

Black: Comments from reviewer 1

Blue: Replies

Comment R1-1

I have read the manuscript “Oleamide as a novel insoluble organic marker for reconstructing Arctic wildfire history in the ice core from southeastern Greenland” with interest. The authors investigate the SE-Dome II ice core from South Greenland by applying different methods to derive the mineralogy and composition of microparticles. They identified, among others, the organic aerosol oleamide and discuss its application as a proxy for past wildfires in the boreal regions of the northern hemisphere. I enjoyed reading this manuscript and congratulate the authors on this interesting and sound approach. However, at its current stage, I suggest major revisions. This is mainly due to the manuscript’s unclear structure, which could be greatly improved to better support the interesting scientific findings. Below, I present my general and specific/technical remarks.

Thank you very much for your thorough review and for your highly encouraging remarks regarding our study. We are truly pleased that you found the manuscript engaging and recognized the value of our approach. We fully agree with your critique regarding the structure of the manuscript. Taking your valuable suggestions into account, we will comprehensively reorganize and rewrite key sections of the paper to enhance its logical flow and clarity. Specifically, we will restructure the manuscript to better highlight the identification of oleamide and its proxy application for past wildfires. We believe these substantial revisions will greatly strengthen the manuscript and make our scientific findings much clearer to the readers.

Comment R1-2

General comments

My main concern with this manuscript is structure, which can be confusing and repetitive at times. The Introduction and most of the Methods read well and contain the necessary information. However, the Results section is very short and omits most of the actual results, while including some details that belong in the Methods section (e.g., l. 126-129 and l. 146-148). Further, methodological details appear several times in the Discussion, which disrupts the reading flow. Below, I provide some suggestions for restructuring the text to improve readability and understanding.

We fully agree that strictly separating the Methods, Results, and Discussion sections will greatly improve the logical flow and readability of the paper. In accordance with your suggestion, we will undertake a comprehensive reorganization of the text: all methodological descriptions currently located in the Results (including lines 126–129 and 146–148 in the original manuscript) and Discussion sections will be moved to Methods. We will revise the Results section to provide a fuller account of the actual results that were previously omitted. Discussion will be thoroughly revised to focus exclusively on interpreting our findings, contextualizing them within the published literature, and evaluating their broader scientific implications. We will also address all of the specific comments you brought to our attention in the revised manuscript. Please find our point-by-point responses below.

Comment R1-3

I would also welcome more details on data processing and analysis. For example, have Raman spectra data been smoothed and calibrated? How much effort/emphasis was placed on identifying dust minerals or unidentified spectra? This is hard to quantify, and identifying spectra can be tedious; it is thus perfectly fine to focus on specific groups of spectra/minerals.

We appreciate the opportunity to clarify our analytical workflow and spectral identification criteria. We will expand Methods to include specific details regarding Raman data smoothing and calibration procedures. Regarding mineral identification, because the specific identification of individual dust minerals proved challenging, we will classify these spectra under the broader category of “minerals,” following your helpful advice. Furthermore, in response to your comment regarding our citation review, we will incorporate key references to prior studies on black carbon (BC). To maintain a concise narrative, we will remove standard descriptions of well-known BC characteristics and instead revise the Discussion to

focus more sharply on oleamide—a novel key finding of this study—rather than discussing organic matter in overly broad terms.

Comment R1-4

Section 4.3: This first sentence should occur earlier to motivate your tests regarding NPs. The following sentences explain the method and should therefore move to the Methods section. The second and third paragraphs present the results and should therefore move to the Results section. For the Discussion, summarise your findings and put them into context with previous research as you did in the last paragraphs and nicely summarise in the very last sentence.

