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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global carbon monoxide as retrieved from SCIAMACHY by WFM-DOAS" *by* M. Buchwitz et al.

M. Buchwitz et al.

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Authors answer to interactive comment S1199 from C. Clerbaux (Referee) on "Global carbon monoxide as retrieved from SCIAMACHY by WFM-DOAS" (M. Buchwitz et al., 2004).

First of all we (the authors) would like to thank the referee (C. Clerbaux) for carefully reviewing our manuscript and for providing useful suggestions for improving the paper. Below we give detailed answeres to all referee comments. All suggestions for improving the paper will be considered as good as we can for the revised version of the paper (see details given below).

Answers to "General comments"

In the referee's comments it is stated that the first part of the paper (algorithm description etc.) contains interesting information about the current status of the SCIA-

MACHY spectra and the problems encountered in deriving useful information about atmospheric CO from them but that the second part (Section 6.3) - where the SCIAMACHY data have been compared with MOPITT - needs substantial improvement.

We entirely agree with this and will do our best to especially improve the second part of the paper (e.g., by adding a detailed description and discussion (as quantitative as possible) of all relevant figures and by summarizing the major results from the quantitiative comparison of SCIAMACHY with MOPITT in a table (as suggested by the second referee in its technical comments)).

The referee states that "the existence of at least two different retrieval algorithms developed by separate teams should help to provide improved CO NIR measurements products, ...". There are in fact other groups working on CO retrieval from SCIAMACHY nadir measurements using algorithms different from WFM-DOAS. The manuscript does not contain any reference to the work done by these groups because no publications were available in the open literature at the point in time when the paper has been submitted. After the submission of the initial version of this manuscript some results have been published for the second ENVISAT validation workshop (ACVE-2) organized by ESA (http://envisat.esa.int/workshops/acve2/). The focus of these papers is the comparison with independent data (mainly ground based FTIR measurements). References to these (new) results will be added for the revised version of this paper along with a short summary of the major findings (mainly with respect to initial validation results). A detailed comparison of the results from the different groups including the identification of problem areas and how they might be related to differences in the algorithms is an interesting goal for the future but outside the scope of this study. This future intercomparison study should include a characterization of the algorithms in terms of vertical sensitivity and error budget. This has (at least partially) been done for WFM-DOAS (see Section 4 (summary of error analysis) and Section 5 (averaging kernels) of our manuscript) but as far as we know not for the other algorithms (at least nothing has been published in the open literature).

ACPD

4, S1566–S1571, 2004

Interactive Comment

Answers to "Specific comments"

Abstract:

Line 9: We agree: "Good agreement" will be changed to "reasonable agreement".

Line 21-23: Scaling factor: We also think that the scaling factor cannot (at least not entirely) be explained by the higher sensitivity of SCIAMACHY to the lower troposphere. To avoid misunderstandings we will remove the last sentence of the abstract.

Section 1:

Lines 19-28: We agree: We will extent the discussion by more clearly pointing out the strength of thermal IR measurements as proposed by the referee (e.g., profile information and high sensitivity to middle troposphere).

Section 4:

Spectroscopic data: We will add that HITRAN 2000 (plus updates) has been used for the radiative transfer simulations.

Section 5:

Line 10-13: We agree: The paper will be adjusted along the lines suggested by the referee (see also our answer to a similar comment on Section 1).

Section 6.2:

Line 2: For a number of reasons we would like to show (in addition to the plots showing only the cloud free data) also the plots for all SCIAMACHY pixels which contain pixels which are flagged "cloud contaminated" by our cloud detection algorithm - including the comparison of these data with the MOPITT data. First of all our cloud identification algorithm is not perfect. For example pixels over snow and ice covered surfaces are typically flagged cloud contaminated althought they might be cloud free (of course we aim at improving this in the future by using a more advanced algorithm). As a result

ACPD

4, S1566–S1571, 2004

Interactive Comment

of this all SCIAMACHY data over Antarctica (Figures 11 and 12) are flagged cloud contaminated. When comparing these data with MOPITT it is shown that at least on average good agreement has been obtained. This is important information that should be included in the paper. Second, our cloud identification algorithm is very strict, probably too strict for CO. The algorithm has been optimized also for methane and carbon dioxide retrievals (for these applications a very conservative approach is needed due to the demanding accuracy and precision requirements). Among the many things that need further study and optimization is to find out what the acceptable level of cloud contamination is for SCIAMACHY CO retrieval to maximize the number of "useful pixels". For the present study we cannot show this optimum which is somewhere in between the two extreme cases discussed in the paper, namely "all pixels" and "strictly cloud free pixels". We will explain this in the revised version of the paper to make clear why both sets of data are shown and why they have been compared with MOPITT.

Section 6.3:

This section will be substantially improved taking into account the referees comments (see also our answer to "General comments" given above).

Concerning the cloud contaminated data: see our answer above (Section 6.2).

We agree: A detailed description and discussion of each figure will be provided for the revised version of the paper.

Concerning the suggested restriction of the comparison to MOPITT daytime data: In principal, the referee is right. Restricting the comparison to MOPITT daytime data would result in a time difference between the SCIAMACHY and the MOPITT data of about one hour. Including also the nighttime data increases this time difference to about half a day. This is still small compared to the lifetime of CO which is on the order of months. Nevertheless, at least for certain locations where the concentrations are far from background levels due to, e.g., regional sources, a significant variability of the columns during the day can be expected. On the other hand, filtering out the nighttime

ACPD

4, S1566–S1571, 2004

data from the daily data product of MOPITT results in less data being available for the comparison. As we only present comparison for a very limited amount of data (3 days) we think that, for the present study, it is acceptable to compare the daily data products of both sensors without additional filtering. For a detailed validation, however, it should at least be investigated if and by how much the comparison is affected by increasing (or decreasing) the time difference between the measurements. For a number of reasons we think that - for this study - a restriction to MOPITT daytime data would not change the outcome of this study significantly. This is because we have selected the days used for the comparison based on the requirement of good orbital overlap of the daytime measurements of SCIAMACHY and MOPITT. For these days the daytime measurements show a nearly perfect orbital overlap whereas interference of the MOPITT nightime with the SCIAMACHY daytime measurements is limited to certain latitude bands (where the orbital tracks of the MOPITT nighttime measurements are crossing the orbital tracks of the SCIAMACHY daytime measurements (see Figures 14 and 15: the daytime orbital tracks go from ``top right to bottom left" for MOPITT and SCIAMACHY; the MOPITT nightime orbital tracks are crossing these tracks at a certain angle)). As a result of this the number of the MOPITT nighttime measurements effectively used for the comparison is significantly smaller than the number of daytime measurements. In addition, as suggested by Figure 14, the MOPITT nighttime and daytime data are very consistent, i.e., there are no obvious jumps between the nighttime and daytime MOPITT data (near the crossing points). Also for this reason we believe that the results presented in this study will not change significantly when restricting the comparison to daytime data.

Section 7:

We agree: We will re-phrase the abstract and the conclusions along the lines suggested by the referee.

Answers to "Technical corrections"

ACPD

4, S1566–S1571, 2004

Reference to MOPITT validation: Will be added. Wrong hyphenation: Will be corrected. Hadji: Typo will be corrected.

Figure 4: Will be replaced by new plot valid for October 27, 2003. Figures 5-13: Recommended removal of figures for cloud contaminated data: See detailed answer given above (answer to "Specific comments on Section 6.2"). Description of each Figure (5-13) in main text: This will be added for the revised version of the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2805, 2004.