Revision of 'Modeling the Coupled and Decoupled states of Polar Boundary-Layer Mixed-Phase Clouds '

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December 18, 2025

This document contains the response to the second review of 'Modeling the Coupled and Decoupled states of Polar Boundary-Layer Mixed-Phase Clouds ' submitted to EGUSPHERE for possible publication in Atmospheric Chemistry and Physics. Comments from the Reviewer are in black and answers are in blue. Paragraphs that have been added or modified during the revision process are copied in purple.

Reviewer #2

Overview

The authors present a clearly written model development work that relies upon single-column model (SCM) simulations of two well-studied cases of mixed-phase cloud (MPCs). Their modeling approach takes on the revision of phase partitioning to account for the production of supercooled liquid water in updraft regions of turbulent layers that may or may not be coupled to the surface. Another approach that has also proven effective (e.g., Silber et al. 2022 appendix) is application of microphysical process rates in a procedure where a moist turbulence scheme operates on thermodynamic and microphysics fields (e.g., mixing ice species), cloud liquid water is diagnosed from a macrophysics scheme (rapid equilibration), ice formation rate is diagnosed from ambient aerosol-modulated immersion freezing (in addition to multiplication schemes), ice cover is diagnosed by macrophysics, and ice growth then follows from thermodynamic conditions with associated sedimentation offset by turbulent mixing. In the latter approach, phase partitioning is only indirectly affected by turbulence. It would be very interesting to see this new parameterization approach tested against other approaches in a case with rapidly evolving boundary layer depth and cloud-top temperature, such as the ongoing COMBLE-MIP community project (https://arm-development.github.io/comble-mip/README.html). I see no major methodological flaws in the presented work and recommend publication of this relevant work after addressing my comments below (especially comment 3).

We would like to sincerely thank the referee for the review of our manuscript and insightful comments. Please find here below or responses to the comments. We completely agree that assessing the behaviour of the new parameterization in a rapidly evolving boundary layer would be very interesting. In fact this is a work we are planning to do considering two case studies of warm air intrusion and cold conveyor belt during the HALO-AC3 campaign `https://halo-ac3.de/halo-ac3/campaign/`. Even though this is beyond the scope of the present manuscript, we followed your recommendation and included the COMBLE cold-air outbreak case study to our library of SCM cases. We are able to run LMDZ on this case, and assess how the new parameterization captures the structure of the convective MPC. The left panel in Figure 1 below shows that our model captures the formation of a mixed-phase cloud layer along the cold air outbreak trajectory – note that it is a Lagrangian case – that gradually deepens. Ice precipitation increases in the second part of the simulation, along with a decrease in cloud fraction (not shown) which qualitatively agree with the transition from rolls to open-cell clouds during the case. The ice water path and ice precipitation flux (right panels in Figure 1) have a magnitude comparable to that of LES (see `https://arm-development.github.io/comble-mip/README.html`). However, to properly evaluate the model and set LMDZ in a broader international framework of MPC modelling, we would like to officially participate in the intercomparison exercise and benefit from the jupyter-notebook evaluation tools developed by the COMBLE community. This is a request we are planning to send very soon.

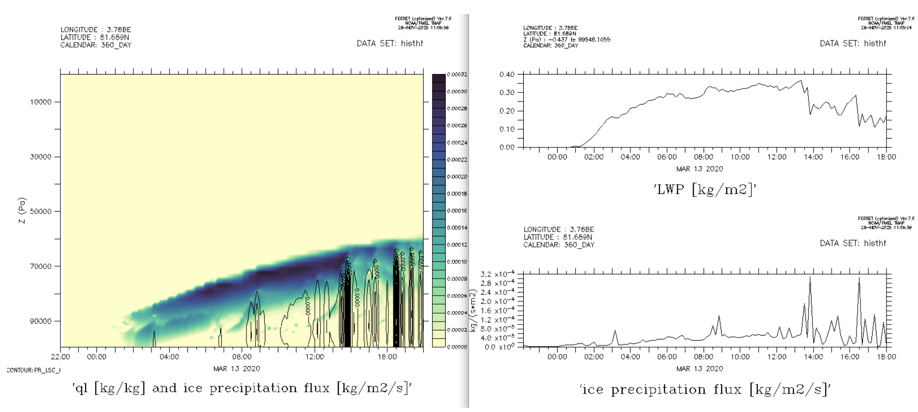

Figure 1: Illustration of recent LMDZ SCM simulations on the COMBLE cold-air outbreak case. The left panel shows a time height plot of the specific liquid water content (shading) and of the ice precipitation flux (contours). The right panels show the time series of the liquid water path and surface snowfall.

