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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 lines266-269, 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 **$N_{INP}(T)$.** 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 $N_{INP}(T)$ 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 °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 °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 “(PM_{10} 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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