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Biogeosciences
Discussions

Interactive comment on "Constraining a global ecosystem model with multi-site eddy-covariance data" *by* S. Kuppel et al.

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The authors would like to thank the Reviewer for the very helpful comments and suggestions. The comments have been taken into consideration in the revised manuscript. We answer them individually in the following.

1) General Comments

In this article the use of eddy covariance observations of latent heat (LE) and net ecosystem exchange (NEE) to constrain the parameters of a land-surface model (ORCHIDEE) is shown. Most of the article is well written and the results, especially the comparison of multi-site with single-site data assimilation, are a significant contribution to the use of eddy-covariance data to constrain global carbon cycle models and hence the article is suitable to be published in Biogeosciences. Nevertheless the manuscripts needs to be clearer in the description of some methodological aspects and – to my understanding - some results deserve further explanation and discussion (details are given in the 'Specific comments'-section). After the authors have considered these points, I suggest to accept this manuscript to be published in Biogeosciences.

2) Specific comments

Some methodological aspects regarding optimization, uncertainties and parameter values need further descriptions: Those observations with less then 20% gaps are used to aggregate to daily averages. Might this approach introduce biases to the data (e.g.: given that gaps might be unevenly spread over the course of a day)? And are there other sources of potential observational biases (e.g. the non-closure of the energy balance would bias LE). The authors should comment on how they dealt with those issues.

As now added in the revised text, we checked that the observation gaps are somewhat evenly spread along the day, so that we assumed that no significant bias would derive from this averaging method. We modified the sentences in section 2.2:

'The remaining gaps in observations are distributed somewhat evenly along the courses of the day. Note that individual days with more than 20% of missing half-hourly observations were not included in the assimilation.'

The non-closure of the energy balance is indeed a potential source of bias for LE, possibly altering the reported general prior overestimation by the model. Note that we recently used energy-balance-corrected flux using the Bowen ratio at the sites where the ground heat flux was available, and the results were not significantly affected. Nonetheless, in the conclusion we have tempered our results regarding energy balance parameters. We added the following sentences:

'Regarding the water cycle, the prior model generally overestimates the latent heat flux, and both MS and SS optimizations generally improve the model-data fit with a reduced stomatal conductance and a larger vegetation albedo. This result should however be tempered given the lack of energy balance closure at the sites, a potential source of bias in the measurements values of LE.'

The description of the data covariance matrix R could be clearer (p. 3323/15-18). Have all the diagonal elements of R the same number per data stream (hence one uncertainty for NEE and one for LE?). This would contradict the statement of Richardson et al (2008) stated a few lines above. And it is not clear to me, how observational uncertainties of the eddy covariance measurements have been taken into account. This should be described more precisely.

Indeed one uncertainty is defined for each type of observation for the whole period. However, these uncertainties do vary between sites; we made this clearer in the manuscript. Because we assume that model uncertainties are much larger than the eddy-covariance measurement error, we chose to neglect the latter in the total observation error (model+measurements), also because Lasslop et al. (2008) showed that the measurement error correlation structure is negligible on a daily timescale. Our choice of constant error in time is thus linked to our ability to characterize model errors. Thus we see no actual contradiction with Richardson et al. (2008) who only dealt with the (random) measurement error. For simplicity, the paragraph describing the construction of R has been moved to section 2.3, where the data assimilation is described. It is now:

'Regarding the observational error statistics, the error covariance matrix R should include both the error on the measurements and the error on the model process representation. On the one hand, the random measurement error on the observed fluxes is known not to be constant and can be estimated as the residual of the gap-filling algorithm (Richardson et al., 2008). On the other hand, model errors are rather difficult to assess and may be much larger than the measurement error itself. Therefore, we chose to focus on the model error whose correlations cannot be neglected (Chevallier et al., 2006). The difficulty of evaluating the structure of the model error leads us to keep R diagonal and, as compensation, artificially inflate the variances (Chevallier, 2007). First, the variances in R are defined as constant in time for each type of observation (NEE and LE). Second, their values are chosen based on the mean squared difference between the prior model and the observations, multiplied by the inflation factor k_σ, which represents our estimation of the characteristic autocorrelation time length (in days) of the model error. The value of k_σ is fixed to 30, which for the error propagation is equivalent to assimilating one observation every 30 days.'

