list of select variables.
The list of select variables was determined by running a series of bivariate analyses with chi square test between a number of independent variables in the dataset and the dependent variable and only those variables which had significant association with contraceptive use were included in the list.

7. Discretionary Revision: Did you consider including variables that measure media exposure or discussion of family planning with a partner or friend?

Media exposure and discussion of family planning with a partner or friend were initially considered; but had to be subsequently excluded because they had no significant association with contraceptive use. Also, information on them in the dataset was “insignificant”.

8. Discretionary Revision: I think it would be very interesting to see if your results change when looking at just modern or just traditional use. Can you stratify by these two categories of method type?

Yes it may be interesting. But this will be more suitable when considering all the women aged 15 to 49 in the dataset. However, extracting women age 15 to 19 from this dataset may generate “insignificant” figures or no responses and that was one of the reasons why both modern and traditional methods were combined. Perhaps, my further study has to consider that among all women aged 15 to 49.

Results

1. Please start by stating the overall contraceptive prevalence in the overall sample of 1037 women. By my calculation, it is approximately 18 percent but this needs to be stated. Please also break down the overall contraceptive prevalence by modern and traditional methods. Ideally, I would like to see a pie graph that presents the method mix for all
currently available methods so that I can consider what percent of participants are using long acting or permanent methods, etc.

Page 5. Lines 120-122: The overall contraceptive prevalence had been stated. However, the currently available methods in the dataset are too many and insignificant to be presented with pie graph.

2. Presentation of Table 1: There are several things to address here. First, you need to change the title of your table to be more descriptive. If I were to stumble upon your table outside of the context of this paper, I need to be able to interpret it perfectly. Currently this is not possible. I would suggest something like this for the title of the Table: “Prevalence of contraceptive use among a sample of 1,037 women in Ghana ages 15 to 19, by select characteristics. 2008.” Secondly, you don’t need to list the Frequency column. We can easily determine the frequency if we know the percent and the total sample size. Also, frequency is not really interesting compared with percent. Next, change “percent” to “percent distribution” so that the reader understands this is the percent of participants in each of these categories. Last, change “proportion” to “percent currently using contraception”. I would also change “Variables” to “Characteristic”. Spell out GDHS.

Page 15. Lines 347, 348: Title of Table 1 has been revised as recommended. However, even though we can determine the frequency if we know the percent and the total sample size, I also feel that not everybody has the luxury of time to compute the frequency if they want to know. Therefore, I think it is better to maintain it than to remove it. The “percent” and “proportion” have been revised
as recommended. GDHS was already spelt out in the methods section in pages 88, 89.

3. Discretionary Revision: Line 169: Remove the word “binary”.

See page 164: the word “binary” has been removed.

4. Lines 169-171: In the methods section you state that you are using bivariate analysis but in the first paragraph of the regression results you state that “all the selected independent variables were included in the model.” This is really confusing. Please clarify the following: a. When you stated this, do you mean that all the selected variables were entered into the same model? If this is the case, then you used multivariate (not bivariate) analysis because you have multiple independent variables in the same model. If this is the case, you need to state clearly throughout the paper that you are using multivariate analysis and at the bottom of Table 2 you should list the covariates included in the model. b. Or do you mean that all the independent variables were entered into separate bivariate models (in which case you have many models, each with only two variables)? c. Either way – whether you used bivariate or multivariate analysis – the type of analysis you used needs to be stated very clearly so that the readers/audience have a very clear understanding of what you have done to arrive at your results. You also need to justify your decision if you used bivariate analysis and discuss the limitations of this type of data analysis in the discussion section. d. Also, if you used multivariate analysis but removed those variables that were not significantly associated with your outcome from your one model, I’m not confident this is a correct model-building approach. Please justify this approach.
Dear editor, these issues had been already addressed in comment 5, lines 109-112 under the methods section. Also, the non-significant variables have been added.

