

Corrections the referees did not raise

egusphere-2026-3543 — companion to RC1QnA.pdf

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

What this document is

This document lists the errors in the discussion paper and its Supplement that the referees did not raise. The answers to the referees are in RC1QnA.pdf and RC2QnA.pdf; where an item here underlies one of those answers, the question number is given. The items are grouped by kind: errors in the published text (1), quantities shown as data that were not data (2), corrections that follow from these (3), and the data archive (4). Each item ends with the place in the revised manuscript where the change is made.

Summary

	Item	Status	Bears on
1.1	asia-aq6 operation-level rejection reversed	Reversed, with evidence	Table 2 Jeju rows
1.2	Table 1 stated 20/20 operations retained, contradicting Sect. 2.5.1	Corrected to 16/17	1.1
1.3	QC scheme diagram did not match the four criteria of Sect. 2.5.2	Diagram corrected	Criterion (3)
1.4	Supplement contradicted itself on whether $\times 11.2$ was applied	Disclosed; removed	RC1 C1
1.5	Table S4 PaCE-2022 site wrong; the table described a panel the regression does not use	Withdrawn	Sea-ice panel
2.1	Supplement Figs. S3, S4 drawn from a hard-coded curve	Withdrawn	RC1 C1
2.2	Fig. 4a floor of 0.15 L^{-1} was a plotting clip constant	Corrected	RC1 A5
2.3	Fig. S1 sensitivity read a hard-coded, withdrawn slope	Recomputed	RC1 A4
3.1	Narrative sections still carried the superseded estimator	Rewritten	RC1 A4
3.2	Calibration-drift argument rested on two saturated values	Withdrawn	RC1 A4
3.3	Table S3 concluded with the reversed inter-campaign ordering	Corrected	RC1 A4
3.4	Fig. S7 computed on a different panel from Fig. 6	Recomputed	RC1 C4
3.5	Table S8 operation count; Table S10 Jeju run count	Corrected	1.1, 1.2
3.6	Sect. 3.3 latitudinal profile computed off-product, zeros excluded	Recomputed	RC1 A3
3.7	ARA16A relative-wind file held 10 of 39 days; five sixths of the campaign discarded	Rebuilt	Tables 1, 2, S6

	Item	Status	Bears on
3.8	Fig. 5 on the bin-level estimator; peak spike understated 70-fold	Recomputed	3.7
3.9	Diurnal-bias dismissal rested on quantum-saturated quartile medians	Withdrawn	3.7, RC1 A4
3.10	Condensation pass; two stale values found in the process	Corrected	RC1 D1
3.11	Wilbourn (2024) ratios 3.3–79.7 and 0.5–1.2 are not in that paper	Withdrawn	RC1 C1, C2
3.12	Barry (2024) value quoted to false precision, without its seasonal context	Corrected	3.11
3.13	Lacher (2024) temperature qualifier absent from that paper	Corrected	RC1 C4
3.14	Möhler (2021) loss quantified above $2\mu\text{m}$ after all	Corrected	RC1 C3
3.15	The -0.07 to -0.09 literature band has no traceable source	Disclosed	RC1 A4
3.16	Literature slope band derived; the steepness contrast withdrawn	Withdrawn	3.15
3.17	New Appendix A: ice crystal sizes, mostly a negative result	Added	beyond RC1
3.18	Appendix A relocated to Supplement Sect. S17	Moved	RC1 C1, D1
3.19	Criterion (2) cited a paper that documents no such threshold	Corrected	3.8
3.20	Literature says ship exhaust does not affect INP; our data disagree	Disclosed	3.8, 3.19
3.21	PINE performance cited two papers that never measured PINE	Withdrawn	RC1 C4
3.22	Hartmann et al. (2021) listed as a fixed station; it is a ship track	Corrected	Table S4; 1.5
3.23	Four model names attached to papers that do not use them	Corrected	3.36
3.24	Asian dust enhancement unsupported by all three papers cited	Replaced	Jeju spectra
3.25	Two Arctic campaigns attributed to the wrong platform	Exchanged	3.22
3.26	Gong et al. (2020), a Cabo Verde study, cited for the Arctic MIZ	Removed; relocated	3.17
3.27	The instrument named for the FBAP input is not the one used	Corrected	3.29
3.28	Criterion (1)’s second citation does not screen for exhaust either	Moved	3.19
3.29	Two references reported unverifiable were in the archive	Resolved	3.27
3.30	Parameterisation comparison written against a censored sample	Narrowed	RC1 A4
3.31	Meyers curve evaluated in the wrong variable, outside its fit range	Corrected; Fig. 3 redrawn	3.30
3.32	Hypothesis stated over a temperature window never tested	Corrected	Sect. 3.2 regimes
3.33	Murray et al. (2012) does not carry the Arctic-amplification claim	Removed	3.29
3.34	A citation introduced by an earlier correction landed twice	Merged	3.23
3.35	Warm-regime values are upper bounds, and stronger read that way	Restated as bounds	3.30

