

Response to Referee Comment 2, point by point

egusphere-2026-3543

Companion to the revised manuscript (v11) and its Supplement (v7)

How to read this

The report is written as prose keyed to line numbers of the discussion paper. We have numbered its requests R2-1 to R2-10 in the order they appear; the quoted text is the referee’s own. Several points had already been addressed in the revision made for RC1, and R2-3 is the same finding as RC1’s A2–A4, so these are cross-referenced rather than repeated. Section, table and figure numbers under “Where” and “Carried into” are those of the revised manuscript and Supplement, which will be submitted to the editor after this discussion and differ in places from the preprint.

Summary

	Referee’s point	Verdict	Carried into
R2-1	Cite MOSAiC in the introduction	Added	Sect. 1
R2-2	Drop campaign-wide medians	Accepted in full	Abstract; Sects. 3.1, 4.2, 4.3; Supp. Table S11
R2-3	Arctic floor at 3 L^{-1} inconsistent with Fig. 4b	Accepted ; same finding as RC1	RC1QnA.pdf A2–A4; Sect. 2.6; Table 2
R2-4	Attribution limited to marine biogenic and dust	Accepted ; qualifier narrowed	Sects. 1, 3.5, 4.1, 4.5, 5
R2-5	Kawai (2023)/Tobo (2019) described as mineral	Accepted ; our text was reversed	Sects. 4.1, 4.5
R2-6	Give concentrations at stated temperatures	Accepted ; changed the finding	Sect. 4.2
R2-7	Discuss PINE against the PICNIC offline methods	Accepted ; our bound was misattributed	Sect. 4.4; Supp. Sect. S19
R2-8	Storage-degradation claims too strong	Accepted ; direction withdrawn	Sect. 4.4
R2-9	Filter IS and CSU Ice Spectrometer are the same	Accepted ; table already withdrawn	Discussion-paper appendix withdrawn

	Referee's point	Verdict	Carried into
R2-10	Compare with the online (SPIN) part of Hartmann (2021)	Followed; constrains nothing	Sect. 4.4; Supp. Sect. S19

R2-1. MOSAiC in the introduction

Lines 39-53: Probably a good idea to include the MOSAiC work here in the introduction, either from Barry et al., 2025 and/or Creamean et al., 2022.

Added. The introduction now cites the MOSAiC drift as the first annual cycle of INP and bioaerosol measurements in the central Arctic, with a summer maximum dominated by heat-labile organic particles and with marine and terrestrial source tracers present in every season (see also R2-4).

Where: Sect. 1, the paragraph on shipborne and airborne Arctic campaigns. Barry2025 added to the reference list.

R2-2. Campaign-wide medians

Lines 323-325 and others: Campaign-wide median INP concentrations are not recommended. Instead, it is more interpretable to give concentrations at specific temperatures, which are already done, so I would remove these.

Accepted. The campaign-wide medians are removed from the Abstract, Sect. 3.1 and Sect. 4.2. Sect. 4.3 mentions them once, as an example of two statistics whose agreement says nothing about calibration drift, and refers to Supplement Table S11 for the values.

A headline sentence went with them. Sect. 3.1 had said that “Jeju therefore exceeds both Arctic campaigns by close to two orders of magnitude”, which rested only on the pooled medians. Taken temperature by temperature from Table 2, the ratio ranges from 2.5 (Clean-marine against ARA15A at -28°C) to 158 (Asian-dust against ARA16A at -25°C), and the text now gives this range.

Where: Abstract; Sect. 3.1; Sect. 4.2; Sect. 4.3.

R2-3. The Arctic floor at 3 L^{-1}

Figure 4: Why does the individual run data for the Arctic observations in Figure 4b show values lower than 1 L^{-1} , which makes sense as a limit of detection for the PINE, whereas the other plots show the lowest concentrations at around 3 L^{-1} ? This seems like a major point that affects interpretation of this dataset and really does not make sense.

Accepted. RC1 made the same point independently, and the full answer is in RC1QnA.pdf, items A2 to A4. In short, expansions that counted no ice crystal are now kept in the denominator, and no measurement is removed for being small. At -20°C the ARA15A daily-pooled median falls from 3.68 to 0.099 L^{-1} , and the temperature dependence the referee expected appears in the corrected spectra.

Where: Sect. 2.6; Table 2 and its caption; Figs. 3 and 4.

R2-4. Source attribution beyond marine biogenic and dust

Lines 396-397, 440-442, and others: The authors qualify that source attribution isn't definitive, but there are strong statements in this paper suggesting that anything sampled that is not mineral is marine biogenic. I would like to point out recent Arctic studies, including those over the central Arctic such as during MOSAiC, have found indications of terrestrial INPs throughout the year. This is detailed in Barry et al. 2025. Besides replacing Barry 2024, which was a student thesis, with more updated work, this is consistent with other Arctic work like Tobo et al. 2024 [...] and Perring et al., 2023 [...]

