Title: Association between serum 25-hydroxyvitamin D and prostate cancer in middle-aged and elderly Americans: A national population-based analysis of NHANES 2001–2018

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Reviewer: Anna-Therese Lehnich

Reviewer's report:

Dear authors,
your manuscript deals with the interesting association between vitamin D levels and prostate cancer. Due to the large established dataset of NHANES and the sound analyses, the results add value to this topic. However, I think that there are some limitations in your study that should be considered or be more extensively discussed.

As you pointed out in the introduction, half of the patients are diagnosed in an advanced stage. Therefore, underdiagnoses might be the biggest source of bias in this analysis. In the limitations, you only refer to memory bias. In this regard two things raised my attention. First, the higher frequency of prostate cancer in CVD cases might result from patients diagnosed with CVD are better checked regarding their health status and that's why more cancer cases are reported in this group. Second, even though the differences between covariates are not statistically significant regarding the OR for prostate cancer, I think it is surprising that the categories no smoking, no alcohol and lower age showed higher ORs than the other categories, that are expected to be associated with higher cancer risk (figure 3). Therefore, please discuss the problem of underdiagnosis more extensively in the limitations.

Some minor comments:
As different methods for the assessment of vitamin D were used, please consider a sensitivity analysis separating the two methods.
In the abstract and some parts of the manuscript you use the phrase "risk" regarding prostate cancer. As you work with cross-sectional data, please refer to prevalence or frequency as in the rest of the manuscript.
There might be residual confounding due to broad categories for physical activity and alcohol consumption.
Missing data should be reported more transparently (table 1).
Please consider adding some more information regarding the vitamin D levels (what are typical mean values, what values are recommended).
In the discussion section, you report on the results from the Scandinavian studies by Ahonen and Tuohimaa and state that the results are "biased" due to the low level of vitamin D. Please consider rephrasing, as in my view, this is not a methodological mistake. Vitamin D levels in different populations might not overlap much, that's why different studies analyze different parts of the spectrum of vitamin D levels.

In table 2 there is a typing error: "prastate" cancer