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Atmospheric Measurement Techniques Discussions

Interactive comment on "An improved tropospheric NO_2 column retrieval algorithm for the Ozone Monitoring Instrument" *by* K. F. Boersma et al.

K. F. Boersma et al.

boersma@knmi.nl

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Response to review of 'An improved tropospheric NO_2 retrieval algorithm for the Ozone Monitoring Instrument'.

The manuscript presents several important improvements of the TEMIS OMI NO_2 retrieval. It is well written and should be published on AMT after minor revisions.

We thank Ref#2 for his or her constructive comments and suggestions. Below we address these one by one.

General comments:

- I propose to change the order throughout the paper: As the de-striping algorithm is applied to the slant columns, it would make more sense to discuss it first (since it is also applied first).

As a matter of fact, the destriping is actually a post-hoc correction. It is not applied first, but instead at the last step, because we have chosen to stick to the original, uncorrected slant NO_2 columns as input to our data assimilation scheme to estimate the stratospheric slant columns (that are destriped automatically because of the data assimilation on a 2 deg $\times$ 3 deg grid). This limits the impact of the correction to only one, post-hoc correction step instead of a prior correction step that would then propagate through the retrieval. The other advantage of our post-hoc correction is that it is still possible to extract the original, striped, tropospheric NO_2 columns from our datafiles, which may be relevant to data users sceptical of corrections that are not based on instrument calibration. Because the correction is done as the last step, and because the improvements to the air mass factor are more important, we prefer to present these first, and describe the less important stripe corrections later.

- Section 3.3.1: An updated O_2-O_2-cloud product is presented. Is this an update of the operational OMCLDO2 product? Is it available for the public? If not, is this planned? Or was the new O_2-O_2 product just processed for the NO_2 product? If so, was the algorithm identical to that of the OMCLDO2 product except for albedo? Please give more information and provide an introduction to this section. Furthermore, to achieve full consistency between the O_2-O_2 and the NO_2 retrieval, also topography should be updated for the cloud product.

Actually, the O_2-O_2 cloud product has been updated on 17 February 2009, when the new OMI surface albedo data from Kleipool et al. [2008] was implemented. Before that date the algorithm used the TOMS/GOME albedo. To repair this inconsistency in the data record, we have now reprocessed the O_2-O_2 retrieval for the October 2004

– February 2009 timeframe. This ensures one consistent cloud dataset throughout the OMI mission. The topography used in the complete cloud retrieval is DEM_3KM, i.e. identical to that used in the DOMINO v2.0 algorithm. We now updated section 3.3.1 as follows: "Here we compare cloud parameters from the O_2-O_2 algorithm retrieved with surface albedo data from [Kleipool et al., 2008], to cloud parameters based on the TOMS/GOME surface albedo, as in DOMINO v1.02. The reprocessing of the O_2-O_2 algorithm for the October 2004 - February 2009 timeframe ensures that we now have one consistent cloud dataset at our disposal for the complete duration of the OMI mission. The topography used in the O_2-O_2 retrieval is DEM_3KM, i.e. identical to that used in DOMINO v2.0. For more information on the O_2-O_2 algorithm, we refer to the readme-file, available on htp://disc.sci.gsfc.nasa.gov/Aura/data-holdings/OMI/omcldo2_v003.shtml."

- Section 6: The aspect of a (partly) intrinsic aerosol correction within the cloud correction is interesting. However, it is quite different and independent from the improvements presented before. I thus propose to deal with this topic in a different paper (with additional case studies) and remove section 6 from this paper. This would strengthen the focus on the algorithm improvements in the current work. If the section is kept here, it has to be pointed out that the situation is probably different for strongly absorbing aerosols.

What we hope to achieve with section 6 is to point out that cloud retrievals are inherently sensitive to the presence of aerosols and allow for -at least- a partial correction for aerosols. This aspect of DOAS retrievals has been overlooked or neglected in many retrieval papers that instead proposed explicit aerosol corrections based on external (climatological or model) aerosol data without taking into account the sensitivity of cloud algorithms to aerosols. We feel that our Figures 11, 12, and 15 now provide reasonable empirical evidence that a relationship between aerosols and cloud parameters cannot be neglected for scattering aerosols. Whether an accurate intrinsic correction for aerosols is achieved through the modified cloud parameters remains to

be seen, but we think that section 6 might be the start of a worthwhile discussion on this topic.

