Dear Editor and Referees,

Thanks again for your valuable comments and suggestions. Attached please find our detailed point-by-point responses (blue texts) to all referees' comments, and changes (purple texts) we made in the revision. We look forward to hearing from you.

Sincerely,

Yilin Fang and Co-authors

Response to Anonymous Referee #2

We thank the referee for the suggestions to further improve our manuscript.

Section 3.1 should have a more scientific name (and does not say much), could be the title of the caption of Figure 3. Then it is clear that this section is all a sensitivity analysis. In some instances the text does not refer to simulated AGB or simulated GPP, (lines 479-482) when it should. Best to make sure either refer to simulated or observed fields, so it is clear.

Thanks for the suggestion. The title of Section 3.1 is now changed to "Model sensitivity to lateral flow representation". We clarified in lines 479-482 by referring to "simulated AGB".

Discussions section lines 714-717.. this is all model word..not obs, authors should revise the text and refer to simulations when it should be.

Thanks! We removed lines 714-717 in response to another reviewer's comment.

-Discussion on observed AGB variability : need to include the fact that wood density also affects mortality.

Thanks! We added the following discussion of wood density effect on mortality and the observed AGB variability:

Also not accounted for by the model is the negative relationship between the wood density and tree mortality rate at BCI found in McDowell et al. [2018] using data from Wright et al. [2010]. Including spatially variable tree mortality based on the negative relationship between mortality and wood density may substantially improve model representation of vegetation carbon as indicated by the modeling study in Hancock et al. [2022].

References:

Hancock, M., Sitch, S., Fischer, F. J., Chave, J., O'Sullivan, M., Fawcett, D., and Mercado, L. M. (2022), Modelling the impact of wood density dependent tree mortality on the spatial distribution of Amazonian vegetation carbon, Biogeosciences Discuss. [preprint], https://doi.org/10.5194/bg-2022-87, in review.

McDowell NG, Allen CD, Anderson-Teixeira K, Brando P, Brienen R, Chambers J, Christoffersen B, Davies S, Doughty C, Duque A et al. (2018), Drivers and mechanisms of tree mortality in moist tropical forests, New Phytol, 219(3), 851-869, https://doi.org/10.1111/nph.15027.

Wright SJ, Kitajima K, Kraft NJ, Reich PB, Wright IJ, Bunker DE, Condit R, Dalling JW, Davies SJ, Díaz S et al. (2010), Functional traits and the growth–mortality trade-off in tropical trees. Ecology 91: 3664–3674, https://doi.org/10.1890/09-2335.1.

Response to Anonymous Referee #3

Review of 'Modeling the topographic influence on aboveground biomass using a coupled model of hillslope hydrology and ecosystem dynamics'

We thank for the reviewer for the constructive comments and suggestions. Please see our point-by-point responses below.

This study presents a unique effort to link an earth systems model, a hillslope hydrology model and a structured forest and vegetation function simulation model at a well known tropical forest study site in Panama.

Primary findings in my view are that while the model has not been tuned for the site and has unrealistic mortality and biomass response (quantitatively), qualitatively it shows an interesting and promising ability to produce biomass, soil moisture, WTD and biomass turnover gradients over hillslopes. This is the result of coupled hydrological ecosystem and forest regeneration dynamics played out over periods of water stress expected in this seasonal tropical forest, unique outcomes for a unique modeling effort. This in and of itself represents key progress for the field. The value of this work at BCI will only increase from here if further steps are taken to improve model structure, tune the model and compare it more rigorously against the rich datasets available, particularly for biometrics/forest dynamics inside and (if possible) outside of the 50 ha plot. A rigorous validation analysis through time could even be used to test mortality models against one another in a model testing framework (rather than to show, as this study does as a first step, that different water stress mortality formulations have different outcomes).

Thanks for the nice summary of our study and your kind recognition of the promising ability of our modeling of coupled key processes and recommendations for future studies. We added discussions in response to your concerns and suggestions with respect to the boundary conditions, the relevance of the regression analysis, the importance of soil texture and PFT compositions, and places identified for clarifications. Please see the details provided below for each point.