We will reorganize this entire manuscript exactly as recommended, ensuring that each section contains the appropriate content, as follows:

- **Motivation:** The introductory sentence motivating our investigation into ice-nucleating particles (INPs) will be moved earlier to establish a clear rationale and scope for the experiments.
- **Methods (Section 2):** The sentences detailing experimental procedures and droplet-freezing assays will be relocated to this section.
- **Results (Section 3):** The second and third paragraphs of Section 4.3. in previous manuscript, which detail the measured freezing temperatures and ice nucleation activities will be transferred to this section.
- **Discussion (especially Section 4.3):** This section will strictly focus on summarizing our findings and placing them in the context of previous research, particularly previous atmospheric and ice-core studies, and conclude with an implication of this work.

Comment R1-5

Lastly, the approach of characterisation and dealing with the “unidentified particles” can be confusing as it mixes results and discussion. In my experience, spectra (including those so far unidentified/not observed) should be mentioned in the Results, aligned with the identification of the respective minerals. The Discussion can then discuss e.g. particle origins, chemical reactions etc. Currently, you try to build a story of identifying these particles, which becomes confusing throughout the manuscript (e.g., figures displaying “unidentified particles” and the identified mineralogy directly below). The Results section is very short; consider framing the identification of these spectra as a Result.

We fully agree with this constructive criticism. In accordance with your suggestion, we will completely restructure these sections. **1) Removal of "Unidentified Particles":** We will remove the term “Unidentified Particles” throughout the entire manuscript, including all figures and figure captions. All analyzed spectra will be clearly classified and presented under their respective mineral and organic categories. **2) Reframing Spectral Identification as a Result:** All descriptions detailing spectral features, matching criteria, and microparticle identification—including the definitive identification of oleamide—will be moved from the Discussion into Results. This revision will ensure that the spectra, including those previously unidentified, are presented alongside the identification of the respective minerals, , thereby strengthening the Results section. **3) Refocused Discussion:** Discussion will be thoroughly revised to focus strictly on interpreting the results, including particle origins, potential transport mechanisms, atmospheric chemistry, and the implications of oleamide as a proxy for past wildfires.

Specific and technical comments

Comment R1-6

Title: Mention the used ice core or change it to “...history in an ice core from...”

We will change this phrasing to “history in an ice core from”.

Comment R1-7

I. 15: To avoid confusion I would place “in ice cores” behind “tracers” or change it to “from ice cores”

We will change to “from ice cores” .

Comment R1-8

I. 16: consider adding something like "... (covering the years 1799-2000)..."

We will include the phrase "covering the years" in the revised manuscript.

Comment R1-9

I. 22: Can you specify long-range and long-term?

We will specify "long-range" and "long-term"

Comment R1-10

I. 24: I might have missed something, but I don't think you can define the geographic location of wildfires with your method. "Arctic wildfire" history might thus be misleading. Further, it would need more elaboration how you explore the fire-climate interaction?

You are right and thank you for this kind comment. We will delete "Arctic" and "fire-climate interaction" from the abstract.

Comment R1-11

I. 34: Why does the complex chemical structure and environmental impact of OAs make elucidating them essential? I understand the general idea, but this should be rephrased to strengthen the motivation.

We will mention the reason why the complex chemical structure and environmental impact of OAs make elucidating them essential in the revised manuscript. Specifically, we will add "and ice nucleation ability of OA is different in each chemical structures" and will explain the importance and motivation to know ice nucleation ability of the fire-derived aerosol.

Comment R1-12

I.41: What do you mean with fire-climate interaction? Temporally, fires (hours to weeks) and climate (>decades) work on different scales, please explain.

After following your comment, we consider that it is inadequate to mention fire-climate interaction here. We will change to "forest fire activity".

Comment R1-13

I. 49: 1 m water equivalent per year, later abbreviated – be consistent.

We will correct here following your suggestion.

Comment R1-14

I. 51-54: These claims need some references.

We will add references of Avak et al., 2019 and Baccolo et al., 2021.

Avak, S. E., Trachsel, J. C., Edebeli, J., Brüttsch, S., Bartels-Rausch, T., Schneebeli, M., Schwikowski, M., and Eichler, A.: Melt-Induced Fractionation of Major Ions and Trace Elements in an Alpine Snowpack, JGR Earth Surface, 124, 1647–1657, <https://doi.org/10.1029/2019JF005026>, 2019.

