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Interactive comment on "Importance of succession in estimating biomass loss: Combining remote sensing and individual-based forest models" *by* Ulrike Hiltner et al.

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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

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.

2. In a similar vein, 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. 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.

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)?

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).

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?

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 tau 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.

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.

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?

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"?

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

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?

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"?

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

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.

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.

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

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