The study may however be limited by a few important pieces, even when recognizing its place as a stepping stone. One is that the primary variation of interest appears to occur between the slope outside of the plot and the state inside the plot, which is relatively homogeneous (an issue familiar to 50 ha plot network research, an advantage of the data for cross site work and perhaps disadvantage for within site work). Nevertheless, this study provides model evidence that including hillslope hydrology creates WTD and associated seasonal soil moisture gradients that can make slopes and topographically lower areas more resilient to water stress, and enhance biomass in these areas. That is interesting and addresses current hypotheses and debate (see suggested consideration of Costa et al 2022 New Phytologist and Sousa et al 2022 GEB references below). Nevertheless, it seemed that this finding--of biomass differences over hillslopes--could have been a greater point of focus for the discussion in the manuscript particularly given that the Mascaro et al 2011 citation appears, which supports this with remote sensing analysis (this citation concludes that there is lidar based evidence with a map of biomass increasing on the slopes). I was also perplexed by the following: Is the spatial correlation between modeled and observed biomass non significant inside the 50 ha plot for all of the simulations? That might be expected due to low driver variation. However, I would clarify this as a result, and will return to the issue below.

Thank you for the references. We have included them in the discussion and details are provided later in each relevant point below.

The spatial correlation between modeled and observed biomass is not significant inside the 50 ha plot for all of the simulations reported. We made the clarifications and explanation in the revision in Section 3.1:

The spatial correlation between modeled and observed biomass is not significant inside the 50 ha plot for all of the simulations because of homogeneity of the meteorological forcing, soil properties, and gentle topography.

A concern that I saw in relation to the particular hillslope modeled results was the issue of the impermeable boundary conditions/no lateral flow at the boundary. If I understood this, there is no outflow on the edges of the modeled scene, which all fall just below the plateau … ? Is there any chance that the modeled water is unrealistically 'pooling' in the soil, increasing WTD artificially here..? If it is easy to refute this, great! I would add a small statement to such an effect. I know that computation is a limitation (as is geography of the island), but I wondered if a larger scene would serve the analysis better in the future for this reason.

There is no outflow at the outer boundaries of the model domain. We agree that the water table may be artificially increased near the outer boundary because of the imposed boundary conditions. We did an independent test using the water level in Gatún Lake at the outer boundaries and we found that only the results in the grids within 100 m of the outer boundaries are affected. There are locations with shallow water table away from the outer boundaries (Figure 3) that falls in the range with negative AGB relationship with WTD, so our conclusion holds. Future studies will consider a larger domain as suggested. The following statement is added in Section 3.3:

Note that the water table may be artificially increased near the outer boundary due to the no flow assumption, but a sensitivity experiment using the water level in Gatún Lake at the outer boundaries shows no impact on the conclusion that lower areas are more resilient to water stress and has more biomass.

I see the value of the regression tree analysis to summarize and capture the predictability of drivers of the simulation outcomes. However, I felt that the specific details and discussion could have been de-emphasized relative to other issues, such as those related to improving the model realism to improve predictions across demographic, flux, and soil moisture variables. Specific RT results are not likely to remain of interest if the model is improved, suggesting that the focus should be on what RT analysis can reveal and how eventually it can complement empirically driven analysis.

Thanks for the comment. The purpose of the regression tree analysis is not to summarize and capture the predictability of drivers of the simulation outcomes. We used the analysis to determine if similar relationships between the selected features (topography, soil water state, etc.) and AGB can be established from the simulations and from the observations in the 50-ha plot. For example, if the RF model indicates that the observed AGB can be explained by the soil water state but the simulations do not capture such relationship, we can then focus on how to improve simulation of soil water state and its relation with AGB using the coupled model. On the other hand, if neither the observed nor simulated AGB can be explained by the selected features, our model needs to account for processes that are not currently represented (e.g., nutrient gradients). We made changes in the Introduction as shown below to further clarify the purpose of the RT analysis and in the Discussions and Conclusions section, we removed the discussion of RF that was built based on the modeling results and focused only on what we learn from the RT analysis of observed AGB with selected features, which highlights that factors other than the selected features may play more important roles in controlling AGB in the study site.

The purpose of the RF models is to reveal whether there are similar nonlinear relationships between topography, soil water states, and AGB in the coupled simulations and in the observations. This analysis may reveal model limitations in capturing certain nonlinear relationships found in the observations and inform future efforts to improve modeling of coupled hydrology-vegetation processes.

