Author responses to reviewer comments

Sensitivities of a simulated Arctic stratocumulus to aerosol and microphysical parameters

(*Previously titled:* Cloud-phase sensitivities of a simulated Arctic stratocumulus to aerosol and microphysical parameters)

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We thank all three reviewers for the constructive feedback. Below, we copied the reviewers' comments in black and added our responses in blue. Respective changes in the main manuscript are added in italic font.

1 Response to reviewer 1

1.1 Review report

The authors simulate a case of Arctic Sc using the MIMICA model, perturbing several aerosol and cloud microphysical parameters to examine their impact to cloud macrophysical and radiative properties. The study addresses an important topic and is well-presented. I have a few concerns that the authors should address prior to publication.

1.2 Major concerns

1. Fixed aerosol (ll. 118-120) – What is the rationale behind fixing aerosol concentrations? It would be good if the authors could explain how strong of an assumption this is compared to a more realistic simulation using prognostic aerosol

concentrations. Assuming the authors tested it, would prognostic aerosol create swift aerosol depletion? And would that suggest missing sources in the model, such as replenishment via free-tropospheric entrainment?

We thank the reviewer for raising this important point regarding our aerosol treatment. Prescribing fixed aerosol concentrations was a configuration choice driven by both quantitative physical arguments and technical model setup considerations. First, for this specific case study, holding the aerosol population constant represents a robust assumption. Peak droplet activation in the baseline simulation reached 57.6×10^6 m^{-3} (Fig. 6b), drawn from a total aerosol concentration of 220.6×10^6 m^{-3}. This leaves a large unactivated aerosol reservoir of 163×10^6 m^{-3} in the cloud layer. Furthermore, cumulative surface precipitation remained below 0.1 mm, meaning that wet deposition scavenging was negligible over the 12 h period. Consequently, activation and precipitation scavenging would not have caused swift aerosol depletion even in a prognostic framework.

Second, in a fully prognostic framework, we would not expect rapid or unrealistic aerosol depletion. Full prognostic aerosol implementations in MIMICA include transport (such as turbulent entrainment from the free troposphere) and aerosol regeneration upon droplet evaporation, alongside activation and impaction scavenging. In persistent stratocumulus clouds with low precipitation rates, free-tropospheric entrainment and droplet evaporation continually replenish the activated aerosol pool, maintaining relatively stable aerosol concentrations.

Third, regarding our model configuration, the prognostic aerosol module was not fully operational for this specific mixed-phase setup in the utilized version of MIMICA. An alternative option would have been to prescribe the cloud droplet number concentration directly (similar to our treatment of ICNC). We deliberately chose the fixed aerosol approach instead, as it preserved the physical coupling between the aerosol population and cloud microphysics.

2. Evolution of thermodynamics – The authors present very little information on how temperature and humidity evolve during the simulation. I am a bit worried the simulations gloss over important temperature and moisture source terms that could substantially affect relative humidity profiles (wrt. water and ice) that are crucial for WBF process rates. Since there are 6-hourly radiosonde profiles, it would be good to show how simulations evolve compared to observations.

We agree that the thermodynamic evolution is an important aspect of the cloud development. Figure R1 shows the hourly thermodynamic profiles of the baseline simulation, plotted alongside the initial sounding and the two subsequent radiosonde profiles.

The main focus of this paper was to evaluate microphysical sensitivities using an idealized setup, rather than to perfectly replicate the temporal evolution of the cloud. As explained in Section 2.4.1 of the manuscript, the simulations ran without transient external forcing, relying solely on a constant divergence. Therefore, the simulated profiles naturally diverge from the subsequent radiosonde observations.

Despite this idealized forcing, both the simulations and the observations demonstrate a clear tendency toward an increasing boundary layer depth. The observations show a faster deepening and warming than the simulations, indicating external forcing mechanisms uncaptured by MIMICA. Most notably, the observations recorded a surface temperature increase of roughly 2 K (shown in Fig. 1f in the manuscript), whereas MIMICA maintained a constant surface temperature. Other observed variables presented in Fig. 1 also exhibited notable variance after the radiosonde launch at 05:35 UTC on 8 June. This shift in external conditions motivates our decision to restrict the evaluation period to the time window prior to this launch.

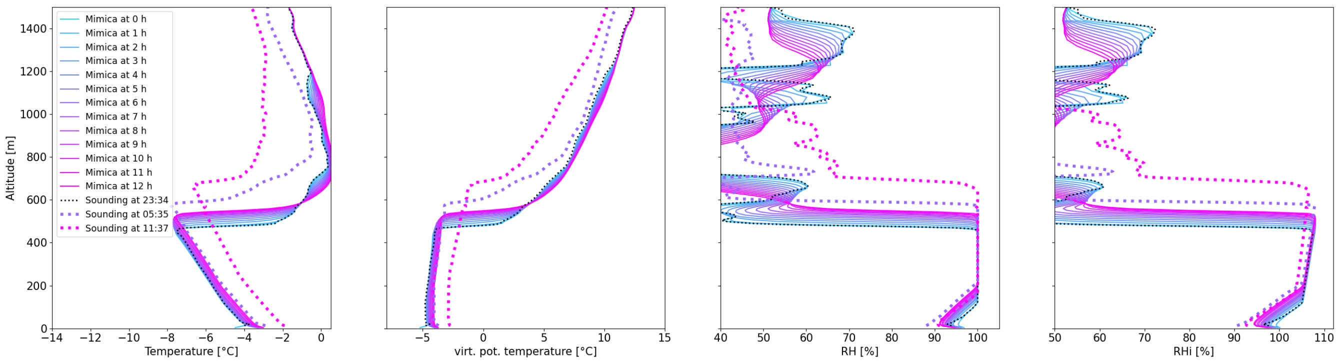

Figure R1. Horizontally averaged thermodynamic profiles of the baseline simulation (solid lines), and radiosonde profiles from 7 June 2023, 23:34 UTC (the initial profile for the simulation), 8 June 2023, 05:35 and 11:37 UTC (dotted lines).
Will make labels larger and maybe change to 2x2 plot.

