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Interactive comment on "Daily CO_2 flux estimates over Europe from continuous atmospheric measurements: 1, inverse methodology" *by* P. Peylin et al.

P. Peylin et al.

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Passing information from the first inversion step to the second:

We agree with the reviewer that from a methodological point of view it would be more consistent to include in the second step inversion the full posterior covariance matrix of the first step (including the correlations). We indeed only include the diagonal terms. However, we decided to keep this simpler approach for the following reasons:

1) First, the correlation terms between the errors of the posterior fluxes of the first step inversion are rather small (lower than 0.4) between the European and North Atlantic regions. Including them would thus add only a few lines as extra constraint (vector

Xcoarse define by the reviewer) to the second step inversion with nearly no impact on the solution. These low correlations directly result from the fact that we solve for fluxes over large regions with a monthly time step. If we would have performed a first step inversion at higher temporal and spatial resolutions, we would have obtained stronger error correlations and thus used the solution proposed by the reviewer.

3) Finally, the solution proposed by the reviewer, although conceptually simple, involves the extension of the huge H transport matrix (217106 x 180) which significantly complicates the matrix multiplications of equations 1 and 2, for no major benefit in our specific case.

Initial concentration for second inversion step:

We agree with the reviewer that we should expect positive and negative error correlations between the different initial concentrations. However, our argument to increase the errors on the reduce set of initial conditions (180) based on positive correlations between the initial concentrations still holds. In the case of negative correlations, we could then afford to decrease the uncertainties on the reduce space of initial conditions. Nevertheless, we kept the conservative choice of positively correlated errors to define the final uncertainties in order to decrease the possible nuisance from the initial conditions. We have modified the last two paragraphs of section 2.3 in order to include the case of negative correlations.

Adjoint:

Jacobian of fluxes at the transport model resolution have indeed been employed for CO2 inversion before this study. However, the use of ‘‘retro-plumes” to compute these Jacobians is new and we believe it is an important alternative to the use of an adjoint that operates on the level of the statements in the code. We thus keep ‘‘the retro-plume approach” as one of the new features to be highlighted in the conclusion.

However, we agree with the reviewer that the distinction made in the introduction be-

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tween the ‘‘tedious” construction of an adjoint transport model and the ‘‘easy” construction of the retro-plume approach was probably misleading and overstated. We removed these qualifiers.

Uncertainty in continuous concentrations:

The use of the standard deviation of the hourly average concentrations to compute the observational uncertainty is indeed a rather crude choice. Although it is not directly related to uncertainties in the observational process or in the model, it is the most common approach use so far for the different temporal resolution. We believe that at the first order it captures the general feature that model data mismatch is likely to be large for sites and days with large hourly concentration variations, as mentioned in the text. The example of the diurnal rectifier (cited in the text) supports this idea given that a small diurnal cycle will be associated to a small rectifier which is one component of the uncertainty (recall that we do not solve for the diurnal cycle). However, it is likely that model errors can be systematic and specific to a given site depending on geographical characteristics (mountain site, ...). Such components are very hard to quantify and were left for subsequent analysis.

Overall, we believe that for this methodological study our choice of data uncertainty is not critical and that the major conclusions of the paper would remain even with a more physically base choice.

Additional diagnostics:

We agree with the reviewer that additional diagnostics could have been used. However we did not implement them in the paper for the following reasons:

1) Fit to the flask sampling sites: We have checked that the posterior fluxes of the second step do not significantly modify or deteriorate the fit to the remote sites from Europe compared to the first step. However, Michalak et al. (JGR, 2005) proposed new diagnostics that are more relevant to check the quality of an inversion. Given

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that these diagnostics were not available at the time of our study and that they involve radical changes, we did not include them and we also do not discuss the in the text the fit tot the flask sampling sites.

2) Correlations among the posterior flux uncertainties: We believe that the discussion of both the error reduction maps (figure 4) and the time evolution of the integrated error over a few large regions (figure 5) is a simple and powerful way to present most of the information contained in the posterior covariance matrix. Diagonalization of this matrix is infeasible and the usual alternative, the singular value decomposition of the Jacobian, overlooks the impact of the prior error covariances.

Identical twin experiments:

See the replies to comments from other reviewers.

Particular formulations and the text and typos:

p 1648 | 26: changed

p 1649 | 18: We added the word ‘‘crude” to mention that it is a crude alternative.

p 1650 | 11: We believe that the word “acts” is appropriate and more concise than ‘‘is employed as a component of”.

p 1652 | 5: We added the reference of Peylin et al. for the problem in the temporal domain.

p 1652 | 26: we change the word ‘‘construction” by ‘‘use”.

p 1653 | 2: changed

p 1655 | 5: We believe that there is still no consensus in the CO2 inverse community about the time resolution of fluxes. While most of the articles use the same temporal resolution for the fluxes as for the data some studies, like Law et al. (GBC, 2002), or Rayner et al. (JGR 2002) justify their use of monthly fluxes with higher data frequency.

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p 1658 | 15: If we consider the globe, an emission in a given hemisphere will take about one year to be widely spread in the other hemisphere. Reported values for the inter-hemispheric exchange time usually range between 1 and 2 years. We thus keep this rough value of 1 year.

p 1658 | 23: We agree that we could never observe more than 180 directions in the source space. However the use of a limited domain for the location of the observations might lead to less than 180 directions. For particular days, we might have redundant observations in terms of ‘‘footprint” on the initial condition space and this is quite unlikely with observations evenly spread over the earth. We thus believe that it is worthwhile to notice the use of a limited domain.

p 1662 | 6: With the sentence ‘‘if the inversion .... affect the neighbouring pixels”, we do not intend to justify the use of spatial correlations with atmospheric transport considerations. We only want to mention that any flux corrections for a given day would apply to only a few pixels if there was no error correlation (according to air mass flow). It would then create spurious distortions with the neighbouring pixels based on considerations of the prior flux field characteristics (such as spatial coherence of the uncertainties). We believe that our sentence is not misleading especially if you account for the last part not raise by the reviewer ‘‘... pixels, or at least the pixels with the same climate and the same cover type”.

p 1662 | 18: Our argument to define correlations length between flux uncertainties is indeed based on the analysis of the fluxes themselves. The underlying hypothesis is that flux errors and correlations are likely to be large over areas of large flux variations. This is the most common approach used so far. We refer in the text to Rodenbeck et al (2003) where they use this approach at the monthly time scale and the sentence ‘‘... is probably smaller, as ... flux patterns” criticised by the reviewer directly follows from this argument. Adding a new sentence to justify our hypothesis would unnecessarily load the text.

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p 1662 | 25: Adding a reference to NASA-GMAO, would probably not help any reader from our CO2 community.

p 1663 | 15: We have drop the word ‘‘degrees” to better capture the continuous nature of the point that is made.

p 1663 | 17+21: changed

p 1663 | 17+21: changed

p 1664 | 23-25: We do not see any equation that could help to understand our notion of ‘‘contribution”. However, we slightly reformulate the 2 sentences in order to clarify the text.

p 1665 | 28: We agree with the reviewer and have dropped the word ‘‘probably”.

p 1667 | 21: We have decreased the size of the symbols so that the values are only partly masked.

p 1668 | 8: We do not mean a percentage from the standard case but a shift in the value that is itself expressed as a percentage. We changed the text to be more precise.

p 1668 | 17: We mean that in the 2000 km length case, the flux corrections do not reflect spatial structures that can be related to the back trajectory of the air masses at the different sites. We have clarified this sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1647, 2005.