Authors’ response to the comments of referee #1 on the article "Identification of a general light use efficiency model for gross primary production" by J.E. Horn and K. Schulz in Biogeosciences Discussions

Judith E. Horn[1,2] and Karsten Schulz[1]

[1]Department of Geography, Ludwig-Maximilians-Universität München, Luisenstr. 37, 80333 Munich, Germany
[2]*now at* Institute of Hydromechanics, Karlsruhe Institute of Technology (KIT), Kaiserstr. 12, 76131 Karlsruhe, Germany

RC - Referee Comment; AR - Authors’ Response

1 Response to the general comments of referee 1

RC: "I reviewed the paper chiefly from the methodological point of view. I found it interesting and appreciated the substantial amount of work that it contains. However, the authors have not paid enough attention to the careful documentation of their work: as its present form, the article does not describe the methods sufficiently clearly and explicitly so that a reader could easily follow the ideas or that other scientists could reproduce the work. The overall structure of the paper is nice and clear, but the language would need to be clarified and revised.

A major scientific question that arose in my mind is the motivation for using dynamic linear regression (DLR) and state dependent parameter estimation (SDP) in the estimation of the time series of light use efficiency (ε) from the time series of gross photosynthesis (FG) and radiation (S0, APAR), as opposed to estimating ε as a simple ratio of FG and S0 or APAR. The application of these novel methods DLR and SDP is one of the points of the paper, and therefore their validity should be explained and discussed thoroughly. However, if the time series of ε are utilised merely in search for new model structures and not in the final model estimation and evaluation, as I believe is the case, this question is not crucial regarding the validity of the final model presented in the paper. I address this issue in more detail in the specific comments below (point 5)."

AR: We very much appreciate the intensive and thorough work referee #1 put into her/his review. Her/his detailed comments and suggestions helped to (hopefully) significantly improve our manuscript and to make it more understandable and readable to the potential readership of Biogeosciences. In particular, we have tried to clarify the methodological descriptions of the used DLR and SDP methods as much as we think is necessary and have referred to more detailed descriptions where possible in order to keep the overall length of the document acceptable. In the following, we respond in more detail to the specific and technical comments made by Referee #1.

2 Response to the specific and technical comments

2.1 RC #1

RC: "The time scale (half-hourly, daily) should be explicitly stated in different parts of the work (evaluation of the Jarvis model, finding new model structures, formulating the generalized model, model calibration), as it makes difference whether the data contain both within-day and between-days variation, or only between-days variation. If different time scales were used in different stages (e.g. finding new model structures vs. final model calibration), this should be motivated. The time scale issue is repeatedly pointed out in the comments below."

AR: The data were downloaded on a hourly and half-hourly basis. The measurement gaps of the meteorological variables were filled at this original time scale. The fluxes were - for computational reasons - filled on a hourly time scale. The time series of all variables were finally aggregated to time series with daily time steps. These daily time series were used throughout the study. We have hopefully clarified this in the data description section (p. 7, r. 7; p. 7, r. 18; p. 7, r. 29ff) and have indicated the use of daily data whenever it seemed appropriate.

2.2 RC #2

RC: "In the introduction, the purpose of this study need be more clearly stated: (i) why to build yet another LUE-type model, i.e., which deficiencies of the existing models this new model is thought to remedy (cf. the vague formulation in p. 7676, r. 22-25), and (ii) why to apply DLR and SDP to estimate the time series of ε (see the specific comments in point 5 below; cf. the obscure motivation in p. 7677, r. 5-11)."

AR: Current light use efficiency models use a theoretical maximum light use efficiency which is downscaled by one or more modifying factors. The latter are based on predefined functions. The parameters (the maximum light use efficiency and the parameters of the limiting function(s)) are set to biome-specific or even global constants, i.e. it is assumed that e.g. a maximum/potential light use efficiency or a global temperature optimum exist that hold for all sites regardless of their vegetation types and climate region. In a recent paper, Garbulsky et al. (2010) state that the relationships between the light use efficiency and its climatological drivers for different biomes are still not clarified, and, furthermore, "a substantial number of those relationships were derived from models rather than using evidence from actual measurements" (Garbulsky et al., 2010, p. 254). The usage of fixed, biome-specific maximum/potential light use efficiencies (such as in the MODIS GPP algorithm) or even global constants (such as used in the model proposed by Yuan et al. (2007)) "is far from optimum and is the possible cause of the low performance of the photosynthetic uptake models" (Garbulsky et al., 2010, p. 255). The above motivation for this study is stated in a more explicit manner in the revised manuscript (p. 4, r. 24ff).

