Author's response to reviews

Title: Prevalence and determinants of chronic kidney disease in rural and urban Cameroonians: A cross-sectional study

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Author's response to reviews:

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Prevalence and determinants of chronic kidney disease in rural and urban Cameroonians: A cross-sectional study
Response to reviewer's comments
Dear Editor,
We are grateful to the reviewers for their time and important comments on our manuscript. We also appreciate the invitation from the editorial office to submit a revised version of our paper.

The reviewers' comments and suggestions have been used appropriately to improve the manuscript where relevant. Additions in the revised document are always indicated in a red colour. In addition, we provide below a point-by-point response detailing how we addressed each suggestion from the reviewers.

We look forward to the outcome of your assessment.
For the co-authors,
Dr Kaze

Reviewer's report: John Stanifer
The authors have posed an important question which is to define the community prevalence of CKD in Cameroon. It has not been well-described before and this work represents an important step forward in defining the epidemiology of CKD in sub-Saharan Africa. Based on this, the work is important and merits dissemination; however, there are a few important points where I think the authors could make their argument/message stronger and many that need clarification.

Our answer: Thanks for your appreciation.

Major Revisions:
1. In the introduction, the authors claim that CKD is 3-4 times more common in Africans (by virtue of their ethnicity) than in Caucasians. The citation (#3) does not fully support this, and I am not sure that it is accurate. I think what they mean to say is that the prevalence is 3-4x higher in Africa than in high-income countries, but even this is not entirely clear as other works suggest that it approximates the prevalence in high-income countries but there is no strong evidence to say 3-4x times higher.

Our answer: Thanks for raising this point. In the above mentioned citation (#3), it is clearly stated that “….there is a general impression that it (CKD) is at least 3–4 times more frequent than in more developed countries ….”. We have therefore rephrased the sentence to read: “Studies have revealed that Africans are at higher risk of CKD which seems to be at least 3-4 times more frequent than in developed countries”.

2. On lines 12-14 (page 3), the authors state that previous work in the region (Central Africa) has been sub-standard and thus are likely inaccurate. They should expand on why they believe this to be the case as two of these citation (by Sumaili, et al) are of high quality with good sampling and use reasonable definitions of CKD. Therefore, the authors need further justification in why the estimates by those works are ‘likely inaccurate’. There is no doubt however that the data in the region are sparse and that more epidemiological studies are needed.

Our answer: According to 2012 KDIGO guidelines (K/DIGO Guidelines. Definition and classification. Kidney Int Suppl 2013, 3(1):19-62.), CKD is defined as abnormalities of kidney structure or function, present for > 3 months, with implications for health. However, in the above mentioned studies, there was a lack of three months control of positive findings to confirm the chronicity of renal injury; this is the reason why we stated that these results were based on non-optimal definition of CKD according to KDIGO guidelines, and have likely provided inaccurate estimates of the disease burden. We therefore rephrased this sentence to read: “However, existing studies have been based on non-optimal definition of CKD according to 2012 KDIGO guidelines [13], and have likely provided inaccurate estimates of the disease burden”.

The methods are appropriate for answering the research question, and the authors have been thorough in their design and sampling methods. However, there are a few questions that I have pertaining to them.

Our answer: Thanks for your appreciation.

3. There is no citation for the region/district level population data; the authors need to cite the National Census or an equivalent source.

Our answer: The mentioned data were provided by the Cameroon Western regional delegate of public health. We therefore rephrased this sentence to read: “According to data from the regional delegation of health for Western Cameroon, Dschang health district is the largest health district in the region, with an estimated population of 309,285 inhabitants in 2012, distributed across 22 health areas (19 rurals and 3 urbans) (2012 annual activity report of the regional delegation of health for Western Cameroon)”.
4. Under sampling procedures, lines 4-18 (page 4) are quite dense and required multiple readings on my part. A flow diagram would be helpful (though not necessary) here.

