We thank the reviewers for their time and effort in evaluating our manuscript. We appreciate their constructive comments and suggestions, which have helped us to improve the clarity and robustness of the work. We have done our best to meet their requests. Even if not explicitly requested, given the general tenor of the comments, we decided to reformulate the title as follows: “Ice Nucleating Particles at Ny-Ålesund: a study of condensation-freezing by the Dynamic Filter Processing Chamber”.

In the present document, original comments from the reviewers are reported in black, while answers are in blue.

REVIEWER #2

The manuscript by Rinaldi et al. presents a new dataset of ice-nucleating particle (INP) concentrations from the Gruvebadet observatory in Ny-Ålesund (Svalbard), covering spring, summer, and autumn periods during the years 2018–2020. This dataset is valuable, as aerosol–cloud interactions are of particular importance in the Arctic and are considered a potential mechanism contributing to the region's accelerated warming.

However, in my opinion, the overall quality of the study is compromised by a lack of robustness in the discussion of sample representativeness, as well as limited rigor in the application of statistical analyses and comparisons, which rely heavily on qualitative descriptions. These constitute the main methodological weaknesses of the manuscript.

The discussion of the results should be strengthened by making the connections between the presented data and the derived conclusions more explicit and well-supported. The manuscript would also benefit from a language revision and clearer articulation of its goals and main take-home message. While the development of new INP concentration datasets for the Arctic is undoubtedly valuable, it would be helpful for the authors to clarify what specific new insights their study contributes to the existing body of knowledge.

I believe the manuscript has potential, but major revisions are necessary before it can be accepted.

MAJOR COMMENTS

In this section, I provide major comments divided into two parts:

1. **General Comments:** These refer to overarching issues that affect the manuscript as a whole, including concerns about sampling representativeness, statistical treatment, and the clarity of data interpretation.
2. **Section-Specific Comments:** These address issues related to individual sections of the manuscript, including suggestions for clarification, methodological details, and improvements in presentation.

General

Sampling and representativeness of the study period

The first issue concerns the reliability of the collected filters and whether the derived data can be considered representative of the seasons in which they were collected. I am skeptical that a two-week sampling period within each season is sufficient to yield seasonally representative results. Do the authors have a justification for this choice? It may be useful to test the representativeness of such subperiods using other, longer datasets, to evaluate how well short sampling windows reflect broader seasonal patterns.

In our previous work (Rinaldi et al., 2021), we presented campaign based DFPC daily measurements in parallel with immersion freezing measurements (by WT-CRAFT) collected continuously from April to end of July 2018 (at 4-day resolution). The seasonality of INP described by the two different sampling approaches (limited to spring vs summer) was overall similar, nevertheless, it is true that the longer time coverage of immersion freezing measurements captured a variability that could not be observed relying only on the campaign-based DFPC observations. Other longer datasets are not available at Ny-Alesund, at least not at sea-level. Continuous datasets exist for the Zeppelin station, as reported in the introduction, but comparing such diverse sampling locations may lead to misinterpretation of the results. In the end, we outline that as far as regards sea-level measurements at Ny-Alesund, although with all its limitations, this work constitutes a significant step forward in terms of data coverage.

Considerations about these limitations were added in the revised text: “An additional limitation of this study arises from the fact that measurements did not span entire seasons but were instead restricted to short-term campaigns. This constrains our ability to resolve intra-seasonal variability and may, in turn, affect the robustness of our seasonality estimates. Rinaldi et al. (2021) compared campaign-based DFPC daily measurements with continuously collected immersion freezing data from April through late July 2018. While both approaches yielded broadly consistent descriptions of INP seasonality, the extended temporal coverage of immersion freezing measurements captured variability that could not be resolved using only campaign-based DFPC observations. For this reason, we advise readers to note that, hereafter, the term spring refers primarily to samples collected in April, whereas summer denotes samples collected in July. In contrast, autumn samples encompass a broader temporal range, spanning from September to November.”

