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Discussions

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

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General comments:

Kuppel and colleagues advance from previous approaches to optimize process-oriented terrestrial ecosystem models (TEMs) against eddy covariance data by calibrating sets of common parameters simultaneously for several sites. Most TEMs rely on the plant functional type (PFT) classes to prescribe parameter vectors (x) that control functional responses of carbon and water fluxes to environmental drivers. However, once multiple sites of the same PFT are used independently to estimate x it is common that these are often different. By performing multi-site (MS) optimizations, the current study circumvents the need to aggregate x or to regionalize x according to any factors other than PFT classifications and shows that the model performance does not decrement significantly to single-site (SS) optimizations. In some cases it even improves. It is then an approach that has significant advantages regarding practical applications in parameter regionalization and parametric uncertainty propagation. It also holds promise for deeper analysis of the PFT concept in TEMs as the most important parameter vector “covariate”.

The current manuscript is a robust piece of work that in my view only fails in clarifying and corroborating certain methodological options:

– By defining a set of common parameters the authors implicitly assert them as linked to the plant functional type. Since these range between vegetation to soil and energy balance parameters, the authors should clarify if this was purely a practical decision (because in TEMs parameters are prescribed per PFT). If so, discuss limitations given some soil water availability parameters would also vary between sites or the soil decomposition parameters could be widely considered constant.

– The “performed optimizations” are more than just MS vs SS, since the authors also explore the role of individual data streams (LE and NEE jointly and separately) and do heterotrophic respiration experiments – which are seen first only here. Some introduction/motivation should be also given previously about these experiments.

Regarding the evaluation of the different optimization exercises:

– A table on model performance for the different optimizations (MS versus SS) would be very helpful in synthesizing the current results. In this regard the current exercise is solely based on the RMS metric to evaluate the model performance at site level. Other metrics like correlation or model efficiency which translate changes in model behavior could also help understand if the improvements would also be paralleled by changes in model sensitivities, specially since for some sites the parameter differences between SS and MS is very significant.

– Given the importance of KsoilC in the MS and SS optimizations it could be relevant to compare modeled estimates of soil C pools against site level observations at the sites [e.g. Schrumpf et al., 2011] as an independent data source that would corroborate the different approaches and the heterotrophic respiration experiments.

– Since MODIS products also include LAI and FPAR products, long with NDVI and EVI, it would be good to justify why no direct comparisons between the same quantities (LAI or FPAR) were performed.

Particular comments:

P3319, L14-17: Liu and Gupta [2007] refer the initial states as another source of uncertainties / mismatch between model and observations.

P3323, L4-5: how much data was it actually used for all sites and individually per site?

P3323, L6-18: from the construction of R described in this section it is not clear how different are the observational and prior model error and what are the properties and distributions of the model errors used in R (e.g. is the error heteroscedastic? What the mean error is in NEE and LE to the cost function? Does it also vary by site?). It is also not clear the role of the factor $k\sigma$ in the construction of R...

P3325, L9-11: analogous to Carvalhais et al. [2008].

P3327, L5-6: but performance statistics are computed on daily data?

P3332, L2-8: a small table showing the reductions in RMS according to the experimental setup would be very helpful in grasping these more objectively.

P3332, L20-22: could also occur because of the correlation in the drivers.

P3333, L15-16: known as the equifinality problem [see for example Franks et al., 1997; Medlyn et al., 2005].

P3336, L1-7: the Ra parameters also include a scalar that implicitly scales Ra to mismatches in vegetation biomass GRfrac, which could explain the summer mismatch in NEE if Ra is in general overestimated during this period. We should see that this de-

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duction is very linked to the model structure since Rg does not seem to depend on instantaneous productivity, as is usually assumed in other models.

Also here we see c0,i,j – how does this coefficient vary?

P3336: Shouldn't the energy balance parameters be site dependent?

P3338, L14: "green" should be grey.

P3338, L16: "purple" should be black.

P3340, 3.5.1.: Different methods to decompose NEE into GPP and RECO have been compared before [Desai et al., 2008]. These would probably stand as a better benchmark for the modeled fluxes here. The comparison shown here is very oriented to a site where the actual NEE from both approaches is very different (Granier: -289 gC m-2 yr-1 against -562 gC m-2 yr-1 from the "Lathuile" dataset). Another point to consider would be to see if the uncertainties from both flux partitioning and modeled fluxes intersect.

P3342, L6-9: this could be something very specific to the structure of ORCHIDEE (see above) and see Keenan et al. [2012].

P3343, L2-4: since the (mis)matches in NDVI/FPAR time series stemming from the land cover component are already considered (P3343, L4-10) the ignored cells for comparison should only target those where no seasonal cycle is seen in the observational data, and not in the observational and model data. The comparisons would be biased optimistic by excluding cells with no seasonal cycle from model outputs that might have it in the observations.

P3343, L13: "grey" should be yellow.

P3345, L18-20: Could the link between the current work and the work of Santaren et al. [2012, which is not published yet] be more concrete?

P3345, L24-27: would it be expected that the bias in snow sublimation propagate to

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the current parameter sets?

P3346, L15-26: It seems also that the DBFs in the Northern Hemisphere are in much colder regions than in the Southern Hemisphere. The sites considered are between 36° and 56°N, and only 2 sites below 42. There is no indication if the mean annual temperature (or any other climate diagnostic) would differ significantly this optimization convergence would occur. Could this be given as a reason behind the different conclusions in this study and Groenendijk et al. [2011]?

P3356, Table 2: the addition of a column with the posterior parameter values and uncertainties would be very useful.

P3359, Figure 2 (and beyond): no uncertainties in the data? In this case (Fig. 2b NEE) we also see that the MS optimizations perform better than the SS. It seems to happen in some cases. The reason this is happening could be related to the uncertainties included in the cost function (R), which vary between SS and MS settings.

P3361, Figure 4: Does KsoilC correlate with NEE?

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