

Like the previous reviewer, I commend the authors for a well-written and comprehensive manuscript describing a new, impressive and important dataset representing the biomass stocks embodied in global tree roots. I particularly appreciated the thorough retrospective in the introduction describing the history of root biomass estimation at the global scale, the simple ecological analyses the authors include in their discussion, and the transparency with which they describe [most of] their methods. Combined, I think these aspects make the manuscript an accessible descriptor that will appeal to readers across many disciplines.

Response: Thank you.

My only substantial concern pertains to the way in which the author's 'central' estimate was determined distinct from their uncertainty estimate. The 'central' estimate (142 Pg) was generated using models trained and executed using covariates each represented by a singular data source. The corresponding uncertainty estimate, though, was generated using an ensemble approach that used multiple [alternated] data sources to represent those covariate values. It's unclear to me why the authors favored the data sources they did when generating their 'central' estimate and, having gone the lengths of creating an ensemble, why they didn't instead just use the ensemble mean or median as the 'central estimate'? I ask that at the very least, the underlying rationale be made clear in the text.

Response: Thank you for the constructive suggestions. Aboveground biomass is the most important predictor of the root biomass. We decided not to use the average among multiple aboveground biomass products as these products vary in quality. We think the recent GlobBiomass dataset is more reliable than other cited global datasets. Spawn et al., 2020 also acknowledged the fidelity of GlobBiomass and explained the rationale to use it in their study. For environmental datasets, for some variables we could get access to multiple sources, while for some variables, we have only one data source. For consistency, if one database could provide us several variables needed in our study, we favoured this database as inconsistency among databases (e.g., spatial and temporal resolutions) also brings errors in the estimation. We added the justification for why we used the "central estimate" around Line 140. Here and after, Line numbers in responses refer to the tracked manuscript.

In addition, I have several minor suggestions (largely related to clarity) that I ask the authors to consider and address:

Line 31: change "root plays the" to "roots play a"

Response: We modified the text as suggested.

Lines 55-80: This is a really nice summary of the discipline to date.

Response: Thank you.

Line 85: please add "n = " before 10307.

Response: We added "n="

Lines 90-91: Consider rephrasing to emphasize that *after comparing the results of all three of the candidate techniques*, you chose the RF approach because it performed best and only used it (not the others) for subsequent mapping and analysis. Right the text seems to abruptly drop any reference to the other two approaches (ANN and MARS).

Response: We modified the text as suggested.

Line 98: "Combining _____ with tree density..." please fill in the blank I've added to the text.

Response: We changed the text into "Combining our map of root biomass per tree with tree density".

Lines 103-105: Could these data be available from the authors? Was an attempt made to find out?

Response: This dataset was deposited at the figshare. We added this note.

Section 2.3: Text explaining why you chose the covariates you chose and why you selected particular datasets (over others) to represent those covariates is needed in this section.

Response: We chose these variables based on their relevance to root dynamics from field studies and their availability at the global scale. We added the explanation around Lines 123-124 (tracked manuscript).

Lines 124-129: Specifying that the "BIO_" variables are simply the WorldClim bioclimatic indicators help clarify why these [otherwise] seemingly odd abbreviations are used.

Response: We added "BIO is the abbreviation for WorldClim bioclimatic indicators" to the main text.

Line 129: Why not also include slope and/or aspect?

Response: We did not have the slope and aspect as topographic factors after a preliminary test which shows minor effect of elevation from our database. We assume slope and aspect would also have minor effects for our global dataset as samples from mountainous and non-mountainous regions do not show big differences. It is likely elevation (and also slope and aspect) indirectly affect our prediction through modifying climate, vegetation and soil factors. We therefore did not include slope and aspect. We added this explanation at Lines 136-137.

Line 130: Personally, I think it would be useful for table S1 to be included in the main text.

Response: We understand Reviewer's preference. We feel Table S1 is long and makes the main text less readable. We therefore opted to keep this Table as supplementary information.

