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Question or comment by the reviewer

Our answer to a reviewer's question

Reviewer Comment

October 7, 2025

The manuscript provides a good use case of connecting ice-nucleating particle (INP) measurements with synoptic conditions. The study is scientifically relevant and it will be interesting to see future more extensive measurements. The structure is clear and the used methods are explained well. The data is available online and in parts also published on Zenodo. The manuscript needs some minor clarifications, but after addressing those, it will be a good and important addition to the journal.

1 General comments

1. Lines 109-110: Could you clarify what you mean with “the trajectories might not have completely passed Melpitz”? Trajectories are always associated with some uncertainties and also of course depend on the input data.

Thanks to reviewer #1 for hinting at this formulation. The formulation is inaccurate and confusing. What we wanted to say is that the uncertainties of the trajectories are large and we cannot be sure if indeed Melpitz was passed before HPB and Eri. In the updated version of the manuscript, we now leave out the second part of the sentence: “even though the trajectories might not have completely passed Melpitz before Hohenpeißenberg and Eriswil”. We believe that this was the confusing part.

2. Lines 127-128: This is quite a large pore size with a high flow rate as well. Checking the literature (e.g., Cyrs et al. 2010), Nuclepore filters are to my knowledge typically used at lower flows to also make sure that particles are collected within the pores due to a longer residence time. Can you elaborate on this, how you make sure that 1) the Nuclepore filter does collect a representative aerosol sample from ambient air, and 2) what the pressure drop across the filter is? These Nuclepore filters are quite elastic and might be impacted negatively by a larger pressure difference across itself.

Regarding 1) We participated in an intercomparison campaign called “Puy de Dôme ICE Nucleation Intercomparison Campaign PICNIC” (Lacher et al., 2024) with the same instrumentation that was used for this manuscript. During PICNIC also filter sampling with differently sized pores and at different flow rates was compared and no significant influence of these parameters was found: “This shows that filters with a pore size of 800 nm and applied flow rate still have a sufficiently high collection efficiency for the majority of atmospheric INPs present during the PICNIC study. This is in agreement with Soo et al. (2016), who examined the collection efficiencies of a range of different filter materials and pore sizes for test particles with rather small sizes between 10 and 412 nm. They reported that the collection efficiency for polycarbonate filters with 800 nm pore sizes and the flow rates used here ($>11 \text{ L min}^{-1}$) are above 97 % for all particles in the examined size range (10–412 nm).” (Lacher et al., 2024)

Regarding 2) The pressure was about 200 - 220 hPa.

3. Line 152: Could you elaborate on the temperature measurement? It sounds like you use the temperature of the cryostat for your measurements. Did you ever check if there are inhomogeneities in the ethanol bath, which might lead to a temperature inhomogeneity within the PCR plates? This could also be answered by citing a relevant publication, if it exists.

We do not have a dedicated publication about our DFA, but internally we of course performed numerous tests and established several calibration routines to ensure good data quality. We repeat a temperature calibration of our instruments yearly, and did not observe large changes over the past years. Temperatures used are then those resulting from the calibration.

Temperature homogeneity across a PCR plate is tested as part of that annual calibration. Inhomogeneities exist and the annual calibration relevant for the presented data showed a standard deviation of 0.3 K for the horizontal temperature distribution. But it should be noted that the inhomogeneity is not systematic, i.e., there is no persistent cold/warm spot in the same location across all measurements. This means that if the same sample is measured again, the freezing temperature of an individual well might change, but if looked at the full measured spectrum, the curves are essentially identical.

4. Lines 163-170: Did you check the validity of the backward trajectories? I.e. calculation of the integration error using forward trajectories or investigating the change by moving the receptor some km in latitude and longitude direction? The trajectories are of course only an indication, but this indication could be increased in significance with additional quality checks.

Yes, we calculated forward and backward trajectories and found a consistent situation of the trajectories arriving from northeasterly directions. The weather situation was also quite stable over many days and some of the calculated trajectories are also shown in the supplementary material. Motivated by the question raised by reviewer #2, we also checked arbitrary trajectories in a few km lat/lon distance of HPB and Eri and found similar results for the trajectories. So, we are confident that we have a robust result here.

5. Lines 251-255: I agree that heat-labile INPs can be seen as a proxy for biological INPs, but heat-labile INPs are not necessarily biological INPs and vice versa. Are you able to provide additional information to show that those are biological INPs, especially since you already measure them in early January, where the biological activity should be at a minima in Germany. I think you should either formulate it a little bit less strict, i.e. heat-labile is a proxy for biological, or you need to provide additional indications for the nature of the INPs.

We agree that heat-labile INPs and biological INPs are not synonymous, and that heat-labile INPs are merely a proxy for biological INP, which was stated in L157-L158 (line of the original manuscript), when the heat treatment was first mentioned. Since it was not our intention to evoke a wrong impression, we adjusted our phrasing when the heat-labile INP were mentioned in the following locations (Line numbers refer to the revised manuscript):
L246: "Heat treatment causes a decrease in INP concentrations, indicating a fraction of heat-labile, probably biological INPs, [...]"

L257 : "[...] the fraction of heat-labile INPs is negligible at Eriswil"

L274: "Concerning the heat-labile, and therefore potentially biological INPs, [...]"

L281 – L282: "The presence of these potentially biological INPs is noteworthy [...]"

L319: "[...] showed similar INP spectra and generally a low biological INP fraction (indicated by the low fraction of heat-labile INP)."

L322: "[...] showed a larger fraction of heat-labile, potentially biological INPs."

