

Interactive comment on “Seasonal variation in ecosystem parameters derived from FLUXNET data” by M. Groenendijk et al.

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General comments

In the paper entitled “Seasonal variation in ecosystem parameters derived from FLUXNET data” Groenendijk et al. calibrate a simple carbon exchange and evapotranspiration model to a wide range of FLUXNET data on a weekly basis and analyse the variability of the calibrated model parameters over time. The authors formulate three research questions: (1) are the model parameters clearly different between PFTs, (2) do the model parameters vary in an understandable way and (3) does the variation in model parameters have implications for our understanding of the feedback between vegetation and climate? In the data analysis, the authors differentiate between deciduous broadleaf forest, evergreen needleleaf forest and grassland, and stratify the data

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in boreal, temperate-continental, temperate and subtropical-mediterranean climates. The model parameters analysed are the reference ecosystem respiration rate (R_{ref}), a parameter relating ecosystem respiration to air temperature (E_0), a parameter representing photosynthetic capacity (J_m), a parameter representing the quantum yield (α) and the slope of the CO₂ assimilation to evapotranspiration relationship ($1/\lambda$). The authors show that the calibrated parameter values have different intra-annual variations for different climates and vegetation types. No systematic analysis of parameter differences between different PFTs is given. Instead, the authors concentrate on the seasonal variations of the model parameters. They show that the seasonal variability of the different parameters is more or less strongly pronounced depending on the climate and vegetation type. The authors did not find any linear correlations between model parameters and climate variables. They hypothesise that the relationships might be more complicated and non-linear.

There are some deficiencies in the model description that are likely to hamper proper assessment of the model results. First of all, the units of the model variables in equations (A1) to (A3) are not consistent (see ‘Specific comments’ below). It is not obvious to me whether correction of these errors will change the results qualitatively, but this definitely has to be fixed and the analysis repeated with sound unit conversions. Secondly, it is not correct to omit dark respiration from Equation (A3), as dark respiration has an effect on C_i . Thirdly, the authors do not seem to differentiate between transpiration and overall latent heat flux. They simulate transpiration rates but calibrate them against observed total latent heat flux, which includes bare soil evaporation as well. At sites with substantial bare soil evaporation rates they could hence significantly over-estimate stomatal conductance, which would have to be compensated by under-estimating photosynthetic capacity. For those inaccuracies that cannot be resolved, an estimate of the resulting errors would be helpful.

Some of the model description and discussion is a bit misleading. The authors mention that the model “implicitly accounts for variation in LAI” (p. 2878, 2). At the same time,

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they describe the model parameter α as “the quantum yield or light use efficiency (mol mol⁻¹)”. The term quantum yield is often used to represent the initial slope of a light response curve, while the term light use efficiency is generally used to represent the effective return in carbon per unit of light absorbed. In Equation (A8), α is a factor coupled to irradiance, implying that it represents the fraction of absorbed light, which is quite different from the implications made by the expressions used in the paper. In fact, variations in α do represent variations in LAI to some extent (at low LAI values were LAI is proportional to the fraction of absorbed light). I think that this recognition is important, as it explains where the model accounts for variations in LAI. In this context, I find it a bit confusing that the authors express the electron transport capacity (J_m) per unit ground area. A slight modification to Equation (A8) would allow to interpret α as the surface area fraction covered by vegetation and J_m as the electron transport capacity per unit vegetated surface area. This would allow separating parameter variations due to phenology (greening up and leaf decay) from variations due to e.g. adaptation to changes in temperature, light intensity or water availability.

I also found the description of λ a bit misleading. On page 2869, the authors describe λ as “the ratio between water loss (E) and carbon production(A)” and call it the “intrinsic water use efficiency”. On page 2875, they call $1/\lambda$ the water use efficiency, which is even more misleading, as water use efficiency usually refers to the effective assimilation rate per unit of transpired water, while λ is the slope of the trade-off between assimilation and transpiration. These are two very different things.

In summary, I think that a corrected/improved version of the presented model could be used to assess similarities and differences in behaviour between different vegetation types and that such an analysis could be helpful for models based on plant functional types. However, I agree with the anonymous referee #1 in that it would be better to represent vegetation behaviour by constant parameters (e.g. J_m as a function of irradiance with constant parameters) rather than tuning the parameters week by week. This would allow comparison of model parameter sets between plant functional types

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(PFTs). Perhaps it would even be a good idea to use a vegetation model that represents different PFTs in a similar way as a DGVM instead of the 5PG model. If the aim is to test whether a distinction of different PFTs is reasonable based on the data, it would at least be instructive to present a typical PFT representation from a DGVM and compare it with the 5PG model used here. In any case, a statistical analysis of the similarities between the parameter combinations of the different PFTs would be important, which has not been presented in this manuscript.