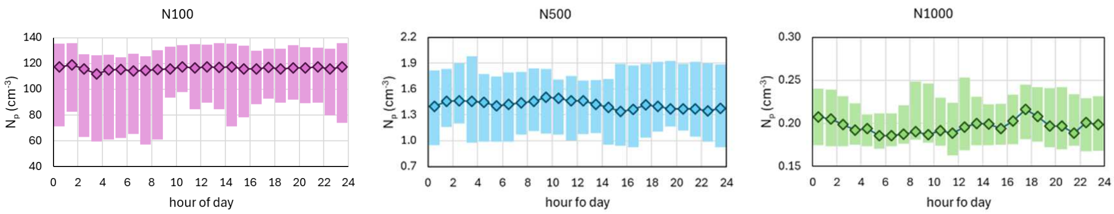
A second concern related to sampling representativeness is the limited daily sampling duration (only 3 to 4 hours per day) without discussion of expected intraday variability or justification for how representative these short time windows are of daily conditions. Additional explanation or analysis would strengthen the credibility of the dataset.

As stated in the manuscript, the sampling duration was constrained by the necessity of avoiding overloading the filters which would spoil the analysis. Unfortunately, logistical and resource limitations did not allow us to sample more than one filter per day in order to get a full coverage of the 24 hours. We have discussed this limitation of our dataset in the revised version of the manuscript. However, we can provide support to our working hypothesis that the 3-4 h snapshot we took with our sampling can be considered broadly representative of the daily concentrations.

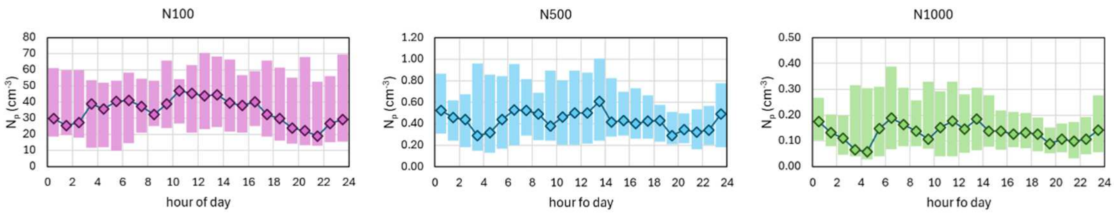
First, given the characteristics of the sampling time, conspicuous daily patterns in INP and particle number concentrations are not expected. Indeed, the site is not influenced by anthropogenic activities, so there are no peaks associated with traffic rush hours or night time domestic heating as it happens in more anthropized environments. Furthermore, the high latitude reduces the daily excursion in radiation and boundary layer height for the majority of the time (in July there is always light, while from the end of October it is constantly dark). To assess if our sampling strategy may have influenced the observed concentrations (impacting our inferred seasonality), we analysed the daily trends of particle number concentration in the following size ranges: from 100 nm to 10 µm (N100), from 500 nm to 10 µm (N500) and from 1000 nm to 10 µm (N1000). The median daily trends reported in the plots below typically do not show strong daily patterns in the particle number concentration, which appear relatively flat or showing (and small) fluctuations that appear more random than related to systematic features. This likely indicates the absence of strong daily trends for INPs as well and means that the selection of the daily sampling window could have hardly biased the INP measurements in a systematic way.

“Given the need to coordinate with other scheduled activities at GVB, sampling could not be performed at the same time during all campaigns. Specific information on the sampling intervals is provided in Table S1. The relatively short and variable sampling durations (3–4 h) may have introduced biases in the quantification of INP levels during the single campaigns and, consequently, in the estimation of their seasonal variability. However, analyses of continuous particle number concentrations did not reveal pronounced diurnal patterns, suggesting that potential biases arising from the variable sampling times were likely minimal”.

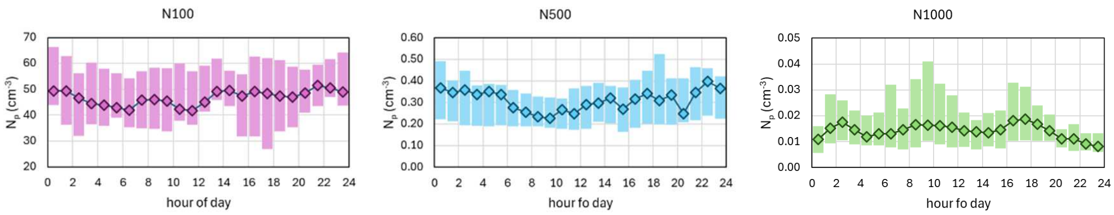
(a)



