

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

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General comments

We thank the referees for their comments and suggestions to improve the paper. Before replying point-by-point to the reviewers, we want to reiterate our objectives because in our first version we may have stated these not clearly enough. This may have hindered the referees from fully appreciating our objectives.

Many vegetation models use a simple physical description to simulate the water and carbon fluxes of ecosystems. When tuned for individual sites these models perform in general quite well. When used in global vegetation model, all types of vegetation occurring in reality are grouped together into plant functional types (PFTs), assuming

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PFTs have characteristic parameter combinations. Little is known about what causes variation in these parameters between and within the PFTs, and this is precisely what we wish to investigate. It is thus not our intention to present a ready-to-use prognostic model, but to use a simple model in a diagnostic sense. Ultimately this may serve to improve prognostic models, but first we need to understand what drives the apparent variation in parameter values.

As observations we use the FLUXNET database containing fluxes of water vapor and CO₂. There is little prior knowledge about the values of the model parameters to be used. Hence, we have to derive the model parameters by optimizing the simulated fluxes with observations. This compels us to use a simplified model with a reduced number of parameters, to prevent the optimizing problem from becoming underdetermined. This implies of course that some degree of incompleteness of the model is unavoidable. However, we emphasize that our simplified model is able to explain 80% of the observed variation in the fluxes.

The choice for working with time-dependent vegetation parameters, contrary to the custom to keep the parameters in global vegetation models constant for each PFT, was insufficiently explained in the discussion paper, but is well-founded. There is much uncertainty about these relations, and often there is also lack of appropriate data. Moreover, the number of unknowns to be optimized from flux observations etc. has to be restricted. For these reasons, we decided that we could not rely on a model with many constant parameters but work with (time-dependent) vegetation parameters which are smaller in number and which have an intimate relation with the fluxes.

We can now concentrate on the question: how are the (variable) model parameters controlled? In particular, we group the FLUXNET sites within PFTs to see what the variation of model parameters is between and within the different PFTs. Specific questions are: To which extent are the parameters different between PFTs? And if there is variation, do the parameters vary in an understandable way? Factors influencing the variation are the vegetation type and climate, but also the management, history of the

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site and other anthropogenic factors. Finally, we want to investigate if our parameter variation is compatible with the predictions of current vegetation models.

Specific comments of anonymous Referee #1

The authors have undertaken a large model optimization exercise, using data generated by a large number of groups. . . . But instead we have a new model of little obvious value that is used for a mystifying purpose - identifying how parameters change in time - which simply shows up the major model failings.

Answer: The referee misinterprets the objectives of the paper: to understand how CO₂ fluxes vary across climate zones. This type of research is very innovative and needed for an accurate representation of the carbon cycle in climate models. Until now there are only a few publications with similar research (van Dijk et al., 2005; Beer et al., 2009; Reichstein et al, 2007). Therefore the argument that our paper does not contain any novel research is not well founded. As to the argument that the paper generates unsupported conclusions, below we will address the cases where the referee specifies this.

The 5PM model is presented for the first time in this paper. It seems to be a combination of models from Farquhar and Cowan. The Farquhar photosynthesis model is relatively complex, process based and well verified. The components from Cowan, that predict internal CO₂ concentration and thus stomatal conductance, are more theoretical, based on assumptions about optimisation of water use, and largely untested.

Answer: The main part of the model is based on the models of Farquhar et al. (1980) and Cowan (1977) as referee #1 correctly noticed. According to this referee the components from Cowan are largely untested. We do not agree with this, because the model was previously used to simulate the transpiration fluxes by Arneeth et al. (2002), van der Tol et al. (2007) and Schymanski et al. (2007).

Other components of the model seem to be missing - there is no radiative transfer scheme to distribute light through the canopy, no plant-water routines to include hy-

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draulic stress, no phenology routines to schedule plant activities.

Answer: This is a scale issue. These are small-scale processes and site specific details about which sufficient information is in practice not available, and it is therefore not feasible to include them in a large-scale model.

It is never made clear how the model switches between enzyme limited and limited constraints on photosynthesis.

Answer: This is not the case, as indicated between equations (A2) and (A3) the model operates at any time in that mode which is most limiting for the photosynthesis.