For the reasons stated above, we think it is worth trying to find an alternative modeling approach. In this study, we therefore explore the benefits of applying a data-based modeling approach (p. 5, r. 5ff), which deduces the functions constraining the maximum light use efficiency directly from the measured data. SDP is a useful diagnostic tool for this task. It facilitates the identification of dominant behavior of the light use efficiency in relation to the drivers under consideration in an objective manner. The noise inherent to eddy-covariance measurement data is explicitly addressed by the so-called noise-variance-ratio (NVR) which largely determines the filter-characteristic of both methods. Therefore

SDP is superior to a mere plotting of the light use efficiency (as coefficient of the absorbed radiation and the gross primary production) vs. its potential drivers. These relationships estimated with SDP form the basis for the subsequent formulation of the subfunctions down-regulating the maximum light use efficiency. The so derived model is then site-specifically parameterized. In a next step, these parameters can be regionalized – this is done in a follow-up paper by the authors. The motivation for the usage of SDP as diagnostic tool is more explicitly stated in the revised manuscript (p. 6, r. 3ff; p. 9, r. 3ff; p. 11, r. 18ff; p. 12, r. 14ff).

2.3 RC #3

RC: "In the description of the micrometeorological data, the precise meaning of the criterion "no lengthy measurement gaps" (p. 7678, r. 8) should be given and the time scale (half-hourly?) told; it would also be useful to show in Table 1 the measurement years included in the study for each site. The quality of eddy covariance measurements of net photosynthesis (FN) varies a lot according to air turbulence: were any quality criteria (e.g. site-specific friction velocity thresholds) used for filtering inadequate FN observations in this study, and if not, why? Further, it would be worthwhile to have a concise description of the semi-parametric methods that were used for the gap-filling of FN, for the extraction of "the signal component" of it, and for the computation of gross photosynthesis FG from this signal, since the time series of FG are really the basis of this study: would these semi-parametric methods artificially remove some true and natural variation in FN and introduce in FG regularity following some a priori model?".

AR: The "lengthy measurement gaps" are specified in the revised document (p. 7, r. 1f).

The meteorological time-series of the scalar variables were gap-filled on the original hourly or half-hourly time scale. The fluxes were all gap-filled on a hourly time-scale. The net CO_2-flux partitioning was performed on this hourly time scale, too. Finally, all variables were aggregated to time series with daily time steps. We stated the different time scales during the pre-processing of the data more precisely (p. 7, r. 7; p. 7, r. 18; p. 7, r. 29ff) and point out the usage of daily data throughout the study (p. 7, r. 29ff). We included the utilized measurement years in Table 1.

Whether and how to apply a friction velocity (u*) threshold or not is still an open discussion (Jarvis et al., 2004; Falk et al., 2005; Papale et al., 2006; Acevedo et al., 2009), although most often, night-time data are screened. Knowledge about the specific situation of a measurement site – such as the occurrence of specific micrometeorological weather patterns or the special characteristics of the terrain – is beneficial for this decision. Papale et al. (2006) estimated that the choice of the data-processing method (including no filtering) leads to about 10% uncertainty on gross primary production and respiration flux. We tested the effects of the application of a friction velocity threshold and found that it had only marginal consequences on the daily time series which is of interest to our study. Applying a threshold, however, reduced the number of data available for the spline interpolation. It is worth noting that our gap-filling and partitioning method method is somewhat less sensitive to a night-time screening than other methods, since it makes use of all F_N data for its multi-dimensional spline-hypersurface to partition the net flux. Against this background, we decided to use no threshold, accepted in some cases a slight bias in the daily data in favor of a more robust gap-filling and net flux partitioning method due to a higher data retention. We added an explanation for not using a u*-filter (p. 7, r. 7ff).

On the methodology used: The partitioning comparison presented in Desai et al. (2008), in which our method participated, stresses that the tested methods reveal differences in the variability and the annual sums of the fluxes but are able "to identify cross-site differences and spatial patterns of GPP and RE, as long as the same method is used to partition NEE across all sites" (Desai et al., 2008, p.836). (GPP - gross primary production, RE - respiration, NEE - net CO_2-flux). We clarified the description of the gap-filling and partitioning methodology in the manuscript (p. 7, r. 18-29). It would though go beyond the scope of this manuscript to give a detailed description of the procedure and we would like to refer to the mentioned publications.