	Item	Status	Bears on
3.36	Three corrections had not reached the Conclusions	Corrected	3.23, 3.35
3.37	Screening statistics counted temperature bins, not runs	Corrected; contrast reverses	3.7
3.38	The restored Jeju operation was still dropped by the pipeline	Rerun; every study total moves	1.1, 1.2
3.39	Jeju run counts came from a product the tables do not use	Corrected	3.38
3.40	The uncertainty budget named a component PINE does not have	Corrected	Sect. 4.3
3.41	Temperature uncertainty attributed to a mechanism no source describes	Re-attributed	2.3
3.42	Sensitivity propagated through a slope the same section rejects	Recomputed per regime	2.3, 3.50
3.43	A withdrawn correction factor still described as in use	Corrected	RC1 C1
3.44	Figure 7’s caption described a different figure	Rewritten	RC1 C4
3.45	Cross-references to Supplement sections and tables that do not exist	Renumbered; Table S12 added	3.16
3.46	The sea-ice sensitivity analysis was corrected twice	Recomputed	3.4, RC1 C1
3.47	Operation counts and aggregation scales in the independence argument	Corrected	1.2, 3.39
3.48	Supplement Fig. S1 drawn from hard-coded slopes	Recomputed	2.3
3.49	Bibliographic errors in the Böhmländer et al. (2025) entry	Corrected	3.40
3.50	Two definitions of the Jeju Polluted category	Classifier corrected	1.1, 3.44
3.51	Timestamp parser dropped 9–19 % of runs; QC verdicts borrowed across sessions	Rebuilt; every study total moves	Tables 1, 2, S5–S9, S11
4.1	QC flags for all 47,860 expansions absent from the archive	Deposited	Reproducibility
4.2	Figure pipeline as deposited could not be run	Repaired, verified	Reproducibility
4.3	Criterion (3) appears in the prose and nowhere else	Disclosed	1.3

1. Errors in the published text

1.1 The Jeju “Polluted” row, and a quality-control decision we are reversing

The Polluted row of Table 2 rests on 2–3 March 2024. Both days fall in the operation that began on 28 February (`asia-aq6`), which Sect. 2.5.1 of the discussion paper declared rejected in full for inlet instability (1,430 runs). The row was therefore computed from data that the same manuscript described as unusable. (The per-day counts in the category files, 261 and 259, are the runs of this operation whose timestamps carry fractional seconds; the Level-1 file holds 288 on each day. See item 3.51.)

On re-examination the rejection is not supported. We found no record linking the inlet-instability note to this operation. It was probably carried over from the RV Araon deployment, where the same fault is recorded, but we cannot document that. The reversal rests on the archived diagnostics instead:

	asia-aq5	asia-aq6	asia-aq7	asia-aq8
Status in the discussion paper	retained	rejected	retained	retained
End-pressure s.d. (hPa)	2.35	3.00	4.80	4.20
PIA automated QC pass	88.8 %	83.4 %	91.7 %	61.6 %
Flush carry-over $> 1 \text{ L}^{-1}$	0.7 %	1.0 %	0.9 %	0.8 %

The operation is more stable in end pressure than the two that follow it and passes PIA’s automated QC more often than the retained 5 March operation. The flags PIA does raise (particle contamination at refill, 237 runs) are already handled at run level, and no run trips the spike criterion. It is now treated like every other Jeju operation, with run-level QC only, which returns 1,193 runs to the valid set.

Where: Sect. 2.5.1.

1.2 Table 1 stated that all 20 Jeju operations were retained

Table 1 and Sect. 3.1 reported 20/20 valid Jeju operations, contradicting the three operation-level rejections in Sect. 2.5.1. That inconsistency is why the exclusion behind the Polluted row went unnoticed. With the 28 February rejection withdrawn and the operation inventory taken from the deposited product (item 3.39), the count is 16/17. Jeju valid runs are now 14,345 of 20,977 (68.4 %; 12,196 in the discussion paper) and the study total 21,103 of 43,136 (35,370 in the discussion paper).

Where: Table 1 and its footnote *d*; Sect. 2.2.1; Sect. 3.1; Supp. Tables S8 and S10.

1.3 The QC scheme diagram did not describe the scheme

Sect. 2.5.2 defines four ship-contamination criteria. Fig. 1 of the discussion paper left out criterion (3), included a step (“remove high expansion speed”) that is not in the text, and gave thresholds and a temperature window that differ from those the text specifies. The omission matters: criterion (3) removes only 35 of 9,565 Arctic runs, but at -21°C it lowers the pooled concentration from 196.9 to 0.129 L^{-1} . The diagram had no data dependency, so it drifted from the text without anything failing. It now states all four criteria.

Where: Fig. 1 and its caption.

1.4 The Supplement contradicted itself on the headline regression

Sect. S14 said the Sect. 3.6 regression placed the offline Filter-imm subset on the PINE scale with the $\times 11.2$ factor; the caption of the reproducibility table said the factor was not applied. Neither the Supplement nor the archive shows which was true. Since the correction factor is now withdrawn and the regression uses unscaled values throughout, the question no longer arises, but we cannot say which version was plotted in the discussion paper.

Where: Supp. Sect. S14; caption of Supp. Table S10.

1.5 Table S4: a site correction, and a table that described the wrong panel

The PaCE-2022 row was listed as “Station (Svalbard), 78°N ”; the instrument files give Sammaltunturi, Finland, 67.97°N . Corrected, the row became the same site and year as one of the Böhmländer et al. (2025) rows. On checking what that duplication affected we found that Table S4 did not describe the regression at all: the Sect. 3.6 panel is the 55 dataset-months of Table S10 (Wex et al. 2019, Hartmann et al. 2021, Sze et al. 2023 and the two ARAON campaigns), which contains neither row. The table is withdrawn and Sect. S4 now points to Table S10; the reported regression and its confidence intervals are unaffected.

Where: Supp. Sect. S4 (table withdrawn); Supp. Table S10.

2. Quantities shown as data that were not data

2.1 Supplement Figs. S3 and S4 were not computed from data — withdrawn

Both figures were drawn from a hard-coded five-point curve, with the filter curves anchored to literature values at -20°C . The PINE anchor, 3.08 L^{-1} , matches no product of this study (Table 2 gave 3.68 in the discussion paper and gives 0.099 L^{-1} now), yet one caption described the curve as real binned data. The figures existed to support the fixed correction factor; with the factor withdrawn at RC1’s request they have no purpose, and the measured comparison is in Fig. 7. They are withdrawn rather than recomputed. Removing them renumbered the Supplement figures, and all twelve main-text citations were remapped and checked.

