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Comment

Interactive comment on "Inverse modeling of European CH$_4$ emissions: sensitivity to the observational network" *by* M. G. Villani et al.

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Replies to P. Rayner's comments

"There are two approaches to setting up an OSSE for an inversion. The first, ideal method calculates the statistics of the posterior covariance. This can be by explicit calculation of the posterior covariance matrix (e.g. Rayner et al., Tellus, 1996 or Gloor et al., Glob. Biogeochem. Cyc., 2000). One can also do this via a MonteCarlo technique (e.g. Chevallier et al., Geophys. Res. Lett. 2007) but one must be careful when setting up the problem. In particular the statistics of the differences between the prior and true fluxes must be those described in the prior covariance matrix. In surface flux inversions we can rarely know these statistics

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well enough so an alternative approach is to give up on statistical consistency and choose for our prior another 'reasonable' flux. This was the approach used by Law et al., glob. Biogeochem. Cyc., (2002) and the two studies of Carouge et al., ACPD, 2008 on which this study draws. This is perfectly valid but the authors need to remember there is now another variable in play ... how good is their prior? They note this e.g. the discussion on P21085 around line 25 but the real impact of a good/bad prior compared to information added from the atmosphere needs the more limited but rigorous experiment where the prior is properly consistent. the paper would benefit from performing such a test for at least one of the networks."

We had actually addressed the issue on the consistency of the prior distribution. This can be found at P21081 L09. As a rough estimate, at the grid scale the mean absolute difference between true and *a priori* emissions amounts to about 200%. This is quite consistent with assuming the error on the *a priori* emissions to be equal to 300% of the emission values.

The choice of having a homogeneous *a priori* distribution is motivated by our aim to test the model setup in a very extreme situation, where we basically know very little of the *a priori* emissions. In first experiments (not shown in the paper), we started from more realistic distributions, which produced *a posteriori* emissions very close to the true values, as expected from the semplified setup of these experiments.

If the reviewer thinks that it may be helpful, rather than performing an inversion where the prior is constructed such that it is perfectly consistent with the error covariance (e.g. Chevallier et al., 2007), currently unfeasible to perform, we could analyze the frequency distribution of the true emissions versus the *a priori* in the European domain. In the revised version of the manuscript, after our discussion in P21081 L09, we could then provide a summary description of the results.

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"I suspect another artifact of the set-up is the surprisingly high rejection rate from the first step of the inversion. In the ideal statistical set-up the data residuals have the statistics of the data covariance matrix meaning that about 1% of them should lie outside the 3σ range. The authors do not quote the numbers but say 'generally less than 10%' (P21082 L18) which suggests higher than 1%. This is not a problem but is worth some more explanation."

As the reviewer said, in an ideal statistical set-up, the data residuals have the statistics of the data covariance matrix meaning that about 1% of them lie outside the 3σ range. This is also what is found in real case inversions such as presented in Bergamaschi et al. 2009, where we start from *a priori* distributions, which we believe to be our best knowledge of the emissions. Differently, in our synthetic experiment framework, we should expect much higher data rejection. This is due to the fact that, if the sampling site is located in an area with large true emissions, the synthetic observations will show high concentrations and the perturbations to these observations can be potentially large. Since the homogeneous *a priori* emissions are much lower in these areas, the inversion may experience problems in reproducing these observations. The explanation can be found at P21082, lines 5-19.

"My only other comment concerns the prior correlations discussed on P21081. We learn that the authors use a Gaussian correlation (usually of the form $exp(-(x/L)^2)$) where x is the separation of the points and L the correlation length. We need to be a little careful here since there sometimes a factor 0.5 included by analogy with the Gaussian probability density. The authors use a correlation length of 50km and a mesh of 1 degree (about 75km in Europe) and claim correlations about 0.6. I would expect rather smaller values. I suspect this is a misunderstanding on my part but recommend always that the actual equations describing the correlation functions be included. there are always subtleties such as whether the distances considered are great-circle or linear etc etc that otherwise make it hard to replicate the study.

To calculate the Gaussian correlation, we used the formula:

$$corr = \exp\left[-\left(\frac{D_{st}}{corlen}\right)^{iexp}\right],$$ (1)

where $corr$ is the correlation between grid cells, D_{st} is the distance in km, $corlen$ is the correlation length in km, and $iexp$ is a coefficient such that: $iexp$ is equal to 1 or 2 for exponential and gaussian correlation respectively. The revised manuscript will contain this formula in section 2.2. We will also investigate on the correct value for the correlation between grid cells.

References

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