Our answer: We rephrased the sampling procedures to read: “The sequence below was followed to select clusters and corresponding health areas. 1) We first assumed the number of clusters needed to be 30; 2) We then determined the sampling interval (SI) which corresponded to 10310, by dividing the population of Dschang health district by the number of clusters; 3) We determined the first cluster or random number (RN) by selecting the four last numbers of a randomly selected bank note which corresponded to 1399; 4) We next estimated the cluster number size (C(n)) from the formula C(n)= RN+(n-1)*SI where n is the cluster number; 5) The various health areas (with their corresponding population size) were then sorted in alphabetic order and progressive cumulative population size estimated (see supplemental Table 1); 6) The last step consisted of selection the health areas. For a health area to be selected, the size of the corresponding cluster number had to be inferior to the health area population. This action was repeated until the size of the cluster number became superior to the health area population then changing to the following health area in the table. The selected health areas and corresponding clusters are presented in supplemental Table 1.”

5. On line 7 (page 4), I assume that ‘cluster step’ is something similar to sampling interval (which equals total population/#clusters) which is usually added to a random number between 1 and the sampling interval (in this case 1399) in a repeated fashion until the number of necessary clusters are chosen. This is usually done (as the authors have) from a list of the sampling areas with the population successively added. However, this is not so clearly stated in this section and I think could be much simplified by just stating something like, “we used population proportional to size at the first level to identify the required number of clusters” and all the details about sampling interval, etc could be moved to an appendix. I also will add here as a note - though I do not expect the authors to necessarily respond to it - that the list of health areas listed in the appendix is in alphabetical order which probably does not affect the randomness of it so much, but usually in this technique the list is shuffled around randomly.

Our answer: Thanks for these points which we hope to have addressed in point 4 above. We further now refer to ‘sampling interval’ in lieu of ‘cluster step.

6. The authors took such care and thoroughness in describing their first stage of sampling, but seem to move over the next three stages very quickly. At the second level of sampling (village), it is not clear how the villages were randomly chosen. Did the authors have a full list of all villages in each health area? If so, did they use population proportional to size at this level as well? That is, if they did not use population proportional to size and one of the villages has 1000 persons while another has only 100 then the probability of being selected is not the same (thus the randomness is affected) and this would require weighting techniques in the data analysis. The same questions apply for the third level of sampling (neighborhood).
Our answer: We had the full list of all villages/neighbourhoods in each health area from the administrative and health authorities. As we mentioned on page 4 line 20, in a selected health area, one village was randomly drawn when they were more than one regardless of the population size. This was the case for neighbourhood. According to the data provided by the authorities, villages/neighbourhoods had almost similar size; reason why we did not use proportional sampling. We have rephrased the sentence to read: “In a selected health area, one village/neighbourhood was randomly drawn when they were more than one regardless of the population size.”

7. At the fourth level (household), the authors do not state how they chose the starting point. Was this based on random geographic points or some other way? It should also be noted that by starting at a market, church, health centre, or school that they will disproportionately select individuals who live close to these and these people may be systematically different than those who live farther away (e.g. they may have a higher socioeconomic standing and thus can afford to live closer to these conveniences). This is fine, but the authors should acknowledge this as a limitation or source of selection bias.

Our answer: This was mentioned already on page 4 lines 21-22. “The starting point was randomly selected from the market, church, health centre or school in the village/neighbourhood.” We would not consider the starting point from the list above as a limitation or source of selection bias because there is no difference between people in the village/neighbourhood according to their residency. People of all classes are almost equally distributed across the village/neighbourhood.

8. One of the most important omissions here in the methods is the non-response rate (this could also be stated in the results instead of the methods but it must be stated somewhere). This could potentially represent one of the biggest sources of bias in the study and must be acknowledged and clearly stated. In addition to the non-response rate, the authors might also state whether they used any techniques to reduce it like second visits, or evening visits, or weekend visits.

Our answer: Globally, we obtained a good response rate of 97.5% from participants and 97.9% from households. This high rate could be related to the sensitization campaign through community leaders, posters, leaflets and word of mouth announcing the survey. We therefore included these data at the beginning of the results section. It reads: “A total of 238 households were included in the survey from which 439 subjects (two participants from 201 households and one participant from the remaining) participated in the study. Eleven (2.5%) participants from seven (2.9%) households refused to participate in the survey. These households were replaced by the following one. The reasons for non-participation were fear (3 participants, 2 households), absence from home after several visits (2 participants, 2 households) and lack of time due to work constraints (6 participants, 3 households).”