5. Overall: Let’s talk about the presentation of odds ratios. There is a common misinterpretation when dealing with ratio measures of effect. In the following two examples, the wording does not mean the same thing. a. The 5-year risk of ovarian cancer among women who have ever used oral contraceptives is 1.65 times the 5-year risk of ovarian cancer among women who have never used oral contraceptives. b. The 5-year risk of ovarian cancer among women who have ever used oral contraceptives is 1.65 times higher than the 5-year risk of ovarian cancer among women who have never used oral contraceptives. c. The reason these two statements do not mean the same thing is because the use of the words “times higher than” implies that the RR=2.65. d. Therefore, in lines 177 through 186, you need to change your language. Here is an example: “In effect, the odds of contraceptive use among females ages 18 to 19 were 3.49 times the odds of contraceptive use among women ages 15 to 17 years of age.”

Pages 7, 8. Lines 169-182: the language has been revised as recommended.

6. Table 2 title, please consider something like this: “Table 2: Correlation between select characteristics and current contraceptive use among a sample of 1,037 women ages 15 to 19 years in Ghana. 2008.”

Page 17. Line 350. Table 2 title: this has been addressed as recommended by Reviewer 2 in comment 12.

Discussion

1. Overall – please be careful when using cross-sectional data not to over-reach the meaning of your results. You are only able to demonstrate a correlation or association, not
causality. Therefore you have to think about the direction of possible cause and effect since temporality cannot be established.

This advice is well noted

2. Lines 192-198: The fact that women 18-19 are so much more likely to use FP compared to women 15-17 could be due to the fact that they are more likely to be married, working, and have obtained more education than is possible at the earlier ages. It could also be the case that they have greater need because they could be more likely to be sexually active. Have you excluded women who are not sexually active and, if not, you need to justify this decision early in the article.

Lines 192-198: the recommendations have been incorporated. However, the nature of DHS dataset is such that it captures women in general irrespective of sexual activeness due to its use of probability sampling techniques.

3. Lines 208-216: There must be other studies that demonstrate the correlation between work status and contraceptive use other than the one Bangladesh study (Khan). It would be a good idea to try to find additional studies from SSA to cite in addition to Khan.

Page 9. Line 206: Other studies from sub-Saharan Africa such as Nketiah-Amponsah et al and MacPhail et al. had been added to Khan as recommended, even though Nketiah-Amponsah was already cited.

4. Lines 217-223: This could be a case of reverse causality. It could be that women who use contraception (particularly those who use traditional methods) have a greater likelihood of knowing their ovulation cycle rather than the other way around.

Page 9. Lines 218, 219: This recommendation has been noted and incorporated into the work.
5. Line 224: Insert the word “association” after “significant”.

   Page 9. Line 221: the word “association” has been inserted after “significant” as recommended.

6. Lines 224-234: This could also be a case of reverse causality. Those women who are using or want to use FP are more likely to have visited a health facility compared with women who are not using a method.

   Page 10. Lines 229-232: This recommendation has been noted and incorporated into the work.

Conclusion

1. Line 253: You would want to target those ages “17 and below” not “15 and below”.

   Page 10. Line 249: the recommended correction has been noted and effected.

2. Please say more about how programs and policies can target these groups – i.e. how can they be reached? You need to have more concrete recommendations.

   Pages 10, 11. Lines 249-258: This recommendation has been noted and revised accordingly.

3. I’m not convinced that increasing the number of female adolescents visiting a health facility will increase contraceptive prevalence. If they are not interested in using FP and/or if they are not sexually active, the visit is not relevant to them and will not lead to more FP use.

   Page 11. Lines 257, 258: This recommendation has been noted and revised accordingly.

