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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-species inversion of CH_4, CO and H_2 emissions from surface measurements" *by* I. Pison et al.

I. Pison et al.

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D. Section 3:
(a) Is the a priori forward simulation done with SACS?
Yes, it is. This will be added in the text.
(b) Line 12 p.20696: 'Note that MCF emissions are very small since early 2005, after years of large industrial use.' This does not say anything about 2004 and has not quantitative information.
It is actually '1995' (with emissions of about 0.235 Tg decreasing to less than 0.095 Tg the following year) instead of '2005'.
(c) Line 19: Add 'after 20 iterations, the norm of the cost function gradient [. . .]'
This will be modified.
(d) What is the lag for the inversion? How far back in time will observations on day d

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will impact surface fluxes? Is this mentioned anywhere?
Our variational systems is not limited by the length of the inversion window. Therefore, we process the whole year at once. In this context, the problem is not that of the duration of the backward influence of an observation.
(e) Table 2 is interesting but can you say something about the regional differences on how well the filter performs and what causes them?
No outstanding regional differences were found for the analysis of the results at the station level.
(f) Which units should be used: Mt or Tg?
Tg will be used everywhere.
(g) How are the uncertainties on the fluxes modified by the inversion?
The uncertainties on the analyzed fluxes are not obtained directly in our method. Nevertheless, we can compute them as explained in the paragraph 'Error reduction' where the maps show how the uncertainties on the analysed fluxes stand with regard to the prior uncertainties.
(h) The CO ocean source seems to be changed: Can you provide the actual numbers?
The CO ocean source is changed by the inversion from 17.2 Tg prior to 21.5 Tg analyzed.
(i) Provide a table with regional total estimates for all 3 species.
These data are given here in Table 1. Since the interest of our method is to provide emissions at a pixel scale, we have added maps of the prior and posterior emitted masses for CO and CH_4 in Figure 5 (here `http://www-lsceorchidee.cea.fr/ACPD/acpd-2008-0504-f05ter.eps` **with (a',b') and (a"-b"): prior (') and posterior (") emitted masses per pixel for the year 2004 for CO and CH_4).**

(j) When citing other studies, compare your CO regional fluxes with these other studies. For example, how much CO was emitted by fired in boreal North America in Pfister et al. [2005] and Turquety et al. [2007]? How confident are you in your inversion results?

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Table 1. Prior (in brackets) and posterior emitted CH_4, CO and H_2 masses (Tg) during 2004 for the thirteen regions. Ocean sources for CO are (17.2) 21.5 Tg and for H_2 (4.5) 4.6 Tg.

Region	CO	CH_4	H_2
North America Boreal	(20.8) 33.8	(17.1) 16.3	(0.6) 0.5
USA	(115.7) 154.4	(49.9) 51.4	(5.2) 4.8
America Tropical	(34.7) 51.0	(18.8) 19.1	(1.5) 1.4
South America Temperate	(111.3) 80.3	(53.0) 48.1	(3.2) 1.9
Northern Africa	(146.4) 188.7	(36.1) 38.3	(4.1) 4.0
Southern Africa	(98.8) 89.0	(17.8) 18.1	(2.6) 2.3
Europe	(102.5) 128.0	(59.6) 56.2	(4.6) 4.3
Eurasia Boreal	(24.1) 79.3	(33.1) 28.5	(0.8) 0.8
Middle East	(19.0) 39.4	(7.6) 7.7	(0.8) 0.8
Indian peninsula	(86.5) 153.7	(67.0) 73.6	(2.5) 2.4
East Asia	(185.5) 305.7	(85.3) 89.0	(6.0) 5.7
Australia	(25.4) 12.8	(11.1) 10.4	(1.0) 1.0
Indonesia	(70.7) 75.3	(34.0) 34.4	(1.9) 1.7

We will add this comparison in the text: "For the area defined as "North America boreal", the study by Pfister et al. (2004) gives posterior emissions of CO in summer of 30($\pm$ 5) Tg from fires and Turquety et al. (2007) give 19 Tg from fires and 11 Tg from peat. Since we do not separate processes in our inversion, this is to be compared to our posterior of 18.8 Tg in the same area for the same summer. Our results are then close to the lower bound of the range of analyzed emissions."

(k) The authors should explain that they do not compute the a posteriori uncertainties on the fluxes in their variational inversion set-up. The numbers given in the 'error reduction' section are clearly optimistic and do not take into account transport model errors.

