Author's Response to Reviewer Comments

Manuscript egusphere-2026-1486

Sensitivity of Arctic mixed-phase cloud simulations to ice microphysical modifications in the WDM6 scheme of WRF (v4.3.1)

We sincerely thank both referees for their careful reading and constructive comments, which have substantially improved the manuscript. Below we first summarize the main revisions made in response to the reviews, followed by point-by-point responses to Referee #1 (RC1) and Referee #2 (RC2). In the point-by-point responses, the original text is given first, followed by the revised text. Response-only figures are labeled Fig. R# and are included within the corresponding responses.

Summary of main revisions

1. Model–observation comparison along the aircraft track. The region-averaged aircraft comparison was replaced with track-collocated sampling along Flight 10a, and the affected figures and the Methods (Section 2.3) were revised accordingly. (RC1.2)

2. Thermodynamic and cloud-top analysis. A thermodynamic-profile figure (temperature, potential temperature, relative humidity, and water vapor mixing ratio) with diagnosed cloud-top heights was added (Fig. S5), showing that the low vertical bias is common to all configurations and originates in the ERA5 boundary conditions. (RC1.3)

3. Precipitation, snow, and ice-budget diagnostics. The cloud-ice-to-snow conversion rates (Psaut, Psaci) were added as a new panel to the process-rate figure; the surface precipitation rate and accumulated snowfall (Fig. S4) and the accumulated surface precipitation (Table 3) were added; and the total ice water content was decomposed into cloud ice, snow, and graupel (Fig. S3). A discussion was added to Section 4.1. (RC1.4)

4. Total condensate and ice fraction. Vertical profiles of total condensate and ice fraction were added (Section 3.1), clarifying that WDM6_ICE recovers the total condensate mainly by shifting to the liquid phase rather than reproducing the observed mixed-phase layer. (RC1.minor 5)

5. Single-case scope and regime-dependent interpretation. The case-selection rationale was clarified, the single-case scope was made explicit in the Abstract, Introduction, case description (Section 2.2), and Conclusions, and the regime-dependent interpretation was softened accordingly. (RC1.1, RC1.5, RC2 general)

6. Surface radiative biases. Shortwave and longwave biases relative to the observations were added to Table 3, and Section 4.3 was revised to report them. (RC1.8)

7. Spherical-ice motivation and CCN floor. The motivation for the spherical-ice assumption and the role of the WDM6_SP experiment were clarified in the Methods, and the minimum CCN concentration was identified as the default value of the WDM6 scheme (Table 1). (RC2 general)

8. Consistency and presentation. Microphysical process-name capitalization and figure numbering were unified, process-rate units were corrected to per second, the Fig. 1 caption and the Data availability statement were updated for the GEBCO_2023 grid, and the reference list was completed. (RC1.6, RC1.7, RC1.minor 1, RC2 specific)

A point-by-point response to each comment follows below.

Reviewer 1
This manuscript evaluates the sensitivity of Arctic mixed-phase cloud simulations to ice microphysical modifications in the WDM6 scheme using WRF for the M-PACE case. The comparison between the original WDM6 scheme, the full WDM6_ICE scheme, and intermediate sensitivity experiments is valuable. The study addresses an important problem: how bulk microphysics schemes partition water between liquid and ice phases in Arctic mixed-phase clouds, and whether modifications developed or tested in other regimes behave consistently under Arctic conditions.
The manuscript is generally well structured and contains useful diagnostics, especially the decomposition into ice-shape and ice-nucleation modifications. The paper is clearly written and easy to follow. However, several aspects require clarification before the conclusions can be fully supported. My main concerns are the justification and generality of the selected case, the model-observation sampling strategy for aircraft comparison, the vertical displacement of simulated cloud structure, and the strength of the regime-dependent interpretation. I therefore recommend major revision.

: We sincerely appreciate the reviewer's careful reading and constructive comments. The manuscript has been revised accordingly, and detailed responses are provided under each comment below.

Major comments

1. The selection of the 9–10 October 2004 M-PACE case should be better justified. Although M-PACE is a well-established benchmark for Arctic mixed-phase cloud studies, the manuscript should clarify why this particular case is suitable for evaluating regime dependent responses of WDM6_ICE. In particular, the authors should discuss whether the relatively narrow and warm subzero cloud-temperature range and the single-layer boundary-layer structure make this case representative of Arctic MPCs more broadly, or whether the conclusions should be interpreted as case-specific.

: We thank the reviewer for the opportunity to clarify the case-selection rationale. The 9–10 October 2004 single-layer stratocumulus sub-case is suited to the mechanistic decomposition pursued here for two reasons. First, the single-layer, well-mixed structure removes the complications of multilayer cloud systems and provides a clean testbed for isolating the microphysical pathways (Klein et al., 2009). Second, its cold subzero cloud-temperature range (about −15 to −10 °C), which lies outside the Hallett–Mossop window, together with low ice-nucleating particle concentrations and shallow boundary-layer coupling, is thermodynamically distinct from the mid-latitude precipitation cases of Park and Lim (2023), making it a suitable Arctic counterpart for evaluating whether the WDM6_ICE modifications respond consistently across regimes. We interpret the conclusions within this scope rather than as broadly representative of all Arctic mixed-phase clouds. Accordingly, we have added a paragraph to the Conclusions stating that the findings are based on a single M-PACE case and that their quantitative generality across the broader range of Arctic mixed-phase conditions remains to be tested with additional

cases in future work, with consistent statements in the Introduction and the case description (Section 2.2). The broader within-Arctic generality is discussed in our response to Major Comment 5.

2. The comparison with aircraft observations should be more carefully justified. The manuscript appears to compare aircraft profiles with model profiles averaged over a flight region rather than strictly collocated along the aircraft track and sampling time. Since Arctic MPCs can exhibit substantial spatial variability, regional averaging may smooth cloud structures and affect the diagnosed peak height and magnitude of LWC/IWC. The authors should either provide a track-collocated model–observation comparison or justify why regional averaging is more appropriate.

: We thank the reviewer for this point. We have replaced the region-averaged comparison with a track-collocated sampling along Flight 10a, and Figures 3 and 6 have been updated accordingly. For each D03 grid cell crossed by the flight track during 01:10–02:00 UTC, the model profiles are sampled and averaged, using the hourly model outputs at 01:00 and 02:00 UTC, and then binned onto the observation altitude grid. During this window the aircraft sampled the cloud by executing repeated vertical spirals over a confined area near Barrow (McFarquhar et al., 2007), so the observed profile is itself a composite of multiple profiles over that area rather than a single instantaneous transect. Averaging the model over the same set of track cells and time window is therefore the consistent counterpart to the observed profile. The hourly model output makes an instantaneous, point-by-point collocation along the 1-Hz track infeasible, but restricting the model sample to the track cells limits the spatial smoothing that a broader regional average would introduce. The Methods (Section 2.3) have been revised to describe this track-cell sampling, and the figure captions now state that the model profiles are sampled along the track between 01:00 and 02:00 UTC.

