Atmos. Chem. Phys. Discuss., 2, S158–S163, 2002
www.atmos-chem-phys.org/acpd/2/S158/

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.

F. Dentener et al.

Received and published: 7 June 2002

We thank both reviewers for their careful and insightful comments on our paper. Both reviewers have substantial methodological comments. In addition Dr. Rayner poses more specific questions regarding the applicability of the method to derive some of the more detailed regional flux estimates as well as the use of correlations.

In general we agree with most of the remarks made by the two reviewers on the suitability of our semi-inverse mass-balance method to derive emissions and trends. In theory, the more conventional inversion methods, e.g. the Green's function approach, can provide optimized emission estimates including a model consistent error estimate. However, at the same time we should be modest on the usefulness of the present generation of inversion techniques. E.g. Houweling et al. [1999] use an inversion of the

Full Screen / Esc
Print Version
Interactive Discussion
Original Paper

methane cycle to calculate a global emission of 528 +/- 90 Tg CH4 yr-1 (or 17 %). On the hemispheric scales the relative uncertainties are even higher (SH: 50 %, NH: 20 %). Different models and inversion techniques [e.g. Hein et al., 1997] give rather different results. All techniques are computationally quite expensive, and all modeling efforts suffer from a serious lack of measurement data. In addition, there are a number of issues in the publications on inversions that I have seen so far, which are either ignored or only superficially mentioned. They for instance consist of not-quantified errors of the transport models, and the way in which the measurements are used, including the representation of the measurements on the model grid scale. These errors are common to the inversions described above as well as in the method applied in our paper. Another uncertainty is the non-linearity of the OH chemistry, which in the inversions based on one single year did not seem to be of great importance, but is of increasing importance in multi-annual time dependent simulations. Clearly our work has taken this impact into account in a consistent way.

In this context we decided to perform our mass-balance inversion study and to assess the consistency of the present-day information we have available on methane emissions, atmospheric chemistry, and observations. Relative to very complex multi-annual inversion studies we think that there is already a lot to learn from such intelligent forward modelling studies, especially in terms of large-scale properties such as emission trends. Our analysis and results may further stimulate in future more detailed inversions and hopefully serve as a point of reference for the upcoming multi-annual inversion studies.

We know discuss the problems mentioned by P. Rayner.

1st problem As indicated by P. Rayner, and as mentioned in our paper on p. 253, inversions may yield a family of solutions, that may provide an equally good fit of model results and measurements. Our particular method provides only one solution out of many. We argue that this is a problem of inversion methods in general. The size of this family of solutions is strongly dependent on the fact that we are dealing with an

ACPD

2, S158–S163, 2002

underdetermined system (i.e. a small amount of measurements relative to the number of unknowns) and is therefore strongly dependent on the a-priori assumptions and the attributed uncertainties. Ideally, the uncertainty of the inversion-derived emission estimates should give an indication of the size of the pool of probable solutions. However, Dr. Rayner 'counsels against this' because he probably realizes that these posterior uncertainties are in fact quite uncertain themselves. Most likely these uncertainties are larger, since they do not account for the uncertainties of certain assumptions that are 'hard-wired' in the method. To our opinion we can only get a more realistic view of the real uncertainties by trying out and comparing different methods. In fact this supports our approach. We agree with Dr. Rayner that our model is probably more suited to determine the inter-annual variability and trends than the long-term mean.

2nd problem

Regarding the construction of the surface boundary conditions in our model we have been admittedly too brief in our description. We briefly repeat. The procedure uses the calculated zonal and monthly averaged methane surface concentrations obtained from the TM2 (8-10 degrees-9 layers) model of Hein et al. [1997]. This, largely independent model study, is representative for the year 1987, which is in the middle of our model period. The results of an independent inverse modeling study are useful as a constraint to our model since the calculated and observed concentrations are quite well fitting.

Given the long time scales and weak gradients involved in the surface field of methane we assume that our results are not too sensitive to the chosen surface methane field. Anyway, the model is only relaxed to the zonal mean field. Some model studies with TM3 in which explicit methane emissions were used instead of prescribed surface methane concentrations support our intuition that the calculated methane loss is quite similar for other surface boundary conditions.