The release of bioaerosol (including biological INP), is of course strongly linked with the biological activity and hence with the seasons in mid-latitudes, nevertheless can bioaerosol also be observed in winter. For example, while not as numerous as during summer, fungal spores can be also found during wintertime (Havis et al., 2023; Lagomarsino Oneto et al. 2020; Oliveira et al., 2009; Pady, S. M., 1957). Also the transport of microbes via aerosolized soil dust (Salawu-Rotimi et al., 2021) is a viable pathway during winter.

6. Lines 281-285: You discuss here well, that you see a larger ice crystal number concentration compared to the concentration of INPs that can be activated at the given temperatures. Do you consider any secondary ice production processes, which could also be an explanation for what you observe?

It is true that we only discussed the observed difference as an indication that the gap between INP and ICNC originates from free tropospheric INP intrusion. Therefore, we reformulated the whole paragraph (line 312-320): “Specifically, mean ICNC were between 10^{-3} and 10^{-2} L^{-1} from 11 through 12 January 2024, when cloud-minimum temperatures were about $-7^{\circ}C$. It is, however, remarkable that the INP observations of around 10^{-3} L^{-1} at HPB, when this site was above the PBL (Fig. \ref{Fig:INP_contrast}e,f), were on a similar order as the ICNC concentrations observed during the same period at Eri. This supports the hypothesis that INPs are entrained from the free troposphere via turbulence and afterwards immediately removed as they interact with the Bise cloud layer, leading to reduced availability of INPs downwind. However, it must be noted that an ICNC concentration which is higher than the observed INP concentration can in principle also be a result of secondary ice formation processes (Korolev et al., 2020). Nevertheless, secondary ice formation processes generally lead to orders of magnitudes of increase in ICNC, which was, besides occasional peaks in the ICNC, not observed in the average ICNC values during the investigated time periods.”

7. Lines 289-291: See my comment above regarding the equivalence of biological and heat-labile INPs. Also try to be consistent with the use of biogenic and biological.

We replaced “biogenic” in L327 with “biological”

8. Line 302: Could you elaborate on what you consider a significant difference? Or is it just a visual check?

In this instance it is simply based on the overlap of the confidence intervals in the relevant high temperature regime ($>-15^{\circ}C$).

2 Specific comments

1. Line 34: “Mineral dust becomes effective as an INP at temperatures...” since mineral dust is used as a singular here.

Reviewer #1 is right. Rewriting the sentence is reasonable. We changed the sentence in the manuscript accordingly.

2. Line 40: "Given there is such an INP available and is immersed in a droplet at a temperature where the INP is active, the droplet freezes immediately." The sentence reads not nicely to me. Maybe it could be reformulated in a bit simpler terms, i.e. "Upon the availability of an aerosol particle, which is immersed inside a supercooled droplet, at temperatures where the aerosol can act as an INP, the droplet freezes immediately." To me it makes most sense to discuss an aerosol particle, which can act as an INP below its activation temperature.

We thank reviewer #1 for the suggestion of a more reasonable sentence structure. We changed the sentence in the manuscript (lines 42ff).

3. Lines 104-105: "The distance between the two stations is 250 km." Saying that a distance is between two points sounds wrong.

We changed the formulation in the manuscript.

4. Line 105: Planetary boundary layer has already been defined as PBL above. Insight should also be insights, since you are not just getting a single insight from your measurements.

This is correct. We use the introduced abbreviation PBL now.

5. Line 106: wihtin PBL -> within PBL.

It is corrected now.

6. Lines 108-109: ...which is located approximately 440 km northeast of Hohenpeißenberg.

We changed the sentence in the manuscript accordingly.

7. Line 122: Can you provide the conditions you refer to as standard? This can be widely different for different fields and groups and would provide needed information for comparison.

0°C and 1013 hPa, which was added to the respective line (line 131 of the revised manuscript).

8. Line 145: Are there plans to make the Python code available online or publishing it on i.e. Zenodo?

No. As this is quite specific to our set-up, we do not see the added value.

9. Line 150: You discuss that you filled tubes with your sample, but then discuss wells of PCR plates. I assume you mean the same, therefore I would suggest to use wells throughout.

Yes, we mean the same and now use “well” throughout the manuscript.

10. Lines 157-158: The sentence could be structured differently to make it a bit more clear that the heating removes the ability of heat-labile INPs to induce ice nucleation, i.e. “Afterwards, the PCR plates were heated to 90 °C for 30 min to remove the ability of heat-labile INPs, which are a proxy for the fraction of biological INPs, to induce ice nucleation.”

We included the suggestion of reviewer #1 to reformulate the sentence (lines 167ff).

11. Line 183-184: Above ground could be abbreviated via a.g.l., same as above ground level.

We changed “above ground” to “a.g.l.”

12. Line 201: Above ground could be abbreviated via a.g.l.

We changed “above ground” to “a.g.l.”.

13. Line 226: Heating causes -> Heat treatment causes.

We changed “Heating causes” to “Heat treatment causes”.

14. Line 258: Above ground level could be abbreviated via a.g.l.

We changed “above ground” to “a.g.l.”.

15. Line 266: are around -> is around.

We changed “are around” to “is around”.

16. Line 272: You already abbreviate ICNC in the previous paragraph, therefore I would suggest that you do not need to do it here again.

We removed the redefinition and only write ICNC now in the manuscript.

17. Line 306: colder temperatures -> lower temperatures.

We changed “colder temperatures” to “lower temperatures”.

2.1 Comments on tables and figures

1. Table 1: There should be spaces around the equal sign in the third column.

We added spaces around the equal sign in the third column.

