

ANSWERING REVIEWERS

October 10, 2012

Dear Editor,

Please find enclosed the edited manuscript in Word format (file name: ESPS manuscript No 495-review.doc).

Title: Clinical significance and risk factors of post-embolization fever in patients with HCC.

Author: Chung Hwan Jun, Ho Seok Ki, Hoon Ki Lee, Kang Jin Park, Seon Young Park, Sung Bum Cho, Chang Hwan Park, Young Eun Joo, Hyun Soo Kim, Sung Kyu Choi, Jong Sun Rew

Name of Journal: *World Journal of Gastroenterology*

ESPS Manuscript NO: 495

The manuscript has been improved according to the suggestions of reviewers:

1. Format has been updated
2. Revision has been made according to the suggestions of the reviewer

COMMENT-1

1. As this is a retrospective study, it is desirable to fully explain, how the primary data were collected: did the authors document all data prospectively in an electronic data base and then evaluate retrospectively or did they just routinely document each case history and at the time of the study evaluate each case history "by hand"? → We collected and analyzed the each past data at the time of the study (2011.07-2011.12) based on electronic medical record.
2. Body temperature is a highly variable parameter, showing circadian variations and being dependent also on the site (oral, rectal, axillary etc.) and method of measurement. How frequently during the day the body temperature was taken and documented and who was

doing these measurements (patients, nurses, others?). These aspects need to be clarified in the paper. Why was 38.0 C taken as the cutting point? Would the results be equal at different cutting points, for instance 37.8 or 38.5 C? → *Temperature was measured q.i.d. by nurses using axillary thermometer, we added this paragraph in the paper. About cut off value of BT, recent most article define the PEF as body temperature ≥ 38.0 C after TACE. The result may be different at cutting points BT ≥ 37.8 C or 38.5 C. For example, some people had 37.8 C or 37.9 C, but we exclude them.*

3. 117 episodes of PEF in 443 HCC patients are NOT 27 % but 26% (exactly 26.41) (RESULTS, paragraph 2, page 5) → *We corrected the paragraph as you pointed out.*
4. The parameter "ALT elevation after TACE" should be specified: is this an elevation above normal or an elevation in comparison with the ALT measured prior to TACE. How many u/L - difference were accepted as relevant and not just a variation within the laboratory? (RESULTS, Association of PEF with clinical variables, page 5 and Table 2). → *Sorry, it means ALT value after TACE. we correct the paragraph and table.*
5. The last sentence on page 5 seems to be incomplete. → *We completed the last sentence.*
6. Figure 1 seems to be incomplete, as in addition to the statement "Follow up CT after 4 weeks " the statement "and then at every 3 months" from page 4 should be added. Furthermore, Fig 1 is not mentioned or referred to in the text. → *We revise the manuscript as you recommended*
7. Figure 3 is not referred to in the text. → *we added the mention*

COMMENT-2

1. I think there has to be a clear comment about the method how the authors ruled out true infection among the patients with PEF. Unless, this ambiguity could cause fundamental defect in the enrollment of the patients. → *"Bacterial culture from blood and urine and chest X-rays were performed in patients who had fever after TACE, to detect any possible infectious agents. Ultrasonography or a CT scan was carried out if there fever persisted despite the use of antibiotics, to*

detect the possible development of abscess formation". We describe above mention in manuscript (method and material , Monitoring and management of post-embolization fever). If any culture or image such as chest X-ray, US, CT suggest infection, we exclude them. But, because of retrospective study, we might not completely rule out hidden infection.

2.Liver function (ChildPugh score) was not significant prognostic factor, but the dose of lipiodol was. This is quite strange and different from other studies and the author should explain this in discussion. → *most enrolled patient had child A (Pt with PEF child score: 5.66 vs without PEF child score: 5.77). So, liver function did not affect prognosis. As well known, tumor size was independent prognostic factor, lipiodol dose and tumor size had a positive trend in our study. lipiodol dose was associated with prognosis in univariate analysis, but lipiodol dose was not associated with prognosis in multivariate analysis*

3 References and typesetting were corrected

Thank you again for publishing our manuscript in the *World Journal of Gastroenterology*.

Sincerely yours,



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