Author's response to reviews

Title: Prevalence and correlates of vitamin D status in African American men

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Author's response to reviews: see over
June 14, 2009

Dear Sir/Madam:

Enclosed please find a revised copy of our manuscript titled, ‘Prevalence and correlates of vitamin D status in African American men’ (9044962362503751). We have made the following revisions, as requested:

1. revised author names to be identical to the list in the submission system.
2. revised Table 2 (and entire document) to fit into an A4 portrait page.
3. added superscripts for each author to link to affiliation.
4. reformatted author email addresses
5. added country to author affiliations
6. removed abbreviated title, key words, and word count from title page
7. renamed ‘Introduction’ to ‘Background’
8. renamed ‘Materials and Methods’ to ‘Methods’
9. formatted abbreviations as paragraph rather than as list
10. removed text ‘table 1 continued’ and repeated column titles for continuing pages of table 1
11. removed blank page 20

I would also like to point out my new affiliation, effective July 1, 2009: Department of Kinesiology, California Polytechnic State University, San Luis Obispo, CA 93407, USA. If you should need further information, please contact me at my new email address mtseng@calpoly.edu.

Thank you.

Very sincerely,

Marilyn Tseng, Ph.D.
Associate Member
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Referee 1

**Major comments**

1. **A principal problem with this paper, is that the authors did not specify whether they had used continuous external quality assessment of the 25-(OH)D assays (DEQAS) for assay vitamin D status. The external control permit to test the validity of results and comparisons of the results obtained in the different laboratories.**

   Our laboratory assays had good reproducibility, supporting at least the internal validity of our results (p.5 paragr.1), and our results are consistent with the relatively low levels found in other samples (p.8 paragr.1). Nevertheless, we appreciate the reviewer’s comment: We did not use an external control and agree that comparison with studies that used other laboratories is difficult; we now note this in the text (p.8 paragr.1).

2. **Discussion: It is really the impact of physical activity on vitamin D status that is highlighted? Why the authors did not consider the sun exposure as the factor that contributing to the positive effect of physical activity? There is no direct link between a physical activity practices indoors, and 25-(OH)D levels. The authors should not conclude by recommending increased physical activity without specifying whether it is an activity inside or outside.**

   We agree that a direct link between physical activity (not specific to outdoor activity) and 25(OH)D levels is tenuous and have removed the statement from our abstract and concluding paragraph (p.10).

3. **Abstract and discussion: There is a discrepancy between the conclusion. In the abstract the authors talk about a bias of recruitment as regard to the BMI of the subjects (season of collecting sample) whereas in the discussion, the conclusion is more focused on the lack of association between BMI and vitamin D status. The discussion is too focused on the role of adiposity on vitamin D status.**

   We have substantially reduced our discussion of adiposity in relation to vitamin D status and believe that our discussion of BMI and vitamin D status is now more consistent between abstract and discussion (p.9 paragr.2).

**Minor essential comments**

1. **Statistical analysis: What software was used for statistical analysis?**

   We have included a reference to SAS as the software used for these analyses (p.6 paragr.2).

2. **Tables**
   a. **Table 1: 25-(OH)D mean± SD or SE, must be indicated on the table. Significant differences are calculated compared to which group?**

      We have revised Table 1 to clarify that SDs are presented along with means. We have also revised the footnote to clarify that categories with different superscripts are statistically significantly different from each other.

   b. **Table 2: R2 regression coefficient should be indicated in the table.**

      These have now been added.

3. **The discussion is too long and should be reduced by at least 33%-50%.**

   We have substantially shortened our discussion from 4.5 pages to <3 pages.

Referee 2
Discretionary revision

The most important public health question is – what is the relevance of these low levels on the health of the subjects? The authors suggest that African Americans are less prone to osteomalacia (page 12) – I am not sure that this is true, but they are less prone to osteoporosis (ref 2). Thus what are the deleterious non-skeletal effects of vitamin D insufficiency on African Americans and if there is good evidence for these (most of the suggested non-skeletal effects of vitamin D insufficiency have been based on epidemiological studies rather than trials), how should the problem of vitamin D insufficiency be addressed? Is it appropriate to supplement all African American males or does one target those most at risk of vitamin D insufficiency and who are these? What about food fortification?

We have replaced osteomalacia with osteoporosis and have also included more specific references from epidemiologic studies demonstrating deleterious non-skeletal effects of vitamin D insufficiency in African Americans (p.10 paragr.2). With respect to addressing the problem of vitamin D insufficiency, while fortification with vitamin D remains debatable (O’Donnell S et al. Efficacy of food fortification on serum 25-hydroxyvitamin D concentrations: systematic review. Am J Clin Nutr 88:1528-34, 2008), we have revised our abstract and conclusions to emphasize recommendations targeting African Americans for supplementation, which we believe is the most reasonable approach.