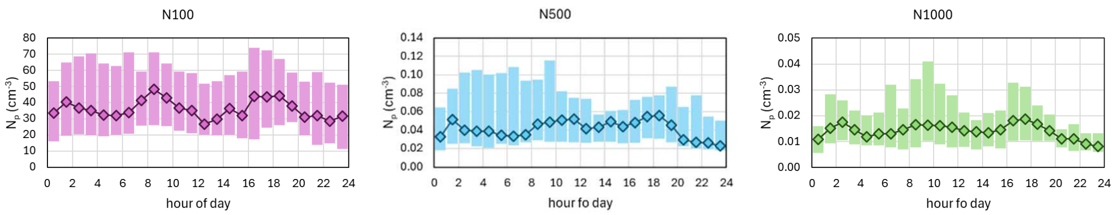
(b)



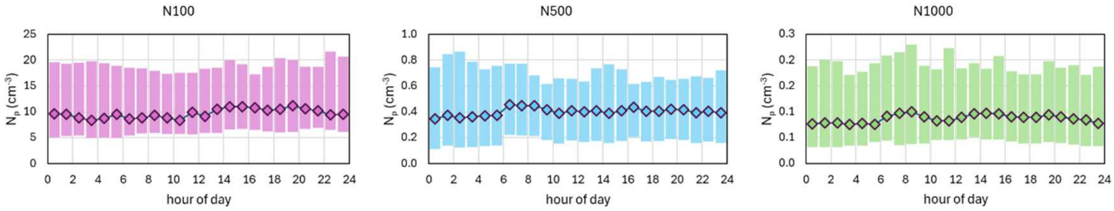
(c)



(d)



(e)



(f)

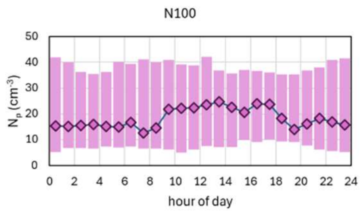


Figure A1. Daily median and interquartile range of the particle number concentration in the size ranges from 100 nm to 10 µm (N100), from 500 nm to 10 µm (N500) and from 1000 nm to 10 µm (N1000), during the (a) spring 2018, (b) summer 2018, (c) spring 2019, (d) summer 2019, (e) autumn (2019) and (f) autumn 2020 campaign.

Statistical tools and analysis

Multiple comparisons are presented in this study, using varied metrics for quantification, when provided, for example: difference in % (e.g. line 193), reduction factor (line 195), correlation (e.g. line 311), fold increase (line 226). There is no test specified establishing the statistical significance, even though in most cases, p-values are provided. The level of significance is, however, not uniform throughout the manuscript. A mix of p-values (<0.01, <0.05 and <0.1) is used depending on the specific analysis being done. Additionally, agreements and correlations are often described in a qualitative way: the use of "agreement… fairly good" (line 181), "good agreement" (line 187), "very good level of agreement" (283), or "no clear correlation" (line 311) are some examples.

Metrics for comparisons have been uniformed through the text.

We have now summarized our statistical approach to testing differences between datasets more in detail in the new Sect. 2.3.7.

In our elaborations, we have used both the t-test (assuming normal distributions of the data) and the non-parametric Wilkoxon-Mann-Whitney test (not requiring normally distributed data). For 85% of the considered cases the outcomes were the same, suggesting that the normal distribution assumption was not so far from reality in many cases. However, to be conservative we decided to consider only the outcomes of the non-parametric tests. For homogeneity, we now report through the new version of the text only the indication of the minimum tested significance level ($p<0.05$) even in cases that resulted significant for higher confidence levels.

We have also removed or modified statements that where too qualitative through the text.

Clarity of data interpretation

The overall tone of the results discussion is somewhat convoluted and overly lengthy. In several instances, the inclusion of summary tables presenting key values and statistics would help condense the information and improve readability. The main text could then focus on highlighting the general trends and key observations, rather than listing detailed numerical results. As it stands, it is difficult for the reader to retain or identify the main messages of the analysis.

We acknowledge that at times the discussion may appear complex; however, this is often inherent to the nature of scientific writing. We believe that presenting detailed numerical results is essential to substantiate our arguments and provide transparency in the interpretation of the data. Unfortunately, the need to discuss results obtained at various activation temperatures further adds to the complexity, but this is an unavoidable aspect when dealing with INP analysis. Nonetheless, we appreciate the suggestion and have included a summary table to help condense the content and improve readability (Table 2).

Section-specific

Introduction

The authors should mention the coarse particle contribution to the INP concentration. The analysis of both PM10 and PM1 filters should be justified and contextualized from the beginning.

A clearer presentation of the data gap or the specific type of analysis targeted by the authors would help to sharpen the study's purpose and make it more transparent to the reader.

We have highlighted that one of the contributions of this work is to provide "size segregated" INP information which are extremely rare in the Arctic.

