Reviewer’s report

Title: Change over time in factors associated with domestic violence by intimate partner in Egypt: analysis of two surveys

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Reviewer: Muhammad M. M Haj-Yahia

Reviewer’s report:

Review of the paper entitled “Change Over Time in Factors Associated with Domestic Violence by Intimate Partner in Egypt: Analysis of Two Surveys”

This is an interesting paper, which presents partial results of two surveys — the 1995 and 2005 Demographic and Health Surveys (DHS) — conducted in Egypt. In this paper, the authors assess the changes in prevalence of domestic violence and help seeking and factors associated with both over the decade 1995-2005 in Egypt. Undoubtedly, the paper presents important results from a country where the problems of wife abuse and battering, in particular, and family violence, in general, have rarely been investigated.

The following are my major comments on each section of the paper.

Abstract

- The authors use the concept “prevalence of domestic violence” throughout the paper. However, in the Abstract they also use the concept “occurrence of domestic violence”. First, they should maintain consistency in using concepts that indicate the rates of the problem. Second, based on the authors’ description as well as on my own acquaintance with both surveys, I believe the incidence of wife beating was measured rather than the prevalence of the problem.

- When the authors use the concept “domestic violence”, both of the surveys examined in the paper have used the concept “wife beating”. Hence, because the authors of the paper conducted a secondary analysis of the two surveys, they should refer to the same concepts used in the surveys.

- The authors use the concept “decreasing heterogeneity” in the Abstract as well as throughout the paper. However, I doubt they have succeeded in explaining the concept.

- In the Abstract as well as in the Conclusions section, the authors indicate that “education programs targeting only women might not be sufficient”. Although the authors tested the relevance of education to domestic violence as well as to help seeking, they did not investigate the extent to which education programs can be sufficient in combating wife abuse. That
conclusion is not based on the results of the surveys, and is therefore not acceptable in a scientific paper of this nature. By the way, when the authors claim that the education programs ‘might not be sufficient’ what exactly do they mean? It seems to me that the authors expect the readers to assume that such programs might not be sufficient to decrease domestic violence and increase help seeking. Nevertheless, assuming that this factor was tested in both of the surveys analyzed in the paper, the authors should be more explicit in their description rather than leaving it to the interpretation of the reader.

Background
- The authors refer to the consequences of wife beating, when this dimension is neither among the variables that were investigated in the surveys nor among the variables investigated in the paper. It is commonly agreed and accepted that a literature review should focus on the specific and main dimensions of the paper and study(ies) rather than on aspects that the paper and study(ies) have not addressed.

- The authors correctly use the concept association (factors associated with domestic violence, p. 3, 3rd line from the bottom). However, they mistakenly use the concept ‘effect’ interchangeably with ‘association’ in this section, as well as in other parts of the paper (e.g., the effects of education, p. 3, 2nd line from the bottom). Please note that reflects causality or semi-causality, whereas the designs of the two surveys do not elicit information on the causality of domestic violence factors.

- The authors should clearly state the hypotheses or research questions they tested in the paper.

Methods
- One major drawback in the comparison conducted in the paper is the reliance on data that were collected on wife beating in two surveys that used different operational measures for this variable. More specifically, whereas the first survey measured women’s experience with violence on the basis of one very general question about being beaten, the second survey used several concrete questions about being pushed, shaken, having something thrown at them, being slapped or having their arm twisted, being punched with a fist, kicked or dragged, strangled or burned, threatened with a knife or gun, or other concrete acts of violence. Undoubtedly, the measure used in the second survey is more operational and detailed than the one used in the first survey. As such, it is more likely that the women’s experience with domestic violence was measured in a more reliable and valid manner in the second survey. Theoretically speaking, it is a great idea to compare women’s experience with domestic violence based on measurements conducted 10 years apart. However, the specific comparison in this paper is implausible and invalid it is like comparing oranges and apples. It should also be noted that the single question about being beaten leaves it up to the women’s personal
discretion as to what they consider beating and what they do not. Hence, I doubt that the measurement of domestic violence in the first survey has good internal validity.

- The authors should present the internal reliability of the measure of wife beating in the second survey.

- In both surveys, the women were asked about talking to anyone about the beating to try and get help (in the first survey) and about trying to get help to prevent or stop him from hurting you (in the second survey) as measures of help seeking. It is clear that in both studies, different questions were used to measure this construct, i.e., help seeking. Because the construct of help seeking in both surveys is based on different measures, the validity and reliability of the comparison is questionable. Moreover, asking the women only one question about getting help to prevent being physically hurt leaves it up to their subjective discretion as to what they consider being hurt. This is another major drawback in the measurement that should be taken into consideration with regard to the comparisons. These drawbacks should also be discussed in the limitations subsection of the Discussion (on page 9).

- The authors indicate that we also included the blood relationship with blood relationship (pp. 4-5). I wonder how the authors provide empirical support for their assumption that beating occurs more frequently in partnerships with blood relationship (pp. 4-5).

- I wonder why the authors treated education as a dichotomous/categorical variable in their statistical analysis, despite the fact that it was measured as an ordinal variable. I believe that treating it as an ordinal variable in the analysis would have produced more valid results.

Results

- Consistent with my above argument regarding the lack of consistency in measuring domestic violence in both surveys, I believe that most of the comparisons presented in the Results section are based on invalid interpretations and statistical conclusions. Although I believe that both surveys yielded important results that are worth publishing, the findings of each survey should be presented separately, even in the same paper, without conducting comparisons.

- I suggest that the authors refrain from indicating that it was only higher by 34% in multivariable analysis (p. 6). After all, 34% is not a negligible percentage, and in this case the word only is unnecessary and even inappropriate.

Discussion

- The use of the concept improvement / no improvement in this section is not appropriate for reaching conclusions on the basis of the results presented in the paper.

- As indicated, the authors do not present any hypotheses or research questions. Hence, I wonder why they are surprised about some of their results
(with a surprising finding that p. 7).
- The authors indicate that their behaviour was most likely affected by recent developments (p. 8) which developments?
- In this section, the authors present numerous interpretations without providing any theoretical and/or empirical references to support their arguments and interpretations. In addition, I believe that relying on some theories (e.g., resource theory could be appropriate) to interpret the results of the study could enrich this section considerably.
- I wonder why the authors assume that educated women might face more difficulties in Egypt (p. 8).
- I definitely agree with the authors that because the topic of domestic violence is expected (p. 9). Nevertheless, they should provide a reference for that argument.
- I tend to disagree with the authors since population-based studies will persist in future surveys (p. 9). Although I indicated that underreporting of domestic violence is likely to happen in surveys, experience has shown that the problem can be overcome, albeit not totally, through several methodological techniques, e.g., by using self-report measures rather than face-to-face interviews, by incorporating social desirability measures in the questionnaire, and by asking several family members (both partners, a child, etc.) about domestic violence, among other techniques.
- The authors should provide a reference for their argument that in fact, forms of violence (p. 9).
- The authors argue that finally, studies based on the same process (p. 9). This argument is not clear, and needs to be illuminated.

Conclusions
- The authors argue that thus, selective educational reduce violence levels (p. 9). This is an unclear argument that needs to be explained further.
- The authors suggest that cultural, rather than individual interventions can be expected to be effective (p. 9). This recommendation does not derive directly from the results, and the authors should be cautioned against reaching conclusions and/or providing recommendations that are not supported by their findings. Nevertheless, it has been proven that structural changes (e.g., enhancing the educational, political, economic, social, and religious status of women) can be effective in combating violence against women.

Figures
The title of Figure 1 should indicate that it refers to the second survey.

I hope the authors find my review helpful in revising their paper.

What next?: Unable to decide on acceptance or rejection until the authors have
responded to the major compulsory revisions

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: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.