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.

RC2

The submitted article presents a modelling study of the responses of coastal forest ecosystems to rising sea levels at Lake Erie (freshwater) and Chesapeake Bay (saltwater). The manuscript is well written, the methodology is clear and the results are presented in an intuitive manner. The results are discussed logically and lead to hypotheses for future research. I recommend the article for publication after some improvements and corrections. A few comments that the authors should consider.

(i) The most significant criticism coincides with the first point of the other reviewer: You parametrised the model for broadleaf and coniferous forests. Later in the text, however, you refer to these vegetation types as 'species'. This is incorrect. You mention the issue of different species' adaptation strategies (e.g. mangroves), but I think you should add a paragraph to the discussion about parameter ranges for each type, explaining why your parameter sets are representative of coastal forests and specific to either broadleaf or coniferous species. For example, the trade-off between hydraulic conductivity and hydraulic safety can differ tremendously between species (e.g. McElrone et al., 2004).

McElrone, A. J., Pockman, W. T., Martinez-Vilalta, J., & Jackson, R. B. (2004). Variation in xylem structure and function in stems and roots of trees to 20 m depth. New Phytologist, 163(3), 507–517.

We agree with this important point. As noted in our response to RC1, we now consistently refer to "broadleaf tree species" and "conifer tree species" in place of PFTs. We have also added a paragraph in the Discussion highlighting that parameter choices were based on representative species, and that interspecific variability (e.g., trade-offs in hydraulic traits as described by McElrone et al. 2004) could lead to different outcomes. We emphasize that further studies are needed to determine whether our findings can be generalized to the PFT level.

(ii) The article evaluates model simulations from the FATES-Hydro model. As the results are deterministic, some of the statements in the discussion are somewhat unsatisfactory. For example, in line 362, the authors state that the results were unexpected. Unlike a field study, a modelling approach makes it possible to track the reasons for some model behaviour. A sensitivity analysis could provide insight into which parameter combinations could produce the desired model outcome. Whether or not to include a sensitivity analysis is up to you, but I would recommend tracing unexpected results back to the source. With such information, the discussion could explain more specifically why an expectation is not met by the model results.

We agree that the word "unexpected" was misleading. If invited to revise, we will revise the text to: "We had anticipated that broadleaf trees might experience greater carbon limitation due to higher leaf area and photosynthetic demand. However, the simulations showed that hydraulic failure associated with root loss occurred before substantial NSC depletion, leading to similar mortality trajectories across tree types." While we did not conduct a full sensitivity analysis here, we note in the Discussion that parameter adjustments within observed ranges consistently led to the same outcome: hydraulic failure dominated tree mortality.

Further I got some minor remarks:

Line 139: 20th or 21st century? **We will correct to 20th century.**

Lines 170-180: Is there a connecton between transpiration and root water uptake in the model? W**e will revise the Methods to clarify that transpiration is the sum of root water uptake from all soil layers.**

Line 195, eq. 2: kr_red,sat, in Fig S2: y-axis is named **We will correct to kr_red,sat in the figure label**

Line 198 and fig S2: How is b in the the graphs? Parameter b defines the intercept (scaling of the logistic function)**. Parameter b is set to 1 and its role is affecting the interception of the reduction function. We will add this in the revision.**

Line 203, eq. 203: doesn't that depend on the time step? To avoid that issue, I would suggest to be consistent with units and perhaps add a time step length delta_t. E.g. kc in psu^-1 * day^-1, acc_sal in psu*day, and in eq. 3B multiply with delta_t

We will add unit clarification and note that including Δt in Eq. 3b would resolve timestep dependence

Lines 212ff: Although you refer to the table in the sup. mat., I would suggest to explain briefly new symbols in the text**. Revised to introduce key new symbols (e.g., Vcmax, P50gs) directly in the text.**

Line 249: Fig. S5 **We will correct**

Line 268: … initialization. The … **We will correct**

Line 294: Fig 3a, b **We will correct**

Line 306: Table S3 **We will correct**

Lines 329ff: This sentence doesn't match very well with fig. 7. **We will correct fig citation to be (Fig 5 and 6)**

Line 359: McDowell et al**. We will correct citation format**

Line 372: It is not about species, is it? **Revised wording to avoid implying species-level generality: "Simulated k/kmax and mortality of broadleaf and conifer tree types changed similarly with root loss (Fig. 4), despite large differences in their leaf economic traits, wood anatomy, crown allometry, and phenology."**

Line 374: Why is that "… whole-tree k/kmax can only be as high as the lowest k/kmax of any pathway between the soil and foliage." That doesn't seem logic to me. **Thank you for pointing this out. If invited to revise, we will revise to: "Whole-tree hydraulic conductance is constrained by the lowest conductance along the soil–plant–atmosphere pathway. In our simulations, root loss strongly reduced soil-to-root conductance, which therefore set the limit for whole-tree k/kmax.".**