"Furthermore, this work provides size segregated information on INPs, presenting the ice-nucleating capacity of sub-micrometre particles compared to super-micrometre ones and the relative contribution of such size classes to the INP pool in different seasons. Size segregated INP information is extremely rare in the Arctic environment, being provided by only a few other papers to the best of our knowledge (Mason et al., 2016; Creamean et al., 2022)".

Methods

Please review and consider the following gaps in the Methods section:

- A brief explanation of how INP concentrations are derived from the Dynamic Filter Processing Chamber measurements should be included.
- Information on particle number concentration measurements is missing and should be provided.
- The correlation analysis and statistical tools used for comparisons are not described and should be clearly outlined.
- Section 2.3.4 should be renamed to explicitly reflect the inclusion of ground-type contributions. Additionally, the methodology for assessing ground-type contributions should be clearly separated from the description of the satellite-derived ground maps and their categorization.

All the above requests have been addressed. Regarding the last point, to avoid breaking the text in too many very short sub-sections, we modified the title of Section 2.3.4 in "2.3.4 Satellite ground-type maps and ground type contribution calculation".

Results and Discussion

- The authors state that, after analyzing the seasonal distributions of their data, they observe a "good agreement" that justifies merging data from different years by season to study trends. However, this agreement is not supported by any statistical analysis, or at least none is presented in the manuscript. Furthermore, it should be noted that only in 2018 and 2019 is there a partial overlap in the sampling periods for the campaigns classified as spring and summer.

A quantitative analysis to support our statement of "very good agreement of the data distributions for the same season over different years" is now provided (Table S4). Briefly, we compared consistent periods over different years, namely April (spring) 2018 with April 2019 and July (summer) 2018 with July 2019 in terms of INP concentration. Median INP concentrations differ typically by less than 30% between the corresponding seasons, with the exception of $nINP_{-15°C}$ in spring. In these case, the difference is by 73%, which is still modest. Statistical analysis of the data distribution was performed by the Wilkoxon-Mann-Whitney test, resulting in no statistically significant differences between the compared seasons. No such a quantitative comparison was possible for autumn as the two campaigns occurred in different periods (Oct-Nov in 2019, Sep in 2020). Nevertheless, we still believe that visual analysis of Figures 3 shows a clear consistency of INP levels between the 2019 and 2020 autumn campaigns. Also Figure 5 shows a progressive reduction of super-micrometre INP contribution from September to November which is consistent with the reducing trend evidenced across the longer 2019 campaign.

- According to the authors, "seasonal variations in nINP are lower than the day-to-day variability observed within each campaign". How does the interquartile range influence the comparison between seasons? Is it taken into account in the values provided?.

Seasonal variations were described using the median values of the data distributions as a simple metric to quantify eventual difference between seasons. Interquartile and max-min ranges are however clearly shown in the Figures (e.g., Fig. 3, 4 and 5) and now also in the new Table 2.

MINOR COMMENTS

Introduction

- **Line 86:** The authors should mention that the new dataset corresponds to INP concentrations measured in the condensation freezing mode. Additionally, a brief comment on the relevance of this mode for cloud formation in the Arctic would strengthen the context.

The requested comment was added.

"All the data presented in this work have been obtained by measuring INPs in condensation-freezing mode. Condensation-freezing may play a role in Arctic cloud formation, depending on ambient temperature and relative humidity. However, its relevance remains uncertain, as observational evidence is limited by the difficulty in distinguishing this mechanism from immersion freezing - processes that may, in some cases, be physically indistinguishable (Wex et al., 2014; Hiranuma et al., 2015)".

Methods

Section 2.1

- Have blank filters been collected to evaluate background signal levels? How were they collected and stored?

Blank filters were filters not exposed to sampling which travelled and were stored together with the sampled ones. More details on blanks and blank levels are now added to the revised text.

- **Line 111:** The authors note that the samples were stored at ambient temperature until analysis. However, storage at ambient temperature can affect the ice-nucleating ability of the collected aerosol particles, particularly those of biological origin. Have the authors tested how their storage protocol may influence the results, for instance, by comparing it with cold-storage conditions?

Unfortunately, such tests were not done. Actually, there is still no consensus in literature about the effect of storage conditions on INPs. As such we would prefer not to open this line of discussion for which non conclusion can be provided, having already clearly indicated how we stored the samples.

Section 2.2

- **Line 115:** Please briefly introduce the operating principle of the Dynamic Filter Processing Chamber (DFPC).

