

Response to reviewers for ESSD paper “GloPINE dataset: model-ready measurements of INP concentrations using PINE instruments”

Dear editor, we have considered all of the comments and suggestions by the two reviewers and respond to each of these in turn below. Reviewer comments are in black with a unique identifier (e.g., Comment R1.1), whilst our responses are in red and indented. Where appropriate, we have included the new or amended passages of text that are included in the revised manuscript.

Reviewer 1

This study documents an extremely valuable resource for the global observational and modeling-based ice nucleating particle communities. The data set is indeed a special one in containing data from multiple sites in the Northern Hemisphere that were collected in some cases for very long periods of time and over diurnal cycles in many cases. I did stumble with accepting the position that a special advantage of the sole use of PINE data removes the need for multiple instruments. While I understand that it could be possible to operate a network of such instruments to emphasize a procedure (not unified in this study) to focus on collecting higher temperature data at INP concentrations higher than about 0.01 per standard liter, the study also makes a compelling statement to me that confirms the need for integrating measurements such as those made offline using filter samples to adequately cover the full span of cloud conditions expected in the atmosphere. The PINE data represent a great advancement on the acquisition of substantial data that has special application to modeling needs but cannot easily access the range of temperatures and lower INP concentrations that may be most critical to representing and simulating mixed phase clouds. Am I wrong to conclude that? I also emphasize below the need to be a little more explicit about the methodological analysis of INP concentrations using raw PINE data. It may be obvious to the PINE community, but it bears attention for detailing to those not versed in using this instrument. Further, discussion of inferred sample volumes will help the casual reader understand better the extent to which the global atmosphere can be interrogated using real-time sampling instruments. My recommendation is that this paper could be modestly revised and be fully acceptable for publication.

We thank the reviewer for taking the time to carefully review our manuscript and for their positive summary. We respond to each comment in turn below.

Comment R1.1

Lines 4,10: The statements regarding hours of sampling or numbers of INP measurements are important and relevant, but also important and relevant are the volumes collected. Certainly, that information is more revealing about the sampling possible with real-time measurements. Perhaps give it in standard liters or standard cubic meters. Is that possible? I think, based on what I learned later in the discussion around Figure 2 it means that the data set herein represents an assessment based around 800 m³ of air at defined conditions. It is a lot by the standards of historical INP measurements but not necessarily by the standards

of trying to represent what is present in the global atmosphere. This is not a negative point I am asking to be documented, just a practical one.

We highlight the number of campaigns and the number of expansions to give an indication of the value of this dataset. But, this does not mean it is less or more useful than other datasets. As detailed below, we have included the volume sampled as an additional parameter. But, sampled volume alone does not provide a measure of how representative a dataset is of the atmosphere as a whole.

In the revised manuscript we now include the volume sampled per expansion. We include the following text in Sect. 2.2 of the revised manuscript:

“...A brief characterization of each campaign is provided below and includes the mean volume sampled per expansion during each campaign. The volume sampled is calculated as the inverse of the minimum measurable value of N_{INP} (per litre of sampled air per expansion) during the campaign, which is consistent with a measurement of one ice crystal (per expansion); see Sect. 3 for more details.”

In the revised manuscript we include campaign-mean values of volume sampled per expansion to Sect. 2.2 (Campaigns) and we also include the variable V_{tot} in the GloPINE dataset, which is the total volume of air sampled during each GloPINE time and temperature interval. For example, during the ExINP-ZEP campaign, the mean volume sampled per expansion was $\sim 2.1 \times 10^{-3} \text{ m}^3$, whilst V_{tot} has a median of $\sim 0.21 \text{ m}^3$ (~ 10 expansions within the interval) and has a maximum of $\sim 1.25 \text{ m}^3$ (~ 70 expansions within the interval). In calculating V_{tot} , we noticed a minor error in some values of $\lim N_{\text{INP}}$. This has been updated in the revised manuscript and has a minor impact on the determination of the quoted CV_{ss} (now $\text{RSD}(T,t)$) values.

We can sum the number of expansions within each GloPINE interval over all campaigns to estimate the total volume sampled in the dataset, which we report in the revised manuscript. The value for non-zero INP concentrations is 816 m^3 , which is close to the value estimated by the reviewer. This is now included in the abstract and the conclusions section.

Comment R1.2

Line 51: I find the statement that INP concentrations are “known to vary by several orders of magnitude during a single diurnal cycle” to be a bit misleading regarding the more typical scenarios at one set of conditions. I suggest “can sometimes vary” describes the situation more accurately. Certainly, with air mass changes there can be such a variation in a diurnal cycle and I believe this has been captured in a few earlier papers regarding changes following atmospheric aerosol perturbations (e.g., ones documenting changes following rain events, or passing through smoke plumes when operating continuous flow chambers) besides the one referenced. But such data, including the reference cited, also shows sometimes days at a time without a major diurnal cycle. This can vary by site and the season. Your point though is understood and valid, nonetheless, so only recommending this small change.

We have changed the wording of this phrase as suggested.