2. Figure 3: Using the rainbow colormap has been the standard for remote sensing data, but I would recommend to use a colormap that first of all can be understood by readers with colour vision deficiencies. This is not given with the currently used colormap. In addition, the rainbow colormap has numerous other issues, for example the linearity, which is not given for example the lightness of the colormap (e.g., Kovesi 2015). This is especially pronounced when looking at the yellow or cyan color of the colormap. These are quite sharply separated from the other colors and therefore the data itself looks different due to the used colormap. I would recommend a perceptually uniform colormap, such as viridis or cividis.

We thank reviewer #1 for the comment. Indeed, the rainbow colormap is the standard for remote-sensing data. We acknowledge that readers with colour vision deficiencies would have difficulties with the interpretation of the figure and also the nonlinearity is an issue with the colormap. We changed the colormap of the figure towards viridis. It is updated in the manuscript.

3. Figure 5: This is a nice graphic and really helps the reader to understand the different scenarios. I do have some questions regarding the uncertainties. Could you elaborate on the calculation of the uncertainties for the different instrumentation? It seems like the relative uncertainty lowers for higher INP concentration, which I guess is related to using Poisson statistics? The second part is just a bit of curiosity. When looking at panel (h), the heated samples from Hohenpeißenberg shows a very similar slope compared to the other locations at around -12 °C, but then the curve almost falls flat with only around 10 freezing events. Do you have any idea, why that could be or what that could indicate?

Yes, this is the statistical uncertainty stemming from the Poisson distribution, calculated according to Agresti and Coull (1998). This method has become the de facto standard for statistical uncertainty in droplet freezing array measurements. Therefore, we did not include details in the manuscript but added a reference to the method in the caption of Figure 5: "Error bars represent the 95% confidence interval of the statistical uncertainty calculated after Agresti and Coull (1998)."

In general, such plateau-like features (here, approximately between -15°C and -26°C) indicate that no additional INPs can initiate freezing at that temperature. These features can occur when the sample consists of sufficiently distinct INP populations of a certain concentration or as an artifact of the dilution. In this case it is most likely an dilution artifact. It is difficult to answer with certainty why the heated Hohenpeißenberg sample shows this plateau but the samples from the other two locations do not.

4. Figure 6: Same comments apply for the used colormap as given for Figure 3.

Same as two comments before, we changed towards viridis colormap.

3 Technical comments

We thank reviewer #2 for carefully reading through the manuscript and for giving technical comments. We incorporated all of the following 33 comments.

1. Line 20: space is missing between number and unit.
2. Lines 23-24: space after author last name should be removed.
3. Line 24: space is missing between number and unit.
4. Line 31: space is missing between number and unit.
5. Lines 34-36: space is missing between number and unit.
6. Line 67: space is missing between number and unit.
7. Line 69: space is missing between number and unit.
8. Line 82: latin phrases should not be hyphenated.
9. Lines 79-81: coordinates need a degree sign and a space when naming the direction (e.g. 30° N, 25° E).
10. Line 92: space is missing between number and unit.
11. Line 94: space is missing between number and unit.
12. Lines 102-103: coordinates need a degree sign and a space when naming the direction (e.g. 30° N, 25° E).
13. Line 108: coordinates need a degree sign and a space when naming the direction (e.g. 30° N, 25° E).
14. Line 113: latin phrases should not be hyphenated.
15. Line 121: units should be written upright and not italic.
16. Line 122: units should be written utilizing exponents.
17. Line 124: space is missing between number and unit.
18. Line 127: units should be written upright and not italic.
19. Line 128: units should be written utilizing exponents.
20. Line 133: space after CDC should be removed.
21. Line 141: units should be written upright and not italic.
22. Line 143: units should be written utilizing exponents.
23. Line 149: units should be written upright and not italic.

24. Line 153: units should be written utilizing exponents.
25. Line 157: space is missing between number and unit.
26. Line 181: earlier you used a single unit when showing a range (i.e. line 42), this should be consistent.
27. Line 194: earlier you used a single unit when showing a range (i.e. line 42), this should be consistent.
28. Line 245: North-Germany is not capitalized and should probably be “northern Germany”.
29. Line 259: earlier you used a single unit when showing a range (i.e. line 42), this should be consistent. In addition, there is a space missing between number and unit.
30. Line 268: space is missing between number and unit.
31. Lines 278-279: space is missing between number and unit.
32. Lines 280-281: latin phrases should not be hyphenated.
33. Line 334: Cloudlab is capitalized before (see line 75), this should be consistent throughout the manuscript.

References

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- Havis, N.D., Kaczmarek, J., Jedryczka, M. et al. Spore dispersal patterns of the ascomycete fungus *Ramularia collo-cygni* and their influence on disease epidemics. *Aerobiologia* 39, 213–226 (2023). <https://doi.org/10.1007/s10453-023-09787-6>
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Oliveira, M., Ribeiro, H., Delgado, J.L. et al. The effects of meteorological factors on airborne fungal spore concentration in two areas differing in urbanisation level. *Int J Biometeorol* 53, 61–73 (2009). <https://doi.org/10.1007/s00484-008-0191-2>

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Question or comment by the reviewer

Our answer to a reviewer's question

Reviewer Comment 2:

Ohneiser et al. provides a unique study of measuring ice nucleating particles (INPs) over time and distance in a region of the world where stratus clouds can persist with Bise winds and models cannot accurately reproduce them. Therefore, I believe this study is a valuable addition given the novelty of the measurements, despite more cases being valuable to reinforce the hypotheses and initial findings presented here. I think it is suitable for publication, after consideration of my comments. There are a few issues regarding clarity, and I take issue with how some of the statements are worded (e.g. need more qualification or interpretation of the data). Some of the statements can be argued against as currently written.