The definition of uncertainty in the results section on the model-data misfit (section 3.1.1) confused me. It is not fully clear to me how the model uncertainty has been calculated. And to my understanding, the model-uncertainties already have been incorporated in the observational uncertainties (as model-data mismatch) and hence it is already part of the posterior parameter uncertainties. Could the authors comment why they again account for the model-data mismatch?

We calculated the model uncertainty using a posterior analysis of the errors statistics at each site. More specifically, we make use of Eq. (3) in Desroziers et al. (2005) to derive the standard deviation of the observational error (**R**) at each site, assumed equal to the model error (see previous comment). We added this to the text. Note that we feel confident in this method as the model error estimations brought at each site by

MS and SS optimizations (ideally identical) do not differ by more than 10%.

Concerning the total uncertainty used in the error bars, we disagree with the last part of the referee's comment: the model uncertainty is not affected by the optimization and should be present in the error budget together with the parameter uncertainty, in both prior and posterior cases. Indeed the model uncertainty has been integrated in the assimilation procedure and eventually in the parameter uncertainty, but only as long as we remain in the parameter space. The later projection in the observation space to calculate σparam requires the model sensitivities (the Jacobian) but the latter can be 'independent' from the model used for assimilation, as we could propagate the parameter uncertainties of one assimilated model into another model. As in our case the optimized model is the same as the 'uncertainty-propagation model', we account for this 'same' model error in the observation error budget. This part of the paragraph is now:

'The former is calculated using the parameter error covariance matrix and Jacobian matrix of the model, similarly to Eq. (25) in Rayner et al. (2005) (assuming linearity at the mininmum of the cost function). The model error is reported at each site as a standard deviation from the statistical analysis of the prior and the posterior residuals (model-minus-observations), following Eq. (3) in Desroziers et al. (2005). The all-site average values are estimated to be 1.8 gC/m^2/d (NEE) and 24.1 W/m^2 (LE).'

The authors should explicitly name the parameters for which a finite difference scheme has been applied and some details on the 'finite-difference' algorithm should be provided. The authors might also discuss why they think the mixture of two methods to calculate the gradient is appropriate.

The two parameters for which the finite-difference approach has been used are those controlling the temperature dependence of foliage onset and senescence, $K_{phenocrit}$

and T_{senes} (formely $c_{T,senescence}$), both linked to a threshold function. In these cases we compute the slope of the cost function when applying a perturbation equal to 4% and 2% of the allowed range of variation, respectively. These values were derived from prior sensitivity analyses to assess the smallest perturbation not subject to numerical errors. To our view, combining the two methods only result in a general smoothing of the functions related the optimized parameters. We added this to the text:

'Examples include the threshold functions controlling the temperature dependence of foliage onset and senescence (parameters $K_{phenocrit}$ and T_{senes}, see Table 2). In both cases we use a finite difference approach with prescribed perturbations steps respectively equal to 4% and 2% of the allowed variation range. These values were derived from prior sensitivity analyses to assess the smallest perturbations not subject to numerical errors.'

How have the authors assessed that the assimilation finds a global minimum of the cost-function and not a local one?

We cannot directly assess that we found a global minimum, and there are indeed chances that our gradient-based variational method ends up in a local minimum, given the non-linearities of the model and the fact that we start from a single prior set of parameters. However, the large number of optimized parameters (14 to 32) makes an exhaustive exploration of the space of solutions a daunting task (the infamous 'curse of dimensionality') where computational resources rapidly become the limiting factor. This is why, instead of ensemble methods, we chose to use a gradient-based variational technique which provides a fair convergence of the cost function after a reasonable time of calculation (e.g. one day for one MS optimization). The consistency between SS and MS optimizations further hints towards a robustness of the convergence of our algorithm, and we checked that multiplying the number of iterations by three had a negligible effect on this matter. We have modified the text to explicitly raise some caution about finding the global minimum:

'Besides, we acknowledge that the optimized parameter sets might still correspond to local minima. Ensemble methods could possibly provide a better model-data fit, but the relatively large number of optimized parameters makes our variational method much more affordable in terms of computational time.'

The modelling protocol needs to be more detailed in the way the spin-up has been performed. Has the model been spun up in each iteration of the assimilation or has one single spin-up been used (and with which set of parameters)?

The spin up is performed only once at each site, using the prior parameterization of the model. Given that we optimize a scaling coefficient of the initial carbon pool sizes (K_{soilC} in Table 2), the spin up is only to provide a first rough estimate of the three soil carbon pools. We added this information to the revised manuscript:

'Importantly, the modeled carbon pools are initially brought to equilibrium by cycling the available meteorological forcing over a long period (1300 years), with the prior parameterization of the model. It ensures a net carbon flux close to zero over annual-to-decadal timescales.'

