Referee comments 1

Review of "Ice Nucleating Particle Concentrations over the Eurasian-Arctic seas" by Li et al., submitted to ACPD (egusphere-2025-2798)

This study presents a comprehensive dataset of ice-nucleating particle (INP) concentrations measured during the Arctic Century Expedition (August–September 2021) across the Barents, Kara, and Laptev Seas. The authors combine online (HINC) and offline (DRINCZ) immersion freezing measurements with aerosol chemical and physical analyses to examine INP abundance, sources, and variability in the Eurasian Arctic. The work expands observational coverage in a remote and data-scarce region and provides insights into the local versus long-range sources of Arctic INPs.

The manuscript is well structured and clearly written, with a logical flow from methods to results and interpretation. The inclusion of a focused case study contrasting low and high INP periods adds significant value by integrating aerosol measurements, chemical tracers, meteorology, and air mass origin to support the broader conclusions. This is a well-executed and much-needed observational study in a remote and understudied Arctic region. I recommend publication after minor revisions to address the points below.

We thank referee 1 for the nice summary and positive feedback on our manuscript egusphere-2025-2798. In response to the questions and suggestions, please find our answers and revisions listed below. **Referee comments are reproduced in bold** and author responses in normal font; *extracts from the original manuscript are presented in red italic* and *extracts from the revised manuscript in blue italic*.

1. **The use of 2-day back trajectories in Figure 7 provides useful context for identifying short-range influences but may be insufficient to fully assess the role of long-range transport, particularly for mineral dust or aged aerosols that can persist in the atmosphere for several days. While the authors conclude that local sources dominate INP variability, this conclusion may partly reflect the limited temporal scope of the trajectory analysis. Extending the trajectory duration could potentially reveal additional source regions or transport pathways not captured in the current analysis, such as high-latitude terrestrial dust or recirculated Arctic aerosols. As it stands, the statement of limited long-range influence, though plausible, remains somewhat constrained by the methodology. A brief discussion of this limitation and consideration of longer trajectory timescales would be welcome.**
   We thank the reviewer for this constructive comment. Indeed, the use of 2-day back trajectories focus on identifying local to regional influences on INP concentrations, particularly in the Arctic boundary layer, as trajectories were initialized near sea level. This approach was chosen to directly link near-surface measurements with recent surface interactions. We agree that this approach may not fully capture potential long-range transport events, such as those associated with aged mineral dust or recirculated aerosols that have residence times exceeding two days.

   To address this limitation, we have extended our back trajectory analysis to 7 days (see Fig. E3 for all measurements at -34 °C and -15 °C; and Fig. E4 for selected high and low N_{INP} cases at -20 °C, respectively, in the Appendix) and complement our discussion in Section 3.5 (see lines 335-338 in the revised manuscript): *"This finding is corroborated by extended 7-day back trajectories (see Fig. E3 in the Appendix). Notably, elevated N_{INP} observed near Novaya Zemlya at both -34 °C and -15 °C coincide with air masses characterized by prolonged residence over the western Siberian coast - a hotspot acting as a potent source for both mineral dust and biological particles as discussed previously."* and section 3.6 (see line 368 in the revised manuscript): *"…from the west Siberian coast, suggesting INP influence from both local source and long-range transport."*

2. **The weak correlations between INP and chemical tracers in Figure 4 limit the strength of source attribution to mineral dust or marine biogenic components. While the observed trends, such as modest associations with AlSiCa or sulfur, are directionally consistent with expected sources, the low**

correlation coefficients and lack of statistical significance in many cases suggest that the chemical composition data alone are insufficient to differentiate dominant INP types. This highlights the need for either more specific tracers (e.g., molecular markers) or multivariate approaches to better resolve complex and potentially co-varying source contributions.

We thank the reviewer for this critical comment. We agree that the correlation analysis between INP concentrations and bulk elemental composition (e.g., AlSiCa for mineral dust, P and S for marine biogenic sources) shows generally weak to moderate relationships and often lacks statistical significance at several temperatures. This indeed limits the strength of any direct attribution of INPs to specific sources based on the current data alone. The reviewer's point touches on two fundamental challenges of in-situ field measurements that are worth elaborating on.

First, as the reviewer correctly points out, the Arctic system is characterized by a mixture of multiple aerosol sources, many of which are highly dynamic in space and over time. Given the limited temporal and spatial coverage of observational data in the remote Eurasian Arctic, particularly over oceanic regions, our study was designed to serve as a first-order assessment using widely accepted tracer classes. However, we acknowledge that these tracers (e.g., bulk Al, Si, Ca, S) lack the chemical specificity required to isolate IN-active subfractions such as certain organics, biological macromolecules, or specific mineral phases, since our classification does not resolve the composition of individual particles or identify specific organic molecules known to be IN-active. Highly effective INPs may constitute only a tiny fraction of the total aerosol mass, and their specific chemical signatures can be diluted in a bulk sample, leading to weak correlations.

Second, field-based studies usually face unique challenges in disentangling source contributions due to complex atmospheric mixing, limited sample volumes, and operational constraints (e.g., storage and processing of filter/impinger samples). In a dynamic environment like Eurasian Arctic Ocean measured from a moving ship, air masses are rarely influenced by a single, isolated source. Instead, they contain a complex mixture of aerosols from marine, terrestrial, and biogenic origins that are often co-emitted or mixed during transport, making it statistically challenging to attribute a change in N_{INP} to any single tracer compared to a fixed-site measurement where a lot more data for a given location is available. The resulting weak or insignificant correlations reflect this complex, mixed nature of the aerosol population, where no single component likely dominates the INP population across all conditions. To more informatively reflect the intricate multivariate associations, both a larger number of independent samples and a broader suite of tracers (e.g., molecular-level organics, isotopic signatures) would be beneficial, which are, however, not available from our dataset from this ship-based expedition. Even with such data, multivariate source apportionment would remain challenging due to the relatively low INP concentrations and high variability of natural INP sources, particularly in Arctic environments.

To address this important point, we have now expanded the discussion in Section 3.3 to better frame the limitations of source attribution in our study and to highlight specific future directions that could enhance the quality of INP source identification in the Arctic, or generally, field studies (see lines 282-292 in the revised manuscript): *While directional trends between N_{INP} and elemental tracers (e.g., AlSiCa, P, and S) are broadly consistent with potential mineral dust and marine biogenic influences, the generally weak to moderate correlations and limited statistical significance at several temperatures suggest that bulk elemental composition alone cannot fully resolve the diversity of INP sources. This is due to the chemical and physical complexity of the ambient aerosol population that are influenced by multiple potentially correlated factors in the Eurasian Arctic, where marine, terrestrial, and biogenic particles frequently co-emit and mix during transport. Furthermore, highly effective INPs constitute only a small fraction of total aerosol mass and are not adequately represented by bulk tracer signals. To disentangle the overlapping terrestrial and marine contributions to INPs, particularly in the remote and dynamic Arctic oceanic environment, future field studies would benefit from the use of more specific molecular tracers (e.g., molecular organics, biochemical tracers like DNA and lipids, and isotopic ratios, etc.), single-particle analyses, larger sample sizes, and multivariate data approaches, the latter of which are not possible with the limited dataset we have from this single cruise.*

3. **The use of coarse elemental proxies (e.g., AlSiCa) without complementary biological markers (e.g., DNA, lipids) constrains the ability to resolve source types (marine vs terrestrial biogenic).**
While bulk elemental proxies offer broad insight into mineral and mixed biogenic inputs, we agree that they are not sufficient to resolve finer distinctions between marine and terrestrial biogenic INP sources. We acknowledge that incorporating more specific biological markers, such as DNA, lipids, proteins, or other molecular-level indicators, would provide a stronger basis for distinguishing between biogenic INPs of terrestrial and marine origin. However, such biological analyses were not available from the current study. Instead, we had additional heat treatment of the aerosol samples to attribute the heat labile (proxy for biological molecules) in another accompanying study (Li et al., 2025, doi: 10.1039/D4FD00160E).