I like how the authors have laid out the text, with hypotheses early on, and coming back to the initial hypotheses at the end of the text. I especially like the schematics in Figure 1 and 5 to improve understanding. I think it is an important conclusion and nice finding to have evidence of INP removal under the cold Bise conditions. Nice work.

Lines 22-24: Secondary ice processes are mentioned here but are not mentioned anywhere else in the manuscript. See below where I suggest including it later on.

We thank the reviewer for this comment and we included the suggestion later on.

Lines 31-32: “At a temperature of $-20\text{ }^{\circ}\text{C}$ the fraction of aerosol particles able to act as INP is on average one per million.” This statement needs a citation as it is very dependent on the global region of the study as well as vertical location of measurement.

We changed the sentence to the following: “ When comparing typical INP concentrations (as e.g. given in Petters and Wright, 2015) with typical particle concentrations, it can be estimated that roughly only one in a million particles can initiate freezing at a temperature of $-20\text{ }^{\circ}\text{C}$.”

This can, of course, only be a very rough number, given the spatial and temporal variation of INPs, but in the context here it is merely thought to demonstrate how rare INPs really are.

Lines 39-40: You should cite Tobo et al. 2024 here, which found greater warm temperature INPs with snow-free conditions: <https://www.nature.com/articles/s43247-024-01677-0>

Added.

Lines 64-65: “Therefore, INPs active at temperatures between $0\text{ }^{\circ}\text{C}$ and $-10\text{ }^{\circ}\text{C}$, which is a typical Bise cloud minimum temperature, are required to form precipitation.” This statement seems too restrictive with later text that discusses the seeder-feeder effect. My interpretation of your text suggests that these INPs would not necessarily be required, especially in the times of year with fewer bio sources.

We added “in the absence of seeder clouds” to this sentence (line 67).

Line 88: I know it is defined in the abstract but given you define what a warm Bise is in this section, it is best to define cold Bise again.

Okay, we included a redefinition of the cold Bise situation.

Lines 87-89: It would be good to add any information of typical precipitation or snow accumulation amounts in the region, as this is unfamiliar for me, so I have no idea what “typically no significant precipitation” means.

In the manuscript, we added the sentence (lines 93ff): “During Bise situations, low, geometrically thin but optically thick stratus clouds form over the Swiss Plateau where drizzle or light snowfall can be expected.”

Line 116: Good to put a brief sentence discussing these other projects (I know CLOUDLAB makes an appearance in the intro).

We added the following information in the manuscript (lines 120ff): “Both campaigns took place side by side. The CLOUDLAB campaign involved a unique set of ground-based and airborne in-situ cloud and precipitation sensors and remote-sensing instruments. During the

two wintertime campaigns between 2022 and 2024, LACROS enhanced the remote sensing capabilities of the CLOUDLAB campaign with a large number of ground-based equipment, such as a scanning 35-GHz and vertically-pointing 94-GHz cloud radar from TROPOS, Raman polarization lidar, Doppler lidar, ceilometer, micro rain radar, photometer, disdrometer, and microwave radiometer.”

Lines 122-124: How did you determine when you were in a Bise situation? It would be good to include at least briefly in this section.

We added to the manuscript (line 133f): “We checked the weather forecast continuously and when northeasterly wind conditions were forecasted for at least four consecutive days, we started the measurements”.

Line 125 and subsequent uses: Is “blind” a typo? I have only seen field “blank”.

“Field blank” is now used consistently throughout the manuscript.

Section 3.1 General: Why were the pore sizes of the INP samples different? I would recommend including in this section how many filters were collected.

Melpitz is a TROPOS-operated continuous field station, and including its samples was not part of the initial project idea. It was only after discovering during the analysis that the air masses sampled at HPB and ERI also traveled close to Melpitz that these samples were included. However, this is also why Melpitz was not included in the efforts to unify the sampling.

We added “During the campaign 21 and 17 samples were collected at HPB and ERI, respectively.” (L137 in the revised manuscript)

Line 132 and subsequent uses: It is better to use “University of Wyoming” instead of “Uni Wyoming” at least at the first use, and likely the full name in the references “University of Wyoming Atmospheric Science Radiosonde Archive”. I have never heard it called “Uni Wyoming” before. It is commonly abbreviated as UW. Also, the link you put in the reference is a general link, can you include information such as the station number somewhere so readers can more easily reproduce the data?

We changed the citation name to University of Wyoming. In addition, we added the station number 93844 to the citation.

Line 150: “Well” is probably better here than “tube”.

“Well” is used now consistently throughout the manuscript.

Line 151: I assume you don’t mean aluminum foil here since you view it optically, something like a “clear adhesive film” might be clearer than “a foil”.

Changed.

Lines 157-158: Have you compared or seen any effect of “reusing” aliquots for the heat test versus pipetting out fresh aliquots and heating those?

We never did such a comparison. If one would want to do the heat tests with fresh aliquots, more sample volume would be needed, and hence the sample would have to be diluted by about a factor of two. This would then negatively impact the detection limit. This is one of the main reasons we do not do this. The other reason is that by using the exact same aliquots we know that the observed changes are due to the heat treatment and not the natural uncertainty of the underlying Poisson distribution.

Figure 2: The legends in c and e are smaller than the rest and are hard to read.

We increased the size of the legends in c and e in the updated version.

Lines 196-199: How can you know that is supercooled liquid from the reflectivity alone? It sounds like you may have other data to inform this statement, so I would suggest including that here.

We added: “Especially from the radar depolarization ratio (not shown) it can be seen that it is a liquid cloud. In addition, the Holimo measurements (Holographic Imager of Microscopic Objects in the frame of the CLOUDLAB campaign, see Henneberger et al., 2023) regularly confirmed this statement during the campaign.” (lines 211ff)

Line 208-212: All supplemental trajectory figures should be labeled the same way as Figure 4. Why are the heights listed in the figure captions different from those described in Lines 169-170? I think it should be qualified that some of the trajectories (especially the green ones) do not go toward Hohenpeißenberg.

