Response to Anonymous Referee #1:

We would like to thank Anonymous Referee #1 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.

1) **The ODE definition (section 2.2) could be presented in a much more compact way by saying that ODEs and MODEs are defined by O3 falling below thresholds of <15 ppb and 10 ppb, respectively, for > 1 hour. Starting times are defined by O3 levels falling below 90% . . . (ODE) or below the threshold level, respectively while stop times . . . All remaining information (including the review of ODE definitions by other authors) could be transferred to the supplementary material.**

-Section 2.2 (beginning p. 12, line 260) has been rewritten more concisely according to the referee's suggestion. The remaining material has been moved to the Supplement.

2) **Section 2.4 could also be shortened by saying that there are basically to (extreme) explanation for ODEs: (1) Advection of already depleted air (dynamic hypothesis, DH), (2) in-situ chemical destruction (chemical hypothesis CH), of course also combinations are possible (and in fact likely). Throughout the manuscript these two hypotheses are frequently mixed, which is rather confusing. For instance all arguments made about the size of ODEs rest on the DH, while the discussion about measured BrO-levels being too low assumes the CH being correct. The DH and the CH in their pure form are mutually**

**exclusive (unless one assumes some combination, but this is not attempted in the manuscript) and this should be clearly said. Since it may be impossible from the data to decide which hypothesis is correct it is of course warranted to study both under the headlines "assuming the DH being correct we can conclude . . ." (e.g. conclusions about the spatial extent of ODEs can be drawn) and "assuming the CH being correct we can conclude . . ." (e.g. about the level of BrO and other halogen species), respectively.**

We did note in the original manuscript that observed $O_3$ depletion can be some combination of local, in-situ chemistry, and the advection of air already depleted in $O_3$, but the extent to which each mechanism dominates is unknown based on this data set (e.g., p. 30235, line 10-11; p. 30247, line 11-13 of the original manuscript). But, we like the referee's suggestion, and so we have incorporated two abbreviations throughout the main text in order to make our assumptions clearer for the analyses performed (introduced in the revised manuscript in Sect. 1, p. 5, paragraph beginning on line 91): CM (chemical mechanism(s)) and the TM (transport mechanism(s)) (analogous to the referee's suggested "chemical hypothesis (CH)" and "dynamic hypothesis (DH)"). It is now noted more clearly when one scenario dominates the other, e.g.:

i) p. 17, line 384-386: we state that the we explore the observed $O_3$ decrease timescales for the limit of a dominant CM and minimal TM for this section.

ii) p. 23, sentence starting on line 508-512: we state that the spatial scale and Monte Carlo analyses in this section are performed for the limit of a dominant TM and minimal CM.

3) **Section 2.4, Monte Carlo "Experiment": The justification and usefulness of the Monte Carlo study (or numerical experiment) does not become clear, in particular, why do the Monte Carlo numerical experiments "provide statistical support" (page 30246, line 6) to**

**the DH? The description of the Monte Carlo numerical experiments could be deleted altogether or moved to the supplementary material. Likewise Fig. 9 does not appear to provide much information and could be deleted or moved to the supplementary material.**

The Monte Carlo experiment is presented as a simplified thought experiment. That is, if we have large $O_3$-depleted air masses (per the estimated size distribution), this presupposes that large portions of the Arctic are at least partially depleted of $O_3$, and thus large areas could be conducive to $O_3$ depleting chemistry. For example, (as now stated on pp. 14-15, lines 316-318 of the revision) in the limit of an ODE the size of the Arctic Ocean, it is impossible for this ODE to be observed primarily due to TM. The question is then raised whether, given our distribution of ODE sizes, it is statistically reasonable that we observe such large events and are not in the presence of significant amounts of in-situ chemistry (i.e. is it a possible scenario that all observed events result from a TM mechanism, and the local CM is minimal?). The results of the Monte Carlo simulation do not provide evidence on whether TM mechanisms dominated over CM mechanisms in our observations, merely that this scenario is not implausible (given our size distribution). We have made these points clearer in Sect. 2.4 (beginning p. 14, line 313) of the revision, and maintain that this analysis is useful to the discussion that the TM is not inconsistent with the observed depletion spatial scales (as discussed in Sect. 3.2, p. 25, paragraph starting on line 563).