“In addition, in many campaigns, the duration of the deployment is typically on the order of days to weeks. This restricts model evaluation to daily or monthly mean comparisons, which is likely an inadequate test for a variable that **may vary** by several orders of magnitude during a single diurnal cycle **under certain conditions** (e.g., Canzi et al., 2025, Fig. 5a).”

Comment R1.3

Line 62: Is the “less than 0.01 L⁻¹” lower bound before or after consolidating measurements into the 6 h time intervals?

Yes, this is after consolidating. To avoid confusion we have moved this sentence and rewritten for clarity:

“The interval-averaged measurements span a range of concentrations from less than 0.01 L⁻¹ to more than 1000 L⁻¹ and temperatures between 240 K and 263 K.”

Comment R1.4

Line 91: Before the next paragraph on uncertainties, I expected to learn how the concentrations are defined. Are they continuously measured during the expansion and somehow integrated over narrow temperature ranges or is some time used around the lowest pressure and temperature of the expansion for which concentrations are defined by the volume sampled? This information is in older papers I believe, but it is not in section 2.3. The size distribution is mentioned, but over what time interval and acquired volume is that defined in each expansion?

We have expanded the section to briefly explain the principles of the instrument, though we refer to Mohler et al. (2021) to provide the full details. We determine the cumulative ice crystal concentration on cooling to X K (units: L⁻¹), rather than the differential concentration (units: L⁻¹ K⁻¹).

The new sentences in Sect. 2.1 read:

“The cumulative number of ice crystals (n_{ice}) measured by the OPC in the course of the expansion is used to calculate the cumulative concentration of INPs (N_{INP}) per liter of sampled air by dividing n_{ice} by the total volume of air passing through the OPC detection volume. For the welas sensor (on PINE-01-A), this volume is 10% of the total volume passing the OPC, whilst for the fidas-pine sensor (on all other PINES), this volume is equal to the total volume passing the OPC. For additional details see Möhler et al. (2021) and Büttner et al. (2025); typical output for a PINE expansion can be seen in Möhler et al. (2021, Fig. 3).”

Comment R1.5

Considering the previous comment, could information on single INP measurement sample volume also be included in the information for each project. Otherwise, there are details relevant to PINE users about times spent flushing and expanding and refilling, but not on effective sample volumes per sample. Is it always the same in each study?

This has been done. See the response to Comment R1.1.

Comment R1.6

Lines 227-228: Is the OPC particle size distribution mentioned as critical information that is measured at the minimum temperature? Or over what time to that point? Again, trying to confirm understanding of how concentrations are determined, exactly, and what volume is represented.

Along with the revisions made in previous comments to address related comments, we have rewritten the lines in question to better describe the process:

“Post-processing software is used to convert the raw Level 0 data from each PINE run into relevant Level 1 data; a full description and evaluation can be found in Büttner et al. (2025). The key information taken from the Level 0 data includes the minimum temperature during the expansion and the hydrometeor size distribution measured by the OPC between the start and end of the expansion, which the software uses to establish a size threshold for each expansion. This threshold, typically on the order of 10 μm (Möhler et al., 2021), is used to separate the smaller liquid droplets from the larger ice particles, thus determining n_{ice} ; N_{INP} can then be determined following the process described in Sect. 2.1.”

Comment R1.7

Line 244: This is the first mention of the 2 L volume. Please integrate this into the above discussions to make it clear how this is determined.

This has been done. Please see the response to Comment R1.1.

Comment R1.8

Figure 2: This is a very nice figure!

Thank you!

Comment R1.9

I understand that this is a database paper emphasizing the utility of a unique and comprehensive data set for constraining model representations of INPs. Still, is there a need to make any statement regarding the suitability of the method for providing more

measurements at above 255 K, or if this method can be stand alone to characterize that regime where there are still many time periods below detection limits? I have also not raised the issue that concentration measurements are but one way to characterize INPs, though surely even the modeling community is going to ultimately want to know more information about validating their representation of sources and if ways are imagined for integrating PINE technology with other techniques to address that question.

The last comment above should perhaps be addressed here instead. At least one statement on the deficiencies of single instrument use could be helpful. The limitations at higher temperatures seem clear and a continued need for the community. One imagines that PINE data is no less valuable if combined with another, especially for “robust” constraint of model representation of INPs. Otherwise, it seems that model parameterizations are wholly trusted for comparison to total INP concentrations.

We respond to these two comments together. We agree with the reviewer and have included a new paragraph in the conclusions section to discuss how this dataset, and specifically its current temperature range, fits alongside other INP measurement methods and techniques used by the community.

“The GloPINE dataset is complementary to other INPs datasets, and together they will be important for fully characterizing the role of aerosols as INPs across the entire temperature spectrum. Alternative INP measurement techniques, such as those using offline filter-based sampling methods (e.g., Creamean et al. (2025)), often report INP concentration measurements at higher temperatures (typically above 255 K) than the bulk of the GloPINE data in its current iteration. This is achieved through sampling over longer time periods (typically several hours to days), resulting in a relatively coarse temporal-resolution that may fail to capture diurnal cycles in INP availability. Filter samples also provide an opportunity to assess aerosol composition, size distributions, and source attribution using additional analytical techniques. Current technical developments aim to couple PINE with aerosol characterization instruments, thereby complementing online INP measurements with simultaneous, high temporal-resolution information on the sampled aerosol (Lacher et al., 2026). In summary, we recommend combining the GloPINE dataset with other INP datasets to enable robust model evaluation, improve source attribution, and inform parameterizations across the wider temperature range over which aerosols influence primary ice production”

We have also amended the abstract by removing the following sentence:

“The frequency and duration of measurements combined with the lack of instrument or methodological variability provides a means to robustly evaluate and constrain global models on a scale that has not previously been possible.”