2.4 RC #4

RC: "The computation of daily FPAR values in MODIS Land Products should be succinctly explained, or at least adequately referenced. What was the justification for the noise reduction of the MODIS FPAR time series, which is already a modelling product, by cubic smoothing splines and unequal weighing of observations? Clarify what is meant by multi-annual mean of FPAR (p. 7680, r. 12): the mean of daily FPAR over all the days and the measurement years, or the mean of FPAR in the particular day over the measurement years?"

AR: We added a short explanation and a reference for the LAI/FPAR retrieval scheme (p. 8, r. 12f).

We use the spline interpolation, since the time series from the MODIS LAI/FPAR product are typically very noisy and since we disaggregate the time series composed of 8-day-values into a daily time series. To calculate the average of the several pixels at each time step, the values are weighted according to their inverse difference to the multi-annual mean of FPAR values for the whole subset at the particular day over all measurement years. We use a weighted mean since often implausible differences occur for neighboring pixels with unrealistic values for single pixels (Horn and Schulz, 2010). In the revised manuscript, we specified the method of processing the MODIS FPAR values and provided references (p. 8, r. 13ff).

2.5 RC #5

RC: "For anyone not familiar with the original methodological papers by Young and his coworkers, or with the work by Jarvis et al. (2004) introducing SDP in eddy covariance context, the section describing DLR and SDP is impossible to grasp. This is unfortunate, if the study aims to further demonstrate the applicability of DLR and SDP with eddy covariance data. Surprisingly, Jarvis et al. (2004) are not even referred to in this section, although the text is fragmentarily following their methodology description – and even directly citing them, without quotation marks (p. 7679, r. 20-21 vs. p. 940-941 in Jarvis et al.)! The section should be rewritten so that the connection to the data of this study becomes clear, with (i) the basic model in Equation 4 as the starting point, (ii) stating clearly that the purpose is to estimate the time series of ε (half-hourly or daily?) from the time series data of FG and S0, (iii) giving precisely the model assumptions (the distributional assumptions of the error series $\zeta(t)$, and the random walk assumptions of the "parameter" series $\varepsilon(t)$) and separating them from the description of the resulting estimator of ε(t), and (iv) giving adequate references. For this, Jarvis et al. (2004) set a good example.

Yet a major question is the motivation for using DLR or SDP to estimate the time series of ε. Jarvis et al. (2004) used SDP for separating from the time series of FN provided by eddy covariance measurements the following two components: (1) FG as a product of parameter ε and measured above-canopy solar radiation S0, and (2) respiration FR as a parameter. Here, however, no such separation was needed, as FG was computed from the signal component of FN with another semi-parametric method before modelling. So why not then to estimate the time series of ε as a simple ratio of the time series of FG and S0, as Equation 4 would imply? With DLR or SDP, the value of ε at a time point is obtained as a non-linear function of the values of FG and S0 in the vicinity of the point in a time space or in a state space sorted according to the selected state variable, respectively. What is the rationale of estimating ε in this non-transparent way, and how does it compare with the simple ratio approach (certainly it results in smoother time series, but is this smoothing justified)? This issue pertains to the evaluation of the Jarvis model (see point 8 below) and to the finding of new model structures (point 9 below)."

AR: The methodological section explaining DLR and SDP (p. 9-12) is rewritten in the revised manuscript. The purpose of using SDP (as a mean to analyse the daily data which facilitates the formulation of functional model descriptions of the relationships of ε as time-varying parameter of the light use efficiency equation to potential environmental drivers) is pointed out more clearly (p. 6, r. 3ff; p. 9, r. 4ff; p. 11, r. 18). The advantages of using SDP over a mere plotting of the time-varying light use efficiency parameter (as direct coefficient of the absorbed radiation and the gross primary production) vs. its potential drivers are stated (p. 6, r. 7ff; p. 9, r. 7ff); see also AR to RC #2. The stochastic properties of the SDP model are specified (p. 9, r. 21f, 25f; p. 11, r. 13f). More references regarding DLR and SDP are provided in the whole section (p. 9-12). It should be mentioned here that even going into the original papers by Young and co-workers the mathematical description of the DLR and SDP methods is far from "easy-to-grasp". So we here have the difficult task to balance between mathematical completeness and correctness as well as briefness and illustrative simplicity of the description. We hope that the modified text is appropriately balancing between these requirements, however references to the original papers could not be avoided.