Where: Withdrawn from Supp. Sect. S8, with a note in place; Sect. 3.3 and Sect. 4.4 of the main text.

2.2 The 0.15 L^{-1} floor in Fig. 4a was a plotting constant

The 0.15 L^{-1} lower edge of the Jeju boxes in Fig. 4a was the axis clip constant of the plotting routine, not a concentration. For Jeju Clean the 10th and 25th percentiles and the median across the 27 temperature bins are all zero. This is the value RC1 asked about in A5 and used to infer a sample volume of about 6 L. The corrected panel draws nothing where the data are zero.

Where: Fig. 4a and its caption. Answers RC1 A5.

2.3 The temperature-offset uncertainty was understated roughly six-fold

Fig. S1 gave a worst-case impact of about 5 % for a 1 K temperature offset in the Arctic. That value came from a hard-coded slope, the $\beta = -0.022\text{ K}^{-1}$ withdrawn under RC1 A4. Using the regime slopes of the revised spectra, a 1 K offset changes the concentration by 31.8 % (ARA15A), 25.7 % (ARA16A) and 20.9 % (Jeju) over the cold regime; the largest value in the study is 42.0 % (item 3.42). The conclusion that the offset does not change the sign of inter-campaign differences still holds, but the uncertainty on absolute concentrations is now given at its actual size.

Where: Supp. Fig. S1 and its caption; Sects. 2.6 and 4.3 of the main text.

3. Corrections that follow from these

3.1 The narrative sections still carried the superseded estimator

After Table 2, the Abstract and the figures were revised, seven passages in the Results and Discussion still used the old temperature-bin values. The largest was Sect. 3.2, which restated the old Table 2 in prose, including a “spectral crossover” that does not exist. Sect. 3.1 gave campaign medians of 8.02 , 6.83 and 6.57 L^{-1} as broadly comparable; on the daily-pooled basis they are 12.1 , 0.524 and 0.265 L^{-1} , and Jeju exceeds ARA15A by a factor of 23. All seven passages are rewritten.

Where: Sects. 2.6, 3.1, 3.2, 4.2, 4.3 and 4.4.

3.2 The calibration-drift argument rested on two saturated values

Sect. 4.3 bounded inter-campaign calibration drift below 5 % because the clean-period medians agreed (6.83 and 6.57 L^{-1}). Both values were the single-crystal counting quantum, so their agreement says nothing about the instrument. On the daily-pooled basis they are 0.524 and 0.265 L^{-1} , which reflects different air rather than drift. The claim is withdrawn; drift is not constrained by these data.

Where: Sect. 4.3, item (iv).

3.3 Table S3 concluded with the reversed inter-campaign ordering

Table S3 concluded that “Arctic $>$ Jeju at -20°C ” held under all threshold combinations. On the revised basis Jeju exceeds the Arctic at every temperature where both are measured. The statement about threshold robustness is kept, with a note that the test has not been rerun on the daily-pooled basis; the same ordering is withdrawn from Sect. 2.5.

Where: Supp. Table S3, overall conclusion; Sect. 2.5 of the main text.

3.4 Fig. S7 was computed on a different panel from Fig. 6

The correction-factor sensitivity figure read a panel whose offline rows already carried the $\times 11.2$ factor and whose ARAON values were the superseded medians, so it did not match Fig. 6. It has been recomputed on the unscaled panel; the current values are given in item 3.46.

Where: Supp. Fig. S5 (formerly S7), Table S9, Sect. S14.

3.5 Counts that followed from the operation-level reversal

Supp. Table S8 used 20 valid Jeju operations and Supp. Table S10 the old Jeju run count of 12,196. Both now follow Table 1.

Where: Supp. Tables S8 and S10.

3.6 The latitudinal profile was computed off-product

Sect. 3.3 used intermediate files from which every expansion with no crystal had been dropped (47.9 % of the 2024 record), took medians over all temperatures at once, and had not applied the exhaust screen. The 2025 transit values came entirely from the inlet-malfunction window that Sect. 2.5.2 excludes, and one sentence about 2024 quoted 2025 values. The section has been recomputed from the run-level product with zeros retained and with the two contamination criteria that do not need wind data; criteria (1) and (2) cannot be applied to the transit, and the text now says so. The bimodal structure remains, but the minimum moves from 45–55 to 55–60° N and the profile now rests on 2024 alone. Pooled over $\geq 65^\circ$ N it reproduces the ARA15A cells of Table 2 to within 21 %. No observation exists between 59.6 and 64.4° N, and latitude and date cannot be separated on a single northbound leg. The recomputed transect is shown as Fig. 4(f).

Where: Sect. 3.3 (*Latitudinal distribution of INP*); Sect. 4.2; Conclusions; Fig. 4(f).

3.7 Most of ARA16A was being discarded for a gap in a processed file

The exhaust screen drops any run it cannot match to a wind record. For ARA16A the processed relative-wind file held only ten of the 39 measurement days, so 200 runs survived and the ARA16A column of Table 2 rested on one or two days per temperature. The raw 1 s wind and GPS archives cover every day. The file has been rebuilt with the original method, which reproduces all 14,340 existing minutes to the fourth decimal. ARA16A now has 1,162 screened runs in 164 daily periods, its cold-regime slope is -0.129 (previously -0.089), and its warm regime gives -0.038 instead of an implausible positive slope. The two Arctic campaigns now agree, and ARA15A and Jeju are unchanged.

