Author’s response to reviews

Title: Trends in prevalence and correlates of intimate partner violence against women in Zimbabwe, 2005-2015

Authors:

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Author’s response to reviews:

Response to reviewer # 1

Comment 1:
Response: Thank you for the comments and suggestions. As indicated in the paper, the objective of this study was to examine the relationships between socioeconomic factors and IPV in Zimbabwe, over time. And to explore the trends in prevalence and risk factors associated with IPV against women in Zimbabwe, which hitherto was overlooked and not examined. Indeed, other macro factors may influence IPV, but they may be beyond the scope of this current study. As pointed out by the reviewer, there may be a link between IPV and alcohol consumption that may be worth exploring. However, in our previous study (see, Mukora-Mutseyekwa, Fadzai, et al. "Trends in Prevalence and Related Risk Factors of Overweight and Obesity among Women of Reproductive Age in Zimbabwe, 2005–2015." International journal of environmental research and public health 16.15 (2019): 2758 ), we observed that only about 3% of women in Zimbabwe drink alcohol. Thus, alcohol abuse may not have any significant impact on the magnitude and direction of our findings. Regarding the legal framework, we have now included additional information. (see, page 4, line 15-21).

Response to reviewer # 2

Comment 1: In the abstract It will be Important to note which type of IPV were assessed.
Response: Thanks for the suggestion. The type of intimate partner violence that were assessed were physical, sexual or emotional violence. We have now included them on Page 1, line 11-12.

Comment 2: It is unclear whether or not a trend analysis was conducted to see if changes in prevalence seen were statistically significant or not.

Response: We have examined the differences in IPV rates between the three survey years using chi-square test. We added a statement on page 7, line 13-14 and a note under Table 1 to this effect.

Background

Comment 1: The first sentence could be broken into two sentences one stating a definition of IPV and one stating global prevalence.

Response: Thanks for the suggestion, we have addressed it (page 2, line 3-4).

Comment 2: In the introduction the authors could also discuss Sustainable Development Goal 5 target 2 which aims to end all forms of violence against women by 2030.

Response: Thanks for the suggestion. We have discussed and incorporated the aims of SDG goal 5 target 2 into our introduction (page 2, line 22-23).

Comment 3: Page 3 line 38- the authors use the term ignorant which I would consider re-framing to 'unaware' as the term ignorant has a negative connotation and places blame on women.

Response: As suggested we have reframed the term from ‘ignorant’ to ‘unaware’ (page 3, line 17).
Comment 4: On page 3 the authors end the second in "this further contributes toward their exposure". This sentence needs further explanation or should be tied into the next paragraph discussing the link between education and vulnerability to IPV.

Response: Thanks for the suggestion. We have tied the sentence into the paragraph that was above it (page 3, line 17-18).

Comment 5: In the introduction after the authors begin to discuss the case of Zimbabwe they mention that IPV rates are 'very high', this should be quantified. If rates are around 35%, in reality this is not much higher than the global prevalence of ~30%.

Response: We have corrected our claim to ‘high’ (page 4, line 7).

Comment 6: The authors second objective of the research around media exposure is not fully justified in the introduction. I would suggest either removing it from this analysis or incorporating prior research and research gaps around the topic of media exposure and IPV in both global and context specific studies. This second research question is also not mentioned anywhere in the abstract.

Response: Thank you for the suggestion. We have justified our second research objective around the media exposure and incorporated research gaps around the study on media exposure and IPV (Page 5, line 4-5). The second research question was not mentioned in the abstract because we believe that the media is also a risk factor of IPV (See, page 1 line 3).

Methods

Comment 1: It is likely that the question around IPV was limited to only asking women who are cohabitating or married, however this limits the ability to assess any experiences of IPV that women in intimate relationships not living with or married to their partners. In some contexts where marriage is not common (e.g. South Africa) this is a major limitation. It would be helpful to know a bit more about marriage patterns in Zimbabwe and I would also suggest that the authors add a statement about this limitation in the discussion.
Response: In Zimbabwe marriage is common, as communities strongly disapprove other forms of intimate relationships. There are two kinds of marriage that are socially recognized: customary and civil marriage. We also regarded our delimitation as a limitation which suggests further research (page 15, line 7-9).

Comment 2: In the description of the outcome variable the authors mention that responses were dichotomized, could the authors indicate what the response options were prior to dichotomizing the score?

Response: Thank you. We have now changed the word “dichotomized” to “grouped” to make it easier for the reader (page 6, line 13).

Comment 3: In the description for the independent variable (which should be listed as independent variables) they describe that a wealth index was used as a covariate. Is there a reference for where this index came from? What was the criteria for determining the different levels of wealth?

