

Response to Reviewer Comments on “Impacts of Environmental Conditions and Ice Nuclei Recycling on Arctic Mixed-Phase Cloud Properties”

Response to Reviewer 1:

We would first like to thank the reviewer for offering their initial thoughts on our manuscript. Their valuable comments and suggestions are appreciated during the revision process.

In. this study, different sensitivity experiments are run for Arctic stratocumulus clouds at a temperature of -13-15C. Simulations are run using a single-layer mixed-phase Arctic stratocumulus cloud field with the System for Atmospheric Modeling, applied to the ISDAC field campaign.

Going back to the McFarquhar (2011) and other ISDAC studies, I extracted the following. Measurements of both size-resolved and bulk cloud parameters were made from 20 instruments. Aerosol size, composition, concentration, morphology, and optical and nucleating properties were measured. A continuous flow diffusion chamber (CFDC) measured IN concentrations.

Here’s the assumption of your IN composition and activity. The probability of a given Super Droplet freezing is calculated using a parameterization based on the behavior of SNOWMAX bacteria, which become active INP at approximately -5C and maintain approximately the same ice nucleating activity between -10 and -20C. The IN activity/composition measurements from ISDAC should have been used in your simulations. I’m concerned about how the IN were activated in cloud. They are all activated at the same time, one hour after cloud initialization. This is very unrealistic as there is a range of activation of the IN, even at a given temperature. Some of the ISDAC in-situ measurements could have been used for this purpose.

The reviewer raised concerns with our use of SnowMax INP instead of observed INP from the ISDAC campaign. The reviewer quite correctly notes that detailed information on INP size, number concentration, and freezing activity was collected during this campaign. Hirunama et al. (2013) gives a good overview of aerosol and INP concentrations during 26-27 April 2008, the time period which we simulate. Savre et al. (2015) also describes in some detail the IN composition

and number concentration during this time period. As both studies note, the INP population was dominated by dust, not biogenic aerosol. This has a very different temperature-dependent freezing activity relationship than SnowMax bacteria, and one could quite reasonably ask why we didn't use their data.

In this study, we did not examine the temperature-dependent freezing behavior of INP, but rather their presence or absence and depletion through precipitation to the surface. For our purposes, it was sufficient to have simulated particles which could nucleate as ice upon reaching subfreezing and liquid-supersaturated conditions (simulating immersion/condensational nucleation). The ingestion of these particles into the cloud from below increased the number of ice particles in the cloud, while their removal through precipitation limited the number of INP available for future ice formation. We tried to constrain our simulations to have a reasonable cloud ice crystal number concentration, rather than exactly reproduce INP properties. Though our ice crystal number concentrations (INC) are somewhat lower than observed (approximately 0.16 L^{-1} between hours 2 and 8, compared to observed INC of 0.35 L^{-1}), our values actually agree quite well with those of Savre et al. (2015), who used much more detailed ice nucleation parameterizations with Classical Nucleation Theory and contact angles which varied with time. Average modeled ice crystal number concentrations during Research Flight 31 (corresponding to 27 April 2008) were approximately 0.18 L^{-1} in their simulations (or 0.15 L^{-1} in simulations using a different nucleation parameterization).

We should emphasize that the approach used in this study would not be appropriate for deeper cloud systems with a wide range of temperatures, which would drastically affect the nucleation probability of different IN. However, the single-layer cloud simulated here is quite shallow with a narrow temperature range. The minimum and maximum in-cloud (defined where cloud water vapor mixing ratio is $> 0.01 \text{ g/kg}$) temperatures are -15.52 and -11.78 Celsius, respectively. However, even this overestimates the temperature range within the cloud. The mean temperature is -14.07 Celsius, with 5th and 95th-percentile temperatures of -14.91 and -13.27 Celsius, respectively. This means that 90% of the cloud experiences temperatures within a temperature range of 1.64 degrees Celsius. Examining the dust parameterization of Niemand et al. (2012), this results in an ice nucleation active site (INAS) density and INP frozen fraction which varies by a factor of approximately 2.33 across the cloud. While this

sounds large, it is significantly less than the two orders of magnitude difference in INAS density which have been found at the *same temperature* between different examinations of the highly-standardized Arizona Test Dust (Hoose and Möhler, 2012).

Finally, we did attempt to run a simulation with the Niemand et al. (2012) dust parameterization. However, this produced only tiny quantities of ice, far below that recorded or modeled in other studies (Ovchinnikov et al., 2014; Savre et al., 2015, Solomon et al., 2015). This is due to a current limitation of our Lagrangian model. The model uses “superparticles”, large collections of particles with identical properties such as mass, aspect ratio, aerosol mass, etc., which are tracked throughout the simulation. This poses a problem for ice nucleation with inefficient ice nuclei such as dust. According to the Niemand et al. (2012) parameterization, at -14.07 Celsius, we expect a frozen fraction of monodispersive 0.7 μm -diameter dust particles of $1.7\text{e-}5$. This means that each superparticle has only a 0.0017% percent chance of being frozen at a given temperature (the actual process in our model is slightly different, but the end results are similar). With 100 superparticles per gridbox, 20 of which are classified as “IN-containing”, we would only expect one ice particle for every 2970 gridboxes! This is obviously wholly inadequate for modeling a mixed-phase cloud.

To summarize, while our parameterization of ice nucleation was simplistic, it fit the purposes of our study, where we were largely interested in whether IN were available to nucleate ice within the cloud, rather than their exact temperature-dependent freezing behavior. This was acceptable for a shallow, single-layer cloud with a narrow temperature range. However, future modeling work involving deeper clouds with much larger temperature ranges will use a different approach to ice nucleation.

The reviewer also voiced concern that all IN were activated at the same time, 1 hour after the start of the simulation. This was done to allow radiative cooling from a warm-phase cloud to produce a strong circulation within the decoupled boundary layer, producing a source of supersaturation before ice was initialized. We worried that initializing ice at the start of the simulation would cause rapid glaciation before sufficient supersaturation production from radiative cooling and updrafts was present. We also did this to emulate the methodology of

Ovchinnikov et al. (2014), who initialized ice two hours after the start of the simulation. We determined that 1 hour spin-up time was sufficient for our simulation. The formation of a “pulse” of ice at 1 hour is the result of allowing (nearly) instant nucleation at this time. However, a similar “pulse” occurs when ice formation is allowed from the very beginning of the simulation (visible in Figure R1, where ICESTART indicates the simulation in which ice formation is allowed from the beginning).

