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Atmospheric Measurement Techniques Discussions

Interactive comment on "Tropospheric BrO column densities in the Arctic from satellite: retrieval and comparison to ground-based measurements" *by* H. Sihler et al.

Anonymous Referee #2

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In this paper, the authors report on a new approach to derive tropospheric BrO columns from nadir observations of UV/visible satellite instruments. The novel aspect of the retrieval is to use a parametrisation of the dependence of the stratospheric BrO column on SZA, O3, and NO2 based only on the measurements themselves. In addition, a sensitivity filter is developed to identify those measurements having substantial sensitivity to the lower troposphere using O4 columns and the reflectivity of the measurements. Based on a large number of radiative transfer calculations, the same observations are also used to estimate the most appropriate airmass factor for the tropospheric column. The data are compared to an AVHRR scene and to measurements from the

CALIPSO lidar to verify the cloud treatment. The resulting tropospheric BrO columns are validated against ground-based observations and very good agreement is found in several cases.

The topic of the paper is very relevant, and in spite of a significant number of publications on the retrieval and application of tropospheric BrO columns from satellite observations, there still is an on-going debate on the applicability and the reliability of these retrievals. The methods presented here are novel and have the potential to address several of the problems present in existing algorithms. The study fits well into the scope of AMT, is relevant for developers and users of polar tropospheric BrO products alike. The paper is clearly structured and generally well written in spite of some repetitions and a few difficult to follow sections. I therefore recommend the paper for publication in AMT once the comments below have been addressed.

Major comments

1. The proposed retrieval approach is impressive and has the beauty of relying solely on the measurements themselves. However, it is also complex and relies on a multitude of more or less arbitrarily selected thresholds, parametrisations, and choices made on scenarios. While most of these seem reasonable to me (although it is often hard to judge), my overall impression is that the method is much more complex than necessary. I had this impression for the algorithm determining the stratospheric mode and the selection of the partitions (does the difficulty of the problem really justify the complexity of an approach which even after reading the text twice carefully I have not yet fully understood?) but is even more obvious in the filtering approach illustrated in Fig. 8. In my opinion, a simple O4 AMF threshold of 3.75 would do as good a job as the proposed 6 parameter model using h, g0, g1, g2, R, and O4 AMF. This becomes obvious when looking at the real data in Fig. 8b. While I cannot judge if there are other scenarios where things are less simple thus justifying a complex approach, I'm convinced that it

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is beneficial for an algorithm to be simple and transparent to the user. I think the algorithm should be simplified or the need for the complexity be shown.

2. Not in all cases, the choice of parametrisations is clear. For example, I'm not convinced that for a daily image of stratospheric BrO columns, the NO2 column is an important parameter. While in Fig. 6 (left), some dependence on the NO2 column is apparent, I suspect that this is really a latitude dependence and could just as well (and simpler) be parametrised by latitude. I agree that reaction with NO2 is a relevant sink for BrO but the NO2 columns measured from satellite are dominated by NO2 altitudes much above the BrO layer making a direct link between NO2 and BrO less obvious. I'm also surprised by the choice of region for determination of the relationship – by including latitudes down to 30N, polar, mid-latitude and sub-tropical regimes are mixed, complicating parametrisation without clear benefit. Please comment on why these parameters and this geographical region were chosen.

3. On a similar note, most of the scenarios in Fig. 8 will never be used as they have too little sensitivity to the surface layer. The remaining scenarios above g are small in number, most of them coming from interpolating values which is a complex step in the procedure and interestingly not well reflected in the real data shown in Fig. 8b. From these few values, a relation is formed linking the AMF 500 to R and A0 using a linear model and "selected scenarios", but at least in Fig. 8, I cannot see a clear pattern that would justify such a model. I'm therefore again not convinced that this complex parametrisation of AMF 500 is adding accuracy to the AMF relative to just using the mean or median value. I think it would be good to show in a scatter plot how the parametrisation of the AMF and the values from the model runs and interpolations compare for the example shown in Fig. 8.

4. The section on validation is not very convincing in its current form although it shows that the algorithm is performing as expected.

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- Testing the column separation algorithm with a purely statistical data set with normal distributed values is in my opinion just showing that the algorithm is properly implemented. The real test would have to be done on BrO, O3, and NO2 slant columns as modelled by a radiative transfer model based on CTM profiles with some occasional BrO events added in the lower 500 m of the troposphere. This would test the algorithm on a (more) realistic ensemble of data which might not follow the idealised assumptions in the test data.
- The comparison with the AVHRR data is again not adding much – reflectivity at 630 nm should have similar information as reflectivity at 337 nm as used in the GOME-2 data and the discussion of the clouds is qualitative at best.
- The comparison with CALIPSO data is very interesting but I'm confused by the quantities shown. Why are there AMFs of 3.5 for both COT of 0 and 1.5? Why is that proving that the algorithm works? What I would like to see in such a comparison is the dependence of BRO AMF on COT and CLT and which of the points were classified as sensitive. If the majority of the points at high COT are classified as possibly obscured, then the algorithm has performed well in detecting cloud shielding over ice as suggested in the abstract.

Minor Comments

- page 3201, "polar regions reach about full coverage. . ." => "at polar latitudes, full coverage is reached. . ."
- page 3207, one point that should be briefly discussed is the fact, that the method implicitly relies on similar vertical profiles of BrO and O3 (when taking the ratios of the SCs).
- page 3210, normalisation of BrO columns – why is this step needed at all?
- page 3211, "depends only slowly" => "depends only weakly"

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- page 3213, “still a reasonable small residual” => “still a reasonably small residual”
- page 3216, if O4 is integrated from sea level to top of atmosphere for the VC, what about elevated regions such as Greenland?
- page 3217, “derivation the” => “derivation of the”
- page 3219, “SZA = 60°” – in the figure caption, it says 66°
- page 3221, discussion of errors – this comes as a bit of a surprise here as it is not discussed elsewhere in the paper
- page 3227, reference to Heue et al, 2011 – as the data sets used are different, this cannot really be seen as validation of this product which is fine tuned for observations of surface BrO in the Arctic

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Interactive comment on Atmos. Meas. Tech. Discuss., 5, 3199, 2012.