Our study uses the same principal model building methods (SDP to analyze the data as basis for the formulation of a model) as that of Jarvis et al. (2004). There are two main differences, though:

- While the study of Jarvis et al. (2004) was interested in the net flux, F_N, and therefore analyzed the gross flux as well as the respiration and consequently formulated a model component for both of them, this study focuses on the gross primary production. We therefore use their representation of this F_G model component as starting point of our study and analyse in a first step its suitability and deficiencies.
- The study of Jarvis et al. (2004) used SDP not only to analyse the data as basis for an appropriate model formulation, but also to fill measurement gaps and partition the net flux. It turned out, however, that this method is not optimal if the measurement gaps exceed a certain length. Therefore, a more adequate method (with a semi-parametric spline hyper-surface) was established by them (our working group) to gap-fill and partition the data (Stauch and Jarvis, 2006; Moffat et al., 2007; Desai et al., 2008); this new method is used in this study. SDP is consequently only used as diagnostic tool to analyze the data and estimate the dominant behavior of ε in relation to its potential drivers in the same way Jarvis et al. (2004) did. And as in their study, a simple plotting of ε vs. this variables is not satisfyingly given the noisiness of the data and the complexity of its drivers.

With these replies we also hope to have clarified the major aspects of the "General Comments" (see above).

2.6 RC #6

RC: "As to model evaluation criteria, several definitions for the coefficient of determination r^2 exist (see e.g. Kvålseth 1985). The definition used here (p. 7680, r. 18) should be given (I guess it is the squared Pearson correlation coefficient between observed and predicted values). In the Equation 3 giving the definition of EC referred to as "the Nash-Sutcliff efficiency criterion", the differences in the sums in the numerator and denominator should be squared to meet the verbal description (p. 7680, r. 19-21). In fact, also this (corrected) EC is a coefficient of determination, corresponding to another commonly used definition (Kvålseth 1985)! Of these two definitions of r^2, the latter is certainly preferable, for the reasons stated in the text (p. 7680, r. 23-24)."

AR: The coefficient of determination r^2 is specified and references are given (p. 12, r. 4ff). The description and equation of the Nash-Sutcliff efficiency criterion EC are corrected (p. 12, r. 7ff).

2.7 RC #7

RC: "In the model identification section, all the variables and parameters in the equations must be explained and their units given (now many explanations are not only incomplete but simply missing, e.g. S0 in Equation 4; α in Equation 6; E, λ and ζ(t) in Equation 7; $P(t)$ in Equation 8). Specify whether S0 was (above-canopy?) global radiation or PAR. Specify from which depth TS was measured; how does it compare with "measured surface temperature" used as TS by Jarvis et al. (2004)? Specify from which layer SWC was measured. Specify what value was used for the recession constant κ in the computation of API (Equation 8, p. 7684, r. 4-6)."

AR: All the variables and parameters in the equations are explained and their units specified (Eq. 1-17).

The soil temperature, measured in the top-layer, is the same as that used in the study of (Jarvis et al., 2004), in which they called it "measured surface temperature" (Jarvis et al., 2004, p. 941). The measurement depth of the soil water content and the soil temperature is given (p. 11 r. 25; p. 15, r. 25f).

2.8 RC #8

RC: "In the evaluation of the Jarvis model for ε, it need be clarified how the model was "applied to all study sites" (p- 7681, r. 16-18). From Fig. 2 I conjecture that (i) the time series of ε was estimated with DLR from the time series of FG and S0 for each site, and (ii) the model in Equations 5 and 6 was fitted in these estimated ε data using the non-linear least squares method. What was the motivation for estimating ε with DLR and not with SDP (with TS as the state variable) as Jarvis et al. (2004) did when constructing the model? What was the time scale (half-hourly or daily)? And more importantly, if the aim was to find whether the Jarvis model reproduces well the daily course of FG in the study sites (cf. p.7681, r. 18-19), should not the model have been fitted directly to the daily data comprising only FG and S0, as Jarvis et al. (2004) did when evaluating the model (cf. their Equation 5)? Related to this, it need be specified whether r^2 and EC were computed from the measured (DLR-estimated) and predicted ε or from measured and predicted FG."

