Dear Dr. Krystyna Koziol,

Thank you very much for your kind comments. We revised our manuscript following your comments. We filled in your comments in the black and author replies in the blue. All line numbers of this manuscript have linked to our revised manuscript. In the revised manuscript, edits made based on your comments are highlighted in green.

Editor comment:

1. In lines 61-62 of Response to Reviewer#1, the Authors mention: "We removed the oil contamination on the precleaned materials and tools using ethanol, and performed then ultrasonic cleaning in ultrapure water." It is unclear to me why there would be any oil contamination involved. Did you mean potential contamination with organic compounds in general?

Author reply:

As you have pointed out, we used ethanol to remove the potential contamination by organic compounds in general, not specifically oil, on materials and tools used for contamination removal. We have revised this sentence at line 116–117 in our revised manuscript.

Editor comment:

2. Was there a certified reference material used to ensure the quality of the ion chromatography analyses? If yes, please describe; if not, please justify.

Author reply:

We used standard solutions for ion chromatography produced by FUJIFILM Wako Pure Chemical corporation for the absolute calibration determination by ion chromatography. We have added this description at line 130–132 in our revised manuscript.

Editor comment:

3. In lines 144-145 of Response to Reviewer#1, the Authors mention: "The snowpack from 0.72 m–1.15 m corresponded to spring to summer in 2022 from existence of ice layer" - such phrasing is awkward, I would assume that it was deduced from the existence of ice layer that the snowpack layer corresponded to a certain time.

Author reply:

We have corrected the phrase you have pointed out as follows:

"The snowpack from 0.72 m–1.15 m was interpreted to correspond to the spring–summer period in 2022, due to the presence of ice layer, high $\delta^{18}O$ value and high MSA concentrations (Fig. 2 and Fig. S2)."

This revised phrase has been added at line 197–199 in our revised manuscript.

Editor comment:

4. In the suggested revised Section 2.3., there occur phrases such as "probability of existence", "existence probability", "existing probability" - what do they mean? Do they refer to the probability of air mass inflow from a certain direction or sector?

Author reply:

The existence probability was defined as the proportion of backward trajectories originating from the St. 9 that passed through each 1°×1°grid cell. The calculation procedure was as follows:

First, we counted the number of times the backward trajectories originating from St. 9 passed through each grid cell. Second, we normalized the count for each grid cell by dividing it by the total number of the trajectory passes across all grid cells.

We have added this description at line 168–171 in our revised manuscript.

Editor comment:

5. In lines 289-290 and 301-302 of Response to Reviewer#1, the Authors mention: "The mean temperature differences in autumn and winter were more negative than that in summer." and "that the altitude gradient of surface air temperature in the western side of Prudhoe Land was steeper in winter than in summer.". The "more negative" phrasing is confusing, since the difference between temperatures is an absolute value and therefore cannot be negative. However, the proposed sentence for the manuscript reads correct if the difference was higher in winter. Please double-check that this was the intended meaning.

Author reply:

As you have pointed out, the original phrasing was ambiguous and may have caused confusion. We have corrected the unit of temperature from degrees Celsius to Kelvin. With that in mind, we calculated the temperature difference between St. 3 and St. 9. The temperature difference was smallest in summer and increased toward winter (Fig. S6). The mean temperature differences in autumn and winter were larger than that in summer (Fig. S7). We have revised this description at line 249–253 in our revised manuscript.

Editor comment:

6. There is also a typo in line 80 of Response to Reviewer#2 ("care" instead of "case").

Author reply:

We have corrected the word “If that is the care” to “If that is the case”.

Figures:

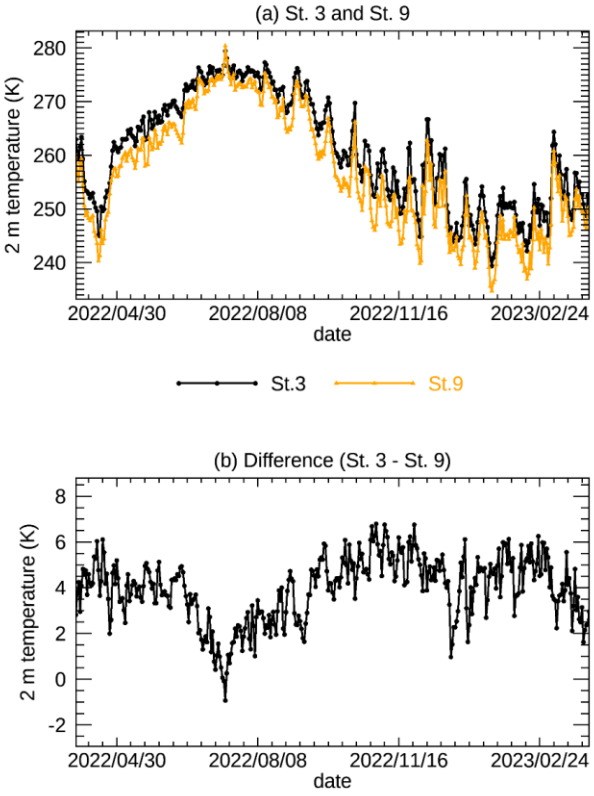

Figure S6: Diurnal variations in (a) 2 m air temperature at St. 3 and St. 9, and (b) 2 m air temperature difference between St. 3 and St. 9.

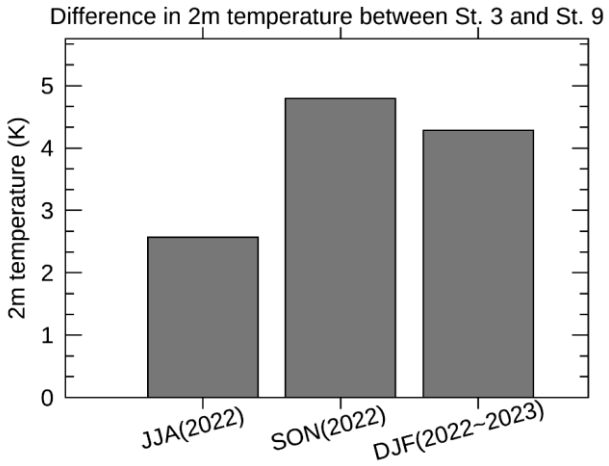

Figure S7: Seasonal variations in the difference of 2 m air temperature between St. 3 and St. 9.

