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Biogeosciences Discussions

Interactive comment on "Leveraging 35 years of forest research in the southeastern U.S. to constrain carbon cycle predictions: regional data assimilation using ecosystem experiment" *by* R. Quinn Thomas et al.

Anonymous Referee #2

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Quinn Thomas et al. present a model-data fusion, or data assimilation, study that gathers 35 years of carbon cycle-related observations and manipulation experiments taken in Loblolly Pine ecosystems in the Southeastern US to optimize parameters of the 3-PG model within their new framework DAPPER. The authors examine the ability of the observations to constrain model parameters using a number of approaches for assimilating the different types of data, and they further examine the differences in model behavior/sensitivity and change in biomass stocks across the southeastern US as a result of the different experiments.

The authors have carried out an impressive and exhaustive collection of data for constraining the 3-PG model in this study. This, and their investigation into different approaches for assimilating different types of data, in particular manipulation experimental data, make this study a noteworthy contribution to model–data assimilation literature in forested ecosystems, and therefore I would recommend publication in Biogeosciences. However, as it stands the manuscript is quite long and dense, which is understandable given the amount of detail that is required to present such a wide array of data and experiments. This being said, I recommend that the authors try to edit the article following some of the suggestions below (and their own views) to improve the clarity and readability of the text before this article is published.

Overall, the objectives and key points of this study can get lost in the text. I think a few more sub-sections in the main text and supplementary, references and links between sections would help the reader to better follow and absorb the necessary amount of detail presented in the manuscript. I would also find it useful if the authors posed a few key scientific questions to help them highlight the main messages of the study.

Some sections in the methods could do with more explanation for why certain approaches were used (see comments below) or better links to the supplementary material, as I have just mentioned. The introduction and discussion are quite long and this can prevent some of the key points from being highlighted. I suggest the authors try to cut down the text where they see fit, including some sentences that essentially are repetitions of earlier statements.

The paragraphs in the results section could be separate sections with sub-headings in order to guide the reader, while at the same time the results could benefit from stronger links between each section, especially before line 522, in particular comparing the between the 1st and 2nd stages, or the different 2-stage approaches with the 1-stage approach. At the moment, the results section before line 522 is a bit fragmented, making it harder to weave together a coherent story that brings out the key points.

Reading this manuscript I found myself asking: What do you expect from each experiment/approach? What will you gain/lose? Which approach is the right approach, going forward? These questions were largely answered in the discussion, and therefore I have made a suggestion below that perhaps some of the results and discussion could be merged within the sub-sections suggested above. This is a personal style issue however.

Finally, the authors may consider cutting other sections of the discussion that are not fully pertinent to the results as the paper is already quite full of detail. I would like to stress that despite this suggestion I did find the discussion to be interesting and comprehensive, but I would like to see the key messages highlighted more and am concerned the length of the paper may overwhelm the reader.

Introduction

Line 97: "relative contribution of each environmental control should be separated in order to correctly parameterize the sensitivity to changes in the environment". I agree to some extent but this is very hard to do and should we be separating each environmental control, as the interaction between different environmental changes may produce different outcomes than if each were treated separately? I would be interested to hear the authors thoughts on this and what they think the impact of assimilating manipulation experiments data separately has on their results.

Line 124-128: See previous studies Wutzler and Carvalhais (2014) and Section 2 of MacBean et al. (2016) for further discussion on debate of how to deal with the issue of weighting to account for the number of observations and/or using a multi-stage assimilation approach to address challenges of assimilating a diverse set of observations. Both issues are the subject of debate in the literature. On the issue of weighting by the number of observations, from a mathematical standpoint there would be no need if the error covariance matrix is properly characterized; however, this is difficult to achieve in practice. Similarly, a joint or simultaneous assimilation, in which all observations are

assimilated together, is mathematically more rigorous as the error covariance between the observations can be properly taken into account. I appreciate that you have discussed the benefit of weighting by the type of data in the discussion, but this debate in the literature (for and against weighting, due to the abovementioned reasons) should perhaps be referred to more clearly in this study.

