

Interactive comment on “Near-real time retrieval of tropospheric NO₂ from OMI” by K. F. Boersma et al.

Anonymous Referee #3

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Review of the paper "Near-real time retrieval of tropospheric NO₂ from OMI" by K. F. Boersma et al.

The paper by Boersma et al. describes a new retrieval system set-up to provide tropospheric NO₂ columns from OMI measurements in near-real time. It briefly describes the instrument and data flow, discusses some aspects of the algorithm used in comparison with a similar system used for GOME and SCIAMACHY data and the standard off-line OMI NO₂ product. Some aspects of the error budget are discussed including precision and cloud treatment and a comparison to SCIAMACHY data is shown.

The paper is well organised and clearly written although repetitive in some places. The topic is rather technical for ACP as it is mainly a description of an improved retrieval

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system with error discussion and only very brief examples of atmospheric applications. As the product (tropospheric NO₂ columns from OMI) is of large interest to many scientists working in the field of tropospheric chemistry, I recommend publication after careful consideration of the points listed below.

Major comments:

* As stated above, the paper is rather technical for this journal and I recommend removing or drastically shortening section 2.2.

* The authors try to clarify the relation of this retrieval system to the other systems used at KNMI and for the standard off-line processor. However, it is often not clear to me, which parts are identical and which are new. For example, the description in section 5.1 - does it apply to this product only or is it the same in GOME and SCIAMACHY data on the TEMIS web page?

* The one comparison that most readers will like to see, namely to the standard OMI off-line processor is not really given. Here, a table would be useful to show all the differences (fit, stratospheric correction, AMF calculation, cloud treatment etc.)

* The comparison with SCIAMACHY data is rather qualitative. I'd strongly recommend adding scatter plots, if possible coloured or separated by region. Also, it would be interesting to show the difference in tropospheric column the TM4 model predicts for the two observation times to support the author's claim that the observed 30% differences can be explained by the different overpass time. I understand that this will be the topic of a follow-on paper, but I don't think that such a significant difference can be ignored when presenting a new data product.

* In the slant column fitting, no mention is made of H₂O and O₄ which both have significant absorption in the fitting region used. If these two species are in fact not included in the analysis, the author's must provide evidence that this does not introduce systematic errors in their NO₂ columns, for example by comparing the results for one

day of OMI data with and without accounting for these two absorbers.

Minor comments:

* Pixel size: I'm confused by the OMI pixel sizes given in the paper as they don't agree with the description in the OMI web page (<http://www.knmi.nl/omi/research/instrument/characteristics.html>): The small size given is only for the nadir point which should be noted in the text, and also at the edges of the scan, the pixels seem to be larger than 120 km, not 60 km as stated.

* Pixel size SCIAMACHY: This actually is 30 x 30 km² in some seasons over some areas, e.g. over Europe now.

* Page 12313: "The AMF is computed as described in Sect. 3.1 " - actually, I don't think the computation of the AMF is described in this section or anywhere else in the paper

* Section 4.1, across track variability: While this correction will probably work well in reducing the apparent stripes it does assume that the average value of the NO₂ slant column is correct. Experience with the GOME diffuser plate problem however shows, that even when making the data set consistent, there remains an uncertainty in the absolute value which should be added to the error budget.

* Section 4.2., slant column precision: it is not clear to me, how much data was used for this analysis. If several OMI orbits were used, there must have been some overlap in mid- and high latitudes and such pixels must necessarily have different AMFs. Please clarify.

* Section 4.2., slant column precision: Why would one expect not to see a latitudinal dependence? Please indicate the SZA range covered by the measurements. In GOME and SCIAMACHY data, a clear dependence of noise on the SZA can be observed as expected from the variation in intensity.

* Section 5.1: The details of how the model is forced to the measurements (which

weights are used?) and how the weighting for polluted scenes is down are not clear to me. Please clarify or add a reference to a publication which gives more detail.

* Comparison with SCIAMACHY data: As said above, I think more figures are needed. Also, I think Fig. 9 should show data from August 2006 for consistency and include the corresponding SCIAMACHY plots at the same resolution for comparison.

* I might have missed that point, but somehow I didn't understand how you deal with the difference in local time across a OMI swath in the assimilation system and when subtracting the stratospheric column Could you please give a bit more details?

* One important difference between SCIAMACHY and OMI is the swath width which can be quite important for the tropospheric airmass factor in particular in the presence of aerosols. Can you please explain how this is treated in the retrieval?

* section 3.1:

* Typographic corrections:

* page 12305: current the level 1 => current level 1

* page 12307 and OCIO => and OCIO columns

* page 12312 as soon meteo => as soon as meteo

* reference van Noije has been published in ACP

* reference van der A now published

* Table 2: I'm missing either * and ** in the caption

* Several figures: left and right is used but in fact the figures are on top of each other

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12301, 2006.