And replacing with:

“Together with complementary INP datasets, GloPINE provides a valuable resource for advancing model evaluation, improving INP source attribution, and informing parameterizations across the full temperature range over which aerosols influence ice formation in clouds.”

Reviewer 2

Herbert et al, present a collation of PINE-based INP measurements for the community to use. This is an excellent resource for atmospheric science community and then manuscript is generally well written. That said, some clarifications on the INP concentration calculation and counting statistics should be included in the manuscript to make it more assessable for the community, who has for the most part, are used to using INP measurements from offline techniques. Below are some additional comments the authors should consider before publication.

We thank the reviewer for providing us with this positive summary. We have amended the revised manuscript to include the suggestions provided by the reviewer and detail our responses to each comment below.

Comment R2.1

There is no description of how the humidity in PINE is controlled or measured. If ambient air is introduced into the chamber and the walls are set to subzero temperatures, what keeps ice from forming on the walls of the chamber or for a cloud to already form due to isobaric mixing before the expansion even begins? Are cloud droplets in the OPC a necessary precursor (immersion freezing) for ice crystals to be counted as INP?

Humidity is controlled using membrane diffusion dryers on the inlet air and maintained at a level that prevents excessive frost from forming on the walls and producing ice artefacts. We have expanded Sect. 2.1 to briefly describe the treatment of humidity. The new passage of text reads:

“The humidity of the air being drawn into the chamber is controlled by two Nafion membrane diffusion dryers in parallel, which are set to keep the relative humidity high enough for cloud droplet formation to occur and low enough to avoid excessive frost formation on the chamber walls. If excessive frost does form on the chamber walls, frost particles can become dislodged and counted as ice crystals by the optical particle counter (OPC). Regular zero checks are performed where HEPA filtered air is passed through the chamber (see Möhler et al. (2021) for details); the buildup of ice on the chamber walls is avoided by regularly warming the chamber to remove any ice. The frequency with which the chamber is warmed varies between campaigns, depending on factors such as the humidity of ambient air and the flow rate through the dryers.”

With regards to the second part of the comment, the PIA software that converts the raw Level 0 data into an INP concentration uses an ice threshold that can only be determined if a liquid cloud is visible in the OPC. Therefore cloud droplets are implicitly required to be present in order for ice crystals to be counted.

Comment R2.2

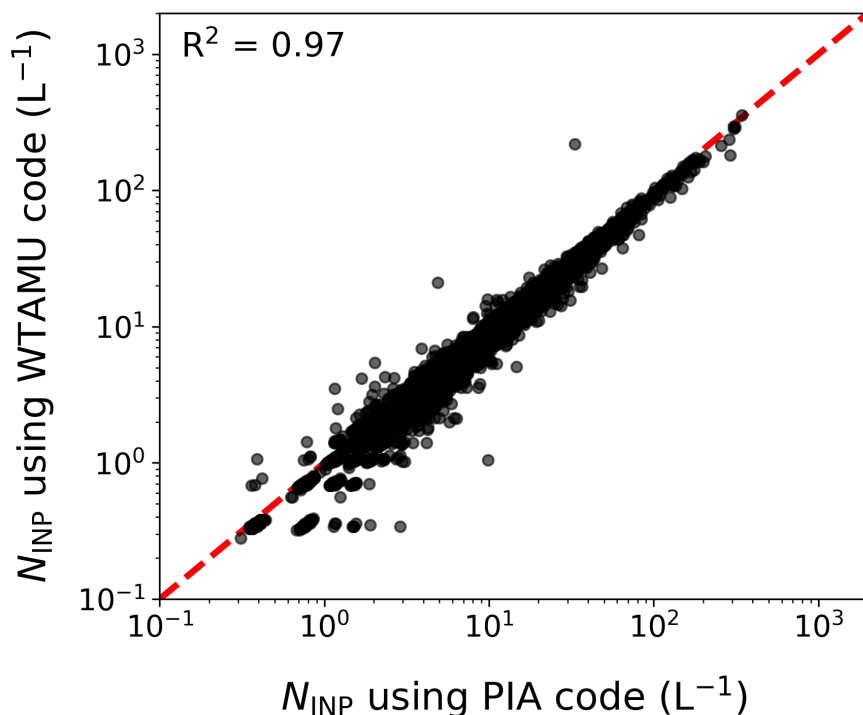
Is there a reason the flush varies so much between campaigns?

Slowing the flush flow allows the dryers to work more effectively, removing more water. Hence, individual users have adjusted the flush flow to control the RH of air entering the chamber. We find that in very humid environments, we can avoid ice build up on the walls if we use a slow flush.

