

Answer to RC2

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.

We follow the suggestion here, especially because other reviewer also suggested a restructuring of the methods part. Section 2.2.1 to 2.2.4 (Detailed JSBACH description) have been moved to the appendix) and some more description of JSBACH and the relevant parameters have been added to the section on model parameters.

Sometime the discussion sections repeat the descriptions of the results in section 4, which could thus be avoided.

We agree with the reviewer. The duplication parts in the manuscript have been removed.

The conclusion seems could maybe be grouped with the outlook

In the outlook section, we express our opinion about promising further development of the CCDAS. We think it is work putting such a section into a manuscript that describes a model development and we also think that this content does not belong to conclusions. We thus have renamed this section to „Further development of the system“ and have merged some parts with the discussion to further streamline and shorten the text.

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.

Here the reviewer misunderstood some parts. The temperate deciduous ecosystems are not omitted from the assimilation because they showed a poor correlation with the data, but as a result of the other selection criteria of omitting crop-dominated ecosystems: The temperate deciduous ecosystems for Europe and the US are collocated in grid-cells with a large fraction of crops. This leads to their omission. Because several PFT's occur in one grid-cell, it is not meaningful possible to summarize the reduction of PFT's in only a few numbers. We adapt the manuscript in section 2.4.2 to make this point clearer:

“First, owing to the fact that no specific crop-phenology is implemented in JSBACH, grid cells with fractional crop coverage of more than 20 % have been filtered out, as we cannot expect the model to fit cropland phenology. A consequence of this filter is to mask the deciduous broadleaf PFT in the

US and Europe, because in these areas, this PFT is collocated in crop-dominated pixels. Hence, the phenological parameters of the deciduous broadleaf PFT are only constrained by observations from other locations - a fact that should be kept in mind when interpreting the deciduous broadleaf parameters.”

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.

This description is perfectly in line with what the authors think about the East Siberian sink. We will state this clearer in section 4.4.2 where we added the following:

“This largely increased sink in Eastern Siberia could be an artefact of the set-up used for the data assimilation in this study. No nearby atmospheric stations constrains the net carbon sink in this region adequately, and the CD PFT only occurs dominantly in this region. In consequence, the PFT’s parameters can not be adequately constrained by carbon cycle observations from other parts of the globe. This relative scarceness of observations and independency of other regions allows the East-Siberian net carbon uptake to compensate for other regions fluxes in order to match the global growth rate. Additional observations would be required to allow for spatially higher resolved estimation of the net fluxes.”

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.**

The paragraph was not intended to give an ample review on the assimilated data streams, more it was thought of method review while still mentioning the assimilated observations. We rewrite this paragraph to clearer separate aspects related to the method and to observations. We also refer to more works related to NDVI/fAPAR in the revised manuscript. Nevertheless a complete review on the topic of assimilating NDVI/fAPAR (or LAI) from satellite would deserve much more space then is available in this manuscript. Hence we keep it short.

Luke (2011) uses the MODIS collection 5 LAI product, see the paragraph with heading "MODIS LAI" in her section 6.4.1 (page 174).

***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.**

We refer to the BETHY-CCDAS (with a reference on the overview article of Kaminski et al. 13).

We added this to the manuscript and state clearly that the CCDAS assimilates more then one data-stream.

*** 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.**

We agree with the reviewer, that the complementarity of atmospheric CO₂ and TIP-FAPAR is a major outcome of the study. Since this was not the primary intention of the work, we did not put this as an objective into the introduction. Nevertheless we add a short sentence to make this important point clear already 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).**

This algorithm approximates the Hessian of J. We added this clarification to the text:

“Technically, J is minimized by a quasi Newton approach with so-called Broyden-Fletcher-Goldfarb-Shanno (BFGS) updates of the Hessian approximation, in the implementation provided by the Numerical Recipes (Press et al., 1992, dfpmin routine)”

***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) ?**

In fact, all code of the forward model that contributes to the calculation of J(p) needs to be differentiated. As long as the net-flux of CO₂ is involved this requirement is met by almost the entire JSBACH-code. We clarified this in the text:

“The application of gradient-based minimisation procedures is facilitated by a differentiable calculation of J(p). According to the chain rule, this ultimately requires all code parts of the forward model that depend on the control variables and impact the cost-function to be differentiable.

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

The differentiation of a code with sqrt(0) leads in the differentiated code to 1/sqrt(0). We clarified this in the text:

“e.g. through differentiation of 0 in the forward mode leading to $\sqrt{1/0}$ in the differentiated code”

*** 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**

We have moved the detailed JSBACH description into the appendix and added only a brief description of the model with a focus on the control parameters into the methods section.

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

The PFT-dependent GPP is aggregated to a grid-cell GPP according to the fraction cover of each PFT. This is clarified in the text:

“GPP - values per PFT are integrated to grid-cell averages according to the cover fractions of each PFT within each grid-cell.”

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

The soil layer scheme has 5 layers. This has been added to the description of the soil-scheme.

