Reviewer comments 2 (https://doi.org/10.5194/bg-2021-195-RC2)

This paper is a revision of Hiltner et al. (2020, https://doi.org/10.5194/bg-2020-264). As with that manuscript, I find that the current one provides very interesting insights into how biomass mortality rates vary as a function of successional stage, as well as providing a useful upscaling method that uses satellite products to extrapolate to country-scale. The study is clearly motivated, structured and written, and the interpretation of the results appropriately caveated. I see that the authors have addressed most of my comments on their previous manuscript and I only have a few minor points on this one. If they are addressed, then I very much recommend publication.

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.

1. The term “biomass loss” is used throughout, but this is a bit of an ambiguous term as it could refer to either the loss rate or the flux. It’s clear from the units that it’s the rate, but I also think the clarity of the text would be improved if the term “biomass loss rate” was used instead. Similarly, the settings in Table 1 are described as “tree mortality rate” or “tree mortality intensities”, I suggest calling them instead “stem mortality rates”, as this clearly differentiates from biomass loss rate (maybe it’s only in my head, but “tree mortality” feels to me a more general term). Being very specific in the text about what the prescribed and simulated rates are might help emphasise the point being made in the discussion about one not equalling the other.

Thank you. We will change the terms 'biomass loss’ to 'biomass loss rate' and ‘tree mortality rate’ or ‘tree mortality intensity’ to ‘stem mortality rate’ throughout the text (e.g., lines 1, 14, 17, etc.).

2. Given that one of the key take-home messages of the manuscript is how successional stages influence how stem mortality rate links to biomass loss rate, it would be very helpful to quantify stem mortality rate in a way that makes it directly comparable to the simulation results. The values in Table 1 are a simple average of the rates of the 8 PFTs, but the actual stem mortality will be the combination of this and the prevalence of the PFTs. So, a fair comparison of the two rates (start of section 4.2) requires the actual stem mortality realised in the simulations.

Thank you for the suggestion. A scenario was defined by changing the mortality parameters. We prepared a figure illustrating the simulated biomass loss rates versus the realized stem mortality rates of each scenario. We calculated the stem mortality rate (m_{SN}) as the ratio of the number of dead trees to the number of trees in a forest stand at each simulation time step. In each tree mortality intensity scenario, the biomass loss rate (m_{AGB}) is on average greater than the stem mortality rate (see Fig. 1: panels). We will include this figure in the supplementary material and add text to the methods and results sections. We will also revise Table 1 to report resulting mean stem mortality rate rather than the average of tree mortality parameters.

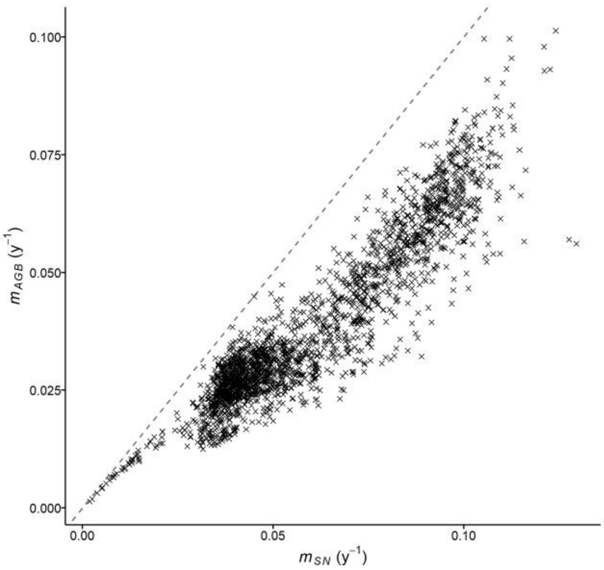

Figure 1: Biomass loss rates m_{AGB} versus stem mortality rates m_{SN} for all simulated forest stands. The dashed lines indicate the 1:1-lines.

3. In lines 179-189 it's not entirely clear to me which definition is being used for NPP. Is it woody NPP only, or true NPP (i.e., GPP minus autotrophic respiration)? Clarity on this is important because it influences comparisons of the results for turnover time to others in the literature. For instance, a direct comparison to Erb et al. (2016) (line 479) would only be fair if true NPP is being used, as that is (as far as I can tell) what is used by Erb et al.

Thank you for asking. In our study, NPP of a tree is the difference between gross primary production and autotrophic respiration (can be summed up to stand level). Accordingly, one can compare the values of NPP of both studies. We will add this point to the manuscript and add the equation of our NPP calculation to avoid misunderstandings.

4. Line 294, "relationship between single forest attributes and biomass loss can be imagined… but the regression statistics were not convincing", and line 295, "linear regression models using only one proxy variable already show high significance", seem to contradict one another? In fact, I think the whole text on lines 296-304 is distracting and unnecessary. It describes linear regression results (e.g. LAI and biomass loss) that, whilst statistically significant, a glance at Fig. 5 shows are not useful, because the relationship is clearly non-linear. Can be enough simply to show Fig. 5 and state that?

Thank you very much for the comment. We agree with the reviewer that the wording of the mentioned text is complicated. We will shorten and reword it as suggested (lines 294 – 304).

5. The last sentence of Section 4.4 is, I think, still on shaky ground. By definition, forests in the early stages of succession are not in equilibrium, or anywhere near it. The biomass loss rate is following a pretty clear evolution over time in the first 100 years of succession (Fig. 3a), so a derivation of turnover time based on instantaneous biomass loss rate is going to be misleading. It's not going to represent well how long the carbon being fixed at that moment stays in the system. I think it's reasonable to make the calculation over a large area which incorporates forests in various successional states and where one can reasonably expect that those states are fairly close to dynamic equilibrium (as in the rest of the paragraph). But a forest in the early stages of succession is really a long way from equilibrium. I suggest deleting the last clause of this sentence.

Yes, will be done.

6. Line 27. Is this biomass loss at equilibrium?

These are average values over the entire simulated time (years 0 - 300). We will revise the text.

7. Table 1 caption. “a^{-1}” is used here, but normally “yr^{-1}” in the rest of the manuscript.

Thank you, we will adjust the unit.

8. Fig. 5 caption. ODM seems to be defined for the first time here but is first used on line 242. Could you also define it when first introduced?

Thank you. We will define ODM at the place where it is used for the first time (line 242).

9. Fig. 8. I’m being really picky here (sorry), but I found this a bit awkward to read because it’s a scale that spans zero, but the colours are not centred around zero. Could you maybe centre the grey range on zero?

Yes, we do. Thanks for the hint.

10. Line 378. I think “confirmed” is too strong. Maybe “supported”?

Thank you. We will implement the proposed amendment.

11. Line 498. “death of other trees”

Thank you. We will rename ‘dying of other trees’ to ‘death of other trees’.