9. The data collection procedure are well described for the most part, and they authors have certainly used high quality measurements for CKD which is to their credit. It is especially impressive that they were able to obtain repeat confirmation
of renal abnormalities which is of the highest standards. However, I would ask that they elaborate on line 14 (page 5) how the blood pressure was measured. Was this a one-time measurement? Was it an automated or manual sphygmomanometer? Did the participants have adequate time to rest before hand and were the cuff sizes appropriate? These are all important sources of error in BP measurements.

Our answer: We added the following information to clarify the measurements of blood pressure. It reads: “Blood pressure was measured according to the World Health Organization (WHO) guidelines [14] using an automated sphygmomanometer (OMRON HEM705CP, Omron Matsusaka Co, Matsusaka City, Mie-Ken, Japan) on the right arm with participants in a sitting position after 30 minutes of rest with a cuff bladder measuring 23 x 12 cm or larger for obese individuals. All anthropometric measurements were performed three times and their average used in all analyses.”

10. Under definitions and calculations, I have a few comments that I think will make the paper much more readable and understandable. First, the authors use three different formulas to estimate GFR but then mostly end up discussing the MDRD. I would suggest that for the primary analysis/results, they stick with only one estimator (preferably MDRD or CKD-EPI). Then as a secondary analysis they could present the differences that they observed among the three formulas and discuss what these differences may mean.

Our answer: Thanks for this suggestion. In both the definition and statistical analysis section, we now indicate that primary analyses are based on MDRD defined eGFR, while secondary analyses are based on CG and CKD-EPI defined eGFR. We have further captured all results relating to CG and CKD-EPI defined GFR in a new section of the results labelled “secondary analysis”. However, considering our focus on CKD which was assessed based on MDRD defined eGFR, we have refrained from further discussion of the discrepancies between estimators. However, previous studies in Africa, including by our group have substantially discussed the issues surrounding the low agreement between kidney function estimators in the African setting. Please see the following references: BMC Nephrol 2013, 14:75; Nephrol Dial Transplant 2010, 25(7):2178-2187.

11. I would also suggest that the authors use more standard nomenclature when discussing CKD, i.e. use the KDOQI or KGIDO staging classifications. The term chronic renal failure (CRF) is old to me and not typical, and it is especially confusing when discussed alongside chronic kidney disease (CKD). By sticking to the staging, they would avoid these confusing parallels, and it would also make the results more clear. For example, rather than the reader having to figure out how many people overlap between CKD, CRF, and albuminuria, he/she would easily know that if the prevalence of CKD Stage I or II is XX% then those patients by definition have albuminuria without a reduced GFR. The same would go for Stage III, IV, and V. This would also correct a major problem of Table 3 which is the column of ‘Persistent Albuminuria’; the denominator in this column is not correctly specified because not everyone in the sample (439) was tested for persistent albuminuria (only the 85 who returned after the first measurement returned for the second test), but the way that the column is titled would make it
suggest that everyone (439) was tested for persistent albuminuria. If the column was titled CKD Stage I/II then the denominator would be correctly specified because everyone in the sample (439) was tested for CKD. This may seem like minor semantics but it speaks to the confusion caused by the authors’ odd disease classifications.

Our answer: In 2012, KDIGO guidelines recommend that CKD is classified based on the GFR and albuminuria categories. We therefore use this classification to stage CKD in the revised Table 3 and used appropriated terms in the revised version of the manuscript as well as in Table 4.

12. My last comment for the methods is from the statistical analysis section. The authors appropriately point out early on that they accounted for the cluster design effect when calculating their sample size, but they do not state whether the accounted for the clustering effect on the variance. That is, the cluster design will increase variance around a point estimate which will increase confidence intervals. Given the 4 stage cluster design used here, I would suggest that they authors try to account for this effect in variance. SPSS software is well-equipped for complex survey analysis and using something like the Taylor series linearization technique would easily do this. This point is not necessarily major but if if the authors do not use this in their analysis, then they should at least state it as a limitation especially when it comes to making assumptions about the statistical significance of relationships (that depend on these confidence intervals).

