Response to Benjamin Stocker (Referee)

Thanks for your comments and suggestions. We will revise this paper according to your review comments. Below are our responses and plan for revision. Your comments are in italics, and our responses are in roman.

This paper describes a model and its prediction for competitively optimal allocation (ratio of root to foliage surface area) and how it varies with a range of nitrogen levels and two CO2 levels. The same (or a similar version of the same? See point 12 below) model has been described and applied previously to investigate optimal phenological strategies in Weng et al. (2016, GCB, doi: 10.1111/gcb.13542) and a carbon only version was presented in Weng et al. (2015, BG, doi:10.5194/bg-12-2655-2015).

Thanks for the summary.

The present manuscript addresses allocation as a single variable parameter, although multiple traits affect plant functioning in the face of N availability and CO2 concentrations. However, the focus on allocation is justified, in my opinion, for two reasons: First, allocation warrants particular attention as it is a key process that is known to be responsive to changes in above and belowground resource availabilities and affects the carbon cycling in ecosystems when an allocation shifts occur between long-lived wood and short-lived foliage or fine roots. Second, most vegetation models simulate allocation either based on fixed parameters, or based on empirical relationships. However, as pointed out in the manuscript (l. 528), predicting allocation from first principles is key to realistically and robustly simulating vegetation changes in response to multiple changing environmental factors. The present model embodies a promising way forward to resolve this challenge, determining a competitively optimal allocation strategy, based on height-structured competition for light in the form of a Vegetation Demographics Modelling approach (cohort-based, not average individual-based as is common in Dynamic Vegetation Models). In this respect, the present model takes a pioneering path, that only less than a handful (or even less?) other models can follow.

Thanks for the comments.

*I see two main weaknesses of the present study. **First, predictions are not tested against observational data**. What data needs to be used as a test (see comment 10)? However, I don't expect much additional insight from a comparison to observational data at this stage, and consider the theoretical predictions to deserve to be presented as "naked predictions". However, for a paper that deals with just the theoretical side of the problem, some aspects may warrant additional attention (additional figures for results and extended discussion) in the manuscript (see comments 6, 8, 10, 11). **Second, the modelling approach with regards to the excess-C-sapwood allocation** (see l.245) raised some questions for me. Is it compatible with our understanding for what controls sapwood area (or what determines the Huber value, defined as the ratio of sapwood area: leaf area)? I worry that this model formulation may cause unrealistic model behaviour in some instances. Anyways, the conclusions need to be drawn carefully with regards to this aspect (see comment 5 below).*

I agree with Dr. Stocker. We will update the description of allocation scheme (Please see responses to referees 1 and 2) and discuss the comparison of our simulations with observations and simulations of other models if invited to submit a revised version.

Specifically for the Huber value (sapwood area/leaf area) here, it is kept constant for a PFT, following Eq. 6 in the main text, where Huber value is a PFT-specific parameter (α_{CSA}). Since the allometry equations are correct (Eq. 2 in the main text) and the model doesn't have processes of plant hydraulics, Huber value is just used to differentiate sapwood and heartwood and does not affect the functions of the trees. Please see a revised description of this equation below:

"We set *targets* for leaf (L^*), fine root (FR^*), and sapwood cross-sectional area (A^*_{SW}) that govern plant allocation of non-structural carbon and nitrogen during growth. These *targets* are related by the following equations based on the assumption of the pipe model (Shinozaki, Kichiro et al., 1964):

$$L^*(D,p) = l^* \cdot A_{CR}(D) \cdot LMA \cdot p(t)$$
$$FR^*(D) = \varphi_{RL} \cdot l^* \cdot \frac{A_{CR}(D)}{SRA} \qquad \text{(Eq. 1)}$$
$$A^*_{SW}(D) = \alpha_{CSA} \cdot l^* \cdot A_{CR}(D)$$