We thank the reviewer #2 for reading the manuscript so carefully. Indeed, the description in lines 160-170 was wrong. We changed it in the manuscript. All trajectories are for arrival heights of 100, 250, and 1000m above ground. We also checked arbitrary trajectories in a few km lat/lon distance of HPB and Eri and found similar results for the trajectories, making it a robust result. Of course individual trajectories themselves did not necessarily move directly from MEL via HPB to Eri, however, it can be assumed that the air mass is the same over all places coming roughly from the north-east. This is what we also added to the manuscript.

Lines 251-253 and Figure 5: I don't agree that the heat fraction is low during the cold Bise, especially in Figure 5b and c, as the log scale can make the spectra look closer than they actually are. I would suggest providing some numbers here of heat fractions at a given temperature between the cases to make a better statement. 5b and c suggest Eriswil has quite a high heat fraction, while I agree the heat fraction is quite low in 5e at the downwind sites. Also, the heat data for 5h Hohenpeißenberg site looks strange below -15, could that plateau be an artifact of dilution? It may be best to cut that one where the untreated sample for the site stops since it is hard to compare.

In this part of the text, we want to describe the clear difference between INP spectra measured during the cold and the warm Bise situation. They are clearly different in that there are elevated concentrations of INPs at temperatures above -10°C , which are not observed for the heated samples any more. As heating obviously destroyed INPs that are ice active at these high temperatures (to concentrations below detection limit), there is no data available to calculate the heat-labile fraction at these temperatures. Having said that, we agree that referring to the heat-labile fraction in this regard might be misleading. We therefore reformulated the sentence as follows (lines 274-281):

“Concerning heat-labile biological INPs, we observe elevated INP concentrations above -10°C in all unheated samples during the warm Bise situation, regardless of sampling location. This manifests itself as bump-like structures in the INP spectra. Similar bumps were observed for all Melpitz INP spectra shown in Fig. 5, and for the INP spectra from HPB while it was in the free troposphere during the cold Bise (Fig. 5 d to f), but not for the cold Bise case, when ERI and HPB were within the PBL (Fig. 5 a to c). In comparison to that, INP spectra from ERI show that feature only in the warm Bise case (Fig. 5 g to i). These elevated concentrations vanish for all heated samples and were hence comprised of proteinaceous, biological INPs. Calculating the heat-labile frozen fraction for these data is hindered by the fact that all INP spectra for heated samples only start below -10°C .”

For the heated HPB sample in panel h, we share the reviewers interpretation that this is an artifact of dilution. We prefer though to show the unaltered data and therefore added the following sentence to the caption of Figure 5:

“Please note, that the plateauing of the INP spectrum of the heated HPB sample is most likely an artifact of the dilution.”

[Line 260: Can you please clarify what you mean by natural cloud seeding here?](#)

We added (line 287): “(meaning that ice crystals generated by a higher cloud fell into the Bise cloud system)”.

[Line 279: You could include a statement about potential secondary ice production here in this paragraph.](#)

It is true that we only discussed the observed difference as an indication that the gap between INP and ICNC originates from free tropospheric INP intrusion. Therefore, we reformulated the whole paragraph (line 312-320): “Specifically, mean ICNC were between 10^{-3} and 10^{-2} L^{-1} from 11 through 12 January 2024, when cloud-minimum temperatures were about -7°C . It is, however, remarkable that the INP observations of around 10^{-3} L^{-1} at HPB, when this site was above the PBL (Fig. [\ref{Fig:INP_contrast}e,f](#)), were on a similar order as the ICNC concentrations observed during the same period at Eri. This supports the hypothesis that INPs are entrained from the free troposphere via turbulence and afterwards immediately removed as they interact with the Bise cloud layer, leading to reduced availability of INPs downwind. However, it must be noted that an ICNC concentration which is higher than the observed INP concentration can in principle also be a result of secondary ice formation processes (Korolev et al., 2020). Nevertheless, secondary ice formation processes generally lead to orders of magnitudes of increase in ICNC, which was, besides occasional peaks in the ICNC, not observed in the average ICNC values during the investigated time periods.”

Line 288: I don't agree with the low biogenic INP fraction except in 5e. Even 5f you could make the argument of a high heat fraction at Eriswil above -15 °C. It depends on what temperature you are looking at, and quantitative information earlier would inform if this statement could be made. At the very least, it should be qualified.

You are right, writing about "low" or "high" numbers is a matter of perspective. We only intended to say that the INP concentrations were low in comparison to our other test case - the warm Bise situation. Therefore, we revised this part of the sentence to: "... and generally a comparably low biological INP fraction when compared to the warm Bise situation." This will make it clearer that we only refer to the comparison with the warm Bise.

Abstract: Can the "no INP contrast was found between Hohenpeißenberg and Eriswil if both were within the PBL" really be made given the higher values at Eriswil above -15 °C in 5b, and higher values Hohenpeißenberg in 5c? I think it needs some more definition and characterization.

You are right in that the INP spectra at Hohenpeißenberg and Eriswil are not completely identical. But they mostly agree within statistical measurement uncertainty. And there is a much clearer contrast when looking at data from Melpitz.

There are no general (e.g. temperature dependent) trends in similarities and differences between all the observed curves. This makes it difficult to describe the situations in more detail in an abstract, which is why we added the word "almost":

"Under these conditions, both stations of Eriswil and Hohenpeißenberg showed almost similar INP spectra ..."

