Reviewer’s report

Title: PREDICTIVE FACTORS AND PREVALENCE OF MICROALBUMINURIA IN HIV-INFECTED PATIENTS: A CROSS-SECTIONAL ANALYSIS

Version: 0 Date: 01 May 2017

Reviewer: Denyse Thornley-Brown

Reviewer's report:

Summary:

The purpose of this study was to: (1) evaluate the prevalence of microalbuminuria in a cohort of HIV positive patients; and (2) to assess the association of microalbuminuria with different therapeutic regimens.

The authors studied 326 unselected sequential patients receiving HIV care at time 0 and 48 weeks, measuring urinary microalbumin concentration as well as a number of metabolic and demographic parameters and therapeutic regimens.

Major criticisms:

1. The authors conclude that "We showed a very high prevalence of microalbuminuria, much higher than the literature data..."; however, the authors use an unconventional definition of microalbuminuria, i.e., "a urinary albumin excretion rate(sic) greater than 1 mg/L." While it is possible to measure such a low concentration of microalbumin, the generally accepted cut-off concentration for clinically significant microalbuminuria is higher (e.g. The cutoff used in reference 6 [Glassock] was a microalbumin concentration of 3-30 mg/dL).

2. The authors compare baseline to 48 week microalbumin outcomes and divide the population into three groups: Equal, Improved and Worse. In the Equal group, microalbumin concentration remained constant at 1.2 mg/L at 0 and 48 weeks; in the improved group it went from 1.5 mg/L to 0 mg/L, and in the worse group from 0 mg/L to 1.5 mg/L at 0 and 48 weeks, respectively. While these values are statistically significant, I would question their clinical significance, especially given that the serum creatinine concentration was unchanged at 48 months and the change in cystatin C concentration at 0 and 48 months was no different between the three groups. It would also be helpful in interpreting the data to know the coefficient of variance for the assay used.
3. Since the outcome variable used was urinary microalbumin concentration (as opposed to microalbumin to creatinine ratio), the authors should address the issue of differences in hydration (i.e., urinary osmolality) between 0 and 48 weeks as a limiting factor in the interpretation of their results.

4. The authors compare the prevalence of microalbuminuria in their study to those of three other studies (references 20-22); however, these studies were in children and this manuscript is looking at an adult population.

Minor criticisms:

1. I could not find definitions for the abbreviations used in Table 1 (specifically omo, TD, entero).

2. The majority of patients infected with hepatitis C had stable or improved proteinuria. How many received treatment for their hepatitis C during the course of the study?

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.

No

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Not suitable for publication unless extensively edited

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