Atmos. Chem. Phys. Discuss., 14, C626–C630, 2014
www.atmos-chem-phys-discuss.net/14/C626/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "CarbonTracker-CH_4: an assimilation system for estimating emissions of atmospheric methane" *by* L. M. Bruhwiler et al.

Anonymous Referee #1

Received and published: 21 March 2014

This is a very interesting study, which provides plenty of useful information about methane fluxes. Most of the results confirm finding of earlier studies, although it takes a slightly different angle by assuming that anthropogenic emissions were constant during the analyzed period. This has some implication, which can mostly be readily understood. Special attention is paid to emissions from high northern latitudes and North America, which provides useful new information, although as explained below, the significance of these findings remains somewhat unclear. Some further effort will be needed to clarify this and a few other issues as detailed below. After that the manuscript should be ready for the next stage of ACP.

GENERAL COMMENTS

The treatment of posterior flux uncertainties is not always clear. In the abstract no uncertainties are mentioned at all. When statements are made about emission adjustments of a few Tg/yr (like over the USA and high northern latitudes), the question arises whether such differences are significant. What are the limits of the resolving power of the inversion? On top of this comes the question how well the ensemble kalman filter allows quantifying of annual uncertainties. In my opinion, numbers are needed in critical places (abstract, conclusions) quantifying uncertainties, plus a few sentences explaining how to interpret those uncertainties in the context of the ensemble kalman filter. In addition, in places where uncertainties are given they should be more carefully defined. In figure 4 doesn't provide information about the red lines. The caption of Figure 6 mentions error bars, which don't show up in the plot (the same for Figure 7). Figures 8, 10, and 13 show error bars, but it is not clear what interval they represent. In addition, to be able to judge the significance of changes from one year to the next it is useful to show the combined uncertainty of all processes.

The growing mismatch at Lamont is interesting. The recent developments in shale gas mining is an explanation that comes to mind quickly. However, it should also be noticed that a large fraction of the mismatch already shows up in 2002. For a big region inversion such mismatches are expected in source regions, since the inversion cannot change the emission patterns within a region. In this inversion the problem may even be larger because of the use of EDGAR3.2 emissions, which are based on the 1990 – 1995 emission distribution. It would be useful to compare the patterns with those of EDGAR4.2, to see if a shift in emissions is actually picked up by the inventories.

One of the outcomes of this study is a shift in emissions from high latitudes to the tropics. It is mentioned that this outcome is consistent with what is found in earlier studies. What is not mentioned, however, is that several of those estimates were made using the TM family of transport models. Recently a concern was raised (Patra et al, 2011; Monteil et al, 2013) that the inter-hemispheric transport in TM5 is too slow. This may explain part of the shift in emissions from north to south reported in this study.

Some sentences are needed to raise attention for a possible role of transport model uncertainty.

Some discussion is needed of the potential impact of changes in the operational ECMWF model on the inversion-estimated inter-annual variability. The disadvantage of the operational forecast that is used, is that the atmospheric dynamics are not treated consistently throughout the analyzed period. As mentioned in the text, the vertical coordinate system changed in 2006. Comparisons between posterior fluxes before and after 2007 are consistent other studies, which used the ECMWF reanalysis. This provides some confidence that the change in the dynamical model might not play a significant role. Nevertheless, the possibility of artificial inter-annual variations due to the use of OD meteo should be mentioned.

The use of constant anthropogenic emissions is an interesting experiment to find out to if trends in the anthropogenic emission inventories are recovered by the inversion, in particular, in light of the debate about the magnitude of such trends before 2007. There could be a concern that this will lead to an underestimated posterior trend, unless the measurements resolve this trend so well that the prior doesn't play a significant role. Looking at Figure 6 the trend seems to be recovered well, however, there is still a systematic bias in the mean concentration. The question is why this happens. It made me wonder how the initial concentration was treated in the inversion, which I couldn't find back. Is it optimized or do emission corrections in the first year account for errors in the initial field that is used. If the latter were the case then this would influence the posterior trend. Some further discussion is needed here.

SPECIFIC COMMENTS

P2179, line 10: There is no mentioning of the time dimension of the state vector. I presume the 121 refer to a single month?

P2179, line 19: What is meant by satellite observed "hot spots"? Fire counts, burned area, ..?

P2180, line 11: This argument is more often used to justify short assimilation windows. I wonder, however, if there is any evidence of transport model errors accumulating over time. One may argue also that errors representing synoptic scale variations may dissipate on longer time/spatial scales that are better resolved by the course resolution transport model. Much of the observational constraint that inversions make use of come from larger scale mass balances. By reducing the response functions, this signal may end up being aliased to shorter scales. It is difficult to quantify the significance of this, but a more careful formulation seems needed here.

P2183: Since Bergamaschi et al 2007 refers to an inversion, a reference is needed of where natural wetlands emissions come from that where used in that study (or the model that was used to generate them).

P2187, line 15: Which global model is 'a global model'?

P2188, line 18: The model resolution of 6x4 degree seems more relevant here than the 1x1 degree of the emission inventories. Besides this, the inversion doesn't allow changing small-scale emission patterns. It makes me wonder how valid it is to include tall tower measurements in the analysis. An additional error on top of the representation error seems needed here.

Some figures are either quite small (7 and 10). This is true also for Figure 9, but that one doesn't seem to provide much information and could probably be left out.

TECHNICAL CORRECTIONS

P2179, line 24: '. . . ARE not captured . . .'

P2181, line 4: sigma i.o 's'

Table 2: "concentrations 3 sigma" add "deviate more than"

Figure 5: This figure and caption needs a more careful look. Where is station DNP? The x-axis has no labels. The caption is a copy of Figure 4 mentioning PRIOR-OBS

residuals that are not shown.

Figure 7: The caption can be shortened mentioning that panel b) is the same as a) but for the tropics.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2175, 2014.