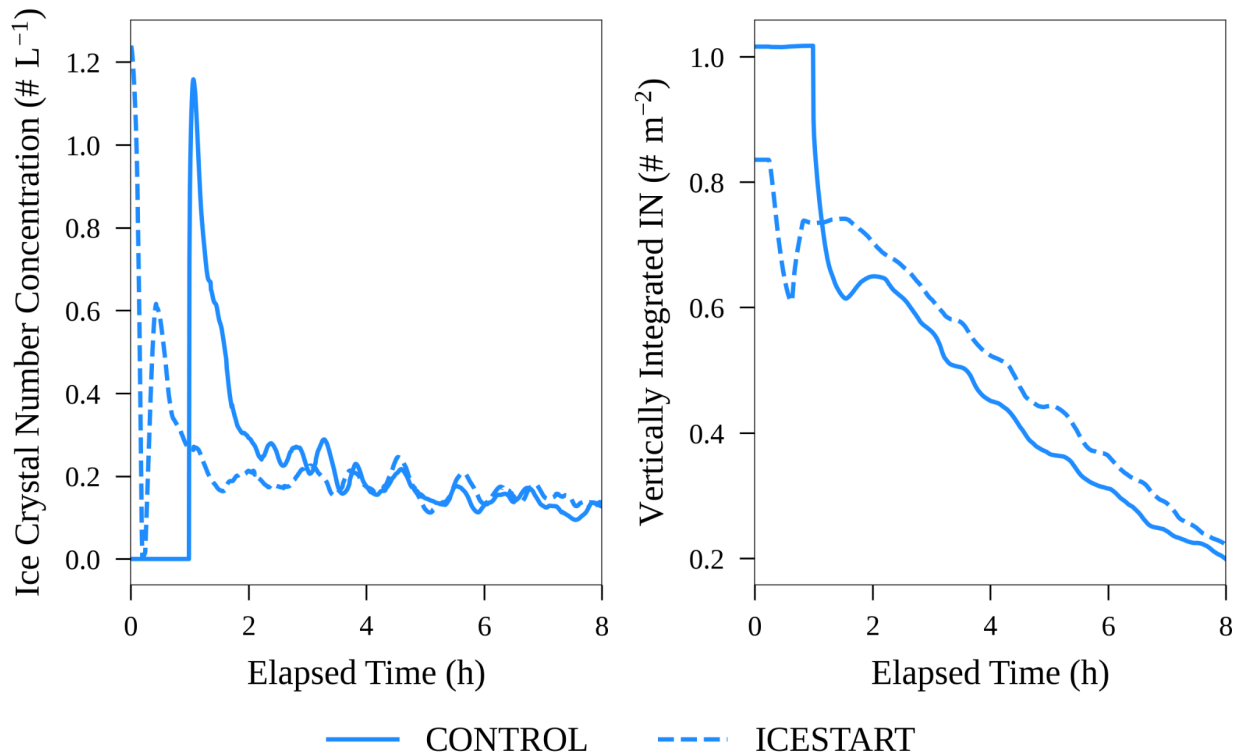


Figure R1: Time series of in-cloud average ice crystal number concentration (left) and vertically integrated ice nuclei number concentration below cloud (right).

I would have liked to see plots of the air vertical velocity with your different cases. These are the main comments I have now, more to come.

We’ve attached time-height plots showing the square root of vertical velocity variance for all simulations (Figure R2). Though there is weak background subsidence (approximately 4 mm/s above the inversion layer), this field is a good

approximation for the average absolute vertical velocity within the domain. Please let us know if this is what was meant, or if the reviewer would prefer X-Z vertical cross-sections, or something else.

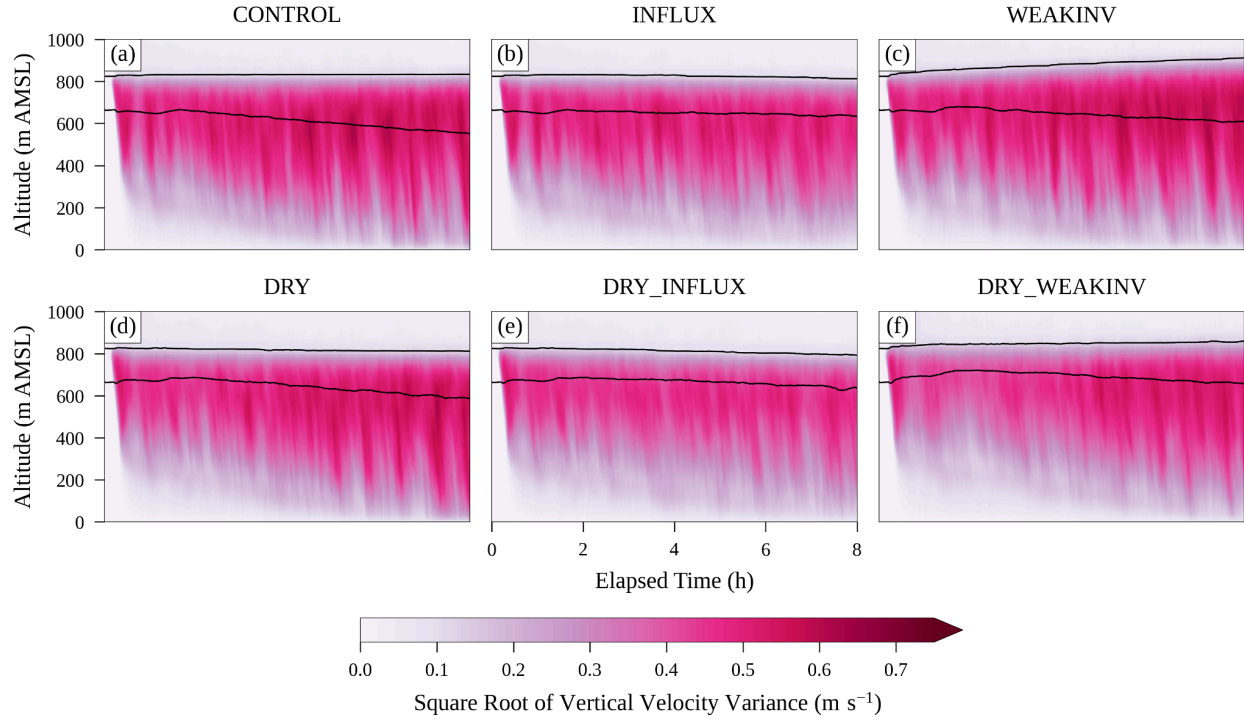


Figure R2: Time-height plots of the square root of vertical velocity variance for all six simulations. Vertical velocity variance is indicated in shading, while cloud boundaries (defined as regions where cloud water mixing ratio is > 0.01 g/kg) are marked in black lines.

