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Atmospheric Chemistry and Physics Discussions

Interactive comment on "What can we learn from European continuous atmospheric CO_2 measurements to quantify regional fluxes – Part 2: Sensitivity of flux accuracy to inverse setup" *by* C. Carouge et al.

C. Carouge et al.

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Reply to reviewer #2 on "What can we learn from European continuous atmospheric CO2 measurements to quantify regional fluxes, Part 2: Sensitivity of flux accuracy to inverse setup."

C. Carouge, P. Peylin, P. J. Rayner, P. Bousquet, F. Chevallier, and P. Ciais

The referee suggests adding a sensitivity test on the choice of prior fluxes. This suggestion was also made in a review of part I. We refer to our response there, notably that the sensitivity test of prior uncertainty in fact acts as a test of the prior flux. Also, in this

study, we chose to build our prior fluxes using a different year of meteorological fields and a different biosphere model than used in the true fluxes. The question finally turns on whether the statistics of the differences between the prior and truth in our pseudodata world are realistic. Noone knows this. Even the comparisons with flux towers which are the closest thing so far to a direct calculation may well be more a measure of small-scale heterogeneity than an estimate of errors at the 40km scale. What we wanted to avoid was making the inversion's life easier than it would be in a real case where, for example, the TRANSCOM experiments used a climatological prior.

The referee would like to see some extrapolation of the results to other regions in the world. Concerning the pseudo-data results, most of the results are directly adaptable to other regions in the world with a same density of measurement sites. However, the quality of the results in inverting real observations is highly dependant on the geography and the fossil fuel emissions of the region. The geography of the region is important because it determines the quality of the transport model. For example, transport models are generally better over plains than mountains or the flux discontinuity at the coast lines might create additional problems that do not exist in our pseudo-data setup. In conclusion, the results would be the best over flat, homogeneous regions with low human activity. The question of how many and which stations are optimal takes us into the area of network design which is beyond our scope here.

Specific comments: The conclusion was extended to summarize findings of all experiments, including the network density experiment.

Technical comments: In this part of the study, all the fluxes are deseasonalized before calculating R and NSD statistics. It was made clearer in the text by adding "To focus on synoptic changes, we only compare deseasonalized fluxes (see CA08)" on p. 18630, line 3. p.18622, line 5: We deleted the term. p.18623, line 4: "That part" was changed to "CA08" p.18623, line 6: "These choices" was changed by "the setup choices"

p.18623, line 10: It is true that if the setup of our pseudo-data experiments is really unrealistic so the results from the inversions can not be extrapolated to real conditions, either in good or bad. For this reason, we chose physically based prior fluxes and not a mathematically constructed prior.

p.18623, line 15: The problem in designing inversions is to equilibrate all errors to end up with an optimized setup. If we aggregate prior flux over regions before inversion, then we fix the spatial pattern of the fluxes into this region. If the prior pattern is wrong, then the estimated flux would present the same wrong spatial pattern. Doing so, we create what is called aggregation error. At the opposite, if the prior fluxes are not aggregated together, then we would often end up with many more unknown fluxes to solve for than observations to constrain them. Then, estimated fluxes would be uncertain because of a too high representation error. The optimal setup would be to balance the aggregation error and the representation error. A discussion on this subject can be found in Peylin 2001. Using prior flux error correlations is a way to realize "soft aggregation". The correlations add some constraint between the prior fluxes but let some freedom to fluxes to change differently of other fluxes. Then, if the prior flux spatial pattern is not reliable, the aggregation error might rise quickly and then it is probably better to use a higher resolution for fluxes than if using a more reliable prior.

p.18623 line 29: "chosen" was removed. p.18624, line 1: The suggested change was done.

p.18626, line 20: We do not think there is anything special about 2001 or the NDVI from April 1998-April 1999. We mean to say that choosing two different biosphere models and two different years for the meteorology, the prior and true fluxes are more likely to be different. If we had chosen the same biosphere model with two different years or two different models for the same year, we were more likely to observe smaller differences between the prior and true fluxes. Note that the choice of two different models and two different years does not ensure large differences in the fluxes everywhere. In the first part of this study (CA08), we notice the deseasonalized prior fluxes were in good agreement with the true fluxes over Scandinavia. Probably, the meteorology for 2001

was closed to the one for 1998 over this region. If we wanted to study Scandinavia, we would then have to choose an other prior. An explanation was added in the text.

