

Biogeosciences Discuss., referee comment RC2  
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## **Comment on bg-2022-87**

Anonymous Referee #2

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Referee comment on "Modelling the impact of wood density dependent tree mortality on the spatial distribution of Amazonian vegetation carbon" by Mathilda Hancock et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-87-RC2>, 2022

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Hancock et al. derive spatial maps of wood densities using three independent approaches based on (1) species distribution models, (2) a random forest model and (3) a spatial interpolation method based on inventories. They then apply an ensemble of four different empirical wood density- mortality relationships to derive location-specific mortality rates. Finally, they apply these mortality rates in the DGVM TRIFFID to improve the spatial patterns of aboveground biomass in the Amazon rainforest. The authors find that specific combinations of the wood density maps and mortality-wood density relationships improve spatial patterns of vegetation aboveground biomass in TRIFFID.

The manuscript is for the most part very well written and each section is logically structured and easy to follow. I very much appreciate the effort the authors took to derive the three different maps of wood density which makes their analysis more robust. Furthermore, I also like the fact that (on top of the wood density maps) the authors also test several mortality functions.

However, I am not convinced by their finding/conclusion that these new spatially-explicit mortalities improve the models' performance in reproducing the aboveground biomass patterns in the Amazon rainforest. The spatial patterns in Figure 3 do not look very much different compared to control simulations despite just showing lower (M2, M4) absolute aboveground biomass (AGB) throughout the whole Amazon basin.

The authors apply several indices (absolute bias, CRMSD and Pearson correlation coefficient, Table 3) to support their results. However, most of the indices CRMSD and Pearson's R do not change much (or consistently) between the simulated combinations and the observations. Only the absolute bias is consistently lower for most of the simulations and the authors mainly argue around this index and its improvement.

If I understood it correctly this index compares mean observed AGB across all grid cells with mean simulated AGB across all grid cells. If that is the case, I do not agree that this measure should be used here at all. The lower values of this measure do not support an improvement in spatial patterns, but rather a canceling/averaging out of over-and underestimation of AGB as seen in Figure 5.

I am puzzled why this index was chosen at all, as I think a more 'fair' comparison here is to apply indices such RMSE that do pixel-by-pixel comparison and only then aggregate across the whole basin. I think it would be very helpful to also see RMSE between observed AGB and modelled AGB.

The authors do apply a similar indicator CRMSD which does not show lead to pronounced differences across all simulations.

Furthermore, the authors argue that they find a 40% improvement in Pearsons' R which is only true for one of the simulations and lower for most of the other simulations. I think it is not fair to just pick the best candidate out of all the simulations and argue that there is an improvement.

Finally, three of the four mortality wood-density relationships are similar (Figure A1), but lead to very different overall aboveground biomass distributions (Figure 3) which shows that AGB is very sensitive to the mortality rates. I am wondering if the relationship between mortality and wood density is necessary overall to improve the spatial patterns or if just an adjustment of the standard constant mortality rate would lead to similar improvements.

Overall, I am not convinced by the authors conclusions that the spatial explicit mortality rates based on wood density lead to an improvement in the AGB patterns of the Amazon basin and would therefore not recommend it for publication in *Biogeosciences* in its current state.

However, if the authors would show (1) that the RMSE is consistently improving between simulations and observations and (2) that a similar improvement cannot be achieved by simply adjusting the standard constant mortality rates, I would be happy to review the manuscript again.

Other comments

Lines 56-59: I think this part should be rather moved toward the discussion. Ending a paragraph with 'more data are needed to support this hypothesis' and then starting the next paragraph 'Given the evidence above...' does disturb the flow of reading.

Line 104 and equation (1): what are the indices  $i$  and  $j$ ?

Line 112: I would reframe 'It was only through...' to 'We modified gamma in Eq (1) to ...'. Otherwise, this would mean that there is literally no other way/process/function how wood density could affect vegetation carbon in JULES.