Reviewer #2: Wendy Heywood

Major compulsory revisions
1. My main comment has to do with the structure of the results section. The article would flow better if table 1 and the description of table 1 only reported on participant characteristics (frequency and percent). Furthermore, the prevalence data should be moved to table 2 as this is what the logistic regression examined (i.e. if the prevalence of contraceptive use was different according to a variety of characteristics). At the moment the information is essentially presented twice. For example, the first paragraph of the results states “The prevalence of contraceptive use was higher among female adolescents aged 18 to 19 (31.4%) than female adolescents aged 15 to 17 (9.2%).” While the first paragraph under the section Correlates of female adolescent contraceptive use states “the odds of contraceptive use were 3.49 times higher for female adolescents aged 18 to 19 compared to female adolescents aged 15 to 17.”

While the reviewer thinks the article will flow better if participants characteristics are presented alone and the contraceptive prevalence moved into the logistic regression model, I also feel that this is not necessarily appropriate. This is because the contraceptive prevalence only occupies a single column and can suitably be placed alongside the frequencies to save creating a full table for it. Also, even though logistic regression is generated from proportions or prevalence, the prevalence is quite different from the odds ratios generated by logistic regression though they may sometimes go the same direction. Therefore, the information they generate is not always the same and not presented twice as thought by the reviewer. I have a number of publications using similar presentation and reviewers had not actually complained about that. The
advantages here will be to reduce the number of tables used in the article, save some space and prevent overcrowding the logistic regression model.

2. Table 2. The confidence intervals for level of education are extremely wide. This is due to small cell size in the reference category (Only 3.5% of women with no education (n=87) reported contraceptive use. By my calculations only about three women in this category reported contraceptive use.) Please consider 1) combining no education and primary education categories or 2) changing the reference category and not placing too much importance on the findings in relation to no education as you don’t quite have enough data to produce a stable estimate.

   While it is true that the confidence intervals for level of education are extremely wide, it could also be due to the removal of the insignificant variables from the model. Therefore, the analysis was done again and these variables were therefore added to normalise the confidence interval margin. Also, I feel that neither combining “no education” and “primary” education nor changing the reference category may not resolve the situation. This is because women who had no education and primary education may have quite different behaviour toward contraceptive use while making secondary education a reference category will render the odds ratios of the other categories “insignificant” due to the degree of the odd ratios.

3. The manuscript would also benefit from editing and proof reading. Although the language, in general, does not prevent the reader from understanding the content, certain phrases are used in the wrong context, there are a few minor spelling mistakes/errors (e.g. second paragraph of the results there is an s before highest), the use of tense is not always
consistent (e.g. in the method) and there are some places that would benefit from tightening or removal of unnecessary words (e.g. phrases such as Besides or In effect can be removed from the beginning of sentences).

The manuscript has been edited and proofread to remove spelling mistakes and unnecessary words as recommended.

Minor essential revision

Methods

4. Please include the number of women aged 15-19 years in the final sample.

Page 4. Line 98. The number of women aged 15-19 years has been included in the final sample as recommended.

5. After the sentence “The dependent (outcome) variable for the study is current contraceptive use including both traditional and modern methods” Please include a list of the specific types of contraception measured in this study.

Dear editor, the specific dependent variable used for the study had been already categorised “traditional” and “modern methods” in the original dataset. Therefore, a list of all the specific types of contraception measured in this study could not be provided. Also, even though a similar variable in the dataset listed all the specific types, the list was so copious that it could not be provided in this article.

6. In the final sentence of the method please change “STATA” to “Stata” and include a citation. Please see http://www.stata.com/support/faqs/resources/citing-software-documentation-faqs/

Page 5. Line 109: This is well noted. “STATA” has been changed to “Stata” and a citation included as recommended.
**Results**

7. At the start of the results please include the overall prevalence of contraceptive use in the sample of adolescent women aged 15 to 19. And if possible include a comment about the most common type of contraceptive used in the sample.

   Page 5. Lines 120-122: This has been noted and addressed as recommended.

8. Can you please specify if the sample consisted of all women aged 15 to 19 years or only sexually active women aged 15 to 19 years? If all women are included my guess is that those who have not had sex were coded as not using contraception?