4. **The discrepancy between impinger and PM10 filter data is well noted in Appendix A, but two important methodological issues remain unaddressed. First, the vertical offset between sampling inlets (2nd vs 6th deck) could introduce a height-related bias, particularly under stratified conditions where aerosol concentrations may vary significantly with altitude. Second, the potential for contamination from the ship's own exhaust emissions is not discussed, despite the low ambient aerosol background and proximity of the sampling locations to possible emission sources.**
We thank the reviewer for raising these two important methodological questions. We agree that these details are crucial for interpreting our results and will add them to the manuscript. We will address each point separately.

   1) We acknowledge that in a strongly stratified atmosphere, this vertical separation could potentially introduce a bias in the measured aerosol and INPs. However, the marine boundary layer is often well-mixed, which would minimize small altitude-related concentration differences for the aerosols sampled. In addition, the LVS showed higher INP concentrations than the impinger measurement which was at a lower altitude. Nevertheless, this is a methodological detail that needs to be mentioned. We now add a statement to Section 2.2.1 acknowledging the different sampling heights and the small potential for a related bias (see lines 86-91 in the revised manuscript): *The impinger and LVS were located on the 2nd and 6th decks, respectively, with a vertical separation of over 10 meters. Under stratified atmospheric conditions, especially near the ocean surface, the vertical variability of aerosol properties between different sampling heights cannot be entirely ruled out. However, given that both are in the boundary layer, we do not expect large differences in aerosol number and composition between the two sampling locations, except those introduced by the sampling method (impinger samples particles > 0.5 μm aerodynamic diameter).*
   2) Concerning the potential for contamination from ship exhaust emissions, we took active steps to minimize its influence. For the PM_{10} filter samples (LVS on the 6th deck), sampling was automatically halted whenever the wind direction was from the ship's funnel to avoid contamination. For the impinger and HINC measurements (on the 2nd deck), we used CPC data to identify time periods as being affected by exhaust plumes based on sharp transient spikes in total particle concentration, which were filtered out and excluded from INP analysis. These measures substantially reduce the risk of exhaust contamination in the reported data, although we acknowledge that low-level or short-lived influences may not be fully eliminated. A corresponding clarification has been added to Section 2.2.1 in the revised manuscript (see lines 91-96): *In addition, to minimize contamination from ship exhaust, specific procedures were followed for the two sampling locations. On the 6th deck, the PM_{10} filter sampling (LVS) was automatically paused whenever wind direction sensors indicated air flow from the ship's funnel. For impinger and HINC measurements on the 2nd deck, spikes in total particle number concentration measured by a CPC were used to identify exhaust plumes; these periods were flagged and removed from the dataset. These measures substantially reduce the influence of exhaust emissions on the reported INP data, although minor residual contamination cannot be fully excluded.*

5. **The weak to moderate correlations between INP concentrations and aerosol surface area shown in Figure 3 suggest that existing parameterizations may not fully capture the complexity of INP behavior in this environment. It would be helpful if the authors could expand the discussion to explore possible**

contributing factors, such as the potential diluting effect of non-IN-active sea salt particles or the presence of mixed aerosol types. A brief reflection on these mechanisms would strengthen the interpretation.
Excellent point. As the reviewer points out, sea salt particles, which are abundant over open ocean and IN-inactive at temperatures we measured, can contribute substantially to total aerosol surface area and thus dilute any direct relationship with INP concentrations. Moreover, the coexistence of multiple aerosol types (e.g., sea salt, biogenic organics, mineral dust) can lead to variability in IN-activity even at similar surface area concentrations, especially when effective INPs make up only a minor fraction of the total aerosol population.

To address this, we have expanded the discussion in Section 3.2 to reflect these factors and to clarify that aerosol surface area alone may not be a sufficient predictor of INP variability in mixed-source marine environments (see lines 262-265 in the revised manuscript): *Additionally, the high abundance of sea salt particles, which contribute significantly to total surface area but are largely inactive as INPs, could further dilute the relationship between N_{INP} and surface area concentrations. Moreover, the presence of mixed aerosol types with varying ice-nucleating efficiencies can further obscure any direct scaling with bulk surface area.*

Minor comments:

1. **Table 1, please clarify that CPC measures particle number concentrations.**
The entry of Function in Table 1 is changed to *"Particle total number concentration".*

2. **In Figure 2, the yellow circles, grey open triangles, blue bars, and pink diamonds all correspond to data at the same temperature points, but this is not immediately clear to the reader. Adding a guiding arrow or visual connector between the temperature axis and the symbol legend would help direct the eye and improve interpretability.**
We agree, the data points are only plotted at -15 °C; as such, they are only relevant for that temperature as plotted. To better navigate the reader, we add the temperature in the legend for the said data points to better guide the reader in the revised manuscript.

Reference

Li, G., A. Welti, A. Rocchi, G. Pérez Fogwill, M. Dall'Osto and Z. A. Kanji. Terrestrial and Marine sources of ice nucleating particles in the Eurasian Arctic (2025), Faraday Discussions, 258, 94-119, DOI: 10.1039/D4FD00160E

Referee comments 2

Review of "Ice Nucleating Particle Concentrations over the Eurasian-Arctic seas" by Li et al., submitted to ACPD (egusphere-2025-2798)

This paper investigates ice nucleating particle concentration (nINP) in the Eurasian Arctic seas during the summer of 2021. The authors collected data from aboard the RV Akademik Tryoshnikov and deployed both DRINCZ (offline) and HELC (online) measurement techniques to collect and assess INP activity. The researchers utilized the LAGRANTO tool for backward air parcel trajectories, categorizing them by passed-over surface types, including ice-pack, the ice-free ocean, and land. Additional testing was performed to find chemical composition and particle size distribution. The research revealed the highest nINP near Novaya Zemlya and linked the air parcels to originating on the Siberian coast. While the authors found evidence of high terrestrial impact, statistical tests revealed negligible correlation between nINP and air-parcel time over land, suggesting local dominance of INPs over long-range transport. The manuscript is well organized and easy to follow with a strong experimental design. The reviewers recommend publication after revision of the following minor comments.

We thank referee 2 for the nice summary and positive feedback on our manuscript egusphere-2025-2798. In response to the questions and suggestions, please find our answers and revisions listed below. **Referee comments are reproduced in bold** and author responses in normal font; *extracts from the original manuscript are presented in red italic* and *extracts from the revised manuscript in blue italic*.