The discussion of the dependency on water stress mortality kernels ended up somewhat superficial relative to my expectation from the presentation of the different models (which included some specification and description of the mechanisms). Why was it the M1 mortality function that led to such relatively weak dependency of AGB on water table depth while the others showed such drastic (and likely unrealistic) dependencies? That these models have different impacts overall is not surprising.

The weak dependency of AGB on water table depth with the M1 mortality function is because the plant wilting factor (Eq. 12) calculated at the site is much larger than the prescribed threshold of 10^{-6}, which results in no hydraulic failure induced mortality. The drastic dependencies of AGB on water table depth for the M2 and M3 mortality function seems to be unrealistic, other functions or thresholds to trigger the hydraulic failure should be explored.

The following sentence is added to explain the effect of the M1 mortality function and point out the drastic AGB dependencies with M2 and M3:

AGB from ELM-PF-F1-M1 is the least sensitive to water table depth because the plant wilting factor (Eq. 12) calculated at the site is much larger than the prescribed threshold of 10^{-6}, which results in no mortality due to hydraulic failure.

However, the fast decrease of AGB with WTD using the M2 and M3 functions seems to be unrealistic and requires future exploration.

What is the role of vertical soil water stratification and how was this implemented in the model? In a few cases it seemed averages over vertical soil water profiles were taken for analysis (but not totally clear). I realize that this is not a focus and thus does not justify great expansion, however, the paucity of discussion seemed incomplete with negative consequences for understanding. For example, FATES hydraulic redistribution capacity seemed potentially important, which made sense to me when mentioned I think only because I know a little bit about FATES structure. I would suggest a clear sentence or two about vertical soil gradients in the methods. Related to this, it was unclear how the model produced water table depth predictions and how this was related to soil moisture. Finally, I will add that while it seems not a direction of this study at the moment, a lot of attention is being given to vertical stratification of water uptake, which may differ by tree size and function strategy (e.g. Chitra-Tarak et al 2021. New Phytologist)

The vertical soil water stratification is solved by the model from the mass balance equation in Eq. 1 and the mass equation of water from the soil to the plants and atmosphere in FATES-hydro. When discretized numerically into grids in 3D space, Eq. 1 equates the time rate of change of water mass within a grid with the mass fluxes of water across the surfaces of each grid and water source/sink. This results in a matrix equation including every grid, both horizontally and vertically. The water table is the surface where the water pressure head is equal to the atmospheric pressure. It was calculated by the hydraulic head of the first water saturated (i.e., the soil moisture equals the porosity) soil layer from the ground surface. Similarly, a matrix equation is solved for FATES-hydro, which automatically takes into account of the vertical stratification of water uptake depending on the total potential between the soil and the plant.

We added the following statement for clarification in the revision in Section 2.1.2:

When discretized numerically into grids in three dimensions, Eq. 1 equates the time rate of change of water mass within a grid with the mass fluxes of water across the surfaces of each grid as well as water source/sink. This results in a matrix equation including every grid, both horizontally and vertically. The water table is the surface where the water pressure head is equal to the atmospheric pressure. The surface was calculated by the hydraulic head of the water saturated (i.e., the soil moisture equals the porosity) grid near the ground surface.

The vertical grid resolution was added in Section 2.4:

The grid resolution for ParFlow in the x and y directions is 90 m and varies from 7 mm (near the ground surface) to 35 m (near the bedrock) in the z direction.

Soil structure is a huge issue that was not clearly addressed in the 'factors limiting inference of field data' discussion. Soil structure is an essential complexity related to hillslopes in tropical forests. In much of the tropics uplands are more clayrich than lowlands, which are sandier or siltier in general. I do not know about BCI. This has a big effect on flow dynamics obviously. See discussion of this in the Costa et al 2022 paper.

Thanks! We added the following sentence in Section 4 of the revision:

It is necessary to have a better quantification of the soil texture and related hydraulic properties as the distribution of biomass is the combined result of plant traits, soil properties, climatic and groundwater conditions [Costa et al., 2022]. AGB can be influenced by soil texture which directly affects the time interval between precipitation inputs and groundwater recharge [Sousa et al. 2022] and the capillary fringe above the water table that supplies water to the rooting zone

[Costa et al. 2022]. For example, results in Sousa et al. [2022] suggest a contribution of clayey texture in increasing AGB in dry climates with a shallow water table.