Details were added.

- **Line 117:** Can the authors justify why the selected temperatures and supersaturations were chosen for the analysis?

T = -22 and -15°C are the lowest and highest temperatures at which the DFPC can operate providing reliable results. T = -18°C was chosen as a convenient intermediate T setup.

Regarding supersaturation, operating in condensation freezing mode at S_w = 1.02 is the standard approach with the DFPC, even though higher supersaturations could be achieved as, for instance, in Belosi et al. (2018). A supersaturation of 1.01-1.02 is typically associated with atmospheric clouds (Pruppacher and Klett, 1997; DeMott et al., 2011) and this is the motivation of this choice.

Belosi et al. (2018), Tellus B, 70, 1–10, https://doi.org/10.1080/16000889.2018.1454809

Pruppacher , H. R. and Klett , J. D. 1997 . Microphysics of Clouds and Precipitation , Kluwer Academic Publishers , Dordrecht , 954pp .

DeMott , P. J. , Möhler , O. , Stetzer , O. , Vali , G. , Levin , Z. and et al. . 2011 . Resurgence in ice nuclei measurement research . Bull. Amer. Meteor. Soc. 92 , 1623 – 1635 . DOI: https://doi.org/10.1175/2011BAMS3119.1 .

- **Line 120:** The reported uncertainty in DFPC-based INP concentrations is 30%. How was this uncertainty accounted for in the subsequent analysis of seasonal trends, for example?

The measurement uncertainty was considered when comparing single *n*INP data points. Nevertheless, it would not be correct to interpret the seasonal variability (which we report as season median values) in light of the random uncertainties associated to each individual sample: random uncertainties can be assumed to compensate each other when the median of a sufficiently large number of data points (as in our case) is calculated. Median seasonal values would be affected, in case, by systematic errors, which we have no reason to assume to be present in the dataset, but not by random errors. To provide a framework for interpreting the variability of the seasonal median values, we show and report their associated interquartile range and 5-95th percentile range, which is most appropriate.

- **Line 121:** How was the background correction applied? Please specify the method used and provide appropriate citations.

According to the campaign, the DFPC background at the three activation temperatures was obtained measuring 3 non-sampled filters. The correction of the measurements for the background occurred in two steps:

1) the number of crystals counted on each sampled filter after the analysis was subtracted of the average number of background crystals (i.e., average of 3 or 4 non-sampled filters)
2) the standard deviation associated to the mean number of background crystals was accounted for in the INP uncertainty calculation by error propagation.

For the majority of the samples, the background level was small or even negligible with respect to the number of counted crystals.

The above information are now added to the test.

Section 2.3

- **Line 126:** Please specify the location from which the rain data was obtained.

Details were addedd: "Meteorological parameters (T; pressure; relative humidity; wind speed) were provided by the Amundsen-Nobile Climate Change Tower positioned less than 1 km N–E of GVB (Mazzola et al., 2016), while precipitation data (type and amount, measured in the center of Ny-Alesund by the Norwegian Meteorological Institute) were taken from the eKlima database (https://seklima.met.no/observations/, last access: 21 September 2022)".

- **Line 130:** What is the temporal resolution of the black carbon data? How were they temporally aligned with the INP observations?

Hourly BC data were used. We simply averaged the BC data in order to match the INP sampling time.

Results and discussion

Section 3.1

- **Line 171:** Please specify how particle number concentration was measured.

Done. A new Section was added (2.3.2 Black carbon and particle size distribution measurements).

- **Line 179:** Could the authors calculate the activated fraction (AF), as done in Li et al. (2023), to quantitatively assess the agreement? Additionally, a quantification of the discrepancy due to the use of different lower size cutoffs would be beneficial.

Calculation of AF500 was performed for the autumn 2019 campaign. The obtained AF range is between 3.1 $\times 10^{-7}$ and 1.1 $\times 10^{-3}$ in line with the findings by Li et al. (2023). We have reported this in the manuscript and added a Table in the Supporting (Table S3).

Nevertheless, we decided against adding further investigations of the discrepancy in AF due to the use of different lower size cutoffs in the text as this will go beyond the purposes of this study.

Section 3.2

- The results and discussion throughout this section should be restructured and better organized to improve readability. Several different aspects are addressed (such as the seasonality of nINP, the activated fraction, and the contributions of fine and coarse particle fractions to nINP). I recommend reorganizing the section to clearly distinguish and structure the discussion around each of these key themes, including subsections if necessary.