3. Spinup in cloudy conditions – the author mention inhomogeneities imposed to the temperature field at simulation start. Since there is an immediate cloud field, how do these inhomogeneities translate into cloud properties and subsequent vertical wind fields? Would the results change if the spatial scales of the inhomogeneities were different? Since these are rather idle conditions, it would be good if the authors could explain how long their spin-up takes?

We thank the reviewer for this question regarding the initialization. We applied the imposed temperature perturbations of maximum ± 0.1 K exclusively within the lowest six model levels, reaching a maximum altitude of 41.2 m. This altitude remained well below the initial cloud base of 125 m, preventing the direct introduction of inhomogeneities into the cloud layer.
Rather than dictating the final cloud structure, these perturbations serve only to kick-start the turbulent boundary layer circulation and are a standard technical procedure in LES simulations. To demonstrate the insensitivity to the initial noise field, Fig. R2 presents the spin-up phase for four different realizations of the baseline simulation (using different random seeds). By the end of this 3 h period, the thermodynamic and microphysical variability among these ensemble members was very small.
Finally, to address the duration of the spin-up under these idle conditions, we added a sentence to Sect. 2.4.1. stating that the spin-up period took 3 h.

Addition in the middle of Sect. 2.4.1:

To accelerate the onset of turbulence, we perturbed the initial potential temperature profile with random noise of up to ±0.1 K across the lowest six model levels ***(i.e., up to 41.2 m). The first 3 h of simulation time are considered as spin-up period.***

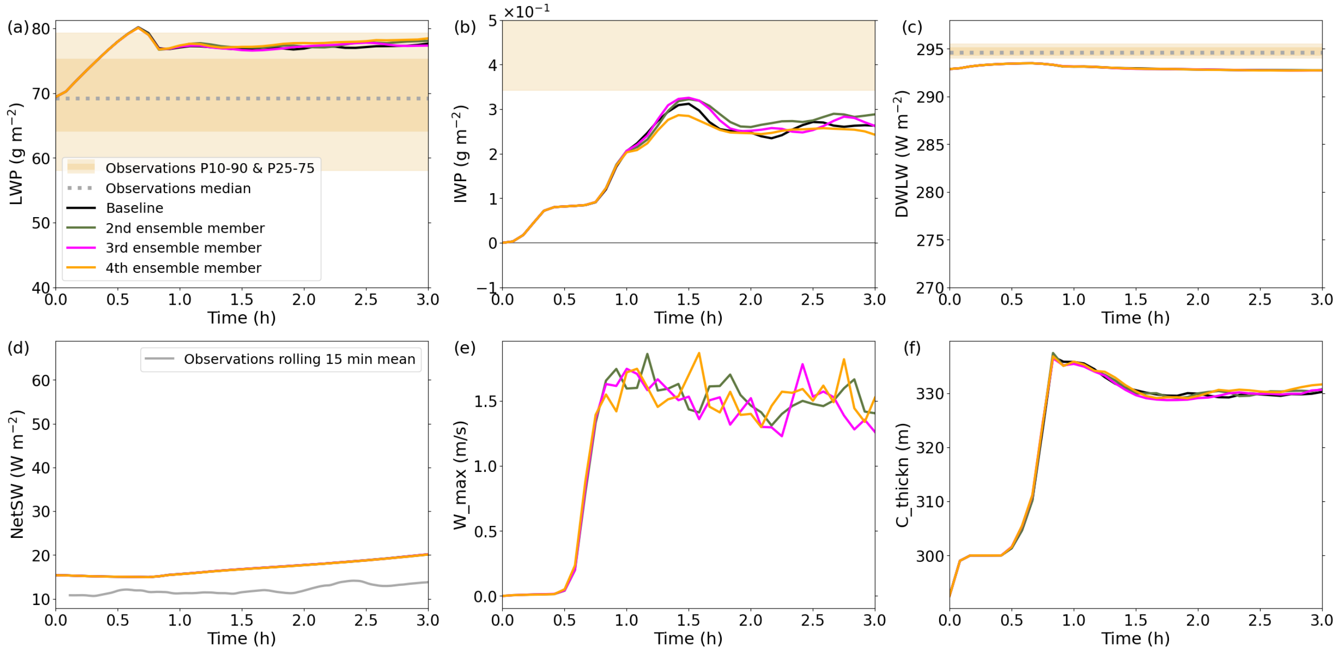

Figure R2. LWP, IWP, DWLW, netSW, maximum vertical velocity w, and cloud thickness during the three hours of spinup time for the baseline simulation, as well as three additional realizations visualizing the limited variability imposed from the initial perturbations.

4. Other measurable properties – the authors limits themselves to LWP, IWP, and radiative fluxes. I am wondering if other properties (that should be available from observations) are changing as well, such as cloud cover and surface precipitation rates. Even if there are no impacts to these quantities, it would good to briefly mention that. In case there are in-situ-probed hydrometeor size distributions, it would be great to compare these with simulations, thereby substantially strengthen the microphysical choices.

We agree and added some details regarding continuous observations to Sect. 3.1 of the manuscript (see below).
Regarding in-situ hydrometeor size distributions, the Light Optical Aerosol Counter (LOAC) operated during all Helikite flights on this day. We chose to exclude these measurements from the manuscript, however, as the data exhibit very large uncertainty. To still provide a comparison, Fig. R3 presents the total droplet number concentrations for all Helikite ascents and descents alongside the baseline simulation (corresponding to Fig. 6b in the main manuscript). Even after averaging the LOAC data over 10 m altitude intervals, the measured droplet concentrations fluctuated considerably,

ranging from approximately 40 to 120 cm^{-3}. We added an assumed concentration uncertainty of 50 % based on Renard et al. (2016) taking into account the standard counting uncertainty and the uncertainty in the droplet classification. However, future work will require a more in-depth evaluation of the droplet concentration determined by the LOAC using independent observations. The peak baseline simulation concentrations of 57.6 cm^{-3} fall into the lower uncertainty range of the LOAC measurements.

We emphasize again that the primary objective of the manuscript is not to achieve observational closure, but rather to investigate the sensitivity of the cloud to the four selected parameters.

