

Interactive comment on “Global satellite validation of SCIAMACHY O₃ columns with GOME WFDOAS” by A. Bracher et al.

Anonymous Referee #1

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The subject of the paper by Bracher is a direct comparison of GOME and SCIAMACHY total ozone columns. These kind of comparisons are an essential part of the validation exercise, and is crucial to create a single coherent ozone data set based on the two instruments. However, to my opinion the discussion by Bracher et al. needs major revisions before it is acceptable for publication, as detailed below. - The conclusions should be based on the results presented and need careful reconsideration. - Additional plots, in particular global latitude-longitude maps of the SCIAMACHY-GOME differences and plots of seasonal biases as found with GOME GDP 2.4 are helpful and could easily be added. - Because two different retrieval algorithms are applied to different satellite instruments there is an attribution problem. This can be improved by involving a third

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data set. There are a few options, for instance a comparison of GDP 2.4 with WFDOAS (both GOME) or a comparison with GDP v4. - The introduction and list of references should give credit to retrieval and validation activities by other groups.

After reading the paper it is not clear to me what I have learned about SCIAMACHY. One important reason for this is the fact that two quite different algorithms are applied to two instruments. Are differences detected related to details of the retrieval algorithms, or to level-1 issues in SCIAMACHY? In particular, because the paper is submitted to the SCIAMACHY validation special issue, I would especially like to learn how level-1 calibration issues influence the ozone column retrieval. This can be studied by applying one algorithm with fixed settings to both SCIAMACHY and GOME. Indeed, the reprocessing of SCIAMACHY with the WFDOAS algorithm (mentioned by the authors) would serve this purpose, and may result in much clearer answers concerning the quality of ozone retrievals that are achievable with SCIAMACHY. Another possibility would be the comparison between WFDOAS and the old GOME GDP processing version 2.4 (if the data is still available). Comparisons with such an additional product would provide a means to distinguish instrument aspects from retrieval aspects.

Looking at the results without prior knowledge about the retrieval methods one first of all would draw the conclusion that both SCIAMACHY and GOME are in surprisingly good agreement, to within a few percent. With this level of agreement, and based on figures 1 to 5, it is not so easy to judge which of the two is actually better. However, the authors claim that the differences should be attributed to SCIAMACHY. This conclusion is based on WFDOAS validation results with ground-based observations which are mentioned to agree within typically 1%. I find these very good agreements between WFDOAS and WOUDC quite surprising: Dobson (and Brewer) instruments are normally quoted to have uncertainties and seasonal dependencies of a few percent (inter-calibration; temperature and profile dependence of the derived columns) with larger uncertainties under extreme conditions (e.g. ozone hole). The authors also mention WFDOAS vs. WOUDC differences of 5-8 % for large solar zenith angles. This

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is also where the larger GOME-SCIAMACHY differences are observed. Can the authors be sure that differences at high SZA can be attributed to SCIAMACHY (instead of WFDOAS-GOME)? With all this in mind I would not be able to draw the conclusion that the SCIAMACHY retrieval is of lower quality.

Unfortunately the period of overlap between GOME and SCIAMACHY is less than 6 months. After studying the figures (figure 3) I would claim that evidence for a seasonal bias is rather weak. The authors quote seasonal biases that were identified in the GOME-GDP 2.4 product (Lambert, 2000). It would be very instructive if the authors could include the result found by Lambert as additional curve in figure 3 (if possible). This could add more credibility to the claim that seasonal biases are observed.

The authors mention that "A reprocessing with an algorithm equivalent to GOME GDP version 4.0 and/or GOME WFDOAS V1.0 will improve significantly the quality of the SCIAMACHY ozone product" (abstract). How can the authors be sure? The quality seems to be quite good at the moment. Is this a conclusion which is drawn based on the results of the present study or is it just a belief? For instance, there are serious problems with the radiometric calibration and polarisation of SCIAMACHY, and I would argue it can not be excluded that this will give unexpected results when WFDOAS is applied to SCIAMACHY measurements. The statement should either be justified or be removed.

The paper mentions on p 799 and in the conclusion that the current GOME version is GDP v3.0. GDP 4 has now become available (December 2004). In fact it would be quite interesting to see results for both the WFDOAS and GDP 4 algorithms.

The list of references is very Bremen oriented. The reader would benefit from a more balanced introduction which gives credit to recent ozone column retrieval developments for GOME, SCIAMACHY, but also TOMS (version 8). Also a short summary of past SCIAMACHY ozone validation activities and main conclusions (the paper by Lambert etc.) would be very helpful to understand and judge the additional value of

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the presented work. In the conclusion the authors should indicate what new results are obtained with respect to existing validation papers, like the paper by Lambert et al. (the small negative bias of 1% was already reported).

The comparison approach consists of a gridding on a 2.5 degree latitude-longitude grid. Subsequently the results are discussed as function of latitude alone. Because of the possible dependence of the retrieval on the surface albedo (LER), surface altitude, snow cover, it is very interesting to see the difference between the two products as a global map. I would encourage the authors to include such a map (maps) in an updated version of the paper.

It is strange that the authors put so much emphasis on the gridding approach and computational speed issues, even mentioning this in the abstract. I regard the gridding as a rather straightforward approach and even extended comparisons with some added search criteria should not be a problem for the relatively modest satellite data sets considered (compared to modern day computer power). I suggest to remove this remark from the abstract.

On page 806: "In summary, the current operational SCIAMACHY total ozone data Version 5.01/5.04 shows an insufficient data quality with a clear dependence on season, latitudes and total ozone." As mentioned above, to me it is not clear that this follows from the inter-comparison presented.

p 806, before acknowledgements: "An adaptation of WFDOAS algorithm to SCIAMACHY is currently planned and it will ensure a better consistency between GOME and SCIAMACHY." Such a conclusion can not be justified and should be removed.

Figures 2, 4, 5 seem to suggest a common reason for the differences. Could all plots be explained with for instance only an ozone column dependent bias ? The authors mention: "Both figures show a clear tendency in the difference to GOME as a function of the SCIAMACHY SZA" (Fig.4) The figure shows changes in the order of 1-2 %, which I suggest may also be classified as good agreement !

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Finally let me make clear that I regard the algorithm developments implemented in WFDOAS as very important improvements with respect to the older versions of GDP. The paper, however, is a validation paper and the authors should base their conclusions on the comparison material presented and not on algorithm considerations.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 795, 2005.

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