Atmos. Chem. Phys. Discuss., 11, C2650–C2656, 2011
www.atmos-chem-phys-discuss.net/11/C2650/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Application of SCIAMACHY and MOPITT CO total column measurements to evaluate model results over biomass burning regions and Eastern China" *by* C. Liu et al.

C. Liu et al.

thomas.wagner@mpic.de

Received and published: 3 May 2011

The manuscript of Liu et al., ACPD, 2011, covers an interesting topic appropriate for ACP. However I share most of the concerns as detailed in the interactive comment of J. de Laat, from 1 March 2011. Detailed answers to the issues raised by de Laat have already been provided by corresponding author T. Wagner on 6 March 2011. Therefore, I do not repeat these issues here but list a number of additional items which

need to be considered by the authors before the manuscript can be published by ACP.

Many thanks for this positive assessment!

Introduction, various places: Please cite also: Tangborn, A., Stajner, I., Buchwitz, M., Khlystova, I., Pawson, S., Burrows, J., Hudman, R., and Nedelec, P., Assimilation of SCIAMACHY CO observations: Global and regional analysis of data impact, J. Geophys. Res., 114, D07307, 1-11, doi:10.1029/2008JD010781, 2009. Here SCIAMACHY CO retrievals have been used for model comparisons and to get information on emissions.

Author comment: Many thanks for this hint! We included this reference at two places in the introduction and in section 2.3.

Please site also the following publications where SCIAMACHY CO retrievals have been validated by comparison with ground-based observations: Dils, B., De Maziere, M., Blumenstock, T., Hase, F., Kramer, I., Mahieu, E., Demoulin, P., Duchatelet, P., Mellqvist, J., Strandberg, A., Buchwitz, M., Khlystova, I., Schneising, O., Velazco, V., Notholt, J., Sussmann, R., and Stremme, W., Validation of WFMDOAS v0.6 CO and v1.0 CH4 scientific products using European ground-based FTIR measurements, proceedings of the Third Workshop on the Atmospheric Chemistry Validation of ENVISAT (ACVE-3), 4-7 Dec. 2006, ESA/ESRIN, Frascati, Italy, ESA Publications Division Special Publication SP-642 (CD), 2006. Dils, B., M. De Maziere, J. F. Müller, T. Blumenstock, M. Buchwitz, R. de Beek, P. Demoulin, P. Duchatelet, H. Fast, C. Frankenberg, A. Gloudemans, D. Griffith, N. Jones, T. Kerzenmacher, I. Kramer, E. Mahieu, J. Mellqvist, R. L. Mittermeier, J. Notholt, C. P. Rinsland, H. Schrijver, D. Smale, A. Strandberg, A. G. Straume, W. Stremme, K. Strong, R. Sussmann, J. Taylor, M. van den Broek, V. Velazco, T. Wagner, T. Warneke, A. Wiacek, and S. Wood, Comparison between SCIAMACHY and ground-based FTIR data for total columns of CO, CH4, CO2, and N2O, Atmos. Chem. Phys., 6, 1953-1976, 2006.

Author comment: We added both references in section 3.

Introduction, page 1269, line 24 following: The authors say that previous studies "suffer from two general problems": Under (a) it is stated that "Many of the comparisons were qualitative" but under (b) it is shown that "some comparison studies were performed in a more qualitative way". Sorry, but (b) shows that (a) is hardly a general problem! Please add Tangborn et al and Dils et al to the papers cited under (b). In all these publications quantitative comparisons are shown! I strongly recommend to replace the statement "in a more quantitative way" by "in a quantitative way". As a consequence one of the two general problems "disappears". Under (b) the authors refer to biases of the SCIAMACHY CO retrievals of the other algorithms and "missing cloud correction". Under (a) they say that the effect of clouds "was not adequately corrected". If adequate or not, there is no "missing cloud correction". All previous algorithms deal with clouds one way or the other. For example, Buchwitz et al., 2007, correct for clouds using simultaneously retrieved methane.

Author comment: We rewrote this part in order to avoid wrong interpretations. We now state: a) For many of the comparisons no exact quantitative agreement could be expected (e.g., Buchwitz et al., 2004; Gloudemans et al., 2005; Straume et al., 2005; Buchwitz et al., 2006a; Buchwitz et al., 2007), because they were affected by the different spatio-temporal sampling of SCIAMACHY and other satellite observations: Besides different height sensitivities of near-IR and thermal IR sensors often also not strictly collocated observations were compared. For example, even night-time observations of the thermal IR sensors were included in the comparison, whereas SCIAMACHY and other VIS instruments only make daytime observations. Also, the effect of clouds on the SCIAMACHY observations was considered in different ways: often just observations above a certain cloud fraction threshold were skipped. Nevertheless, in general a good agreement of the observed patterns was found. b) Some comparison studies were performed in a quantitative way (e.g., Turquety et al., 2008; Tangborn et al., 2009; de Laat et al., 2010; Kopacz et al., 2010). Only collocated observations were used for these comparisons and the different height-dependencies of near IR and thermal IR sensors were adequately considered (e.g., using model simulations as transfer tools).

In particular Turquety et al. (2008) retrieved a boundary layer partial CO VCD from the comparison of SCIAMACHY and MOPITT observations, and related these partial CO VCDs to the respective quantity from the model simulations. However, in these comparisons often inconsistencies between the satellite sensors and/or model results were found, limiting the quantitative interpretation of the retrieved results. Besides possible errors of the MOPITT observations and/or the model simulations, these biases can be related to SCIAMACHY CO retrievals.

