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Geoscientific
Model Development
Discussions

Interactive comment on "Seasonal leaf dynamics for tropical evergreen forests in a process based global ecosystem model" *by* M. De Weirdt et al.

Anonymous Referee #1

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This paper presents a series of modifications to the ORCHIDEE land surface model, aimed at improving predictions of seasonality of photosynthesis and litterfall simulations, tested primarily at t humid tropical forest field site in French Guiana. The major modification is to the algorithm predicting canopy leaf loss, in which leaf turnover becomes equal to net primary productivity on any given day. The justification for this scheme is that newer leaves are produced when photosynthetic productivity is high. These leaves, being younger (in this model) have a higher photosynthetic capacity, and thus the total canopy photosynthetic capacity tracks, in this case, varying light levels. Old leaves are lost, ostensibly, to prevent self-shading. The other alterations for the model concern the allocation fraction to leaves, and root respiration rates, which are adjusted to match observations from previous analyses of tropical forest ecosystems.

The new model produces notably improved simulations of fluxtower measured GPP, with much higher peaks in the dry season, and has a significantly improved seasonal cycle of litterfall. The former is at least partly due to an increase in the leaf and canopy level Vcmax, which is because in the new model, a significantly shorter leaf lifespan results from the high leaf turnover. Therefore, a larger fraction of leaves possess the high Vcmax values attributed to leaves in the youngest age class.

The increase in top-of-canopy Vcmax, from 45 to $\sim$59 (with seasonal variation) does appear to be at the very high end of the range of values reported for tropical broadleaf trees by Kattge et al. (GCB, 2009), who report mean values of 41 and 29 mol m-2 s-1 for non oxisol and oxisol forests respectively. These are leaf- and not canopy- level estimates, as is assumed on page 656.

This paper is interesting and well written, and the model improvements do appear to notably increase the fit to observations. My main issue with this method is one that is partly recognized in the conclusions. Namely, the approach appears to assume a constant LAI, as for all biomass allocated to leaves, an exactly equivalent amount is lost at the same time. It is therefore puzzling to me how this model can be spun up in a way that predicts leaf area index? Maybe the idea is that it is driven with observed LAI? It is also unclear how the LAI of 6.0 used in this study was obtained - from the model or from site level observations?

My other comment is on the assumptions of the optimality approach. The authors assert that LAI is consistent between seasons at these sites but do not provide data to demonstrate this. If we are to assume that forests are light-limited at these two sites, and that light availability varies between seasons, is another potentially optimal strategy to make use of the extra light by growing a thicker canopy during the dry season, thus allowing variation in LAI? Part of the confusion here stems from the fact that the precise goals of the optimal theory used here are never defined, so it is difficult to determine whether the plants are infact generating a solution which optimal with reference to some hypothesized goal, as the authors assert.

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Specific Comments

P640 L2: . . . representation in global models _is_ highly simplified.

P640 L14: . . . patterns are analyzed in _detail_ (not details)

P642 L11: It would be easier to assess whether or not evidence is overwhelming in favor of dry season litterfall peaks if the data in these tables were quantitative, or contained an estimate of the magnitude of the effect.

P643 L20: I think it would be interesting to show these data. It is difficult to determine the strength of the correlations from only the timeseries.

P643 L 25: This definition of optimality is too vague. What available resources are considered? How is the 'chance of survival' quantified here? It is normal when invoking an optimality concept to state precisely what exactly is being optimized, ideally in a quantitative/equation form.

P644 L3: An old leaf will only necessarily be lost if conditions are constant (see comment on LAI seasonality above), and also if that leaf is in fact shading other leaves. Are these leaves assumed to be distributed through the canopy, or could they all be at the bottom, and thus not shading any younger leaves?

P644 L8: Given that the optimality criteria are only vaguely defined, is it not clear to me how this approach produces 'constantly optimized' canopy.

P644 L8: As far as I know, there is no nutrient cycle in the ORCHIDEE model. How is the nutrient availability determined?

P644 L27: It would be good to have separate simulations that demonstrate the different impacts of the altered litter model and the adjusted allocation and respiration parameters.

P646 L5: Krinner et al is the original ORCHIDEE paper. From what data they establish 65 as an appropriate Vcmax for tropical trees? Also, it's not clear that it is neces-

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sarily optimal here - maybe 'maximum' would be more appropriate? P646 L10: Why do Johnson and Thornley use this approach? Is it based on photosynthetic capacity acclimating perfectly to light attenuation?

P647 L14: What are the implications here of fixing the leaf allocation and turning off the Friedlingstein 1999 allocation model? Surely this will affect the capacity of the model to simulate resource limited systems and make it less applicable elsewhere? On this note, it would be good to indicate whether the changes in this model are recommended changes to the whole ORCHIDEE model that will work in other ecosystems, or whether they help in understanding this system but are no generally applicable. My feeling is that the latter is more true, and in that case, it is difficult to see how this can be moved back into a more general land surface scheme. Are there any alterations to the methods that would make the model generalisable?

P650 L18: The name ORCHIDEE-NLT really does seem like it is concerned with needle-leaf trees. I would consider an alternative naming scheme.

P653 L8: The word 'evaluated' here seems to imply that they were tested against data, to me. Maybe 'examined' might be more appropriate?

P654 L24: The justification for the overestimation of litterfall at Guyaflux might well also apply at Tapajos, which would make matters worse?

P654 L26: Apologies if I've missed this, but I really don't understand how this model predicts LAI, given the litterfall calculations. This needs to be explained before this paper can be finalized, in my opinion.

P657 L25: It's nice to see that this model actually predicts the right-way round response to the dry season, given how very difficult this appears to be in land surface models in general...

Figure 3: Leaf litterfall data is not presented in this figure, in contradiction to the legend.

Figure 5: Are there no data on LAI against which this can be assessed?

Figure 8: The GPP at low irradiance for the original model (green) look very high. Is this the right output you have plotted?

Interactive comment on Geosci. Model Dev. Discuss., 5, 639, 2012.