Response to Anonymous Referee #2:

We would like to thank Anonymous Referee #2 for his/her careful reading of this manuscript and for his/her comments and suggestions. We have addressed all of his/her comments in the revised manuscript and below in the order in which they were raised. All page numbers and line numbers are in reference to those in the revised version of the manuscript, except where indicated otherwise.

Page 30237 around line 15: true, but there must be salinity in these snowpacks; this point should be made.

-It was made clearer on p. 4, line 87 that the snowpack must be a saline snowpack.

Page 30240 line 6: spell out what you mean by "long-term" – these are of the order months, not years.

-The sentence on p. 8, lines 159-161, was clarified to emphasize that these are several month measurements. We also now state in Sect. 2.1 (p. 9, lines 180-181) that we focus mostly on springtime and early summer data, though we do possess data for some deployments during fall, winter, and summer months.

Page 30242 line 19: Fayt et al is not in the reference list

- Fayt et al is now included in the reference list on p. 36 lines 788-789.

Section 2.3: I find this section quite confusing in the way that it's written. I think I know what you've done, but it's hard to extract that information from the text. Surely you've run

the trajectory according to the duration of the observed ODE. You should then calculate the spatial scale according to the distance between the start and end point of the trajectory. The text (and figure caption for Fig 4) talk about determining the ODE spatial dimension by calculating the maximum distance between any two points along the trajectory. The points along a trajectory are arbitrary, and determined by the resolution of the output. Do you mean that you integrated all these distances..? That would be fine, but it's not what I see shown in Fig 4. Please carefully clarify this section. Also, this entire analysis rests on the observed ODE reflecting transport, rather than local chemistry. Please make this point in the text.

-It is correct that the trajectories were run for the duration of the observed ODE. The referee's assessment of how we determined the spatial scales is also correct. We determined the spatial scale by calculating the maximum distance between any two points along the trajectory (referenced henceforth as Method 1 here), as this should represent an upper limit to an event's spatial scale. We have added text to make this clearer (p. 13, lines 285-291). In addition, we performed the analysis according to the referee's suggestion using the start and end time of the isobaric trajectories (henceforth referenced as Method 2 here) and found that the distribution stayed essentially the same, with the majority of the distribution laying between $10^{2.75}$ – $10^{3.25}$ km (562 – 1778 km) in both cases, specifically, 74% and 66% for Method 1 and Method 2, respectively. Additionally, we find the means of Method 1 (1013 ± 379 km) and Method 2 (947 ± 238 km) to be equivalent at the 95% confidence level. We have added a sentence in the main text (Sect. 3.2, p. 24, lines 531-532) that presents the means of this analysis in comparison with the other spatial scale estimation methods. We have also clarified that this analysis must rely on transport mechanisms dominating (Sect. 3.2, p. 23, lines 508-512).

Page 30247 line 6: amend the text to read "38 ODEs were observed between the months of February and June" – because they were not all in the same year.

-This has been corrected, on p. 16, lines 343-344 of the revision. We have also added the number of events observed by each O-Buoy to Table 1 on p. 48.

Page 30247 line 10 – it would be good to show an example plot for 1 case

-We have included an example plot for the case presented in Fig. 3 (p. 53).

Page 30247 line 14 – that the lifetime of O3 reached 14 days is not shown in Fig 6a, as this information is embedded in the bar >50 hours. Adjust.

-This has been corrected on p. 16, line 352.

Page 30247 line 18 – ranging from 0.24 to 7

-This has been corrected on p. 16, line 356.

Page 30247 line 20 to 25 – you write what the >50 hour events are not due to – could you suggest why they might be so extended..?

- A likely cause for these extended events is poor vertical mixing in the absence of frontal passages. Recent work by Moore et al. (2014) (now cited in the revised manuscript, p. 41, lines 911-913) provides evidence of coastal O_3 recovery to background levels when air passes over open leads, hypothesized to occur due to increased convective mixing. So, a long depletion event would require relatively quiescent conditions, without convection from upwind leads, or vertical mixing from, e.g. frontal passages, wind shear, etc. We have added this discussion on pp. 16-17, lines 363-368.

Fig 6: this analysis depends critically on how the ODE start/end time is defined. Did you try other definitions? How sensitive is your result to the choice of definition? Also, please make the legend box on the figure larger!