Accepted. Without compositional measurements we can argue that the warm-regime INP were probably not mineral and probably biogenic, but not that they were marine. The text has been narrowed accordingly:

- Sect. 3.5 no longer says “likely marine biogenic” and does not assign an origin.
- Sect. 4.1 no longer credits Porter et al. (2022) and Creamean et al. (2022) with identifying marine-biogenic INP as the dominant type. Creamean et al. (2022) conclude that “*strong seasonal variability in local marine, and episodic long-range transported terrestrial emissions largely control the central Arctic INP population*”.
- Sects. 4.5 and 5 now treat the open question as a three-way one. Sect. 4.5 also states the methodological consequence: heat treatment separates biogenic from mineral INP but not marine from terrestrial biogenic INP, so this dataset cannot decide between them.

Sect. 4.1 now lists terrestrial bioaerosol from snow- and ice-free land as a fourth possible mechanism, citing the references the referee suggested. Perring et al. (2023) is the most relevant to us: over the Bering and Chukchi Seas, the transit sector of both our campaigns, they found low bioaerosol loadings in the marine boundary layer and higher loadings aloft in air that had recently passed over land. Tobo et al. (2024) attribute highly active INP over Svalbard to local or regional terrestrial dust and biological material, and Hartmann et al. (2021) find that sea spray from their measured seawater falls 4 to 5 orders of magnitude short of the airborne concentrations.

We have not weakened the biogenic part of the claim, because Barry et al. (2025) support it: biological INP dominated the whole spectrum in summer, with June to August samples close to 100 % heat-labile at -15 and -20°C .

Where: Sect. 1; Sect. 3.5; Sect. 4.1; Sect. 4.5; Sect. 5. Barry2025, Tobo2024 and Perring2023 added to the reference list; Tobo2020 in that file is Tobo et al. (2019).

On replacing Barry (2024). Agreed. The thesis is the source of only one number, the MOSAiC filter value at -20°C in Sect. 4.4. We have recomputed it from the archived data; see the note at the end of this document.

R2-5. Kawai (2023) and Tobo (2019)

Line 475 and others such as 660-662: Kawai et al., 2023 utilizes a parameterization from Tobo et al. 2019, which was from glacial outwash sediments. They attribute most of the IN activity to organic matter rather than mineral, which is inconsistent with your statements in the paper detailing minerals when discussing this reference.

Accepted. Our text said the opposite of the source. Sect. 4.1 listed glaciogenic dust as a pathway that “*could contribute to warm-temperature INP without a biological mechanism*”, citing Kawai et al. (2023) and Sanchez-Marroquin et al. (2020), but the parameterisation Kawai et al. use comes from Tobo et al. (2019), who conclude:

The high ice nucleating ability of the sediments is probably governed by the presence of small amounts of organic matter (<1 wt% organic carbon) rather than mineral components.

The pathway is still listed, as an alternative to a marine source but not to a biological one, and the organic-carbon figure is quoted. The parallel sentence in Sect. 4.5 is corrected in the same way and now cites Tobo et al. (2019).

Where: Sect. 4.1, first of the four alternative mechanisms; Sect. 4.5, final paragraph.

R2-6. Concentrations at stated temperatures

Section 4.2: Best to give concentrations at associated temperatures.

Accepted. The one value in Sect. 4.2 without a temperature was the inter-campaign contrast in the 75–80° N band (0.474 against 0.087 L⁻¹). Broken down by temperature, it changes the finding:

75–80° N	ARA15A	ARA16A	ratio
pooled over T	0.474	0.087	5.4
$T = -28^{\circ}\text{C}$	1.381	0.210	6.6
$T = -25^{\circ}\text{C}$	0.417	0.071	5.9
$T = -22^{\circ}\text{C}$	0.115	0.095	1.2
$T = -20^{\circ}\text{C}$	0.096	0.088	1.1

The factor of about 5 comes entirely from the cold regime; in the warm regime the two campaigns cannot be distinguished. The sentence saying they “*order the same way at every scale examined*” has been replaced. The text also notes that ARA16A has only 2 to 3 daily periods at each temperature (17 to 20 for ARA15A), and that its -25°C cell is an upper bound, since no day reaches the detection limit of 0.137 L⁻¹.

Where: Sect. 4.2.

R2-7. PINE against the PICNIC offline methods

Line 578: I would discuss a little more between the PINE and offline measurements made during PICNIC, such as the CSU Ice Spectrometer, to be consistent with the ending of the paragraph.

