

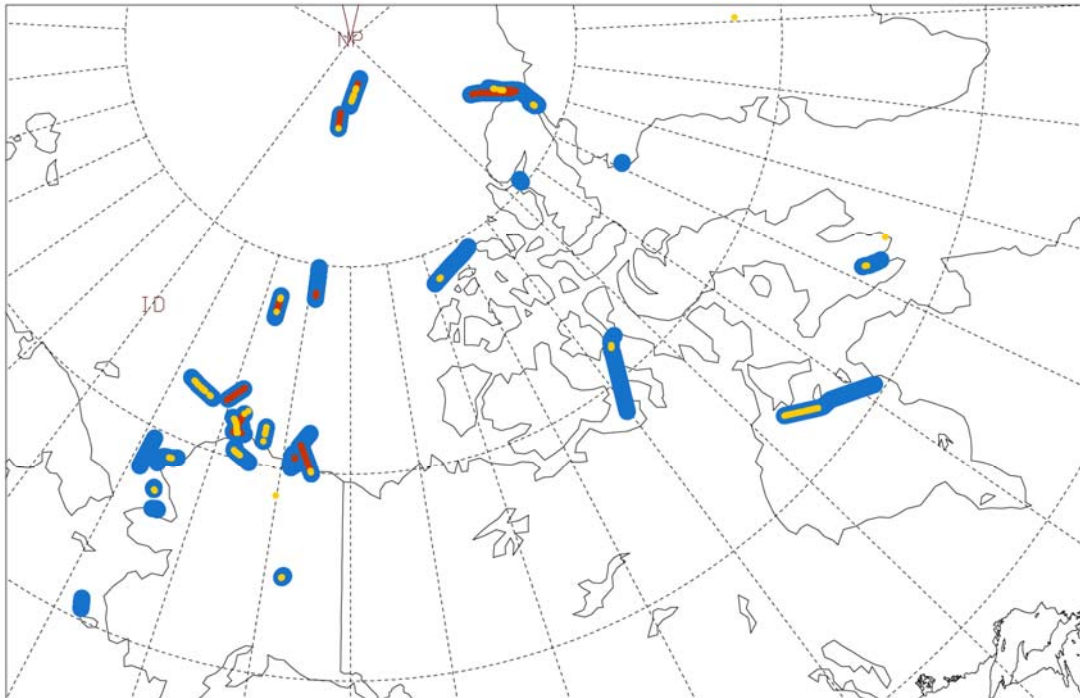
Responding to Referee #2

1) Table 1. It would be useful to include the location (lat/lon) and altitude of the MDEs which would be much more useful for the reader than the UTC. Some studies have suggested that high halogens occur near leads. Are there any observations available (for observers on the aircraft or from the flight cameras on-board the aircraft) that can give information on the state of the sea-ice and the potential occurrence of leads when MDEs were observed?

We added the altitude of MDEs and state of sea ice based on the nadir camera view on-board the DC-8 in Table 1. We also added lat/lon in Figure 1. Unfortunately, we did not find any distinguishable open ocean from the nadir images. We examined the MDE and non-MDE cases and did not find any difference in the surface of the sea ice/snow surface. We also added the sentences; “In this regard, we examined the MDE and non-MDE cases and did not find any difference in the surface of the sea ice/snow based on views from the nadir camera on the DC-8. In fact, there did not appear to be any open leads that the DC-8 flew over” in line 23 on p.9 – line 2 on p.10 in the manuscript.

2) Page 10081. The authors present 14 MDE cases which all appear for be below 5 km altitude (and generally close to the surface). Did Hg^0 levels always drop to below 50 ppqv when the aircraft flew below 5 km altitude? Or were there instances when the aircraft flew at low altitudes above sea-ice and Hg^0 was not depleted? It would be interesting to include a discussion of these cases in order to understand the conditions conducive to AMDEs.

