Reviewer's report

**Title:** Genotypic Distribution and Hepatic Fibrosis Among HIV/HCV Co-infected Individuals in Southern China: A Retrospective Cross-sectional Study

**Version:** 1  **Date:** 2 March 2015

**Reviewer:** Thomas Reiberger

**Reviewer's report:**

- **Major Compulsory Revisions**

1. this should be a cross-sectional study on HCV epidemiology, but however, the authors did include only ART-naïve patients. This exclusion criteria seems to introduce an important bias that could easily be avoided. Also the authors excluded patients with decompensated cirrhosis, severe cytopenias, etc… since this is a non-interventional study, there is no reason to exclude those patients – especially when the study aims to give a perspective on HCV epidemiology.

2. The authors report results only for FIB-4 scores and not for APRI. Although the discuss it in the methods. In general, since the authors exclude patients with cirrhosis and cytopenias (thrombocytopenia as a marker of portal hypertension), this analyses does not really reflect the actual severity of fibrosis in this HCV cohort, since patients with most pronounced liver disease were excluded.

3. Data presentation, tables is poor. Table 1 should list HCV-mono and HIV/HCV coinfected patients separately. And compare their characteristics. It is also clear that information is missing for some categories, e.g. marriage status and education status. This should be added as “unknown”.

4. Statistical analyses: It is not clear how missing data was handled in multivariate analyses – especially since patients with missing data are usually excluded for MVA, but the authors seem to include all “n=175” in the logistic regression model. The adjusted OR generated by their model seems extraordinary high with 46 !!! and 17”” fold increased risk for HIV/HCV coinfection in case of IVDA as transmission route and no highschool graduation, respectively. This should be assessed by an biostatistician!!

- **Minor Essential Revisions**

4. Abstract

4.1 This is not really a “molecular epidemiology” study. Determination of HCV genotype is a standard procedure and the term molecular should probably be reserved to experimental/mechanistic studies.

5. Introduction

5.1 Age of successful HAART should be changed to “era of...”
5.2 HAART should be changed to ART, this terminology has been proposed by international guidelines

5.3 The number HCV patients, HIV patients, and HIV/HCV coinfected in China should be given.

5.4 “underlies” should probably read as “underlines”

5.5 I would suggest to change the sentence to “HIV coinfection accelerates HCV-associated fibrosis progression... especially when CD4+ cell counts are low...” compare reference: Reiberger et al, Journal of Viral Hepatitis 2010

5.6 References for recent advanced in HCV therapeutics should also include those studies for HIV/HCV coinfection, please cite
- Photon-2 study: SOF/RBV, Molina et al, Lancet 2015
- Turquoise-1 study: 3D/RBV, Sulkowski et al, JAMA 2015

5.7 Add reference for poor treatment uptake: Reiberger et al, Antiviral Therapy 2011

6. Methods

6.1 Now the authors refer to ART (before they use HAART), please just use ART from the beginning

6.2 The methods again state, that there is a high prevalence of HIV and HCV infections in China, but again they don’t give actual number – this should be done in the introduction

6.3 Did the authors mean “IFN-free” when they state “providing free HCV therapy” ??

6.4 What is a “convenience sample of treatment naïve ...”?

6.5 Why were only anti-retroviral naïve patients included?

6.6 The authors should give the APRI and FIB-4 formulas they used for calculation of the respective scores. Not only refer to class1, class2, and class3m but give actual values that were actually used for their cutoffs in their study.

7. Results

7.1 The authors conclude that “most” patients had not completed high school and were married, however, the percentage were 49.1% and 35.4%, respectively. The formulation should be changed to reflect that these were not the majority in the respective category.

8. Discussion

8.1 It is not clear to the reader what the others mean by “separate health system
in China allow mono-infected indiviudals to persist ...???

9. References

9.1 Ref 31 is not necessary. This should just be added to the methods section.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

The reviewer has no public conflicts of interest related to this review or financial competing interests related to this review. 'I declare that I have no competing interests'