Baccolo, G., Delmonte, B., Di Stefano, E., Cibir, G., Crotti, I., Frezzotti, M., Hampai, D., Iizuka, Y., Marcelli, A., and Maggi, V.: Deep ice as a geochemical reactor: insights from iron speciation and mineralogy of dust in the Talos Dome ice core (East Antarctica), The Cryosphere, 15, 4807–4822, <https://doi.org/10.5194/tc-15-4807-2021>, 2021.

Comment R1-15

I. 60: since this is a geochemical study, was it dry or wet drilled?

We will add "dry-type mechanical drill".

Comment R1-16

I. 63: 1.04 m w.e. according to Kawakami et al. (2023) I.64: is enabled.

We will correct from "1.02" to "1.04" and "is enabled".

Comment R1-17

I. 65: mention that NH₄ data are not available for the deepest depths.
We will add the sentence “The NH₄⁺ data are not available for the deepest depths”.

Comment R1-18

I. 69: How much was roughly removed?
We will add the sentence “the surface layer of the ice core was removed about 3 mm thickness”.

Comment R1-19

I. 70: no space between number and °C.
We will delete the spaces between number and °C through the manuscript.

Comment R1-20

I. 74: could be confusing if you refer to ice samples or the particles within, which are samples from hereon. Maybe replace with “particles” to stay consistent with the subsection heading. In the following subsection, you mention aluminium foil samples; try to be consistent.
We will change the words from “ice samples” to “the particles” or “aluminium foil” here and in the following subsection.

Comment R1-21

I. 78: Consider placing table A1 here. Are the optical observations done with the raman set-up or a different microscope? How were the foil samples treated to avoid contamination during the measurement/sample placement.
Table A1 is currently placed at the end of the manuscript in accordance with the journal submission guidelines, but we agree that it should be referenced and placed closer to this section in the final layout. The optical observations were performed using a microscope installed directly within the Raman facility. This setup effectively prevents contamination that could occur when transferring equipment or samples between locations. Additionally, the aluminum foil supporting the particles is stored in a sealed container prior to analysis and placed on a closed microscope stage continuously purged with dry air during measurement. We will add these specific methodological details to the revised manuscript.

Comment R1-22

L. 79: A total of 1139 particles from 15 depths results in an average of 76 spectra per sample or did I miss anything? Please also give some more information regarding the conditions and set-up (e.g. is it located in a clean lab?). Clarify the different gratings, which was used for what? 2 accumulations are comparably few, is there a reason to not use more accumulations but less accumulation time?
We apologize for the counting error in the original manuscript. We will correct the total count to 998 in the revised manuscript. We will also include additional details regarding the experimental conditions and setup. Specifically, the measurements were conducted using two accumulations, following standard procedure in our laboratory for Raman analysis of ice cores (e.g., Ohno et al., 2005; 2006). During these analyses, the Raman laser beam was directed at microparticles within the ice; however, measurements requiring longer acquisition times or repeated exposures often failed because the particles migrated within the ice matrix. Thank you for your valuable advice. In future analytical work, we will increase the number of accumulation cycles accordingly when taking these measurements.

Ohno, H., Igarashi, M., and Hondoh, T.: Salt inclusions in polar ice core: Location and chemical form of water-soluble impurities, *Earth and Planetary Science Letters*, 232, 171–178, <https://doi.org/10.1016/j.epsl.2005.01.001>, 2005.

Ohno, H., Igarashi, M., and Hondoh, T.: Characteristics of salt inclusions in polar ice from Dome Fuji, East Antarctica, *Geophysical Research Letters*, 33, 2006GL025774, <https://doi.org/10.1029/2006GL025774>, 2006.

Comment R1-23

I. 88: With optical methods or did you also conduct some geochemical tests?

We will describe in the revised manuscript “With optical methods”.

