

ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Hepatology

ESPS manuscript NO: 14041

Title: Completion of 48-week course of telaprevir-based triple therapy for HCV recurrence post liver transplant

Reviewer's code: 02444986

Reviewer's country: Turkey

Science editor: Yue-Li Tian

Date sent for review: 2014-09-20 14:02

Date reviewed: 2014-10-13 17:00

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	PubMed 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 type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C: Good		<input type="checkbox"/> Duplicate publication	
<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
<input type="checkbox"/> Grade E: Poor		<input checked="" type="checkbox"/> No	<input checked="" type="checkbox"/> Minor revision
	<input type="checkbox"/> Grade D: Rejected	BPG Search:	<input 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

authors evaluated telaprevir-ribavirin-PEG IFN treatment in post-transplant HCV infection. overall sir were 58%. RVR, genotype and bilirubin were determinants of SVR and there were no severe adverse effects. my minor comments: *result section should be shortened. * table 3 should be omitted and NS3 should be added to table4 * figure 1 is unnecessary so should be omitted.

ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Hepatology

ESPS manuscript NO: 14041

Title: Completion of 48-week course of telaprevir-based triple therapy for HCV recurrence post liver transplant

Reviewer's code: 02530754

Reviewer's country: Spain

Science editor: Yue-Li Tian

Date sent for review: 2014-09-20 14:02

Date reviewed: 2014-11-08 20:59

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	PubMed Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B: Very good	<input checked="" type="checkbox"/> Grade B: Minor language polishing	<input type="checkbox"/> The same title	<input type="checkbox"/> High priority for publication
<input checked="" 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 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

The manuscript by Herzer et al described a cohort of 19 LT patients with recurrence of hepatitis C who received a telaprevir based triple therapy in a single institution. The study is somehow timely with the new direct antivirals already marketed in many countries. However the manuscript contains some relevant information which may add to the literature. There are several concerns, some of them important, to be addressed: 1- Although the title is striking, it does not mirror the actual results of the paper. The sustained virological response rates in the whole cohort (intention to treat analysis) were 58%, which may be higher than those reported in previous studies, but they are far from the 100% rates appearing in the title. Only after excluding those patients who received a shorter course of treatment a 100% SVR rates were obtained. This is a clear example of selection bias since some of the excluded patients met any stopping rule. Thus the title should be changed. 2- The authors should clarify whether any data of the present manuscript has been previously published or submitted elsewhere. In methods the authors said that the demographic data of the patients may be found in table 1 and reference 9. Does this reference 9 contain any data included in the present manuscript? If

it is so the manuscript should be unsubmitted to avoid duplication. 3- The authors should describe the median time between the liver transplantation and the treatment in the whole cohort. 4- Explain the inclusion criteria. Which patients were eligible for telaprevir-based triple therapy? Were they selected by clinical, biochemical and/or histological parameters? 5- There are some typos to be corrected in table 1 and in methods (page 7, first paragraph). 6- In the statistical methods used, the authors said that a p value <0.05 was considered significant, and two lines below they said that p values ≤ 0.05 was considered significant. Please clarify and eliminate one of these statements. If the contrasts were two-tailed please add this information. 7- In page 10 the authors said that none of the patients had histological signs of cirrhosis, but the fibroscan values reported were high (the maximum value was 46 KPa). 8- The figure 1 is difficult to read. There are two different diagrams in the same figure. Please consider eliminating one of them. 9- There is a citation quoted several times along the manuscript but it does not appear among the references (Papadopoulos-K?hn, Transplantation, in press). If the manuscript is already accepted for publication, please quote it properly among the references. Similarly in page 17, first paragraph, the authors based one of their statements in a "manuscript submitted". If this paper has not been published or at least accepted for publication, it should not be quoted. 10- The limitations of the study, including the reduced sample size, the retrospective design and the lack of control for possible confounding factors (by multivariate analysis) should be highlighted in the discussion. 11- Some of the conclusions made by the authors are not supported by their results. In their first conclusion the authors said that telaprevir based triple therapy is superior to dual therapy in LT patients with HCV recurrence. This comment should be either eliminated or significantly softened since the present study did not perform any randomized comparison between both approaches. Later in the same paragraph the authors said that RVR, low baseline bilirubin and GT1b were independent predictors of beneficial course and outcome of telaprevir based triple therapy. However there is no multivariate analysis (nor enough sample size to attempt it) to control for possible confounding factors and therefore the identified predictors were not "independent". 12- Please describe the non-standard abbreviations used along the manuscript to be included in the title page.