Author's response to reviews

Title: Assessment of Reproductive Health and Violence Against Women Among Displaced Syrians in Lebanon

Authors:

Amelia Reese Masterson (areesemasterson@gmail.com)
Jinan Usta (ju00@aub.edu.lb)
Jhumka Gupta (jhumka.gupta@yale.edu)
Adrienne S Ettinger (adrienne.ettinger@yale.edu)

Version: 4 Date: 20 December 2013

Author's response to reviews: see over
December 20, 2013

Dr. Peter O'Donovan  
Executive Editor, BioMed Central  
e-mail: editorial@biomedcentral.com

RE: MS# 1836865283102774, “Assessment of Reproductive Health and Violence Against Women Among Displaced Syrians in Lebanon”

Dear Dr. O'Donovan:

Thank you for the opportunity to submit a revised second set of minor revisions to our research article, “Assessment of Reproductive Health and Violence Against Women Among Displaced Syrians in Lebanon.”

This manuscript represents some of the first quantitative data on the refugee population in Lebanon related to the ongoing conflict in Syria. This study provides much-needed baseline data to assess the rapidly evolving situation.

Enclosed please find a revised manuscript and a version with changes tracked. Below this letter, we provide a point-by-point response to reviewers’ comments.

Thank you for your thoughtful consideration of our revised work. Please do not hesitate to contact me should you require additional information or clarification.

Sincerely,

Adrienne S. Ettinger, ScD, MPH  
email: adrienne.ettinger@yale.edu

Adrienne S. Ettinger, ScD, MPH  
email: adrienne.ettinger@yale.edu
Reviewer 1

Please make sure the figures are consistent. In the discussion, for example, the authors note: “Exposure to conflict-related violence, abuse, and/or sexual violence was reported by over 25% of women” although the results section finds that it was over 30% (which is more striking and reflected later on in the discussion).

- We thank the reviewer for pointing out the inconsistency and have revised the percent reported in the discussion from “25%” to “30%” to correctly reflect the results.
- Upon checking the entire manuscript for other inconsistencies as the reviewer recommends, we made several corrections in the abstract on page 2, which had not been updated with the text and tables. In Table 4, we also changed “vaginal infection” to “reproductive tract infection” to match the text.

The statement “Although the indicators differ, we can see that the 30.8% prevalence of conflict violence that we found in our sample is close to WHO’s 37.0% prevalence of intimate partner violence in the Eastern Mediterranean region [31]” doesn’t make sense to me. It may be useful to present the IPV rate at 37% to stress that the 30.8%, which did not include intimate partner violence, is in addition to violence women may be experiencing at home (therefore to make the case that the needs for support may be much greater if one were to take into account IPV exposure as well).

- We agree with the reviewer that it is more meaningful to point out that the conflict violence prevalence we found may be in addition to intimate partner violence. We have revised the text accordingly on page 14, as follows: “Additionally, WHO found a 37.0% prevalence of intimate partner violence in the Eastern Mediterranean region, which suggests that the 30.8% prevalence of conflict violence that we found in our sample may be in addition to violence women are experiencing in the home.”

It is still a bit unclear to me whether stress was considered a 'mediator' between exposure to conflict violence and poor health outcomes (as mentioned in the intro), or as a separate question: presumably women who weren't exposed to violence also felt stress due to their refugee status - was their stress assessed and included? Some clarity here could be useful since the authors note that the majority of women reported having stress-related symptoms but is this the majority of women exposed to violence or the whole sample? I couldn't find this answer in the tables since prevalence rates of stress seem to not be there. Please re-insert in some form either in the tables or the text.

- We thank the reviewer for making this point. It is indeed the majority of all women (not just those exposed to violence) who reported higher indicators of stress than normal. We have clarified this in the text on page 12 and added prevalence (>75%) for experiencing all seven stress indicators more than usual.
- We have also revised the text on pages 12, 13, and 14 to reflect the mediating role of stress in the relationship between conflict violence and some health outcomes.
• While reviewing our analysis related to the stress score variable (based on the reviewer’s comment), we came across an error in the multivariate analysis for Table 4. We have corrected the manuscript accordingly in Table 4 and the text. On page 11 we added the phrase, “though not with self-rated health.” On page 12 we added, “Among the entire sample, women with higher levels of stress were more likely to have poor self-rated health (p<0.01).” We do not feel that this correction changes our interpretation of results significantly.

• Additionally, on page 12, we corrected a typing error in the reported stress score loading, changing “7.0” to “0.7”

Reviewer 2

The authors need to provide references to the text which they have added on page 5 (following my initial suggestion) on the pathways linking violence, reproductive health and potential confounders e.g. demographics. (see for example Okenwa L, Lawoko S., Jansson B. Contraception, reproductive health and pregnancy outcomes among women exposed to intimate partner violence in Nigeria. Eur J Contracept Reprod Health Care. 2011;16:18-25; and Emenike E, Lawoko S, Dalal K. Intimate Partner Violence and Reproductive health of women in Kenya. International Nursing Review 2008;55:97-102. etc)

• We thank the reviewer for this comment and the suggested references. We have accordingly added citations to the appropriate paragraph, and added these two references and one other to the manuscript.

They could add to the end of that text that "accordingly, we adjusted for potential confounding of such variables in our analysis"

• As recommended, we have added this sentence to the end of the paragraph on page 5.

Under data analysis (page 5), the authors for some reason choose a rather stringent condition for inclusion of covariates associated with both outcome and risk factors in further analysis when they choose a p-value of p<0.01. It is not clear why. In the explanation they provide in the separate file however, they have indicated p<0.05, so this is possibly an error?? In any case, a p-value of 0.05 is usually the norm. In some cases p<0.1 (not 0.01!) could also be used.

• We thank the reviewer to pointing out this error, which we have corrected on page 7 since we did, indeed, use p<0.05 as our inclusion condition for covariates.

Editor Comments

1. Please ensure that email addresses for all authors are provided on the title page.

• We have added all author emails to the title page.

2. Please ensure to include a separate "Conclusions" section after the Discussion.

• As requested, we have added a separate “Conclusions” section.

3. We would like to ask whether local ethical approval was sought from the study site. You state in the manuscript that approval was obtained form an IRB in Yale and from the UNFPA, but there is no
mention of approval being sought from a local ethics committee. We would appreciate if you could clarify this and if approval was not sought, to provide an explanation as to why.

- The study was carried out at several local clinics, none of which have IRBs, under the auspices and at the request of UNFPA’s Lebanon office in Beirut. The participating clinics were a combination of both public and private, all receiving some type of support from UNFPA. Therefore, we sought and obtained ethics approval from the IRBs of Yale University and UNFPA/Lebanon using standard procedures for written approval of study protocol and we adhered to all ethical standards throughout the study period.