Minor Comments:

Title:

- **By adding "During Summer" to the end of the title, the author can better articulate what the manuscript has to offer.**
  Inspired by this, we changed the title to: "Summertime *Ice nucleating particle concentrations over the Eurasian-Arctic Seas*".

Page 4, Section 2.2.1 Ambient aerosol samples: Lines 80-81

- **How were subsequent flow rates chosen for high flow rate and low volume samplers? The author mentions that the high volume of air being processed in the impinger samples affects the detectable range of nINP but does not justify why that flow rate was selected.**
  For LVS, the flow rate was selected to be consistent with the PM_{10} inlet design, ensuring the aerodynamic cutoff size of 10 μm was maintained. A trade-off was made between sampling flow rate (38.3 Lpm) and duration (12h) to maximize total sampled volume, achieve suitable temporal resolution, and match the INP detection range of the DRINCZ. For the impinger, we operated at its maximum flow capacity (300 Lpm) to collect as much air volume as possible over a shorter sampling interval (3 h). This setup allowed higher time resolution while still providing sufficient sample volume for INP analysis using DRINCZ. We have clarified the flow rates and sample times on lines 83-86 in the revised manuscript.

- **Could the flow rates be optimized?**
  For the impinger sampler, we were already at the maximum flow rate, so no optimization options were possible unless we opted to sample for longer than 3h, which would be possible but would reduce the sample number overall. To capture the PM10 aerosol distribution, we also opted not to change the low volume sampler flow rate of low volume sampler and to keep the samples to 2/day, i.e., 12 hours per sample.

Page 4, Section 2.3.1 Impinger and filter samples: Line 89

- **The author uses the acronym DRINCZ without defining. This reviewer thinks it would be better to introduce the instrument by its full name. (Especially since the next section is about it).**
  Thanks for pointing that out. We have added the full name for DRINCZ at its first appearance on line 104.

Page 6, Section 2.4.2 Chemical Composition Analysis: Line 133; Line 135

- **What is the accuracy and uncertainty of the bulk instrument (ICP-OES) technique? Was it dependent on the volume of the sample to be analyzed? Dependent on the element?**
  Instrumental uncertainty typically ranged from 5% to 10% of the reported concentration. The uncertainty and detection limit are element-dependent, particularly for trace elements or those with lower emission line sensitivity in ICP-OES (e.g., the LOD of S is > 50 times compared to that of Zn). For our measurement with impinger samples, sufficient air was collected for most detected elements, while some are below the detection limit (e.g., Fe, S) due to low concentrations in the background environment. We clarified the above statement in the revised manuscript (see lines 155-159).

- **Why were ICP-OES samples diluted with Nitric acid? Was the instrument calibrated using standards in HNO3, etc., or was it to ionize metals?**
  The use of nitric acid is prior to ICP-OES analysis and does not function to ionize the metals directly (the ionization occurs in the plasma during the measurement). Instead, it ensures chemical stability and solubility of metal ions in solution, minimizing adsorption onto container walls. In addition, it maintains consistency with the calibration standards, which were also prepared in 2% HNO_3.

Page 7, Section 2.4.4 Backwards Trajectory Analysis: Line 160

- **The authors are presuming that >0.1mm/h of rain was chosen due to its potential to wash out aerosols from the airmass. If so, it should be stated in the manuscript for clarity.**
  We clarified the statement in the revised manuscript (see lines 179-180): … *which would introduce washout effects on boundary layer aerosols due to wet scavenging.*

- **Why were the trajectories calculated for two days (48hrs)? Why not longer? Does the shorter time for back trajectory limit the number of long-range aerosols being observed?**
  The use of 2-day back trajectories focus on identifying local to regional influences on INP concentrations, particularly in the Arctic boundary layer, as trajectories were initialized near sea level and removed if they exceeded the boundary layer height. This approach was chosen to directly link near-surface measurements with recent surface interactions. We agree that this approach may not fully capture potential long-range transport events, such as those associated with aged mineral dust or recirculated aerosols that have residence times exceeding two days.

  To address this limitation, we have extended our back trajectory analysis to 7 days (see Fig. E3 for all measurements at -34 °C and -15 °C; and Fig. E4 for selected high and low N_{INP} cases at -20 °C, respectively, in the Appendix) and complement our discussion in Section 3.5 (see lines 335-338 in the revised manuscript): *"This finding is corroborated by extended 7-day back trajectories (see Fig. E3 in the Appendix). Notably, elevated N_{INP} observed near Novaya Zemlya at both -34 °C and -15 °C coincide with air masses characterized by prolonged residence over the western Siberian coast - a hotspot acting as a potent source for both mineral dust and biological particles as discussed previously."* and section 3.6 (see line 368 in the revised manuscript): *"…from the west Siberian coast, suggesting INP influence from both local source and long-range transport."*

Page 7, Section 3.1 Temperature-dependent variability of Arctic INP Concentrations: Lines 174-175

- **Isn't LOD a part of the reason for the observed "smaller" difference? The line at 5x10^-5 INP L^-1 above -17 dC is virtually horizontal. Has this been considered? If so, it should be stated in the manuscript, otherwise the author should justify.**
  We thank the reviewer for this sharp observation. Yes, the flattening of N_{INP} above −17 °C is influenced in part by the lower limit of detection (LOD) of the DRINCZ method under the given sampling volumes. This has already been partially addressed in the original manuscript (see lines 214-218): *At most freezing temperatures, N_{INP} from PM_{10} filters was observed to be higher than that from impinger samples. This difference could be influenced by the larger volume of air processed by the impinger (approximately 54 m^3) compared to the PM_{10} filters (approximately 27.6 m^3), which shifts the detectable range to lower N_{INP}*

for the impinger samples. The minimum detectable N_{INP} with DRINCZ decreases as the air volume increases. Therefore, the N_{INP} detectable from impinger samples is lower, which is particularly relevant at the highest freezing temperatures.

We now further add that the smaller variability could also be due to reaching the minimum possible detectable INP concentration for our methods in the revised manuscript (see lines 220-222): *"For T < -10 °C, the variability in the impinger INP data and therefore the difference to the lower limit of the Petters and Wright line could potentially be even more than currently presented because our data are limited by reaching the lower limit of detection of our analysis method".*

- **To this reviewer, the variability and difference look larger and more prominent at higher freezing temperatures.**
  Indeed. Apart from the difference in sampling volumes that led to different LOD we stated above, several other explanations were provided in Appendix A (original manuscript), which we bring back to the main text in Section 3.1 for clarification (see lines 216-241).