On top of these fields we imposed a trend using the stations listed in Table 2 (note that in Table 2 station Samoa was missing). All stations had measurements during the

ACPD

2, S158–S163, 2002

full period of 1984-1993 (before 1984 only the ice-core records of Etheridge were available). The global annual trend was obtained by equally weighting all stations. This thus yields a trend that is somewhat biased to the Northern Hemisphere (NH), which had most measurements. However, there is no evidence that the magnitude of the methane north-south gradient has been changing in a different way in the NH compared to the SH. This supports that at least in the 1980s the trends in the NH and SH have been the same. It should be noted that after the eruption of Pinatubo, this assumption may not be fully correct. Giving specific weights to certain stations in determining the trend, would depend on the knowledge of the representativity of the station for the trend in a larger region. It can be questioned if the few Southern Hemispheric stations available (especially those in the tropics) give a better representation of the SH trends than just using the full available record for the global trend. Furthermore, we have performed one study (not described in the paper) in which we use separate NH and SH trends (the latter only based on 3 stations, of which 2 in the SH tropics). The results differ, but not very strongly, which corroborates that also this particular assumption does not alter our global conclusions strongly. The choice of using a nudging technique on a time scale of 10 days, was motivated by the fact that we tried to have the meteorological influence on synoptic timescales included, without constraining too strongly to the measurements. We have not found in the literature a good guideline on choosing the best timescale for a specific component. We will include this improved description in our paper.

We agree with Dr. Rayner (p. 106, l. 18) that the comparison of the regionally derived emission signal and the temperature sensitivity presented in our paper on p. 262 and 263 is very uncertain and our results can not yet be considered as "a proof" for the existence of regional emission variability. On the other hand, the absence of any significant variability would possibly be a more surprising result. Furthermore, any other inversion technique would likely to be equally uncertain, simply because of the dearth of measurements in the tropical regions. Our results thus strongly depend on how much of the tropical emission variability is seen in the middle and high latitude sta-

ACPD

2, S158–S163, 2002

tions. Dr. Rayner pointed to the possibility that auto-correlation of the data sets could obscure the statements on statistical significance. Using the yearly numbers on the variation of emissions in the four regions as well as the annual average numbers in the 12 world regions, we found that considering time lags of 1-6 years, very few data-sets were potentially auto-correlated. The data used to illustrate the correlation between the temperature in Africa and East Asia and the residual emission in the region 0-45 S, were not auto-correlated. The reference for dealing with auto-correlation problems is interesting, but the full implementation would not be in balance with the weigth we attribute to our results. The remark by Dr. Rayner on the probability of correlation of random data-sets is important to keep in mind.

Regarding the remark by P. Rayner to the 'slow' mode of CH4 and to which extent it is included in our procedure we note that the slowest timescale (mode) is due to the coupling of CO and CH4 chemistry and is about 13 years. We avoid having to deal with this longest time-scale by assimilating CH4 concentrations at the earth surface, and the time-scale involved is essentially that of tropospheric mixing (months) which is well within the spin-up time of 2 years considered in our model. The stratospheric relaxation times may indeed be somewhat longer than 2 years. However it should be mentioned that the initial conditions for January 1977 were already quite realistic (i.e. we did not start ab-initio), and that the stratospheric methane budgets did not show unexpected variations or drifts.

The remark on the variability of biomass burning in Langenfelds et al: the multi-component constraint on biomass burning emissions measured during 'high' emission years suggests a range of 15-90 % of biomass burning related CO + CO2 emissions compared to the inter-annual mean (taken as 3.9 Tg C per year). Applying this range to an annual biomass burning emission of about 40 Tg CH4 year would result in a range of 6 to 36 Tg CH4 emissions a year. If this range is true, than even the lower end of the range could explain a substantial part of our retrieved variability. The upper range would be inconsistent with our model results. We note that an independent

ACPD
2, S158–S163, 2002

satellite based study by Barbosa et al. (1999) indicated a variability of biomass burning of the order of 25 % for the African continent. It is not clear if this number can be extrapolated to other continents and to global emissions. Note that also for biomass burning emission variability we would expect some relationship with meteorology (i.e. temperatures).

References:

Barbosa, P.M., Stroppiana, D., Gregoire J.M. and Cardosa Pereira, J.M., An assessment of vegetation fire in Africa (1981-1991): burned areas, burned biomass, and atmospheric emissions, Global Biogeochemical Cycles, 13, 933-950, 1999.

Hein, R., P.J. Crutzen, and M. Heimann, An inverse modeling approach to investigate the global atmospheric methane cycle, Global Biogeochem. Cycles, 11, 43-76, 1997.

Houweling S., T. Kaminski, F. Dentener, J. Lelieveld, and M. Heimann, Inverse modelling of methane sources and sinks using the adjoint of a global transport model, J. Geophys. Res., 26137-26160, 1999.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 249, 2002.