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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric methane and carbon dioxide from SCIAMACHY satellite data: initial comparison with chemistry and transport models" *by* M. Buchwitz et al.

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Authors answer to the interactive comment of anonymous referee number 4 on manuscript Buchwitz et al., Atmos. Chem. Phys. Discuss., 4, 7217, 2004 (manuscript number: acpd-2004-0135).

General:

First of all we would like to thank the referee for the constructive comments on our manuscript. Each comment will be carefully considered for the revised version of the

paper. Below we give answers to each of the comments made by the referee.

Concerning validation: We fully agree that "It is extremely important that these data be validated to the level necessary to support the conclusions given, ...". Recently first validation results have been presented at a SCIAMACHY Validation Meeting (Bremen, 6-8 December 2004). The SCIAMACHY WFM-DOAS methane and carbon dioxide columns presented in our manuscript have been compared with FTIR column measurements of a network of ground stations. The results are still preliminary as more analysis is needed and the findings have not yet been published. Nevertheless, for the revised version of the paper we will add at least a short summary of the information currently available concerning the validation of our data products. At this point we would like to mention that validation with independent ground based measurements has limitations (only a few stations measure columns of methane and CO2, cloud free scenes are required, many stations are located on top of mountains with complicated topography in their surroundings and only observe overhead columns, many stations are on islands or near the coast which is problematic because the near-infrared satellite measurements are less reliable over water because of low ocean reflectivity, etc.). Therefore, validation with independent measurements is limited and not straight forward and it is unclear if a validation "to the level needed" is possible with the current network of ground stations. Because of this it is very important to also compare with models as done in the paper. Because of this it is also important to formulate the conclusions carefully.

Answers to "Specific comments":

Pre processing (p.7223):

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In the revised version of the paper we will shortly describe the difference between the "standard" dark signals and the "improved" dark signals we use (in short: the improved dark signals have been measured closer in time (mostly during the same orbit) to the nadir measurements and they have been recorded for the same exposure times as the nadir measurements thereby avoiding interpolation errors). We will also add information on how the "dead and bad pixel mask" has been obtained.

Understanding of errors (p.7226, l.8):

The referee is right: The retrievals errors are on the order of the signal of interest, which is a very small signal. For the revised version of the paper we will add more details concerning error analysis and on the influence of errors on the conclusions drawn in the paper.

Cloud identification (p.7230):

We will add more details to give the reader more confidence in our cloud identification method (e.g., we will consider a comparison with MODIS as suggested by the referee) and provide information concerning the stability of the results with respect to the selected threshold.

Scaling factors (p.7232,l.1):

The scaling factors currently used deviate significantly from unity (1.27 for the CO2 columns; 0.85 for the O2 columns; no scaling of the methane columns) and we agree that the use of (large) scaling factors is not satisfactory. The scaling factor

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used by Yang et al. is in fact significantly smaller. In this context one has to point out that Yang uses ground based measurements which for a number of reasons (calibration/maintenance of the instrument, direct sun observation versus reflected and scattered solar radiation, etc.) can be expected to suffer less from systematic errors than satellite observations. For future versions we will aim at improving this situation (by investigating in depth possible reasons for the observed bias: quality of the calibration, slit function, spectroscopy, limitations of the retrieval algorithm). For the current version we in fact assume that the scaling factors are constant. The paper shows that at least for methane (where we use a scaling factor of 1.0, i.e., no scaling factor) this is not a good assumption because of the time dependent bias introduced by the "ice issue". For the current version the scaling factors have been introduced as a simple first order correction for an obvious bias. We are aware that this has implications for the conclusions as we cannot entirely be sure that the retrieved variability is free of any influence of our assumption of a constant scaling factor. We will point this out more clearly in the revised version of the paper.

O2 measurements (p.7240,l.12-24):

For the revised version of the paper we will add more information concerning our O2 column retrieval as this is relevant for XCH4 and XCO2.

Restriction on conclusions (p.7244,l.3):

It is right that our conclusion concerning CO2 is only valid after application of the scaling factor. This is an important restriction and will be added for the revised version of the paper.

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Answers to "Stylistic issues":

For the revised version of the paper we will use a revised selection of figures. Figures which are not absolutely necessary for the main part of the paper will be removed or shown in a separate Appendix, if appropriate.

The typos listed by the referee will be corrected in the revised version of the paper.

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