Reviewer’s report

Title: The relationship between low maternal serum vitamin D levels and glycemic control in gestational diabetes mellitus assessed by HbA1c levels

Version: 1

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Reviewer: GV V Krishnaveni

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This is an important study considering a widely prevalent low vitamin D levels during pregnancy in the world and escalating levels of gestational glucose intolerance and its related consequences. In the backdrop of reporting of inconsistent associations a between gestational diabetes and vitamin D status, this paper may be an addition to the existing literature. However, the authors need to address several important issues:

Major compulsory revisions.

1. The authors present a detailed Introduction. It can be shortened to make it more succinct. Eg. Paragraph on vitamin D absorption/ conversion in the body can be cut short. If required, a brief reference to this can be made in the Discussion.

2. Introduction section does not give a strong rationale for their study, or it is not laid out clearly. Add a sentence in the end to categorically state the purpose of doing this study and how does this add to the existing literature.

3. The authors should give more details as to the method of selecting cases and controls. How many women were approached, how many agreed to participate, what were the eligibility criteria to determine the cases (eg. What test was used to diagnose GDM) need to be given. In page 8, lines 136-137 give the impression that of the 160 women selected, 80 each happened to be GDM and control women, which is not correct. The correct statement would be that 80 GDM women were recruited and 80 controls were selected subsequently. Details on how controls were selected should also be given.

4. Mean 25(OH)D levels were similar in Controls and GDM, suggesting no association between GDM and vitamin D status. It is not clear if the analysis (of inverse association between glycaemic indicators and vitamin D3 levels) takes into account both the GDM and the control women. I don’t understand the idea behind recruiting a group of control women too, when the current paper describes associations only in GDM women. Was there a significant difference in the prevalence of vitamin D deficiency between the two groups? The authors can make an effective use of the control group also to see if there was an association within the normal range of glycaemia.

5. The analysis models do not include any confounders such as socio-economic status, seasonality of testing, diet etc. which may have major influence on the
associations observed. The authors at least state that this is a limitation of the study.

6. The authors should discuss the reasons for high prevalence of vitamin deficiency in the region.

7. The discussion section should include mechanisms of the observed associations, direction of causality etc. As the vitamin D deficiency was higher in women with poorer metabolic control, the possibility of reverse causality should be discussed.

8. Background section, line 95, please reference the sentence “Vitamin D deficiency may increase the risk of chronic diseases”

9. Please use terms diabetic women or pregnant women etc. instead of diabetics, non-pregnant.....

10. Page 14, lines 257-260, the meaning of this sentence is not clear. Please rephrase.

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

**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