The respiration model is a simple temperature response function, completely decoupled from photosynthesis, and with no separate components for autotrophic and heterotrophic sources.

Answer: Our objective is to explain the variation in fluxes between PFTs and climate zones. In that perspective respiration rate at a reference temperature and temperature sensitivity are on the large-scale the most important parameters. The reference respiration rate is a function of the size of carbon stocks. A more complex approach with pools of different stability and temperature and humidity dependent turnover times may result in a better formulation of the carbon balance, but it is infeasible to obtain the corresponding parameters from the available data. We will add the autotrophic respiration as a constant fraction of photosynthesis to equation (A3). The heterotrophic respiration is described with the temperature response function.

Overall 5PM is confusing, a mix of complex components, simple components, missing components, and containing no internal feedbacks, such as a carbon mass balance. . . . Firstly the model must have been constructed with some representation of the key processes that are expected to govern behaviour. Secondly, any observed variation in parameters must be diagnosed and use to improve the model. The authors do not meet my caveats.

Answer: These comments have been addressed in the introductory statements.

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The 5PM model is deficient in key areas for the purposes of this analysis, the most important of which are the lack of mass balance and the lack of any phenological component. The 5PM model does not describe canopy light interception, nor its variation in time.

Answer: It depends on the case which parameters should be considered as key parameters. The mass balance is not of immediate importance for the short term calculations presented here (which implies admittedly that it is hard to draw conclusions about the mass balance from the present work). The other parameters mentioned are of only secondary importance for a model that is intended for larger scales, with insufficient knowledge about the fine details of for instance canopy interception.

A key conclusion of the paper is that "broadleaf forests... have large seasonal variation in ... parameters". This result could have been determined without any model runs at all, from basic knowledge of forest ecology. ... If they had substituted a more complete model for 5PM then this very interesting line of enquiry could have been followed up.

Answer: We agree with the referee that the conclusions about the seasonality of the broadleaf forests are to a large extent related to LAI. We will include LAI in the model to being able to focus on other causes of intra-annual variations. However, we still learn about bulk variation (including LAI) in the present analysis. We start from a position where we question the a priori assumption of fixed parameters.

I am very concerned by the conclusions generated by this paper. The authors state that their analysis focuses "on short term processes". They then conclude from their analyses, in a manner that is not clear at all, that "climate change will have the largest impact on the terrestrial carbon fluxes in boreal regions and for deciduous forests, and less for grasslands and evergreen forests". How has this conclusion been reached? How can a model that lacks mass balance be used to make any such claims about climate change impacts, which necessarily involved complex feedbacks over years and decades?

Answer: The text will be clarified. We agree, that the scope of the present work is in

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general limited to the short term, and hence statements about climate change impact have to be weakened.

The authors state that the model "can be applied globally", but I cannot see for what purpose it can be applied - certainly not for prognosis.

Answer: The setup is tailored for global application in a diagnostic sense, as explained in the introduction of this response.

The finding that C fluxes are not linearly related to climate is a trivial rehashing of basic knowledge.

Answer: The issue is that it is difficult to find a single relation between PFT-parameters and climate from our observations. However, this is implicitly assumed in many state-of-the-art global vegetation models.

Specific comments of Referee #2 (S. Schymanski)

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.

Answer: In the final paper we will include a clear statistical analysis to show the differences between and within PFTs.

The units of the model variables in equations (A1) to (A3) are not consistent.

Answer: There was a regrettable mismatch between the model code and the description in the paper. The units within the model code were correct, so apart from the corrected description, nothing will change.

It is not correct to omit dark respiration from equation (A3), as dark respiration has an effect on C_i .

Answer: The reviewer is correct, but if one assumes dark respiration to be a constant fraction of assimilation, the error is small. Furthermore, it is impossible to obtain dark respiration from our observation.

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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 ... For those inaccuracies that cannot be resolved, an estimate of the resulting errors would be helpful.

Answer: As described in the paper (p2870, lines 11-14), we did not use data when precipitation fell in the preceding 3 hours. This reduces the problem with interception evaporation and part of the bare soil evaporation when the bare soil is wettest. The analysis is focused on forests, where bare soil evaporation is relatively unimportant. But we do agree that this can still introduce an additional uncertainty, which is larger for open forests than for dense forests. An overestimation of the observed transpiration flux can lead to deviations in the parameter values of J_m and λ . In the final version we will estimate the uncertainty.