Accepted. The referee’s phrase “*to be consistent with the ending of the paragraph*” points to a real problem: we bounded an online–offline offset with PINE against the CSU-CFDC (71 % of 34 points within a factor of 2), and both of those instruments are online. The paragraph now uses what Lacher et al. (2024) report about online against offline methods:

1. The offline methods disagree among themselves more than PINE disagrees with the CSU-CFDC. Relative to INSEKT, the other six fall within a factor of 2 for 19–49 % of points and within a factor of 5 for 65–91 %. The CSU Ice Spectrometer, which produced the MOSAiC record we use as the Arctic offline reference, has the lowest agreement (27 % and 65 % over 300 points).
2. Where PINE ran through full 8 h filter periods it was sometimes biased low, which Lacher et al. attribute to its 50 % size cut at 4 μm . That is the opposite of what an online recovery advantage would predict.

3. Particle break-up during immersion analysis was found to be minor below about $10\text{ }\mu\text{m}$.

One advantage of online sampling remains and is stated: it resolves variability of up to an order of magnitude within a single 8 h filter period. This concerns time resolution, not accuracy.

Where: Sect. 4.4 carries the finding and the conclusion drawn from it; the statistics behind it, including Table 3b of that paper, are set out in Supplement Sect. S19.

R2-8. Storage and under-recovery

Lines 605-606; 640-642; 670-671; 704: [...] there isn't the evidence or literature cited required to make these claims (or imply in some cases) that the INPs sampled were fragile marine that were degraded during storage, or undersampled by offline methods. These statements are way too strong and need to be scaled down. [...] It would be good to look at the storage study done by Beall et al., 2020 [...] showing that $-20\text{ }^{\circ}\text{C}$ was the best storage practice.

Accepted. Beall et al. (2020) show that storage changes INP concentrations by up to an order of magnitude in either direction. Storage at $-20\text{ }^{\circ}\text{C}$ reduces the effect without removing it: at $-11\text{ }^{\circ}\text{C}$ the average loss was 16 % at $-20\text{ }^{\circ}\text{C}$, against 51 % at room temperature, and their measurements extend only to $-19\text{ }^{\circ}\text{C}$, warmer than our comparison window. Since the sign of the storage effect is not established, the sentence in Sect. 4.4 that assumed a direction has been removed. The statement that the stronger claim “*is not supported by these measurements*” remains.

Where: Sect. 4.4. Beall2020 added to the reference list.

R2-9. Filter IS and the CSU Ice Spectrometer

Appendix: Make sure to update with Barry et al., 2025, and what is the difference between Filter IS and CSU Ice Spectrometer? They are identical but are shown separately here.

The referee is right. In our correction-factor table, “Filter IS” (the MOSAiC filters analysed with the Ice Spectrometer) and “CSU Ice Spectrometer” (Creamean et al., 2022) were the same instrument and campaign entered twice, which gave one technique double weight. The appendix containing that table has been withdrawn in full following RC1 (RC1QnA.pdf, item C1), so the duplicate no longer appears.

Where: The correction-factor appendix of the discussion paper is withdrawn; Sect. 4.4 no longer contains a method-scaling table.

R2-10. The online part of Hartmann et al. (2021)

General: It might be useful to also compare with the Hartmann et al., 2021 online portion, detailing the SPIN.

Followed, but it cannot constrain the offset. Hartmann et al. (2021) ran filters and the online SPIN chamber side by side on RV *Polarstern*, but over disjoint temperature ranges: filters at $-22\text{ }^{\circ}\text{C}$ and warmer, SPIN at $-26\text{ }^{\circ}\text{C}$ and colder (their Fig. 2). Sect. 4.4 now reports this as part of the reason for adopting no transfer function: the Arctic datasets that could constrain one either do not overlap in temperature or, as at ENA, disagree by one to two orders of magnitude.

Where: Sect. 4.4 states the null and what follows from it; the temperature coverage of the two instruments is given in Supplement Sect. S19.

An item this response leaves open

The MOSAiC comparison in Sect. 4.4 mixes estimators. We recomputed the MOSAiC value from the archived CSU Ice Spectrometer files (DOE ARM Data Archive, doi:10.5439/1804484). The published figure is confirmed: the June to August untreated *mean* at $-20\text{ }^{\circ}\text{C}$ is 0.442 L^{-1} ($n = 18$ filters). The record is skewed, however; its *median* over the same period is 0.080 L^{-1} and its geometric mean 0.132 L^{-1} . Our 0.099 L^{-1} is a daily-pooled median, so we have been comparing a median with a mean, and on a median basis the ARAON value is the higher of the two. The comparison will be restated on one estimator. Use of the archived files requires prior permission from the PI, which we have requested; the revised manuscript will either quote these statistics with that permission or make the comparison qualitatively.