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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 #2

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This paper modifies the leaf phenology and fine root respiration treatments in the ORCHIDEE model. The idea is that more realistic seasonally-varying leaf properties can "improve the correspondence of global vegetation model outputs with the wet-dry season biogeochemical patterns measured at flux tower sites." This type of research is valuable, and can be useful in obtaining insights into biogeophysical behavior.

Let's start with a statement from the abstract: "…recent flux tower and remote sensing studies suggest that seasonal phenology in tropical rainforests exerts a large influence over carbon and water fluxes…". The authors are exactly correct in this statement: canopy physiology needs to be addressed holistically. We would certainly expect that changes in model treatment of leaf biomass and carboxylation capacity would influence overall canopy conductance, and therefore influence simulation of energy partition (la-

tent and sensible heat flux). However, this one mention in the abstract is the only mention of latent heat anywhere in the paper (no mention of sensible heat anywhere). I know that LE and H observations are available from the K67 site, and I assume that they were recorded at the Guyaflux tower as well. These additional observational constraints are crucial to the analysis, and provide checks on overall model behavior and self-consistency.

I am also not sure that I accept that the modifications make the simulations more realistic. At the outset, the model has almost constant litterfall while LAI is variable during the year. After modifications, litterfall is variable and LAI is constant (Figures 4 and 5). Is this really an improvement? The authors use site-level data to justify modifications such as the dramatic lowering of LAI and raising of Vcmax in the simulations, but positive changes in ORCHIDEE performance when confronted with eddy covariance flux data (at both sites, for more variables than just GPP) would be much more compelling. I believe it may be possible to cherry-pick site-level observations to support the exact values for LAI and Vcmax; comparison to fluxes would be more robust. Again, the inclusion of energy flux in the analysis would be helpful.

I like the time-varying Vcmax formulation. I believe that this will be a feature of most, if not all, land surface models in the near future. I am interested in papers that address this component of canopy behavior. This brings up some interesting questions about leaf age, nitrogen content and allocation, and leaf-to-canopy scaling within models. It would have helped me, a member of the paper's target audience, to see more about these model components than just references to other papers. I don't suggest an exhaustive treatment, but a quick review and some discussion of how the model modifications will fit into the overall architecture would be helpful.

I would like some more explanation of the two litterfall models, particularly with respect to partitioning of leaf mass and ages. How does leaf biomass by age class change with the new formulation? Is it realistic? I am not sure I understand how the leaf biomass/age classes spin up.

Fine root maintenance respiration was chosen for modification, citing field observations from Malhi et al. (2009a). A little more explanation here is warranted, to give insight to readers who may not have read the Malhi paper. How is maintenance and growth respiration partitioned in the field? Are all the other respiration values realistic? More information about why this one model component was selected, over other similar model features, is needed. Also, why was only the base maintenance value (C0maint) modified, and not the slope of the linear relationship? Are there quantitative analyses that support this choice?

When fine root respiration is modified, what happens to pool size? For that matter, how does increasing litterfall by a factor of more than 2 (for parts of the year, anyway) change the overall carbon pool structure in ORCHIDEE? I assume that the surface pool is enlarged. Does this change the overall respiratory efflux in the model, or do the changes subside after pinup?

In section 2.2.2 a leaf carbon content of 73% was used to obtain a value for falloc,leaf, while in section 2.4 a carbon content of 50% was assumed. Is this difference due to comparing leaf carbon with overall litter carbon content? This needs clarification.

Comments on figures:

Figure 3: the caption lists litterfall as being shown; figure must be made consistent with caption.

Figure 4: Litterfall from guyaflux is shown, but not K67. From the text, I gathered that assumptions about leaf mass in litterfall were made at guyaflux, while the K67 obs were taken more frequently, and explicit determination of components was performed. Why weren't both sites shown, and if only one was shown why was it the one that had more uncertainty?

Figures 5 and 6: Were these plots identical for both sites? If so, then say so; if not, then more explanation is needed.

Figure 7: Where is K67? I know that these data are available.

Figure 8: are these data from guyaflux?

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