Response to Reviewer 2

We would like to thank the second anonymous reviewer for the time they spend reading our manuscript, and their insightful comments, which we very much appreciate as we work to improve the manuscript before publication.

We will first address the reviewer's major comments before moving to point-by-point responses.

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.

The reviewer raised concerns regarding the limited domain size of our simulations. This is a valid concern, given that stratocumulus clouds typically have large aspect ratios and would therefore be much larger than modeled in our simulations. We ran a simulation with a 5.12 km x 5.12 km by 1.4km domain as a comparison. Unfortunately, due to long queue times and repeated maintenance cycles on the supercomputer used for these simulations, we were only able to run the large-domain simulation for 5 hours compared to the full 8-hour simulation used in the other cases. However, over the 5 hours, we do see that the evolution of the two simulations, in terms of liquid water path, ice water path, cloud-top longwave radiative cooling, and vertically integrated TKE is similar. The larger-domain simulation does have a slightly lower IWP and cloud-top longwave radiative cooling rate, and slightly larger TKE than the smaller-domain simulation, as seen in Figure R3. However, the size of updrafts and downdrafts near cloud base are similar, as seen in Figure R4.

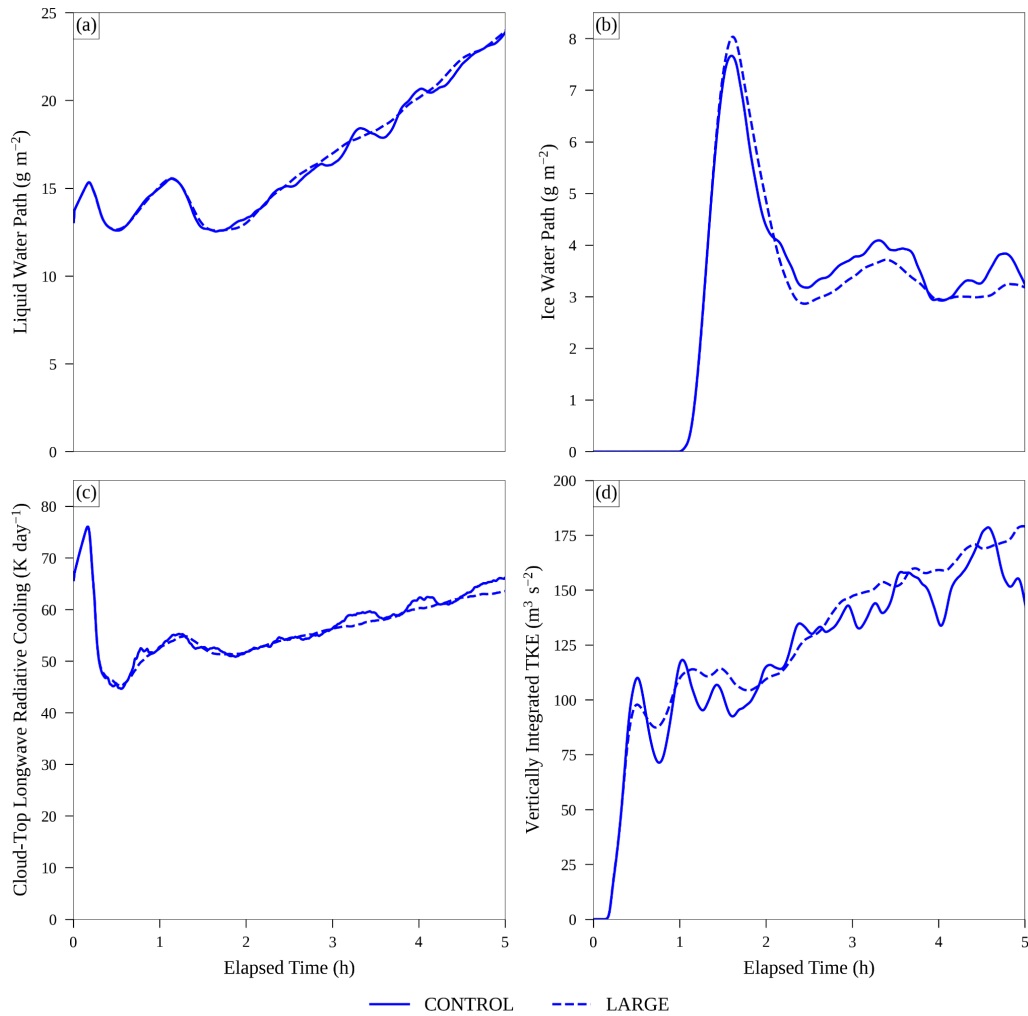


Figure R3. A time series comparison of CONTROL (solid blue line) and LARGEDOMAIN (dashed blue line) simulation quantities: (a) - vertically integrated cloud water, (b) - vertically-integrated ice, (c) - cloud-top radiative cooling rate, and (d) - vertically-integrated TKE

Plan View of Vertical Velocity and Cloud Ice Mixing Ratio at $z = 500$ m and $t = 5$ Hours

