

Interactive comment on “An Improved Total and Tropospheric NO₂ Column Retrieval for GOME-2” by Song Liu et al.

Anonymous Referee #2

Received and published: 13 September 2018

This study presents an improved retrieval of SCD and VCD of NO₂ from Global Ozone Monitoring Experiment-2 (GOME-2), based on an updated GDP 4.9 algorithm. The topic of the manuscript is within the scope of AMT and it is of interest to the scientific community. Below are a few comments.

Major comments

While the paper presents a good description of SCD calculation and stratosphere-troposphere separation, its discussion on tropospheric AMF and VCD is relatively weak. 1) The paper includes updates on surface albedo (from TOMS-based to GOME2-based) and NO₂ profile (from monthly climatology to daily varying). The importance of these updates are well known (e.g., see OMNO2, DOMINO, QA4ECV and POMINO for OMI), thus the significance of the finding here should be presented in a

way that it confirms (or contrasts against) the findings of previous studies, aided with sufficient relevant citations in Sect. 1 and 6.3. Also, since the surface albedo climatology is used, what would be the implication of ignoring interannual variability and trends of albedo (as shown in many land cover change studies) on the AMF? 2) For tropospheric AMF, the most important sources of errors come from cloud retrievals and aerosols (e.g., Lorente et al., 2017 and references therein). However, the description of cloud retrieval and (especially) how it is affected by other ancillary parameters (e.g., from TOMS to GOME2 surface albedo) is not clear (e.g., in Sect. 6.4). 3) The crucial role of aerosol treatment (in polluted cases) is only vaguely described, and should thus be better analyzed. If explicitly representing aerosols in the algorithm is not possible, a better description of implied errors should be presented, aided with relevant citations. 4) A global map of tropospheric NO₂ VCD (e.g., monthly climatology, not just one specific month) and how these algorithm updates/uncertainties affect the VCD should be given and discussed.

At its current form, the comparison with MAX-DOAS data does not tell much information regarding the causes of satellite errors (especially slope and bias), thus providing little new knowledge beyond previous findings. The discussion on sampling difference and retrieval algorithm should be aided with quantitative tests (of the algorithm assumptions/parameters, within the context of this study), particularly on how the data sampling (50 km is a relatively loose criterion; the temporal matching is not clear), cloud treatment and aerosol treatment each affects the comparison. Also, the main text only emphasizes the comparison at Xianghe (Figs. 14-16), which gives best consistency. However, the SI shows much poorer comparisons (in terms of bias and slope) at other sites. A better description in the main text of these comparisons (e.g., by better reference to SI, or moving some figures from SI to the main text) is needed to accurately present the overall quality of the satellite product.

Specific comments:

Title: change “total” to “slant”. The term “total column” is misleading.



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P1, L18: Please give the full name of VLIDORT and clarify which version

P1, L20: “a large effect” is somehow ambiguous. Does it mean decrease or increase?

P2,L6 – VOC is not necessary for aerosol formation from NOx.

The title and content of Sect. 3 should be clarified such that the reader understands the section is describing the old algorithm.

P3,L16-17 – the treatment of aerosol is crucial especially for polluted cases.

Table 1 – check the formulas of intensity offset for QA4ECV

Figure 2, right panel – discuss the discontinuity in 2015

Figure 6 – the error appears to be stabilized after 2010. Please discuss.

P15,L10 – “latitudinal” should be “longitudinal”

P19, the paragraph “Figure 11 illustrates...” – what is the impact on cloud retrieval, and how would this affect the sampling (with criterion of $CRF < 0.5$)?

P22, L8: All discussion in this section only shows the improvement of cloud retrieval itself. How about its influence on NO₂ retrieval?

P25,L3 – test the choice of 50 km, which is relatively loose. Also, how is the temporal interpolation done?

P26, first paragraph – need a better description of comparisons at other sites (see my major comment)

P26,L8-11 – The explanation is qualitative. A more quantitative explanation of slope and bias is needed. For example, to what extent the slope and bias can be explained by sampling difference and algorithm limitations? See my major comment.

P28,L9-10 – the sentence is not clear.

P28,L17-21 – again, the statement is too general.

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P29,L32 – the uncertainty in tropospheric NO₂ AMF appears underestimated, especially given the large error in cloud and aerosol treatment (e.g., Lorente et al., 2017).

P30, first paragraph – the consistency at other sites is much poorer. This point should be presented here.

Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2018-235, 2018.

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