4) **Section 3.1, On page 30249 the authors state that the measured BrO levels lead to an underprediction of the rate of O3 loss by a factor of 3.6 (on average). Is this finding not a clear indication that the CH is wrong and the DH correct? This point should be discussed**

-We believe this is a misunderstanding and have clarified this discussion in the text (paragraph beginning Sect. 3.1, p. 17, line 384 of the revision). The factor of 3.6 (recently updated to 4.1 based on model revision) is obtained from a regression between two different calculations of $d[O_3]\ dt^{-1}$ from Stephens et al. (2013) (now Thompson et al. (2014)): the net $O_3$ chemical destruction rate (Eq. 4 in the revised manuscript), and Eq. 3 of the revision (based on Le Bras and Platt (1995) and Platt and Janssen (1995)). It was found that if we used only Eq. 3 to estimate the $O_3$ depletion rate, we could be underestimating $d[O_3]\ dt^{-1}$ by a factor of 4.1 as a result of neglecting other chemical mechanisms accounted for by Eq. 4. In other words, this is a comparison of two different calculations of the magnitude of $-d[O_3]\ dt^{-1}$, assuming CM dominates. That Eq. 3 underestimates the net $d[O_3]\ dt^{-1}$ is also consistent with Liao et al. (2012) and Liao et al. (2014) (both now cited in the revised manuscript: pp. 40-41, lines 887-897) who report that the $O_3$ loss rate due to the BrO self reaction amounts for only 35% of the loss rate due to $BrO + HO_2$. Liao et al. (2012) report significant $Br_2$ concentrations, which produce Br atoms via photolysis, a process that competes very favorably with that represented by Eq. 3. Note that Fig. 6 (p. 56) was updated to reflect the altered required BrO numbers based on the updated factor from Thompson et al. (2014), though the updated BrO values were not significantly different from before.

5) **Page 30252 and 1st para of page 30253: The attempt to "potentially test for missing chemistry" should be deleted in view of the fact that the CH is probably not correct (see point 4, above).**

-Given that point #4 resulted from a misunderstanding, we have left this paragraph (pp. 21-22, paragraph starting on line 474) within the revised text.

6) **Section 3.3 describes interesting conclusion, it is convincingly written and should be retained, but shortened. For instance the text on page 30258, lines 14 to 24 could be replace by saying that the same analysis as for the T-dependence was performed for wind speed.**

-This section was rewritten in a more concise way, at the suggestion of the referee (starting on p. 27, line 613 of the revised manuscript).

**Minor comments**

**1) Abstract: The changes in the main body of the manuscript (e.g. DH vs. CH discussion) must be reflected in the abstract**

-We have updated the abstract to be more consistent with the changes made throughout the manuscript. This includes references to the assumptions made for each analysis (i.e., p. 2, lines 33–39, BrO estimations were made assuming the CM dominates, while the spatial scales were estimated assuming the TM dominates).

**2) Page 30236, lines 9ff: "the prominent regional tropospheric oxidation pathways . . . other than OH radicals, notably . . ." What is the evidence for this statement?**

-This statement has been clarified on p. 3, lines 54-56. We now specify that the regional tropospheric oxidation pathways shift for hydrocarbons in low ozone / OH conditions, and have included an additional reference (Cavender et al., 2008).

**3) Page 30236, line 21: R4 is not destroying O3 (the O3 consumed by Br+O3 is re-generated by the photolysis of OClO making R4 part of a null-cycle. However the other two product channels of the BrO + ClO reaction lead to O3 destruction**

-The correct reactions are now listed in Sect. 1, p. 3, lines 66-67. A note was also included (p. 4, lines 69-70) to explain why we did not include the formation of OClO in our reaction listings. Additionally, the BrO + ClO rate constant used in Sect. 3.1 to calculate expected BrO was for the OClO pathway (i.e., $k_{BrO+ClO\rightarrow OClO+Br} = 8.7 \times 10^{-12}$ $cm^3$ $molecule^{-1}$ $s^{-1}$) in the ACPD version of the manuscript. This rate constant has been updated (p. 19, line 420) to ($k_{BrO+ClO\rightarrow BrCl+O2}$ + $k_{BrO+ClO\rightarrow Br+Cl+O2} = k_{BrO+ClO} = 8.2 \times 10^{-12}$ $cm^3$ $molecule^{-1}$ $s^{-1}$ (Atkinson et al., 2007)) to reflect the $O_3$ destroying pathways, and all relevant calculations were redone. However, the calculated expected BrO in each case was only altered by ~0.2-0.3 pmol $mol^{-1}$, and therefore no changes were made to the figure (p. 56) or the discussion within Sect. 3.1, beginning on p. 17, line 384.