In general, I appreciated that the discussion was concise, but I would have foregone some discussion of the Random Forest results and focused on what is needed next to make the model produce more realistic results! The RF model does make clear and interesting points, but would have been stronger if it was comparable to, or could be extended to, the empirical data. Perhaps utilizing the broader lidar derived estimates of Mascaro et al 2011 (or another updated effort) would offer some hope for this approach in the future. As it stands, the detailed discussion of RF would be more satisfying if the model were performing more realistically. I am not sure what to take away from the relative importance comparisons without such grounding.

We agree with the reviewer that the RF analysis would have been more useful if we were able to develop a generalizable RF model that performs well using both training data and test data based on observations. This way we can evaluate whether our simulations are able to capture the relationships revealed from observations, which is the main goal of the RF analysis. Please see our previous response clarifying the purpose of the RF analysis. As we are not able to develop a generalizable RF model based on observations, as likely limited by the amount of observation data, we have significantly shortened the discussion of the RF analysis in the Discussions and Conclusions section (Section 4) in the revision.

The plant functional type comparison is a nice additional component. It is understandable to start with something simple like a few present functional groupings (PFTs). That said, it would perhaps be worth noting that these should, in the future, also include specifications for differences in water stress responses. There are citations suggesting that tropical pioneers are more hydraulically vulnerable (cited/discussed in Costa et al 2022). Generally, this is necessary to get the best quantitative mortality response, and incorporate it into the model to produce ultimately realistic mortality rates. On that point, I think that M2 and M3 just are sending mortality too high too fast with soil water stress, and that is driving your rapidly asymptoting to too low level AGB with increasing WTD responses. Trait/PFT composition will impact these rates and should be a key part of advancing these models too.

Thanks for the suggestion and reference. We included your comments in the revised discussion as the following:

As a demonstration, only two plant functional types were considered in this study. When water stress is considered, the negative response of AGB with WTD simulated by the model is supported by previous studies (e.g., Esteban et al., 2021) that species associated with deep water tables had decreased growth and increased mortality compared to those associated with shallow water table depth during severe drought. However, the two hypothetical hydraulic failure models (M2 and M3) result in a strong positive relationship between mortality with the soil water stress, driving unrealistic response of AGB to increasing WTD. In reality, the resistance of trees to water stress also depends on the severity of droughts, plant traits, and environmental conditions [Costa et al., 2022; and references therein]. For example, previous studies found that hydraulically-vulnerable trees can delay dehydration by accessing deep water during droughts in BCI [Chitra-Tarak et al., 2021]. How plant traits and PFT composition will impact these rates should be a key consideration in advancing coupled modeling in the future.

Additional reference:

Esteban, E. J. L., C. V. Castilho, K. L. Melgaco, and F. R. C. Costa (2021), The other side of droughts: wet extremes and topography as buffers of negative drought effects in an Amazonian forest, New Phytol, 229(4), 1995-2006, doi:10.1111/nph.17005.

A few more specific points…: There were some shortcuts in model description that would only be clear to insiders; e.g., talking about target biomass, carbon storage, etc. That would be out of the blue and not make much sense to someone that had no knowledge of flavors of ED I think… I would try to include some descriptive topic sentences about how that is important for determining plant relative performance and is impacted by resources and competition, etc. when first mentioned

Thanks for the comment. We included descriptive sentences before "target biomass and carbon storage" were first mentioned as below to put them in a meaningful context:

At the daily time step, daily carbon increment calculated in FATES is sequentially allocated per cohort [Koven et al., 2020]. The amount is subtracted from the cohort's storage pool if the carbon increment is negative. If the carbon increment is positive, the cohort first replenishes the carbon storage pool and tissue turnover is then compensated. The cohort will allocate the remaining increment to any organ pools (leaf, stem, coarse root, fine root, and seed) that are below their allometric targets. The cohort will grow its stem diameter, allocating to each pool proportionally to that pool's derivative with respect to stem diameter using the remaining carbon increment (if any).