Comment R2.3

For the postprocessing software, has a comparison been done on the same dataset to see the impact of how the size threshold is obtained. This would be an important thing to check for harmonizing the dataset.

We performed a comparison of the two methods using the ExINP-ENA dataset as an example. The comparison, shown below, shows a strong correlation with an r-squared value of 0.97. The largest variations are seen at the lowest concentrations, which is consistent with differences in how the threshold diameter is calculated, which has a relatively larger impact on concentrations that are close to this threshold. Additionally, an evaluation of the ice-detection threshold sensitivity in PIA was performed by Büttner et al. (2025), and we now include this reference.



We have revised the text to include this r-squared value:

“...in the PIA software, the threshold is determined for each PINE expansion (see Büttner et al. (2025) for an evaluation of this method), and in the WTAMU software, a single threshold is determined over an ‘operation period’ which may include anywhere between one run and more than 100 runs (Wilbourn et al., 2024, Sect. S8). Both software packages include automatic quality control to flag the data, which is used by each campaign team to remove poor quality data. A comparison of the two methods for the ExINP-ENA Level 1 data yields an r-squared value of 0.97, demonstrating good consistency between the methods.”

Comment R2.4

Is the INP concentration only reported at the minimum temperature of the experiment and is it calculated from the number of ice particles detected at said minimum temperature during an expansion? Or is the number of ice particles detected during the expansion used to measure the cumulative INP concentration until the minimum temperature? If it is the latter, how much of the volume sampled through the OPC is truly coming from the minimum temperature during the expansion? Is there a correction accounting for this when the minimum limit of detection is calculated. More specifically, if during an expansion only 0.5 L of air are measured through the OPC while the chamber is at its minimum temperature, then the limit of detection at T_{min} is not based on the entire 2 L sampled during the expansion but on 0.5 L. This makes calculating the true limit of detection at T_{min} extremely challenging, especially when the ability of an aerosol to be an INP is so strongly dependent on temperature. This should be clarified and discussed in the manuscript (see specific location below).

In answer to the first part of this comment, the INP concentration reported is the cumulative INP concentration integrated between the start and end of the expansion. We have expanded Sect. 2.1 to explain this in more detail:

“The cumulative number of ice crystals (n_{ice}) measured by the OPC in the course of the expansion is used to calculate the cumulative concentration of INPs (N_{INP}) per litre of sampled air by dividing n_{ice} by the total volume of air passing through the OPC detection volume. For the welas sensor (on PINE-01-A), this volume is 10% of the total volume passing the OPC, whilst for the fidas-pine sensor (on all other PINEs), this volume is equal to the total volume passing the OPC. For additional details see Möhler et al. (2021) and Büttner et al. (2025); typical output for a PINE expansion can be seen in Möhler et al. (2021, Fig. 3).”

In response to the reviewer’s points on the limit of detection, this value is not a function of temperature, but is due to the instrument configuration and ambient conditions resulting in the implicit limitation of a single ice crystal being measured over the course of the expansion. Therefore, although N_{INP} is temperature dependent, the minimum measurable N_{INP} (which we refer to as $\underline{\lim}N_{INP}$) is not. $\underline{\lim}N_{INP}$ is

dependent on the volume sampled and since this was almost constant throughout each campaign, so was $\lim N_{\text{INP}}$.

As this value cannot be measured during every expansion we use a method (described in Sect. 3) to determine this throughout the campaign, which essentially picks out the expansions that measure the lowest N_{INP} . We are confident that this does not result in considerable bias, as the lowest N_{INP} values in Figure 2a are almost constant throughout the campaign, which we would not expect to be the case if there was bias in $\lim N_{\text{INP}}$. We have rewritten Sect. 3 to improve our explanation of $\lim N_{\text{INP}}$:

“...We provide time series of the mean measured INP concentration from the intervals ($N_{\text{INP}}(T, t)$, where T and t denote the temperature and time interval) and the associated mean minimum temperature measured during the subset of PINE expansions ($T_{\text{min}}(T, t)$). We averaged the data for two reasons. The lowest measurable nice in PINE is implicitly one ice crystal ($n_{\text{ice}} = 1$) per sampled volume; this corresponds to a lower limit of the measurable INP concentration ($\lim N_{\text{INP}}$) that is a function of the instrument configuration and ambient conditions. In order to improve counting statistics and reduce $\lim N_{\text{INP}}$, we use data collected from multiple expansions to increase the total volume of air that is considered and the statistical likelihood of measuring at least one ice crystal in this volume. This likelihood is inversely scaled with the number of expansions (and therefore total sample volume) that are included in the interval, as demonstrated in Fig. 2.”

Comment R2.5

Following up on the previous comment, is the probability of an INP being removed before the minimum expansion temperature accounted for i.e. 2 L of the original volume has been removed before the end of the expansion in the ExINP-GVB example (lines 248-250)?

We assume that there is no loss of ice crystals (or INP) in the course of the expansion, hence we can determine the cumulative ice concentration. A good test of this is demonstrated in Robinson et al. (2026) where the authors performed successive PINE expansions with ‘Snomax’ extract aerosol at different starting temperatures yet saw consistent results.

