Reviewer’s report

Title: Traditional Medicine Practices Among Community Members with Chronic Kidney Disease in Northern Tanzania: An Ethnomedical Survey

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Reviewer: Andre Kengne

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Traditional Medicines Practices Among Community Members with Chronic Kidney Disease in Northern Tanzania: An Ethnomedical Survey

In this manuscript, Dr Stanifer and coworkers have assessed traditional medicine (TM) use in relation with prevalent CKD and CKD risk in a community-based sample of 481 adults Tanzanians. They found TM use to be very high in this population, many of whom were at high risk of CKD. The authors conclude on the importance of accounting for TM uses in strategies aiming to address the growing CKD in this population.

The manuscript has merit and will definitely contribute to a better understanding of practices associated with CKD risk among African populations, who are already at higher risk of CKD. Although the sample size is small, the design of the study is acceptable, and importantly data analysis fully account for the design effect. The investigators must be commended for this. This said; there are also some areas that will benefit from clarifications and further improvement in the manuscript.

1) While the authors tend to project the combination of quantitative and qualitative approaches as a strength of the study, the current presentation of the manuscript is not integrating well those two components, with a trend toward shadowing the quantitative component, particularly in the discussion section. In the reviewer's view the qualitative component should really be only an appendix to explain findings from the quantitative component of the manuscript.

2) The investigators have used HbA1c cut-off of >7.0 to diagnose diabetes in this study. It is of note that HbA1c has not been validated for diabetes diagnosis in African populations, and the few available studies clearly show disparities in tresholds of HbA1c to diagnose diabetes in Africans, mostly in line with a recent report in the Lancet Endocrinology and Diabetology by the NCD-RisC consortium. The authors may consider discussing this in the limitation section of their manuscript.

3) The definition of diabetes and hypertension control need to be reformulated, currently both look redundant (see page 7-8)

4) While the design of the data analysis is correct, the reporting particularly in table is less optimal. The effect of accounting for the design is mostly perceived on the sampling
variability. As such I will encourage the authors to present the results everywhere with the 95% confidence intervals.

5) At the start of the result section, the authors should consider describing the process for reaching the final analytic sample. How many participants were approached? How many declined, and for what reasons? Was there any difference between respondents and non-respondents?

6) The presentation of the results is non-optimal. This is a community-based survey where those with prevalent CKD are mostly screen-detected cases. There is therefore no a priory while practice regarding TM use should occur in differential ways by prevalent disease status. As such the stratified presentation of the results is really not helping the flow of the manuscript, and this is further compromised be the switch between two to three groups. I would suggest that the authors stick to their three groups (Normal, at risk of CKD, prevalent CKD); and then show tables of baseline characteristics, TM use, and risk factors always across those three groups; and the description in the narrative section of the results should also integrate directly the comparison of the three groups and instead of been done separately. Therefore the authors should consider modifying Table 1 to integrate the 3 subgroups as well as a column for the overall sample and a column for the p-value to compare the three groups. All the pie charts should be replaced by tables based on the structure described above. A table should also be created to show the results from regression analysis; with the focus being on the effect of CKD status.

7) Page 9 last paragraph: PRR is not a measure of strength of the association, by a measure of the effect size. The authors should therefore refrain from referring to it as a measure of strength.

8) Page 14: strength of the study: The suggestion that this is the first study on TM use and CKD in community based study may apply to Tanzania, but not the African region where similar and event recent studies exist. The authors should therefore tone down on this claim.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

**Quality of written English**
Please indicate the quality of language in the manuscript:

Acceptable

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