Atmos. Chem. Phys. Discuss., 12, C3930–C3935, 2012
www.atmos-chem-phys-discuss.net/12/C3930/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observing the continental-scale carbon balance: assessment of sampling complementarity and redundancy in a terrestrial assimilation system by means of quantitative network design" *by* T. Kaminski et al.

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Received and published: 22 June 2012

We thank the reviewers for their two comments and Dietrich Feist for his interactive comment. These comments are very constructive and highly valuable!

In the following we address the comments in chronological order. A revised version of the manuscript is included as supplement.

Response to Comment by Dietrich Feist We agree with you: It would have been very interesting to also include the effect of XCO2 observations, in particular the TCCON

network. The methodology is capable of handling XCO2. This is demonstrated in the study of Kaminski et al. (2010), who investigate the benefit of a space-borne sensor for XCO2.

To integrate the TCCON network into the network designer will require, as observational operator, the simulation of the FTIR sampling in the model, based on the vertical weighting function that is imposed by the instrument. From this observation operator we can construct a Jacobian matrix that quantifies the sensitivity of simulated XCO2 at the sampling locations to changes in the process parameters. We mention this in the last paragraph of the revised manuscript. This sensitivity would be a significant calculation and we think the manuscript's contribution is significant enough without waiting for this extra information.

Response to Comment by Han Dolman and Antoon Meesters

General, problems of the PFT concept: We added a discussion with reference to the Groenendijk work to the last but one paragraph of the introduction in the revised manuscript and emphasised again in 4.2 that the number of PFTs and their distribution in the real world is not known. There is, of course, an evolving methodology for describing the biosphere without using the PFT concept and, once models for it have been developed, we could examine its implications for network design.

General, biased towards understanding calculations vs understanding the real world: The point is valid and is a general critique of quantitative network design. The problem with an alternative "realistic" setup is that its conclusions are often dependent on details and hard to generalise. Here we are presenting a new concept and its implementation. This is why we deliberately started with simple/instructive examples to explain basic mechanisms before addressing real world applications in the later sections.

Sect. 2.1, wrong place for the period: We agree that this would usually go into Sect. 3, Experimental Setup. In fact, in Sect. 3, we deal with the setup of the network designer, which uses the precomputed Jacobians. We have the other details (including the observational period) about these pre-computations in Sect. 2, as well. This is why we think it helps the reader to mention the simulation period where we introduce the model.

Sect. 4, results which are not shown in figures: We included "not shown" as suggested.

Sect. 4.3: We moved the introduction of the multiplicity to Sect. 3, as suggested. "Copies" is not so good, so we use "fraction" instead. The wording you suggest is good. It is a bit restrictive, because BETHY's default setup has up to three PFTs per grid cell. We added your wording to the description. With multiplicity larger than 1 we have no longer global parameters, because what used to be a global parameter is now only valid for its 'fraction' of the global vegetation (also included this wording in the revised manuscript). Regarding the lower uncertainty reduction for the flask network with multiplicity 25 compared to BETHY's default configuration we need to keep in mind that multiplicity 25 is characterised by a prior uncertainty that is five times lower. We added a small discussion to the last paragraph in Sect. 4.3.

Conclusions, to be restricted to continental scale: In fact we think that the mechanisms/effects we demonstrate (integrative effect of atmospheric network, risk of incomplete flux networks) are not restricted to continental scale. They would probably also apply for a national scale analysis of, say, Great Britain.

Where the conclusions get more quantitative, we say explicitly that we address the continental scale. And in the title, too. And in the revised manuscript we included the restriction also in the first paragraph of the conclusions.

Figures: Thanks, all points fixed. We added a map showing the target regions to the revised manuscript.

Response to Comment by Christoph Gerbig

We first respond to the specific comments:

atmospheric data types, averaging out the synoptic/diurnal signals: This is right. We

added the suggested discussion at the end of Sect. 2.2.

Specify Model: Yes, this is the terrestrial model. Included specification in text.

Choice of prior uncertainty: Prior uncertainty values for target quantities are computed by propagating prior parameter values of Scholze et al. (2007) through the model. We added this explanation.

site 143-9: Yes, this was confusing. To clarify we extended the text in Sect. 4.1 of the revised manuscript: "Note that we can populate any given location with up to three PFTs. For the current experiment we take the location of “site143-9” from Table 3 but populate it to 100 % with PFT 9.

averaging out fine scale structure: We refer here to the footprint. For a sample over a given day, we may see a footprint with fine scale structure, reflecting the given synoptic situation. But if we collect daily samples over 20 years, the average footprint will be similar to the average footprint of monthly mean samples. The paper by Pillai et al. (2010) is very interesting. We refer to it elsewhere in the revised manuscript.

Global Parameters: With multiplicity larger than 1 we have no longer global parameters, because what used to be a global parameter is now only valid for its 'fraction' of the global vegetation (included this wording in the revised manuscript).

Range of PFTs vs range of climates: Sect. 4.1 has demonstrated that it does not matter whether we observe a given PFT at a site in Scandinavia or Spain. Sect. 4.2 has demonstrated the trouble caused by missing a PFT. We'd say the conclusion that we have to sample PFTs not climates is valid. For this it does not matter whether we add a structural error term in Eq. 2 or not. The structural error would just show up as an offset on the target uncertainty and mask the effects.

Now we address the general comments:

Comments 1-3 relate to data uncertainty.

This is a notoriously difficult point. We have extended the text on the data uncertainty in Sect. 3 and have also conducted an extra experiment, where we increased the data uncertainty of the continuous network by a factor of 4, which had little impact (uncertainty reduction over Europe dropped by about 3 percentage points). Following your calculation, this factor of 4 raises the uncertainty on the monthly average of the daily continuous samples for 0.27 (default case) to above 1 ppm (which corresponds to our data uncertainty for the flask data type).

As mentioned, in its present form, the network designer cannot explicitly treat correlated data uncertainty (see Sect. 2.4), and we inflate the data uncertainty to take correlations into account, and do not use the data at its full temporal resolution as is explained in Sect. 3 of the revised manuscript.

More specific responses to the points 1-4:

1) Yes: For the computation of the target uncertainty we ignore the structural error. The structural error would just show up as an offset on the target uncertainty and mask the effects we would like to demonstrate and discuss. Differences between two cases might well remain the same however.

In fact the little added value in reducing data uncertainty by a factor of 100 demonstrates that even with our default data uncertainty it is very efficient to sample a PFT over 20 years to infer a 20 year mean flux.

Comments 2 and 3 were addressed above.

4) Continuous data are treated with a higher resolution model, because we thought it might be useful to better resolve the synoptic signature in this data type (see above discussion). There were also practical considerations related to availability of pre-computed Jacobian transport representations. Obviously in any real inversion we would use consistent modelling frameworks (not necessarily the same model everywhere, we might choose the nesting technique pioneered by Rödenbeck et al., 2009).

References Kaminski, T., Scholze, M., and Houweling, S.: Quantifying the benefit of A-SCOPE data for reducing uncertainties in terrestrial carbon fluxes in CCDAS, Tellus B, 62, 784–796, doi:10.1111/j.1600-0889.2010.00483.x, 2010.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C3930/2012/acpd-12-C3930-2012-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7211, 2012.