Line 129: It is true of course that to constrain changes in biomass monthly time-scale models are sufficient, but note that monthly time-scale models are not the only way to overcome computational challenges associated with inverting a complex ecosystem model. There are sophisticated yet simple algorithms that dramatically improve the sampling of parameter space in a limited number of iterations. See the work of Jasper Vrugt: https://scholar.google.co.uk/citations?user=zkNXecUAAAAJ&hl=en&oi=ao

Methods

Section 2.1 It would be good if you could refer to references and/or relevant sections in the Supplement in Section2.1 to depict between standard characteristics of the 3PG model specific additions or alternative choices you made and (and to explain why you made those choices). For example: - Line 201-202: Was this additional function based on a published study? - Line 209: Is the site-index a new addition to the model that you developed? If so, from where? - Lines 218-220: Why did you remove the dependence of total root allocation on FR for the DA study? - Line 229-231: A reference for or further explanation of this modification would be good here. - Line 245: "implicit irrigation in very dry conditions." Is this a realistic feature of these sites? How does this affect the results? Especially for the water availability manipulation experiments.

Line 250: do you mean to say "mean monthly GPP"?

Line 251-252: How did you select the 31 parameters to be optimized?

Table 1: Please can you give the equation for how the sensitivity is calculated? Also, please could you explain why there is both a number and "vague" given for the uncer-

tainty of some parameters? If “vague”, please can you detail how you defined the prior uncertainty/ranges in the text? Finally, I appreciate you have a lot of information to convey and the tables are large, but it might be good to have all optimized parameters here and just indicated which ones are referred to in the discussion. As a general comment, it is hard to find some of the information you refer to in the Supplement (e.g. the other optimized parameters you refer to in the caption of Table 1). Please could you split the Supplement into numbered/indexed sections and then refer specifically to the relevant section to help the reader?

Line 255-265: How did you initiate the biomass pools? Based on site-level data for the start of the simulation period? Please detail with references. If no site data were available, how sensitive were your DA experiments on the method used to initiate the biomass pools? Later note: I see you have addressed this in Section 2.4. It might be useful to refer to that section here so the reader is not questioning this in this section.

Section 2.2 Table 2: Last column – Table 3 instead of Table 4. Also, please could uou explain, or give references, for why the SD for observations sometimes varied between 10% and 2.5% of the observation.

Section 2.3 Equation 4: Please explain why you picked a uniform distribution between 0.001 and 100? Lines 348-349: Please explain why (only) 3 MCMC chains were run? Was a convergence metric such as R-hat used?

Section 2.4 Lines 398-399: Although I understand the reasoning that these sites are close together and the most data rich, I don’t understand why you lump the Duke CO2 enrichment site with DK3 and NC2 in the 1st stage when you stated that you wanted to test the influence of the CO2 fertilization – why not just test the Duke CO2 enrichment site by itself in the 1st stage and the remaining sites/plots in the 2nd stage to answer this question?

Further to the above point, I appreciate the extra experiments to understand the influence of the CO2 fertilization on the posterior parameters, and the further experiments

to determine the influence of the water treatments and nutrient addition. But how dependent are your results on which type of observation and/or treatment is assimilated in the 1st stage vs 2nd stage? Would the results different if you reverse the stages you have in your current set-up? Again, see Wutzler and Carvalhais (2014) and/or MacBean et al. (2016) who discuss these issues (as well as the issue of the weight of different types of data, as you discuss below. A pseudo-test with synthetic observations would have been useful prior to assimilating real data to determine whether the exact set-up of a 2-stage assimilation is sensitive to the order of observation assimilation as well as to confirm if the assimilation system is able to constrain the parameters to their correct values.