Line 134-136: This sentence is unclear to me. Are you saying that the two input datasets by Baccini and Santoro are the most reliable sources or that the layer you derive from them is? Please clarify in the text.

Response: We deleted the reference to Baccini to reduce confusion. Baccini focused on the tropical region. Our study targeted at the global scale and we did not merge different regional products. Santoro is therefore the right reference.

Line 138: Did you also use the Baccini dataset? From the preceding text and references I was led to believe that you somehow combined the GlobBiomass and Baccini maps but here it seems like you only use GlobBiomass. Whichever is true, please clarify in the text.

Response: We only used GlobBiomass. As above, we deleted the reference to Baccini.

Line 145: Please include an explicit reference to the canopy height map, here.

Response: We added the reference Simard et al., 2011.

Line 148: Please clarify: I don't believe Hansen reports a tree count, just area. So how can the consensus dataset give the same tree count as Hansen?

Response: Sorry for the confusion. Crowther et al., 2015 tested on both the Tuanmu and Jetz (2014) and the Hansen et al. (2013) land cover maps, which give the same tree count. We modified the text as “Crowther et al. (2015) showed the total tree count from tree density map based on the Tuanmu and Jetz (2014) land cover is the same as from the Hansen et al. (2013) land cover product” (Lines 160-161)

Line 152: Given your amendments and modifications described in this paragraph, can you state explicitly here what definition of 'forest area' your map adopts? This'll be important to facilitate future comparisons much like the comparison you make here.

Response: In our study, we define the forest based on the tree canopy coverage. “A pixel was labeled as forest when the tree canopy density was larger than 15% according to Hansen et al. (2013)’s dataset (hereafter Hansen2013) averaged at 100 m” (Lines 152-153). We also tested the impact of forest definition through varying the threshold value from 15% to 0% and 30%.

Lines 153-159: Do all of these age maps use the same reference year? I.e. Age in/as-of what year? Please clarify in the text.

Response: Not all age maps use the same reference year. GFAD V1.1 represents the 2000-2010 era, where the Zhang map was derived for the period 2009-2013. We added the reference year information around Lines 170-175. We used these maps as a coarse approximation of age information and mentioned “the quality of the global forest age data might also affect the power of this variable in improving root biomass predictions.” (Line 301).

Line 163: Does this imply that one candidate map was made by simply applying allometric equations to a map of forest height (and presumably stratified by taxa)? If so, that exercise isn't yet clearly explained in the methods text, here. Please do so.

Response: We compared among the allometric equation and three machine learning algorithms. For the allometric equation, we assume one universal equation is underlying the nature of aboveground and belowground biomass partitioning, as reported in Enquist and Niklas (2002). We did not stratify our data by taxa. We added this explanation to Lines 182-183. Supplementary Table 7 also provided information on the allometric fitting.

Line 163: Please also provide the long-form names when introducing these acronyms.

Response: We provided the long-form names as suggested.

Line 190: It's hard to know how you actually chose your final model given that you considered three distinct criteria. Can you elaborate here a little bit? Presumably you considered a hierarchy in these criteria? For example, if one model had the highest MAE and another had the highest R2, which did you choose? Why? How?

Response: We considered MAE, R2 and the number of predictors. We chose the model that has the lowest MAE, highest R2. If models have comparable R2 and MAE, we chose the model with smallest number of predictors. In our tests, these criteria allowed us to choose a reasonable model. Please check on Supplementary Table 8 we added in this revision, on the performance of different models. The model has a high R2 generally also has a low MAE. If R2, MAE and the number of predictors did not give us a unique choice, bringing in another metric would be an option. In our case, these three criteria works. Please also check our response to the following comment.

Line 190: From an ecology perspective, I can understand that minimizing the number of covariates can help more clearly explain the drivers of predicted patterns (e.g. the variable importance assessment described below). But, from a mapping perspective, where you ultimately rely on RF and where I would think accuracy is the ultimate goal, I wonder if it would be more appropriate to retain all predictors? Can you at least explain in the text your reasoning for culling covariates/favoring parsimony?

