Biogeosciences Discussions

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Comment on bg-2021-255

Anonymous Referee #1

Referee comment on "Predicting mangrove forest dynamics across a soil salinity gradient using an individual-based vegetation model linked with plant hydraulics" by Masaya Yoshikai et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-255-RC1, 2021

Summary:

Yoshikai et al. provide a novel modeling study to understand and predict mangrove forest dynamics across a soil salinity gradient. The study added a plant hydraulic module, dynamic allocation module, and nutrient (nitrogen) limitation on growth into SEIB-DGVM. The new model allows for consideration of soil salinity effects on plant ecophysiology as well as soil nutrient levels in mangrove forests. After calibration of two parameters determining allocation and stomata that are unavailable from literature, the model can well represent the spatial gradient of forest structure (mean DBH) and biomass (AGB) across a salinity gradient in a mangrove forest in Japan. Other model-data comparison is also presented. Altogether, the authors conclude that including hydraulic trade-offs and differences in the ability to deal with salinity is critical and adequate for predicting dominant forest dynamics in mangrove forests.

Comments:
I really like the study, which extends the existing plant hydrodynamic modeling framework (often used and calibrated in arid/semi-arid ecosystems) to coastal saline ecosystems (also water-stressed). The idea of plant hydraulic control on mangrove forest dynamics existed for some time but the study presents a novel modeling study to evaluate the idea together with field data. Overall, the manuscript is well written and includes adequate details for understanding the model. I have three major comments about model diagnostics, which hopefully can improve the manuscript.

First, in my opinion, the key evidence to the manuscript's conclusion is Fig. 7&8, which shows how simulated forest structure and biomass match with observed values across the salinity gradient after only modest model tuning (2 parameters in Table 2). However, it is always more important and interesting to know why the model can reproduce the observations. What trait/parameters/processes are dominant in driving the model output. Is it salt filtration efficiency? P50?, Psi_lk?, or beta_0? I would suggest running some sensitivity tests to show what traits/parameters lead to the pattern in Fig.7 and how

important is the tuning of Psi_lk and beta_0 (their differences seem to be small). In fact, I am curious about whether salt filtration efficiency or P50 is more important, or maybe they have to be coordinated in the model to explain the observed pattern. Such information will make the study more useful.

Second, compared with the plant hydraulics-salinity interaction, the efficacy of two other new modules - dynamic allocation and nutrient limitation is not well demonstrated. For example, Fig. 9 shows the huge plasticity of allometry in the model without much support from empirical data. Fig. S1 seems to suggest the allometric plasticity is observed but it is really hard to relate. Meanwhile, Fig.5 shows that including a more realistic DIN gradient did not improve the model results. Consider either including some more empirical supports or make them less central to the manuscript.

Third, it is strange that no outputs from the new hydraulic module (e.g. leaf water potential diurnal cycle and seasonality) is presented, which is important to show the performance of the new plant hydraulics module. Finally, why not make the codes publicly available (line 579), especially given the new model is built on several other open-source models.

Some minor comments along with the order of the text:
L. 26-27: maybe I missed it but which figure shows the self-thinning process supported by field data?

L. 120. Figure 1a, please use Mainland China or Fujian (the province) when all the other names are at province/prefecture level

L. 171. Fig. 2 I remembered most physiological processes use daily time steps in the original SEIB-DGVM? So the photosyntheis module was rewritten to hourly time step in this study?

L. 184-186. I am not an expert on hydraulics in saline waters but is the osmotic potential also determined by temperature?

L. 397. I guess tree size distribution is also available? Why not compare the simulated and observed size distribution in addition to mean DBH (maybe in supplementary)

L. 405. Fig. 8. It seems the model generally underestimates tree density? Any explanations?