Lines 430-465: While the tests and approaches put forward here are interesting, the text is dense. Any efforts the authors could make to simplify the description of the experiments and simulations performed (perhaps with the use of a table and simulation/experiment code names?) would likely help the reader.

Lines 467-475: The cross-validation exercise presented here is a useful one. Was a similar test used to assess the validity of the posterior distributions of the manipulation experiments, even though there are fewer sites?

Results

Line 480-484: Description of the sensitivity analysis and choice of parameters should be in the methods. Was this a one-at-a-time sensitivity analysis or a full global method? What is the justification for using this approach versus an existing global sensitivity analysis that accounts for correlations between parameters and explores the whole parameter space (unless I have misunderstood what was done)? Why did you fix the light extinction coefficient as opposed to the quantum yield parameter?

Supplemental Table 3 and Table 5: As mentioned above I would suggest having all the optimized parameters in one table. I would also suggest putting the prior min/max in Table 5 even though it might mean having an extra line/column per parameter and

taking this information out of table 1 so it is easier to see how well the optimization has constrained the parameters. Finally, I would suggest splitting up the parameter tables into the sections you refer to in the text, e.g. “temperature sensitivity of quantum yield” or “physiological parameters” etc. This will make it easier for the reader to refer to the tables when reading the text.

Which experiment do the supplemental figures correspond to? The “ALL” experiment? This should be detailed.

Are you talking about the 1st stage experiment in the first paragraph of the results? If so, it would be good to specify this, and I would further suggest splitting the results into sections to more easily guide the reader.

Do you discuss DK+NC2-fert in the results, or have I missed it? Perhaps more needed on the 1-stage versus 1st and 2nd stages before you discuss the experiments with and without nutrient and water addition (i.e. before line 522)?

Figure 5 comes before Figure 4 in the text – switch around?

Lines 507-515: I am a bit confused by the sentence “The two-stage assimilation was critical for constraining the CO2 quantum yield enhancement parameter (Calpha700)” as you then go on to say (and show, in Figure 5) that the 1 stage resulted in a narrower uncertainty interval? I guess you mean that despite the higher 95% confidence interval, the 2-stage approach results in a more realistic parameter value but I am not at all sure on that? Please could you clarify this in the text?

Line 517: I would suggest putting the names of the soil fertility parameters in brackets to aid the reader, or again put sub-headings in the parameter tables.

As you did not have a strong difference in predictive capability between experiments with and without nutrient or water addition, even though you had different parameters, that presumably means you have a certain amount of model equifinality? You discuss and show the difference in model behavior as a result of the different approaches in

Figures 5 – 7, but you do not discuss which one you think leads to the right behavior? Do you have an idea? Perhaps a synthetic experiment with pseudo-observations taken from the model simulations might help with this (a so-called “observing system simulation experiment”, or OSSE)?

Lines 522 onwards show very interesting results. However, I would suggest that the patterns detailed in last two paragraphs (Lines 553-572) would benefit from explanations linking back a bit more (not just referring to figures) to the different model behavior/mechanisms identified and discussed in the RW-fert and RW-water sections just above.

Discussion

First paragraph is more of a summary than a discussion and could be cut or added to conclusions.

Although perhaps a little too long, this is a useful discussion that ties the results together and answers some of the questions I raised in my comments on the results. Perhaps it would be useful to combine some of the summary points raised in the discussion with relevant sections in the results with separate sub-headings as I mentioned above.

Lines 650-652: Interesting point and in addition, as I have mentioned above, I think a synthetic experiment would also be very helpful in this regard.

Minor comments

Line 87: Do you mean the “assimilation of manipulation experimental data”, rather than the “assimilation of experiments”?

Line 88: two or more

MacBean, N., Peylin, P., Chevallier, F., Scholze, M., and Schürmann, G.: Consistent assimilation of multiple data streams in a carbon cycle data assimilation system, Geosci.

Model Dev., 9, 3569-3588, doi:10.5194/gmd-9-3569-2016, 2016

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-46, 2017.