Comment R2.6

The calculation, description and purpose of CV could be made clearer. It sounds like $N_{\text{INP,ss}}$ is based on the volume of air sampled during the 6 hourly period, so the minimum detectable INP concentration should be a function of the total air sampled during all the expansions at the given temperature in that period (it should be made clearer that CV should be calculated at each temperature interval). Therefore, should there be a metric like CV to calculate the minimum sampled volume of air needed to make a measurement significant? Using CV (essentially a metric for the variability over the 6 hourly period) seems a bit counter intuitive when the motivation of the dataset is to capture the variability in INP concentrations at the sub-diurnal/plume scale. That said, it seems like 0 ice crystals would lead to an undefined CV and therefore, would be removed from the dataset regardless of the

thresholding. Is that intended? Either way, CV should be explained better, especially as it is left up to the user to choose a threshold when using the dataset. In addition to showing the amount of data removed for each campaign (Figure 3), it would be helpful to see how the CV thresholding impacts the data presented in Figure 2b, so it becomes clear which points would be lost and when.

There is a misunderstanding here, which has in part come about through an error on our part. CV_{ss} is a statistical measure of how robust the data point is, given the total number of events (ice crystals in our case) that have been measured. It is not, as the referee says, a metric for the variability over 6 hrs. We erroneously referred to CV as the coefficient of variability rather than the correct term coefficient of variation. To avoid further confusion, we have renamed this as the relative standard deviation (RSD), which is an alternative term often used in place of 'coefficient of variation'. We also express this as a percentage in the revised manuscript.

The reviewer is correct in thinking that applying any threshold would remove intervals where $N_{INP}(T,t) = 0$. We decided to keep these in the dataset for consistency with the Level 1 datasets.

Given that the thresholding application is demonstrated in Figure 3, we have decided to leave Figure 2b as it is.

The revised text in Section 3 is below.

“Some portions of the data sets suffer from poor counting statistics; hence, for each collection of subset data, we determine an associated error based on counting statistics, which we calculate as the relative standard deviation (RSD) of the subset data using the equation $RSD(T,t) = \sigma_{N_{ice}(T,t)} / \mu_{N_{ice}(T,t)}$, where σ and μ are the standard deviation and mean of $n_{ice}(T,t)$, the cumulative total number of ice crystals measured in the interval. $RSD(T,t)$ is a statistical measure of how robust the data point is, given the total number of events (ice crystals in our case) that have been measured. The ice crystal detections will follow a Poisson distribution, therefore the equation can be rewritten as $RSD(T,t) = 1/\sqrt{n_{ice}(T,t)}$ and expressed as a percentage. $n_{ice}(T,t)$ is calculated by summing up n_{ice} from each expansion within the interval, which can be calculated with the assumption that $\lim N_{INP}$ from the Level 1 data is the concentration that correspond to $n_{ice}=1$ being measured in a single PINE expansion; dividing N_{INP} by this value results in n_{ice} . ”

Comment R2.7

As the temperature space for the different campaigns and even with an individual campaign varied, can you provide a recommendation on how to handle working with the 6 hourly 2 K binned data when there are different numbers of observations for each of the 2 K bins.

We recommend that each observation is treated independently, with model (or reanalysis data etc) data collocated in time, latitude, longitude, and altitude. When

calculating the INP concentration, we would recommend using the mean temperature of the interval.

This recommendation has been added to the end of Sect. 4.1:

“For application of the GloPINE dataset with simulated data or reanalysis products, we recommend that the user collocates their data with the latitude, longitude, time, and altitude of each individual GloPINE data point. When calculating INP concentrations, we recommend applying the interval-mean temperature and, for consistency with the measurement technique, only using the portion of the aerosol size distribution below $D = 5 \mu\text{m}$ (see Sect. 2.1).”

Comment R2.8

With the previous comment in mind, including the sampled volume of air in each interval (both time and T) would fix this and therefore, should be provided in the dataset to allow for volume-weighted averages rather than just "observation-number" means.

We now include total sampled volume in each interval as a variable in the GloPINE dataset. Please see our response to Comment R1.1 for full details.

Comment R2.9

Line 24-27: Not sure if including details on the ranges in aerosol ERF are really needed here.

The uncertainty (ranges) in aerosol ERF is a primary source of motivation for the community. We have therefore decided to leave this sentence as is.

Comment R2.10

When describing the different campaigns, consider omitting “but no accompanying paper is available” as it is quite repetitive and is not really needed

We chose to do this to make the reader fully aware that there is no accompanying study yet (though some of these are currently being written up). Although this is repetitive, we feel it is appropriate and does not negatively impact the manuscript.

Comment R2.11

Line 78-79: What is the importance of the flow “by-passing” the instrument during the expansion? Consider omitting

The by-passing flow maintains the flow of air being sourced from the sampling inlet, which is important if multiple instruments are drawing air from the same inlet.

We have rewritten the sentence for clarity, and have removed the reference to the by-pass flow as this is not required in order to understand the principles of the expansion chamber:

“In the expansion mode, the inlet valve to the chamber is closed, whilst the outlet flow of air exiting the chamber is pumped at a rate of 3 to 4 L min⁻¹, causing the air within the chamber to expand.”