Page 8, Section 3.1 Temperature-dependent variability of Arctic INP concentrations: Lines 185-186; Fig. 2

- **What makes the author say this? To the reviewer, nINP does not look similar or overlapping in the min-max range, even at -15 dC. The authors' measurements represent lower nINP overall.**
  We agree that our N_{INP} values are generally lower than typical midlatitude observations, and the initial sentence may have overstated the similarity. Our intent was to suggest that some observed concentrations fall within the broader range reported for biological INPs in continental regions, particularly at T > -12 °C, but we acknowledge that the overall distribution is shifted lower. We have now revised the sentence to reflect this more cautiously (see lines 247-249): *Some of the observed N_{INP} values fall within the lower end of the range reported for biological INPs in continental mid-latitude regions (T < -15 °C). This supports the contribution of MBAs with ice-nucleating potential over the Eurasian Arctic Ocean, despite the overall N_{INP} in our study being lower than typical mid-latitude values.*

- **Are the references used all coming from summertime? Anything including the data from the Arctic Spring, which may not be directly comparable?**
  Yes, all reference data were filtered for summertime only.

- **It's better to compare the freezing temperature-binned averages and/or medians from each study.**
  Thanks for the suggestion. We believe that showing the complete natural variability in INP concentration is important to highlight which is why we do not compare means or medians only, which would make the plot neater, but mask that the true variability in INP concentrations spans orders of magnitude.

- **Figure 2 is difficult to read. The number of references seems excessive for no reason. What is the justification for each reference, and why are they included here?**
- The purpose of Figure 2 is to provide an overview of INP concentrations across different Arctic and sub-Arctic environments. Specifically, we had a selection of representative studies covering marine and sea-ice environments (e.g., Bigg 1996; Creamean et al., 2018); Arctic Ocean shipborne campaigns (e.g., Hartmann et al., 2021); and coastal or sub-Arctic sites (e.g., Welti et al., 2020). To improve clarity, we have simplified the figure legend by categorizing reference INP data by temperature to better navigate the reader.

Pages 9-10, Section 3.1 Temperature-dependent variability of Arctic INP Concentrations: Line 195, Line 196, Figures 3 and 4

- **1 in 10^5 particles seem pretty high as compared to previous measured arctic INP fractions from Spitsbergen reported in Rhinaldi et al. (2021) (ACP at -18 dC) and from Alaskan Arctic (Pantoya et al, (2025) AR at -20 dC). Their previous values are an order of magnitude lower at even lower freezing temperatures. The author should discuss why.**
  We thank the reviewer for pointing out our mistake. The 1 in 10^5 as INP is an estimation from the global average however, we intended to refer to the Arctic value here, which his indeed lower (1 in 10^6). We

corrected this mistake and indicated "*1 in 10^6*" in the revised manuscript, based on the approximation from our observed activated fraction of INPs.

- **Are there any other reports on freezing fraction or IN efficiency from other arctic areas for comparison?**
  Recent study reports freezing fraction from Svalbard ranges between 9.0×10^{-9} and 7.4×10^{-6} (median of 4.5×10^{-7}) at T = -15 °C (Rinaldi et al., 2025). We have added this information to the revised manuscript (see lines 258-259) "*A recent study (Rinaldi et al., 2025) observed even lower N_{INP} at -15 °C in Svalbard in spring, summer, and autumn (9×10^{-9} - 7.4×10^{-6} L^{-1}), demonstrating that lower concentrations than what we report are possible*".

- **Shouldn't the correlation be looked into for similar local meteorological conditions? Could be dependent on wind speed and direction, etc.**
  We appreciate the reviewer's suggestion to investigate the potential influence of local meteorological conditions on N_{INP}. While local meteorology can influence aerosol loading, our previous extensive studies at other Arctic sites (e.g., Ny-Ålesund, Svalbard) have consistently shown that local parameters such as wind speed and relative humidity often exhibit weak or negligible correlations with N_{INP}. For example, in Li et al. (2022) (Table S1), we performed a correlation analysis between N_{INP} and various meteorological parameters. We found that wind speed and direction show consistently low Spearman's correlation coefficient with N_{INP} at all investigated freezing temperatures. This demonstrates that local meteorology may not be a consistent predictor of N_{INP} across the entire Arctic, despite that it can be a temporarily dominant driver during extreme events when local mechanical production (e.g., wind-blown dust or sea spray) becomes important, as discussed in Section 3.6.

Page 11, Section 3.3 Chemical Composition and Sources of INPs over the Eurasian-Arctic Ocean: Line 204, Line 214;

- **The reviewer believes it should be made clearer why the author chose to use impinger samples. Impinger collects large particles and can miss some fine particles. Wouldn't this bias the results?**
  The choice to use impinger samples for chemical composition analysis was based on two main considerations. First, impinger allowed us to sample a larger volume of air (54 m^3 over 3 h) compared to the PM_{10} filter samples (27.6 m^3 over 12 h). This larger volume improved the analytical sensitivity for ICP-OES measurements, which was critical given the typically low aerosol mass concentrations in the Arctic, despite sacrificing fine particles (< 0.5 µm). Second, impinger samples offered finer temporal resolution (3 h vs. 12 h), enabling us to better resolve short-term changes in chemical composition and potential source influences. We now clarify this in Section 2.4.2 (see lines 150-152 in the revised manuscript): *Impinger samples were selected for chemical analysis due to their larger sampled air volume (54 m^3 over 3 h), which improved detection limits in ICP-OES measurements under low aerosol mass conditions. The shorter sampling duration also provided higher temporal resolution, allowing clearer attribution of changes in chemical composition to specific air mass transitions.*

- **From Fig. 2 it looks like impinger data show lower nINP as compared to the data from the other assay and previous studies.**
  Indeed, our measurement covers remote Eurasian Arctic Ocean, which was first explored for INP abundance that is intrinsically low due to the pristine environment (e.g., large marginal ice zone and ice-pack). We now add several explanations of lower observed N_{INP} from impinger in Section 3.1 (see lines 216-241 in the revised manuscript).

- **Suggesting phytoplankton blooms can enrich the INP population is speculative without evidence.**

Our statement of potential INP enrichment during phytoplankton blooms was intended as a hypothesis supported by prior literature rather than a definitive conclusion from our dataset. Although our study lacks direct biological or molecular markers (e.g., chlorophyll-a, DNA, or lipids) to confirm bloom conditions, the presence of elevated phosphorus and sulfur concentrations, coinciding with periods of enhanced INP concentrations in marine air masses, is consistent with previous studies linking marine biogenic activity to INP emissions. We have now added references to support this hypothesis (see line 371-373 in revised

manuscript): "*During this time, there is a substantial presence of the melt ponds up to 20 - 30% of the sea ice surface (Webster et al., 2015), the peak primary productivity has already passed by late summer (Ardyna et al., 2020), leading to low local sources of INP coming from the ice-covered region.*"

- **What season is the bloom in the author's study area? In some locations there is a time lag between phytoplankton bloom and the release of DMS and other byproducts.**
  In the Eurasian Arctic seas, phytoplankton blooms typically occur during mid to late-summer (July to August), coinciding with sea ice retreat and increased light availability. Our measurements were conducted mainly in August, which falls within the expected peak or post-peak period of phytoplankton activity in this region. While we did not directly measure biological or chemical markers such as chlorophyll-a or DMS, our observations of elevated marine tracers (e.g., sulfur, phosphorus) and INP concentrations during certain marine-influenced periods are qualitatively consistent with previous studies reporting similar associations during summer bloom phase.

