

The presented study follows up the research by the same authors with observations showing counterintuitively more ice crystals over sea ice than over open ocean in Arctic ice boundary-layer clouds. The presented study aims to explore the potential causes behind this contrast by comparing the satellite observation results from DARDAR-Nice and ICON-LAM atmospheric model. Three hypotheses were tested to explain the relative difference in ice crystal numbers: 1) potentially more INPs over sea ice than over open ocean; 2) difference in contribution from blowing snow; and 3) difference in secondary ice production (SIP). The authors conclude that INPs and blowing snow could determine the difference in observed ice crystal numbers over sea ice and open ocean, while SIP is not a determinant in this contrast.

Despite the intriguing scientific question of what drives this unexpected observation, the manuscript faces major shortcomings in several critical areas, which make its conclusions scientifically unconvincing:

1. Inadequate or over-simplified model representations

Insufficient and not well-presented results to support the interpretations and conclusions due to inappropriate selection of tool, i.e., the km-scale ICON model, which does not explain the atmospheric dynamics that are likely responsible (and definitely worth considered as one hypothesis or impacting factor) for the observed difference in ice crystal numbers over different landscape based on the authors' original hypotheses. E.g., turbulence that forms ice differently (as stressed by referee 1), atmospheric transport of aerosols that could shift the INP abundance in ice boundary-layer clouds, changing in leading mechanisms of SIP due to the differences in vertical temperature and saturation profile, etc. In addition, within the chosen method, the setup of models (e.g., scaling parameters) are usually selected arbitrarily without proper reasoning or citations and were largely simplified, leading to subjective and less representative results for the studied scenario. I strongly support the idea from referee 1 to dig into the observational data from different dimensions instead, not limited to the field observations but maybe also reanalysis data, including (in addition to particle size distribution suggested by referee 1) surface wind field for further investigation of blowing snow and air mass history analysis by backward trajectories to find out the sources and sinks of aerosols (INPs) during the investigated period.

2. Lack of detailed scientific explanations

The Result section focused on describing the obvious statistics from the presented figures without further digging into the science or physical mechanisms behind the "phenomenon". In general, lacking solid scientific explanation makes the

presented paper more like an experimental report rather than an academic paper. Many open questions remain unexplained, including but not limiting to e.g., what could be the extra INP sources over sea ice? Does organic INPs representative enough in the Arctic environment? What are the driving forces behind more blowing snow/SIP over ice compared to the ocean? Why the contrast in ice crystal numbers different in different setup and temperature ranges? Etc. The storyline is completely unclear without addressing these questions arise from your results.

3. Selective reporting of results

Even for the presented results, they are sometimes not accurately demonstrated, or the authors seem to manually pick the results that are more supportive or significant to explain their hypotheses. It was mentioned several times some results are not shown when they are not significant or against the hypotheses (e.g., lines 194, 294, and 309). In my opinion, it is unfair to conclude directly without showing the results because the readers should reserve the rights to make judgement out of an unbiased dataset provided by authors.

4. Insufficient and misapplied citations

As stressed above, in the Method and Result sections regarding ICON model setup, many configurations/parameters were announced without convincing reasonings or appropriate citations of relevant work. In addition, the author did not credit the proper citation of Arctic data, e.g., in lines 333-334, Petters and Wright (2015) was wrongly cited for comparing the Arctic INP data, which represents the global precipitation samples.

5. Missing Discussion Section

A Discussion section is missing, which should necessarily illustrate the limitation of the presented study, including e.g., the experimental setup (e.g., selection of scaling parameters, shortage of data due to very wide temperature bins); factors/hypotheses that are not examined but could have influence in the resulting difference in observed ice crystal numbers.

Regarding figures and data presentation, Figures 3–5 could be improved substantially:

- Consider using more effective color scales and symbols to make the data clearer.
- Eliminate redundant information or merge panels if they convey similar points; for instance, the right panels might be replaced with a more concise statistical significance test.

- Narrowing the temperature bins or rearranging the experiments into a single figure could offer a clearer comparative perspective.

Finally, the manuscript's language and structure would benefit from a thorough review, ideally by a native English speaker. Several informal expressions should be replaced with more precise academic language, unnecessary repetitions can be removed, and grammar/spelling errors need to be addressed. The paper would also benefit from overall conciseness, especially in the Introduction.

I look forward to following up this interesting research topic and the improved version of this paper with more convincing scientific storyline and evidence, clearer and more concise demonstration of results, and more professional and precise language.