Author’s response to reviews

Title: Contextual-relationship and stress-related factors of postpartum depression symptoms in nulliparas: a prospective study from Ljubljana, Slovenia

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Author’s response to reviews:

Thank you for submitting your research manuscript to BMC Reproductive Health. We have received the reviews from the group of expert reviewers which are as follows.

Overall comments: This study brings to light the importance of measuring attachment styles and other contextual factors when screening women for postpartum depression. These factors can definitely impact women's depressive symptoms. Moreover, this study is conducted among a population where there is not much information available, thus increasing the knowledge in the field about the global prevalence of postpartum depression. Below are some comments to strengthen the study.

R: thank you for your comments. We followed all the suggestions, as much as possible. We discussed the models with a statistician, as advised and followed and included her answers accordingly.

Background: The authors do a great job describing PPD and how there are other factors that start during the antepartum period. However, it would be beneficial to organize the introduction around previous literature on how social, environmental, and personality factors have been shown to be associated with PPD, particularly in Slovenian populations. Moreover, more information as to why this study is based solely on first-time mothers would be helpful. Also, the last paragraph of the introduction section is too redundant with the study aim section that
follows. It is advised that authors keep one of the two paragraphs; either the last under the introduction section or the study aim section, but not both.

R: The studies about PPD in Slovenian population are scarce. The few studies that have been conducted, mostly evaluated the prevalence of PPD. A study on a sample of 449 new mothers found 21.3% of the new mothers had a score of 10 or higher on the EPDS. The estimated incidence of postpartum depression in Slovenia is 21% (Koprivnik & Plemenitaš, 2005). Few other studies that have been conducted analysing the peripartum mental health problems in Slovenia, mostly focused on evaluating the prevalence of anxiety and depressive symptoms in pregnancy (Podvornik, Velikonja, Praper 2015). Another study examined the postpartum depression symptoms frequency in the group of mothers of infants in the neonatal intensive care unit (NICU) at Pediatric Clinic of Ljubljana, the study is not relevant to compare with our study/population (Pucer & Kodrič 2014). To the best of our knowledge, no studies have analysed the social, environmental, and personality factors associated with PPD in Slovenian population.

We added more information about first-time mothers and the reasoning for conducting a study on nulliparas.

We deleted the study aim section and kept the last paragraph under the introduction section, as advised.

Methods: The authors do a good job describing the study sample and population of the study and the procedure of the study. However, this section can be greatly improved. First, the name and acronym of the tool used to measure depression was the Edinburgh Postnatal Depression Scale (EPDS). The sociodemographic and pregnancy information needs to be more thorough and include more information on level of emotional support received, particularly how it was measured (e.g. using a scale from 0 to 10 or was it a yes/no answer?) and also more information around "stress due to loss of employment/illness/financial problems" is required. It is not clear whether all these stressors were measured separately and the type of measurement used. The main weakness of the study is the the statistical analysis conducted.

R: We corrected the name and acronym EDS into Edinburgh Postnatal Depression Scale (EPDS).

We also added the information on measuring the emotional support and stress.

Constant (before and after giving birth) emotional support from partner, parents, friends and emotional support from co-workers during pregnancy was measured on the 5-point scale that was dichotomized prior to analysis into categories “weak to moderate support” including
answers “almost none” to “moderate support” and “strong support” including answers “strong” and “very strong support”.

Stress in the last year due to unemployment, family member or close friend’s illness, nulliparas’ physical or mental illness, financial problems was measured on the 5-point scale that was dichotomised prior the analysis (1 thru 3 = low stress; 4 thru 5 = high stress).

Analytic sample: although the authors claim that there were 181 women in the study, the final analysis only included 90 women. However, there is no discussion around missing data and it was treated. It seems the authors used listwise deletion in this study, so this information should be provided to the reader.

R: At baseline, there were 325 women included in the study, of which 181 women at least partially filled in the whole questionnaire. From these, 156 had complete data on the dependent variable (EPDS score). These were included in the statistical models. For this reason, the descriptive data is now provided for these women only.

Women that were included in the statistical analysis (n = 156) did not differ from women initially included in the study (n = 169) in any of the variables, but in positive experience of birth (p = 0,002) and baseline attachment anxiety (p = 0,012). Women with positive experience of birth and lower baseline attachment anxiety were less likely to drop out of the study.

From 156 women with complete data on EPDS, 90 women were included in the multivariable linear regression model due to missing data and listwise deletion. Women included in the regression model do not differ from the women not included in the model in any of the variables, but in experience of the stress due to their own illness. Women experiencing high stress due to their own illness in the last 12 months were less inclined to provide complete answers (p = 0.013).

Statistical analysis: in the first paragraph, the authors claim that "no multiple logistic regression was built"; however, it's not clear why that's the case, specially because they do run univariate logistic regression models. Thus, more information around the reasoning not to run multivariable logistic regression is necessary.
As there was a smaller number of nulliparas with afterbirth depression score above or equal to 10 points (n = 25), no multiple logistic regression model was built. Peduzzi et. al (1996) and Agresti (2007) claim there should be no fewer than 10 cases in the smaller category of the dependent variable per included independent variable to obtain valid and stable results of the logistic regression model. Therefore, with 27 cases in the smaller category (that is nulliparas with the score 10 or more on the EPDS at the end of the study), up to 2 independent variables could be included in the multivariable logistic regression model.

