Author's response to reviews

Title: Intimate Partner Violence Against Women in Maputo City, Mozambique

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Author's response to reviews: see over
Dear Editor,

We have finally managed to revise the manuscript. On the whole, we feel that the reviewers made excellent points, and we have done some changes (in track changes). In other cases, we provide answers. These concerns are clearly stated in the manuscript, and we have been now provided explanation about it. If there are more concerns, we will be pleased to address them.

Yours sincerely,

Antonio Eugenio Zacarias

Reviewer 1 - Koustuv Dalal

Reviewer's report:
1. Major compulsory revision. Authors failed to focus in background, in some part of methods and results... At its current form, the study has enough scope to be improved, concised and focused.
   Author's response: A major revision of the manuscript has been done as the reviewer requested.

2. Reference style should be in according to journal guideline.
   Author's response: This has now been corrected as the reviewer requested.

3. Quality of written English:
   Author's response: This has now been carried out as the reviewer requested.

4. Abstract
   Abstract: Objective is not representing the aims/objective stated in the study. Method is not properly explaining the same what was done in the study. Conclusion is not exclusively representing the study.
   Author's response: This has been rewritten as suggested.

5. Manuscript
   Introduction
   Introduction should be concised and focused. Rationale of study should be constituted more precisely.
a) Rationale of study should be constituted more precisely. Objective should be stated once and in a focused way. In page 6 the study aims readers may have problem to focus on the study objective.

Author's response:
The rationale for the study and objects has been revised, and is now concise.

b) Authors have referred only one study on IPV in Mozambique (Ref 23). However there are other studies also. For example, “Characteristic of victim of family violence seeking care at health centers in Maputo, Mozambique”, J Emerg Trauma Shock, 2011 Jul-Sep; 4(3): 369-375.

Author's response:
This has now been corrected as the reviewer requested. In addition, the publication the reviewer suggested (i.e. Jetá EA, Lynch CA, Houry DE, Rodrigues MA, Chilundo B, Sasser SM, Wright DW: Characteristic of victims of family violence seeking care at health centers in Maputo, Mozambique. J Emerg Trauma Shock 2011, 4:369-73) has been incorporated in the manuscript, as the reviewer requested.

6. Methods
a) What is meant by consecutive cases?

Author's response:
“Consecutive case” means that all women (victims of IPV by a male partner, aged from 15 to 49 years old, resident in Maputo city) that entered in the Forensic Services at Maputo Central Hospital between 1st April, 2007 and 31st March 2008 were included in the study.

b) Of this sample, 1442 participated in the study and 58 refused (response rate, 96.1%). In this study, the number of women answering questions about violence in some outcomes amounted to 1432 and 1433 respectively (see Table 1). What is meaning of last sentence?

Author's response:
This indicates that “Of the 1433 women in this study, 1432 of them, the number of women did not answer answering some questions about violence in relation to some outcomes”.

c) Table 1 the title is confusing and not self-explanatory.

Author's response:
The title has been revised as the reviewer suggested.

d) Authors should clearly mention the dependent variables in the analyses. Inclusion of certain variables, such as BMI I should be justified. What do authors mean by endsm eat?

Author's response:
a) This has now been carried out as the reviewer requested.
b) The rationale for using these variables are that they are relevant in Mozambique and usually found in literature about IPV.

c) “… concerns about how to make ends meet” refers to how the women were able to economically provide for their needs.

d) Authors can use separate sub-headings to describe variables under four blocks used in multiple block-wise logistic regressions. Statistical analysis should describe exact statistical methods… … .

Author’s response:
This has been done as the reviewer suggested.

e) Please clarify: Page 9: In regressions, the independent factors included were variables significantly related to each IPV types…. …

Author’s response:
This has been rewritten and clarified as the reviewer suggested.

7) Results

a) Authors have used same table in methods and results. Please correct it.

Author’s response:
This has been corrected as the reviewer suggested.

b) Presentation of results should be in sequence. The authors went back and forth to describe tables. It is not friendly. What the necessity to run multivariate analysis with all independent variables, esp. when we know from table 2, the bivariate significant relationships of certain variables?

