Atmos. Chem. Phys. Discuss., 6, S393–S400, 2006
www.atmos-chem-phys.org/acpd/6/S393/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric carbon gases retrieved from SCIAMACHY by WFM-DOAS: improved global CO and CH_4 and initial verification of CO_2 over Park Falls (46° N, 90° W)" *by* R. de Beek et al.

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Received and published: 30 March 2006

Authors answers to Anonymous Referee #1 on paper de Beek et al., Atmospheric carbon gases retrieved from SCIAMACHY by WFM-DOAS: Improved global CO and CH_4 and initial verification of CO_2 over Park Falls (46°N, 90°W), Atmos. Chem. Phys. Discuss., 6, 363–399, 2006

First of all we would like to thank the referee for the constructive comments on our paper. Each comment will be carefully considered for the revised version of the paper.

Below we give answers to each of the comments made by the referee.

Answers to "General comments":

Progress CO compared with previous work

The referee criticizes that it is not clear from the results shown by how much the CO has been improved. We think that we have clearly shown that the CO has improved on average by about a factor two because the initial (previous) version 0.4 product (Buchwitz et al., 2004) was scaled with a constant factor of 0.5 but the new version 0.5 product not, i.e. the v0.5 product is not scaled. The version 0.4 product was scaled with the constant factor of 0.5 to compensate for a systematic overestimation (e.g. relative to MOPITT) by about a factor of two. This clear systematic and large overestimation is not present any more in our new version 0.5 product which is retrieved from a different spectral fitting window using essentially the same spectral fitting algorithm as used for version 0.4. The referee wonders what new insights have been gained ? We think that the following can be learned from this: First of all that it is very important to carefully select the fitting window by analyzing real data (with simulations a change by a factor of two simply by shifting the fitting window is not possible except perhaps under very special circumstances). Second, that the version 0.4 scaling issue was not caused by the retrieval algorithm because the same algorithm applied to a different spectral region produces reasonable CO columns without scaling. There is no need any more to apply a scaling factor because there is no obvious large and systematic CO overestimation of the version 0.5 product (at least not a factor of two compared to MOPITT as observed for the version 0.4 product). We consider this a clear and major improvement of the version 0.5 product compared to the version 0.4 product which was heavily criticized because of the scaling factor (see ACPD on-line discussion of Buchwitz et al., 2004). The referee states that agreement with MOPITT within mostly

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30% has been reported for both (the version 0.4 and 0.5) products - therefore it is not clear what has been improved. It is right that both products agree with MOPITT mostly within 30% but this is the level of agreement with MOPITT for the scaled (!) version 0.4 product but for the unscaled (!) version 0.5 product. So even if the overall agreement with MOPITT is similar for the two versions this demonstrates a major improvement. In addition to this we have implemented a correction based on simultaneously retrieved methane to reduce (e.g., ice layer induced) errors common to both gases. This important additional aspect of the improvement will be discussed in more detail below. The referee is right that the comparison with MOPITT should be more quantitative. We will revise the paper in this respect and add a more detailed quantitative comparison with MOPITT. In addition we will add an analysis especially for South America where the disagreement with MOPITT is large.

Difference between methane and Frankenberg's results

The referee is right that our results basically confirm the findings of Frankenberg et al., 2005. However, we do not use the same method as Frankenberg et al., 2005, because we use a different retrieval method (WFM-DOAS versus IMAP-DOAS). We use however the same spectral regions and also normalize the methane columns by CO_2. In view of the importance of the findings of Frankenberg et al., 2005, we think that it is quite interesting to show that the main findings of Frankenberg et al., 2005, can be confirmed using a different retrieval method (different radiative transfer forward model and inversion procedure) and a different model for comparison. In addition we present a larger data set including first results on seasonal variations. The referee is right that the methane bias issue needs additional clarification. For the revised version of the paper we will add more details concerning the solar zenith angle dependent bias problem and its first order correction.

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Park Falls FTIR validation

As pointed out by the referee, the CO_2 comparison over Park Falls raises many questions related to SCIAMACHY and TM3 CO_2 and the method of comparison. To address these interesting and important questions a significant amount of additional study is needed. We will substantially revise the CO_2 parts of the paper. Concerning SCIAMACHY CO_2 retrieval improvements we will focus on one important aspect, namely a discussion of the CO_2 scaling factor issue (see also next item). We will limit the CO_2 part of the paper to this important aspect. A comprehensive SCIAMACHY / model / FTS CO_2 comparison taking into account all comments of the referee is out of the scope of this paper. We will aim at covering this interesting topic in a separate future publication.