The Section was dived into Sub-sections.

- **Line 187:** Please support the statement "very good agreement" regarding the data distributions with appropriate statistical evidence.

Done. New text was added and Table S4.

- **Line 192:** Clarify that the values being compared are medians.

It is already clearly stated in the following line: "nevertheless we report a slightly higher median *n*INP in spring both at T = -22°C (by 33%) and at T = -18°C (by 17%)." If the reviewer refers to the statistical significance tests, they where not run on the medians but on the whole data distributions.

- **Lines 192–200:** Statistical significance should be assessed and reported for all the comparisons presented. In addition, please standardize the way seasonal changes are quantified—e.g., use either percentage change or increase/decrease factors consistently—or justify the use of different metrics.

Statistical significance tests are now all presented in the same way and the approach is described in Sect. 2.3.7. All the seasonal differences are now treated in the same way.

- **Line 200:** Please indicate where in the figures or data the snow and sea-ice melt is shown. Reference the specific figure, panel, or dataset.

This is a general consideration, based on our knowledge of the site: in July the land surface is free of snow and sea-ice extent is at its minimum.

- **Line 209:** Can the authors clarify what is meant by "background of cold INPs"?

This statement refers to the conclusions of Sze et al. (2023). According to their study, the difference in INP concentrations between winter and summer decreases with decreasing activation temperature, due to a persistent background of mineral dust particles present in the Arctic year-round. As a result, the seasonal increase in INPs during summer is evident at warmer activation temperatures, but becomes progressively less pronounced at colder ones. To account for the fact that we observe little to no summertime enhancement already at −18 °C, we suggest that the cold INP background may be even more dominant at Ny-Ålesund compared to more northerly locations. Sze et al. (2023) states that "it is likely that the observed background INPs originate from long-range transport," given that "local Arctic sources are sparse in winter, due to the surface being covered in snow and ice." This may help explain why Ny-Ålesund is particularly influenced by such transport processes, as it is located closer to lower-latitude, snow-free regions that are likely sources of these INPs.

A few more explanatory considerations were added to the text: "A common finding across all these studies is that the difference in nINP between summer and the rest of the year diminishes with decreasing activation temperature. According to Sze et al. (2023), this trend is attributed to a persistent background of mineral dust particles (active at T < -15°C) present in the Arctic throughout the year, which progressively obscures the summertime increase at colder temperatures".

- **Line 210:** How do the studies cited compare quantitatively with the summertime increase reported by the authors?

Sze et al. (2023) report and increase of *n*INP at T=-15°C of ~4 times in summer (quasi-snow-free months) than in the rest of the year, while it is ~1.4 at T=-22°C.

This was added to the text.

For Creamean et al. (2022) only a visual analysis of Figure 4 was possible, due to problems in reading the data files provided with the paper. The following description was added: "Recently, Creamean et al. (2022) confirmed these findings by ship-borne measurements in the high Arctic, evidencing a remarkable increase in June and July, with respect to the rest of the year, for INPs active at T < -15°C and a less evident increase for higher Ts".

Section 3.3

- This section seems more appropriate as a case study that could be incorporated under Section 3.2.

We have now divided Sect. 3.2 into sub-sections. We prefer to leave Sect. 3.3 as a separate section even though with a new title.

- **Line 283:** The authors mention a "good level of agreement" between their nINP ranges and those measured in parallel by Li et al. (2023) in immersion mode, supported by Figure S6. Please include a quantitative assessment of this agreement. How well do the two datasets align when compared as time series? Additionally, how different did the authors expect the datasets to be, given that different freezing modes were investigated?

We have provided some details to make our statement quantitative: "For instance, the Pearson's correlation coefficient (R) between the two *n*INP time series at T = -15°C is 0.69, with a mean absolute error (MAE) of 13.1 m^{-3} and 84% of the points lying within a factor of 5 from the 1:1 line". Nevertheless, being an instrumental intercomparison not the purpose of this work, we would like to keep this to a minimum level.

- **Line 293 and Figure 6:** The three study periods should be more clearly identifiable in Figure 6. As currently presented, the lines marking the "dark period" and "polar night" distract from clearly distinguishing the discussed periods.

Done.

- **Line 302:** The reference to Figure S3 appears to be incorrect. Figure S3 does not depict air mass trajectories.

Corrected.

