

Review of
« impact of environmental conditions and ice nuclei recycling on arctic mixed-phase cloud properties »

by B. Ascher and F. Hoffmann

This paper investigates the effect of free-tropospheric conditions (humidity and temperature) and ice nuclei recycling on the microphysical structure of Arctic stratocumulus clouds. To this end, idealized Large Eddy Simulations of a radiatively driven mixed-phase stratocumulus are performed, varying the temperature and humidity above the cloud layer and either including or excluding the re-emission of ice nuclei at the surface.

The results show that ice nuclei recycling is a critical process, explaining differences in liquid and ice water contents within turbulent downdrafts and updrafts. The role of cloud-top entrainment is analyzed in detail, and its impact on the boundary-layer structure and on liquid and ice condensate concentrations is shown to strongly depend on both free-tropospheric humidity and inversion strength.

This paper is a valuable contribution to Arctic mixed-phase cloud modeling and is overall well written. I particularly appreciate how the authors meticulously analyze their results and their efforts to disentangle the relative contributions of each individual process. I have a number of comments that, I believe, should be addressed to strengthen the study and clarify the text; however, if properly revised, I would recommend the publication of this manuscript in ACP.

Major comments :

- My main comment concerns the size of the domain considered. I understand that running a Lagrangian microphysical scheme is computationally expensive; however, 64 grid points in the horizontal with a resolution of 20 m corresponds to a domain that is only 1280 m wide. The typical scale of convective cells in Arctic stratocumulus is around 1 km (see, for instance, <https://doi.org/10.1029/2024MS004296>), which means that your domain size is at the very limit for capturing the full range of turbulent scales.

Figure 8a shows that you simulate a single convective cell (one large updraft and one large downdraft), but I wonder to what extent this structure is imposed (and constrained) by the limited domain size, which may prevent turbulence from developing at larger scales, as it naturally would.

I would therefore suggest performing an additional simulation over a larger domain to ensure that the size of the updrafts and downdrafts (or, even better, the turbulent spectrum) is consistent with that obtained in the smaller domain.

- You state in the introduction that Arctic stratocumulus clouds are maintained by radiative cooling but note that such clouds can also be maintained by surface fluxes in case the clouds are coupled to the ocean surface. This is the case for cold-air outbreak clouds (see for instance Klein et al. 2009, Griesche et al. 2021, Sotiropoulou et al. 2014). Please rewrite the relevant paragraphs in the

introduction and in the conclusion to distinguish between surface-coupled and surface-decoupled clouds.

Klein, S. A., McCoy, R. B., Morrison, H., Ackerman, A. S., Avramov, A., Boer, G. D., Chen, M., Cole, J. N. S., Del Genio, A. D., Falk, M., Foster, M. J., Fridlind, A., Golaz, J.-C., Hashino, T., Harrington, J. Y., Hoose, C., Khairoutdinov, M. F., Larson, V. E., Liu, X., Luo, Y., McFarquhar, G. M., Menon, S., Neggers, R. A. J., Park, S., Poellot, M. R., Schmidt, J. M., Sednev, I., Shipway, B. J., Shupe, M. D., Spangenberg, D. A., Sud, Y. C., Turner, D. D., Veron, D. E., Salzen, K. v., Walker, G. K., Wang, Z., Wolf, A. B., Xie, S., Xu, K.-M., Yang, F., and Zhang, G.: Intercomparison of model simulations of mixed-phase clouds observed during the ARM Mixed-Phase Arctic Cloud Experiment. I: single-layer cloud, *Quarterly Journal of the Royal Meteorological Society*, 135, 979–1002, <https://doi.org/10.1002/qj.416>, 2009.

Sotiropoulou, G., Sedlar, J., Tjernström, M., Shupe, M. D., Brooks, I. M., and Persson, P. O. G.: The thermodynamic structure of summer Arctic stratocumulus and the dynamic coupling to the surface, *Atmos. Chem. Phys.*, 14, 12573–12592, <https://doi.org/10.5194/acp-14-12573-2014>, 2014.

Griesche, H. J., Ohneiser, K., Seifert, P., Radenz, M., Engelmann, R., and Ansmann, A.: Contrasting ice formation in Arctic clouds: surface-coupled vs. surface-decoupled clouds, *Atmos. Chem. Phys.*, 21, 10357–10374, <https://doi.org/10.5194/acp-21-10357-2021>, 2021.

