Biogeosciences Discuss., 10, C7631–C7636, 2013
www.biogeosciences-discuss.net/10/C7631/2013/

Biogeosciences
Discussions
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Interactive comment on "Ecosystem model optimization using in-situ flux observations: benefit of monte-carlo vs. variational schemes and analyses of the year-to-year model performances" *by* D. Santaren et al.

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Received and published: 30 December 2013

In general, this is a well performed study on an important current subject that should be published.

The comparison between two parameter estimation methods is informative although I am left wondering why these two were chosen and how these methods would compare to others (e.g. Haario, Saksman and Tamminen (2001) An adaptive Metropolis algorithm. Bernoulli, 223-242.). Of course it would be ridiculous to demand more methods

to be assessed but it would be good to know why these two methods were chosen for this specific study.

The comparison in model performance when fitted to different time slices is also informative and sends a clear message about the fact that while these models are "process inspired" we should not necessarily expect (and I would say rarely) the parameters in the model to map on to what they are meant to represent in reality (indeed I personally would expect most to NOT map directly on to reality, and if we force them to do so then we should expect the model to weaken elsewhere).

The insight into the benefits in model performance to using a wide range of environmental conditions as training data is excellent – this is exactly what is hoped for. However, I disagree with the assertion that "This suggests that ORCHIDEE is able to robustly predict the fluxes of CO2 and latent heat of a temperate beech forest after optimisation." This is wishful thinking – there is no more evidence presented as to the wider applicability of the model (e.g. evaluate but not train against independent data on different forests for example), . All we know is that the model performs to a particular measure of performance under the range of conditions provided for the 4 years for this specific beech forest. I would expect decreases in performance when applying it to other temperate beech forests and/or under other environmental conditions for similar reasons to why the model performs poorly for the whole dataset when only trained to a year worth of data.

However, overall a very good study needing minor revisions. Revision requests follow.

P18015 L23 "enhance the model fit to observations equally?"

P18016 L1 This question needs rephrasing. Perhaps "Does the information gained from parameter optimization indicate anything new about modelling carbon fluxes in deciduous forests?"

P18017 L1 I'm not sure you define the acronyms NEE and LE before this.

P18017 L4 u* star (is star redundant?), this is too much jargon and needs a brief definition

P18017 L20 given->for

P18017 L20-21 this sentence needs to be made more readable... especially the “having a very long turnover rate”

P18017 L26 delete “and”

P18018 L5 LSMs needs definition

P18018 L17 and rest of paragraph. This is a key paragraph that is slipped in here. This should be thoroughly discussed in a paragraph in the discussion. I will come back to new items that I’d like to see in the discussion below.

P18018 L27 how do justify your data probability distributions as Gaussian? Is there any evidence that sampling or observational error is Gaussian? Please justify this assumption. Similar for your parameters. Some parameters may be log-normally distributed (especially rate parameters)

P18019 L27 – Why are data uncertainties chosen? Are they not provided with the data themselves (e.g. for a given datapoint do you not have NEE plus and minus a certain amount representing your measurement uncertainty?). Are you ignoring the true data uncertainties? Or are you using the sampling errors to give relative weights within datasets (e.g. an NEE with high uncertainty has less influence than an NEE with low uncertainty) but then applying an additional weighting between datasets (e.g. LE versus NEE) to balance their effects on the parameters?

P18020 L3 – “chosen to be relatively large”

P18020 L3 – What was the basis for selecting these ranges at all? Expert knowledge? Publications? Based on the original hand chosen values used in previous studies? I see you include a paragraph on this in P18023 L18-21. This should be stated before.

P18020 L20 – please explain what you mean by “sorely”

P18022 L21 – but is the model linear with Gaussian distributions for data and initial parameters {this should be parameter} errors? This assumption should be discussed in the discussion.

P18025 L1-2. This may not be due to the data assimilation framework but simply the fact that you lack adequate data constraints. This needs to be addressed in the discussion.

P18025 L7-8. I don’t understand what you mean by “The importance of the equifinality problem is directly related to the posterior uncertainty of the parameters.” Do you mean that the more equifinality you have (parameter correlations?) the more uncertainty you have in the posterior parameter estimates?

P18025 L16-19. This is hardly a surprising conclusion – indeed you can say a lot about parameter dependencies/correlations/equifinality from a variance-covariance matrix. This is not a result, it is common knowledge. I’d rather you say how you intend to use this information in interpreting the parameter estimates when using the real data.

P18027 L2 in->on

P18028 L23 – what is a “proper simulation”? Please be more informative.

P18028 L24 At->On

P18031 L16 an->a

P18032 L21 to converge->from converging

P18033 L14 “A lot of effort has to be put”. Please reword.

P18033 L24 “The lack of convergence problem usually” please reword

P18038 L5 Table 2-> Table 1

P18038 “where vegetation is treated as a single equivalent surface for the carbon cy-

cle." Please reword

P18042 L4-L7 – the sentence starting "Soil litter laid..." is a bit garbled and would be better being reworded

P18042 L7 increasing-> decreasing

P18042 L8 – do you mean 3 soil carbon pools?

P18056. You need axis labels (BOTH AXES) – remember units on the y-axis.

P18057. You need axis labels (BOTH AXES) – remember units on the y-axis. Also I don't understand your error bars – They appear to be symmetric about the mean – consistent with Gaussian distributions, but then this doesn't make sense when the error bar extends beyond the parameter maxima and minima or go past zero – what's going on here?

P18058. Axis labels. Label legend

P18060 Label x axis. More importantly, you should convey the observation errors. Either shade in the background or simply include a representative error bar somewhere on the graph. It is not acceptable to show these plots without conveying observational error. This will allow us to gague the relative adequacy of the different model prediction lines.

P18061. Same comment as immediately above.

Figs. 5, 6 and 8: I'm disappointed that there is not propagation of parameter uncertainty through the predictions. Given you have parameter uncertainty estimates, these should really be propagated into the predictions to convey your uncertainty in those predictions. I'm not insisting that you do this though.

Discussion My most substantial criticism is that the discussion is not thorough enough. For me, it appears too much like a reiteration of results and not enough genuine discussion. What data would you recommend being collected to obtain better constraints

on parameters? The simplest thing, if the data exists, is to include a wider variety of data on different aspects of what the model is designed to represent. It is asking a lot to constrain so many parameters with just two sorts of data, even if they do vary over multiple timescales (of course it's also asking a lot to ask you to somehow magic up a wider variety of relevant datasets). Would simplifying or reparameterising the model be a sensible future step to avoid wasting time with an underconstrained model? Does it even matter that some of these parameters are unconstrained (in terms of the predictive performance)? In what ways would you expect the current version of ORCHIDEE to be structurally adequate or inadequate to predict in terms of carbon flux dynamics in different sites or under different conditions? Are there other ways to deal with the equifinality problem that could be attempted? For example, cleverly re-parameterising the model (e.g. fitting the product of two parameters rather than the parameters separately) can help – as can using schemes that automatically estimate the parameter covariance matrix and use that in MCMC parameter estimation (various adaptive MCMC methods do this). I simply encourage you to put more value in the discussion for a general reader – few are going to want to use the paper to decide whether to use ORCHIDEE for this specific site – they are more interested in the general insights, the general applicability of the model, the lessons learned that should be considered in other studies. How would you take what was done here to learn about how to fit ORCHIDEE to many more sites to end up with a data-constrained global model? Please provide the reader with a bit more generality to your conclusions.

Congratulations again on a very good study.

Interactive comment on Biogeosciences Discuss., 10, 18009, 2013.