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Interactive comment on "The impact of SCIAMACHY near-infrared instrument calibration on CH_4 and CO total columns" *by* A. M. S. Gloudemans et al.

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General comments

The paper by A.M.S. Gloudemans et al. describes CO and CH_4 total column retrievals from the SCIAMACHY near–infrared channels. Important calibration issues are highlighted and the way they can be tackled is described. CO and CH_4 distributions over land are presented and compared to MOPITT data for CO and to model distributions for CH_4. The paper addresses questions of general interest for the ACP readers, and presents important results in the frame of atmospheric remote–sensing. The paper is

also well written and well structured, and I therefore recommend that it be published in ACP. Below are some specific comments and suggestions for improvements.

Globally, after several papers on the same topic, the results presented here make a further demonstration that the retrievals of trace gases from the near–infrared reflected/backscattered solar radiation provide useful information for atmospheric chemistry and climate applications. The reasonable agreement between the SCIAMACHY CH_4 and CO products with independent data sets (models or other satellite–based measurements) opens promising perspectives for future scientific studies as well as for instrumental developments. What remains somewhat unclear to me, even after this thorough study and although some clues are given, is whether the NIR measurements will enable reaching the target accuracies in the CH_4 and CO retrievals (1–2 % and 10–20 %, respectively –see page 1736–), which is needed in order to determine sources and sinks of methane and CO emission estimates. In a further study, a global error budget, dealing with both instrumental and non–instrumental error sources (impact of albedo, scattering but more importantly water vapor, temperature –and even methane for the CO retrievals–), would help.

Specific comments

Page 1738, line 11: "...leads to total columns that are to high...". Referring to Figure 8, which comes later in the manuscript, this is the case for CH_4, but it does not seem to be straightforward for CO. Maybe consider revising this sentence accordingly?

Page 1740, line 9: In the retrievals, a single temperature profile is used. What is the temperature profile and how has it been chosen? An estimate of the error introduced by not using the adequate temperature profile is given for CH_4 (about 2 %). Is this a global mean value? How large are the errors in more extreme situations, where the actual temperature profile differ strongly from the one used by default? And what are the expectations for CO? To my opinion, these error estimates are important in the discussion of the results. Indeed, if the errors introduced by uncertainty on the

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temperature are significantly smaller that the instrumental calibration problems investigated (as stated page 1740), they probably remain substantial, when comparing the instrument–corrected distributions to the model TM3 or to the MOPITT data. Reference to this assumption should be made somewhere in the discussion section.

Page 1740, line 24: The authors report on a qualitative agreement with other data products, obtained using the same dataset but different retrieval algorithms. Wouldn't it be possible at this point to give a quantitative agreement?

Page 1741, line 23: The retrieval results for CH_4 and CO are compared to ground–based FTIR measurements, and reference is made to the paper by Dils et al. (submitted to ACP, 2005). This paper is not yet published and I therefore suggest that the authors report values of the validation exercise in the text, so the reader can better judge on the quality of the satellite–derived columns.

Figure 3 vs Figure 5: Although I understand that the authors have filtered out data for which the instrument related errors are larger than a given threshold (and that the latter is –relatively speaking– very different for CH_4 than for CO), I am surprised that the distributions obtained for the two data products differ strongly at some locations. In particular, there are no CH_4 measurements in several regions where there is strong CO (e.g. vegetation burning events in South America and Africa, polluted region above China). Referring to page 1742, part of the lack of data for CH_4 (e.g. in the Amazon basin) appears to be due to the presence of clouds. But in that case, why are there CO data in these regions? There are also more CO measurements above oceans, where the CH_4 measurements have been rejected because of a too low surface albedo. Obviously the errors on the CO column will also be very high in these regions. Accordingly, is the error threshold for CO not too loose?

Page 1743–1745: The authors report on a good agreement with the MOPITT data for November 2003. In light of Figure 5, this appears to be overstated, as even some large–scale features are not reproduced. A "reasonable agreement" may be more

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adapted. The comparison between the retrieved CO columns from SCIAMACHY and the MOPITT data is interesting, especially in that SCIAMACHY measurements provide, on the contrary to MOPITT, high CO levels in regions such as India or the East–Coast of the USA, where one expect indeed the emissions to be fairly important. As explained on page 1744, a more detailed comparison between the two datasets, which would account for the respective sensitivity of the instruments through the averaging kernels, would be an asset for a future study.

Figures 4 and 6: Minor point: Measurements are given only from 40 degrees South, whereas, at least for the month of November, it seems that measurements are possible at higher latitudes for both CO and CH_4 (Figures 3 and 5). Is there a reason for this cut–off in September? From the error bars in Figure 6, MOPITT CO measurements appear to be more uniform than SCIAMACHY data. Is it due to the fact that SCIAMACHY is more sensitive to the surface, where one can expect to observe larger CO variations, or does that simply reflect a better precision of the MOPITT data?

Figure 7 and related text (pages 1745–1746): It is not clear whether the authors want to point to a different behavior of the CO and CH_4 retrievals as a function of the broadening of the slit function, or not. Part of my misunderstanding may simply be related to Figure 7, where the relative differences in the total column are shown for CO but not for CH_4. I would suggest that the authors add the CH_4 data in Figure 7c, so the reader can appreciate how the effect varies from one species to the other.

Technical corrections

Page 1735, lines4–6: The two sentences are misleading. The authors state, with reason, that MOPITT has been unsuccessful to retrieve accurate CH_4 columns but their first sentence say that global distributions have been measured.

Page 1759: One "2005" too much in the reference Krijger at al.

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