Dear Editors,

Thank you and the reviewers very much for reviewing our manuscript and invaluable comments. The following is our responses to the reviewers' comments.

Reviewer #1:

To the author: The idea of the article was clear, understandable, and the research cycle was long (144 weeks), which was the innovation of the paper. It could also reflect the extensive work done by the author and their team. After reading the entire article, there are the following issues:

1. In the 'INTRODUCTION', emphasis can be placed on the relationship between TDF and TAF drugs, and the discussion section can describe the comparison of the efficacy results obtained from previous studies of the two drugs

Thank you for the comment. The sentence "TAF is a prodrug of tenofovir with greater plasma stability, allowing for more efficient uptake by hepatocytes with less peripheral drug exposure of metabolites compared to TDF." was added in introduction section and explained in discussion section. (MS page 3, para 1, last sentence; page 9, para 2; and page 10, para 1, and references Murakami E et al and Agarwal K et al).

2. The 'Study Design and Patient Enrollment' section mentions "informed consent was waived", and it is unclear whether it is a misunderstanding or a narrative error. Although no intervention measures were given, collecting clinical data from subjects for clinical research still requires informed consent.

Thank you for this question. This was a retrospective study, and it was conducted by retrospective chart review. The IRB approval was obtained and informed consent was waived by IRB. This is a very standard process of retrospective studies.

3. The inclusion and exclusion criteria for the enrolled population are too simplistic. The article studies the therapeutic effect of drugs on liver fibrosis. Do the subjects need to first exclude existing underlying diseases that may cause liver fibrosis, or merge with other liver diseases?

Thank you for your comments. As you can see, this was an observative study, the pre-switching lab results were used as control. We appreciate the reviewer's comment. Indeed, before starting TDF, all patients were screened for other liver diseases, and patients who had only CHB and no other liver diseases was started on TDF, then switched to TAF. Thus, the sentence "*who had CHB and ruled out other chronic liver diseases*" was added in the revised version (page 4, para 2).

4. There is an error in the format of statistical P-values.

Thank you for your suggestions. The format of P-value was updated throughout the MS.

5. In the section "Renal Function Benefits after Switching at Week 144", I personally believe that the results section only needs to describe the research results. This statement applies to relevant literature and is not the result of this study. It can be written in the discussion section.

Thank you for your suggestion. We believe that we should keep the first sentence and references to introduce why we included these results to the MS. However, we fully agree only data, but not comments should be included in RESULTS. Thus, we delete "*the renal function was stable and improved at switching week 144*."and added the related results without any discussion (see page 7, para 3).

6. In the 'Discussion' section,"another goal of HBV treatment is to achieve hepatic fibrosis regression.", early liver fibrosis may be reversible, but due to the limitations of diagnostic methods, most early liver fibrosis is difficult to detect. It can be expressed as: "Another goal of HBV treatment is to slow down the progression of liver fibrosis, and even achieve early resolution of liver fibrosis."

Thank you for this excellent suggestion. Revision is done accordingly. (page 9, para 1, 1st sentence).

Reviewer #2:

It is a well-organized manuscript about TAF superiority over TDF in liver biochemistry and liver stiffness measurement. The abstract is OK, the methods are sufficient, and the results are clear. Since no extensive data exists about this issue, I think this manuscript should be published.

However, I think a significant limitation is using SWE instead of transient elastography. All significant published data about liver fibrosis have used transient elastography. Authors should indicate it in the manuscript.

Thank you for this great suggestion. *We fully agree* and the change has been made accordingly. (page 9, para 2)

I think elastography results are more important than those of FIB-4 or APRI. So, the figures must be modified to show the results of SWE.

Thank you for the great comments and suggestions. *We fully agree*. Figure 3 and the related legend are updated to show the changes with standard deviation in mean SWE reading along with APRI and FIB-4 scores. (page 7, para 2, legend, and figure 3C).

The amount of patients with "clinical cirrhosis" is small. This is a significant limitation of the study.

Thank you for the comment. *We fully agree*. The related statement has been added to the revised MS. (page 10, para 3)

Authors should indicate the cut-off values for liver fibrosis stages used in SWE, and clearly define the F4 stage (or F3/F4 stage). So, all data analyses should be modified according to the SWE-based criteria.

Thank you for your suggestion. *We fully agree*. The reference and cut off values of SWE that we used in this study were added. The analysis discussion was updated in the MS to show the improvement of SWE reading from the baseline mean 7.05 to 6.3 kPa, after switching to TAF for a mean 109 wks. (page 7, para 2 and page 9, para 2)

Authors, in the discussion session, have to give some explanations about TAF superiority. Is there a pathophysiological explanation? Other reason?

Thank you for great suggestions. *We fully agree*. The related statement was added in page 3, para 1; page 9, para 2; and page 10, para 1.

The benefits and better safety profile of TAF *might be due to* its great plasma stability and lower peripheral exposure of the active metabolite, tenofovir diphosphate. The explanation of superiority of TAF compared to TDF was added to the MS. (page 9, para 2 and page 10, para 1)

Finally, in the discussion, the authors should clarify the importance of this superiority in clinical practice. For example, what is the clinical point of the "… the improvement rate to fibrosis stage 0-1 was increased from 64% to 86%"?

Thank you for the comment. *We fully agree.* The improvement rate of fibrosis stage 0-1 from 64% to 86% indicated the benefit and superiority of TAF in the improvement rates of liver fibrosis compared to TDF. Our study results demonstrated and confirmed the great benefit of switching to TAF in improvement of fibrosis stage 0-1 with 34.4% improvement (page 9, para 2).

As you can see in the revised version, we also rechecked the entire MS and made some changes to further improve in English writing.

We sincerely hope that our revised MS will meet the standard of *World Journal of Hepatology*.

Thank you for your reconsideration and we look forward to your positive response.

Sincerely,

Ke-Qin Hu, MD, FAASLD