

Interactive comment on “

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

P. Bousquet et al.

philippe.bousquet@lsce.ipsl.fr

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We thank the referee #2 for his/her comments and provide detailed answers to all his/her comments below :

Answers to Interactive comment on “Source attribution of the changes in atmospheric methane for 2006–2008” by P. Bousquet et al.

Answers to Anonymous Referee #2 (→)

Since 2007, global methane concentrations have grown, following almost 10 years of stability. This paper is the first attempt to attribute a cause to this growth, using a

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three-dimensional inverse modeling framework. Two established inverse methods are used to investigate the anomaly, along with a wetland model. The main conclusions are that tropical wetland emissions increases were the dominant contributors to the 2007 and 2008 growth, with a significant contribution from Boreal wetlands in 2007. This paper tackles an important subject in a thorough manner and I recommend it for publication in ACP, provided the following comments can be addressed.

-One of the main conclusions is that “good agreement” is obtained between the anomalous emissions derived in the inversions and those calculated by the wetland model in 2007. However, very poor agreement between the top-down and bottom-up anomalies is obtained for 2006 and 2008 (including disagreement on the sign of the anomaly in 2006). It therefore seems to me that this conclusion is over-stated. If the wetland model (or the inversion) is not correctly attributing the change in 2006 and 2008, why should it be able to provide useful information about the cause of the 2007 increase?

→In this study, we compare two inversion methods (top-down) and one process-based model (bottom-up) estimating CH₄ emissions. This is quite a challenging task as top-down and bottom-up are independent methods in our case. We should have distinguished more clearly two levels of comparison: INV1-total with INV2-total, and INV1-wetlands with ORCHIDEE. We revisited two issues raised by the reviewer in the revised manuscript, that improve the agreement between the different models:

- First, we extended the inversion period of INV1 until end 2009 so the 2008 fluxes are now constrained by 2009 observations (thus avoiding last year lack of constraints, see answer to referee1). This time extension has an impact on INV1 results in 2008 and the INV1-total and INV2 agree better at global scale with these new inversions, both having the same sign of anomaly in 2006, 2007, and 2008.

- Second, we have modified the assumptions made for the calculation of the meteorological forcing used by the ORCHIDEE model. In the forcing used for the submitted version, there was a temporal discontinuity in the climate forcing in 2002 caused by the

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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.

We agree that the agreement between INV1, INV2 and ORCHIDEE is not so good in the submitted version of the paper for 2006 and 2008. Considering the above corrections, the agreement between the three models is improved but we will be careful in defining the level of agreement between the three models in the revised manuscript.

-It is stated that, since the variational inversion (INV2) does not resolve source types, only total fluxes can be compared with the analytical inversion (INV1). However, could process-based changes not be inferred from the INV2 emissions by comparing the optimized flux field with the source distributions used in INV1? For example, if anomalously high emissions were obtained for grid cells over a predominantly wetland region, a wetland anomaly could likely be inferred.

→ This statement from the reviewer is true if the inversion does not redistribute the prior flux spatially. Actually, this is one of the interests of the 4D-var inversion (INV2) to be able to change the spatial pattern of emissions, as each model cell is inverted, thus reducing the so-called aggregation error possibly made in INV1 (large region approach). As only the total methane flux is inferred with INV2, we do not have information on the redistribution of emissions for the different categories due to the atmospheric observations integrated in the inverse model. Therefore, we are not in favour of applying the source distribution from INV1 to INV2 estimates (or more surely from the components used to calculate the total flux of INV2 prior).

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-Column-averaged optimized mixing ratios are compared to SCIAMACHY observations from 2006 to 2008 over South-America, with the intention of demonstrating that the inferred tropical anomalies are reasonable, even though few surface observations are available in the tropics. However, whilst the trend is relatively well modeled from 2007 onwards, it is not clear to me that this demonstrates conclusively that the tropical emissions anomaly is consistent with the observations, since much of the signal could reflect any change in the global background. Would similar agreement be obtained with an emissions increase everywhere else except the tropics? Perhaps a second model run could be performed in which the anomalous emissions were distributed to the extra-tropics, with tropical emissions not being permitted inter-annual variations. If poorer agreement with the SCIAMACHY column observations were obtained over e.g. South America, would this not strengthen the case the tropical emissions changes were being captured adequately by the surface network?

-Line 22 on page 27614 states that we cannot discriminate between the INV1 and INV2 anomalies using SCIAMACHY in 2006. It is not clear what is meant by this, and this statement needs clarification.