The authors should describe where from the a-priori uncertainties and upper and lower bound of the parameters are taken and it is not fully clear to me, which parameters have been constrained (e.g.: referring to eq 13 - 15; is $b_{T,min}$ part of the optimization or not?). The values used for the non-constrained parameters should also be given alongside with a statement why those parameters are not considered (e.g.: α_p and τ_p on p. 3331/1).

The boundaries for the optimized parameters were chosen following ORCHIDEE's modelers' physical and empirical expertise. Most equations are empirical and their parameters are usually derived from an ensemble of in situ or laboratory measurements with admitted range of variations following these experiments/observations. For instance, the TRY database (Kattge et al., 2011) provides some indication for common values of photosynthetic and phenology parameters. We added this information in the description of the data assimilation system (3324/10-12) with the following sentences:

The standard deviation for each parameter used for variances is equal to 40% of the range between lower and higher boundaries, which have been carefully specified following the physical and empirical expertise of ORCHIDEE modelers, based on literature reviews or databases (such as TRY, Kattge et al., 2011) providing estimated parameter values from in situ or laboratory measurements.'

All the constrained parameters are listed in Table 2. Also, we realized that for the PFT studied here, $a_{T,min}$, $b_{T,min}$, $a_{T,opt}$, and $b_{T,opt}$ (not optimized) are equal to zero. Thus Eqs 14 and 15 have been removed and the parameter $c_{T,min}$ and $c_{T,opt}$ are now simply identified to the temperatures T_{min} and T_{opt} (see Eq. 13), respectively. Also, we modified the text in the manuscript, below Eq. (13):

'In this study, only the parameters T_{min} and T_{opt} are optimized, as we found little sensitivity to the value of T_{max} (fixed to 38° C) in preliminary tests.'

As a general approach, we restricted the optimization to the parameters that are the most sensitive with respect to the daily NEE and LE fluxes. Following Santaren et al. (2007), we have identified the parameters listed in Table 2 as the most critical ones. However, the model contains a much larger set of parameters that could also have been optimized. Our choice was guided by the fact that adding parameters with only little influence on the selected outputs may hamper an efficient convergence of the optimization algorithm. The value for most parameters can be found in Krinner et al. (2005), the reference paper of the ORCHIDEE model, or in a few other articles describing specific modules. For instance, and as specified in the manuscript, the values of pool-specific repartition coefficients α_p can be found in Parton et al. (1988), and so can the characteristic decomposition times τ_p, as our TEM soil dynamics is based on the CENTURY model of Parton et al. (1988). These parameters were not optimized given their smaller influence on the day to day variations of the NEE, compared to the selected ones (Q10, . . .). Note that our selected set allowed a significant improvement in the modeled heterotrophic respiration.

As for $c_{0,i}$ (g/g/day), this coefficient is prescribed per PFT and its value also depends on the biomass compartment considered: 2.62×10^{-3} (leaf), 1.19×10^{-4} (sapwood, fruits, and carbohydrate reserve), 1.67×10^{-3} (roots) and 0 (heartwood). This parameter was not optimized also given the number of different components, and additionally because the temperature relationship (with MR_a and MR_b) is already a multiplicative factor for the maintenance respiration. We modified the text below Eq. (10), which now reads :

'...where T_i, B_i and LAI are respectively the soil or surface temperature, the biomass content and the leaf area index, while $c_{0,i}$ (g.g^{-1}.day^{-1}) is the maintenance respiration coefficient at 0°C, which is prescribed depending on the PFT and the biomass pool i: 0 (heartwood), 1.19×10^{-4} (sapwood, fruits, and carbohydrate reserve), 1.67×10^{-3} (roots) and 2.62×10^{-3} (leaf). MR_a and MR_b are two critical parameters that are optimized (Table 2).'

$G_{s,c}$ is not optimized because $G_{s,slope}$ has a much greater leverage on the stomatal conductance relationship as it relates directly with the GPP.

The results are well presented, but they sometimes lack a profound discussion:

The authors state unfavorable model performance at various sites (p. 3338/25–28 and p. 3339/12) and for the global model runs (p. 3334/last paragraph and p. 3346/23-26). Do the authors have any specific ideas why the model does not perform so well in these cases?