Comment R2.12

Line 85: Does this mean that no aerosol are seen by the OPC? This could be rephrased to hydrometeor size distribution if it is excluding interstitial aerosol.

This is correct. As suggested by the reviewer, we have rewritten this sentence to read:

“The hydrometeor size distribution (both liquid and ice) is measured with an optical particle counter (OPC) positioned downstream of the chamber in the pump line establishing the expansion flow; interstitial aerosols are too small to be counted as ice crystals.”

Comment R2.13

Line 92-94: It should be made clearer if each expansion yields one INP concentration at a prescribed temperature or if the INP concentration is reported at multiple temperatures during an expansion.

We have included additional detail on the process of determining the INP concentration in Sect. 2.1 and Sect. 2.3. Please see the response to Comment R1.4.

Comment R2.14

Line 100-105: I understand that these larger particles may be lost but if they do make it, are they misclassified as ice crystals due to their large size? After all, one large aerosol in an expansion would already result in an INP concentration of 0.5 L⁻¹.

The threshold size that is used to separate the water droplets from the ice crystals is typically greater than 10 µm (see Mohler et al. 2021 Fig. 3c). Measurements by Wilbourn et al. (2024) show that the transmission efficiency of particles of this size is zero, hence it is extremely unlikely that aerosol particles are counted as ice crystal.

In response to Comment R2.12 we have revised the manuscript to state that “interstitial aerosols are too small to be counted as ice crystals.”

Comment R2.15

Line 230: Here it is still not clear how these measurements are made. By using the minimum temperature and pressure does this mean that all ice crystal counts down to this temperature

are considered as the cumulative number of INPs at the minimum temperature? If yes, and in line with the general comment, what volume is used for calculating a concentration? The total volume of air sampled by the OPC is not representative of the INP at the minimum temperature as only a very small fraction of the air sample is observed at that temperature. Is this somehow accounted for? This seems different than other immersion freezing techniques where a fixed volume of air is sampled and then the cumulative INP concentration is measured at a range of temperatures.

This comment is similar to previous comments from this reviewer, and reviewer 1.

Briefly, we have revised the manuscript to explain the method by which the ice crystal number concentration and INP concentration are measured and provided a reference to a study that provides detail on the post-processing method. We now include the mean volume of air sampled in each expansion during each campaign and include a total volume sampled per interval in the GloPINE dataset.

For full details, please see our response to Comment R1.4.

Comment R2.16

Line 254-257: This section could be streamlined as it is a bit bulky for motivating the “regular intervals” used in this dataset.

We feel it is important to highlight these various applications to the reader to appeal to as many scientists as possible. We have therefore decided to leave this sentence as is.

Comment R2.17

Line 260: Consider making it clearer that these expansions and resulting subset data are different than previous immersion freezing data sets in that each temperature is considered independently here. So if the subset data set covers e.g. 16 different temperatures, then the actual number of 6 hourly samples is closer to 2250 “6-hourly spectra” that cover the entire temperature range e.g. compared to drop freezing techniques. Either way, it would be worthwhile mentioning how much volume of air was sampled here as well to put the measurements into context.

The line in question reads: “The subsetting method applied to the Level 1 data from the 20 campaigns takes N_{INP} measurements from 423,678 expansions (367,487 at or above $\text{lim}N_{\text{INP}}$) and results in 35,997 non-zero $N_{\text{INP,ss}}$ measurements in the dataset.”

With regards to estimating an equivalent number of spectra, as the temperature cycling method was unique to each campaign, and often changed throughout the campaign (see Figure 2a for a good example of this), it is not possible to provide an estimated number to report.

To answer the reviewer’s second comment: we do not agree that comparing sampling volumes between techniques is a useful metric for establishing whether one

dataset is more or less robust than another. However, we now include campaign-mean values of the volume sampled per expansion to Sect. 2.2 (Campaigns) and we also include the variable V_{tot} in the GloPINE dataset, which is the total volume of air sampled during each interval. Please see the response to Comment R1.1 for more details.

Comment R2.18

Line 265-266: It is not clear why $\lim N_{\text{INP}}$ is used here? Is this just meant to say above this limit? Please clarify what is meant here.

We can use $\lim N_{\text{INP}}$ to calculate the total number of ice crystals measured during each expansion as we assume that this INP concentration corresponds to a single ice crystal being measured during the sampling period (the expansion). This is dependent on the instrument configuration and ambient conditions, rather than the temperature of the expansion, and is therefore determined as a function of time during the campaign. By assuming $\lim N_{\text{INP}} = 1 n_{\text{ice}}$ we can divide N_{INP} measured during each expansion by $\lim N_{\text{INP}}$ to obtain the equivalent n_{ice} from the expansion.

Comment R2.19

Line 266-268: It is also not clear why a moving average is needed for $\lim N_{\text{INP}}$? Is this not just dependent on the volume of air sampled in the single expansion and then summed over the 6 hourly period?

