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

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 thank the reviewer for this constructive critism. We have revised the manuscript to consistently use "broadleaf trees" and "conifer trees" . We have also added a new paragraph to the Discussion as suggested, highlighting that parameter choices were based on representative species of coastal forests (*Carya*, *Quercus*, and *Pinus*), 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 broader 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.

The reviewer makes an excellent point about tracing model outcomes. We have removed the word "unexpected" to avoid ambiguity. In the revision, the following revised text now clarifies our initial hypothesis and the model's mechanistic

outcome: "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 trees". While we did not conduct a full sensitivity analysis here, we note in the Discussion that our exploration with different parameter sets within observed ranges consistently showed that hydraulic failure was the dominant tree mortality driver.

Further I got some minor remarks:

Line 139: 20th or 21st century?

Corrected to "20th century".

Lines 170-180: Is there a connecton between transpiration and root water uptake in the model?

We have clarified this relationship in the Methods, stating 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

Corrected the y-axis label to kr_red,sat.

Line 198 and fig S2: How is b in the the graphs? Parameter b defines the intercept (scaling of the logistic function).

We have added clarification to the text and Table S1 that parameter b is set to 1 and its role is affecting the interception of the reduction function.

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

The reviewer is correct that the formulation as written would be timestep-dependent. As our model has a fixed time step, we have revised the formulation and description for clarity:

$$acc_{sat} = \max\left[0, \sum_{i=0}^{n} (Sal_{Soil,i} - Sal_{cr}\right] \qquad \text{(Eq. 3b)}$$

where acc_{sat} represents the cumulative salinity stress, calculated by summing the difference between soil salinity (Sal_soil,i) and a critical threshold (Sal_cr) over all timesteps (i) up to the current step n. All terms are in PSU. As this formulation is dependent on the model's timestep, all simulations were run with a fixed temporal resolution of 30 minutes. Lines 212ff: Although you refer to the table in the sup. mat., I would suggest to explain briefly new symbols in the text.

We have revised the text to introduce key new symbols (e.g., Vcmax, P50gs) directly in the main text.

Line 249: Fig. S5

Corrected.

Line 268: … initialization. The …

Corrected

Line 294: Fig 3a, b

Corrected

Line 306: Table S3

Corrected

Lines 329ff: This sentence doesn't match very well with fig. 7.

The figure citation has been corrected to (Fig 5 and 6).

Line 359: McDowell et al.

The citation format has been corrected.

Line 372: It is not about species, is it?

The wording has been revised to avoid implying species-level generality: "Simulated k/kmax and mortality of broadleaf and conifer trees 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. We have revised it for clarity: "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.".