

Interactive comment on “Carbon monoxide, methane and carbon dioxide columns retrieved from SCIAMACHY by WFM-DOAS: year 2003 initial data set” by M. Buchwitz et al.

Anonymous Referee #2

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General Comments

This paper is well-written and presents some important measurements which, if confirmed, provide some new insights into the global distribution of important greenhouse gases.

Specific Comments

My main reservation with the paper concerns the lack of validation of the data in the presence of clear problems in the retrievals. The only other measurement set that

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is referred to is the MOPITT dataset and the comparison with that dataset is by no means rigorous. In two cases (CO and CO₂) large multipliers are used on the data to “compensate for an obvious over-estimation”(page 1950), but there is no justification (in this paper) for the magnitude of these numbers.

In the case of the CO figures, the multiplier is 0.5x (page 1950) and this nullifies any comparison with the general levels of the MOPITT data, although it does leave the door open for comparisons of the variations. Thus the comment on that for the September data “the overall agreement is good. Both sensors show that the columns are typically between 1.7 and 2.5 x 10¹⁸ molecules/cm²” (page 1951) is a statement on the magnitude of the adjustment factor, not on the comparison of the data. It is true that the SCIAMACHY averaging kernels are substantially different from those of MOPITT and therefore a point-by-point comparison is futile. There are similarities in the major features, but there are almost as many differences as there are agreements when the agreement on general levels is considered as simply an artifact of the retrieval process. Much more work is required in this area.

There are similar comments for the CH₄ and CO₂ measurements as well. The normalizing process to compensate for the air column amount is a reasonably sound one, but it can also obscure points in the retrieval and the physics that are hard to discount. When considering the changes in CH₄ and CO₂ that are of the order of 1-5%, the multiplying factor of 1.27 for CO₂ (page 1957) is disturbing.

My overall comment is that the variations shown are intriguing, and should be published with appropriate caveats. I think that the authors should be more cautious in their conclusions. Thus the statement that “the CO₂ columns show agreement with the model simulations within a few percent.” (page 1960) should be more cautious since “a few percent” covers almost the entire dataset variation and ignores the 1.27x multiplying factors.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1943, 2005.