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ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Hepatology

ESPS manuscript NO: 19716

Title: Liver transplantation and sleeve gastrectomy in the medically complicated obese: New challenges on the horizon

Reviewer's code: 00054102

Reviewer's country: Spain

Science editor: Fang-Fang Ji

Date sent for review: 2015-05-19 17:36

Date reviewed: 2015-05-22 13:13

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 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
<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 type="checkbox"/> Plagiarism	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		<input 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 type="checkbox"/> No	

COMMENTS TO AUTHORS

This is a very interesting manuscript about an ongoing relevant issue. The manuscript is well written and comments are very relevant. I would like only to comment to the authors a couple of questions. Firstly, could you comment on which BMI would you recommend to be indication to add a SG to LT, specially in case of patients with metabolic syndrome? Secondly, at the view of current experience, could you make any proposal of the timing of SG depending on the severity of liver disease? It seems that any patient without indication for LT or compensated liver disease may benefit of SG previously to LT. And, in the other hand, we could agree that decompensated patients should not undergo SG previous to LT, but consider it simultaneoulsy. Could you further comment on this aspect? Which would be your opinion?



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ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Hepatology

ESPS manuscript NO: 19716

Title: Liver transplantation and sleeve gastrectomy in the medically complicated obese: New challenges on the horizon

Reviewer's code: 00053958

Reviewer's country: Canada

Science editor: Fang-Fang Ji

Date sent for review: 2015-05-19 17:36

Date reviewed: 2015-05-25 07:57

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 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
<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 type="checkbox"/> Plagiarism	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		[Y] No	<input type="checkbox"/> Major revision
		BPG Search:	
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		[Y] No	

COMMENTS TO AUTHORS

This is a well-written editorial on a very important emerging issue facing liver transplantation. I have 2 minor comments: 1) Can the authors comment on the effects of sleeve gastrectomy in someone who may require LT in the future- in terms of changes to the surgical field? 2) In the conclusion, I think the term 'reverse transcriptase inhibitors' should be changed to DAA (Direct acting antivirals).



ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Hepatology

ESPS manuscript NO: 19716

Title: Liver transplantation and sleeve gastrectomy in the medically complicated obese: New challenges on the horizon

Reviewer's code: 00053490

Reviewer's country: United States

Science editor: Fang-Fang Ji

Date sent for review: 2015-05-19 17:36

Date reviewed: 2015-05-30 04:50

Table with 4 columns: CLASSIFICATION, LANGUAGE EVALUATION, SCIENTIFIC MISCONDUCT, CONCLUSION. It contains checkboxes for various review criteria like 'Grade A: Excellent', 'Priority publishing', 'Google Search', etc.

COMMENTS TO AUTHORS

The authors submitted an editorial piece discussing the utility of weight loss surgery in the setting of liver transplantation. The authors have done a nice job summarizing the literature. I have a few comments: 1. In the authors' first paragraph, it appears reference 2 is cited before reference 1. These should be reversed. 2. In the second paragraph, the sentence stating that NASH cirrhosis as the primary indication for liver transplantation is confusing. Do the authors mean that the incidence has increased from 1.2% to 9.7%, or do the authors mean that the incidence ranges between 1.2% and 9.7%? This was unclear to me. 3. The authors used a lot of acronyms, and most are defined. Some are not, however. Most of them may be obvious, but still should be defined (e.g., ICU=intensive care unit; HCV=hepatitis C; US=United States; BMI=body mass index). 4. On page 8, the paragraph implies there are several factors involved in why the Heimbach study was significant. They start a sentence with "First, ..." However, subsequent sentences do not use a "second" or "next." This paragraph construction is awkward. Likewise, the last sentence of the following paragraph that goes onto page 9 is also awkwardly constructed. 5.The final paragraph



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prior to the conclusion paragraph states that "More importantly, they were able to tolerate a major bariatric surgery without any overt decompensation of their newly diagnosed liver disease." I would submit that patients with stage 2 fibrosis will tolerate major surgery. Even well compensated Childs A cirrhotics (no evidence of portal hypertension) can tolerate major surgery, so patients with only stage 2 fibrosis should be able to tolerate surgery. I think the authors' sentence might be worded too strongly. 6. In the conclusion, the authors comment that NASH cirrhosis will "supersede all other indications for LT in the near future." Although this may be a true statement, the authors never cited any references in the body of the editorial to support this conclusion. Although this manuscript is an editorial, it seems that the above statement should be based on some fact presented somewhere in the manuscript.