

Interactive
Comment

Interactive comment on “Model–data fusion across ecosystems: from multi-site optimizations to global simulations” by S. Kuppel et al.

Anonymous Referee #1

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This study presents results of parameter optimization experiments of the ORCHIDEE model and evaluates global model results against observed data. Specifically, parameters that affect photosynthesis, phenology, respiration, soil moisture and energy balanced were optimized against site measurements of net ecosystem exchange (NEE) and latent heat (LE) using two different approaches. In the first approach, parameters were estimated for each site separately and in the second approach, parameters were estimate for all site of the same plant functional type (PFT). In a next step, global model simulations with optimized parameters were evaluated against atmospheric CO₂ measurements and satellite-based NDVI time series. In conclusions, the authors present improved model performances regarding NEE in temperate, boreal and grassland sites, and regarding seasonality of NDVI and CO₂. They also state an improved inter-annual variability of CO₂. The manuscript is well written, the analysis is sound and the results

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are well presented. I think this manuscript is a good documentation of a development step in the ORCHIDEE model. Nevertheless, besides some other concerns, the significance of this work for other optimization experiments or development of other land surface models (LSM) should be better highlighted in the manuscript.

1. General comments

1.1 Extrapolation to different climate conditions

The authors optimized parameters for single sites as well as for all sites of a PFT (multi-site). Then they applied these parameter sets for global model runs for global evaluation. Nevertheless, there is no analysis about how the optimized parameters can be used to predict NEE under different temporal, spatial or environmental conditions. A basic procedure in model optimization and evaluation is to optimize the model against one part of the data and evaluate it against the second part of the data. Specifically, I would like to see an analysis about how the multi-site parameter sets can predict NEE at sites that were not used during the optimization. Optimally, these sites should be also selected in a way that climate conditions are different from the optimization sites. This could provide us confidence how the model with the optimized parameter set performs under different climate conditions. This is an essential test for a model that is likely applied for climate change projections.

1.2 Evaluation of inter-annual variability

The authors state an improved model performance regarding inter-annual variability (IAV). I would be very interested in these results but unfortunately I cannot find any corresponding figures or tables. Could you please provide figures that demonstrate the improved IAV of ORCHIDEE regarding the following points? - CO₂: Demonstrate the improved IAV of CO₂. - NEE: Is there an improved IAV in comparison to sites with long time series? - NDVI: Do you see improved IAV in mean growing season or peak NDVI? How do simulated NDVI trends compare with observed NDVI trends?

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1.3 Comparison of modelled FAPAR with NDVI

Could you please provide some more details regarding the comparison of simulated FAPAR with NDVI? NDVI is also affected from non-vegetation changes like soil and snow reflectance. Especially, snow melt in spring can result in a fast increase in NDVI. In the computed FAPAR there is no snow effect and also no factor that accounts for background reflectance. Thus, the computed correlation is meaningless if one compares modelled FAPAR with NDVI that is affected by such non-vegetation related seasonal transitions. You should exclude NDVI observations that are possibly affected from snow or that are at the beginning or end of the growing season to draw more pure conclusions about model performance. Additionally, as the title states “to global simulations”, I’m expecting to see some global model results and evaluations. Especially the NDVI comparison is highly aggregated into one table that does not provide much insight into model performance. I would rather expect maps and the corresponding discussion of correlation coefficients between modelled FAPAR and observed NDVI (weekly data, mean seasonal cycle, mean growing season comparison, trends). In which regions does the model perform well or why not?

1.4 Global total carbon stocks and fluxes

In optimization experiments, a parameter was introduced that regulates the initial soil and vegetation carbon pools in order to match the observations. I did not understand how this information was translated into the global model simulations. Did you account for spatially varying initial carbon pools? If yes, how? If not, how were carbon pools initialized and how might this affect model evaluation results? Additionally, I would like to see a table and discussion of global total carbon stocks and fluxes from the prior, single-site and multi-site experiments in comparison with estimated ranges from independent datasets.

1.5 Parameter variability and distributions

The manuscript misses a discussion on parameter uncertainty and variability. What

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is the spatial and within PFT-variability of parameters? How does a multi-site parameter value compare with the single-site variability? Which parameters were well constrained? Which are uncertain? Are posterior parameter values plausible? I'm surprised not to see such results in a model-data fusion manuscript.

1.6 General discussion and significance of the study

The discussion of model limitations is currently distributed over the entire results section. I would suggest adding another section before the conclusions that summarizes the limitations and potential need for improvement of the model that were identified in optimization experiments. Additionally, this section should also discuss the relevance of this work for other modelling groups or for model-data fusion in general. This can potentially improve the importance and impact of this manuscript for other groups.

2. Specific comments

page 2962, line 3-4: Please write “net ecosystem exchange” to introduce the abbreviation NEE.

page 2966, line 27-29: I don't understand why the multiplier for LAI was not applied for deciduous PFTs. It should be the same like for evergreen and herbaceous PFTs that the maximum annual coverage of deciduous PFTs depends on the site history. Can you please clarify this?

page 2968, line 15-17: I don't understand this sentence. Is this reproducible?

page 2969, line 7-10: How was the optimization done, if the remaining 30% of the grid cell were covered by another PFT (i.e. understory, grass?). Was the minor PFT represented in the optimization? If not, what is the risk that the dominant PFT accounts for changes that are due to the minor PFT? Or were both PFTs optimized at the same time or sequentially?

page 2971, line 19-21: How were snow or albedo changes considered?

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page 2971, line 25: Does this refer to the coverage of the dominant PFT or of total coverage of all PFTs? Did you evaluate also in grid cells that had a mixture of several PFTs? If not, why not? If yes, how was the model performance?

page 2973, line 12-13: Why there were only small improvements in evergreen PFTs? Could this be linked to the phenology routine?

page 2982, line 17: Please demonstrate this with a corresponding figure or table.

page 2983, line 15-16: Why? Is this because evergreen PFTs don't have a phenology in your model and there are no seasonal effects of snow cover?

Table 1: There are no values underlined but some are in bold font. Please clarify.

Table 2: This table is very long but not very informative. I would suggest moving this to the appendix or supplementary material like the table for the CO₂ stations.

Table 3: This table is not very informative. The differences are small. Could you please provide an estimate of the significance of these differences? Even better would be a map of correlations or boxplots of the global distributions of correlations.

Figures 1 and 2: It is not clear if (a) and (b) refer to the mean seasonal cycle or to the full length of the time series. Please add a legend with colours and symbols to the plot to improve the readability of the figure. I would not expect biases in the posterior of single site optimizations. What are the reasons for these biases? The y-axis scale in Fig. 1 c for TempDBF is not very different; thus please use the same scale in order not to confuse the reader.

Figure 3, 4, 5: Please add colour legends to all figures.

Figure 5 and 6: It would be valuable information to have some model performance measures (RMSD, correlation) included above the cycles for each PFT.

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