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Interactive comment on "Constraining a land surface model with multiple observations by application of the MPI-Carbon Cycle Data Assimilation System" *by* G. J. Schürmann et al.

Anonymous Referee #2

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REVIEW OF: Constraining a land surface model with multiple observations by application of the MPI-Carbon Cycle Data Assimilation System By G. J. Schurmann et al.

GENERAL COMMENTS:

The paper describes a new carbon cycle data assimilation system based on the JSBACH land surface model and the assimilation of two major data streams: FAPAR and atmospheric CO2 concentrations (using TM3 model to relate surface fluxes to concentrations). The paper highlights the benefit of using the two data streams as well as their potential complementarity to constrain the carbon cycle. The study is relatively comprehensive and provides an honest description of the strength and weaknesses of the system. It is relatively new in the sense that it uses an advanced process-based

land surface model that serves as the land surface component of an Earth System model. It provides some new insight on the potential of CCDAS and I thus recommend its publication in GMD. However, I have several comments and question as well as few recommendations that I would like to be taken into account to improve the manuscript.

As a general remark the paper is quite long and there are several redundancies that could be avoided: - First i would suggest to put the detailed description of the model equations in an appendix with only a section in the main text that resumes the principles and highlights the main parameters. This is not mandatory but a suggestion. - Sometime the discussion sections repeat the descriptions of the results in section 4, which could thus be avoided. The conclusion seems could maybe be grouped with the outlook

The selection of TIP-FAPAR data: I do not understand that the criteria to reject data (i.e. a prior correlation with the model output lower than 0.2) leads to disregard completely the temperate deciduous ecosystems (Europe, USA,. . .). Figure 1 reveals that mainly the boreal ecosystems and the tropical ones are kept. The result of such selection poses some questions that are important to discuss; The authors should mention how many PFT are kept after the selection and how many grid-cell are retained for each PFT as well as why the model behaves so badly for temperate ecosystems so that these grid cell are rejected. This is interesting as usually most LSM perform relatively well for deciduous temperate PFTs.

One important results concern the distribution of the net C terrestrial uptake. The larger sink in the northern high latitude compare to the other latitude bands (temperate around 40°N or the Tropics) is a strong feature of the MPI-CCDAS. The fact that suc sink occurs mainly in Siberia where the needle-leaf deciduous trees (Larix) dominate (East Siberia) can also be related to the fact that there are not many atmospheric stations around this area (except in the southern part). The differences in terms of NBP with the adjacent ecosystems (western part of Siberia) need to be discussed. To my mind this may be an artifact of the system and may not reflect the "true" distribution of the land carbon sink.

Given the implication such spatial pattern may have for our understanding of the carbon cycle I suggest a stronger discussion of the potential weaknesses of the systems for the attribution of the net C flux; especially with a discussion of the “confidence” the author have in this partitioning. Section 4.4.2 describes the differences between the tests in these boreal regions but I think it should discuss more how “reliable” the main results are.

SPECIFIC COMMENTS:

Method (section 2 and 3):

* P2,L25-40: The paragraph mixes a review of data assimilation system based on different data stream and different methods. I would suggest to separate more the two issues (data and method). Also the review about the different data streams is not complete and misses studies that have assimilated satellite NDVI/fAPAR observations for example. The Luke (2011) PhD reference is not informative, as the data that are used are not mentioned.

*P2, L55-60: It would be clearer if the authors define what is the “original CCDAS” and clarify that CCDAS encompasses the assimilation of several data stream and not solely atmospheric observation.

* P2, L55: The introduction should clearly mention the use of the two types of observations they are considered. The objectives and the questions that are posed do not reveal a major focus of the study: the complementarity of atmospheric CO2 and TIP-FAPAR data. This should definitely be presented in the introduction.

* P2, L101-: The authors should provide briefly the principle of the “Davidon-Fletcher-Powell” algorithm (whether it needs and approximates the hessian of J).

*P3, L25: “differentiable implementation of J(p)”: This is not clear and I guess it is more a differentiable implement of some equation in the code but not of J(p) ?

