Editor comments

Thanks for your careful revision, which was re-reviewed by referee #1 (the other referee was not available). Referee 1 acknowledges many improvements but, as you can see, still has a number of concerns; the referee is concerned that not all of their initial concerns were appropriately addressed/acknowledged, particularly with respect to regression model performance and the sensitivity analysis.

I have read the revised manuscript, the response to reviewers, and Referee 1's assessment. I agree that much improvement has been made--thank you--and think this manuscript is well on track for final acceptance. Nonetheless, I agree with many of R1's thoughtful comments, and would ask that you consider them carefully, and use them to make (hopefully final) improvements and clarifications. The one exception is with respect to the sensitivity analysis; while ±30% is arbitrary, it still provides a useful outer bound, and improving it (as the referee suggests) is entirely optional. Conversely, please address the referee's regression model comments fully.

Thank you very much for your helpful, constructive comments. As main changes to the manuscript, we thoroughly revised the regression model and tested different model types. We have found a new variant that solves the criticisms of the reviewer. This even leads to a partial improvement of the results and now no correction of the remote sensing data is necessary anymore. We believe that the analysis with the ±30% limits (Fig. 7.d) can already give the reader a useful indication about the sensitivity of the biomass loss rates. We would therefore like to follow your suggestion and leave the sensitivity analysis as it is. We added text to indicate that a calculation of an error propagation with provided uncertainty products could be performed (lines 267 fol.), but this is behind the scope of this study (since the map is only an example application of the presented workflow). For our detailed responses, please see the response letter.

Reviewer comments 1

This study uses simulated forest data to explore how mortality rate variation influences the structure and dynamics of tropical forests, and to evaluate whether forest structural attributes can be used to predict biomass loss rates from large scale remote sensing data. This revision appropriately addresses some of my recommendations/questions from the previous submission, and I think that the general approach would be a valuable contribution to the field.

Thank you very much for your helpful comments. As main changes to the manuscript, we thoroughly revised the regression model and the comparisons of forest height and LAI from the remote sensing products with the forest model simulation results. Please find our replies to your comments below (highlighted in blue). The line numbers given in our replies refer to the ones in the manuscript with changes tracked.

With the additional information provided in the supplement, I have some remaining concerns about how well the multiple linear regression model describes variation in the simulated dataset, and about the application of that model to the remote sensing data.

First, as you mention, Figure S7b still seems to show some "smile" effect—there is a negative trend with LAI for LAI ~2.5-3.5, and then a positive trend for LAI > ~3.5. It's a little hard to evaluate how much this matters because the units here are not in biomass loss rates, but this suggests to me that a linear relationship with LAI might not describe the trend well, especially because much of the MODIS LAI data appear to fall in the 3-4 range.

Thank you for the comment. The slight "smile" effect in the residuals of the LAI indicates a non-linear component. Therefore, we tested transformations of the input data for the independent variables (i.e., LAI and forest height) and we tested non-linear models, such as generalized additive models (GAMs; Fig S15). Using a GAM, the trends in the residuals of LAI and forest height could be removed (Fig. S15.c and S15.d).

When looking at the residuals of the fitted biomass loss rates and the frequency distribution of the residuals of the GAM (Fig. S15.b and Fig. S15.e) compared to those of the multiple linear model (Fig. S6.b and S6.c), the GAM shows only slight improvement. Since GAMs are not easy to interpret and to communicate (no closed model equation), we decided to focus on an improved linear regression approach. This improved regression model reduced the overall residual trends (new Figures S6.c and S7).

To address the Reviewer's comment in detail, we added the GAM approach as an alternative to the linear model but present the results only in the Supplements (Fig. S15). We improved the regression model (Eq. 7, Tab. S3) and adjusted all analyses and results based on it accordingly (e.g., Ch. 3.2, Fig. 6, Fig. 7, Fig. S6 - S8, Fig. S11 - S14). Finally, we revised the corresponding text in the Methods (lines 215 fol.) and the Discussion (lines 453fol.).

Second, it appears to me that even after the added correction in this revision, the remotely sensed LAI-height parameter space is not very well described by the simulated data—indicating that the "factory forest" approach assumption that the simulated data "cover most of the forest states in French Guiana" is not met. Figure S9b shows that the simulated data are a highly uneven and incomplete sample of the possible values of LAI/height seen in the country-wide data. For example, forests less than ~35m are common in the remote sensing data but only appear in young forests in the simulated data, and much of the simulated data is still lower LAI than the remote sensing data. Also, LAI and height are indeed correlated in the simulated data (Table S4), but it appears that they are not correlated in the remote sensing data.

