

**Comments on “The impact of blowing snow above sea ice on
atmospheric ice nucleating particles in the Central Arctic” by
Malpas et al.**

This manuscript investigates the association between blowing snow events (BSEs) and ice-nucleating particles (INPs) using year-round MOSAiC observations collected from October 2019 to October 2020. It combines INP measurements from two-day aerosol filters and surface snow samples with weekly aerosol ion composition measurements to assess whether changes in atmospheric INPs can be attributed to the sublimation of blowing snow. The study addresses an important topic and presents a valuable dataset. The manuscript is well organized and represents a substantial amount of work. However, several conclusions exceed the available evidence, and methodological and numerical inconsistencies need to be resolved. I recommend major revision for the reasons outlined below.

Major comments

1. Please clarify the background correction, detection limits, and measurement uncertainties. In particular, uncertainties associated with Poisson counting statistics should be explained and included in the INP concentration plots. Some field blanks exhibit higher freezing activity than some filter samples. Were the field blanks accounted for, and how were these cases treated? If sufficient sample material remains available, dilution measurements on representative samples would help extend the measurements to lower temperatures.
2. Figure 3: I suggest moving this figure to the Supplement and replacing it in the main text with a heatmap of INP concentrations. Because the sampled air volumes differ among filters, their frozen fractions are not directly comparable. The concentration heatmap should account for the field-blank background, as suggested above.
3. Figure 10: Neither the differences in INP concentrations nor the shifts in median freezing temperature are statistically significant according to the reported tests. Nevertheless, the abstract and conclusions state that blowing snow increased activation temperatures. Please report the exact p-values, effect sizes, and confidence intervals, and describe the temperature shift as an observed tendency whose association with blowing snow remains uncertain.
4. The snow-to-atmosphere calculation requires an explicit equation and an uncertainty assessment. The calculation assumes complete release of INPs from

a specified snow layer and uniform mixing throughout a 230 m boundary layer. This provides a useful estimate of the potential INP contribution from the snow reservoir, but its interpretation depends strongly on release efficiency, sublimated snow mass, mixing depth, and removal processes. Please provide the conversion equation with units and assess its sensitivity to these assumptions. Snow salinity, sublimation rates, and emission rates are also subject to substantial uncertainties. Do the conclusions remain robust when these uncertainties are taken into account?

Minor comments

1. Lines 33–35: Given the focus on the effects of BSEs on INPs, please acknowledge that blowing snow may also be an important aerosol source during winter and spring, alongside the influence of Arctic haze.
2. Lines 40–49: Please specify the temperature ranges covered by the cited studies. These studies focus on temperatures relevant to mixed-phase clouds, whereas BSEs may also be important at lower temperatures.
3. Figure 3: The colour sequence appears to be reversed. The frozen fraction should increase as temperature decreases, so the colours should transition from red at warmer temperatures to blue at colder temperatures. However, the figure appears to show a transition from blue to red as the frozen fraction increases for each sample. Please check the colour mapping.
4. Lines 236–237: “We measured INPs with activation temperatures ≥ -15 °C in 75% of samples during June, July, and August, compared with 19% of samples taken during October to May.” Does “activation temperatures” here refer to the median activation temperature, or to the detection of any INPs active at or above -15 °C? Please clarify.
5. Lines 271–273: “We observed the lowest INP concentrations across all temperatures selected (-15 °C, -20 °C, -25 °C) during November and December (Fig. 4).” Please explain the basis for identifying November and December as the months with the lowest INP concentrations. In Fig. 4, the concentrations appear relatively stable throughout much of the year, apart from the summer 2020 enhancement.
6. Figure 7: Only ten data points are used to assess the correlation between CH_3SO_3^- and nss-SO_4^{2-} , and four points lie relatively far from the fitted line. Given the small sample size and scatter, the evidence for this relationship appears weak. Please interpret the correlation cautiously.
7. Figure 10: Is the “median activation temperature” defined as the temperature at which the frozen fraction reaches 0.5? Please define this quantity explicitly. Filters collected during BSEs are defined as those with $\geq 20\%$ temporal overlap with BSEs, and their INP concentrations at -20 °C and -25 °C are compared with those of filters with $< 20\%$ overlap in Fig. 10a. However, the differences between these groups appear small relative to the experimental uncertainty. Similarly, the median

activation temperatures show little change across the >25%, >50%, and >75% overlap thresholds. I therefore do not see clear evidence that either INP concentrations or activation temperatures change during blowing snow events.

8. Line 347: Please change “October 2020 to December 2020” to “October to December 2019”.
9. Line 447: Please check the reported snow density of “ $0.283 \pm 77 \text{ g cm}^{-3}$ ”, which appears to contain a numerical or unit error.
10. Figure 4 caption: The stated “7-week surface snow resolution” conflicts with the one-week resolution described in the Methods. Please clarify and correct this discrepancy.