Dear Reviewer, #1

Thank you very much for your valuable comments. We revised our manuscript following your comments. We filled in reviewer comments in the black and author replies in the blue. All line numbers of this manuscript have linked to our revised manuscript. In the revised manuscript, edits made based on reviewer comments are highlighted in yellow.

Reviewer comment:

In this paper entitled "Characteristics of snowpack chemistry on the coastal region in the northwestern Greenland Ice Sheet facing the North Water", authors present an interesting observation of the effect that polynia North water (NOW) has on aerosol circulation and precipitation. the results are obtained from measurements of major ions, MSA and water isotopic analyses at 9 surface snow sampling sites, 2 snow-pit sites and 1 ice core. The text is well structured a detailed introduction, however the drafting in general should be improved as there are numerous repetitions and in some parts the reading is difficult to understand. In particular, the section 3.2 has to be improved. The conclusions have to be focused on the main goals obtained in this paper. It is very long and I suggest to summarize, avoiding to repeat the results and discussion.

Author reply:

To improve the overall logical flow and readability in section 3.2, we have added individual sub-sections for $\delta^{18}O$ and ion species. Figures of $\delta^{18}O$ and each ion concentration were presented separately within their respective sub-sections.

We also revised the Conclusion section by summarizing its content and removing some repetitive statements.

Reviewer comment:

On lines105-106: "The snow sampling intervals at St. 3 were 0.02 m from 0.00 to 0.20 m and 0.03 m from 0.20 to 1.01 m, and the snow sampling intervals at St. 9 were 0.02 m from 0.00 to 0.20 m and 0.03 m from 0.30 to 1.08 m." Why was the sampling interval changed?

Author reply:

If we could sample the entire snowpack at short intervals, we would have been able to discuss the temporal variations in chemical components with short time intervals. However, we changed sampling interval partway through the snowpack, because we had limitations on the number of snow samples that could be transported by dog sledges.

Reviewer comment:

Lines 104, 108, 110. The authors told of precleaned materials and tools, but the cleaning procedure is not described.

Author reply:

The cleaning procedure has been added to the Method section at line 116–117.

Reviewer comment:

On line 107: Why was the ice core only sampled at one site? could be used for comparison at least with st3.

Author reply:

We prioritized sampling as much as possible at St. 9 because of the limitation on the number of snow samples that could be transported by dog sledges.

Reviewer comment:

Line 115: "methane sulfonate− (hereafter referred to as MSA)" already defined in the introduction

Author reply:

The definition of MSA moved at line 51–52.

Reviewer comment:

Lines 116-118. Please add several details about the analytical methods or some references. In particular, the authors declared only the columns used for cations and anions without any specific important details such as dimensions. Other important details are flows, injection volumes, instruments used, suppressors, detectors. No specific details about the quantification methods are reported. I suppose that you used external calibration curves, but which are the linear ranges, and which are the RCM used for quantification. In summary, please improve the method and quality control section about the ionic analysis.

Author reply:

For the cations, separation was carried out with a Dionex CG-12 (4 × 50 mm) guard column, followed by a Dionex CS12-A (4 × 250 mm) separation column. Injection volume of samples was 500 µL. MSA (20 mM) was used as eluent, and flow-rate was kept 1.0 mL min-1. Dionex CDRS600 dynamically regenerated suppressor was used for conductivity suppression before conductivity cell. For the anions, separation was obtained with a Dionex AG-18 (4 × 50 mm) guard column and Dionex AS-18 (4 × 250 mm) separation column. Injection volume of samples was 1000 µL. KOH (23 mM) was used as eluent, and flow-rate was kept 1.0 mL min-1. Dionex ADRS 600 dynamically regenerated suppressor was used for conductivity suppression before conductivity cell. The absolute calibration curve method was used for quantitative determination of each ion concentration. For the absolute calibration determination by ion chromatography, we used the standard solutions for ion chromatography produced by FUJIFILM Wako Pure Chemical corporation, diluted to 20, 50, 100, and 200 ppb with ultra-pure water. If the ion concentration of samples were outside the calibration range (> 200 ppb), it was remeasured using 500, 1000, 2000–3000, and 6000 ppb standard for the anions and 500, 1000, 2000, and 4000 ppb standard for the cations. Blanks were always evaluated before the calibration procedure.

We have added this text regarding the analytical method of the ion chromatography at line 123–134.

Reviewer comment:

Lines 117-119: Has the ion chromatography method used been validated in previous works? If yes, indicate them, if not, insert a section on validation.

Author reply:

The ion chromatography method had been validated by the previous work (Kurosaki et al., 2020; Kurosaki et al., 2022). We have added this description to the method section at line 136–137.

Reviewer comment:

Lines 119-120: "The samples exhibiting large peak were measured multiple times, to confirm that any large peak in ion concentration was not caused by analytical errors." What is meant?

Author reply:

If the ion concentration of samples were outside the calibration range (> 200 ppb), it was remeasured using 500, 1000, 2000~3000, and 6000 ppb standard for the anions and 500, 1000, 2000, and 4000 ppb standard for the cations. Blanks were always evaluated before the calibration procedure.

We have added this description at line 132–134.

Reviewer comment:

Lines 156-165 Text is not clear

Author reply:

We have revised the text you kindly pointed out at line 194–204.

Reviewer comment:

Section 3.2. Following stratigraphic analysis and evaluation of snowpack density, it may be more informative to express data in terms of fluxes rather than concentrations, so in the subsequent data analysis one could avoid distinguishing peaks attributed to atmospheric deposition from those of melting and refreezing

Author reply:

As you have pointed out, the deposition flux is sometimes more suitable when discussing the deposition amount of atmospheric aerosols for quantitatively. However, we cannot discuss the deposition flux because we did not collect the snow density with high resolution along the snow depth. Therefore, we qualitatively discussed the seasonal characteristics of ion species based on their concentration.

Reviewer comment:

Lines 188–190: Introducing all figures at the beginning of the section may lead to confusion. Since the discussion begins with Fig. 5, it would be more effective to present the figures sequentially, in alignment with the narrative.

Author reply:

In accordance with your comment, we have revised the order of figures. We have presented the $\delta^{18}O$ and each ion concentration within the relevant sub-sections of section 3.2, displaying the figures sequentially.

Reviewer comment:

Line 194: "We applied the concentration unit as μeq L−" Information that is already made explicit in the following graphs

Author reply:

This sentence notes that "μ eq L^{-1}" was used as the unit of ion concentration in equation (1). The editor requested that the concentration unit should state clearly for the equation (1).

