Author’s response to reviews

Title: Does exposure to interparental violence increase women's risk of intimate partner violence? Evidence from Nigeria Demographic and Health Survey

Authors:

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Author’s response to reviews:

The Editor,

BMC International Health and Human Rights,

Response to Reviewers Comments

Introduction

Thank you for the privilege. The reviewer comments are not numbered, however, I separated the comments based on the issue(s) raised and responded to each of them. Twelve (12) citations have been added in line with reviewer’s suggestion. My responses to specific comments are outlined below:

General Comment

This paper examines the association between exposure to interparental violence and involvement in intimate partner violence (IPV) in a sample of women from rural regions in Nigeria. Strengths of the study include the ability to examine these patterns in a large sample from Africa, a well written introduction, the inclusion of key information about the societal and cultural context, and inclusion of several covariates. The discussion is also very well-written.

Response

Satisfactory
Comment

Despite these strengths, I have numerous concerns that focus mainly on wondering why the authors did not differentiate types of intimate partner violence but instead lump them all together (I see this as a major weakness and hindering substantial contribution to the literature).

Response

Types of intimate partner violence are differentiated at the univariate level (Table 1) and in cross tabulation with interparental violence (Table 2). However, types of intimate partner violence were combined at the bivariate and multivariate levels. Though, this is now acknowledged as a limitation (Line 488-496), two reasons accounted for combining the types of violence in the study. One, in some previous studies (Okenwa et al, 2009[34]; Antai & Adaji, 2012[36]), all types of violence were also combined as ‘Any IPV’ or ‘Any abuse’ and analysed separately. Two, by combining types of violence, the sample of women who experienced at least one type of intimate partner violence was relatively larger permitting more valid analysis and conclusion regarding association between interparental violence and IPV than are possible with specific type of violence.

Comment

I also feel the authors have not adequately reviewed the extant literature on prospective research that already links exposure to interparental violence and involvement in IPV.

Response

Additional literature included (Line 127-131)

Comment

It's not clear what this study is adding, beyond that it was conducted in rural, under-developed country in Africa with a large sample. The authors need to more clearly justify the ability of the study to make a novel contribution to the existing literature.
Response

Additional contributions included (Line 131-133, 140-142).

Comment

In the results section of the abstract it's not clear what the differences are between models 1-3. I would suggest either clarifying this or taking out this particular section from the results. This level of detail is not necessarily needed in an abstract.

Response

Clarification made (Line 68-69). Redundant aspect deleted in the background.

Comment

The first paragraph of introductions states that although IPV has been reported against men it is universally evident that they main perpetrators are men and women are the key victims of IPV. This statement is not accurate. There are several studies (perhaps particularly from the US), which suggest that particularly in community samples, IPV tends to be reciprocal, with both men and women serving in the perpetrator and the victim role (e.g., Ehrensaft et al., 2003; Fergusson et al., 2008; Langer et al., 2008; Magdol et al, 1997). It is indeed true that more severe forms of violence such as physical and sexual battery that involves assault and injury tend to be more perpetrated by men. However, it is not the case that women are always the victims. There are several studies to suggest that women also perpetrate violence sometimes at equal rates as men. I suggest the authors reword the above statement and also include more balanced citations. Alternatively, they could make the case that SEVERE violence is more likely to be perpetrated by men, with appropriate citations that have found this.

Response

Sentence reworded with additional citations (Line 101-103)
Comment

On page 4, I also find that the others are missing a lot of citations for studies that have previously found support for the intergenerational transmission of violence, particularly from witnessing interparental violence to being involved in intimate partner violence. Several prospective studies have already found these patterns (e.g., Ehrensaft et al., 2003; Fite et al., 2008; Narayan et al., 2013, 2014). The authors need to more thoroughly review the literature before making their claims. Furthermore, as noted with the citations, several studies have already documented prospective associations, however, the present study is not prospective. Therefore, the authors need to justify what the present study adds to the literature. Obviously, it is very interesting that it is done in a country such as Nigeria; this is a major strength. The authors need to elaborate on this.

Response

Additional literature on intergenerational transmission of violence included (Line 127-131)

Comment

The Intro starts out with a nice definition of intimate partner violence, however, it then lumps together many different forms of intimate partner violence, such as physical and psychological and sexual. There is a wealth of literature that already suggested pathways to each of these types of IPV are different. The authors need to include the citations and review the literature more thoroughly on this topic. They also need to clearly justify why they have collapsed all three types of IPV into one outcome variable. With such a large sample here, it would make more sense to me, and it would more strongly contribute to the literature, if they were able to keep these types of IPV differentiated as separate outcomes.

Response

Reasons for collapsing the three types of partner violence provided (Line 232-236)
Comment

The Introduction is lacking hypotheses.

Response

Study hypothesis included (Line 160-161)

Comment

Relatedly there are also a lot of covariates included in the models described in the Method, many of which would be theoretically expected to be associated with IPV, such as attitudes towards aggression, whether the partner drinks alcohol, etc. These constructs need to be incorporated into the Introduction with justification based on past empirical work, about why these variables are included.

Response

With every sense of humility, the suggestion that these constructs be included in the introduction is a matter of style. It was stated in the ‘explanatory and control variables’ sub-section of the Methods that these construct were selected for analysis because previous studies have established their association with partner violence (Line 248-249).

Comment

The part on Context in the first part of the Method section should be incorporated into the Introduction. This section provides key information on the context that the participants live in, and the sociopolitical climate. This is very interesting information; however, it should be in the Introduction. The Method section should start with the section on Data Source and Sample Design.

Response

Suggestion effected (Line 164-191)
Comment

It's unclear on page 7 how exposure to interparental violence was measured (e.g., how many questions/items were used, what the age range of exposure included, etc.).

Response

This is now discussed in the limitations (Line 481-487). The range of exposure was not provided in the DHS data.

Comment

It would be helpful to have information on who administered the questions about exposure and IPV. Were the research staff who administered the questionnaires culturally matched with the participants? Do the authors feel that the participants were provided a safe and confidential environment to answer these personal questions? I wonder whether there were reporter biases operating here. Are there any cultural stigmas associated with acknowledging violence in the home that may have contributed to under-reporting of exposure or IPV?

Response

Additional information provided (Line 198-203). Possibility of underreporting discussed under limitations (Line 475-481)

Comment

Several major limitations exist, which are not acknowledged in the Discussion section. First, the authors have only relied on a few items to access the key constructs. It seems as though exposure to violence was only assessed by one item, however this is unclear. Second, it is unclear whether validated measurements of these constructs were used - and were they found to be valid/reliable for use with Nigerian women. Third, both the independent and dependent variables were drawn from the same reporter. This increases the possibility of a reporter biases. Furthermore, the authors need to explain whether or not they think that women were likely to under-report rates of exposure to violence and involvement in IPV because of social or cultural stigmas. Finally, I see
the inability to differentiate the different types of IPV (physical, psychological, sexual) as a major limitation that needs to be acknowledged, and I also see the focus exclusively on male-to-female violence (both in terms of exposure - what the women saw - and involvement in IPV) as a big limitation.

Response

Section reworded. Limitations now grouped into three, namely, non-inclusion of qualitative data; nature of data analysed; and method of data analysis. The revised sections provided most additional information requested by the reviewer.

Editorial Policies

The declarations are provided in the submitted manuscript.

Thank you.

Dr. Bola Lukman Solanke