

***Interactive comment on* “The impact of
SCIAMACHY near-infrared instrument calibration
on CH₄ and CO total columns” by
A. M. S. Gloudemans et al.**

Anonymous Referee #2

Received and published: 8 April 2005

Referee comments on manuscript acpd-2005-0045 Title: "The impact of SCIAMACHY near-infrared instrument calibration on CH₄ and CO total columns" Authors: Gloudemans et al.

General points:

The paper is one of the first publications on global measurements of methane and carbon monoxide columns using satellite nadir observations in the near-infrared spectral region. As pointed out by the authors near-infrared measurements have high potential to significantly enhance our understanding of the sources and sinks of these green-

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

house gases as near-infrared measurements are sensitive to trace gas concentration changes in the boundary layer (in contrast to thermal infrared sensors who have their sensitivity maximum in the middle to upper troposphere). Especially for methane the required accuracy and precision for such an application is demanding (its on the order of 1%) and even small errors cannot be neglected. The paper focuses on three important error sources related to the preliminary calibration of the SCIAMACHY spectra. As pointed out by the authors these error sources have not been discussed in sufficient detail in any of the previous papers (which are listed by the authors).

A good understanding of the issues discussed in the paper is important if the data are to be used to better constrain the sources and sinks of methane and carbon monoxide. Therefore, the paper covers an important topic. The paper is well written and I recommend publication. However, I also recommend that the authors should carefully take the following points into account:

Specific points:

Abstract:

The authors say that "the impact on the CH₄ total columns is more pronounced than for CO". A clarification would be helpful: Is this because the effects discussed in the paper result in larger (relative) errors for CH₄ compared to CO or simply because the requirements for CH₄ are much higher?

The authors say that the effect of a dead/bad pixel is "more a random effect". Obviously this is expected for a bad (= noisy) pixel but for a dead pixel (with no or a constant signal) one would expect a more systematic effect. This needs clarification.

Introduction:

End of page 2: Concerning the statement: "an accuracy of approx. 1-2% for CH₄ and approx. 10-20% for CO is required": Please add appropriate references. Do these values refer to accuracy (bias) or to precision (random error) or both types of errors?

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

This needs clarification.

Section 2 Instrument calibration:

Top of page 4:

The authors say that "Scattering of light in the ice layer gives rise to extended wings in the slit function... The latter has a significant effect on the retrieved columns.". In the following the authors say that this effect is difficult to measure. It has been measured using on-board SLS measurements but it is said that these measurements are not representative for the nadir measurements because the slit is only partially illuminated. Therefore, it is not clear where the statement that the nadir spectra are affected by a slit function with changing wings is coming from? Is this a speculation or are there clear indications for this which have been derived directly from analyzing the nadir spectra? If yes, these indications should be mentioned in the paper. (See also the comment given below related to Figure 7)

Section 3 Retrieval algorithm:

Middle of page 5:

As described by the authors the methane correction algorithm is based on adjusting the measurements over the Sahara to model simulations. This approach is not unproblematic! It basically assumes that the model is perfect which is probably not the case (if the model would be perfect, measurements would not be needed anymore). At least this assumption and its implications should be mentioned in the paper (I propose to mention this already in the Abstract because this is a severe limitation).

Bottom of page 5:

"Scattering ... is expected to introduce only small errors (Buchwitz and Burrows, 2004)". Because the requirements are high for methane, even small error matter. How small is "small"? A short quantitative statement concerning the results given in Buchwitz and Burrows, 2004, should be added.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Section 4 Retrieval results:

Beginning of Section 4.1 CH₄: How have the signal-to-noise ratios be determined? Have they been derived directly from the in-flight measurements or are they a result of a simulation (if yes, some infos on the estimated accuracy of these simulations should be added)?

Middle of Section 4.2 CO: There are many obvious differences between the MOPITT and the SCIAMACHY results shown in Figure 5. I recommend to change "good agreement" to "reasonable agreement".

End of Section 4.2 CO: Discussion of Figure 6: "It can be seen that there is good agreement between the two data sets for September 2003." This statement is true for the averaged data but not for the individual measurements as indicated by the standard deviation which is significantly larger for SCIAMACHY (typically 50 percent). Is there an explanation for the large standard deviation of the SCIAMACHY data (is this standard deviation consistent with the signal-to-noise etc. considerations)?

Section 5 Effects of instrument calibration on the retrieved total columns:

Beginning of Section 5.1 Effects of the growing ice layer: Discussion of Figure 7 (see also comment on Section 2 given above): It would be very interesting to see also the root-mean-square of the fit residuum (or something equivalent) in Figure 7 (e.g., as panel number d) for the case where no correction has been performed. One would expect that the quality of the fit gets worse with increasing time after the last decontamination. Has this been observed ? This would give the reader more confidence that the slit function (the one relevant for the nadir measurements) is in fact changing as described by the authors.

The correction algorithm is based on adjusting the methane columns over the Sahara to model columns. It is not discussed in the paper if this approach is also appropriate to get better columns outside the Sahara. The approach assumes that this is the

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

case but the paper gives no clear indications that this is in fact the case. At least this assumption should be mentioned in the paper. Panel a of Figure 7 shows in blue the corrected methane columns. They are pretty constant as expected but they have been constructed to be nearly constant (as constant as the model data used). Therefore Figure 7 is not an independent proof that the correction works. To show that the correction leads to a significant improvement a comparison (with the model) outside the Sahara should be shown.

It would be helpful for the reader to get a feeling on how large the correction of the dark signal is. This information should be added.

Caption of Figure 8: The authors write that "It is clearly seen that ...the CH₄ columns ... are up to approx. 4% too high on the Northern hemisphere". What can be seen is that the CH₄ columns are 4% higher over the Northern hemisphere compared to the other latitudes but as there is no comparison with an independent references one cannot conclude that the columns are 4% too high (relative to what?).

Stylistic issues:

No typos etc. have been identified.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1733, 2005.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)