Author’s response to reviews

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

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Author’s response to reviews:

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Please find a revised version of our manuscript on “Prevalence of Intimate Partner Violence Against Women in the Arab World: A systematic review” based on the reviewers’ comments we received. All edits were made as track changes.

We carefully examined every comment, and below are detailed point-by-point responses to the reviewers’ comments.

In addition to specific questions about details of the review, the selection of articles, the quality assessment of included articles and ethical considerations and the presentation of the results, we received comments about the geographic coverage of the study, how the Arab region is defined, and the wide variations within the region regarding women’s status and other socioeconomic factors. We were also given suggestions to strengthen the discussion section including future research to overcome challenges collecting data on IPV in Arab world.

We found the comments to be helpful and the below point-by-point response shows how we have addressed each of them. We believe that the manuscript is considerably improved as a result, and we hope you will agree that it is now suitable for publication in your journal.
We thank you in advance for your consideration.

Sincerely

Prof. Carla Makhlouf Obermeyer

REVIEWER 1

1. Background: Please cite/provide reference to what is meant by "Arab League"? which countries by WHO's regions fall in this league? Few basic characteristics like majority religion and customs they share besides language.

We added a reference to the first sentence.

WHO regions don’t exactly correspond to the list of countries that are considered Arab countries, based on belonging to the Arab League Organization. Most (19 of 21) EMRO countries are members of the Arab league, but not Afghanistan or Pakistan. Three Arab League members are in part of the WHO AFRO region (Algeria, Comoros and Mauritania).

In recent UN publications about the Arab region (including by UNDP, UNICEF, and UN Women), the Arab region is well recognized as a distinct subregion. For example, see http://www.lasportal.org/ar/Pages/default.aspx and www.arabstates.undp.org/content/rbas/en/home.html

We added an explicit mention of the Arabic language and mentioned that Islam is the religion of the majority of the population in the region. We rewrote the first and second paragraph to refer to the status of women in the region – including both the commonalities and the differences.

2. Methods: Can you illustrate in a table how did you select/drop any article on the basis of quality? What indicators you have used and what scores did the selected articles achieve?

We excluded some articles based on quality criteria, as described on page 7 lines 14-46 in the original manuscript. We mention, for example, that to be included, articles had to be a research study with a clear description of study design and population, sample size and respondent selection; we excluded studies which did not fit this description and those that only included abused women. In keeping with PRISMA guidelines, the selection process and numbers of articles excluded and reasons why are presented in Figure 1, not a table.

If by “indicators,” the reviewer means risk of bias/quality criteria, the 11 criteria are listed in the methods section of the original submission on page 8 lines 53-58 and Page 9 lines 4-14. Quality
scores for all selected articles can be found in the columns titled ‘Risk of bias’ in Tables 3a, 3b, 4, and 5. (Please note that articles were scored only if they met the original inclusion criteria.)

3. Discussion: The violence among pregnant women may be due to selection bias in the studies. What are authors' views about it?

Is the reviewer asking why rates of IPV during pregnancy were higher among currently versus ever pregnant women? If so, we think it may be related to recall of recent versus past events and we added this to the paper.

4. Ethics: Although systematic reviews don't require ethics committee approvals. I hope all data-sets used in this study were permitted to be used and wherever necessary, permissions have been sought.

No raw datasets were used in this study so no permission to use datasets was required. The article was a review of findings published in articles from peer-reviewed, scientific journals.

General comment: Like all studies conducted on IPV, we know that it is an important global health issue. What is new that you want to inform to authors? Can you explicitly mention something unique you have extracted from this extensive exercise? It is assumed that all Arab countries are predominantly Muslim countries. Did you search whether the individual studies or surveys investigate about the perception that "there is a role of religion in legitimizing violence". I think such an angle added to this study could make it unique.

This paper provides a synthesis of the evidence on prevalence of IPV in the Arab region, which is missing in the current literature. The key take-away from this paper is that research on IPV in the Region is fragmented; as such, comparisons are difficult and must be made very carefully, particularly when trying to link observed levels to social context.

Since this was a review of prevalence, not risk factors or cultural context, we did not examine the role of religion. An analysis of cultural factors, including religion, is outside the scope of this review and would have called for a paper on its own. We did however note in the discussion, that research from the region has documented norms and systems that reinforce male authority over women page 24 lines 53-56 of the original submission.
Julienne Corboz (Reviewer 2): 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.

