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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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Summary of the paper:

The paper is a lucidly written account of studies in CO2 network optimalization with synthetic data.

All exercises use the same (BETHY) vegetation model. This translates vegetation parameters (table 2) for PFTs (table 1 and figure 1), together with meteorological conditions, to fluxes on a coarse global grid. Subsequently, these are translated to either (1)

measured fluxes (straightforward), (2) flask concentration means or (3) continuous time series (using transport models). For these three quantities, measurement networks are indicated in figures 2 (flask, global) and 3 (flux + continuous time series [legend contains an error], only for Europe). Table 3 also indicates the flux network (PFT-type is listed after the name), and table 4 the network for continuous sampling. Measurements are assumed available over 20 years in the past. The posterior fluxes from the network configurations are always averaged into "target quantities" such as NEP and NPP over three areas (Europe, Russia and Brasil, domains are not indicated in a figure).

The key assumption in this paper is, and this is still common practice, that all global vegetation can be classified into a few PFTs, and that a perfect vegetation model (no "structure errors") can "translate" these vegetation parameters to the exact CO2 fluxes. Additional error sources (meteorology influencing flux; subsequent transport) are allegedly included in the prescribed errors, the sigma's, otherwise they are not considered. The working hypothesis is thus that the PFT vegetation parameters determine everything.

A well tested carbon data assimilation scheme is then used to translate observation errors directly to target quantity errors, in dependency on the network. The intermediate step with the errors in the vegetation parameters are not a point of consideration. The results are in general expressed in the figures as error reduction from prior to posterior.

The experiments differ in the choice of the network(s). Section 4.1 presents result with only very few flux measurements (only done in Europe, but because of the PFT universality this also yields results for the other domains). Section 4.2 presents results with full networks, combinations of them, and networks which are slightly reduced. Section 4.3 presents results with the same kinds of network as section 4.2, but with several PFTs per grid cell; it is now assumed (the text here is rather poor) that the flux network monitors fields of each of the local PFTs separately, whereas the concentration networks sample concentrations calculated from the combined fluxes.

General points.

The paper is written quite methodically, and usually clear.

The approach is very theoretical. Skipping problems pertaining to the uncertainty in meteorology influencing the fluxes, and pertaining to the route from-surface-flux-to-measurements, the greatest problems are likely to occur with the assumptions about the vegetation model, and in the first place the assumed constancy of the vegetation parameters within each PFT. [e.g Groenendijk et al., 2010). For instance, for most of the experiments (4.1 and 4.2), it is assumed that all crops have exactly the same vegetation parameters, independent on species, season, climate or management ....

On the other hand, the theoretical model approach is charming in that it highlights those causal relations which are probably the most important. Moreover, if the model is well used, it makes its own limits visible. The last results section 4.3 is very important: it shows that if one assumes many more PFTs than was done from the onset (table 1), the flux network (much more than the atmospheric networks) appears not quite as capable to deal with even a relative small number of unmonitored PFTs. Based on real-world experience, one would think that this is just what will happen in practice (again Groenendijk et al). An additional complication is that if there are many PFTs, it may be hard to identify them; thus the structure of the vegetation maps becomes critical).

We suggest to âĂć retain the present calculations; these are good! âĂć present the results so that the impression is avoided that a model with few global PFTs can be considered realistic; not only should the problems encountered with many PFTs be more highlighted, but the essential problems with applying such a model to the real world should be highlighted already from the introduction; âĂć in describing results, more attention should be paid to the question whether differences between network performances are really robust; at present there is sometimes more attention paid to small rather than large differences, suggesting that the authors are biased towards understanding their calculations rather than understanding the real world.

Specific comments

Section 2.1, first paragraph: This is not the appropriate place for the description of the period.

At the end of sections 4.1-4.3 there are often results described which are not shown in figures; this should be indicated in the text.

Section 4.3: âĂć The method should be described in section 3. âĂć The present description of the method is unclear. The term “copies” is strange, and some essential information is missing. I would suggest a terminology like this : gridcells are now composed of equal subgrid patches, each with their own PFT; the corresponding surface fluxes add up to one grid cell flux to be used for the atmospheric networks (hence the patches can be said to have the same location) but they are separately monitored by the flux network. âĂć I do not understand the sentence “In this case there are no global parameters”. The preceding description suggests a multiplication of global parameters. âĂć Last paragraph: I infer from the text that the performance of the flask network is now considerably lower than that with less PFTs, this should be indicated (and preferably explained) more explicitly.

Conclusions: âĂć It should be emphasized from the onset that these conclusions pertain to the chosen target quantities (hence, balance on continental but not much smaller scale). Little can be concluded about concentration-networks on smaller scales, though perhaps some of the conclusions about the flux network can be extended further.

Figures: âĂć The domains for the target quantities are not shown âĂć Figure 3 has wrong legend. âĂć Figure 5: indicate the experiment in the legend. âĂć Figure 7: the legend neglects the last bar.

Reference

Groenendijk M., et al., 2011. Assessing parameter variability in a photosynthesis model within and between plant functional types using global Fluxnet eddy covariance data.

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