Why would different mortality functions not impact AGB variation as is suggested?

The M1 mortality function does not impact AGB variation because the stress factor is larger than the prescribed threshold (see our response to an earlier comment). M2 and M3 affect the AGB variability in the whole domain, but not much in the 50-ha plot because of the relatively homogeneous soil hydrology there. This statement is added in Section 3.3 in the revision.

I have not said much about the time series of ecosystem function components. That is a nice analysis, and a benefit for the paper. A note though, I would be a little careful in explaining what this is and how it operates clearly in your model experiment description. It took me a little while to understand the need for an extra 16 years of run time (i.e. when the met. drivers were available, right?). Maybe I missed something earlier but this section required a little head scratching before I realized that this pertained to comparison of time series ecosystem data.

We apologize for the lack of clarity. The 16 years of simulation was chosen such that the meteorological forcing aligns with the years of observation. This sentence was added in the revision for clarification.

In sum, I have raised some concerns and suggested action points for improvement. I want to step back though and say that this manuscript encompasses a nice progress report on new modeling developments and applications in the field. I just think that shifting the focus a little could do a better job to highlight what this paper is showing effectively that is novel and good, and should be further developed, i.e., that we can model/capture hillslope function and biomass responses. This will, in my view, entail stepping away from some detail about the specific predictions of RF that will end up less relevant when a better version of the model is built and running. I really hope that such future work is a serious plan; this could bring great leaps towards model-based testing for model structure comparisons. Furthermore, in discussion, I think there could be fewer generalities about the merits of such modeling, and more about how this will be improved by addressing its limitations for BCI or other tropical sites (while addressing workers rights).

Thank you very much for your kind words, concerns and suggestions, which help further improve our manuscript. We removed the discussion of RF for the models in response to this comment and an earlier comment above. We have work in progress using our coupled model at another site (Manaus), where there are ongoing water and vegetation related measurements along the transect of a hillslope for us to evaluate the hillslope function and biomass responses.

Some more points on potentially overlooked citations:

Thanks for the references. We added them in the revision.

Sousa, T.R., Schietti, J., Ribeiro, I.O., Emílio, T., Fernández, R.H., ter Steege, H., Castilho, C.V., Esquivel-Muelbert, A., Baker, T., Pontes-Lopes, A. and Silva, C.V., 2022. Water table depth modulates productivity and biomass across Amazonian forests. Global Ecology and Biogeography.

Costa, F.R., Schietti, J., Stark, S.C. and Smith, M.N., 2022. The other side of tropical forest drought: do shallow water table regions of Amazonia act as large-scale hydrological refugia from drought?. New Phytologist.

Mascaro et al 2011 is cited but the finding that AGB increases on BCI hillslopes is surprisingly not mentioned as far as I noted. Instead it is stated that biomass spatial structure is not known for evaluation in the results section. I am a bit confused by this. Within the 50ha plot it certainly is known, and this lidar analysis suggests that there is a ready source for exploration in the literature for quantitative BCI hillslope biomass effects.

The variability explained by the multiple regression approach in Mascaro et al [2011] is 14% and 33% depending on the resolution, which is about the same skill as the RF model for our training data. We added the following sentence to include the finding from this reference:

Even though it finds that slope is an important driving factor in the training data of the observed AGB, as supported by Mascaro et al. [2011] using the multiple regression method to examine controls over AGB derived from airborne Light Detection and Ranging (LiDAR) at BCI, it cannot master the test data (negative explained variance).

Yes, the spatial biomass structure at BCI is known, which has been indicated in our results and discussions. The first sentence in section 3.3 “As there is no spatial observation of the relationship between AGB and WTD at the site” refers to no spatial obversion of WTD. We changed the sentence to “As there is no spatial observation of WTD at the site” for clarification.

Chitra-Tarak, R., Xu, C., Aguilar, S., Anderson-Teixeira, K.J., Chambers, J., Detto, M., Faybishenko, B., Fisher, R.A., Knox, R.G., Koven, C.D. and Kueppers, L.M., 2021. Hydraulically-vulnerable trees survive on deep-water access during droughts in a tropical forest. New Phytologist, 231(5), pp.1798-1813.