Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-13-AC4, 2016

Geoscientific
Model Development
Discussions
Open Access

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.

P. Peylin et al.

peylin@lsce.ipsl.fr

Received and published: 14 June 2016

General comments: The manuscript presents a sequence of parameter estimation exercises for the ORCHIDEE Land Surface Model using a CCDAS data assimilation framework. Firstly, NDVI data are assimilated at point scale. Secondly, FLUXNET data are assimilated at point scale. Thirdly, atmospheric CO2 data are assimilated at global scale. The presentation of the material is excellent, despite some minor inconsistencies.

We thank the reviewer for having commented our manuscript. We explain below why we disagree to some of his major comments.

The novelty of the material is limited. What the authors present as a step-wise system, are in fact three systems that are operated in a sequence. The interface between these systems is minimal: It consist of selected parameters with error bars but excluding the error covariance that are passed in one direction. The step-wise approach is not new. It is desribed, for example, by Rayner et al. (2005): They assimilate NDVI in the first step and atmospheric CO2 in the second step. The system for assimilation of NDVI is described in more detail elsewhere (MacBean et al., 2015). The system for assimilation of FLUXNET data is described in more detail elsewhere (Kuppel et al., 2012, 2014). What is left is the system for assimilation of a single data stream, i.e. the atmospheric CO2 data from 2002 to 2004. The description of the assimilation method is provided elsewhere (see above references). The ORCHIDEE LSM, the LMDz CTM and the use of influence functions was also described elsewhere (see references in section 2.3.2). The assimilation of atmospheric CO2 using a combination of an LSM and a CTM and prescribed emissions from other components of the carbon cycle is not new either. It was presented by Rayner et al. (2005) and applied for a time span of two decades. In summary the manuscript is not suitable for GMD because it fails to present "substantial new concepts, ideas, or methods".

We disagree with the main criticism that our study does not provide new ideas or methods. In order to explain this further we need to lay out the evolving state of carbon cycle data assimilation.

Systems that apply the well-established methods of data assimilation to models of the carbon cycle at various scales have been around for nearly two decades. Wang et al. (2001) and Kaminsky et al (2002) antecede Rayner et al. (2005). The problem pointed out by Rayner et al. (2010) is that information was not transferable between either different sites or different datastreams. Rayner et al. (2005) for example, did not expose any of the phenological parameters of the assimilation from Knorr et al. (2001) in their assimilation so there could be no test of consistency. Rayner et al. (2010) pointed out that evolution of both models and methods was necessary for comprehensive assimilation. The current paper represents an important step in that evolution though by no means the final one.

âĂć The paper describes for the first time (to our knowledge) a system that is able to assimilate three major carbon cycle data streams (vegetation activity from satellite, FluxNet data and atmospheric CO2) in a process-based land surface model used as the land component of an Earth System Model (ESM). No such system has been described so far, although this is a major challenge given the differences obtained for the carbon cycle in the last collection of model used for CMIP5 exercise (last IPCC report).

âĂć The reviewer slightly overstates the achievement of Rayner et al. (2005). Although it did use soil moisture and radiation fields from an earlier assimilation from a related model (a simpler version) this was irrelevant to the narrative of that paper. The fields could just as easily have come from a direct satellite product. There was little comment made on the consistency between the two assimilations and no parameters passed between them. Since then, numerous global scale carbon cycle data assimilation studies have been published (above 10) all of them contributing additional aspects to these systems. However in those 10 years a third and crucial data stream, namely the widely used FLUXNET network of net CO2 and latent heat flux observations, has not yet been included with FAPAR and CO2 data in a global scale assimilation. All other studies to our knowledge use FAPAR data and CO2 or FLUXNET data, but none have used all three.

âĂć The focus of the paper lies in the combination of these three data streams while the individual papers MacBean et al. 20015 and Kuppel et al. 2012, 2014 focus on the impact of each individual data stream. The major result of the paper is that for the first time "a state of the art global land surface model part of an ESM is able to capture with a reasonable accuracy the information content of three major data streams following a data assimilation procedure". This is no insignificant feat as it opens new perspectives to reduce the spread of the land carbon sink simulated by the CMIP5 suite of models

and thus to reduce possibly the uncertainty in long-term climate predictions.

âĂć We acknowledge that the chosen approach may not be the optimal one in a statistical sense with only the propagation of the error variance of the optimized parameters (see more detail in the response to the next comment).

