First reviewer:

1. In the introduction, it wrote "To our knowledge of the current literature, a study that solely focuses on the relationship of esophageal variceal bleeding and patient health outcomes between different hospital settings has not been addressed." However, there are several similar studies focusing on the relations between hospital settings and esophageal variceal bleeding, such as ①Relationship between hospital volume and outcomes of esophageal variceal bleeding in the United States. Clin Gastroenterol Hepatol. 2008 Jul;6(7):789-98. ②Hospital experience and outcomes for esophageal variceal bleeding. Int J Qual Health Care. 2003 Apr;15(2):139-46. I suggest authors need not to emphasize that this issue has not been addressed before. They don't necessarily need to cite the above two references, either.

   This part was taken out of the manuscript. We agree that other studies looking at different hospital characteristics were reviewed.

2. In the study, (ICD-9 CM) diagnosis codes (456.0 and 456.2) were used to identify patients (≥18 years) hospitalized with a primary diagnosis of esophageal variceal bleeding admitted between 2008 and 2014. However, the ICD-9 code 456.2 is "Esophageal varices in diseases classified elsewhere" not necessarily EV bleeding. Can they explain ?

   For some reason we included this in our initial methods section but in reality we did not include anyone with the icd 9 code 456.2. All 12,024 patients had the code of 456.0

3. In the method Predictive Variables section, they wrote "These included blood transfusions (ICD-9-CM 99.00, 99.04, 99.05, 99.06, 99.07), balloon tamponade (ICD-9-CM 44.93 and 96.06), and portosystemic shunt (ICD-9-CM 39.1). Are these actually ICD-9-PCS code?

   These are all ICD9-CM procedure codes. The NIS database does not allow CPT codes but instead employs ICD9-CM codes.

4. In the statistical analysis section, they said they used Wilcoxon signed-rank test for continuous variables. However Wilcoxon signed-rank test is used for related samples. Were patients in teaching vs. non-teaching hospitals related? Please explain.

   That is correct. We changed the statistics to employ independent sample T test since these variables were two independent groups. When recalculating the statistical difference for the only two continuous variables in this study (LOS and cost) there was still a statistical difference between groups and this is reflected in Table 5.

5. In table 2, although comorbid conditions a as determined by the Elixhauser comorbidity index were not statistically significant between groups, liver comorbidities showed significant difference. It

means that more patients had liver comorbidities in teaching vs. non-teaching hospitals. So in table 3, this confouding factor should be added for adjusting mortalities. My suggestions and question is, did they consider put all items in table 3 and 4 for multivariate logistic regression to avoid all these confouding factors? That would be more convincing when they made the impression that mortality were higher in teaching hospitals versus nonteaching hospitals when controlling for other confounding factors.

6. In table 3, in the unadjusted mortality, the 95% CI of Medicaid insured spanned across 1, thus they did not show significance. Please also check the result mortality section on the description of medicaid.

   That is a good point. I deleted that wording from the results section under mortality. Thank you.

7. In table 4 in the SBP variable, the mortalities between teaching and non-teaching hospitals was25.2% vs. 6.9%a respectively, a great difference. Please explain why there were not significant difference between them.

   This has been corrected. There was a statistical difference. It was not portrayed in the initial manuscript but now has been adjusted. Thank you.

Second reviewer:

First, There are selection biases in this study. Teaching hospitals had a greater percentage of transfers from outside acute care hospitals compared to non-teaching hospitals and teaching hospitals were more likely to admit patients with hepatic decompensation, hepatorenal syndrome, and HCC when compared to non-teaching hospitals.

Moreover, teaching hospitals had a higher rate of balloon tamponade and TIPS procedure. This factors contribute a higher mortality and morbidities in teaching hospital.

Second, Important variables such as initial hemoglobin, blood pressure, endoscopic finding and endoscopic hemostasis rate should be analyzed. This factors contribute mortality and mobidities.

All of the above factors are indeed true and thus were limiting factors in this study. We attempted to control for such factors as liver decompensation (HCC, ascites, HE, SBP) as well as transfer to and from other hospitals and still found a higher mortality in teaching hospitals.

Values for hgb, vitals, and endoscopic findings would definitely add value to the study but are not available in the NIS databse. Thus we attempted to decipher the patient's severity of illness by incorporating Elixhauser comorbidities and coagulopathy, etc to capture how sick the patient was.

Revision Review

Reviewer's code: 02860885

Question 5:

In table 2, although the Elixhauser comorbidity index were not statistically significant between groups, liver comorbidities showed significant difference between teaching vs non-teaching hosopitals. It means that more patients had liver comorbidities in teaching than in non-teaching hospitals. So a multivariate logistic regression combining all variables listed in Table 3 and 4 is suggested, to avoid all these confounding factors.

Otherwise, the authors should change the conclusions such as: In patients admitted for esophageal variceal bleeding, mortality, length of stay and cost were higher in teaching hospitals versus nonteaching hospitals. However, this conclusion is limited by some confounding factors that are not available in NIS databse.

Answer:

I apologize that I did not answer this question on the first round of edits. You are correct in noting that liver related comorbidities and management were confounding factors that may have affected the mortality findings. However, we included both of these in the final logistic regression analysis to decrease the confounding factors. The liver related comorbidities were written as "evidence of decompensation" in our paper and were all included in the final logistic regression analysis. We therefore feel that our findings are robust and have lower chance of bias. However, as noted in the conclusion the NIS database has its own inherent bias due to being an administrative database. We hope that this answers your question. We did not have to edit the manuscript in light of the above answer. Thank you!