Response: In our case, when we put all variables in our model building, we actually got a lower R2 or a higher MAE. Our previous explanation is likely to cause confusion. We modified the text as ""We favoured the model with a smallest MAE and a highest R2. For models with comparable MAE and R2, we favoured the model with the minimum number of predictors" (Line 210). We gave the priority to R2 and MAE, and then the number of predictors.

Line 193: Consider including a table with the validation stats of each approach to clearly illustrate why RF was chosen over the others.

Response: We added Table 8 for the performance of different machine learning methods.

Lines 200-201: This sentence is vague. I assume you mean you combined it with the Crowther map? Either way, please make this step clearer in the text.

Response: Yes. The random forest model generated predictions of root biomass per individual tree because our training data is based on individual trees. So we used the tree density (how many trees per area) map to estimate the root biomass per area. To make it clearer, we rewrote this part as "Our RF model was built upon individual woody trees. We combined the RF model with global maps of selected predictor variables to produce the map of root biomass for an average woody tree which has a unit of weight per tree. This map was multiplied by tree density (number of woody trees per area) (Crowther et al., 2015) at 1-km resolution to obtain the final root biomass map with a unit of weight per area (Supplementary Figure 1). " (Around Line 225)

Section 2.6: Above you used a single dataset for each covariate to generate your 'central' estimate of root biomass. Here you describe using a separate ensemble approach to generate your error estimates. Why not instead use the mean/median of the ensemble approach as your 'central estimate'? In other words, why did you decide to prioritize the datasets listed in table S1 over those in S2? At the very least, this should be clearly explained in the text.

Response: Please see our responses above.

Line 230: I suggest noting that this is Pg of dry biomass so that it's not wrongly compared to estimates by others that might be in units Pg C.

Response: We added a note saying "Note here we reported values in unit of Pg of dry biomass instead of Pg C"

Lines 231-232: Is this the actual definition of forest cover you used? Above in the methods, it seemed a little more convoluted than this?

Response: Please see our responses above.

Discussion/Figure 1: Both Mokany et al. 2006 and the 2019 refinement to the IPCC guidelines report mean/median R:S's that are larger than 0.50 for some ecotypes --namely some dry woodlands, savannas, oak forests, and boreal forests. Visually it looks like your predicted patterns generally agree with these sources (at least in a relative sense) but because the colour scale is capped at 0.50,

it's hard to know if the magnitude of the estimates in these areas are comparable? Are they? Perhaps an R:S comparison with one or both of these widely used sources would be useful to include in the discussion?

Response: Thanks for this nice point. Our predictions of locations with high R:S values are in line with Mokany et al. 2006 and the 2019 refinement to the IPCC guidelines report. We added to the discussion “And our predicted pattern of relatively large R:S in regions such as dry forests, savannas and boreal tundra woodlands (Figure 1c) are in line with data compiled from Mokany et al. (2006) and the 2019 IPCC refinement (IPCC2019, 2019)” (Around Line 365)

Table 1: The new maps by Spawn et al. (2020) may also be worthy of comparison here in Table 1. Near the end of their manuscript, they report 122 Pg C in global root biomass with 28.3 Pg C embodied in herbaceous (i.e. grass) roots. So the comparable value is likely 94 Pg C in tree biomass which, if you simply assume wood biomass contains 50% C, equates to 188 Pg biomass. Note too that they appear to use a more liberal definition of tree cover than you.

Response: We added the result of Spawn et al. (2020) to Table 1. We also added this point to the abstract and discussion (Around Line 365).

Lines 326-330: A similar comparison with the 2019 IPCC refinement and or Mokany would likely be well-cited but is not required.

Response: Please see our responses above. We added this point to our discussion.

Lines 333-363: Nice point and supporting analysis.

Response: Thank you.

Reference

Enquist, B.J., Niklas, K.J. (2002) Global allocation rules for patterns of biomass partitioning in seed plants. Science 295, 1517-1520.