# 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$_{\mathrm{INP}}$ *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 $N_{\mathrm{INP}}$ 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$_{\mathrm{INP}}$ *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$_{\mathrm{INP}}$ *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 $9.0 \times 10^{-9}$ and $7.4 \times 10^{-6}$ (median of $4.5\times10^{-7}$) 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 ($9\times10^{-9}$ - $7.4\times10^{-6}$ $L^{-1}$), 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-$N_{\mathrm{INP}}$ case study provides supporting evidence for the predominance of local sources. Elevated $N_{\mathrm{INP}}$ 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-$N_{\mathrm{INP}}$ 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.

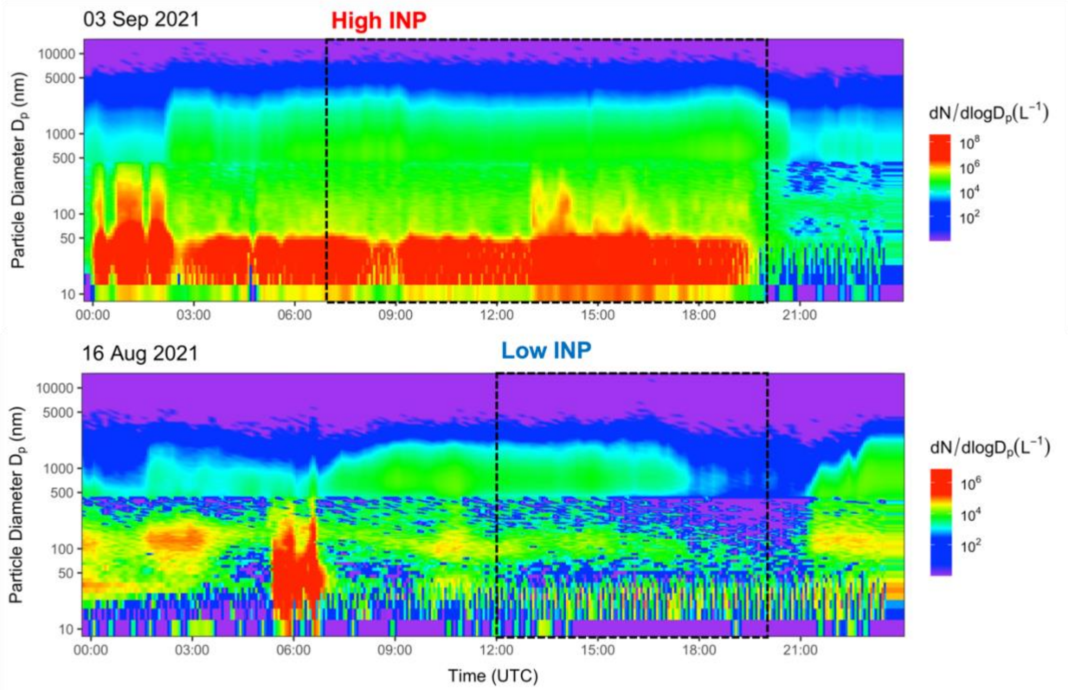
**Pages 19-21, Section 3.6 Case Study: Figure 9, 10, and Line 284**

- **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

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