Atmos. Chem. Phys. Discuss., 13, C11816–C11821, 2014
www.atmos-chem-phys-discuss.net/13/C11816/2014/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A multi-year methane inversion using SCIAMACHY, accounting for systematic errors using TCCON measurements" *by* S. Houweling et al.

S. Houweling et al.

s.houweling@uu.nl

Received and published: 3 February 2014

The comments of the reviewer are pasted below in bolt font. They are answered in roman font, including the corresponding modifications to the manuscript.

General remarks: The overall impression is that this is a solid and clearly presented study using sound and valid scientific methods giving credibility to the main conclusions. It clearly takes the method of inverse modelling to derive methane fluxes one step further. Uncertainties and challenges in the applied method are to a large degree well explained and the need for better coverage of

measurement data is rightfully highlighted. I recommend publication but have some (mainly minor) comments I suggest the authors should address. See detailed comments below.

Thank you for your interest and time to study our manuscript in detail and for providing useful contributions for improving it further.

Detailed comments:

Page 28120, Line 1-3: I miss references to former studies on these issues. A lot of references in the next paragraph none in this.

The following references were added:
- Dlugokencky et al. (2009)
- Kirschke et al. (2013)
- Aydin et al. (2011)
- Pison et al. (2013)

P28123, L 27: Missing "of": measurements of SF6

Corrected.

P28124, L10: Why is Edgar 4.1 and not Edgar 4.2 used? This would avoid much of the extrapolation since Edgar 4.2 has emission data until 2008. If 4.2 wasn't available at the time of this study it would be good to compare the values you get from your extrapolation with Edgar 4.2 for total emissions and major sectors and state how the uncertainty in apriori emissions are affected by the extrapolation.

EDGARv4.1 was used because v4.2 came out during analysis of the inversion results, which are rather CPU time consuming. Therefore it was decided to keep the v4.1 results, with the advantage that it allowed us to document our procedure for emission extrapolation (which we plan to continue using in the future). We took the suggestion to validate the extrapolation using the currently available 4.2FT2010.

Figure 1. (see supplement) Global methane emissions using EDGARv4.1 extrapolated after 2005 using BP and FAO statistics (black) compared with the use of EDGARv4.2FT2010 (red).

As can be seen in Figure 1 there are differences. Interestingly the EDGARv4.1 extrapolation shows a more prominent signature of the economic crisis in 2009. Note that differences are also seen between the overlapping years of EDGARv4.1 and EGARv4.2 (before 2005). The same is true for year-to-year updates of the FAO and BP statistics. All this reflects the inherent uncertainty in CH4 emission estimates. To plot differences by sector is complicated by the fact that the sectors in EDGARv4.1 and v4.2 are not exactly the same. They show larger differences, which partly cancel out in the global total – confirming that there was indeed some reshuffling between sectors.

P28129, L28-29: "It is not clear whether water vapor is the cause of the seasonal bias discussed here, or that it only happens to covary with a different underlying cause." I understand that the cause is uncertain. I would however think the cause is an area of research for those working on SCIAMACHY retrievals. Earlier in the text you write: "In practise, well quantified biases are usually directly corrected in the model or the measurements." Do I interpret things correctly if this in the future (if the cause is found) could be a so called well defined bias corrected in the measurements (?)

That is possible. However, the role of water vapour spectroscopy in the SCIAMACHY CH4 retrieval has already been investigated by Frankenberg et al (2008). The corrections proposed therein worked well for IMAPv5.0. The version that we use (IMAPv5.5) has been modified to account for the loss of critical detector pixels in 2005. An attempt was made to further correct IMAP5.5, which hasn't been very successful so far (some aspects improved, others not). It explains why we couldn't simply account for the problem in the retrieval.

P28130,L9: The TCCON network really plays a key role in this study. I miss a map

figure showing geographical coverage and a table with information on temporal coverage. What about retrieval errors, uncertainties and biases in these data. Though these things might be described in referred studies I suggest including such information here given the central role of the TCCON measurements.

