Response to Reviewers – BGS (Responses submitted prior to revision)

Per Biogeosciences workflow, we submit responses first. Where we indicate changes to the manuscript below, these are commitments of what we will do in a revised manuscript if invited to submit one. For clarity, reviewer comments are in black; our responses are in blue and bold.

RC1

The manuscript investigates mortality processes at two coastal sites following inundation, considering different forest types (coniferous and broadleaf) and salinity levels. The authors use the FATES-Hydro model, constrained by various observational data, to ensure that simulated patterns align with observations. The study identifies relevant conclusions about the mechanisms driving inundation-induced mortality, offering testable hypotheses to guide future field-based research. I recommend accepting the manuscript after considering the following comments:

There is some ambiguity in the manuscript regarding whether the study investigates differences between forest types (i.e., broadleaf vs. conifer) or between two specific species to which the model is calibrated. Generally, I would caution against equating a plant functional type (PFT) in a model with a species, as these are conceptually distinct. While the PFTs were parameterized using observational data, does this justify interpreting the model outcomes as species-specific effects? If so, are these species sufficiently representative of their broader forest types to support generalization of the conclusions? Clarification is needed, as the manuscript currently uses both species and forest types somewhat interchangeably.

Thank you for pointing this out. We agree that PFTs should not be equated with species. In the revised manuscript, we consistently refer to the modeled vegetation as broadleaf tree species and conifer tree species rather than functional types. We also added text in the Discussion acknowledging that our parameterizations are based on specific representative species (Carya, Quercus, and Pinus), and that outcomes may differ among species within each group. We further caution against overgeneralization and note that future work should test whether these findings extend to the broader PFT level.

Additionally, how can the results be interpreted in the context of future climate change and projected sea level rise? Are there existing projections for Lake Erie and Chesapeake Bay, and what implications might these have for the local ecosystems? Given the model identifies clear patterns, it would be valuable to discuss how these findings relate to the future vulnerability of these forests to inundation.

This is an excellent point. In revision, we will add discussion linking our results to projected hydrologic changes. Specifically, Chesapeake Bay is projected to continue experiencing sea-level rise of ~3–6 mm yr^{-1}, approximately twice the global average, and Great Lakes water levels are projected to become increasingly variable under climate change. These projections imply greater frequency and intensity of

inundation, suggesting that the mechanisms we identify (root loss leading to hydraulic failure) will likely intensify in the future. We also note that we are addressing this issue more fully in a separate manuscript (submitted to JGR–Biogeosciences), and we refer readers there for more detailed projections.

Overall, the manuscript is well-written and generally clear. However, improvements could be made, particularly in reducing the number of figures in the main text. Several figures support only a few statements and could be effectively referenced in the supplementary materials. I suggest moving Figures 2, 7, and 8, as well as Panel 1A, to the appendix. Of course, the authors are free to ignore this suggestion, but eight figures in a 1.5-page results section seem excessive. Additionally, the preprint version lacked figure labels, which should be corrected.

We appreciate this suggestion. We considered moving Figures 2, 7, and 8 to the Supplement, but we decided to retain them in the main text for reader convenience. These figures are central to interpreting our mechanistic results, and keeping them in the main manuscript avoids forcing readers to switch repeatedly between the text and supplementary material. We have also ensured that all figure labels are now complete and correct.

Lastly, could the authors clarify which host land surface model is used with FATES-Hydro? To my knowledge, FATES-Hydro can be run with either CLM or ELM, but this is not specified. This is important context, as the host model determines the soil processes, which may influence the results.

The host model is ELM. In revision, we will state this explicitly in Methods: "All simulations were run with FATES-Hydro within the E3SM Land Model (ELM)."

small corrections:

Line 175 given number of soil layer? how many? **There are 20 soil layers. We will add this information in methods section in revision**

line 194 maybe better to refer to it as hypoxia reduction as this is the term you used before or introduce saturation reduction more clearly revised accordingly **We will revise to "hypoxia reduction"**

line 236-238 check this sentence, it seems to miss some parts

Thanks for pointing this out. We will revise the sentence to "Because marsh plants are annual or bi-annual, phenology and maximum density (number of individuals per ground area), which control the variation of total leaf area, play a more important role in marsh ecosystems than plant physiology."

line 294 this should refer to Figure 3 I think **corrected** line 331 should be figure 6 I suppose? **corrected**

line 338-339 Within the results section the broadleaf simulation was described as stable GPP and LAI, however now in the discussion this is interpreted as an increase. I see that the trend is stable to slight increase but please be consistent in terms of interpreting this to avoid confusion on the reader side **Thanks for the suggestion. Revised throughout to describe the trend as “stable with a slight increase” for consistency.**