We produced one figure below in order to know whether MDEs occurred across the entire Arctic Ocean. The figure included horizontal flight routes below 500m altitude (blue), horizontal location of MDEs in 5 km altitude (red), and horizontal area of sampling low Hg^0 (50-100ppqv) in 5 km altitude (yellow). As you can see, entire sampling area below 500m altitude did not show MDEs. The paper by Mao et al. (2010) shows that the MDEs had thicknesses that varied from 0.1 to 1 km, and that they only occurred over the Arctic Ocean (Figure 1 and 3 in manuscript). The chemistry near the surface was fairly consistent between MDE and non-MDE locations. It appears that the main difference was the presence or not of reactive halogens. This was added to the text on p. 9.



3) Figure 1. It is very difficult to see the red dots. I suggest that the authors modify the figure for readability.

We made Figure 1 into two panels to better distinguish the features we were trying to show. Figure 1-a is the spatial distribution of $\text{Hg}^0 < 50 \text{ ppqv}$ (yellow dots) and high $\text{Br}_2 > 2 \text{ pptv}$ (blue dots) below 5 km altitude and Figure 1-b is the spatial distribution of $\text{O}_3 < 10 \text{ ppbv}$ (pink dots) and high $\text{Br}_2 > 2 \text{ pptv}$ (blue dots) below 5 km altitude (b).

4) Figure 2. What do the different colors of the trajectories correspond to? Please clarify in the legend.

I added the sentence in the caption of figure 2; The color bar is pressure level and the unit is hPa.

5) page 10083. line 9. It doesn't make much sense to compare H_2O levels observed during ARCTAS to levels observed over Hawaii... It would make more sense to compare to other observations over the Arctic.

The reason why we utilized the Hawaii measurement is that we wanted to refer the H_2O level in another marine environment in springtime and it was available from a recent NASA field campaign measurement data over the marine boundary layer (<1 km) was the Hawaii. Therefore, we utilized the data. However, we also found the range of H_2O level in northeastern Pacific marine boundary layer near Anchorage, Alaska in the springtime during INTEX-B field campaign; 4500 – 11700 ppmv. It also indicated that H_2O level in marine boundary layer over

the Arctic Ocean is very low value to compare the level in northeastern Pacific. We replaced the H₂O levels from sampling in the vicinity of Hawaii to sampling near Anchorage, Alaska (line 21 on pp. 7 – line 1 on pp. 8).

6) page 10088. Could the authors include in the base case results discussion a discussion of the key reactions and assumptions that lead to AMDEs? My understanding is that you only really need 3 things: high Br₂, light to photolyze Br₂, and then a fast reaction of Hg⁰ with Br. Is that true?

Yes, you are right. In addition, a continuous Br₂ source is more important than just high Br₂ as we mentioned in section 4.3. We also added the sentence “The dominant product, HgBr₂, indicated that the reaction of Hg⁰ with Br radical, which principally produced by photolysis of Br₂, is very important to Hg⁰ depletion” in lines 3-5 on p. 14)

7) page 10088 and figures 4-7. It is confusing that the authors use 3 different units for Hg concentrations: ng/m³, ppq, molec/cm³. Similarly for other species they use mixing ratios in the Table but then concentrations in molec/cm³ in the figure. I suggest using mixing ratios for figures 4-7 for consistency. Also the Figures 4-7 are of very poor quality in terms of resolution and the axis can be barely seen.

We modified figure 4-7. All units are now mixing ratios in ppbv, ppmv, pptv, or ppqv.

8) page 10089 line 7. The authors contradict themselves. "...did not affect the time it took to reach depletion.." is followed by a statement saying that the rates did affect the time it took to reach depletion. Please clarify.

For Cl, OH, and Br₂, different reaction rate constants of Hg⁰ did not affect the time it took to reach depletion, while different reaction rate constants of Hg⁰ with Br radical affected the time.

9) page 10093 line 21. Instead of citing (Selin, 2009; Sigler et al. 2009) which do not discuss observations of Hg⁰ over the Arctic ocean, it would be more relevant to cite the study of Andersson et al. "Enhanced concentrations of dissolved gaseous mercury in the surface waters of the Arctic Ocean" Mar Chem (2008) which report high supersaturation of DGM in Arctic waters.

We agree and have modified the text appropriately.