Dear Editor,

We appreciate positive and constructive responses from the reviewer and mainly agree with the issues suggested. We have responded on them as shown in the author's reply, point by point and the appropriate changes made in the manuscript.

We are now submitting a revised version of the manuscript and authors reply.

Sincerely

Dr Lutale J.

Author's reply

Major Compulsory Revisions

Point 1
1. It is necessary for the authors to emphasize that this is a selected population from a diabetic clinic. I would prefer that they make this point when they compare their findings with other reports from Caucasian and African populations.

Response:
The suggested idea has been dealt with in the discussion part to reflect the type of patients studied.

Point 2
The number of type 1 diabetics is low, limiting the statistical power for explanatory variables. This should also be commented on in the discussion.

Response:
We agree this shortcoming is mentioned as one of our study limitations.
Point 3
Table 5 compares their results with other (rather small) studies from the African continent. The discussion is too long and should be shortened by at least one page. I would prefer that the rather detailed discussion on differences between small studies related to table 5 is condensed to discuss the following topics:

Point 3 a) Is it a time-trend that the prevalence of microalbuminuria/diabetic nephropathy is decreasing on this continent also (as it is in the Western world)? It could look like this from the table. Or is it just accidental?
Point 3 b) In general terms, what were the selection criteria for the studies reported in table 5?
Point 3 c) How can the different ways of assessing albuminuria have influenced the variability in prevalence numbers? Exclusion of bacteriuric patients (which is appropriate) has probably not been performed in other studies?

Response:
The discussion have been shortened by a page in accordance with the above suggestions. We agree that the prevalences of microalbuminuria, nephropathy and microangiopathic complications show a decreasing trend in the Western world which has been attributed to better treatment of hypertension, glycaemic control and also the use of angiotensin converting inhibitors and ARB’s. We may also speculate that the observed decreasing prevalence in the reports from African diabetic patients may be partly explained by better treatment compared to earlier years. This observation has been incorporated in our discussion section.

There are few studies from the Sub Saharan continent on the topic. So the four mentioned African studies are the ones that at least show details of microalbuminuria testing. In all but one of these studies, microalbuminuria was assessed using a semiquantitative method (MICRAL) while in the current study quantitative immunoturbidimetry was used. Spot urine samples are subject to a high rate of false positive results. In all the studies except one screening for bacteriuria was reported to be performed.

Point 4
The conclusion section should be shortened, and confined to the main findings of the study.

Response: The conclusion has been shortened reflecting the significant findings of the study.

Point 5
Frozen urine samples were measured. There is decay in albumin concentrations in frozen urine samples being thawed. How can this have affected the results compared to other studies?

Response:
We are aware of this issue, and there are uncertainties on the effect of freezing and thawing urine samples [1-5]. We used the suggestions given in the clinical guideline for renal disease prevention[6]. Urine albumin concentration stability appears to be satisfactory for many months at -40 degreesC and -70 degreesC. (in the absence of urinary tract infection)[6]. We believe that by freezing at -80 degreesC did not cause significant decay in urine albumin.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. The method sections counts altogether 272 patients, but the result section counts only 271 patients. Why?
The accurate number of patients recruited is 271. We apologize for the typographical error. The necessary corrections have been made in corresponding section the text.

2. The expression NIDDM should be replaced by ‘type 2 diabetes’ in the discussion.
* The use of NIDDM was retained when referring to earlier studies in which by then it was one of the term of classifying patients. As we are not sure if they real represented Type 2 patients as the term is being used now, we thought we should let the readers have its conceptual meaning as it was used then.

3. Could the authors comment why ARBs are not used in this population?
* ARBS are new in the market and relatively expensive for our patients and the government does not subsidize ARBs. Hence ACEi are preferred to ARBs. In the current study of all the 45 patients know to have
hypertension prior to the study, 3 (8%) patients were using ARB (Losartan) and 13 (29%) were using ACEi as a single drug or in combination with other ant hypertensives. ARB use was not mentioned in the earlier version as it was just included in ‘others’ section. This have been changed in the results section to show those who were using ACEI and ARB's separately.

Reference