Reviewer's report

Title: Assessment of Reproductive Health and Gender-Based Violence among Displaced Syrian Women in Lebanon

Version: 1 Date: 17 September 2013

Reviewer: Stephen Lawoko

Reviewer's report:

Major compulsory revision

The authors need to re-examine some issues concerning terminology, inconsistencies in analysis of outcome variables and inclusion of variables as covariates, discussed in detail below:

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". 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 data set

2) 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.

3) 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.
4) 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. A more serious omission in this work however concerns testing whether these potential covariates are related to specific study health outcome using bivariate analyses. In essence, 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 summary therefore, my concerns in point 4 are i) what was the rationale for choice of potential covariates in this study? ii) Why were some of these dropped from the regressions analyses while others where included?

Minor essential /Discretionary revision

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

6) 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)

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests