

Interactive comment on “Seasonal leaf dynamics for tropical evergreen forests in a process based global ecosystem model” by M.

De Weirdt et al.

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Anonymous Referee 2

Dear referee,

Thank you for your time for reading the paper and providing your positive inputs and comments, which help to improve this paper. We are pleased to send you our responses (C – Referee’s comment, R – Authors’ response)

C: 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.

R: We agree.

C: 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 (latent 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.

R: Yes, it is true that additional observational constraints add to the analysis. A graph showing comparison between model outputs and field observations of latent heat flux (Q_{LE}) at Tapajós km67 (2002-2004) and Guyaflux (2004-2009) can be added in the revised paper, showing the correspondence between model and flux data. ORCHIDEE modeled latent heat flux shows very good agreement with the flux data at Tapajós km67 and less good (small over estimations) agreement at Guyaflux. The correspondence between modeled and flux derived latent heat does not improve or change substantially with the introduced leaf litterfall modification.

C: I am also not sure that I accept that the modifications make the simulations more realistic.

R: The sentence in the abstract that includes ‘realistic’ was rephrased as follows: ‘[The results show that the introduced seasonal leaf litterfall corresponds well with the field inventory data](#)’

and times with its seasonality.’ In section 3.1 ‘realistic’ was replaced by ‘improved’. In the discussion section, ‘realistic’ is replaced by ‘spatial’.

C: 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 V_{cmax} 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 V_{cmax}; comparison to fluxes would be more robust. Again, the inclusion of energy flux in the analysis would be helpful.

R: We will add validations with several additional datasets to make our study more robust in the revised version of the paper. In the first place we can add a comparison with observed litterfall data for Tapajos K67 and Barro Colorado Island in Panama. Secondly, we can add a comparison with GPP and latent heat fluxes for both sites, as requested. In addition we added observed PAI data at Guyaflux in Figure 5. We are confident that these additional validations make our study more robust and comprehensive. It is true that additional observational constraints add to the analysis.

We refer to our response letter to Reviewer 1 regarding the assumption of a constant LAI. We agree that there is a large range in field measurements of LAI and V_{c,max}, and there are considerable differences in measurement approaches and different reports in literature. But LAI was compared to reports from Malhado et al. (2009) at 50 measurement plots at the K67 site and at Guyaflux PAI was measured. To our knowledge the only estimates based on field measurements in tropical evergreen forests of V_{c,max} are the ones reported by Kattge et al., 2009 and Domingues et al., 2005 (as in section 3.2 of manuscript and the specific comment 10 referee 1).

C: I like the time-varying V_{cmax} 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.

R: Thank you for this remark. Section 2.1 with modeling strategy was rewritten to explain more about the model components itself than referring to other papers:

‘The canopy leaf biomass is hence modelled to be at steady state with an ever optimal maximum. The introduced seasonal changes in leaf litterfall result in seasonal changes in leaf age in ORCHIDEE because leaf biomass is linked to a leaf age class bookkeeping model (see section 2.2.3) that keeps track of the leaf age structure and replaces older leaves by new young ones, created from NPleaf. Carbon is allocated to the youngest leaf age class first and then ORCHIDEE leaf age is updated daily through leaf biomass conversion from one leaf age class into the next one. The seasonal changes in leaf age result in seasonal changes in

photosynthetic capacity because the value of the latter is parameterized as a function of leaf age in ORCHIDEE as explained in section 2.2.1.'

C: 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.

R: To see if the leaf biomass change by leaf age is realistic, data are needed and we are not aware if such data exist. A graph showing leaf age distribution in each class can be added in the revised manuscript. In the spin up phase, the first leaf biomass is created from the first photosynthates, after subtracting the amount of carbon lost through respiration, the resulting NPP_{leaf} is allocated on a daily basis to the youngest biomass leaf age class (there are four leaf age classes modeled in ORCHIDEE). Leaf biomass is passed from younger to older leaf age class with a time constant (Eq 6: $\Delta B_{i \rightarrow i+1} = B_i \frac{\Delta t}{\tau}$). Leaf litterfall is lost from the oldest leaf age class and will only start after LAI of the canopy has reached a threshold value that was chosen at 6 based on field observations (see reply to other comments concerning LAI).

C: 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 ($C_{0,maint}$) modified, and not the slope of the linear relationship? Are there quantitative analyses that support this choice?

R: Investigating the NPP total outcome of the ORCHIDEE standard model output, we found a discrepancy for total NPP and looked into more detail into all the NPP components for which comprehensive analysis of field estimates were made by Malhi et al. 2009a. The most striking difference in order of magnitude was the fine root respiration. The aim of the modification was to model a more realistic order of magnitude for total NPP. As also reported in Malhi et al., 2009a, leaf and fine root respiration are the largest (and also most uncertain) components of tropical forest ecosystem respiration. Modeled leaf respiration order of magnitude was similar to field estimates (6.07 modeled versus 7.4 Mg.ha⁻¹.yr⁻¹ reported by Malhi et al.) while modeled fine root maintenance respiration was twice as high as field estimates of fine root respiration. The slope (0.12) of the linear relationship with temperature could have been adapted as well, but $C_{0,maint,r}$ was adapted as it is coded as a PFT dependent parameter value in ORCHIDEE. We had no access to fine root respiration data to fine tune this relationship. Nevertheless, studying this temperature relationship in detail is a good suggestion for future research.

C: 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) changes 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 spinup?

R: the changes in carbon pool structure at Guyaflux (mean over 2004-2009) due to the modifications made in ORCHIDEE-TrBE are summarized in the table below (in Mg C ha⁻¹):

	ORCHIDEE STAND		ORCHIDEE TrBE	
Leaves	4.54	± 0.033	3.91	± 0.00
Fine roots	2.2	± 0.57	24.26	± 7.29
Fruits	0.28	± 0.05	0.32	± 0.11
Coarse roots	47.64	± 0.17	18.01	± 0.17
Aboveground woody biomass	186.53	± 0.32	84.93	± 3.75
TOTAL	241.19	± 1.143	131.43	± 11.32

changes in maintenance, growth and heterotrophic respiration at Guyaflux are (in Mg C ha⁻¹ yr⁻¹):

	ORCHIDEE STAND		ORCHIDEE TrBE	
R _{maint}	12.41	± 0.95	8.25	± 0.51
R _{growth}	16.21	± 0.04	26.57	± 0.08
R _{heterotrof}	4.93	± 0.47	3.49	± 0.42
R _{total}	33.55	± 1.46	38.31	± 1.01

The fine root carbon pool increases due to the lowered maintenance respiration R_{maint}. The aboveground woody biomass decreases because the spill-over mechanism is no longer activated (as explained in the paper on p655, r 12). The fruit carbon pool does not change much and the coarse root carbon pool decreases. R_{maint} decreases because of lowered fine root R_{maint}. Total ecosystem respiration increases from 34 ± 1.46 to 38 ± 1.01 Mg C ha⁻¹ yr⁻¹ while flux derived ecosystem respiration over the same period (2004-2009) is 33.27 ± 6.63 Mg C ha⁻¹ yr⁻¹.

C: 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.

R: The value of 73% is for leaf carbon content in total litterfall (twigs, leaves, fruits), while in section 2.4a it is about carbon content in leaf dry matter, standard taken of 50%. The sentence was rewritten: .. assuming that the percentage of leaves is 73% (Malhi et al., 2011).. to be more clear.

Comments on figures:

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

R: Caption was adapted.

C: 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?

R: In the revised manuscript we add a comparison with litterfall data of two additional sites: K67 and Barro Colorado Island, Panama. At Tapajós the magnitude of the measured leaf turnover is higher than modeled leaf turnover (2.91 ± 0.42 model versus 4.31 ± 1.86 Mg C ha⁻¹ yr⁻¹ field data), but the new model improves the seasonality substantially compared to the standard constant leaf litterfall. Another comparison with leaf litterfall data at Panama show (3.29 model versus 4.27 Mg C ha⁻¹ yr⁻¹ field data) an improvement and good seasonal patterns in the output of ORCHIDEE-TrBE compared to the modeled leaf litterfall output from ORCHIDEE standard.

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

R: The plots were not identical for the sites, but similar in average values. The seasonal cycle in Tapajós km 67 is less pronounced and there is difference between the years. We will mention this in the revised manuscript.

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

R: LE and GPP plot K67 can be added (see also page 1 comment on additional constraints)

C: Figure 8: are these data from Guyaflux?

R: Yes, this was added in the caption.