Atmos. Chem. Phys. Discuss., 4, S912–S918, 2004
www.atmos-chem-phys.org/acpd/4/S912/

Atmospheric Chemistry and Physics Discussions

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

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Received and published: 15 June 2004

Author comment in response to interactive comment SC S896 from A. Maurellis and I. Aben et al. on "Global carbon monoxide as retrieved from SCIAMACHY by WFM-DOAS" (M. Buchwitz et al., 2004)

from Michael Buchwitz (corresponding author) on behalf of all co-authors

In their opening comments Maurellis et al. state that the subject of the paper "Global carbon monoxide as retrieved from SCIAMACHY by WFM-DOAS" covers an important topic for publication in ACP. Maurellis et al., however, who are also working on CO retrieval from SCIAMACHY nadir spectra but have not yet attempted to publish any results as far as we know, recommend to "withdraw this manuscript" and to delay its publication "until some point in the near future ...".

We appreciate very much comments of experts having hands-on experience in this area (there are not many people in the world working on CO retrieval from satellite) but, and this might be a bit problematic and should not be overlooked, these experts have (of course) their own interests.

Maurellis et al. are drawing strong conclusions based on a collection of weak arguments including obvious misunderstandings. In this reply we will comment on this point by point. We do not see any reason for withdrawing the manuscript. Nevertheless, Maurellis et al. provide some interesting suggestions for further improving the paper and we will consider this as good as we can for the revised version of the paper.

We know, and this is clearly stated in the manuscript, that we can only present first results concerning CO retrieval from SCIAMACHY at this stage. What we present in the submitted manuscript is definitely not the last word but it is in our opinion a very interesting first word that will stimulate future discussions and demonstrates for the first time using real in-orbit data that SCIAMACHY is in fact sensitive to atmospheric CO variability (as predicted theoretically) although currently not all (calibration and retrieval algorithm) problems have been solved. We submitted this paper because we think that it contains interesting information about what can already now be achieved with SCIAMACHY despite of a number of problems to be investigated further (and solved in future version of the retrieval algorithm). In addition we summarize interesting theoretical results, such as an error analysis of our retrieval algorithm and the sensitivity of SCIAMACHY to CO concentration changes as a function of altitude. In addition, we present a global comparison with the independent measurements of the MOPITT instrument.

In the following we will comment point by point on the issues raised by Maurellis et al. (we use the same numbering as used by Maurellis et al.):

1. Sensitivity to boundary layer CO / averaging kernels: Maurellis et al. say that we "failed to address the bizarre inconsistency that nearly all the averaging kernels exceed

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unity for most altitudes...". In fact there is no bizarre inconsistency but we have probably failed, namely to explain this good enough for readers not familiar with averaging kernels. We will do our best to better explain this in the revised version of the paper. For now we only would like to point out that there is no mathematical or physical reason that prevents averaging kernels to deviate (even significantly) from unity in one or the other direction. We checked our averaging kernels by various means (e.g., by using weighting functions calculated (quasi)analytically or by numerical perturbation of the vertical profiles used for the radiative transfer calculations) and we are quite confident that the averaging kernels are correct. We have also computed averaging kernels for other gases, for example, for CO2, which also exceed unity in the lower atmosphere. Our CO2 column averaging kernels are in good qualitative agreement (perfect agreement is not be expected as the averaging kernels dependent on the measured spectrum AND on the retrieval algorithm) with, for example, the CO2 averaging kernels shown in Crisp et al., The Orbiting Carbon Observatory (OCO) mission, Advances in Space Research, 2004 (in press), which has a values around 1.2 in the boundary layer and above.

2. We do not "seem to suggest that one of the main reasons for the discrepancies between ... SCIAMACHY and MOPITT ... could be a ... higher sensitivity ... to the boundary layer ... of SCIAMACHY". But we point out that this is expected to contribute to the observed differences. We expect discrepancies because SCIAMACHY sees the boundary layer and MOPITTs thermal infrared measurements have low sensitivity in this region (which is typical for nadir measurements in the thermal infrared and a well documented effect discussed in many papers on various satellite instruments). Maurellis et al. are right in stating that we should better consider the accuracy of the MOPITT measurements including giving reference to main MOPITT validation papers. We will cover this aspect in the revised version of the paper.

3. Clouds: In our paper we show results of CO from SCIAMACHY with and without including cloud contaminated pixels (for example: Figures 14: MOPITT CO without cloudy pixels, Figure 15: SCIAMACHY CO with cloudy pixels, and Figure 16: SCIA-

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MACHY CO without cloudy pixels). It is not clear for us why Maurellis et al. think that this "adds further to the confusion" thereby referring to Figures 14 and 15.

