Responses to M. Smith's comments on Weng et al. manuscript (LM3-PPA model)

Thanks for the comments and suggestions provided by M. Smith, which were extremely helpful in guiding us to a revised paper that more clearly communicates the scope of the work and its importance. Our responses are below (*Reviewer comments are in italics*, and our responses are in roman):

M. Smith (Referee)
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I congratulate the authors on a thorough piece of work that must have taken a lot of effort. In their study the Authors assess and provide evidence that they have successfully coupled their DGVM to an Earth System Model. On the basis of the evidence it appears as though they have been successful, although that evidence is presented for only one type of vegetation.

The introduction lays the foundations well and the program of work undertaken is important to establishing whether the LM3-PPA coupling works – however I was left feeling disappointed as I read the results and discussion that the results being presented were so limited, in terms of rigor of assessment for deciduous Northeastern US forests and in terms of vegetation globally. While "The model is formulated to be the land surface component of an Earth System Model" I would expect that the present analysis falls far short of establishing the LM3-PPA as being adequate for that purpose. Though the authors are very open in terms of the scope of the present analysis. Overall I recommend that the paper is published with minor revisions because it requires a different study to establish global performance.

Larger recommendations:

a) On the subject of using the PPA globally – my understanding is that PPA works fine when you have closed canopy forests, but that precisely what to do when that is not the case, and how it works when LAI<1, is not well established. It would be disingenuous to imply that extending the PPA to work globally would be straightforward when its not clear how it should be applied in nonclosed canopy forests. That said, if it genuinely would be straight forward then I have no problem with this point not being mentioned.

We have added a new Discussion section (4.6 Future challenges) to address this concern. To summarize: (1) we are working on extending the PPA to non-tree vegetation types; and (2) the main challenge to deploying LM3-PPA at the global scale involves the parameterization of a suitable global plant functional diversity scheme, rather than the need to incorporate new processes into the model. These points are explained in the new Section 4.6.

Also, by "LAI<1", we presume that the reviewer means that the canopy is not closed because the sum of the area of all tree crowns is less than the land area (note that the PPA algorithm applies to the horizontal arrangement of crown area, not leaf area). Such cases are well understood. They occur at equilibrium of the model presented in the paper when water becomes sufficiently limiting. The relevant mathematical results can be found in Farrior et al. (2013), which is cited in the paper.

b) Methods – it is unknown to me why you restrict your analysis to a maximum of 3 tree species. I understand your experiments to establish competitive dominance and evolutionary optima etc.. but is a maximum of 3 species how you'd propose to model stand dynamics for northeastern deciduous forests? If not then you're not even showing how you'd model these forests in an ESM.

The three-species simulations are not intended as a functional diversity scheme for ESMs, but rather to evaluate certain aspects of model performance at a single site (Willow Creek). Even at this site, we make no assumptions about the adequacy of the three-species approach, but rather present it as a contrast to the more typical ESM approach that lacks any successional tree diversity. In our view, the competition experiments with multiple allocational types (as opposed to species-level approaches) that are presented later in the paper are more relevant to ESM applications. We have re-written the first paragraph of Section 2.2.1 to clarify all of these points.

c) Results: It is unclear to me what, of what you have found out, is novel compared to the previous work. I have seen PPA outputs for different forests throughout the US and the core formulation has been established for some time. So what new does this paper bring to the table? What specific things did you learn about how to model stand dynamics when moving form a presumably uncoupled PPA to an ESM coupled PPA? If it is to just report that you have successfully coupled it to an earth system model then that is not a scientific paper – it's a technical report (and maybe should be published in a different journal). Now, of course, you do show this coupled model working, to the extent you declare at the end of your introduction – and I agree that should be published on that basis – but there is a big difference between the grandeur of what is raised in the introduction and what is delivered in the results.

We have added a new "Overview" section (4.1 in the revised MS) at the beginning of Discussion to clarify the novel aspects of our work. We agree that although the technical aspects of our work are formidable, this alone would not warrant publication in a top scientific journal. The novel scientific aspects of our paper largely concern the novel predictions about how competition may affect plant carbon allocation to wood (a long-lived C pool) vs. fine roots (a short-lived C pool), and showing how these predictions can be understood in the context of a mathematically tractable version of the PPA model. To our knowledge, the linkage we present between an ESM land-model component and a mathematically tractable forest dynamics model (that includes a representation of height-structured competition for light) is the first of its kind, and has important implications for the global modeling community.

Smaller corrections

1) P17759, L20: It's not clear to me what you mean by "Empirical rules". Empirical relationships? It's the "rules" part that causes me to wonder.

"Empirical rules" was meant to indicate that allocation and PFT distributions in most DGVMs are determined by a set of "top down" rules that prescribe model outcomes so as to match (at least crudely) empirical patterns. We changed this to the following (Lines 74~75 of revised MS): "Model-specific rules (often empirically derived) are used to allocate C to the different pools, …" We hope this is more clear.

2) P17760 L12: Add citation

Added (Friedlingstein et al., 2006)

3) P17761 L9: "around half" -> "equivalent to around half"

Done.

4) P17765 L9: SD undefined

Here and elsewhere, we changed "SD" to "standard deviation". (Our original MS had "standard deviation", but this was changed to "SD" by the journal when the formatted online version was produced.)

5) P17770 L9: I think 1-neta would read better if it had an additional set of parentheses around it e.g. ([1-neta] times: : :

Done.

6) P17771 L2: matters->matter

Done.

7) P17782 L7: large-> larger

Done.

8) P17783 L15: space needed

Done.

9) P17786-17787: The statements at the start of this discussion after the numbered list are not discussion – they are a statement of facts from the past and assertions. They should really be in the introduction with citations, although I suspect most of this text is not necessary at all.

We deleted this paragraph, and we also moved the numbered list to the Introduction to make the Discussion more focused on discussing the results and future directions.

10) Discussion. I encourage you to focus on discussing the findings and insights that are genuinely new. It's not a surprise, for example, that root and leaf carbon equilibrate quickly compared to wood – I don't think that should even be discussed – it is of no use to the reader beyond what is already published elsewhere. If you focus on discussing the genuinely novel findings then it'll be of more use to the reader.

As noted above, we inserted a new "Overview" section at the beginning of Discussion to clarify what aspects of the paper are novel, and we removed some extraneous text from Discussion.

11) P17789 L8: I don't think it's true that "Trees in the baseline LM3–PPA model (version H0 in Table 2) currently do not senesce" – it's just that they don't senesce quickly enough when they're getting really old.

By "do not senesce", we meant that mortality rates remain constant (rather than increase) with age. To avoid confusion, we have removed the word "senesce", and the text now reads (Lines 853~854 in the revised MS): "In the baseline LM3-PPA model (H0 in Table 2), canopy tree mortality rates are constant and independent of tree size and age, …"

12) P17798: "When consider leaf only" – grammar

We decided this phrase was unnecessary, so we deleted it. The sentence (which occurs just before Eq A2.7 in Appendix A) now begins "The dynamics of leaf biomass…"

13) Table C1 and C2 legend – needs to be improved.

We updated the legends of N, L, FR, SW, HW, L^*, FR^*, SW^*, Λ, τ_C, Ps, μ, f_{WF}, f_{LFR}, GDD_{crit} and T_{crit} in Table C1, q_c, $[O_2]$, α_{LUE}, P_{ref}, and SRA in Table C2, Λ, l^*, q, μ_{C0} and μ_{U0} in Table C3, making them consistent with those in Table C1. We also deleted some unnecessary symbols related to Soil Water Budget from Table 2.