Editor decision: Publish subject to minor revisions (review by editor)

ANSWERS TO EDITOR'S COMMENTS

I carefully read your paper and also in view of your responses to previous referee reports that were not rereviewed by the referees. I came to the conclusion that it still needs further revision. Due to the numerous changes, your paper contains now several inconsistencies and also lacks essential information that was included in your responses to referees but is not included in the paper. Please thoroughly read the paper and **check for logical structure, completeness in terms of essential information and quality standards in terms of scientific and presentation quality.** Please consider my comments below as exemplary; if you see additional opportunities to improve the paper, I will highly appreciate it. After careful revision, I will be happy to accept it for publication.

We thank the Editor for her time and suggestions, and we answer all the comments one-by-one, revising the manuscript accordingly.

I. Context and implications

E1) Upon the comments by referee #2 you had already toned down the possible role of Arctic Amplification. Further Referee #4 also asked you to remove wording about AA - But yet, your abstract prominently starts with 'Arctic Amplification' implying that this is the topic of the paper. In the remainder of the manuscript, Arctic Amplification is only mentioned at the very end of Sect 4 in a rather vague and general sentence where I even wonder whether AA could be replaced by 'interannual variability'. If AA is the main topic of the paper, it should be discussed and also show up in the discussion and conclusion sections; if not, the abstract should be modified such that it properly introduces the topic.

A1) We thank the Editor for this valuable suggestion. Our initial intention in mentioning Arctic Amplification (AA) was to provide a broader scientific framework: while AA is not the focus of our study, it constitutes the background in which Arctic interannual variability takes place. However, we acknowledge that this framing may have given the impression that AA was a central topic of the manuscript, which is not the case. To avoid any ambiguity and to better reflect the actual scope of our work, we have now removed all references to AA from the abstract and the main text, and revised the manuscript accordingly.

E2) l. 383 – 389: In which figures can these trends be observed? Please elaborate this discussion (also considering my initial comment regarding AA above). If these are key findings from your analysis, they deserve a clearer description and the 'valuable insights' should be more clearly discussed.

A2) We appreciate the Editor's suggestions, and we decided to add the appropriate references to the correspondent figures and tables for major clarity. The mention to AA was removed, and the

*discussion about "valuable insights" has been extended as follows: "*These contrasting patterns suggest that variability in atmospheric circulation and oceanic state exerts a direct control on the timing and intensity of aerosol deposition to the snowpack. In particular, stronger winds and warmer air masses enhance the transport and deposition of crustal and sea-salt species, whereas negative oceanic anomalies appear to modulate their availability as sources. This highlights how combined shifts in atmospheric and oceanic conditions drive the observed interannual variability of ionic and elemental concentrations in surface snow."

E3) Conclusions:

The conclusions can still be improved:Structure them according to our author guidelines (see below) and pay particular attention to the last point. Currently, the main conclusions are mostly related to snow composition. Of course, snow composition is related to atmospheric composition, but it is not explicitly stated here.

ACP guidelines for the title, abstract, and concluding section: https://www.atmospheric-chemistry-and-physics.net/policies/guidelines_for_authors.html

*A3) We thank the Editor for this kind suggestion, and we agreed to improve the conclusions following the guidelines for concluding section. Conclusions were modified including main quantitative results (e.g., "*the highest concentrations of marine species recorded in late spring 2020 (e.g., Cl^- = 110 mg m^{-2}, Na^+ = 52 mg m^{-2}, SO_4^{2-} = 28 mg m^{-2}). These elevated levels coincided with the most extensive sea ice in Kongsfjorden in March 2020 (FI = 113.28 km^2, DI = 16.53 km^2),"*), and providing a comparison with previous studies to put them in context (e.g., "[…]*as also indicated by elevated Br_{enr} mean values (17.7 compared to the usual < 1; Barbaro et al., 2021)", "[…] This approach mitigates potential biases for EFs results that may come from reference element selection, assumptions about crustal composition, and background variability (Reimann and de Caritat, 2000)."*). Caveats and limitations were clearly stated (e.g., "*Limitations such as weak correlations between meteorological variables and concentrations suggest that further multivariate and long-term analyses are needed to quantify these relationships more robustly."*), and implications were concisely presented.*

II. Referee comments & responses

E4) Several pieces of information that is in the reply to reviewers has not been implemented into the text. Examples are included below, but I ask you to carefully go through all responses and see what essential information may be interesting for the reader:

A4) We thank the Editor for raising up this point. We carefully checked all the replies to reviewers and we think the most crucial part are now reported within the manuscript.

In the main text, we added this part, which was clarified answering to R-46 (Referee#1) in l. 677-680: "[…] a Hierarchical Cluster Analysis (HCA) method was carried out using the whole dataset, which encompasses all three sampling campaigns (2018-2021) to capture the full variability in sulphate sources and atmospheric processes across different seasons and years.".