AR: With the term "applying the Jarvis-model" it was meant, that the model is run at each study site, whereas the four model parameters are directly fitted to the data. The optimization criterion is the minimum of the sum of error squares between the modelled and the measured F_G-fluxes. SDP and

DLR play no role in this model application step. To show why the Jarvis-model produces in some cases good results, in other cases however fails, the subfunction of the Jarvis-model describing the relation of ε to T_S is plotted and compared to the "measured", but somewhat smoothed ε-values to better depict the seasonal dominant behaviour. It could have been simply plotted with the light use efficiency as direct quotient of gross primary production and radiation. However, it was decided to plot the light use efficiency obtained with DLR, since we focus on the seasonal dynamics of ε. This is stated in the revised manuscript (p. 11, r. 1ff; p. 14, r. 15ff; caption of Figure 2). All r^2- and EC-values given in this section – as well as in the following ones – are calculated with regard to the measured (without application of DLR or SDP) and modelled F_G-values (p. 14, r. 12f; p. 14, r. 22f; Table 2).

2.9 RC #9

RC: "New model structures were searched by estimating the time series of ε with SDP, with one explanatory variable (radiation) and one or two state variables (soil temperature, four measures of water availability), and then studying how these time series depended on the state variables. The time scale of the modelling (half-hourly or daily) need be given. Further, the explanatory variable of the SDP model should be specified also in the case of one state variable: S0 or APAR, and if S0, why (using S0 follows Jarvis et al. (2004) but deviates from the two-state-variable case in Equation 9)? It would be helpful to see the SDP model explicitly written also in the case of one state variable, specifying the explanatory variable and the dependence of the "parameter" ε on the state variable. In the case of two state variables (Equation 9), it would facilitate understanding if ε was explicitly included in the equation or its explanation (see the technical corrections below). Finally, tell how the performance of the SDP model was assessed (p. 7683, r. 9-17): with EC and r2 computed from measured and SDP-model-predicted FG? The content of Figs. 4 and 5 should also be explained: what are the error bands, is the course of ε some average of the SDP-estimated values?"

AR: Equations for all SDP models applied to analyze the data are given in the revised manuscript (Eq. 2, 3, 5, 10, 12-14). APAR was used in all cases for the reasons stated in the text (p. 15, r. 12ff). The performance of the various SDP models was determined in terms of r^2-values and the width of the standard errors encompassing the estimated variation of ε (p. 15, r. 21f). As stated in the figure's captions, Figures 4 and 5 show the relationships of ε to its potential environmental drivers as estimated with SDP for different sites (Duke Forest, Vaira Ranch, Roccarespampani, Audubon); so these plots are examples for the SDP results.

2.10 RC #10

RC: "Concerning the Monte Carlo simulations performed to study the sensitivity of the final model on the potential variation in parameter values, the distributions assumed for the parameters should be specified (normal?) and the determination of the parameters of these distributions (means and standard deviations?) outlined. The explanation of how the correlations between the parameters were taken into account ("to account for interrelations of the parameters the samples were generated by Cholesky decomposition of the covariance matrix of the model residuals applied to normally distributed sample"; p. 7686, r. 18-20) is inaccurate: it would make sense to apply the Cholesky decomposition to the correlation matrix of the parameters to get the lower-triangular matrix, with which a vector of uncorrelated parameter samples should then be multiplied to obtain a vector of properly correlated parameter samples. How was the correlation matrix (or the covariance matrix of the model residuals, if it really was erroneously used) obtained before fitting the final model? What was the point of performing the Monte Carlo simulations before the estimation of parameters of the final model (model calibration)? Or were the simulations performed in two phases, before and after the model parameter estimation?"

AR: Before the calibration of the derived model, a Monte Carlo simulation was performed to test the sensitivity of the seven parameters. One parameter was found to be quite insensitive and therefore is set constant. Subsequently the model is finally fitted to the data with six free parameters. For these six free parameters, an additional sensitivity study with a Monte Carlo simulation is performed.

