Atmos. Chem. Phys. Discuss., 14, C7149–C7156, 2014
www.atmos-chem-phys-discuss.net/14/C7149/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Top-down estimates of European CH_4 and N_2O emissions based on four different inverse models" *by* P. Bergamaschi et al.

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Received and published: 17 September 2014

reply to Anonymous Referee 1

We thank the referee for her/his comments. We have included our replies to the comments of the referee below.

(1) The setup of each inverse model is different and needs to be justified. How the choices of model setup affects the inversion results needs to be discussed.

In our model comparison only the basic settings were prescribed (as described in section 3.1), leaving the freedom for each group / model to choose the detailed settings (such as correlation parameters) considered optimal for the specific model. For several

models the dependence of inversion results on specific settings are described elsewhere (e.g. for TM5-4VAR in [Bergamaschi et al., 2010] (CH4) and [Corazza et al., 2011] (N2O) and for NAME-INV in [Manning et al., 2011]). As shown in [Bergamaschi et al., 2010] and [Corazza et al., 2011] the impact of the specific settings on the derived emissions was surprisingly low for the TM5-4DVAR inversion (for the performed specific sensitivity experiments).

(2) The ensemble is very small. With only 3-4 inverse modeling results, the statistical significance of the inter-model difference is low. I am not convinced that good agreement among three inverse modeling results would necessarily indicate that the uncertainties in the a posteriori emissions are low.

We agree that - given the small ensemble size - the statistical significance of the inter-model difference is limited. Future studies would certainly benefit from including additional, state-of-the-art models (however, only a limited number of inverse modelling systems is currently available). We will include a statement about this issue in the conclusions of the revised version.

(3) An important and unexplored aspect of the study is to understand the contributions of the observation data at 9-10 sites to the inverse modeling results. In addition, the models simulate some sites poorly after the inversion. The biases at some sites are consistent among the models. Would it be an indication that the inter-model difference under-represents the uncertainties of model estimates (Figs. 5 and 11)?

To analyze the impact of each observation site to the inversion results would be a considerable additional effort and is beyond the scope of the present study. How well models can simulate individual stations, strongly depends on the character of the sites. Stations with relatively high a posteriori RMS are typically sites with large regional emissions relatively close to the station (e.g. CB3, LON). Please note that Figures 5 and 11 show correlation and RMS, but not biases. The relatively high RMS and relatively low correlation coefficient for sites like CB3, LON is probably mostly due to

the mentioned significant regional emissions (and their sub-grid variability which is not resolved by the models), rather than a general bias of all models.

1. Inversion setup

(a) Table 5 could be more informative. Please add the information on spatial and temporal correlation scales and give the type of a priori emissions used in S2 inversions.

The information about spatial and temporal correlation scales is given in the text (section 3.2). Given the complex details for some models (e.g. for TM5 four categories are optimized in S1-CH4 and S1-N2O, with different temporal correlations lengths for the different categories) it is difficult to squeeze this additional information in an easily understandable way into the table.

(b) How were spatial and temporal correlation scales chosen for each model? If the assimilated model is representative of the state of the atmosphere, why was there significant difference in correlation scales among the models? Why did the spatial and temporal correlation scales change from S1 to S2 inversion using the same model?

The correlation scales were chosen independently by the different groups, usually adopting typical standard settings considered optimal for the specific model (see also reply to general comment (1) above for additional sensitivity experiments performed for some models). For S2 (starting from a flat distribution over land (for TM5-4DVAR and TM3-STILT)), much smaller spatial correlation lengths and much larger a priori uncertainties were applied, to give the inverse system enough freedom to retrieve smaller scale spatial emission patterns. This is mentioned in section 3.2.1.

(c) Why was there no S2 inversion using LMDZ-4DVAR? Why was a random a priori emission field used in NAME-INV but not the other two models in S2 inversions?

Unfortunately, S2 was not available for LMDZ-4DVAR. The specific settings for the 'free inversion' S2 were not specified in the modeling protocol. It would be certainly interesting to explore these 'free inversions' and their dependence on the specific setup

(including treatment of land vs. ocean) in future studies in more detail and in a more consistent way.

(d) Please give the spatial and temporal correlation scales (or something equivalent) for NAME-INV inversions?

Within NAME-INV the core grids (0.42 x 0.27 degrees) are aggregated together depending upon the influence of each grid on the observational network. Therefore more distant grids from the network form into larger inversion regions prior to the inversion. It is assumed that during the one-year inversion period the emission across each inversion region is uniform, i.e. the core grids within each inversion region are entirely spatially correlated. The separate inversion regions are assumed uncorrelated. The observations are assumed uncorrelated in time.

(e) Only TM5-4DVAR inversions had 4 difference source groups. All the other inversions had 1 source (the total emission). How would TM5-4DVAR results change if the inversion is for the total emission only?

For CH4 the impact on the derived European CH4 emissions is expected to be small (due to the small contribution of wetlands, rice and biomass burning). For N2O the impact might be larger, since soil emissions have a significant seasonality, while industrial emissions should be relatively constant throughout the year. However, this has not been tested separately.

(f) The emission mask description (P. 15699) should be presented in Section 3.2. Why would NAME-INV use a different emission mask from all the other models?