Additionally, we added this clarification (answer to R-17, Referee#1) in l. 256-258: "Average loads are presented in place of medians to avoid underrepresentation of the occasional high concentration events, which are critical for understanding the snowpack chemistry dynamics in the Arctic environment."

*and this sentence in l. 553-555 "*The coefficient 0.12 is based on the molar ratio of SO_4^{2-} to Na^+, while the value 0.175 represents the observed slope corresponding to the pre-industrial nss-SO_4^{2-}/nss-Ca ratio in mineral dust found in snow (Schwikowski et al., 1999, and reference therein).*"*

E4.1: Sampling strategy of collecting the upper 3 cm as pointed out by Referee #2. There is a lengthy discussion in your response – however, this seems important information and its essence should be moved to the text or at least into a separate supplemental section, referenced in the main text.

A4.1) We changed this part as follows, trying to capture the main essence of the discussion with Referee #2:

l. 97-108 "[…] Each sample was collected 10 cm apart from the previous one, along a precise path. This method was designed to minimise the temporal variability between consecutive samples and reduce the impact of potential spatial variability (within the 5-15% range, according to Spolaor et al., 2019).

During the first sampling campaign, carried out from October 4th, 2018 to May 10th, 2019, 133 snow samples were collected at the Gruvebadet Snow Research Site (GSRS), a clean-area located about 1 km south of Ny-Ålesund, nearby the Gruvebadet Atmospheric Laboratory (GAL), dedicated to the chemical and physical monitoring of the seasonal snowpack (Scoto et al., 2023; Fig. 1).

The surface snow was sampled within the upper 3 cm, as this layer is the most directly affected by atmospheric deposition and snow-atmosphere exchanges (Spolaor et al., 2018, 2021b). Sampling only the uppermost layer reduces the risk of signal dilution in deeper snow layers, and ensures a simple, robust protocol suitable for long-term campaigns with minimal disturbance of the snowpack."

E4.2: "A more focused and contextualized discussion is necessary to justify the broader implications claimed by the authors."

Yet, the abstract only states "By comparing the snow chemical composition of the 2019-20 season with 2018-19 and 2020-21, we provide insights into the interplay between short-term meteorological variability and the long-term climatic impacts of AA in Svalbard, as well as associated shifts in aerosol production process. "

A4.2: We removed any AA mentions in the abstract and within the whole text. These sentences were thus removed "By comparing the snow chemical composition of the 2019-20 season with 2018-19 and 2020-21, we provide insights into the interplay between short-term meteorological variability

*and the long-term climatic impacts of AA in Svalbard, as well as associated shifts in aerosol production process." and modified as follows "*This study provides a detailed characterisation of how snow chemistry in this area responds to sea ice extent, atmospheric circulation, and broader Arctic climatic variability."

III. Structure

E5) Sections 4.1 – 4.3 could still use some restructuring or even different titles. It seems that section 4.1 " Ny-Ålesund seasonal and interannual trends variability in surface snow " could end in line 392. Then a new section could start about 'enrichment factors' that could first properly introduce this factor (how is it defined?) with two subsections 'ER in surface snow' (l. 398 – 409) and 'Main ion sources and ER in seasonal snow' (current sect 4.2). Section 4.3, solely dedicated to ER of Br, seems not correctly weighted, i.e. it should be a subsection to one the previous sections. If you disagree, please make the current structuring clearer by either changing section titles or clarifying why the text as is belongs into the sections as indicated.

A5) We carefully went through these sections and we retained they could be changed as follows, to maintain the proper logical flow:

4.1 Ny-Ålesund seasonal and interannual trends variability in surface snow – removing the Pb discussion

4.2 Enrichment Factors and source attribution – with a brief description of what EFs are

4.2.1 Enrichment Factors in seasonal snow of Ny-Ålesund – presenting EFs analyses results

4.2.2 Main ion sources in seasonal snow of Ny-Ålesund – leaving this part as it was

4.2.3 Bromine enrichment

IV. Technical Comments

E6) l.90: Define MSA here, at its first use

A6) We thank the Editor for raising this point. We defined MSA as its first use, as suggested.

E7) l. 345: What are 'wintry concentrations'? It sounds rather colloquial.

A7) We see the point of the Editor and we agreed to change this expression to "wintertime concentrations".

E8) Fig 2: Please improve the figure caption. Ideally, add a, b, c, d to the panels and describe them accordingly. Right now it is rather confusing for the reader, e.g. "The green horizontal line above the wind speed graph" implies that the green line is located in the panel above the wind graph plot. "The colour of the line refers 375 to the Δh color scale, which indicates negative values of Δh" – do you mean the color of the green line? If so, can't you just state the value?

