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Interactive comment on "Scaling from individuals to ecosystems in an Earth System Model using a mathematically tractable model of height-structured competition for light" *by* E. S. Weng et al.

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This paper is quite clearly an exceptional study, summarizing a huge quantity of model development effort. I suspect it will become an important part of the dynamic vegetation modeling literature going forwards. The authors report the initial single-point testing of the coupling of the PPA model within the GFDL Earth system model, specifically including the fine-scale and biogeochemical processes from LM3 into the 'mathematically tractable' Perfect Plasticity Approximation model, the properties of which, regarding the elucidation of competitive and evolutionarily stable strategies, have previously

been discussed by Farrior et al. (2013, in review).

Here, Weng at al. show for a site in the Eastern United states, that the model is capable of simulating carbon uptake, biogeochemical processes and succession all with some skill. They also describe a set of experiments that illustrate that the model can distinguish, by virtue of its competitive interactions driven by campy light interception, that it predicts different outcomes for polyculture/competition than for the species that maximizes productivity in isolation. This is an important result, as it further demonstrates, I think, that the use of 'many monocultures' in traditional DGVMs does not necessarily produce the correct competitive outcome.

The paper is extremely well written, and the explanations of emergent ecological phenomena are some of the best that I have come across, despite the often complex nature of the subject matter. I have few if any major objections to this paper and think it could be published in its present form. My main points are mostly suggestions that might increase the clarity of the text.

I do not agree with one other referee that the introduction is overly grandiose, since it is made quite clear at the end that this paper does not pertain to global simulations and is in fact a first step towards them. I do agree somewhat that the use of 'Earth System Model' in the title rather implies that the ESM capabilities have somehow been deployed in this paper. Clearly, implementation of the model within the code of the ESM is a hugely challenging task, and should be acknowledged somehow. Maybe "A mathematically tractable method for scaling individual trees to ecosystems within Earth System Models" would work? Or something along those lines.

Generally, given that the PPA uses a more empirical information in the parameterization of, for example, its canopy and understorey mortality rates, than a standard BGC model might, I was often left wondering which elements of the model testing process were genuine tests of the emergent properties of the model, and which derived from the model being 'told' the answer in advance. I think that a greater transparency (or maybe, just illustration) of this to the reader wold be helpful, since I do not think that the specific outcomes of the simulations for the Eastern US are the main interesting feature of this paper, given that all such models are prone to extreme parametric uncertainty, which would most likely comprise the main difficulty in scaling up to larger regions or the entire globe.

Specific Comments and Questions 17770 L3: While it is clear that the flat top assumption is very beneficial from a simplifying perspective and therefore appears justifiable, it is of course not physically realistic. What do you lose, in theory, by adopting it? Are there any downsides that might be considered, with regard to simulating co-existence, etc., that might cause this assumption to be revisited in future versions?

17772 L17: Does the NSC target scale with biomass, leaf area, etc?

17773 L7: Does mortality of the entire cohort occur immediately if the average NSC for that cohort drops below zero, and if so does that not cause unrealistically 'thresholdy' behavior in the model? Perhaps the cohorts are large enough in number that killing a whole cohort does not produce a large deviation to the model state? Otherwise, I would imagine that some kind of continuous function relating NSC to mortality might be appropriate.

17773 L22: Does the PPA apply to all the vegetated areas, including cropland and pasture? It isn't clear here.

17773 L10: How is mortality a function of size in the understorey? I think this needs fleshing out or deferring to the appendix.

17766 L30: This paper does a very good job of carefully explaining the theory behind the PPA, but I think that, as written, this particular section risks alienating large fractions of its potential readership, and will decrease its impact accordingly. The ED model code -actually- discretizes the cohort and patch properties, and thus is realized as a relatively simple set of differential equations determining the growth of different biomass pools

and mortality of the different cohorts, etc. It is therefore much easier to understand than the more abstract descriptions of this theory that exist in the literature.

This is something of a style point, but I find that the use of this type of language in model description papers, borrowed from more physical disciplines, does more harm than anything else in making demographic model theory less accessible to those who might wish to understand it. This barrier of understanding is a genuine problem, and is most acute when trying to communicate how models abstract the real world to scientists from fields that do not make use of such principles and to land surface scientists who do not habitually conceptualize ecological processes in this way. Is it possible to rephrase this section to reduce the likelihood of losing the audience at this rather critical point?

17774 L8: Given that this paper doesn't pertain to managed forests, this seems like a little too much detail, and also raises the question of why age-since-disturbance dynamics are operational for managed forests and not for the natural vegetation (my interpretation of how this operates at present). I think discussing the managed forest component is confusing if it isn't presented at all in the simulations.

17775 L15: In this discussion of ED, I think it would be good to state more clearly what is happening in the PPA at present - i.e. 'we assume that there are no stand replacing disturbances', or that 'we do not run the model to old-growth equilibrium, on account of the absence of disturbance dynamics'. Otherwise it isn't clear what once might stand to gain from adding in ED-like dynamics. Also, why does this section only seem to propose using gap-phase disturbance dynamics for secondary vegetation and not for primary vegetation?

17775 L18: Why choose these N American species in particular? What is the general purpose of this illustration? 17777 L20: It isn't clear to me here where the extra carbon goes, if it is not allocated to dbh increment, does it go into roots, or storage, etc?

17778 L12: I think you should state the purpose of this comparison with the BGC models here? Is the intention to have the BGC processes mirrored exactly, or to more generally test the implications of modeling succession on carbon accumulation?

17781 L24: Is the understorey aspen mortality parameterized, or emergent from the properties of the carbon starvation model?

17792 L20: What is the difference implied here between computational and mathematical tractability? If alternatives are computationally intractable, that implies that they are impossible to compute with given computational resources, which would be hard to demonstrate. Maybe computational efficiency rather than absolute tractability per-se, might be a better term to use?

Interactive comment on Biogeosciences Discuss., 11, 17757, 2014.