Answer to RC3

Authors describe the assimilation of FPAR and atmospheric CO2 data into the MPI- CCDAS framework and the paper concludes that the assimilation of these two pieces of information allow to tune parameters of the terrestrial ecosystem component so that it performs better after it runs unconstrained.

The manuscript is interesting and GMD is a proper avenue for its publication but in its current format the manuscript is too long, or it appears too long because of its arduous reading since several points are not clear. The framework is not very well described so a reader is left to wonder.

If fact, a missing bit of text has probably obscured that the assimilation procedure is straight forward. We are very sorry for that. 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 ….“
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. Unfortunately the missing text (see later) included some parts of the description of the framework. We also added more details to further describe the system. To reduce the length of the manuscript, we have moved the model description to the Appendix. We have furthermore reworked the text in terms of style and grammar to make the issues at hand clearer.

I am always struggling with the fact how inversions and carbon data assimilation handle the fact that the model must be spun up properly before it can be used. This issue is addressed somewhat in Section 5 but still needs more discussion. In particular, even after reading this manuscript, I am still unclear what value does a prior have when the parameter values have been suddenly changed. In a climate-mode a change in parameter values mean that the model must be spun up again to make its pools reach new equilibrium. As a result, don't the optimized parameters in the MPI-CCDAS system also account for the fact that the model wasn't spun up and brought to the present day using optimized parameters. Also, as soon as the new optimized parameters are used (without the model being spun up properly) doesn't it mean that if the model were to run long enough it will eventually start drifting towards its "true" equilibrium.

In principle we agree here with the reviewer in that the parameter estimates we obtain are somewhat influenced by the choice of the spin-up method. The challenge with the carbon cycle is that the global carbon cycle is not in equilibrium and it is difficult to assess how far it departs from the equilibrium state (e.g. because of lack of historical information on land-use change etc. pp). Simply spinning up the model into equilibrium with the new parameters will therefore not be sufficient, because one will additionally have to run the model in a transient phase with the driving forces (CO2, climate, land-use etc.), which cause the current imbalance. At the current state, this is, albeit desirable, computationally not feasible. An alternative to a correct spin-up procedure, is to have accurate initial carbon pools. Hence we decided (also for runtime consideration – a spin-up is computationally expensive) to allow the MPI-CCDAS to also change the initial carbon pools directly. The relative simple approach adopted with only one global modifier was motivated by the long runtime of the framework (a few months). The results and discussion then reveal that the framework needs to be improved on that aspect. We also agree that the system will drift towards the „true“ equilibrium which is – rather then a deficit – a behaviour of any transient system. Whether the models equilibrium is in accordance with the „true“ equilibrium can only be reasonably assessed with long time series and potentially with repeated applications of systems like an MPI-CCDAS.
We already have this sentence in the text, which to our understanding describes the point very

clearly:
„In addition, we accounted for non steady-state conditions of the net carbon flux by estimating a global scaling factor for the size of the initial slow pool“

I have several handwritten comments in the attached supplement (an annotated ver- sion of manuscript) which indicates the places where sentences and words were un- clear.
See the comments below

The choice of colors in Figures 3 and 7 is really bad which doesn't allow a reader to evaluate results.
Without being more specific here, it is difficult for us to guess where the problems with the colours arise from. Maybe the reviewer was surprised by the fact that the difference maps do not show very large differences (with exceptions, but those have been discussed in the text). Since the other two reviewers did not mention this problem, we leave the figures as they are, unless we get more specific comments on how to improve the colours.

Finally, had the manuscript been in a single column mode with double spaced lines it would have been an easier read.
So far as I know I have no control over the layout that GMD produces with the input files. Sorry for this.

The reply to the handwritten comments follow here:
P1L10:
Computationally efficient refers to runtime, which is a limiting factor in global carbon cycle assimilations. We added a statement to the introduction to clarify this.

P1L16-17:
Assimilation of two data streams does not guarantee to fit both data streams equally well. There could be conflicting model formulations that avoid a good fit to all data streams.

P2L62:
Corrected

P2L63-67:
These processes are the simulated phenology, 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.

P2L91:
Corrected throughout the manuscript (following the GMD-standard)

P2L97:
p_{po} are the posterior parameters. We clarified this.

