

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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General Comments

This paper presents an analysis of some of the mechanisms controlling the observed interannual variability of the growth in atmospheric methane concentration. The authors do this using a simple mass-balance inverse procedure coupled to a relatively complex model of atmospheric transport and chemistry. The paper makes a series of conclusions about the variability of methane sources and sinks at global and regional scales. I am relatively comfortable with the largest scale findings of the paper but think the conclusions become more problematic as the focus narrows. I think the authors also recognize the problem in their general comments but I recommend that

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the caveats, especially the implications of the methodological details, be discussed in conjunction with some of the results.

The source of the difficulties is the inversion method chosen in this paper. It should be pointed out that the choice of the mass-balance method is not merely a matter of convenience. While the atmospheric chemistry community knows in theory how to perform the more conventional (and, I think, reliable) synthesis inversion in the presence of nonlinear chemistry (it is just a large variational problem after all) the task of setting up such a calculation is immense. It is probably only justified in the presence of the detailed concentration data now becoming available from space-based platforms. That said, the method does present two problems. Both are acknowledged by the authors and discussed thoughtfully in the paper but they do have implications for the weight we should place on the findings.

The first is that the mass-balance method provides one solution to the inverse problem. Speaking loosely, the inverse problem is to find a set of sources or emissions consistent with the observed concentration data. The sparseness and uncertainty of the data makes a lot of parameter space available as a solution. As a telling example see the comment from Kaminski and Heimann, *Science*, Vol.294, p.5541, 2001. The existence of this family of solutions is acknowledged in this paper, e.g. the comments at the top of p.253, but it is hard to get any feel for just how large the family might be. It is tempting to suggest comparing the variations seen in sources with the uncertainties from Hein et al. but I would counsel against this. It is often true that interannual variability is better determined than the long-term mean in these inversion problems.

The second problem concerns a step in the mass balance procedure: the construction of the surface concentration boundary condition from the sparse observations. This is necessary since it is the difference between the modelled and constructed surface concentration field which is used to compute the surface source. There is no obvious recipe for the construction of this concentration field. One must decide in advance on what structure functions to use to fit the data, how closely should one fit the data,

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should one maintain a constant network throughout or use new data as it appears.

The authors talk about the choices they have made as the primary limitation of their study. Curiously, though, the authors do not explain the rationale for their particular choices. The choices set the boundaries of what levels of interpretation of the results are reasonable. The authors therefore might add some discussion of their choice of fitting procedure (probably in section 3.2) and discuss the implications of that choice for each set of results in the paper. I will return to a specific problem in this regard below.

In a little more detail, the authors construct a surface concentration field consisting of the gradients and seasonality from 1987 added to a time-varying global mean concentration taken from analysis of 12 stations with longlasting records or the analysis (presumably based on firn records) of Etheridge et al. 1998. I have two concerns with this. the first is a matter of explanation. There are many ways the global mean trend used in the procedure could be calculated. One could calculate annual means from the station values for example, or one could construct the usual "flying carpet" and take the area-weighted mean. One could use various levels of smoothing in time etc. This information is probably in the antecedent papers but the reader's life would be easier if it was included here. I suggest adding this to section 3.2.

The second concern is more serious. The concentration field used seems incapable of capturing *regional* variability in methane concentration. In an inverse analysis regional concentration variability is likely to be the largest driver of regional source variability. So it is likely that estimates of regional source variability are being made in the absence of the key piece of information. Now it's risky to second guess the authors' reasons but a likely explanation is that there just isn't enough data to specify the regional concentration variability so zero seems the safest choice. In fact there is nothing especially conservative about zero, it is as strong a constraint as any other estimate.

For some results this is not such a serious difficulty. The main focus of the paper is global, e.g. global trends in OH. But the implications do seem quite serious when,

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for example, the authors calculate temperature sensitivities in different regions and compare them with process models. Note also the use of zonal mean concentration boundary conditions makes longitudinal descriptions of the fluxes problematic. For these and some more technical reasons below I am wary of the comparisons on p.262 and 263. My recommendation would be to delete them but at least some more support or qualification should be supplied.

Not all the regional analysis is subject to the above concern. The comparison of the impact of transport and chemical variability, for example, probably stands independently. This comparison is, I believe, the most important contribution of the paper.

Specific Comments

P.260: I am not so confident the inversion method avoids the problems with the equilibration of the slow modes from Wild and Prather. Yes the methane distribution will relax quickly to one compatible with the surface observations but this distribution itself is contingent on other factors, e.g. various OH precursors and stratospheric methane which may relax more slowly.

P.263: I found the correlation analysis rather unsatisfying or at least the analysis of statistical significance. I have already noted my concern about the concentration boundary condition but there are two more specific issues with the correlation analysis. Firstly I would guess the time-series for both variables are somewhat autocorrelated. It isn't clear whether this was accounted for in considering the statistical significance of the correlations. (See, for example, Ebisuzaki, J. Clim., Vol.10, p.2147, 1997, for a method to deal with this.) The other is the correlation of several time-series then concentrating on those which turn up significant correlation. It seems almost unfair to question this since the behaviour is nearly universal but one should bear in mind that one expects a certain number of randomly chosen time-series to be significantly correlated

P.264: The work of Langenfelds et al. (e.g. 6th CO₂ conf. extended abstracts, p.9, or Global Biogeochemical Cycles, in press), might suggest even larger variability for

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biomass burning, e.g. that its distribution in time is so skewed that the coefficient of variation may be greater than one.

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