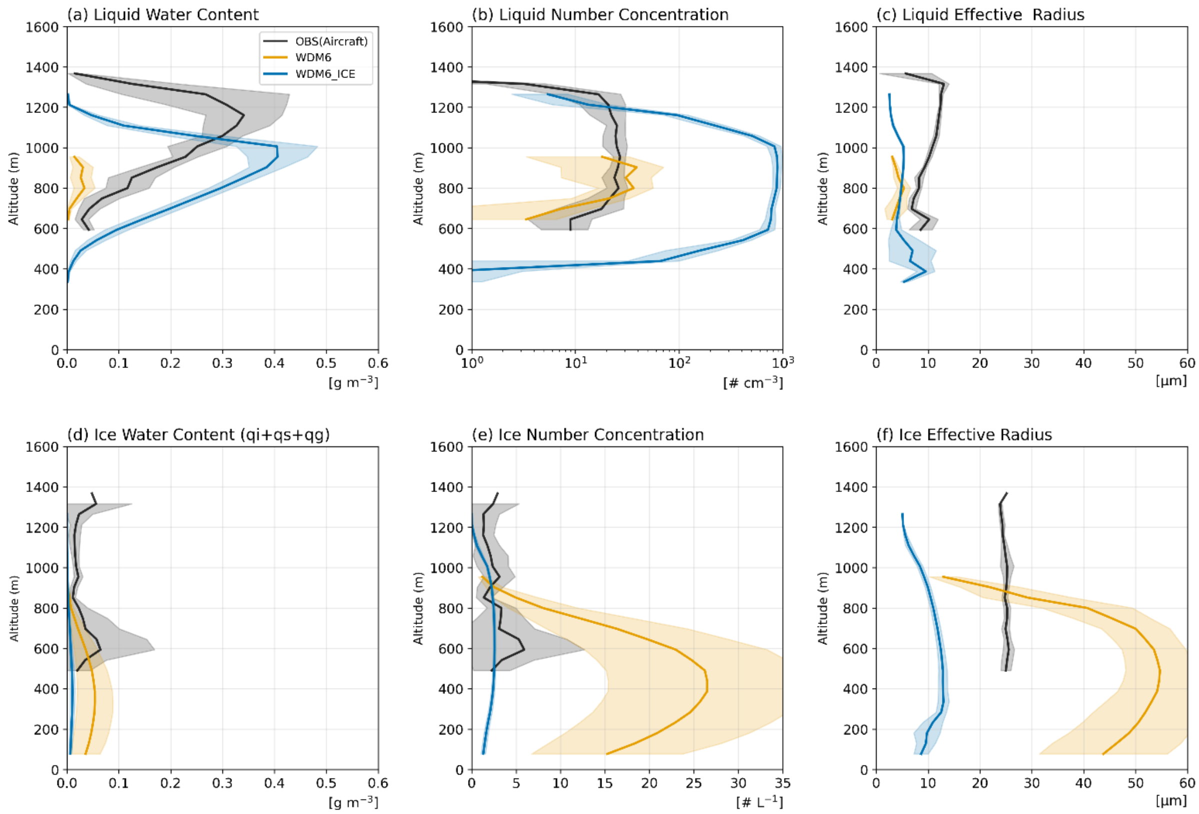

Figure R1: Vertical profiles of cloud microphysical properties along the flight track (Flight 10a). Panels show (a-c) liquid water content (LWC), number concentration, and effective radius, and (d-f) ice water content (IWC: q_i+q_s+q_g), number concentration, and effective radius. Black lines represent aircraft observations (01:10–02:00 UTC 10 October 2004) with gray shading indicating ±1 standard deviation (σ). Colored lines denote model simulations sampled along the track between the 01:00–02:00 UTC, WDM6 (orange) and WDM6_ICE (blue) with shading indicating ±1σ across the track grid cells.

3. The vertical location of the simulated cloud layer appears biased low relative to the aircraft observations. For example, the observed LWC peaks around 1200 m, whereas WDM6_ICE peaks around 1000 m and WDM6 even lower. This vertical displacement may affect the evaluation of LWC/IWC magnitude, phase partitioning, and radiative coupling. The authors should discuss possible causes, such as boundary-layer height, inversion height, thermodynamic profile biases, sampling strategy, and assess whether this vertical mismatch influences the main conclusions.

: We thank the reviewer for this important point. We have expanded the thermodynamic-profile figure as show in Fig. R2 (Fig. S3 in the original manuscript) to show temperature, potential temperature, relative humidity, and water vapor mixing ratio together with the diagnosed cloud-top heights at BAR, using the radiosondes on 9 October 2004. The observed cloud top is near 1371 m, while all simulations are lower (1162 m in WDM6_ICE and 873 m in WDM6). The potential temperature profiles show that the modelled inversion and boundary-layer top are lower than observed, and this bias is already present in the ERA5 boundary conditions (cloud top 1182 m). Because all experiments share the same ERA5 forcing and boundary-layer scheme, this low bias is common to every configuration

and does not affect the relative comparison of the microphysical sensitivities that is the focus of this study. Consistent with this, the microphysical sensitivity hierarchy is preserved when the boundary-layer scheme is changed (Fig. S4 in the original manuscript), confirming that the relative differences do not depend on a particular boundary-layer configuration. We have added a paragraph discussing this in Section 4.4.

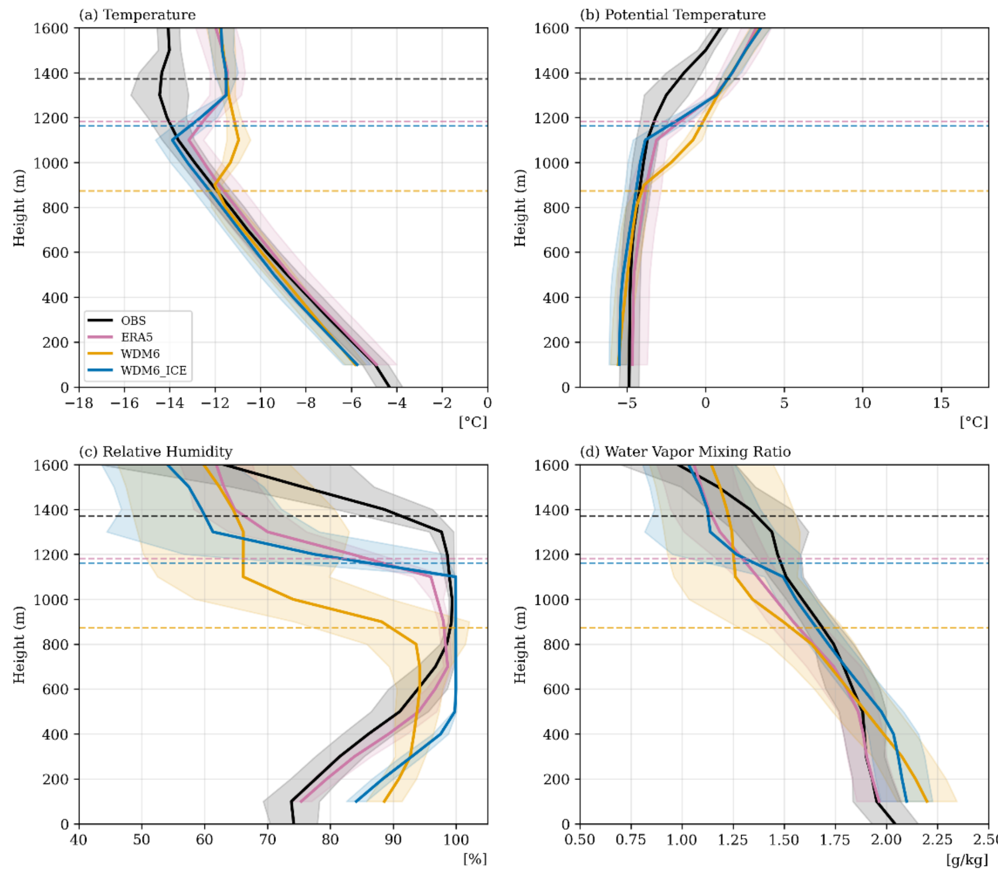

Figure R2: Vertical profiles at BAR averaged over three times (approximately 12:00, 17:00, and 23:00 UTC) on 9 October 2004 of (a) temperature, (b) potential temperature, (c) relative humidity, and (d) water vapor mixing ratio. Black lines show the radiosonde observations (launched at 11:50, 17:00, and 23:02 UTC) and pink, orange, and blue lines show ERA5, WDM6, and WDM6_ICE, with shading indicating ±1σ across the three times. The dashed horizontal lines mark the diagnosed cloud-top height for each profile (same colors as the lines).

