Reviewer's report

Title: 25-Hydroxyvitamin D level is Inversely Associated with Serum MMP-9 in a cross-sectional study of African American ESRD Patients

Version: 2 Date: 13 December 2010

Reviewer: Stefan Pilz

Reviewer's report:

This is an interesting work on the association of 25(OH)D and biomarkers of vascular remodeling in a cross-sectional study among dialysis patients. The findings are of interest but I suggest to revise the manuscript before final acceptance.

Major compulsory revisions:

Statistical analyses need to be improved and/or explained in more detail!
Some analyses are not entirely clear to me. For instance, the authors stratify the patients into those with 25(OH)D levels higher and lower than 15ng/mL and then they perform a statistical test to evaluate whether those two groups have significantly different 25(OH)D levels (?).

Data presentation in Table 1: The authors present e.g. a mean 25(OH)D level of 18.78 but I wonder whether their assay measures 25(OH)D that precise; I therefore guess that 18.8 would be correct, the same applies also for other parameters.

The authors categorised for some analyses patients into those with 25(OH)D levels higher and lower than 15 ng/ml and for other analyses they used three groups (<12, 12-20,>20). It is a little bit confusing for the reader to have these different classifications and it is unclear to me why the authors do not present their data consistently by the use of a single vitamin D status classification.

MMP-2 measurements are presented in the methods section but the authors did not present further analyses on this interesting biomarker (should also be mentioned in the introduction and discussion).

Do the authors have data on active vitamin D treatment in these patients? If not please discuss and mention as a limitation.

There was no significant difference in PTH levels between the vitamin D status groups. This also needs to be discussed because it could be expected from the literature that patients with lower 25(OH)D have higher PTH levels.

The authors did not consider seasonal variation of 25(OH)D in their analyses. It might be interesting to see whether patients with sampling in summer had higher 25(OH)D levels compared to those with blood sampling in winter. The authors should at least discuss the possible impact of seasonal differences in 25(OH)D levels and may consider seasonal influences in their statistical analyses.
Minor essential revisions:
Introduction: there are two rather redundant sentences: "...few studies addressed the association between 25(OH)D concentration and increased inflammation on biomarkers of vascular remodeling" and "little is known regarding the association between 25(OH)D and biomarkers reflecting vascular remodeling"
The authors should carefully check for typos e.g. "25(OH) D" and for grammar and style.
What do the authors mean with "antimetabolite medications". Please specify.
What was the main study aim?
How was log transformation performed? log(10)?
Discretionary Revisions:
The authors stratify their analyses for vascular access type. Vascular access type may have an impact on inflammation per se but I wonder how vascular access type may influence the 25(OH)D inflammation link.
Was there any sample size calculation for that study?
I would like to see more discussion on the association of 25(OH)D and IL-10.
The authors may want to comment on the KDIGO 2009 suggestion to correct reduced 25(OH)D levels in CKD.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
I declare that I have no competing interests