**Responses to B. Poulter' comments on Weng et al. manuscript (LM3-PPA model)**

Thanks for the comments and suggestions provided by B. Poulter, 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):

**B. Poulter (Referee)**
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*The manuscript by Weng et al. describes the implementation of a size-structured approach to model forest dynamics in the LM3 DGVM by using the perfect plasticity assumption (PPA). The authors argue that the approach provides a mathematically tractable solution for modeling vegetation dynamics, i.e., where stochastic simulations and/or non-linear dynamics can be solved deterministically. The model is applied to a temperate broadleaf forest in northeastern USA to demonstrate how the approach can be used to analyze optimal allocation strategies and their evolution under elevated CO2 concentrations, which lead to competitive exclusion.*

*The technical accomplishment presented here, where PPA is implemented in LM3, represents a very important scientific advance in the field of dynamic global vegetation modeling. The paper articulates this point very clearly and provides sufficient information to follow most of the work in some detail. The challenge of efficiently representing light competition is very elegantly solved by PPA and the coupling of PPA to LM3 opens up many opportunities for studying forest dynamics.*

*My main concern is that the authors make the point that this development is for Earth System Modeling (ESM) and that the approach now makes ESM solutions tractable at the global scale. However, the example simulation is made for just one location and for just a handful of the tree species that co-exist at that location. The tractability of the modeling approach thus seems limited for grasslands, savannas, systems experiencing gap dynamics, and this is not fully covered in the paper. Beyond the technical presentation of the paper, which is superb, it would be more convincing to see a global simulation performed.*

As explained in the cover letter, we would argue that the scientific scope of the paper is quite broad, even if the geographic of the model evaluation is limited. We have added an Overview section at the beginning of Discussion (Section 4.1 in the revised MS) that highlights the novel aspects of our analysis. For example, the last sentence of the new Section 4.1 states: "Our paper

is novel because we present novel land-model predictions of how resource competition affects allocation to wood (a long-lived C pool) vs. fine roots (a short-lived C pool) at different CO2 levels, and because we show how these land-model predictions can be understood in the context of analytical predictions derived from a mathematically tractable version of the PPA model." The novel features of the LM3-PPA model are also highlighted on Lines 183~195 of Introduction, and the novel results are explored in detail in Discussion Section 4.5.

In addition to clarifying what is scientifically novel about the work presented, we also now include a new Discussion section (4.6) that addresses the challenges of extending our formalism to the entire globe. This section also states that the formalism does indeed extend to grasslands, savannas, and systems experiencing gap dynamics. The PPA was developed for forests as a way to deal with gap dynamics. Strigul et al. (2008) show many examples in which the PPA predicts stochastic gap simulator results. Farrior et al. (2013) and Dybzinski et al. (2011) derive conditions for systems with open canopies, and systems in which competition for water obviates competition for light. Finally, as mentioned in Section 4.6, we have developed parameterizations for shrubs, forbs and grasses.

*Please find my more detailed comments below:*

*1. The compute times for the single grid cell, three species simulation for one year would be appreciated. Obviously a key limitation in DGVM development has been computational constraints – how fast does LM3-PPA run?*
A 500-year run for three species on a single processor (~3GHz) takes about 6 hours.

*2. P17762, line 5: spell out the LM3 acronym*
Done as suggested: inserted "land model version 3"

*3. P17762, line 15: the idealized conditions are mentioned here and then once in the Discussion. Some more detail on what the idealized conditions refers to would be very helpful.*
The "idealized conditions" mean constant climate (i.e. every day the same). We have now clarified this on Lines 149 and 179~180.

*4. P17765, line 9: need to define SD*
Done. We have changed it to standard deviation.

*5. P17773, 2.1.5: currently, the model has mortality from the non-structural carbohydrate pool being in deficit, and the authors point out that disturbance (insect, windstorms, etc..) are not included. Please can you define then what sort of mortality 'background mortality' refers to?*
The version of the model implemented in this paper divides mortality into two sources: carbon starvation mortality and background mortality. Mortality due to carbon starvation happens if NSC pools cannot meet maintenance demand. This can occur noncompetitively (say due to

drought), but it is most likely to occur in heavily shaded forest understory, where "self-thinning" is well documented. In contrast, background mortality includes all non-competitive sources that create single-tree gaps, including windthrow and insect attack. These sources of mortality are described in the rewritten section on mortality, beginning on Line 415. Although the simulations in this paper do not include stand-level disturbances, a new Discussion section (4.6) and Section 5 in Appendix A explain that LM3 already included the ability to simulate stand-level disturbance, which LM3-PPA inherits. This capability, and that fact that it was not implemented in the current work, is also highlighted in several additional parts of the revised MS: 435-437., and Lines 441-444.

*6. P17774, line 11: what does 'sufficiently similar' refer to? Please define this in a quantitative way, presumably as implemented in the model.*
This whole section is moved to Section 5 of Appendix A since the tile dynamics is not used in this study. The details of tile dynamics can be found in Shevliakova et al., 2009, which is cited in the main text (Line 441) and appendix A.

*7. P17812, line 16: Medvigy reference is missing in References section*
Added and we also corrected the citation to this paper in the main text.

*8. P17788, line 9: If I understand correctly, the macroscopic equations are coming from ED? Throughout the paper, the approximation offered by ED (used by LM3) and the tractable solution to size structured population modeling (PPA) gets confusing. At this point, it would be helpful to distinguish the two.*
The macroscopic equations are from Strigul et al. (2008) for deriving the analytical solution of the spatial stochastic processes that are simulated by individual-based forest simulators (e.g., SORTIE). We added a citation to Strigul et al. (2008) after this sentence to indicate the origination of the macroscopic equations.

The ED model represents another way to produce macroscopic equations from an individual-based forest simulator, but one that does not produce mathematically tractable equations. The main text previously contained a description about how the tiling scheme in LM3 could be used to include the fundamental approximation in the ED model (in the old section entitled: **Land use change and ED gap approximation**). Several reviewers objected to this section, or seemed to be confused by it, so we removed it from the main text.

The relationship between ED and LM3 was part of an earlier version of the paper, but we removed it before submission because the issue is complex, takes a lot of space, and is peripheral to what is new in this paper. But, just for your interest, we originally developed LM3 from the ED code by *removing* ED's fundamental approximation, because we were concerned that we would never get a global version running when we could not interrogate the macroscopic equations analytically. This is why we eventually developed a tractable approximation and are now extending it to the globe as LM3-PPA.

*9. Equation B17: should variabl qc be qa, there seems to be an error here.*
Corrected. Thanks.