   The secondary data used was based on a survey that used a probability sampling techniques in selecting participants. Therefore, the sample consisted of all women aged 15 to 19 years and not only sexually active women. Concerning coding of the variables, this was originally done by the owner of the data (DHS program) and not the author.

9. In the second sentence of the results please change aged 18 to 18 to aged 18 to 19.

   Page 5. Line 122. Thanks for the correction. This has been well noted and changed as recommended.

**Conclusions**

10. A sentence in the conclusion states “Consequently, pragmatic adolescent and reproductive health programmes can target younger female adolescents aged 15 and below…” This study however, examined women aged 15 to 19 years. Perhaps rephrase to match what the study found.

   Page 10. Lines 249, 250: This has been noted and addressed as recommended.
References

11. Reference 19. Please bold the title of this article to be consistent with other journal article references.

   Reference 19: This has been noted and bolded as recommended.

Tables

12. Table 2. Consider renaming table “Logistic regression of correlates of female adolescents contraceptive use.”

   Page 17. Line 350: Table 2 has been renamed as recommended

13. Table 2. Consider adding a table note such as “Adjusted for all other variables in the model”.

   Table 2: Dear editor, I strongly believe that adding “Adjusted for all other variables in the model” will not make any significant changes to the findings and will rather go a step further to overcrowd the logistic regression model.

14. Table 2. Please remove “Level of education” from the bottom of the table.

   Table 2: Thanks for the correction. Level of education has been removed from the bottom of the table as recommended.

Discretionary revisions

15. Table 2. Consider rounding 95% Conf. Interval to 2 decimal places to be consistent with the odds ratio.

   Pages 17, 18. Lines 350, 351. Table 2: Rounding of the confidence interval to 2 places has been noted and applied as recommended.

16. Table 2. Consider removing the p-value column as this information is already conveyed with the asterisks next to the odds ratios.
Dear editor, even though it is true that the asterisks next to the odds ratios already conveys the information in the p-value column, I strongly believe that some readers may be more interested in the specific degree of the p-values of the significant variables, and therefore has to be maintained.

Reviewer #3: Takudzwa Sayi

Major compulsory revision

1. The author can make the case for adolescents stronger by highlighting how they compare with women at other ages. Are these issues they face unique?

   Dear editor, even though these issues female adolescents may not be unique, this paper examines only “prevalence” and “correlates” among female adolescents and not a comparative study as insinuated by the reviewer. It is just a case where there is little knowledge on this in the Ghanaian context. What the reviewer is recommending is just a different area that can be considered for future studies.

2. Prior to the description of methods and results, the author needs to provide some sense of what we can expect to find, some context – a section explaining the contraceptive situation in Ghana. Does Ghana stand out from other countries in Africa? Have there been changes over time in the prevalence among adolescents? Do we expect the determinants to be changing as well over time? Has anything been done by the Ghanaian government to deal with adolescent issues. Just placing the conversation in context.

   Dear editor, information on contraceptive situation in Ghana in terms of female adolescents is lacking. Even though Ghana does not empirically standout from other countries in Africa, information on prevalence and determinants among
female adolescents is lacking, which is the rationale for the study and not a trend studies. Lack of literature makes it difficult to provide information on whether the determinants will be changing over time and therefore information on this section could not be provided. Of course the Ghanaian government has developed the adolescent reproductive health policy to deal with adolescent issues; but it does not deal directly with contraceptive use among female adolescents. It is therefore hoped that more of these types of studies in the Ghanaian context could provide more information on these issues.

3. This paper is descriptive. As a policymaker wishing to address this issue, one might want to have a better sense of the context, as explained above. The recommendations made in the final section may also make more sense if we knew, beforehand, that young women had no source of knowledge about menstruation….do Ghana schools teach this; or are women usually educated by female relatives?

Dear editor, it is well known in Ghana that young women’s sources of knowledge about menstruation are their mothers and/or their female guardians; but this is not done adequately or effectively. So, all that is needed is for mothers/guardians to intensify the education and this has been specified in the conclusion in lines 253 and 254, page 11.

