Author's response to reviews

**Title:** The Association of Unwanted Pregnancy and Social Support with Depressive Symptoms in Pregnancy: Evidence from Rural Southwestern Ethiopia

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Author’s Response to Reviewers Report

Title: The Association of Unwanted Pregnancy and Social Support with Depressive Symptoms in Pregnancy: Evidence from Rural Southwestern Ethiopia

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Cover letter

Ref: Revised Manuscript

This is to submit a revised version of our manuscript based on the comments received from three reviewers. We have described point by point on each comment provided by the reviewers. Moreover, we have made an extensive language edit. Thank you for considering our manuscript for publication on BMC pregnancy and Child Birth Journal.
Reviewer 1: (NO. 2078002281896087)

Abstract

1. This is a cross-sectional data from a cohort study.

Thank you for pointing out this. We have made the following correction: Data for the present study comes from a cross-sectional baseline study conducted as part of a prospective cohort study (see the revised abstract).

Introduction

1. There are studies in developing countries on the association between not planning pregnancy and depression during pregnancy, and two examples given by the reviewer.

Yes, there were studies that showed no association between unintended pregnancy and maternal depression during pregnancy. We have cited two examples of such studies where association was not found in this revised version (introduction section-paragraph 4). However, in majority of studies, unintended pregnancy was associated with depressive symptoms during pregnancy. See for instance, a systematic review by Lancaster et al (reference, 34) on the risk factors of prenatal depression.

2. The author’s state that women receive less support from their partners during pregnancy is based on research data? If so they should mention the reference.

Thank you for pointing out this. We have included a reference now (Paragraph 4, ref 41).

Methods

1. The results are derived from cross sectional data analysis (not from the cohort study).

This comment is accepted and modified to: Data for the present study comes from a cross-sectional baseline study conducted as part of a prospective cohort study (see Methods section, paragraph 2).

2. How many pregnant women were included and how many refused to participate until the final composition of the sample?

The study targeted all pregnant women in their 2nd and 3rd trimester living in the eleven kebeles\(^3\) (Villages) in Gilgel Gibe DSS area, and 627 pregnant women were identified from the DSS registration and from the records of Health Extension Workers who work in each village. But 622 women participated in the study, (response rate was 99%). We have clarified on this information in the methods section (2nd paragraph) and in the first paragraph in the results section as well.
3. How many pregnant women are usually seen in these clinics per month (or year)?

This is a community based study, not clinic based. We didn’t have information as to how many are seen in a clinic per month.

Measurements

1. How the variables; Perceived work burden and Intimate Violence Partner measured?

We have clarified on the definitions of these two variables in this revised version; perceived work burden and intimate partner violence and some other independent variables (measurement section, paragraph 4&5).

2. It was unclear how the variable “household food insecurity” was assessed. I would suggest to clarify this phrase

We have improved on the description of this variable measurement section, paragraph 6). The responses to the 6 food security questions were recoded as follows: the affirmative (‘yes’) responses were corted as one and the negatives (‘no’) as zero. Then their responses were summed to produce an index of food insecurity. Households were classified as ‘food-insecure’ if the respondent answered affirmatively to two or more of the six household food security questions.

Results

1. 59% of the women wanted pregnancy and only 13% did not plan. This is an unexpected result according to the literature developed countries.

Though the figure for wanted pregnancy (59%) is correct, the proportion of women with Unplanned (unintended) pregnancy is 41% not 13%. Unintended (unplanned) pregnancy consisted of women with mistimed pregnancy (28%) and those with unwanted pregnancy (13%). In the literature, these two categories are sometimes combined, but here, they were treated separately.

2. Second paragraph: How was defined and rated "previous experience of pregnancy complication" (not described in the methods section)?

The phrase previous experience of pregnancy complication was used to refer to experiences of miscarriage and still birth. A correction has been made in this revised version (results section, paragraph 2).
Only 3% of women suffered physical violence. This data in the context of poverty is unusual. Research in developed countries have validated this statement (see for example: Ludermir et al, Lancet.2010; 376(9744):903-10; Faisal-Cury et al, Matern Child Health J. 2012)

In this study, Intimate Partner Physical Violence during pregnancy was measured by asking whether she has ever been beaten (pushed, hit, slapped, kicked, choked, or physically hurt) during the current pregnancy. However, a standard questionnaire such as the one used in the WHO multi-country study was not used. Accordingly, only 4% (there was rounding down to 3% previously) of women reported that they experienced physical violence during pregnancy. This level of intimate partner physical violence during pregnancy is low, but there has been no similar study in this study area for validation. In your study of temporal relationship between Intimate Partner Violence and Postpartum Depression in a Sample of Low Income Women, the prevalence of Physical violence was 23.4%. In the literature, low income (of women as compared to their husbands) is one of the factors that predispose women to Violence. However, according to a literature review cited in a study by Ludermir et al (Lancet, 2010 – ref [27], “rates of violence perpetrated by intimate male partners during pregnancy vary worldwide from 3% in London to 31% in Mexico City”. This may include other forms of violence apart from physical violence, but yet our finding is still within the range of findings from such studies.