*A8) We added the panels to the figure, and we discussed them in the figure caption, as suggested by the Editor. We changed the figure caption as follows: "***Fig. 2.** a) AO Index; b) air temperature (°C); c) precipitation (mm); d) snow height (cm) and Δh snow height (cm); e) wind speed (m sec^{-1}) and wind direction (°) from the NCEP/NCAR Reanalysis data. The green horizontal line in panel e) indicates the 5 m sec^{-1} threshold, above which wind drift may occur on surface snow layers. The colour of this line refers to the Δh color scale shown in panel d), where green indicates negative values of Δh. NOAA Physical Sciences Lab's daily composites tool was used to calculate the near-surface air temperatures across the Northern Hemisphere from October to May. Grey bands across the panels indicate the winter periods.*"*

E9) Figure 3: Improve the caption. At a minimum, describe the panels from top to bottom (right now, you first explain the bottom plot, followed by the top left, moving eventually to the right …). Explain all species, incl Br(enr). Why is the windspeed plot shown twice?

*A9) We accept the Editor's request and we improved the caption of Fig. 3, modifying it as follows "*Ionic loads (mg m^{-2}) of Na^+, Cl^-, Mg, SO_4^{2-}, nss-SO_4^{2-}, MSA, Br^-, Br_{enr}, Ca, Sr, Mn, Fe, Al, Pb, V, Ni in the surface snow for the three sampling campaigns: 2018-19, 2019-20, 2020-21. Seasonal trends are here presented for specific elements to provide a detailed view of how concentrations vary across distinct sampling periods. Precipitation trends (mm), wind speed (m sec^{-1}), and wind direction (°) are reported twice for visual clarity."

The panels were arranged in two columns, separating marine species from crustal and anthropogenic ones. Instead of describing the panels strictly from top to bottom, we chose to group together species showing similar trends or peaks, as this allowed us to provide a more cohesive and integrated discussion of the results. We therefore believe that a purely sequential description of the panels would not effectively convey the relationships among the different species.

Wind speed, together with wind direction and precipitations, is shown twice for visual clarity, considering the Referee #1 suggestions:

- R-5 I suggest the concentration time series be plotted along with the precipitation data and wind direction. That way the reader can directly match fresh precipitation and sea salt (or crustal) input.)

- R-30 l.373-375: trace elements (and ions) concentration time series would be best plotted with wind direction and speed.

E10) Fig 5: Improve the figure caption such that its content is self-explanatory. Are these empty cells for MSA in 2019-2020? If so why?

*A10) We changed the figure caption as follows, for clarity: "***Fig. 5.** Spearman correlation plots. a) Correlation plot for the three sampling seasons; b) Correlation plot for the 2018-2019 season; c) Correlation plot for the 2019-2020 season; d) Correlation plot for the 2020-2021season. Higher positive correlations are presented in shades of red, while lower negative correlations are shown in shades of blue.*"*

For the 2019-2020 correlation plot, the "empty" cells referred to MSA were values approaching to zero, as indicated by the previous legend. However, for clarity, we have now changed the colour scale.

E11) l. 521 – 526: try to break these very long sentences in shorter ones or remove unnecessary information.

*A11) We thank the Editor for raising this point. We are not sure the Editor was referring to l.521-526 or the following paragraph. However, we changed both as follows, breaking these very long sentences in shorter ones: "*Contrarily, in the 2018-19 season, sea ice lasted only until April and was restricted to the inner, shallower parts of Kongsfjorden (Assmy et al., 2023). This limited duration may not have provided enough time with adequate sunlight for substantial biological activity to accumulate beneath or within the ice. This occurred despite the dominance in 2019, unlike the following year, of *Phaeocystis pouchetii,* a phytoplankton species known for its capacity to generate DMS in significant quantities (Assmy et al., 2023).

Finally, the ammonium (NH_4^+) load showed positive correlations with several ions (Fig. 5). It was strongly correlated with Na^+ (ρ_{load} = 0.71, p-value < 0.05), Cl^- (ρ_{load} = 0.52, p-value < 0.05) and K^+ (ρ_{load} = 0.72, p-value < 0.05). Positive correlations were also observed with SO_4^{2-} (ρ_{load} = 0.54, p-value < 0.05), NO_3^- (ρ_{load} = 0.45, p-value < 0.05), MSA (ρ_{load} = 0.37, p-value < 0.05) and Br^- (ρ_{load} = 0.53, p-value < 0.05). These results suggest a close link with sea-salt ions and biogenic emissions. However, some contribution from biomass burning events and potential influence from anthropogenic activities cannot be excluded."