Comment R1-24

I. 92: SEM-EDS not explained, avoid the abbreviation

we will change here “scanning electron microscope with an energy-dispersive X-ray spectrometer (SEM-EDS)”.

Comment R1-25

I. 94: Is there a specific reason for the 1908 and 1911 layers?

The selection of the 1908 and 1911 layers for SEM-EDS analysis was conducted as a representative initial screening to evaluate particle elemental compositions in key depth horizons, rather than an arbitrary choice.

Comment R1-26

I. 100: Could you quantify how often “rarely” is? With 30 analysed spots, this could be important.

We will specify the detection frequencies for the other elements in the revised manuscript.

Comment R1-27

I. 106: Again, is there a reason for the 1804 layer – why not staying consistent with SEM-EDS and analyse 1908 or 1911? You here also mention for the first time “the unidentified spectra”, which is confusing at this part of the manuscript.

We will remove the term “unidentified spectra” from the revised manuscript. We selected the particle from the 1804 layer because it is larger than 10 µm, which makes it suitable for FTIR measurement.

Comment R1-28

Sect. 2.6: This section should be revised to clearly state the purpose and approach. The title is confusing at first as ice core samples are already frozen and the melting process and the thinking behind it are not displayed. Did you choose a specific part of the sample, or did you melt a random area (or everything) and then took a random droplet? You also have time-lapse footage, why not show the device and some examples?

We will add the purpose of the research and overall approach to the Introduction section. Additionally, since the substance measured was the oleamide reagent, we will correct the description accordingly. We will also include a representative photograph taken by the time-lapse camera in the Appendix.

Comment R1-29

I. 150: What is “high”? An absolute value or a relative one in comparison to other samples or the surroundings?

Considering the comment in Rev 2 (that the presence of an ammonium ion peak does not necessarily indicate a forest fire event), we have decided to delete this sentence from the revised manuscript.

Comment R1-30

I. 151: replace “present” with “observed”, they might be there but not analysed/identified

We will replace as “observed”.

Comment R1-31

I. 152: be more specific about the “specific sample layers”

We will change to describe specific time-series variations in inorganic compounds, BC, and graphite in the revised manuscript.

Comment R1-32

I. 153: higher absolute frequencies or in relation to the number of identified spectra? You mention it in the last sentence on page 5, but this is hard to put in context without knowing all relative and absolute abundancies.

Specifically, we mean “in relation to the number of identified spectra.” We will revise the relevant sentences accordingly to clarify this point in the revised manuscript.

Comment R1-33

I. 155: This feels a bit out of place, and I am not sure if it is statistically sound considering the missing NH₄ data in the deepest 5 samples and the varying numbers of analysed/identified spectra per sample.

We will delete the description for all statical treatment of NH₄ data in the revised manuscript.

Comment R1-34

Section 4.1: The paragraphs need refining and restructuring as they currently mix different aspects (e.g. Raman spectra and particle size and shape from SEM-EDS). Further, I would move this section up into the Result and restructure the Results sections into 1) Raman spectra (known spectra/minerals and newly observed ones), 2) SEM-EDS results, 3) FT-IR, and 4) ice nucleating (mentioned in the Methods but not in the Results?).

A perk of Raman spectroscopy is the possibility to identify minerals, not just classes. “Mineral dust” has thus to be explained more by e.g. identifying typical minerals (quartz, feldspar, mica, etc) and then group them for the purpose of this paper as mineral dust.

As you pointed out, we will completely revise Section 4.1, and in the revised manuscript, we will distribute the results of the Raman, SEM, FTIR, and ice nucleation experiments across their respective subsections in Section 3 (Results). Accordingly, we will remove Section 4.1 from the Discussion. Furthermore, recognizing that detailed mineral classification is challenging, we will state in the revised text that we identified typical minerals (such as quartz, feldspar, and mica) and grouped them under the broader category of “mineral dust” for the purpose of this paper.

Comment R1-35

I. 164: Raman spectroscopy enables the identification of the unique mineral (structure) (if Raman-active) and thus only indirectly the chemical composition, which can be highly complex for certain minerals.

