

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 CLDLLOW 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.

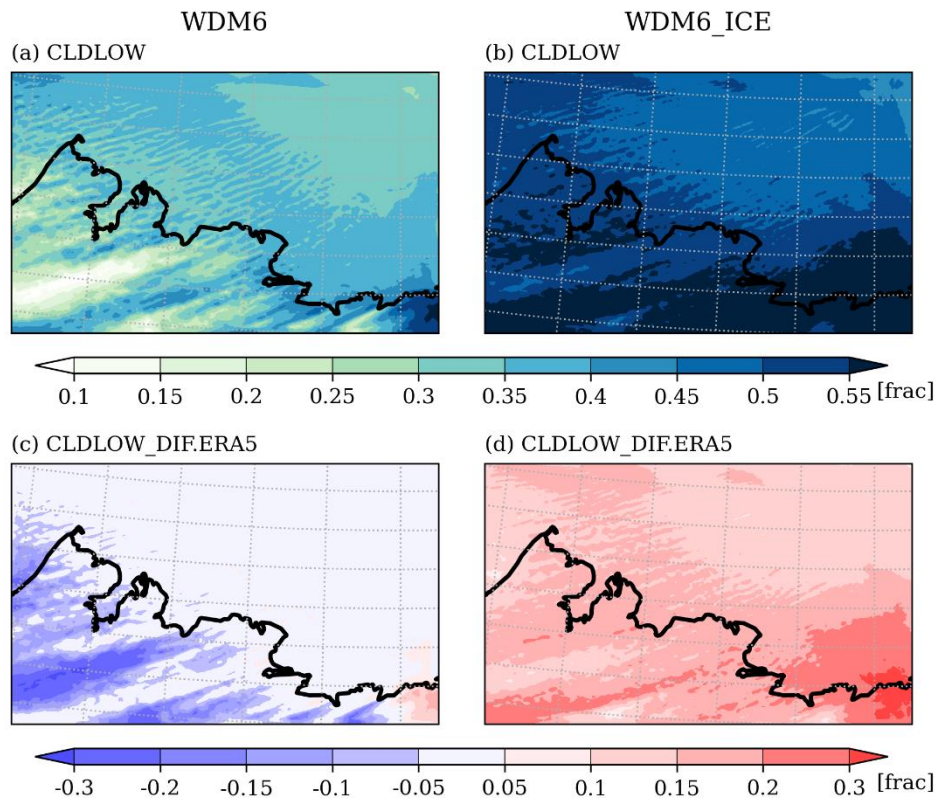


Figure R1: Time-averaged (12:00 UTC 9 October – 11:00 UTC 10 October 2004) low-level cloud fraction (below 1500 m) over domain D03 for (a) WDM6 and (b) WDM6_ICE, and the corresponding differences from ERA5 for (c) WDM6 and (d) WDM6_ICE.

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."

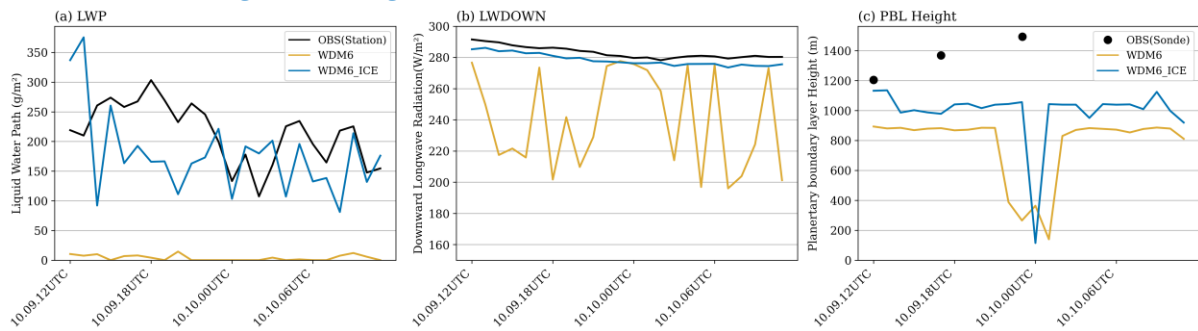


Figure R2: Time series (12:00 UTC 9 October – 11:00 UTC 10 October 2004) at BAR of (a) liquid water path (LWP), (b) downward longwave radiation (LWDOWN), and (c) planetary boundary layer (PBL) height. Observations are shown in black, with station time series in (a) and (b) and radiosonde-derived estimates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

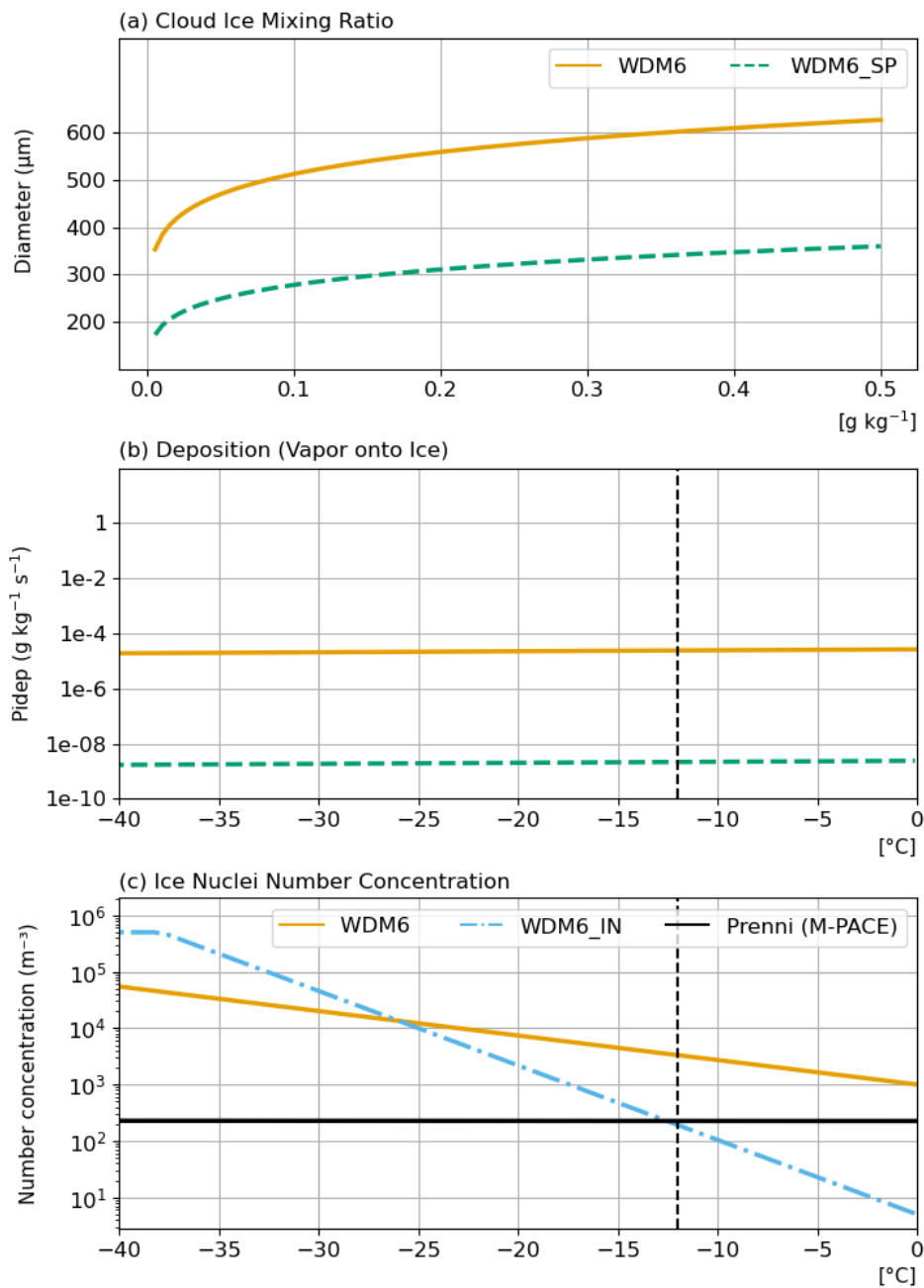


Figure R3: (a) Relationship between cloud ice mixing ratio and diameter in WDM6 (orange, solid) and WDM6_SP (bluish green, dashed). (b) Deposition rate of water vapor onto ice (assuming ice mixing ratio of 0.1 g kg⁻¹ and supersaturation of 10%) as a function of temperature. (c) INP number concentration (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 ($-8\text{ }^{\circ}\text{C}$ versus $0\text{ }^{\circ}\text{C}$), 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.