Reviewer comments 1 (https://doi.org/10.5194/bg-2021-195-RC1)

“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:

Thank you very much for your helpful, constructive comments. Below you will find our replies to your comments (highlighted in blue). The line numbers given in our replies refer to the ones in the manuscript.

Note that we changed the terms 'biomass loss’ to 'biomass loss rate' and ‘tree mortality rate’ or ‘tree mortality intensity’ to ‘stem mortality rate’ (proposed by Reviewer 2).

1. **A)** 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. **B)** 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). **C)** 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.

Thank you for the comment.

Reply to A: By varying the stem mortality rate and the photosynthetic rate over a broad range, we generated a large dataset of forest stands that has been explored in detail. This artificial dataset of forest dynamics covers a wide range of possible forest states such as the variability in tree species composition, successional state, and tree size distribution. We assume that we can use it to partially cover almost every state of forest stands in French Guiana (so-called forest factory approach, see Bohn et al. 2017). We will amend the text (e.g., in the Abstract, Introduction, and Methods section) to make this point clear. We would like to emphasise that the country-wide biomass loss map has been included in the manuscript as a possible application example for the purpose of showing what is possible with such a model-derived artificial dataset. We will point this out in more detail. In perspective, it would be important to validate such maps with more field data (currently not available to us).

Reply to B: To generate variability in photosynthetic rate, we varied the model parameter 'maximum photosynthetic rate' of the light response curve (Tab. S2). We will add this explanation to Fig S8.

Tab. S2 incorrectly stated the effect of forest management was included in the current simulations. This is not correct and will therefore be deleted from Tab. S2. However, silvicultural interventions shape forest structures, and thus, forest states locally. It would be interesting to consider them in follow-up studies to provide additional information when fitting statistical models to estimate biomass losses regionally. We will mention this point in the discussion.

Reply to C: Thank you for your input. The following further factors can vary on a regional scale and their effects can be considered by adjusting model parameters:
1) Forest management and fire can be simulated.
2) Effects of weather variables such as temperature, rainfall variability, and solar radiation can be considered.
3) Relationships describing tree geometry can vary in space and time.

We will ensure in the revision that the potential limitations due to our assumptions of the dynamic forest model regarding site-specific environmental factors and the representativeness of the model parameterisation for the entire country are discussed in more detail so that they are no longer perceived by the reader as downplaying (e.g., lines 513–526).

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.

Thank you for pointing this out. We assumed the homogeneous variations of ±30% in the input variables (LAI and forest height) as a "worst-case" scenario. We follow the reviewer's suggestion and will add a sensitivity analysis as suggested (sampling random numbers per pixel as factors for the LAI and forest height variations).

3. A) 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. B) 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?

Thank you very much for the comment.

Reply to A: We agree with the reviewer. As noted, we unfortunately cannot eliminate the limitation regarding the values of forest height derived by Simard et al. (2011). We will add the aspects the reviewer mentioned in the discussion on the limitations of the remote sensing data (lines 425 fol.).

Reply to B: We think that both factors have an impact on forest height, LAI, and biomass loss. For example, drought, uprooting due to storms and flooding, fire, insect calamity, forest management, etc. may be possible drivers of variability in the LAI. Forest height can vary due to uprooting from storms and flooding, fire, forest management, etc. Those environmental drivers may also interact with each other, too. The analysis of this question is interesting but is beyond the scope of our study. It should be explored in follow-up studies. We will add text in the discussion of the limitations of our linear regression model, because unfortunately we cannot perform analyses regarding the question in this study.

4. **A)** 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? **B)** 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.

Thank you for the comment.

Reply to A: We agree with the reviewer, the value range of the remotely sensed data is partly outside of the simulated value range (Fig. 7). In the revision, we will test whether harmonising the remote sensing products against the simulated data will improve the estimated biomass loss rates. For example, we will reduce the remotely sensed LAI values following the rationale that no understory trees (DBH < 10 cm) are considered in FORMIND. We will then recompile the biomass loss map.

Reply to B: Very good idea. We will provide a heat map as suggested.

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.

Thank you for the comment. We followed the reviewer's suggestion and created the plots already (Fig. R1). The residuals attributed to the LAI contain no remaining trend for forest stands older than 40 years. The ones attributed to forest height still contain a remaining trend, which is small and age-dependant. So we can prove that forest height and LAI can be used as proxy variables to estimate biomass losses.

We would like to note that to further improve the linear regression model (further minimize the residual's remaining trend), additional proxy variables would need to be included. In this study, we tested various forest attributes available as remote sensing products (see Table S3). We decided to use the simplest possible linear regression model (in terms of the number of proxies) that estimated biomass loss rates best.

We will supplement the analyses (Fig. R1) in the Appendix and describe how we derived the residuals attributed to the forest attributes. Also, we will discuss this aspect in the limitations of the multiple linear regression model.

