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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.

Anonymous Referee #1

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Overview: The authors attempt to integrate simulation model predictions, remote sensing, and field data to scale above ground necromass in French Guiana. The model predictions of biomass mortality under differing mortality rates are then used to fit a statistical linear multiple regression model with the covariates of LAI and height. The statistical model is used to upscale biomass mortality across the entirety of French Guiana.

General Comments: It is clear the authors have done a lot of analysis on these model simulations. The figures are nice and clear, and the text is mostly well detailed. However I have major reservations about the underlying reasoning of the manuscript. The

model might work nicely - I don't really know and it doesn't appear anyone would if there was not a large body of field data to test it with. Moreover I don't see the utility of 'upscaling' to the entirety of FG from one plot. Sure you can generate an estimate, but is the estimate viable or defensible? There are certainly some interesting aspects to the simulations such as the interaction between PFT and disturbance frequency - yet that is not really what the outcome of the manuscript is focused upon. Overall, these issues and the following underlie my objections to the methodological approach used here, and subsequent conclusions derived.

1)Does the paper address relevant scientific questions within the scope of BG? I suppose this would fall within the scope.

2) Does the paper present novel concepts, ideas, tools, or data? This might be a novel application of FORMIND simulation results. I am not deeply read in the FORMIND literature. Otherwise, I think these approaches have largely been attempted before.

3) Are substantial conclusions reached? An estimate of mortality and biomass turnover for the entirety of French Guiana is derived.

4) Are the scientific methods and assumptions valid and clearly outlined? No, this is lacking with respect to the assumptions of the underlying model products used to make the assertions of the results.

5) Are the results sufficient to support the interpretations and conclusions? I do not believe so. They present a rate of mortality, but there is no ground based observation presented to compare it to, neither is an appropriate comparison made with field derived tree mortality estimates from the region.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No, the specific version and parameterization of the FORMIND model and its outputs are not made available. The large scale predictions are also not made available.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? No, I believe they actually miss a lot of relevant work - especially with respect to field observations. It is possible because some of this work is contrary to their conclusions.

8) Does the title clearly reflect the contents of the paper? Not exactly. LAI is not entirely representative of successional state. Succession usually has a species assemblage connotation, which cannot be derived from the remote sensing products used here. There are many intrinsic edaphic and topographic effects that can also limit LAI, in addition to intra & inter-annual variability of LAI from phenological responses to anomalies of climate.

9) Does the abstract provide a concise and complete summary? The abstract is not concise, and I think some of the statements should need to be edited to make it completely clear that every result presented here is conditional upon the veracity of the predictions of FORMIND being simulated for the Paracou plot. Also mention of climate change is made, but that's not really at all thematic of the manuscript.

10) Is the overall presentation well structured and clear? I was confused by some aspects of the methods, but the structure of the presentation seems ok.

11) Is the language fluent and precise? There are some areas where the language is a bit informal, but this could be easily remedied and is not a major concern. Some sentences should be re-written in the 'direct voice'. The last sentence of the conclusion reads very awkwardly as is.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? I think this is mostly ok. There might be a small issue here with terminology. For example, what is called "rate of biomass loss due to tree mortality" is actually a proportion.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced,

combined, or eliminated? The text does seem a bit long. The figures look nice.

14) Are the number and quality of references appropriate? No, I think the references are very much inadequate. There is a large omission of comparison with field based studies, and tropical forest remote sensing studies.

15) Is the amount and quality of supplementary material appropriate? It seems ok. The model, simulation outputs, and derived country level predictions should be made available (without requesting access).

RS LAI data: Changes in LAI are not at all indicative of tree mortality - especially at the relatively coarse 500m MODIS scale. Intrinsic biological phenology and drought responses can also trigger large fluctuations in LAI. I think something on the scale of a large windthrow event would be required to really reduce the LAI at the scale of resolution in the MODIS product. I see this as a problem that undermines the underlying approximation of mortality for the manuscript - and by extension I think undermines the effort of upscaling mortality. Lines 60:63 also seem to make this point.

Statistical model: The high R2 of the statistical model approximating mortality derived from the simulation model is not very meaningful when (1) it is completely unclear that the model can accurately simulate mortality. (2) There also appears to be a scale mismatch between the simulation outputs the statistical model was fit with, and the RS derived inputs used for upscaling it. Was the model fit with 40x40 m subsets or 60 ha? It is not really clear. Given the number of points in figure 6, I am guessing it is the 40x40m subsets. The native scale of the tree height product (which is also a model derived product) is 100 ha. I am sceptical of fitting a model on simulated mortality predictions of 0.16 ha, and then applying it at 100 ha.

Numerous papers have shown that tree mortality and necromass do not scale linearly. If this manuscript was actually based upon field data (which it is not), then perhaps there would be merit to this counter argument. However, the results of this manuscript and its thesis is effectively entirely based upon simulations. The authors do not seem to acknowledge previous research on the topic - which again, is strongly contrary to the results presented here.

I think it is extremely speculative to assume that changes in a modeled LAI estimate are proportional to % mortality, or total necromass. Virtually all allometric equations for biomass are nonlinear. A lot of hard work has been done in this area. See Marra et al. 2016 Biogeosciences. A number of papers have shown that non-linear size responses occur with common drivers of tree mortality. Droughts are thought to disproportionately kill large trees (Nepstad et al 2007 Ecology). The same goes for wind mortality (Rifai et al 2016 Ecological Applications), fire & wind (Silvério et al 2018 Journal of Ecology). But otherwise there are so many other drivers of mortality that cannot simply be linearly scaled by height and LAI.

