Atmos. Chem. Phys. Discuss., 4, S896–S902, 2004
www.atmos-chem-phys.org/acpd/4/S896/


Atmospheric Chemistry and Physics Discussions

ACPD
4, S896–S902, 2004

Interactive Comment

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

**A. Maurellis**

a.n.maurellis@sron.nl

Received and published: 11 June 2004

**ACPD**

4, S896–S902, 2004

Interactive Comment

# Comment on “Global carbon monoxide as retrieved from SCIAMACHY by WFM-DOAS", Buchwitz *et al.*, *ACPD*, **4**, 2805, 2004

Ahilleas N. Maurellis, Ilse Aben, Anne-Grete Straume, Annemieke Gloudemans
SRON National Institute for Space Research

11 June 2004

## 1. Opening comments

A paper on carbon monoxide retrieval from SCIAMACHY is definitely an important subject for a publication in ACP and the authors have taken a brave first stab at such a study. However they fail to address a number of important points which would convince one that their retrievals are doing a fair job of fitting the spectra and generating columns, notwithstanding an *ad hoc* global correction factor. We believe they could do much to improve the overall presentation of their arguments, assumptions and conclusions. In particular, it is disturbing that the discussion on the main topic of the paper (Section 6.3: Global CO column retrieval results) is so extremely short. Six sentences cover the main results of the paper, and the readers are left to meander through 14 figures (figures 5-18) in order to convince themselves that the claims are justified. In short, we feel that a publication portraying such results is very preliminary and that the authors should consider withdrawing this manuscript and delaying publication until some point in the near future when calibration and validation have been properly attempted.

## 2. Specifics

There are a number of specific issues we would like to see addressed in the manuscript.

1. Section 5: Sensitivity to boundary layer CO. The authors determine an averaging kernel for a column product in what appears to be a fundamentally correct and useful way. However, the averaging kernels they obtain (figure 1) are not interpreted correctly. They rightly claim that the averaging kernels are nearly all equally high at altitudes below 100 hPa. However they fail to address the bizarre inconsistency that nearly all the averaging kernels exceed unity for most altitudes and in some cases the sensitivity appears to be higher at higher altitudes than at lower ones. This can only happen if the retrieved column is higher than the true column (cf. the averaging kernel equation based on differences in section 5) which indicates an intrinsic overestimation in the retrieval. This overestimation is apparently on the order of 10% (reading directly from figure 1) and suggests that the retrieval is at least 10% more sensitive to the stratosphere than to the lower troposphere (for higher solar zenith angles this appears to be worse). Therefore the averaging kernels do not appear to be meaningful or representative and certainly do not support any claim that the authors' retrieval is sensitive to boundary layer CO.

2. As a result of the above claim the authors seem to suggest that one of the main reasons for the discrepancies between the retrieved SCIAMACHY and MOPITT CO columns could be a perceived higher sensitivity to the boundary layer on the



ACPD

4, S896–S902, 2004

Interactive Comment

part of SCIAMACHY. However they fail to address or even consider the accuracy of the MOPITT data set which is used to adjust the SCIAMACHY data set (there are no references to the many validation/verification publications in the refereed literature on MOPITT). Whether or not SCIAMACHY is more sensitive to the boundary layer simply cannot be verified at this stage and certainly not with the material presented in the paper.

3. The authors do not make clear that the MOPITT data has in fact had clouded pixels removed, which adds further to the confusion since no attempt has been made to mask out the clouds in corresponding SCIAMACHY plots (cf. figures 14 and 15).

