

Reply on the revision 1 of the Manuscript BG-2020-264

Reviewer 2

Overview

This interesting study uses a forest model to explore how biomass mortality rate varies as a function of changes in stem mortality rate for a tropical forest location in French Guiana. It then uses these simulations to create a simple emulator linking indicators of successional stage and the resulting rate of biomass mortality. The emulator is then used to estimate biomass mortality rates across the whole of the country. The model has been previously been evaluated at the reference site used in this region. Overall, I find this a novel approach to investigate spatial variations in biomass mortality rates as a result of differences in forest age. Such efforts are important to provide baseline levels of mortality against which future changes can be compared, as well as to provide insights into the mechanisms driving mortality rates and any associated trends. I would like to see the manuscript published, but prior to that there are several aspects that I think should be clarified or expanded upon, including some additional analysis to identify the robustness of the results.

Main comments

Thank you very much for your helpful constructive comments. Below you will find our replies to your comments (highlighted in blue).

1. A stand-level relationship between height, LAI and biomass mortality rate, is being used to scale up across a broad geographical region. Forest height here is almost purely an indicator of age of the largest trees, since there is relatively little difference between the disturbance scenarios at equilibrium. LAI appears to be both an indicator of age and composition. Between them they appear to characterise well how biomass mortality changes over the successional sequence. But when using this relationship to scale up, what happens if resource availability is not constant over the region being scaled over? Different levels of resource availability may also influence height and LAI - I should imagine particularly in the equilibrium stage for height and LAI from ca. 80 years onwards. Does the derived relationship in Fig. 6 hold across a productivity gradient? This is touched on in the limitations discussed in section 4.5, but I think it really needs to be tested (and presumably would be relatively straightforward to do). Even if the productivity gradient across French Guiana is small (as effectively assumed on L103), I think it is important for readers to know how robust the relationship and method are for application to more diverse regions.

Reply 1: Thank you for this comment. In our study, productivity varies across different successional states and across scenarios of different disturbance intensity (see Fig. S1). We believe there is a productivity gradient already accounted for.

However, we will conduct additional simulations in which we investigate the influence of a broader productivity gradient (by varying tree photosynthesis rates) on forest biomass mortality. The results will be reported in the paper.

We will be happy to include this interesting aspect in the discussion.

2. In a similar vein, 1) is it appropriate to liken the increase in biomass mortality rate with forest height in this study (driven by a uniform mortality rate change) to the increase in stem mortality rate with individual height in Stovall et al. (2019) (L306)? I think the mechanisms are quite different. Biomass mortality rate would be expected to increase as forests approach equilibrium biomass, as the size of biomass losses must start to approach that of biomass growth. But this does not have to imply that stem mortality rates increase with tree size - it could simply be that the trees that are

dying are typically larger. This is distinct from a mechanism in which individual tree mortality rates scale with individual height (e.g. Holzwarth et al., 2013; Rowland et al., 2015; Stovall et al., 2019). I suggest to add a bit more nuance in the discussion of this point. 2) As an alternative comparison, in section 4.2, can the regression slopes instead be linked to the biogeographical patterns for the wider region from Johnson et al. (2016)? These patterns have been linked to a gradient in disturbance intensity and whilst Johnson et al. present biomass and stem mortality, rather than height and biomass mortality, FORMIND is simulating all components, facilitating a comparison.

Reply 2: Thank you for this comment.

@ 1: We agree with you and we will add your helpful thoughts to the discussion section.

@ 2: Thank you for the literature, which we will take up in the discussion.

Johnson et al. (2016) investigate forests of the entire Amazon region. Without conducting further research, the regression model should not be applied to other forest types (other than Terra Firme forests). We will provide additional extrapolations using our regression model on remote sensing products from LAI and forest height of the Guiana Shield (outside French Guiana). Then, we will compare our results of biomass mortality with values from the study of Johnson et al. (2016).