Page 12, Section 3.3 Chemical Composition and Sources of INPs over the Eurasian-Arctic Ocean: Figure 4

- **How did the authors estimate PPM of each categorized species? Function of total aerosol concentration and dilution factor during sample prep? Please clarify it in section 2.4.2.**
  The elemental species was measured in the unit of mg/L in dilute aqueous solutions, which is equivalently converted to PPM. We mentioned a dilution factor of 10 in the original text (see line 153 in the revised manuscript): *The impinger samples were diluted by a factor of 10 with 2 % HNO_3 solution prior to the chemical analysis.*

Page 13, Section 3.4 Geographical variability of INP concentrations across the Arctic marine and ice landscape: Lines 27-28

- **Other more relevant papers seem better suited here. The reviewer suggests replacing the current citation with something like: (https://iopscience.iop.org/article/10.1088/1748-9326/ab87d3).**
  I assume the reviewer refers to lines 227-228 in Section 3.4. The reviewer is correct about the reference on the INP source of thawing permafrost, to which we have referred to Creamean et al., (2020) in the original manuscript in Section 1 (see line 36 in the revised manuscript). We also clarify the INP sources and add Creamean et al., (2020) here as the reviewer suggested (see line 310 in the revised manuscript).

Page 14-15, Section 3.4 Geographical variability of INP concentration across the Arctic marine and ice landscape: Figures 5, 6

- **Was there any impact of local precipitation and wind properties on nINP or ice active fraction?**
  In our dataset, we did not investigate the relationship between N_{INP} and local precipitation events, due to the limited temporal overlap between precipitation periods and our sampling windows.

  For wind speed, as noted in the Section 3.6 (see lines 383-385 in the revised manuscript), we observed substantially higher surface wind speeds (18 - 24 m s^{-1}) during the high-N_{INP} period compared to the low-N_{INP} period (3 - 6 m s^{-1}), likely enhancing sea spray aerosol emissions through wave breaking and bubble bursting, which may have introduced more ice-active marine biogenic aerosols (MBAs). This supports a mechanistic link between elevated wind speeds and INP enrichment during specific meteorological events. However, we did not find overall significant correlations between N_{INP} and wind speed, thus these observations remain qualitative rather than conclusive.

- **The reviewer thinks that the better matrix to present additionally with Figure 6 is the activated fraction spectra (or other spectra showing IN efficiency).**
  We now add Fig. 6b for the activated INP fraction as suggested. Accordingly, we added the discussion in Section 3.4 and Section 4, respectively. Section 3.4 (see lines 302… in the revised manuscript): "*Figure 6 (b) summarizes the activated INP fraction at selected temperatures based on the locations of measurements. For aerosols in the ice-pack region, despite being less numerous, may possess higher intrinsic IN efficiency. Similarly, at warmer temperatures (T >= -20 °C), the activated INP fraction in the*

MIZ is notably higher than in the open Ocean, suggesting a lower total aerosol background and a higher relative abundance of warm-temperature INP, likely driven by marine biological activity or wave breaking at the MIZ. Potential sources include biogenic INPs originating from marine biota, such as phytoplankton exudates…" Section 4 (see lines 412-414 in the revised manuscript): *"Specifically, the activated INP fraction at MIZ and ice-pack is notably higher compared to that in the open ocean, particularly towards warmer temperatures. suggesting a higher relative abundance of biogenic INP."*

- **The authors have PSD data alongside their INP data. Does the nucleation efficiency show the same trend as nINP, and give the same conclusion? If not, it should be explained why.**
  The main difference between the trend of INP concentration and the activated fraction is discussed in the previous answer.

Page 16-18, Section 3.5 Influence of Air mass origins on INP variability: Lines 236-237, Figure 7

- **Between local terrestrial sources and long-range aerosol transport, which is more important? Why?**
  Very good question. As discussed in Section 3.6, our high-N_{INP} case study provides supporting evidence for the predominance of local sources. Elevated N_{INP} periods coincided with short-range air masses from surrounding Novaya Zemlya and enhanced surface wind speeds associated with cyclonic activity - conditions favorable for wind-driven sea spray generation. These observations point to contributions from both regional terrestrial dust and marine biogenic aerosols. The 2-day trajectory shows another air mass source rich in INPs from the western Siberian coast, which would imply longer-range terrestrial sources are also important. This is clarified in the revised manuscript (see lines 366-371).

- **Is the air mass traveling near the ground surface or aloft? No information is provided on air mass altitude.**
  We agree that information regarding the altitude of the air masses is critical for validating source attribution, particularly for identifying terrestrial influences. To address this, we have analyzed the vertical transport history of the 7-day backward trajectories and added two new figures to the Appendix (Figs E1 and E2), displaying the spatial distribution of pressure along the trajectories and their vertical profiles relative to the planetary boundary layer height.

  The analysis demonstrates that the air parcels remained predominantly within or near the boundary layer during transport. Crucially, during the Novaya Zemlya high-N_{INP} event, air parcels tracking over the Western Siberian coast maintained low altitudes (> 900 hPa), confirming strong coupling with the surface and validating our assessment of regional terrestrial dust and biogenic source contributions. We have updated Sections 2.4.4 (see lines 178-180) and 3.5 (see lines 338-345) to explicitly discuss these altitude metrics.

- **Figure 7 or Table B3 do not offer the site-specific altitude information.**
  We have also added the starting altitude of the trajectories (i.e., the ship location at sea level) to the caption of Figure 7 in the revised manuscript.

- **Perhaps the author could offer the time series of altitudes as subpanels in Fig. 9 for high and low- INP episodes.**
  In this study, we focused specifically on near-surface aerosol transport, as our INP measurements were taken at the surface level. All trajectories were initialized at 1000 hPa and filtered to remain within the boundary layer, excluding any that ascended above it. As a result, we did not track or present multi-altitude trajectories or altitude time series in Fig. 9.

- **Figure 7 is very busy. It is difficult to find a takeaway message from this figure.**
  Figure 7 is intended to provide an overview of the air mass pathways for different INP regimes across the cruise track. The takeaway message is given in lines 316-318 and 346. We agree that the figure is visually dense, as it combines multiple trajectories across space and time and reflects how these connect to INP concentrations. In order to connect the trajectories of their origin to the INP concentrations, we need to

show all this on one plot. However, a detailed discussion in section 3.5 guides the reader through the figures and discussing the case study in Figure 9, further refines the discussion.