-In our analysis, we make the assumption that O_3 depletion primarily occurs as an exponential decay. Therefore, the ODE timescales are derived from the slope of the regression between –ln[O_3] and time (p. 30247, line 7-10 of the ACPD manuscript). To isolate the most linear portion, we analyzed the 10-90% of the O_3 concentration range. Because we analyzed the slope, our analysis is mostly independent of the ODE start / depletion stop time. We have added a statement in the text to clarify this point at the beginning of Sect. 3.1 (p. 16, lines 349-351 of the revised version). Additionally, we have also made the legend box in this figure larger (p. 56).

Section 3.1 – it troubles me that much of the work in this section refers to work by Stephens that is either an PhD thesis form (and thus very hard to get hold of) or in un-published papers. Have these papers since been published? In particular, the number 3.6 is critical to this paper, and the reader has no way of independently checking it.

-The Stephens et al. (2014) (now Thompson et al. 2014) manuscripts are not yet submitted. However, the factor of 3.6 (now 4.1 per model revision) is not critical to the analysis or the conclusions. Estimating the required BrO mole fractions for the observed τ_{O_3} using Eq. 2 (p. 30248, line 23 of the ACPD version of the manuscript), derived from already published manuscripts, yields a distribution of required BrO mole fractions with very high values. To further investigate this, we included the factor of 4.1 from the yet unpublished model results of Thompson et al. (2014), which compares chemical O_3 loss from Eq. 2 and Eq. 3 in the ACPD version of the manuscript (now Eq. 3 and Eq. 4). Thompson et al. (2014) and the published Liao et al. (2012, 2014) (now cited in the revised manuscript: pp. 40-41, lines 887-897) showed that Br atom production from Br_2 production is more important than from BrO reaction with BrO and/or ClO. Thus we needed to make two points - 1) the amount of BrO needed to explain the results (assuming local scale chemical mechanisms (CM)) is much smaller when including the factor 4.1 (now clarified on p. 19, lines 424-431; this value comes from Thompson et al., which is why we cite it), which accounts for the importance of Br_2, which is now established in the literature, and 2) as described on p. 20, lines 443-450 of the revision, even when taking this more important source of Br atoms into account (via Eq. 5), most of the observed BrO is below that required from Eq. 5, indicating that CM (as currently best understood) cannot account for the observed distribution of depletion rates. There are thus no conclusions in the paper that rely solely on the as yet unpublished Thompson et al. data. Note that Fig. 6 (p. 56) was updated to reflect the updated factor from the Thompson et al. (2014) model, though the updated estimated BrO levels did not change significantly.

Page 30249 line 24 – how sensitive are the results to the assumption that temperature was 248K?

-For $k_{BrO+BrO}$, the rate constant decreases from 3.8 x 10^{-12} molecules cm^{-3} s^{-1} at 248 K to 3.5 x 10^{-12} molecules cm^{-3} s^{-1} at 273K, a decrease of ~8%. For $k_{BrO+ClO}$, the rate constant decreases from 8.2 x 10^{-12} molecules cm^{-3} s^{-1} at 248K to 7.6 x 10^{-12} molecules cm^{-3} s^{-1} at 273K, a decrease of

~7%. Therefore, temperature has a fairly minimal effect on this analysis. We have expressed this result on p. 19, lines 419-423.

Page 30251 line 15 – keep references to unpublished work to a minimum. Unless now published, remove the Stephens et al 2013a reference here, and use the Saiz-Lopez et al 2007 Science paper (Science, 317, 348-351, 2007). Certainly in the discussion be- low (line 16 onwards), use the Saiz-Lopez paper, where they report a 4-fold increase in calculated surface O3 loss rates when including IO as well as BrO in their photo- chemical box model. This is equivalent information to the Stephens et al paper, but it is already peer-reviewed and published.

-As suggested, we have amended this portion (p. 21, lines 461-464) to include the Saiz-Lopez reference and have removed the Stephens et al. (2013a) reference from this section.

Page 30251 line 25 – state that it's the enhanced salinity of first year sea ice that could be the reason for enhanced chlorine...

-This has been clarified on p. 21, lines 469-471).

-Page 30253 line 22/23 – Fig 8 shows that the median was 908km, not the mode

-The figure was correct. This has been corrected in the main text (p. 23, line 518).

Page 30253 line 28 – amend text to "The results presented here..." rather than "These results..." which could be taken to mean the results of Ridley et al and Jones et al.

-This has been corrected on p. 24, line 523.

Page 30254 line 6 – 341 km

-This has been corrected on p. 24, line 529.