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

Yes it is equation 15. We add this reference to the text

***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.

We have not explicitly accounted for biomass burning fluxes and rather treat it as a respiratory flux. This can impose problems in the parameters optimisation, since this simplification may yield to compensating effects in the parameters estimates – especially because we only have a few degrees of freedom to adjust respiration, a fact well discussed as limitation in the manuscript. The alternative of adding the biomass burning fluxes as a background term (similar as fossil fuel and ocean carbon fluxes) introduces an inconsistency in the model, because the burnt carbon would need to be removed from the carbon stocks and post-fire dynamics would need to be accounted for. We already briefly discussed this issue in the manuscript but for this first application of the newly developed MPI-CCDAS we decided to focus on the most important processes and leave the inclusion of others to the further development. To make clear that we ignore the biomass burning fluxes, we add a statement about it to section 2.2.5:

“Biomass burning fluxes are not explicitly included (see also discussion in Sect. 5.6) and these fluxes are consequently mapped to the respiratory part of JSBACH during the assimilation of atmospheric CO₂.”

The latitudinal gradient of CO₂ is stable after one year of spin-up. The difference between Mauna Loa and South-Pole in January is 0.4 ppm and for the second year it reaches 4.8 ppm. For the subsequent years it is variable (without a visible trend) within the range of 4.7 to 5.5 ppm. We added the following statement about this:

”After the second year, there is no visible trend in the difference of observed CO₂ at Mauna Loa and South Pole. Thus 2 years are sufficient to spin-up the atmosphere”

***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)**

The slow pool has a turn-over time-scale of 100 years where for example leaf litter has turn over times of a few years (depending on PFT). The reason for including only one modifier for the slow pool is mainly a computational one in order to limit the length of the parameter vector to be optimized. This is one of the main factors controlling the run-time of the system. We have chosen the slow-pool because it shows by far the longest turn-over time of 100 years, whereas for example the leaf litter has turn over times of a few years (depending on the PFT).

We admit that this might influence the estimation of the slow pool, since any discrepancies in any of the faster pools will be compensated by the slow pool. We added this clarification to the parameter description:

“For this first application of the MPI-CCDAS, the most slowly varying pool has been selected (i.e. the soil carbon pool with a turn-over time of 100 years). The initial conditions of other carbon pools were not included in the control vector to avoid the associated increase in the computational burden (e.g. run time). This consequently includes the risk of assigning any misrepresentation of modelled pools sizes to the soil carbon pool and the changes in the carbon pool sizes after the assimilation should be interpreted with care.”

***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”.

Our initial intention was to put the TM3 description to the CO2 observation as an observational operator, because in principle any atmospheric transport model could be used to produce the matrices. But we agree with the reviewer, that it fits better to the description of the atmospheric transport. We also agree that 4 by 5 degrees is not a fine grid, but the TM3 naming is such that this grid is called „fine grid“ and that is the reason why we mention this here.

We moved the TM3-description to the section about atmospheric transport.

***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, Ba- cour 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.**

Yes there is saturation. It is, however, intrinsically addressed through the large uncertainty ranges over dense canopies. This is now clarified in the manuscript: “In this context we note that the per-pixel uncertainty ranges in the TIP-FAPAR product also reflect limitations of the information content that can be derived from sunlight reflected to space in the optical domain (i.e. the input to TIP) in particular over dense canopies.”

We recall that the focus of this study was not to assess solely FAPAR as a data stream but the joint benefit of the data streams. We also discuss the issue of correcting for the bias in the prior model and observations and that this has a pronounced impact to the posterior GPP and respiration. Further we also clearly discuss, that GPP is not well constrained.

***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.**

We used 4x5 for the atmospheric transport, but 8x10 for JSBACH. We will make this point clearer.

Results (section 4)

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

This has been changed

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

We reformulated that sentence

***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.**

Since no relevant information is lost, we now only show 2 years of data.

***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.**

Thank you for pointing this out. We changed the sign of the plots and their title accordingly

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

It is model – observations. This has been added to table 4 and 5

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.

A phase change in the atmospheric CO₂ is hardly visible, which is the reason why we did not analyse it in more detail. At the monthly temporal resolution we apply here, we doubt that a metric for the phase change can be meaningful interpreted given that the change will be smaller than one month. Hence we decided not to add this diagnostic.

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”.

We now mention, that this will be discussed later.

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

We shortened this sentence to:

“Through the effect of net photosynthesis on canopy conductance (Eq. A14), the potential transpiration rate (E_{pot}; Eq. A5) was strongly decreased.”

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 HWSO 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.

Vegetation carbon in JSBACH is including carbon stored in all living parts of the vegetation above and below ground. The total carbon of the ecosystem is then the sum of this vegetation carbon, litter carbon and soil carbon. A more precise description of vegetation carbon is given in table 6 and the text.

