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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.

Anonymous Referee #1

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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.
- 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 has been taken into account. This should be described more precise.
- 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.
- 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 calculated the gradient is appropriate.

- How have the authors assessed that the assimilation finds a global minimum of the cost-function and not a local one?
- 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 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.: reffering to eq 13 - 15; is b_{Tmin} 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 result are well presented, but they sometimes lack a profound discussion:

- The authors state unfavourable 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.
- Model structural errors are reported. (p. 3345/9). Could the authors give any ideas what these structural problems might be?
- The use of only LE observations as constraint degrades the modelled NEE (section 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.

- 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. Otherwise it might be discussed, why the estimates for GPP and Reco (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 Ra and Rh are consistent with the estimates of Granier et al. (2008), especially because the ratio of the two is rather different.

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.
- p. 3322/12: How have the meteo-data been gap-filled (or where from taken)?
- p. 3322/24: Any particular reason to take 70%?
- p. 3322/27: Where from is the gap-filling for the FLUXNET sites taken.
- Figure 2: It should be made clear, that the brackets give the uncertainties of the annual averages.
- 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 contribution from the summer. This should be made clearer.
- p. 3332/20-22: It should be made clear if LE-observations have been incorporated in this assimilation or not.

- 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.
- 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?
- Conclusion: The term ”globally”, often used in the conclusion, is somewhat misleading, since only some sites in the Northern Hemisphere have been studied.
- 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.
- Figure A13: The authors should specify - in the figure caption - from which run the posterior covariances are taken (MS or SS – which site?).

3 Technical correction

- p. 3319/24: ”rather difficult” is a very subjective term.
- p. 3331/8 (equation 6): $C_{p,soil}(t_0)$ appears on each side of the equation.
- p. 3339/12 I think US-WCr should be US-UMB.
- p. 3339/17: Should be LE not LEE
- p. 3343/13: The boxes are in orange not in grey.

- p. 3345/8: ”... still does not go deep enough ...”: Is this correct English?
- p. 3345/24: How big is ”abnormally”?

4 References

Baldocchi, D. (2008), ‘Breathing’ of the terrestrial biosphere: Lessons learned from a global network of carbon dioxide flux measurement systems, Aust. J. Bot., 56, 1–26.

Baldocchi, D., et al. (2001), FLUXNET: A new tool to study the temporal and spatial variability of ecosystem-scale carbon dioxide, water vapor, and energy flux densities, Bull. Am. Meteorol. Soc., 82, 2415–2434.

Granier, A. et al. (2008), Ten years of fluxes and stand growth in a young beech forest at Hesse, North-eastern France, Annals of Forest Science, 65, 704–71.

Richardson, A. D. et al (2008), Statistical properties of random CO2 flux measurement uncertainty inferred from model residuals, Agr. Forest Meteorol., 148, 38–50.

Interactive comment on Biogeosciences Discuss., 9, 3317, 2012.