Our answer: Thanks for raising these points. We have repeated the analysis completely with the use of the survey analysis procedures of SAS (surveymeans, surveyreg and surveylogistic) to account for the cluster design. In these procedures, we have used the Taylor expansion methods to estimate the sampling variance. The tables and result sections have been updated accordingly, as well as the statistical analysis section which reads below: “Data analysis used SAS/STAT v9.1 software for Windows® (SAS Institute Inc., Cary, NC, USA), and the survey analysis procedures ('proc surveymeans', 'proc surveyreg' and 'proc surveylogistic') to account for the multilevel sampling design of the study. We have reported the results as means, counts and percentages and the accompanying 95% confidence intervals. The sampling error was estimated with the use of the Taylor expansion method.”

13. Overall, the results appear to be sound. One major limitation is that confidence intervals are not presented for the point prevalence estimates, e.g. it is very important to know the variance around the final reported prevalence of CKD.

Our answer: Thanks for raising this point. In the updated results and tables, all estimates are now provided with a 95% confidence interval.

14. It is also a bit confusing to present in detail how many participants had renal abnormalities on the first round of testing and then how many have abnormalities on the second confirmation. If the definition of CKD, as used by the authors, is persistent albuminuria/GFR reduction, then I would simply state what the prevalence of CKD is that they found. This, by definition, would mean that it was
persistent. If the authors want to show the readers how many people tested positive in the first round, then a flow diagram might be a clearer way to do so, but otherwise why present (in the authors’ words) a sub-standard definition of CKD?

Our answer: Thanks for raising this point. There is a rationale for showing the results based both on single and repeated assessment. While we are keen on providing accurate estimates based on repeated assessment, it is of note that available prevalence data on CKD in Africa are essentially based on single assessment, hence the importance of providing the equivalents figures from our sample for comparison purpose.

15. On line 20 (page 7) the authors make mention of awareness and unawareness of disease status; this awareness/unawareness variable should be defined in the methods. I would also suggest to the authors that rather than using awareness of disease status among all participants as an outcome they may want to use awareness of disease status among those who tested positive, i.e. how many people with CKD, diabetes, hypertension were aware that they had it? This would be more valuable because presumably large numbers of any population (Africa or anywhere) are unaware of their disease status because most people do not have disease (I myself am unaware of my status for these diseases). Also during this section the authors make mention of gout, but from what I can tell they did not test for gout. Therefore, the value of asking people whether they think they have gout (awareness of it) when the authors themselves cannot confirm either way seems small.

Our answer: While we understand and agree with the suggested approach by the reviewer, we are constrained by the fact that diagnosis of some concomitant conditions like diabetes or gout in the current study was based exclusively on self-reports. It probably makes sense in this situation to make a difference between those who will answer ‘no’ for a history of existing condition because they have been tested and found not to have the condition, and those who will answer ‘no’ when they simply don’t know if they have the condition or not (for they have never been tested). With regard to CKD the condition of focal interest in the study, prior knowledge largely suggest the very few (if any) of those diagnosed with CKD during the survey will actually be aware of their condition, therefore making meaningless any attempt to study awareness of CKD among those diagnosed with prevalent CKD during the survey. Among patients receiving clinical care for major CKD risk factors such as hypertension or diabetes in Cameroon, referral for CKD almost always occur at the advanced stage of the disease (ERSD), confirm the low awareness of CKD even among high risk patients and their health care providers.

16. In the results section, the authors also need to be careful not to use the words proteinuria and albuminuria synonymously. For example, on line 1 (page 8) they status albuminuria prevalence is 19.6% but what they mean is dipstick proteinuria (this is important because one is qualitative and the other is semi-quantitative).