where L^* (D, p), $FR^*(D)$, and $A^*_{SW}(D)$ are the targets of leaf mass (kg C/tree), fine root biomass (kg C/tree), and sapwood cross sectional area (m²/tree), respectively, at tree diameter D; l^* is the target leaf area per unit crown area of a given PFT; A_{CR}(D) is the crown area of a tree with diameter D; LMA is PFT-specific leaf mass per unit area; and $p(t)$ is a PFT-specific function ranging from zero to one that governs leaf phenology (Weng et al., 2015); φ_{RL} is the target ratio of total root surface area to the total leaf area; SRA is specific root area; and α_{CSA} is an empirical constant (the ratio of sapwood cross-sectional area to target leaf area). The phenology function p(t) takes values 0 (non-growing season) or 1 (growing season) following the phenology model of LM3-PPA (Weng et al., 2015). The onset of a growing season is controlled by two variables, growing degree days (GDD), and a weighted mean daily temperature (T_{pheno}), while the end of a growing season is controlled by T_{pheno}."

The "excess-C-sapwood allocation" is a numerical step to adjust the growth of leaves, fine roots, wood, and seeds according to available carbon and nitrogen. The model uses these steps to numerically solve the following growth/allocation equations at given carbon and nitrogen:

$$G_C = G_W + G_L + G_{FR} + G_F \qquad \text{(Eq. 2.1)}$$

$$G_L + G_{FR} = Min\begin{bmatrix} [L^* + FR^* - L - FR], \\ (1 - f_{W,min})\, G_C \end{bmatrix} \cdot r_{D/S} \qquad \text{(Eq. 2.2)}$$

$$G_F = v[G_C(t) \cdot r_{D/S} - (G_L + G_{FR})] \qquad \text{(Eq. 2.3)}$$

$$G_W = (1 - v \cdot r_{D/S})G_C - (1 - v)(G_L + G_{FR}) \qquad \text{(Eq. 2.4)}$$

$$\frac{(FR+G_{FR})SRA}{(L+G_L)/LMA} = \varphi_{RL} \tag{Eq. 2.5}$$

$$\frac{G_L}{CN_L} + \frac{G_{FR}}{CN_{FR}} + \frac{G_F}{CN_F} + \frac{G_W}{CN_W} \leq G_N \tag{Eq. 2.6}$$

$$L + G_L \leq L^* \tag{Eq. 2.7}$$

$$FR + G_{FR} \leq FR^* \tag{Eq. 2.8}$$

where, $r_{D/S}$ is a nitrogen-limited allocation factor to be solved in this set of equations; $f_{W,min}$ is the minimum fraction of G_C for stems (0.15 in this study); v is the fraction of carbon for seeds (0.1 in this study); CN_L, CN_{FR}, CN_F, and CN_W are the default C:N ratios of leaves, fine roots, seeds, and sapwood, respectively. Parameter $r_{D/S}$ ranges from 0 (Highest nitrogen limitation; no nitrogen for leaves, fine roots, and seeds at this step) to 1 (nitrogen is sufficient for all tissues).

In the "SPECIFIC POINTS" described below, I am suggesting some modifications in the description of the model and to improve readability, and some changes in the presentation to distill the most relevant points and most interesting insights from this research. GENERAL POINTS may warrant particular attention. The paper is generally written very well and the presentation of results is clear and clean. If these points can be addressed satisfactorily, I may recommend a revised version of this manuscript for publication in Biogeosciences.

GENERAL POINTS

1. Dynamic adaptation (adaptive plastic responses) of allocation occurs also within species. The present model embodies the assumption that species follow a fixed allocation strategy, and changes in allocation are induced by replacement of species with different allocation strategies. ***Please add a discussion for the assumption of fixed within-species allocation may affect results.***

Our allocation scheme (Eq. 2) itself is flexible and can change with environment even with one fixed scheme of allocation (i.e., fixed φ_{RL}) by numerically maximizing carbon usage at nitrogen limitation. The key step in solving this set of equations is to numerically solve $r_{D/S}$ in each growth step (daily in this simulator). The parameter $r_{D/S}$ changes with relative nitrogen availability. When there is no nitrogen limitation, $r_{D/S}$ equals to 1 and the allocation follows the conditions defined by Eqs. 2.1~2.5 (carbon only model). When $r_{D/S}$ equals to 0, G_N does not meet the nitrogen demand even if all the G_C is allocated to wood tissues and the model has to return the excessive carbon to the NSC pool (this is a very rare case because of low carbon input long before $r_{D/S}$ approaches to 0 due to nitrogen limitation. However, our codes must be able to deal with all possible cases.). When $r_{D/S}$ is in between ($0 < r_{D/S} < 1$), the leaves and fine roots cannot reach to their targets after this step of growth (i.e., plants maintain a low LAI in low nitrogen environments). At low nitrogen availability, the parameter $r_{D/S}$ keeps a low LAI, a relatively constant NPP/GPP ratio, and a relatively stable NSC for each single PFT.