Reviewer comment:

Line 201: "We suggest that the spatial variation in the $\delta^{18}O$ results from water vapor transport from the southern coast to the northern inland area by southerly winds." Might it be useful to indicate figure 9 by referring to the direction of the prevailing winds?

Author reply:

Thank you for your comment. We suggested that the south-to-north gradient of the $\delta^{18}O$ results from water vapor from the southern coast to northern inland area by the southerly winds. We have performed the backward trajectory analysis and analyzed the probability map of air mass transportation to make this assumption more reliable (Fig. 1 in this file). The 7-days backward trajectory of air mass arriving at St. 9 showed that the majority of air mass was transported from the south of St. 9, situated on northern Baffin Bay and eastern NOW.

We have added this description at line 226–229. We have also added the method of backward trajectory at line 162–174. Figure 1 in this file has been added to the supplementary materials.

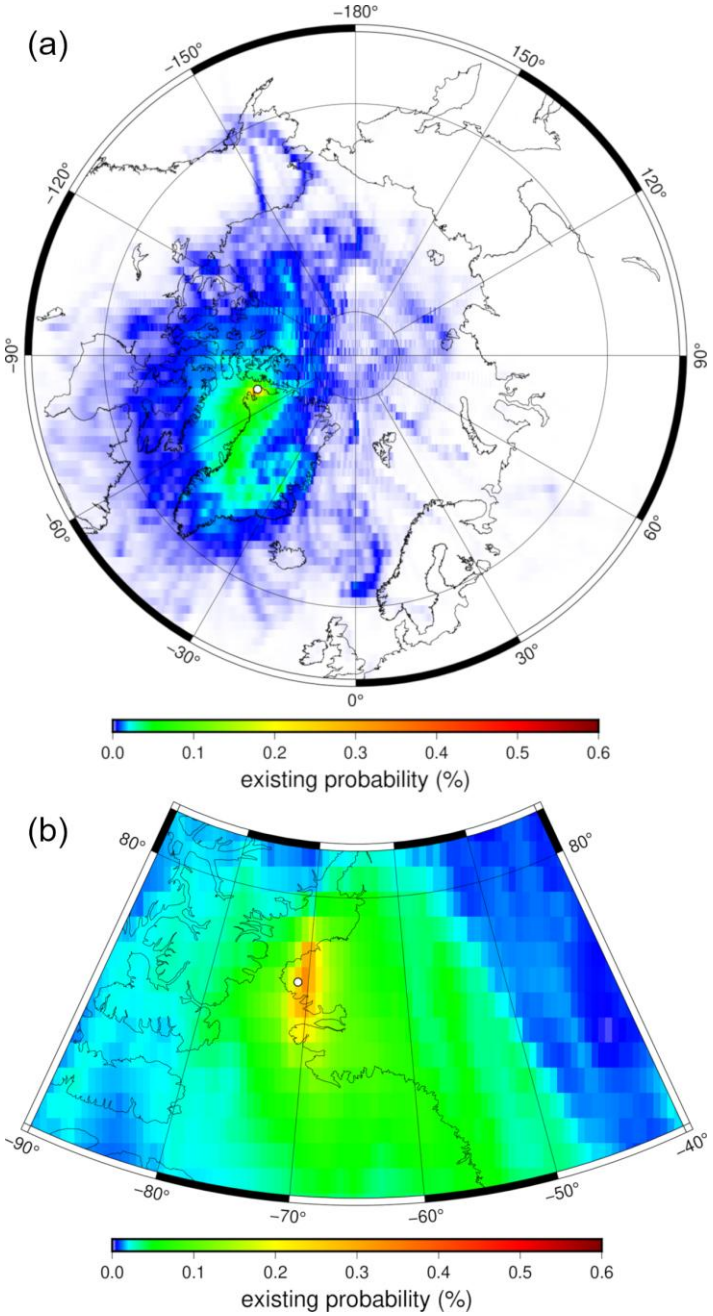

Figure 1 (in this file): Existence probability of an air mass occurring during the past during 7 days reaching the St. 9 in whole of year from 2019–2023. (a) and (b) display Arctic area (> 60°N) and around northwestern Greenland, respectively. Black circles show the position of the St. 9.

Reviewer comment:

Line 210. Please add "(figure 6)" to help readers or start the sentence introducing the Figure 6 and its meaning.

Author reply:

We have added an explanation of the relevant figure at the beginning of the paragraph discussing the vertical profiles of $\delta^{18}O$ at St. 3 and St. 9 (lines 238–239).

Reviewer comment:

Figure 6: I suggest using the season and year instead of Roman numerals, as this would facilitate interpretation. This recommendation may also apply to the other figures. It is somewhat difficult to follow the discussion, as it requires frequently switching between different figures.

Author reply:

To improve the overall logical flow and readability in section 3.2, we have added individual sub-sections for $\delta^{18}O$ and ion species. Figures of $\delta^{18}O$ and each ion concentration have been presented separately within their respective sub-sections. The seasonal divisions in each figure have been revised from Roman numerals to explicit labels indicating the season and year (ex. Fig. 2 in this file).

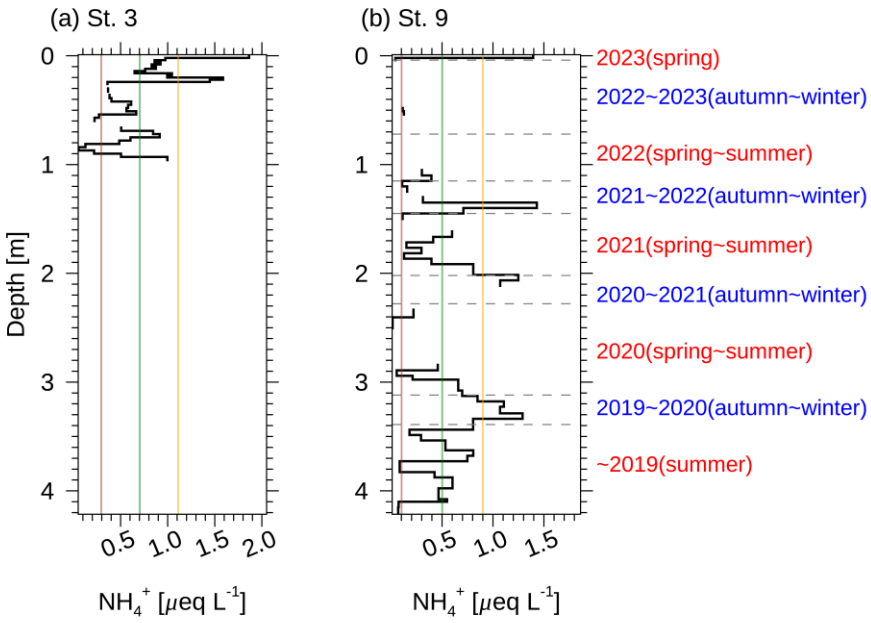

Figure 2 (in this file): Vertical profile of NH_4^+ at (a) St. 3 and (b) St. 9. Green lines denote mean NH_4^+ across all observation depths. Orange and brown lines denote the mean NH_4^+ plus and minus one standard deviation across all observation depths, respectively. The LOD of NH_4^+ was < 0.0055 μeq L^{-1}.