Response: As suggested, we have now provided further details about the wealth index on (page 7, line 4-9)

Comment 4: There are some instances where the authors mention the inclusion to be cohabitating and married and others where they mention cohabitating or married. Please review manuscript for consistency.

Response: We have now corrected it to cohabiting or married. For example, see page 5, line 23 and page 15 line 7.

Comment 5: In the methods section it is a bit unclear how the data was combined for analysis. Please provide additional details in the statistical analyses process for how data was pooled across time periods in order to conduct the analysis. Also, I believe that the year of the data collection should be controlled for in the multivariate analyses.
Response: Thanks for the comment. We used three different DHS datasets for the analysis, and this was mentioned in the method section. Regarding the analysis, we combined all three datasets (2005/2006, 2010/2011 and 2015) which we called “pooled”. We now included additional information on page 7, line 15, to this effect. We also believe that controlling for survey years may not be relevant for our research objectives because we have discussed IPV prevalence by survey years.

Results

Comment 1: Your results state that IPV increased marginally however in the abstract you mention that IPV is rising. I feel that a statistical test should be conducted in order to confirm this statement?

Response: We have examined the differences in IPV rates between the three survey years using a statistical test (chi-square test). We added a statement on page 7, line 13-14 and a note under Table 1 to this effect.

Comment 2: The authors mention a couple areas where they discuss trends (e.g. pg. 9 line 13-"The trend analysis showed..."), however there is no description in the statistical methods section detailing how trend analyses were conducted.

Response: Thank you. We have now included additional information on how the trend analysis was conducted (page 7, line 14-15)

Comment 3: Regarding the third model conducted in the study, the authors need to include additional details in the introduction regarding the justification for conducting this specific model.

Response: Thanks for the suggestion. We have now included additional detailed in the introduction (Page 5, line 5).
Discussion

Comment 1: The authors discuss that younger women had rates of IPV because "they are likely to engage in aggressive and violent behaviours." I think a discussion around increased vulnerability among younger aged women who experience violence in married and when cohabitating. It is likely that these young women face increased vulnerability as they may be less likely to be pursuing educational opportunities and are likely more dependent on their partners than women of older age who may have delayed marriage or cohabitating with partners. This could be tied into the discussion around children and poverty, where women who are more dependent and have children with their abusers are less able to leave violent partners.

Response: We did discuss the patterns of the prevalence of IPV and child increase, challenges faced by women with children when they want to leave their abusive husbands (page 12, line 12-15). We also included some discussions around early marriage and IPV (page 12, line 7-11).

Comment 2: The authors discuss the finding around number of children and IPV. There has been numerous studies looking at IPV and pregnancy. I would suggest that the authors discuss their findings in relation to existing literature on IPV and pregnancy.

Response: Thanks for the suggestion. We have added some information on IPV and pregnancy in the discussion (page 12, line 15-17).

Comment 3: Moreover, findings regarding higher IPV in rural areas due to higher levels of traditionalist could be supplemented by discussions around different forms of femininities and how different constructions of femininities influence experiences of violence and HIV-risk.

Response: Thanks for the suggestion. We have included discussions around forms of femininities and how they influence IPV (page 12, line 22-25).

Comment 4: Pg.12 second paragraph the authors discuss how men who may feel threatened by their female partners’ empowerment may exercise increased risk for IPV. There are a number of papers among men in South Africa showing an association between the gender role strain scale and perpetration of IPV. The authors could consider discussing this evidence alongside their results.

Response: Thanks for the recommendation, we have added discussions on how gendered systems enforce male dominance and women subordination and IPV (page 14, line 8-13).
Comment 5: Often in national survey data, data is analyzed using survey weights was this done in this analysis?

Response: All analysis were weighted to adjust for the DHS sampling design. We have now included a statement on (page 8, line 3-4).

Comment 6: The authors should cite the WHO's regulations and guidelines for collection of data related to domestic violence.

Response: We have now cited the WHO’s regulations and guidelines for collecting data related to domestic violence (page15, line 11-13).

Comment 7: In the conclusion, the authors state that women of reproductive age are at high and increasing risk of IPV. Without a statistical trend test showing a significant increase in IPV over the time periods, I don't think that the results can warrant concluding that there is an increasing risk. The authors are also not assessing IPV incidence.

Response: Thank you. We have now included additional information on the statistical test that was performed regarding IPV trends over time (see, page 7 line 13-14 and a note under Table 1).

References

Comment 1: In the reference list there are at least two instances where the same reference is listed (16 and 17) the authors should double check to ensure that there are no duplicated references and that references are formatted to the Journal's requirements.

Response: Thanks, we have corrected our references.