The rebuild keeps the original arithmetic mean of wind direction, which is wrong across $0/360^\circ$. Using a vector mean changes the screening of 24 of the 6,739 runs with a wind record and moves no result in Table 2 by more than 5 %, so the arithmetic mean is kept for consistency with the original ten days; a vector-mean option is provided in the rebuild script.

Where: Tables 1 and 2 and their notes; Sects. 2.5.2, 2.6, 3.1; Abstract; Supplement Table S6.

3.8 Figure 5 used the estimator that hid the screening effect

Figure 5 was drawn from the temperature-bin product. Recomputed at run level, the screening separates clearly: in 2025 the stern sector pools to 52.2 L^{-1} against 0.54 for the clean category, and in 2024 to 31.4 against 1.02. Both stern-sector values are carried by the few runs that criterion (3) also flags (8 and 1); without them the stern sector pools to 0.50 and 1.55 L^{-1} . Two corrections follow. The peak spike concentration was given as 311 L^{-1} , about two orders of magnitude above clean conditions; at run level the plumes reach about $2 \times 10^4 \text{ L}^{-1}$, four orders of magnitude above. And criterion (2) does not separate: the stagnant-air runs it removes pool to 0.53 (2024) and 0.59 L^{-1} (2025), close to the clean categories. The criterion is kept (it costs 288 and 190 runs) but is now described as precautionary.

Where: Fig. 5 and its caption; Sect. 2.5.2.

3.9 The diurnal-bias assessment dismissed a signal it could not see

Sect. 4.3 dismissed a significant time-of-day effect because the quartile medians spanned less than 2 %. Those medians were saturated at the counting quantum and could not vary. At run level the four ARA16A quartiles pool to 0.465, 0.516, 0.682 and 0.504 L⁻¹, a spread of 47 % ($H = 15.4$, $p = 0.0015$). The dismissal is withdrawn. We do not interpret the difference: the zero-crystal fraction also varies (52 to 66 %) and the campaign is too short to separate a diurnal cycle from changes in air mass. No diurnal correction is applied.

Where: Sect. 4.3 (*Uncertainties and limitations*), item (ii).

3.10 Condensation pass in response to RC1 D1

Following RC1 D1 we removed 685 words and one page from the main text: the abstract, which had reached 504 words in an intermediate draft, was cut to about 370 words (the discussion paper's abstract had about 280), repeated counts and cycle times were removed, long paragraphs in Sects. 2.5.2, 3.4 and 4.4 were split, and seventeen numerical substitutions moved to Supplement Table S11. In the process we found that six lines of the Fig. 4 caption had run off the page without any error, and two stale values (in the Fig. 4 caption and in the diurnal test of item 3.9).

Where: Abstract; Figs. 3–5 captions; Sects. 2.2, 2.5.2, 3.4, 4.4; Supplement Table S11.

3.11 Two numbers attributed to Wilbourn et al. (2024) are not in it

Sect. 4.4 attributed to Wilbourn et al. (2024) online/offline ratios of 3.3–79.7 at ENA and 0.5–1.2 at SGP, and an 80-fold divergence at -15°C . None of these figures is in the paper, whose ENA online record stops at -20°C , and we cannot trace them. The ratios shown in Fig. 7 and Supplement Sect. S9 (3.3–80 at ENA, 0.47–1.18 at SGP) are recomputed from the PANGAEA deposit and labelled as such; what is withdrawn is the attribution to the paper's text. What the paper does report supports the same direction: at -20°C the online and offline values agree at SGP (2.33 and 1.90 L⁻¹) and differ by a factor of 20 at ENA (0.40 against 0.02 L⁻¹). Those values are now quoted. Wilbourn et al. leave the cause of the ENA difference open; linking it to the filter wash-off mechanism of Robinson et al. (2026) is our interpretation and is now labelled as such.

Where: Sect. 4.4; RC1QnA.pdf C1 and C2.

3.12 A borrowed value carried more precision than its source

We quoted 0.436 L⁻¹ at -20°C from Barry et al. (2024); the source gives 0.44 L⁻¹. It is the summer mean of the MOSAiC record (the autumn value is 0.016), which we now state. Our campaigns are also in summer, so the comparison is season-matched.

Where: Sect. 4.4; RC1QnA.pdf C1.

3.13–3.15 Further checks of the sources in Sect. 4.4

Lacher et al. (2024). The main figures are correct, but the temperature range we attached to their factor-of-five statement is ours, not theirs, and has been removed. We also now mention their finding that filters collected without an aerosol inlet gave higher concentrations.

Möhler et al. (2021). We wrote that they quantify inlet loss only below 2 μm . They also give about 50 % loss at 4 μm , which supports our argument about coarse-mode losses. They give no 5 μm figure, so the value the discussion paper attributed to them remains unsupported.

The -0.07 to -0.09 slope band. We could not find this range in any of the papers it was attributed to; it was carried over from the discussion paper without a source. Item 3.16 replaces it.

Where: Sect. 4.4; Abstract; RC1QnA.pdf C3 and C4.

3.16 The literature slope band, derived from the data, does not support the contrast

We fitted the deposited filter spectra in the same way as our own data. Over each record's resolved range they give -0.131 (Alert), -0.129 (Ny-Ålesund), -0.165 (Utqiagvik) and -0.117K^{-1} (Villum). The ARA16A cold-regime slope (-0.129) lies inside this range and ARA15A (-0.166) at its steep end, so the claim that the ARAON spectrum is one and a half to twice as steep as single-location

Arctic records is withdrawn from the Abstract and Sect. 4.4. Three of the four records do not reach our cold-regime window at all, and the fourth (Villum) is flat there because the assay saturates.