4. The choice of the *ad hoc* scaling factor of 0.5 is not adequately addressed in the paper leaving the reader with the impression that it is chosen simply for practical reasons, ie. the fact that it “works”. In fact, it does not even do this, as close investigation of the scatter plots in figures 12, 13, 17 and 18, reveals, unless the error bars on the derived offsets (a0 if we are not incorrect) are as large as the offsets. In other words, it is not at all clear why the scaling factor was chosen to have the value it has for all the cases presented. For example, it could be a function of time because of instrumental effects (related to the changing ice layer on the SCIAMACHY channel 8 detectors) or ground scene (albedo differences) or even a function of the column itself due to sampling problems in the retrieval algorithm. Some indication of the choice of correction factor should also be given in the figures, since it is otherwise only mentioned in passing in the paper and once in the abstract.

5. The language in section 6.2 is garbled, particularly the third sentence beginning "This results in errors...". If the second "as" is replaced by "because" then the sentence appears to be saying that the retrieval is sensitive to columns above the clouds. Would this be the correct interpretation? If so, then a comparison



ACPD
4, S896–S902, 2004

Interactive Comment

between figure 7 and figure 8 shows immediately that clouded pixels frequently display much higher columns of CO than unclouded pixels (we refer the reader to the red pixels in figure 7 which are grayed out in figure 8 and also suggest making a similar comparison between figures 10 and 11). Clearly there are significant, poorly understood retrieval effects which have not been properly considered by the authors in making this claim.

6. We do not understand the choice of figures from figure 4 onwards in that they fail to provide any consistent picture of what has been retrieved. For instance, why show cloud plots in figure 4 for January 24, 2003 and then show a MOPITT global column retrieval in figure 5 for October 27, 2003? Surely one should show the same cloud mask for figure 5 and, most importantly, a global SCIAMACHY retrieved plot for figure 5, to make some kind of logical link. Clearly they have tried to make some link of this kind between figures 9 and 10. However the cloud plot (such as figure 4) is once again missing and perhaps the most important question is not addressed, namely, what kind of correlation is expected between two instruments measuring differently-sized air volumes which are only partially colocated in space and time.

7. Section 6.3 refers. At first glance we would question whether a correlation coefficient of r between 0.40 and 0.67 indicates "good" agreement especially since the corresponding r-squared values indicate useful correlations of 20% to 45%. In fact, the correlation coefficient (0.67, figure 13) for land-only pixels on January 30, 2003 should be compared to the correlation coefficient (0.40, figure 17) for land-only pixels on May 27, 2003. Note that the high correlations are only obtained when measurements over sea are completely removed from the correlation comparison.

8. Latitude and longitude are completely missing from figures 6 and 7 so it is extremely difficult to gauge the scale of the plots and hence relate them to other



ACPD

4, S896–S902, 2004

Interactive Comment

plots in the paper such as figure 5.

## 3. Closing Remarks

In the above we have simply tried to highlight some basic problems with the manuscript and have not even covered all the issues we think should be addressed. What the authors have in fact shown is that there is an order of magnitude agreement between CO results from a new instrument, SCIAMACHY, and another somewhat more established instrument, MOPITT and they have also shown one instance of a plume event identified by both instruments. The results are heavily dependent on the calibration employed and the authors make very scant treatment of this apart from mentioning an ad hoc slit function which has been tuned to produce the best residuals. As readers we are left clueless as to how this tuning has even been carried out.

Ideally, it is our view that papers on a new instrument should await at least some of the results of the validation campaign which have, in fact, recently been presented. Such collaborative work would provide the scientific readership of this journal and the community at large with a much-needed source of properly-understood CO data indeed! Therefore we would recommend that the authors consider withdrawing this paper in order to correct the many instances listed above of poor representation of the data and its many interconnections. They should also include some of the validation work that took place recently and much more discussion on the comparison with MOPITT. Only then can they ground their claims that

- quantitative information can be retrieved (not clear from these results),
- biomass burning is detectable (only one instance has been presented),
- they have good agreement with MOPITT (not clear from the plots or the correla-



ACPD

4, S896–S902, 2004

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

tion coefficients),

- their retrievals are sensitive to the lower troposphere (given that the averaging kernels are impossible to understand).

---

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



ACPD

4, S896–S902, 2004

Interactive Comment