Please find the reply to comment R1-34.

Comment R1-36

I. 165: BB is defined several times; define it here and delete the rest.

Where “biomass burning” first appears, we will introduce the abbreviation “BB” alongside it and subsequently use “BB” throughout the remainder of the manuscript.

Comment R1-37

I. 169: Would be good to include studies from ice cores here. BC was identified in Greenland by e.g. Eichler et al., 2017, Stoll et al., 2022, 2023 and in Antarctica by e.g. Eichler et al., 2019.

We sincerely apologize for this oversight. We had inadvertently overlooked this relevant literature. Thank you for bringing this to our attention. In the revised manuscript, we will cite the study by Eichler et al. (2019) and frame these findings as already established in prior ice-core research.

Comment R1-38

I. 184: switch the sentence about acid peaks with the sentence above it or clarify “these acid standards” again.

We will switch the two sentences in the revised manuscript.

Comment R1-39

Sect. 4.2: This is arguably the most interesting section of the manuscript and should thus lead the discussion (4.1 and shift current 4.1 to Results). Similarly, I. 214-215 are also results (see comments above about absolute and relative counts). I would further be interested in discussing your method in regard to established methods using e.g. NH₄ and if it could also be applied to Antarctica.

We will move Section 4.1 of the Discussion and lines 214–215 from the original manuscript to Section 3 (Results). Furthermore, we are uncertain whether the NH₄⁺ peak method can be effectively applied to Antarctica, given that NH₄⁺ concentrations in Antarctic ice cores are generally extremely low, even in coastal ice cores.

Comment R1-40

I. 207: Repetition from Results section, combine with the following sentence a la “the observed correlation may be attributed...”

We will delete the sentence (mentioning about correlation coefficient) from the revised manuscript.

Comment R1-41

I. 211: what do you mean with “structurally ordered”?

We intend to mention the internal mixtures of several substances. We will revise the sentence.

Comment R1-42

I. 232: citations for stable long-range transport and low reactivity within ice matrix or clarify that this is an assumption? The following sentences are good and an important finding; consider restructuring to give it more emphasis.

We are unaware of any prior studies regarding the preservation of oleamide in an ice matrix. Therefore, we will explicitly state in the revised manuscript that these remain working assumptions

Comment R1-43

I. 237-239: Repetitive and should not be the first sentence, consider starting this paragraph with your second sentence.

We will delete the first sentence.

Comment R1-44

I. 245: Is it really limited to Arctic wildfires or could they also be from lower latitudes? Occurs throughout the text, change accordingly.

We will delete “Arctic” from the revised manuscript.

Comment R1-45

I. 295: combine with first one-sentence paragraph

We will combine with first one-sentence paragraph

Comment R1-46

I. 308: are these suggestions realistic for ice cores?

We will revise here to “such as mass spectrometer combined with a gas chromatograph (Mazur et al., 2020)”.

Mazur, D. M., Latkin, T. B., Kosyakov, D. S., Kozhevnikov, A. Y., Ul'yanovskii, N. V., Kirilov, A. G., and Lebedev, A. T.: Arctic snow pollution: A GC-HRMS case study of Franz Joseph Land archipelago, *Environmental Pollution*, 265, 114885, <https://doi.org/10.1016/j.envpol.2020.114885>, 2020.

Comment R1-47

I. 314: Do you really characterise the morphology in detail? OAs and FT-IR abbreviations not needed as not used in the conclusion.

We will delete the “morphology” and abbreviations from the revised manuscript.

Comment R1-48

I. 324: I would be careful with these claims. Your single particle data only goes back to 1908 or 1804 and there is a growing amount of evidence of chemical reactions taking place in (old/deep) ice. In my opinion, it remains to be shown if OAs are stable in deep ice, but this

would be a great follow-up study on longer ice cores.

As you correctly pointed out, there is no evidence to suggest that oleamide is better preserved in older ice. Accordingly, we will delete the corresponding sentence on line 324 of the original manuscript.

