Answer to RC1

General comments:

G.J. Schürmann et al., in their manuscript "Constraining a land surface model with multiple observations by application of the MPI-Carbon Cycle Data Assimilation System", describe the MPI-CCDAS system, and a parameter optimization/state estimation experiment with it. The authors optimize various parameters of the JSBACH land surface model, utilizing remote sensed FAPAR data and CO2 flux measurement data from around the globe. They also analyze, how each dataset constrains carbon-related model variables, and what parameter values the MPI-CCDAS system optimizes the model to.

The topic at hand is important, since estimating the terrestrial carbon fluxes is difficult, and uncertainties in carbon stocks and fluxes are still large. Tuning a process based model makes it possible in principle to improve forecasts of how the terrestrial carbon stocks develop in the future.

There are several good things to say about the research at hand. For instance, the MPI-CCDAS is a new and seemingly useful tool for these kinds of experiments, and valuable in itself as a further development of the CCDAS system. The case study done with the system and presented in the manuscript is reasonable and the results generally seem to be good. The authors also nicely discuss and analyze why the results look as they do.

However, the manuscript is needs to be refined, expanded and clarified in some ways.

These are listed in the Specific comments section

Specific comments:

According to already the first sentence of the abstract, the paper is supposed to de-scribe the MPI-CCDAS system. However, the description of the system is unclear and there seems to be text missing between pages 2 and 3. Currently the section is written to vaguely describe that there is some data assimilation and some generic likelihood function being minimized. It would be important to include more specifics about the CCDAS method. What algorithm, how the data is used to update the state, when new parameter vectors are drawn etc. I'd enjoy explanations with formulas when needed. It would be also good to describe how the error covariance matrix for the likelihood function is constructed.

We apologize for the missing text. The missing text was:

„Technically, J is minimized through an iterative procedure using the Davidon-Fletcher-Powell algorithm in the Broyden-Fletcher-Goldfarb-Shanno variant in the implementation provided by the Numerical Recipes (Press et al., 1992, dfpmin routine). The required gradient $\partial J/\partial p$ is evaluated by the tangent-linear model …."

If fact, the missing text has probably obscured that the assimilation procedure is straight forward:

The minimised likelihood-function is given in Eq. 1. The assimilation procedure is given in the reference of the missing text and more details are given in Kaminski et al. (2013) and in reference therein. There is just one assimilation window with one set of control variables and one observational vector to be matched. We think Section 2.1 provides exactly the right level of detail on the methodology (which is a standard variational approach), with references to more elaborated descriptions. We have, however, added more explanatory text.

The differences of the parameter values obtained in Table 6 is large. They are discussed in the text, but there is no compact description of different error sources and their relative importances (like initial states, observation error, model bias, land cover type parametrization errors etc.). I understand that it was not the objective of this research to quantify uncertainties in the parameter and carbon stock values. Anyway, discussing the topic a bit more would be appreciated.

Currently we cannot assess the relative importance of the different error sources with our system.

Discussing these points would considerable lengthen the manuscript. The manuscript is already pretty long (as already mentioned by Reviewer 2). The focus of the manuscript should remain on the model description part. Nevertheless we already have some discussion of the topic in the outlook-section. This part will be extended to also name other potential reasons for uncertainties in the modelled carbon cycle components.
The extension reads as:
„Further assessing the relative importance of different error sources (e.g. in the land cover type parametrization, model biases or observational errors) with a system such as the MPI-CCDAS would allow to highlight priority areas to reduce their uncertainties and further constrain the global carbon cycle numbers as given in table 6).“

The language of the manuscript is not particularly good. Some sections are better than others. Very carefully checking grammar, breaking up too long sentences, checking capitalization rules etc. needs to be done. Some corrections are listed below, but they also could be wrong as I'm not a native speaker.
We have gone trough the manuscript and improved the language (without highlighting these changes).

There is a maybe a bit too much discussion-related content in "results", and some of it could go to the discussion part.
We checked for discussion related parts in the results section and moved this to the discussion which also helped removing some duplications in the text.

It is stated that the "prognostic capabilities of the model have been largely improved" (section 4.3) ... which is deduced from the two-year validation period. I'd like to believe that, but two years is not much. Could you please discuss this a bit further in the discussion part.
We refer here to the two year period which is also (in the layout of the experiment) a prognostic period. Hence for this two years the prognostic capabilities have been largely improved (reduced bias from 5.18 ppm to -0.05 ppm). We have not assessed longer periods (due to lack of data) and could only speculate on that. Thus we avoid opening a discussion on this topic.
To make the point clearer, we add a statement to the results section that we only refer to the 2 years period:
"In other words, the short-term (1-2 years) prognostic capabilities of the model have been largely improved for a 2 years horizon after assimilating CO_2-observations, also at the evaluation locations."

Technical corrections
section 1 / line 63: "certain processes..." is too unspecific. Please clarify.
These processes are the simulated phenolgy, and its seasonal and interannual climate sensitvity, as well as the simulated seasonal net land-atmosphere carbon flux. We added these details to the text:
"Dalmonech et al. (2015) have shown that the simulated phenolgy, and its seasonal and interannual climate sensitvity, as well as the simulated seasonal net land-atmosphere carbon flux are reasonably robust against climate biases in the MPI-ESM."

s. 2.1 title: Phenology-module => The phenology module, or something
Changed to „The phenology module“

s. 2.2 / l. 61: what is "smoothly averaged temperature with a "memory"-time scale of 30 days"? There must be a more precise way of saying this.
This „smoothly averaged temperature“ is not representative for one single day or point in time. Rather it is the temporal average over the entire period with exponentially decaying weights with a

time scale of 30 days. The details are presented in the given reference.
We reformulate this to:
„The transition is controlled either by the length of the day t_d or a temporally averaged temperature T_m with exponentially decaying weights for older periods with a time scale of 30 days."