Concerning our original description of the derivation process for the 95% uncertainty bounds we agree with the reviewer who stated some misunderstanding and we provide a succinct description of the method together with a reference (p. 20, r. 6ff). We derived the 95% uncertainty bounds as follows: Model calibration is carried out using the Matlab nonlinear least-square optimization routine "lsqnonlin" which applies a subspace trust-region method and is based on the interior-reflective Newton method as described in Coleman and Li (1994). Besides optimum parameter values, the routine provides an approximation of the Jacobian matrix from where the parameter covariance matrix is derived (see e.g. Thornley and Johnson (2002)) allowing to calculate parameter confidence intervals based on the student t-distribution. In order to estimate the uncertainty bounds/confidence intervals of the predicted F_G time series as a result of the propagation of errors due to parameter calibration uncertainties, parameter sets are randomly chosen within an Monte Carlo exercise. A Cholesky decomposition of the parameter covariance matrix is carried out to obtain the lower triangular matrix from where appropriate parameter sets are generated.

The sensitivity tests with seven and six free parameters and the derivation of the prediction uncertainty bounds due to parameter uncertainty are stated more clearly in the text (p. 19, r. 7ff; p. 20, r. 6ff; p. 21, r. 8f).

2.11 RC #11

RC: "Regarding the estimation of the parameters of the final model (model calibration), it would be useful to explicitly tell on which data and time scale this was done: daily observations of FG, APAR and water availability measures (instead of estimated half-hourly time series of ε and half-hourly time series of APAR and water availability measures)."

AR: As stated under RC #1: Beside the preprocessing of the measurement data (gap-filling and net flux partitioning), all steps of this study were performed with daily time steps. The study of Jarvis et al. (2004) came to the conclusion that the analysis and modeling with daily time steps is more appropriate due to the dominance of seasonal behavior of F_G, and used for the derivation of the model and its application daily time steps, too. The usage of daily data is now stated explicitly (p. 7, r. 29ff) and repeatedly indicated wherever suitable.

2.12 RC #12 (#11 in the "Specific comments" of the review)

RC: "In the abstract, it need be clarified what is meant by "a model formulation allowing a variable influence of the model parameters modulating the light use efficiency" (p. 7674, r. 5-6)."

AR: A clearer formulation is used in the revised manuscript (p. 2, r. 5ff).

2.13 RC #13 (#12 in the "Specific comments" of the review)

RC: "Some remarks concerning terminology: Strictly speaking, ε should not be termed light use efficiency in the case where radiation above canopy (and not radiation absorbed by canopy) is used as S0 in Equation 4. Further, it is confusing to use word "parameter" in two different meanings, referring both to the time series of ε (which is indeed a time-varying parameter in DLR and SDP models) and to the unknown constants of the models of ε, or FG, constructed on the basis these time series; to avoid confusion, some other term should preferably be used for the time series of ε."

AR: For the reasons stated by the referee, Jarvis et al. (2004) termed ε "radiation capture and utilization coefficient" (Jarvis et al., 2004, p.940). In the revised manuscript, we use this term, too, in the context of the Jarvis-model (p. 13, r. 7ff).

In the revised manuscript, the term "parameter" is still used referring to ε, since this is the typically used term. However, the light use efficiency as used in our study is introduced as "time-varying" parameter (p. 11, r. 6) and the optimized constant model parameters are further specified by the adjective "constant" optimized (p. 14, r. 5; p. 19, r. 6.; p. 20, r. 26; Table 2).

2.14 Technical corrections

- **RC:** "Table 1: Explain the abbreviations of the vegetation types and climate classes in the caption – you can move the explanation in the caption of Fig. 3 here."

  AR: We added an explanation to Table 1.

- **RC:** "Table 2: In the caption, "confidence interval" should probably read "standard error". Are the incredibly high confidence intervals obtained for Topt and kT in IT-Cpz and BE-Vie true?"

  AR: As mentioned above (RC #10), these uncertainty bounds are 95% "confidence intervals" indeed. We added the specification "95%" in the caption of Table 2. The large confidence intervals at the mentioned sites are true: At Cpz, however, the temperature-subfunction f_T has a low influence (p is small), ε depends mainly on the moisture function. At Vielsalm, the fluxes cannot be explained well by the combination of T_S and EF. To reproduce the fluxes, a very flat curve of T_S has to be fit to the data. For this, a quite large part of the f_T-subfunction comes into question which makes the parameter estimation uncertain.

- **RC:** "Fig.1: The locations of the sites in the map appear inaccurate (e.g. Swedish Flakaliden is located in the sea, Finnish inland Hyytiälä on the coast, and French Le Bray almost in Spain). Further, explain the abbreviations of the vegetation types included in the legend – you can now refer to the caption of Table 1."

  AR: The center of the symbols marked the geographical locations, not the bottom. However, this figure is omitted in the revised manuscript in favour of a plot with examples for DLR and SDP.

