Reviewer’s report

Title: Portal vein thrombosis in liver cirrhosis: Incidence, management, and outcome

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Reviewer: Quirino Lai

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This is an interesting retrospective study from Japan investigating the role of danaparoid sodium for the treated of PVT in cirrhotic patients. The population is relatively small (n=90) but the results are interesting and relatively innovative.

Major comment:

1) It is not clear how the Authors constructed their multivariable model. I think it is correct to report that this statistical method has been used into the appropriate "Statistical Analysis" section. Moreover, the Authors reported in Table 3 that the model was constructed using patient survival as dependent variable. Such a datum is coherent with the results (age and HCC as risk factors and patent PV as protective factor). However it is not clear when the patency was defined, as the Authors wrote in the definition of Table 3: "Determinants of survival of patients with liver cirrhosis and portal vein thrombosis after initial treatment, repeat treatment and maintenance therapy." Which is the exact timing when they categorized the PV patency for doing this analysis?

In Page 13, line 238-240, the Authors report the erroneous sentence that "Multivariate analysis identified HCC (HR 3.2426, P=0.0038), age (>65 years; HR2.9734, P=0.0178), and main portal vein patency (HR 0.4149, P=0.0205) as significant independent risk factors of PVT". Such a statement is in conflict with Table 3, in which, more accordingly, the dependent variable is patient death and not PVT. In fact, all the patients investigated in the present series had a PVT. Moreover, why the Authors did not investigate the risk factors for definitive PVT treatment failure? I suppose such an analysis is surely more intriguing respect to the risk factors for patient death.

Minor comments:

1) English style reevaluation is needed.
2) Page 8, lines 131-134: I suppose the following sentence presents an error: "We divided the patients into two groups; patients of the acute type (n=27) developed cirrhosis within one month after hepatectomy (16 cases) or splenectomy (11 cases), while those of the chronic type (n=63) comprised all other patients."

I suppose the Authors intended that 27 patients developed PVT after hepatectomy or splenectomy.

3) Page 26, Table 1. I suggest to report in a more homogeneous way the reported numbers, using always the same digit of numbers after the comma. Also all the percentages need to be reported homogeneously. Such a coherence should be extended to the entire text.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

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I am able to assess the statistics

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