Reviewer comment:

Figure 6c, it is not clear why the authors used the difference between St3 and St.9, instead of a ratio.

Author reply:

To discuss the seasonal variation of the surface air temperature difference between St. 3 and St. 9, we calculated the difference of $\delta^{18}O$ values at the two stations.

We propose that the difference between St. 3 and St. 9 can be discussed for the following reasons.

The depths of the negative and positive peaks of $\delta^{18}O$ at St. 9 agreed well with those at St. 3 (Fig. 3 in this file), and the vertical profile of $\delta^{18}O$ between 0.00 and 1.01 m at St. 9 correlated significantly with that at St. 3 ($r = 0.69$, $p < 0.01$). The snowpack corresponding to autumn−winter from 2022−2023 at St. 3 and St. 9 at the same snow

depth were most likely accumulated with precipitation attributed to the same snowfall events, and $\delta^{18}O$ in the snowpack had not been changed by the post depositional processes, which is water molecule diffusion, wind blowing, and sublimation. Therefore, we propose that the vertical profile of $\delta^{18}O$ between 0.00 and 1.01 m at St. 9 can be reasonably compared with the profile at St. 3 based on their differences.

We have already described the above discussion at line 239–245.

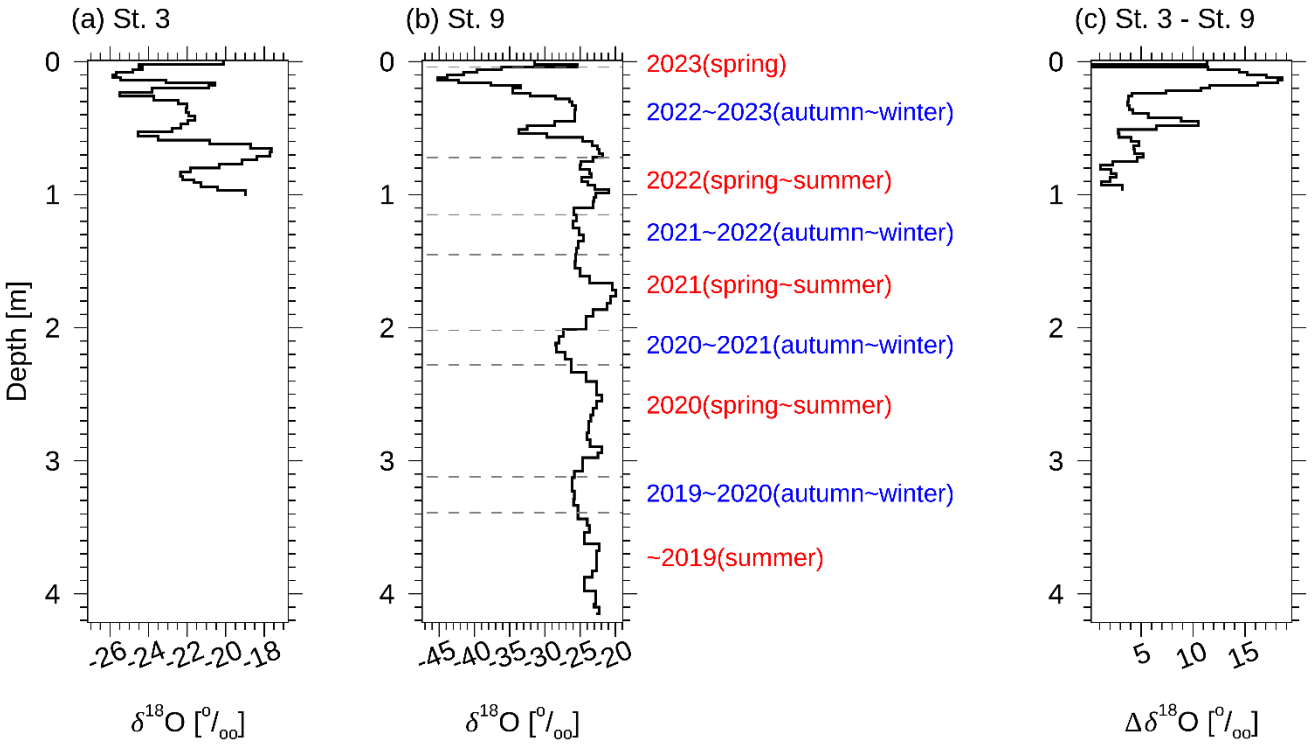

Figure 3 (in this file): Vertical profile of $\delta^{18}O$. (a) and (b) show $\delta^{18}O$ values at St. 3 and St.9, respectively. (c) shows difference between St.3 and St.9 in terms of $\delta^{18}O$. i–vii denote seasons from 2019 to 2023. i, iii, v, and vii denote from autumn to winter period from 2022–2023, 2021–2022, 2020–2021, and 2019–2020, respectively. ii, iv, and vi denote from spring to summer in 2022, 2021, and 2020, respectively.

Reviewer comment:

Line 217-218: "We suggest that the altitude gradient of the surface air temperature in winter was greater than that in summer in the western region of Prudhoe Land." could this statement also be confirmed using atmospheric models for specific sites?

Author reply:

We estimated the difference in surface air temperature between St. 3 and St. 9 using ERA5-Land reanalysis dataset (Fig. 4 and Fig. 5 in this file). The temperature difference was smallest in summer and increased toward winter. The mean temperature differences in autumn and winter were larger than those in summer. This result supports our suggestion, based on water stable isotope, that the altitude gradient of surface air temperature in the western side of Prudhoe Land was steeper in winter than in summer.