Our answer: This has been changed as suggested.
17. Also, on line 6 (page 8) they authors state ‘longstanding users of street and herbal medicines’ but what is meant by ‘longstanding’ and what is meant by ‘street medicine’? Are the over-the-counters, traditional medicines, biomedicines, contaminated drugs, illicit drugs?

Our answer: The term “longstanding used of medicines” referred to recurrent used of medicines at least once every three months. Herbal medicines referred to drugs from Africa pharmacopeia because the term “traditional medicines” is considered as pejorative term. The term street medicine (read now ‘street medication’) referred to all modern (western) drugs, usually of uncertain origins that are sold in shops and regularly along market streets, instead of pharmacies, and without any control.

18. Table 2. The units in the row labeled ‘Mean serum Creatinine’ do not appear to be right. The mean serum creatinine for the population could not have been 10.6mg/dL as this would be grossly abnormal. Likewise, I do not think that it would have been 10.6 mmol/L either as this would be too low, so I do not know where these units come from but perhaps I am mis-reading the table. I also do not understand where the numbers come from in the Rows for MDRD, CG, CKD-EPi, and Albuminuria. For example, Table 2 would make it appear that 47 people (10.7%) had an eGFR <60 by MDRD but the authors clearly state in the text (and in Table 3) that the prevalence of ‘chronic renal failure’ was 2.5%. Likewise, the numbers for albuminuria do not match anything in the text. How can 120 participants (27.3%) have albuminuria >30 and eGFR <60 when only 113 (25.7) participants (as stated on lines 13 (page 7) had any renal abnormalities at all on the first screen? For these reasons I cannot figure out how Tables 2 and 3 sync up and Table 2 does not seem supported by the text or the results. The authors needs to clarify themselves on these points.

Our answer: The unit of serum creatinine is mg/l instead of mg/dl. This has been corrected. In Table 2, we present the baseline kidney function test and urinalysis by sex and urban/rural location (see the title); therefore the numbers referred to those who presented at baseline either albuminuria or an eGFR<60 according to each eGFR estimator, whereas the numbers in Table 3 referred to participants who presented persisting albuminuria (>30 mg/g) and/or eGFR <60 at three months control to confirm the chronicity. After verification, there were some mistakes in reporting the number in the text, which have now been corrected: “In all, 120 (27.3%) participants had renal abnormalities requiring repeated tests to confirm the chronicity, including 73 (60.8%) with only albuminuria (ACR # 30 mg/g), 34 (28.3%) with only decreased eGFR (< 60 ml/min/1.73 m2) and 13 (10.8%) with both”.

19. The manuscript does not state what the funding source for this work was.

Our answer: We did not received funding for this study, which was supported by collective efforts of the medical student (and family) whom this work was part of the MD dissertation, and his supervisors.

20. Yes, the discussion and conclusions are adequately supported by the data, but I believe that the authors could dig a little deeper into their findings and they have over-stated their findings in some places (e.g. lines 5-6 on page 10). One
place where they could dig deeper is in the rural/urban differences. Why do the authors think that they saw this? The association with herbal medicine use and the other NCDs (diabetes and hypertension) is also very interesting...

Our answer: Concerning the discussion about rural/urban differences and the association between risk factors and CKD, we rephrased the discussion section to read: “Compared to urban setting, CKD seems to be more prevalent in rural area despite the lack of statistical difference as noticed in the metaanalysis by Stanifer et al [9]. The likely high prevalence observed in rural setting could be related to the high frequency of well known clinical and socio-demographic risk factors for CKD occurrence and progression to ESRD [13].”

21. I also do not think that lines 12-20 (page 10) add much to the discussion. The authors suggest that single time point measurements are inaccurate in estimating eGFR but there could be many factors at play. For example, what about the role of survivor bias? It is certainly the case that people die more frequently (especially when healthcare access is poor) as their GFR falls; therefore, it could be that other studies see difference rates of advanced stage CKD due merely to the fact that healthcare access or death rates are different. Either way, this doesn’t seem to add much as the strength of this paper is in the community-based estimates of CKD in context with other NCDs.