References

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Color legend:

Question or comment by the reviewer

Our answer to a reviewer's question

Review of “Ice-nucleating particle depletion in the wintertime boundary layer in the pre-Alpine region during stratus cloud conditions” by Ohneiser et al.

This review is a combined review from three reviewers (Dongwook Kim, Georgios Dekoutsidis, Jaydeep Singh) as a part of the 2025 EGU Reviewer Training program.

This manuscript investigates the fate of the ice-nucleating particles (INPs) within cloud particles based on the measurements from the two pre-Alpine central-European sites of Eriswil, Switzerland, and Hohenpeißenberg, Germany, during the “Bise winds” periods in the winter of 2024. The study addresses an interesting question: whether supercooled boundary-layer clouds in wintertime central Europe can deplete INPs along the Bise path. The authors noticed a discrepancy between the model and observations, regarding precipitation in the Swiss plateau under the Bise conditions. They found that INPs are depleted during the transport from Hohenpeißenberg to Eriswil during the cold Bise period, which is perhaps due to the removal after the activation within the supercooled stratus cloud, rather than removal within the PBL. While the study is limited to a very specific area (even more so, a very specific situation in said area), the experimental design, with two stations aligned along the wind and an additional upstream site, is a strength. The findings herein can serve as a basis for future studies with a broader focus and applicability. The topics discussed in this paper are in the scope of the journal and of interest to its readers. We recommend publication after addressing our comments below.

General comments

Despite it not being common practice, I find the decision of the authors to include a separate section, where they emphasize their scientific questions and running hypotheses, very useful. It provides an easy, direct comparison later in the discussion and provides an easy look-up when studying the results. The introduction is overall well structured, well written, and the results are presented in a clear, concise way.

We thank the reviewers for the evaluation of this section including the hypothesis. As the hypothesis is quite complex, we also thought that a dedicated section is appropriate for it.

In chapter 3, the authors provide an in-depth explanation of the instruments, their capabilities, and how they were used in this study. Although it is a very interesting topic, some parts of the description of the instruments and measurement practices feel overly detailed and unnecessary in order to understand the results and follow the discussion. This is potentially a part that can be shortened if the editor sees fit.

We already kept the chapter at a minimum and actually reviewer #2 asked for more detailed information in this chapter. Also, we think it is relevant to know about the experimental setup and the instruments that were used - together with a minimum of information about the instruments' capabilities - in order to understand the following discussion.

This study investigates the fate of INPs during the transport from Hohenpeißenberg to Eriswil. While they discuss the potential losses of INPs within the PBL, they do not account for the emission/formation of INPs between the two cities, e.g., resuspension or emission of dust particles. Please discuss such potential sources of INPs between the two cities.

In lines 266-269, we added: “Of course, INP sources between HPB and Eri must also be taken into account. However, during the cold Bise the region in between both sites was snow-covered and temperatures were below freezing which keeps the biological activity at a minimum. Therefore, additional INP sources are considered to be of minor importance.”

The proposed removal pathway of INPs is by removal after the interaction within the supercooled stratus cloud (line 305). Does it mean chemical decomposition? Also, according to the introduction (line 31-39), only biological particles may be activated during the cold Bise temperature range during the study (-10 to 0 degree C). Are the activated INPs mostly biological particles?

The removal pathway of the INPs works in the following way: once the INPs initiated ice formation in the supercooled liquid cloud, the ice grows also at the expense of available liquid water (Wegener-Bergeron-Findeisen effect). And when the ice crystals fall out of the cloud, the INP is still immersed in the ice crystal. If it reaches the surface before evaporating again, the INP is not available anymore within the cloud / PBL. Not necessarily all INPs must be biological particles, however, in the temperature range (-10 to 0°C) these are the most efficient INPs and it is therefore expected that most of the INPs are of biological origin in this temperature range.

The description of HPB as “in the free troposphere” is misleading; it is more accurate to state that HPB was above the local inversion (decoupled from the PBL), avoiding any implication of a zero-thickness boundary layer. The site setting also remains unclear—please specify whether HPB and Eri are on ridge/slope/valley, provide station elevations, and relate these to inversion height (a simple time–height plot would help). An apparent inconsistency exists where HPB is described as within the PBL during cold-Bise but “free troposphere” during warm-Bise, although the PBL typically deepens in warmer conditions; a physical mechanism (e.g., terrain-driven decoupling, subsidence, radiosonde timing) should be provided or the claim revised. Finally, link this more explicitly to the INP interpretation: depleted in-cloud INPs at Eri versus higher INPs above the inversion at HPB are consistent with cloud scavenging plus episodic entrainment from aloft; consider secondary ice production and avoid wording that suggests an absent PBL.

Regarding the confusion about the free troposphere vs above the local inversion, we added now to lines 198-199: “Due to that, HPB is no longer situated in the PBL but above the local inversion (decoupled from the PBL) hereafter denoted as in the free troposphere.”

Regarding site setting: We already provide the station coordinates and elevation in lines 79-85 and we added now “on top of a hill” for Eri and HPB as well as “flat terrain” for MEL.

Regarding inconsistency about HPB in the free troposphere: In the entire manuscript, we write about three scenarios for HPB: 1) cold Bise, HPB within PBL, 2) cold Bise, HPB above PBL, 3) warm Bise, HPB within PBL. So, HPB above PBL during warm Bise would indeed be wrong for our scenarios. However, we checked the manuscript and did not find any place where we wrote something like that. If the author of the community comment still sees the need to discuss this topic, we would be happy to have a corresponding line number provided.

Regarding depleted in-cloud INPs at Eri versus higher INPs above the inversion at HPB: We already discuss this in lines 305-316 and regarding secondary ice formation, we discuss this in lines 312-320, also after advise of reviewer #2.