We have added this description at line 249–253. Figure 4 and 5 in this file have been added to the supplementary material. We have also added an explanation of ERA5-Land in the Method section at line 159–160.

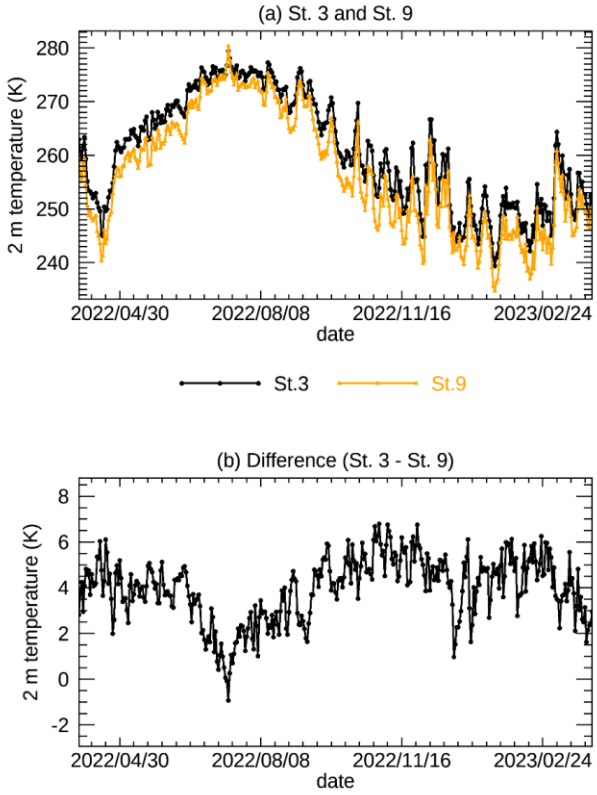

Figure 4 (in this file): Diurnal variations in (a) 2 m air temperature at St. 3 and St. 9, and (b) 2 m air temperature difference between St. 9 and St. 3.

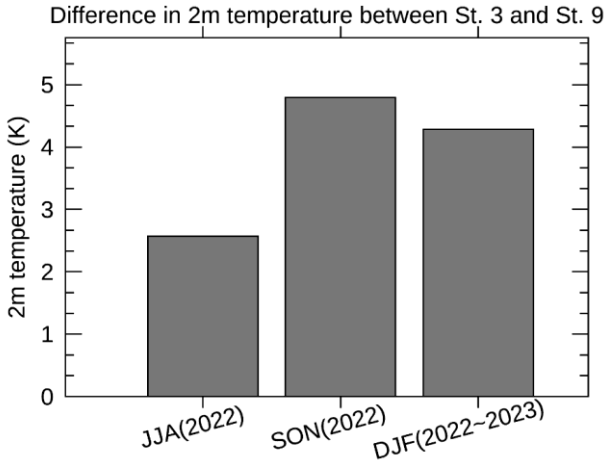

Figure 5 (in this file): Seasonal variations in the difference of 2 m air temperature between St. 9 and St. 3.

Reviewer comment:

Lines 306-309: there are many repetitions of “the concentration of MSA”. Same in the conclusions with “The snowpack on the western side of Prudhoe Land”.

Author reply:

Thank you for your kind comment. We have revised the text that you have pointed out at line 403–406 and 471–486.

Reviewer comment:

General comment on the conclusions: from figure 1 sampling sites 1 to 5 (or 6) are in a valley. has this aspect been taken into consideration? could it have an impact on the final considerations?

Author reply:

I appreciate your valuable comment.

Because the topography in the western side of Prudhoe Land is smooth (Fig. 6 in this file) and the glacier is broad and relatively low gradient, we think that the enhancement of vertical convection and downslope wind caused by the valley topography are insignificant on the large-scale water vapor and aerosol circulation around the western side of the Prudhoe Land.