Conclusion

The study found no association between depression and Intimate Partner Violence. Therefore it should not be included in this paragraph.

We have reported that the association between intimate partner violence and depressive symptoms during pregnancy was marginal (p=0.051). After adding a variable called occupation in the model (with suggestion from another reviewer), the association improved slightly (P<0.05). Given the significant association, we preferred to keep the issue in the discussion section.

Final suggestions

The article does not have a specific item for discussion of its limitations. I would suggest to do that discussing the 3 points below:

1. The inability to establish causal relationships due the type of study
2. The instruments used to define and evaluate some variables (for example: IPV)
3. The possibility of recall bias to explain the association between “not planning the pregnancy” and “depression during pregnancy”.

We accepted the comment and included a limitation section in this revised version of the manuscript.
Reviewer 2: (No. 1268408645900416)

Main Comment: Quality of written English: Not suitable for publication unless extensively edited.

We have made an extensive edit this time. Due to shortage of budget to pay for a language editor, we have read through the manuscript thoroughly and corrected the errors. Specifically, the third author, a native English speaker based in Johns Hopkins University (USA), has extensively edited the manuscript. We feel that the quality of written English is improved this time.

No, other comments/concerns were raised by the second reviewer.
Reviewer 3: (No. 1089720981900415)

Thank you for thoroughly reading through the paper, and these important comments

General

1. The main issue is that the article lacks a theoretical conceptualization and is descriptive only. However, given the few studies in this area in Ethiopia, this manuscript is still important.

The comment is accepted. However, as briefly stated in the introduction section, the focus of the study is the intersection between reproductive health and mental health. Even though it wasn’t stated clearly, we could hypothesize that unwanted pregnancy increases the risk of antenatal depression.

2. A second issue is that the manuscript overall is poorly written, detracting the reader from understanding the goals and methodology of the study. Proofreading is necessary prior to the next submission. See additional details in point 11 below.

This comment was addressed along with the details in point # 11. We have made extensive language edits, and manuscript is revised based on the comments given.

3. Abstract: include EPDS cut-off scores used to obtain 19.9% prevalence rate.

We have included information on EPDS cutoff point used to obtain 19.9% in this revised manuscript (See abstract section).

Introduction

A. The presence of comorbid mental health conditions could greatly influence the results of the current study. However, it is unclear if the presences of other mental health conditions, such as anxiety disorders, are available for analyses and if so, controlled for in the statistical analyses.

It is true that the presence of comorbid mental health conditions could influence the results. However, anxiety and other mental health problems mentioned in the introduction section were not studied. The study focused on antenatal depressive symptomatology only. This is mentioned in the limitation section in this revised version of the manuscript (see limitations of the study).
B. In their reviews of the literature, the authors need to clarify what they mean by “maternal depression,” i.e., whether at the symptomatology or clinical level.

*We have now clarified that the study focused on depressive symptomatology, not clinical level (paragraph 2 in the introduction section).*

C. The second paragraph of the introduction compared rates of antenatal depression in developing vs. developed countries. However, only prevalence estimates for developing nations are listed. In addition, although it is stated that magnitude and risk factors in Ethiopia are not well known, it is not clear if Ethiopia was included in the prevalence rates listed for Sub-Saharan Africa.

*We have included prevalence estimate for developed countries now, (see paragraph 2, introduction section). However, Prevalence estimates for Ethiopia is not included because there has been no study that examined the prevalence of antenatal depression from our literature review.*

D. The final paragraph of the introduction notes the relationship between unintended pregnancy, high parity rates, and maternal mental health. The relationship between unintended pregnancy and social support is then discussed. However, the connection between high parity rates and the other factors is not addressed. This section would be stronger if the relationship between all mentioned variables was further outlined.

*The last paragraph highlights the focus of the research, and the two variables of interest – unwanted pregnancy and social support were discussed. In the process, some variables such as parity were left out from discussion.*

**Methods**

A. It’s unclear whether this “population based cohort study” included other variables other than what was reported here (e.g., anxiety, as noted in point 4.a. above).

*As mentioned above, other mental health conditions such as anxiety were not studied. This is mentioned in the limitations of the study.*

B. Use the term “interviewers” instead of “enumerators”.

*Comment accepted and correction made accordingly.*

C. The authors cited two studies that have used the EPDS previously. However, the authors did not state what these cut-off points were for these samples. If there are prevalence reported, this should be reported also in the introduction. Instead, the
authors cited two additional studies for the 13 cut-off score, based on a Turkish and S. African (urban) sample. This is confusing and needs clarification.

