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Comment on acp-2022-29

Anonymous Referee #3

Referee comment on "Technical Note: A High-Resolution Autonomous Record of Ice Nuclei Concentrations Between -20 to -30 °C for Fall and Winter at Storm Peak Laboratory" by Anna L. Hodshire et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-29-RC3, 2022

Review of Hodshire et al. (2022): **Technical Note: A High-Resolution Autonomous Record of Ice Nuclei Concentrations for Fall and Winter at Storm Peak Laboratory**

General comments:

In this technical note, the authors present modifications to an automated, near-continuous INP counter, the Handix Scientific CFDC Ice Activation Spectrometer (CFDC-IAS), and data measured with the CFDC-IAS for four months at the Storm Peak Laboratory (SPL). Specifically, measurements of atmospheric INP concentrations at 10-minute resolution are presented between October 9, 2020, and January 29, 2021, with the chamber in operation for the entire period except January 3-10. Parallel APS and SMPS measurements at SPL allowed estimation of the density of active surface sites. In addition, a period of high and low INP concentration was qualitatively analyzed using NOAA HYSPLIT back trajectories.

The writing (from an editorial standpoint) is to be commended. However, relevant technical details on the design changes are missing for a technical note. Validation of the changes is lean, but what is presented is solid. The discussion of validation is brief and must be done by the reader through study of Figure 1 itself. There is too much information on atmospheric parameters other than INP concentrations (e.g., Fig. 2b-d) for a technical note. It is debatable whether the single design change qualifies for a technical note or whether it would be better included in a manuscript with an in-depth analysis of its measurements. Potentially, the manuscript aims for the latter, but in my opinion misses to qualify, as the manuscript fails to analyze and discuss important elements. An example is the observed lower INP concentrations at -30 °C than at -25 °C and -20 °C, which does not reflect the consensus of previous studies and may indicate an invalid measurement methodology, thus, should be critically reviewed by the authors. The topic of the paper is well suited for ACP. However, I suggest that the manuscript undergo a major revision to reflect the following comments:

Major comments:

- Please provide more technical information on the design changes. Also, relevant parameters and statistics are missing for an automated near-continuous INP counter. E.g.: how long are the gaps needed to renew the ice layer? What is the percentage of atmospheric measurements within the total time (atmospheric measurements divided by total time including atmospheric measurements, background measurements, cooling, warming, or temperature compensation periods, ice layer renewal, and maintenance)? How does the signal-to-noise ratio change over time (not only qualitatively, but also quantitatively)? What happens with the water needed to form the ice layer and how is it recycled?
- Are there any indications for the lower INP concentrations at -30 °C compared to -20 °C? This does not reflect the consensus of previous studies and may indicate an invalid measurement methodology, thus, should be critically reviewed by the authors.
- Please provide numbers and discuss the implications in more detail. For example, lines 136-138 state that there was little difference in medians or IQR between observations inside and outside clouds or between daytime and nighttime observations, but lack a more detailed discussion, e.g., of what these results mean and what the causes might be when comparing observed patterns of total aerosol number concentrations or other aerosol quantities. Missing discussions on implications are also true throughout the manuscript. Analysis using back trajectories has been done extensively in numerous previous studies. However, new insights remain missing, and thus, the back trajectory analysis can be left out from the manuscript. In addition, the advantage of continuous, high-resolution INP measurements is not exploited as only two events were analyzed.

Specific comments:

Abstract (lines 15-21): a very concise summary. However, it lacks an introductory sentence or two on why measuring atmospheric INP concentrations is relevant. Since the focus of the manuscript is on autonomous INP measurements, more information should be provided on the frequency of site visits required (e.g., 1/week to replace desiccant).

Chapter 2.1 (line 56): please provide the amount of snow in standard international units (mm).

Chapter 2.2 (lines 95-98): please provide measurements or estimations of the transmission fraction of particles <2.5 µm through the sample line and the diffusion dryers until entering the CFDC.

Chapter 2.2 (lines 95-108): I infer that the only supply needed to run the chamber continuously are electric energy, desiccant and nitrogen. How is the water for the ice layer recycled?

Chapter 3.2 (line 136): Please quantify "little difference" for both cases (in cloud vs. out of cloud and night vs. day).

Chapter 3.2 (line 142): Please quantify "No strong correlation" for all cases.

Chapter 3.2 (lines 143-148): Food for thought: Given the large amount of data collected over the four months, more than a qualitative comparison of two periods would have been interesting.

Chapter 3.2 (lines 143-148): Whether it is a qualitative comparison of two periods or a quantitative analysis of the entire four months, the limitations of the used tools must be addressed: what are the limitations of the analysis using back trajectories from a 1-degree GDAS reanalysis, since they are unlikely to fully resolve local features? Please explain the degree of uncertainty in the back trajectories used, e.g., by referring to previous studies where this has been analyzed at SPL.

Chapter 3.2 (lines 151-153): "Other high-elevation free tropospheric INP measurements…" implies, the presented measurements at SPL were sampling free tropospheric air masses. Please provide quantitative evidence for this statement.

Chapter 3.2 (lines 151-156) and chapter 4 (lines 197-198): There are interesting statements within these lines: short-term, high concentration events that were not picked up by previous measurement techniques. Please elaborate on these events. How frequent were they observed? How long did they last? Where there co-located signals in other aerosol parameter such as total number concentration or spikes in aerosol in a specific size bin? Can local pollution be ruled out? To my understanding, such brief events should also be captured during the three times longer sample duration used in, e.g., Lacher et al. (2018) or Brunner et al. (2021). Brunner et al. (2021) also measured continuously for one year, so their measurements should provide near identical temporal resolution. What are other reasons that other studies have missed these short events?

Chapter 3.2 (lines 163-171): As there is much emphasis on surface active site density, I would suggest to show a time series of n_s and discuss n_s in more detail. E.g., also looking at the number concentration of large particles. Furthermore, the fact that n_s for INP at -25 °C and -30 °C are identical is striking and does not align with previous studies. This should be discussed in more detail in addition to the major comment #2.

Chapter 4 (lines 198-199): What are the different transport patterns between this and previous campaigns? Please elaborate in Chapter 3.

Figure 3: Please add the year to the title of the figure.

Figure 4: In panel a, the °-symbol is missing and in panel b, the unit of the temperature categories remains missing. For consistency, I would suggest to add the units to the axis labels (e.g., Temperature category [°C]). Also, "temperature category" form the axis labels is once capitalized and once not. Please use a consistent style.

Figure 5: The data points at -25 °C and -20 °C are shadowed by the data points at -30 °C. I would suggest adding transparency to the markers, such that more information is visible.

Literature:

Brunner, C., Brem, B. T., Collaud Coen, M., Conen, F., Steinbacher, M., Gysel-Beer, M., and Kanji, Z. A.: The diurnal and seasonal variability of ice nucleating particles at the High Altitude Station Jungfraujoch (3580 m a.s.l.), Switzerland, Atmos. Chem. Phys. Discuss. [preprint], https://doi.org/10.5194/acp-2021-710, in review, 2021.

Lacher, L., DeMott, P. J., Levin, E. J. T., Suski, K. J., Boose, Y., Zipori, A., Herrmann, E., Bukowiecki, N., Steinbacher, M., Gute, E., Abbatt, J. P. D., Lohmann, U., and Kanji, Z. A.: Background freeâ□□tropospheric ice nucleating particle concentrations at mixedâ□□phase cloud conditions, J. Geophys. Res., 123, 10,506–10,525, 2018.