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# *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 ($46^\circ$ N, $90^\circ$ W)" *by* R. de Beek et al.

**Anonymous Referee #1**

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This manuscript presents results of SCIAMACHY retrieval of CH4, CO and CO2 using a new version of the WFM-DOAS retrieval algorithm. It is claimed that significant progress has been made. However, it is not quite clear what is learned here that is not already covered by previous publications. The few points that seem to make an original new contribution are either not worked out in detail because it is considered outside the scope of the publication, or not supported by measurements that are available. Therefore, as explained in more detail below, I can only conclude that to make this work acceptable for publication it should go beyond a superficial and mostly qualitative description as presented here and provide an in depth analysis of some of the issues that are raised.

GENERAL COMMENTS

Progress CO compared with previous work

It is claimed that a major improvement has been achieved over previous work, justifying the publication of updated retrieval results here. From the results, however, it is not quite clear by how much the retrieval has actually improved, nor what new insights have been obtained from this improvement. For example, for CO it is reported that results are within 30% of MOPITT, with differences upto 80%, while in Buchwitz, 2005b it was already concluded that the difference with MOPITT is mostly within 30%. The seasonality over Africa does seem to have improved somewhat in comparison with MOPITT (except for Apr-June), although it remains unclear why the situation is so much different for South America. The authors mention that for several reasons perfect agreement is not to be expected, because of neglected differences in altitude sensitivity and spatial/temporal sampling. For a highly variable compound such as CO a consistent spatial and temporal sampling should in my view be considered a prerequisite. Also the difference in vertical sampling may well lead to substantial differences. In light of these limitations it remains unclear what the significance is of the apparent improvement over Africa. Here one cannot claim significant improvements on the basis of a qualitative discription of the results. In comparison with Buchwitz, 2005b no attempt has been made to really improve this situation by making the analysis more quantitative. If it is claimed that the CO product has improved significantly then this should be motivated better.

Difference between methane and Frankenberg's results

For CH4 a clear improvement has been obtained in comparison with Buchwitz, 2005b, however, this seems to be explained entirely by the fact that the method by Frankenberg et al., 2005 has been adopted. Therefore, the results presented here seem to basically confirm findings that have already been published. A worthwhile attempt is made to quantify the performance of the ch. 6 $CH_4$ retrieval. However, the outcome again, basically confirms the performance estimate by Frankenberg et al. (although this is not explicitly mentioned here). New is a solar zenith angle dependent bias, which might explain some of the remaining discrepancies between model and measurements. However, it is mentioned that 'for this study, however, no correction has been applied as this issue needs further study'. This raises the question what is the new aspect that has been studied here. This should be clarified.

Park Falls FTIR validation

Results are shown that highlight the difference between SCIAMACHY and TM3 over Park Falls. It is concluded that FTIR measurements favor SCIAMACHY and that TM3 grossly underestimates seasonal variation. Firstly, it is difficult to judge this statement without the measurements. Therefore it is in fact premature to publish this comparison. Besides this, some basic checks are lacking that allow verification of what is going wrong in the model. For example, how well is the model doing in comparison with surface measurements (e.g. at Park Falls and at other North American stations). This would give an indication how exceptional Park Falls may or may not be. The same reference that is given here for the Washenfelder data also shows a fair agreement with the model results obtained by Olsen and Randerson. This asks for an explanation why TM3 is doing so much worse. In connection with this, it remains unclear what vertical weighting functions have been applied, and how they compare to the vertical weighting function of the FTIR. It is mentioned that 'We hope that this issue can be clarified in the near future by a detailed comparison with $XCO_2$ ground-based FTS measurements at Park Falls and other ground stations'. In my view, however, this important point should really be further clarified here (or be left out of the paper, because the data are not available).

$CO_2$ scaling

The discussion on $CO_2$ scaling is rather confusing. On the bottom of page 371 it is mentioned that 2004/2005 data need not be scaled anymore because of an improved calibration. On page 379, line 23, however it is mentioned that no scaling is applied to 2003 either. Still in Figure 8 the seasonal amplitudes of 2003, 2004, and 2005 are quite similar. This suggests that improved 'data patching' for 2003 has improved the results, making the scaling factor redundant. Then, however, the question remains if the data product presented in figure 8 can still be compared with the model - measurement inconsistency that were reported in Buchwitz 2005a,b. Furthermore, it is unlcear what has caused a substantial improvement in the calibration of channel 6, since no ice layer correction is needed for this channel, meanwhile we are talking here of a 50% effect. These issues should be clarified.

SPECIFIC COMMENTS

Page 366, line 10: 'SCIAMACHY is not a dedicated $CO_2$ and/or $CH_4$ mission ...' SCIAMACHY may not be a dedicated $CO_2$ missions, but it did aim specifically at measuring $CH_4$.

Page 369, line 15: In addition, a correction ... and albedo variability' The approach seems equivalent to taking the ratio of CO over $CH_4$. For $CH_4$ and $CO_2$ the notation $XCH_4$ and $XCO_2$ is introduced, but not for CO. This suggests that CO is treated differently, which, however, doesn't seem to be the case. This should be clarified. A priori, it is not obvious that the ice layer affects $CH_4$ and CO in a similar manner. To what extend do ice-layer induced errors cancel out by taking the ratio? This issue should be addressed.

Page 370, line 3: 'The methane correction factor has to be close to 1.0 (within 20%) ...' If I'm correct this means that a pixel with 20% cloudiness might still pass the cloud filter. By comparing the results in Buchwitz, 2005b with those presented here it becomes clear that this results in a better coverage over the tropics and the oceans. In these regions, however, the discrepancy with MOPITT is largest, which raises the question how valid the assumption is that the ratio of CO and CH4 can account for the effects of partial cloudiness. In this regard it is worth mentioning that the vertical profiles of CH4 and CO may be quite different. It should be demonstrated that this approach works.

Page 375, line 10 - 26. On the basis of the numbers presented here upper limits are derived for measurement precision and accuracy. One might argue why standard deviations of model and measurements are compared. In my view it would have been better to determine the root mean square difference (standard deviations do not represent correlation). What is more disturbing, however, is that the measurements overestimate the model predictions here, while the first sentence on the next page mentions that SCIAMACHY is generally lower than the model. It is already difficult to defend a precision and accuracy estimate on the basis of only two orbits. If it is subsequently demonstrated that these orbits are not representative of the whole dataset, however, this disqualifies the performance estimate.

Page 376, line 13 - 27 What is called ‘a small calibration offset’ here, is in reality a seasonally varying error of a few percent, which for CH4 is highly substantial because its seasonal cycle is roughly of the same order of magnitude. Somehow this error doesn not show up in the comparison of uncorrected SCIAMACHY - TM3, because it only mentions a constant low bias of SCIAMACHY (which raises the question what data have been left out)

TECHNICAL CORRECTIONS

Page 372, line 28: ‘Therefore, the year 2005/2005 results ... CO2 columns’ Presumably 2004/2005 is meant here.

Page 368, line 4: controlled or limited instead of ‘minimized’

Page 368, line 12: ‘has $>>$ to $<<$ be very accurate’

Page 369, line 14: ‘by a’ instead of ‘with the’



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