Biogeosciences
Discussions

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Interactive
comment

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.

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Thomas et al present a data-assimilation (DA) study using constraints from multiple data streams from multiple sites and experiments to optimise parameters in the monthly timestep PG-3 model of loblolly pine production. The study has three specific objectives. Stated on lns 170-171, 1) a new regional and hierarchical data assimilation system with the capacity to assimilate multiple data streams from multiple experiments; stated on ln 179-180, 2) the consequences for parameter estimation and prediction of including or not including ecosystem manipulation experiments (this could be more

broadly stated as evaluation of the DA); and stated on ln 181 3) model predictions with the optimised parameter set of forest biomass changes in response to changes in nutrient addition of precipitation. This study is well thought out and implemented, presents a useful advance to the use of DA in ecosystem modelling and forecasting, and will likely be of interest to many readers of Biogeosciences. My main criticism is that the distinction between the three areas of this study is often not made explicitly throughout the manuscript and consequently the manuscript is not as readable or as clear as it could be. The majority of my comments are an attempt to help improve the organisation and presentation of the manuscript with the goal that this study will be as widely read and cited as possible.

With that in mind, I suggest organising the manuscript as much as possible by the three stated objectives. I suggest combining the sentence on lns 179-180 with the sentence on lns 170-171 and explicitly listing the three objectives together. The results and discussion section would benefit from organisation along the lines of the three stated objectives. I suggest breaking each into three subsections, each dealing with one of the objectives. Again the conclusions section should specifically address each objective.

Abstract

It would be good to be specific about who the target audience is for this research. The research straddles a technical field that develops DA but the technique produces a tool at a level of maturity that could be used by foresters. These ultimate end users could be more explicitly targeted.

Introduction

Is a bit long and could a page or so could be cut without loss of content. Paragraphs on lns 82-105 could be combined and reduced in length. The main point is that ecosystem experiments can help to reduce the problem of equifinality in DA. The paragraph on lns 108-141 makes some nice points but could be substantially shortened without loss of

content. Much of the paragraph is methods like.

Weight to rare experiments (mentioned on ln 125) could also apply to rare data types. Later in the paragraph (ln 135-136) the authors state that data of different frequency is a problem in biasing the cost function toward high frequency data, but offer no solution other than a monthly timestep model. Rare data, or low frequency data, could also be given higher weights. Also high frequency data could be summarised at lower frequency.

Methods

Again long and could probably be made more concise. Also the organisation is tough to follow. I suggest leading with the observations, the various sites, and measurement campaigns/projects. Many of these are not properly introduced. This will provide a comprehensive introduction to the system and what measurements actually go into this DA system. Observation sites and projects are mentioned on ln 409-410, but these are not introduced and need to be described in the observations section of the methods.

I found section 2.3 very difficult to follow. I'm not expert on DA mathematical methods but I have a reasonable conceptual handle on DA, and yet I was lost in the first paragraph. I also ran this section by a colleague who is expert in the mathematics underpinning DA and they agreed that this sections needs to be clearer. Their key criticism was that they could not see the derivation of Eq 7, perhaps the authors could add the derivation to an appendix. And that it is not clear how the MCMC was used to sample Eq 7. A clear description of the details of the MCMC procedure is necessary, along with the presentation of the cost function. Also the first term on the righthand side of Eq 7 is not the same as the righthand side of Eq 1, is this deliberate? And E is never defined.

I strongly suggest reworking section 2.3 of the methods to be extremely clear about the DA process and how it was implemented. Start with a clear description of the goals of the DA – state estimation and estimation of parameter distributions. Then describe all

the various sources of uncertainty and how the method accounts for them. Then take the reader step by step through the method. Perhaps a diagram would be useful. The following comments are an attempt to provide examples of where confusion arises but they are in no way comprehensive. The sentence on lns 281-283 is more or less stating the the the same thing as the sentence on lns 284-285. I suggest fusing these together. Is the reference to a "latent model" really necessary, it is confusing with the mathematical model. Would "true" system states and fluxes convey the same meaning? Do not try to justify the method in comparison with previous methods (e.g. lns 286-291), in the methods this just confuses the description and this can be argued in the discussion. On lns 291-293, this is state estimation right? That's fine but is it really the focus of your method? None of the three stated objectives are for state estimation. How exactly was estimation of the latent state or flux the first step in the process when it includes the optimised parameters etc as described on lns 296-298? Seems like the statement on ln 306-308 should come before the previous paragraph.

Section 2.4 jumps around between objectives. Some text would fit better in section 2.3, for example lns 408-428. Text on lns 454-461 would be better organised if it were to follow the text on 430-444, then the regional simulations can be presented afterwards.

I suggest defining sections 2.3, 2.4, and an additional 2.5 to be organised by the three stated objectives.

Also, while commonly used by the modelling community, I do not agree that you can run "experiments" with models. Models make predictions from a specific set of mathematical hypotheses and defined scenarios. An experiment is designed to test predictions and discriminate among hypotheses.

Results

Why were only 31 parameters optimised, can you describe why this set were chosen from the total 46?

Technically the parameters are not "sensitive" (ln 480), it is the model output that is sensitive to the parameter. "Influential" would be a better adjective to describe the parameters.