3. It would also be good to see some independent evaluation of the extrapolation. Whilst observations for biomass mortality in the region are likely rather hard to come by, how similar is FORMIND simulated height and LAI to the Simard et al. and MODIS data used for the extrapolation? Are they very close to each other, or is a correction factor needed to account for biases in one or the other? I wonder if you could also compare biomass mortality rate with that from other plots in the Guiana Shield provided in Brienen et al. (2015)?

Reply 3: Thank you very much. No correction factors were required for the extrapolation. A comparison shows the density distributions of both data sets of LAI and forest height with the ranges of FORMIND's simulation results (see figure 1). We will include such information in the results part and discuss their implications.

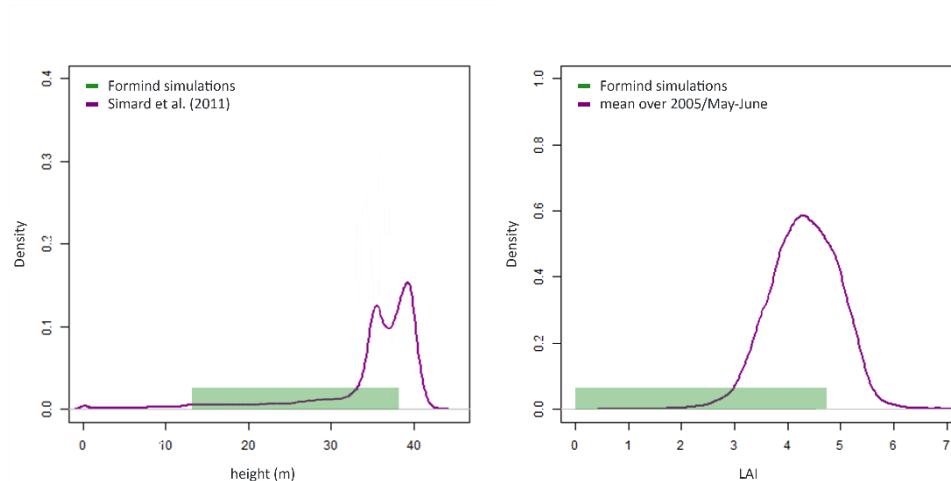


Figure 1: Density distributions of remotely sensed forest height (left panel) and LAI (right panel) of French Guiana. FORMIND simulations are distributed in similar ranges (green area). In order to show the quality of the country-wide biomass mortality estimations, we will compare the map with empirical data from different empirical sources (e.g., Brienen et al., 2015; Soong et al., 2020).

4. In section 4.4 it is stated that the new framework allows to assess residence time as a functional of successional stage, but I think this is a bit misleading. The term residence time comes loaded with implications about how long carbon stays in the system. But in a transient system (as opposed to an equilibrium one), this does not hold for the kind of calculation used here, and during succession the

deviation from equilibrium is quite marked. The mean time a molecule of C entering a 50-year-old forest remains in that forest will likely be very different to the reciprocal of the biomass mortality rate at 50 years, because that molecule is more likely than its predecessors to be incorporated into a longer-lived later-successional PFT. In comparison, the biomass mortality rate is equivalently normalised to biomass but does not come with the same implications – it is unambiguously the rate at which carbon is currently leaving the system at that moment. I suggest only to use the concept of residence time here when averaging over the whole region (and then to term it turnover time, following Sierra et al., 2017).

Reply 4: Thank you for pointing this out. We agree with the reviewer's suggestion and will change the terminology of the term to "turnover time".

5. The result that net primary production remained stable is very interesting and neat. But can you add a bit more discussion about what this result is ultimately based on? To what extent is it an emergent outcome of the model, versus an assumption that went into the PFT parameterisation?

Reply 5: Thank you. We will take up the reviewer's suggestion of broadening the discussion on stable NPP. Although NPP in the old-growth phase is comparable across all scenarios, the functional composition of PFTs differs. We will study the proportion of pioneers and the mean longevity of trees more intensively as this seems to be the explanation behind the phenomenon.