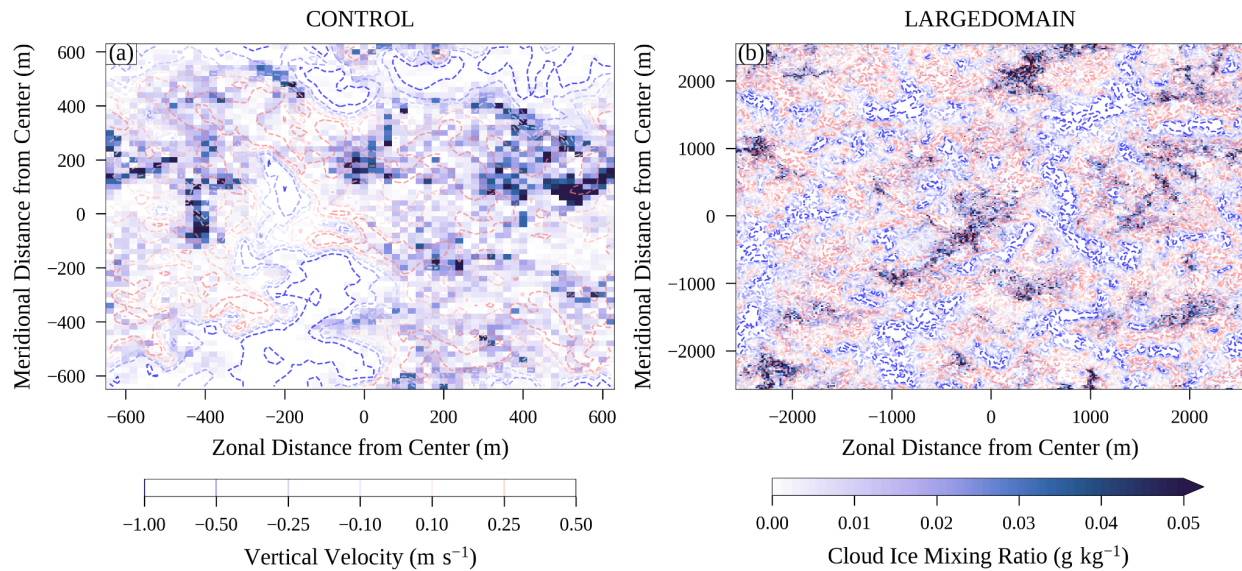


Figure R4. A plan view comparison of CONTROL (a) and LARGEDOMAIN (b) simulations at $z = 500$ m and $t = 5$ hours. Vertical velocity is indicated in the red and blue dashed contours, while cloud ice mixing ratio is indicated in the blue shading. Note the difference in scale between panels a and b.

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 fluxed 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.

We thank the reviewer for catching this error of omission. Surface fluxes are indeed the main drivers of Arctic stratocumulus under some circumstances, such as CAOs. We have updated our introduction to acknowledge the distinction between these clouds and decoupled Arctic stratocumulus clouds.

Lines 29-34 now read as follows (green indicates changed text; blue is unchanged): While many stratocumulus clouds in the Arctic are maintained by surface fluxes, particularly under Cold-Air Outbreak conditions, those Arctic stratocumulus clouds which form atop decoupled boundary layers are typically maintained by cloud-top radiative cooling (de Boer et al., 2009; Griesche et al., 2021; Klein and Coauthors, 2009; McFarquhar et al., 2011; Sotiropoulou et al.,

2014). Since these Arctic stratocumulus clouds atop decoupled boundary layers are maintained by radiative cooling at the cloud top, the reduction in LWP due to the WBF process, and concomitant reduction in cloud-top radiative cooling, can weaken the boundary-layer circulations which maintain the cloud.

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.

Regarding the absence of experiments to test the sensitivity to free-tropospheric IN, we did not run simulations to examine this effect because the strong capping inversion, and associated weak cloud-top entrainment rate, limited the flux of IN from the free troposphere into the cloud and sub-cloud boundary layer in the CONTROL simulation. Therefore, we expected that an increased free-tropospheric IN concentration would have little impact, since the low entrainment rate would prevent the cloud from accessing this increased IN number concentration.

However, to investigate this more thoroughly in response to the second reviewer's question, we ran a simulation with free-tropospheric (above 835 m) IN concentrations of 5.6 L^{-1} . This results in an in-cloud mean ice crystal number concentration between hours 2 and 8 of 0.21 L^{-1} , compared with 0.17 L^{-1} in CONTROL. While notable, this 22.5% increase in IN number concentration with a 460% increase in free-tropospheric ice crystal number concentration is less than the 44.1% increase in in-cloud ice crystal number concentration between DRY and CONTROL.

Minor Comments

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

We agree, and have changed the title to be “**Impact of free-tropospheric thermodynamics and ice nuclei recycling on Arctic mixed-phase cloud properties**”

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

We have corrected this to refer to them as A1, A2, etc.

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

We are not sure that we should go into much detail about LES in particular. We do mention on Lines 44-48 that Arctic stratocumulus have been the subject of numerous modeling studies, including with LES.

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

Agreed. We have added “**shattering of cloud droplets upon heterogeneous freezing**” to our list of SIP mechanisms. Interestingly, however, it seems to be mainly important in the summer, where cloud top temperatures are often >-10 Celsius. In winter or springtime Arctic stratocumulus such as ISDAC, it may play less of a role (Fu et al., 2019; Sotiropoulou et al., 2020).

- I56 ‘Observations’. Can you specify in which context here, and provide references.

“**Observations**” has been replaced with “**Both in-situ aircraft measurements and remote sensing**”. References are at the end of the sentence in the original manuscript, and will be repeated here: (de Boer et al., 2009; Griesche et al., 2021; McFarquhar et al., 2007, 2011; Mioche et al., 2017; Shupe et al., 2006; Verlinde et al., 2007)

-170 As the ice nuclei recycling process is at the core of the study, it should appear somewhere in the objectives.

The strong impacts on IN recycling were unanticipated, and therefore were not part of our objectives. Indeed, we had expected entrainment of free-tropospheric IN to be the dominant source of IN for ice nucleation, and that modification of the boundary layer would lead to changes in cloud-top entrainment which modified cloud-top nucleation rates. The fact that cloud-top nucleation was relatively insignificant in all of our simulations, however, meant that we did not focus on this process much. Despite how much of our paper is focused on IN recycling, that was not actually one of our initial objectives, and we would rather leave them as is.

-185 'are purely a function of temperature'. Should RH be taken into account as well? Please comment on this.

