

September 11th, 2017

Clara Balsano, PhD, Wan-Long Chuang, MD, PhD, and Lucia Pacifico, MD

Editors-in-Chief

World Journal of Hepatology

Dear Drs. Balsano, Chuang, and Pacifico:

Thank you for forwarding the reviewers' comments on our manuscript. The comments were useful, and the manuscript has been revised in light of the suggestions. A point-by-point response follows.

To Reviewer 00061674:

1. We added the following in the Methods section: Comorbidities that could be associated with an increase or decrease in the MPV/PLT ratio such as inflammatory bowel disease, immune thrombocytopenic purpura, myeloproliferative disease, or Bernard-Soulier syndrome were not observed in any of the patients. (p. 4, line 29–p.5, line 2)
2. We added the following in the Discussion section: We could not compare these methods with the MPV/PLT ratio because we do not have these examination devices. (p. 7, lines 11–12)
3. We added the reference to the new Inuyama classification. (Reference 7)
4. We added a description of the F4 category. (p. 4, lines 13)

To Reviewer 02939199:

Major comments

1. We very much agree with your comment; however, we do not have data for patients without HCC. We added following in the Discussion section: Therefore, the results should not be generalized to patients without HCC

before verification. (p. 8, lines 1-2)

2. and 3.

We added the etiology of liver disease in the Results section. (p. 5, lines 14-15) Additionally, we examined the correlation between the MPV/PLT ratio and the degree of fibrosis. We think that more than 2/3 patients without cirrhosis were useful for the results in Figure 2.

Minor comments

1. We added references. (References 1, 2)
2. We corrected references according to the guidelines of the journal.
3. We added units for PLT. (p. 5, line 25)
4. We asked native speakers to edit our English.

To Reviewer 03260503:

1. We added the etiology of liver disease in the Results section. (p. 5, lines 14-15) Recently, HCC in patients negative for hepatitis B and C has increased in Japan. Therefore, we do not think that more than 90% of HCCs appear in patients with liver cirrhosis in the Japanese population.
2. We added the results of the additional examination. There was no significant correlation between the MPV/PLT ratio and pathological inflammation level ($P = 0.214$). (p. 6, lines 20-25)
3. We corrected the Introduction section as you pointed out. (p. 4, line 3)
4. We added the following in the Results section. (p. 5, lines 15-19) The average age was 69.6 ± 9.7 years in the non-LC group and 68.2 ± 7.6 years in the LC group ($P = 0.21$). The ratio of males to females was larger in the non-LC group, with 164 (81.2%) male and 38 (18.8%) female patients; there were 69 patients (69.0%) male and 31 (31.0%) female patients in the LC group ($P = 0.02$). We commented about comorbidities in the Methods section. (p. 4, line 29-p.5, line 2)

5. We corrected as you pointed out. (p. 7, line 23)
6. We added the etiology in Table 1 and the Results section. (p. 5, lines 21-22)
We do not think the difference in etiology induces bias, because we need the patients with F0–F4. It is usual that the incidence of patients with HCV is high in the LC group.
7. We are very sorry. The cut off value of 0.8, sensitivity of 65% and specificity of 78% is correct. (p. 2, line 25–27)
8. We added the Edmonson-Steiner grade for HCC in Table 1 and the Results section. (p. 5, lines 22–24)
9. We added cut-off values in Table 2. We used the median as the cut-off value for all variables.

I hope that these changes will make this manuscript suitable for publication in *World Journal of Hepatology*.

Sincerely yours,

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