âĂć We and the reviewer agree that this is, fortunately, a burgeoning field of activity. Raoult et al (2016) and Sherman et al. (2016) are contemporarily studies, that rely on land surface models of an ESM. They present different strengths and weaknesses from this paper. However we are confident that the paper does represent an advance in available methods. Raoult, et al. (2016) only uses FluxNet observations to optimize the parameters of the JULES model, while Schürmann et al. (in revision) only assimilate two data streams (fAPAR and CO2) in JSBACH model (at coarse resolution, 10° x 10°). Note finally that the level of complexity of the ecosystem model is part of the problem: achieving an optimization with a simpler model does not guaranty that the framework would work with a more complex one.

Overall, the paper relies on old data assimilation concepts (published way before Rayner et al. 2005) but provides a new implementation (3 data streams with a state of the art component of an ESM) and opens the road for improved carbon – climate coupled simulations and improved climate predictions.

We acknowledge that we have not done enough to highlight the new features of this study, and thus we have emphasized these points in the "introduction" and "discussion and conclusion" sections.

The scientific approach of passing reduced information on the parameters from one assimilation system to the next is questionable. The reviewer agrees with the author's statement: "It is important to note that this is an implementation question. Tarantola (2005) recasts the fundamentals of the approach as the conjunction or multiplication of probability densities. This multiplication is associative so it makes no difference whether it is performed in one step or several." However, an implementation of such a step-wise procedure needs to propagate the full Probably Density Function from one step to the next. In the Gaussian framework selected here this requires to propagate the full error covariance matrix and not just the error bars (see comment above). Error correlations are to be expected (see, e.g. Raoult et al., 2016). The change of the parameter space from one step to the next adds a further weakness as well as the dependence of H1 on the last iteration of each step. The degradation of the results in the back-compatibility test is no surprise. Another test that has not been performed here would be to operate the sequence of assimilation systems in the reverse order and compare the final parameters and validation results. The computing effort is the same as for the order presented here.

We agree with the reviewer that with the Gaussian framework it would have been optimal to propagate the full error covariance matrix. We however did not propagate the off-diagonal terms for the following reasons:

âĂć It was a substantial simplification in term of system engineering to only propagate the diagonal terms when we initially built the system.

âĂć The error covariance terms were not large as we obtained correlations that were on average below 0.3.

âĂć Propagating the variances appears to be “sufficient”. This is indirectly verified given that the back-compatibility is achieved to a very good level on average (figure 8). The degradation of the fit is indeed marginal: i) for FluxNet data the change of RMSE between step 2 and step 3 is negligible compare to the improvement achieved during step 2; ii) for NDVI, the change is only significant for Temperate deciduous tree and C3 grasses but the RMSE in step 3 is still much lower than with the prior parameter set. These back-compatibility tests thus indicate that the information provided in previous steps is not lost during subsequent steps.

âĂć Overall, we started this study with only the propagation of the variances from one step to the next, but we also investigated the impact of not propagating the covariance

with simpler models and set up. MacBean et al. (2016, in review) analyzed these issues: their main finding is that not propagating the covariance terms is likely to have a small influence on the posterior parameter values. Note finally that since submitting this paper we are working on improving the system to propagate the diagonal terms.

Our approach provides thus a simple step-wise framework that is able to account for the three sources of information with no significant lost of information from one step to the next as revealed in figure 8 and with coherent parameter changes.

However, we acknowledge that this issue was probably not highlighted enough in the original text. We have thus slightly reinforced it in the discussion section and we also mention it in the method section.

The assimilation of a statistical index, i.e. NDVI, is somewhat beyond state of the art, as assimilation of the related physical variable, FAPAR, has been demonstrated for multiple LSMs (Knorr et al., 2010, Schurmann et al, 2016). The required physical model of FAPAR is available in ORCHIDEE (Naudts et al., 2015).

We strongly disagree with this statement for the following reasons, which have been outlined in Bacour et al. (2015) and MacBean et al. (2015) but we chose not to repeat in this paper so as not to have overlap between the two studies:

âĂć Studies have shown that considerable discrepancies exist between so-called “high-level” satellite products such as LAI or fAPAR, especially when considering their magnitude (D’Odorico et al., 2014; Garrigues et al., 2008; Pickett-Heaps et al., 2014). These differences / uncertainties are attributed to differences in the processing chains, in particular the radiative transfer models that are used to derive these products (based on different physics and assumptions) Figure 1 below, taken from D’Odorico et al. (2014), illustrates the issues with 3 state of the art FAPAR products. The maps highlight the differences in space over Europe, while the frequency distributions for July (at the peak of the growing season) are clearly significantly different between these products.

