Yang et al. explore factors that predict short-term mortality among dialysis patients who experience acute nonvariceal upper gastrointestinal bleeding (ANVUGIB), using a national retrospective cohort of all treated U.S. dialysis patients (USRDS). They identify peritoneal dialysis and dialysis vintage as dialysis patient-specific risk factors for mortality (beyond the risk factors already identified in the general population, including age and comorbid conditions) after ANVUGIB. While the question is important, given the high prevalence of ANVUGIB in this population, and the methods seem generally sound, the paper could benefit from some clarification and acknowledgement of further limitations.

Major Compulsory Revisions

1. The research question appears to be determining the predictors of 30-day mortality after an ANVUGIB episode. However, later in the Methods, the authors discuss the use of a development and validation cohort, the Appendix lists all the AIC information, and in the Discussion they note that they were not able to develop a risk prediction tool (“Hence, the c-statistic of the fully-adjusted model was only 0.68, which precluded us from developing a robust prediction scoring system”). Was this part of the research question as well? If the authors want to include this information they should make this part of the research question to make the information seem less tangential to the reader. Alternatively, they could remove these references and report only on the predictors in overall cohort, which is still interesting and worthwhile.

2. In the methods it seems the definition of ANVUGIB episodes is based on outpatient claims. But later on, the authors have defined “hospitalized” vs. “not hospitalized” events. When the authors discuss ICD-9 codes, they should make sure to note that they are using both types of claims.

3. Do people die of ANVUGIB episodes without ever being hospitalized? If there are any data on this I might include it in the limitations; or, if this is a highly unlikely event, this should be stated as well, so that the reader can be assured that any such bias is minimal.

4. The authors define history of ANVUGIB back to 1996 in the records. However, if a patient is <65 and not yet receiving treatment for ESRD, they would not have any records of previous episodes. How was this handled? This potential ascertainment bias is a big concern since the authors found that having an ANVUGIB history was associated with decreased risk of mortality, which seems
a bit counterintuitive.

5. The authors used complete case analysis and state that “only 0.04% and 0.35% patients had their race and dialysis modality missing”---but what about the many other variables in the model? What was the total number of non-complete cases? And did they differ from the complete cases?

6. It seems from the methods that models were run with main effects and interactions, which is fine, but please ensure that the reported main effects are not taken from the models with interaction terms.

7. The authors may want to further consider the results with dialysis vintage. It is only a significant predictor after adjustment for other factors. Thus, some caution in the interpretation of the association is warranted.

Minor Essential Revisions

8. The authors state “To avoid concerns about multiple comparisons while exploring for interactions among covariates, we randomly split our data...” I am not sure how this addresses multiple comparisons, or why this is a concern. Could the authors clarify?

9. Explain the 10%, 90% range in the Methods, are these just percentiles?

10. Table 1---these are really episode characteristics rather than patient characteristics, since some patients had more than one episode. I would suggest showing patient characteristics as suggested by the title. The % with 1 episode and % with 2+ episodes could be provided. Table 2 is OK at episode level.

11. The authors refer to “first” and “repeat” episodes---note that some may not be “first,” particularly among the younger patients. It is really the “first” episode after baseline, which may or may not be at the start of dialysis.

12. “To our knowledge” should probably be added to “Furthermore, we tested the associations between dialysis specific parameters (such as dialysis modality and dialysis vintage) and ANVUGIB mortality for the first time.”

13. The sex-/CAD-stratified results are somewhat confusing nested within Table 3. I would suggest a separate table, particularly since much of the discussion is devoted to these results.

Discretionary Revisions

14. The paper would benefit from a careful proofreading. In addition to typos and grammatical issues, there are several places where words are missing, e.g. “In spite [of the fact?] that the risk for mortality from ANVUGIB was 5-10 times higher among patients with kidney disease compared with patients with normal or near normal kidney function” in the introduction.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published
**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests