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Review of bg-2021-195

Thomas Pugh (Referee)

Referee comment on "Importance of the forest state in estimating biomass losses from tropical forests: combining dynamic forest models and remote sensing" by Ulrike Hiltner et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-195-RC2, 2021

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.

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 to call 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.

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.

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.

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?

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 to delete the last clause of this sentence.

Line 27. Is this biomass loss at equilibrium?

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

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?

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?

Line 378. I think "confirmed" is too strong. Maybe "supported"?

Line 498. "death of other trees"