- **RC:** "Fig. 2: In the legend, indicate "Jarvis model" by a red line instead of a red point."

  AR: The red point in Figure 2 is replaced by a red line.

- **RC:** "Fig. 3: For better readability, centre the labels of the climate classes and vegetation types (justify them in the middle of the left side of the rows and in the middle of the bottom side of the columns). For the explanation of the vegetation types and climate classes, you can refer to the caption of Table 1."

AR: The labels in Figure 3 are centred in the revised manuscript.

- **RC:** "Fig. 4: Explain the error bands. Consider changing the colour of the yellow line into something easier to perceive. For consistency, use "state variable" instead of "system state" in the caption."

  AR: The error bands (standard error) are explained in the revised manuscript. The term "system state" is replaced by "state variable".

- **RC:** "Fig. 5: Explain the error bands."

  AR: The error bands (standard error) are explained in the revised manuscript.

- **RC:** "Fig. 6: The y-axis labels FT and FW should be fT and fW. In the legend of the figures in the lowest row, the colour of the line showing "modelled" FG should be black."

  AR: The labels are corrected. The grey line is changed to a black line in the new figure's (now Figure 7) legend.

- **RC:** "Fig. 7: Tell what characteristic (sum of squared errors between measured and modelled FG?) the different classes (colours) of the points represent. Tell to which sites (b) and (c) pertain (Wetzstein and Lethbridge, respectively?). Refer to this figure in the text (now no reference is found)."

  AR: The performance criteria is specified (sum of squared errors (SSE) with respect to measured and modelled F_G-values), the colors (indicating classes with respect to the SSE ranking) are explained. The sites are explicitly stated for each plot. The figure is referenced in the text (now Figure 10).

- **RC:** "Fig. 8: In the legend, consider renaming $\mathrm{EF_I}$ into something that more resembles I_W. The numbers in x-axis in (b) should be integers (number of years). Are the y-axis values really confidence intervals and not standard errors?"

  AR: $\mathrm{EF_I}$ is replaced (W_I). The numbers of the x-axis in plot b are converted to integers in the figure (now Figure 9). The term confidence intervals is correct (see RC #10).

- **RC:** "Fig. 9: Refer to this figure in the text (now no reference is found). Explain the abbreviations of the vegetation types and climate classes; you can refer to the caption of Table 1, if you moved the description there. For better readability, centre the labels of the climate classes and vegetation types (cf. Fig. 3)."

  AR: The figure (now Figure 8) is referenced in the text, a reference is given for the abbreviations. The labels are centred.

- **RC:** "Fig. 10: Explain the abbreviations of the vegetation types and climate classes; you can refer to the caption of Table 1, if you moved the description there. For better readability, centre the labels of the climate classes and vegetation types (cf. Fig. 3)."

  AR: A reference is given for the abbreviations in the figure (now Figure 11). The labels are centred.

- **RC:** "Equation 3: The differences in the sums in the numerator and denominator should be squared. Some other index than i might be preferable, in order to avoid confusion with Equations 1 and 2."

  AR: The equation for EC (now Eq. 6) is corrected, the index i renamed (j).

- **RC:** "Equation 4: Add the error series $\zeta(t)$."

  AR: We use $\zeta(t)$ only for SDP models, in which the error series is taken into account explicitly. The mentioned equation, however, is the general equation of the "Jarvis-model".

- **RC:** "Equation 5: Use TF(t) instead of TF (cf. Equation 6)."

  AR: $T_F(t)$ is used (now Eq. 8)

- **RC:** "Equation 6: What is the starting value of the TF(t) series?"

  AR: The starting value is specified in the revised manuscript (mean of the first 30 days of T_S; p. 13, r. 18).

- **RC:** "Equation 7: Consider using a different notation (e.g. with a subscript) for the error series $\zeta(t)$, as it is not the same as that in Equation 4."

  AR: We have not added a subscript since we would have to use a new subscript in all SDP and DLR equations; this would be at the expense of readability and clarity. We note this in the revised manuscript (p. 10, r. 25ff).

- **RC:** "Equation 9: For correspondence to the general SDP model definition and for better readability, consider writing the explicit products of state-variable-dependent parameters and APAR as

  $$F_G(t) = c_1[T_S(t)] \cdot APAR(t) + c_2[W(t)] \cdot APAR(t) + \zeta(t) \quad (1)$$

  and stating that now $\varepsilon(t) = c_1[T_S(t)] + c_2[W(t)] =$.

