

Interactive comment on
“Competitionalterspredicted forest carbon cycle responses to nitrogen availability and elevated CO₂: simulationsusing an explicitly competitive, game-theoreticvegetation demographic mode” by Ensheng Weng et al.

Anonymous Referee #2

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This study presents a modeling comparison where a single model was altered with fixed allocation and competition-driven allocation scheme along a nitrogen availability gradient and under ambient and elevated CO₂. The competition scheme that the author considered are nutrient availability and light availability. The authors found that competition-driven allocation scheme predicted different fractional allocation to fine-root and wood as compared to fixed-allocation scheme. While the results are generally supported by their study, I do have several issues that I would like to bring to author's

C1

attention.

Major comments:

It appears that the allocation assumptions made in L 254 – 256 are key to their predicted results. In particular, it appears to me that the exact order of step 1 and step 2 may have a profound effect on the competition dynamics. I wonder what will happen if plant prioritize NSC allocation to leaf and root first, and chuck the remaining C to wood next? In the current text, I think the author did not provide sufficient discussion or justification to these potentially fundamental assumptions. Moreover, what happen if the extra C under step 4 is respired rather than allocated to wood? This could potentially match with some existing model treatment with the extra C, which deserves some discussion.

Furthermore, while the results indicate a reversed fractional allocation pattern to fine-root and wood under competition-driven allocation scheme, there is no “data” to actually prove that this new allocation scheme is an improvement to the fixed allocation scheme. Many models already consider “dynamic” allocation based on nutrient availability and water, but the author did not make any comparison against those model behaviors. I'd suggest the authors at least to bridge their modeling results with some observations to make a more convincing argument that their scheme has some advantages.

Moreover, the author highlighted that competition for light and nutrient drives successional dynamics (e.g. L 46, 83, 105-107, etc.), which left me with the impression that successional dynamics is a key component of the paper. But in fact, it surprises me that the authors only included results on successional dynamics in the supplementary materials, and there's little discussion around this topic. I'd suggest tightening up Figure S4 and S5, and move them into the main text, with more thorough discussions around them.

Minor comments:

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L 24-26: question: does this mean fixed allocation performs similarly in predicted NPP when compared to those based on competitive-allocation strategy? So the change in allocation pattern does not result in any changes in predicted NPP?

L35-36. It's a bit unclear what the author trying to illustrate here.

L38: "ecosystem-level predictions" of what? You indicated earlier that the predicted NPP was similar, right?

L230. The symbol φ_{RL} was described here, which appears to be too late. Suggest to define it in its first appearance.

Figure 2. The author showed how competition runs compared differently to the fixed allocation runs, based on $\varphi_{RL} = 4$. Since you are talking about succession and competition, it remains unclear what is the community response?

Figure 3. Missing unit on y-axis.

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