Section 3.4

- **Line 314 and Table 2:** I strongly recommend revising the code to report statistical significance using a more standard notation, such as ** ($p < 0.01$), * ($p < 0.05$), and † ($p < 0.1$). Note that p-values < 0.1 are sometimes interpreted as indicating "marginal significance."

Done.

- **Line 323:** Can the authors clarify whether these two subsets were taken from the entire study period?

Yes, indeed they were. This information was added.

- **Line 326:** Are there cases where airmasses spend more than 50% of their trajectory time over seawater but are still classified as land-influenced (i.e., over land for more than 5% of the time)?

Yes, there are. Indeed, this is the most typical case and the fact that the land influenced sub-group presents higher concentrations than all the rest of the samples and also of the sea-water dominated ones confirms that snow-free land is a powerful source of INPs.

- **Line 327:** Please refer to Figure 8a.

We are referring to the whole Figure 8 here.

- **Line 333:** Again, Figure S3 does not show the geographical locations of potential sources.

Corrected.

- **Line 343:** How are marine aerosol emission mechanisms taken into account?

CWT is a purely statistical approach which does not consider aerosol emission mechanisms.

- **Line 348:** Please briefly comment on why the fine size fraction of INPs correlates better with chlorophyll-*a* levels.

This was discussed in Rinaldi et al. (2021): "In our interpretation, the lack of a correlation between surface CHL concentration and coarse INPs does exclude the potential of the ocean surface to be a source of super-micrometer INPs. Rather, it simply shows that CHL is not the appropriate proxy to track the emission of large biological INPs from the oceans. Indeed, while CHL has previously been observed to correlate with the enrichment of organic matter in sub-micrometer sea spray (Rinaldi et al., 2013; O'Dowd et al., 2015), no investigation has ever been attempted with super-micrometer particles. In a laboratory-controlled setting, McCluskey et al. (2017) showed the production of both sub- and super-micrometer INPs (active at -22°C) from controlled algal blooms, pointing out that different particle types and production mechanisms are involved.

Some considerations were added in the revised manuscript as well.

- **Line 362:** Incorrect reference to Figure S3.

Corrected.

- **Line 365:** Can the authors provide an appropriate reference to support their interpretation regarding the lack of dependence on lag time?

Citations were added: Mansour et al. (2020a, b), Mansour et al. (2022). All of the already in the reference list of the paper.

- **Line 378:** Please specify the evidence on which the authors base their statement that the "summertime aerosol particle population appears more related to local (Arctic) sources."

- *n*INP does not correlate with BC in summer and autumn, this excludes major anthropogenic sources (Sect. 3.4.1).

- *n*INP is mostly contributed by coarse particles in summer (Sect. 3.2) and such contribution is still quite high in early autumn reducing progressively through autumn (Sect. 3.3): this points to local sources as super-micrometre particles generally do not travel for long distances.
- In Sect. 3.4.2 and 3.4.3 we show that potential sources of INPs during the period of minimum snow and sea-ice cover are potentially terrestrial sources (rarer occurrence but apparently these sources has a higher magnitude in producing INPs) and marine sources. For the latter we were able to pinpoint the most likely source locations, which were sea regions immediately to the south of Svalbard islands and around Iceland, i.e., local.
- The majority of air mass contacts with seawater occur in summer and early autumn (Fig. 7), reducing in October and November as we show in Sect. 3.3.

All this shows that summertime aerosol particles appears related to local sources (natural, both marine and terrestrial) which progressively reduce their contribution towards autumn.

The text object of this comment and of the previous one now reads as follows:

"The correlation with BC observed only in springtime (Sect. 3.4.1) and the aforementioned dominance of sub-micrometre INPs in this season (Sect. 3.2) support the hypothesis that the primary sources of springtime INPs at GVB may be located outside the Arctic. In spring, the Arctic receives long-range transport of anthropogenic aerosols from lower latitudes (Stohl, 2006; Heidam et al., 1999), during this time, INPs likely originate from the same source regions and are carried northward with the Arctic haze. Consequently, the AF estimates presented above support the hypothesis that long-range transported aerosol particles from lower latitudes nucleate ice less efficiently than local-origin aerosol particles, being spring the season characterized by the lowest AF. This aligns with the results reported by Hartmann et al. (2019), evidencing a minimal influence of human-induced emissions on Arctic INP levels, as evidenced by a comparison of present-day and pre-industrial values from ice core analyses, and with the pioneering study by Borys (1983).