Specific comments

Line 10: Please elaborate on what the “INP contrast” means.

“the difference or degree of difference between things having similar or comparable natures”
<https://www.merriam-webster.com/dictionary/contrast>
In this case it is about the spatial contrast between HPB and Eri as well as the contrast during cold and warm Bise conditions.

Line 71-72: This is the motivation for studying INP in the region. This can be mentioned in the abstract.

We added “because reasons for the lack of ice and precipitation in the supercooled clouds observed over the Swiss Plateau remain unclear and may be caused by the lack of INPs” to the abstract (lines 6-7).

Line 157-158 & Fig. 5: Please provide references regarding the deactivation of biological particles after heating. Also discuss how the heating treatment affects non-biological particles. Also, for Fig. 5 spectra, please use more distinguished markers for the two methods, such as triangles and circles.

Regarding references: we now cite Christner et al. (2008) in L168 of the revised manuscript

Regarding effect of heating on non-biological INPs: Firstly, it should be noted that also biological INPs tend to vary in their response to heating. Proteinaceous INPs are typically strongly affected, while polysaccharidic INPs are rather unaffected (Hartmann et al. 2025). In response to reviewer 1 we now emphasize more that the heat treatment affects primarily proteinaceous INPs, which should be seen as a proxy for biological INPs, but is not synonymous for biological INPs. Studies rarely report an effect of heating on mineral INPs, an exception is Daily et al. (2022). Their laboratory study shows that minerals can also be affected by the heat treatment to varying degrees, but the magnitude is smaller in comparison to proteinaceous biological INPs. Daily et al. (2022) also attest that despite being not unambiguous, the heat treatment is a valid tool for ambient samples.

It has to be considered that in ambient samples, mineral INPs start to dominate the INP population only below -15°C , whereas the proteinaceous biological INPs are most relevant above -10°C . Hence this would not affect the interpretation regarding the presence of proteinaceous biological INPs.

Regarding the markers: The markers (upward and downward triangle) were selected purposefully to be similar. Due to technical limitations it is not possible to measure a complete INP spectrum, i.e. from 0°C until the homogenous nucleation limit. What is possible is to measure the same sample with different instruments that cover different concentration ranges. Since the relevant information is the resulting INP concentration, not the instrument, we chose markers that allow differentiation between the instruments if desired by a reader, but do not distract from the main point, the INP spectrum as a whole and the difference

between the locations. Therefore we will keep the current markers.

Line 195: Please explain the implications of the inversion for INPs measurements

We added to the manuscript: "The occurrence of the inversion leads to an inhibition of the exchange between the PBL and the free troposphere. Below the inversion the air should be well-mixed and therefore, we assume that the INP measurements at the ground are representative for the entire PBL." (line 205)

After introducing the acronyms for the two cities (line 79-80), use these acronyms throughout the manuscript, rather than mixing them with the original names (e.g., lines 209 & 211).

Now, we use the introduced acronyms of Eri, HPB, and MEL throughout the manuscript.

Line 208-212: How was the back trajectory in February?

The back trajectories were mainly approaching from north-easterly directions in the relevant heights as well. However, Melpitz was not necessarily included in the air stream. All the relevant backtrajectories are now available in the supplementary material.

Line 220: I could only find a very brief mention of artificial cloud seeding in line 75, but also without any details there. I think it is very important to provide more information on that, even if there is a publication cited. What type of seeder was used? Can it be an INP, and under which conditions? Were these conditions present in any case you also analyzed? How would you expect this could affect your study?

We added to the manuscript (line 237ff): "A flare was ignited at the drone, releasing silver-iodide that can act as an INP at temperatures below -5°C (Marcolli et al., 2016). The flare was ignited inside the clouds upwind of the measurement site, so the plume of silver-iodide, the potential source of INPs, could have dispersed and reached the measurement site in theory. However, we want to note that we checked the entire dataset for signals from artificial seeding in the INP measurements. No evidence of cloud seeding particles on the ground was observed throughout the whole campaign."

Line 222: How did you check the absence of the particles from artificial seeding?

We did a comparison of INP data for filters sampled in Eriswil while seeding activities with silver iodide were performed 15 minutes upstream of the measurement site and compared it to sampled filters without any seeding impact. Results showed an overlap of data without any visible contrast in the measured INP spectra.

Line 228: Melpitz INPs were higher for the T range from -15 to -5 degree C. However, it is unclear if the total identified INPs were higher than those of the other two cities. Why is Melpitz INPs data below -15 degrees C not shown? Please explain in the main text.

We do not understand what is meant by “total identified INPs” .There is no such thing as a total INP concentration, the INP concentration always depends on the temperature, hence $N_{\text{INP}}(T)$. Also all data is shown, but instruments have limitations and droplet freezing arrays such as the ones used in this study can measure only in certain concentration ranges determined primarily by geometrical factors such as droplet volume and number. Limits in the measurable concentration range then in turn limit the temperature range in which $N_{\text{INP}}(T)$ can be reported. This temperature range depends on the sample and is not fixed by the device. While we answer this question within our reply, we do not think that it is necessary to include this in the main manuscript.

Line 299–301: It is unclear how the dusty-cirrus mechanism supports INP mixing with supercooled stratus in the boundary layer. Could this not be explained directly as the interaction of INPs with supercooled droplets? The dusty-cirrus mechanism usually describes mixing of dusty air with clear moist air.

