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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trends and inter-annual variability of methane emissions derived from 1979-1993 global CTM simulations" *by* F. Dentener et al.

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The authors apply a top down approach for estimating methane emissions at the surface, which they compare to bottom up estimates.

There are a number of methodological problems, which I will describe in the following.

It is notoriously difficult to estimate surface sources and their uncertainty from point measurements collected at a sparse atmospheric network. This is a mathematically ill-posed problem. The mass balance approach as presented, e.g., by Law and Simmonds deals with this ill-posedness by an interpolation step, which yields a concentration at every surface grid point and time step. During a transport model simulation Law and Simmonds can solve a unique inverse problem: The source in each grid cell and time step is adjusted such that the modeled and observed simulation match.

In the present manuscript the observational data are interpolated in yet another way (using a dynamical rather than a statistical approach): If I understand what is happening, then the reference to Hein et al. indicates that the concentration fields derived from their a posteriori estimate of sources are used. The data stream would look as follows:

1. Run Hein et al. model with their a priori sources.

2. Perform a synthesis inversion step: Use the difference between simulated and observed concentrations (at the Hein et al. network) to drive the Hein et al. inverse model and infer sources.

3. Run Hein et al. model with inferred sources and record concentration field.

4. Add trend derived from present studies network. The result of this procedure is used as observed concentration.

The next step of inferring sources is then similar to Law and Simmonds, with the difference that no exact match to the processed concentrations is required, but the modeled concentration is nudged to the zonal mean of the processed one.

To obtain their a posteriori flux field, the authors apply the above listed sequence of procedures involving a synthesis inversion, a number of model runs, and a mass balance inversion. This process involves two different atmospheric transport models, with different transport characteristics (see, e.g., Law et al. or Bousquet et al.). Prior flux fields enter at two points in this chain: First in the synthesis inversion and then in the mass balance inversion, namely the prior fields of Hein et al. and Houweling et al. The idea of propagating uncertainties from the data and the priors through to the posterior sources appears intimidating to me. It is clear that this question could not be addressed.

In this context the uncertainties that are given in the manuscript are misleading. There

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is the danger that readers might confuse them with an uncertainty derived from that in priors and data. Instead here the uncertainty is derived by first computing the trends over subperiods of a year and then computing the standard deviation of the average over the entire period. If a subperiod of two years were chosen instead, than these uncertainties would look different.

The interpolation acts as a very strong constraint. Other source fields that match the observed concentrations equally well are discarded. The strengths of the interpolation constraint is evident from a comparison to a synthesis inversion such as those of Hein et al. or Houweling et al.: To match a concentration datum, their inverse model has the flexibility to adjust the source strengths of many source components back in time. The synthesis inversions only would yield the same result, if the transport Jacobian that links all these source components to this concentration datum had zero elements, except for the ones in the same source region and time step as the datum. To compensate for this loss of transport information in the present inversion set up, one must add this strong interpolation constraint (otherwise only flux components having a datum would be constrained). Law and Simmonds found that the interpolation constraint has a larger impact on the inferred flux field than inter annual changes in the meteorology driving the transport model. This is probably the case here as well. This point is not addressed by the pseudo data test inversion in the appendix.

In the present manuscript the impact of inter annual variations in the meteorological fields on the inferred flux fields was not large either. Given that, as sketched above, the mass balance inversion method limits the influence of transport in the inversion step based on the same transport model, this is not surprising. In a synthesis inversion setup Rödenbeck et al. report on strong impact of inter annual variations. Hence the low impact found here appears to be an artifact of the simplified inversion method.

The main advantage of the method presented here is that it is computationally not as expensive as alternative approaches.

I wonder how the fit to the real CMDL observational time series actually is, when the model is run with the estimated flux field.

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