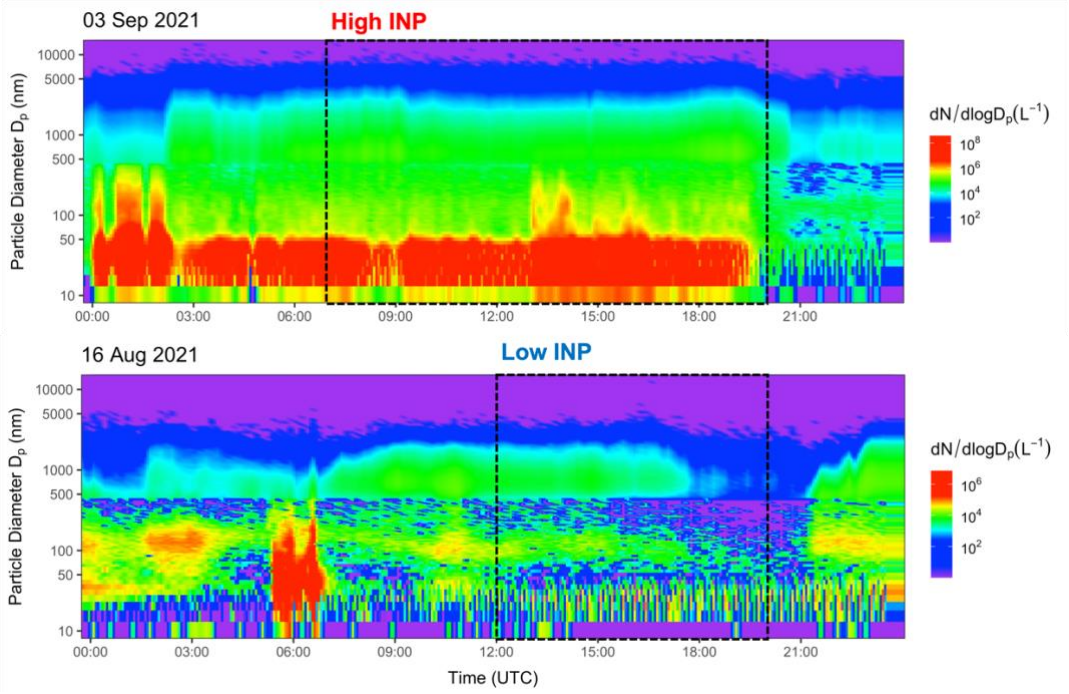
Pages 19-21, Section 3.6 Case Study: Figure 9, 10, and Line 284

- **The author should consider mentioning somewhere in this section the similarities in Pantoya et al. (2025) where they showed low INP episodes from the north and high arctic from increased amounts of ice pack contribution, and high INP episodes from south/mid-latitude, both observed in the Alaskan Arctic. This provides an example of a longer sampling period. (https://ar.copernicus.org/articles/3/253/2025/ar-3-253-2025.html)**
  We add comparisons citing findings from Pantoya et al. (2025) (See lines 368-371 in the revised manuscript in Section 3.6): *Similar patterns have also been observed in the Alaskan Arctic (Pantoya et al., 2025), where lower N_{INP} were linked to air masses originating from the central Arctic and regions with high sea ice coverage, while higher INP episodes corresponded to transport from southerly, mid-latitude regions.*

- **Figure 9 could be improved with the addition of air mass altitude information.**
  Please see our response above. Instead, we add in the caption that the trajectories are restricted to 0 m altitude at the sea level.

- **In line 284 the author uses a citation that should include: https://doi.org/10.1029/2021GL094646**
  We add the reference "Inoue et al., 2021" in line 386 in the revised manuscript.

- **Figure 10 should include an IN-efficiency comparison figure. Does nINP scale to aerosol concentration? This may provide insight for the author on INP composition and source.**
  We add the INP efficiency (activated fraction) for selected case studies in the revised manuscript (see Fig. 10b). It shows consistently lower activated INP fraction during high INP event at all selected temperatures, which could be caused by the dominance of ice-inactive aerosols, such as sea salt particles (see lines 379-381). Additionally, we now include full PSD during the selected high- and low-INP events below (see Fig. F1 in the Appendix). This allows comparison of aerosol loading and size characteristics across the two cases and offers indirect insight into potential source differences.



- **What about the role of organic particles? Is there any indication that biomass burning can be a source of high INPs?**

We do not have further evidence on biomass burning aerosols but did also not have online mass spectrometer that could have detected that.

- **Any large/local biomass burning events coincide with the authors measurement periods?**
  No.

Page 22, Section 4 Summary and Conclusions: Lines 312-113, Lines 315-316

- **In lines 312-313, perhaps the author should discuss what we expect to see in different seasons, especially in the arctic spring, when arctic haze often prevails and delivers mid-lat emissions to the Arctic.**
  We appreciate the reviewer's suggestion. Grounding our summertime "snapshot" within the broader annual cycle is essential for understanding how the Arctic INP population shifts in response to changing sea-ice and atmospheric transport regimes. While our study highlights the importance of local terrestrial and marine biogenic sources during the summer months, the Arctic atmosphere in late winter and spring is fundamentally different, dominated by the "Arctic Haze", during which long-range transport delivers an intensified burden of mid-latitude mineral dust and anthropogenic aerosols to the high Arctic. This influx provides a more consistent background of mineral INPs, which typically dominate ice nucleation at temperatures below -15 °C. In contrast, our summertime observations reveal a transition toward regional sources as the Arctic "dome" contracts and local surfaces (e.g., glacial outwash plains and open marine leads) become exposed. We have added a paragraph to Section 4 to explicitly discuss this seasonal contrast and its implications for Arctic cloud-climate feedbacks (see lines 418-426 in the revised manuscript).

- **In line 315, the author states that "future research" is needed. What research is needed? The reviewer suggests that the community needs longer INP monitoring and vertical distributions. This provides a more concrete outlook for researchers and reviewers.**
  We agree with the reviewer's suggestion and make changes accordingly in the revised manuscript (see lines 427-432): *The accelerated warming in the future Arctic is expected to reduce sea ice cover, increase surface wind speeds, and enhance permafrost thawing, all of which may augment INP emissions and lead to shifts in MPC glaciation temperatures. To better assess the resulting impacts on Arctic sea-aerosol-cloud-climate interactions, future research should prioritize long-term INP monitoring and improved vertical profiling of INP concentrations across different atmospheric layers. Such efforts will be essential for capturing seasonal transitions, identifying persistent versus episodic sources, and constraining cloud microphysical responses to evolving aerosol regimes in a rapidly changing Arctic.*

Page 27, Appendix D: Figure D1

- **Why doesn't MIZ near Severnaya Zemlya show high INPs? This seems inconsistent with the previous figure (Figure 5).**
  The apparent difference between Figures D1 and 5 may be partly attributed to the use of the same color scale, despite differences in the measurement method. Specifically, INP concentrations measured using impinger samples (Fig. D1) are systematically lower than those from PM_{10} filters (Fig. 5), as previously discussed (see Fig. 2). Additionally, impinger samples were collected intermittently with a 3-hour time resolution, rather than continuously, which may have led to the omission of transient high-INP events, particularly in regions like the MIZ near Severnaya Zemlya. In contrast, the PM_{10} filter data represent longer and more continuous sampling, increasing the likelihood of capturing peak INP concentrations. There are several other explanations for the difference in INP concentrations discussed in Appendix A.

Minor Edits:

- Page 2, Line 26: "origins of INP" should be "origins of INPs".
We thank the reviewer for the suggestion and have changed the phrase "to origins of INPs"

- Page 2, Line 31: "spend" should be changed to "spent".

In the revised manuscript, we change it accordingly to “spent” as reviewer suggested.

- Page 2, Line 40: “aerosol” should be changed to “aerosols”.
In the revised manuscript, we change it accordingly to “aerosols” as reviewer suggested.