4. In addition, what do we know about wanted/unwanted fertility, discontinuation, unmet need, sexual frequency…in this group of young women. Sexual frequency may be a big issue at these ages. Which brings the question, were adolescents not sexually active excluded from the study or kept in?
Dear editor, this issue has been addressed as recommended by Reviewer #2 in comment 8.

5. More citations in the introduction section for some of the statements being made. For example, the statement on how adolescent contraceptive use is experimental.

Dear editor, this issue has been addressed as recommended by Reviewer #1 in comment 1 (Introduction).

6. The logistic regression is appropriate for this analysis. But there are some issues to address in the Methods section. (i) Methods were not described very well. For instance, how did the author deal with clustering? Inclusion of an equation for the fitted model will make it easier to understand the modeling process. (ii) The writing up of the methods could be better. Examples include.. Line 104: could just indicate that the dichotomy was between users and nonusers, but not necessarily indicating the ‘0’ and ‘1’. Line 110: ‘The data were analyzed in Stata 11’ sounds better than the passive voice used here and some other parts of the paper (e.g. line 169). (iii) How were the variables used in the model selected? (iv) There was no mention throughout the paper, of how community-level variables can affect these patterns, and why they are not included in the models? (v) What of examining relative odds of using different methods in the multivariate models? Even the descriptives do not tell us which methods are popular among all women and in different groups of these women.

- Page 5. Lines 114, 15. Clustering was dealt with by sample weighting and this has been included in the last sentence of the methods. The study is not about structural equation modelling as thought by the reviewer so there was no equation for the fitted model. It is only about multivariate analysis.
ii. Page 4. Line 100 and 101: Revisions have been made as recommended

iii. Dear editor, this has already been addressed as recommended by Reviewer #1 in comment 6.

iv. Dear editor, there were community level variables such as type of residence and region of residence in the model; only that they had no significant relationship with contraceptive use and for this reason, they were not discussed further.

v. Dear editor, the nature of the data does not allow for examining relative odds of using different methods in the multivariate models as suggested by the reviewer. Furthermore, methods which were popular among all women have been included in the first paragraph of the results.

7. There are too many percentages in the text of the Results section (the descriptives). Since they are also in tables, there is no need to describe all of them in the text. Otherwise it makes the paragraphs very dense and hard to follow.

   Lines 119-161: Some of the percentages in the text of the results section have been deleted as recommended.

8. The description of results would be easier to follow if we knew beforehand what to expect for the effects of each factor. In the methods section the author can describe the expected direction of effects as well as the evidence to support that.

   Dear editor, even though the results would be easier to follow if we knew beforehand what to expect for the effects of each factor, I strongly believe that it will lead to the duplication of findings in the article which may make the reading of the article boring and unenjoyable and for that reason could not be included.
9. Table 2 includes only significant results. The author should include all results and indicate statistical significance. On line 349, the author indicates that the source is the DHS… This should be removed because the results are from their own computations.

   Pages 17-18. Lines 350-351. All results and statistical significance have been included in Table 2 as recommended. On line 351, the DHS source has been rectified to “Computed from 2008 GDHS”.

10. Some of the confidence intervals in Table 2 (e.g. for secondary/higher) are very wide. The author needs to talk about why this is the case.

   Pages 17-18. Lines 350-351. Some of the confidence intervals in Table 2 that are very wide have been rectified after performing the analyses again including the non-significant variables as recommended above.

11. Line 204-207: “This may result from the fact that educated women are more likely to be abreast of available contraceptives and are more likely to appreciate the positive impacts contraceptives have on their lives.” Could it also be due to accessibility, affordability, etc? In general, the discussion reasoning (and speculations) behind the significance of different factors needs to be better placed in the larger pool of other sub-Saharan studies.