In contrast, the summertime aerosol population appears to be more strongly influenced by local Arctic sources. The absence of correlation with black carbon (Section 3.4.1) and the prevalence of super-micrometre INPs (Section 3.2) suggest that the dominant summertime INP sources are natural and located at relatively low distance. Further support comes from Sections 3.4.2 and 3.4.3, which indicate that during periods of minimal snow and sea-ice cover, terrestrial and marine sources with a local Arctic origin contribute to INP levels. Additionally, Figure 7 and Section 3.3 show that air masses had the highest contact with seawater in summer and early autumn, decreasing steadily through the fall. This implies that summertime INP sources may continue to influence the early autumn period, with their impact gradually diminishing later in the season".

- **Line 380:** Please include an explicit reference to the AF mentioned (e.g., figure or table).

We referenced the reader to Figure 4 for an overview of the seasonal evolution of AF.

- **Line 383:** Please clearly connect the results reported by Hartmann et al. (2019) to the corresponding reference.

We are not sure we have interpreted this comment correctly, however we modified the sentence as follows: "This aligns with the results reported by Hartmann et al. (2019), evidencing a minimal influence of human-induced emissions on Arctic INP levels in pre-industrial ice core records". We have checked and the reference is correctly reported in the reference list.

Conclusions

- **Line 421:** Please clarify what the authors mean by "higher cold INP background" and better connect this statement to the supporting evidence**.**

Please refer to the above comments for a detailed discussion about the cold INP background.

This sentence was modified into: "This could be explained assuming a higher background concentration of cold INPs of likely mineral origin at GVB with respect to other stations located at higher latitudes. This may be able to somewhat mask the impact of summertime sources at T = -18 and -22°C".

- **Line 424:** This conclusion is trivial since the activated fraction (AF) depends on both INP and non-INP aerosols.

This sentence was removed.

- **Line 438:** Please elaborate on the potential sources related to the suggested long-range transport mechanism and the associated aerosol types.

We respectfully believe that this is not the right place for further elaborations. The association between spring time INPs and the Arctic haze phenomenon has already been addressed in the previous sections.

Figures and tables

- In Figures 1 and 2, I recommend using semi-transparent symbols for the "all data" points to better illustrate the distribution of individual data points. Alternatively, color-coding could be used to represent the density of observations.

Done as suggested.

- In Figures S1 and S2, the symbols corresponding to **T = –15 °C** appear smaller than the others; please ensure consistent symbol sizing across all temperatures.

Done.

- I suggest several modifications to Figures 3, 4, and 5 to improve clarity. First, I recommend using traditional box-and-whisker plots (similar to Figure 8a) and adjusting the plot widths to reflect the time period of the measurements. I also encourage adjusting the spacing of the x-axis ticks to make it more uniform. Finally, consider merging Figures 3, 4, and 5 into a single multi-panel figure to facilitate easier comparison.

Figures 3, 4 and 5 were modified following the reviewer's suggestions but we decided to keep them separated as it would result in a very complex multi-Figure otherwise.

- Table 1 could include row headers indicating min-max values. "Tabel" should be Table.

We reported that information in caption to keep the Table easy and readable.

- I suggest a few changes to Figure 8a to improve clarity. Temperatures could be placed on the x-axis. The number of colors can be reduced to two: one representing land-influenced and the other seawater-dominated conditions. These modifications would simplify the legend. Additionally, please clarify the meaning of the "x" inside the boxes.

We appreciate this suggestion, the figure was modified accordingly.

TECHNICAL COMMENTS

A thorough review of technical aspects has not been conducted. However, attention should be paid to the citation list to ensure consistent formatting throughout.

References

Li, G., Wilbourn, E., Cheng, Z., Wieder, J., Fagerson, A., Henneberger, J., Motos, G., Traversi, R., Brooks, S., Mazzola, M., China, S., Nenes, A., Lohmann, U., Hiranuma, N., & Kanji, Z. (2023). Physicochemical characterization and source apportionment of Arctic ice-nucleating particles observed in Ny-Ålesund in autumn 2019. Atmospheric Chemistry and Physics, 23, 10489–10516. https://doi.org/10.5194/acp-23-10489-2023.

Hartmann, M., Blunier, T., Brugger, S. O., Schmale, J., Schwikowski, M., Vogel, A., Wex, H., & Stratmann, F. (2019). Variation of ice nucleating particles in the European Arctic over the last centuries. Geophysical Research Letters, 46, 4007–4016. https://doi.org/10.1029/2019GL082311

Citation: https://doi.org/10.5194/ar-2025-13-RC2