Atmos. Chem. Phys. Discuss., 10, C12118–C12122, 2011
www.atmos-chem-phys-discuss.net/10/C12118/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "

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

Anonymous Referee #2

Received and published: 7 January 2011

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 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?

- 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.

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

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).

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

Page 27605

- Line 4. "... represents about 90% of THE ch4 loss..."

- Line 13, space before Simmonds reference.

- Line 19, Krol AND Lelieveld

Page 27606

- Line 6. Delete "out" from "point out"

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

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

Page 27607

- Line 23, delete either "processes" or "source types"

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

Page 27608

- Line 3. "... tHree-dimensional..."

- Line 10. Peylin (2002), does not outline this method in detail. Perhaps Peylin et al (1999) is more appropriate here?

- Lines 12-22. Perhaps this information could be more concisely presented in a table?

Page 27609

- Line 7-8. "The inversion results consist OF eight-day..."

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

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

Page 27610

- Line 10. I'm not sure if something's missing here, or perhaps the "of" needs deleting?

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

Page 27611

- Line 1. I find the "resp." notation confusing. I presume this means "respectively"?

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

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

Page 27612

- Line 2 "The ORCHIDEE model..."

- Line 10. "... significantly LOWER anomalIES..."?

- Line 14. Is this finding about landfills being obtained from EDGAR, or is it being derived in the inversion?

Page 27613

- Line 9. "... over three different types of source."

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

Page 27614

- Line 3-4. "FEW surface observations..."

Page 27615

- Line 16. "In THE inversions..."

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

- Line 26. Which year?

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

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

Page 27616

- Line 3. "... a test with THE LMDZt model."

- Line 14. "In fine"?

Page 27617

- Line 13. "... associated with a negative tropical flux anomaly..."

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