Relative humidity *should* be accounted for in the IGF. But it isn't, and that's one of the limitations of the IGF. We did not include it in our final manuscript, but we also conducted simulations with a different parameterization, known as DiSKICE, which uses attachment kinetics to model ice growth (Harrington et al., 2019). In that parameterization, habit development is a function of temperature, supersaturation over ice, and ambient pressure. While our results were broadly similar, it did produce ice crystals which were slightly smaller and more isomorphic compared to the IGF-based parameterization.

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).

We have added plots of temperature in WEAKINV and of dewpoint in DRY (dewpoint is unchanged in WEAKINV and temperature is unchanged in DRY). We have also added labels for the mixing ratio on the x-axis. Hopefully it's not too cluttered now. We have updated the figure caption to reflect the new plot.

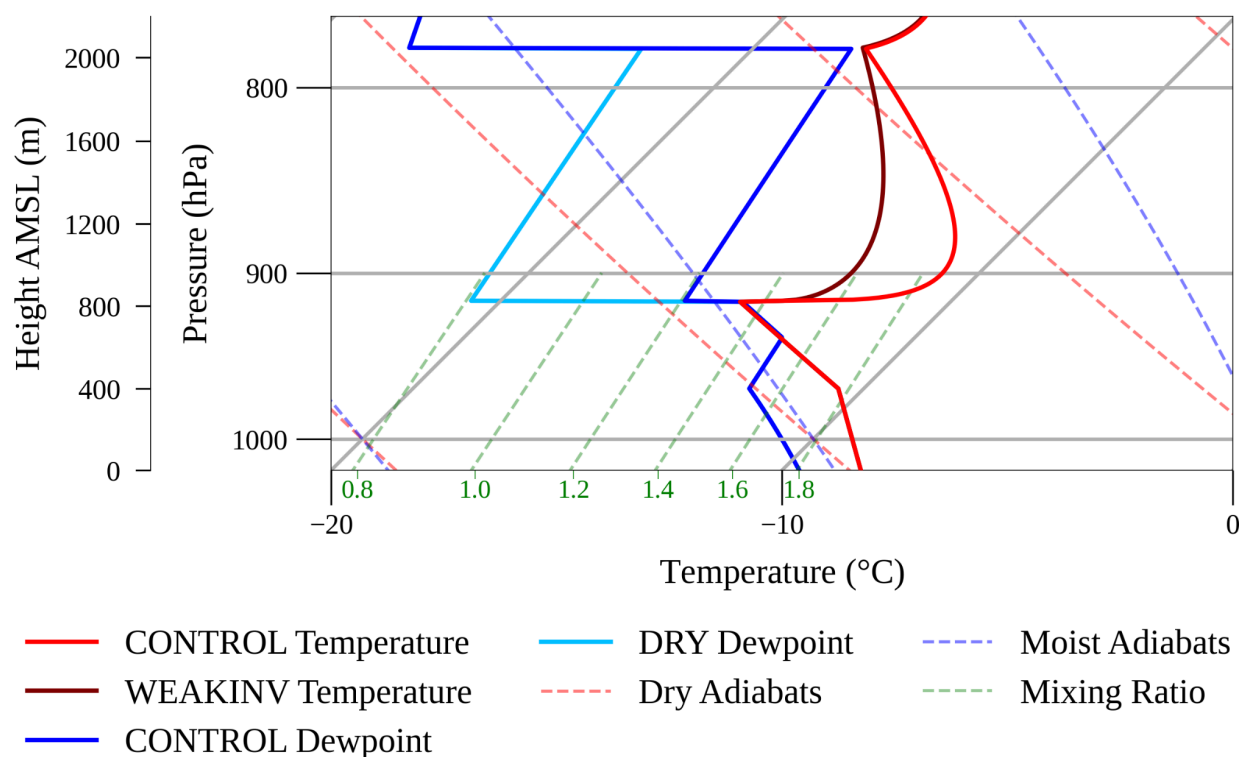


Figure R5 (New Figure 1 in paper): Initial thermodynamic conditions of the CONTROL simulation. Initial temperatures of the CONTROL and WEAKINV simulations are indicated by the red and maroon solid lines, respectively. Initial dewpoints of CONTROL and DRY are indicated in blue and light blue solid lines, respectively. Dry and moist adiabats are indicated by the red and blue dashed lines, respectively. Isolines of constant water vapor mixing ratio are depicted from 0.8 g kg⁻¹ to 1.6 g kg⁻¹, at intervals of 0.2 g kg⁻¹, in the green dashed lines.

- 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.

We appreciate the reviewer pointing this out; we should have been more specific. No, collisional processes are not included at all in the simulations, for either liquid or ice. That has been added to lines 101-103: Drizzle was also absent, indicating a lack of collisional growth among cloud droplets (Ovchinnikov et al., 2014). Therefore, droplet collisional growth, riming, aggregation, and secondary ice production were disabled in our simulations.

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

No, we mean conditions which have a relative humidity of greater than 100% with respect to liquid and which are at less than 0 Celsius. Since we’re using Lagrangian particles, one could theoretically have a situation where there is a grid box at liquid-supersaturated conditions but no CCN, which an IN-containing particle is advected into. Then this IN would nucleate (approximating immersion/condensation freezing). In practice, this doesn’t happen in our simulation since we have approximately 100 superparticles per gridbox.

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

When an ice crystal sublimates, its superparticle type is set to “liquid” rather than “ice.” This means that condensational/evaporational, rather than depositional/sublimational, growth will be calculated for the superparticle . It also means that the superparticle loses its habit and becomes spherical. So if this superparticle were to be advected into an updraft again, it would initially undergo condensational (haze) growth before freezing. This matters much more in simulations of deep convection with less-efficient IN (ones which nucleate around -25 Celsius). There, an IN-containing superparticle can nucleate ice at that temperature, fall out, sublime, convert back to “liquid”, be advected back into the cloud, grow as a cloud droplet, and potentially fall out as warm rain due to collision-coalescence before reaching its freezing temperature again. Here, it mainly helps us keep track of which superparticles have IN and are ice crystals, versus which are “dry IN.” (In colder clouds, one can also have superparticles which are frozen without IN, but those don’t occur here since there’s no homogeneous freezing).

