Atmos. Chem. Phys. Discuss., 5, S412–S416, 2005
www.atmos-chem-phys.org/acpd/5/S412/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inverse modelling of national and European CH_4 emissions using the atmospheric zoom model TM5" *by* P. Bergamaschi et al.

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Received and published: 12 April 2005

This is a well written and methodologically solid investigation of the European methane emissions using the inverse modelling technique. The scientific innovation is not so much on the methodological side, as the applied approach is not new. However, it is for the first time that a rather comprehensive inversion set-up is used to address regional scale sources over Europe. Important steps forward include the combination of the regional and the global scale and the use of an unprecendented number of regional measurement sites. The outcome is a wealth of information. To name the most striking

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results: Overestimation of Finnish wetland emissions, underestimation of emissions in large parts of Western Europe and robustness of the overall European emission budget. As with many inverse modelling studies, however, it is difficult to judge the implications of numerous methodological assumptions that are made. In general, internal consistency of results is bad guidance for correctness. To gain confidence in correctness sensitivity tests are needed, as is done in this study. However, one should carefully select those sensitivity tests that address the major assumptions that might affect the most significant findings (the striking ones listed above). In this study the motivation for the selected sensitivity studies is not made quite clear, and as pointed out below some important tests seem to be missing. Therefore, I my judgment, improvements in this direction are needed to make this work publishable, calling for corrections that probably should not require much additional work.

GENERAL COMMENTS

1) Finnish emissions: An important and interesting outcome of the CH4 inversion is that emissions from the Finnish wetlands seem to have been overestimated. The first question that arises is: Overestimated as compared to what? The prior wetland emissions have been taken from Walter et al. (2000). Firstly, the year 2001 is outside their study period and wetlands emissions are expected to show significant interannual variation. Secondly, the aim of the work by Walter et al. is to provide a highly symplified modelling framework calibrated such that despite its simplicity nevertheless reasonable results are obtained on the global scale. Because of this, it is not necessarily the most reliable emission estimate for Finland that is available. It should be made clearer what is really known about Finnish wetlands, and if the current results have any implications for that knowledge. The fact that the information in the inversion comes from rather distant measurements suggest that the underlying observational evidence is rather indirect, increasing the possibility that transport model problems or the inversion set-up play a role. The first possibility is difficult to check. Regarding the latter, it should be checked if the resolution of the inversion (given the limited density of the observational

ACPD

5, S412–S416, 2005

network) is sufficient to resolve the Finnish emissions. Consider for example a synthetic experiment, where the 'true' emissions deviate from the prior either in Finland, or in some other high latitude region (accounting for measurement noise etc.). One would learn if sufficient information is - in principle - available to the inversion to detect an error in Finnish emissions.

2) Global constraints: The approach that is followed for the atmospheric oxidation of CH4 implies that atmospheric oxidants are assumed to be perfectly known. On regional scales this is a fair assumption (since they don't really influence concentration gradients on that scale). On the global scale, however, it is questionable and global inversions that account for uncertainty in OH show a significant impact. In this study it may be that the European emissions appear so robust because the prior global budget is well balanced (i.e. satisfies the global growth rate). A shift in OH would be compensated by a shift in the large scale emission patterns. This may well have consequences for the apparent robustness of the European budget and potentially for the distribution of emissions within Europe over well and less well observed regions. This study lacks a sensitivity test with reduced and enhanced OH, which would help to gain confidence in the robustness of the European emission total and the emission shift from Finland to Benelux-Germany-France that might appear as a dipole between well and less well observed regions.

SPECIFIC COMMENTS

Page 1012. The wetland emission total (175 TgCH4/yr) doesn't seem to match the emission total reported by Walter and Heimann.

Page 1018. Alpha: It is not clear why the scaling factor alpha is used. In a Bayesian inversion the covariance matrices specify the weights of the constraints. Introducing an additional scaling factor can be viewed as a systematic correction of the (co)-variances. This might be justified by the value of chi2 (to my surprise the factor alpha is not part of the chi2 calculations as formalized in the caption of Table 4. Furthermore, the con-

ACPD

5, S412–S416, 2005

tribution of the prior term to chi2 seems to be missing). Here, however, it is justified by the wish to balance the contribution of the priors and observations to the cost function, which seems to lack a formal statistical basis. It may be that this procedure conceals underlying statistical problems, for example, underestimated model errors. This should be clarified.

Page 1016 100The authors should be aware of a complication arising from the use of 100

Page 1018. Sensitivity scenarios. I would strongly recommend to summarize the differences between the scenarios in some central list or table.

Page 1020. Iterative approach It should be made clear how iterative inversions are performed (to avoid potential confusion about double counting of measurements or inconsistencies between the regional and global domain).

Page 1022. 'Although the model ... freedom for the parameters' This argument is not clear to me. Firstly, the number of parameters or observations alone does not provide any information on the effective information content (imagine an extreme case in which all observations were taken at the same place and the same moment). Therefore, the comparison of dimensions may look impressive, but need not necessarily mean much. Moreover, the outcome of the first inversion is used to exclude outliers. A much more transparant indicator of model performance would be a comparison between the model with prior emissions and the observations. (The deficiencies in the prior emissions should not affect simulated synoptic variability much).

Page 1023. Figure 9. This is an important and interesting comparison. Two questions remain: 1) what does the model simulation with posterior fluxes look like? 2) What might be the consequence of the fact that Pallas is located outside the zoom domain, and may in reality be at further distance of the wetlands than in the model (where they are both in the same grid box).

ACPD

5, S412–S416, 2005

Page 1034. ‘Concerning potential implications ... the Kyoto protocol’ This discussion that follows seems to ignore Figure 10. The main conclusion that one would draw from this figure is that a meaningful verification of bottom up inventories of the EU countries seems not yet feasible. Further it shows that the robustness of the outcome of individual inverse modelling studies may be misleading. It highlights the need for further investigation and intercomparison.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1007, 2005.