Author's response to reviews

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

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Author's response to reviews: see over
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Dr. Peter O'Donovan  
Executive Editor, BioMed Central  
e-mail: editorial@biomedcentral.com

RE: MS# 1836865283102774, “Assessment of Reproductive Health and Gender-Based Violence among Displaced Syrian Women in Lebanon”

Dear Dr. O'Donovan:

Thank you for the opportunity to revise and resubmit our original research article with the revised title, “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 clean 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  
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Reviewer 1

Gender-based violence in simple terms refers to violence directed against a person on the basis of gender. While research suggests that violence against women in refuge could be gender-based, anecdotal evidence suggests that abuse of refugee men by fellow men (in uniform) is not uncommon. As the authors did not report abuse of refugee men in this study, it is misleading to use the term gender-based violence in this paper. I advice they instead use "violence against women".

- We thank the reviewer for this point, and have accordingly changed all previous references to “GBV” or “SGBV” to be “violence against women” (“VAW”) or “conflict violence” throughout the paper, or simply described the type of violence referred to in greater detail.

In addition, the authors used tools for assessment of GBV in conflict affected regions, which is ok. As some measure of contextual validity however, it would be interesting to report reliability coefficients (Cronbach’s Alphas) for the current dataset.

- We appreciate the reviewers comment. It would be interesting to report Cronbach’s Alphas for the current dataset; however, the survey questions came from several sources (the Women’s Commission’s “Gender-based Violence Tools Manual For Assessment & Program Design, Monitoring & Evaluation in Conflict-Affected Settings,” the CDC’s “Reproductive Health Assessment Toolkit for Conflict-Affected Women,” and previous UNFPA surveys in Lebanon) and we adapted them for the Syrian population in Lebanon for cultural and language sensitivity, so any quantitative comparisons to the original instruments would not be valid.

There is some confusion regarding the analysis of the outcomes of study in this paper. While descriptive statistics are presented for a variety of reproductive and other health indicators (tables 1, 2 and 3), It is not clear why the authors run regressions for only some outcomes (i.e. menstrual irregularities, pelvic pain and RTI). Yet, the impact of violence on poor self-rated health, hypertension, smoking, birth control and poor health seeking behaviors (all available in this data set), are documented in the research! Are these other aspects studied (or to be studied) in a separate paper as outcome variables? In that case, the authors should mention so when they are introduced in the methods and thereafter omit any analysis and presentation of these variables as outcomes in this study. Otherwise, they should also be modeled in the regressions analyses alongside menstrual irregularities, pelvic pain and RTI.

- We thank the reviewer for this the suggestion, and appreciate the recognition that this dataset has a rich array of variables, which may be useful to consider through further analysis. Based on the reviewer’s suggestion, we ran additional multivariate analysis on the outcomes: “self-rated health” and “OB/GYN visit in past six months” as these health outcomes of violence or stress are theoretically sound and broaden the picture of women’s health outcomes. In this analysis, we focused primarily on reproductive health outcomes but believe that self-rated health is a relevant outcome, inclusive of reproductive health. Please see Table 4 for the findings from this additional analysis. We have also added some sentences in the results
section on pages 11 and 12, and altered the discussion section slightly to account for these changes.

- We selected “OB/GYN visit in the past 6 months” to represent reproductive health seeking behaviors, but found there to be no association in bivariate analysis. We have explained this in the results section.

- We examined hypertension and smoking as potential confounders in the relationship between violence or stress and reproductive health outcomes.

- We question the reliability of our data on birth control use because we suspect that the question was misunderstood by some participants due to cultural perspectives regarding natural methods of birth control vs. modern methods.

- Additionally, we considered examining pregnancy-related outcomes (such as pregnancy or delivery complications), but found that the subsets of women who were currently pregnant (n=43) or had already delivered (n=38) were too small for meaningful analysis.

Still in relation to outcome measure, it appears from the method section and results that the health outcomes of interest are either binary (e.g. reproductive health) or ordinal (e.g. General health), but the authors introduce some confusion in the text when discussing study weaknesses (page 15) where they argue that the lack of pre-conflict data made it difficult to assess whether the health situation among studied women is due to the conflict. They add that, “to account for this lack of information, questions were phrased in a comparative manner” (e.g. "more than usual"). Unless I am mistaken, none of the data presented in this paper supports this account.

- We agree with the reviewer that this is confusing, and have removed this description from the limitations section, particularly since it only applied to stress-related questions. Although not a limitation to this study per se, the lack of pre-conflict data on Syrian women does pose a challenge to placing our findings in the broader context.

Control for covariates using regressions analyses is good practice but such controls should be motivated. The Authors mention in the methods section (page 8) a great number of potential covariates without a motivation (based on theory and/or analysis) for why these factors should be considered covariates. A paragraph in the introduction should sort out the former where theories/data broadly linking such covariates to health outcomes outcome on the one hand and exposure to violence on the other are introduced. In sum, what was the rationale for choice of potential covariates in this study?

- We thank the reviewer for this suggestion, and have included a paragraph in the background section on page 5 on theoretical reasons for covariate selection, including supporting literature.

