



PEER-REVIEW REPORT

Name of journal: *World Journal of Hepatology*

Manuscript NO: 94098

Title: Improvement of Hepatic Fibrosis after Tenofovir Disoproxil Fumarate Switching to Tenofovir Alafenamide for Three Years

Provenance and peer review: Unsolicited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer’s code: 05267231

Position: Editorial Board

Academic degree: MD, PhD

Professional title: Director

Reviewer’s Country/Territory: Greece

Author’s Country/Territory: United States

Manuscript submission date: 2024-03-11

Reviewer chosen by: AI Technique

Reviewer accepted review: 2024-03-18 17:18

Reviewer performed review: 2024-03-25 06:18

Review time: 6 Days and 13 Hours

Scientific quality	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Very good <input type="checkbox"/> Grade C: Good <input type="checkbox"/> Grade D: Fair <input type="checkbox"/> Grade E: Do not publish
Novelty of this manuscript	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Good <input type="checkbox"/> Grade C: Fair <input type="checkbox"/> Grade D: No novelty
Creativity or innovation of this manuscript	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Good <input type="checkbox"/> Grade C: Fair <input type="checkbox"/> Grade D: No creativity or innovation



Scientific significance of the conclusion in this manuscript	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Good <input type="checkbox"/> Grade C: Fair <input type="checkbox"/> Grade D: No scientific significance
Language quality	<input type="checkbox"/> Grade A: Priority publishing <input checked="" type="checkbox"/> Grade B: Minor language polishing <input type="checkbox"/> Grade C: A great deal of language polishing <input type="checkbox"/> Grade D: Rejection
Conclusion	<input type="checkbox"/> Accept (High priority) <input type="checkbox"/> Accept (General priority) <input checked="" type="checkbox"/> Minor revision <input type="checkbox"/> Major revision <input type="checkbox"/> Rejection
Re-review	<input checked="" type="checkbox"/> Yes <input type="checkbox"/> No
Peer-reviewer statements	Peer-Review: <input checked="" type="checkbox"/> Anonymous <input type="checkbox"/> Onymous
	Conflicts-of-Interest: <input type="checkbox"/> Yes <input checked="" type="checkbox"/> No

SPECIFIC COMMENTS TO AUTHORS

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. 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. The amount of patients with “clinical cirrhosis” is small. This is a significant limitation of the study. 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. Authors, in the discussion session, have to give some explanations about TAF superiority. Is there a pathophysiological explanation? Other reason? 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



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fibrosis stage 0-1 was increased from 64% to 86%”?



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Peer-review model: Single blind

Reviewer’s code: 05447441

Position: Peer Reviewer

Academic degree: MD

Professional title: Doctor

Reviewer’s Country/Territory: China

Author’s Country/Territory: United States

Manuscript submission date: 2024-03-11

Reviewer chosen by: AI Technique

Reviewer accepted review: 2024-03-19 11:16

Reviewer performed review: 2024-03-27 04:37

Review time: 7 Days and 17 Hours

Scientific quality	<input type="checkbox"/> Grade A: Excellent <input type="checkbox"/> Grade B: Very good <input checked="" type="checkbox"/> Grade C: Good <input type="checkbox"/> Grade D: Fair <input type="checkbox"/> Grade E: Do not publish
Novelty of this manuscript	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Good <input type="checkbox"/> Grade C: Fair <input type="checkbox"/> Grade D: No novelty
Creativity or innovation of this manuscript	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Good <input type="checkbox"/> Grade C: Fair <input type="checkbox"/> Grade D: No creativity or innovation



Scientific significance of the conclusion in this manuscript	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Good <input type="checkbox"/> Grade C: Fair <input type="checkbox"/> Grade D: No scientific significance
Language quality	<input type="checkbox"/> Grade A: Priority publishing <input type="checkbox"/> Grade B: Minor language polishing <input checked="" type="checkbox"/> Grade C: A great deal of language polishing <input type="checkbox"/> Grade D: Rejection
Conclusion	<input type="checkbox"/> Accept (High priority) <input type="checkbox"/> Accept (General priority) <input type="checkbox"/> Minor revision <input checked="" type="checkbox"/> Major revision <input type="checkbox"/> Rejection
Re-review	<input type="checkbox"/> Yes <input checked="" type="checkbox"/> No
Peer-reviewer statements	Peer-Review: <input checked="" type="checkbox"/> Anonymous <input type="checkbox"/> Onymous
	Conflicts-of-Interest: <input type="checkbox"/> Yes <input checked="" type="checkbox"/> No

SPECIFIC COMMENTS TO AUTHORS

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 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. 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? 4. There is an error in the format of statistical P-values. 5. In the section "Renal Function Benefits after Switching at Week 144", I personally believe that the results section only needs to



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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. 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."