Our answer: Thanks for raising this point. We are still of the impression that the debate about single time point versus two time-points assessment to define CKD has a contribution in this setting. It has particular relevance regarding the course of action that follows the screening. Our findings for instance suggest that in this resource-limited setting, repeating urine protein test and creatinine measurement will substantially limit the number of those to be referred to the nephrologist for further assessment and follow-up.

21. The limitations are not clearly stated. The authors do not address non-response bias (or report a non-response rate) nor do they fully address any selection biases in their procedures (e.g. their choice in starting points for random household selections). Further there is no discussion about how their study sample compares to the population at large. For example, how does the study breakdown of men/women; urban/rural; age; etc compare to the expected distribution based on what is known from the census? This would help speak to both selection bias and non-response bias in a strong way. The authors even hint at it on lines 22-23 (page 11), but this could actually be measured or quantified rather than just stated.

Our answer: thanks for raising these points. We however feel that we have in major ways stated those limitations to be accounted for in the interpretation of our findings. As said above in response to point 8, we achieved very good response rates both from participants (97.5%) and households (97.9%); as such speculation about the response rate may not hold in the context of this study. Also as discussed in point 7, there is little if any reason to consider the starting point from the list above as a limitation or source of selection bias. There is no indication of a correlation between the starting point and any characteristics (including socio-economic status) that can differentially affect our sampling. Furthermore, there is no recent census data against which we can assess to
what extend the final composition of our sample reflect the composition of the background population (age, sex, urban vs. rural etc.).

22. There are quite a few spelling and grammatical errors that need to be corrected.

Minor Revisions:

1. On lines 5-6 (page 3), the authors should state ‘dual burden of communicable diseases and non-communicable diseases including CKD’.
   Our answer: This has been fixed.

2. On lines 10-11 (page 3), the authors state that GN, diabetes, htn, HIV, obesity, and herbals are the main contributing causes of CKD in sub-Saharan Africa, but part of their argument is that the epidemiology is not well defined. They would be better to state that these are potentially important etiologies.
   Our answer: We rephrased the sentence to read “Furthermore, CKD in SSA tend to affect mostly young adults, with hypertension, chronic glomerulonephritis, diabetes, HIV infection, obesity and herbal medicine consumption being the potential main contributing factors”.

3. The study area is well described in lines 18-22 (page 3) but a map or some type of visual aid would be helpful to readers not familiar with the region or country. This would help readers understand where they are geographically.
   Our answer: A figure 1 has been introduced to address this suggestion

4. On line 7 (page 5), as a minor point, the authors state ‘confidence interval of 1.96’ but the confidence interval would really be the mean +/- standard error x 1.96.
   Our answer: Data have been reanalysed completely and all estimates are now presented with a 95% confidence interval, accounting for the design of the study.

5. I would suggest that the inclusion/exclusion criteria on lines 8-10 (page 6) be stated earlier on in the methods; maybe under study design or sampling procedures.
   Our answer: As suggested, we included this sentence at the end of study design section.

Overall, I think this is a very valuable study and could contribute substantially to the literature. It would be very impactful for both local and global researchers/practitioners, but there are many important points that need clarification. As such, the manuscript is not yet suitable for publication as it stands, and it will require major revision.

Our answer:

Level of interest: An article of importance in its field

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,
and I agree to my signed report being passed on to the authors.

Reviewer’s report: Nomandla Madala

The manuscript by Kaze et al. describes the prevalence and determinants of CKD in rural and urban Cameroonians. It has the potential to contribute towards very lacking data in sub-Saharan Africa. I would suggest that the authors consider the following major and minor revisions.

Our answer: Thanks for your appreciation.

Major compulsory revisions

1. Background

Data from Cameroon have not been adequately given in order to provide the context and background to this study. The authors need to refer to previous work published from Cameroon on the subject. This should be included in the background and the rational for the current study. If there is no prior work this should be clearly stated.

Our answer: Thanks for raising this point. As far as we are aware, no study has been published or performed on the epidemiology of chronic kidney disease in Cameroon. We rephrased the last sentence of background to read: “In the absence of study on the epidemiology of chronic kidney disease in Cameroon, we therefore undertook this study to establish the prevalence and determinants of CKD in rural and urban settings of Cameroon”.