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

We have rephrased lines 142-144 to read “~~Therefore,~~ To examine the impact of perfectly-efficient IN recycling on cloud microphysical and boundary-layer evolution, we ran an additional two simulations, one with CONTROL environmental conditions and one with DRY conditions, in which any particle

which reached the surface was re-injected into the below-cloud boundary layer as a dry CCN/IN particle.”

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.

Great question. This is *not* intended to reflect a real process in the atmosphere. As the reviewer mentions, blowing snow can introduce CCN and ice fragments (though maybe not IN) into a cloud. This was not reported during ISDAC Flight 31, and would seem to be unlikely to occur with the weak surface wind speeds of ~3.2 m/s. According to Eq. 4 of Chen et al. (2022), one would only expect this to happen with a wind speed of approximately 8.2 m/s given our surface temperatures of approximately -8.3 Celsius. In the late springtime, summertime, and autumnal Arctic, leads of open water can serve as a source of IN, but that does not apply to our simulations over sea ice (Creamean et al., 2022). These simulations were done as a counterfactual, to prove our earlier finding that dry-air entrainment does not enhance ice growth through an accelerated WBF process as postulated in Hoffmann (2020). By controlling for (or rather, eliminating) differences in IN-recycling between CONTROL and DRY, we could show that DRY_INFLUX had lower IWP than DRY as a result of reduced depositional growth and smaller ice crystals. However, the results of these simulations were also interesting enough to warrant further analysis.

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).

Water vapor mixing ratio is held constant. As temperature above the cloud is lower in WEAKINV, this leads to a greater free-tropospheric relative humidity. This is easier to see in Figure R5 (revised Figure 1). We have also added the

following statement on lines 139-140: “Note that in WEAKINV, above-cloud water vapor mixing ratio is unchanged, meaning that the above-cloud relative humidity increases relative to CONTROL due to the reduced inversion strength.”

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

See Figure R2. As both reviewers have commented on the need to show vertical motion, we will add Figure R2 as a supplemental figure.

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

This is total TKE.

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

We’ve changed the colors in the left-hand panel to be blue and green, shown below.

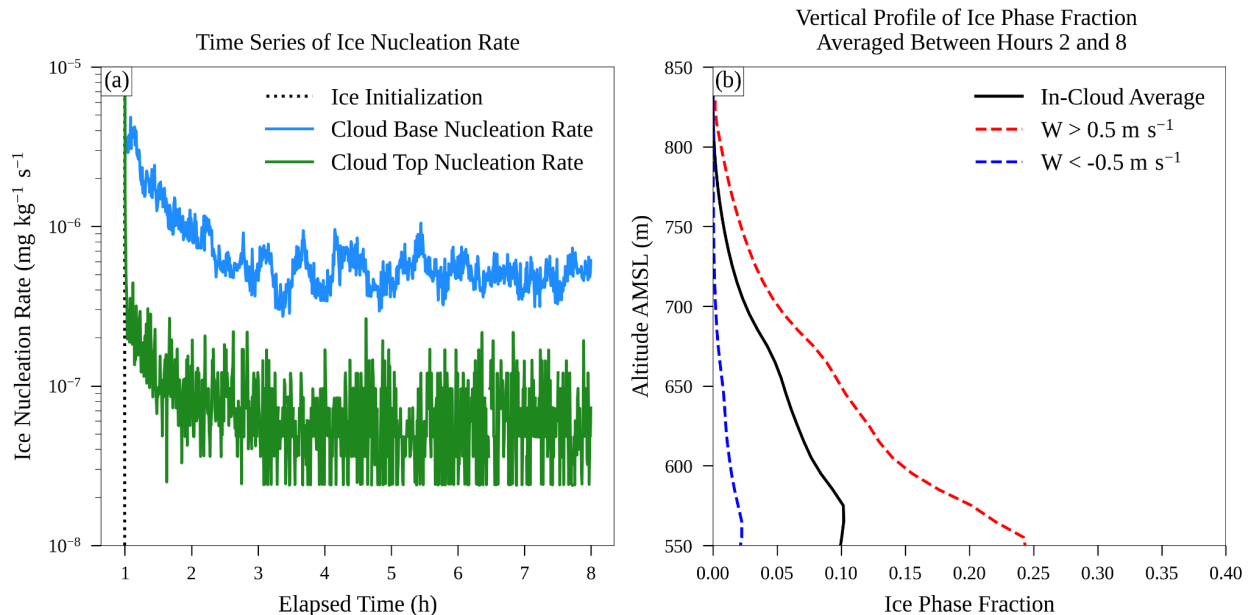


Figure R6 (Revised Figure 7): (a) A time series of heterogeneous ice nucleation rate at cloud base (blue) and cloud top (green) in the CONTROL simulation. Ice initiation at $t = 1$ hour is denoted by the black vertical dotted line. (b) A time-average vertical profile of

ice phase fraction in updrafts (red, dashed), downdrafts (blue, dashed), and over the entire cloud (black, solid) between hours 2 and 8 in CONTROL.

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.

As mentioned above, we ran a simulation with free-tropospheric IN concentrations increased by a factor of 5.6, and saw only a modest increase in in-cloud ice crystal number concentration. We calculated that the IN flux at the top of the cloud in this simulation was 5.66 particles/m²/s, compared to 1.16 particles/m²/s in CONTROL. While this sounds like a large increase, it is an order of magnitude less than the cloud-base IN flux of 48.7 particles/m²/s in CONTROL and 56.1 particles/m²/s in the high-IN simulation. This is primarily due to the strong capping inversion and weak cloud-top entrainment rates in our simulation which limit the flux of IN from the free troposphere into the cloud and sub-cloud boundary layer.

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

Between hours 2 and 5, average IWP in CONTROL is 3.56 g/m², compared to 2.87 g/m² in DRY. Between hours 5 and 8, however, average IWP is 2.73 g/m² in CONTROL compared to 3.65 g/m² in DRY. Looking at the lines for CONTROL and DRY (blue and orange) in Figure 2b, the difference seems notable to us. We've added the following on lines 249-251 to make this clearer in the paper:

Between $t = 2-5$ hours, average IWP in CONTROL is $\sim 3.65 \text{ g m}^{-2}$, compared to $\sim 2.87 \text{ g m}^{-2}$ in DRY. However, between $t = 5-8$ hours, average IWP in CONTROL has fallen to $\sim 2.73 \text{ g m}^{-2}$ but has risen to 3.65 g m^{-2} in DRY.