Answer:
Author’s response:
This has been rewritten and clarified as the reviewer suggested.

8) Discussion

What is the take-away finding/s of the study?

Author’s response:
This has been written and clarified as the reviewer suggested. Please see strengths and limitations section.

9) Conclusion should be based exclusively on the study itself. Page 25 “we are facing a large group of women with extensive experiences of abuse in time, type, chronicity and severity”. Isn’t it contradictory to the limitation? The study has participants only from a city hospital and authors are stating “large group of women”.

Author’s response:
This has been rewritten and clarified as the reviewer suggested.
Reviewer 2 – Rob Stephenson

1) It is important to establish upfront how the authors are conceptualizing IPV.
Author’s response:
This has been written and as the reviewer suggested. Please see conceptual framework in background section.

2) The background and introduction sections are incredibly repetitive (… … ) It is never clear what the exact purpose of the paper is: if the purpose is just to establish the prevalence of IPV in Mozambique and examine associations with standard social and demographic factors, then this does not seem especially innovative and unlikely to add to the literature (and could this be done easily with the DHS?).
Author’s response:

a) – We have rewritten and clarified the manuscript in order to delete repetitions as the reviewer suggested.

b) – One needs different kinds of studies, and our study is indeed urgently needed. For instance, there is a remarkable lack of information concerning IPV, any type of data in Mozambique. Information from police stations, NGOs and Forensic Services indicates that a lot of women are heavily abused in Mozambique. Confident data on this may be helpful to assist abused women.

c) – We would like to emphasize that our manuscript is not only about the relation between demographic/socio-economic and lifestyle variables and IPV but, it is broader.

3) The authors make claim that variations in prevalence estimates may be due to variations in IPV definitions – but how can they be sure that their definition is correct? Has the CTC been adapted for Mozambique – to what extent is it culturally valid?
Author’s response:

a) – We are sorry, we do not understand what the reviewer means because we have used the definition from CDC 2002 (www.cdc.gov) and Saltman et al., 2002.

b) – The CTS has been used in a very large number of countries, cultures, population and languages, including Portuguese. It has been used in Sub-Saharan Africa. The DHS used in many of the Sub-Saharan countries the modified versions of CTS. See also WHO, 2002, 2005 on violence.

4) The results section is long and unwieldy. It is often repetitive: the authors need to do a better job of simplifying the results, and in drawing better comparisons between factors associated with each of outcomes.
Author’s response:
This has been rewritten and shortened as the reviewer suggested.
5) There is no conceptual model or theory telling us why these variables should be significantly associated with the outcomes.

Author's response:
   a) Please see answer 1 about conceptual framework in the background section.

   b) Unfortunately, we do not agree because the reviewer seems to be asking for another study. We appropriately explained in the statistical analysis where in block-wise logistic regression variables are entered into the regression equation block by block. Please see more explanation in statistical analysis section.

6) The major problem with this paper is the sample. The sample is women who have contacted Forensic Services for their experiences, so of course there is going to be a high prevalence of IPV among this group. This is only really addressed in one sentence in the limitations; this fact should be front and center of the paper. The paper needs to be totally reformatted and presented of women seeking care for IPV. Also there is a huge selection bias as to what types of women are able to seek care for IPV. Then serious limitations negate any sense of generalizability of the results.

Author's response:
   a) We already have clarified these concerns. Please answer on these issues provided to the reviewer.

   b) We do mention some limitations, but the focus of this study is not about limitations as shown for instance in the manuscript.

   c) We are aware that our sample is 0.35% of women aged 15-49 years old in Maputo city. However, if the reviewer would care to do a sample size calculation based on the women aged (in for instance 15-49 years old in 10 years age group) he would find that the sample would not be much higher than ours.

   d) We agree with the reviewer, but we have clearly stated that our findings cannot be generalized this is implicit in the limitations section. However, in many ways our findings are consistent with data from different sources.

7) There is almost no policy or public health recommendations arising from the research; we need to know, what do we do to solve the problem?

Author's response:
   This has been written and as the reviewer suggested.