- 3338/25-28: DK-Sor is the only site where GPP and Reco are underestimated by the prior model, so that the parameters sets optimized at others sites, which further decreases these two fluxes, cannot improve (and sometimes even degrade) the NEE fit at this site. Regarding LE, the overestimation of LE at DK-Sor is less pronounced than at most of the sites, but overall the peculiarity of DK-sor is less obvious, thus in the revised manuscript we have focussed our discussion on NEE:

'Besides, the transposition of SS parameters to other sites also gives hints regarding which sites do not 'fit' in the group here studied: for example, the Sorø forest site (DK-Sor) shows high RMS for NEE whenever optimized parameters from a different site or MS parameters are used. It shows that this site is in some way atypical relative to the other deciduous broadleaf forest sites in our analysis, and Fig. A2 suggests that this is because only at this site both GPP and Reco fluxes are underestimated by the prior model, calling for corrections opposed to the general trend observed throughout this study.'

- 3339/12: Fig. A5 shows that UK-Ham is one of the few sites (with DK-Sor) where GPP is underestimated by the prior model, while Reco is only slightly overestimated, finally resulting in a large underestimation of the annual net carbon sink. Thus the reductions of GPP and Reco are respectively erroneous and too large, although the NEE is consistently corrected. The UK-Ham set of single-site-optimized parameters brings more consistent results, and we can infer that this is reflected in the mean set of optimized parameters, hence the better performance as compared to the MS optimization. Regarding LE, US-UMB (not US-WCr, this has been corrected) is the only site where this flux is slightly overestimated by the prior model (Fig. A11) and at FR-Fon the overestimation is lower than at most of the sites (Fig A3). The MS optimization goes in the direction of a systematic reduction at all sites (from the three generic energy-balance parameters) which does not fit these two sites, whereas we can suppose that in the mean set of parameters the peculiarities of the two sites are better taken into account to temper the LE-reducing trend. The manuscript has been modified accordingly:

'Finally, there are a few sites where the mean set of parameters does better than the MS optimization for one, but not both, fluxes: i.e., UK-Ham for NEE, FR-Fon and US-UMB for LE. We can notably notice that UK-Ham is one of the very few sites where the prior GPP is underestimated (Fig. A6), while at FR-Fon and US-UMB the prior LE is either commensurate with observations or even underestimated – as compared to a general overestimation. MS corrections might thus be inconsistent with the local peculiarities, and Fig. 3 shows indeed that in these three cases, the MS RMS for the corresponding flux is significantly larger than the SS one. We conclude that these sites might not fit in the multi-site group with respect to the mentioned fluxes, so that a site-specific parameterization would be needed here.'

- 3343/last paragraph and 3346/23-26: Some regions (e.g. Australia) present a decrease in the correlation between NDVI and fAPAR after the parameter optimization. Several reasons can explain this behavior. First, no site was available in Australia for the optimization of the chosen PFT, temperate deciduous broadleaf forest (DBF). Second, the tree species in this region, classified as DBF, are likely to be more specific of dry regions, with a strong control of the phenology by the available soil moisture, a feature much less prevalent at the northern hemisphere sites used in this study. Such results may suggest that the current PFT classification is not adapted and should be further refined to distinguish broadleaf deciduous tree phenology that is more strongly controlled by soil moisture, like in Australia. However, such work is beyond the scope of this paper and we have thus only slightly modified the text in the conclusion to highlight the need for further investigations:

'From this starting point, the MS optimization brings a slight improvement in the northern hemisphere, and contrasting results in the southern hemisphere, where none of the sites used in the optimization are located: significant improvement in South Africa but degradation in Australia. At the global scale, the correlation median shifts from 0.83 to 0.88. The degradation in Australia might reflect the limits of the phenological scheme of deciduous forests in the model, solely based on a temperature criterion. The tree species in the arid Australian forests, although classified as DBF, are likely to have a phenology strongly controlled by the available soil moisture, a feature much less prevalent at the sites used in this study. At present, we can only suggest the need for further investigations regarding the formulation of the DBF phenology in the model, towards a refinement of the PFT classification.'

Model structural errors are reported (p. 3345/9). Could the authors give any ideas what these structural problems might be?

In the ORCHIDEE vegetation model, the adequate simulation of the carbon assimilation remains limited by the fact that for example the temperature and soil hydric stress controls on photosynthesis are still rather empirical and represented with a simple function (linear from 'no stress' to 'full stress'), and in addition no biotic impact is considered. This comment has been added to the text:

'Comparisons with estimations of GPP derived from NEE indicate that this correction is somewhat relevant, but the summer carbon uptake remains underestimated at half of the sites after optimization, suggesting model structural errors. The latter could be related to the fact that for example the temperature and the soil-water control on the photosynthesis are still simply parameterized, and that no biotic effect is taken into account.'