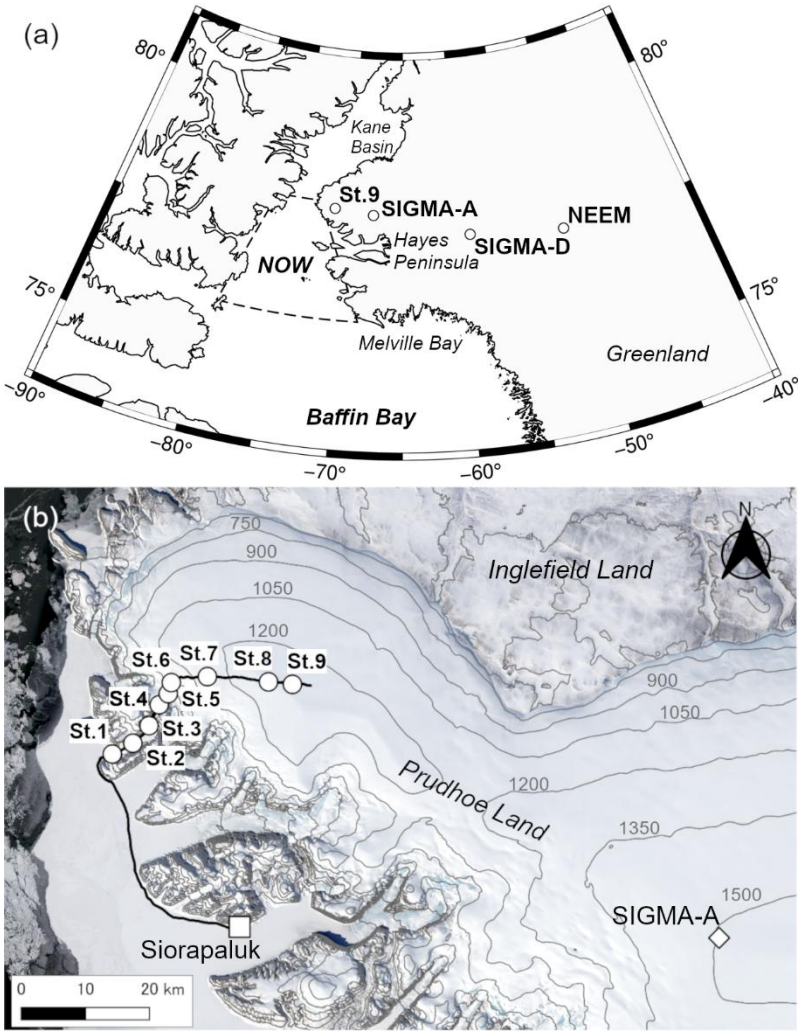

Figure 6 (in this file): Maps of the sampling sites. (a) shows location of the snowpit and ice core sampling sites in this study (St. 9) and previous studies (SIGMA-A, SIGMA-D, and NEEM) in the northwestern Greenland Ice Sheet. The dashed polygon in (a) denotes the approximate location of the NOW. Hayes peninsula in the northwestern Greenland is located between Kane Basin in the north and Melville Bay in the south. (b) shows Landsat-8 image around St. 9 and SIGMA-A of Prudhoe Land, which is located on the northern part of Hayes peninsula, on 13 April 2023. The black circles in (b) denote the sampling sites from St. 1 to St. 9, and the black line denotes dog sledge route. The gray contours in (b) are drawn from the Greenland Mapping Project 2 (GIMP-2) Digital Elevation Model version 2.

Other comments:

Reviewer comment:

In figure 1b it might be useful to include a dimensional scale to give an idea of the distances.

Similarly, in figure 2, in addition to the distance expressed in latitude, could a conversion to km be useful?

Author reply:

Thank you for your ideas. We have added the scale of distance and north arrow in Figure 1b (Fig. 6 in this file), and the distance from St. 1 to each sampling station in supplementary figure S1 (Fig. 7 in this file).

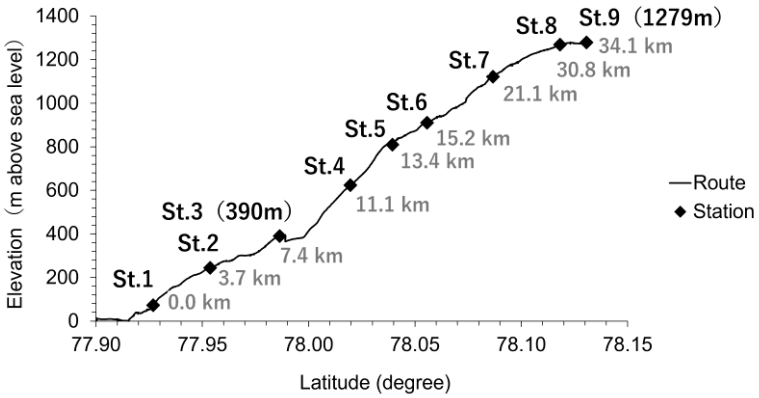

Figure. 7 (in this file): Elevation above sea level of each station. Gray values denote the distance from St. 1 to each station.

Reviewer comment:

In figure 5, in addition to changing colours between total and nss values, it would also be useful to change the symbols

Author reply:

Symbols have been removed from the figure depicting the vertical profiles of $\delta^{18}O$ and ion species, as step-width graph were used in Figure 2–10.

References:

Kurosaki, Y., Matoba, S., Iizuka, Y., Niwano, M., Tanikawa, T., Ando, T., Hori, A., Miyamoto, A., Fujita, S., and Aoki, T.: Reconstruction of Sea Ice Concentration in Northern Baffin Bay Using Deuterium Excess in a Coastal Ice Core From the Northwestern Greenland Ice Sheet, JGR Atmospheres, 125, e2019JD031668, https://doi.org/10.1029/2019JD031668, 2020.

Kurosaki, Y., Matoba, S., Iizuka, Y., Fujita, K., and Shimada, R.: Increased oceanic dimethyl sulfide emissions in areas of sea ice retreat inferred from a Greenland ice core, Commun Earth Environ, 3, 327, https://doi.org/10.1038/s43247-022-00661-w, 2022.

Dear Reviewer, #2

Thank you very much for your valuable comments. We revised our manuscript following your comments. We filled in reviewer comments in the black and author replies in the blue. All line numbers of this manuscript have linked to our revised manuscript. In the revised manuscript, edits made based on reviewer comments are highlighted in blue.

Reviewer comment:

First, data from the ST9 snow pit indicate evidence of summer surface snowmelt. Such melting processes hinder the preservation of proxy records and introduce uncertainty in age-dating. As discussed in the manuscript (lines 164–165), water isotope records tend to become smoothed, and ion concentrations are altered due to refreezing of meltwater. Therefore, the interpretation of vertical variations in proxy concentrations should account for these site-specific characteristics. Particularly in Section 3.2 ("Spatial and temporal variations in water isotopes and chemical species"), the interpretation of ST9 data should reflect the impact of summer melt on concentration variability.

Author reply:

As you have pointed out, the water stable isotopes were smoothed and ion concentrations were relocated due to meltwater refreezing. Therefore, the amplitude of the seasonal variations in water stable isotopes were smaller and ion concentrations showed high peaks in the ice layer. We have already described the impact of the melt water refreezing on the seasonality of water stable isotopes and ion concentrations at line 201–204, 305–306, 327–329, 354–357, and 378–380.

Reviewer comment:

The seasonal classification such as spring–summer vs. autumn–winter should be used consistently, and the discussion of concentration variability should be supported by statistical criteria due to no clear variability of proxies. For example, it is recommended to define peaks using either values above the mean or above the mean plus one standard deviation.

Author reply:

In accordance with your comment, we have unified the description of seasonal classification as spring-summer and autumn-winter.

We have defined positive (negative) peaks of each ion according to statistical criteria, which were values above (below) the mean plus (minus) one standard deviation at line 303–305, 326–327, 353–354, 377–378, and 405–406.

Reviewer comment:

Second, additional evidence is required to substantiate some of the manuscript's interpretations. For example, to support the discussion on atmospheric transport, the inclusion of backward trajectory modeling (e.g., frequency maps and cluster analyses) is recommended as supplementary information to identify source regions and air mass pathways.

Author reply:

Thank you for your valuable suggestion.

This study suggested that the oceanic aerosols were transported from the area near the western side of Prudhoe Land, located on the NOW, throughout the year by analyzing $\delta^{18}O$ and chemical species in the snowpack in western side of Prudhoe Land. To make this assumption more reliable, we performed the backward trajectory analysis and created a frequency map of the air mass transportation (Fig. 1 in this file). Majority of air mass arriving at western side of Prudhoe Land was transported from southern Greenland via northern Baffin Bay and eastern NOW. The existing probability in the eastern NOW was particularly high. The backward trajectory analysis supported our assumption, which the primary source of oceanic aerosols was NOW polynya throughout the year.