Some of the model description and discussion is a bit misleading ... light use efficiency is generally used to represent the effective return in carbon per unit of light absorbed.

Answer: the parameter ? used in the model is the quantum yield, and not the light use efficiency. We will correct this in the text.

In Equation (A8), α is a factor coupled to irradiance, implying that it represents the fraction of absorbed light ... 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.

Answer: As described earlier, we will add LAI to the model.

I also found the description of λ a bit misleading ... These are two very different things.

Answer: In the final paper we will include a descriptive list of symbols. We propose to use the term marginal water cost for λ and marginal efficiency for $1/\lambda$.

I agree with referee #1 that it would be better to represent vegetation behavior by 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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Answer: This has been addressed above.

Perhaps it would be a good idea to use a vegetation model that represents different PFTs in a similar way as DGVM instead of the 5PM model. ... 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.

Answer: A statistical analysis will be part of the final paper. We also note that the Farquhar model used in our paper is a component of some of the DGVM's that are used in climate models.

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 fulfill some external objective function.

Answer: We use the term optimization to describe the minimization of the difference between observed and simulated fluxes; this is done by automatic tuning of the parameters. The term optimizing refers to our aim to find the combination of parameters with which the model best describes the observed fluxes.

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. ...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.

Answer: In climate zones with strong seasonality, where climate change is large (polar amplification) the vegetation reacts on the short term, and this has a large impact on variation of annual carbon uptake. This is what we mean with strong coupling. But indeed there is no reason to think that the coupling per se is smaller in regions with smaller seasonality. We will rephrase this in the text (see also comment above on this conclusion).

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

Answer: Reformulated: A number of efforts have been made to relate terrestrial carbon fluxes (NEE) to climate. We will add some lines on what these efforts have comprised.

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.

Answer: We will address this in the new results section. However, the convergent or gradual behaviour appears more complicated than initially anticipated.

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

Answer: see above. The text will be corrected.

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.

Answer: we will show and emphasize in the results section how we obtained this evidence for light limitation for the largest part of time.

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.

Answer: Indeed the parameter variability appears to be related to phenology and water, light and temperature limitation. This issue is answered already above and we will include it in the final paper. The distinguishing between variability in leaf area and parameter values per unit leaf area will be made when the LAI is included in the model.

P. 2877, 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?

Answer: A more complex model relating climate variables to vegetation properties. The

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text will be clarified.

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.

Answer: "on a daily time scale" should be replaced by "on the leaf level". Farquhar et al derived formulations of photosynthesis rates at the leaf level, but applying them to an entire ecosystem requires upscaling with seasonally varying LAI.

P. 2877, 25-26: Missing data of LAI is not an argument for not including LAI as a model parameter, ... 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.

Answer: We will use this suggestion as method to add LAI to the mode in the next version.

P. 2878, 20-24: It is not true that the results presented here are in contrast with the findings by Schymanski et al. (2007). ... 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".

Answer: The reviewer is correct here and we will rephrase the text. We used one site in the southern hemisphere. For this site we moved the day of year for the observations with 6 months to be more consistently usable with the other sites. This makes the comparison of sites easier.

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 .

Answer: Soil moisture measurements are often made at levels until 50-70 cm deep. It also depends a bit on the timescale, the diurnal variation of soil moisture may be related to air humidity, but the weekly to seasonal variation is certainly an indication of

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vegetation moisture stress.

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

Answer: We mean that daily and/or annual carbon fluxes do not linearly relate to annual mean temperature, moisture, etc.. Therefore nonlinear model tools need to be developed and used to explain the variation in carbon fluxes. The adjective "more complex" was admittedly vague and will be changed.

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.

Answer: This correct and LAI will be part of the model in the new text.

Appendix A: The relation between the diffusivity of water vapour to the diffusivity of CO₂ (1.6) is valid for concentration gradients measured in the same units (e.g. mol CO₂ mol⁻¹ air). If dark respiration is left out of (A3), (A3) cannot be equated to (A2).

Answer: This has been addressed above.

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

Answer: This will be done.

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