Minor essential revision:

It is unclear from the description of the methodology if blood sampling for 25-OHD was done at the same time as the anthropometric and dietary assessment. It appears that 25-OHD was only done in those who had blood taken after July 2000, yet enrolment started in 1996. if they were not done simultaneously, the findings might be more prone to error.

We appreciate the referee’s comment and concern and have added a discussion of this on p.9 paragr.3.

Referee 3

Major compulsory revisions:

1. In relation to the results and statistical analysis, I have a number of comments:
   a. What is the distribution of the 25(OH)D data? The authors should include this in the statistical analyses section.
      The distribution of 25(OH)D is presented in the first paragraph of the results section, and in Table 1.
   b. The post-hoc test(s) for the ANOVA tests used in Table 1 needs to be specified.
      We have revised the text in Table 1 footnote 2 to clarify that least square means were used in post-hoc ANOVA testing.
   c. In addition, some variables are missing superscripts for the ANOVA tests.
      We have added superscripts indicating categories that were not statistically different from each other.
   d. Which statistical test was used to compare the % of subjects with 25(OH)D levels < and >=15 ng/ml between sociodemographic, lifestyle and dietary variables on Table 1?
We have added to the footnote that the Cochran-Mantel Haenszel test was used to compare proportions of subjects with 25(OH)D levels <15 ng/mL across categories.

2. I think the discussion could be presented in a more succinct form.
   As discussed for Referee 1, minor comment 3 above, we have substantially shortened our discussion from 4.5 to <3 pages.

Minor Essential revisions
1. To ensure consistency with the way 25-hydroxyvitamin D is described throughout the manuscript, I suggest using 25(OH)D. Consequently, replace 25-D with 25(OH)D throughout.
   We thank the reviewer for pointing out our oversight and have replaced 25-D with 25(OH)D in three instances.
2. Did any of the subjects go on vacation during winter/spring to a destination where there would be significant cutaneous vitamin D synthesis?
   Unfortunately, the PRAP questionnaire did not include questions assessing sun exposure, including exposure during vacation. This is noted as a limitation in the discussion (p.9 paragraph 3).
3. The portion quantities for cereals and milk needs to be defined.
   We have added portion quantities for milk and cereal in Table 1.
4. Abstract page 2 In the results, I suggest you remove: ‘among the 136 men not missing these data’
   We have removed the text as suggested.
5. Page 1 Email address of Marilyn Tseng is missing an ‘e’ i.e. edu
   We have corrected this error.
6. Table 1 The ‘%’ sign needs to be inserted e.g. under the distribution column
   We have inserted the % sign as suggested.
7. P 12 conclusions line 9: I suggest replacing ‘rather than as….’ with ‘in addition to an increased….’
   We have chosen to leave the original text because African Americans do not appear to be at the risk for skeletal conditions that would be predicted based on their vitamin D insufficiency. We suggest that their vitamin D insufficiency may manifest as increased risk for other disorders instead.
8. Table 1 page 21 Subscripts should be superscripts
   We have corrected this error.
9. Table 1 page 20 The mean (SD) or median (IQR) needs to be inserted for total vitamin D intake
   We thank the reviewer for noticing the omission. Mean (SD) total vitamin D is now in Table 1.

Other comments
Was body fat % measured in these subjects?
Body fat percent was unfortunately not measured in these subjects; only self-reported height and weight were available to estimate BMI.

Referee 4
Minor Essential Revisions

1. The study does not provide evidence of a casual relationship between physical activity and improved vitamin D status. The last sentences in the conclusion both in the abstract and in on page 13 should therefore be revised.

   As described in our response to Referee 1’s major comment 2, we have the statements in abstract and discussion recommending physical activity to improve vitamin D status.

2. Milk does not naturally contain any vitamin D. Vitamin D fortifications in milk differ between nations. It would have been helpful with some information about the content of vitamin D in milk in the country where the study was conducted. Are all milk fortified? Why were cereals included in the analysis – are they also fortified? If so, what is the vitamin D content in cereals?

   We have revised text in the methods section (p.5 last paragr.) to include the following sentence: “Fortified foods such as milk (~100 IU/cup) and cereal (often ~40 IU/cup) are the primary sources of vitamin D in the United States and were therefore considered as predictors of vitamin D status.”

3. The tables are clear and easy to read, but superscript-letters need to be explained.

   As noted in our response to Referee 1’s minor comment 2a, we have revised the footnote to clarify that categories with different superscripts are statistically significantly different from each other.