P3L1ff:
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 ….“
It will be added to the revised manuscript

P3L58:

This is the naturally occurring heterogeneity within the area covered by one grid-cell (e.g.: due to different forest species but also variability within one species). We reformulated this to make the point clearer.

P3L76:
We added more details to the text (also for the temperature memory) and also refer to Knorr et al. (2010) for even more details.

P4L44:
Corrected throughout the manuscript

P5L17:
This has been corrected.

P5L50:
This has been corrected.

P6 Table 2:
The column headings have been clarified.

P7L4-5:
In changing the heterotrophic respiration, the net carbon flux to/from the atmosphere is also changed. As a consequence the atmospheric carbon content and its changes (the growth rate) is also modified. We try to make this clearer.

P7L35:
The term „wider“ is misleading in the text. We didn't intend to say that the set of cross-evaluation stations is larger then the set of stations used for assimilation. We changed the text accordingly.

P7L51ff:
These ancillary flux-fields are prescribed and we give here basically a short reference from where we have taken these field. These fields were not altered during the assimilation. We clarified this in the text

P7 Figure 1:
Yes the colour bar indicates the FAPAR uncertainty (between 0 and 1) and yes the uncertainty of FAPAR estimate is large. We added some clarification to the figure caption to make clear that the colour bar refers to FAPAR.

P8L13:
This has been added.

P8L26:
We mean here the soil carbon pool. This has been clarified

P8L23-36:
No, the model will not approach the prior state, because we changed the model parameters and they will remain at their posterior value also when no constraints are active. We clarified this in the text.

P8L57-58:
Necessary iterations were tens to hundreds and the total runtime was 1-2 months. We clarified this in the text

P8L66/68/70:
This has been corrected

P8L85:
We mean the norm of the gradient of the cost-function with respect to parameters. This has been clarified in the text

P8L87:
Iterations of the assimilation procedure. This has been clarified

P9L13:
Yes this is globally averaged. We clarify this in the text

P9L37:
We meant deciduous needle leaved. We have corrected this in the text.

P9L52:
We assimilate FAPAR observations to optimize model parameters. These are then used to run the model and to simulate FAPAR. So even though the observed and modelled FAPAR should be fairly similar after assimilation, there are still differences (e.g. because of observational or model uncertainties).

P10 Figure2:
The point is the mean and the vertical lines the uncertainties given with the 1*sigma uncertainty. We clarified this.

P12L2-7:
This refers to the period of 2005 – 2009. We clarified this in the caption of table 6.

P12-14:
Yes in all experiments (see table 6). We clarified this in the text.

P13 Figure 5:
We do not show a model vs. observation plot because in the current plot we can give also the information on the latitudinal gradient of the seasonal cycle amplitude (which would be hard to give in a simple model vs. observation plot). Since the behaviour of the latitudinal gradient in the assimilation is a relevant information, we keep this plot, even though it might be more difficult to read then a model vs. observation plot.

P13L3-6:
We directly control the size of the initial soil carbon pool by the modifier f_slow. We clarify this in the text.

P14 Figure 8:
The figure shows the value of posterior minus prior divided by the prior uncertainty. We clarify this in the figure caption.

P14L66-69:
We mean that the difference does not largely influence the models capability to reproduce the high-latitude season cycle of atmospheric CO_2. This has been clarified.

P15L48 - 49:
This is a 3-D data set and also contains temporal information. This is clearly stated in the description of the observational operator representing the atmospheric transport.

P15L80:
We added the suggestion to the text.

P16L75:
We in fact mean what we write. It is not only atmospheric CO2 but it is rather the carbon cycle as represented in JSBACH (e.g. carbon stocks). We clarify this in the text.

P16L86:
The statement is not limited to atmospheric CO2 but is also valid for other observations of the global carbon cycle. Hence we leave this as it is.

P17L40:
With stiffness we mean here, that there are only few degrees of freedom to control the respiration in the MPI-CCDAS. We clarify this in the text.

P17L62-75:
In order to shorten the manuscript we removed this part because the important points are covered elsewhere in the manuscript

P18L11:
We mean, that the current network of CO2 observations only helps constraining the net carbon flux of relatively large regions. Finer resolved features (e.g. on the scale of European countries) are not well constrained.