**Author’s response to reviews**

**Title:** Antenatal depressive symptoms and early initiation of breastfeeding in association with exclusive breastfeeding 6 weeks postpartum: a longitudinal population-based study

**Authors:**

Karin Cato (karin.cato@kbh.uu.se)

Sara Sylvén (sara.sylven@gmail.com)

Marios Georgakis (mgeorgakis91@gmail.com)

Natasa Kollia (natkollia@yahoo.gr)

Christine Rubertsson (christine.rubertsson@kbh.uu.se)

Alkistis Skalkidou (alkistis.skalkidou@kbh.uu.se)

**Version:** 1  **Date:** 11 Nov 2018

**Author’s response to reviews:**

To: The Editorial Board

BMC Pregnancy & Childbirth

Dear Editor,

We thank you for the meticulous review of our original contribution/research article entitled “Antenatal depressive symptoms and early initiation of breastfeeding in association with exclusive breastfeeding 6 weeks postpartum: a population-based longitudinal cohort study” by Cato, Skalkidou et al., and for giving us the opportunity to revise the manuscript as to be considered for potential publication in BMC Pregnancy & Childbirth.

Please find below point-by-point responses to the comments raised by the two Reviewers:

Heather Sipsma (Reviewer #1):

The authors examine an important aspect of hospital care. The focus on this particular hospital practice provides an important direction for potential intervention that could benefit a substantial
proportion of new mothers and significantly affect young families. Overall, I thought the manuscript was well-written but thought that important details and rationales for the methodology were missing. I have listed my comments and concerns below.

We thank the Reviewer for the assessment of our manuscript. We reply below to all comments point-by-point.

1. The authors establish a strong rationale for their study in the background section of the manuscript, including emphasis on the recommended breastfeeding guidelines by the WHO and in Sweden and the importance of breastfeeding for mothers at high risk for depression. I recommend that the middle paragraph, however, focus on early initiation of breastfeeding and not on hospital practices in general. In the last year or so there has been conflicting evidence published on the Baby-Friendly practices in the U.S., and the author may not want to over-generalize, but focus their review of the evidence on early initiation, specifically.

We thank the Reviewer for this comment. As suggested, we have now removed the statements related to hospital practices in early breastfeeding initiation from the middle paragraph of Introduction.

2. In the methodology, it's not clear what the authors mean by "confidential personal data" as an exclusion criterion.

Thank you for this comment. We have now rephrased the description of this exclusion criterion to “protected identity” (page 4, line 83).

3. The authors should include a percentage for their analytic sample, particularly with respect to missing data. It's hard to figure out what percentage of data was valid (non-missing); if a substantial proportion of the data are missing, a kind of attrition analysis would be warranted.

Thank you for pointing this out. Please see Figure 1 for a detailed flowchart of study participants. Unfortunately, we could not perform an attrition analysis on the prevalence of depressive symptoms, as we had no such data on the women who did not participate in the study. However, the prevalence of perinatal depression in the BASIC study is very close to that of other studies, which makes us believe that the attrition might not add significant bias in our analyses. We now have clarified this issue and expanded on it in the limitations paragraph of our manuscript (page 9, lines 237-239).
4. Why did the authors choose 6 weeks postpartum when they (theoretically) had data for up to 6 months in the BASIC study, which aligns with the recommendations of the WHO? It would be helpful if this rationale was included in the manuscript.

We agree with the Reviewer that this is an important point. Following the Reviewer’s comment, we have now clarified in the introduction why we chose 6 weeks postpartum as the endpoint of our analyses, as follows:

“…although they include an amendment declaring that the introduction of “tiny sensations” of solid food from the age of four months is harmless if it does not affect continuous breastfeeding” (page 3, lines 50-51)

and

“The aim of this study was to assess the interplay between antenatal depressive symptoms and early initiation of breastfeeding on exclusive breastfeeding at 6 weeks postpartum, when there is no obvious reason for introducing other foods or drinks, in a population-based cohort of Swedish pregnant women.” (page 4, lines 72-73)

5. Univariate analyses involve only one variable (uni-); if the authors are examining associations between factors and breastfeeding (which involve more than one variable), this should be named as "bivariate analyses."

We have now changed the word “univariate” to “crude” throughout the manuscript to make our approach clearer.

6. Why did the authors choose to focus on 2 hours or less for breastfeeding initiation when the recommendation is that initiation should occur within one hour?

Thank you for pointing out this important aspect of our methodology. According to the updated version of ‘Ten steps to successful breastfeeding 2018’ the recommendation is still that breastfeeding should be initiated within the first hour after birth, but it is also pointed out that waiting to initiate breastfeeding beyond 2 hours might be harmful. The 4th step (of the 10) reads that breastfeeding should be initiated as soon as possible after birth, which we now have changed in the Introduction (please see page 3, line 61) and updated in the Reference list (Reference #3).