Where: Abstract; Sect. 4.4; Supplement Table S6.

3.17 Appendix A, added: ice crystal sizes

PINE records a calibrated diameter for each particle, so we recovered the sizes of all 13,249 crystals counted in the 6,758 screened expansions. Warm-regime crystals appear larger, but the ice threshold is also higher in warm runs, and with a common threshold the difference disappears or reverses. We claim no size difference between the regimes, and the data cannot indicate biological origin. The size argument in Sect. 4.4 rests on the inlet cut-off alone.

Where: Appendix A and Fig. A1.

3.18 The correction-factor appendix moved to the Supplement

The revised Appendix A had become only a notice that the correction-factor appendix was withdrawn. That record now sits in Supplement Sect. S17, with the values of the withdrawn factor ($CF = 16$; $\times 11.2$, $\times 7$ and $\times 61$ by method) kept for reference. The appendix slot now holds the measurements of item 3.17.

Where: Supplement Sect. S17; Sects. 2.7, 4.3 and 4.4; Fig. 6 caption.

3.19 The stagnant-air criterion cited a paper that does not support it

Criterion (2) (relative wind below 2 m s^{-1}) cited Tatzelt et al. (2022). That paper has no wind-speed threshold and does not discuss stagnant air; its exhaust screen is by wind direction. The citation has moved to criterion (1), which it does support. Criterion (2) is kept as a precaution, now stated without a reference and with the note that no measurement establishes the threshold.

Where: Sect. 2.5.2, criteria (1) and (2); Sect. 3.4.

3.20 Ship exhaust and INP: why filter studies and our data disagree

Welti et al. (2020) found no correlation between exhaust exposure and INP on filter samples, and Tatzelt et al. (2022) rely on that result. Our Sect. 3.4 finds exhaust plumes at about 1.7 and $1.5 \times 10^4 \text{ L}^{-1}$, against clean values of 1.02 and 0.54 L^{-1} . The two results are compatible. The Welti test used 8-hour filters at -20 , -15 and -10°C , whereas a PINE run lasts about five minutes; a plume in one run is diluted roughly a hundredfold over a filter’s sampling time. Sect. 3.4 now explains this, and we do not claim it refutes the filter studies.

Where: Sect. 3.4, closing paragraph.

3.21 Two of the three citations for PINE’s counting performance never measured PINE

Sect. 2 cited Vepuri et al. (2021), DeMott et al. (2018) and Lacher et al. (2024) for PINE’s agreement with other methods. The first two do not involve PINE: DeMott et al. (2018) predates the instrument and Vepuri et al. (2021) studies precipitation samples. Both are removed here and in Sect. 4.4. The evidence now rests on one intercomparison (Lacher et al., 2024), where PINE agreed with the CSU-CFDC within a factor of 2 for 71 % of 34 points, and the text says so.

Where: Sect. 2.1; Sect. 4.4.

3.22 Hartmann et al. was listed as a fixed station; it is a ship track

The sea-ice synthesis metadata (Table S4 of the discussion-paper Supplement) gave the Filter-immersion row as Ny-Ålesund. The measurements were made from RV *Polarstern* during PS106 (May–July 2017). The row now reads as shipborne and cites the PANGAEA dataset.

Where: Supplement Table S10, Filter-imm row for PS106.

3.23 Model names attached to papers that do not use them

Sect. 1 named WRF, CAM6 and EC-Earth, citing Tan et al. (2016) and Shi and Liu (2019). Those papers use CAM5 and E3SM respectively, and neither concerns marine biogenic INP. The models

are now named correctly, and the marine biogenic statement is supported by Wilson et al. (2015) and Vergara-Temprado et al. (2017).

Where: Sect. 1; Sect. 5.

3.24 The Asian dust enhancement had no support among the three sources cited

The statement that *hwangsa* events raise INP by one to several orders of magnitude cited Iwata and Matsuki (2018), Chen et al. (2018) and Tobo et al. (2019). None of them says this: Chen et al. find Beijing urban pollution does *not* affect INP, Iwata and Matsuki characterise ice residuals at Kanazawa, and Tobo et al. concern high-latitude glacial dust. The sentence now states what the sources show, transport is cited to Uno et al. (2009), and Tobo et al. moved to the high-latitude dust sentence in Sect. 4.

Where: Sect. 1; Sect. 2.2.1; Sect. 4.1.

3.25 Two Arctic campaigns were attributed to the wrong platform

Sect. 1 called Creamean et al. (2018) a trans-Arctic voyage and Hartmann et al. (2021) a ground station. Creamean et al. (2018) is from Oliktok Point, Alaska, and Hartmann et al. (2021) is the PS106 cruise. The citations have been swapped.

Where: Sect. 1, paragraph on shipborne and ground-based Arctic campaigns.

3.26 Cabo Verde was cited for the Arctic marginal ice zone

Gong et al. (2020) is a tropical North Atlantic study and was wrongly cited for Arctic sea-ice organic sources. It is removed there (the statement remains supported by three other references) and now cited in Appendix A for its finding that supermicron particles carried roughly 70 % of the INP number.

Where: Sect. 4.2; Appendix A.

3.27 The instrument named for the FBAP input is not the one that study used

We wrote that the Tobo et al. (2013) parameterisation needs WIBS fluorescence data. Tobo et al. used a UV-APS at 355 nm. The sentence is corrected; the point that no fluorescence instrument ran alongside PINE is unchanged.

Where: Sect. 3.5.