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

Figures 9c and 11c: Agreed. We have modified these figures and Figure 5 and included the new ones below.

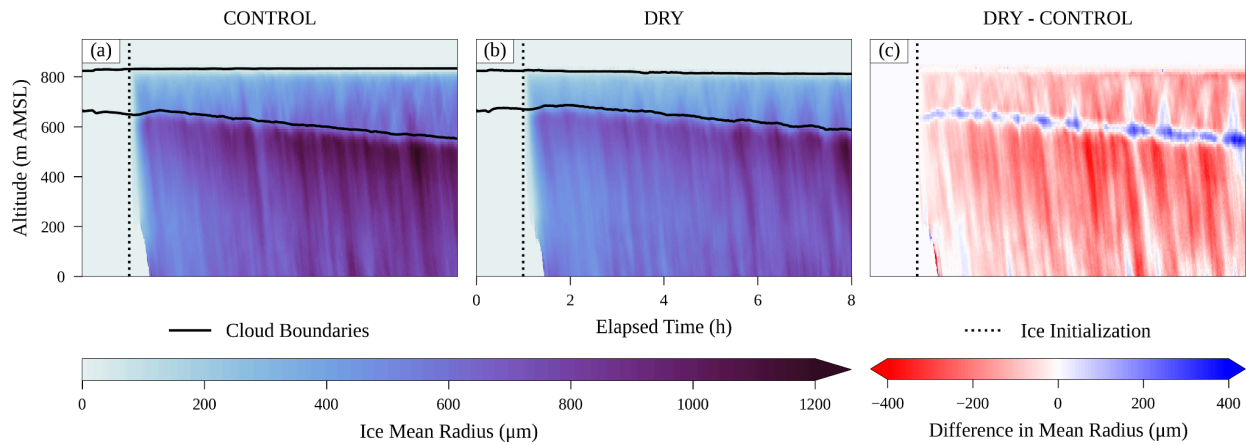


Figure R7 (Revised Figure 5): A time-height comparison of mean ice crystal radius (measured along the maximum particle dimension). (a,b) display radius in blue-purple shading, while (c) shows the difference in radius between DRY and CONTROL in red and blue colors. Cloud boundaries are shown in solid black lines in (a) and (b). Ice initiation at 1 hour is denoted by the vertical black dotted line.

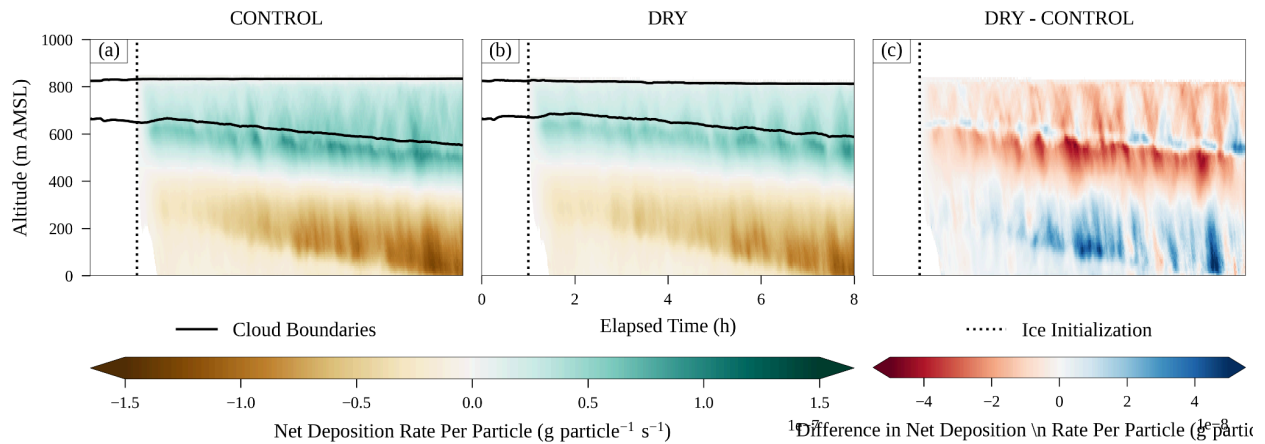


Figure R8 (Revised Figure 9): A time-height comparison of net deposition rate per particle. Deposition rates per particle are shown in green and brown shading in (a,b), while differences between DRY and CONTROL are shown in blue and red shading in (c). Cloud contours for respective simulations are indicated by solid black lines in (a,b), while the initiation of ice processes at $t = 1$ hour is shown by the vertical dotted black lines.

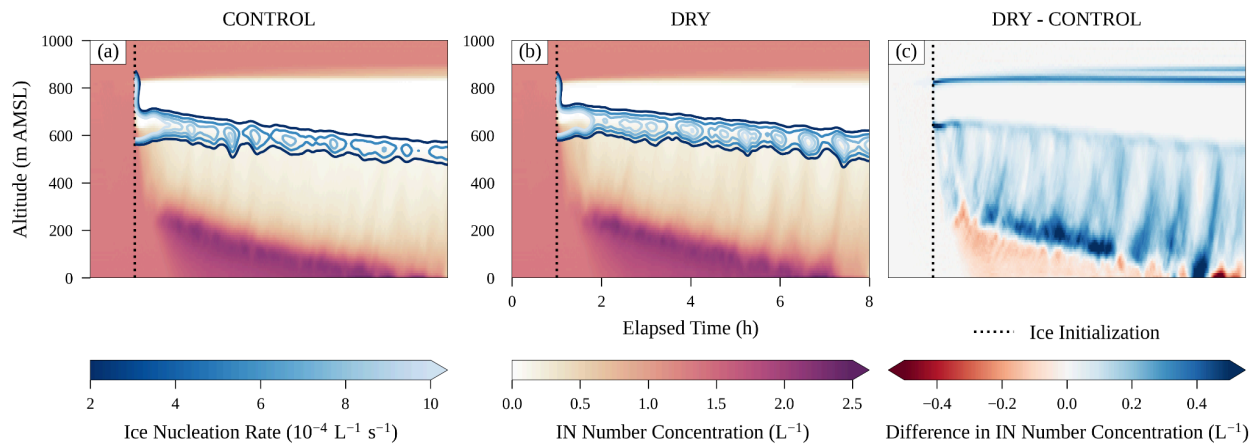


Figure R9 (Revised Figure 11): A time-height comparison of IN number concentration and nucleation rate. (a,b) show IN number concentration in pink-purple shading and ice nucleation rate in blue contours. The contour range is from 2×10^{-4} particle L^{-1} to 1×10^{-3} particle L^{-1} with a contour interval of 2×10^{-4} particle L^{-1} . Contours are smoothed in time with a gaussian kernel with a 2.5 minute standard deviation for clarity. (c) shows the difference in IN number concentration between DRY and CONTROL in red and blue shading.

