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Interactive comment on "A new step-wise Carbon Cycle Data Assimilation System using multiple data streams to constrain the simulated land surface carbon cycle" *by* P. Peylin et al.

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Peylin and colleagues describe a Carbon Cycle Data Assimilation System based on the land-surface model ORCHIDEE optimized against NDVI data, eddy covariance CO2 flux data, and atmospheric CO2 data. For practical reasons, these three data streams are used successively in three steps. The paper describes the system, assesses its performance (especially the self-consistency across the three steps), and some features of the resulting carbon cycle fluxes and stocks. The authors conclude that the ORCHIDEE land-surface model is now structurally adequate enough to bridge the information from the three data streams, though they also highlight further steps that need to be taken to represent the global carbon cycle more accurately.

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Discussion paper

The study represents an interesting and relevant development in the understanding of the carbon cycle consistent with available data. There are open issues (for example the short assimilation period precluding various processes to be constrained and assessed) but these are clearly acknowledged in the paper. I find the presentation clear and convincing. In my opinion, the work should be published in Geoscientific Model Development.

We thank the reviewer for his positive appreciation of the manuscript.

Minor comments:

p 5 l 6: The associativity is true for linear systems, but is it really also true for nonlinear systems? (I nevertheless agree to the arguments given in favor of the step-wise approach.)

The associativity argument, detailed in Tarantola (2005), relies on the combination of probability density functions (PDFs) through the Bayes theorem without any assumption on the linearity or non-linearity of the system. It is a general property associated to the combination of probability distribution (i.e., the description of the probability of an event, based on conditions that might be related to that event). So the non-linearity does not invalidate the step-wise approach; it only highly complicates the computation of the full PDFs at each step and thus their propagation. The necessary simplifications that are made in this case lead to the complication investigated in the paper. In the case of a linear system, we could have easily calculated and propagated the full PDFs. We have completed one sentence in the introduction to reinforce that the associativity does not depend on the linearity of the system: “This multiplication is associative so it makes no differences whether it is performed in one step or several (and whether the system is linear or not).”

p 8 l 13: What is the uncertainty due to incomplete sampling of the diurnal cycle?

We have not estimated the uncertainty due to a possible 20% gap. However, in order

to compute the daily mean values, we have used gap-filled data so that the uncertainty only arises due to errors in the gap-filling procedure. Such uncertainty, usually less than 20 % (Lasslop et al. 2008, would thus only affect 20% at maximum of the diurnal cycle; it would thus have an overall negligible impact compared to the model uncertainty that need to be also accounted for.

p 14 l 25-26: I was wondering whether the presence of step functions, creating discontinuities, still allows a well-defined solution of the minimization?

We agree that step functions may complicate the minimization problem, creating potentially non-smooth cost function, i.e. with a “singular” point where the derivative is not defined and not continuous. However the use of finite differences to compute the gradient for these parameters allows defining a “mean derivative” at any point (mean as for non local). As a result the iterative approach (BFGS algorithm) used to search for the minimum may oscillate if we end up for a given parameter in the vicinity of the discontinuity, but it will not diverge. However such situation is very unlikely to occur and we have checked that no obvious oscillation for each parameter was occurring. We completed one sentence in the text to precise this point: “A finite difference approach was used for these parameters in order to define a mean derivative at any point”.

p 17 l 25-30: Add references for "University of Stuttgart" and "ENSTO-E". Explain abbreviation "IER".

We now explain the “IER” abbreviation and we provide two web references for both "University of Stuttgart" and "ENSTO-E".

p 20 l 14: What does "conditions" mean here?

We have change the text to precise that “conditions” was referring to station sampling air “representative of different geographical regions of influence”.

p 21 l 4: Clarify whether this is the prior before step 1 or before step 3?

It is the prior of step 3 and we have now clarified the text.

p 22 l 21: Can you give just a brief summary of the reasons here?

We have improved the text to provide a brief summary of these reasons: âĂć The NDVI or fAPAR mainly constrain the timing of the ecosystem GPP and only to a small extend the amplitude of the GPP, given that the satellite signal is likely to saturate during the peak of the growing season. âĂć The NDVI or fAPAR do not constraint at all the ecosystem respiration, which is an important component of the NEE at least at seasonal to annual scales.

p 25 l 24: These are clearly not the numbers shown in Fig 10 right.

We apologize for such mistake as the numbers correspond indeed from a previous experiment. We have corrected the text with the exact numbers from figure 10b. Note that the exact numbers from figure 10b do not change the overall message and the rather small reduction of the GPP from the prior to the posterior of step 3.

p 27 l 6: Can you explain (here or earlier) why you used individual grid points rather than the whole grid?

The main reasons for choosing only a set of individual grid points are twofold: âĂć First, we proposed to used only the model grid point that are covered by a vegetation fraction greater than 60% for a given PFT, avoiding the use of grid points with a large mix of PFTs so that the optimization of the phenological parameters for each PFT is more straightforward. This is explained in section 2.4.1 (step 1). âĂć We then limited the selected set of points to 15 in order to significantly reduce the computing time of the step 1 optimization. Doing so further allows evaluating the optimized model at all pixels that were not used for the optimization. We added a sentence in section 2.4.1 (step 1 paragraph) to better explain our choice.

p 29 l 10: I think you should also mention the errors in the prescribed fossil fuel and ocean fluxes.

We added at the end of the paragraph a sentence to mention the importance of the

errors associated to the prescribed fossil fuel and ocean fluxes.

Fig 5: I don't see any grey lines.

We have corrected the caption as we do not included the "prior of step 1" in this version of the figure in order not to overload the figure with too many curves.

Typos and suggested formulations:

p 2 l 7: "uncertainties of simulated carbon fluxes and stocks"

Corrected

p 3 l 15: "Carbon cycle componenets"

Corrected

p 7 l 13: "propagate"

Corrected

p 8 l 7: missing "-"?

Corrected

p 10 l 1: "the third step"

Corrected

p 12 l 5: suprious "the"

Corrected

p 17 eq 7: The first ")" seems to belong to the index. There seems to be a "," missing before "LAT".

Corrected

p 17 l 19 "outgassing"

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Corrected

p 18 l 29 Why "Fig 8"?

We wanted to refer to the figure displaying the parameter values before and after the optimization. This figure is Fig. 9 and not Fig. 8. We have thus corrected.

p 22 l 17: "from" rather than "between"?

Corrected

Fig 5 caption: "optimized"

Corrected

Fig 7 caption: prior before step 1 or before step 3?

We have précised that it is the prior of step 3.

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