   Dear editor, I do not think accessibility or affordability may have a role to play in level of education influencing contraceptive use in this context. Accessibility or affordability could determine contraceptive use for any female adolescent and not solely for educated female adolescents. In this context, the only known sub-Saharan studies are what had been discussed by Tawiah and Nketiah-Amponsah et al. concerning education and contraceptive use.
12. In the Abstract background section, the author mentions that prevalence among adolescents is low, and then that the paper examines prevalence among adolescents. This part needs to be re-written so that it becomes clear what the gap is in existing knowledge.

Page 2. Lines 30, 31. The background section in the abstract has been revised as recommended.

Minor revision


Page 2. Line 33 in the abstract: ‘secondary’ has been deleted as recommended.

2. The term ‘binary logistic’ is mentioned a lot in the paper. I would just call it ‘logistic’.

The term ‘binary logistic’ has been changed into ‘logistic’ as recommended. See lines 31 and 111.

3. Line 66… knowledge of what in particular?

Page 3. Line 65: the knowledge in particular has not been stated by the original article, as against what has been recommended by the reviewer.

4. Line 101 …‘from the full birth history’… could just be ‘were extracted from the dataset’.

Page 4. Line 98: 'From the full birth history' has been revised as recommended.

5. Line 107: “Some of these study variables were re-coded to suit the purpose of the study while some were used as they are in the original dataset.” Which ones?

Page 5. Line 105-108: This has been addressed as recommended.


Page 5. Lines 109 and 110: this has been revised as recommended.

7. Line 123…. 18 to 18?

Page 5. Line 122: Correction has been effected.
8. Sentence beginning on line 169 to 171 can be deleted.

   Page 7. Sentence on line 164 and 165 has been deleted as recommended.

9. Phrase “three folds” appears many times in the manuscript. “three times” sounds better.

   Lines 187 and 204. The phrase “three folds” has been revised into “three times” as recommended.

10. Table 1 caption. ‘Prevalence of contraceptive use by selected variables’. In addition, it is
    not obvious what the difference is between the columns ‘Percent’ and ‘Proportion’ in Table 1.

   Page 15. Line 348: The difference between the columns ‘Percent’ and ‘Proportion’ in Table 1 has been resolved as recommended.

11. Line 115 is repetitive; this was already mentioned on line 103.

   Page 5. Line 114: this has been removed as recommended.

12. Line 132 misspelling ‘shighest’.

   Page 6. Line 132: misspelling has been corrected as recommended.

**Reviewer #4: Kazuyo Machiyama**

The minor point

- Contraceptive prevalence of modern and traditional methods combined do not give us much information. It may be more interesting to assess the prevalence by type of methods, rather than estimating overall CPR, or scrutinize use of condom use which is often the most commonly used method among adolescents in sub-Saharan Africa.

   Dear editor, contrary to the view of the reviewer, contraceptive prevalence of modern and traditional methods combined gives much information in its context.
A number of studies such as Nketiah-Amponsah et al, Oheneba-Sakyi, and Tawiah proved this. Also, even though it may be interesting to assess the prevalence by type of methods, this can also be done in a different study depending on the nature or the flexibility of the dataset. The same applies to the assessment of condom use as recommended by the reviewer. Honestly, this study was done within the confines of the nature of the dataset.

• It would be helpful if you conduct chi-square test or present crude odds ratios in Table 1 to present associations between CPR and the determinants.

  Dear editor, contrary to the view of the reviewer, I strongly believe that it would not be helpful to conduct chi-square test or present crude odds ratios in Table 1 to present associations between CPR and the determinants. This is because the results will duplicate or contrast the results in the multivariate analysis in Table 2 and this will run the findings or purpose of the article into total confusion.

• The column on CPR Table 1 can be more clearly labelled.

  Page 15. Line 348. The column on CPR in Table 1 has been revised as recommended.

• The author can clearly mention that you carried out multivariate analysis in the method section and Table 2.

  Lines 111 and 350: Logistic regression analysis was clearly mentioned in the method section and Table 2 as recommended.