Figure 1 in this file has been added to the supplementary material, and relevant explanations have also been added at line 162–174 and 226–229 in our revised manuscript.

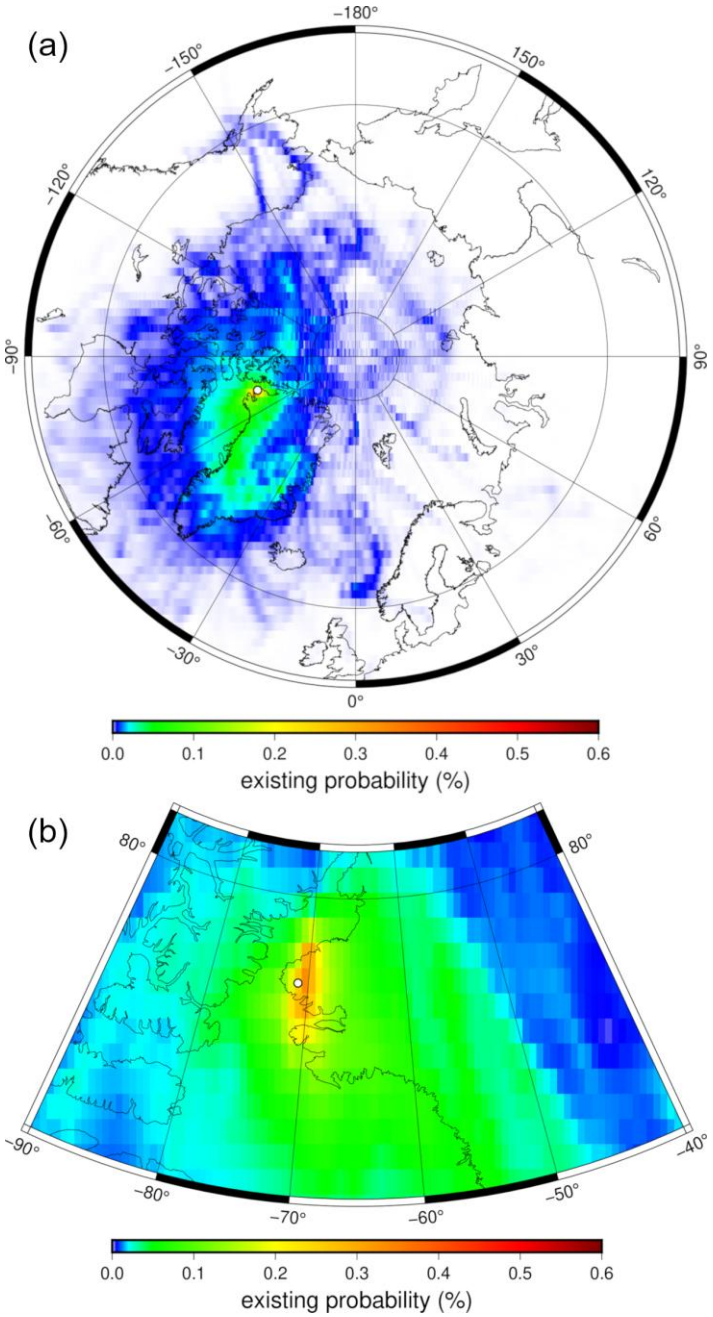

Figure 1 (in this file): Existence probability of an air mass occurring during the past during 7 days reaching the St. 9 in whole of year from 2019–2023. (a) and (b) display Arctic area (> 60°N) and around northwestern Greenland, respectively. Black circles show the position of the St. 9.

Reviewer comment:

Lines 44–70: The necessity of studying past environmental changes in the NOW region is well presented. However, further explanation is needed on how the current study site differs from the nearby SIGMA-A site, especially in terms of meteorological conditions like prevailing wind directions.

Author reply:

In the Prudhoe Land, a gentle valley separates the western region from the eastern region, where the SIGMA-A site is situated. (Fig. 1b in our revised manuscript). This valley could serve as a pathway for air masses transported from inland of the Greenland Ice Sheet to descend toward the coastal region, and these air masses are likely not transported to the western side of Prudhoe Land. If that is the case, the snowpack on the western part of Prudhoe Land could contain aerosols originating from the NOW without being mixed with aerosols from the interior of the Greenland Ice Sheet.

We have added a description of how this study site differs from the nearby SIGMA-A site at line 83–87.

Reviewer comment:

Line 68–69: Rephrase for clarity.

Author reply:

We have revised the text you kindly pointed out at line 66–69.

Reviewer comment:

Lines 104–105: Add information in the Supplementary Information regarding the design and cleaning procedures of the pre-cleaned stainless-steel tools used for snow pit sampling. Clarify the cleanliness specification of the Whirl-Pak polyethylene bags (e.g., part number, manufacturer).

Author reply:

We removed the potential contamination by organic compounds in general on the material and tools used for contamination removal using ethanol, and then performed ultrasonic cleaning in ultrapure water. The ®Whirl-Pak polyethylene bags were produced by Nasco.

We have added this description at line 110, 114, 116–117.

Reviewer comment:

Because sample depth resolution varies (2 cm, 3 cm, 5–10 cm), figures such as Figure 4 should adopt a step-wise format for clarity, not dot and line format.

Author reply:

In accordance with your comment, we have changed figure plots of water stable isotopes and ion concentrations to step-wise format in Figures 2–10.

Reviewer comment:

Line 112: Provide details about possible contamination during snow sample melting and bottling. if possible, field blank should be provided.

Author reply:

We did not make a field brank in this observation. The field brank from our previous research (Kurosaki et al., 2020), which was made following the same procedure as used in this observation, was below the detection limit in the ion chromatography analysis.

Reviewer comment:

Line 117: Include specifications of the analytical column (e.g., length, diameter), model/manufacturer of standard materials, and detection limits for each ion.