In this study a consistent country mask has been applied for all models (as stated in the last sentence on page 15699). However NAME-INV had used a different country mask in previous studies, taking into account also offshore emissions at some further distance from the coastlines [Manning et al., 2011].

(g) The stated model resolution for NAME-INV is 0.56x0.37. However, the inversion

results shown in Figs. 1 and 7 suggest a lower spatial resolution of this model than TM5-4DVAR's 1x1 inversion results. Why is that? Was there an error in NAME-INV inversion results (which could lead to lower posteriori emissions than the other models)?

While the resolution of the meteorological fields applied to run the NAME transport model was indeed 0.56 $\times$ 0.37, for the inversion a resolution of 0.42 $\times$ 0.27 has been applied (see section 3.2.4, and Table 2 (including footnotes)). The effective resolution of the NAME-INV inversion, however, is larger due to the described grouping into larger boxes, depending on the distance from the grid cells from the available observations (see section 3.2.4). NAME-INV inversion results are broadly consistent with the cited previous NAME-INV studies and there is no indication for an error.

(h) Please show the a priori model results in Figure 6. To what extent did the a priori information contribute to the good agreement of the vertical profile comparison?

Unfortunately, the a priori simulations for the vertical profiles are not available for all models. Furthermore, the comparison of a priori simulations is difficult, since for the global models these strongly depend also on the global emissions (e.g. if global a priori emissions and sinks do not match the atmospheric growth rate, the a priori simulations show a time-dependent bias), while for the limited domain models (NAME-INV and STILT) the background is already optimized from the global model.

(i) P. 15700, Line 19-24, please show the equations for the uncertainty estimates.

For uncorrelated errors, the square root of the sum of the squares of individual absolute errors is calculated, while for fully correlated errors the individual absolute errors are summed up. The reader is referred here to standard textbooks on statistics.

2. Ensemble representativeness

(a) Figure 3: If NAME-INV results were removed, it seems that the results of intermodel uncertainties would be very different. I am concerned that one outlier result was given too much weight in this analysis.

Yes, the overall range from all models ('intermodel uncertainties') is significantly increased by NAME-INV, which however, given the small ensemble size, is not considered an 'outlier'.

(b) Figure 9: Assumptions in the inversion, such as a small oceanic source in the a priori emissions, could be a reason that the inter-model difference is relatively small for N2O. It is very difficult to know that the ensemble of 3-4 models properly represented the uncertainties in inverse modeling. How robust were the ensemble results? Uncertainties of a small ensemble are difficult to assess statistically.

In S1-N2O the ocean inventory of [Bouwman et al., 1995] has been applied with global total emissions of 5.7 Tg N2O yr-1 (see Table 4), representing 25% of total emissions. We agree, however, with the general comment of the reviewer that the small ensemble size limits the statistical analysis of the 'intermodel uncertainties'.

3. Correlation and RMS analysis

(a) Please add the model results using the a priori emissions in Figures 5 and 11. It would be useful to understand how inverse modeling improved the model performance.

In general the correlation coefficients and RMS improve significantly with the inversion (e.g. for S1-CH4 and TM5-VAR the average correlation coefficient (average from all stations) is increasing from 0.65 (a priori) to 0.80 (a posteriori; as shown Figure 5) and the average RMS is decreasing from 35 ppb (a priori) to 27 ppb (a posteriori; as shown Figure 5). However, a consistent comparison of a priori simulations is difficult, since for the global models these strongly depend also on the global emissions (e.g. for LMDZ the average a priori RMS for S1-CH4 is 134 ppb (compared to a posteriori 32 ppb; probably mainly due to some global source - sink imbalance in the a priori simulation), while for the limited domain models (NAME-INV and STILT) the background is already optimized from the global model. Therefore, we would prefer, not to include the a priori statistics in the Figures 5 and 11.

(b) If possible, it would be useful to discuss the contribution of each observation site to the inversion results. This would be a function of the correlation scales, of course. It is also important to know the sensitivity of inversion results to the model set up assumptions.

To analyze the impact of each observation site to the inversion results would be a considerable additional effort and is beyond the scope of the present study. The sensitivity of inversion results to the model set up assumptions is investigated e.g. for TM5-4VAR in [Bergamaschi et al., 2010] (CH4) and [Corazza et al., 2011] (N2O) and for NAME-INV in [Manning et al., 2011]). However, a consistent sensitivity analysis on specific settings for the individual models is beyond the scope of this study. Since some important elements (such as modeling of model representation errors) are 'hard-coded' in some models (and approaches differ significantly), a consistent sensitivity analysis for a heterogenous model ensemble is quite difficult.

(c) For both CH4 and N2O, there are some sites that have low R values and other sites that have high RMS values, and the biases can be consistent among the models. What are the reasons? Would this result imply that the ensemble of a small number of models cannot appropriately represent the uncertainties of the inversions?

The capability of the models to simulate individual sites strongly depends on the specific character of the stations (e.g. elevation above surface and local regional emissions). The relatively high RMS and relatively low correlation coefficient for sites like CB3, LON is probably mostly due to the significant regional emissions (and their subgrid variability which is not resolved by the models), rather than a general bias of all models.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 15683, 2014.