p. 18627, line 16: This question of biases and uncertainties is difficult to assess. For this study there are two questions: How closely does the study conform to the statistical assumptions underlying any inversion? Or how closely does the study conform to a real case? The fact that these two questions aren't the same is itself disquieting but the effects of incorrect statistics is not within our scope here. For the theoretical question, what we would like is a set of independent realizations of the two models from which we could calculate ensemble statistics of the differences. This is not possible so we pretend that subsets are independent realizations and use these. Any of these subsets (e.g. a timeseries at a point or the mean flux for a month) are finite so they won't be exactly unbiased but they should be asymptotically so. At the extreme, we calculated the bias over Europe for the year. At 0.03 gC.m-2.y-1 this is indeed comfortingly small. It is quite unlikely that all other subsets are as well-behaved as the continental mean. This, unfortunately, is probably a reflection of the real case. One advantage of our study in this respect is that at least we should expose inconsistencies in prior statistics. In the traditional approach of using only the posterior covariance to measure the success of the inversion such errors as these will be hidden, here they should be manifest in worse performance.

p.18631, line 6: In fact, given that our performance metric is the improvement relative to the prior, the better the prior the harder it will be. We refer to earlier discussions on prior statistics but also note that the spatial structure of the differences (larger in some places, smaller in others) is something of an automatic experiment in this direction.

P.18631, line 15: The way the statistical significance of the results is calculated is explained on p.18630, line 25 to p.18631, line 3.

p. 18631, line 17: In this sentence, we actually speak of the poor improvements seen in the fluxes from prior to estimates not in the prior flux error covariances.

p. 18631, line 20: Seeing some confusion about the correlations construction in the other referee comments, we prefer to leave the sentence to reaffirm this point.

p. 18631, line 26: Engelen et al. 2005 notice that shifting from an inversion of fluxes aggregated over large regions to an inversion of high resolution fluxes with variable correlations is shifting from hard to soft constrains. So the spatial aggregation is a substitute to spatial correlation. However, the inversion results can be quite different depending if we aggregate the fluxes before or after inversion. So it is interesting to compare the effect of aggregation on estimated fluxes of the different sensitivity cases. It is fairly clear that aggregation is not just a substitute for correlation since it improves both correlated and uncorrelated cases.

p. 18632, line 8, line 14 and p. 18634, line 1: The corrections were done.

p. 18634, line 1: It is true that the prior and true fluxes may have offsets in their seasonal cycles and the front systems are not the same for 1998 and 2001. But, in a real data inversion, the prior fluxes might also present an offset in seasonal cycle or be driven by incorrect meteorology fields. So we think the setup of the SP4 experiment is quite extreme but still informative. The results suggest that if one is not sure of the quality of prior fluxes, the safest choice is to use isotropic correlations.

p. 18634, line 10: This part was rewritten to clarify the fact we are making an assumption. We simply note that a test of a full prior error covariance matrix could be interesting to perform in future work.

p. 18635, line 14: We could not run the experiments in this paper on the larger network for computational reasons. What we needed to know was how the extension of the network would translate into information content. The relationship between information content and inversion performance could already be guessed at from the performance of the control inversion over, say, Western Europe.

p. 18635, line 19: The diagnostic we are using at this point is the posterior covariance.

It depends neither on the values of the pseudodata nor the prior or true fluxes.

References Engelen, R.J., A.P. McNally, Estimating atmospheric CO2 from advanced infrared satellite radiances within an operational four-dimensional variational (4D-Var) data assimilation system: Results and validation, J. Geophys. Res.-Atmos., 110 (D18), D18305, 2005 Peylin, P., Inverse modeling of atmospheric carbon dioxide fluxes - Response (vol 294, p. U1, 2001), Science, 294 (5550), p. 2292-2292, 2001

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