We will add a paragraph to discuss this allocation scheme following this suggestion.

2. A competitively optimal strategy is determined for stationary boundary conditions. Does this inhibit such a modelling approach to be applicable for global change simulations (transiently changing boundary conditions)? See also comment 11.

This study is not for directly application at global scale, but the succession patterns can be used to understand transient responses of vegetation to climate changes and the model formulations are being incorporated into an Earth system modeling framework (NASA GISS ModelE). Our analysis can help to understand model behavior at global scales.

*3. Allocation and the plant-level C and N budgets, assume fixed tissue C:N ratios and flexible allocation to sapwood to make C and N budgets work. **This does not correspond to the known controls on sapwood area and may induce unwanted model behaviour.** However, it is difficult to think understand to what degree this affects the results and conclusions. See also comment 5.*

The allocation between fine roots plus leaves and wood does not affect the differentiation between sapwood and heartwood. We have a detailed description of the conversion from sapwood to heartwood in the appendix of Weng et al. 2015. The new tissues of wood are always counted as sapwood, and the oldest tissues are converted to heartwood. Actually, the differentiation between sapwood and heartwood does not affect model function in this model because we don't have plant hydraulics yet in this model. We will update allocation scheme to make it easier to understand.

4. Total N that is circulating in the system is manipulated for the different simulations, with zero N loss and inputs. This sounds like a rather pragmatic than realistic solution. In reality, losses are never zero, and N levels are manipulated in experiments by fertilisation. Why is it not implemented like this? Would you expect any systematic differences between your implementation and one with non-zero losses and manipulated inputs?

The processes of nitrogen inputs and losses are complex, involving nitrogen deposition, fixation, soil nitrogen mineralization, root uptake efficiency, microbial immobilization, leaching, denitrification, etc.. However, at equilibrium state, the total ecosystem nitrogen is determined by relative rates of nitrogen input and nitrogen output. For example, a high nitrogen input and low output lead to high total ecosystem nitrogen level. In this study, we pack all these effects by setting different total N levels to represent implicitly relative nitrogen input and loss rates. For example, a high total ecosystem nitrogen level represents the ecosystems and edaphic conditions with relative high nitrogen input and low loss rates. By packing these processes into different total N levels, we can focus our study on allocation.

We did both nitrogen closed (with zero nitrogen input and loss) and nitrogen open (with non-zero nitrogen input and output) runs in a previous study (Weng et al. 2017, Global Change Biology). In both types of runs, the nitrogen level determines the competition of PFTs and the competitively optimal player since plants can only "feel" the nitrogen availability, and don't "care" how this availability is set (e.g., either by setting the total N or through complex input and output processes).

We will add a discussion about the meaning of the different nitrogen levels if invited for submitting a revised version.

5. The abstract mentions "opposite fractional allocation to fine roots and wood" in competitive and fixed-allocation runs. ***Relatively more allocation to fine roots at high N levels in fixed allocation runs sounds like a result that runs counter to the common understanding of the controls on fine root allocation*** *(e.g. Poorter et al., 2012 New Phytologist, doi: 10.1111/j.1469-8137.2011.03952.x), and appears confusing in view of the fact that the model is based on a fixed root:leaf area ratio.* ***I suspect that the increase in relative root allocation at high N levels in the fixed-allocation run is a result of the excess-C-sapwood allocation in this model.*** *See also my comment 3. If this is indeed the case, I would recommend not to present results in the abstract that are contingent on this, arguably unrealistic, model behaviour. I guess the authors don't want to challenge the common understanding of fine root allocation controls with their results. It would suffice to point out that in the competition runs, relative fine root allocation de- creases with increasing N levels; and present this in the light of the common modelling approach whereby the root:leaf area ratios (and effective relative allocation ratios) are fixed.*

Thanks for the suggestion. High allocation to fine roots is resulted from "**excess-C-sapwood allocation**" in the basal allocation setting (numerical solution of parameter $r_{D/S}$). The model solves two parameters in these model runs: $r_{D/S}$ (at each daily step for both single PFT runs and multi PFTs runs) and φ_{RL} (after more than a thousand years model runs with multiple PFTs).