Barlow et al 2003 Ecology Letters; Fauset et al., 2019 Frontiers in Earth Science; Fisher et al., 2009 Ecology Letters; Chambers et al., 2009 Ecology Letters; Chambers et al 2013 PNAS; Marra et al 2014 PlosOne; Marra et al., 2018 Global Change Biology; McDowell et al 2018 New Phytologist; Negrón-Juarez et al., 2018; Negrón-Juarez et al., 2010 Geophysical Research Letters; Rifai et al., 2016 Ecological Applications; Sivério et al 2019 Journal of Ecology; and many many more.

The x & y axes on figure 6 should be flipped in my opinion. There are some countering opinions on this, but typically observations are on the y-axis. However, there appears to be some non-linear influence between the quasi-observations and simulations of biomass mortality (necromass) that is not (or perhaps cannot be) captured with the linear regression.

Scaling from one forest plot to a large region: Again it should be made absolutely clear, repeatedly and throughout the abstract that these results are based on model simulations. This includes the estimates of LAI, which are indeed modeled and not directly observed. The arguments of this manuscript seems to be heavily dependent upon the veracity of the MCD15 product - however any optical remote sensing product

has saturation effects when forest canopies are dense with an LAI > 4. The assumption of the Paracou forest plot being representative of the entirety of French Guiana is exceptionally misplaced. The climate of Paracou is influenced by its proximity to the Atlantic. The supplemental figure S3, for example shows two aspects of how this site cannot represent the entirety of French Guiana. No one site can really be claimed to be representative of such a large area. The simulations do not appear to be very realistic. Paracou exists upon relatively infertile soil with extremely limited Phosphorus. The simulation of approaching 500 Mg Biomass/Ha in less than 50 years is inconceivable with field measurements of NPP. These numbers should be compared with field observations in around the tropical forests of the Guiana shield.

Data availability: The value of this model focused manuscript is markedly reduced if the data and code are not openly available. I think the unavailability of the data and code is also contrary to the journal guidelines (https://www.biogeosciences.net/about/data_policy.html). If the data is available, then make it available - otherwise a detailed statement is required as to why it is not available. The need to contact the authors is especially burdensome upon the reader, and is unlikely to be robust against the effects of time. Can the authors really guarantee they will always be around to provide the data and code when requested? Finally, even if FORMIND is available through other means, the results of this manuscript are unreproducible if it is not specific to the exact variant of FORMIND used in this manuscript.

Line Comments: Figure 1 caption: What is meant by rejuvenation?

Figure 3 caption: Put parentheses around acronyms that are being defined.

Figure 7: Is the histogram meant to serve as a colorbar for the left panel? This is not very clear if so. It would be better to add a color bar indicating what the color gradient signifies.

Table 2: Report the intercept value of the linear regression.

20: Multivariate regression is when there are multiple response variables in the same regression model. Perhaps 'multiple linear regression' is meant?

25: I cannot tell if this is in reference to a model simulation or field observations?

38: The Pan 2011 estimate of 2.8 is on the higher end and was assembled more or less haphazardly from the available forest census data and country level reports. More recent estimates are available.

41: You might see Korner 2003 Science, Chambers et al 2013 PNAS, and Fisher et al 2008 Ecology Letters.

67-68: I don't see how this statement can be justified.

81-83: I find this hard to justify. See comparisons on Paracou and Nouragues.

86-87: Competition for water is a major axis not mentioned. The Guiana shield has been struck numerous times by severe drought effects.

95: About the regression model, what is the response and what is the covariate?

105-110: The paper using FORMIND v3.2 appears to be focused upon estimation of biomass with respect to changes in forest management. The parameterized version of the model does not appear to be available from that publication either.

119-120: I find the model simulating mortality with arbitrarily distributed spatial patterns to be extremely implausible. Wind, fire, floods $\sim$ these all have a distinct spatial component. This spatial component has implications for who dies, and the post-disturbance light environment. Moreover, disturbance in reality is a punctuated event. If I read the section 2.2.2 correctly - the imposed disturbance intensity is actually just a multiplier on the baseline mortality rate. I don't think this is really anywhere near representative of disturbance in tropical forests.

160: This does not make sense to me. Allometric equations for biomass are typically nonlinear. See the widely used models including height in Chave et al., 2014 Global

Change Biology.

175-180: I don't understand what exactly was done here. Tab S1 in the supplement is actually a paragraph. Eq 5 and 6 appear to be the same equation. Was mortality derived from the simulation model? If this was the case, I don't think there is anything that can really justify this. The manuscript appears to be about upscaling mortality with remote sensing data - but the core critical part, the mortality - is derived from a simulation model. This is making a very large number of assumptions, which I find implausible.

259: Why 2km2 when the coarsest RS data was 1km2?

340-345: I think comparison with field based estimates of tau is important. I suggest reading more into the actual forest census based literature to come up with more comparisons. The Erb 2016 & Carvalhais 2014 papers are focused upon simulation results, and I don't agree that 20-30 years is similar to 40 years.

390: Terra Firme (not Terra Firma) is more commonly used to refer to this type of tropical forest

390: 'successional' -> 'succession'

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