Author reply:

For the cations, separation was carried out with a Dionex CG-12 (4 × 50 mm) guard column, followed by a Dionex CS12-A (4 × 250 mm) separation column. Injection volume of samples was 500 µL. MSA (20 mM) was used as eluent, and flow-rate was kept 1.0 mL min-1. Dionex CDRS600 dynamically regenerated suppressor was used for conductivity suppression before conductivity cell. For the anions, separation was obtained with a Dionex AG-18 (4 × 50 mm) guard column and Dionex AS-18 (4 × 250 mm) separation column. Injection volume of samples was 1000 µL. KOH (23 mM) was used as eluent, and flow-rate was kept 1.0 mL min-1. Dionex ADRS 600 dynamically regenerated suppressor was used for conductivity suppression before conductivity cell. The absolute calibration curve method was used for quantitative determination of each ion concentration. For the absolute calibration determination by ion chromatography, we used the standard solutions for ion chromatography produced by FUJIFILM Wako Pure Chemical corporation, diluted to 20, 50, 100, and 200 ppb with ultra-pure water. If the ion concentration of samples were outside the calibration range (> 200 ppb), it was remeasured using 500, 1000, 2000–3000, and 6000 ppb standard for the anions and 500, 1000, 2000, and 4000 ppb standard for the cations. Blanks were always evaluated before the calibration procedure. The analytical precision of the ion chromatography was < 5 % (at the measurement of 20 ppb standard). The limit of detection (LOD) was < 0.1 ppb. The limit of quantification (LOQ) was < 0.5 ppb.

We have included this text at line 123–136.

Reviewer comment:

Line 122: Specify the standard material used for stable water isotope analysis.

Author reply:

We used the ultrapure water ($\delta^{18}O = -11.583$ and $\delta D = -77.2$), Antarctic iceberg ($\delta^{18}O = -20.4$ and $\delta D = -158.7$), snowpack on the Antarctic ice Sheet ($\delta^{18}O = -46.694$ and $\delta D = -370.7$) for calibration.

We have added this description at line 146–148.

Reviewer comment:

Line 139: Present snow density alongside depth.

Author reply:

We have added the vertical snow density to our supplementary figure 3.

Reviewer comment:

Lines 143–144: Ice layers below 0.96 m in the ST9 snowpack suggest summer melting, which may affect proxy preservation. This is appropriately and kindly described in lines 163–168.

Author reply:

As you have pointed out, we also think that the ion concentrations showed high values in the ice layers due to meltwater refreezing. Therefore, we attributed the peaks at the ice layers to meltwater refreezing and the other peaks to the deposition of atmospheric aerosols. We have already described this sentence at line 201–204, 305–306, 327–329, 354–357, and 378–380.

Reviewer comment:

Line 172: Calculate annual accumulation rates using snow density for each depth interval and present average values.

Author reply:

As you have pointed out, we calculated annual accumulation rates using snow density for each depth interval.

We have described the revised annual accumulation rates at line 208–211.

Reviewer comment:

Line 183: Indicate the MSA detection limit as a line in Figure 4. Clarify dating below 3.4 m at ST9 (the conclusion mentions dating down to 4.5 m).

Author reply:

We have included the limit of detections (LOD) for each ion in captions of Figures 2 and 4–9 because they were too small to be clearly shown in the figures.

Reviewer comment:

Line 188: Include NO3- data.

Author reply:

We have deleted the relevant sentence, following the suggestion of another reviewer.

Reviewer comment:

Line 196: Use nss-Ca2+ to interpret dust transport. Since nss-K+ and nss-Mg2+ mostly show negative values, suggesting major marine influence, omit these from discussion and Table 1.

Author reply:

As you have commented out, we have removed the nss-Mg^{2+} and nss-K^{+} in our discussion and Table 1.

Reviewer comment:

Lines 197–209: Explain shortly the notable difference in $\delta^{18}O$ between the upper layer (0–0.7 m) and the deeper layer.

Author reply:

The snow stratigraphy from 0.0 to 0.96 m at St. 9 were the rounded grains, faceted crystals, or depth hoar, whereas the melt forms prevailed below 0.96 m. The $\delta^{18}O$ in the snowpack below 0.96 m were smoothed by melting, thereby seasonal variations in $\delta^{18}O$ were smaller than the upper layer.

We have described this sentence at line 181–184 and 199–201.

Reviewer comment:

Line 201: Present backward trajectory modeling results to support atmospheric transport path interpretations.

Author reply:

In accordance with your comment, we have added backward trajectory analysis (Fig. 1 in this file). We suggested that the south-to-north gradient of the $\delta^{18}O$ results from water vapor from the southern coast to northern inland area by the southerly winds. The 7-days backward trajectory of air mass arriving at St. 9 supported this suggestion, showing that the majority of air mass was transported from northern Baffin Bay and eastern NOW.

We have added the description regarding the backward trajectory analysis at line 162–174 and 226–229.

Reviewer comment:

Line 216: Interpretation in Figure 6c should align with the seasonal framework in Figure 6b.

Author reply:

We have corrected "summer to winter" to "spring-summer to autumn-winter" according to the seasonal framework of the dating in the snowpack at St. 9.

We have revised the text that you have pointed out at line 245–246.

Reviewer comment:

Lines 222, 232: Revise for clarity.

Author reply:

We have revised the text that you have pointed out at line 281–282.

Reviewer comment:

Line 239: Provide supporting data

Author reply:

We have added the 7-days backward trajectory analysis (Fig. 1 in this file). We suggested the sea salt observed in this study could be transported along a short distance pathway without reactions with H_2SO_4 and HNO_3 during transportation. The backward trajectory analysis supported this suggestion, showing that the air mass frequency passed over the eastern NOW ocean, located near the St. 9, was high throughout the year.

We have added this description at line 287–289.

Reviewer comment:

Line 278: Explain nitrate concentration increases due to melting/refreezing. if possible, explain shortly or provide references. Revise “positive peaks” to just “peaks.”

Author reply:

The NO_3^- tend to move easily with meltwater and become concentrated during refreezing (Matoba et al., 2002).

We have added this text at line 356–357.

We have corrected the “positive peaks” to “peaks” at line 354.

Reviewer comment:

Table 1: Replace nss-K^+ with K^+ and nss-Mg^{2+} with Mg^{2+} data.

Author reply:

In accordance with your comment, we have replaced nssK^+ with K^+ and nssMg^{2+} with Mg^{2+} in Table 1.

Reviewer comment:

Avoid repeating earlier content. Summarize only the most significant findings and implications.

Author reply:

We have summarized the conclusion section at line 471–486, and we have avoided some repetitions such as "western side of Prudhoe Land".

References:

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611 https://doi.org/10.1029/2019JD031668, 2020.
612 Matoba, S., Narita, H., Motoyama, H., Kamiyama, K., and Watanabe, O.: Ice core chemistry of Vestfonna Ice Cap
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