- Page 2, Line 51: “MBA emission” should be “MBA emissions”.
In the revised manuscript, we change it accordingly to “MBA emissions” as reviewer suggested.

- Page 4, Line 79: A space should be added between “10um”.
We add a space as reviewer suggested.

- Page 4, Line 86: Change “fridge” to "refrigerator".
In the revised manuscript, we change it accordingly to “refrigerator” as reviewer suggested.

- Page 5, Line 101: The author uses “nano-pure water” here instead of “ultra-pure water” which is used in the method section. Consistency will help eliminate any confusion.
Thanks for noticing the inconsistency. We used ultra-pure water and change the text accordingly in Section 2.3.2 in the revised manuscript.

- Page 6, Line 130: “sphere” should be changed to “spherical”.
In the revised manuscript, we change it accordingly to “spherical” as reviewer suggested.

- Page 6, Line 145: “directions” should be “direction”.
In the revised manuscript, we change it accordingly to “direction” as reviewer suggested.

- Page 7, Line 154: “Backwards” should be “Backward” when referring to trajectory (in figures as well).
In the revised manuscript, we change it in five places accordingly to “backward” as reviewer suggested.

- Page 7, Line 168: The word “for” should be inserted after “except”.
In the revised manuscript, we add a “for” after “except” as reviewer suggested.

- Page 8, Line 182: “see (Tobo et al., 2019)” should be “see Tobo et al., (2019)”.
In the revised manuscript, we change it accordingly to “see Tobo et al., (2019)” as reviewer suggested.

- Page 19, Line 270: The typo “occured” should be corrected to “occurred”.
In the revised manuscript, we change it accordingly to “occurred” as reviewer suggested.

- The author should remain consistent when referring to plural/non-plural parameters. Many times in the manuscript the author switches mid-sentence. (e.g. Page 24, Line 333: “from filter compared to impinger samples” should be “from filters compared to impinger samples”).
Thanks. We examined the manuscript and make corrections accordingly.

Reference

Rinaldi, M. and Nicosia, A. and Paglione, M. and Mansour, K. and Decesari, S. and Mazzola, M. and Santachiara, G. and Belosi, F.: Ice-nucleating particles at Ny-Ålesund: a study of condensation freezing by the Dynamic Filter Processing Chamber, Aerosol Research, 3, 2, 535-556, https://doi.org/10.5194/ar-3-535-2025, 2025.

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Referee comments 3

Review of "Ice Nucleating Particle Concentrations over the Eurasian-Arctic seas" by Li et al., submitted to ACPD (egusphere-2025-2798)

Li et al. present valuable findings on INP measurements in the Eurasian sector of the Arctic Ocean during late summer 2021. These are paired with collocated aerosol size and composition data, supported by reanalysis and air mass trajectory analysis from shipborne observations. The inclusion of measurements from this sector is particularly notable, given the region's proximity to Russia and the general scarcity of INP and aerosol data from this area. This region is especially important as it serves as a key site for sea ice formation after the sea ice minimum in September. The case study highlighted in the paper is especially intriguing, with a well-supported connection between air mass history, wind speed, and aerosol composition suggesting potential fluvial-marine sources. I believe this manuscript merits publication following consideration of the comments outlined below.

We thank Referee 3 for the nice summary and positive feedback on our manuscript egusphere-2025-2798. In response to the questions and suggestions, please find our answers and revisions listed below. **Referee comments are reproduced in bold** and author responses in normal font; *extracts from the original manuscript are presented in red italic* and *extracts from the revised manuscript in blue italic*.

General comments:

My primary concern with the analysis is the lack of discussion regarding the removal of ship exhaust contamination from the online aerosol and HINC measurements. Ship stack emissions are a well-known source of interference in aerosol datasets and must be carefully filtered out, even when the vessel is in motion, since recirculated air can still impact measurements taken fore of the stacks. While previous studies have shown that ship emissions do not significantly affect offline, immersion-mode INP concentrations, the same cannot be assumed for online measurements. I strongly recommend that the authors apply an established method, such as the one developed by Beck et al. (2022), to identify and exclude periods potentially influenced by ship exhaust. Implementing such quality control steps may also improve correlations with the offline INP data.

We agree with the reviewer and appreciate the suggestion. In our study, we did take active measures to minimize its influence. For the PM_{10} filter samples (LVS on the 6th deck), sampling was automatically halted whenever the wind direction was from the ship's funnel to avoid contamination. For the impinger and HINC measurements (on the 2nd deck), we used CPC data to identify time periods as being affected by exhaust plumes based on sharp transient spikes in total particle concentration, which were filtered out and excluded from INP analysis. These measures substantially reduce the risk of exhaust contamination in the reported data, although we acknowledge that low-level or short-lived influences may not be fully eliminated. A corresponding clarification has been added to Section 2.2.1 in the revised manuscript (see lines 91-96): *In addition, to minimize contamination from ship exhaust, specific procedures were followed for the two sampling locations. On the 6th deck, the PM_{10} filter sampling (LVS) was automatically paused whenever wind direction sensors indicated air flow from the ship's funnel. For impinger and HINC measurements on the 2nd deck, spikes in total particle number concentration measured by a CPC were used to identify exhaust plumes; these periods were flagged and removed from the dataset. These measures substantially reduce the influence of exhaust emissions on the reported INP data, although minor residual contamination cannot be fully excluded.*

Specific comments:

Line 16: "...which is a decrease in cloud albedo upon glaciation…" While this precise statement is true, generally a decrease in cloud cover would lead to less surface warming on average, since MPCs contribute to surface warming most of the year except for a short period in the middle of summer (Intrieri et al., 2002). Thus, while cloud albedo decreases due to glaciation, cloud thinning reduces the stronger downwelling

longwave effects from MPCs. May want to consider reframing this statement in light of the first sentence of the paragraph.
We agree that the full radiative response of MPCs also involves potential thinning and an increase in outgoing longwave. We have revised the sentence to reflect this aspect while maintaining the highlight on the dominant shortwave effects relevant to our study (see lines 14-19 in the revised manuscript): *The phase partitioning of hydrometeors in Arctic low-level MPCs affects the Arctic's radiation budget (Korolev et al., 2017; Serreze and Barry, 2011) through cloud phase feedbacks associated with glaciation. This phase transition reduces cloud albedo, enhancing shortwave absorption at the surface during summer and contributing to Arctic warming (Tan and Storelvmo, 2019). In the wintertime, glaciation may also lead to cloud thinning and an increase in outgoing longwave radiation (Intrieri et al., 2002), the dominant radiative impact during the melt season remains the decrease in cloud albedo.*

Line 43: The authors could consider citing Creamean et al. (2022) and Wex et al. (2019) to support the statement regarding the seasonal cycle, particularly for elevated INP concentrations during summer months.
We added Creamean et al. (2022) and Wex et al. (2019) referring to the seasonal cycle of INP concentrations in the revised manuscript (see lines 44-45).

Line 47: The authors could replace Hall (2004) with more updated citations on Arctic amplification, such as Yoshimori et al. (2025) and/or Rantanen et al. (2022).
Thanks for providing the updated references and we updated accordingly in our reference list regarding Arctic amplification (see line 50 in the revised manuscript).