Comment R1-49

I. 332: Elaborate on the feedback with the warming polar climate? How can past wildfires help here?

We will add a paragraph at the end of the Discussion section to address the feedback mechanisms associated with a warming polar climate and the importance of reconstructing past wildfire activity.

Comment R1-50

Fig.1: I suggest removing the (i)-(o) labels and add two headers, i.e. Raman spectra and microscopic images, combining the rows. Thus, the last sentence in the caption could be deleted. The red dots in (i) and (j) prohibits to see the analysed particle, maybe you can come up with a better idea. As mentioned above, having “unidentified particle X” and the identified particle in brackets below does not make sense.

(h) could be an indication of fluorescence as also seen in other Greenlandic ice cores, e.g. EGRIP (Stoll et al., 2022).

As you pointed out, we will remove the labels (i) through (o) and add two headings to improve clarity. Additionally, in the revised manuscript, we will remove all references to “unidentified particle X”. Regarding panel (h), we will cite Stoll et al. (2022) and note that the observed feature may be attributed to fluorescence

Stoll, N., Hörhold, M., Erhardt, T., Eichler, J., Jensen, C., and Weikusat, I.: Microstructure, micro-inclusions, and mineralogy along the EGRIP (East Greenland Ice Core Project) ice core – Part 2: Implications for palaeo-mineralogy, *The Cryosphere*, 16, 667–688, <https://doi.org/10.5194/tc-16-667-2022>, 2022.

Comment R1-51

Fig. 2: Consider putting this figure in the appendix and focusing only on the identified particles. It’s good to get an impression of unidentified spectra, but the main information discussed here are the known spectra.

Is there a specific reason for the comparably high number of analysed particles in the 1804 and 1908 layers? Was this on purpose or just random? The green “Mineral” part in the legend is unclear, do you refer to “mineral dust (e.g. mica, quartz, feldspar)” here?

It can be tricky to identify Raman spectra, it would be good to explain how much work was spent to identify particles. This is not the focus of this study, but it might draw a biased image if efforts to identify particles differed.

We thank the reviewer for this helpful suggestion. We agree that focusing the main text on identified particles enhances the clarity and flow of the manuscript. As suggested, in the revised manuscript, we will focus exclusively on the identified particle spectra. The higher number of analyzed particles in the 1804 and 1908 layers was not intentional, but rather reflected the significantly higher particle concentration on the aluminum foil. Under our standardized optical scanning and sampling protocol, a higher density of particles naturally yielded a larger number of measurable single particles in these layers. Regarding the terminology, “mineral” refers to mineral dust species, primarily consisting of silicates and related minerals such as quartz, feldspar, and mica. To remove any ambiguity, we will update the figure legend, figure captions, and relevant text in the revised manuscript to explicitly state “Mineral dust (e.g., quartz, feldspar, mica)”. In the revised text, we will explicitly emphasize that a standardized, uniform identification workflow was applied to all analyzed particles across all ice core layers, ensuring that equal effort and identical evaluation criteria were used regardless of the sample year.

Comment R1-52

Fig.3 Caption: You can delete the SEM-EDS abbreviation. Consider adding “Al” in (b) to the

prominent peak described.

We will delete the “SEM-EDS abbreviation” from the caption and will add the “Al” in the EDS spectrum.

Comment R1-53

Fig.4 Caption: Please explain why the red- and green bands and the respective wavenumber ranges matter.

The red- and green-shaded regions correspond to C–H stretching and carbonyl absorption bands, respectively. We will add this explanation to the figure caption in the revised manuscript

Comment R1-54

Fig. 5 Caption: Please mention where the provided spectra are from, did you measure them yourself or used a database such as RRUFF?

We will describe in the caption of the revised manuscript that “These particles and reagents were analyzed using the analytical instruments employed in this study”.

