

General comments:

In this study, the authors employ ice nucleation measurements and ion chromatography of surface snow and aerosol filter samples collected during the MOSAiC expedition to investigate the relationship between blowing snow events (BSEs) and ice-nucleating particles (INPs) in the central Arctic. The immersion freezing measurements of 75 polycarbonate filter samples collected over a full annual cycle represent a significant amount of work. These results, together with the surface snow measurements, present an interesting comparison to gain new insight into potential sources of enigmatic INPs in polar regions.

The paper is generally well written, and the results and analysis are thorough and comprehensive. However, I am not convinced by the argument that these results support BSEs as a source of INPs. In my view, the results presented are more readily interpreted as indicating that blowing snow is unlikely to make a substantial contribution to INPs. As the authors themselves note, they found no statistically significant impact of BSEs on INP activation temperatures. Further, there were no BSEs detected in the summer when INP activity was highest. Even considering the selected BSE case studies, a decrease in ice-nucleating activity was measured in the filter samples showing the strongest blowing snow influence. Taken together, these findings do not support the conclusion that BSEs are a likely contributor to INPs. If anything, they appear more consistent with the opposite interpretation. Overall, the reporting of seasonal variations in INPs and major ion composition in the central Arctic is valuable, but the conclusions regarding the significance of BSEs should be revised and some interpretations clarified, as outlined below.

Specific comments:

Line 8-10: The sentence "... but we did observe an increase in INP activation temperatures" needs clarification. When was this increase observed? The current phrasing implies that it was associated with BSE occurrence, but this does not appear to be generally supported by the results presented.

Line 145 and lines 175-177: According to the singular approximation, the assumption is that a droplet will freeze at the temperature corresponding to the warmest INP activation temperature of all INPs immersed in a given droplet. Only one immersed INP active at a given temperature is required to induce freezing. I therefore suggest taking greater care in the discussion of concentrations here. A higher concentration of INPs in a droplet would increase the likelihood that an INP with a warm activation temperature is present, but a droplet freezing at a warm temperature does not in itself imply that "the number of INPs per droplet is high".

Lines 202-204: This seems like quite a key point and may warrant additional emphasis in the text.

Lines 213-214: Greater care should be taken on the wording here. It is not true to suggest that the singular approximation results in a blanket underestimation of INP concentrations at all temperatures. I also suggest rephrasing "the majority of filters did not share activation temperatures" for clarity.

Lines 347-348: How are you observing low INP concentrations “at temperatures above the detection limit”?

Line 376, Line 405 and elsewhere: I question whether the repeated ‘blowing snow vs long-range transport’ framing is justified. It seems to dismiss concurrent contributions from other local or regional sources and overstate the significance of BSEs as a source of this aerosol type, with little evidence to support the connection.

Line 578: Rephrase “We also observed a lack of increase in INP activation temperatures...”. Were activation temperatures unchanged, or did they decrease?

Lines 587-590: For the sentence: “The low ice nucleation activity in late autumn and early winter followed trends of suppressed concentrations across most ions measured, suggesting that INP concentrations in the Central Arctic track general changes in the strength of various aerosol sources, including both BSEs and long-range transportation.” I think the main point gets lost here, and I’d recommend either removing or rewording this section. I’m not sure that there are “general changes” across discrete and diverse aerosol sources, or that the long-range transport of long-lived dust, for example, could be said to vary seasonally in the same way as local source contributions. I also recommend replacing “suppressed concentrations” with “reduced concentrations”. A more concise and focussed discussion in places would strengthen the paper.