âĂć We have therefore considered a vegetation greenness index, the Normalized Difference Vegetation Index (NDVI) and we only used the temporal information brought by this product using normalized values. The impact of using raw fAPAR data on the optimized model parameters for ORCHIDEE has been detailed in Bacour et al. (2015). This study shows that the maximum fAPAR values during the peak of the growing season imposes strong constraint on the maximum photosynthetic capacity parameters (VCMAX, VJMAX) which could lead to the estimation of spurious parameter values. Similar results have been obtained by Zobitz et al. (2014) who showed that the assimilation of FAPAR data (alone) could result in unrealistic simulated NEE values.

âĂć Given that fAPAR and NDVI are nearly linearly related and that we normalize the signal between 0 and 1, using one or the other variables is thus equivalent.

âĂć Note finally that Schürmann et al., (2016) obtain a very large impact on the gross and net carbon fluxes with the assimilation of raw fAPAR data; they could not evaluate if it degrades or improves the maximum photosynthetic uptake (at least it pulled the GPP towards values much lower than the data-driven product of Jung et al. (2011) based on FluxNet data).

Overall, we appreciate that we have not discussed enough our choice, given our wish to limit the overlap with MacBean et al. (2015) as mentioned above. So we have now added one sentence in section 2.4.1 to justify more clearly our choice: "Given that considerable discrepancies exist between so-called "high-level" satellite products such as LAI or fAPAR regarding their magnitude (D'Odorico et al., 2014), we thus only use the temporal information in the NDVI observations and normalized both the model FAPAR output and the NDVI observations to their 5th and 95th percentiles (following Bacour et al. (2015))."

Note finally that Naudts et al. (2015) describe a version of ORCHIDEE, named ORCHIDEE-CAN, that was not available at the beginning of the study and that has only been validated for European ecosystems (i.e. not the tropical ones for instance).

Specific Comments:

p11: 184 parameters is misleading, as none of the three systems estimates that many parameters Why are KsoilC parameters differentiated per region?

We agree that 184 is the total number of parameters optimized, but that in step 2 and step 3 the number is slightly lower and that in step 1 it is indeed much lower. We have corrected the text to be more precise.

As for the KsoilC parameters, they scale the initial values (after spin-up) of the modeled slow and passive soil carbon pool sizes, in order to take account of all the historical effects not accounted for in the model that would result in a disequilibrium of these pools in reality. It would thus be a strong hypothesis to assume that the "historical effects" impacted the soil carbon content uniformly. Indeed the history of land cover changes and land management largely differ between region/ecosystems and not accounting properly for their impact on soil carbon stock is a crucial point to address. For the global scale optimization step, we used 30 KsoilC,reg parameters corresponding to 30 regions (see Fig. A2). Rayner et al. (2005) used a similar approach with 13 coefficients for their 13 PFTs. In our case we choose to define the region not on a PFT basis but following large ecosystems regions that could be coherent for the history of land cover change, land management as well as ecosystem and edaphic conditions. Note that Schurmann et al. (2016) use only one global scalar and recognize that this is one of the major limitations of their approach. However, we acknowledge that the choice of 30 regions was not enough justified and we thus added one sentence in section 2.3.3: "For the global scale optimization step, we used 30 KsoilC,reg parameters corresponding to 30 regions potentially coherent for land use and land management history as well as ecosystem and edaphic properties (see Fig. A2)."

p23: Why are the FLUXNET assimilations performed per site and not simultaneously? How is the error of the parameter averaged over PFTs calculated.

We guess the reviewer is asking why the assimilations are performed per PFT and not simultaneously for all PFTs. The reason was technical as doing it per PFT was slightly simpler and it allowed us to made several tests independently for each PFT. This allowed running smaller “optimization runs” in terms of requested memory and computing time, which proved to be more efficient given some random system failure (due regularly to failure in disk access).

As a drawback, we indeed had to average the estimated values for few global parameters (not dependent of the PFT). For the uncertainty associated to these parameters we averaged the variances. We have thus improved the text to describe more precisely the treatment of the error for these parameters.

Eq.(1) in the manuscript does not correspond with Eq. (1) in Tarantola (1987).

We agree that this was a mistake and drop the reference to Eq. (1) in Tarantola (1987) and replaced by Chapter 4 (where least square problems are described).

p21: After assimilation of atmospheric CO2 it is no surprise that the trend is close to observations.

We agree that this is probably the strongest constraint in the optimization and that it is clearly expected that we match the atmospheric CO2 trend with the optimization of a large set of parameters. We nevertheless kept the sentence but added at the end the term: “as expected”.

p24: Fluxes are calculated from 2000 to 2009. Why are concentrations in Figure 6 not shown over the same time span?