1. Regarding the second paragraph of the introduction, I would urge the authors to consider another much simpler approach to explaining polar mixed-phase clouds (e.g., Silber et al., 2021), namely by considering their features as a readily understood outcome of weak heterogeneous ice formation from their familiar warm (liquid-only) counterparts. This allows a more intuitive explanation of many widely observed key features. For instance, in a well-mixed coupled case in the limit of weak ice formation (approaching the warm cloud case), cloud liquid is roughly adiabatic (consistent with meteorology 101). Once heterogeneous ice formation is sustained in the immersion mode at the very weak levels it is typically observed (cf. Silber et al., 2021), it serves as a very weak sink of moisture and naturally "the liquid remains at the top". This also readily explains the resilience, which I think should not be unexpected at all and is reproduced by higher resolution models. I therefore suggest to avoid furthering the "unexpected" idea by repeating it here, because it is based on the mistaken baseline assumption that there is no relevant time scale to ice growth and sedimentation. Taking the warm case as a foundation also readily explains the commonality of continuous liquid bases observed by lidar (see examples in Silber et al., 2021), which becomes circuitous in the current explanation owing to dependence on updrafts (note: adiabatic liquid water content in a well-mixed layer is independent of updraft strength). In my opinion, referring to WBF also introduces an unnecessary overlay that is not encoded in models as a "process" because models don't need to add anything to the basic physics: namely, ice is growing everywhere that relative humidity exceeds saturation with respect to ice, continuously both above and below any supercooled liquid cloud bases and there is no need to consider any further explanation separately within versus below liquid cloud base.

Thank you for this insightful comment. We fully agree with your point. The fact that, in the limit of weak ice formation, cloud liquid water remains roughly adiabatic explains why models with a well-tuned shallow convection scheme are able to reasonably reproduce the cloud liquid water content and structure—provided that unrealistic ice partitioning schemes, such as purely temperature-based ones, are deactivated or replaced with more physically based microphysics. In addition, this also explains the rather weak parametric sensitivity of the simulated LWP in the surface-coupled and liquid-dominated M-PACE case in our LMDZ simulations (see Fig. 4a). Similarly, we agree that the WBF process is not directly parameterized in models, but rather emerges from the parameterized water deposition as soon as the relative humidity with respect to ice exceeds 100%, which occurs near supercooled liquid layers. Nonetheless, we think that the originally 'surprising' aspect of the resilience of supercooled droplets in clouds mostly stems from the scarcity of ice nucleating particles and of the frequently weak role of heteogeneous freezing processes. And in fact, the liquid content becomes dependent on updrafts strength as soon as the ice crystal content becomes significant [**Korolev˙2008**] but this does not concern the liquid-dominated MPCs found in many polar boundary-layer contexts. However, this effect becomes non negligible in ice-dominated mid-level or deep clouds such as nimbostratus clouds (e.g., [**Gehring˙2020**]). Following your recommendation,

the paragraph in the Introduction has been rephrased as follows:
Observational campaigns at the poles have revealed the resilience of boundary-layer MPCs which can persist for several days. This resilience could *a priori* be surprising given the thermodynamical instability of SLW droplets at $T < 0$°C and their depletion through vapor deposition towards ice crystals as the relative humidity with respect to ice exceeds 100 % [**Shupe˙2006**, **Morrison˙2012**]. The formation of SLW in polar boundary-layer clouds results from interactions between turbulence, microphysics and radiation [**Korolev˙2017**]. In turbulent updrafts, generated either by convective instability at the surface [**Shupe˙2008**] or by cloud-top eddies induced by radiative cooling [**Simpfendoerfer˙2019**, **Barrett˙2020**], the relative humidity can reach saturation with respect to liquid through air adiabatic cooling during ascent [**Korolev˙2003**]. Cloud droplets can thus form almost adiabatically and are advected upward, thereby forming a thin – a few hundred meters deep – liquid layer. Most of the time, the scarcity of ice nucleating particles (INPs) [**Eirund˙2019**, **Creaman˙2022**, **Wex˙2025**] in polar regions makes heterogeneous freezing processes weakly active. Subsequently, the vapor deposition overall serves as a very weak sink of moisture [**Silber˙2021**] which explains the commonality and resilience of liquid-bearing clouds [**Silber˙2020**] at the poles. The growth of ice crystals through vapour deposition and riming [**Maherndl˙2024**, **Chellini˙2024**] make them sediment below – and separate from – the liquid layer.

