Author's response to reviews

Title: Community-level influences on women's experience of intimate partner violence and terminated pregnancy in Nigeria: A multilevel analysis.

Authors:

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Author's response to reviews: see over
Reviewer 1: Mika Gissler

Major Compulsory Revisions
1. Abstract: The first sentence of results does not have sense. I assume that the comparison group is women without terminated pregnancy, not women who have not experienced intimate partner violence.

Authors’ response
The first sentence in the Abstract “Women who had terminated a pregnancy were more likely to have experienced physical IPV, sexual IPV, and any IPV compared to women who had not experienced these IPV types” is correct!
The outcome in this study is Terminated pregnancy i.e. the “yes” of the dichotomous variable; and the exposure is IPV (different types of IPV). Thus, we are comparing the likelihood of terminated pregnancy among women exposed to IPV compared to women not exposed to IPV.

2. In page 5, the authors refer to dimensions of relationship inequalities. It is described later what relative earning, relative education and relative age are, but here it may cause confusion for the readers, if not re-phrased.

Authors’ response
Though the authors’ do not completely agree with the reviewer’s comments, the phrase “Dimensions of relationship inequality” has been changed to “measures of relationship inequality”, as the reviewer requested.

3. The sample is from a Demographic and Health Survey, and the analyzed data includes 19 226 women aged 15-49 year, currently or previously married or cohabiting. The flow chart in Figure 2 describes something else; since it refers to fully immunised children and women who never have given birth has been excluded.

Authors’ response
This has been corrected as the reviewer requested (see figure 2).

4. Include tables in the main document and not as additional files.

Authors’ response

Minor Essential Revisions
1. Page 13: 11th line from bottom: Add full stop after 'during pregnancy'.
This has been done as the reviewer requested.

2. Page 15: 2nd line from top: to report physical IPV without comma.

Authors’ response
This has been done as the reviewer requested.

3. Please correct 'unwanted and terminated pregnancies', 'individual and community-level factors' and 'physical and sexual IPV' without a hyphen after the first word.

Authors’ response
This has been done as the reviewer requested.

Reviewer 1: Miriam Taegtmeyer

Minor essential revisions:
1. The manuscript is well written and there are very few typos or corrections to the material from an editorial perspective. Page 16 paragraph 3 ‘women’s low level of justification’ – was this intended to read women’s level of justification? I have no other comments under the headings minor essential revisions.

Authors’ response
The paragraph was “The association between women’s low level of justification of wife beating within the community, physical IPV, “any IPV”, and terminated pregnancy reflects community-level norms and attitudes that accept or are indifferent to IPV and gender inequality; this is consistent with prior findings [28, 29], [53] [74]”.

This statement is correct, given that community-level factors were assessed in levels i.e. low, median, and high. “Low” here refers to the wife beating at a low level for the community.

<table>
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<tr>
<th>Community justify wife beating</th>
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<tr>
<td>Low</td>
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<td>Median</td>
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2. The main hypothesis underlying the paper is that aggregated data on the educational level, contraceptive use, mean age at first marriage and justifying wife beating provide a proxy for community attitudes to violence. However the objectives are broader than this and include understanding the relationship between IPV exposure and pregnancy outcome as well as between controlling behaviours and termination in association with IPV. Although the paper is confined to analysis of data from female respondents it makes limited exploration of women’s experiences
and it might therefore be better to change the title “Community-level determinants of intimate partner violence and terminated pregnancy in Nigeria”.

The mixture of objectives means that the questions posed by the authors are less clearly defined than they could be. The primary and secondary objectives could come out more clearly just before the methods.

**Authors’ response**

- The study is guided by the social ecological theory, which facilitates understanding of the influence of factors at multiple levels (individual, dyad, family, community, and societal) on such harmful behavioral outcomes as IPV.

- On the basis of the social ecological theory, it is expected that IPV is a result of factors acting on multiple levels. As such, our 3 hypotheses are formulated to reflect the multiple levels on which these predisposing factors act i.e. i) that IPV-exposed women are more likely to terminate a pregnancy than women not exposed to IPV (Individual level); ii) that women exposed to forms of control or coercive behaviours by their partner have a higher likelihood of terminating a pregnancy in association with IPV (dyad/family level); and iii) that women resident in empowered communities are less likely to experience IPV or report terminating a pregnancy (community level).