Line 48: Define MBA as marine biogenic aerosol.
MBA was defined in the previous paragraph at its first appearance (see line 38 in the revised manuscript).

Figure 1: The ship track is incomplete, as it does not extend to Murmansk at the end. Additionally, it would be helpful to indicate the location of the ship's exhaust stack in panel b, as this is relevant for evaluating potential contamination in the aerosol measurements.
The ship track does not extend to Murmansk due to operational restrictions imposed by the Russian Navy at the beginning of the cruise. During this initial phase, navigation data were classified and cannot be publicly released. Additionally, scientific measurements were not permitted until the ship left the restricted zone, which is why both the track and data coverage begin later.

We modified Fig. 1b to indicate the location of the ship's exhaust funnel as suggested.

Line 86: Out of curiosity, why are samples stored in the fridge overnight prior to analysis?
As part of our standard quality control procedure, the frozen samples are first stored overnight at 4 °C to allow controlled melting at moderate temperature prior to DRINCZ analysis. This approach helps prevent thermal degradation of potentially heat-sensitive IN components, ensuring consistency and reliability in the measurements.

Line 178: It is intriguing that the authors note their results are comparable to those of Hartmann et al. (2021), despite the fact that Hartmann's measurements were taken in early summer (during the onset of sea ice melt and primary productivity) whereas the present study occurs in late summer, following the peak in productivity and during the period of the annual sea ice minimum. It would be interesting for the authors to elaborate on why they consider these datasets comparable, given the seasonal and biogeochemical differences between the two time periods.
While the timing of the two studies differs, we suggest the comparable N_{INP} magnitudes arise from a seasonal succession of source mechanisms. In early summer (Hartmann et al., 2021), the onset of melt drives a significant flux of efficient terrestrial mineral INPs into the marine environment. By late summer, this is replaced by peaking marine biological productivity and maximum exposure of snow-free land, which

enhances biogenic and local dust emissions. This transition maintains a consistent N_{INP} baseline throughout the summer despite shifting biogeochemical drivers.

Figure 2: Why are the Welti, Bigg, and Creamean datasets only presented at –15 °C? Additionally, what datasets do the bright purple and orange boxes at –15 °C represent? The visibility of the Welti, and Bigg and Leck data could be improved for clarity. The authors may also consider incorporating more recent INP data from the MOSAiC expedition (Barry et al., 2025), which includes observations in a similar region west of Svalbard, as well as data from Irish et al. (2014), which, although from the Canadian Archipelago, could provide a useful spatial comparison for July and August. Note that Barry et al. (2025) is currently available as a preprint: https://egusphere.copernicus.org/preprints/2025/egusphere-2025-128/.
These datasets are only shown at -15 °C because their published data were reported exclusively (Bigg 1996) or most consistently at this temperature, making it a common comparison point across all studies for a relatively clear demonstration. The bright purple and orange boxes at -15 °C represent Creamean et al. (2019) and Bigg (1996), respectively. We have now clarified this in the figure caption and made clearer temperature-based categories to avoid confusion.

The suggested references are valuable contributions to Arctic INP research and provide important regional perspectives. We now added comparison data from Barry et al. (2025) in our updated Fig. 2 and corresponding comparison text in the revised manuscript (see lines 205-214): *In addition, our summertime N_{INP} spectra over the Eurasian-Arctic Seas are largely consistent with recent year-long observations from the Central Arctic (Barry et al., 2025; represented by orange crosses in Fig 2). Barry et al., (2025) reports that biological INPs dominate the Arctic spectrum year-round, with a significant seasonal peak in early summer (June-July) reaching concentrations up to 1.4 L^{-1} at -15 °C. While our late-summer observations (August-September) generally fall within the range of these Central Arctic measurements, they do not capture the extreme peaks observed earlier in the melt season. This discrepancy likely reflects the temporal shift in marine productivity and terrestrial runoff, which peaks prior to our late-summer campaign. Nevertheless, the overall consistency in N_{INP} magnitudes and temperature-dependent trends across these two geographically distinct sectors suggests that the Eurasian Arctic Seas share a similar INP regime with the Central Arctic Ocean, characterized by regional emissions from the marginal ice zone and snow-free land surfaces.*

Line 232: Why did the authors choose 2-day back trajectories rather than longer ones that might capture long-range transport? Also, was the boundary layer height obtained from ERA5?
The use of 2-day back trajectories focuses on identifying local to regional influences on INP concentrations, particularly in the Arctic boundary layer, as trajectories were initialized near sea level and removed if they exceeded the boundary layer height. This approach was chosen to directly link near-surface measurements with recent surface interactions. We agree that this approach may not fully capture potential long-range transport events, such as those associated with aged mineral dust or recirculated aerosols that have residence times exceeding two days. Also, the boundary layer height was indeed obtained from ERA5.

To address this limitation, we have extended our back trajectory analysis to 7 days (see Fig. E3 for all measurements at -34 °C and -15 °C; and Fig. E4 for selected high and low N_{INP} cases at -20 °C, respectively, in the Appendix) and complement our discussion in Section 3.5 (see lines 335-338 in the revised manuscript): *"This finding is corroborated by extended 7-day back trajectories (see Fig. E3 in the Appendix). Notably, elevated N_{INP} observed near Novaya Zemlya at both -34 °C and -15 °C coincide with air masses characterized by prolonged residence over the western Siberian coast - a hotspot acting as a potent source for both mineral dust and biological particles as discussed previously."* and section 3.6 (see line 368 in the revised manuscript): *"…from the west Siberian coast, suggesting INP influence from both local source and long-range transport."*

For boundary layer height, it was used to filter backward trajectories obtained from ERA5 hourly output, consistent with the meteorological fields used to compute the trajectories.

Lines 241-243: These statements are supported by Nieto-Caballero et al. (2025) and Weiber et al. (2025). The authors could consider including these citations.

Thanks for providing the updated references, and we updated accordingly in our reference list regarding thawing permafrost as a source of INPs (see lines 324-325 in the revised manuscript).

Line 272: The authors may wish to highlight that, despite the substantial presence of melt ponds covering up to 20–30% of the sea ice surface (Webster et al., 2015), the peak of primary productivity has already passed (e.g., Ardyna et al., 2020). Consequently, local biological sources of warm-temperature INPs are likely to be minimal during this period.

We thank the reviewer for this insightful point. While Webster et al. (2015) and Ardyna et al. (2020) focus on different Arctic sectors, we agree that the general phenological transition from peak productivity to a late-summer minimum likely applies to our study area, perhaps with a spatiotemporal shift. Even with substantial melt pond coverage, the biological matter available for aerosolization is expected to diminish following the main bloom. As a result, we acknowledge that this seasonal biogeochemical constraint likely contributes to the relatively low abundance of warm-temperature INPs observed during the campaign. We now acknowledge this in the revised manuscript (line 371-373) "*During the late summer, there is a substantial presence of melt ponds up to 20 - 30% of the sea ice surface (Webster et al., 2015), the peak primary productivity has already passed (Ardyna et al., 2020), leading to low local sources of INP coming from the ice-covered region.*"

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