Reviewer's report

Title: Association of Decreased Early Pregnancy Serum Brain-Derived Neurotrophic Factor (BDNF) Concentrations with Antepartum Depression

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Reviewer: Bianca Pfaffenseller

Reviewer's report:

In this study, authors examined serum BDNF levels among pregnant Peruvian women and the possible association of this biological parameter with antepartum depression. The present work is partially in agreement with previously published studies by other groups assessing the relationship between BDNF levels and depression among men and non-pregnant women (or in postpartum period). It expands on with the originality of focusing in early pregnancy and antepartum depression in a considerable large sample size. The aim of the study is clear and well defined, the methods (experiments and analysis) appear to be adequately performed, and some results are presented suggesting that lower serum BDNF concentrations in early pregnancy are associated with antepartum depression.

The study is well conducted and contains new and original material and should be published after Minor Essential Revisions listed below.

(1) The title and abstract convey the findings, but some suggestions and/or alterations are proposed. In the Title, the sentence “Decreased early pregnancy serum brain-derived neurotrophic factor (BDNF) concentrations” could sound better in a more fragmented way such as “Decreased serum brain-derived neurotrophic factor (BDNF) concentrations in early pregnancy”.

(2) In Key words, some words (e.g., antepartum depression) could be replaced by others similar that are not in the title, to increase the options in indexing and help this research to be found by interested groups.

(3) The word “concentrations” could be replaced by “levels” to avoid repetitions (through all the manuscript).

(4) In the Abstract, please correct the value of the lowest three quartiles (<=25.31 instead of <17.32).

(5) In the Abstract, please confirm that the enzyme-linked immunosorbent assay (ELISA) performed was competitive type. Usually, ELISA to measure BDNF is sandwich type.

(6) In the ending of abstract, it is said that the findings may point toward improved biomarkers for risk prediction, leading to understand that there are already existing biomarkers to predict the risk of antepartum depression. What are they? This should be cited in the abstract and discussed in the manuscript.

(7) Methods could be more described regarding sample processing and BDNF measurement, with a separated sub-section. I suggest you describe the BDNF
assay briefly because this could be good for the reader and to compare the experiments with other studies. Please confirm again if the assay performed was a competitive or sandwich ELISA.

(8) It is not necessary to protect the blood samples from ultraviolet light for this analysis. If you have a reason for that, please explain it.

(9) The blood withdrawal and the application of the PHQ-9 scale were performed in the same day? Please, let this information more clear considering the relevance to know if the depressive symptoms and BDNF levels evaluated correspond to the same moment.

(10) Besides p values, it would be important to show the effect-sizes of the analysis for a better interpretation of the findings.

(11) The criteria for recruiting participants are not well clear about inclusion/exclusion of women with mood disorders or psychiatric history. Please, detail this information.

(12) In Table 2, please explain that the values between parentheses refer to PHQ-9 scale. In the same table, considering the data are non-parametric, why the mean is also presented?

(13) Some corrections should be made in some references in way they are cited through the manuscript (numerical or author/year system), e.g. second sentence and in the ending of the Introduction; in the ending of sub-section Antepartum Depression of Methods; second and last paragraph of Discussion. Please, review the cited references in the manuscript.

(14) In the second sentence of the Introduction: the acronym “LAMICs” is not necessary because it does not appear in the text again.

(15) It is missing comma: in Methods, Analytical Population section, after “During this period”; in Discussion, second paragraph, after “Satomura et al”; in Discussion, third paragraph, after “Pinheiro et al”.

(16) In Results, cite the Supplement Table 1 to explain that women included and that excluded from analysis did not differ in sociodemographic and lifestyle characteristics.

(17) The last sentence of Results is confused (“women compared with BDNF”). Please, rewrite it.

(18) The discussion contemplates the objectives and discussion about the results; however it could be more concise. Data from other cited studies (e.g. serum BDNF concentrations) do not necessarily have to be showed. The limitations of the study are clearly stated, mainly regarding the design of the study (cross sectional), which does not allow an analysis of the temporal relationship between serum BDNF levels and the onset of antepartum depression. It is important to note this limitation for the interpretation of the data.

(19) In cases of depressed pregnant women, it would be interesting to measure serum BDNF levels before and after pregnancy or/and before and after treatment to better understand the relationship between this neurotrophin, pregnancy and treatment. In addition to BDNF, other biological parameters were measured in
the serum of participants, or there is some perspective in this way? This should
be either more argued.

(20) Please review the fourth and sixth paragraph of the Discussion regarding
some grammatical errors.

(21) In the sixth paragraph of the Discussion, it is said that the study population is
antidepressant-naïve. Emphasize this information earlier, in Methods.

(22) In the ending of Discussion, this study suggests that serum BDNF
concentrations may be used as a biological marker for monitoring treatment
responses, however it is not cited the possibility the BDNF become also a marker
to predict risk for antepartum depression, as mentioned in the Abstract. Please,
include it in conclusions.

(23) The writing is acceptable, however some language corrections are
recommended before being published. It is not clear the acknowledgment of
each author related with any work upon which they are building. Please,
complement this data as usually requested by periodic.

The article is brief and the conclusions of the authors are adequately supported
by the data of this work, presenting clinical relevance for this area.

Level of interest: An article whose findings are important to those with closely
related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the
statistics.

Declaration of competing interests:

I declare that I have no competing interests.