Author’s response to reviews

Title: Women Decision-making Capacity and Intimate Partner Violence among Women in Sub-Saharan Africa

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Author’s response to reviews:

Reviewer reports:

Editor:

1. Please do not over-interpret your results. All statements must be supported by results

The work has been revised to ensure that results are not over-interpreted and all statements are backed by results

2. Please report exhaustively all limitations of this study

A section on data limitation had been added and the limitations discussed

3. Please discuss the representativeness of your sample.

The authors have discussed the representativeness of the sample under the discussion section by saying that, the use of national representative surveys (DHS) and the use of stratified two stage sampling technique made it possible to obtain samples that are highly representative of the target populations. The use of a large sample size and the national representative nature of the data make conclusions from our study valid.
Reviewer #1: This is a generally well written manuscript on an important topic. The strength of the article is the large data used from different countries in Sub-Saharan Africa. However the manuscript has also important weaknesses that are not addressed in a limitation section.

A section on data limitation had been added and the limitations discussed

The most important weakness is that main outcome measure of intimate partner violence (IPV) only comprises emotions and sexual IPV, while physical IPV as one of the most prevalent forms of IPV are not assessed. Why? And how does this impact the IPV prevalence reported?

Analysis has been redone to include physical IPV

Another weakness of the measure is the fact that only lifetime IPV is assessed, but not IPV in the last 12 months. This makes comparisons to other existing data very problematic, as the prevalence largely depends on the age composition of the sample. This problem is not addressed in the manuscript.

The authors have added the non-use of IPV in the last 12 months as part of the limitations of the study.

Also connected to this the logistic regression analyses certainly need to be adjusted for age, which does not seem to be the case.

Based on the influence of age on lifetime prevalence of IPV, the logistic regression analyses has been redone to adjust for age

The analysis also miss to address the cluster sampling (women nested in countries), which should be taken into account since the prevalence of IPV largely differs by country. I suggest to use either a multilevel model with country as random effect or to include country as a dummy variable into the models.
Country variable has been included in the models

In addition, I do not think that the variables on decision making capacity used in the study can be used as a measure of individual empowerment. Although they are directly linked to empowerment, empowerment is a broader concept and should be measured with the available empowerment scales. I recommend to call the index variable "decision making capacity" and only discuss a link to women empowerment.

The empowerment variable has been changed to decision making capacity in the revised manuscript.

Other comments:

Introduction:

In the introduction authors should be named e.g. in line 30.

Some authors have been renamed in the introduction. Example, in line 15, 30, 45, 64, 66, 67 and 68.

Methods

The terms "Binary and multivariate logistic regression models" are used in a wrong manner. Binary means only that the outcome is binary, therefore the right term is univariate.

The term binary has been changed to univariate under the methods section.

I am confused to read about "reproductive decision making index, decision making on condom use and decision making of sexual intercourse" in the methods, although the variable is not mentioned in any of the analyses.

They have been revised and removed in the revised manuscript
In the statistical analysis the way of adjustment needs to be explained and the above mentioned adjustment for age and country needs to be done.

Analysis has been redone to adjust for age and country. This was done by including them in the final model with all the other independent variables. The statistical analysis provides explanation on how the adjustment for age and country was done.

Results

The tables would be easier to read, if the "no" answers would be left out.

This has been addressed by removing ‘no’ answers from Table 1.

On page 8, line 36-40 results of univariate logistic regression analyses are reported with referring to Table 3, although Table 3 does not show these results. I recommend to state the corresponding odds ratios in the text.

In the revised manuscript, the authors have made it clear that Table 3 shows the results of the univariate and multivariate analyses.

The sentence page 8, line 40-41 is incomplete.

This has been addressed in the revised manuscript.

Discussion

The discussion is lacking a limitations section, where sampling and other limitations (see above) are discussed.
Limitation section added

The words "women" and "Sub-Saharan Africa" need a consistent spelling throughout the manuscript.

This has been addressed in the revised manuscript.

Page 10, the comparison with Garcia Moreno does not seem to be adequate, because only sexual IPV was considered.

The use of the findings of Garcia-Moreno et al. to support the findings of the study has been removed.

Page 10, Line 43-49: Are there any explanation for country differences? Policies and legislations?

In the revised manuscript, the authors have explained that one of the reasons for the differences in prevalence of women empowerment in different countries in Sub-Saharan Africa could be due to differences in policies and legislations.

Page 11, line 46-50: Check grammar of this sentence. Also, you should not comment on physical violence from your findings, because you have studied only emotional and sexual IPV

The grammar has been revised in the new manuscript. However, the authors have maintained their comment on physical violence since it has been added to the revised manuscript.

Page 12, line 15-18: I do not understand this highly problematic conclusion ("It can therefore be assumed restriction the number of women to formal education in certain cultures could reduce the prevalence of IPV"), since higher education is protective in your and many other’s studies.

The grammar has been revised in the new manuscript.
Due to your findings "Our data suggest that women who were empowered were more likely to experience sexual or emotional violence compared to those who were not empowered" there is a need to explain better, why you rightly suggest "The findings thus clearly indicate the need to governments in Africa to Invest more in girl child education and empowerment programs to help empower more women." How can the risk of IPV for more empowered women and women in employment be overcome?

All these have been addressed in the revised manuscript.