Atmos. Chem. Phys. Discuss., 8, S11736–S11740, 2009
www.atmos-chem-phys-discuss.net/8/S11736/2009/

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.

Received and published: 18 March 2009

Specific comments

A. Abstract:
(a) The abstract should clearly state which observations are used. The model LMDZ is mentioned so should be the observational networks AGAGE, CSIRO, JMA/MRI, LSCE, NIWA, NOAA ESRL and the MOPITT CO data. A vague abstract has no purpose.
We will remove LMDz from the abstract, since this information is not relevant at this point. Our abstract does not seem vague to us: it is necessary short but contains the main elements necessary to understand the scientific topics treated in the paper.
(b) The optimization of HCHO is only mentioned in the abstract. What has been learnt

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

from doing it?
HCHO is optimized as part of the chemical chain since it links CH_4 to CO but we did not intend to focus on it in this study. We plan to constraint HCHO with observations from OMI and assess the limitations of the use of such measurements with SACS.

B. Section 1
(a) Site the networks and the number of stations today.
We used the networks and number of stations as available for the year 2004, since it is the studied period: these are the figures that are cited.
The statement 'Structured in international networks' is not correct. Each national lab manages its own network of regionally or globally distributed stations.
'International' will be deleted from the text.
GLOBALVIEW is a data product and should not be mentioned here.
GLOBALVIEW is mentioned above the paragraph of the definition of the error statistics for observations. We think it is appropriate to mention this reference here.
(b) When mentioning the 1% accuracy for methane satellite data [Frankenberg, 2005], the authors should cite which instrument they are referring to.
This refers to the UV/Vis/near infrared spectrometer SCIAMACHY on board the European satellite ENVISAT. We will state this clearly in the text.
(c) Replace the assimilation of 'bio-geochemical data' with 'atmospheric concentrations': Line 10 p 20689.
The expression 'atmospheric concentrations' would not be correct for studies such as Rayner et al. (2005) for example.
Rayner, P. J., M. Scholze, W. Knorr, T. Kaminski, R. Giering, and H. Widmann (2005), Two decades of terrestrial carbon fluxes from a carbon cycle data assimilation system (CCDAS), Global Biogeochem. Cycles, 19, GB2026, doi:10.1029/2004GB002254.
(d) Which MCF measurements do you use for OH concentrations optimization?

ACPD
8, S11736–S11740, 2009

Interactive Comment

AGAGE only?
MCF measurements come from NOAA and AGAGE networks, see also references in answer to General Comment 2.

C. Section 2
(a) Section 2.2.1 states that the adjoint of the transport model LMDZ were developed for this work, but they are already used in Chevallier et al. [2005]. Has the model changed since then?
The chemical part of the model, both direct and adjoint, has been developed for this work. The transport part of the model comes from Chevallier et al. (2005). We will make it clearer in the text.
(b) Plots in Figure 2 show global mean differences?
The bias and standard deviations were computed with instantaneous tracer distributions at a pixel scale, not means.
(c) Section 2.3: How do you build your prior emissions for 2004? EDGAR 3.2 is for 1990 and 1995. Table 1 should be redone. The QUANTIFY inventory for CO should be in the Anthropogenic emissions with a reference. The biomass burning is cited in 2 different ways: first GFED then van der Werf. . . . Both need to be there 'GFED-v2 for 2004 [van der Werf et al., 2006]'.
We do not build special prior emissions, EDGAR is used as is for 1995. See also answer to General Comment 3: the paragraph has been re-written and Table 1 removed.
What is the magnitude of the uncertainty on the a priori fluxes?
As stated in the answer to General Comment 3, "In this study, the errors are then set to 100% of the maximal flux over the inversion period for CO and CH_4, 100% of the flux for H_2, 1% of the flux for MCF (in order to constrain OH), [. . .]"
(d) Section 2.4: See General Comments: the authors should describe and reference the observational data they use. I strongly suggest redoing Figure 3 to show the various networks in different colors.
We will add the information on the observational data as stated in the answer to

ACPD
8, S11736–S11740, 2009

Interactive Comment

General Comment 2. Figure 3 does not show the various networks because all the measurements are treated in the same way by the inversion system.
(e) How do you invert for daily averages in your set-up?
In this case, the averaging is part of the observation operator.
(f) Replace NOAA04 scale with 'NOAA 2004 calibration scale [Dlugokencky et al., 2005]'.
This will be modified.
(g) Reference the product GLOBALVIEW with GLOBALVIEW-CH4: Cooperative Atmospheric Data Integration Project - Methane. CD-ROM, NOAA ESRL, Boulder, Colorado [Also available on Internet via anonymous FTP to ftp.cmdl.noaa.gov, Path: ccg/ch4/GLOBALVIEW], 2008. And GLOBALVIEW-CO: Cooperative Atmospheric Data Integration Project Carbon Monoxide. CD-ROM, NOAA ESRL, Boulder, Colorado [Also available on Internet via anonymous FTP to ftp.cmdl.noaa.gov, Path: ccg/co/GLOBALVIEW], 2008.
This will be added in the text.
(h) The whole section discussing the uncertainties on the data is really vague. Where are the uncertainties provided: reference article or website. Which uncertainties are specific to each measurement? Which are upper limit estimates for the whole dataset?
The uncertainties that are described here are not uncertainties on the measurements but the information we use in the observation error statistics for the inversion. This paragraph will be re-written to make this clearer: "The errors on observations were set as follow. For CO and CH_4, we used yearly means of synoptic variability from GLOBALVIEW; for MCF, we used yearly or monthly values from Prinn et al. (2005) or from the NOAA; and for H_2, data from CSIRO or from the NOAA were used. It is assumed here that the synoptic variability is an approximation of the transport errors, which is consistent with the weaknesses of global model in reproducing continental synoptic concentrations of trace gases (Geels et al., 2007). Otherwise, if these data were not available, the uncertainties associated with the measurements were used. In all cases, a minimum uncer-

ACPD
8, S11736–S11740, 2009

tainty of $\pm$3 ppb for CH_4, CO and H_2 and $\pm$1.2 ppt for MCF is fixed, in order to prevent the error getting too small." Note that the uncertainty of a measurement is included, with the transport error, in the observation error.

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

ACPD

8, S11736–S11740, 2009

Interactive Comment