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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the constraint of biospheric process parameters by CO_2 concentration and flux measurement networks through a carbon cycle data assimilation system" *by* E. Koffi et al.

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"Quantifying the constraint of biospheric process parameters by CO2 concentration and flux measurement system" by E. Koffi et al.

Our general comments on the reviews of the above mentioned manuscript:

There were three main criticisms by the reviewers, which we address below:

1) The use of BETHY generated fluxes as a proxy of flux measurements without validating them by observations

2) The use of few PFTs can give conclusions more specific to the CCDAS we are using

3) The description of the methodology is too long

We have addressed the item 1) by comparing BETHY hourly generated net flux (NEE) to those observed at some selected sites of FLUXNET network. Results show that at least for the selected sites, BETHY is doing a reasonably good job. Indeed, the simulated fluxes are in fairly good agreement with the observations. Since the fluxes themselves are not a mandatory quantity in this study, we provide the results from this additional work as supplementary material for the paper. Indeed, the computation of the uncertainty in the process parameters of BETHY does require only the uncertainty in the observations.

For the point 2), we generally agree with the reviewer. In fact the impact of heterogeneity on the relative impact of flux and concentration measurements is the subject of Kaminski et al. 2012, a paper featuring most of the authors of the present study. The point is actually more general than a PFT-based formulation, it holds for any low-dimensional description. The point which surprised us in the current study was the confinement of information contained in high-frequency concentration observations to the vicinity of the underlying PFT. It remains true that atmospheric measurements sample larger footprints than flux measurements but the difference is smaller than we thought.

Regarding the point 3), we have added a flow chart (new Figure1) which is used throughout the text of the methodology to clarify some unclear parts. Since the methodology used in this paper has not been published elsewhere and because we think that the part that describes the concept is helpful for potential readers, we have then maintained the structure of the methodology as it was.

In what follows, our responses follow the comments of the reviewers and start by “Reply:”

Anonymous Referee #3 Received and published: 10 January 2013

KofïňĄ et al. investigate the constraining power of measurements of atmospheric O2 concentration (with differing numbers of stations or different time resolution) or of CO2 ïňĆux, for estimating parameters in a biosphere model in a CCDAS framework. They do not ïňĄnd much gain from using submonthly variations in CO2 concentrations, but strong constraints from using ïňĆux measurements. The paper is an interesting step in the development of CCDAS's, which I would like to recommend for publication after revision. My largest concern is that some of the conclusions will strongly be speciïňĄc for the particular CCDAS system based on few PFT's. The authors ïňĄnally mention this brieïňĆy at the end of Sect 7, but this important caveat needs to be discussed more prominently. Though the PFT concept is certainly attractive for CCDAS's, the rigid coupling of the parameters of hugh portions of the land surface is a great simpliïňĄcation of the high heterogeneity of the biosphere in reality. I expect that the strong impact of the ïňĆux measurements (compared to atmospheric measurements) is an artifact of the small number of PFT's.

Reply:

See our general comments

Small comments: The methods section is very long and somewhat confusing. I suggest to mainly describe the layout of the test cases, and move all the general mathematics and implementation details to footnotes or an appendix. For example, if I understand right, the PYVAR system is essentially used as a wrapper around LMDz that picks the modeled concentrations from the gridded ïňĄeld at the right location and time - if so, put it that simple. On the other hand, I found the description of the various conïňĄgurations not very clear - maybe a ïňĆow chart would help here (also consider to use more easy and more mnemonic codes).

Reply:

Since all the pieces of the paper are needed for an easy understanding for potential readers of the paper, we have maintained the structure of the paper as it was. However,

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we have added a flow chart as Figure 1 as suggested the reviewer. This Figure 1 is used throughout the text that describes the methodology to clarify some points.

p 24132 l 21: What is "evidence-based"?

Reply:

Replace by " . . . the application of carbon accounting using measurements". See page xx.

p 24133 l 1ff: The uncertainties that lead to spread in "approach 1" also affect "approach 2" in the same way, i.e. "approach 2" cannot be put as a solution of these uncertainties.

Reply:

Do you mean here "approach 1" stands for direct inversion and "approach 2" for the CCDAS concept? If so, we do not pretend that approach 2 is the solution of the approach 1, but the two approaches are necessary and especially the use of each of them depends on the objectives one wants to reach.

p 24136 l 4: The wording "ratio of: : :" contradicts the later deïňĄnition Eq (4).

Reply:

Corrected

Headline 2.2 "CCDAS"?

Reply:

Corrected

p 24137 l 6 and later: Avoid spelling out numbers, as this is harder to read.

Reply:

Correct this and consider this correction throughout the text

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Sect 2.3 and 2.4: See comment above

Reply:

These two small sections allow clarifying the main features of PYVAR as used in this study and how the CCDAS is combined to the PYVAR system for the study. We think then that these 2 sections are important for the understanding of the methodology. Hence, they are kept as they were.

p 24138 l 11: Likelihood of what?

Reply:

It has been clarified, i.e., Likehood function.

p 24140 l 1: What does it mean "We use the same linearity assumption: : :"?

Reply:

We use the same linearity assumption as performed for the concentration in Eq. 3 The text has been clarified.

Eq 4: Missing "%" (in "100%")

Reply:

Corrected

p 24141 l 23: If NPP=0 is treated differently, doesn't this introduce discontinuities?

Reply:

No, this will decrease the impact of fluxes having value close to zero since large uncertainties are considered

Sect 4.2 and 4.3 partially overlap with Sect 2 and are partially not actually on "Data".

Reply:

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We do not agree with the reviewer since the small section 4.3 allows clarifying how the PYVAR system is combined with the CCDAS. Again, we want to give more information as possible to facilitate the understanding of the paper.

p 24146 l 3: Did you mean "DM" rather than MM?

Reply:

No. It is MM. Indeed, here we investigate the differences between monthly and daily fluxes of BETHY. The monthly fluxes are obtained by using monthly meteorological and phonological data to force BETHY and BETHY provides as output monthly fluxes (MM). The daily fluxes are obtained by using daily meteorological and phonological data to force BETHY and BETHY provides as output daily fluxes (DD).

Sect 5.3 "FLUXNET": Do you not consider the real PFT (land cover) of the sites, and if not why not?

Reply:

Note that in our approach we consider up to 3 PFTs in each BETHY grid cell. For this analysis using FLUXNET data, we consider i) the dominant PFT that encompasses the FLUXNET site locations and ii) the uncertainties in the flux. Hence, using a mix of PFTs might slightly change the results

p 24148 l 12: Add what a(J,V) is relevant for.

Reply:

The parameter aJ,V is the slope of the linear relationship between the maximum electron transport and Vmax at 25 oC. This has been clarified.

p 24150 l 11: The number of observations does not seem to be a relevant metric, as the type of data is so diffeernt anyway.

Reply:

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This part of the text has been deleted.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C12604/2013/acpd-12-C12604-2013-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 24131, 2012.

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Interactive Comment