Atmos. Chem. Phys. Discuss., 11, C9333–C9337, 2011
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of satellite-derived Arctic tropospheric BrO columns in conjunction with aircraft measurements during ARCTAS and ARCPAC" *by* S. Choi et al.

Anonymous Referee #2

Received and published: 26 September 2011

Choi et al. reports on satellite observations of tropospheric BrO using OMI and GOME-2 and on comparisons with in-situ observations of bromine species during aircraft campaigns. I believe it is high quality work, with a lot of nice interpretations of the results and I found the manuscript rather long but well structured and easy to read. I recommend publications in ACP, provided that the following points are addressed.

-The author uses as stratospheric correction the model-based data introduced by Salawitch et al. (2010) that assumes a very strong supply of Bry from VSLS. There has been considerable debate on these VSLS settings (assuming 9 pptv of Bry in total: 7pptv from PGI +2pptv from SGI) and I'm still not convinced that the resulting

stratospheric correction is appropriate. Indeed the stratospheric vertical columns were not compared to any independent stratospheric BrO observations and it has not been demonstrated that this model-based stratospheric correction could actually correct for the BrO absorption in the stratosphere. Moreover, the author of the present manuscript reported negative tropospheric BrO column in the maps they show which I believe is a sign of an overestimation of the stratospheric correction. However, in the authors' defense, I believe that this aspect is probably not crucial for the comparisons between satellite and in-situ data displayed here. Indeed, they discussed the results honestly and rigorously, using a quite detailed error analysis. The strength of this paper is clearly the link between the aircraft and the satellite data as it brings additional information on the processes involved (e.g., bromine activation after blowing-snow events). Also carefully discussed by the author is the limitation in the use of the aircraft data as in numerous cases the aircraft does not fly low enough to pick up the important contribution of the tropospheric BrO column located close to the surface. Indeed, Friess et al., 2011 showed - using MAXDOAS observations at Barrow - that most of the PBL BrO was actually located very close to the ground, few hundred meters. In its introduction, the author write that Salawitch et al could reconcile the total columns from OMI with the aircraft tropospheric columns and the model-based stratospheric columns using 9 pptv of Bry. How does this finding changes knowing the limitation of the aircraft data (read above) and the fact that the tropospheric AMFs were not treated?

-In section 3.2: I'm not convinced that the algorithm gives useful results for SZA as high as 85°. At these SZA, the photochemistry of BrO becomes important and this can lead to photochemical gradients along the slant stratospheric photon path. More importantly, the tropospheric column accuracy is controlled by the AMFstrato/ AMFtropo ratio. Any error on the stratospheric correction will be amplified by this factor. In this respect, the SZA threshold of 85° given is too optimistic. E.g., it is difficult to believe that the enhanced tropospheric BrO column values North of Greenland (Fig5) and on other figures (8,9,10) are real (no enhanced values in the corresponding total columns). If one calculates a realistic error on the tropospheric columns, one would certainly find huge values there. Personally I find deceiving not to see a plot displaying WFs for thin cloud and high albedo conditions and showing that even for cloudy conditions we have good sensitivity to PBL BrO (provided of course that the cloud optical depth is not too high). It would be convincing and more useful than the results of Figs 3 & 4 that are very well known by people retrieving tropospheric constituents in the UV.

Minor comments:

Introduction:

-I found the list of past studies on BrO remote-sensing nice. The author might want to complete it by referring to recent works of Prados-Roman et al., AMT, 2011 and Friess et., JGR, 2011 (in press).

-P8, l 6-7: the author should be more precise as for the "best case" of Salawicth et al. and for (same page, l8) "quite a bit of geographical variability". It is rather vague.

Data description:

-Binning the profiles on 500 m thick layers and then taking the median mixing ratio as representative at the altitude can potentially leads to a problem: the median as a tendency to be less affected by outliers than the mean calculation. This could be an issue for very high near-surface BrO mixing ratio values (bromine explosions, the 'outlier' in the profile), leading to a likely underestimation of the tropospheric BrO in-situ columns for these conditions (precisely what you want to avoid). What is the original vertical sampling (m) of these BrO profiles?

-The time (UTC) in the Tables often exceed 24:00. It is weird.

-P12: The author refers to Tables 3, 4a and 4b, but the satellite data are not introduced yet. It would be better to use section 2.2 for Tables 3 and 4.

-Fig 7 & 8: the 5th and 8th April DC-8 flights were not mentioned in the section 2.1. They should be added.

-P13: The author probably forgot to mention that O3 is also an absorber considered in the OMI BrO retrieval..

Section 3.1: I don't understand why the AMFstrato are not calculated using equation 8 applied to the simulated stratospheric BrO profiles. This would have avoided an unnecessary source of error on the tropospheric column (not discussed by the way). Error on stratospheric AMFs due to this could be as large as 10%

Section 3.2: Figure 5: the results over the Barents Sea: I feel unsettled by the results because it is dangerous to say anything about the albedo effect here. Indeed the fact that you see high/low BrO values over high/low albedo scenes can always be perturbed by the fact that this could be real (emissions over sea-ice regions).

Section 3.3:

-Why does the author not consider any systematic error on the slant columns?

-P22, l 22: the author states the BrO+NO2 rate constant is uncertain by a factor of 2 without giving any reference. Sinnhuber et al (2002) gives a much smaller uncertainty ($\sim$25%).

-The uncertainty on the stratospheric column due to VSLS uncertainty is likely much higher than 27% especially for low tropopause conditions.

Section 4.1.1

-Fig 6a, P24, l4-8: The patterns in the maps are very dependent on the color bars. The one used (same for total and tropospheric columns) is a bit misleading.

-Fig 6b, P25, l1: There are indeed low values of derived tropospheric columns over thick clouds but it seems that there are also high values (although it is not easy to see from Figure 6). Maybe the author should investigate this a bit more.

Section 4.1.3

-P27, L14: It seems that the GOME -2 are shown on Fig 9a. Is there a mistake in the sentence? “(not shown)”

-Fig 9b, P28, L 13: It is maybe good to recall that the minimum height of the aircraft was 151 m (according to Table 2b) and that is likely that the aircraft miss an important part of the BrO profile.

Conclusions:

-P33, l 13: The author gives thresholds for the albedo (>0.8) and SZA (<80°) that are different from the values given in the manuscript.

-P33, l27: The author should be cautious in the conclusions. Although it is true that the variability of stratospheric BrO is such that it can modify our interpretation of total columns maps, I think that at high SZA (>75°) the error bars are large and the regions of enhanced tropospheric BrO not apparent in maps of total columns should be treated carefully.

Typos: -P10, l14: “miunte”→ “minute”

-P12,l24: what is ‘UV-2 pixel size’? do you mean OMI pixel size?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26173, 2011.