Our results are consistent with Poorter et al. 2012 well (Fig. 1:f of Poorter et al. 2012), because their data should be compared with our multi-PFT, long-term simulation results (i.e., competitively optimal strategy in our Fig. 3). For a single PFT (fixed strategy and short-term responses to nitrogen addition), it should be compared with fertilization experiments (e.g., Lee & Jose 2003, Forest Ecology & Management).
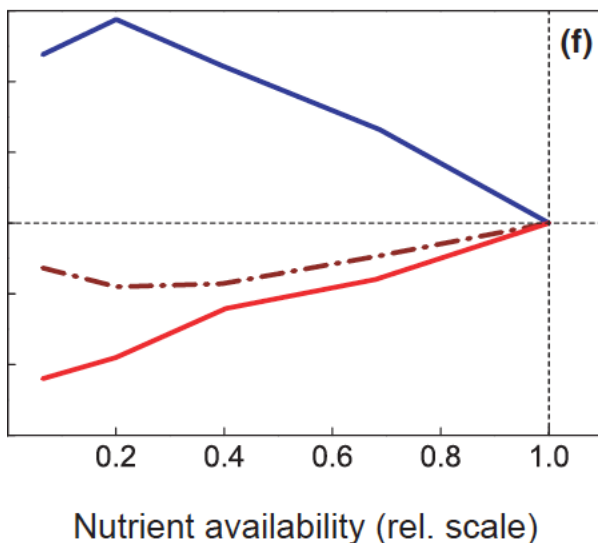
leaves (LMF; red line), stems (SMF; brown line) and roots (RMF; blue line)

We will revise the abstract according to reviewer's suggestions, the data analysis of Poorter et al. 2012 and other nitrogen fertilization experiments.

6. Presentation of results for a fixed φ: In my understanding, the essence of this research is to predict how stand-level relative allocation changes in response to N availability and CO2. The approach to predict it is to derive a competitively optimal allocation strategy at the individual-level. In view of the main aim (essence) of this research, I would expect as a (first) figure something that shows competitively optimal φ for each N/CO2 level, derived from the competition runs.

Thanks for the suggestion. We will add a new figure to show competitively optimal φ for each N and CO_2 level.

7. A question: Does each point in Fig. 3 show the wood/root/foliage NPP (fraction) for the competitively optimal φ at the respective N and CO2 levels (for the competition runs)? Comparing this to values from fixed-allocation runs with a pre-specified φ is maybe not the most interesting. This leads to the next point. . .

