Reviewer’s report

Title: Hepatitis B Reactivation among 1962 Patients with Hematological Malignancy in Taiwan

Version: 0 Date: 13 Nov 2016

Reviewer: Abdullah Yeniova

Reviewer's report:

I have assessed the manuscript. The manuscript is suitable for publication with revisions. You can find the comments and contributions below.

1) This manuscript is worth for publication in the journal because it has made an important contribution in the field of Hepatitis B management for immunosuppressed patient population. This manuscript stated that type of the hematologic malignancy is not important for Hepatitis B reactivation. I believe that this is valuable for literature.

2) The authors did not give a data about Hbe status of the cohort. What is the rate of HBe negative Hbs Ag positive patients? They only defined chronic hepatitis B carrier status by the detection of HBs Ag positivity for 6 months. Although they did not stated that they measured Anti HBe antibody, did they have this data? Hbe status (Hbe Ag negative chronic hepatitis B) may be an independent factor for Hepatitis B reactivation

3) Is there a subanalysis of which oral antiviral agent protects from reactivation? This statistical information can make their manuscript more attractive

4) They reiterated the results in the discussion (Page 17 line 29-35). I think this is not necessary. This part and similar sections can be eliminated from discussion

5) One of the main conclusion is about the period of antiviral prophylaxis. 59 patient suffered form Hepatitis B reactivation although the received prophlaxis. What is the relationship between the discontinuation time of the prophylaxis and the reactivation for these 59 patient. Did they have the information about discontinuation time for these patients?

6) 41 patients had Hepatitis B reaction although they were not HBsAg positive. I understood that none of these patient were AntiHbc positive (Page 17 line 1 "All patients with HBV reverse seroconversion displayed negative anti-HBc serology"). But in Table 1 it was
stated that 19 of 41 HBs reverse seroconversion patient were AntiHbc positive and this was an independent factor for Hepatitis B reactivation in HBsAg negative group. How did authors define these patient as resolved hepatitis B? They were HBsAg negative and AntiHBc negative. Did they define this cohort with AntiHbs positivity and HBeAg positivity? I think it will be more appropriate for redefine the resolved Hepatitis B patients in the manuscript because they reach the conclusion that resolved Hepatitis B patient will not need prophylaxis when they receive chemotherapy or bone marrow transplantation for hematologic malignancy. This is an important theory and it conflicts with recommendation of major guidelines.

7) In table 1 authors gave some p values below 0.05 (Age, Hbs Ag (+) at diagnosis, HBs Ag (-) positive seroconversion, Hepatitis C, Hepatocellular carcinoma, Allogeneic transplantation). What is the significant p value for his Table. There were more than two groups for this comparison? Do they execute a posthoc analyse for significant statistics

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

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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Please indicate the quality of language in the manuscript:

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