Line 117: What are noncompetitive mortality values? Low values? This deserves one more sentence for clarification.

Lines 126 – 129: It is not fully clear to me why there are two different  $\rho_{\max}$  values for M1 and M4. Why not simply including an if-statement to avoid mortalities getting to zero when wood densities are too large?

Line 139: If M2 is an improvement on M1 why not skip M1 entirely from this analysis?

Line 167: Some more information about the RF model setup would be helpful. What were the exact predictors? How many train and test samples? I think it would also be useful to show some validation metrics.

Line 172: I very much like that the authors included spatial autocorrelation.

Line 180: I am also missing some information about the setup of the Kriging? Ordinary or universal Kriging? What about the other parameters such as the nugget?

Line 186: What does a coefficient of determination = 0.35 say? Is that a good value? This should be also mentioned somewhere.

Line 198: I wonder how robust the results are when using another forcing e.g. GLDAS 2.0. I know that this might be a lot of effort, but I think it is worth testing if the results of this study are robust against the choice of climate forcing.

Line 220: I think M1 should only be mentioned in the supporting information as it leads to a dieback and is only shown in Figures 1 and 2 and not in the other figures.

Lines 225 to 235: I find the declarations of vegetation carbon with hats confusing. I think the veg subscript is useless as C is always used in combination with 'veg'. Why not drop the hat and the 'veg' in the subscript in favor of another index? For example, simulated vegetation carbon could be C\_sim and observed vegetation carbon C\_obs.

Equation 3: As I see no index x here, is that the difference between average simulated vegetation carbon and modelled vegetation carbon? If yes, I do not think this is an adequate measure for evaluating spatial patterns (See my main point).

Equation 4: Why not simply use RMSE to compare to the observations? Why CRMSD?

Figure 1: It is hard to compare the plots when all color legends have different ranges. I think they all should be the same range e.g. from 0.42 to 0.81. In the description text kriged map should also have quotes similar to 'SDM-based' and 'RF-based'.

Figure 2: Again, I think equal color legend ranges would be very helpful when comparing the plots. Furthermore, I would drop M1 from this analysis and move it to the appendix. Similar to M1-M4 I would also use consistent naming of the three maps in the plot and the description: 'SDM-based' map, 'RF-based' map and 'Kriged' map.

Table 2: What are Occurrence and Observation? SDM and RF based? If yes, I would use consistent naming here.

Figure 3: The legend says vegetation carbon? Is it that or AGB? I thought vegetation carbon also includes BGB.

Line 288: I do not see a reduction in CRMSD. These values are only very slightly different compared to the control simulation.

Line 290: What does it mean that they are less effective?

Figure 4: This is a nice figure, but apart from g and i the additional spatial variation is very small that M2-M4 have compared to the absolute differences in AGB in Figure 3.

Figure 5: This figure is also nice, but I am questioning if we really need the maps of wood density and mortality to get to such an improvement. I would like to see if simply adjusting the constant value of gamma could lead to quite similar results.

Figure 6: I do not understand the purpose of this figure. Why is it so clumped, e.g why are there no values around 0.008? Some more sentences explaining this figure would be useful.

Line 329: 'will be discussed later'. Where? Please refer to the subsection where this is discussed.

Line 371: What does relatively low mean?

Line 375: This sounds very interesting, and I think it should definitely be tried out here.

Line 402: I do not think that the bias statement is useful. I think this statement is misleading and only shows that in Figure 5 b, e, h the red and blue parts cancel out. I

agree that this is better than a complete under or overestimation, but I do not agree that it is improving the spatial patterns.

Line 446: Good point. How certain are the authors that the wood density – mortality relationship is needed to improve the spatial patterns. Couldn't a height-mortality relationship also be reasonable?

Line 457: I think phosphorus availability and its gradient inverse to AGB could also deserve some sentences here (section 4.3.1).