Reviewer’s report

Title: The prevalence and correlates of suicidal ideation identified by the Edinburgh Postnatal Depression Scale in postpartum women in primary care

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Reviewer: Cindy-Lee Dennis

Reviewer’s report:

Primary reviewer: Dr. Simone Vigod  
Secondary reviewer: Dr. Cindy-Lee Dennis

Manuscript title: The prevalence and correlates of suicidal ideation identified by the EPDS in postpartum women in primary care.

General Comments: The topic of suicidality among women in the postpartum period of great significance. However, there are some concerns about the methodology of the current paper as it is currently presented that limit its ability to extend the literature in this area.

Suggested Major Compulsory Revisions:

1. Title: The title does not reflect either the study design, or the fact that the correlates of suicidal ideation are really only assessed in the subset of women who were identified as being high risk for depression (as per EPDS > 12).

2. Abstract: The results section also does not reflect that the correlates of suicidal ideation were not measured in the whole sample. Also, it does not reflect the fact that 18 week outcomes were measured in women who were being treated for depression in an RCT. The last line of the results section is unclear: It would provide clarity to say that they did not find any association between reported suicidal ideation at the time of a baseline home visit and outcomes 18 weeks later (SF-12, EPDS) in women who were being treated for depression. Likewise, the conclusion that suicidal ideation (SI) does not appear to predict poor outcomes is really only applicable to women being treated for depression.

3. Background:

a. Overall, the background section does not provide a rationale for what the current study will add to the literature. The authors have numerous objectives and there should be a rationale for each one. Please provide in the background section a rationale for each objective and what previous work has been completed. Further it would be helpful to provide a couple of sentences what this study adds to the literature (i.e. measures it in a more valid way, a more representative population, etc…).

b. There are no pre-specified hypotheses accompanying the objectives. It would
be useful to see hypotheses (which follow from a more extensive literature review that outlines the rationale for the objectives)

c. Later in the paper, the authors are “validating” the EPDS item 10 against the CIS-R, yet this is not one of the specified objectives. If validation is one of the objectives of the paper, it should be added.

4. Methods: Overall, it would be helpful to present key elements of the study design up front. This appears to be a prospective cohort study nested within a randomized controlled trial. Specifically, a flow-chart would be helpful (with Ns) to illustrate who is in the cohort for which measurements. There are, I believe, 3 overlapping groups of women: 1) community based sample who received a screening EPDS; 2) women who scored > 11 on the screening EPDS and received a home visit where they completed a baseline EPDS (then I think EPDS at follow-up at 4 and 18 weeks by mail); and 3) women from group 2 who scored >12 on the baseline home visit EPDS and who therefore were also asked to complete the SF-12 and the CIS-R at the baseline visit (then I think EPDS and SF-12 by mail at 4 and 18 weeks). This is quite confusing and limits the reader’s ability to understand the implications of the findings:

Study Population: It is important that women receiving treatment for depression at recruitment were NOT included in the study. Were these women included in the original screening? This is not clear. If women receiving treatment at screening were not included in the study then this should also be discussed in the discussion section.

Recruitment procedure and follow-up visits: Either here (or at the beginning of the results section) is where a flow-chart is needed to help the reader understand:

Women sent postal questionnaire (N=?)# Women returning postal questionnaire (N=) # women with EPDS > 11 (N=?) and therefore eligible for entry into study # Women who entered RCT (N=? ) # women who received baseline home visit (N=EPDS only, N=EPDS plus SF-12 and CIS-R)# women who did follow-up measurements at 4 and 18 weeks (N=EPDS only, N=EPDS, SF-12 and CIS-R). ALSO, within this flow-chart, it would be helpful to include when the RCT intervention began and ended.

5. Statistical analysis:

a. The samples in which the different analyses are being done needs to be put forward. For example, in sentence 3: In women with a screening EPDS >12, we investigated the persistence of SI by …..

b. To assess the agreement between the CIS-R and the EPDS, perhaps a statistical measure of agreement should be used (like kappa). Otherwise, you are not really “testing” agreement, you are just describing the concordance between the measures in a table.

