Biogeosciences Discuss., 11, C7137–C7143, 2014
www.biogeosciences-discuss.net/11/C7137/2014/

Biogeosciences
Discussions
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Interactive comment on "Constraining ecosystem carbon dynamics in a data-limited world: integrating ecological "common sense" in a model-data-fusion framework." *by* A. A. Bloom and M. Williams

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Received and published: 5 December 2014

We thank the referees for having provided thorough feedback and for their suggested corrections. Below we have addressed each individual comment from both referees (referee comments are shown in italics).

Anonymous Referee #1

Bloom and Williams report that incorporating internal 'reality constraints' on model process relations reduces the range of permissible parameter values in a terrestrial

ecosystem model. They also report that the use of these reality constraints additionally improves model performance when compared to measured eddy-covariance flux observations out of sample.

The manuscript is very well written, and the approach intuitive and reasonable. The results clearly demonstrate that introducing these additional reality constraints reduces parameter uncertainty. This is a clear result and indeed including such reality constraints in any model endeavor (be it data assimilation or more traditional model assessment) should be standard practice.

My only issue with the results presented is that the model that uses reality constraints does almost too well when compared against eddy-covariance data. In figure 5 we see that it captures the magnitude and seasonal cycle of net ecosystem exchange almost perfectly at two sites, compared to the model that does not use reality constraints. Both model runs use MODIS leaf area index and soil carbon as constraints, but not the eddy-covariance data.

The authors are therefore claiming that with only information on LAI, soil carbon and some general bounds based on how ecosystems are typically structured, we can predict carbon cycling on seasonal and annual timescales. This is quite remarkable given that in a previous study that also included some measure of reality constraints, and a host of other constraints at one of the sites used here (Howland forest; Richardson et al. 2010), the DALEC model had difficulty in capturing the annual total NEE (i.e. only when annual NEE was used as a constraint, despite being optimized to daily NEE and various other biometric constraints). It is also remarkable in that it suggests that other typically key information such as above ground biomass, photosynthetic potential, soil moisture status, and canopy structure differences between evergreen and deciduous sites (i.e. site specific ACM), are not essential for predicting carbon uptake.

We are confident in the results of our experiments. We link the improved performance particularly to the ecological and dynamic constraints (EDCs) we have

introduced – our new EDC analyses, as suggested below, help to define the contributions of individual EDCs more clearly.

A lacking component in the manuscript is the identification of which of the reality constraints is responsible for the improved model performance.

We agree with the reviewer's recommendation: to identify which ecological and dynamic constraints (EDCs) have resulted in improved model performance, we have conducted an EDC sensitivity test. We now show which EDCs (a) lead to improved parameter estimates and (b) lead to reduced net ecosystem exchange (NEE) confidence ranges. We find that EDCs 8 and 9 lead to substantial improvements in both model parameter estimates and increased NEE confidence. In the revised manuscript we will include the above-mentioned sensitivity analysis.

It is also not clear why the range of annual model carbon cycling not centered around equilibrium, given the wide range of parameter values used, and information only on soil carbon and leaf area, and a forest typical structure.

For both for synthetic and AmeriFlux experiments, the posterior probability density functions of NEE (e.g. Figures 3, 4 and 5) show that ecosystems could be either net sources or sinks of carbon on annual timescales. Therefore, our results demonstrate that soil carbon and LAI are not sufficient to resolve whether each AmeriFlux site is a net source or sink of carbon on annual timescales. We now explicitly state this in the discussion section of the revised manuscript.

Introduction:

The concept of using internal model constraints, here termed ecological and dynamic constraints, was first introduced by Richardson et al. 2010, there termed a reality constraint. This should be acknowledged in the introduction.

In the revised manuscript we acknowledge that Richardson et al., (2010) introduced internal model constraints in carbon cycle model-data fusion analyses.

Page 12736, line 25: "therefore. . .". Consider revising this sentence. It does not logically flow from the paragraph.

We agree with the reviewer, and we have revised this sentence.

Page 12738, line 17: Please do not refer to DALEC2 as a universal ecosystem carbon balance model. It is designed for temperate deciduous and evergreen forests, and will not likely accurately simulate other ecosystem flux dynamics (e.g., tundra, tropical, peatlands, savannah, etc.). Page 12738: Please state the drivers used in the DALEC2 model.

We acknowledge the reviewer's point, and we have re-worded the DALEC2 description.

Page 12739, line 21: Please clarify that omega here represents a turnover rate. What is OmegaMin?

Equation 5: Clarify what f signifies here.

In the revised manuscript, we have now added an explicit reference to Table 1, where all DALEC2 parameters, notations and ranges are reported.

