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Comment on acp-2022-29

Anonymous Referee #2

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-RC2, 2022

Hodshire et al, present results obtained with a newly automated CFDC for the quantification of INPs. Although, the CFDC appears to be working autonomously, which is a major feat, I have serious questions about how the INP concentration observations are reported. The lack of temperature dependence is truly surprising and goes against the previously observed and understood dependence of temperature on the ability of aerosol particles to nucleate ice in the immersion mode. Therefore, I recommend that the authors spend some time to assess the representativeness of the reported values before the manuscript is accepted into ACP. Furthermore, a deeper analysis of the factors controlling the variability in INP concentrations should be presented.

Major comments:

The lack of temperature variability in the observed INP concentrations is truly surprising. The statistical methods to achieve these results need to be discussed and presented. Based on the acknowledged limit of detection (almost 1 L-1) of the CFDC, the presented results are likely not representative of the actual temperature dependence of INPs that are observed at SPL. Please discuss this limitation on the presented results and assess how meaningful the presented values are at -20 and -25 C. With this limit of detection in place, it is clear that only the upper end of INP concentrations occurring at these temperatures will be observable. This limits the meaningfulness of the presented statistics. There are several locations in the paper where this could be discussed/improved as highlighted below.

The main benefit of having high resolution and continuous INP measurements is to understand the factors that control INPs. Unfortunately, it appears as if there is no dependency on previously established controls (e.g. meteorological factors, aerosols) of INPs. Perhaps this is masked due to the discussion spanning all of the temperatures rather than only the observations at -30 C where background issues are likely less important (e.g. Brunner et al., 2021). Regardless, a deeper analysis controlling the variability of INPs should be conducted. Otherwise, the paper is more of an instrument development/proof concept (e.g. AMT paper) rather than an ACP paper.

Minor comments:

Line 30: There are now a few automated CFDC measurements that have conducted continuous measurements for longer periods of time (Möhler et al., 2021; Brunner and Kanji, 2021)

Line 42: Again this is perhaps the first of its kind at SPL but definitely not the first automated long-term mountaintop INP measurements (Brunner and Kanji, 2021).

Line 54: The WRCC climate portal reported snowfall observations are for the town of Steamboat Springs and are not representative of what is observed at SPL. The snow depth sensors and weighing rain gauges around SPL (e.g. Tower Snotel has an average SWE of ~50 inches annually) report a much higher annual snowfall amount. Please double check this.

Line 90-93: Were the aluminum walls sanded such that they were rough and able to better retain water and subsequent ice? This might be an interesting detail to add for future CFDC development.

Line 97: What determined a sampling time of 4 or 6 hours before defrosting and reicing? This is quite a difference in terms of background degradation.

Line 99-101: A background of 1 L-1 is a significant concentration when considering that typical INP concentrations at -20 C have been previously reported to range between ~0.05 and ~1 L-1 based on precipitation samples (e.g. Petters and Wright, 2015) or between ~ 0.01 and ~100 L-1 in the air (e.g. Kanji et al., 2017). How were sampling periods where the INP concentration was below the limit of detection handled? Is this accounted for in the reported statistics?

Line 129: What was the target saturation, was it the same for all temperatures?

Line 136: The lack of dependence of INP concentration on temperature here is astonishing. Furthermore, the lower estimates of the INP concentration are certainly influenced by the limit of detection (the background concentrations). This should be acknowledged here and also how measurements below the limit of detection are handled should be discussed.

Line 136-138: How efficient is the inlet at sampling precipitation particles e.g. cloud droplets? If these particles are not sampled then does this indicate that the INP measurements during precipitation are of interstitial aerosols? Also, the lack of a diurnal cycle is quite striking considering results on the influence of boundary layer intrusions on INPs at other mountaintop observatories (e.g. Lacher et al., 2018; Brunner et al., 2021). Do the aerosol concentrations have a diurnal cycle?

Line 146-148: Was the amount of precipitation along the back trajectories considered? Previous studies have suggested that precipitation can either increase or decrease INP concentrations (e.g. (Stopelli et al., 2015; Huffman et al., 2013; Mignani et al., 2021)

Line 170: Again, the lack in variability between -25 and -30 in ns values is truly surprising. This would indicate that the aerosol particles responsible for the observed ice activation would have the same efficiency at -25 as at -30 C. Typically, the INP concentration increases by an order of magnitude every 5 degrees (e.g. Atkinson et al., 2013; Murray et al., 2012)

Figure 2: Based on panel a. it looks like there are occasions where the INP concentration is higher at warmer temperatures than colder ones. This seems unphysical and again raises the issue of the importance of the background on the measurements.

Figure 4: It would be worth including the number of statistically significant observations used to make the box and whisker plots for each set temperature.

Technical comments:

Figure 1: The legend has filled markers yet the figure has open markers. Also, it might be worthwhile to add uncertainties to the reported data points to account for uncertainties in temperature.

Figure 2: As only three set temperatures were investigated, consider switching to a discrete color bar rather than a continuous one.

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