4. Scaling factor: Maurellis et al. find that "the choice of the ad hoc scaling factor of 0.5 is not adequately addressed ... .it is not clear why the scaling factor was chosen to have the value it has for all the cases presented". It is clearly explained in the paper why the scaling factor has been applied and why it has the value of 0.5. It is explained already in the abstract that we have selected the scaling factor to adjust the SCIAMACHY data to MOPITT (which are the only global scale measurements of CO we can compare our data with). We could understand the arguments given by Maurellis et al. if we would have first scaled the data to MOPITT and then report on how good our average agreement with MOPITT is but this is NOT what we have done. We focus entirely on VARIABILITY (in space and time) rather than on the average agreement (looking at correlations, concentration hot spots seen by MOPITT and SCIAMACHY etc.). We investigate to what extent we are able to capture real atmospheric variability (this is what is most important as most of "the science" is in the variability (pattern) and not in the absolute level). In any case, we have discussed the scaling factor issue in quite some detail in the paper and have also clearly stated that this needs further investigation. Who ever uses the WFM-DOAS Version 0.4 data products and does not like the scaling factor can simply multiply all columns by a factor of two to get rid of it. For our first version of the retrieval algorithm, i.e., WFM-DOAS Version 0.4, we have included the scaling factor because according to our understanding the columns originally retrieved were obviously (for still to be determined reasons) overestimated in a quite systematic way.

5. We apologize for the difficulty to understand the third sentence of Section 6.2. We will improve it in the revised version of the paper. In fact we mean that if WFM-DOAS is applied to cloudy pixels "most probably" the sub column above the cloud is retrieved. The term "most probably" refers to the fact that clouds are typically not simply a reflecting layer perfectly shielding the column below the cloud. In fact radiative

transfer for (partially) cloudy atmospheres is a challenge for radiative transfer modeling and no means exist (today and in the foreseeable future) to compute perfect radiative transfer solutions due to the complexity of real world clouds. This implies that is is not possible except for certain ideal conditions to say what the effect of a real cloud on the retrieval is. Therefore it is more than obvious that there are "poorly understood retrieval effects which have not been properly considered by the authors..." as criticized by Maurellis et al. Because of this we (only) generate a cloud mask to simply flag cloud contaminated pixels because we do not know (and in fact probably everybody else) what exactly the impact of real clouds on the retrieved columns is for all possible cases. Furthermore we do not claim in the paper that we know what the impact is - which is in clear contrast to what Maurellis et al. think we are doing in the paper. On the other hand if we compare the columns for cloud free pixels with columns retrieved nearby for cloudy pixels, the columns for the clouds pixels are typically lower than the columns for the cloud free pixels. This is what is expected "on average" and therefore we use the term "most probably". We will clarify this in the revised version of the paper.

6.+7. Maurellis et al. are right concerning the PMD plot. In the revised paper we will show a PMD plot for October 27, 2003, as this fits better to the following figures presented in the paper. We will add more details concerning the figures presented. This was also an important remark of Reviewer number 1 (in his technical comments) who recommended to add some more Figures and also to add a table giving an overview about the quantitative results for the days discussed. He also recommended (as done by Maurellis et al.) to add a much more detailed description of the comparison with MOPITT. All this are very good suggestions and will be considered in the revised version of the paper.

8. We will add information on latitude and longitude for the revised version of the paper.

Concerning the comments of "Maurellis et al." in their "Closing remarks":

We will make sure that more information will be added in the revised version of the

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paper concerning the last four bullets listed by Maurellis et al. We will do so although already by the results shown in the initially submitted version of the paper it is clearly demonstrated (although not sufficiently explained in the text) that good agreement with MOPITT has been found (clearly shown, for example, in Figures 6-8, which show good agreement (similar pattern) between the MOPITT and SCIAMACHY CO columns). Maurellis et al. state that only "one instance" for biomass burning has been presented which is simply not true (see CO plumes (i) Figures 6-8 for October 27, 2003, east African coast west of Madagascar or (ii) January 30, 2003, (Figures 9-11) near Ghana). Furthermore, we have observed CO plumes for several days at other locations (in agreement with MOPITT and/or biomass burning/fire events) not mentioned in the paper because we wanted to present detailed results only for selected days to not overload the paper (we will mention this in the revised version of the paper). Because we have good agreement with MOPITT and because MOPITT data have been extensively validated and are of known high quality, we have a clear indication that quantitative information can in fact be retrieved from SCIAMACHY. In the mean time we have got some additional information (not available when submitting the paper) from comparison with FTIR ground based measurements and we mention this and will add references to corresponding papers in the revised version of the paper (e.g., reference to Warneke et al., 2004 (ACPD, submitted)).

Concerning the comment given by Maurellis et al. that our "averaging kernels are impossible to understand": (i) see our comments already given above, (ii) because Maurellis et al. are also working on CO retrieval from SCIAMACHY we simply recommend that Maurellis et al. should compute averaging kernels using their algorithm(s). In this context it is however worth to mention that identical averaging kernels are not to be expected because the averaging kernels not only depend on the measured spectra but also on the retrieval algorithm including its underlying radiative transfer model. As far as we know, the radiative transfer model used at SRON neglects scattering at all whereas we use the solution of the full radiative transfer equation, including multiple scattering. We expect that this will result in significantly differences, especially at high

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solar zenith angles.

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

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