The authors are grateful for Referee #2's time and constructive comments and the appreciation of our work. In the following, we address his/her comments. Please, note that specific details on the MAX-DOAS and inversion technique has now been included on a Supplementary Material.

The Referee Comments are in black and Authors Comments in blue. References to pages and lines of the draft are indicated with "P" and line "L", respectively.

Specific comments

MAX-DOAS analysis

The authors claim that BrO is not present in significant amounts above 2 km based on their ground-based measurements, where they retrieve BrO vertical profiles from 0-6 km. I am skeptical that ground-based MAX-DOAS measurements can be used to make this claim. For what it is worth, I am skeptical that the prior studies cited could actually observe BrO at those altitudes as well. The information content outside of the lowest elevation angle measurements simply isn't high enough. The authors should present averaging kernels showing that the measurements are sensitive to changes in BrO above 2 km if they are going to make this claim. I also think the presented vertical profiles should also be limited to 2 km unless the averaging kernels show that a higher altitude is merited.

Tests performed with the ratiative transfer confirm the sensitivity of the technique to up to 4 km and 2.5-3 km (aerosols and BrO, respectively). We kindly ask the referee to see the Supplementary Material (Sect. 3) and also our responses to the General Comments 1.1. of Referee #1.

Sea ice conditions between the two sites

I think the author's points about needing to examine the sea ice conditions at both sites and the heterogeneity being potentially linked to sea ice differences is a good one. However, I think simply describing the sea ice around Marambio as seasonal without providing further detail is potentially misleading, as the ice toward the outside edge of the sea ice in the Antarctic is often the oldest sea ice (excluding the "permanently" sea iced sections surrounding Belgrano) (Nghiem et al., 2016). This older sea ice is likely lower salinity than the newer sea ice regions closer to the coast. These differences may impact the overlying snowpack, which is the likely source of the reactive bromine. Of course the proximity of this older ice to open water may also lead to enhanced snow salinity due to sea spray aerosol deposition (e.g. May et al., 2016). In any case, I'd like to see the authors add a more detailed discussion of the sea ice conditions at the two sites.

Indeed, although the mentioned work of Nghiem et al. (2016) states that the "Antarctic sea ice is dominated primarily by first-year sea ice" which is often linked to bromine explosions (e.g., Simpson et al., 2007), dedicated investigations into the link between the properties of the sea ice and the composition of the tropospheric in Antarctica are definitively worth considering (although not easy to undertake from the logistic point of view...).

With regard the data we present in this work, we have now added a new figure (Fig. 9) with closed-up views of the sea ice conditions nearby Belgrano and Marambio on the selected days of Fig. 8 (from end of September to end of November). Note that this new figure does not substitute Fig. 1 since the later provides the reader with a quick overview of the locations of both stations within Antarctica (something missing in this new zoomed in figure). Note that, as stated on the caption of the new figure, "in barely 1 month (25th September - 29th October) the sea ice surrounding Marambio underwent strong transformation, going from medium/highly concentrated sea ice in September and with barely permanent open waters (upper left figure), to pretty much complete open ocean (disappearing all the sea ice beyond 50° W). During the timeframe of that sea ice transformation, BrO VCD_{2km} peaked at Marambio (Fig. 4). Also, note how the edge of the sea ice nearby Belgrano transforms towards November (e.g., lower right).".

Page1, Line 41: This sentence should have references for these impacts of atmospheric halogens.
As appearing in P2 L4 of the initial draft, the reader is kindly directed to the compendium work of Simpson et al. 2015 on "Tropospheric Halogen Chemistry: Sources, Cycling and Impacts" and the studies referred to in it.

P5 L32: What percentage of the retrievals has a DOF larger than 1?
Details on the DOF are now provided in the Supplementary Material (and indicated on P6 L1 of the new draft).

P5 L40: A summary of the degrees of freedom for the BrO retrievals should be presented here as well.
It is now included in the Supplementary Material.

P6 L39: Can you state the differences in AOD between the two sites more quantitatively?
In the new draft (P7 L14-15) now we add "At Belgrano, 62 % of the AOD_{2km} was lower than 0.05 (12 % between 0.05 and 0.1) compared to 90 % of AOD_{2km} at Marambio that was below 0.05". For clarity, the subscript "2km" has been added to AOD in the vertical axes of Fig. 3 (i.e., AOD_{2km}). Please note that, as mentioned on our draft (P7 L7 of the initial draft), a forthcoming publication will focus on the aerosols observed at the two stations (Gómez-Martín et al. 2018).

P7 L11: 0.8×10^{13} molec cm^{-2} isn't a range as presented. Please clarify, is this a standard deviation?
"range" is changed to "value" on P7 L25 (i.e., 75% of the data BrO VCD2km were < $0.8\cdot10^{13}$ molec cm^{-3})

<u>Suggested figure modifications:</u>

1. Figure 3, 4: I don't really think it is necessary to shade regions without data. It gives the figure a cluttered look.

We appreciate the referee suggestion. However, we do consider that by including the "shades" the reader can have an overview of the amount of data contained in this work at two different sites. Note that by excluding the shaded regions the reader could interpret that we did have MAX-DOAS data throughout the year. We would like to clearly state (not only in the text) that there were periods of time with no MAX-DOAS data (either due to instrumental issues or to SZA > 75°, this now clarified in the caption of both figures and throughout the draft (P7 26-27, P9 L1, P10 L19, L26, L34, and in Figures 3, 4, 6, 7, 10 and 12-14).

2. I think just showing Fig. 6 is sufficient, and the timeseries of wind speed (Fig. 5) isn't really needed.

We agree. In the new draft the Fig. 5 is deleted and the wind roses plots binned by wind speeds so the information is not lots. Please, see our responses to the General Comments (1.2) of Referee #1

3. Fig. 7: Consider plotting both ozone series on the same panel so one can clearly see the differences between the two sites.

Similarly to most of other figures of the work where the information is differentiated between stations (i.e., one plot per station), we'd rather plotting the time series of ozone also in a two panel manner. Moreover, it would be quite complicated to distinguished features when plotting the O3 time series in one single panel (note the noisy data of Marambio, for instance). Also, please bear in mind that we have now added the time periods with MAX-DOAS observations at each station (making the “one-plot” way even more complicated to understand).

4. Fig. 8,9,10: As I suggest above, the portion above 2 km should be cut unless you can demonstrate that your retrieval is sensitive to the true atmospheric state at higher altitudes.

Referee #2 is kindly referred to the Supplementary Material and to our responses to the General Comments (1.2) of Referee #1.