- So the reviewer’s statement that “The main hypothesis underlying the paper is that aggregated data on the educational level, contraceptive use, mean age at first marriage and justifying wife beating provide a proxy for community attitudes to violence” is incorrect!.

- We beg to disagree with the reviewer’s statement that “Although the paper is confined to analysis of data from female respondents it makes limited exploration of women’s experiences”. By including variables at the different levels (individual and community), we have in fact explored the women’s experiences as much as the available data permits, and therefore disagree with a suggestion to change the title!

3. The paper describes a review of secondary data, rather than primary research and the premise underlying the paper that community-level influences are critical to IPV and pregnancy outcome is the least well developed aspect of the analysis. In particular the influence of religion and ethnicity on cultural aspects is not controlled for, yet is key to age at marriage and contraceptive use among other aspects. At times there is confusion between what is seen as a community level influence and what are behaviours resulting from the individual relationships that women find themselves in (and how these are linked).
The discussion is the main area of the paper that could be strengthened. In terms of structure it would really benefit from some subheadings to make it easier to follow the logical argument and how this is built up. Recommendations for potential interventions could be grouped together rather appearing separately as they do at present.

Authors’ response

- We do not understand what the reviewer means by “The paper describes a review of secondary data, rather than primary research”. There seems to be some misunderstanding on the part of the reviewer. The literature review in this manuscript is based on past and current studies. This is an original/primary research carried out on secondary data, which is perfectly in order given that not all studies in the world are or can be done on primary data.

- We disagree with the reviewer’s statement that “the premise underlying the paper that community-level influences are critical to IPV and pregnancy outcome is the least well developed aspect of the analysis”, after having examined four (4) community-level factors that reflect the social contexts in Nigeria (i.e. norms/beliefs, illiteracy) were examined using four indicators: i) community mean education; ii) community attitudes justifying wife beating; iii) community median age of first marriage; and iv) community contraceptive use.

- Factors/variables considered or discussed at the community level are clearly stated e.g. community-level education means education at the community level (as is operationalized as Community mean education). This can in no way be confused with the individual-level Women’s education (which is categorized as no education, primary school education, and secondary school or higher education). We therefore disagree with the reviewer!

4. A number of the findings conflict with the existing literature and also within the data analysis internally and this significance of this is not adequately discussed analytically. For example the issue of control and poor access to contraception leading to higher rates of termination is unclear and the findings are mixed. Are the authors postulating that women are deliberately getting pregnant to avoid IPV and then being forced to terminate or that controlling behaviour means that their fertility is controlled by men who want them to get pregnant and then they terminate?

Authors’ response

- Once again, there seems to be some misunderstanding on the part of the reviewer. The factors on different levels are conceptually linked or associated with the likelihood of IPV occurrence, which in turn increases the women’s predisposition to terminate a pregnancy.
For example, Physical IPV was significantly associated with the higher likelihood of women exposed to controlling behavior by their husband/partner (OR= 3.25; 95% CI= 2.62 – 4.02) compared to women not exposed to controlling behavior by husband/partner.

- The authors are therefore in no way postulating that “women are deliberately getting pregnant to avoid IPV and then being forced to terminate or that controlling behaviour means that their fertility is controlled by men who want them to get pregnant and then they terminate”; the findings are clear and hence not mixed, as explained here.

5. The limitations section is clear and well written. The authors mention alcohol as a confounder and this could have been discussed in more detail as it has been shown to be a critical determinant in a number of other settings.

Authors’ response
The authors chose not to lay too much emphasis on alcohol consumption as in this setting.

6. HIV is also linked to vulnerability and since DHS data are available on knowledge of status is could have been taken into account in the analysis.

Authors’ response
The authors did not take knowledge of HIV status into account in this study because we did not consider it very relevant to the association/interplay between IPV and the likelihood of terminated pregnancy.