Page 12746, line 17-20: Clarify the site selection criteria here. Both Howland and Sylvania have snow cover for far more than two months, which would appear to invalidate the selection criteria based on hydrological concerns.

We agree with the reviewer's remark and acknowledge our oversight. We now clarify that the selected sites exhibit limited water stress and $\leq$ 3 months of below-freezing soil temperatures.

Page 12747, line 1-10: Please report the values of LAI and soil carbon used for each site.

We now report the 5th and 95th percentile LAI values and the soil carbon value used for each AmeriFlux site experiment.

Page 12748, line 3,5: Please do not confuse error with uncertainty. Parameter vectors have uncertainties, not errors, unless compared against known parameter values. This confusion is apparent throughout the manuscript.

Page 12748, line 14: 'and hence improved estimates of s'. I would argue that what you are really reporting are better constrained estimates of s, though the true values of s are remain unknown.

We agree with the reviewer's two points on error and uncertainty: however, the synthetic datasets are derived from known parameter values s. To better convey this point, we now explicitly state this in the introduction to synthetic experiments.

Figure 5: I would suggest plotting all three graphs on the same scale to assist between site comparison

In the revised manuscript, we have now plotted all three graphs in Figure 5 on the same scale.

Anonymous Referee #2

The manuscript by Bloom and Williams proposes to include known model parameter relationships in a data assimilation framework in addition to observations. They claim that in a data-poor context these additional constraints will reduce parameter uncertainties. In general I agree with this statement. However, in my opinion the ecological and dynamic constraints (EDCs) that the authors introduce as a novelty are simply part of the prior information we possess for these parameters. I would suggest that the authors highlight this in the manuscript.

The manuscript is well written and presented, but I think some improvements and clarifications are required (see specific comments below).

Specific comments:

In order to obtain a unique solution in an ill-posed problem additional constraints are required. This is also known as regularization. Within the Bayesian framework prior parameter information are usually included in form of a covariance matrix, which can include correlations between parameters. The authors mention in the introduction that such correlations limit the possible parameter configuration, but in their example they simply assume no prior knowledge other than the parameter ranges. This seems to be an odd choice, because it means that all values within the given range are equally likely and parameters are independent, which is clearly not the case. The parameter space has not been restricted and it is therefore not surprising that additional information in form of ECDs add large constraints to this problem. I am wondering if this would also be the case if a different prior parameter distribution (i.e. Gaussian) with a defined covariance matrix would have been chosen in the first place. I see the ECDs complementary to the knowledge we include in terms of prior distribution and covariance matrix and not as a replacement.

We agree with the reviewer's point: in contrast to a "flat" parameter prior, a parameter variance-covariance structure would serve as an additional constraint on model parameters, and would reduce the ill-posedness of the problem. However, given that we have poor quantitative knowledge on the realistic values for model parameters and their covariances, constructing a generic, ecologically-appropriate covariance structure is exceedingly difficult. For example, most parameter inter-dependencies presented in our manuscript are dependent on local meteorology: therefore, a meteorology-dependent prior parameter covariance matrix would need to be derived for each AmeriFlux site. By prescribing EDCs, we are instead able to impose ecological knowledge in the form of non-Gaussian state and parameter constraints. We agree with the reviewer that EDCs and a parameter covariance matrix can both be used to resolve ill-posed carbon cycle problems. In the revised manuscript, we now state that prior parameter covariance structures can be used as alternative or complementary constraints to EDCs.

A number of ECDs are formulated to constrain the parameters and states and it would be interesting to know their individual contribution, i.e. which ECD provides the largest constraint.

We agree with the reviewer's recommendation. We have performed an EDC sensitivity test, whereby we quantify the improvements in model parameter and state estimates associated with each EDC. We will present and discuss the results of the sensitivity analysis in the revised manuscript.

Minor comments:

We have implemented all of the following suggested corrections. In particular, the ~ on P12745, Eq.(16), denotes the median value of *E*(the ~ was missing from *E* on P12745 line 18: we have now corrected the text).

P12738,L2 + P12739,L 14: EDC has already been introduced in the abstract and introduction (P12737,L18)

P12744, L15 + L20: repetition "We create 40 synthetic experiments ..."

P12745, Eq.(16): What is ~ been used for?

P12759, L1: space between 8 and daily

We have also corrected a minor oversight in the prior parameter ranges shown in Table 1: we used 20-2000 gC m^{-2} for the foliar, labile, fine root and litter carbon pools, and 20-150 day for the leaf-fall period parameter ranges. We have corrected this in the revised manuscript.

Interactive comment on Biogeosciences Discuss., 11, 12733, 2014.