A serious omission concerns testing whether potential covariates are related to specific study health outcome using bivariate analyses. Each covariate should be tested independently for association with outcome of interest (e.g. reproductive health), upon which those showing significant associations would qualify for further analysis using regression. I have not found any such analyses (i.e. each potential covariate vs. reproductive health outcomes) in this work. I suspect the authors may have run such analyses but for unknown reasons not presented them, as they include some potential covariate in the regressions but leave out others. In sum, why were some of these dropped from the regressions analyses while others where included?

- We have edited the methods/data analysis section to describe our covariate selection process in more detail, which included extensive binary analysis of all potential confounders on independent and dependent variables (page 7). We explain that any potential confounders associated with both risk factors and outcomes at the level of p<0.05, and found to be plausible confounders, were maintained in multivariate analysis.
In Table 4, we have edited the footnote to reflect this method of covariate selection. We have also redone the analysis for Table 4 based on the new “stress score” variable (described in responses to Reviewer 2, below) that we created.

It would be useful to have some sub-headings in the results section to enhance understanding of the logic of presentation

- We thank the reviewer for the suggestion and have inserted subheadings in the results section.

It would be interesting to in the discussion, compare the figures presented for GBV in Syria to those reported in other conflict regions. Also, a discussion of how these figures contrasts with population statistics (even if proximal e.g. intimate partner violence) would enrich the discussion. See for e.g. WHO release for references: Global and regional estimates of violence against women: prevalence and health effects of intimate partner violence and non-partner sexual violence, WHO, Geneva, Switzerland).

- We thank the reviewer for this useful suggestion and have cited the WHO reference in the background and discussion sections. In the discussion section (page 13-14), we acknowledge that the WHO data is not perfectly comparable, but does provide a useful frame of reference for our findings.

Reviewer 2

The paper title, discussion, and conclusion that the information contributes to our understanding of gender-based violence among conflict-affected women is somewhat misleading since the data collected on violence exposure seems to include both conflict-related violence which may be due to political or ethnic affiliation, rather than gender, and sexual violence (which may indeed be gender-based). The term “Violence Against Women (VAW)” or simply “women’s exposure to conflict-related violence” may be preferable since it is more encompassing and since the authors did not collect intimate-partner violence data but only data on violence perpetrated by armed persons. In the introduction and discussion the authors can link their findings to the existing GBV literature, but given that men and boys in Syria are also experiencing conflict related violence, which may be similar in type, it is not convincing that all the violence can be grouped as GBV simply because the survivors are female. Table 4, which notes “Exposure to any Conflict Violence” seems more accurate a description of the data collected.

- As stated above, we have changed all references to “GBV” or “SGBV” to “VAW” or “conflict violence” throughout the paper, or simply described the type of violence in greater detail.
- We have also reviewed the background and discussion sections where we draw upon the GBV literature and have found that these references are still appropriate to the paper, as the reviewer suggests.

With regards to the multivariate analyses and table 4, it is unclear why the authors have adjusted for different things based on the outcome as indicated in table 4 (i.e. not adjusting for age and education when looking at RTI as the outcome) and that these are somewhat different from what is included in the Methods section text.

- As stated above, we have included a paragraph in the background on theoretical reasons for covariate selection, including supporting literature.
- We have also adjusted the methods section text regarding covariate selection to match selection process used in our analysis, and supported by the underlying theoretical pathways described in the background.
In Table 4, we have edited the footnote to reflect this method of covariate selection. We have also redone the analysis for Table 4 based on the new “stress score” variable that we created, so numbers may differ from the previous draft.

Also, it would be important to explicitly state the hypothesized relationship/mechanism between violence, stress, and poor gynecologic outcomes (i.e. is stress a mediator variable?)

- We agree with the reviewer and have added a sentence in the background that explicitly states the hypothesized pathway from violence to gynecologic outcomes, with stress as a potential mediator (in addition to being an independent variable).

It would be important to explicitly state why each ‘stress’ outcome is shown separately rather than having one ‘stress’ variable.

- The stress scale used in this study was adapted from a scale previously used by UNFPA on women in a post-conflict setting in Lebanon, and which was based on the validated GAD-7 (generalized anxiety disorder scale).
- We agree with the reviewer that it would be preferable to use a single stress variable in the analysis. Therefore, we have conducted a new principle-component analysis on the stress scale data in order to create a single “stress score” variable for use in the analysis.
- The results of the principle-component analysis can be found on page 12 in the results section.

It is also unclear why beating one’s child is ‘stress’ and not violence and how that would be on the ‘causal pathway’ between violence experience and poor reproductive health outcomes.

- We appreciate the reviewer’s comment. The focus of our study is on violence against women, and not on other forms of violence in the home such as child-beating. This question was included in our study based on focus group discussions (not reported here) in which Syrian refugee women reported that they often turn to child-beating as an outlet for their stress. Therefore, an increase in child-beating could be an indicator of stress experienced by these women.
- Additionally, based on our principle-component analysis, we were able to identify a clear stress/anxiety construct with all retained factor loadings being >0.6. The question about child-beating had a loading of .703 on the stress/anxiety construct, demonstrating the accuracy of child-beating as an indicator of stress in this population.
- We have added more details about this issue in the methods/data analysis section.

