Reviewer’s report

Title: Trajectory of vitamin D status during pregnancy in relation to neonatal birth size and fetal survival: a prospective cohort study

Version: 1  Date: 18 Apr 2017

Reviewer: Anne Marie Zaura Jukic

Reviewer's report:

I appreciate the authors' responses and I have only a couple of residual questions. I've included my original comment and the authors' response for clarity.

Abstract

1) Provide units for the T1 25OHD and pregnancy loss association. The OR of 0.99 is for what amount of increase in 25OHD?

This has been clarified by adding the word continuous to the sentence (page 2, line 37).

I don't think this answers the question? The OR is comparing one level of 25(OH)D to another, even when fit continuously, thus the comparison might be for a 1 ng/ml or 1 nmol/l increase in 25(OH)D. Can the authors clarify that the OR is for a 1 nmol/l increase in 25(OH)D?

2) Consider adding gestational age range of pregnancy losses in lines 34-35.

We would like to accommodate this suggestion, but this information is unfortunately not available. Pregnancy loss is not determined by antenatal care but by the emergency gynecology ward and these medical records were not included in the ethical approval for the study. Data on pregnancy loss was based on self-report information by the women and gestational duration at fetal demise was not always provided. We have tried to clarify this in the paper.

I'm sorry, I'm confused - in a later comment the authors state that they have the LMP date for the losses (see comment 6 of the review response)?

13) Line 114. The third trimester blood sample was drawn after gestational week 31, were any preterm births delivered prior to week 31?

Yes, there were a few (N=10) who delivered before the second blood sample could be drawn.
This has been added to the results section (page 9, lines 181-182).

It would be interesting to mention of these 10 pregnancies also had low 25(OH)D at T1.

14) Would the authors explain the creation of the "season of conception" variable? It only includes two categories and appears to combine spring with winter and summer with fall? Rather than isolating summer (April to September)? (For example, see Lundqvist et al. PLoS One. 2016; 11(3), Vitamin D status during pregnancy: A longitudinal study in Swedish women from early pregnancy to seven months postpartum.)

Season was defined by the months that UVB-exposure is sufficient for endogenous 25OHD production in Sweden. This definition is also based on previous data from the current latitude in Sweden (Brembeck P. Determinants of vitamin D status in pregnant fair-skinned women in Sweden. Br J Nutr. 2013;110(5):856-64). This definition of season previously captured almost 30% of the variation in 25OHD concentration, among pregnant women on the current latitude (57-58°N) in Sweden. The study by Lundqvist et al was conducted on a higher latitude (64°N) which might warrant a different definition.

Would the authors add this information and the reference to the paper?

15) Lines 143 to 146. Please clarify the referent groups for each outcome. For example, was SGA compared with AGA alone or AGA and LGA combined?

This has been clarified (page 7, lines 150-152).

I'm sorry, I asked this because I believe there may be evidence of higher risk of LGA for deficient mothers. (For example, see Morales et al. Deficit of vitamin D in pregnancy and growth and overweight in the offspring. International Journal of Obesity (2015) 39, 61-68; doi:10.1038/ijo.2014.165). Do the authors think this is possible, and have they looked at this in their data?
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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