Reviewer's report

Title: Domestic violence against women in Eastern Sudan.

Version: 2 Date: 22 August 2014

Reviewer: Lotta Nybergh

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Estimating the prevalence rate of domestic violence among women in Sudan is a very important study objective and the manuscript has some potential. However, several suggestions are provided that might enhance the study prior to publication:

MAJOR COMPULSORY REVISIONS

1. The methods-section in the abstract should preferably give information about the statistical methods that were used in this study. Furthermore, as the study is of a cross-sectional design, causality between domestic violence and the associated factors cannot be determined: this should be reflected in the wording of the abstract.

2. The conclusion is very broad and general, and should be amended to better reflect the present study’s main findings.

3. Where did the violence items stem from, did the authors use a validated questionnaire or did they construct their own? If an own questionnaire was used, the reasons for not using a previously established one should be mentioned.

4. In which way were data “double checked” (in statistical analysis)?

5. Regarding ethical considerations: were the WHO ethical guidelines for assessing violence against women followed? What was included in the written consent? Considering that the response rate was 100%, the manuscript would benefit from stating if it was emphasized that participation is optional.

6. Could the authors kindly elaborate on the following sentence: “In case of discrepancy between the results of univariate and the results of multivariate analyses, the later was taken as final.”? Furthermore, the authors should elaborate on how the adjusted OR:s were arrived at.

7. Results from the univariable analyses should be given in the results-section.

8. The reference number 10 in the discussion section is given for the statement that similar prevalence rates were found as in India and Vietnam; however, the referred study considers violence in Pakistan. This should be amended.

9. The authors state that their aim is to “provide the policy makers with fundamental data to reduce the prevalence rate of this practice”. Hence, policy implications of their findings should be discussed in the discussion section of the manuscript.

10. The statement “…it is of no doubt the different forms of violence give
negative impact on the women’s life in particular their care after children and community participation” should be followed by a reference.

11. It would have been interesting to conduct regression analyses of sexual and psychological violence in addition to physical violence.

12. Could the authors elaborate on how past year recall bias might be more significant than when reporting violence experienced previously to the past year, as suggested by the authors in the limitations section?

13. The cross-sectional design of this study merits mention under limitations.

14. If the authors have the possibility, I would recommend a language check to enhance the clarity of the manuscript.

MINOR ESSENTIAL REVISIONS

1. I suggest the authors chose either “sexual coercion” or “sexual violence” and use the term consistently in the manuscript (currently “sexual coercive (sic)” and “sexual violence” are used).

2. The authors write that many theories can be used to “justify violence against women” – I think the authors mean to say “explain” (or something similar).

3. Do the authors mean “marriageable age” or perhaps “reproductive age” (as stated in an earlier paragraph) when they write “union age” under methods?

4. The authors state that “uni- and multivariate” analyses were performed; however, as only one outcome variable was used, this should be changed to “uni- and multivariable analyses”.

5. As a p-value cannot be 0 nor “< 0.000”, I suggest inserting “< 0.001” into table 3 instead.

6. The background should specify that the definition given for violence against women is from the WHO.

7. “There is limited data…” should mark a new paragraph in the background section.

8. Under methods, the study period is stated to have taken place in 2015; this should be corrected.

9. The abstract and methods should specify that data were gathered by face-to-face interviews.

DISCRETIONARY REVISIONS

1. I suggest that the aim of the study be stated in the background section of the abstract instead of in the methods section.

2. It is interesting that the response rate was 100%, and this kind of response rate is generally not found in Western societies. Assuming that ethical principles of voluntary participation were followed, I think this would be an interesting point for the authors to briefly consider in their discussion.

I declare that I have no competing interests. I consent that this report be posted on the website along with the article and the other reviewers’ reports.
**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.