

## ***Interactive comment on “First high resolution BrO column retrievals from TROPOMI” by Sora Seo et al.***

### **Anonymous Referee #2**

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Seo et al. present first BrO data from TROPOMI, made a number of tests for optimal SCDs retrievals and present a number of interesting case studies. It is an interesting work. The manuscript is clearly written and the methodology is sound. This study should be published in Atmos. Meas. Tech. if the authors successfully address the following (minor) comments:

-the main remark is that section 3.1 on sensitivity test is rather long and ends up with findings that are mostly known. The method of Vogel is meaningful because it was originally applied on synthetic spectra. Here the technique is applied on real spectra, hence no firm conclusions can be drawn on the optimal wavelength range. The interference with O<sub>3</sub> and SO<sub>2</sub> at short UV is well known and the Ring effect at longer wavelength is not surprising either. The final selection of fitting interval of 333.5-357nm is not very

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different from other studies and I encourage the authors to cite the past papers.

In section 3.1.1, it is written that additional cross-sections for ozone (Pukite et al., 2010) could improve the fits at shorter wavelengths. I propose to test this (simple) approach as it might further stabilize the retrievals.

References are missing: on page 1, studies on the presence of global BrO background in the free-troposphere should be listed. References for BrO observations by CIMS and long-path DOAS should be added. An important paper on MAXDOAS BrO in polar region is also: Frieß, U., H. Sihler, R. Sander, D. Pöhler, S. Yilmaz, and U. Platt (2011), The vertical distribution of BrO and aerosols in the Arctic: Measurements by active and passive differential optical absorption spectroscopy, *J. Geophys. Res.*, 116, D00R04, doi:10.1029/2011JD015938. Finally the work of Theys et al., *ACP*, 2011 is absent throughout the manuscript and should be added.

Needs for clarifications:

Section 2: band 3 is not starting at 320 nm.

Page 3 l27: it is not only noise that results from interferences but also important are biases. Please clarify.

Page3 l30-31 is unclear. Please rephrase.

Table 1: for volcanic plume, the number of pixels is 1748. Is there not a mistake? It seems a lot, especially that it is mentioned in section 3.1.2 that it a 'small-scale BrO plume'. Please check.

Page 7, l16: It is speculated about the impact of the Ring effect due aerosol loads and cloud formation after the eruption. Is there any indication about this? Is the TROPOMI AAI product suggesting the presence of aerosols.

Section 3.2: a figure illustrating the offset correction would be nice. The approach is not very clear from the text (from l18).

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P12, I28: The shift of OMI compared to other satellites is attributed to the 'relatively high measurement noise'. It is unclear. Fitting residuals from OMI are fine.

Figures 9 and 10 are difficult to read. Coastlines are not always visible. It would be good to improve the figures.

Figure 11: the MODIS pictures are shown but not really commented. What should be seen?

Conclusions, P19, I9: please add the obtained values for the slopes of the regression lines, in addition to the correlation coefficients.

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[Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-365, 2018.](#)

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