6. Equation 3 implies that $GPP = AGB_dead$, which cannot be the case, as autotrophic respiration, allocation to soft tissue and allocation belowground need to be subtracted from GPP in order to get to woody NPP (i.e. woody biomass increment), which would be considered equivalent to AGB_dead at equilibrium (assuming that AGB is only counting the woody component of the total biomass). So, the τ obtained from AGB_total/GPP would be much smaller than that from AGB_total/AGB_dead . As FORMIND simulates GPP, a turnover time metric for comparison with Carvalhais et al. (2014) could be calculated, but it should be defined separately to the biomass turnover time with respect to mortality.

Reply 6: Thank you for pointing this out. We will correct the derivations of the equations concerning turnover time.

7. The LAI and height products used for extrapolation have errors associated with them. To what extent do these errors propagate through to uncertainty in the biomass mortality rates? I think Fig. 7 should be associated with an error field at least based on the input uncertainty, if not also the uncertainty in the regression fit.

Reply 7: Thank you. We will perform an uncertainty analysis to evaluate (1) the impact of the errors associated with the remote sensing products used and (2) the errors related to the linear regression model. The comparison of the map with empirical data (see our reply 3) will also contribute to the error assessment.

8. Why only 1 month of LAI data (L170)? Doesn't this expose your results to potential seasonal LAI variations? Wouldn't an average over several years provide a more like-for-like comparison with the model output?

Thank you for the hint that LAI is very volatile. We will incorporate the mean of the MODIS LAI data by using a longer data series (from the same month as the ICESat data used for Simard's forest height map). We will also investigate the sensitivity of our workflow concerning variations in LAI (see reply 1).

Minor comments

line 33. Imprecise statement. 471 Pg C is much less than half of the terrestrial carbon stock (assuming vegetation + soils) given most estimates. Given the reference to Pan et al. (2011), I think you mean "forest carbon stocks"?

Thank you. We will correct it in the text.

l47. What is an, "increase in associated physiological mechanisms"?

We mean an expected higher frequency and intensity of mortality drivers may result in an increase in tree mortality due to related physiological mechanisms and interactions that may underlie increasing tree mortality in tropical forests (McDowell et al., 2018). We will revise the sentence.

l51. I always find disturbance a slippery term which can be used to refer to a very wide range of things. In this paragraph you give a general list of things that can be referred to as disturbances. I think you are defining disturbance as everything which is not related to competition, which is fine of course, but could you give an explicit definition of what is considered as disturbance for the purposes of this study?

Yes, we would introduce a definition of the term "disturbance" as follows: long-term increase in tree mortality which is not stand-replacing.

L191. GPP is not the right indicator for a statement about "fixing five times more carbon in biomass" (see above). Can you show woody NPP? Or simply say, "fixing five times more carbon"?

Thank you. Yes, we will add woody NPP.

Fig. 4. ODM is not included in the figure, just the caption.

ODM appears in the y-axis labeling as part of the mass unit of GPP and NPP.

L301. The statement needs refining. Tree height is the strongest predictor of tree mortality out of which basket of indicators? At the individual tree level other predictors can be very important (see e.g. Esquivel-Muelbert et al., 2020), so it needs to be clear what is being compared to what.

The statement refers to the two proxy variables LAI and forest height, which are included in the regression model (Eq. 5).

Besides LAI and forest height we pre-tested GPP, NPP, and biomass as possible proxy variables in different combinations (with and without interaction terms, with and without interceptions). We will add this information in the text.

L366. I would suggest that biomass mortality rates depended on functional composition and level of divergence of C input and output fluxes from equilibrium, with LAI and forest height being indicators of these, not the drivers themselves.

Thank you. We agree and would paraphrase the text.

L396. "more biomass is dying", or, "biomass is dying at a faster rate"? (because the map shows rates, rather than fluxes).

Thank you for the hint. We would reformulate the text.

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