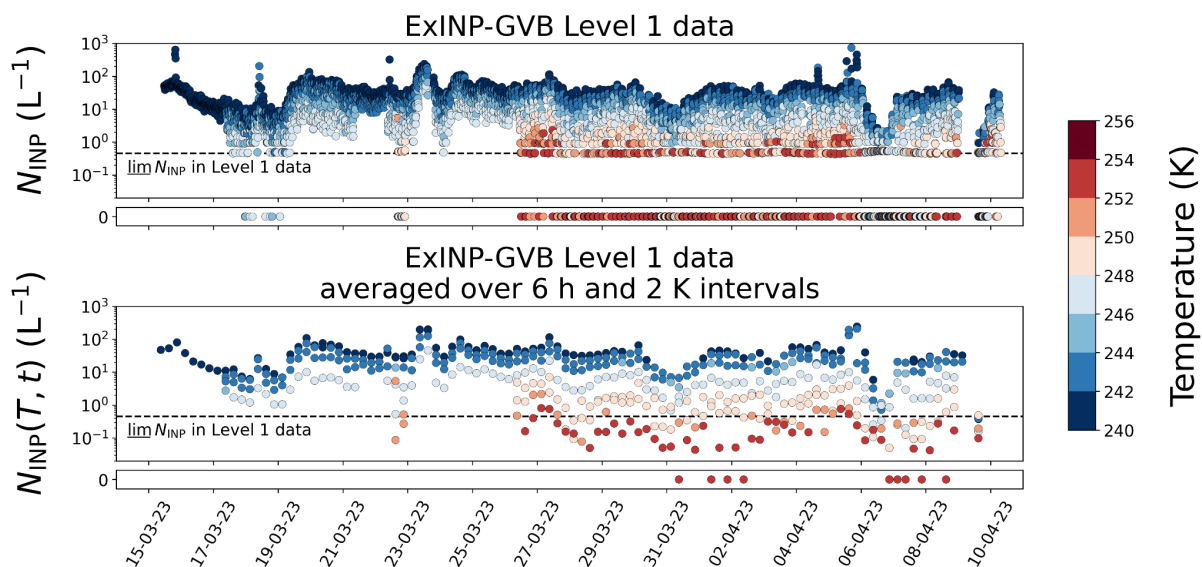
$\lim N_{\text{INP}}$ is a function of the instrument setup and ambient conditions, rather than a function of expansion temperature etc. Therefore, the value of $\lim N_{\text{INP}}$ will remain constant during the campaign, provided the expansion flow rate and target pressure are consistent. We calculate a timeseries of $\lim N_{\text{INP}}$ for the campaign duration that can be used and applied to each individual expansion. We use the moving window to allow for small variations that are related to the ambient conditions. We have amended this paragraph in the revised manuscript. See the response to Comment R2.6 for the revised paragraph.

Comment R2.20

Figure 2b: It is great that the error bars are included here, but an error in INP concentration of an order of magnitude due to the OPC seems quite high?

Good spot - the error bars which should signify a +/- 10% error were incorrectly applied. The figure has been updated and is shown below. As the INP concentration varies on a log scale, the 10% error bars are now too small to see. We note this in the revised caption text:

“Error bars due to the uncertainty in the OPC (see Sect. 2.1) are included on all data points but are too small to distinguish from the symbol.”



Revised Figure 2.

Comment R2.21

Line 283-285: The way this is formulated is a bit misleading as it implies that the dataset includes orders of magnitude more INP information than previous studies. I'm not saying that this isn't an amazing resource and dataset but an implied comparison like this should be based on the volume of air sampled between the measurements techniques conducted and not just based on the number of observations. This is especially the case when each NINPss (or NINP) is for a given temperature while immersion freezing data from a drop freezing technique covers the INP concentration over $\sim 20^\circ\text{C}$. If this dataset were to be divided up to produce an INP spectra, then it would also be on the order of 1000s of spectra.

We do not agree that the volume of air sampled is a necessary metric for making comparisons. Different techniques will require different volumes of air to be sampled. PINE is able to make measurements using a relatively smaller volume of air than other techniques, but this does not mean it is less or more useful than other datasets. Following the response to Comment R1.1, we have included the volume sampled as an additional parameter. But, sampled volume alone does not provide a measure of how representative a dataset is of the atmosphere as a whole.

The '1000s of immersion-mode INP measurements' we refer to is the number of each individual INP measurement that was used to evaluate the models in the referenced studies - rather than complete spectra. As we explain in the response to Comment R2.16, we cannot provide an estimate of the number of spectra as consistent and procedural temperature cycling was not performed during these campaigns. Even if we could estimate the number of spectra, we would also have to do the same for the "1000s of immersion-mode INP measurements..." which would result in a similarly large difference in terms of absolute numbers of reported measurements. We believe our sentence in its current form is accurate and not misleading, and therefore do not make any revisions.