Comment R1-55

Fig. A1: As far as I see it, the 1799 layer is only partly covered by data. The available/visible data do not seem enough to assess if this is a “strong” layer as the NO₃ concentration does not exceed 100 ug/l, which is roughly the average seasonal peak concentration. I might have missed it, but is there are other data supporting the analysis of this specific layer? It’s the deepest/oldest data, which is a fine reason to analyse it, but this should be mentioned.

Maybe also mention where the dating comes from.

Because the timescale for 1799 was only partially established, the ion data for 1799 represents only the concentration of the specific layer analyzed for microparticles.

Specifically, the NO₃⁻ concentration is 77 ppb, and we will report these values in the revised manuscript. Furthermore, as Reviewer 2 correctly pointed out, NH₄⁺ and NO₃⁻ peaks do not necessarily coincide with wildfire events. Therefore, we will substantially revise our logical framework regarding the relationship between the oleamide-detected layers and wildfire events. Finally, we will clearly state the source of the timescale (Kawakami et al., 2023; Hamamoto et al., 2026) in the revised text.

Hamamoto, S., Iizuka, Y., Aizawa, T., Kawakami, K., Matsumoto, M., Matoba, S., Bong, H., Yoshimura, K., Okazaki, A., Dietrich, L. J., Steen-Larsen, H. C., and Uemura, R.: Isotope-Based Age Scale with Multi-Month Precision for the SE-Dome II Ice Core from Southeast Greenland Over the Past 150 Years, <https://doi.org/10.5194/egusphere-2026-3602>, 9 July 2026.

Kawakami, K., Iizuka, Y., Sasage, M., Matsumoto, M., Saito, T., Hori, A., Ishino, S., Fujita, S., Fujita, K., Takasugi, K., Hatakeyama, T., Hamamoto, S., Watari, A., Esashi, N., Otsuka, M., Uemura, R., Horiuchi, K., Minowa, M., Hattori, S., Aoki, T., Hirabayashi, M., Kawamura, K., and Matoba, S.: SE-Dome II Ice Core Dating With Half-Year Precision: Increasing Melting Events From 1799 to 2020 in Southeastern Greenland, *JGR Atmospheres*, 128, e2023JD038874, <https://doi.org/10.1029/2023JD038874>, 2023.

Comment R1-56

Fig. A2: Consider a clearer heading of the three panels with the fitting heading on the left or in the middle of the panel. Currently, it is a bit tricky to distinguish the second and third panel. We will revise the heading of the three panels.

Comment R1-57

Table A1: Can you really date the ice layers to fractions of individual years? Based on the referenced paper, I assume the age is calculated but this should be applied with caution on this scale. From taking samples to cutting them into different sections, it is highly unlikely to date a layer to e.g. 1804.2-1804.5 CE.

Is there a specific reason why there is no NH₄ data below 114.6 m?

To distinguish high concentrations/layers, it might be useful to include a (running, yearly, 10

year) average concentration or something similar.

We will clearly state the source of the timescale (Kawakami et al., 2023; Hamamoto et al., 2026) in the revised manuscript. As shown by Hamamoto et al. (2026), the average uncertainty of this timescale is less than two months for the period 1871–2020. As you correctly pointed out, NH_4^+ was not detected at depths below 114.6 m. In the revised manuscript, we will clarify the potential reasons for this analytical limitation. Specifically, this non-detection is likely attributable to two factors: first, our measurements were conducted using discrete ice samples rather than continuous flow analysis (CFA); second, the high accumulation rate in the SE-Dome region dilutes ion concentrations relative to other Greenland sites (e.g., GRIP, NEEM). Assuming a constant NH_4^+ flux, an accumulation rate five times higher reduces the baseline concentration to one-fifth, which likely caused the concentration to fall below our analytical detection limit.

Comment R1-58

References

l. 420: x+375 pp; is this a typo?

We will correct the typo.

Looking forward to reading the revised version. Best,

Nicolas Stoll

We sincerely thank you for these insightful and constructive comments. Thanks to your valuable feedback, we believe that the revised manuscript will be substantially improved.