

RESPONSES TO REVIEWERS

Ms. Ref. No.: Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-252.

Title: Discrete-wavelength DOAS NO₂ slant column retrievals from OMI and TROPOMI

Response to Anonymous Referee #1

The manuscript “Discrete-wavelength DOAS NO₂ slant column retrievals from OMI and TROPOMI” describes a NO₂ retrieval algorithm based on the DOAS method with discrete spectral channels. The idea of discrete channels has been applied for ozone retrieval, and its potential for NO₂ retrieval is shown in this manuscript, addressing the advantage of simpler instrumental design. The retrieval is implemented for OMI and TROPOMI data with good agreement with respect to reference products (5% difference for OMI and 11% difference for TROPOMI). Critical issues like the selection of discrete channels, uncertainties, and limitations are discussed. The topic of the manuscript is within the scope of AMT.

My major concern with this manuscript is the verification or validation. In principal the overall quality of a retrieval needs to be evaluated by comparisons with independent satellite retrievals or by comparisons with correlative ground-based measurements (e.g., direct sun measurements from Pandora). Since the authors have shown only specific days as examples for comparisons with reference datasets, the retrieval quality can hardly be analysed without a longer time series reprocess of OMI and TROPOMI slant column data and additional comparisons, which are particularly important for discrete-wavelength DOAS (with no wavelength calibration). Therefore I recommend that the authors include more verification or validation results to check for possible systematic bias or temporal drift of differences.

Thank you for your comment. We agree that further validation would be needed if the aim was to establish a new method to retrieve NO₂. However, the work presented in this paper is intended only as a proof of concept rather than a comprehensive validation of a new product. Thus, for this purpose the reference OMI and TROPOMI level 2 products are considered as the “truth” and our retrieval results validated against them. We selected four days from different seasons to get a range of solar angles and prevailing atmospheric conditions, and we used global data to factor in a wide range of atmospheric scenarios and spatial differences. The differences between our retrieval and the reference products are consistent across all the data with the exception of some spatial differences, which we have already discussed in the text. It is expected that the main factor affecting a time series would be noise from the degradation of the instrument, which would manifest in the form of higher scatter in the DW-DOAS retrieval. Therefore, the authors feel that further validation is beyond the scope of this paper, but are currently working on more comprehensive sensitivity analyses and validation which will be the focus of the next paper. We have clarified in the text that the focus of the paper is only to perform a proof of concept.

Another general request is that please follow the standard use of mathematics notation in the literature. For instance, an upright bold symbol needs to be used in the equation and text to make it clear where vectors and matrices are discussed, and also a matrix is usually written enclosed in square brackets.

We have now corrected the mathematical expressions.

The absolute differences are plotted in the appendix, but the analysis in the manuscript only focuses on the relative differences. For instance, “the largest differences around the equator” is actually only valid for the relative difference figure 5 (due to the small absolute values). Please add more discussions of the absolute differences.

Thanks for spotting this. We have added more discussion of the absolute differences.

Specific comments

P2 L21 Generally the observation is separated into in situ measurements and remote sensing measurements, and the remote sensing technique can be further separated into space-based and ground-based category.

Very good point. We have added the ground-based remote sensing technique and included a couple of examples provided by Anonymous Referee #2.

P4 L3 What has been decreased by 0.5%? Do you mean 0.5% of the degradation?

This refers to the performance of OMI's visible channel, which has had a radiometric degradation of ~0.5%. We have now modified the statement to make it clearer.

P4 L19 Please give the full name of SNR.

We added the full name and put the acronym in brackets, i.e. signal-to-noise ratio (SNR).

P7 L5 x shall be a column vector.

Corrected.

P8 Table 1. Should the fitting window for DW-DOAS be 425-450 nm (425-450 nm appears also in Table 2)?

Thanks for spotting this inconsistency. Yes, even though the first wavelength used in DW-DOAS sits around 430 nm, the fitting window should read '425-450 nm'. This range was selected from the literature. The concept of “fitting window” does not apply in the same way as it does in hyperspectral

DOAS retrievals, since we are not using continuous spectra. It should rather be interpreted as a spectral range from where we select our ten discrete wavelengths. We have modified Table 1 and added a line discussing the different interpretation of the fitting window in the context of DW-DOAS.

P9 L26 Why are the negative biases related to the differences in the fitting window? Theoretically the differences in the fitting window shall affect only the scatter of the NO₂ columns (i.e. noise) but not the fitted value of NO₂ column.

Wider fitting windows have traditionally been used to achieve higher signal-to-noise ratios. However, when such windows are used there is a higher chance of introducing other spectral signatures that are not accounted for in the retrieval, resulting in systematic biases (Richter et al., 2011). Evidence of this effect specific to the two windows used in this work can be found in Figure 11 of van Geffen et al. (2015), where changing the fitting window from 405 – 465 nm to 425 – 450 nm causes the fitted SCD to change by up to +0.5E15 molecules cm⁻². We have added a line explaining this.

P10 L17 What is the reason of more outliers for lower cloud radiance fractions for OMI and the opposite for TROPOMI? Also what is the impact of cloud height on these plots? Generally the retrieved column should depend strongly on the bulk height of clouds. High clouds mask the signal from surface NO₂ while for low clouds the satellite observations remain sensitive to the NO₂ in the free troposphere.

We agree with this assessment, and have amended the manuscript accordingly. However, we do not believe that a fair comparison can be made between the two results. For instance, it must be noted that the OMI and TROPOMI observations are over a decade apart and so will be subject to very different cloud structures. Additionally, TROPOMI has a smaller pixel size and so will experience very different cloud radiance fractions to OMI (see Krijger et al, 2007). Finally, there may also be inherent differences between the cloud top heights observed by both instruments based on the different retrieval algorithms they employ; OMI retrieves this parameter using the O₂-O₂ absorption feature at 477 nm (Veeffkind et al, 2016), while TROPOMI makes use of the O₂-A band in its operational retrieval (Loyola et al, 2018). In addition, the QA4ECV product for OMI includes an intensity offset correction, which is not included in the TROPOMI product, and that may explain some of the differences over the ocean (Oldeman, 2018).

In addition, we have updated Figure 8 as there was a plotting error whereby the x and y axes were swapped.

P12 L14 The spatial patterns might be related to the intensity offset correction. The intensity offset correction included in the TROPOMI reference algorithm compensates spectral structures of liquid water, vibrational Raman scattering on H₂O molecules, and possible instrumental issue, leading to a difference over the cloud-free tropical ocean. Please refer to the QA4ECV report for more discussion. In addition, the pattern can also be seen a bit from the OMI absolute difference plot, but it is

overwhelmed in the relative difference plot. Therefore more analysis about the absolute results has been required (see the major comments).

Thanks for the suggestion. The TROPOMI product doesn't include an intensity offset correction. However, the OMI QA4ECV product does and we agree that it could well explain some of the spatial differences. We have now added more discussion about this and the absolute results.

References:

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