The use of only LE observations as constraint degrades the modelled NEE (sec-

tion 3.4), while using only NEE as constraint not affects modelled LE. A description or some ideas of what causes this behaviour of the assimilation system should be given.

Conceptually the LE flux is tightly linked to the GPP through the stomatal conductance; thus any constrain on the GPP (through the assimilation of NEE daily values) will also constrain the LE flux. On the other hand the NEE is the sum of GPP and R$_{eco}$, with thus a respiration term that is completely independent of the LE flux. In this study, the general decrease in the ecosystem respiration is the main driver of the improvement in NEE modeling, together with a milder overall reduction of GPP. When LE data is used alone, the unconstrained R$_{eco}$ combined with the reduced GPP (via the reduced stomatal conductance, due to overestimated prior LE) results in NEE values higher than the in the already-overestimating-NEE prior model, thus degrading the fit. Reversely, the assimilation of the sole NEE barely affects the energy balance parameters as compared to the prior model. Notably, $G_{s,slope}$ is only slightly increased, most probably due the error anticorrelation with the (reduced) $V_{cmax,opt}$. In the end, the modeled LE flux barely varies from the prior state. We have added this discussion to the text:

'Regarding the fit to NEE, the performance of the optimization is very similar at most sites whether LE data is used or not, whereas using only LE data results in a significant degradation of the NEE model-data fit from the prior in most cases (averaged RMS increased by 22%). This degradation stems from LE having no leverage on the modeled R$_{eco}$, while the assimilation of LE still decreases GPP from the prior model, via the reduced stomatal conductance. This difference leads to higher NEE values than for the already-overestimated prior NEE, thus degrading the fit.'

Finally the pages 3341/11-29 & 3342/1-5 (discussion of heterotrophic and autotrophic respiration at Hesse) of the manuscript could be omitted, since I don't see what additional information to support the main conclusions are given. Oth-

erwise it might be discussed, why the estimates for GPP and R$_{eco}$ (and their difference) in the two data sets are different and what are the uncertainties of these estimates. For comparison, also the uncertainties of the a-posteriori modelled flux should be provided. And the authors should explain why they think modelled R$_a$ and R$_h$ are consistent with the estimates of Granier et al. (2008), especially because the ratio of the two is rather different.

Indeed we realize that this paragraph lacks spatial representativity in order to make a robust point out of the data of Granier et al. (2008) at Hesse site. Following your suggestion we have omitted this paragraph in the revised manuscript, and only mentioned that such in-depth comparison with site-specific studies is beyond the scope of the paper but would need further attention in a more systematic way at all sites:

'In-depth comparisons with site-specific gross flux estimates at each site (e.g. Granier et al., 2008) is beyond the scope of this paper but would deserve further attention for a more precise evaluation of the optimization procedure at all sites.'

Some further minor issues as listed below might also be considered by the authors:

p. 3319/7: I suggest to also add Baldocchi et al. (2001, 2008) to the references to FLUXNET.

The citations have been added to the text.

p. 3322/12: How have the meteo-data been gap-filled (or where from taken)?

The meteorological data is taken from the standard Fluxnet La Thuile dataset, with the meteorological gap-filling procedure notably described in Moffat et al. (2007)

p. 3322/24: Any particular reason to take 70%?

All the sites where there is a dominant PFT (above 50%) happen to have this percentage between 70 and 80% (according the retrieved PFT classification).

p. 3322/27: Where from is the gap-filling for the FLUXNET sites taken.

We use the standardized Lathuile dataset gap-filling processing, which combines the artificial neural network (Papale and Valentini, 2003; Moffat et al., 2007) and the marginal distribution sampling (Reichstein et al., 2005; Moffat et al., 2007) methods. We modified the text in the revised manuscript:

'The eddy-covariance flux data used are part of the FluxNet network, with standard flux data processing methodologies (correction, gap-filling and partitioning) of the La Thuile dataset (Papale et al., 2006; Reichstein et al., 2005). The gap-filled measured fluxes of net ecosystem exchange (NEE) and latent heat fluxes (LE) at half hourly time step are used to compute daily means.'