The left is absolute, and right is fraction. Please see the figure with unit:
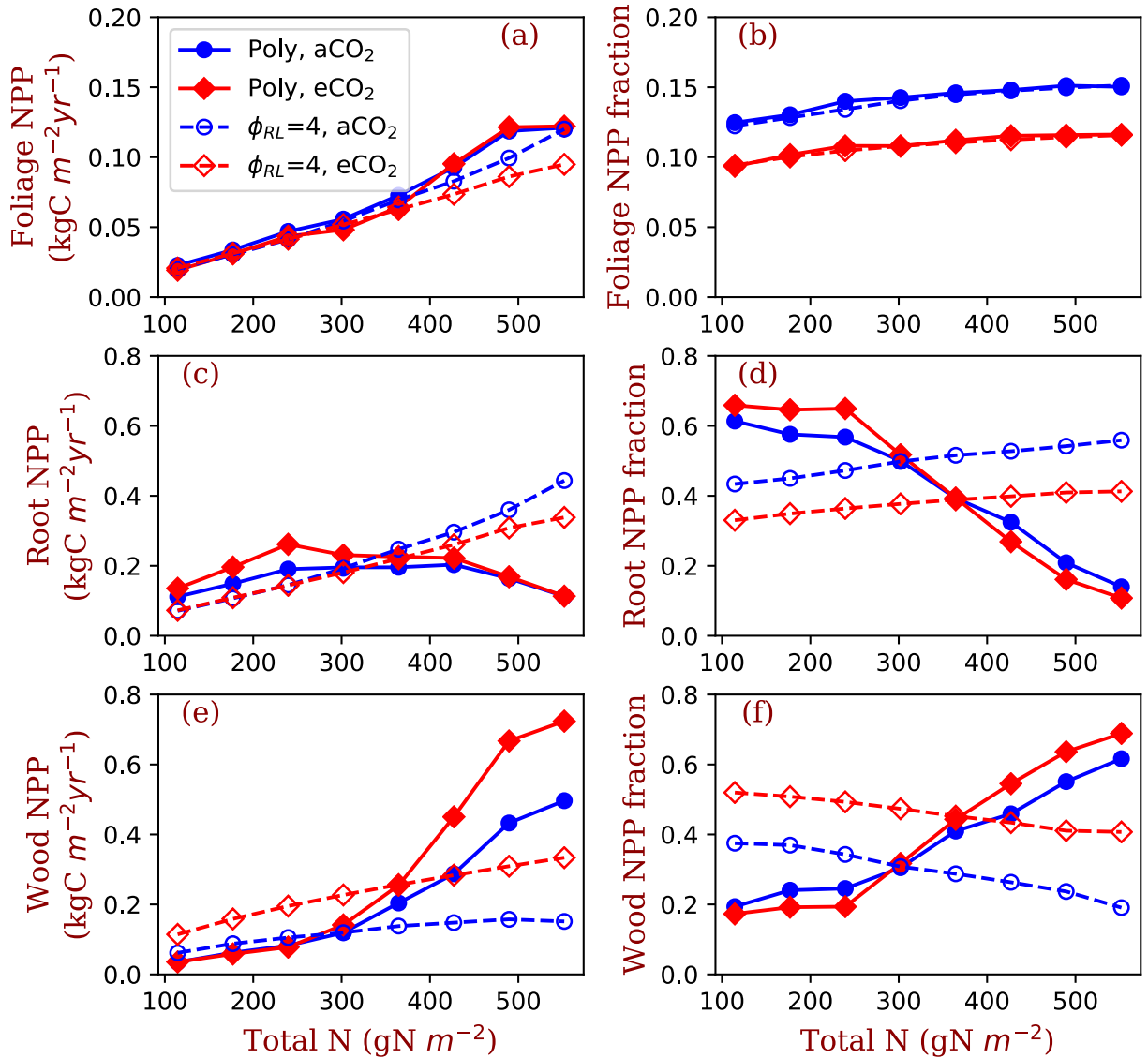
*8. [see Addendum to this comment] In view of my comment 1 (dynamic adaptation of allocation also within species), readers may want to know **if an optimality criterion that is defined for some instantaneous individual-level metric (like foliage NPP, or height growth, etc.) leads to the (qualitatively) same predictions as an optimality criterion that is funded in an evolutionarily stable strategy (ESS, like the one applied here)**. It might be interesting to evaluate the fixed-allocation runs to determine which φ maximises some optimality criterion (like foliage NPP, or height growth, etc.). A (first) figure could then compare this individual-level derived optimal φ to the ESS-derived optimal φ. Furthermore, points in Fig. 3 for the fixed-allocation runs could then be taken to represent the φ level that maximises the optimality criterion for each N/CO2 level respectively. This would also enable a direct comparison of the two optimality approaches (ESS vs. instantaneous). I understand if this suggestion is beyond the scope of this paper, or not feasible. Anyways, I would be interested to learn more about such a comparison. Addendum: I see now (upon reading the Discussion, l. 425), that this point is*

addressed by Supplementary figures S1 and S2. Maybe this is too much of my own personal interest, but I think to generally enhance this point in the presentation of the results (possibly with additional/modified figures as suggested above) would improve the manuscript.