Of course, the dusty-cirrus mechanism does not help in the Bise situation. As we tried to indicate, we used the dusty-cirrus mechanism only as an analogy. Both have in common that one layer has the aerosols/INPs but a lack of humidity and the other layer has the humidity but a lack of aerosols/INPs. Where they mix, clouds can form as well as ice in clouds. For more clarity, we added to the manuscript (line 333f): “The dusty cirrus mechanism describes mixing of a separated dusty air layer with a clear moist air layer. At the interface, mixing of these layers supports cloud formation. Analogously, during Bise conditions, the PBL is the moist layer with limited INP availability, separated from the free tropospheric air that can act as a source of INPs, supported by our measurements.”

Table 1: Are these instruments at the Eriswil site or both Eriswil and Hohenpeißenberg sites? It would be better to list all the relevant instruments at the three measurement sites in the table.

We added a column in Tbl. 1 called “site”, where we indicate if the measurement device was located in Eri, HPB and/or MEL.

Figure 2: In panels c) – f). I am a bit confused regarding the dates that are presented. In both cases, the authors do not include profiles from the whole time period, and the dates do not match between c) and e). Is that a result of data availability/quality? Are all days included in the analysis, even if not shown here?

Yes, c) to f) contains all radiosonde data available for the cold and warm period as defined in the manuscript for both sites. You are right, in c) and e) not the exact same dates are shown which is a result of data availability.

Figure 2 caption for e-f: Does it mean that the shown profile is for Munchen, not HPB? How close were these two locations?

Yes, that is what we wanted to express by adding “Temperature profiles for Hohenpeißenberg [...] represented by München-Oberschleißheim data.” in the caption.

Both locations are ca. 60km apart and the radiosonde launches in Munich are the closest ones to Hohenpeißenberg available. Also, only by using a radiosonde launch that starts below the altitude of HPB, it is just possible to state that HPB is actually located above the inversion.

Figure 4: Why are only cold period trajectories calculated?

We also calculated trajectories for the warm period. We show the trajectories for the warm period now in the supplementary material.

Figure 4 caption: mention that the figure also shows precipitation along the back trajectory path.

We added: "In the lower part, the precipitation along the trajectories is shown."

Figure S1-S2: consider adding a third panel that shows the difference (and/or ratio) between INP number concentration from Hohenpeißenberg and Eriswil.

We thank the reviewers for their comment. We are not convinced that a plot showing the difference or ratio between both sites gives significant additional information. The differences can be visually found in Fig. 5. Figs. S1-S2 are already other ways to visualize the data in Fig. 5. Introducing a difference could be misleading when overinterpreting a non-significant (within the range of uncertainty) positive or negative sign.

Technical comments

Line 8: INP → INPs

We changed INP to INPs

Line 9: First → first

We changed First to first

winterly PBL → wintertime PBL

We changed winterly PBL to wintertime PBL

Lines 21-30: Although this section is very well written and supported by many relevant references, I personally find the structure a bit odd. Crystal growth is followed by secondary ice formation and then by ice nucleation. In my opinion, starting with introducing HOM and

HET nucleation and then ice crystal growth and secondary formation would be a more logical sequence.

While it may be not as common to start with the result, i.e. precipitation, and follow its formation back to root, i.e. the particle, it is not illogical. Since this was not remarked by the other reviewers, we prefer to keep this as it is.

Line 31–32: Please provide a reference for the statement: “At a temperature of $-20\text{ }^{\circ}\text{C}$, the fraction of aerosol particles able to act as INP is on average one per million.”

We changed the sentence to the following (Line 32): “ When comparing typical INP concentrations (as e.g. given in Petters and Wright, 2015) with typical particle concentrations, it can be estimated that roughly only one in a million particles can initiate freezing at a temperature of $-20\text{ }^{\circ}\text{C}$.”

Line 32: A reference could be added here to support this claim.

The sentences following after this elaborate on that claim and also provide the respective literature references.

Line 41: ...mainly on ambient temperature, ice supersaturation and...

Changed.

Line 45: ...their shape and size during...

Added “their”.

Abbreviations: Repeated or undefined (e.g., CLOUDLAB Line 75). Define abbreviations consistently (e.g., PM₁₀, PolarCAP, PCR).

Added “(PM₁₀ refers to particles that pass through the size-selective air inlet, which has a separation efficiency of 50 percent for an aerodynamic diameter of 10µm.)” to L126-127

Added “(Polymerase Chain Reaction)” to L151

Line 132: “Uni Wyoming (2024)” → University of Wyoming (2024)

We changed Uni Wyoming (2024) to University of Wyoming.

Line 211: HBP? → Please check the abbreviation.

We changed HBP to HPB.

Line 272: ICNC repeated too often; reduce repetition across the manuscript.

We removed “ICNC” a few times.

Line 276: “300 m” but cloud top varies up to 400 m, please clarify.

Now, we write 300-400m.

Line 283: Fig. 5b,c)) → delete repeated parentheses.

The repetition of the parentheses was a result of consistency. We always used closing parentheses with a figure that has a subfigure a, b, ... We acknowledge that a double parentheses is not looking well. So, we consistently removed the closing parentheses behind every figure description b, c, etc. throughout the manuscript.

Figure 2: Legends are barely legible

As this was also mentioned by the reviewer #2, we increased the size of the legends as well as the figure size.

Figure 2c–f: y-axis label missing/unclear, please.

The y-axis label was not missing in the latest version of the manuscript. If it was unclear before, now we changed it to: “Height a.g.l. w.r.t. Eri [km]” and “Height a.g.l. w.r.t. HPB [km]”.

Figure 6: extend width; hours are hard to read, though results are discussed in short time ranges.

We increased the size of the written dates in the figure and a bit the figure size.

Figure S4-S7: Please indicate the three measurement sites on the map.

We indicated the three locations now in the trajectory figures of the supplementary material.

References

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