Addition at the end of Sect. 3.1:

The ceilometer data (Fig. 1a) indicate continuous cloud cover starting from approximately 07:00 UTC on 7 June. The baseline simulation successfully captured this persistence, maintaining a fully overcast domain throughout the simulation. Furthermore, cumulative surface precipitation remained below 0.1 mm in both the observations and the simulation.

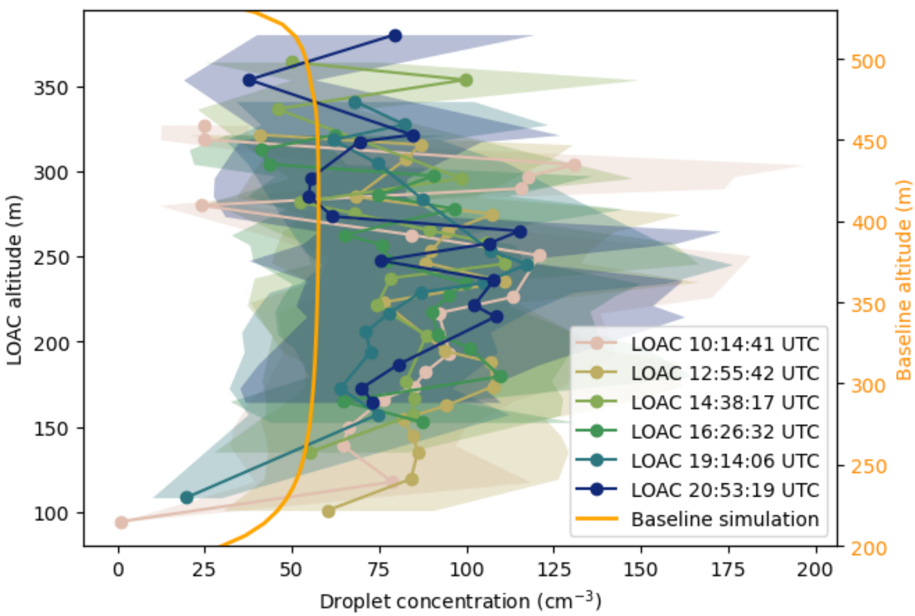

Figure R3. Total droplet number concentration profiles observed with the LOAC during all Helikite ascents and descents on 7 June, compared to the average cloud droplet number concentration profile of the baseline simulation (as in Fig. 6b in the main manuscript). The LOAC data were averaged over 10 m of altitude and the shading marks the uncertainty range of 50 %. Note the different y-axes that scale the LOAC data and the simulation data so that the cloud borders approximately match.

5. Unique response under atypeS in combination with icnc1 (Table 3) – IWP appears to generally increase going from ichabP to ichabC, with the exception of atypeS_icnc1 combinations. I wonder if the authors could expand their analysis

in Section 3 to explain why that is.

We thank the reviewer for noting this diverging behavior depending on the ice habit. The increase in IWP from plates to columns is also evident for low ICNC and Atype dust; the trend in IWP depends exclusively on ICNC. We attribute this diverging behavior to the increased deposition onto plates compared to columns.

For simulations with low ICNC, this enhanced deposition does not trigger cloud glaciation. The ice mass remains insufficient to activate the WBF process, allowing both deposition onto ice crystals and condensation onto cloud droplets to continue throughout the simulation (Fig. R4). Consequently, the elevated deposition in the setup with plates directly results in a higher IWP (cf. total integrals in Fig. R4a and R4b).

Conversely, as detailed in the manuscript, the combination of high ICNC and plates consistently causes cloud glaciation. This glaciation limits the total cloud condensate at the end of the simulation, especially when compared to the setup with high ICNC and columns. For these column simulations, deposition remains higher than under low ICNC conditions, ultimately generating a larger final IWP.

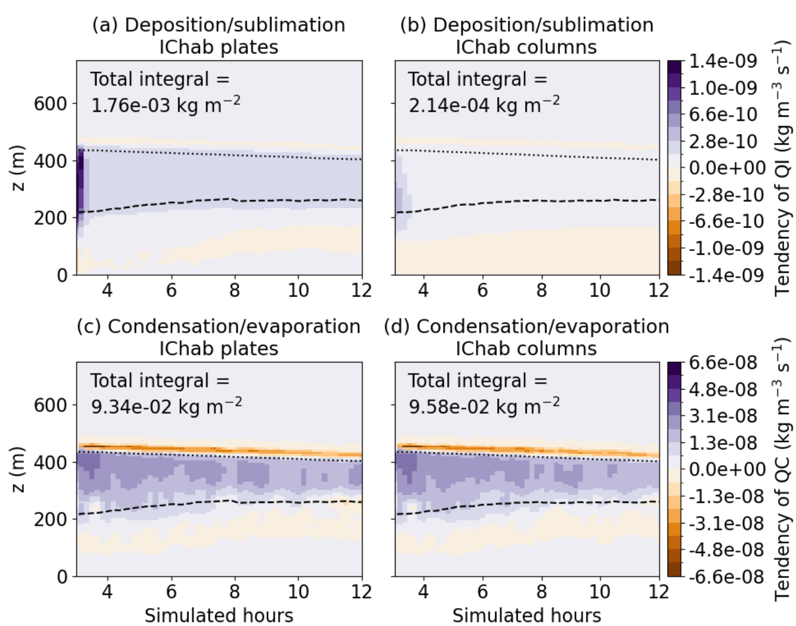

Figure R4. Average horizontal profiles of cloud ice deposition (positive)/sublimation (negative) process rates (top row, kg m^{-3} s^{-1}) and cloud droplet condensation (positive)/evaporation (negative) for **(a, c)** ancLO_atypeD_icncLO_ichabP (with IChab plates), and **(b, d)** ancLO_atypeD_icncLO_ichabC (with IChab column). Dotted lines show the cloud top, dashed lines the cloud base.

1.3 Minor concerns

1. l. 78 Could also cite other recent studies here that fixed ice number concentrations: Wu et al. (2025), Tornow et al. (2021), Juliano et al. (2026).