We revised the first and second paragraph of the background to address these concerns. For example,

To the background section: We added a sentence about how female age of marriage has risen dramatically in some countries but high levels of child marriage persist in others. We also added a new reference (and deleted another reference to compensate).

To the discussion: we added a sentence about how more high quality national data collection could also allow researchers to examine cross-national associations between IPV prevalence and factors related to gender equality, including legal frameworks and harmful practices such as child marriage.

Regarding the inclusion of FGM, this is a topic that we carefully considered in conducting the review. Our decision not to include FGM was based on several reasons: first, unlike IPV, which does affect all Arab countries, the prevalence of FGM is substantial (>60%) in only 4 of 22 countries, Egypt, Somalia, Sudan, and Mauritania, considerably lower in two others (Yemen 19% and Iraq 8%, as documented in UNICEF global databases (https://data.unicef.org/topic/child-protection/female-genital-mutilation/). Secondly, it is unclear whether FGM is associated with IPV, and the available evidence suggests that it is not, unlike other harmful practices such as child marriage. Thirdly, the issue of FGM is extremely complicated; one of us (CMO) has in fact written a comprehensive landmark review of the epidemiological and anthropological evidence on FGM (see Obermeyer 1999), and after considering the complexity of the issue, we did not think that it could be adequately covered in this review.

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%. 

Unfortunately the ILO report did not provide the denominator or baseline level (despite their otherwise detailed analysis). Therefore, we replaced the evidence on female enrolment with similar and equally relevant evidence from the same report – evidence on gender parity in primary and secondary education – for which the ILO did provide a baseline.

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.

The risk of bias score (4) for this study reflected the lack of clarity of their operational definitions. However, the article also stated that their measures were based on the CTS2, which is considered state of the art. Moreover, this was one of the few (non DHS) national, dedicated surveys on IPV. After weighing these two factors, we decided that it was preferable to include it, with the caveat that it had obvious weaknesses.

Generally, we included articles that met all inclusion criteria even if their operational definitions were not entirely clear or were flawed. This was done: a) to ensure that the review could examine the strengths and weaknesses of IPV research methods in the region; and b) because if we had used more stringent criteria, we would have had to exclude a very large portion of the research. In addition, our aim was not to conduct a meta-analysis, so the inclusion of these studies did not distort a quantitative analysis.

To clarify this, we added a sentence to the methods section as follows: To capture a wider range of studies and to assess both the strengths and weaknesses of the evidence base, articles that did not provide clear operational definition of IPV were not excluded if they met all other inclusion criteria.

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)?

We cannot draw conclusions about how IPV levels vary across different sub-regions of the Arab world, given that the major finding of the paper is that the evidence base does not allow for systematic comparisons across countries given the lack of data comparability – except for those with a DHS. However, we added a sentence to the discussion that future research should examine how IPV levels vary by country according to factors such as child marriage.

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

One challenge identified in the article, as noted on page 23 lines 11-23 of the original submission, was the sensitivity of measuring sexual violence. To mention additional challenges, we added another sentence: Barriers to national data collection may range from political instability, armed conflict or forced displacement, or a lack of experience carrying out national household surveys on sensitive topics.

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

One important recommendation – as noted on page 23 line 43-48 of the original manuscript -- is that the next iteration of the WHO instrument for measuring IPV be translated into Arabic, adapted to the cultural context and shared/implemented in the region.

To give this more emphasis, we added a sentence to that paragraph as follows: National data collection using either a DHS violence module or the WHO instruction in additional Arab countries would be one way to expand the evidence base.

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.

The original manuscript returned to this theme in the discussion on page 23 lines 24-36. However, to strengthen this point, we rewrote this section and added additional sentences about how countries to find ways to increase high quality national data collection on the prevalence of IPV for purposes of SDG monitoring.

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?

To clarify, no evidence relevant to this review was excluded from countries with no data. Because the inclusion criteria were broad, there were not many articles “removed” from countries with no evidence. We added a sentence to address the question about why countries may not have data: Barriers to national data collection may range from political instability, armed conflict or forced displacement, or a lack of experience carrying out national household surveys on sensitive topics.