*P3, L35: It is not clear what the author refers to with “through evaluation of sqrt(0) in

the forward mode” ?

* P3 Equation 5: it would be good to precise the meaning of the different “control” parameters already in section 2.2.1 (and units), although the optimized one are described in Table 2

*P4, L58: “PFT values are integrated…”: which PFT values ? the GPP or the parameters?

*P4, L77: how many layers has the soil water scheme?

*P4, L83: It is not clear to which diffusion equation you refer to? (equation 15 ?)

*P5 section 2.2.5: There is no mention of biomass burning fluxes. The authors should justify why they have not also used an estimate of biomass burning as this may play a role especially for the trend at atmospheric station (given that the net biomass burning flux is roughly 1 PgC/year). The choice of only one constant offset for the atmospheric CO2 background poses the problem of the spin up of the atmospheric CO2 gradient. The authors should discuss this issue as it may significantly bias the parameter optimization. The mention later in section 3 that they use 2 years for spinning up the atmospheric gradients, which may be not enough. One way to address this issue is to mention if the simulated gradients after two years are relatively similar to the ones obtained after more years with the prior parameter sets.

*P7, L1: Why do you optimize only the size of the slow pool. You should justify with typical order of magnitude why the different litter pools are not considered (like with the mean residence time of each pool)

*P7,L40: The paragraph on the description of TM3 should not be placed in this section which deals with atmospheric CO2. It should be in section 2.2.5. It is quite strange to mention the “fine grid” of TM3 given that it is at 4 by 5 degree resolution which is a very low resolution compared to existing studies and which thus may have an impact on how you can accurately simulate the spatial gradients between “continental stations”.

*P7,L65-69: This discussion of the uncertainty in the FAPAR data does not touch the crucial point of potential biases. Indeed several previous studies (Kaminsky, 2012, Bacour 2015) have shown that FAPAR satellite data may be biased (because of different issues like saturation at high values,. . .) and that it is crucial to deal with these biases before any assimilation in a process-based model. This crucial issue should be at least discussed! I fear that if you would use a product with higher fAPAR values you would end up in very different estimate for the GPP and still a fit to both data stream.

*P8,L13: It is confusing to mention the resolution of 8 x 10 here while in section 2.4.1 you mention the resolution of 4x5 for TM3. Please make it more clear between the two section to which resolution you effectively used TM3 and if you use the same resolution for JSBACH and TM3.

Results (section 4)

*P 8, L66: should be the “cost function”

*P9, L40: the sentence needs to be corrected.

*P9, L39: Figure 2: This figure is not easy to read and I would suggest to decrease the number of year or to show only a mean seasonal cycle so that we could see more clearly the change in the timing of the model FAPAR.

*P9, L53, Figure 3: It would be more logic to plot in panel b: “Joint minus Prior” as you discuss the reduction of the LAI during the optimization.

*P11 section 4.3: Table 5: you should mention for the biases, which way it is: model – obs or the reverse.

P11 section 4.3: As a general remark it is not easy from figures 4-5 and table 5 to see the improvement in terms of the phase of the seasonal cycle. I would suggest to calculate with the detrended time series a metric that reveal the phase changes, either the correlation or the length of the “carbon uptake period”. This would complement the diagnostic of figure 5 on the mean amplitude.

*P13,L5: The change in the initial soil carbon pools, around 50% is huge and suggests that most of the global CO2 growth rate is matched by adjusting this unique scaling parameter. Although this is discussed later, it should be mentioned already that this will be discussed later as being a potential “limitation of the optimization set up”.

*P14, L30-34: sentence is too long and not clear. Need to be rewritten.

Discussion (section 5):

*P15L14-29 : This paragraph is not precise enough as for the “C in vegetation”: whether you speak about above ground biomass, total biomass, soil C content,. . .. Please be more precise. The comparison to other estimates is interesting but you should have focus in such “discussion section” on a critical evaluation of what may be not accounted for in your model so that it could be pointless to try to be close to some independent biomass estimates. One potential bias is the steady state assumption for the vegetation so that the forest are mature while the “data driven” estimates of biomass account for the fact the most forest are relatively young compared to a mature forest. For the soil carbon the decrease by 50% of the prior initial soil carbon content lead to a value that compares favorably with the HWSD data. So this mean that the model itself tend to produce too much soil carbon or that the turnover of the soil carbon is not appropriated. These issues should be at least mentioned.