CO_2 scaling

We will substantially revise the discussion of the CO_2 scaling by adding more details concerning the improved calibration and how this influences the CO_2 retrieval (see also previous item). We will replace Figure 8 by a figure showing separately the year 2003 (showing old and new XCO2; old CO_2 refers to CO_2 derived from version 4.0x spectra whereas new CO_2 is retrieved from version 5.04 spectra having improved calibration) and the years 2004/2005 (showing the new XCO2). The referee raises the valid question if the SCIAMACHY-model comparison shown in Figure 8 is consistent with previous comparisons as the year 2003 XCO2 is not scaled although it was scaled in previous comparisons. Initially we have produced a figure similar to Figure 8 but with scaled year 2003 XCO2 (for the paper we later have removed this scaling because the year 2004/2005 is also without scaling). The (not shown) figure with scaled year 2003 XCO2 showed some differences to the published Figure 8 (as expected typically larger absolute values of the daily year 2003 XCO2 anomaly values, e.g., at maximum around

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day 100 7 ppmv instead of 5 ppmv; somewhat more sinusoidal seasonal cycle; maximum of time averaged amplitude at +8.0 ppmv instead of +7.5 ppmv; minimum of time averaged amplitude at -5.5 ppmv instead of -6.0 ppmv) but nearly exactly the same peak-to-peak variability (13.5 ppmv) of the average of the years 2003-2005. The main reason for this is that the time averaged seasonal cycle for the years 2003-2005 shown in Figure 8 is only marginally influenced by year 2003 data. Although Figure 8 allows to distinguish the results for the different years due to different colors we now think that it is better if the CO_2 for the different years (produced with spectra with different quality of the calibration) are not combined in a single figure but shown separately. Therefore, for the revised version of the paper, we will show the CO_2 for the different years separately.

Answers to "Specific comments":

Page 366, line 10

What we meant is that SCIAMACHY is not a "CO_2 (or CH_4) only" mission (as OCO) and that SCIAMACHY has not been specified to measure CO_2 and CH_4 accurate enough to provide quantitative information on CO_2 and CH_4 surface sources and sinks. This will be clarified in the revised version of the paper.

Page 369, line 15

The notation XCO2 and XCH4 has been introduced to point out that these are (column averaged) mixing ratios (in ppmv and ppbv, respectively), not absolute columns (in molecules per cm^2). For CO the notation XCO is not used because the CO product is the absolute column in molecules per cm^2 and not a mixing ratio. CO is ratioed with simultaneously retrieved methane not to compute a (column averaged) mixing ratio but

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to reduce errors which are common to both gases. We will clarify this in the revised version of the paper. We will provide more details to what extend errors cancel when the CO to methane ratio is computed.

Page 370, line 3

For the revised version of the paper we will add a detailed quantitative comparison with MOPITT. The SCIAMACHY WFM-DOAS version 0.5 CO column year 2003 data set has recently been compared with a network of FTS ground stations (submitted revised version of Dils et al., ACPD (Special Issue Geophysical Validation of SCIAMACHY), 2005). The additional information on the quality of the SCIAMACHY CO columns and a short discussion of the main findings of the FTS comparison will be added to the revised version of our paper.

Our approach to correct the CO by normalizing with simultaneously measured methane works best if the profiles of CO and methane are similar but will only provide a rough correction if the profiles are very different. For a case where CO is significantly enhanced in the boundary layer but methane not the CO correction will push the CO in the right direction (to higher CO) but the corrected CO will be underestimated. Therefore this approach is only used for pixels with quite small cloud amount as determined by the deviation of the retrieved methane column from the a priori column for a cloud free scene. This approach significantly enhances the number of pixels compared to strict cloud filtering. We agree that for the reasons given above the method is not perfect and that more studies are needed to find out how the measurement error depends on the cloud situation.

Page 375, line 10-26

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For the revised version of the paper we will add a more detailed quantitative comparison with the methane model. We agree that giving precision/accuracy estimates based on two orbits is not appropriate. We will change this for the revised version of the paper. The SCIAMACHY WFM-DOAS version 0.5 XCH4 year 2003 data set has recently been compared with a network of FTS ground stations (submitted revised version of Dils et al., ACPD (Special Issue Geophysical Validation of SCIAMACHY), 2005). Comparisons have been done with and without bias correction. The additional information on the quality of the SCIAMACHY XCH4 and a short discussion of the main findings of the FTS comparison will be added to the revised version of our paper.

Page 376, line 13-27

For the revised version of the paper we will add more details concerning the solar zenith angle dependent bias (see also comment given above) and will replace the methane maps where low values have simply been filter out by maps generated using a more appropriate procedure.

Answers to "Technical corrections":

Page 372, line 28

Should be 2004/2005. Will be corrected.

Page 368, line 4

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Will be improved.

Page 368, line 12

Will be corrected.

Page 369, line 14

Will be corrected.