Reviewer's report

Title: Does vitamin D3 play a significant role in type 2 diabetes?

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Reviewer: Ambika Ashraf

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The manuscript by Sheth et al includes a large number of subjects with T2DM from western India. It is well written. I have the following comments.

The methodology used to measure 25-hydroxy vitamin D (25OHD) assay is ELISA which is inferior to LCMS/MS and hence may not be reflective of both 25-hydroxy vitamin D2 and D3 which constitute the total concentration of 25-hydroxy vitamin D, which indicates vitamin D status. This has to be mentioned under the limitation section.

The interpretation of the study results and conclusion appears to be weak (vide infra) - Table 1.

Abstract:

1. Background: Instead of ‘found to be associated’, it will be better to use ‘reportedly is associated’.
2. Type 2 DM is represented as T2DM and T2D. Authors need to be consistent in the abstract and throughout the manuscript.
3. Methods: explain F1, PPBS as they are being used for the first time.
4. Clarify ‘vitamin D3 level’- are the authors measuring 25-hydroxy vitamin D3 or 1, 25 di hydroxy vitamin D3.
5. Results: vitamin D3 deficiency: what does that mean- is it based on vitamin D status indicator which is 25- hydroxy vitamin D.
6. Results: clarify the sentence, “There was no association of its deficiency on HbA1C of …..” Do the authors mean “There was no association between serum 25-hydroxy vitamin D and HOMA IR or HbA1C?
7. Conclusion: ….deficiency of vitamin D3 is incorrect. It is either vitamin D deficiency or low serum level of 25-hydroxy vitamin D3.

Main manuscript:

1. Background: lines 7 and 8: Even though the intake of cholecalciferol (D3) is reportedly superior than intake of D2, when discussing vitamin D deficiency, it is incorrect to state vitamin D3 deficiency. It is vitamin D deficiency. Both D3 and D2 have contributions to the vitamin D status.
2. Background: lines 11, 16 and 20: it is 25- hydroxy vitamin D (the term vitamin D is loosely used in the manuscript. Whenever serum concentration is mentioned it has to be 25- hydroxy vitamin D), as that indicates vitamin D status.

4. Subjects: study design: is it prospective or cross sectional.

5. Page 4, lines 6, 8, 18, 25, 33: clarify ‘vitamin D3 levels’?

6. Page 5, lines 12: clarify ‘vitamin D3 deficiency’?

7. Page 5, line 15: groups division could be 25OHD3 <20 ng/ml/ >20 mg/ml instead of using the confusing terminology vitamin D3 deficient

8. Table 1: instead of using column title as ‘vitamin D3 non- deficient/ vitamin D3 deficient’, may use ‘serum 25OHD3 >20 ng/ml/ 25OHD3 <20 ng/ml’.

9. Table 1: The subjects with T2DM who had 25OHD3 >20 ng/ml, had lower HOMA-IR and lower fasting insulin compared to those with 25OHD3 <20 ng/ml. This is also observed in the control group indicating a role of vitamin D in insulin resistance. As the early pathophysiology of T2DM involves insulin resistance this finding may be of significance.

10. Page 5, line 20--, under results (and based on table 1): Subjects with poorly controlled T2DM of >8 years duration, will have poor beta cell reserve. Hence it will be difficult to find any association between HbA1C and 25OHD levels. Did the authors adjust for duration of diabetes? This could be also commented under discussion page 7.

11. Is the association between 25OHD levels and HOMA IR adjusted for duration of diabetes and HbA1C?

12. Need to include a limitation section: cross sectional study design, use of ELISA which does not measure 25OHD2 and D3 concentrations, inclusion of subjects with poorly controlled T2DM of long standing duration etc.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests