

## ***Interactive comment on “OMI total bromine monoxide (OMBRO) data product: Algorithm, retrieval and measurement comparisons” by Raid M. Suleiman et al.***

### **Anonymous Referee #3**

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### **General Comment**

In the paper "OMI total bromine monoxide (OMBRO) data product: Algorithm, retrieval and measurement comparisons" Raid M. Suleiman and co-authors present the operational retrieval algorithm for bromine monoxide (BrO) columns from measurements by the Ozone monitoring instrument (OMI). Since BrO is a trace-gas with significant impact on atmospheric chemistry, the paper fits to the scope of AMT. In my opinion, however, the scientific quality would need more than major revisions because the presentation of the retrieval method in its current form is far from being scientifically publishable. Therefore, I suggest to reject the current manuscript but I would like to

C1

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encourage resubmission after the following issues have been addressed.

The decision to suggest the rejection of a manuscript is never easy. In this particular case, however, especial care must be given to scientific quality because operational products are potentially applied by fellow scientists, which may not be trained enough to assess the quality and reliability of the product by themselves. The manuscript, however, rather obfuscates potential quality issues instead of presenting a transparent analysis of the algorithm performance.

## Specific Comments

1) The most critical aspect of the presented algorithm for the retrieval of BrO from OMI measurements lays in the choice of the wavelength range. The presented algorithm applies a fitting window between 319 to 347.5 nm. Not being an expert for the retrieval of BrO myself, I found the arguments of Vogel et al., 2013 concerning the fit interval of OMBRO particularly alarming. Vogel et al. state in the caption of their Fig. 11 that "Wavelength evaluation ranges with a lower limit <325 are dominated by O3 and SO2 features". In my opinion, this is really alarming since BrO, O3, and SO2 chemistry are highly correlated. The manuscript itself even contains proof that the applied wavelength range might be an issue:

a) Figure 2 shows that the applied AMFs ("OMI current") are structured by O3 absorption indicating that interferences with O3 are close to inevitable.

b) Section 3.6 and Figure 11 reveal interferences with SO2.

c) If scaled properly, Figure 2 would reveal many absorbers to have much larger structures than BrO.

d) Figure 4 is clipped below 330nm. Why? Please show the whole story.

In the current manuscript, however, the choice of the new fitting window is justified in (p.7, l. 2) by the simple statement: "to reduce fitting uncertainty by including more BrO spectral structures". This does not convince me and I really would like to urge the authors to present significant arguments to justify the applied wavelength range, which is far-off compared to the wavelength ranges used by other groups (cf. Table 1 in Vogel et al., 2013). The least the authors could have done would be to include a plot showing results using the "Traditional" and "OMI current" wavelength ranges.

I would like to propose some questions that may lead the authors to find profound arguments: How does the residual change when changing the fitting window? What about systematic structures in the residual? How large are biases by other absorbers depending on the fit range? These questions may be tested using real and synthetic measurements as well using the methods by Chan Miller et al., which was co-authored by many coauthors of this paper. Hence, I wonder why methods for reliably comparing different wavelength ranges were not applied here even though they are existing at SAO. Studies building on this data skate on thin ice if the above issues are not addressed appropriately.

Furthermore (p. 1, l. 18), the authors detail that also "the average fitting residual spectrum" is included in the fit. This approach may obfuscate potential systematic interferences and is, in my opinion, only appropriate if its influence is thoroughly studied. Please provide more information. Please investigate the cross-correlations with other absorbers and include it in a revised Figure 1.

2) It is not clear to me to what extend the presented paper is dedicated to validation of the retrieval results. The title suggests "measurement comparison" and the abstract details that the paper "shows some validation", which is confusing. Please be more specific on the purpose and the results of the validation exercises.

Example: The measurements at Harestua are not ideal for evaluating BrO close to the surface due to a lack of tropospheric BrO events. Therefore, I suggest to state that

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the sensitivity of OMBRO towards near-surface BrO may not be evaluated using those measurements. If the authors aim at near-surface BrO with their product, which they do because they claim to have detected BrO over the Great Salt Lake, I suggest to use a data set featuring a significant measurement sensitivity for BrO columns at the ground, for example Frieß et al., 2012.

3) I would like to suggest to review the selection of references in the introduction. There are many citations of papers (co)authored by the coauthors of this paper while papers from other groups seem to be often ignored. E.g. for the sources of tropospheric BrO mostly satellite papers are cited even though there are many observations by groups using ground-based methods. This way of introducing the different findings may be misleading for the readers. Even more, informed fellow scientist readers may be offended if their contribution is not appropriately acknowledged. I suggest to be a bit more generous here. Simpson et al., 2015 may provide a start for a comprehensive list of publications.

Specifically, I miss the following references in an up-to-date BrO satellite paper:

- Please add (Hörmann et al., 2013), which is one of the most comprehensive surveys of volcanic BrO sources using satellites.
- Please add (Liao et al., 2011) and (Frieß et al., 2012) to the references for Barrow, Alaska since both papers present significant BrO observations of near-surface BrO.
- Further BrO satellite papers well worth citing: Begoin et al., 2010, Toyota et al., 2011, Sihler et al., 2012, and Blechschmidt et al., 2016

4) The investigation of the BrO over the Great Salt lake is insufficient and I am missing a rationale for including this issue in the paper at all. However, the results may be due to systematic effects caused by a variety of geophysical parameters (see investigation by Hörmann et al., 2016). Without an appropriate discussion of these influences I would not accept the authors claim that the signal is really due to emissions from the Great

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Salt Lake.

5) In my opinion, the treatment of the interferences with SO<sub>2</sub> is not appropriate. If the issue is known, why not solve it right-away and then publish an improved version? What is the purpose of an OMBRO product featuring this imperfection? I suggest to solve this issue together with choosing an appropriate fitting window before resubmission.

Specifically:

a) Fig. 11 is really hard to interpret. I suggest to show a comparison plot based on single OMI measurements instead of gridded maps.

b) In the introduction (p.2, l.31): Please rephrase "a known issue" for something more specific. In my opinion, the interference with SO<sub>2</sub> is not just an issue but a significant flaw.

c) Fig. 4: The x-axis must contain the entire fitting window at least. I find the figure in its current form rather disturbing.

6) The following plots need to be improved:

a) Fig. 1: I strongly suggest to refrain from scaling arbitrarily to allow an open discussion of the results. For example, the amplitude of O<sub>3</sub> and SO<sub>2</sub> cross sections and the Ring spectra seem to be strongly manipulated in order to downplay their potential impact. I suggest to apply an y-axis in optical density space and scale the cross-sections according to a typical fit.

b) Fig. 6: Please use orthogonal regression for the comparison. Linear regression is not appropriate for independent data sets.

c) Fig. 7: The frequency of the time series is too high to allow a one-to-one comparison. I recommend to also show a zoomed plot of two months or so.

d) Fig. 8: see 6c)

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e) Fig. 10: This plot does not allow an independent judgment whether this is a significant signal or not. Suggested improvements:

- Increase area significantly
- Use full colorscale
- Thicker coast lines
- Align with other geospatial properties: cloud statistics, albedo, precipitation etc.

### Further Comments

(p. 5, l. 16) "Unlike the often-used DOAS fitting method (Platt, 1994), radiances are not ratioed to irradiances, logarithms are not taken, and no high-pass filtering is applied." I wonder whether this is an advantage or disadvantage of the described method. What is the intention behind this statement? My suggestion would be not to confuse the reader and just remove it from the manuscript.

(p. 11, l.17) Why are OMI and GOME-2 data treated differently with respect to spatial averaging? Without discussion, the reader may assume that OMI data are more noisy and needed some smoothing. Please be more specific.

(p. 13, l.21 -> l. 25) -> move to introduction

(p. 14, l. 2 -> l. 99 -> move to introduction

(p.2, l. 29): "briefly analyze" characterizes an approach not suitable for a scientific article. An analysis is either profound or not scientific. In my opinion, an AMT paper should only contain profound content.

(p.3, l. 22): Please add reference documenting the OMI row anomaly: <http://projects.knmi.nl/omi/research/product/rowanomaly-background.php>

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