

Interactive comment on “Atmospheric carbon gases retrieved from SCIAMACHY by WFM-DOAS: improved global CO and CH₄ and initial verification of CO₂ over Park Falls (46° N, 90° W)” by R. de Beek et al.

Anonymous Referee #2

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General comments

The paper by de Beek et al. describes the advances made in the retrieval of CO, CH₄ and CO₂ by means of WFM-DOAS using SCIAMACHY spectra. Especially for CO and CH₄ major improvements seem to have been achieved. However, once the authors start to describe interesting results (such as CO over industrialised regions

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in China), they don't further the analysis but refer to future work that still has to be performed. In that sense, the paper resembles a status report of the current retrieval version. Instead of describing the results of all three gases in a premature way, it would be better if the the authors elaborated some specific aspects in more detail (e.g. a more quantitative SCIAMACHY-MOPITT comparison). At the current stage, too many issues are left open for future studies but should be (at least some of them) elaborated in this paper before publication in ACP. Further, some aspects (like the oversimplified methane accuracy/precision estimates) should be refined.

Specific and technical comments are given in the following:

Specific comments

Abstract, line 15: How come that SCIAMACHY CO agrees within 10% if the standard deviation is 30%?. Taking the globally averaged SCIA-MOPITT difference doesn't seem to be a good measure for the accuracy but this statement would suggest the accuracy to be 10%

page 366, lines 17-19 I don't fully understand what the authors want to say by mentioning that the OCO team is also working on SCIAMACHY retrievals.

page 369, line 13 Why exactly did the CO scaling factor disappear? Was it just the fitting window that changed?

page 369, ice correction How can you be sure that the ice layer affects CO in the same way as it does for methane?

page 371, line 26 The authors state that the new version of SCIAMACHY data have an improved calibration. What exactly has improved and how come that this improvement changed the CO₂ columns by almost 30%? (if I understood it right, the 1.27 factor is

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not necessary any more). line 28: year 2005/2005?

page 372, PMD 1 cloud detection Why don't you make use of other freely available cloud products such as FRESCO?

page 375, lines 8-9 "The TM5 simulations are currently being refined". What exactly do you want to say by this?

page 375, lines 18-25 I do not understand how the authors can derive their precision and accuracy estimated from this simple comparison with the model. This seems to be a far too simple method of quantifying the SCIAMACHY retrieval quality.

page 376, line 19 How did you derive the BIAS correction, i.e. how did you optimise your results with the expectations? What information did you use to derive this correction? Again, it is said that this issue needs further studies. This could be also done in this paper in a more quantitative way. Otherwise, new future corrections would make this paper obsolete.

page 377, lines 1-3 How can you conclude that you actually see emissions from fossil fuel, wetlands etc. Please give a reference.

page 378, description of SCIA processing, lines 23-29 I am not sure whether a detailed explanation of data processing is of interest for the general reader.

page 379, line 11 The latest HITRAN edition contains substantial updates, also in the spectral regions considered in this study. Did you already try to use the latest version?

page 380, lines 11-13 Again, the section on CO₂ ends with a statement that the results presented here cannot yet be fully understood but warrant further investigations.

page 380, line 23 The appearance of industrial regions in CO maps is really interesting and one of the major advantages of near infrared retrievals. However, there is only one quantitative sentence describing it and then the authors refer to future work. This study could be further elaborated.

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page 381, lines 27– Since the CO₂ results are indeed hard to interpret, they could as well be skipped. If I understand the colors in Fig. 11 right, the peak to peak amplitudes in northern parts are (at least) on the order of 345-375ppm (i.e. 30ppm amplitude) which seems to be far higher than in Park Falls. Could you comment on this? Instead of showing this plot over Asia, it would be more worthwhile to show a similar timeseries as in the case of Park Falls. While I understand showing CO and CH₄ with focus on Asia, I don't understand why the authors also include CO₂ for this area-specific analysis.

Fig.1 You stated that SCIAMACHY is, on average, 10% higher than MOPPIT. However, for most regions but the tropics SCIA seems to be lower. Is this bias dominated by the 80% offset in South America? Further, in Fig. 1c the colorscale hardly allows any discrimination in the 10-40% range (all in green). However, discrepancies on this order would be very interesting!

Fig.6 Why are there some block-like patterns in the TM5 model simulations? It should be clearly stated that the colorbars are not always the same (esp. the middle figures have a different SCIAMACHY and TM5 colorscale!).

technical corrections

page 381, line 24 replace 'were' by 'where'

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 363, 2006.

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