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Atmospheric Chemistry and Physics Discussions

Interactive comment on "

Source attribution of the changes in atmospheric methane for 2006–2008" *by* P. Bousquet et al.

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We thank the referee #1 for his/her comments and provide detailed answers to all his/her comments below (answers marked with "–>") :

This study makes an important contribution to our understanding of the dominant controls on the global methane cycle and their variations in recent years. A few attempts have been made to investigate the causes of the renewed increase of methane starting in 2007 using measurements only (in-situ and satellite). The added value of this study, as clearly demonstrated in the last figure, is to use a modeling approach that takes into account atmospheric transport. Overall, the approach that is taken is rather

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straightforward and defensible. The results look reasonably internally consistent. The authors are wise enough to limit the discussion to latitude bands, since the robustness of inversion results usually quickly reduces going towards smaller scales. However, it would have been interesting if the discussion had elaborated a bit more on the role of e.g. Asia/North America regarding anomalies of northern wetlands and South America/Africa/Indonesia regarding tropical wetlands. At least the results Orchidee should provide some information on the expected relative importance of these regions. If the inversions aren't robust enough to verify these estimates that would be worth mentioning too.

–> We agree with this point. Atmospheric inversions constrained by the uneven surface network do not allow us to provide a regional estimate of methane fluxes. However, the process-based model ORCHIDEE has the capacity to provide insights on regional fluxes, although the direct flux measurements used to validate ORCHIDEE are not so numerous close to wetland areas. We propose to add additional text in the revised version, describing the regions contributing to anomalies in ORCHIDEE (see also below).

Below are a few suggestions for clarification and improvement as well as some comments that need to be addressed to make this paper ready for publication in ACP.

GENERAL COMMENTS

Target period

It is not really clear for what target periods INV1 and INV2 are run. Are the reference period and analyzed period 2006-2008 part of the same inversion? Why was INV2 run for an extended period into the first part of 2009 and not INV1? The extension seems a prerequisite for analyzing the year 2008. The requirement of S4 for valid measurements along the period 1984-2008, suggests a much longer target period. If not, why should the stations cover 1984? Information about the applied target period is critical to judge, for example, if the reduced emissions in 2006 could have a relation with the initial condition of a possible 2006-2008 inversion.

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–> At the time of the submission INV1 had been run for 1984-2008 and INV2 for 1990-2009. Then the period 2006-2008 has been extracted and anomalies have been calculated using the 1999-2006 period as a reference. We agree that a proper estimate of methane fluxes for the year 2008 in INV1 requires 2009 observations. Therefore, in the revised version of the paper, we will present results with INV1 extended to 2009, and estimate the impact of adding 2009 observations on 2008 results. The target periods of the different models will be stated in section 2.1 (INV1, INV2) and section 2.2 (ORCHIDEE).

The 2006 minimum is an interesting finding, which in my opinion deserves some further discussion.

–>We will add the 2006 anomaly details in table 2 and write related text to reinforce the 2006 anomaly analysis in the tropical section (3.2).

What could have caused it if the wetland model suggests there should rather have been a positive anomaly?

–>Indeed, ORCHIDEE suggests a positive anomaly in 2006. We have questioned the assumptions made for the calculation of the meteorological forcing of ORCHIDEE.. In the forcing used for the submitted version, there was a temporal discontinuity in the climate forcing in 2002 caused by the intrinsic difference between the two products used to generate the forcing file (CRU and NCEP). The corresponding 2002 discontinuity in the modelled wetland emissions was enhancing the positive anomalies for the period 2006-2008. The CRU+NCEP forcing has been reconstructed based on a climatology of the difference between CRU and NCEP and not only on the single year 2002. This leads to a reduction of the large positive anomalies of ORCHIDEE in 2007 and 2008 and to a very small negative anomaly in 2006. These new results, included on the revised version, are in better agreement with INV1 and INV2 over the three years.