6. Results: I would like to see some baseline characteristics of each of the
samples – screened, baseline who were included in the risk factor prediction model, women who were included in the EPDS item 10 validation (including mean EPDS scores).

a. Paragraph 1, sentences 3 and 4. It should be clarified that this applies to the group of women with screening EPDS >11 who entered the trial.

b. Paragraph 2: Please clarify that this was measured in women who score > 12 on the EPDS at the baseline visit only (because these were the only women who were given the CIS-R, correct?). Also, you should highlight the really important finding in the text that all women who reported significant SI on the EPDS (6/6) reported > 2/4 on the CIS-R.

c. Paragraph 3: Please clarify that this was also measured in women who scored > 12 on a screening EPDS.

7. Discussion:

Main findings:

a. Sentences 3 and 4: You describe the concordance of the EPDS item 10 and the CIS-R, but you did not show “associations” or “agreement”. Please change the language to reflect this.

b. Sentence 5 should be preceded by the caveat: In women with screening EPDS > 11…. 

c. Sentence 6 should be preceded by the caveat: In women treated for depression in the RESPOND trial (either by medication or psychotherapy), SI at baseline was not associated with…. 

Strengths and Limitations:

a. Sentence 1: you did not really “validate” this as discussed

b. Please explain why SI endorsement on self-rated scales being higher is a limitation (in fact, it might more accurately reflect the truth).

c. It is true that it is a limitation that this study did not measure the relationship between SI and attempt/completed suicide, but the study did not set out to do this. If you are trying to justify why it is important to measure SI (even though the study you cited found that despite high levels of suicidality only resulted in 1 attempted suicide), then this should probably be done more clearly in the introductory section. In addition, the clinical significance of SI may not be clear, but your study DID find that SI at baseline did not predict poor outcomes in women undergoing treatment. To me, this might imply that we should treat women with SI because they have good outcomes when treated (although of course you have no comparison group of untreated women).

Implications:
a.2nd sentence: please compute a kappa or chi-square statistic if you are going to use the word associated.

b.2nd sentence from the end: “suicidal ideation does not appear to predict poor outcomes… please add: in women who were treated for depression.

c. There is no section discussing the implications of and the potential mechanisms behind your findings in the context of existing literature. There is no discussion of the generalizability of the findings (and the limits of the external validity of this study).

Suggested Minor Revisions

1. Background:

   a. Para 1, 3rd sentence. I believe that the authors are trying to explain that studies have shown that pregnant/postpartum women are less likely to commit suicide than their non-perinatal counterparts. However, the issue is that these are two very different populations (i.e. people at extremely high risk for suicide are less likely to be having children).

   b. Para 1, 4th sentence. Is the issue that the relationship between SI and completed suicide has not been clarified (in which case a summary of the existing literature should be presented) or that it has not been studied? I don’t disagree that SI may be a marker of increased risk for suicide, but has this ever been looked at in a perinatal population? If not, please cite the appropriate general population reference.

2. Statistical Analysis:

   a. Please confirm when you say “baseline”, that you mean the “baseline home visit”, not the screening questionnaire.

3. Results:

   a. Paragraph 1, sentence 1: What is “time-2”, is that the screening questionnaire? “time -2” has not been previously mentioned.

   b. Table 1 should have total Ns for each group and the table requires a legend for abbreviations (EPDS, SI) and to define the timing (i.e. time 0 = time of baseline home visit, etc…)

   c. Paragraph 2: Sentences 2 and 4 do not reflect what the authors are trying to say. I think that what they are trying to say is: “Almost all (99%) of women who reported “never/hardly ever having suicidal thoughts” on the EPDS scored < 2/4 on the CIS-R. However, 75% of women who reported “sometimes” having suicidal thoughts on the EPDS also scored < 2/4 on the CIS-R.

   d. Paragraph 3: I assume that SI at baseline means baseline home visit. Also, the text indicates that the EPDS is associated with SI in multivariate analysis. However, there is no EPDS in Table 3.
Table 3: What is the EQ-5D? Other abbreviations also need to be defined in footnotes

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

We declare that I have no competing interests - Simone Vigod and Cindy-Lee Dennis