Fig S2 – the paper talks, throughout, of the 17 events, but Fig S2 shows 18...

As shown in Table 1 (p. 48), nineteen events were observed between the O-Buoy1 2010 and O-Buoy2 deployments. We excluded one of the events because its spatial scale was undefined (Sect. 2.3, p. 13, lines 281-283). The second event excluded was larger than the area of interest, as noted in Sect. 2.4 p. 30246, lines 18-22 of the ACPD manuscript, p. 15, lines 331-332. However, Fig S2 does include this event, and this has been clarified also in both the Supplement (Sect. 2.4, p. 3-4, lines 67-71 of the Supplement), and the Fig. S2 caption (p. 13 of the Supplement).

Page 30256 line 2 – which OB1 deployment, 2009, 2010 or both?

-It was for the O-Buoy1 2010 deployment. This has been clarified on p. 26, line 585 of the revision.

Fig 10 – what would the wind rose look like for air masses with no ODE? i.e. is it the wind direction per say that matters, or the sea ice conditions which the air passes over..?

-We have added a third wind rose plot to Fig. 10 for the non-ODE air masses (p. 60). While the ODE and MODE cases have slight preferences for northern or eastern winds, the non-ODE cases do not appear significantly different from the ODE and MODE cases. As recently presented by Moore et al. (2014), it is possible that O_3 recovers in some cases when air passes over open sea ice leads due to convective mixing, and air that passed over unbroken ice was more often O_3-depleted. We have added this discussion to Sect. 3.2, beginning on p. 27, lines 609-612.

Section 3.3 paragraph 1, and fig 11 - Did you filter your temperature data according to wind speed, so that you looked only at temperatures when wind speeds were low? i.e. to remove temperature data when depleted air masses were being transported, and to focus in on local depletion conditions. Also, why were average temperatures examined from Hysplit trajectories..? Surely you need to look at the extremes, i.e. the minimum temperatures that the air mass experienced. This information is lost when you calculate averages. Also, state the height of the trajectories – were they all close to ground level..?

1) We attempted to examine cases in which local wind speeds ≤ 2 m s^{-1}, but only four events satisfied this condition. Three were observed by O-Buoy1 in the Beaufort Sea, and the fourth by O-Buoy3 in Hudson Bay. Characteristics were contradictory (depletion timescales, temperatures), and there were not enough events in similar locations/conditions to draw meaningful conclusions or warrant unambiguous discussion.

2) We analyzed the average temperatures using HYSPLIT because we believe it was important to gain a sense of the overall temperature experienced by the air mass, i.e. without biasing the analysis to fit our hypothesis. However, at the referee's suggestion, we analyzed the minimum temperatures both from HYSPLIT and from the O-Buoy temperature probe. For the HYSPLIT temperatures, the median minimum temperatures are 250 K, 254 K, and 255 K for the ODE, MODE, and non-ODE cases, respectively. Similarly, the median minimum temperatures observed at the O-Buoy are 251 K, 253 K, and 257 K for the ODE, MODE, and non-ODE cases, respectively. In both cases, only half of the events were observed with minimum temperatures

less than the eutectic temperature of NaCl (252 K). The results for the ODE and MODE cases are included and discussed in the revised manuscript (Sect. 3.3, pp. 28-29, lines 631-638).

3) We used isobaric trajectories with a starting height of 10 m (p.13, lines 280-281). Concerning heights throughout the duration of the trajectory, all but one of the trajectories stayed near the surface (≤ 200 m above ground level). The outlying trajectory (ODE occurred during 2009 O-Buoy1 at Barrow, AK) traveled above 800 m above ground level and likely did not represent near surface air characteristic of ODEs; this event was therefore excluded from all HYSPLIT analyses, as discussed in Sect. 2.3 pp. 13-14, lines 291-295. We have updated relevant figures to exclude this event, including Fig. 8 (p. 58), Fig. 11 (p. 61), and Fig. 12 (p. 62), as well as central tendency values (p. 2, line 37; p. 23, line 518; p. 24, line 528; p. 28, lines 625-628, 634-635; p. 29, lines 658-659; p. 30, line 680; p. 31, line 694).

Section 4 – update the conclusions depending on what you find when addressing the issues raised above.

-In addressing the issues raised, we discussed the new insights gained throughout the text, and amended the conclusions to reflect these changes. We sincerely appreciated the suggestions Anonymous Referee #2, as they have significantly improved the readability of the manuscript.