   We agree and added all these references:

   Addition in Sect. 1:
   To force the formation of ice, we bypassed the interactive ice nucleation schemes entirely by prescribing ICNC, following the approach of previous LES studies (e.g., Savre et al., 2014; Loewe et al., 2017; Stevens et al., 2018; ***Tornow et al., 2021; Wu et al., 2025; Juliano et al., 2026****).*

2. l. 112 It would be good if the authors were more specific here, since they strongly advocate for replacing it. For example, what is the default configuration of MIMICA's primary ice formation.

   Additionally following reviewer 2's minor comment 2, we added both ice nucleation parameterizations that are being used in the manuscript:

   Addition in second paragraph of Sect. 2.1:
   *Primary ice formation is parameterized using different schemes****; here, we present simulations using both prescribed ICNC and temperature-dependent INP concentrations****.*

3. l. 126 A missing link between cloud ice and radiative transfer could substantially affect the cloud evolution (perhaps even explain the collapse of fully glaciated clouds). The authors should discuss this shortcoming in section 4.

   We appreciate the reviewer raising this important point. We addressed this directly by running new sensitivity simulations, as ice-radiation interactions for the ice habit plates became available in MIMICA following the submission of our manuscript. Introducing this radiative coupling increased the simulated IWP and DWLW, and delayed the complete depletion of LWP (Fig. R5). Crucially, however, the cloud still fully glaciated.
   Consequently, the missing ice-radiation interactions influence the temporal evolution of the cloud, but they do not change the ultimate phase state or the main conclusions drawn from our factorial analysis. We have added this to the limitations section in the manuscript:

   Addition in Sect. 4.1:
   An additional limitation regarding radiation concerns the absence of interactions with frozen hydrometeors. Liquid hydrometeors, however, modulate radiation much more strongly, and sensitivity simulations conducted with a newer version of MIMICA that includes ice-radiation interactions (not shown) demonstrated only minor differences compared

to the simulations presented here. Consequently, this omission does not affect the main conclusions drawn from our factorial analysis.

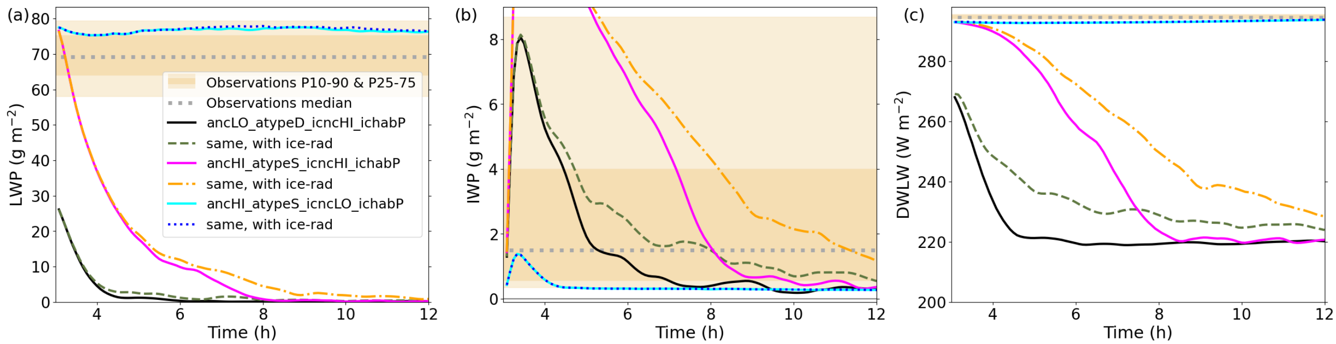

Figure R5. (a) LWP, **(b)** IWP, and **(c)** DWLW for three original simulations, as well as including ice-radiation interactions.

4. Fig. 3 It would be good to list total aerosol number in the figure or in the caption.

   This was added to the end of the caption:

   This results in total aerosol number concentrations of 220.6 $\times 10^6$ m^{-3} for high ANC, and 2.2 $\times 10^6$ m^{-3} for low ANC.

5. Sec. 2.4.3. Please clarify if the filter collected interstitial aerosol particles only.

   We thank the reviewer for this comment. It was already mentioned in original manuscript line 246 that an interstitial inlet was used for sampling INPs. We now also added it to the beginning of Sect. 2.4.3:

   Helikite filter samples were collected only during flight periods ***through an interstitial inlet****, resulting in limited sampling durations and corresponding air volumes (25×10^{-3} and 120×10^{-3} m^3 for in-cloud and above-cloud, respectively).*

6. ll. 476ff Does ice habit affect the particle fall speed? And it would also be good to repeat here which habit was mostly seen via in-situ probes.

   This is a very good point and has been added to the discussion:

   Addition in the beginning of the third paragraph of Sect. 4.1:
   Observations on the ship deck identified ice needles, and the baseline simulation accordingly assumed columns as the ice habit. *The crucial role of ice habit in the simulations arises from how the morphology affects depositional growth* ***and the sedimentation velocity*** *in MIMICA.*

Addition at the end of Sect. 4.1:

Additional to the direct impact of habit-dependent size on the deposition rates, the sedimentation velocity depends on the ice habit. For the two ice crystals with the same mass, but different habits, the columnar crystals sediment at 0.47 m s^{-1}, while the plates sediment at 0.19 m s^{-1}. This means that the plates reside longer in a cloud with constant depth, accumulating water vapor for a longer time and thus driving additional cloud glaciation.

7. ll. 508ff Could a different INP parametrization help here, such as time-dependent activation (e.g., Knopf et al., 2023)?

Incorporating the time-dependence of the parameterization described by Knopf et al. (2023) might yield somewhat higher freezing rates than our simulation using observed INP concentrations. At the relatively high temperatures characterizing our case, however, these rates would still remain too low to produce an appreciable IWP.

To explore this temperature constraint, we tested the case using an ice nucleation scheme based on random drawing from log-normal INP concentration distributions (Frostenberg et al., 2023). This approach successfully increased the IWP for a slightly colder Arctic mixed-phase cloud (minimum in-cloud temperature of -10 °C) relative to the Fletcher (1962) parameterization. In our warmer setup, however, it did not generate considerable ice mass. Some freezing occurred and formed ice crystals, but the simulated IWP remained below 10^{-3} g m^{-2}. We therefore expect that other freezing parameterizations would yield similarly negligible ice mass under these specific conditions.