I will explain the differences between optimal vs. competitively optimal if invited to submit a revised version. Basically, optimal strategy is not necessarily the competitively optimal. From Figures 1 and 2, you can see that low root allocation is optimal in many N levels. However, they cannot outcompete high root allocation strategies if invaded by them. That means, in the environment created by the optimal strategy individuals, another strategy can out compete the resident, though it may have low NPP or fitness in monoculture. I will add a figure in supplementary materials to show this.

9. A description of how the competitively optimal allocation is determined (description of the algorithm), would be helpful. May be added before current Section 2.2.

I will add a description following this suggestion.

10. The differences in predictions based on different allocation schemes are interesting, but the missing comparison to observational data prevents conclusions to be drawn about which is more realistic or leads to better model performance. The question is: What is the key observation that can be used to test predictions? And of course: What is the key prediction that authors want to test? In my view, it is a viable option to remain with theoretical predictions, not actually using data from observations. However, this may require an extended discussion in the light of generally observed patterns with additional references to the literature. The challenge remains that the overproliferation in root growth predicted by the competitively-optimal allocation scheme may not directly be testable. How much is over-proliferation in reality? What would be a suitable observation to evaluate this prediction?

I will add a paragraph of discussion to discuss the value of this study in experimental perspectives.

11. Regarding implications for Earth system modelling: From reading this manuscript, it's not entirely clear whether the approach for determining the competitively-optimal allocation strategy is applicable for typical Earth system simulations, where boundary conditions change transiently. In my understanding, the approach chosen here determines a system steady state, formed by a monospecific stand with a certain allocation strategy, that cannot be invaded by any species with a different allocation strategy. As explained in the manuscript, this requires the model to be run into steady state for 1200 simulation years. How would this be implemented for a typical Earth system simulation setup? I think it would be very informative to complement Section 4.3 with a discussion on this point.

It is helpful for understanding model behavior and track competitively optimal strategy.

We are exploring one more layer of complexity, adaptive dynamics (or successional dynamics), on the top of the pool-flux dynamics and demographic dynamics. For a robust DGVM in Earth system models, it is necessary to analyze these processes and their effects on model performance in detail. We will discuss it in detail if invited for submitting a revised version.

12. The description of how the present model version differs from model versions used in Weng et al., 2016, 2015, could be made clearer.

I will put more technical details of this model into a supplementary material.

SPECIFIC POINTS
l.96: Should be a gap in 'trait (s)'?

Done as suggested. But, we will remove this equation following Reviewer 1's suggestion.

Eq. 2: To solve the model, ds/dt has to be set to zero, I guess. Shouldn't this be reflected in Eq. 2? Or how exactly is the competitively optimal strategy determined?

This equation is used to show the temporal dynamics of a plant trait in evolution, which, conceptually, is the numerical shifts of a plant trait approaching its ESS in simulations.

For analytically solve this equation, one needs to build up a fitness equation, such as:

$$Fitness(s_I | s_R) = Benefit(s_I | s_R) - Cost_{I,R}(s_I | s_R) - Cost_I(s_I)$$

where, s_I is invader's trait and s_R is residence. Let the derivative of the right side to be zero and according to the definition of ESS $s_I = s_R$. Then, it is possible to solve it analytically if you are lucky.

We will remove this equation following Reviewer 1's suggestion. We agree with the Reviewer 1 that this equation is not very helpful here. We will add a paragraph to explain the processes of approaching ESS by succession in VDMs. The numerical simulations are complex enough, we don't want to complicate the paper with more discussions of this equation.

l.148: Apart from variations in across-species allocation patterns (e.g., oak species tending to invest more into roots), there are also clear patterns in within species and within-individuals (flexible adaptation) variations in allocation when subjected to shifts in resource availability. In my understanding, such fast allocation responses are not captured by the modelling approach here. This should be clarified. Connects to Comment 1 above.

They have to follow the same rules, otherwise they will be replaced by those who follow the rules. Actually, we used only one PFT, which only differs in fine root – leaf area ratio (φ). If plants have enough plasticity in allocation, they will approach the ESS much quicker. We will make it clear in a revised manuscript.

l.159: The simulation experiments are described in the abstract and intro to be done along a "nitrogen availability gradient". How did you manipulate N? This is described at a later stage, but could already be made clear here.