  AR: The equations are shown in a form with brackets and in a form factored out (Eq. 12, 13). The term describing $\varepsilon(t)$ is explicitly given (Eq. 14).

- The typing and language errors pointed out by the referee are corrected.

References

Acevedo, O., Moraes, O., Degrazia, G., Fitzjarrald, D., Manzi, A., and Campos, J.: Is friction velocity the most appropriate scale for correcting nocturnal carbon dioxide fluxes?, Agricultural and Forest Meteorology, 149, 1–10, 2009.

Coleman, T. and Li, Y.: On the convergence of reflective Newton methods for large-scale nonlinear minimization subject to bounds, Mathematical Programming, 76, 189–224, 1994.

Desai, A., Richardson, A., Moffat, A., Kattge, J., Hollinger, D., Barr, A., Falge, E., Noormets, A., Papale, D., Reichstein, M., et al.: Cross-site evaluation of eddy covariance GPP and RE decomposition techniques, Agricultural and Forest Meteorology, 148, 821–838, doi:10.1016/j.agrformet.2007.11.012, 2008.

Falk, M., U, K. T. P., Wharton, S., and Schroeder, M.: Is soil respiration a major contributor to the carbon budget within a Pacific Northwest old-growth forest?, Agricultural and Forest Meteorology, 135, 269 – 283, doi:10.1016/j.agrformet.2005.12.005, 2005.

Garbulsky, M., Peñuelas, J., Papale, D., Ardö, J., Goulden, M., Kiely, G., Richardson, A., Rotenberg, E., Veenendaal, E., and Filella, I.: Patterns and controls of the variability of radiation use efficiency and primary productivity across terrestrial ecosystems, Global Ecology and Biogeography, 19, 253–267, doi:10.1111/j.1466-8238.2009.00504.x, 2010.

Horn, J. E. and Schulz, K.: Identification of a general light use efficiency model for gross primary production, Biogeosciences Discussions, 7, 7673–7726, doi:10.5194/bgd-7-7673-2010, 2010.

Jarvis, A. J., Stauch, V. J., Schulz, K., and Young, P. C.: The seasonal temperature dependency of photosynthesis and respiration in two deciduous forests, Global Change Biology, 10, 939–950, doi:10.1111/j.1365-2486.2004.00743.x, 2004.

Moffat, A. M., Papale, D., Reichstein, M., Hollinger, D. Y., Richardson, A. D., Barr, A. G., Beckstein, C., Braswell, B. H., Churkina, G., Desai, A. R., Falge, E., Gove, J. H., Heimann, M., Hui, D. F., Jarvis, A. J., Kattge, J., Noormets, A., and Stauch, V. J.: Comprehensive comparison of gap-filling techniques for eddy covariance net carbon fluxes, Agricultural and Forest Meteorology, 147, 209–232, doi:10.1016/j.agrformet.2007.08.011, 2007.

Papale, D., Reichstein, M., Aubinet, M., Canfora, E., Bernhofer, C., Kutsch, W., Longdoz, B., Rambal, S., Valentini, R., Vesala, T., et al.: Towards a standardized processing of Net Ecosystem Exchange measured with eddy covariance technique: algorithms and uncertainty estimation, Biogeosciences, 3, 571–583, doi:10.5194/bg-3-571-2006, 2006.

Stauch, V. and Jarvis, A.: A semi-parametric gap-filling model for eddy covariance CO2 flux time series data, Global Change Biology, 12, 1707–1716, doi:10.1111/j.1365-2486.2006.01227.x, 2006.

Thornley, J. and Johnson, I.: Plant and crop modelling. A mathematical approach to plant and crop physiology, Blackburn Press, New Jersey, USA, 2002.

Yuan, W. P., Liu, S., Zhou, G. S., Zhou, G. Y., Tieszen, L. L., Baldocchi, D., Bernhofer, C., Gholz, H., Goldstein, A. H., Goulden, M. L., Hollinger, D. Y., Hu, Y., Law, B. E., Stoy, P. C., Vesala, T., and Wofsy et al., S. C.: Deriving a light use efficiency model from eddy covariance flux data for predicting daily gross primary production across biomes, Agricultural and Forest Meteorology, 143, 189–207, doi:10.1016/j.agrformet.2006.12.001, 2007.