We see some value in simply putting the modelled vegetation stocks (and their changes) in context to other estimates without a detailed discussion of the shortcomings of all the estimates. We decided to give the global number of all relevant stores and fluxes of the modelled global carbon cycle to allow for later comparison of our study with others, and also to allow identifying any major biases in the simulated global carbon cycle. We agree that a more in depth comparison of the different estimates would be desirable, but also agree with the reviewer that potential model shortcomings prevent such a close and in-depth evaluation.. We have not done this in the current work, because the focus was on the CCDAS model description and the implication of the data assimilation and not on the evaluation of the prior model itself, and potentially model biases that directly result from imperfections in the model formulation. We add the following note on this to the paragraph:

„A detailed comparison on the simulated vegetation and soil carbon stocks of the prior model is beyond the scope of this paper, partly because of the simplifications of the spin-up procedure entail biases in predicted vegetation carbon stocks, as transient land-use changes and forest management, affect-ing forest age structure are ignored. It is nevertheless instructive to provide context for the simulated vegetation and soil carbon stocks by comparing them to the global totals of independent

estimates.“

There is indeed a strong reduction in modelled soil carbon of JSBACH after the application of the MPI-CCDAS. But whether this means, that the prior model produces more carbon or whether the uncertainty of the HWDS data is too large to avoid such a conclusion is out of the scope of this manuscript. But since one of the main conclusions is, that the systems needs to be improved in terms of flexibility in constraining the respiration parts of the model, too much interpretation of the 50% reduction in soils stocks should be avoided.

***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).**

So far land use change emissions have not been accounted for in JSBACH. We have clearly discussed this in the outlook-section and similar reasons as for biomass burning fluxes (that imposing this flux would lead to inconsistencies with the stocks and fluxes simulated by JSBACH, as regrowth effects would have been ignored) led to the decision not to include this in the first MPI-CCDAS setup. We reported the „residual terrestrial sink“ of the GCP estimate, which does not include land use change emissions. We clarified these points in the manuscript.

***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.**

Yes this would be in fact interesting. But we have not conducted experiments with different resolutions and it was not the intention of this article to touch every unresolved point in applying a CCDAS. It was rather a systems description, that allows assessing these critical points in later works. Hence we do not feel capable of adding anything of substance about this point to the manuscript.

***P16 , L10-25: the discussion about the unique “F_{slow}” 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 F_{slow} parameter) ?**

We refer here to the discussion in section 5.3.2, which covers this aspect. We disagree about the added value for giving initial soil carbon stocks computed with a posterior-parameter spin-up.

We clearly made the point in section 5.3.2 that this is a weak point of the current system and further discussing this point without improving on the shortcomings seems not appropriate.

We added the following statement about the run time to the discussion in section 5.3.2 „Parameter set-up“:

“This choice was made because allowing to control the spatial structure of the carbon pools would require several more parameters to be optimized, which would very likely suffer from a strong equifinality problem, and which would considerably extend the already lengthy run-time of the MPI- CCDAS”

***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.**

Our argument refers to both, GPP and ecosystem respiration (the gross fluxes). Once these two fluxes in the tropics are well constrained, this also counts for the net-flux. A well-constrained tropical net flux will have beneficial impact on the estimation of the global net fluxes. We clarify that we refer to GPP and respiration.

***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.**

We agree that one could skip this paragraph. We follow the suggestion of the reviewer and delete this paragraph.

***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?**

See the following comment.

***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, . . .).**

We follow here the suggestion in the next comment to largely drop this section. Parts of it are included in the outlook section

***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.**

We may have formulated our argument too strictly but we still think that our argument is valid and gives important insight in how to set up an assimilation system. If one would be able to compute to full posterior PDF (probability density function), the underlying model likely is computationally as fast that it is not necessary to employ a tangent-linear assimilation procedure, but one could chose a more costly algorithm (like e.g. MCMC; Monte Carlo Markov chain). Further, the need to implement a sequential design (sequentially in the order of the ingestion of the data streams, not sequential in time as is the case for e.g. Kalman filters) often comes with limiting the parameter vector for the one or the other data stream. In doing so, the linkages between parameters is broken (you cannot propagate information to a parameter that is not optimized in one of the steps of the sequential approach). Our example points towards problems with such implementations and we think it is worth leaving this part of the discussion in the manuscript. As the reviewer suggests, we reformulate this paragraph to make this point clearer:

“An implementa-

tion of such a sequential assimilation likely reduces the number of parameters to be optimized in each step, and therefore allows a quicker solution of the optimisation problem. However, this advantage comes with the cost of breaking the linkage between parameters can lead to situations, where the posteriori results of a sequential assimilation experiment will not match the observations

equally well as with a simultaneous assimilation.”

***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.

We further agree with the reviewer that the proposed tests are not solely to resolve this issue but also will shed light to other questions regarding the application of a CCDAS.

We removed the redundancy in the results section since it seems more appropriate in the discussion and we added the reviewers idea of how to assess this problem to the text:

“Investigation of the non-linear nature and potential numerical issues regarding the computation of the gradient $\partial J / \partial p$ (Eq. 1) might be needed. Further tests with alternative station network settings, parameter priors or time-periods will provide more insight into approaches to tackle this issue.”

***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).

We refer here to the discussion above and consequently do not add the number of the carbon pools

***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.

Basically we agree with the reviewer that this paragraph belongs more to the method sections. There the reduction of prior LAI already is mentioned and hence we omit this paragraph.