**Reply to reviewer #1's comments**

We thank the reviewer for the helpful and constructive suggestions regarding how to improve the manuscript. The manuscript was revised carefully after considering the two reviewers' comments. Our responses are provided below in dark blue (and quoted texts added to the revision in italics), with the reviewer's comments included in black.

**Reviewer #1:**

Yang et al report observations of snow concentrations of ions along with surface atmosphere ozone and BrO concentrations at an Arctic location in the early spring of 2018 and 2019. To my knowledge, these are the first depth-dependent observations of snow bromide, which may be useful for understanding processes determining reactive bromine emissions from snow. They use time and depth-dependent measurements of bromide and nitrate to calculate the net deposition flux during the observational time period. They find that nitrate and bromide in snow are correlated and suggest that they are linked to one another through the formation and hydrolysis of bromine nitrate. They also find that deposition is confined to the surface skin layer.

This paper is very difficult to read, especially the long results section. There are a lot of details and numbers and it is presented in a way that makes it very difficult to discern the big picture. It reads like a first draft. The paragraph starting on line 424 is a particularly good example of this. There are more numbers than words in this paragraph and it is not readable. In general, the paper needs some reorganization and needs to be presented in a more succinct and readable manner. It often reads as a list of disconnected observations.

Response: We have revised the manuscript accordingly. In particular, we rewrote the paragraph which contains old lines 424-430.

*"Surface snow [$Br^-$] and nss[$Br^-$] show a very similar increasing trend (Figure 6(f) verse 6(g)), this is due to the large bromine enrichment factor or weak sea water impact. The 2019 tray sample nss[$Br^-$] slope at 0PAL is 0.023±0.006 $\mu M\ d^{-1}$ (R=0.64, $p<0.001$, N=24 ), which is very close to the first layer slope values (Figure 7(d)) at Onshore and at Sea ice where the trends are statistic significant ($p<0.02$, Table S5). In the second snow layer, their slope values (Figure 7(e)) are still positive but with large p values (0.13-0.18). Tray sample nss[$Br^-$] slopes at PEARL is 0.013±0.006 $\mu M\ d^{-1}$ (R=0.56, p=0.04, N=14)) which is smaller than that at 0PAL. In the first and second snow layer at PEARL, slope values are positive and statistic significant (Table S5)."*

A large portion of the results section focuses on salinity, but in the end, it is not clear what they learned from it as the results section is difficult to read and there is no follow-up on the salinity observations in the discussion or conclusions section. It is also unclear how an iceberg will impact snow salinity on sea ice and land.

Response: We added a few sentences in the text to address the link between snow salinity and snow ions observed. For example, in section 3.2:

*"Like snow salinity results, major snow ions on sea ice also have a large perturbation from 2018 to 2019. For example, the 2018 column mean snow sodium on sea ice (Tables S2 and S3) is 3-4 times of the 2019 column mean, which is consistent with the relatively low snow*

*salinity observed in 2019 due to the presence of a large iceberg grounded in the valley. In 2018, the column mean (1.5–20 cm) bromide on sea ice is 10.74±8.52 μM (N=80) (Table S2), while in 2019, it is only 6.47±5.36 μM (N=66) (Table S3). The lower 2019 snow bromide on sea ice is likely attributed to the freshwater dilution by the iceberg. However, they are much smaller than mean 30.6 μM on thick first year ice (FYI) and 92.5μM on thin FYI at Barrow, Alaska (Krnavek et al., 2012).”*

In section 5 of conclusion, we added a paragraph below to highlight the impact of the iceberg on snow salinity:

*“Skin layer snow salinity at the inland site has a double-peak distribution, with the first peak at 0.001-0.002 psu corresponding to the precipitation effect, and the second peak at 0.01–0.04 psu is likely due to the salt accumulation effect. Snow salinity on sea ice has a triple-peak distribution, and the third peak at 0.2–0.4 psu is clearly related to the sea water effect due to the upward migration of brine on sea ice. The presence of an iceberg in the valley could significantly dilute ice and column snow salinity as observed in 2019 samples.”*

Abstract line 26: missing a unit after 0.024.

Response: Done

Methods: State the eluants used for the IC measurements.

Response: The eluents for ion chromatography were generated with a Dionex hydroxide eluent generation cartridge (EGC) for anion analyses and a Dionex methanesulfonic acid EGC for cation analyses.

Line 342: I think you mean to say that the “concentrations” are larger, not the “profiles”.

Response: You are right. corrected.

Line 419: What is a near zero increasing trend? Does this mean that the increasing trend is not statistically different from zero?

Response: Yes, the sentence has been rewritten to “the increasing trend is not statistically different from zero in PEARL tray samples and in the first layer.”

Section 3.5: I think these calculations represent a net deposition flux (deposition minus emissions) and this should be explicitly stated.

Response: This point has been clearly addressed in the manuscript, e.g. in section 3.6, we stated *“where Flux is mean net deposition flux (deposition minus emission)”.*