1434 : 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 ?

That's quite likely. In our simulations, ice crystals which desiccate below cloud leave behind IN. When this desiccation occurs beneath the mixed layer (in the near-surface stable layer), the IN accumulate. As the mixed layer deepens beneath the cloud, however, they are continuously mixed into the sub-cloud boundary layer and the cloud itself, fostering further new nucleation and ice formation within the cloud. In the scenario proposed by the reviewer, a similar process would occur with marine IN emissions. While the boundary layer is still uncoupled, IN emissions would lead to an accumulation of IN near the surface. However, once the boundary layer becomes coupled, these IN would be rapidly transported into the cloud through updrafts. The rate at which this occurs would depend on the local TKE, but could theoretically exceed the rate of emission of IN from the surface, resulting in a temporary surge of nucleation and in-cloud ice crystal number concentration. If strong enough, this could theoretically glaci

and destabilize the cloud. If this doesn't occur, then it is likely that ice number concentration would subsequently drop due to sedimentation and the incorporation of IN from the cloud at the same rate as they are emitted from the ocean surface.

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

See our response to the comment on line 256.

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

This means that 100% of IN are returned to the sub-cloud boundary layer rather than removed through precipitation to the surface. We have added the following sentences on lines 145-147 to make this point more clearly: Note that in this manuscript, the "efficiency" of IN recycling refers to the fraction of ice crystals which return their IN to the boundary layer through sublimation rather than precipitate to the surface. "Perfectly efficient" IN recycling thus indicates that all IN are returned to the boundary layer, rather than removed through precipitation.

L489 and 490 : please cite the recent Vignon et al. 2026 which clearly illustrated this point.

We have added a citation to Vignon et al. (2026) which also supports this point through their analysis of the ISDAC case.

- 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.

Supplemental Figures: We agree that Figures A4 and A5 could be included in the text. However, we do not spend much time discussing particle aspect ratio in this paper. We would therefore prefer to leave Figure A1 as is.

Other Changes

In addition to changes in response to reviewer comments, we have also made the following changes to our manuscript to better illustrate certain key points.

Lines 103-106: This particular cloud has been examined extensively by other modeling studies, making it a good case to simulate as our results can be compared with those of previous work (Ahola et al., 2020; Das et al., 2025; Fu and Xue, 2017; Paukert and Hoose, 2014; Raatikainen et al., 2022; Savre and Ekman, 2015; Sulia and Kumjian, 2017).

Lines 148-151: Finally, to assess the sensitivity of cloud properties to increased free-tropospheric IN number concentration, we conducted a simulation, HIGHIN, in which initial IN number concentrations above the inversion layer are increased by a factor of 5.6 (reflecting the high-IN conditions in Hiranuma et al. (2013)). The results of HIGHIN are discussed very briefly in section 3.2, but not examined in detail. Table 1 illustrates the full set of simulations.

Lines 166-167: As the ice crystals grow larger, they begin to sediment out of the cloud, resulting in a rapid decrease in ice crystal number concentration within the cloud. Indeed, by $t = 2.3$ hours, the average ice crystal number concentration within the cloud has plummeted to only $\sim 0.2 \text{ L}^{-1}$ (Fig. 3a), a value which is somewhat below the observed ice crystal number concentration of $\sim 0.35 \text{ L}^{-1}$ (Savre and Ekman, 2015).

Lines 186-188: The cloud itself also deepens during this time; though cloud top height remains nearly constant due to cloud-top entrainment balancing large-scale subsidence, the cloud base sinks from ~ 659 m at $t = 2$ hours to ~ 551 m at $t = 8$ hours (Figs. 5, 6a).

Lines 238-239: This reduction in LWP is due to the entrainment of drier above-cloud air than in CONTROL, reducing cloud depth and condensational growth within the cloud (Figs. 5b, 10a,c).

Lines 284-289: Notably, the increase in in-cloud ice crystal number concentration in DRY is much greater than that in HIGHIN. Between hours 2 and 8, average in-cloud ice crystal number concentration in HIGHIN is $\sim 18.1\%$ greater than that in CONTROL, compared with a $\sim 44.1\%$ increase in DRY (Fig. A4). This indicates that the strong cloud-top inversion prevents the overturning circulation at the top of the cloud in HIGHIN from accessing many of the IN in the free-troposphere, while the greater IN recycling below cloud base in DRY

increases the number of IN in the below-cloud mixed layer, which can be advected back into and nucleated within the cloud.

Lines 343-345: This reduction in LWP is driven by a reduction in both condensational growth and cloud depth compared to CONTROL, DRY, or WEAKINV. Between hours 2 and 8, the average in-cloud condensation rate in DRY_WEAKINV is ~ 17.3%, ~ 10.1%, and ~16.6% smaller than those in CONTROL, DRY, and WEAKINV, respectively (Figs. 13b, 10).

Lines 545-547: As we noted in the Introduction, several ice growth mechanisms which affect mixed-phase Arctic stratocumulus are not represented in these simulations, particularly riming, aggregation, and ~~SIP~~ice-ice collisions, meaning that the results of our simulations may not be exactly representative of real-world conditions.

Note: The model data from the HIGHIN simulation is currently in the process of being added to the OpenData LMU repository (it's currently under review). We have not added the LARGEDOMAIN time-height NetCDF to this repository as it's not discussed in the text of the paper. We are of course happy to provide all data from these simulations.

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