3.28 Criterion (1)'s second citation does not concern exhaust screening

Modini et al. (2015), cited with Tatzelt et al. (2022) for criterion (1), does not describe a sector screen. Criterion (1) now cites Tatzelt et al. alone, and Modini et al. moved to the sea-state paragraph of Sect. 4, where its sea-spray measurements apply.

Where: Sect. 2.5.2, criterion (1); Sect. 4.3, item (iii).

3.29 Two references reported as unverifiable were in the archive

An earlier round listed Tan et al. (2019) and Tobo et al. (2013) as unverifiable. Both were in our archive under other file names. Tan et al. (2019) supports the sentence citing it; Tobo et al. (2013) is item 3.27.

Where: Sect. 1 (the citation is confirmed; the text is unchanged).

3.30 The parameterisation comparison was made against censored data

Sect. 3.5 stated that DeMott et al. (2010) underestimated and Meyers et al. (1992) lay below the Arctic data in the warm regime. That is true of the bin-level scatter in Fig. 3, whose warm end is bounded by the one-crystal quantum (4.42 L^{-1}) because zeros cannot be plotted. Against the daily-pooled ARA15A median at -20°C (0.099 L^{-1}), DeMott et al. (2010) is 44 times higher, Meyers et al. (1992) 87 times higher, and Welts et al. (2020) within a factor of three. The subsection now states which basis it uses.

Where: Sect. 3.5; Sect. 4.4.

3.31 The Meyers curve was evaluated in the wrong variable

Meyers et al. (1992), Eq. (2.4), is a function of ice supersaturation in per cent, not of temperature, and Fig. 3 had passed $-T$ instead. The curve was 19 to 75 % too low. It is now evaluated at water saturation, as DeMott et al. (2010) do. Meyers et al. also fitted the relation only between -7 and -20°C , so every ARAON temperature colder than -20°C is an extrapolation; the text now says so. Corrected, the curve lies above the observations throughout.

Where: Fig. 3 and its caption; Sect. 3.5.

3.32 The hypothesis was stated over a temperature window the paper does not test

Sect. 1 stated the hypothesis for -15 to -22°C and $\leq -25^{\circ}\text{C}$, while all tests use the Sect. 3.2 regimes (-23 to -19 and -31 to -24°C). The hypothesis now uses those regimes.

Where: Sect. 1, paragraph 4.

3.33 Murray et al. (2012) does not support the Arctic-amplification sentence

Murray et al. (2012) is a review of immersion freezing and says nothing about Arctic amplification. The “two to four times” figure is removed and the sentence is merged with the next, which cites Tan and Storelvmo (2019).

Where: Sect. 1, paragraph 1.

3.34 A duplicated citation introduced by an earlier correction

After item 3.23, Pasquier et al. (2022) was cited in two consecutive sentences. They are now one sentence.

Where: Sect. 1, paragraph 1.

3.35 The warm-regime values are upper bounds

Sects. 1 and 5 offered the values at -20 to -25°C as observational benchmarks. Several sit at their own detection limit (Table 2) and are upper bounds. Read that way the argument is stronger: the DeMott et al. (2010) and Meyers et al. (1992) curves exceed even the bounds, by factors of 25 to 118. Both sections now describe the values as bounds.

Where: Sect. 1; Sect. 5.

3.36 Three corrections had not reached the Conclusions

Sect. 5 still named CAM6 and EC-Earth (item 3.23), called the values benchmarks (item 3.35) and described the work as “multi-year”, although each campaign is a single summer. All three are corrected.

Where: Sect. 5, opening sentence and closing paragraph.

3.37 The screening statistics counted temperature bins, not runs

Sect. 2.5.2 and Table S5 gave the exhaust removal as 705 of 6,261 runs (ARA15A) and 128 of 6,138 (ARA16A). Those are counts of 0.5 K bins, about three per run, and the ARA16A figure also predates the wind-file rebuild of item 3.7. Counted in runs, ARA16A is the more exposed campaign, not the less, and the inlet-position explanation for its low rate is withdrawn.

Where: Sect. 2.5.2; Supplement Table S5.

3.38 An operation the manuscript restored was still being dropped by the code

The text of Sect. 2.5.1 restored the 28 February Jeju operation (item 1.1), but the processing code still listed it as rejected. With that fixed, the operation contributes the 1,430 runs and 1,193 valid runs the text states, Jeju has 20,977 runs, and all nine Jeju cells of Table 2 reproduce from the deposited product.

Where: Sect. 2.5.1 (text unchanged; the code now matches it); Table 2, Jeju rows.

3.39 The Jeju run counts came from a different product than the tables

The text gave 18,350 Jeju runs, a number that matches no product in the deposit, and subtracted two different totals from it. The counts now follow the deposited product: 21,429 runs in 17 operations; less the 10 May operation (25 runs) and 427 runs with no invertible concentration record, leaving 20,977 runs in 16 operations; of these 14,345 (68.4 %) pass run-level QC. The 9 May operation holds 261 runs, not the 3 given earlier.

Where: Sect. 2.2.1; Sect. 2.5.1; Table 1 and its notes.

3.40 The uncertainty budget named a component the instrument does not have

Sect. 4.3 attributed the 10 % Type B uncertainty to sampled volume and impinger collection efficiency. PINE has no impinger. The 10 % comes from Böhmländer et al. (2025), who assign it to optical detection of ice crystals and treat the volume terms as negligible. The text now says so and cites them.

Where: Sect. 4.3, uncertainty budget.

3.41 The temperature uncertainty was attributed to the wrong mechanism

We cited Möhler et al. (2021) for an offset of up to 1.0 K between the gas and droplet temperatures. That paper describes no such offset; its ± 1 K is the temperature inhomogeneity across the chamber. The magnitude stays and the attribution is corrected.