**4) Page 30243, line 7: Detection limits for BrO between 2-4 E13 molec./cm2 are quoted, this does not seem to fit with a stated noise level of the measured BrO-column density of 4 E13 molec./cm2. The detection limit is usually taken as twice or three times the noise level.**

This has been clarified in the main text within Sect. 2.1. The stated noise level on p. 30242, line 24 of the ACPD manuscript is an upper bound, while the detection limit quoted on p. 30243, line 7, is based on a distribution of dSCD errors. However, because each individual measurement is the product of multiple differential slant column density (dSCD) measurements, our sensitivity to BrO is higher than what the dSCD errors would indicate. To make this point clearer, we have modified the manuscript (pp. 11-12, lines 237–250 of the revision) to discuss our detection limit in terms of a vertical column density (VCD) rather than a dSCD, which is more comparable to

satellite BrO measurements. We also added additional details on our determination of the detection limit within the same paragraph.

**5) Page 30246, line 9: Why are the depletion regions assumed to be circular? The satellite observations clearly show that they are not.**

While a lot of recent work has gone into more accurately dissecting satellite measurements of BrO (Choi et al., 2012; Salawitch et al., 2010; Theys et al., 2011), we believe there are still questions as to whether current methods can accurately probe the BrO profile in the surface layer, where the O-Buoy measurements are conducted. Thus, it is not clear to us what component of the satellite-retrieved shapes is actually in the boundary layer vs. the free troposphere, and the effect of tropopause variability. Since we do not have accurate information on the shapes of ODEs, we make a default assumption of a circular shape. Sect. 3.2 (p. 24-25, lines 543-546 of the revision) has been modified to include language about the assumptions of circular depletion regions; that is, the assumption that ODEs are circles could underestimate the area if only a secant passes over the buoy, or that the areas could be overestimated if the shapes are more irregular, as discussed below.

**6) Page 30253, section 3.2: Could one not just simply say that the diameter Dode = vwind times tode (with vwind = average wind speed, tode = ODE-duration)? However, this assumes that the (circular) ODE is blown across the measurement site in such a way that the centre of the ODE crosses the buoy. If just a secant crosses, then the above Dode is just a lower limit to the true diameter of the ODE! Likewise, if the ODE is not circular, its area**

**might be overestimated by calculating it as 0.25 x D2 x Pi. These points should be discussed.**

Such an analysis is mentioned in the original manuscript in Sect. 3.2 (p. 30254, lines 5-7 of the submitted ACPD version) when comparing the means of the size distributions between methods. However, the method used was not explicitly stated, though it was performed as the referee described. We have now included this method and discussed the associated assumptions in Sect. 2.3 (p. 14, lines 295-301) and Sect. 3.2 (p. 24-25, lines 543-546 of the revised manuscript), respectively.

7) **Section 3.1: When the DH is correct (which is likely, see above) then not only the O3 depletion times are interesting but also the O3 recovery time scales should be analysed.**

-We agree that $O_3$ recovery is an important topic, as discussed by the very recent Moore et al. (2014) (now cited on p. 41, lines 911-913), and we believe there is merit in performing such an analysis using O-Buoy data. However, we feel that such an analysis is outside of the scope of this manuscript, which focuses primarily on the timescales of $O_3$ depletion and the ODE spatial scales. Recovery is likely a result of synoptic scale events, the observational data for which is problematic for the Arctic Ocean, and a large effort will be required to produce a paper on that subject.

8) **Page 30248, line 13: did Morin et al 2005 really observe O3 depletion within 3min?**

-Morin et al. (2005) did report $O_3$ depletion occurring within 3 minutes on the sea ice (5 km off the coast of Alert) at the conclusion of a meteorological event, as discussed on p. 17, lines 378-380.

9) **Page 30249, Eq. (3): this calculation and the assertion that BrO + HO2 dominates over BrO+ BrO only rests on Stephens et al. 2013b "in prep." The arguments used by these authors can not be verified by the reader, therefore this part (including Eq. (3) should either be explained or removed.**

- As discussed above, Liao et al. (2012) and Liao et al. (2014) also report that the $O_3$ loss rate due to BrO self reaction is only 35% of the loss rate due to BrO + $HO_2$ during OASIS. These citations have been added to Sect. 3.1, p. 18, lines 390-391. That $Br_2$ is an important source of Br atoms in the Arctic boundary layer is now verified and certain.

10) **Page 30256, Sentence starting in line 24 is redundant and should be deleted.**

-We have removed this sentence as suggested.

We would like to thank again Anonymous Referee #1 for his/her comments and suggestions, and feel sincerely the manuscript has benefitted from them.

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 $\tau_{O_3}$ 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 $\leq 2$ 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.