Appreciable, yet small, IWP values emerged only in simulations using an active-site ice nucleation parameterization with very large INP aerosol (D $\leq$ 2000 nm, i.e., more than one order of magnitude larger than the geometric mean diameter of the aerosol used for cloud droplet activation).

2 Response to reviewer 2

2.1 Overview

This study performed a factorial analysis on LES of an Arctic Mixed Phase Cloud (AMPC) case study and compared it to observations from a tethered balloon system and ship-based ground observations. With observational targets of LWP, IWP, and downwelling longwave (DWLW) radiation, they perturbed the aerosol number concentration, aerosol type (i.e., hygroscopicity), ICNC, and ice crystal habit. They found that for these observational targets, the most influential variables describing the variance in the cloud properties were ranked: (1) aerosol number concentration, (2) ICNC, (3) ice crystal habit, and (4) aerosol type, though the ranking depends on the target observable. They then show that ice habit is important for simulating the cloud glaciation. Overall, the study's objectives are well-constructed, the presentation quality is excellent, and the writing is clear and concise. I have some verbose comments below that are ultimately all addressable without substantial revision. Upon addressing these relatively minor points, the manuscript will be well-suited for publication in ACP.

2.2 Major Comments

1. My only major comment isn't really that major. The factorial method is a tidy way to present these findings and I really enjoyed it. However, describing the results can become quite difficult, and interpretation for the reader even more difficult. I have some recommendations below regarding technical things that could help (e.g., naming convention), but I note another place or two where adding some brief physical inference could help (e.g., Minor Comment #7). You do a great job of extrapolating this inference in the Discussion section, and I don't think you have to go into it very much when the results are presented, but a brief mention of physical inference *when the results are provided* would be very helpful for the reader. Otherwise, several areas seem like a data dump without much purpose.

   We thank the reviewer very much for this comment. See our responses on the specific points below.

2.3 Minor Comments

1. Abstract, Conclusions (Lines 529-531): The last 2 sentences seem to particularly highlight the influence of ice habit, despite the statement that it ranks "third" in "importance". Although you mention that it determines the *final* phase state of the cloud above, it seems to be introduced as, by definition, a tertiary control and then is given more clout further down, which is a bit confusing. Also, the factorial analysis method maybe recognizable to some readers, but not all; I'm not suggesting you remove that from the abstract as it's crucial to your methodology, but I think something should be included for context of these "rankings" of importance and what that actually means, if you could do so briefly. My understanding is the primary deliverable of the factorial analysis is ranking the contributions to explained variance of the target observables. But a big part of your science question is phase partitioning, which doesn't make ice habit necessarily "less important". That depends on what is important: is it a target LWP, or is it phase partitioning? There is a brief but better way to conjure this, perhaps by stating that ice habit ranked third in explaining variance of X observables but

controlled Y. This applies to your conclusions as well, since a majority of readers are going to reader the abstract and conclusions, primarily.

We agree entirely and adjusted the abstract and the lines in the summary to include the analysis of final phase state of the simulated cloud. Additionally, we added a column stating the final phase state in Table 3.

Adapted sentences in the abstract:
We used factorial analysis to rank these parameters according to their explained variance in simulated liquid water path (LWP), ice water path (IWP), and downward longwave radiation at the surface (DWLW) of a slightly supercooled Arctic mixed-phase cloud. Additionally, we analyzed how these parameters affect the final phase state of the simulated cloud.
Addition in the second paragraph of Sect. 5:
Furthermore, we analyzed the effect of the parameters on the final phase state of the simulated cloud.

2. Lines 75-78, 112, Section 2.4.3: In the Introduction (lines 75-78), you state that you prescribed ICNC and bypassed the interactive ice nucleation schemes. Then on line 112, you state that primary ice formation is parameterized using different schemes. I was initially pretty confused. Section 2.4.3 finally clears this up by explaining that the baseline experiment uses prescribed ICNC while you also did experiments with INP(T). However, up until reading Section 2.4.3, I couldn't get a handle on it. I would make it clear, around lines 75-78 and/or 112, that both methods are evaluated and compared.

We agree that it makes the model description easier to read with this suggestion. We added both schemes to the model description:

Addition in second paragraph of Sect. 2.1:
*Primary ice formation is parameterized using different schemes**; here, we present simulations using both prescribed ICNC and temperature-dependent INP concentrations**.*

3. The chronology of the model setup is a little confusing. Following are some specific points:

    a. Line 121: When you define the initial Qc profile here, it's a little bit of putting the horse-before-the-carriage. The reader doesn't know yet how or why this is being initialized this way and whether it was observationally guided. Of course, this is revealed in Section 2.4.1. If you want to introduce that Qc profile here, I would add something referencing that this is observationally constrained and explained in the upcoming section.

    b. Lines 115-120: Somewhat similar to the previous comment, this reads as you giving very little information on the aerosol input constraints, and the reader has no reason to expect more information at this point. It's fine to introduce it this way, but again I'd reference the upcoming section in which more details will be provided. I would also say here very briefly that you are representing the aerosol distribution via observationally derived Aitken and accumulation modes (more details in Section 2.4.2).

c. Lines 124-125 and 190: Okay, so sensible heat fluxes are guided by the constant snow temperature. How are latent heat fluxes handled in this capacity over (snowcovered) sea ice?

We thank the reviewer for these comments.

a. We agree with this suggestion and added this information:
Addition at the end of Sect. 2.1:
This profile is usually based on observations, as was done in this study (see Sect. 2.4.1).

b. We added this as suggested:
Addition in the third paragraph of Sect. 2.1:
Here, we represent the aerosol size distribution via observationally derived Aitken and accumulation modes (see Sect. 2.4.2).

c. Latent heat fluxes are calculated based on the assumption that the surface moisture is saturated (see line 203 in updated manuscript). The humidity of the lowest model level determines the direction of the latent heat flux.