sections 2.2-2.2.4 These sections are a bit long or unstructured somehow, as they describe just standrad JSBACH model physics. Particularly when compared to sections 2.2.5 and 2.1. More conciseness and clarity are needed.
The reason for giving this degree of detail is explained by the importance of the parameters for the CCDAS. In fact we only describe JSBACH-parts, where parameters have been taken from and some of the JSBACH parts are not standard JSBACH (the phenology). The reason for the shorter section 2.2.5 („Atmospheric transport") and 2.1 („CCDAS-Method") is, that they are described already elsewhere and that the details are not of importance for the optimized parameters.
In order to keep the details, we put these sections (2.2.1 – 2.2.4) to the appendix. To further improve readability, we also extended the description of the parameters in the main text with some more details (as suggested by Reviewer 2).

equation 5: Please state the mean and standard deviation of psi in the explanation, even though it looks obvious. As it reads, psi could be a distr with funny values.
We added the suggested clarification.

l. 75: "memory time-scale" (compare to "memory"-time scale earlier)... please be consistent and choose as comprehensible expression as possible
We corrected this

s. 2.2.2 / l 13: multiplication sign is not usually a star when printed. Use something like latex \times instead. Repeated many times in formulas, fix them all, please
We corrected this throughout the manuscript

l. 18 should it not be exemplified "by" instead of "for"?
We corrected this.

l. 20 ", gas" => ", and gas"
We corrected this.

sentence spanning the lines 29-39: restructure for readability
We restructured this sentence and it has gone to the appendix

equations 13,14,16: exp and min are not normally italicized in formulas
We changed this in the entire manuscript

s. 2.2.4/l.15 turns over to => turns to
We corrected this

s. 2.2.5/l.36- please clarify where "these" transport matrices refers to. The "responses" or what? I would not mind if this section was a bit expanded as well.
We did not explain TM3 in more detail, because this is standard TM3 and we do not optimize anything inside TM3. We compute the responses of the atmospheric CO_2 concentration C to the fluxes F at the surface with the adjoint of TM3. The transport M itself is a linear process which leads to the formulation of $\Delta c = M*f$ and hence we refer to M as transport matrices. We add this formulation to the text. According to reviewer 2 we also moved some parts from the experimental

description to this section which gives further details about how we dealt with atmospheric transport.

2.3/51 why not say just "the assumed prior Gaussian uncertainty"?... and ...the posterior values from the assimilation experiments.
We changed this

Funny spacing in equations 20 & 21
We changed this

page 7, l. 15 "uncertainties...are based on expert knowledge" is quite subjective and ad-hoc. It's probably tricky, but I'd appreciate being more specific here. The expert knowledge has to be based on something, anyway. Please consider working on it.
All of these prior uncertainty estimates are not based on a formal uncertainty consideration, but rather on the authors interpretation of the recent literature. Q10 mainly is based on the experiences related to the work of Mahecha et al. (2010; Science). The uncertainty of f_aut_leaf is inspired by the sensitivity study of Knorr (2000; Global Ecology&Biogeography). For the initial uncertainties of the slow pool, we assumed arbitrary 10 % uncertainty, because we assumed no strong deviation from the equilibrium. For the CO2-offset we assumed only a change of a few ppm which led us to give the uncertainty of 3 ppm. This relatively large value allows a rather strong deviation from the prior without putting a strong penalty on the parameters. We added these clarifications to the manuscript.

l. 55 reference to EDGAR could go to references
This has been put to the references

s.4.1 / l. 69-72 the conclusion drawn is not immediately obvious to me, especially when "consistency" is not defined. I understand the basic idea here, but still, please clarify and explain.
The model can fit both data streams jointly and the costs sum up. In terms of a Bayesian optimization this is an indication that the model "fits" to the data-streams. The model is capable of reproducing the observed data streams without degrading other parts of the model (at least not those discussed in this part).
We moved this to the discussion where the arguments is getting clearer, because of a related discussion. This now reads as:
"The results clearly show that two data-streams can be successfully integrated with the MPI-CCDAS. The posterior parameter values (Table 2) were different between the FAPARalone and JOINT, as well as the CO2alone and JOINT experiments, showing that the joint use of the two data streams added information to the posterior parameter vector by preventing the degradation of the phenology simulation when trying to fit the CO 2 observations (Table 5 and 4). This is also supported by the fact that value of the cost function of the JOINT assimilation roughly equals the sum of the single data-stream experiments, indicating consistency of the model with both data streams."S

s. 4.1 /l. 85 norm of the gradient, but it's missing of what? costfunction? with respect to what? Please be more explicit here. It's possible to guess what you mean, but that should not be needed.
It is the norm of the gradient dJ/dp. We added this.

4.2/27 what is "magnitude of the phenological seasonal cycle"?
We mean here the average LAI. We clarified this in the text.

l.39 "For the other"... slightly odd sentence, please check

We clarified this sentence

p.12 l.14 f_photo => f_photos - usage not systematic in the text throughout it

We corrected this throughout the text

4.4.1/38 I read it as "an FAPAR" constant instead of "a"

This has been corrected

4.4.2/l57 C uptake , better maybe carbon uptake?

This has been changed in the entire manuscript

4.4.2/last sentence could be better formulated

We reformulated this

5.1/l.80 ranging from 111-151 => ranging from 111 to 151

We changed this

p. 15/l.1 References are quite old. Are there any newer ones available?

Unfortunately there are no more recent references on this

p.18/l.26 Northern extra-tropic => northern extra-tropical.

We have corrected this throughout the manuscript

Last paragraph of conclusions: first sentence quite long, please consider restructuring

We reformulated this