Where: Sect. 2.6; Sect. 4.3.

3.42 The temperature sensitivity was propagated through a rejected slope

Sect. 2.6 rejects the full-range slope as uninterpretable, then propagated the temperature uncertainty through it, and Sect. 4.3 called the result a maximum. Both sections now use the regime slopes: 31.8, 25.7 and 20.9 % (ARA15A, ARA16A, Jeju) for the cold regime and 0.2, 8.5 and 24.0 % for the warm regime. The largest value in the study is 42.0 %, for Jeju Polluted in the warm regime.

Where: Sect. 2.6; Sect. 4.3.

3.43 A withdrawn correction factor was still described as in use

One sentence in Sect. 4.3 still described $CF = 16$ as a working approximation, contradicting Sects. 2.6, 2.7 and 4.4, and three Supplement passages still called the withdrawn factors “adopted”. All are corrected.

Where: Sect. 4.3; Supplement Sects. S8, S9 and S14.

3.44 Figure 7’s caption described a different figure

The figure had been rebuilt but its caption had not: 197 of its 198 words were unchanged. It listed PaCE-2022, which is not plotted, described bin medians and a box plot, and repeated the withdrawn flatness claim. The caption is rewritten, and all sixteen compiled figures have been checked against the scripts that generate them.

Where: Fig. 7 caption.

3.45 Cross-references to Supplement sections and tables that do not exist

The main text and Supplement referred to “Sect. S22” in twelve places, but no such section existed, and Sect. 4.4 cited a Table S12 that was missing. The Supplement sections are now numbered S1 to S19, the references point to Sect. S17, and the derived slopes of item 3.16 are given in Table S12.

Where: Sect. 4.4; Supplement section numbering throughout, Sect. S17 and Table S12.

3.46 The sea-ice sensitivity analysis was corrected twice

The script testing the sea-ice association against the correction factor assumed an unscaled panel, but the panel still carried an intermediate $\times 3.6$ factor; the Supplement table also used the pre-rebuild ARA16A record. On the current panel the unscaled case gives slope -0.2451 , $\rho = -0.5715$ and $p = 5.2 \times 10^{-6}$ ($n = 55$). Across all factors ρ ranges from -0.38 to -0.57 , and the claim that $p \leq 3 \times 10^{-4}$ in every case is withdrawn (the largest is 3.7×10^{-3}). The synthesis has 55 dataset-month pairs, not 56.

Where: Sect. 3.6; Fig. 6 caption; Supplement Sect. S14, Table S9 and Fig. S5.

3.47 Operation counts and aggregation scales in the independence discussion

Sect. 4.3 gave 20 Jeju operations (16 is correct) and 30 to 40 air-mass segments per Arctic campaign. Measured on the screened product there are 44 and 18 segments, over 26 and 22 sampling days. An unreproducible worked example ($n = 8$) is removed. Of the four ARA15A operations only two contribute screened runs, but they give 336 daily periods, so the limitation is air-mass diversity rather than precision.

Where: Sect. 4.3; Supplement Sect. S12 and Table S8.

3.48 A Supplement figure was drawn from hard-coded slopes

Fig. S1 used a hard-coded set of slopes, two of them out of date, and its panel (a) silently omitted the Jeju series. Both panels are now computed from the deposited product.

Where: Supplement Fig. S1 and its caption.

3.49 Bibliographic errors

The Böhmländer et al. (2025) entry had the wrong title and pages (6165–6185 instead of 6165–6171). Two statements were narrowed: Barry (2024) does compute temperature slopes, in two-degree intervals, and the Wilbourn et al. (2024) values we quote differ slightly from the medians of the PANGAEA deposit used in Fig. 7, so the source of each is now stated.

Where: Reference list, Böhmländer et al. (2025); Sect. 4.4, provenance of the Wilbourn et al. (2024) values.

3.50 Two definitions of the Jeju Polluted category

The processing code defined Jeju Polluted as eleven days in March 2024, while Sect. 2.2 and Table 2 use 2–3 March; the two sets share no day. Table 2 and Fig. 3 already used the right days, but Figs. 7, S1 and S6 and Table S6 did not, and plotted a different population under the same label. With the code corrected, Table 2 is unchanged and the Polluted slopes are -0.075 (cold) and -0.236 K^{-1} (warm). The warm-regime fit rests on two days and is the weakest in the set, which the text states.

Where: Sect. 2.2.1; Table 2 note; Supplement Table S6 and Fig. S1.

3.51 Two faults in the run selection: lost timestamps and borrowed QC verdicts

Both faults were found while the deposit was being rebuilt from the raw PIA exports, after the rest of this document was written, and both change every study total. Values elsewhere in this document are the current ones.

Timestamps. PIA writes most timestamps with fractional seconds and about one in ten without. The pandas version in use read the first format only and set the rest to missing, so 1,111 ARA15A runs (9.4%), 974 ARA16A runs (9.4%) and 4,013 Jeju runs (19.1%) had no timestamp. Shipborne runs without a timestamp could not be matched to wind data and were dropped; Jeju runs without one entered no daily pool. All timestamps are now parsed.

QC verdicts. The code looked up each run's PIA verdict by operation folder and run number, but a folder can hold two sessions, so runs of a second session took the verdicts of the first: 131 ARA16A runs and 543 Jeju runs whose own verdicts are *fail*. The lookup now includes the session.

