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ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology

ESPS manuscript NO: 32025

Title: Early hepatitis B viral DNA clearance predicts treatment response at week 96

Reviewer's code: 02921008

Reviewer's country: Iran

Science editor: Yuan Qi

Date sent for review: 2016-12-23 14:44

Date reviewed: 2016-12-26 20:13

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	Google Search:	<input type="checkbox"/> Accept
<input checked="" type="checkbox"/> Grade B: Very good	<input checked="" type="checkbox"/> Grade B: Minor language polishing	<input type="checkbox"/> The same title	<input checked="" type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C: Good	<input type="checkbox"/> Grade C: A great deal of language polishing	<input type="checkbox"/> Duplicate publication	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade D: Rejected	<input checked="" type="checkbox"/> Plagiarism	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		<input checked="" type="checkbox"/> No	<input type="checkbox"/> Major revision
		BPG Search:	
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

A very good article, suitable for publication. The research methodology is nice, the article is well written and clear, and the conclusions accord with the results. Only, the English needs to be rechecked for few small grammar corrections. A few minor improvements would be: 1. The title can be more clear. 2. In the abstract, once the ROC abbreviation was defined, do not use the full term again. 3. Please elaborate on the design type in the abstract. 4. Observational studies differ from clinical trials. But I see the paper is registered as a clinical trial at the journal's website. Please clarify. 5. Please clarify if patients were administered any antivirals in the initial treatments? 6. If so, did you account for the dose and types and duration of medications administered in the initial treatment? (in the M&M section, I saw you had excluded such patients. It would be nice to report this in the abstract too --optional). 7. In the introduction, please correct "Other therapy strategy " to "Other therapy strategies " 8. Please make sure the text is free of grammar errors. 9. "As this is clinical outcome " should become "As this clinical outcome " 10. In the M&M section, how did you decide which patients should receive which treatments? 11. I see different interventions have been administered. So this study seems to be a clinical trial rather than an Observational study (as stated in the abstract).



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Please double check and make corrections. 12. Was the intervention randomized? 13. Was it blinded? 14. The conclusion at the end of the text should be expanded considerably. There are more valuable points to be reported as a take-home message. 15. Results, supplementary info, tables, and figures are nice. I suggest you to also publish your raw data. 16. Discussion is currently more of an interpretation of the results. It has 4 paragraphs. The first one is interpretations. The second one describes that there were no previous similar reports and briefly compares few aspects of this study with previous ones. The third one is more suited for the Introduction. The fourth one is the limitations. The discussion can be expanded, taking into account more comparisons with previous studies, and explaining the reasons for controversies and agreements. It is accepted that the lack of very similar studies can shorten the discussion, but still many other aspects can be compared with previous studies. Good luck



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ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology

ESPS manuscript NO: 32025

Title: Early hepatitis B viral DNA clearance predicts treatment response at week 96

Reviewer's code: 02860911

Reviewer's country: Taiwan

Science editor: Yuan Qi

Date sent for review: 2016-12-23 14:44

Date reviewed: 2017-01-06 09:28

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input checked="" type="checkbox"/> Grade A: Priority publishing	Google Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B: Very good	<input type="checkbox"/> Grade B: Minor language polishing	<input type="checkbox"/> The same title	<input type="checkbox"/> High priority for publication
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<input type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade C: A great deal of language polishing	<input type="checkbox"/> Plagiarism	<input type="checkbox"/> Rejection
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		BPG Search:	<input checked="" type="checkbox"/> Major revision
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

This study is interesting in illustrating the HBV DNA level on Nuc treatment by week 24 to predict the viral suppression and HBeAg seroconversion rate by week 96. However, the patient numbers in each arm is relatively small and there are several questions listed as below: 1. The compliance issue shall be evaluated in three arms of patients 2. The male to female ratio in the 3rd arm is not the same proportion to the other two arms, is gender be a factor for poor viral suppression by week 24? 3. Does different potent Nuc have different viral suppression result? How's the viral suppression result if separate the patients into ETV/TDF and others? 4. Does the inadequate viral suppression leads to increase incidence of HCC during follow-up? since mutated virus has been reported to be carcinogenic effect 5. What's the suggestion derived from this finding? Change to other treatment?