Reviewer's report

Title: Intimate partner violence against women and its association with pregnancy loss in Ethiopia: Evidence from a national survey

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The authors of this submission have used a secondary analysis of cross-sectional survey data from the 2016 Ethiopian DHS to show a statistical association between intimate partner violence and pregnancy loss, and also an association between multiple acts of controlling behaviour and pregnancy loss, all in married couples. Although not unexpected, this may be useful information on which to base further more focused research. It is also of value with its subject matter based in Africa.

To tighten up the submission, I suggest the authors cut the introduction by more than 50%. Informed and interested readers will not need reminders on the burdens and definitions of pregnancy loss, and also don't need an introduction to Ethiopia's health service and the state of unmet need for contraception.

There are significant limitations to the interpretation of the results. While limitations have been acknowledged to a degree, there remains a sense that the authors believe in a causal relationship, with pregnancy loss labelled as the as the 'outcome variable'. The authors have not adequately acknowledged the limitations. The results as they stand cannot be used to make any statement about causation. They can only serve to generate hypotheses. First, the outcome of pregnancy loss is a broad category that includes both spontaneous losses and deliberate terminations. The need to merge these two quite different phenomena suggests that the authors had a sample size problem. Ideally, the effects on the 'outcomes' of pregnancy loss from intended termination of pregnancy and pregnancy loss from accidental stillbirth or miscarriage should have been examined in separate analyses. Second, so-called reverse causality is a real and even likely possibility here. Third, the authors were faced with secondary data and could not design the research to include all ideal covariates for analysis, hence the likelihood of residual confounding, which can easily explain the low derived effect sizes (adjusted odds ratios less than 2). And so it is not appropriate for the authors to write, on page 12, that 'this study provides insights into the potential role of IPV and partner controlling behaviour in maternal mortality'. That statement is not supported by the data provided. The matter is far more complex than can be explained by speculation on 'potential direct and indirect pathways'.

The conclusion as submitted goes far beyond what can be supported by the data. But I do agree with the important public health message, as given in the abstract, that 'there is a clear need to develop IPV prevention strategies'. This is supported by the descriptive data from the 2016 DHS. Whether intimate partner violence causes pregnancy loss or maternal death is irrelevant - intimate partner violence remains unacceptable. The study provides some foundation for further research on the how and why of pregnancy outcomes in association with intimate partner violence.
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?
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Yes

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No

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