Reviewer’s report

Title: Prevalence of Intimate Partner Violence Against Women in the Arab World: A systematic review

Version: 0 Date: 19 Jun 2019

Reviewer: Julienne Corboz

Reviewer's report:

The paper is well written and has merit in its attention to IPV in a geographical context that little is known about in the literature. But the descriptive nature of the paper makes it a little difficult to critique. The methodology and results are clearly laid out, but there are some gaps in the background and discussion sections in particular. The paper would benefit from addressing the following recommendations.

The background section of the paper lacks some important context. The first sentence in the background states that the 22 countries that make up the Arab League share commonalities in language, culture and religion, and although this is true, there are also very wide differences across the Arab League in relation to practices that are linked to gender equality, including early and forced marriage, FGM and others. Although the paper is about IPV, including some content on some of the variations across the Arab World in relation to gender inequality and harmful traditional practices would be helpful for the reader. Although the authors have done this to some extent, they could go a bit further in this regard.

Page 4, line 12 - can the reader be provided with some examples of total figures here to examine the magnitude of change? For instance, a three-fold increase in female school enrolment from 3% to 9% is quite different to an increase from 10% to 30%.

Page 15, line 19 - it states that a study from Jordan may have included witnessing, not just experiencing violence in prevalence rates. Should this study have made it into the selection given that a criteria for selection is women reporting IPV? Perhaps the authors can explain a little more the context of the measurement here as the sentence seems to be a throw away but leaves the readers asking whether the data is appropriate to this study.

Overall, the paper is very descriptive. There is nothing wrong with the description in the results section, and it is helpful to see the evidence summaries laid out clearly in the tables. However, I feel that the paper lacks some substance. This may be because I was expecting some kind of meta-analysis. This kind of analysis would have been very interesting, although the limitations section notes that a meta-analysis has not been done in this paper due to the fragmented nature of the evidence. But in this case, I would expect a more extensive discussion about the results of the
review and the implications. Currently the discussion is partly a reiteration of the findings, but the reader is left wondering "and what now?" Some examples of content that would enrich the discussion include the following.

Are the authors able to make some statements about different forms of IPV and prevalence in different Arab regions (i.e. as noted above, despite some cultural and religious similarities, there are also some wide differences in cultural and harmful traditional practices across different regions)?

What are some of the challenges collecting data on IPV in Arab countries to contextualise the descriptive findings?

Are there any specific recommendations in terms of alignment of methodologies and the possible challenges in alignment across such diverse Arab contexts?

In the final paragraph of the background section, understanding the extent to which Arab countries are progressing on the SDGs seems to be an important objective of the paper. Can the authors return to this issue in the discussion and include some content on the SDGs, including a more direct statement about what needs to be done in terms of evidence building to track whether SDG 5 sub-indicators will be reached by 2030.

I also was left wondering about the Arab countries for which no data was selected for the review. Can the authors comment on what kind of evidence was removed in these countries due to not reaching the set criteria? Are there any reasons for why there is little or no data (e.g. difficulties with access due to conflict)? Are there any specific recommendations or opportunities for countries with little or no available data?

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

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