4. Fig. B1: It appears to me that you are evaluating the simulation around the middle radiosonde launch, but this is after the Helikite flights. Can you explain why you did that? I'm assuming because you wanted a thermodynamic profile representative of the incloud launch? (This is more for my own edification)

We agree that this requires more explanation. We initialized our simulations using the radiosonde launched just before midnight, as it captured the lowest in-cloud temperature of the day and thus maximized the chance for interactive ice formation in the simulated cloud. The initial thermodynamic profile heavily influences the simulated cloud evolution, making an evaluation period close to the initialization time necessary. We focused predominantly on the hours following the launch (i.e., six hours following the launch vs. 30 min before the launch), as the simulation is a forward-in-time integration of the cloud from that initial state. The observed cloud remained relatively stable during this entire period, ensuring a consistent comparison.
We have also added some of this detailed information into the manuscript:

Addition at the beginning of Sect. 2.4.1:
The LES simulations were initialized using T, RH, u, and v vertical profiles from the radiosonde launched at 23:34 UTC on 7 June 2023 (Fig. 2a-d), during a period when the cloud was relatively steady (Fig. 1) ***and the mimimum in-cloud temperatures were observed****.*

Addition at the end of Sect. 2.4.2:

This focus on the post-launch period reflects the simulation setup, which initialized from the radiosonde sounding and integrated forward in time.

5. Lines 163 & 167: While I understand that surface measurements and the balloon-system camera provide evidence of a mixed-phase cloud, some brief mention should be made about the algorithm used within CloudnetPy via combined ceilometer, radar, and radiometer to classify the cloud as mixed-phase, especially since it does not appear that depolarization capabilities were present. It's not that I'm doubtful, but curious, and I think a reader would be too. Is it because of detectable IWC/IWP via Hogan et al. (2006)?

The exact configuration of the CloudnetPy used to generate this dataset is described in detail on the dataset metadata page: https://bolin.su.se/data/oden-artofmelt-2023-cloudnet-1. The processing decisions that were made to optimise CloudnetPy performance with our specific instrumentation and in the Arctic environment are described in the 'comments' section.

The reviewer is correct that there were no lidar depolarisation capabilities present. Cloudnetpy version 1.66.10 by default initially identifies liquid layers based on attenuated backscatter from the lidar as described in Tuononen et al. (2019). Above lidar attenuation, we implemented the Voodoo algorithm to estimate liquid water presence based on the radar doppler spectra in regions where the CloudnetPy algorithm identified ice (Schimmel et al., 2022). The default 'ice' identification above lidar attenuation is based on the radar reflectivity, doppler velocity, and temperature - hence it is possible that supercooled liquid could get misclassified as ice if the radar return was sufficient. However, if this was the case we'd expect it to be completely misclassified as ice even with Voodoo, since we would expect the droplet spectra to have a narrow peak. We think this is unlikely based on the observed doppler spectra.

The caveat in the above is that the Voodoo algorithm performs poorly when the radar spectra are dominated by turbulent processes or/and when the ice crystal population is very small and has a similar fall velocity to liquid droplet population. For this particular case study we suspect this is the case based on the examination of individual radar doppler spectra. This is one of the factors that fed into our methodology for determining a best guess LWC algorithm that is not simply based on the CloudnetPy defaults (described in detail here: https://bolin.su.se/data/oden-artofmelt-2023-cloud-liquid-water-1). Basically, our confidence that this particular cloud was mixed phase is based on the liquid water path measurements from the radiometer, the radiosonde profile, the surface and in cloud observations of ice and liquid, and the broad radar doppler spectra. The CloudnetPy categorisation product is essentially only used to determine the cloud boundaries and the IWC retrieval (which is likely highly uncertain, but is the best we have at the moment).

Addition at the end of Sect. 2.2:

Full details of the specific CloudnetPy implementation and the assumptions used to generate the LWC profiles are described in Guy et al. (2025) and Guy and Brooks (2026), respectively.

Adaptation in Sect. 2.3:

CloudnetPy classified the cloud as mixed-phase based on the presence of hydrometeors not meeting the criteria for classification as droplets in the ceilometer attenuated backscatter data (indicative of ice phase particles), and a positive liquid water path measured by the microwave radiometer.

6. Simulation naming convention: I mostly like how you did this, but once I got to the discussion ∼ Line 365, I didn't like having to go back and see what "anc1" versus "anc2" meant. The underscore-style naming convention is indeed appropriate and there is no way around it for this many simulations, but it will always plague the reader's memory and desire to spend the time understanding them. Perhaps consider using "ancLO" and "ancHI", which would be more intuitive, like you did for the type and habit with letters. Recommend a similar convention for ICNC.

   We thank the reviewer for this great suggestion, we changed the simulation naming convention accordingly!

7. Lines 383-386: I've read this paragraph about a dozen times and am still really struggling to piece it all together, and I think this is just a paragraph of statements of relative performance. The figures and table already show this, meaning I think this paragraph isn't very useful in its current state. It would be much more insightful if some physical inference were given for *why* these relative relationships are presenting themselves and why they're important. Otherwise it's just redundant with the figures and tables. You did this *super* well for the following paragraph regarding glaciation and again in the Discussion. Would suggest doing the same here, or removing if it's just not important/insightful. In general, it's helpful to at least provide a brief phrase of the physical reasoning and optionally referring to "more on this in the Discussion" or something like that.

   We agree that this text indeed was difficult to digest. We have added a clarification on this first time an interaction is mentioned in the results part, as well as a physical explanation to this phenomenon:

   Adaptation in Sect. 3.3 from line 402 in updated manuscript:
   ANC shows a considerable interaction only with ICNC and for LWP (Fig. 9; Table 2). ***Consequently, the sensitivity of LWP to changes in ANC depends on the ICNC value.*** *The LWP is more sensitive to ICNC for high ANC than for low ANC (Table 3). For low ANC, simulations with a high ICNC value (e.g., ancLO_atypeD_icncHI_ichabC:* ***10.7 g*** $\mathbf{m^{-2}}$*) can still result in an LWP similar to those using the low ICNC* ***(e.g., ancLO_atypeD_icncLO_ichabC: 12.6 g*** $\mathbf{m^{-2}}$***)****. Conversely, for high ANC, all simulations with high ICNC produce considerably less LWP than their low-ICNC counterparts* ***(e.g., ancHI_atypeD_icncHI_ichabC: 53.4 g*** $\mathbf{m^{-2}}$ ***vs. ancHI_atypeD_icncLO_ichabC: 81.7 g*** $\mathbf{m^{-2}}$***). Enhanced deposition rates for high ICNC drive this pronounced difference, resulting in a stronger depletion of the liquid cloud water.***

8. Lines 440-449: This is a nice summary of trying to explain why INP(T) w/ SIP couldn't realize any ice. You could go a bit further by referencing the study of Fridlind et al. (2007), who simulated an MPACE AMPC case and also found ice-ice collision-induced mechanical breakup to be irrelevant (in addition to drop-shattering and phoretic scavenging).