Beside, as noticed in the submitted version of the paper, ORCHIDEE seems highly sensitive to precipitation in both the tropics and boreal regions. The consequence of this sensitivity is that the positive anomaly that runs from 2005 in atmospheric forcings (CRU+NCEP) of ORCHIDEE generates a large and increasing positive anomaly in CH4 fluxes from wetlands, (although a bit reduced in the revised version) which may hide some other effects and bias anomalies toward positive ones. This point was discussed in the submitted version and is kept in the revised version.

Anthropogenic emissions

The trend in the anthropogenic emissions receives little attention. The reference to Olivier and Berdowski suggests that a rather outdated anthropogenic emission inventory was used as prior. It is mentioned that according to EDGAR4 these emissions should have increased significantly in recent years. However, the results in Table 2 suggest only minor changes. Can the inversion results be reconciled with the anthropogenic emission estimates or not?

–>At the time we started this work, EDGAR4 was not yet released and therefore we used the spatial distribution given by EDGAR3.2 for the prior anthropogenic emissions. Note that no interannual variability is prescribed in the prior values of anthropogenic emissions for the different years, letting the inversion determine the trend. EDGAR4 inventory data are available per country for the period 1990-2005. The trend of EDGAR data is about +1.5Tg/an for the full period (1970-2005), but much larger for 1999-2005 with about +6 Tg/an. Unfortunately EDGAR4 does not yet provide a full set of data after 2005. We do find a positive anomaly of anthropogenic emissions of +5/+2/+5 (table 2) for 2006/2007/2008, which lay between the recent trend (+6 Tg/yr) and the long-term trend (+1.5Tg/yr). In Bousquet et al. 2006, our analysis suggested that the increase in anthropogenic emissions in the early 2000s is hidden by a decrease in wetland methane emissions due to droughts, leading to the stagnation (on average) of methane mixing ratios in the atmosphere for this period. With this work, we show that anthropogenic emissions increase after 2005, but wetland emissions also, both explaining the atmospheric increase (table2), although wetland emissions dominate. We agree that this part deserves more text and we propose adding the subsequent text in

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section 3.1 (Global scale) when discussing emissions other than wetlands: ”The partition between anthropogenic and natural anomalies in methane emissions reveals that, although dominated by changes in natural wetland emissions, anthropogenic emissions contribute significantly to the global emission anomaly at a rate ranging from 2 to 5 TgCH4/year for the 2006-2008 period. Indeed, the new EDGAR4 database shows that the trend of anthropogenic CH4 emissions has been, on average, of +6Tg/yr for the 1999-2005 period. Bousquet et al. (2006) proposed that this increase has been hidden by a simultaneous decrease in wetland emissions due to droughts in the northern hemisphere. After 2006, the scenario proposed here by INV1 is an increase of wetland emissions together with an increase of anthropogenic emissions at a rate slightly slower than estimated by EDGAR4 for the 1999-2005 period, but still faster than the mean growth rate for the 1970-2005 period.”

Pulse experiment

These results provide a convincing explanation of why the inversion-derived emission anomalies are largest in the tropics. Looking closer, however, a few questions remain. Why are the concentrations rescaled on an equal area grid? The response of a 1 Tg pulse on the concentrations doesn't involve any area per grid conversions, does it? If so please provide further explanation.

–>Figure 3 presents the mixing ratios for model cells sorted from the highest to the lowest value. We wanted to compare the differences between boreal and tropical mixing ratios (intensity and horizontal spread) generated by the 1 Tg emission. As our transport-model grid is regular in longitude-latitude, the surface area of model cells decreases with increasing latitudes. To perform a fairer comparison between tropics and high latitudes, we did the calculation of figure 3 on an equal area grid. The impact of doing so is to decrease the number of model cells at high latitudes compared to the tropics. Therefore, the decrease of mixing ratios is sharper for the boreal pulse than without the regridding (less model cells with high values in ppb). Not doing so would have increased the boreal versus tropics differences in ppb observed on figure 3. This

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can be considered as a normalisation by the surfaces. However, please note that this regridding does not change the conclusions and that this test is only semi-quantitative, which limits the importance of the assumptions.