*P15, section 5.2: last paragraph about the net carbon flux. You don’t mention the fact that your system neglected the net deforestation flux that would in principle add another C source to the atmosphere and would thus lead to a larger biosphere C uptake to balance the atmospheric CO2 growth rate. This should be at least raised as a caution when comparing to GCP estimates (or precise if you took for the GCP the net flux including deforestation).

*P15 section 5.2 first Paragraph: It would be interested to know whether the use of different spatial resolution with the JSBACH model may change or not the results.

*P16 , L10-25: the discussion about the unique “Fslow” parameter could be a bit strengthened. First you should mention the additional cost (computation wise) that has prevented from the split of this parameter into several regions ? Also it would be interesting to see what the model provides in terms of soil carbon after a spin up with the new optimized parameters. How much the decrease in GPP lead to decrease the soil C content at equilibrium compared to the 50% requested decrease (through Fslow parameter) ?

*P16, L27: the conclusion that a better estimate of GPP in the tropic with additional constraint will likely improve the net CO2 flux is not obvious. As you say above the constraint on the net C flux does not lead to a direct constraint on GPP so the reverse is probably the same. Else the authors should detail the argument.

*P16, last Paragraph of section 5.2: I found the discussion about the NPP not very informative for a general audience and I would suggest to drop it, given the current length of the paper.

*P16, section 5.3, first paragraph: The first sentence is difficult to understand? Please consider rewriting; Line 60: it is not clear what the “alternative method” refers to?

*P16: Overall section 5.3 is not really informative and does not really provide a critical appraisal of the current MPI-CCDAS (the title). I would either just drop it, or discuss more fundamental issues due to the resolution of the transport model, the limited set of parameters (like Fslow), the restricted coverage of FAPAR data, the key potential limitation of the system to fully “model/explain” the net carbon fluxes (biomass burning, N cycle, land use change, forest age, ….).

*P16, L85-90: I disagree with the argument that using a sequential design for assimilating several data streams leads by principle to a different result than using a simultaneous approach. Theoretically the Bayesian theorem could be recast in terms of conjunction or multiplication of probabilities so that it could be equivalent to use a sequential or simultaneous approach, provided that you can carry all the information

about the parameter PDF from one step to the next. However, the practical implementation of the optimization system (such as for instance the use of Gaussian errors, the inability to calculate fully the whole PDFs,….) generally lead to differences between the two approaches but it is quite difficult to fully establish which one is superior as you may also have “some benefits” of not exposing certain parameters to certain data streams in a sequential approach. I thus strongly recommend to rewrite this part in order to clearly state that the difference comes from the implementation of the CCDAS rather than from a theoretical point of view.

*P16-17, Section 5.3.1 last paragraph: there is some redundancy concerning the gradient of the cost function not approaching zero for CO2 data with the same description in section 4.1, second paragraph. To decrease a bit the length of the paper it could be good to avoid repetition between these two paragraphs. But more importantly I fear that the proposed tests are not really going to help resolving this issue, as it is most likely due to a “minimization problem” related to the computation of an accurate gradient of the cost function or to limitation of the chosen algorithm in specific non linear circumstances.

*P17, section 5.3.2, second paragraph: As mentioned above it would be good to discuss here the value of the soil carbon content following a spin up performed with the optimized parameters to see how much of the decrease would arise from lower GPP. Potentially the discussion on this initial C pool scalar that occurs in several place in the paper could be group in this section (a suggestion).

*P17 section 5.3.2, last paragraph: the discussion on the “reduced prior estimate for the coniferous evergreen PFT” (L74) is not easy to follow. You should precise that the reduce prior estimate concerns the maximum foliar area in this sentence. I think that this pertain more to the method section and does not need a whole paragraph.

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