Review of Theys et al., "Global observations of tropospheric BrO columns using GOME-2 satellite data", ACP-2010-767

The manuscript presents the derivation of tropospheric observations of bromine monoxide from the GOME-2 instrument, drawing on techniques previously by the same group for removing stratospheric BrO loading from total column measurements. The paper is well written and the approach is fundamentally sound. The overall quality conforms to the high standards we have come to expect from the BIRA group. The topic of tropospheric BrO loading is important, and the manuscript is timely, particularly in light of recently published work regarding stratospheric bromine loading. It is well suited for publication in ACP.

In general, there is little to be found at fault with the manuscript, aside from the over-interpretation of snow-blowing events. This as already been addressed by *Anonymous Reviewer #1,* and I second that criticism. In addition, I want to point out that Figures 8 and 13,particularly where they show monthly averaged GOME-2 BrO in the Antarctic during October 2007, seem to fail to support the snow-blowing hypothesis: Strongly elevated BrO in the observations is almost entirely limited to coastal and off-coastal areas, while p-TOMCAT results show no such localization. If snow-blowing events were an important contributor to elevated BrO, would one not expect a more even distribution between on- and off-shore regions around the coast or sea ice edge?

The methods for separation of stratospheric and tropospheric BrO, as well as the spectral retrievals of BrO from GOME-2 observations are sound and accurately described. On a side note that has practically no impact on the retrievals, I would suggest to the authors to replace the Meller HCHO cross-sections with those of Cantrell scaled by 0.9. This is the current recommendation of the HITRAN advisory board, and the authors may find Cantrell to yield slightly lower RMS values compared to Meller. However, the impact of this change on the retrieved BrO columns is in all likelihood negligible and does not warrant a reprocessing of the data for the analysis presented in the manuscript.

Some specific comments, mainly of cosmetic nature, in the order they appear in the manuscript:

1. Page 3 Line 21: "OMI/Aura"

2. Page 5 Line 11: "poleward of ±45° latitude"

3. Page 10 Line 10: "smaller"

4. Page 16 Line 7: "data for southern hemisphere winter time"

5. Page 16 Line 30: "for the polar spring period"

6. Page 17, Lines 6 and following: The authors are noting the excellent correlation of elevated BrO with sea ice extent. Is it possible to provide a quantitative estimate on how sea ice edge effects contribute to bromine explosions, compared to snow-blowing events?

7. Page 18 Line 8: "can have stratospheric origin"

8. Page 20 Lines 7/8: "frequency is 20%"

9. Page 20 Line 21: "BrO production"

10. Page 21 Line 6: "(temperature, illumination, availability of surfaces for heterogeneous reactions, *etc*.)"

11. Page 21 Line 21: Does the model include bromine emissions from the above-mentioned heterogeneous reactions, *e.g.*, interactions of of ocean water with fresh ice surfaces?

12. Page 22 Lines 5/6: What is "a strong indication that [snow-blowing] probably plays an important role" supposed to mean exactly?

13. Page 22 Line 12: "it can reinforce"

14. Page 22 Line 25: As it stands, Figure 13 does not "confirm the existence of the mechanism of bromine release from blowing snow events"

15. Page 38 Figure 4: What exactly is plotted, Air Mass Factor or Weighting Function? To show the vertical distribution of the response of BrO to atmospheric composition, it may be more illustrative to plot Scattering Weights which, integrated over altitude, give the AMF.

16. Page 39 Figure 5: This figure wastes some real estate. I suggest limiting the y-axis plot range to 2-8x10^{13} mol/cm^2 to provide more detail and dynamic range.

17. Pages 40/41 Figures 6/7: If space permits, I suggest adding a panel of BrO total VCD to the comparison. This should help identify where, if any, discrepancies exists between ground-based and satellite observations that manifest in differences of stratospheric and tropospheric VCDs as shown in the two panels of the figures.

18. Page 44 Figure 10: The inverted color scale of the "Tropopause height" row makes the visual comparison to the BrO columns more difficult than it needs to be. I suggest either inverting the color scale or plot "Tropopause pressure".

19. Page 45 Figure 12: The legend in the plots is too small to read. I suggest to move the legend to a separate panel below the figures, with enlarged font size.