Specific comments

Throughout the document: I would prefer the term “calibrate” or “tune” instead of “optimize” to distinguish between parameter tuning and the search for parameter combinations that would fulfil some external objective function.

P. 2864, 20-21 and P. 2880, 18-20: The authors state that a strong seasonality of the model parameters indicates a strong relation between vegetation and climate and that ecosystems displaying strong seasonality would be stronger affected by climate change than those with a weaker seasonality. This argument needs to be explained a bit more as it does not seem logical to me. The seasonality in model parameters is likely a direct consequence of climate seasonality, so this statement would suggest that vegetation in climates with a strong seasonality would be strongly affected by climate change. I would expect the opposite to be true, as vegetation adapted to strong climatic seasonality is likely to be accommodating for a wider range of climates than vegetation adapted to a less seasonal climate.

P. 2865, 23-25: This sentence seems incomplete

P. 2867, 11-13: The convergence of parameter values into PFT groups would have been a very interesting result, but the according analysis has not been presented in the paper.

P. 2869, 20: In my understanding, quantum yield and light use efficiency are not the

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same thing.

P. 2869, 22-25: Evidence for light-limitation of photosynthesis across the investigated sites would have been a very interesting result by itself. Please provide it for the readers' benefit.

P. 2875: It is not clear how the results described here relate to phenology. It would be helpful to modify the model in such a way that one could distinguish between variability in leaf area and parameter values per unit leaf area.

P. 2878, 4-5: What do the authors mean by a complex model? Do they mean a more complex vegetation model or a more complex model relating climate variables to vegetation properties?

P. 2877, 14-18: It is not clear to me why inclusion of seasonal variation in big leaf properties can be considered an upscaling exercise.

P. 2877, 25-26: Missing data of LAI is not an argument for not including LAI as a model parameter, since it could have been included as a calibrated parameter, just like the other 5 parameters. In fact, the parameter α does account for LAI, as described above. I would suggest to modify the model such a way that leaf area is a variable, while the other variables are expressed per unit leaf area.

P. 2878, 20-24: It is not true that the results presented here are in contrast with the findings by Schymanski et al. (2007). It is interesting to note that the average grassland λ presented here is higher than the average evergreen needle-leaf forest λ . In the study by Schymanski et al. (2007), vegetation was dominated by grasses in the wet season (hence lower λ), while it was dominated by evergreen trees in the dry season (hence higher λ in the dry season). Clearly, the conclusion that water use is most efficient during the drier summer months is limited to sites where the dominant vegetation type does not change seasonally. It is also not quite clear to me how the authors deal with the fact that the "warmer summer months" are not necessarily the same for all sites.

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Even if sites on the southern hemisphere were excluded (are they?), some sites that are dominated by the monsoon would have a very different cycle of temperature and precipitation than the one implied when the authors refer to the "warmer summer months".

P. 2878, 27-28: As far as I know, available soil moisture data for eddy flux sites is usually limited to the top 10 cm of soil. This is probably more sensitive to relative humidity than to the plant available water in the soil, so I would not expect a strong relation between these numbers and λ .

P. 2880, 9-11: I was not able to follow this argument.

P. 2880, 5-17: This suggests to me that changes in leaf area dominate the detected seasonality, indicating that it would be important to separate leaf area changes out of the other model parameters.

Appendix A: The relation between the diffusivity of water vapour to the the diffusivity of CO₂ (1.6) is valid for concentration gradients measured in the same units (e.g. mol CO₂ mol⁻¹ air). Here units of mol m⁻³ are combined with ppm, so it appears that the use of the same g_s in Equations (A1) and (A2) is wrong. In Equation (A3), Γ^* is given in units of bar, and then divided by C_i , which is in units of ppm. This would lead to an error in Γ^*/C_i by 6 orders of magnitude. In Equation (A3), dark respiration cannot be omitted, as the diffusive A in Equation (A2) has to be balanced by the difference between the CO₂ sink (represented by equation (A3)) minus dark respiration. If dark respiration is left out of (A3), (A3) cannot be equated to (A2).

Figure 8: It would be clearer to express $1/\lambda$ in units of mol/mol, otherwise it is not clear whether the units of mmol refer to transpiration or assimilation.

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