We will clarify the setting of ecosystem N.

Section 2.1: A separate paragraph on how CO2 assimilation is simulated, would be helpful.

We will add a brief description of the photosynthesis model, which is the same as it in LM3-PPA and LM3.

Eq. 3: To be consistent with Fig. 1, I would suggest to use the symbol X_FR as the pool size (or C_FR in this case), distinguish community- and individual-level variables for example using a bar over the variable for the community-level, and use a separate letter for the parameter 'Root_0' (e.g., K_FR).

Done as suggested.

Eq. 4: Should be clarified that this is the community-level total root biomass (if I am correct). Clarification is needed to understand Eq. 4.

Clarified. C_{FR}, total is the total roots in soil, with unit of kgC/m2 and $C_{FR,tree}$ is the total root carbon of a tree (in a cohort with identical trees) with unit of kgC/tree. This equation transforms nitrogen uptake rate from gN m-2 hour-1 to gN tree-1 hour-1.

Eq. 5: D is not defined. Diameter?

Yes, it is diameter. defined.

l. 215: Add bracket: ". . . targets for leaf, fine root, and sapwood cross-sectional area (L*_k, FR*_k, and A*_SW)" here for a better overview of the description.

Done as suggested.

Eq. 5/6: How is D incremented? The way the system is described now, the tree doesn't grow in D or am I missing something?

We add a new equation to show diameter growth.

l. 238: Can you add f_1 and f_2 to the description in this sentence? E.g., "capped by a larger fraction of NSC (f_1)"?

done as suggested.

l. 241: Are Eq. 7 and its parameters f_1 and f_2 identical throughout the year? The description here suggests that something is different during leaf flush versus the period of "normal growth". Or maybe I'm just misunderstanding it the way it's formulated now.

They are constant over time. Generally, "$f_1 NSC$" defines the maximum NSC availability at the beginning of a growing season when $(L+FR) \ll (L^*+FR^*)$; "$f_2 NSC$" defines the normal growth of stems after $(L+FR)$ is close to (L^*+FR^*) (i.e., normal growth during the growing season;

"[(L*+FR*) -(L+FR)]" are the carbon for the new leaves and fine roots due to stem growth in last time step.

The term "f_1NSC" also prevents overspending of NSC when NSC is very low in some bad years.

l. 246: Units of 0.15? g C?

It's the fraction of NSC. Clarified.

l. 254: Since sapwood production requires N as well (although relatively less than production of other tissues), and "excess C" sounds like this is the amount of NSC left, after NSN is used up (hence zero), I would assume that some iteration is necessary to perfectly match the use of NSC and NSN in the allocation procedure. How is this solved? Either more detail should be given here, or the description should be modified to avoid misunderstanding.

It can be analytically solved. In the codes, we numerically solve it by iterating at daily step and allowing the wood C:N ratio to variate from day to day.

We will clarify it in a revised version.

l. 260: I would welcome a summarising sentence on the mechanisms determining C:N stoichiometry. The connection between sapwood allocation and the NSC:NSN budget implies that a plant that acquires relatively little N in comparison to assimilated C (in other words: an "N-limited tree") would produce relatively more sapwood. Does this mean that Eq. 6 (the A_SW sub-equation) is "over"-satisfied? What are the implications of this in the model? Does it affect the relationship between height growth vs. crown area expansion?

According to the allometry equation (Eq. 5 in the main text), tree height and crown area are functions of diameter. So, how to define sapwood does not affect anything in the current version of the model. The A_{SW} sub-equation Eq. 6 is used to separate sapwood from the whole trunk. As mentioned in the responses to General comment 2, it does not affect anything of the model. We did this because the model needs this variable. (We really need it when incorporating plant hydraulics.).