2. Methods

2.1 It would be useful to include more detail on inclusion and exclusion criteria, particularly how the authors ensured the exclusion of other non-renal causes of albuminuria such as gynaecological causes.

Our answer: We excluded any pregnant women based on their last menstrual period or breastfeeding women. However, by repeating any positive dipstick test 2 to 3 weeks after and 3 months later, we thought eliminated any participant with transient abnormalities.

2.2 How was urban vs rural status decided? Authors must clarify whether this was arbitrary or was it based on certain pre-determined definitions that must be appropriately cited?

Our answer: The distinction between rural and urban health areas was provided by the data from the regional delegation of health for Western Cameroon (2012 annual activity report of the regional delegation of health for Western Cameroon).

3. Results

3.1 In Table 2 creatinine clearance seems to have been used interchangeably with eGFR. This must be clarified.

Our answer: Under definitions and calculations in the method section on Page 6 line 23, we added that “Estimated glomerular filtration rate (eGFR, mL/min) corresponded to creatinine clearance”.

3.2 The prevalence of CKD by CG-eGFR was 21.9% (double that by MDRD and
CKD EPI equations). This discrepancy is likely to influence clinical practice on which equation is used in this population, even while all equations have not been validated. It would enhance the paper if the authors include information on this finding in the discussion and explore possible explanations.

Our answer: The above mentioned prevalence of decreased eGFR by CG corresponded to baseline characteristics. This eGFR estimation with the three equations was to give us an idea of decreased eGFR according to each equation. We did not emphasized in the discussion based on the fact that only the MDRD equation was applied to assess the chronicity. Moreover, we felt that this high prevalence of decreased eGFR with CG equation could be easily understood considering the fact that the weight influences the formulae and could explained this discrepancy.

4. Discussion

4.1 The meta analysis by Stannifer et al. cited by the authors did not show a difference in CKD prevalence between rural and urban communities therefore line 10 (page 10) must be reviewed.

Our answer: We rephrased these sentences to read: “Compared to urban setting, CKD seems to be more prevalent in rural area despite the lack of statistical difference as noticed in the metaanalysis by Stanifer et al [9]. The likely high prevalence observed in rural setting could be related to the high frequency of well known clinical and socio-demographic risk factors for CKD occurrence and progression to ESRD [13].”

4.2 The higher prevalence of CKD by albuminuria in the rural while lower when defined by eGFR (CRF) has not been sufficiently argued/ postulated on. Could HIV in rural Cameroonians be a possible explanation?

Our answer: According to « Enquête Démographique et de Santé et à Indicateurs Multiples » [www.statistics-cameroon.org/.../EDS-MICS11/EDS-MICS], HIV prevalence is high in urban (4.8 %) compare to rural (3.8%) area. As such, a differential contribution of HIV across settings is very unlikely. However, as in our previous answer (4.1), the high prevalence of clinical and sociodemographic risk factors for CKD could be an explanation. In particular, rural participants were older, and age is a well-known driver of other risk factors for CKD such as hypertension, diabetes mellitus etc.

While the limitations of not including HIV in this study have been acknowledged, it might be worth mentioning its prevalence in the Cameroon as it is an important risk factor for CKD in sub-Saharan Africa. This might help put into context the urban vs rural CKD prevalence.

Our answer: Please see the above answer.

Minor revision

1. Were there any attempts to verify personal history like accessing medical records?

Our answer: Unfortunately the opportunity of doing so is virtually impossible in the study setting, including in major cities with better access to health care
facilities.

2. Provide detail on how 'gout' was determined.

Our answer: The knowledge on “history of gout” was only based on the informations that the participants received from the treating physicians/nurses. For instance, we obtained a high prevalence of people who were unaware of such disease status. Therefore, we think that this survey gives information on such disease to carry actions on the promotion and prevention of these diseases.

3. Consider using 'equation or prediction equation' as a more common term than 'estimators' used throughout the manuscript.

Our answer: This has been changed

Level of interest: An article of importance in its field

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.