Comment R2.22

Figure 4: The nonzero Nss values in the corners of the subpanels are not immediately clear. Are these numbers the number of 6 hourly ss

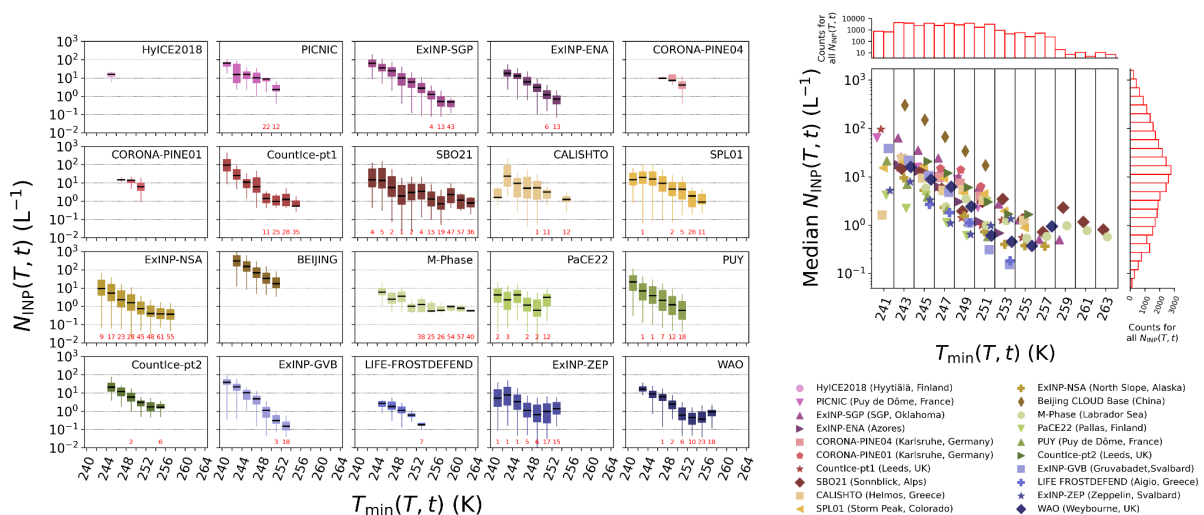
We explain what these figures signify in the Figure caption, and have revised the text for clarity:

“...The integers in red shown below each box plot are the percentage of GloPINE data where $N_{\text{INP}}(T, t) < \lim N_{\text{INP}}(T, t)$.”

Comment R2.23

Figure 5: It would also be nice to show the number of subset observations in each T interval.

We have included this in Figure 5 as suggested. This includes the distribution of temperatures and the distribution of $N_{\text{INP}}(T, t)$ concentrations. The revised figure is shown below.



Revised Figure 5.

Comment R2.24

Line 308-310: So if a single expansion during an interval led to no ice, the entire interval is set to 0? This seems inconsistent with the information shown in Figure 2, consider reformulating.

The sentence in question is

“Values of zero in the variables INP_6h2K and CV_6h2K represent intervals when at least one PINE expansion was performed but no ice crystals were measured.”

If an interval contains at least one expansion and results in a total of zero counts across the interval, then we keep this in the dataset as a zero concentration. If a single expansion contains a zero count in the interval whilst others have non-zero counts this zero is included in the mean.

The sentence has been rewritten as:

“Values of zero in the variables INP_6h2K and RSD_6h2K represent intervals when at least one PINE expansion was performed but the sum of measured ice crystals was zero; we keep these in for consistency with the Level 1 datasets.”

Comment R2.25

Using the *ss* subscript could be a bit confusing for the community who are used to having *ss* represent supersaturation. Consider using a different subscript.

We have removed the subscript and replaced it with the functional form $N_{\text{INP}}(T,t)$ and $T_{\text{min}}(T,t)$ to denote the interval.

Comment R2.26

Line 227: consider using hydrometeor size distribution (see comment above)

This has been done. See our response to Comment R2.12.

Comment R2.27

Line 265: ‘the’ is repeated twice before Level 1

Good spot, thanks!

New references included in the revised manuscript:

Büttner, N., Fösig, R., Böhmländer, A., Lacher, L., Vogel, F., Tarn, M., Bogert, P., Nadolny, J., Murray, B., and Möhler, O.: Automated Analysis and Quality Assurance of Ice-Nucleating Particle Data: The PINE INP Analysis Software PIA, EGU sphere [preprint], <https://doi.org/10.5194/egusphere-2025-5586>, in review, 2025.

Creamean, J. M., Hume, C. C., Vazquez, M., and Theisen, A.: Long-term measurements of ice nucleating particles at Atmospheric Radiation Measurement (ARM) sites worldwide, *Earth System Science Data*, 17, 6943–6963, <https://doi.org/10.5194/essd-17-6943-2025>, 2025.

Lacher, L., Hallar, A. G., McCubbin, I. B., Bail, J., Froyd, K. D., Jacquot, J., Shen, X., Rapp, C., Möhler, O., and Cziczo, D.: Strong springtime increase of ice-nucleating

particle concentration in the Rocky Mountains, *Atmospheric Chemistry and Physics*, 26, 6703–6726, <https://doi.org/10.5194/acp-26-6703-2026>, 2026

Robinson, J., Daily, M. I., Foster, P. B., Macklin, J. P., McQuaid, J. B., Tarn, M. D., and Murray, B. J.: Challenges in measuring sticky biogenic ice-nucleating macromolecules, *Aerosol Research Discuss.* [preprint], <https://doi.org/10.5194/ar-2026-17>, in review, 2026.