The discussion section should expand on the limitations related to the sample and timing of the survey. For example, one of the limitations of the study that hasn’t been acknowledged is that although the authors note in their introductory paragraph that the number of displaced Syrians seeking refuge in Lebanon has increased from 48,000 in August 2012, the date their needs assessment was completed, to over 400,000 in April 2013, the possible different characteristics (age, SES, ethnic/religious affiliation, etc) of newer refugees are not discussed, nor the potential different needs. Although the authors acknowledge that the results cannot be generalized to all Syrian refugee women, data may be available from other sources, such as UNHCR, in terms of these characteristics that could be mentioned in the discussion.

- We appreciate the reviewer’s suggestion and have updated the statistics in the opening paragraph of the background section to reflect currently available numbers.
- As the reviewer points out, the situation is constantly evolving and the needs of this refugee population are changing rapidly as this is an ongoing conflict setting.
We have added this limitation to the discussion. ["Since this is an ongoing conflict setting and the numbers of Syrian refugees is increasing over time, it is possible that newer refugees may have different characteristics (e.g., age, SES, ethnic/religious affiliations, etc.) or potentially different needs. However, this study provides much-needed baseline data to assess the rapidly evolving situation."

Similarly, another limitation related to the findings around help-seeking behaviors and barriers to access to care, which hasn’t been fully acknowledged, is that the sample was from clinics (selected because of their size and provision of reproductive health services) which may mask the unmet needs of women unable to even reach those clinics. It may be useful for the reader to have a sense of what percent of female refugees in Lebanon do not have access to such clinics (i.e. did these clinics receive tens of thousands of patients given the 48,000 refugee figure or hundreds which may be a distinct subset?)

As recommended by the reviewer, we have further explored UNHCR data on the situation of Syrian refugees in Lebanon. We have added statistics on the number of refugees who have been able to access health clinics up to present, and included a few sentences on this in the discussion section on page 16. UNHCR was a partner in our research and the their data is only now becoming available. Data is not available for the time period of our study and available data is still not disaggregated by gender so it is of limited value in describing the population of women not attending clinics at the time of our study.

We have recognized this as a limitation in our paper: "Finally, the survey location at health care centers poses a limitation on generalizability and prevalence estimates, as women presenting to these centers may differ from the general population with respect to health status, behaviors, or knowledge about health services" (page 15-16).

The abstract needs to be revised to make note of the multivariate analyses under ‘methods’.

We have noted the use of multivariate analysis in the abstract under methods as the reviewer recommends.

The conclusion that the paper “highlights the need to tailor the humanitarian response to the cultural background of the refugees and to the existing situation of the host country” doesn’t seem to be derived from the data (or it is unclear what cultural background is referenced).

Based on the reviewer’s comment, we have removed this sentence from the conclusion. Although we have some limited data on cultural barriers to accessing reproductive health care among this population, the majority of this is qualitative data from focus groups, which is not reported here.

Our study, however, some of the first data on the Syrian refugee population in Lebanon on a situation that only seems to be worsening with time. We maintain that it is critical for humanitarian responses to attend to the cultural background(s) of refugees needing assistance.

Although the needs assessment conducted was broad and addressed both reproductive and general health status, the paper is currently written and structured in a way which makes it seem like there are several sub-papers with each having a distinct aim, namely to:
A) document the characteristics of the Syrian refugee population and their unmet needs;
B) examine the relationships between exposure to violence, stress, and gynecologic conditions; and
C) describe the particular vulnerabilities of pregnant women.
This makes the paper harder to follow and somewhat less coherent. The authors may wish to reconsider the emphasis given to each. From my perspective part (A) could be better presented in the form of a table, since the text in the first paragraph of the Results section is somewhat long and only relevant in the analysis in terms of control variables and the emphasis should be placed on (B).

- We thank the reviewer for his/her comment that this data might allow for more than one paper. However, given the limitations of our dataset, we believe that this is important information, but may not warrant three papers. Hence, we have targeted a journal of a broad scope on issues of women’s health and with open access to a wide readership.
- We did choose to focus primarily on (B), the relationships between exposure to violence, stress, and gynecologic outcomes. Hence, per the reviewer’s comments, we have cut down the text in the results section that covers (A), characteristics and needs of Syrian refugees (page 9). We instead refer readers to Table 1, which further enumerates these characteristics. Our primary focus in this paper is on reproductive health outcomes, in relation to violence/stress, and including pregnancy outcomes.

Other Edits

In addition to the edits mentioned above and based on reviewer comments, we have edited the text slightly to reduce number of words, correct for minor grammatical errors, and improve clarity and readability of the text.

Please note the following changes to numbers:

- Page 4: We have updated UN figures to reflect the current numbers to-date.
- Page 4: In the background, we replaced the phrase “most of these refugees are women and children” with a more specific phrase relevant to our study population, “about 24% of these refugees are women between the ages of 18-59.”