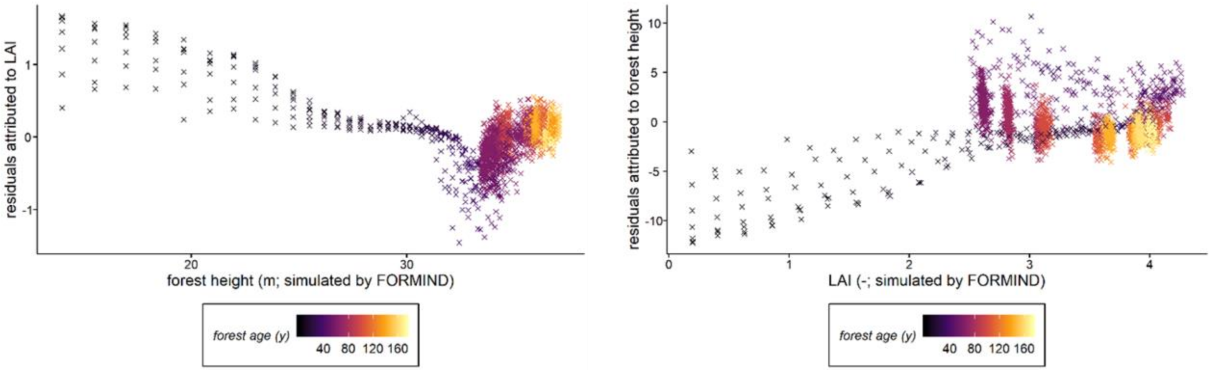

***Figure R1:** Relationship between the residuals associated with the proxy variables of the LAI (left) and the forest height (right) and the simulated forest attributes. Colours indicate the mean forest age.*

A few more minor line items:

6. Line 24: In "changed the forests' gross…", consider replacing "changed" with a more specific word—increased, decreased?

Thank you. We will reformulate the sentence. Here, higher stem mortality rates led to increased levels of GPP.

7. 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?

Thanks for the hint. We will refer to sustained elevated temperatures and reduced soil water availability as examples.

8. 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?

Thank you for asking. We acknowledged that when fitting linear regression models, it is important that the proxy variables are independent of each other. We tested this using a covariance matrix of the predictors (unpublished results) and we will visualize that in the diagrams accompanying your comment 5. Also, we will rephrase the text to make our point more understandable and add the covariance matrix of the predictors to the supplementary materials.

9. 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.

Thank you for the comment. We included all simulated years in the analyses (years 0 – 300). We will reword the sentence in line 312 so that there is no misunderstanding.

10. 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.

Thank you for this question. Yes, the model outputs for different modes of mortality do exist, however, we did not explore them in this study because it was not part of the research question. It is a good suggestion to include them in future studies. In this study, we analyzed the relationships between stem mortality rates with biomass loss rates, GPP, NPP, and biomass stock (see Fig. R2 on the relationship with the biomass loss rate). We will include more details on this issue in the supplementary material and add text to the methods and results sections. We will revise Table 1 to report resulting stem mortality rate and biomass loss rate.

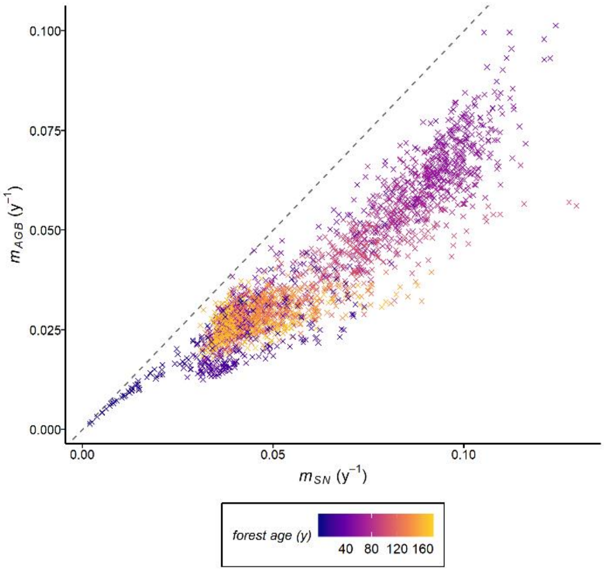

Figure R2: Biomass loss rates m_{agb} versus stem mortality rates m_{sn} for the simulated forest stands. The dashed lines indicate the 1:1-lines.

11. Line 507: Are there any results from Hiltner et al. (2021) that could be briefly compared the predictions in this study?

Thank you for the question. If additional effects, such as climate change and forest management, were added to the dynamic forest model's simulations, the reasons of biomass losses could be accurately determined. This would be interesting for further studies where the methodology presented here can be used. We will address this when we revise the discussion (see also reply 1.C)

12. 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.

Thank you. We will publish FORMIND's simulation results and make that clear in the "Data Availability" section.

Literature

Bohn, F. J. and Huth, A.: The importance of forest structure to biodiversity–productivity relationships, R. Soc. Open Sci., 4(1), 160521, doi:10.1098/rsos.160521, 2017.