Thank you for the comment. We agree with the Reviewer that despite the corrections made to the remote sensing products, it looked like they did partly not match the simulation data so well. We therefore carefully checked once again our approach for the comparison of the two data sets and have been able to correct an inconsistency:

- We now omit pixels of the map for which the regression model (see eq. 7) did not predict biomass loss rates, i.e., we excluded negative biomass loss predictions (s. Fig. 7.a, blank pixels). This was mainly the case for populated coastal and aquatic areas, i.e., this approach eliminated pixels that we assume are predominantly unforested (see e.g., Methods and Discussion lines 252 fol., 488 fol.).
- In our revised comparison, we include only those pixels of the remotely sensed LAI and forest height which are also considered in the final biomass loss map. In Fig. S9 (new), it can be seen that the centroid of the remote sensing products and the simulations are now in good agreement, with only a few combinations deviating (shown in light-grey) and without the need to correct the remote sensing products. Therefore, we reversed the correction of the remote sensing data (lines 241 fol.), which is no longer needed due to the improved regression model (see our reply 1). We updated Figure 7.a showing this biomass loss map and affected analysis (e.g., Fig. S13, Fig. S14, Fig. S12). We added a description in the Methods (lines 246 fol.).

Third, I also think that the uncertainty analysis can/should be further improved. Instead of using the arbitrary +/- 30% range, I recommend propagating the actual uncertainty from the remote sensing data, and from the model with forest attributes. There is an uncertainty product associated with each MODIS LAI measurement (LaiStdDev_500m2)—is there a similar metric associated with the height product? For example, instead of sampling from an arbitrary uniform distribution one time, the value for each pixel could be repeatedly sampled from the actual uncertainty distributions, and delta mAGB could be calculated using the 95th percentile range of resampled values. My apologies for not being clearer with this recommendation in the previous review.

Thank you for the suggestion. This study is mainly about method development for estimating biomass losses from forest attributes and the map serves as a sample application (e.g., see lines 98 fol., line 120, caption of fig. 1, and section header 3.3). We discussed the Reviewer's suggestion and found that an additional sensitivity analysis based on available uncertainty products would be extensive and not needed. We think that the analysis with the ±30% limits (Fig. 7.d) can already give the reader a useful indication about the sensitivity of the biomass loss rates in the sample map. We added text in the Methods (lines 262 fol.) to emphasize more clearly that error propagation could be considered in follow-up studies.

I also have a few specific recommendations that can be easily/quickly addressed (line numbers refer to the revised manuscript without tracked changes):

- Line 30: I recommend changing “and mapped” to “to map” to make clear there isn’t a separate remote sensing dataset of mapped biomass loss.

Thank you, done (line 30).

- Lines 142-143: I find the sentence “Our assumption…is that forest stands also have no explicit position” a little unclear. Does this mean that different 1 ha forest stands within the 16-ha forest area don’t have explicit position/interact with each other, or something else?

Thank you. The forest stands of 1 ha do not have an explicit position within the landscape. We reformulated the sentence to make our point clearer (lines 174 fol.).

- Line 158: Perhaps reference Table S2 here for difference in the PFTs?

Thank you, done (line 159).

- Line 170: Perhaps reword to “in combination with the subsequent effects on the other models of modeled mortality” or something similar, if that is accurate?

Thank you. We reformulated the sentence (lines 170 fol.)

- Lines 226-227: I recommend describing what data were used to product the forest height estimates (i.e. GLAS).

Thank you, we added the description (line 231 fol.).

- Figure 4: Is this the background mortality rate (consistent with the ranges shown here) or the resulting average stem mortality rate as defined in Table 1?

The x-axis shows the background mortality rate. We revised the axis titles to make this clearer (see Fig. 4)

- Figure 7: The map pixel values in the table (c) have not been updated from the previous draft.

Thank you for the hint. We corrected the numbers in Figure 7.c.

- Line 497: I think the last sentence should the header for the next section, correct?

Yes, thank you. We re-formatted it as header (line 514).

- Line 575: I recommend using wording such as “The resulting sample map of biomass loss predicted…” instead of as “The resulting sample map of biomass loss indicated…”.

Thank you, done (line 594).