

Interactive comment on “New Aura Microwave Limb Sounder observations of BrO and implications for Br_y” by L. Millán et al.

Anonymous Referee #3

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Review of New Aura Microwave Limb Sounder observations of BrO and implications for Br_y by Millan et al., MS No. amt-2011-199

This paper reports new retrievals of BrO profiles from the Aura MLS instrument and interprets these observations in terms of how much bromine is supplied to the stratosphere by very short lived substances.

I believe the paper could be acceptable for AMT after major revisions. The article falls short of adding much to our knowledge of atmospheric bromine in its present form.

According to the AMT website, the main subject areas of this journal are the development, intercomparison and validation of measurement instruments and techniques of data processing and information retrieval for gases, aerosols, and clouds. The submit-

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ted paper falls particularly short in the areas of intercomparison and validation.

Most of the paper is devoted to a description of the new BrO retrieval. This is overall solid but still needs work before it can be considered up to the norm for AMT. It looks like the grey line for retrieval is clipped at ~ 1 hPa for the lower left hand panel of Figure 5. Most importantly it is stated that “throughout most of the profile, the main source of systematic bias arises from retrieval numerics. While unsatisfactory this is expected due to overlapping O₃ signals in contrast to the small BrO signature . . .” (page 331). In other words, I think, the authors are stating that interference from O₃ is a major limiting factor. But Figure 5 contains a line for Contaminant species errors, and the uncertainty for shown by this line is small. So, I am honestly quite confused as to whether, or not, uncertainty in overlapping O₃ is driving the large bias shown by the grey line in Figure 5. If so, the decision to use average T, O₃, HNO₃ (page 329) from the standard product, and apply to the averaged radiances, needs a much more thorough consideration. Upon revision, need a more thorough description of the grey line in Figure 5 and, if overlapping gases are an issue, should calculate how uncertainties in each overlapping species impacts the BrO retrieval. If it is not overlapping species, then need to explain what exactly is meant by retrieval numerics.

The real weakness of the paper is “intercomparison” and “validation”. Essentially there is no validation despite the fact that over the time span of Aura there have been a number of balloon-borne measurements of stratospheric BrO (i.e., see for instance <http://www.sciamachy.org/products/index.php?species=BrO&subspec=BrOp&institute=IUP>).

The “intercomparison” in the paper is presented entirely in Figure 7, which is qualitative at best. Upon revision, would like to see some accounting for differences in local solar time of observations. This is straightforward to accomplish; see for example Appendix A of: <http://www.agu.org/pubs/crossref/2006/2005JD006479.shtml>) Once this is done, a scatter plot including correlation coefficients, estimates of mean offsets, etc. is needed.

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The main science result of the paper, an estimate of Bry from VLSL, is at best a “fuzzy message” because:

1) between about 20 and 50 hPa, the Bry values inferred from MLS agree quite well with the lower limit for Bry, represented by the blue WACCM curve (WACCM neglected VLSL Bry) whereas at altitudes above ~ 10 hPa, the MLS value of Bry agrees quite well with the green SLIMCAT curve (model that includes significant Bry from VLSL). This “jumping” of MLS Bry from one curve to the other is not discussed. Of course, it is hard to interpret physically, but this “jumping” would lead many readers, including me, to question whether the new retrieval is adequate to use for quantification of VLSL Bry. Or perhaps the MLS team believes they have pushed forward our understanding of the shape of the BrO profile. Regardless, the shape of inferred Bry should be addressed. Had balloon-borne BrO profiles been part of the analysis, we could possibly assess whether the shape of BrO profile reported by MLS is realistic.

2) the value for VLSL Bry is leveraged to an estimate for the tropospheric burden of bromine from Montzka et al., 2003. The difficulty in using a tropospheric CH₃Br time series is that, due to its short tropospheric lifetime, CH₃Br at the tropopause is almost certainly lower than CH₃Br at the surface. This ~ 10 to 15% difference between surface and tropopause level CH₃Br, which is well known to the aircraft measurement community, is neglected here (as well as many other studies). But it is likely important for the accounting that is being attempted

3) uncertainties in the inference of Bry from BrO are similarly neglected. Nearly all other papers on this subject examine the uncertainties of chemical kinetics and J values, including (but my no means limited to) the aforementioned Sioris et al. paper.

END OF REVIEW.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 325, 2012.

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