Lns 486 & 488 variability is described as being reduced but no data are provided. Can you quantify these statements. There are many statements like this throughout the results and they ought to be quantified (e.g. lns 502, 508). Also on 508, is mean correct, isn't this the median of the parameter distribution?

Some kind of visual representation of the data in table 5 would be useful.

Ln 492 what do you mean strong priors? Well defined from measurements and literature with low variance? Could you quantify this?

Lns 494 the process uncertainty parameters are mentioned here and in the methods, but results are barely presented (only in the supplement) and are not discussed, or not that I noticed. This is a very interesting concept and I would like to see these data presented a little more and at least a little discussed. What kind of impact does including these parameters have on the optimised parameter distributions? I understand you are already presenting a lot, but this is fairly novel as far as I'm aware and is of interest.

Figure 10 and 11 would be more in keeping with your stated goal of forecasting on lns 65-68 if you removed the b panels in both plots. If you think that the parameter estimates when including the data from the manipulations gives a better estimate of those parameters then the data in panels b are not particularly useful for forecasts. In my view, and as stated on line 67 & 68 "provide information on both the expected future state of the forest and the probability distribution of those future states", the final figures would be much stronger if the probability distribution of the future states shown on the a panels were represented on the b panels.

While it is interesting to show the consequences for prediction of inclusion of manipulations or not, and the opposite sign of the change in predictions when water and nutrient

manipulations are included, you already show this in Figures 6 & 7. If you want to keep the b panels in 10 and 11 I suggest you add them as extra panels to figures 6 & 7, showing the absolute delta (or similar) from the simulations that include the manipulation delta. This will allow you to address the question: what are the consequences of not including data from manipulations? Without confounding the predictions from the most appropriate DA product for the scenarios tested. Also the scale ought to be the same for the data presented in Figs 10 and 11.

Was CO_2 change included in the above projections of removal of nutrient limitation and precipitation reduction? Furthermore, it seems you have included data from water manipulation experiments, nutrient manipulation experiments, and CO_2 manipulation experiments. But you have only made projections for nutrient and precipitation change. Why not CO_2 change? CO_2 projections would complete the study.

Additional points

I think the title would benefit from the addition of "Loblolly Pine".

Ln 50 Duke FACE experiment had 4 replicate plots, so where does the 5 come from on this line. An additional plot from the unreplicated prototype?

Ln 48 – 50 the sentence on this line would help flow if it were before the preceding sentence.

Ln 65 I don't think I would classify the three areas mentioned in the previous sentence as tools. They are more than tools, they are also knowledge.

Ln 67 What do you mean by "based on" here. Can probably delete. Also while I think your methods could be used for "forecasting" you don't really use the method in that sense.

Ln 73 insert "can" in between "that generate"

Ln 85 86 "carbon allocation and turnover" This is worded a little awkwardly

Ln 97-99 awkward way to start a paragraph.

Ln 111 suggest replacing "important" with "useful" or something more descriptive

Ln 155-157 suggest replacing "nutrients" with "nutrient addition". Also suggest removing hyphens.

Ln 162-163 Awkward

Ln 171 Again I think you need to call out loblolly pine here

Ln 175 The authors chosen acronym, in my view, somewhat undersells what they are doing. The DA method is hierarchical and considers data from multiple sites and of multiple different types. The acronym gives not indication of this and suggests that the DA method is only suitable for Pine Plantations. Of course it is the authors' choice though.

Ln 307 insert "considered" between "was a"

Ln 446 replace "regional" with "region"

Ln 522-524 I'm not sure what you mean here, could you clarify?

Ln 528 delete "a"

Ln 576 replace "detangling" with "disentangling"

Ln 582 I think "synthesised" would be a better word to use than "organised"

Ln 591-591 I take your point about equifinality but can you really say this if predictions were not improved in some way? Just a thought. Is there a way that you can be sure that the mechanisms were correctly distinguished?

Ln 633-634 Agreed, but did your method strictly weight the data? Wasn't it more that the hierarchical method gave priority to the CO_2 manipulation data?

Ln 646 replace "than" with "that"

Ln 656 quantify this statement

Ln 662-663 this was news to me when I read this sentence. I think this would become clearer once the methods can be clarified as suggested above.

Ln 668 suggest changing "prior" to "previous", just to maintain the meaning of prior in the Bayesian sense.

Ln 673 you do not show any data on covariation of parameters.

Ln 676-680 I like this statement, makes a lot of sense. But is it most appropriate here? This point should be made clearly in the methods.

Ln 685 suggest deleting "Multivariate Constructed Analogs (MACA)" it is not needed.

Ln 692-697 This is a good point but I'm curious why the change in biomass in response to precipitation reduction was small given the large change in parameter values when water manipulations were included in the DA. Can you try to explain this based on the process hypotheses embedded in the model.

Ln 698 replace "reduced" with "reduction"

Ln 707 insert "as a function of"

Ln 719 insert space in "fromadditional"

Ln 760 While I'm sure the methods and tools developed by this study could be used for ecological forecasting, strictly speaking this study is not ecological forecasting. The third objective, which concerns optimised model predictions, is a scenario analysis rather than a forecast.

Ln 769 no need to cite Medlyn et al 2015 here