The authors also claim that a "multiple linear regression model was built with postpartum EDS as a dependent variable", but it seems that what the authors refer to is that a multivariable linear regression model was built using the EDS continuous variable. It is necessary to clarify these details in the manuscript.

R: [This was not clarified as it should be evident that linear regression model includes continuous dependent variable, but will be explained for clarification].

Multivariable linear regression model was built instead with the continuous after-birth Edinburgh depression score as dependent variable.

The models chosen for the study might not be the most appropriate ones and it is recommended that the authors discuss the most appropriate model with a statistician. Since this is a longitudinal study, the initial measurements need to be taken into consideration when running a regression model. The current model that is being used is usually used for cross-sectional studies.

R: Thank you for your comment. The logistic regression was rerun and adjusted odds ratios were calculated. The adjustment included the baseline dichotomous EPDS (threshold above or equal to 10). The main aim of the research is afterbirth depression and factors that are associated with it. We agree that the before birth depression should be taken into account when evaluating possible risk factors associated with the afterbirth depression.

We agree that other statistical models could also be considered with regards to the repeated measures design of the study. However, we are not interested in the depression in the pre and postpartum period and factors associated with it, but solely in the depression in the post-partum
period. GEE model would be appropriate in the first case, where the repeated measurement of depression, anxiety and attachment would be taken into account, but the results would indicate which factors are associated with the depression in the pre and postpartum period. This would not satisfactorily answer the aim of the current research, namely which factors are associated with depression in the postpartum period. The repeated measurement nature of the anxiety and attachment style could also be considered and included as such in the statistical model. However, as all these constructs are vulnerable to change after giving birth, it is important to find out whether the post-partum depression is associated with the attachment style or anxiety in prepartum or in postpartum period (or both).

As multivariable logistic analysis was not appropriate due to small number of nulliparas with postpartum depression, multivariable linear regression model was built to obtain an insight into the most important risk factors associated with postpartum depression, measured as score on EPDS.

Taking into account the aim of the current study and the restraints arising from the small number of nulliparas with postpartum depression, we believe that the statistical model was chosen appropriately.

Furthermore, given the model used, the results drawn from it need to be carefully discussed and the authors should avoid using predictive language (e.g. X predicted Y).

R: Thank you for your comment. We agree. The text was changed accordingly.

Moreover, more information needs to be provided around how many models were built and run and whether the authors used Bonferroni corrections to correct for multiple testing.

R: No adjustment for multiple testing was done, however, greater emphasis was put on the results of multivariable linear regression analysis.

Results: the authors do a good job providing information around the demographic characteristics of the participants. However, the way that the prevalence of PPD is reported is rather confusing
because the response numbers keep changing. For example, the authors mention that 181 women were included in the study, but then they give prevalence rates of (25/156) and (27/166). Thus, it is not clear for the reader how many people were included in the study and how much information was missing.

R: We corrected the prevalence rates of PPD according to the final number of participants (=156).

Results of univariate logistic regression and multiple regression analysis: this section was redundant because the authors first try to provide an overall picture of the findings and then they discuss them more in detail. One suggestion is to start the paragraph describing the results in depth. What were some of the associations found and their direction?

R: we reorganized the Results of univariate logistic regression and multiple regression analysis and started the paragraph as suggested. We also replaced the results with the new table (156 participants included).

Discussion: The authors do a good job relating their study back to previous literature and situating the findings of their study within the field. However, the summary of results is not completely accurate. The main weakness of the discussion section is that the authors make broad claims from univariate associations. However, the multivariable linear model demonstrated that many of the associations were not present in the full model; thus, they should not be discussed as the main findings of the study because in reality, there is no association, once other factors are controlled for. Overall, only education and anxiety after birth should be discussed as the findings of the study. Moreover, as previously stated, given the analysis, it is imperative that the authors refrain from causal language ("PPDS can be caused by multiple etiologic factors") since this study does not test causation. Discussion of results: This section is thorough and revises previous literature. However, it discusses univariate results as results obtained from the full model. This section should only reflect and discuss the results from the full model.

R: We corrected the Summary of results and the Discussion section according to the new statistical analysis.
Conclusions: this section should be heavily revised based on the previous comments. Also, the last paragraph of this section is confusing and should be explained more thoroughly and also why it is advised that risk/no risk of depression might be more important. Also, this section would probably be better after the implications section.

R: We revised the Conclusions section. We excluded/deleted the confusing sentence and moved the Conclusions section after the Implications section.

Strengths and limitations: the authors do a good job describing some of the limitations of the study. This section would benefit from addressing issues of data missingness and sample size as well. The sample size of the study was rather small and future studies should focus on trying to have larger sample sizes.

R: We addressed the issues of data missingness and sample size as suggested.

Implications: The authors do a good job describing some of the implications of the study. However, this section would greatly benefit from more clinical discussion of how the results can inform better clinical practices. Also, in this study the authors investigate an issue of importance, considering attachment styles and other contextual factors when screening women for depression, but the significant findings from the univariate analysis and then non-significant findings from the full model suggest that more research needs to be done in the area with larger samples.

R: We added suggestions for clinical practice and highlighted that more research needs to be done on larger samples.

Literature:


