

Review comment to: “Sensitivity of Arctic mixed-phase cloud simulations to ice microphysical modifications in the WDM6 scheme of WRF (v4.3.1)” by Sung et al.

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.

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.
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.
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.

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.
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.

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.
2. Please clarify that the 20 levels below 800 hPa are included within the total 50 vertical levels, rather than additional levels.
3. Please indicate in the Fig. 2 caption that the plotted spatial fields correspond to D03, if that is the case.
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.
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.
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.

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.
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.