What changed. Screened runs: ARA15A 5,062 $\rightarrow$ 5,596, ARA16A 1,097 $\rightarrow$ 1,162, Jeju 14,888 $\rightarrow$ 14,345, total 21,047 $\rightarrow$ 21,103. Table 2 at -20°C : ARA15A 0.105 $\rightarrow$ 0.099 and ARA16A 0.123 $\rightarrow$ 0.090 L^{-1} . Cold-regime slopes: ARA15A $-0.162 \rightarrow -0.166$, ARA16A $-0.136 \rightarrow -0.129$. Sea-ice slope $-0.249 \rightarrow -0.245$ (ρ unchanged at -0.57). The other Table 2 cells, the Jeju slopes, the sensitivities of item 3.42 and Appendix A are recomputed in the revised manuscript and Supplement.

What did not change. No conclusion changes direction. Two statements are reworded: the ARA15A slope now lies 0.001 K^{-1} beyond the steep end of the literature range of item 3.16, and the ARA16A cell at -20°C (0.090 against a limit of 0.104) is now reported as below its detection limit.

Where: Abstract; Tables 1 and 2 and their notes; Sects. 2.2.3, 2.5.1, 2.5.2, 2.6, 3.1–3.4, 3.6, 4.2, 4.3; Appendix A; Fig. 1 caption; Supplement Tables S1, S5–S9, S11; Figs. 1–7 and A1 and Supplement Figs. S1–S6 regenerated.

4. The archive

4.1 The instrument quality-control flags were not deposited

The original archive did not include the per-expansion QC flags, so the run selection could not be checked. All 47,860 expansion flags are in the updated deposit.

Where: Code and data availability.

4.2 The figure pipeline as deposited could not be run

When the code was reorganised, a shared path module and two input files were left out, so the figure scripts could not run from the archive. This is fixed: the scripts now run from the deposit alone and reproduce the published figures.

Where: Code and data availability.

4.3 Criterion (3) appears in the prose and nowhere else

Criterion (3) is implemented in the analysis code (it flags 79 runs, 35 in ARA15A and 44 in ARA16A), but the discussion paper’s Table S5, QC diagram (item 1.3) and cruise-track figure did not reflect it. Whether it was applied to the concentrations in the discussion paper cannot be determined.

Where: Supp. Sect. S4b; Fig. 1; Fig. 2 caption; Sect. 2.5.2.

Figures: what changed in each

Every figure of the revised manuscript and Supplement was regenerated from the rebuilt run-level product (item 3.51). The two tables below say, figure by figure, what changed and which item or referee point explains it, so that a reader comparing a figure with its discussion-paper version does not have to search this document for the reason.

Figure	What changed	Why
1 QC flow	redrawn: the boxes now state criteria (1)–(4) of Sect. 2.5.2	the published diagram showed two criteria and a step that appears nowhere in the text (items 1.3, 4.3)
2 Cruise tracks	regenerated on the current product; caption rewritten	the caption now states the filters the map applies and that it is not the screened product of Table 1 (item 4.3)
3 Temperature spectra	split over two pages (Arctic; Jeju); counting quantum, detection limit and the daily-pooled spectrum drawn on every panel; Meyers (1992) recomputed	shows directly why the published Arctic values sat at about 3 L^{-1} (RC1 A2, A4, A5; RC2 R2-3; items 3.31, 3.50)
4 Latitudinal distribution	panels (c)–(e) added: Level-1 run values with the one-crystal floor for each campaign; panel (f) added: the 2024 transect on the daily-pooled basis; zero quantiles drawn as zeros	requested by Referee 1 (RC1 A5, A6); the 0.15 L^{-1} floor was a plotting clip (item 2.2); the transect had excluded zeros (item 3.6)
5 Ship contamination	recomputed at run level, by criterion	the published panels were flat by construction; the plume runs reach 2.2×10^4 and $2.8 \times 10^4\text{ L}^{-1}$, not 311 (items 3.8, 3.37)
6 Sea-ice synthesis	redrawn without any harmonisation factor; $n = 56 \rightarrow 55$	correction factors withdrawn (RC1 C1); the published count included a July 2025 ARAON month in which that campaign has no record (item 3.46)
7 Integrated comparison	(a) daily-pooled spectra with the co-located ENA online–offline pair; (b) cold- and warm-regime slopes	the published flatness was the counting quantum (RC1 A4, C4; item 3.44)
A1, A2 (old)	withdrawn with the correction-factor appendix	RC1 C1; RC2 R2-9 (item 3.18)

Figure	What changed	Why
A1 (new)	ice-crystal optical diameters from the PINE counter	added; a largely negative result, reported as such (item 3.17)

Supplement figures. Two are withdrawn and the rest renumbered.

Discussion paper	Revised	Change
S1 temperature offset	S1	recomputed per regime from the daily-pooled product (items 2.3, 3.48)
S2 integrated Arctic spectra	S2	ARAON curves on the daily-pooled basis
S3 INP versus latitude	withdrawn	drawn from a hard-coded curve, not data (item 2.1)
S4 PINE versus filter spectra	withdrawn	as S3 (item 2.1)
S5 PaCE-2022 comparison	S3	redrawn on the daily-pooled basis
S6 SGP versus ENA	S4	offset annotations computed from the plotted ratios
S7 CF sensitivity	S5	now a “what if a factor were applied” analysis on the unscaled panel (items 3.4, 3.46)
S8 per-operation slope	S6	regime slopes; read as evidence against, not for, intrinsic flatness

Unresolved

1. The ARA16A wind direction is averaged arithmetically (item 3.7). It affects 24 of the 6,739 wind-matched runs and no conclusion; whether to use the vector mean in the final version is open.
2. The route by which the Jeju value of 0.134 L^{-1} arose is not established. This is the open part of RC1 A5.
3. Whether criterion (3) was applied in the discussion paper cannot be determined (item 4.3).
4. The 991 published ARA16A runs cannot be reproduced from the archive.