Yes, we have cited two studies that have used the EPDS in the introduction [ref 14 &17], and in the methodology section for EPDS cut off point of 13. Both studies have been cited in the introduction part (including prevalence of depression), and they are cited again because they are examples of studies that used EPDS cut off 13 and above.

D. Under Study Setting and Sample, second paragraph, a total of 627 women were reported to be part of the sample. However, in the results section, first paragraph, a total of 622 women were reported.

We have clarified this point as follows (also comment from another reviewer). The study targeted all pregnant women in their 2nd and 3rd trimester living in the eleven kebeles (Villages) in Gilgel Gibe DSS area. Accordingly, 627 pregnant women were identified from the DSS registration and from the records of Health Extension Workers who work in each village. But 622 women participated in the study, (response rate was 99%). We have clarified on this information in the methods section (2nd paragraph) and in the first paragraph in the results section as well.

Results

a. Several variables in table 1 need operationalization: a) wealth tertile, b) presence of domestic violence (see #8 below), and (c) work burden. Also, did the authors have data on employment (types, full time/part-time).

Thank you for pointing this out. We have now included the operational definitions of all these variables. As to data on employment, we have data on occupation; it is included in this revised manuscript. However, most women reported that they were house wifes, and do not have a regular occupation. Apparently, the variable (occupation) had a significant association with depressive symptoms, and this has slightly changed the regression (OR) results for some other variables.

b. Age is discussed as a variable of interest in the “Results” section but not in the “Measurements” section.

Measurement of age is included in the latest version of the manuscript
Data Analysis

1. The “Data Analysis” section is unclear regarding the exact analyses conducted. It would be helpful to clearly identify that the women were separated into “Depressed” and “Non-Depressed” dichotomous groups based upon their EPDS scores. Similarly, it is unclear if several simple or multiple logistic regressions were conducted. It would be helpful if the exact models used in the analyses were identified by all depended and independent variables included. Further, it may be helpful to include a table describing the results of the chi-square analyses.

In the data analysis section, we have stated that bivariate analysis was done to compare depressive symptoms among different groups using Chi-square test. In this revised version, we have included a table describing the results of the chi-square analysis separately (see table 2). The same table also shows the number of women by depression status (classified in to depressed and non-depressed) based on the EPDS cut off point of 13.

We used multivariate logistic regression analysis to identify factors independently associated with depressive symptoms in pregnancy. The multivariate model consisted of the following variables: women’s education, occupation, wealth index, perceived work burden, pregnancy intention, social support, food security status, history of miscarriage or still birth and presence of intimate partner physical violence during pregnancy. (see table 3 for results of binary logistic (unadjusted) and multivariate logistic regression (adjusted) analysis.

Results/Discussion

1. The results/discussion indicates that a relationship exists between the presence of partner violence and depression during pregnancy. It is necessary to clarify what “partner violence” includes as the term could refer a wide array of verbal and physical behaviors.

In this study, Partner Violence during pregnancy referred to intimate partner physical violence, which was measured by asking whether she has ever been beaten (pushed, hit,
slapped, kicked, choked, or physically hurt) during the current pregnancy. The concern raised by the reviewer is logical given the fact that partner violence can take different forms such as physical, sexual and psychological.

2. The study also identifies food security as one of the variables significantly associated with depressed mood during pregnancy. Although it is noted that food security varies greatly over the course of the year, it is not clearly stated when the data was collected from this region of Ethiopia.

   We have clarified (see methods section, paragraph 2) that data collection took place in the summer months of June and July, 2012. Moreover, it is stated in the discussion section that the summer months are times when more households/people become food insecure. We have also stated that food insecurity is a major problem in Ethiopia and in the study area in particular. A study conducted in the same area in 2008 (ref. # 20), also showed higher food insecurity.

3. A limitations section is needed in this paper. In this section, one of the limitations that should be addressed is the representativeness of this sample compared to other parts of Ethiopia. For example, should “rural” be in the title of this study?

   We have included a limitation section in this revised version. Moreover, we have modified the title of the manuscript, by including ‘rural’ in the title.

4. Lastly, the manuscript is poorly written. Specifically, it contains numerous grammatical, diction, and formatting issues that need to be addressed.

   Thank you for pointing out the grammatical and editorial errors we made. In the absence of budget for a language editing, we have read through the manuscript thoroughly and corrected the errors. Specifically, the third author, a native English speaker based in Johns Hopkins University (USA), has made a thorough edition of the manuscript. We feel that the quality of written English is very much improved in this revised version.