Author's response to reviews

Title: Predictors of mistimed, and unwanted pregnancies among women of childbearing age in Rufiji, Kilombero, and Ulanga districts of Tanzania

Authors:

Amon Exavery (aexavery@ihi.or.tz)
Almamy M Kanté (amk2213@columbia.edu)
Mustafa Njozi (Mustafa.Njozi@jhpiego.org)
Kassimu Tani (ktani@ihi.or.tz)
Henry V Doctor (hvdoctor@gmail.com)
Ahmed Hingora (ahingora@ihi.or.tz)
James F Phillips (jfp2113@cumc.columbia.edu)

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Author's response to reviews: see over
Attn: Dr. Jose M. Belizan  
Editor-in-Chief  
Reproductive Health Journal  

Dear Sir,

Submission of a revised manuscript (MS: 9005256391139850) – “Predictors of mistimed, and unwanted pregnancies among women of childbearing age in Rufiji, Kilombero, and Ulanga districts of Tanzania”

On behalf of my co-authors, I am pleased to submit the revised manuscript based on comments received on 27 May 2014 from two reviewers. The concerns raised by the reviewers have been addressed and a description of how we responded to the issues is presented in the subsequent pages.

We are very grateful to the reviewers for their time and intellectual evaluation of the manuscript. Their comments have greatly improved the quality of the manuscript and we hope that you will find the current version of the manuscript suitable for publication.

Sincerely yours,

Amon Exavery  
For the authors
Responses to reviewers’ comments

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| Ilene Speizer | **Reviewer's report:**  
Major compulsory revisions:  
1) The final multivariate model included in the paper does not feel complete. In particular, place of residence (urban/rural) which was significant in the bivariate results is not in the model; it is, however discussed in the discussion.  
2) I expect that the variables gravidity and number of children under 5 in the household are somewhat correlated. The authors explain their reasons for including this variable but the level of correlation should be presented and if it is high, perhaps just gravidity should be included in the model.  
3) The approach to creating the wealth variable should be described in more detail.  
4) Writing issues: The authors need to review the paper closely for grammatical errors. The order of the presentation and the tables should follow one another to help the reader follow. Tighten the discussion and avoid too much repetition to what is discussed in the results section. |  
1) We have included place of residence (urban/rural) in the multivariate model, even though it is not significant. Its discussion has been deleted in the Discussion section.  
2) True - therefore we included gravidity in the multivariate model and removed the number of children under 5 in the household because the former had higher predictive power than the latter.  
3) We have included the details. This included describing the principal component analysis (PCA) and the variables involved.  
4) The manuscript was critically revised as per the suggestions. |
|          | **Minor essential revisions:**  
1) Abstract – the conclusions of the abstract do not relate to the results of the paper. These could be written without the analyses presented.  
2) Page 4, end of first paragraph – the authors state “Further documentation of serious consequences of unintended pregnancy exist in other settings.” It is not clear what the authors are referring to, other as compared to what? The |  
|          | **Minor essential revisions**  
The conclusion was revised to make sure that it wholly relates to the findings.  
2) We have clarified further that this refers to low- and middle-income countries. |
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<td>paragraph is about low- and middle income countries, I think.</td>
<td>3) The primary survey used a household as a sampling unit. Therefore, all eligible women in each household sampled were interviewed if they consented. Age cut-off was 15 to 49, but women over 49 were also interviewed only if they were total caretakers of at least one child under 5 in order to obtain information for that child. We have clarified that only women were surveyed and they were asked about their children under 5. The spelling error on ‘patterns’ was corrected.</td>
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<td>3) Data – the survey was among women aged 15 and above – does that mean that ALL women in the household were surveyed or was there some age cut-off? Also, “and children aged less than five years” infers that the children were surveyed but in fact, I expect that women were asked about their children under 5. Make this presentation clearer (and note mis-spelling of “patterns” and MDG written out is capitalized).</td>
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<td>4) The authors drop education and wealth (because they were not significant in the bivariate analysis), however, these are important control variables that should be included in the final model.</td>
<td>4) In reanalyzing the data, education and wealth quintile were included in the multivariate model as important control factors as advised. The results however changed negligibly as expected.</td>
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<td>5) In the first paragraph of the description of the multivariate results, you might want to tell the reader that the model controls for marital status. Given the important distinctions by age group, it is good to know that these are controlling for whether a woman is single, ever married, or currently married.</td>
<td>5) In actual fact, the variables in the final model are controlled for one another, including marital status. We have stated this in the revised manuscript.</td>
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<td>6) The effect of religion paragraph needs to be checked as it seems that Muslim was significant for unwanted pregnancies.</td>
<td>6) Yes, the risk of unwanted pregnancy was significantly lower for Muslim compared to Christian. We had no clear understanding why this was the case. We therefore proposed further research.</td>
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<td>7) In the model results, the authors put stars on values that have a p-value of less than 0.10 but then they do not discuss these in the text. I suggest not</td>
<td>7) We accepted the advice – effects with p-values &lt;0.10 are no longer marked in the revised manuscript. They are also not mentioned in the discussion.</td>
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<td>marking these in the table if they do not intend to discuss. In addition, in the write-up the authors mention effects that were not statistically significant which may be misleading.</td>
<td>8) The conclusion was revised in accordance with the advice given. Also the paragraph about Connect Project was removed.</td>
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<td>8) Conclusions – the conclusions provided are somewhat disconnected. Yes, the findings point to the need for inter-spousal communication but the authors say “point to the need for programs to enhance availability of, access to, and inter-spousal communication about FP services.” I am not even sure what this means and specific examples need to be given on how you would promote inter-spousal communication. Moreover, the authors should blend the last paragraph about the Connect Project to better fit the study findings and the recommendations. Otherwise, it feels like it is just tagged onto the end of the paper.</td>
<td>9) Table 1 intended to present only background characteristics of the respondents, not everything as there were no inferences one could make using this table. Place of residence, education, and wealth quintile were added in multivariate analysis which produced Table 3, but as already noted, the results changed subtly following this addition.</td>
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| 9) Tables: Table 1 is missing number of children under 5 in the household, interspousal communication, and contraceptive use. As mentioned above, Table 3 at least needs place of residence but should also include education and wealth. | **Discretionary revisions**
1) At the end of data paragraph, it would be good to include the number of women who meet the study criteria. |
| **Discretionary revisions**
1) This has been included. | **Quality of written English:** Needs some language corrections before being published. |
<p>| <strong>Quality of written English:</strong> A native English speaker was found and proofread the manuscript. She is acknowledged in the Acknowledgements section of the revised manuscript. |</p>
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| Lawrence Ikamari | **Reviewer's report:** This is generally a good article. It is well written and conveys clearly the key findings. I recommend it be accepted for publication subject to the following compulsory corrections:  
1) Providing a brief summary of the survey methodology particularly about how households were selected and the number of eligible women selected in each household. This will help the reader understand the discrepancy between the number of households (2183) selected (line 32) and the number of women (2,199) aged 15-49 (line 40).  
2) In line 161 to clarify whether all the children under five in the household belonged to the women included in the analysis otherwise this variable will not be a good proxy for birth interval. Fostered children or grand children and other children less than five in the household that did not belong to the women in question should be removed from this analysis.  
3) To clarify the meaning of the statement in line 169 that ‘this variable was sufficient enough to be included in the analysis of pregnancy intentions’.  
4) To provide a justification for the inclusion of the contraceptive use status variable in the analysis. This is a current status variable (as at the time of the survey). It does not refer to the time the pregnancy in question occurred. Some women may have initiated contraceptive use only after experiencing an unintended pregnancy! | 1) More clarification has been provided. Households were selected at random, and all eligible women in each household sampled were interviewed (only if they granted consent).  
2) This variable has been removed in the analysis. The other reviewer also proposed that we remove it, but on different grounds. For the parent study, all children under five in a household were considered even if they were not from one woman in question. Our analysis focused on gravid women between 15-49 years inclusive. This is clear in the Methods section of the manuscript.  
3) This statement was deleted and the remaining text was revised.  
4) No, the “contraceptive use status” variable captures all time situations. The variable has three categories: currently using, ever used, and never used. Therefore, if some women were using contraception only after experiencing an unintended pregnancy and continued using until at the time of the survey, they were classified into the ‘currently using’ group. And also ever used but currently not using in the ‘ever used’ group and never used if she reported that she has never used |
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<td>Additional material submitted by the reviewers</td>
<td>As pointed by one reviewer the text still needs better English edition. I am suggesting you to see if you can take the service of Edanz <a href="http://www.edanzediting.com/">http://www.edanzediting.com/</a> We are having a good experience about the quality of the English edition of this initiative. I also consider that the Discussion section should be better organized and reduced. Please try to follow this structure in the Discussion section: * statement of principal findings of the study. Summarise key results with reference to study objectives * strengths and weaknesses of the study * strengths and weaknesses in relation to other studies, discussing important differences in results and what your study adds. Whenever possible please discuss your study in the light of relevant systematic reviews and meta-analyses (eg Cochrane reviews) * meaning of the study: possible explanations and implications for clinicians and policymakers and other researchers; how your study could promote better decisions * unanswered questions and future research</td>
<td>A native English speaker was sought and proofread the manuscript. The proposed Edanz service was not used for some fiscally-related reasons. The Discussion section was reorganized as advised in order to make it better. The guideline given was followed where possible. Statements and conclusions were also kept within the results of the study. Anything beyond or unrelated to the findings was removed.</td>
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