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Comment on bg-2021-195

Anonymous Referee #1

Referee comment on "Importance of the forest state in estimating biomass losses from tropical forests: combining dynamic forest models and remote sensing" by Ulrike Hiltner et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-195-RC1, 2021

"Importance of the forest state in estimating biomass losses from tropical forests: combining dynamic forest models and remote sensing**"** uses an individual-based model to predict how forest structural attributes (LAI, forest height, others) and carbon fluxes (GPP, NPP, rates of biomass loss) are associated with different levels of tree mortality. The model-derived relationship between LAI, forest height, and biomass loss is applied to remote sensing-derived LAI and forest height data across French Guiana to estimate biomass loss across the entire country.

I think that this manuscript is clearly written and makes a compelling argument for the methodological approach (combining individual based models with remote sensing data to investigate carbon fluxes). In this submission, the authors have added some analyses in response to feedback from previous reviewers. However, I still share some concerns that were raised in the initial submission, and I think that further additional information would help clarify whether it is appropriate to apply the model-derived biomass loss regression to characterize variation in biomass loss across all French Guiana. Here are some main points for consideration:

1. I agree with previous comments that this analysis is potentially limited by the field data used for model parameterization, particularly given that the plot has considerably lower biomass loss than the predicted country-wide average. Is the parameterization really representative enough of the whole region? I think that the additional analysis with "altered productivity rates" is meant to address this, but it is difficult to determine whether this analysis is sufficient without additional details. What does "photosynthesis intensity" mean, in Figure S8? What other model parameters might, plausibly, vary over the study region? From Table S2 I would guess that management parameters (especially give the main conclusions in line 535-537), site-specific climate, and potentially geometric terms could vary (but I'm not an expert in variation within French Guiana). I don't think that it will be possible to completely resolve this in the study, and I don't think that is necessarily needed for this manuscript to be useful/interesting. However, I think that it would be appropriate to include a more explicit discussion of the limitations of this study. Some of these issues are briefly mentioned at the end of the discussion (Lines 513-526), but I feel that section downplays—rather than acknowledges—the potential limitations of applying results from one model parameterization to all of French Guiana.

2. I think that the sensitivity analysis to remotely sensed LAI and height data is a valuable

addition to this revision, and I appreciate that the authors added it in response to previous comments. However, I have some concerns about how this analysis was performed. Why were constant values of +/- 30% chosen? Do the original data sources give estimates of uncertainty associated with these data products? I think it is overly simplistic to assume that the data product would be off by a consistent factor across the entire country—a more useful analysis would be simulating heterogenous variation in LAI and height estimates across the country, perhaps using a Monte Carlo approach. Given that these data are input to a simple linear relationship, I don't think this change would be computationally unreasonable.

3. In addition to the differences in spatial scale between the datasets mentioned by previous reviewers, I am concerned that "forest height" as estimated in the model and quantified at the 1 km scale are not interchangeable. Simard et al. (2011) use the mean height of the 3 tallest trees to validate GLAS data at the footprint level, but not to validate the gridded 1-km data product, which tends to be shorter and less variable (Table 2 in Simard et al.). The gridded product is based on a biome-level Random Forest model using other ancillary data (tree cover %, precipitation, elevation, temperature, protection status), so variation in 1-km forest height across French Guiana doesn't necessarily reflect "measured" variation in forest structure, but instead predicted variation based on biome-level correlations with other factors. I understand that the authors might not be able to do much about this limitation—but I do think that it deserves at stronger caveat in the discussion section, at least. What factors (tree cover and/or climate?) do you think are most important for driving the height and/or LAI maps, and subsequently the predictions of biomass loss?

4. The methods section claims "No correction factors were required for the extrapolations (see Fig. S7)" for LAI and height data, but Figure S7 shows that much of the country-wide data (perhaps ~25%?) falls outside of the range of simulated values. What information was used to determine that no correction was necessary? In addition to (or instead of) Figure S7, it would be helpful to have a figure showing the range of data in 2D LAI/forest height parameter space from simulations and from remote sensing. I recommend something like Figure S6, but with a heat map showing the density of remote sensing data in the background, and the simulation trajectories overlaid.

5. It would also be helpful to have some additional details to evaluate how well the multiple linear regression model characterizes the modeled relationship between LAI, forest height, and biomass loss. In figure S5c, it does look like the residuals have an apparent "smile" shape—in particular, the residuals are consistently positive in the range of the only field data included for comparison (0.011-0.015 y^{-1}). For example, I would like to see (supplemental) figures showing the relationship between forest height and residuals of the single attribute LAI/biomass loss regression, and vice versa, colored by forest age.

A few more minor line items:

- Line 24: In "changed the forests' gross…", consider replacing "changed" with a more specific word—increased, decreased?
- Lines 144-145: This sentence identifies "extreme climate events, forest fires, wind-throw, and diseases" as possible disturbances relevant to this simulation but I think that all of these would cause disturbances of limited duration. Perhaps other examples, like sustained increased temperature and/or reduced water availability would be more appropriate?
- Line 195: The linear regression model does assume that all forest states are independent, but is this a valid assumption? In the model trajectories, there is clear temporal autocorrelation—one state is used as input to the next state in time, correct?

- Line 321: It is unclear to me whether forests age 0-20 were included in the multiple linear regression model. This line indicates they were, but from the sentence above (Line 312) I thought they weren’t.
- Line 497: Are mortality rates from these different mechanisms available as model output? Obviously there is a lot in this study already, but I wonder looking at how different mortality modes respond to the uniform increase in “base mortality” could provide more mechanistic predictions into how forests respond to sustained increased mortality.
- Line 507: Are there any results from Hiltner et al. (2021) that could be briefly compared the predictions in this study?
- Data availability statement: I think it would be useful to include a supplemental file with the model output used to make Figures 2-6. This would allow others to look more closely at the data without having to learn to run FORMIND.