7. How did the authors decide which variables to include in their original/initial multivariable model as main effects? What was the unadjusted or bivariate analysis used for?
Thank you for this comment and we apologize if we were not sufficiently clear regarding the process of covariate selection for our multivariable analyses. To decide which variables to include in the initial multivariable model we used a conceptual model constructed based on the current evidence (please see the DAG in Supplementary Figure). The unadjusted crude analysis was used to present comparisons across groups and confirm the validity of our a priori defined conceptual model. We have now described in more detail the two models (please see page 6, lines 130-136).

8. Did the authors check for multicollinearity among covariates? For instance, is history of depression over-correlated with depressive symptoms during pregnancy? Was depressive symptoms in pregnancy correlated with delayed breastfeeding initiation?

We thank the Reviewer for raising this issue. Indeed, we did check our variables for multicollinearity and there seemed to be no such issue. Following the Reviewer’s suggestions, we have now added a relevant comment in the Statistical analysis paragraph in page 6, lines 129-130.

9. The description of the DAG was a bit confusing to me as I felt it lacked important detail in the analysis section. For instance, it's not clear to me what Model 2 shows - how did the authors examine mediation? What was the hypothesis here?

Thank you for this comment. Based on the predefined conceptual model (presented in the DAG), Model 1 examines the total effect of early breastfeeding initiation on breastfeeding at six weeks postpartum (direct effect plus the effect mediated through the examined mediators), whereas Model 2 examines only the direct effect of early breastfeeding initiation on breastfeeding six weeks postpartum (thus the effect that is not mediated through the mediators presented in the DAG). We have now clarified this in more detail in the manuscript (please see page 6, lines 132-136).

10. There is a typo in (b), line 36 on page 6 - should be after 2 hours, I believe.

Thank you. This has now been corrected.

11. Why did the authors choose to stratify by delivery mode? This rationale is unclear.

Given the known impact of delivery mode on the timing of breastfeeding initiation, we performed this analysis to examine if early initiation of breastfeeding significantly interacted
12. What was the retention rate from pregnancy through 6 weeks postpartum? This should be made clearer in the manuscript.

For this substudy, we have included only women with data on breastfeeding at 6 weeks postpartum. In the whole of the BASIC project, 81.3% of those giving consent complete the questionnaire in gestational week 32 and 80.9% at 6 weeks postpartum. This is now included in the methods section (page 4, lines 87-88).

13. A final overall comment might be that although the authors present their findings cautiously, avoiding using casual language, it might be appropriate for them to mention that delayed initiation might be a result of lower or weaker commitment to breastfeeding, which is also reflected in not exclusively breastfeeding at 6 weeks. So the delayed initiation may be a proxy for maternal attitude towards breastfeeding.

Thank you for this comment. We have now incorporated it in the Strengths and limitations section of our manuscript (page 10, line 229-231).

14. All references should be properly formatted, particularly the references that are not journal articles.

Thank you for this valuable comment, we have now corrected the reference list.

Parvin Abedi, PhD (Reviewer 2):

1. Method: what is the difference between two following groups: women with no depressive symptoms during pregnancy who initiated breastfeeding within 2 hours after birth (set as the reference category), (b) women with no depressive symptoms during pregnancy who initiated breastfeeding within 2 hours after birth
We thank the Reviewer for the assessment of our manuscript. Regarding the first comment, we apologize for this error. We have now corrected the description of the (b) group (page 6, lines 145-146).

2. Method: please add some information about the EPDS. How you scored this questionnaire and also validity and reliability of this questionnaire in your country.

We agree with the Reviewer that the EPDS is an important methodological aspect of our approach and should be described in more detail. To address the Reviewer’s comment, we have now added in the Methods section information on the sensitivity and specificity of EPDS, as derived from the references by Rubertsson and Wickberg, which represent Swedish validations of the scale for prenatal and postnatal depression, respectively (page 5, lines 118-122).

3. The hands-on approach needs to be explain in the method section.

Thank you for pointing this out. Accordingly, we have now added a more detailed description of the hands-on approach in the Methods section (page 5, lines 114-115).

4. I would like to see the power of study for this sample size.

Thank you for raising this issue. As recommended, we performed a power calculation based on our sample size, which showed that we were sufficiently powered (1-β>80%) to detect as statistically significant (α=0.05) a minimum OR of 1.59, as an association estimate between breastfeeding initiation later than 2 hours after birth and not exclusively breastfeeding at 6 weeks postpartum. This has now also been added in the Results of our manuscript (page 9, lines 208-211).

5. Assessing the exclusive breastfeeding only 6 weeks postpartum cannot predict the exclusive breastfeeding in the 6 months. I understand that authors wanted to consider the weeks after postpartum the postpartum depression might happen more (6 weeks), but we are concerning about 6 months exclusive breastfeeding.

We share the Reviewer’s concern regarding exclusive breastfeeding at six months. To address this comment, we have now tried to clarify in more detail in the Introduction (please see page 3, lines 50-51 and page 4, lines 72-73) the reasoning behind the use of breastfeeding at 6 weeks instead of 6 months postpartum as an endpoint of our study.
6. Results: table 1: we would like to see the mean (SD) of continuous variables such as age and BMI.

As the length of Table 1 is already quite extensive, in order to address the Reviewer’s comment, we have now added the means and SDs of the continuous variables in the text of the Results section (please see page 7, lines 153-158).