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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of six years of SCIAMACHY carbon monoxide observations using MOZAIC CO profile measurements" *by* A. T. J. de Laat et al.

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The paper "Validation of six years of SCIAMACHY carbon monoxide observations using MOZAIC CO profile measurements" by de Laat et al. continues the SCIAMACHY validation set of papers and involves in consideration geographically scattered locations that were not used in their previous publications. SCIAMACHY was designed to use solar scattered NIR radiation and its data complement the data of MOPITT, AIRS, and other TIR sensors, widely used by the community of users. The expected advantage of this approach was a higher sensitivity to the boundary layer in comparison to TIR instruments. The paper is well written and logically organized. Presentation of main results (Figs. 2 and 3) is very convenient for a reader. However, the usefulness of this paper is very limited. First of all, the advantages of NIR before TIR instruments have not been demonstrated.

The authors briefly describe the problems of SCIAMACHY data in the Discussion section: these problems partly neutralized its advantages. The question is: partly or completely? This issue is not discussed in the paper, but it is the most important for further development of the satellite based CO sensors.

This paper practically avoids mentioning averaging kernels (AK). They write (page 1988, lines 11-13): ”In this paper, we assume that the SCIAMACHY CO total column is the real total column. De Laat et al. [2010] estimated that the effects of the SCIAMACHY CO a priori and averaging kernel were of the order of only a few percent.” Meanwhile, it is not correct. De Laat et al. [2010] obviously estimated the AK for the ideal atmosphere, i.e., without clouds, thin cloud, aerosols, etc.

Validation can be treated as a verification of AK. If AK is correct, then for any true vertical profile of the gas (measured from an aircraft, or using a sun-tracking GBS) the convolved total column (TC) should agree with the retrieved TC. The authors are advised to re-evaluate AK using an approach like that by Eskes and Boersma (2003). Without this significant additional work any validation does not make sense. It is well known, that in cases of similar real and a priori profiles, using any AK, wrong or correct, would give similar result. Conversely, in cases when the real profile is different from a priori, AK is very important, and the correct AK allows to reconcile retrieved and true TC. So, the areas with strong surface emissions, resulting in high surface concentrations, are mostly useful for validation of AK. The paper clearly demonstrates that in these cases (locations ## 11, 21, 23, 24) the bias is maximal. Would the MOSAIC profiles be convolved with correct AK and a priori, the agreement would be better, and the authors should not waist the words for long explanations of these disagreements.

So, the paper needs a significant revision. Complete calculations of AK for the entire

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data set is not necessary, however; calculations for the most interesting cases like central China would be sufficient. A comparison with MOPITT or other TIR instrument, as well as comparison with the results of Liu et al. (2011) is necessary.

Validation of satellite sensors at source areas is necessary because these data are used for top-down estimates of emissions. If a sensor is not sensitive to the boundary layer, then estimates obtained from the data are not accurate. The paper includes MOSAIC data for such areas and this is an advantage of the paper.

Some specific remarks.

Page 1988, line 9. A sentence or two with a brief description of the IMLM algorithm should be added. Page 1991, lines 8-12. Try to rephrase these two sentences to clarify what you are saying. Page 1997, lines 2-13. Here and in other places the explanation of disagreements by "local effects" is not convincing. I guess, using correct AKs would improve the agreement.

References

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