Reviewer’s report

Title: Chronic kidney disease and HIV in the era of antiretroviral treatment: findings from a 10-year cohort study in a West African setting

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Reviewer: Frank Post

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Kabore and colleagues report the burden of CKD in a cohort of West African patients, with a prevalence of 3% at baseline and a low incidence of 1.9/1000 PYFU. In line with previous studies from Africa and the USA (Reid et al, CID 2008; 46: 1271-81 and AIDS 2008; 22: 481-7), they noted eGFR decline during follow up on ART in those with eGFR >90, and improvements in renal function among those with CKD and mildly impaired eGFR. They report associations between eGFR decline and known risk factors such as immunodeficiency, TDF/PI exposure and hypertension.

The authors may wish to contextualise their findings with data from the UK CHIC cohort, which reported a prevalence of CKD of 3% among patients in the UK who were born in West-Africa (JID 2018; 218: 1767-1172). By contrast, the incidence of CKD during follow up in this cohort (mostly on ART) was substantially higher (7.9/1000 PYFU), and no association with TDF exposure was observed.

HIVAN is a major cause of ESKD in West Africa, and it would be useful to understand to what extent early mortality (ESKD with early mortality may have precluded enrollment into the cohort, and ESKD during follow up may have remained undiagnosed. This might have been a major source of survival bias and an over-representation of low risk individuals in the present analyses. The exclusion of patients who did not start ART and those that left the cohort early (with no renal follow up data) may have further skewed the results towards a group of "relative well patients who started ART". These biases may explain the low CKD incidence rate, should be mentioned as major limitations of the study, and the conclusion should probably reflect this.

Comments:

The statistical methods deserve further detail: were mixed effects models used to create eGFR slopes or were absolute changes analysed? What were the models adjusted for? Were viral load data included in the models?

I prefer the term "greater eGFR decline" over "faster eGFR loss"

In Table 3, it would be helpful to include the number of patients included in the analyses (reminding the reader that the number of participants with eGFR <60 at baseline was small)
I would remove or rephrase line 245/246 "this could be explained by a catch up effect linked to restoration of kidney function induced by ART" - I am not sure what the authors are trying to describe.

The authors may wish to acknowledge that PI's have an effect of tubular secretion of creatinine and this may in part explain the greater reductions in eGFR observed with this class of drugs.

The conclusions refers to a CKD prevalence of 0.5% which is confusing - with infrequent measurement of creatinine, attrition and exclusions, this is more likely to reflect the prevalence of CKD in the cohort. I am also not sure that the authors' data support kidney function monitoring in those on TDF + PI; after the initial (benign) effects on creatinine clearance, renal function was fairly stable during more prolonged follow up.

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