4. The role of precipitation and sedimentation is not sufficiently discussed. Since the manuscript argues that WDM6_ICE shifts the ice budget from cloud ice toward snow and modifies liquid-to-ice/snow conversion pathways, changes in snowfall, precipitation flux, and sedimentation should be examined or at least discussed. These processes directly affect cloud water content, cloud lifetime, and the interpretation of LWP/IWP differences. The authors should clarify whether precipitation/sedimentation differs among experiments and whether it contributes to the reduced total water path and altered phase partitioning.

: We thank the reviewer for this point. To examine the role of precipitation and sedimentation, we added the autoconversion of cloud ice to snow (Psaut) and the accretion of cloud ice by snow (Psaci) as a new panel (d) to Fig. 5. We also examined the surface precipitation rate and the accumulated snowfall for all five experiments, shown in Fig. S4, and we have added the accumulated surface precipitation (PRECIP) to Table 3. To document the accompanying shift of the in-cloud ice budget from cloud ice toward snow, we also decomposed the total IWC along the flight track into cloud ice, snow, and graupel mixing ratios for all five experiments (Fig. S3; Fig. R5 of this response). A discussion has been added to Section 4.1.

The precipitation, which is almost entirely snowfall under the subzero conditions of this case, differs substantially among the configurations. WDM6 and WDM6_IN produce the largest amounts (about 2.6 mm), whereas WDM6_ICE produces the smallest (0.39 mm), with WDM6_SP and WDM6_SP_IN intermediate (about 0.9 mm). In WDM6, strong vapor deposition onto cloud ice (Pidep; Fig. 5c) generates abundant cloud ice that is efficiently converted to snow (Psaut; Fig. 5d) and removed by sedimentation. In WDM6_ICE, the suppression of Pidep limits cloud ice and weakens Psaut (Fig. 5d), so the precipitation is much smaller.

These precipitation differences contribute to the total water path (TWP) differences among the configurations: the configurations with the largest precipitation (WDM6, WDM6_IN) remove column water most efficiently through deposition-grown snow and sedimentation, which lowers their TWP, whereas WDM6_ICE produces little snow and retains a persistent liquid layer with a higher TWP. Regarding the phase partitioning, the reduced precipitation in WDM6_ICE and the shift of the in-cloud ice composition toward snow both accompany the suppression of vapor deposition onto cloud ice, so we describe them as linked aspects of the same change in ice microphysics rather than assigning a causal direction, which the present experiments cannot isolate. We also note that the snow-dominated ice budget described in Section 3.1 refers to the in-cloud composition, in which snow exceeds cloud ice within the residual ice phase, and is distinct from the surface snowfall rate. Figure S3 illustrates this in-cloud composition for each configuration. In WDM6 the residual ice is carried almost entirely by cloud ice, whereas in WDM6_ICE cloud ice becomes negligible and snow dominates the residual ice. In WDM6_IN the cloud-ice mass is only slightly lower than in WDM6 and is offset by a corresponding increase in snow, so the total ice content is largely conserved, consistent with the redistribution rather than reduction of ice by the nucleation modification.

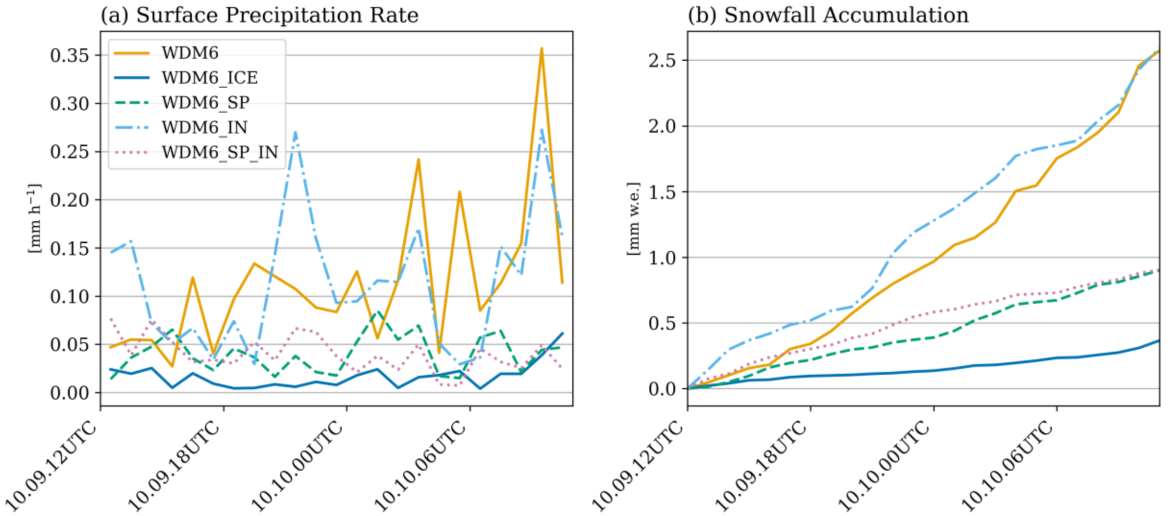

Figure R3: (a) Surface precipitation rate and (b) accumulated snowfall (water equivalent) at BAR over 12:00 UTC 9 October – 11:00 UTC 10 October 2004 for WDM6, WDM6_ICE, WDM6_SP, WDM6_IN, and WDM6_SP_IN.

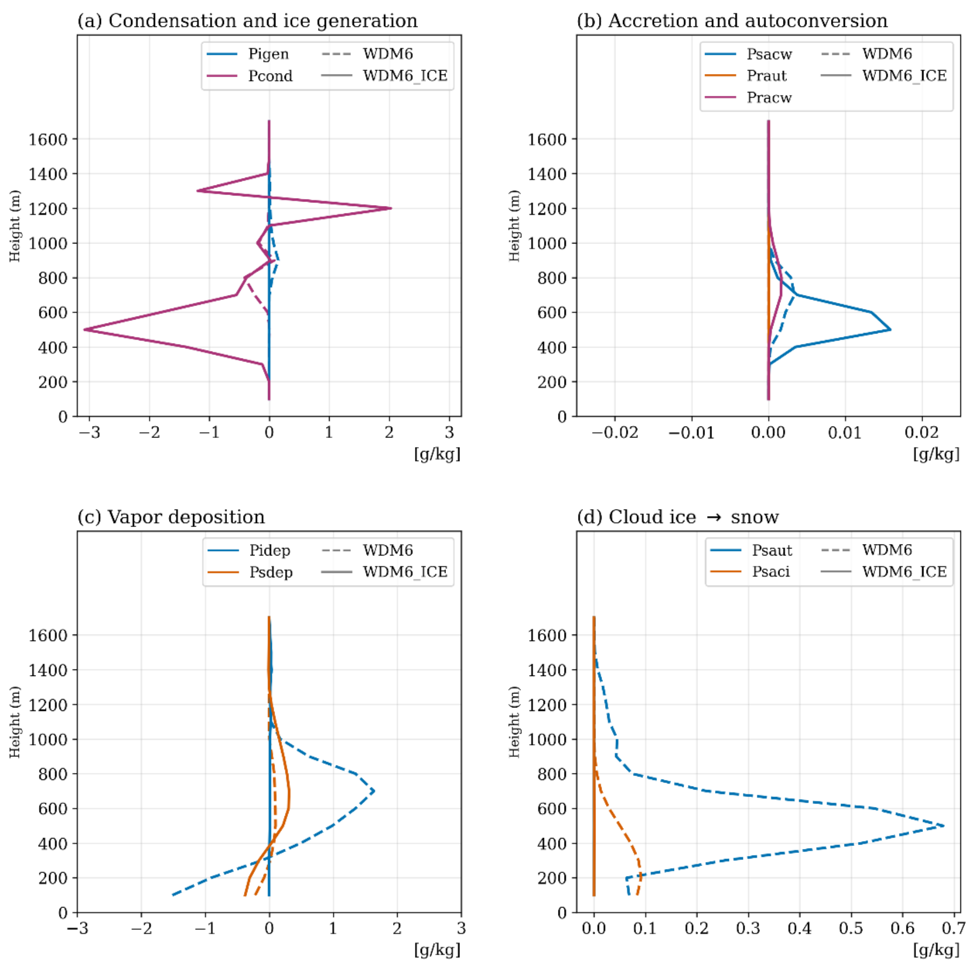

Figure R4: Time-averaged (12:00 UTC 9 October – 11:00 UTC 10 October 2004) vertical profiles of microphysical process rates at BAR: (a) condensation and ice generation (Pigen, Pcond), (b) accretion and autoconversion (Psacw, Praut, Pracw), (c) vapor deposition (Pidep, Psdep), and (d) cloud ice to snow conversion (Psaut, Psaci). Dashed lines represent WDM6, and solid lines represent WDM6_ICE. Note that negative values in (a) indicate evaporation.

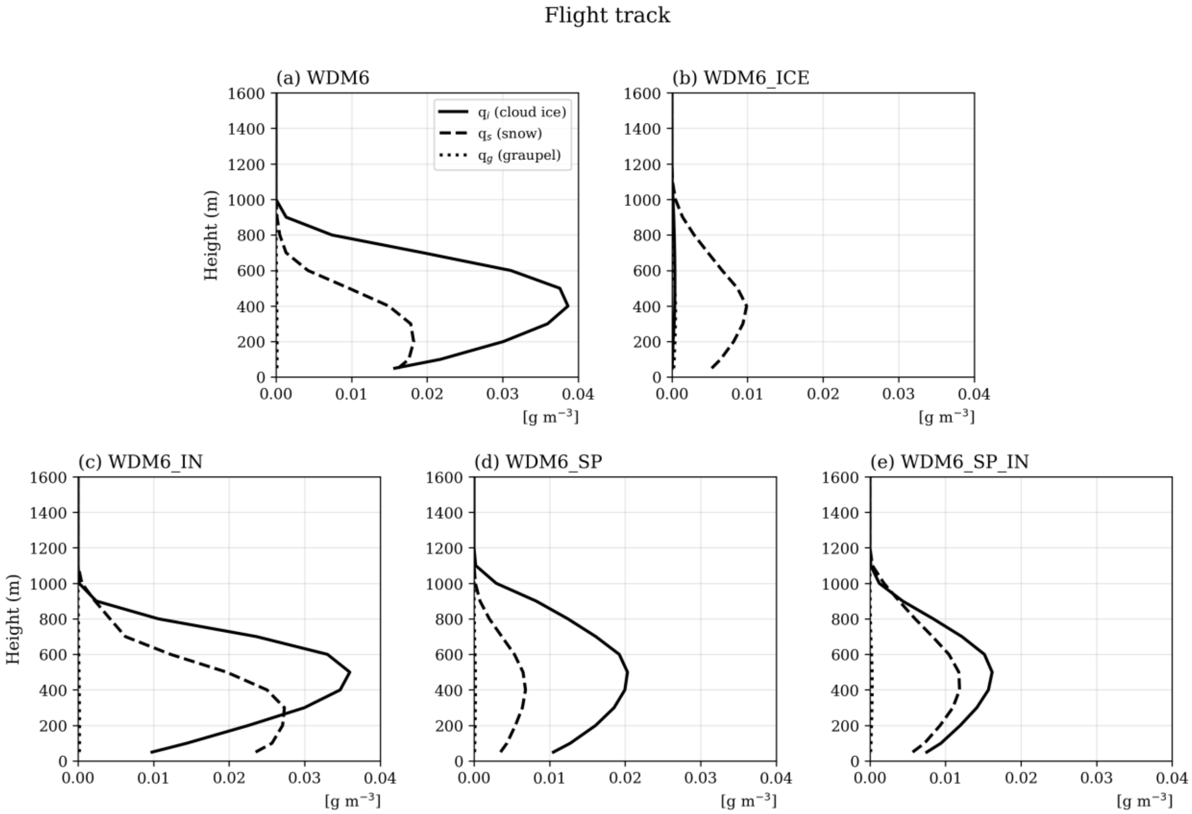

Figure R5: Decomposition of the total IWC along the flight track (Figure 3d) into cloud ice (q_i, solid), snow (q_s, dashed), and graupel (q_g, dotted) mixing ratios for (a) WDM6, (b) WDM6_ICE, (c) WDM6_IN, (d) WDM6_SP, and (e) WDM6_SP_IN, sampled between 01:00 and 02:00 UTC 10 October 2004.

5. The conclusion that the response is "regime-dependent" should be stated more cautiously. The present evidence is based on a single M-PACE case with a relatively narrow cloud-temperature range of approximately −15 to −10 °C. The authors themselves note that this temperature range lies outside the Hallett–Mossop secondary ice production window. Therefore, the strong ice suppression in WDM6_ICE may be partly case-dependent or temperature-regime-dependent rather than generally representative of Arctic MPCs. Other Arctic cases, for example cases within the Hallett–Mossop temperature window or cases with stronger primary nucleation, could produce different responses. The authors should either provide additional cases/sensitivity tests or soften the generality of the regime-dependent conclusion.

: We agree, and have softened the generality of the regime-dependent interpretation to reflect its single-case basis on the Arctic side. The interpretation is retained but explicitly qualified as resting on single-case evidence, since Park and Lim (2023) examined multiple mid-latitude ICE-POP cases whereas the present study examines a single Arctic single-layer case. Section 4.2 has been revised from "This asymmetric response demonstrates that the sensitivity of microphysical parameterizations to environmental conditions is itself regime-dependent: modifications that produce quantitative corrections in one regime can trigger qualitative shifts in cloud ice budget structure in another." to "This asymmetric

response indicates regime-dependent behavior of the microphysical modifications, although it is based on a single Arctic case and should be interpreted with caution. Whether the contrast persists across the broader range of Arctic mixed-phase conditions remains to be tested in future work." Consistent single-case statements have been added to the Abstract, the Introduction, and the case description (Section 2.2), and the Conclusions now note that additional cases, including cases within the Hallett–Mossop temperature window or with stronger primary nucleation as the reviewer suggests, are needed to test this interpretation.

Minor comments

1. In Fig. 1b, please clarify what the color shading over the ocean represents. If this is a MODIS visible/true-color image rather than a quantitative variable, this should be stated explicitly in the caption. If a color scale is used, the variable and units should be provided.

: The color shading over the ocean and land in Fig. 1b shows ocean depth and land elevation from the GEBCO_2023 grid (GEBCO Compilation Group, 2023), as given by the color bar (Depth and Elevation in meters), overlaid with the grayscale MODIS visible image. We have revised the Fig. 1 caption to state this and added the GEBCO_2023 grid to the Data Availability

2. Please clarify that the 20 levels below 800 hPa are included within the total 50 vertical levels, rather than additional levels.

: The 20 levels below 800 hPa are part of the total of 50 vertical levels, not additional levels. We have clarified this in Section 2.3 and Table 1, and unified the terminology to "levels" throughout.

3. Please indicate in the Fig. 2 caption that the plotted spatial fields correspond to D03, if that is the case.

: The spatial fields in Fig. 2 correspond to domain D03. We have added this to the Fig. 2 caption.

4. In Fig. 6, the height of peak LWC appears to shift upward from WDM6 to WDM6_ICE as supercooled liquid water increases. The authors should briefly explain whether this reflects changes in boundary-layer depth, cloud-top radiative cooling, thermodynamic structure, or microphysical suppression of ice deposition.

: The upward shift of the LWC peak from WDM6 to WDM6_ICE reflects the higher simulated boundary layer and cloud top in WDM6_ICE. As shown in the thermodynamic profiles (Fig. S5) and the boundary-layer height (Fig. 4c), the inversion and cloud top are higher in WDM6_ICE (cloud top about 1160 m) than in WDM6 (about 870 m), and the LWC peak, which forms near cloud top below the inversion, shifts upward accordingly. The other

factors raised by the reviewer are consistent with this picture: the microphysical suppression of ice deposition allows WDM6_ICE to maintain more supercooled liquid and a colder cloud top, and the associated stronger cloud-top radiative cooling is consistent with a deeper, better-mixed boundary layer, which in stratocumulus-topped layers is driven by cloud-top cooling (Wood, 2012). We have added this explanation to Section 4.4, together with the cloud-top height discussion in our response to Major Comment 3.

Reference
Wood, R., 2012. Stratocumulus Clouds. Mon. Weather Rev. 140, 2373–2423.

5. Given that WDM6_ICE improves LWC but substantially underestimates IWC, it would be useful to show the vertical profile of total condensate water content, e.g., LWC + total IWC, and/or the ice fraction. This would help evaluate whether WDM6_ICE improves the overall cloud water structure or mainly shifts condensate from ice to liquid. Such a diagnostic would also support the discussion of balanced phase partitioning and total water path biases.

: We have added a figure showing the vertical profiles of total condensate (LWC + IWC) and ice fraction along the flight track and at BAR (Fig. R6 of this response; added to the main text). Along the flight track, the observed ice fraction decreases from near unity at cloud base to about 0.04–0.09 in the upper cloud, indicating a liquid-dominated mixed-phase layer. WDM6 retains a high ice fraction of about 0.7–1.0 throughout the layer, whereas WDM6_ICE follows the observed decrease toward low values aloft, reaching about 0.01 above 900 m. The total condensate shows that WDM6 produces a near-negligible amount, while WDM6_ICE recovers a pronounced peak close to the observed magnitude. WDM6_ICE therefore recovers the total condensate toward the observed value, but the ice fraction shows that it does so by shifting almost entirely to the liquid phase, in contrast to the observed mixed-phase layer in which ice and liquid coexist. We have added this diagnostic and its description to Section 3.1.

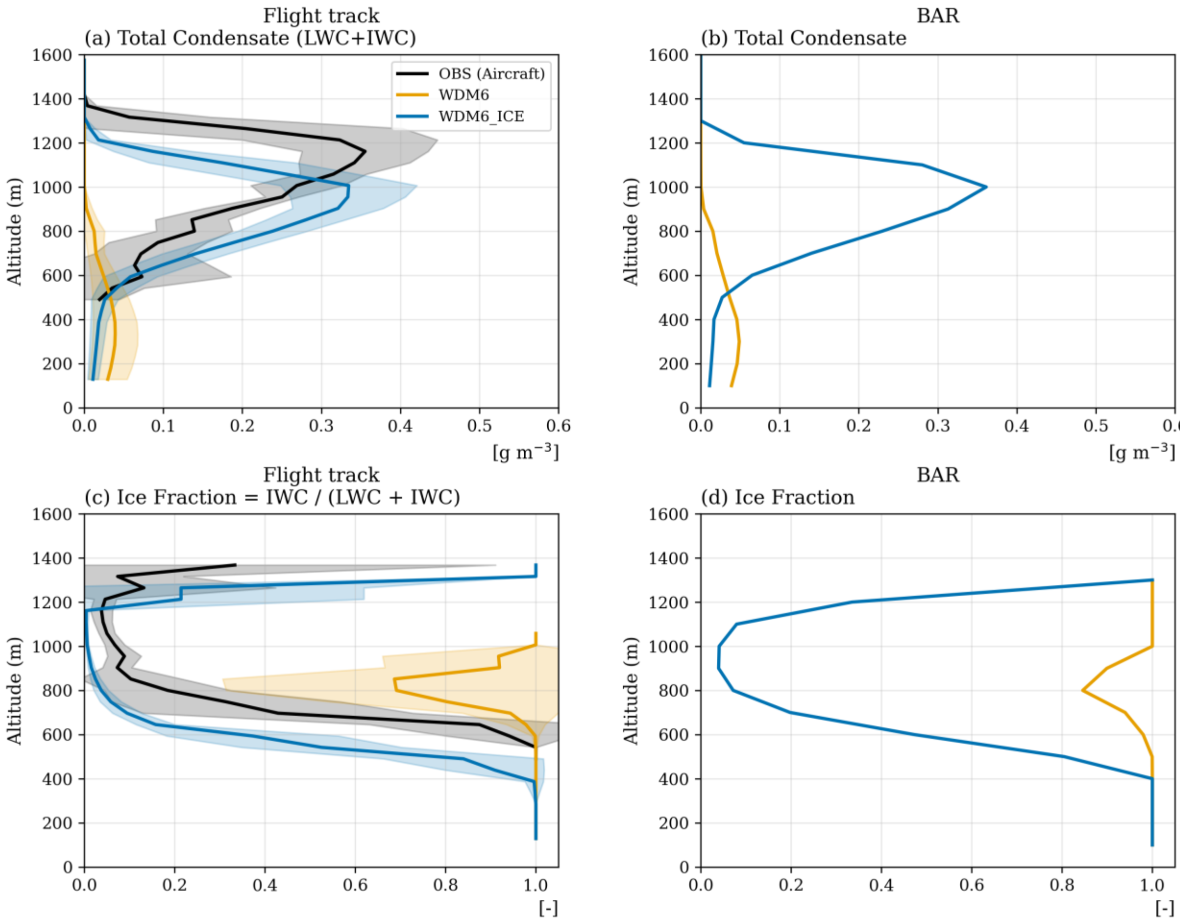

Figure R6: Vertical profiles of (a, b) total condensate (LWC + IWC) and (c, d) ice fraction (IWC / (LWC + IWC); IWC = qi + qs + qg). The left column (a, c) shows the profiles sampled along the flight track of Flight 10a during 01:00–02:00 UTC 10 October 2004, and the right column (b, d) shows the time-averaged (12:00 UTC 9 October – 11:00 UTC 10 October 2004) profiles at BAR. Black lines with gray shading indicate aircraft observations (01:10–02:00 UTC 10 October 2004; ±1σ). Colored lines denote model simulations: WDM6 (orange) and WDM6_ICE (blue).

6. Please clarify in the Fig. 6 caption whether model IWC is calculated consistently with Fig. 3, i.e., as the sum of cloud ice, snow, and graupel mixing ratios.

: We confirm that model IWC is calculated consistently in Figs. 3 and 6 as the sum of the cloud ice, snow, and graupel mixing ratios (q_i+q_s+q_g). We have clarified this in the Fig. 6 caption to match Fig. 3.

7. Line 330 , Please use consistent capitalization for microphysical process names, e.g., PIDEP and PINUD/PNUD, and define each process clearly at first use.

: We have unified the capitalization of all microphysical process-rate names throughout the manuscript, including Line 330 and the figure captions. The process names now appear consistently as Pidep, Psdep, Pigen, Piacw, Pinud, Psacw, Pcond, Praut, Pracw, Psaut and Psaci, and each process is now defined at first use.

8. In the discussion of radiative biases, the authors should report the longwave and shortwave radiation biases relative to observations, rather than only the absolute fluxes. Since Arctic MPCs strongly affect surface longwave radiation, the LWDOWN bias is particularly relevant for evaluating whether improved liquid water representation leads to improved surface energy balance.

: We have added the longwave and shortwave radiative biases relative to the observations directly in Table 3, given in parentheses next to the absolute fluxes (simulation – OBS). The revised Section 4.3 highlights that the longwave bias decreases from −43.3 W m^{-2} in WDM6 to −4.7 W m^{-2} in WDM6_ICE, with the intermediate-LWP configurations showing the smallest combined biases.

Reviewer 2

The authors have examined the influence of ice shape and ice nucleation parametrizations on cloud properties inside the WDM6 microphysics scheme in WRF. The manuscript is well written and of good scientific quality. However, there are some aspects that are not clear enough yet that I will list in the following comments.

: We sincerely appreciate the reviewer's thoughtful comments and positive assessment. The manuscript has been revised accordingly, and detailed responses are provided under each comment below.

General comments:

1. I miss a clearer motivation for why ice shape is modified to begin with. Is spherical ice shape known to be more common than bullet rosettes or is the implemented change just to test what happens if ice surface area is minimized?

   : The spherical ice shape is not a modification introduced in this study, nor is it intended as a claim that spherical particles are more representative of real Arctic ice habits. The spherical assumption is one of the design choices of the existing WDM6_ICE scheme (Park and Lim, 2023), which replaces the bullet-shaped cloud ice of the original WDM6 (Lim and Hong, 2010). Although the observed ice particles during M-PACE included a variety of non-spherical habits (McFarquhar et al., 2007), the aim of this study is not to evaluate the physical realism of either shape assumption but to decompose the combined modifications in WDM6_ICE so as to isolate which one governs the phase partitioning change. The WDM6_SP experiment serves this purpose by applying only the spherical shape change to WDM6. We have clarified this motivation in the Methods section.

2. The discussion part contains comparisons with other studies from the M-PACE campaign and that is good, but the manuscript could benefit from a broader picture. If the authors want to convey that their conclusions are only relevant for this specific cloud regime/case, that should be made clearer. On the other hand, if their conclusions should be valid more generally, it is necessary to better describe how the selected cloud case compares to others. Other cases may be other cases from M-PACE, but may also be cases from different Arctic campaigns (e.g. MOSAIC, NASCENT).

   : The study is intended as a case-specific, mechanistic analysis rather than a general performance assessment, so we have made the single-case scope explicit rather than comparing against additional cases. A paragraph has been added to the Conclusions stating that the findings are based on a single M-PACE case and should be interpreted accordingly, that the case was selected as a persistent, single-layer mixed-phase deck under a steady cold-air outbreak which isolates the microphysical pathways, and that the regime-dependent behavior is presented as a mechanism demonstrated for this case whose quantitative generality across the broader range of Arctic mixed-phase conditions remains to be tested with additional

cases in future work. Consistent statements are given in the Introduction and in the case description (Section 2.2).

3. Regarding the cloud droplet nucleation: Why was the minimum CCN number chosen to be 100 cm-3? The authors state that this is a higher value than realistic for Arctic conditions and discuss the implications, but I miss an explanation for why this value was chosen from the beginning. (Even if it simply is, because that's the default value of the WDM6 scheme, this should be mentioned.)

   : The minimum CCN concentration of 100 cm $^{-3}$ is the default value of the WDM6 scheme (Lim and Hong, 2010), and it was not modified in this study. We have clarified this in Table 1 and in the text, where the CCN floor is now explicitly identified as the default WDM6 value. As discussed in Section 4.4, this default value is higher than typical Arctic CCN concentrations, which contributes to the larger-than-observed droplet production in WDM6_ICE.

Specific comments:

Abstract: I suggest to include in the abstract that this study analyses a case from the M-PACE campaign.
: The abstract now states that the study analyses a case from the Mixed-Phase Arctic Cloud Experiment (M-PACE).

Line 42: What do you mean by "distinct ice representation behavior"? Please consider reformulation.

: This sentence has been reformulated to state the intended meaning more precisely. The revised text specifies that the diagnostic ice number treatment in WDM6 causes it to produce more cloud ice than other microphysics schemes in mid-latitude environments, consistent with the cited studies (Comin et al., 2018; McMillen and Steenburgh, 2015).

Line 50-52: This statement needs a reference.
: The reference (Park and Lim, 2023) has been added to this statement.

Line 78: The city of Barrow is now called Utqiagvik.
: The city name has been updated to Utqiagvik (Barrow).

Section 2.1/2.2: I think, the introduction of section 2.2 would better fit at the beginning of the whole Section 2. Maybe you could move it there or merge 2.1 and 2.2 in general?
: The introduction of the case description has been moved to the beginning of Section 2, so that the case description is now Section 2.1 and the data description follows as Section 2.2, as suggested.

Line 109: I am not sure if I understand what you mean by "Arctic boundary layer conditions" and suggest to reformulate.

: The phrase "Arctic boundary-layer conditions" has been clarified by describing the specific boundary-layer structure of this case in the revised case description (Section 2.1). The shallow, well-mixed boundary layer under a steady cold-air outbreak is now described explicitly ("Its single-layer, well-mixed boundary-layer structure under steady cold-air-outbreak flow makes it particularly suitable for isolating the individual effects of ice-shape and nucleation modifications…"), and the original sentence referring to "Arctic boundary-layer conditions" has been removed accordingly.

Line 116: Correct the reference: Skamarock et al., not Wet al., 2021.
: done

Fig. 2: Could you compare cloud fraction with ERA5 as well?
: A comparison of the simulated low-level cloud fraction with ERA5 is shown in Fig. R1. Both the model (CLDFRA) and ERA5 cloud fractions are averaged below 1500 m, consistent with the CLDLOW field in Figure 2. Relative to ERA5, WDM6 underestimates the low-level cloud fraction over most of the domain, whereas WDM6_ICE overestimates it, consistent with the contrast in cloud cover between the two schemes shown in Figure 2. This comparison is provided here in response to the reviewer rather than added to the main text, where Figure 2 already presents the cloud fraction fields.

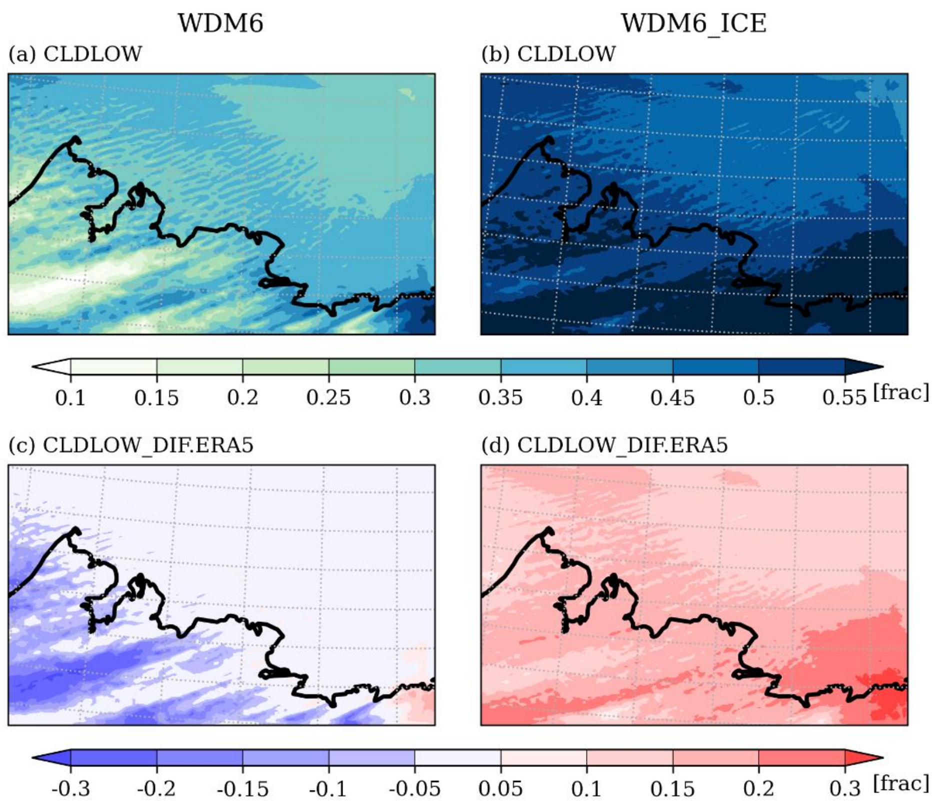

Figure R1: Time-averaged (12:00 UTC 9 October – 11:00 UTC 10 October 2004) low-level cloud fraction (below 1500 m) over domain D03 for (a) WDM6 and (b) WDM6_ICE, and the corresponding differences from ERA5 for (c) WDM6 and (d) WDM6_ICE.

Line 189: Please start a new paragraph when starting to write about WDM6_ICE.
: A new paragraph now begins at the description of WDM6_ICE in this section.

Line 208-209: Can you compare PBL with observations, too?
: Radiosonde-derived PBL height estimates at BAR have been added to the PBL height comparison in Fig. 4c (Fig. R2 of this response). The radiosonde PBL height, defined as the altitude of the maximum potential-temperature gradient, is 1205, 1369, and 1494 across the three radiosonde times (12:00, 17:00, and 23:00 UTC 9 October 2004), higher than both simulations (774.4 m for WDM6 and 992.3 m for WDM6_ICE). Both schemes therefore underestimate the observed PBL height, with WDM6_ICE closer to the observations. The text has been revised from "PBL height also differs, with WDM6 simulating 775.4 m and WDM6_ICE producing 992.3 m (Fig. 4c)." to "PBL height also differs between the schemes, with WDM6 simulating 774.4 m and WDM6_ICE producing 992.3 m (Fig. 4c). The radiosonde-derived estimates at BAR (1205, 1369, and 1494 m) are higher than both simulations, so both schemes underestimate the observed PBL height, although WDM6_ICE is closer to the observations."

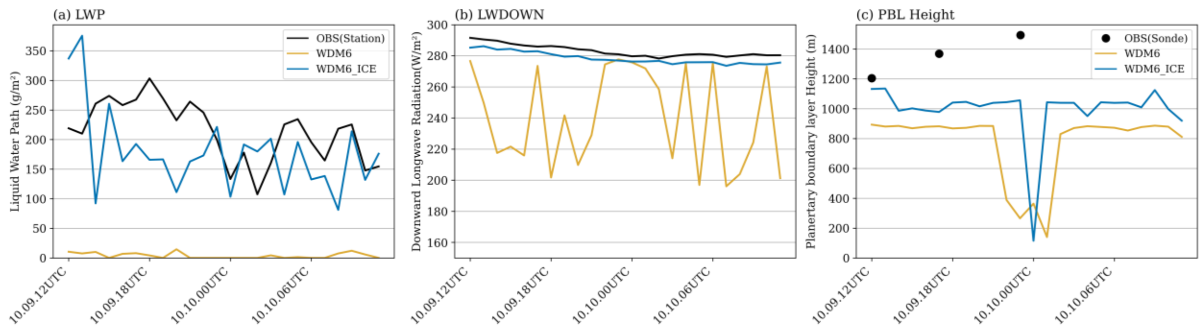

Figure R2: Time series (12:00 UTC 9 October – 11:00 UTC 10 October 2004) at BAR of (a) liquid water path (LWP), (b) downward longwave radiation (LWDOWN), and (c) planetary boundary layer (PBL) height. Observations are shown in black, with station time series in (a) and (b) and radiosonde-derived estimates in (c) as filled circles for the three radiosonde times (12:00, 17:00, and 23:00 UTC 9 October 2004)

Line 235: It is quite a surprising finding that the LWC in WDM6_ICE is higher than in WDM6_SP_IN even though WDM6_ICE has more ice formation processes. Please highlight and try to explain, don't just go on to IWC that quick.
: We have highlighted and explained this point rather than moving directly to the IWC discussion. A passage has been added to Section 3.3 noting that WDM6_ICE produces a higher LWC than WDM6_SP_IN despite including additional ice-formation processes, and that this coincides with a lower total ice content in WDM6_ICE (Fig. 6; Table 3). The added text explains that the additional processes contribute little under the narrow temperature range and constrained ice sizes of this case, while the prognostic cloud ice number treatment keeps the cloud ice number low once it is removed, in contrast to the diagnostic treatment that restores it from the ice mass at each step, leaving more vapor available for condensation. The detailed mechanism is discussed in Section 4.4, where the limited role of the additional processes and the effect of the prognostic cloud ice number treatment are described.

Figure 7: To combine orange and green lines in a plot is not very colorblind-friendly. Please adapt your color choice.

: The color scheme in Figures 6 and 7 has been revised for colorblind accessibility. The revised Figure 7 is shown in Fig. R3.