The stations and coordinates are now included in figure 1. In addition we included the following information in the 'Measurements' subsection:

"The SCIAMACHY bias correction, described in detail in the next section, makes use of TCCON data from the GGG2012 release. Figure 1 shows the sites that were used. The estimated 1σ single measurement uncertainty for CH4, after correction for systematic errors using aircraft profile measurements, amounts to 3.5 ppb (Wunch et al. 2010, 2011)."

And in the "Bias correction" section:

The corresponding bias coefficients α are determined by linear regression to the SCIAMACHY-TCCON residuals, using TCCON measurements from the sites shown in Figure 1 for the years 2009 and 2010.

P28131,L18: I suggest to use other names than "flex" and "fix". They could be mixed and are hard to discern. Especially in figures with small text sizes.

We removed the subscripts from all occurrences of "fix" and "flex" to avoid this problem.

P28136,L25-26: I am a bit doubtful about the usefulness of combining aircraft measurements from different locations and times into a single profile and then comparing it with a model with coarse resolution. I miss some information on how this is done and what temporal resolution you have in the model output that is used for comparison.

The primary motivation for combining aircraft measurements was to reduce the number of figures. Some averaging of data is needed to filter out short-term variations that are not reproduced by the model. In the case of aircraft measurements, this directly implies averaging of data that differ in space and time. We are aware, however, that there may be a more significant loss of information when averaging between campaigns. The underlying analyses were done for the individual campaigns. And care was taken only to combine campaigns if the general characteristics of the comparisons were similar and preserved in the mean. The alternative is to pick out one specific example, in which case you start wondering about the other campaigns, or compile a stack of plots as supplementary information, which will probably not be used.

To clarify how the model was compared to the measurements we added the following sentence: "In all comparisons between model and measurements, the model was interpolated to the coordinates and times of single measurements."

P28139, L6-7: "This is not easily explained, as such a reduction would rather have been expected during the anomalous drought of 2005." This is a bit unclear since you in the paragraph above state that there is a reduction in observed total columns over the tropics in the end of 2005. Does this mean that the drought was earlier in 2005? Please clarify.

This part was indeed not clear enough. It has been replaced by: "In the inversion, this signal is attributed mostly to tropical South America, which shows a minimum in the inferred flux in early 2006. In 2005 the Amazon basin experienced the driest conditions in 40 yr. Most of the region experienced rainfall deficiencies starting in the wet season of late 2004 to early 2005, extending into the dry season until October 2005 when the rains returned (Marengo et al, 2008). Our inversion-derived flux anomaly is not easily explained by this climatological anomaly, since reduced emissions would have been expected during the first half of 2005, rather than the second half extending into 2006."

Discussion: The inversion set-up does not separate fluxes from different sectors. It would however be good somewhere in the discussion to elaborate a bit more on the wetland emissions. Wetland emission inventories have large spreads both in total emissions and spatial distributions. What are the strength,

weaknesses (you mention for instance missing year to year variation of wetland area) and uncertainties of the apriori LPJ-WhyMe emissions? How is LPJ-WhyMe different from other wetland emission inventories? Findings from other inverse studies are included in the discussion but could other type of studies (forward modeling, emissions inventory comparisons, LPJ studies) indicate support of a movement of LPJ/wetland emissions from the extratropics to the tropics?

The uncertainties of process modeling of natural wetlands have been discussed extensively in the recent literature (see for example the WETCHIMP intercomparison, Melton et al., BioGeosciences, 2013 and also the recent study by Ringeval et al, BioGeosciences Discussions, 2013). In the context of the 2007 growth rate anomaly there is the paper by Pison et al, ACP, 2013. It is clear that uncertainties are large, in particular for tropical wetlands. Our only additional constraint comes from atmospheric measurements, which is why we prefer to keep that focus.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28117, 2013.