Reviewer 1:

Dear Authors, you presented a well-written manuscript presented a retrospective study with the research question if the acute esophageal variceal bleeding, which is a common complication of liver cirrhosis, might precipitate to the development of multi-organ failure, causing acute-on-chronic liver failure (ACLF). I found that your inclusion and exclusion criteria were precisely defined and your research results were clear presented. The conclusion that ACLF is independently associated with higher mortality in liver cirrhosis patients with acute esophageal variceal bleeding may find important clinical implications. I have no further questions or queries, pertaining to your manuscript. Best Regards

Thank you for your positive feedback on our manuscript, we appreciate your thoughtful comments. We are glad to hear that you found our inclusion and exclusion criteria to be well-defined and that our research results were clearly presented. We worked diligently to ensure that our study was rigorous and that our conclusions were supported by our data. We agree with your assessment that the association between ACLF and higher mortality in liver cirrhosis patients with acute esophageal variceal bleeding is an important clinical implication. We hope that our study can contribute to the ongoing efforts to improve the diagnosis, treatment, and outcomes for patients with liver cirrhosis and acute esophageal variceal bleeding. Thank you again for your review and feedback.

Reviewer 2

The authors analyze the association of ACLF with mortality of cirrhotic patients, hospitalized with acute esophageal variceal bleeding. The adverse effect of ACLF and its mechanisms in this clinical situation has been established and well described previously. Several studies have also shown that portal pressure control in patients with ACLF, in particular, by non-selective beta-blockers or TIPS, reduces the risk of variceal rebleeding and improves survival. Thus, the authors should more clearly show the novelty of their research. They should think about stratifying patients by risk groups and identify those who need more aggressive treatment than only non-selective beta-blockers (e.g., TIPS), reducing the severity of systemic inflammation, as well as liver transplantation (I do not know the capabilities of this institution). This requires a large-scale prospective cohort. I strongly recommend that the authors re-read the text and correct typos (e.g., "thypersplenism"), explain the abbreviation "DC", make Figure 1. more readable.

We appreciate your positive feedback on our manuscript and we would like to respond to your comments. Firstly, we agree that the novelty of our study is the association between ACLF grade and mortality in AEVH patients. We have highlighted this in the discussion section of our manuscript.

Secondly, we understand your suggestion to stratify patients by risk groups; however, this is a historical cohort study, and the number of patients in our dataset is not adequate to allow for further stratification.

Thirdly, we agree that TIPS or liver transplantation could be potential treatment options for patients with AEVH and ACLF. However, our institution does not perform TIPS or liver transplantation, and patients would need to be transferred to another city for these procedures, which is challenging.

Fourthly, we have corrected the typos in our manuscript and thank you for bringing these to our attention, and we have attached a pptx version of Figure 1.

Lastly, we agree with your recommendation that a large-scale prospective cohort study would be more adequate to further investigate the association between ACLF and AEVH. We will consider this for future research.