Reviewer's report

Title: Predictors of Esophageal Varices in Patients With HBV-related Cirrhosis: A retrospective study

Version: 1 Date: 8 July 2008

Reviewer: Edoardo Giovanni Giannini

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Major Compulsory Revisions.

1. The Authors studied a rather homogeneous population, in that 50% of patients were Child-Pugh class B. This likely justifies the observation that they found no difference in Child-Pugh score between patients with and without varices. In fact, this is at odds with previous observations reporting that patients with advanced liver disease are more likely to have varices. Table 2 should report Child-Pugh class distribution rather than Child-Pugh scores. Indeed, Child-Pugh classes are not quantitative variables and should not be managed as median and SD but rather as class distribution.

2. An interesting observation is that the Authors found that portal vein diameter was a predictor of the presence of varices both in uni- and multi-variate analysis. However, median portal vein diameter in the whole series was considered normal (i.e., <13 mm. Table 1), and was below 13 mm even in patients with varices. This result is at odds with what reported by Schepis and Colleagues who found an enlarged median portal vein diameter in their patients with varices. This taking into account the fact that Schepis and Colleagues included Child-Pugh class A and B patients alone, while in the present study 35% of the patients are Child-Pugh class C. Therefore, the Authors should report on the normal values of portal vein diameter in their population and on the variability of portal vein diameter in the present series of HBV patients.

3. I do agree with the Authors that non-invasive means aimed at evaluating the presence of oesophageal varices should be easily applicable to clinical practice. This is the reason why I do not understand the usefulness of calculating RF model. As a matter of fact, the RF model showed to be significantly superior to platelet count for prediction of varices. However, it is not better than either portal vein diameter or spleen width. Therefore, what can be gained by using this parameter? On clinical grounds measurement of both portal vein diameter and spleen width is easier than calculating RF model.

4. The Authors should try to identify, by means of ROC curves, the best cut-off for non-invasive diagnosis of oesophageal varices of both portal vein diameter and spleen width. If necessary they can use these cut-offs sequentially so as to correctly identify the highest number of patients.

5. Did the Authors try to normalise platelet count by spleen width (a sort of
platelet count/spleen width diameter)? Maybe calculation of this parameter may obviate for the use of multiple parameters.

Minor Essentials Revisions.

1. How many patients were on treatment for HBV?

2. The Authors state that none of the patients was on treatment with diuretics (page 4, Methods section, 7-8 line). However, 35% of the patients were classified as Child-Pugh C, this meaning that they likely had moderate/severe ascites. Anyway, 60.3% of the patients had ascites (page 6, 4th line). How were these patients managed? Did the Authors perform endoscopy before any therapeutic attempt aimed at relief of ascites?

3. The Authors introduce spleen width as a means for non invasive diagnosis of oesophageal varices. However, they should report data regarding the validation of this parameter in their hands (i.e., intra- and inter-operator variability).

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interest.