

Interactive comment on “Inverse modeling of European CH₄ emissions: sensitivity to the observational network” by M. G. Villani et al.

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This paper performs a series of Observing System Simulation Experiments to assess the constraint on methane sources over Europe afforded by various observing networks. It uses the standard technique of generating pseudodata using known sources then attempting to recover those sources from an inversion using a different prior. It follows the methods previously used for CO₂ with the adjustments necessary to account for the different characteristics of the two species. The two key conclusions, that a significant extension of the current network will be necessary to constrain sources throughout Europe and that the introduction of asymmetric prior PDFs biases the solution relative to symmetric ones are useful if not dramatic. Although I don't suggest significant change to the manuscript, there are a few places where methodological

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choices impact the results and some points I can't make consistent within the text. I detail these below:

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 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. 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 don't 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.

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

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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.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21073, 2009.

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