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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

Anonymous Referee #1

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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 con-

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straints 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.

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

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

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constraint. This should be acknowledged in the introduction.

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

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.

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

Equation 5: Clarify what f signifies here.

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.

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

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.

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

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