We have shown in figure 6 only the time period when the atmospheric concentrations are used in the optimization. For the fluxes, given that we wanted to compare with other approaches, such as the GCP estimates, we have run the optimized ORCHIDEE model over a longer period to provide an mean estimate over the 2000 decade. Note finally that restricting the period in figure 6 to three years also help to see more clearly the improvements in term of seasonal cycle.

p41: 36 regions while in text it is 30.

It was a mistake. Corrected

Technical Corrections:

p e l 16: "remains"change to "remain"

Corrected

p 13 l 13: "we did not propagated" change to "we did not propagate"

Corrected

p 9 l 12: "et al., (1980)" change to "et al. (1980)"

Corrected

References cited:

Kaminski T, Knorr W, Rayner P, Heimann M: Assimilating atmospheric data into a terrestrial biosphere model: A case study of the seasonal cycle. Global Biogeochem Cycle 2002, 16:1066 doi: 10.1029/2001GB001463.

Knorr, W., and J.-P. Schulz (2001), Using satellite data assimilation to inferglobal soil moisture and vegetation feedback to climate, in Remote Sensing and Climate Modeling: Synergies and Limitations, edited by M. Beniston and M. Verstraete, pp. 273– 306, Springer, New York.

Knorr, W., T. Kaminski, M. Scholze, N. Gobron, B. Pinty, R. Giering, and P. P. Mathieu (2010), Carbon cycle data assimilation with a generic phenology model, Journal of Geophysical Research-Biogeosciences, 115.

Rayner, P. J., M. Scholze, W. Knorr, T. Kaminski, R. Giering and H. Widmann (2005), Two decades of terrestrial carbon fluxes from a carbon cycle data assimilation system (CCDAS), 19, doi:10.1029/2004GB002254.

Rayner, P. (2010) The current state of carbon-cycle data assimilation, Current Opinion in Environmental Sustainability, 2, 289-296.

Raoult, N. M., Jupp, T. E., Cox, P. M., and Luke, C. M.: Land surface parameter optimization through data assimilation: the adJULES system, Geosci. Model Dev. Discuss., doi:10.5194/gmd-2015-281, in review, 2016.

Schurmann, G. J., Kaminski, T., Kostler, C., Carvalhais, N., Voßbeck, M., Kattge, J., Giering, R., Rodenbeck, C., Heimann, M., and Zaehle, S.: Constraining a land surface model with multiple observations by application of the MPI-Carbon Cycle Data Assimilation System, Geosci. Model Dev. Discuss., doi:10.5194/gmd-2015-263, in review, 2016.

Wang YP, Leuning R, Cleugh H, Coppin PA: Parameter estimation in surface exchange models using non-linear inversion: How many parameters can we estimate and which measurements are most useful? Global Change Biol 2001, 7:495-510.

Zobitz, J. M., D. J. P. Moore, T. Quaife, B. H. Braswell, A. Bergeson, J. A. Anthony, and R. K. Monson (2014), Joint data assimilation of satellite reflectance and net ecosystem exchange data constrains ecosystem carbon fluxes at a high-elevation subalpine forest, Agric. For. Meteorol., 195–196, 73–88.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-13, 2016.

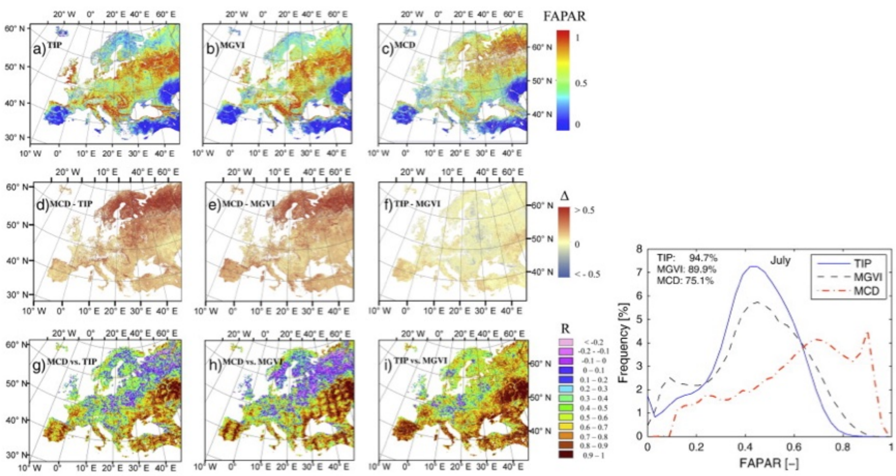

Fig. 1. Figure 1 (from D'Odorico et al., 2014): Left: Maps of FAPAR from TIP, MGVI and MCD products (a–c), their differences (d–f), and their correlations (g–i). Temporal resolution: a–f) July monthly composi