Still unable to realize observed ICNC, they considered the following additional processes: (1) ice nuclei formed via the residual of evaporating drops, (2) drop freezing during evaporation, (3) spontaneous freezing per unit drop volume, and (4) per unit drop surface area, with the latter two meant to account for chemical pathways. These mechanisms were found capable of sustaining observed ICNC, though some are arbitrary and sought an all-encompassing chemical influence. They also discuss other potential mechanisms not considered (see their Section 4.5) that involve evaporation processes. At any rate, it could be worth discussing some of these possibilities, however briefly.

We thank the reviewer for the suggestion, and added the following text:

Addition at the end of Sect. 4:
Additional potential ice nucleation mechanisms include the conversion of evaporating drop residuals into INPs, drop freezing during evaporation, and cloud chemical processing that alters the ice-nucleating efficiency of aerosol particles (Fridlind et al., 2007).

9. Line 534: I think your phrasing here undersells how important it is to have in-situ ICNC measurements. This has plagued the community for a long time, and even with in-situ measurements is marred by uncertainty historically (via instrument shattering). In an ideal world, ICNC would be an observational target in your study, not a sensitivity test. Since you're advocating for designing future field campaigns, this seems like an ostensible target, lest we resign ourselves to prescribing ICNC forever.

   We agree that this is an important point to make and added the following:

   Addition in the middle of Sect. 5:
   In-situ ICNC measurements represent a long-standing observational bottleneck that requires targeted progress. Ideally, such measurements would serve to evaluate model performance rather than being a prescribed parameter.

10. Lines 476-484: Very elegantly written, concise explanation. Kudos.

    Thank you!

11. The observed evolution of the cloud system in Fig. B1 is perhaps more important than you think. Most readers are probably not going to go there to see it. For example, the glaciation discussion was very interesting, though I had to remind myself of whether or not the observed system glaciated. Even though you're not trying to simulate the exact evolution of the case and rather the steady-state solution, I think Fig. B1, perhaps in a somewhat condensed form, should be moved to the main manuscript. And perhaps when you discuss glaciation (and other aspects of the cloud system evolution), you can reference back to what the correct solution is, broadly. Again, I know the simulated evolution isn't necessarily the target, but context helps us connect a lot more dots than just presenting the sensitivities, especially with the large uncertainties in some of the perturbed quantities (e.g., ICNC, IWP).

We agree and moved the entire figure to the main manuscript, since we actually mention the cloud boundaries and the snow temperature in the main text.

Additionally, we have added the information on the real cloud system in the discussion of glaciation:

Addition in the fifth paragraph of Sect. 3.3:

*The simulations experiencing complete glaciation (resulting in an LWP of nearly zero and a minimum DWLW of approximately 220 W m^{-2}**, in contrast to the observed cloud that persisted as mixed-phase**) all have high ICNC (ancLO_atypeD_icncHI_ichabP, ancLO_atypeS_icncHI_ichabP, ancHI_atypeD_icncHI_ichabP, and ancHI_atypeS_-icncHI_ichabP; Table 3).*

Addition in the third paragraph of Sect. 4.1:

The glaciation in some of the sensitivity simulations diverges from the actual observations and the baseline simulation, where the cloud persisted as a mixed-phase cloud for at least 12 hours after the radiosounding that initialized the MIMICA simulation. Observations on the ship deck identified ice needles, and the baseline simulation accordingly assumed columns as the ice habit.

2.4 Technical Corrections

1. Line 67: Maybe say "naturally scarce active INP concentrations" instead? The concentrations could still be there but their ability to initiate freezing may not.

   We agree, this was adjusted.

2. Line 72: I know you discuss this in Section 2, but briefly saying "ship-based expedition" here would help prepare the reader for what to expect from an observational standpoint (static ground vs. ship vs. aircraft). Most of us will have never heard of this campaign (I hadn't).

   We agree, this was adjusted.

3. Line 74: Again, I know you go into more detail in the next section, but I recommended briefly adding "with bulk microphysics" somewhere here to prepare the reader for the scheme structure. I had actually been anticipating a bin microphysics scheme for some reason.

   We agree and added this here.

4. Lines 133-138: You state that the aerosol measurements are taken via an interstitial inlet. It would also be helpful to state how the cloud droplets are measured (open- versus closed-path). Or is it the same instrument? A few additional details would be helpful. A brief mention of the size range that each instrument measures would also be helpful.

We thank the reviewer for this comment. We added details to section 2.2:

Addition at the beginning of Sect. 2.2:
A miniaturized scanning electrical mobility spectrometer (mSEMS model 9404, Brechtel Manufacturing Inc., USA) measured aerosol size distributions covering electrical mobility diameters between 8 and 280 nm, and a portable optical particle spectrometer (POPS, Handix Scientific, USA) covered optical diameters between 186 and 3370 nm. Mounted on the MoMuCAMS, an in-situ light optical aerosol counter (LOAC, Meteomodem, France) with a vertical inlet measured aerosol and cloud droplet size distributions from 200 nm up to 50 μm.

5. Fig. 6 Caption: Please define what the mean diameters represent in the bulk scheme context (e.g., median volume diameter? Mass-weighted? Number-weighted? Medianmass diameter?)

   We agree that this needs to be stated and added the type of mean to the caption (mean-mass diameter).

6. Line 165: It's not clear to me what you mean by "physical sampling" and how that is different from "successfully conducted measurements"

   This was well spotted, we actually meant "filter sampling" here and adjusted the text.

7. Line 319: Is this line really blue? Looks black to me. If it's actually blue, there's probably not a reason to just not make it black.

   We agree and made all dark blue lines black.