Figure 2: It should be made clear, that the brackets give the uncertainties of the annual averages.

From the daily uncertainty, the propagation to annual budgets/averages requires a precise knowledge of the temporal structure of the error in the modeled fluxes, which is still poorly known. For this reason, we decided limit restrict the calculation of uncertainties to the daily timescales.

p. 3329/8 -10: This statement about importance of the seasons is related to the relative error reduction. The absolute reduction shows an as important contri-

bution from the summer. This should be made clearer.

It has been specified in the revised manuscript with the new sentence:

'More specifically, the best relative improvement of the fit happens in winter (Fig. 3, second row), where the RMS is reduced by an even larger amount (MS: -55%, SS: -69%).'

p. 3332/20-22: It should be made clear if LE-observations have been incorporated in this assimilation or not.

In this sentence, we meant that using only NEE and LE observations does not allow full separation of temperature and humidity impacts on the heterotrophic respiration. The text has been modified to clarify this point with:

'This indicates that using only NEE and LE measurements does not allow full separation of temperature and humidity impacts on R_h.'

p. 3336/1-7: This should be explained in some more details. Especially the fact that excluding Ra-parameters from optimization should alter GPP estimates. I think this is rather difficult to follow for someone not very familiar with optimizations and hence needs more explanations.

In the revised manuscript we have completed the text to be clearer in that regard. The new text is:

'We checked whether leaving R_a parameters out of the optimization would lead to an increase of GPP (as the NEE constrain remains the same in the inversion system, but with an higher modeled R_a), yet the effect is rather weak: the average annual assimilated carbon changes by less than 2% (not shown), with a substantially worse fit to summertime NEE. We deduce that climate dependencies of photosynthesis and autotrophic respiration are different enough so that the two processes can be distin-

guished in the ORCHIDEE model with only daily mean NEE data.'

p. 3343/8: What might be the effect of this 50% threshold? Could large part of the discrepancy between model and satellite observations arise from the remaining part of the grid-cell?

In the cells grid dominated by the DBF ecosystem, the remaining PFT is mainly C3 understory (i.e. grasses under the trees) with thus a small contribution to the NDVI signal, as seen from space. As preliminary tests, we performed the same correlation procedure using different value of the thresholds, for example 80%. Although there are a smaller number of available points with an 80% threshold, the results are not significantly changed. We thus chose to keep the 50% threshold in order to retain enough points for statistical significance.

Conclusion: The term 'globally', often used in the conclusion, is somewhat misleading, since only some sites in the Northern Hemisphere have been studied.

This term has been replaced in several places in the conclusion by :

- *'The autotrophic respiration (R_a) is also generally reduced after optimization...'* -
- *'Comparisons with estimations of GPP derived from NEE indicate that this correction is somewhat relevant, but...'* -
- *'Regarding the water cycle, the prior model generally overestimates the latent heat flux, and both MS and SS optimizations generally improve the model-data fit...'*

p. 3345/18-20: This statement is not relevant for the presented work, especially having in mind that the cited manuscript is not yet published. I ask to omit this statement.

This reference has been deleted.

Figure A13: The authors should specify - in the figure caption - from which run the posterior covariances are taken (MS or SS – which site?).

The legend specifies that these correlations are those of the MS optimization, all sites involved (we notably display the K_{soilC} components of all sites). We have modified the caption to reflect that more clearly:

'Figure A13. All-site MS posterior parameter error correlation matrix.'

3) Technical correction

p. 3319/24: 'rather difficult' is a very subjective term.

This term has been suppressed, and the sentence modified to:

'In this context, the choice of a representative value for each parameter becomes a critical step that might add significant error to the simulated fluxes for a given PFT.'

p. 3331/8 (equation 6): $C_{p,soil}(t0)$ appears on each side of the equation.

Eq. (6) has been corrected to: $C_{p,soil}(t0) = C_{p,soil}^{spinup}(t0) \times K_{soilC}$

p. 3339/12 I think US-WCr should be US-UMB.

Corrected.

p. 3339/17: Should be LE not LEE

Corrected.

p. 3343/13: The boxes are in orange not in grey.

Corrected.

p. 3345/8: '... still does not go deep enough ...': Is this correct English?

We have replaced it by *'remains underestimated'* in the manuscript.

p. 3345/24: How big is 'abnormally'?

This term is indeed imprecise, this part of the sentence has been changed to *'...we observed highly fluctuating modeled values of LE in winter at some sites in contradiction with observations, most likely...'*.

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