In section 6 we state emphasize that the our findings hold for predominantly scattering aerosols: We conclude that OMI cloud retrievals are sensitive to the presence of scattering aerosols, particularly in situations with predominantly scattering aerosols, such as over the southeastern United States in July 2005.

Specific comments: 2330/3: "a correction" -> "an empirical correction"

Done.

2332/15: "The AMFs of the current OMI retrievals are based on external datasets which have coarse spatial resolution compared to the OMI ground pixels".

Done.

2339/15: "but the absolute reduction"

Done.

Section 3.2: Please discuss the effect of the terrain height on the cloud product as well. Acarreta et al. applied ETOPO, thus the OMCLDO2 cloud product has systematic errors over terrain with structured topography.

We refer here to our updates to section 3.3.1, where we describe the use of the high resolution DEM_3KM data in both cloud and NO_2 retrievals.

2340/23-24: I am quite surprised that such a small change of 30 m in terrain height can affect the AMF that strongly, and I am sceptical that this is actually real. Could it be that there are remaining/combined effects of the treatment of the lowest layer in the old version (section 3.1)? Or is there a possible interference with the cloud product which used another topography? In any case, more information should be provided, which effect in detail actually leads to such a strong change in the AMF; from my experience, the (box-)AMFs for a typical tropospheric profile over 0m or 30m are more or less the *same.*

30 m is the net (mean) increase in terrain height of the 16 0.25 deg × 0.25 deg grid cells that have been used in the 1deg × 1deg Los Angeles area. Bearing in mind that within this 1deg × 1deg area, the individual grid cells had significant changes in terrain height with opposite sign, it should not come as a surprise that the mean change of 30 m nevertheless can have a 4% effect on the tropospheric columns.

2342/14: Why is the difference so small (in contradiction to 2341/18)?

We should have used more careful phrasing. The old TOMS/GOME dataset is more likely to overestimate in the albedo on the one hand (because of residual clouds) and underestimate the albedo on the other (because of the effects mentioned on 2341/18). Therefore, we now state in 3.3 that the TOMS/GOME albedo dataset, when suffering from residual clouds, will be too high. The probability distribution of the differences thus includes values at either side of zero difference.

2349/19: New aspect raw anomaly -> new paragraph. Please provide an update of the performance of the de-striping after 2007.

Ref#1 also asked a question about the performance of de-striping for later years and we have now replaced Figure 8 by a new one that includes the corrections for the 1st of January for 2005 up to 2009 (we haven't reprocessed later years yet). We updated the text as follows: "Our correction method discards rows (from the boxcar averaging) that have been affected by row anomalies occurring along the complete orbit, but works the same otherwise. From the corrections we obtain for 1 January 2008 (when row 53 was discarded) and 1 January 2009 (when rows 38-43 and 53 were discarded), we see that the method still results in similar but somewhat stronger corrections for the later years. The root mean square correction (per row) has increased from 3.6×10^{14} for 1 January 2005 to 5.3×10^{14} molec.cm^{-2} for 1 January 2009, reflecting the degradation in OMI and the fact that the 2005 average irradiance measurements are less appropriate as reference spectra for later years. Nevertheless, the monthly mean tropospheric NO_2

column fields shown on www.temis.nl suggest that the destriping remains effective, also for later years."

Figures: Some figures (e.g. Fig. 2) have strange grey stripes.

This has been caused by reducing the format of the Figure when converting ps to pdf. We have redone this figure now.

Fig. 2 upper panel: I would expect that, on average, positive and negative deviations of terrain height should cancel out, but I have the impression that red dominates the picture. For instance, the mountains in northern Chad show up as red spots without any blue around.

The new Fig. 2 shows that there actually also some faint blue areas around the mountains in Chad. This was probably obliterated by the grey bars in the previous version.

Fig. 8: The three displayed corrections are very similar and could lead to the impression that one universal correction would be sufficient for the whole timeseries, which is in contradiction to the text on 2349. So please skip 15th and 31st, and instead add the patterns for other days (e.g. 1st of January for all available years).

Done.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 2329, 2011.