We also added the Tangborn et al. (2009) reference here. We did not add the Dils et al. reference here (but in section 3, see above), because it describes a comparison between satellite and ground based observations (instead comparisons between satellite and model data).

It is interesting to investigate if this is adequate or not, but the authors merely present statement rather than detailed comparisons and analysis where they show that their approach is in fact superior. Instead they argue rather "qualitative". For example, they draw a red circle around the Sichuan province region in their Fig. 7 and write in the caption that "other SCIAMACHY algorithms (Buchwitz et al., 2007) show much smaller column densities, which is probably related to the effect of clouds". Sorry, but this is not acceptable. Neither the Fig. of Buchwitz et al., 2007, nor the spatial averages shown in Liu et al., are "true multi-year averages" which can directly be compared to drawn such conclusions as other aspects also play an important role such as the spatio-temporal sampling. Either provide a detailed comparison with the Buchwitz et al., 2007, data set, or remove this guess. In addition: If statements are made which of two or more algorithms gives better results, as in the Sichuan case, please do so only if you can provide a clear proof. Only higher values near a source region are no prove as the algorithm giving lower values may be better one !

Author comment: We still think that there is good evidence from the results presented in Fig. 6 that the high SCIAMACHY CO VCDs in our data set at Chongqing (compared to other studies) are related to the cloud correction. Nevertheless, we agree with the

reviewer that also other reasons might contribute. We therefore removed the statement in the revised version.

Section 2.3, page 1273, line 20: Please cite Tangborn et al and Dils et al (see above).

Author comment: Both references were added in section 2.3

Section 2.3, page 1274, line 23: I think "weaker" needs to be replaced by "stronger".

Author comment: Many thanks for this hint. Corrected.

Section 2.3, page 1275, line 4 and following: As shown in Gloudemans et al., 2005, and discussed in Buchwitz et al., 2007, there is in fact a problem related to the instrument slit function, but mainly concerning its shape (wing versus center) but not concering its width. It is therefore not clear for me if the discussed determination and consideration of a time dependent slit function width improves the CO retrieval or not. Please show that the implemented approach results in an improvement (not only in a change).

Author comment: In the PhD thesis of Liu Cheng it is shown that also the width of the slit function changed (between about 0.2 to 0.3nm FWHM around the CO absorptions). These changes were determined from the stronger absorptions of CH4 and H2O. However, applying the changing slit functions to the CO analysis lead only to small improvements. We changed the statement in our paper from '. . .and broadening of the instrument slit function' to '. . .and change of the instrument slit function'

Section 4, page 1283, line 17 following: The authors state that "The importance of a proper cloud correction is illustrated in ...". What is shown is that there is a change (sensitivity) but no proof is given that the approach really results in more accurate retrievals. Perhaps this can be addressed with the additional information on validation the authors intend to include as can be concluded from the authors answers to the comments of de Laat.

Author comment: We agree that additional evidence should be included to support the need and usefulness of the cloud correction. Therefore we added a) a sensitivity study

showing the dependence of non-cloud corrected CO VCDs on cloud top height (new Fig. A1). It is clearly shown that clouds have a systematic shielding effect even for the small cloud fractions used (<20%).

b) detailed validation results using ground based FTIR measurements at different latitudes (new Fig. 7). FTIR data are compared to different stages of the CO retrieval: to uncorrected data, after the normalisation with MOPITT, and after additional cloud correction. It is shown that in most cases the cloud correction improves the agreement between the satellite observations and ground based data.

Conclusions, page 1294, line 26: See comment given above on "proper cloud correction".

Author comment: To avoid misunderstandings we replaced the text by' To our knowledge, no explicit cloud correction has so far been applied to SCIAMACHY CO observations. An implicit cloud correction based on simultaneously retrieved CH4 absorptions was, however, applied by Buchwitz et al. (2007). According to our study, omission of a cloud correction can lead to systematic errors up to >100%, especially over polluted regions. Unfortunately, no ground based FTIR stations at largely polluted sites are available for validation. Thus, such large cloud effects could not be demonstrated by comparison with FTIR observations. Nevertheless, comparison of the our SCIAMACHY CO data set with several ground based stations shows in most cases improved agreement after the cloud correction was applied.'

Conclusions, page 1295, line 8: The averaging kernels do not support the conclusion that SCIAMACHY is "especially sensitive ... to ... the surface". Instead sensitivity is nearly equal for all altitude layers.

Author comment: We replaced the text by 'Compared to MOPITT observations, SCIAMACHY observations are more sensitive to the atmospheric layers directly above the surface.'

Fig. 1: Please comment on the large drop of the uncorrected SCIAMACHY CO after 2005.

Author comment: We don't exactly know what causes the change in 2005 (the change seems to happen in the summer of 2005 and not at the beginning). However, from our analysis of the SCIAMACHY slit width (PhD thesis Liu Cheng) we found that in summer 2005 the slit width increased systematically from about 0.24nm FWHM to about 0.30nm FWHM. At least part of the drop of the CO VCD might be related to these changes. We added the following text to the figure caption: 'The jumps of the uncorrected data in 2003 and 2004 are related to several ice decontamination phases. The reason for the drop in summer 2005 is not completely clear, but might be related to a broadening of the effective slit function (Liu, 2010).'

Fig. 7, caption: As already noted above: Remove speculation about other algorithms or provide a detailed analysis.

Author comment: We removed the statement about the potential cloud effect.

Fig. 8, caption: 3rd line. I think "right column" needs to be replaced by "middle column".

Author comment: Corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1265, 2011.