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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.

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The original text of the reviewer has been pasted below in bold font. It is answered in roman font, including the modifications in the revised manuscript.

General comments

This manuscript examines biases in SCIAMACHY XCH4 retrievals and how these may best be dealt with in atmospheric inversions. Moreover the manuscript examines the sensitivity of optimized CH4 emissions from several inversions using different bias corrections. This is a sound study, which is well presented

and thorough in its analyses. I recommend publication, however, I do have a few minor comments and suggestions.

Thank you for your time and interest in our study and useful comments, which we tried to accommodate as much as possible.

Specific comments

Abstract, L20: The authors should specify which systematic errors these are as it is not clear from the context.

This was reformulated as follows: "systematic errors in the SCIAMACHY measurements"

P28126, L17: The authors should state approximately how big the correction to CH4 is above 50hPa.

Using ERA interim this correction is actually unimportant, because the age of air is much improved compared with earlier reanalyses. There is still a tendency to overestimate methane, which may be large in ppb in the upper model level (order of 100 ppb) but insignificant in mass. The correction method was still in place, which is why it is mentioned, but in our opinion there is no need to give it further attention.

P28127, L5-6: Is the total error in observation space for NOAA data calculated as the quadratic sum of the representation and measurement errors? Could the authors please specify.

The text mentions that all errors are assumed independent. This implies that they are summed up quadratic.

P28127, L22: How large is the retrieval uncertainty?

We added: This approach yields 1σ uncertainties in the range of 15 to 45 ppb.

P28127, L25-27: If the SCIAMACHY data are averaged over 3-hourly intervals at 4

by 6 degree, then there are no data at higher frequency/resolution so why do you need to account for correlation of retrievals within 3 hours and 4 by 6 degree?

We need a method to determine the uncertainty of the 3-hourly averaged retrievals. This sentence describes how to get from the uncertainty of single retrievals to that of the corresponding 3-hourly and 6x4 degree average.

Section 2.3: Although TCCON data are not used in the inversion, they are used to calculate the biases in the SCIAMACHY data, therefore, a short description of the TCCON data should be added to this section.

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: The authors should include a brief description of the "fix" inversions. In this test are the a priori values of parameters a2 and a3, respectively, set to one (as in Table 3)? This is not clear.

The Table 3, α2 and α3 are called 'normalized coefficients'. In the text we now refer to this and explain that they are scalar multipliers of the TCCON derived regression coefficients. In the example of the water vapor correction it is now mentioned that the TCCON derived 3.42 ppb/(g/kg) corresponds to α2=1.

P28134: The results shown in Fig. 5 are discussed only in terms of how well the bias correction performs compared with the NOAA-only inversions, however, it is conceivable that part of the NOAA-only versus SCIAMACHY difference may be due to the detection of a real signal by SCIAMACHY that is not detected in the NOAA surface data, which is particularly sparse in the tropics. Could the authors please comment on this?

We mentioned that: "These differences can be interpreted as the signal that is brought in by SCIAMACHY, in addition to what is provided by the surface network." We added the following sentence in the end to make it even clearer: "Note that the differences that remain after bias correction in Figure 5 include real signals of methane sources that are not represented by the NOAA-only inversion."

P28135, L9-13: Deviations from a Gaussian distribution centred on the origin may also result in the SCIAMACHY inversions if the TM5 model has errors in vertical mixing?

Yes, and therefore the 'flex' bias corrections probably correct for model biases in addition to left over measurement biases.

P28139, L2-8: The authors state that there was a reduction in the SCIAMACHY derived flux in 2005, and 2005 was a drought year. However, the authors state that this reduction is not easy to explain and discuss other possible reasons such as the instrument performance. The way this is presented doesn't quite follow. Perhaps the authors are referring to the flux minimum over South America in 2006?

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."

P28140, L14-15: From Fig. 13 there appears to be a small trend in NOAA-only derived fluxes, so likely there is some real trend in the flux. In the inversion SQGflex, the trend is weaker compared with NOAA-only and SQGfix because the latitudinal bias parameter is optimized for each of the 4-year interval inversions, hence this compensates for part of the trend. However, if the SQGflex was performed over the entire period, the trend should be similar to that in SQGfix?

The SQGfix has actually a stronger trend than NOAA-only and SQGflex. It is not easy to see, but for example in Figure 10, panel tropics, SQGflex and NOAA-only are rather well aligned. The stronger trend of SQGfix compared with SQGflex can be seen in the corresponding panel in Figure 12. The most logical explanation is that the trend of SQGflex has been corrected towards that of the surface measurements, through the optimization of the α1's of the subsequent inversion blocks. The differences between the optimized α1's is consistent with a reduced trend.

P28142, L27: Add the uncertainty estimate to the contribution from South East Asia

Changed to 9$\pm$13 TgCH4/yr.

Conclusions: "Comparisons with inversion results from Bergamaschi et al. (2013) show that the SCIAMACHY-derived inter-annual emission variations become less robust when the bias correction is extended with additional degrees of freedom on inter-annual timescales." It is not clear where this conclusion is supported in the main body of the manuscript. Could the authors please add additional information to support this conclusion?

This comes from the discussion of Figure 13, which shows comparisons of inversion-

derived inter-annual variabilities. Earlier, it was argued that the temporal variability of our bias corrected inversions was relatively robust. The comparisons with the SCIAMACHY inversions from B13, however, show that with a different setup using more parameters in the bias correction, the SCIAMACHY-derived inter-annual variability can look quite different.

Fig. 4: SQfix performs more poorly for Jan-May at the beginning of the timeseries compared to at the end of the timeseries, why?

I have no clue. There is inter-annual variation in both the TCCON and the SCIAMACHY data that does not necessarily match.

TECHNICAL COMMENTS

P28122, L7: "joined" should be "joint"

Done.

P28123, L6: "alternate" should be "alternating"

Done.

P28123, L6: suggest stating that the adjoint is the adjoint of the transport model TM5

Done.

P28123, L24: add "rate" after "inter-hemispheric mixing"

Done.

P28131, L18: revise use of "with" and "without" as this is confusing, perhaps just state that there are two sets: "flex" and "fix".

It has been changed into: The SCIAMACHY inversions consist of two sets: One with and the other without optimization of bias parameters (referred to as 'flex' and 'fix' respectively).

P28131, L18: not sure what the parentheses are supposed to signify after "flex" and "fix", perhaps these should be removed

This was some strange Latex formatting problem, which has been solved now.

P28140, L21: "factor of 5"

Done.

P28140, L22: "variation" should be "variations"

Done.

P28141, L29: ". . .parameters that are. . ."

Done.

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