We do compute the a posteriori uncertainties as stated above (g). Of course,

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the level of confidence in these values is limited by the confidence in the error statistics to begin with and by the number of members in the ensemble (96 in our study, which is not small). An error reduction up to 50% (the maps show values between 1 and 50%) does not seem so optimistic to us. The transport model errors are taken into account and included in the R matrix as explained in the answer to C-h).

(l) When you do the mono-species inversion, how many iterations does it take to converge? The reader needs to understand the cost of the multi-species approach.

The mono-species inversions were stopped at the same number of iterations as the multi-species one and it took the same time i.e. about two months on an Opteron-64 bits architecture (sequential computing) per inversion.

Giving relative changes of the fluxes is not very meaningful. Please put the regional fluxes estimates in the same Table where you will out the prior and the multi-species inversion posterior.

These figures are provided in Table 1 here. See also answer to question i): we will add maps of the prior and posterior emitted masses for CO and CH_4 in Figure 5 (here `http://www-lsceorchidee.cea.fr/ACPD/acpd-2008-0504-f05ter.eps`**).**

Regional balances are not our main focus here, this paper exposes our methodology and a first attempt at validating the inverted fluxes.

Can you do an inversion with CO + MCF and another one with CH4 + MCF. It would be interesting to see how a different OH impacts your results.

In this paragraph, we just aimed at illustrating that inverting several species in a consistent chemical system makes some differences with mono-species inversions. Performing such inversions is very expensive in computing time, this is why we did not perform inversions considering only CH_4+MCF or CO+MCF.

Are the OH concentrations from these 2 inversions and the multi-species one presented in the paper any different?

The prior OH are always the same. In the multi-species inversion, they become

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different because they are optimized whereas in the mono-species inversion, they remain the same as the prior.

E. Section 3.4
(a) 2004 is not covered by Bousquet et al. [2006]. Please clarify.
This was not clear: we will state that we use here an update of Bousquet et al. [2006] that covers up to 2007.
Page 20701 Bousquet et al. [2005] is cited: mistake?
This is a typo, it will be corrected.
The two inversions were done with the same transport model so the inversion systems are NOT completely different.
The mathematical principles of the two methods are very different, as is the chemistry. LMDz in the only common point with the prior flux space and time distributions.
(b) See also General Comments: When using the POC, AOC and WPC data: reference the labs and PI doing this work. Same for HNG and ORL. Observational data are not 'off-the-shelf' items.
The sentence will be re-written: "For the year 2004, 456 mobile surface observations of CH_4 concentrations are available through the Pacific Ocean, Atlantic Ocean and Western Pacific Cruises (POC, AOC, WPC) (Conway et al., 1994), ships in the North Atlantic (CVS) and in the Indian Ocean (MDF, Marion Dufresne ship) (Schmidt et al., 2006) and 202 three-dimensional observations are given by aircraft measurements made in Hungary (HNG, 12 flights in the year, none in February and March, 2 in April and December, 1 for the other months) and in France (ORL, 25 flights in the year, 1 in January, March and April, 2 in June and August, none in December, 3 for the other months) (Schmidt et al., 2006) [. . .]"
(c) Fig 9, use ppb instead of ppm
This Figure will be modified.
(d) See if redoing the comparison with MOPITT over the continents only and for daytime changes the numbers in Table 5.

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As explained in the answer to General Comment 5, it is a comparison using daytime and the two hemispheres were separated so as to show the impact of the ocean.

F. Conclusions

(a) The CO emission under-estimations in India and China is not only due to fossil fuel burning but also biofuel burning.

It is actually all the emissions that have increased, traffic, domestic and industrial. We will change "rapidly industrializing areas" to "developing areas (increased emissions by industry, traffic and population)".

(b) Put your study in the context of what the state of the science is and what other authors have shown. Once again, relative differences between your various inversions are not useful to compare with other authors' quantitative estimates of sources.

As stated in the answer to D-l), this is a first attempt at validating the inverted fluxes. Analyzed inventories will be made available at a later stage on a website, like for Bousquet et al. (2006) and its update, for quantitative comparison to other inventories in the next step. Carboscope website: `http://inversions.lsce.ipsl.fr/carboscope/index.php` **.**

(c) See also General comment # 6.

As stated in the answer to General Comment 6., we will re-phrase this part of the conclusion.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20687, 2008.

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