Else, it is not clear why the pulse has been transported for 1 year, after which most of the signal has dispersed already. Looking closer at the plot, hot spots are visible that suggest a much shorter transport time. The caption mentions 'cumulative', but if all the plumes are added their sum must end up much higher than a few ppb/TgCH4. This confusion must be resolved.

–>Figure 3a & 3b have been plotted as the mean of the monthly plumes generated by 1 month of emission followed by 11 months of transport. This explains why mixing ratios are not much larger. The word "cumulative" was confusing. We will rephrase the figure caption: "Impact at the surface of a 1TgCH4 pulse emitted from two regions (see text): boreal Asia and tropical Asia. a/ Mean plume for Boreal Asia, calculated as the mean of the 12 monthly plumes generated by 1 month of emission followed by 11 months of transport without emission (in ppb/TgCH4); b/ Same as a/ for tropical Asia. c/ Maximum increase in mixing ratio detected for all transport model pixels, at the surface, for emissions in boreal Asia (black line) or in tropical Asia (red line). Model pixels have the same surface in order to normalize the comparison between a boreal and a tropical region. Model pixels are sorted . . ... "

MINOR COMMENTS

p. 27608: The 150% uncertainty applies to monthly fluxes? –>Yes, text will be modified

p. 27608: Flux filter: doesn't this procedure affect the much better resolved annual fluxes? –>This procedure removes large unrealistic month-to-month differences due to the underconstrained nature of the inverse problem. Indirect impact on yearly fluxes is possible, but the sensitivity test with no filter shows that these impacts remain small and in the range of other sensitivity tests.

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Table 1: why are the results of INV2 scaled to those of INV1? (one could just take out the mean?) Are the scaled results used as reference for calculating INV2 anomalies in subsequent tables? –>We assume here that variations in emissions for a mean emission of 200 Tg will be larger than variations in emissions for a mean emission of 150 Tg. Therefore we decided to scale INV2 and ORCHIDEE means to INV1 for comparison and to use this scaling to calculate anomalies (table 2).

Table 1: several numbers are left out with the argument that regional anomalies are small, but this table does not deal with anomalies ..(?). –>We do not show the mean regional fluxes as we do not discuss their anomalies. We will modify the table caption: "....For INV1, regional fluxes are not shown for sources other than wetlands, as we do not discuss their anomalies (table 2) which are generally very small...."

Table 1: Do the results for INV1 represent the mean of the 11 experiments? –>Yes. Table caption will be modified: "For INV1, values are the mean of the 11 inversions performed"

TECHNICAL CORRECTIONS

p. 27607, line 8: remove "of" –>OK

p. 27608, line 3: "3" instead of 'tree' (=arbre!) –>OK

p. 27609, line 24: "as" the transport model –>OK

p. 27611, line 4: "This" makes an average : : : –>OK

p. 27613, line 20: "for" three main reasons –>OK

p. 27616, line 14: "In fine" ? –>We mean "Finally". Changed.

p. 27618, line 21: wetlands are assimilated to peatlands? –>No distinction about the different type of wetland (marsh, swamp, peatland, etc) is done for the modelling of CH_4 emissions with ORCHIDEE. We know that is a limitation of our approach: for example, a peatland is a different ecosystem from a marsh and have a different pro-

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ductivity, soil carbon dynamics, etc. Currently, there no wetland plant functional type with different parameterizations in the model relative to the different types of wetlands. To counterbalance that, a supplementary constraint is used a posteriori for peatlands (whose the spatial covering is assumed to be limited to boreal regions) : a soil carbon map is used to limit the peatland extent given by the model. We will rephrase the desciption of ORCHIDEE in appendix to clarify this point.

p. 27618, line 23: “accumulation” : : : “to provide” substrate –>OK

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