

Interactive comment on “A stand-alone tree demography and landscape structure module for Earth system models: integration with global forest data” by V. Haverd et al.

Anonymous Referee #1

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Review of Haverd et al

This paper presents a calibration exercise for a potential alternative approach to woody biomass simulation in vegetation models, called 'POP' (Populations-Order-Physiology), which is a methodology for separating the woody biomass dynamics from ecosystem physiological calculations in a land surface scheme with minimal computational load. This paper follows on from an earlier presentation of this idea, and extends it to include an empirical representation of canopy self-thinning, and a calibration/validation method against forest inventory data and allometry database.

This (POP) remains an interesting idea, but I have the impression that the authors are

C285

over-selling its utility as a full alternative to the dynamic vegetation models that they introduce at the beginning. It seems that the approach as proposed will only work under static disturbance regimes where the rate of return of catastrophic events is constant, and therefore the equilibrium landscape distribution can be estimated trivially. There is no mention of how carbon resources might be partitioned among different plant functional types, nor how competing plant functional types might obtain more or fewer resources in different light regimes represented by the model. Only one number (biomass increment or biomass turnover) is passed between the CABLE and POP models. How would the structure deal with multiple plant types contributing to both of these pieces of information? Maybe these are implicit, or the model is not supposed to predict these properties, but either way, the approach is introduced and its use is promoted without any discussion of what the potential caveats or limitations might be, compared to the models they are supposed to replace. Maybe the authors intend to develop these capacities later, but it still needs to be mentioned. This opaque discussion makes this paper much less interesting to me, as the approach appears promising and parsimonious, in a field where such innovations are clearly required. I hope the authors can modify the paper so that the pros and cons of their method are clearer to other researchers who might like to adopt a similar method in their own studies.

Specific Comments

2345:Line 16: 'as' not 'ass' Line 2346: Line 16 - The ED model, as I understand, does not have a stochastic component. 2348: Line 15 - You haven't defined here what is meant by a 'patch'. Given the complicated and inconsistent use of this term in vegetation model literature, this is extremely important. 2348: Line 18 - why mention the second class of disturbance here if it is not used at all? Is something an 'input variable' if it is a constant parameter? 2348: Line 21 - Do you mean age or size? They are not the same thing, as it is possible to have old, small individuals with suppressed growth, etc. Can you make this clearer? 2348: Line 22 - Are the 'neighborhoods' spatially explicit of statistical concepts? 2348: Line 25 - Is this the total biomass in-

C286

crement of the whole grid cell? What about variation between plant types? How is that accounted for here? 2349: Line 4 - This method for dividing up the NPP between cohorts is so central to the argument that I think it should be in the main section of the paper. . . For example, it isn't clear to me at this point how the model deals with cohorts of the same size that might be shaded in some late successional patches and fully lit in early successional patches. 2349: Line 14 - If the disturbance is episodic, and the patch is reset completely by it, then how can this not invoke some kind of stochastic behavior? Is 'episodic' the right word to use here? 2349: Line 16 - Where you say "this' threshold' I'm not sure what 'this' refers to in the context of the sentence or the following equation. 2350: Lines 1-5: The parameterization of the first two terms in this growth efficiency based model need more detailed justification. Models of mortality are notoriously poorly parameterized, and so a description of why these numbers (0.75 and 0.3) are used is needed. What data or methods were used to justify them originally? It is OK if this is a difficult subject, and a discussion of the provenance of the model would make this more interesting. 2350: Line 19: $A_{c,y}$ is defined in the text, but doesn't seem to be actually used in the equation? 2351: Line 4 - I think there is some punctuation missing here. 2351: Line 12 - Does this assumption still hold if the disturbance interval is not static in time? (if not, this caveat need to be mentioned here, because it is likely that fires, pest and wind throw will all change in a non-static climate) 2352: Line 3 - It is assumed, then, that all of the patches are biogeochemically equivalent, and that the lag in recovery of all the other processes (LAI, in particular) is negligible? 2352: Line 15 - is each grid cell just one plant functional type? 2352: Line 21 - cold deciduous, presumably? Also, why is phenological habit a relevant input if LAI is specified by MODIS? 2352: Line 28 - Why is the model set up like this - (driven by LAI, only for some grid cells, no vegetation dynamic predictions etc.) I guess it is to compare against the biomass data with as few degrees of freedom as possible, but some sort of justification statement would be useful here (of what you are and are not testing). It is quite strange for a paper whose introduction is about DGVMs to specify both vegetation cover and disturbance rates as static, so at this point in the paper I am

C287

a little confused about the direction it is taking. 2355: Line 9 - The model self-thinning algorithm is calibrated against all of the forest data. Given that self thinning is driven by growth rates, ultimately, and that these will likely change through time, is this empirical fitting process applicable to future simulations? 2356: Line 12 - On the previous page, you describe how the parameters controlling these observations are fitted to the data, so which parts of the model-data comparison illustrate the the model structure is performing adequately, and which illustrate that it has been tuned to the data against which it is being tested? 2357: Line 15- This section (4.1) seems more like results than discussion to me. 2539: Line 8 - I do not yet have a feeling, so far in this manuscript, for why it is important to specifically simulate the size distribution of trees in the forest, and how, for example, altering this property might change the overall response to forcing variables, in this framework (given they all have the same physiology anyway). I can imagine many possible reasons, but I think the specific motivating factors need to be spelled out here. 2360: Line 2 - You can use inventory data in outer ways - e.g. to determine the gross turnover and recruitment rates, not to mention total biomass.

Figure 6: The text here is very small, and the labels (i,ii, etc.) appear to be missing.

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