We will update the whole section with a new description of the model's allocation scheme. The partitioning of carbon and nitrogen into the plant pools is constrained by allometric equations, targets of leaves and fine roots, and the C:N ratios of these plant pools. The plant growth (and therefore allocation) is simulated at two steps: 1) calculating the amount of carbon and nitrogen that will be used for plant growth at this time step (which can be hourly, daily, weekly, etc.); 2) allocating the available carbon and nitrogen to leaves, fine roots, seeds, and sapwood following rules of first principles. We let the plant growth follow the rules below as they are in the first version (Weng et al. 2015, Biogeoscieces. carbon only model) and second version of LM3-PPA (Weng et al. 2017, Global Change Biology. carbon and nitrogen). Overall, this is a flexible allocation scheme and still follows the major assumptions in the previous versions of LM3-PPA (Weng, et al., 2015, 2017). It prioritizes the allocation to leaves and fine roots, maintains a minimum growth rate of stems at nitrogen limitation, keeps the constant area ratio of fine roots to leaves, and optimizes resource usage by relocating carbon and nitrogen to wood tissues when

nitrogen is not sufficient for full growth of leaves and fine roots. When nitrogen is limited, leaves and fine roots are lower than their targets, reducing photosynthesis and carbon supply.

Section 2.1: A description of how the competitively optimal allocation is determined (description of the algorithm) would be helpful. May be added before current Section 2.2.

We will add a description of "competitively optimal" in Section 2.2.

l. 292: "Full factorial" suggests that all combinations of treatment factors are applied to force the runs. But here, this is a mix of a treatment factor (N levels) and model parameter (phi). I suggest to rephrase this.

We will rephrase it as suggested.

Table 1: If i=(114.5, . . ., 552) g N m-2, then 4.5-0.5*i is a negative number. Is this correct? Maybe N levels in units of kg N m-2 are used here?

Here, i takes the value of 1, 2, 3, 4, …, 8, following the order of the nitrogen levels from 114.5 to 552 gN m^{-2}. We will clarify it.

l. 362-364: The decrease in fractional allocation to fine roots with elevated CO2 at high N levels is surprising. May it be a result of the excess-C sapwood allocation approach implemented in this model? May warrant a brief discussion of this aspect.

For competition runs, it's because of the competition (competitive φ).

*l. **386-388: In my reading, this is a main result and should be shown in** a separate figure, shown at the very beginning of the results section.*

We will move this paragraph in 390~406 to the beginning of the results section and the figure S1 to the main text as Figure 2.

l. 418-419: See my comment 5.

We will rephrase this sentence.

l.425: See my comment 8. This is an interesting point, but is dealt with rather briefly here. How is "maximising growth rate" implemented exactly? NPP? NPP of a specific pool? "Allocating very little" is vague. The crucial aspect is that for a given N level and uptake half-saturation constant, the plants allocate much less to fine roots in the best- performing (by what measure?) fixed-allocation run than in the competitively optimal- allocation run.

We will rephrase this sentence and make it clear. The measure is the height growth in a competitive environment.

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l. 430-433: Sentence is hard to follow. Is the height at transition into the canopy (reaching critical height) increasing or decreasing with increasing N?

We will rephrase this sentence: "Changes in the height at which understory trees transition to the canopy from low nitrogen to high nitrogen indicate a shift from the importance of competition for soil nitrogen to the importance of competition for light as ecosystem nitrogen increases." to:

"Increases in the critical height, which is the height of the shortest tree in canopy layer, from low nitrogen to high nitrogen indicates a shift from the importance of competition for soil nitrogen to the importance of competition for light as ecosystem nitrogen increases."

*l. 493 ("succession"): Discussing competitively optimal strategy shifts during succession confused me here. I understood, that the competitively optimal strategy is determined for a *steady-state*, and (based on my understanding from reading previous papers of this group of authors) an ESS is determined from competition upon invasion into a mono-specific stand. But now **I realise that the algorithm for determining competitive optimality has never been described in the present paper. A gap that should be filled (see also comments above).***

We will add a brief description of "competitively optimal strategy" in model description.

l. 569: I had a great laugh when I read the short description of that repository on github ("BiomeESS: for simulating multiple plant forms, on-going, unpublished, with ridiculous processes and many bugs.") Maybe the author wants to change that upon publication of this manuscript (and if necessary resolve some known bugs). If not, I appreciate the honesty.

Thanks for taking a look! We have updated the codes and will update the description upon accept of this paper.