→ These two points go together:

Our aim with figure 2 is to see whether the inverted methane emission changes are consistent with independent SCIAMACHY retrievals of CH₄ and if this consistency is very sensitive or not. It is not to demonstrate conclusively that another scenario with large anomalies in the extra-tropics could not do as well as INV1 or INV2 scenarios to fit SCIA data. This is beyond the scope of this work. However, the fact that INV1 and INV2 flux scenarios give a similar fit to SCIA retrievals with very different tropical anomalies in 2006 for instance and very different latitudinal partition (as in 2008) is an element of answer to this question: it indicates that SCIA columns are not very sensitive to the amplitude of regional anomalies of surface fluxes, but probably integrate larger regions. This is not such good news for the use of satellite data to constrain regional surface fluxes. We agree that the text was not clear on this point and we will rephrase

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last part of section 3.2 about figure 2 and SCIA data.

-It is noted that there may be some under-estimation of the 2008 anomaly in INV1, because no 2009 data were assimilated. Since this data is available for INV2, why can it not be included in INV1?

→Actually, INV1 inversion requires full years of observation. At the time of the submission, the last months of 2009 were missing and we could not run INV1 for 2009. This real issue has been corrected since. In the revised version of the manuscript, year 2009 will be included for INV1 and an additional analysis on the impact of extending the inversion period to 2009 will be proposed in the text.

Why would it be more likely to lead to an under-estimate than an over-estimate?

→As no observations were used in 2009, fewer observations were constraining the fluxes emitted during the second part of 2009 than during the previous months. Without constraints, the inversion stays close to the prior estimate, which more probably lead to an underestimation of the variations. This sentence will disappear in the revised version and the entire paragraph will be re-written.

Minor comments:

Page 27604: - Line 22, reference to IPCC 2007. I think the reference should be Forster et al. 2007 (if Chapter 2 is being referred to).

→OK, this reference will be modified. More generally, in the text, IPCC will be replaced either by Forster et al. (chapter 2) or by Denman et al. (chapter 7)

- Line 25. I think Rigby et al. 2008 were the first to note the anomaly.

→OK, Reference will be added.

Page 27605 - Line 4. "... represents about 90% of THE ch4 loss..." →OK

- Line 13, space before Simmonds reference. →OK

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- Line 19, Krol AND Lelieveld. →OK

Page 27606 - Line 6. Delete "out" from "point out". →OK

- Line 15. Perhaps "... temperature is more important at high latitudes", rather than "leads". →OK

- Line 28. "... chemistry AND transport...". →OK

- Line 23, delete either "processes" or "source types" →OK, we will write "categories of sources and sinks" instead.

- Line 26, perhaps "... nudged TO analysed winds...". →OK

Page 27608 - Line 3. "... tHree-dimensional...". →OK

- Line 10. Peylin (2002), does not outline this method in detail. Perhaps Peylin et al (1999) is more appropriate here? →Both references are relevant and complementary. We will add Peylin et al. (1999).

- Lines 12-22. Perhaps this information could be more concisely presented in a table? →A table (1) will be added. Tables 1 & 2 will become tables 2 & 3.

Page 27609 - Line 7-8. "The inversion results consist OF eight-day...". →OK

- Line 13. "For comparison, THE global flux from...". →OK

- Line 24. "... AS the transport model...". →OK

Page 27610 - Line 10. I'm not sure if something's missing here, or perhaps the "of" needs deleting? →The sentence will be clarified.

- Line 23-24. "As INV2 does not separate source types...". →OK

Page 27611 - Line 1. I find the "resp." notation confusing. I presume this means "respectively"? YES. →OK.

- Line 16. "... enables US to better.."? →OK

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- Line 23. -4-+14% (as mentioned earlier in the paper). ->OK

Page 27612 - Line 2 "The ORCHIDEE model...". ->OK

- Line 10. "... significantly LOWER anomalies..."? ->OK

- Line 14. Is this finding about landfills being obtained from EDGAR, or is it being derived in the inversion? ->It is derived from the inversion. We will add the word "inverted" in the text.

Page 27613 - Line 9. "... over three different types of source." ->It is not only three because we grouped all anthropogenic sources in table 2. We will modify the text : : "...not attributed to wetlands but is spread over other types of sources"

- Line 20 "... for three main reasons. ". ->OK

Page 27614 - Line 3-4. "FEW surface observations..." . ->OK

Page 27615 - Line 16. "In THE inversions..." . ->OK

- Line 26. "For INV1, the high latitude ..." . ->OK

- Line 26. Which year? 2007. ->It will be added on the revised version.

- Line 28. "... less than one third of the tropical..."? YES ->OK

- Line 28. "These results MAY APPEAR TO CONTRADICT the larger anomaly..." . ->OK

Page 27616 - Line 3. "... a test with THE LMDZt model." . ->OK

- Line 14. "In fine"? ->We mean "Finally"

Page 27617 - Line 13. "... associated with a negative tropical flux anomaly..." . ->OK

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27603, 2010.

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