Responses to T. Kohyama's comments on Weng et al. manuscript (LM3-PPA model)

Thanks for the comments and suggestions provided by T. Kohyama, 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):

T. Kohyama (Referee)
kohyama@ees.hokudai.ac.jp

*This article presents full description of the new LM3-PPA model that integrates tree population processes in height-structured forest (PPA), physiological and allocation properties of trees, and biogeochemical processes of forest ecosystems. The key assumption of PPA is that the condition of foliage crown position of a tree in vertically structured forest patch is determined by its relative height in dynamic k layers divided by threshold heights (Z*_k), and each layer (from the top layer-1 downwards) is determined by the sum of crown areas from the highest tree of the layer to cover the entire area of forest patch. Under conditions that a forest has only one closed layer of top canopy (k < 2 with Z*_1 height) and that physiological properties are in two states of either sun-exposed or shaded, previous PPA studies provide the analytical solution of tree height distribution of a single-species (or single-functional-type) population. As authors suggest and show insightful examples with simulation experiments, such simplicity of PPA is advantageous in functional understanding of ecosystem properties. I congratulate authors' success in coupling PPA and biogeochemical ecosystem model.*

As is also pointed out by other reviewers, to show generality of PPA framework in global scale earth system modeling, it is better address the application to non-forest vegetation types. PPA of land-surface filling can also be applicable to short vegetation types of shrubland and grassland, as far as crown area is related to other functional dimensions of plants (either genets or ramets), such as crown-LAI, and allocation allometries. We therefore anticipate the extension of LM3-PPA with general parameterization for earth system modeling.

We agree with the reviewer on these points, and we are currently working to apply the PPA to non-tree vegetation as the reviewer suggests. We now address these points in a new section of Discussion: Section 4.6 Future challenges.

Meantime, there are a couple of limitations of PPA framework. For example, PPA does not describe spatial heterogeneity in gap dynamics as is briefly stated in Discussion. Authors suggest using patches (tiles) in varied states since disturbance (as well as invaried states of land use). Authors may rather explicitly suggest the alternative way of coupling patch-age approximation with PPA for dynamic landscape modeling.

We agree with the reviewer that the lack of horizontal heterogeneity is a limitation of the PPA model, including applications with old-growth forests undergoing gap-phase dynamics. We also agree that the ED patch-age approximation (which is already available in the LM3-PPA code, but was not implemented in the current study) could address this limitation. These points were mentioned briefly in the original MS, and we have added new text for further emphasis and clarification. In the revised MS, these points are discussed on Lines 438-444, 841-850, 940-944, and Section 5 of Appendix A.

Authors suggest k < 2 for most boreal and temperate forests, such as their example of temperate forests in north America, while maximum k can be 3 in tropical forests (Bohlman and Pacala). It is indeed an exciting question what determines k in forest ecology. Besides, at the scale of forest ecosystems, I wonder k in temperate forests usually have k < 2. In case of east Asian temperate forests, dwarf bamboo layer often entirely cover the understory (k > 2), and bamboos contribute to considerable proportion of biogeochemical processes. Another exception is non-continuous upper canopy layer in such ecosystems as tropical rain forests with emergent trees and tree savanna. These are to be improved in the next step (e.g. including shrub parameterization in PPA, and incorporating patch-age distribution into PPA).

We have modified the language to acknowledge a greater diversity of temperate forest types: Lines 323-329 now read "In many temperate and boreal forests, the potential crowns of all individuals add up to less than two (do not fill a second canopy), and so Eq 6 has no solution for k>1. However, in some forests (e.g., tropical rainforests, and temperate forests with multiple understory layers), the sum of the crown areas of all individuals combined is typically 3 to 4 times the land area…"

Also, we agree with the reviewer that a patch-age approximation could accommodate heterogeneity due to emergent trees, and we now mention this on Lines 845-849.

A potential importance of dynamic vegetation models (DVMs) is to predict population transition with climate change under dispersal limitation and inhibition by resident vegetation (e.g. TeeMig by Lischke et al.). As authors deal with recruitment/invasion processes in simulation experiments, it is worth comparing LM3-PPA with alternative simple height-structured tree-based ecosystem models that focus on transition prediction (e.g. TreeM-LPJ by Scherstjanoi et al. 2014).

We agree that these are important issues that could affect the time-scale of ecosystem response to global change. Our experiments were not designed to address these issues, but we now discuss them in the second paragraph of the new Discussion section (4.6 Future challenges), where we cite the suggested paper by Lischke et al. The first sentence of this paragraph is "In addition to developing parameterizations for global-scale applications, another important area for future work is to better understand the transient dynamics of vegetation response to global change." The paragraph then discusses multiple processes, including dispersal limitation, that could affect the time-scale of ecosystem response to global change.

It is an important suggestion that the optimal tree strategy under light and water competition does not bring about ecosystem-level maximum productivity, which is usually ignored in earth system simulators. I wonder there also be a miss-match between longterm genetic change towards tree optimal strategy and short-term plastic responses to rapid climate change, which can also be addressed by the present model.

We agree that this is an important question, but not one that our experiments were designed to address. We have now clarified this point on Lines 951-953, which reads: "…our competition experiments were designed only to identify the eventual outcome of competition under a given set of conditions, and are therefore agnostic about the rate and pathway of response." This sentence is in the second paragraph of the new Discussion section (4.6 Future challenges), where we discuss the issues raised in the last two reviewer comments: dispersal limitation, plastic vs. competitive responses, etc.