8. Fig. 5b: Just curious, would IWP be better displayed on a logarithmic axis?

   We absolutely agree and changed the IWP plots accordingly.

9. Fig. 7 and all time-height series: How are these values horizontally-averaged? Are they conditioned on condensate in any capacity? I'm assuming the cloud layer is effectively horizontally homogeneous, which helps, but if there is any sense of mesoscale structure then we would need to know.

   We thank the reviewer for the question. All horizontal averages are simple domain averages, since the cloud layer is horizontally homogeneous as the reviewer expected.

10. Lines 343-344: Isn't the lower size limit for rime-splintering, as defined by Hallett-Mossop, 24 microns? Why is a diameter of 50 microns used (perhaps you meant radius...)? That range makes a very big difference in CDNC number concentrations available for the process to be active. This could be related to the definition you use for the representative parametric diameter, which is not described here (see earlier comment).

We thank the reviewer for catching this discrepancy. The 50 μm diameter threshold represented an implementation error in MIMICA, which we corrected to properly align with the standard 24 μm diameter threshold for the Hallett-Mossop process. Rerunning the baseline simulation with this corrected threshold, however, still produced zero ice via this mechanism. The conditions of our case do not trigger this secondary ice production process, because the mean cloud diameter is still smaller than the 24 μm, confirming that the simulations utilizing the initial threshold remain fully valid for our analysis.

11. Fig. 10: I'm assuming the plus signs are distributed horizontally across each box as a temporal evolution? Neat method. But I would state that in the caption.

We thank the reviewer for this comment; the plus signs actually represent the values of the individual simulations and appear in ascending order for each boxplot. We added a clarification to the figure legend:

Addition in caption of Fig. 10:
Plus signs mark the values of individual simulations, arranged horizontally in ascending order.

3 Response to reviewer 3

3.1 General

I think this is an important study for improving our understanding of the relative roles of different physical parameters and processes governing the evolution of Arctic mixed-phase clouds. Overall, the manuscript is well written, and the methodology is presented clearly and is generally easy to follow.

I agree with many of the comments raised by Reviewers #1 and #2. In particular, I think the manuscript would benefit from additional analysis of simulation variables and more physical interpretation when presenting the results, rather than primarily describing the figures. These additions would strengthen the scientific discussion and improve the accessibility of the manuscript.

My remaining concern is how the presented findings should be interpreted under more realistic conditions, where some of the prescribed assumptions used in the simulations may not hold. Although the factorial analysis provides useful insights into the relative importance of different processes, it is not entirely clear how robust these conclusions are when more realistic representations of aerosol and cloud processes are considered. Addressing this point would help clarify the broader applicability of the results.

Overall, I think this is a nice piece of work that has the potential to improve our understanding of Arctic mixed-phase cloud processes.

We thank the reviewer for this comment.
Regarding the second paragraph, we consider that our reactions to reviewers 1 and 2, specifically 1.2.2, 1.2.4, 1.3.6, and 2.3.7 have improved these points.
Regarding the third paragraph, see our 3.2.4 answer below.

3.2 Major Comments

1. L107: wereby → whereby.

   We thank the reviewer for this comment, this was adjusted.

2. L130: describe → describes.

   In this sentence, the two authors are the subject, so we kept the plural form of the verb.

3. L240–242: If the elevated in-cloud INP concentrations result from entrainment of long-range transported air, why do the above-cloud measurements show much lower INP concentrations, even when the effects of detrainment are considered?

   We agree that we should elaborate more on the elevated in-cloud INP concentrations and broadened the discussion, adding further possible explanations:

Adaptation in Sect. 2.4.3:

Furthermore, below-cloud sources likely contributed a fraction of the observed in-cloud INP population. Although an interstitial aerosol inlet was used, it likely collected smaller cloud hydrometeors alongside the aerosol, increasing the measured in-cloud INP concentration. The interpretation is additionally complicated by a temporal gap of more than four hours between the above-cloud and in-cloud filter sampling. Both spectra converge toward warmer temperatures, indicating that the small sampling volume may obscure similarities in this warmer regime. Consequently, the apparent differences between the spectra might seem disproportionately large simply because the observations are restricted to colder temperatures.

4. Although the authors acknowledge some of the study limitations in the discussion, it is still unclear to me how robust the conclusions from the factorial analysis are when more realistic treatments of aerosol number concentration, aerosol-INP interactions, and aerosol-INP-ICNC coupling are included. For example, the analysis suggests that ANC has a much weaker influence on IWP than on LWP. Could this result simply reflect the lack of coupling between aerosol and ice nucleation in the simulations, rather than the actual underlying physics? Some discussion of how these modeling assumptions may influence the conclusions would be valuable.

We thank the reviewer for this comment. We consider the limitation section containing most of these concerns, see the bold text below:

A further caveat is that the ICNC was estimated instead of being measured in situ, even though employing an aerosol-dependent ice nucleation parameterization would have been more physically consistent. Using such a setup, ICNC could not be prescribed directly, but would instead be controlled indirectly by aerosol concentrations and properties. ***Under such conditions, ANC and likely Atype would exert stronger influences on IWP and the other variables—consistent with the large sensitivity we found with respect to the prescribed ICNC.***

However, we additionally added this point to the summary of our main results (bold below):

Addition in Sect. 5 (bold), second paragraph:

We applied factorial analysis to rank the relative variance contribution of the four identified parameters, which additionally quantifies how much the impact of one parameter depends on other parameters. Furthermore, we analyzed the effect of the parameters on the final phase state of the simulated cloud. The factorial analysis highlighted a clear hierarchy: ANC dominated the response of LWP and DWLW, while ICNC controlled IWP. IChab consistently ranked as the third most important parameter, yet it critically dictated the final phase state of the cloud. Whether the cloud remained mixed-phase, transitioned to purely liquid, or completely glaciated depended entirely on habit. Conversely, aerosol type exerted a negligible impact on the simulated variables relative to the other three parameters. ***The uncoupled representation of***

aerosol and ice nucleation in the model setup heavily constrained this last result—linking ice nucleation directly to the aerosol type would amplify the influence of this parameter, at least for IWP, and likely for LWP as well.

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