Reviewer's report

Title: Prevalence of Hyperglycemia among Adults with Newly Diagnosed HIV/AIDS in China

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Reviewer: Jennifer H Han

Reviewer's report:

The authors performed a cross-sectional study with the aim of evaluating the prevalence of hyperglycemia, IFG, and diabetes among patients with newly diagnosed HIV infection. This is an interesting topic that is of clinical and public health importance, particularly given rising rates of both DM and HIV infection worldwide, as well as specifically in China. While several studies have been previously published evaluating the prevalence of glucose disorders in the HIV-infected population (both ART-experienced and ART-naïve), this particular study seeks to address this question specifically in recently-diagnosed HIV-infected patients who are antiretroviral-naïve. Furthermore, the majority of prior studies have been conducted in American and European populations, so evaluating an Asian population contributes to its novelty. The manuscript is informative, although it does require moderate editing for mostly grammatical issues as outlined below. The methods are in general adequate, although the final multivariable model was limited to evaluation of only a few variables. Furthermore, the study could be strengthened and/or clarified to improve readability, understanding of methodology, and clinical applicability as suggested below:

Major Compulsory Revisions

1. How were patients selected? In particular, for readers who are not familiar with China’s healthcare system, it would be helpful to clarify this. For example, were patients with new positive HIV-antibody tests randomly selected from the named provinces/municipalities? Or were ALL patients with such positive tests enrolled? This information would greatly aid in assessing the generalizability of the study results, as well as potential for selection bias. Were there any exclusion criteria applied to the initial source population? For example, were patients with a known diagnosis of diabetes excluded?

2. When a fasting plasma glucose is used to diagnose diabetes, as was done in this study, usual criteria include two FPG >=126 performed on two separate days (or one FPG and an additional criteria, such as elevated random glucose, on a different day). The investigators relied on only one FGP level in this study, and this should be discussed. For example, this may have led to over-reporting of the true prevalence of DM. Along these lines, was there other available data that the investigators could have used to diagnose prevalent DM? For example, medication data (e.g., oral diabetic medication or insulin)? Physician-diagnosed
diabetes?

3. The statistical analysis section needs clarification. In this section, the authors state that essentially all “relevant covariates” were included in the model (e.g., age, sex, CD4 count). However, in Table 2, the final model includes only age and CD4 count, coded as categorical variables. Were all variables with a certain P value (e.g., P<0.20) on univariable analysis considered for inclusion in the final multivariable model? If so, a table showing results of univariable analysis would be helpful. Or were ALL the variables included in the final model regardless of significance on univariable analysis, in which case, their ORs, 95% CIs, and P values should be shown in Table 2. This is critical information in determining what the ORs for age and CD4 count were “adjusted for.” I would also suggest stating explicitly in the statistical analysis section that a multivariable logistic regression model was developed.

Minor Essential Revisions

1. While this is more of a descriptive study, a major limitation of the MV model was that the investigators were not able to evaluate important potential risk factors for DM in HIV-infected patients, namely BMI, family history, hepatitis C co-infection, along with other comorbidities. This should be stated as a limitation of the MV model specifically (for example, if these variables had been controlled for, CD4 count may not have remained significant in the model).

2. Discussion, 2nd paragraph, the authors state that their study cannot confirm that HIV infection results in hyperglycemia because no randomized case-control study was performed in this study. Please clarify, or delete. A case-control study would not be randomized, and a randomized controlled trial would not be used to address the question of HIV infection versus no infection leading to diabetes.

3. Table 1 – what does “blood” mean, regarding HIV transmission category? Intravenous drug use? Blood transfusion?

There are several grammatical issues that need to be edited, some of which are detailed below:

1. In multiple sections (e.g., Figure Legends, Abstract, Results): “prevalences” should be “prevalence.”

2. Abstract, Conclusions: “Hyperglycemia are…” should be “Hyperglycemia is…”

3. First paragraph of Background, the 2nd and 3rd sentences should have an “and” after the comma.

4. Background, second paragraph, 2nd sentence: “…exposure to some of antiretroviral drugs” should be “…exposure to some antiretroviral drugs.”

5. Background, last sentence: needs to be split into 2 sentences.

6. Last sentence of Results should be “…an association…”

7. Discussion, 1st paragraph, should be “…hyperglycemia should be screened FOR…” “…AND hyperglycemia screening …”

8. Several other minor spelling/grammatical corrections are needed in the Discussion.
Discretionary Revisions:

1. It would have been interesting and informative to also include viral load (by category or at the least, undetectable versus detectable) in the analysis, in addition to CD4 count. A potential mechanism leading to hyperglycemia in HIV is chronic inflammation, for which VL would be somewhat of a surrogate marker. This would have been especially informative given that the study population was limited to patients with recently diagnosed HIV who are ART-naïve and have relatively low CD4 counts.

2. Percentages could be rounded to one decimal point for ease of reading, and not subtract from the informativeness of the value (e.g., 19.99% could be rounded to 20.0%). Similarly, odds ratios could be rounded to 2 rather than 3 decimal points.

3. I believe that the Discussion could be streamlined, and the clinical relevance of the study findings could be expanded upon. For example, routine screening for diabetes is already recommended in both HIV-infected and noninfected patients. Is this different in China, and if so, these study results would provide an important impetus for routine screening to be adopted more uniformly and should be discussed.

**Level of interest:** An article whose findings are important to those with closely related research interests

**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