Color legend:
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 °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 °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 °C and -10 °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 Eriswel 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

Petters, M. D. and Wright, T. P.: Revisiting ice nucleation from precipitation samples, Geophysical Research Letters, 42, 8758–8766, https://doi.org/10.1002/2015gl065733, 2015.