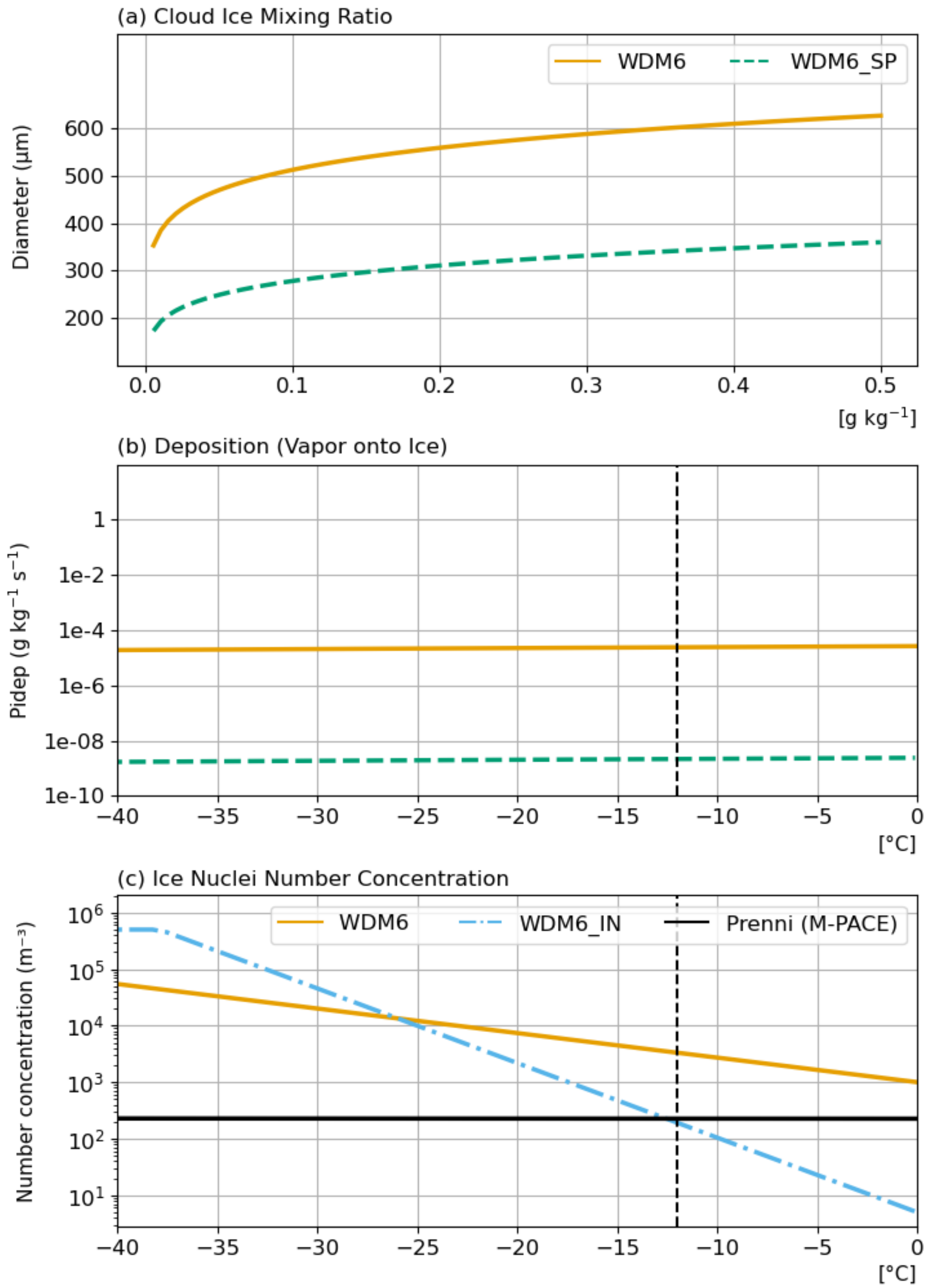

Figure R3: (a) Relationship between cloud ice mixing ratio and diameter in WDM6 (orange, solid) and WDM6_SP (bluish green, dashed). (b) Deposition rate of water vapor onto ice (assuming ice mixing ratio of 0.1 g kg^{-1} and supersaturation of 10%) as a function of temperature. (c) INP number concentration (m^{-3}) as a function of temperature for WDM6 and WDM6_IN (sky blue, dash-dot). The black line shows M-PACE observations (Prenni et al., 2007). Note that the observational upper limit of 500 L^{-1} (DeMott et al., 2010) corresponds to 5 × 10^5 m^{-3}. The vertical dashed line marks −12 °C, representing the approximate cloud-top temperature during M-PACE.

Line 275-277: This is an interesting teaser. Could you expand a bit on the impacts of the remaining structural differences?

: The impacts of the remaining structural differences have been expanded in Section 4.4, which now describes the three structural factors separately. For the prognostic cloud ice number treatment, the text explains that, in contrast to the diagnostic treatment that restores the number from the ice mass at each step, the prognostic number concentration stays low once particles are removed unless new nucleation occurs, so it remains persistently low in this case. For the restricted vapor-deposition-nucleation threshold (−8 ℃ versus 0 ℃), the text notes that it prevents nucleation of new cloud ice over the warmer part of the cloud layer and narrows the range over which the ice number can be replenished. For the additional ice-phase processes (contact freezing, riming, and Hallett–Mossop multiplication), the text states that they contribute little under the temperature range and constrained ice sizes of this case, with the detailed reasoning given in the following paragraph. The prognostic treatment is identified as the most fundamental of the three.

Line 308-311: This statement is too general in its current formulation. This sentence implies for example that secondary ice production is "effectively inactive under the present Arctic conditions". While this might be true for the studied case, many other studies find that secondary ice production is important in Arctic mixed-phase clouds. Please change accordingly.

: We agree and have revised the statement to make clear that the limited role of secondary ice production applies to this particular case rather than to Arctic mixed-phase clouds in general. The text now specifies that the Hallett–Mossop mechanism represented in WDM6_ICE is limited under the temperature range of this case, while noting that secondary ice production has been reported to be important in many Arctic mixed-phase clouds, including for the M-PACE period when mechanisms such as droplet shattering and ice–ice collisional breakup are considered (Zhao et al., 2021; Pasquier et al., 2022). The revised text states that the limited role identified here reflects both the specific conditions of this case and the particular secondary ice mechanism represented in the scheme, rather than a general inactivity of secondary ice production.

Reference

Pasquier, J. T., Henneberger, J., Ramelli, F., Lauber, A., David, R. O., Wieder, J., Carlsen, T., Gierens, R., Maturilli, M., and Lohmann, U.: Conditions favorable for secondary ice production in Arctic mixed-phase clouds, Atmos. Chem. Phys., 22, 15579–15601, https://doi.org/10.5194/ACP-22-15579-2022, 2022.

Zhao, X., Liu, X., Phillips, V. T. J., and Patade, S.: Impacts of secondary ice production on Arctic mixed-phase clouds based on ARM observations and CAM6 single-column model simulations, Atmos. Chem. Phys., 21, 5685–5703, https://doi.org/10.5194/ACP-21-5685-2021, 2021.

Line 318, 330, and more places: In the majority of the manuscript, processes are abbreviated with capital letters, but not in these lines. Please make that consistent across the whole manuscript.

: We have unified the capitalization of all microphysical process-rate names throughout the manuscript, including Line 330 and the figure captions. The process names now appear consistently as Pidep, Psdep, Pigen, Piacw, Pinud, Psacw, Pcond, Praut, Pracw, Psaut and Psaci, and each process is now defined at first use.