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ESPS PEER REVIEW REPORT

Name of journal: World Journal of Gastroenterology
ESPS manuscript NO: 13833
Title: Oxaliplatin and 5-fluorouracil hepatic infusion combined with lipiodolized chemoembolization in large hepatocellular carcinoma: Safety and efficacy at a single center
Reviewer code: 00053441
Science editor: Ya-Juan Ma
Date sent for review: 2014-09-04 16:15
Date reviewed: 2014-10-03 21:04

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
[ ] Grade A: Excellent	[ ] Grade A: Priority publishing	Google Search:	[Y] Accept
[Y] Grade B: Very good	[Y] Grade B: Minor language polishing	[ ] Existing	[ ] High priority for publication
[ ] Grade C: Good	[ ] Grade C: A great deal of language polishing	[ ] No records	
[ ] Grade D: Fair		BPG Search:	[ ] Rejection
[ ] Grade E: Poor	[ ] Grade D: Rejected	[ ] Existing	[ ] Minor revision
		[ ] No records	[ ] Major revision

COMMENTS TO AUTHORS

The paper by Li et al evaluates the transarterial-chemoembolization protocol in hepatocelular carcinoma in a series of 103 patients. The manuscript is clear and provides valuable information.

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ESPS manuscript NO: 13833
Title: Oxaliplatin and 5-fluorouracil hepatic infusion combined with lipiodolized chemoembolization in large hepatocellular carcinoma: Safety and efficacy at a single center
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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
[ ] Grade A: Excellent	[ ] Grade A: Priority publishing	Google Search:	[ ] Accept
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[Y] Grade C: Good	[ ] Grade C: A great deal of language polishing	[ ] No records	[ ] Rejection
[ ] Grade D: Fair	[ ] Grade D: Rejected	BPG Search:	[Y] Minor revision
[ ] Grade E: Poor		[ ] Existing	[ ] Major revision
		[ ] No records	

COMMENTS TO AUTHORS

There were lots of studies assessing the role of TACE for unresected HCC. However, this study focuses on HCC with size larger than 10 cm. This kind of subgroup of HCC is not so common in western countries and would be less in China duo to the early diagnosis via regular screen. Thus the data provided by the manuscript is valuable. I think this paper can be accepted after revision. My concerns are as follows: 1. In introduction section, the authors stated that 'TACE significantly prolongs the survival of patients with HCC who are not candidates for curative treatments', however, a systematic review in Cochrane database showed that there was no evidence supporting the benefits of TAE/TACE for unresectable HCC (doi: 10.1002/14651858.CD004787.pub2). 2. Why the authors used 16-week disease-control rate to evaluated disease control? Was there any 3. The threshold of 25% for the changes of AFP should be explained. 4. As overall survival was the primary outcome, participants without well-document imaging report but was not lost to follow-up should also be included for survival analysis. 5. In methods section, the duration that complications were measured should be stated. Does any in-hospital mobility or complications within 30 days after TACE was included? 6. The four patients with lung metastases should be excluded when analysing survival, since they could have significantly poorer prognosis. 7. The proportion of patients with cirrhosis should be reported in baseline characteristics. The median and range of follow-up duration should be

reported. In addition, post-embolism syndrome needs to be defined. 8. Since the survival varies in different institutes and population, it should be very cautious to demonstrate 'effective' of this protocol if there was no `comparable control group. 9. I would like to see the disease free survival curve, if possible.