- Several studies have highlighted the importance of the free-tropospheric INP reservoir in determining the microphysical structure of Arctic mixed-phase clouds, especially surface-decoupled ones (see, for instance, Griesche et al., 2021). Given the nearly exhaustive investigation of the role of entrainment in Arctic stratocumulus clouds carried out in this study, it is somewhat unfortunate that the authors do not elaborate further on this aspect and that no sensitivity tests have been performed by varying the free-tropospheric INP concentration. Such concentrations can vary dramatically depending on the region considered and during dust plume advection events, for instance (see <https://www.nature.com/articles/s41612-024-00811-1>), and could therefore significantly influence the results and their broader applicability.

Minor comments :

- title : 'environmental' has a quite vague meaning. What about 'free-tropospheric' ?

- Note that supplementary figures are referred to with an S rather than an A

- Introduction : I would add a few sentences emphasizing the potential and use of LES to understand the physics of boundary-layer MPCs.

-l48 : Note that droplet shattering is also an efficient SIP in Arctic MPCs. See for instance <https://doi.org/10.5194/acp-22-15579-2022> .

- l56 'Observations'. Can you specify in which context here, and provide references.

- I70 As the ice nuclei recycling process is at the core of the study, it should appear somewhere in the objectives.

-I85 'are purely a function of temperature'. Should RH be taken into account as well ? Please comment on this.

Figure 1 : please add the contour label for mixing ratios on the plot. I would also appreciate if the authors add the vertical profiles of temperature and humidity corresponding to the sensitivity tests (in Table 1).

- I95 : It is not clear to me if collisional processes are parameterized and taken into account in the simulations presented in the paper or not. Can you clarify please. Same question for SIP.

- I114 : 'in liquid supersaturated conditions' : do you mean in presence of SLW droplets ?

-I120 : Can you explain here what happens when ice crystals totally sublime (→ dry IN).

- I129 : 'we also discovered...' I would rather introduce these simulations to address a specific research question on the re-emission of INPs.

L 124-135 : There is something not very clear to me. As you explain very well further in the manuscript, this is not the IN recycling process (from turbulence within and below the sublimation layer), but a re-emission process. Can you clarify how does this intend to mimic or not a process that actually occurs in Arctic MPCs. When biogenic-origin INP fall down to the sea-ice surface for instance, which process are responsible for their re-emission up to cloud altitude ? I am familiar with the emission of CCN through surface-snow erosion and blowing snow from the sea-ice, but not INPs.

Table 1 : The WEAKINV experiment consists in a colder free troposphere but how humidity is prescribed in this case. DO you consider a constant mixing ratio or a constant RH wrt the CONTROL experiment (this is near the end of the paper, but this should be explained much earlier).

L137 : 'very little vertical motion' : this is not shown/illustrated.

Figure 4b and text about TKE : is TKE the total TKE or only the subgrid-scale one ?

Section 3.1 : I read very carefully this section and I have nothing to say except it is very clear and well illustrated.

Figure 7.a : Can you use more distinct colors for the cloud base and cloud top nucleation rates ?

L235 : As the flux of IN flux (entrainment) from the free troposphere is weak. This is related to one of my major comment above. Such flux should be quantified and the sensitivity to the free tropospheric IN concentration assessed.

L256 : 'eventually becomes larger' : well, I am not very convinced. I would rather say they are quite similar in magnitude.

Figure 9c and 11c : I would rather show TEST-CONTROL

L434 : When the mixing layer reaches the surface, one may expect an enhanced vertical transport of surface-emitted INPs in Arctic MPCs. Can you comment on that ?

L450 : 'higher final IWP' : well, sure of that ? IWP are overall quite similar.

L484 : 'perfectly efficient' : what does that mean ?

L489 and 490 : please cite the recent Vignon et al. 2026 which clearly illustrated this point :
Vignon, É., Raillard, L., Borella, A., Rivière, G., and Madeleine, J.-B.: Modeling the coupled and decoupled states of polar boundary-layer mixed-phase clouds, *Atmos. Chem. Phys.*, 26, 1847–1865, <https://doi.org/10.5194/acp-26-1847-2026>, 2026.

- There are too many figures in Appendix and I would recommend the authors to include some in the main manuscript to facilitate the reading. To my opinion, Fig A1, A4 and A5 are sufficiently important to be included in the main text.