2. Regarding the introduction to model capabilities (lines 60), I would suggest to add more than one reference showing that many higher resolution models can perform very well indeed for both coupled and decoupled MPCs as long as ice formation rate and ice properties are realistic. For instance, for a coupled case, Tornow et al. (2025) illustrate mixed-phase simulations that can reproduce basic features of sustained liquid water path, precipitation onset and subsequent cloud cover and droplet number concentration reduction. For a decoupled case, Silber et al. (2019, 2020) present large-eddy simulations that reproduce the progressive development of supercooled liquid in a stable layer, turbulence onset, and in that case, the sustained coexistence of liquid and ice precipitation processes. I would also add that the Lagrangian approach taken in those studies provides a strengthened foundation for large-scale model development because it allows a test of whether a mixed-phase cloud can realistically form within an initially cloud-free environment and proceed to reproduce observed transitions. Silber et al. (2022) also illustrate that the NASA ModelE3 GCM code can well reproduce the decoupled cloud case in single-column model (SCM) mode (see appendix), including onset of turbulence and co-existing precipitation in two phases.

Thank you for this suggestion. Following your recommendations, the corresponding paragraph in the Introduction has been modified as follows:
Some large eddy simulation (LES) models, cloud-resolving models and mesoscale models can reasonably capture the structure of both surface coupled and surface-decoupled boundary-layer MPCs as long as ice formation rate and ice properties are realistic (e.g., [**Klein˙2009**, **Ovchinikov˙2014**, **Arteaga˙2024**, **Silber˙2019˙2 Tornow˙2025**]).

However, General Circulation Models (GCMs) still struggle to simulate the vertical structure and microphysical properties of surface-coupled clouds (e.g., [**Liu˙2011**, **Gettelman˙2015**, **Zhang˙2020**]). These shortcomings in GCMs are even more pronounced for surface-decoupled MPCs, even though recent single-column simulations with the NASA ModelE3 model show promising results, including onset of turbulence from a purely liquid stratiform cloud and subsequent triggering of ice precipitation [**Silber˙2022**]. Overall, shortcomings in the representation of polar boundary-layer MPCs in GCMs lead to substantial biases in the representation of the surface-based temperature inversion over the wintertime Arctic sea ice in cloudy conditions [**Pithan˙2014**].

3. Regarding the leading problems in large-scale model parameterization, I would have placed first the extreme uncertainty in parameterization of ice formation rate in the immersion mode (e.g., Knopf et al., 2023) and via ice multiplication where it far outpaces the immersion mode (e.g., Korolev et al., 2024; likely same process as in deeper convection from long evidence of colocation with drizzle in MPCs). These are but a few examples of decades of evidence that we cannot capture ice formation rates to one or even several orders of magnitude. No degree of improving other processes can readily cover for that. The Knopf et al. (2023) study further shows how a diagnostic scheme following DeMott type INP parameterizations can produce extremely unrealistic rates of ice formation. Can the authors show that their ice formation rates in these cases are consistent with simple rough estimates of INP source strength and activation rate?
Thank you for this suggestion that we took into account. We now place first the treatment of ice microphysical processes as follows:
Overall, the parameterization of MPCs in GCMs remains extremely challenging and the difficulty mostly lies in: (i) the parameterization of ice microphysical processes [**Forbes˙2014**, **Barrett˙2017**, **Vignon˙2021**], in particular the parameterizations of ice formation rate in the immersion mode (e.g., [**Knopf˙2023**]) and of secondary ice production processes (e.g., [**Pasquier˙2022**, **Sotiropoulou˙2020**, **Possner˙2024**]); Regarding the second part of your comment, we are indeed aware of the limits of the DeMott type INP parameterizations. Note however that in our approach, this parameterization only aims to provide an estimate (diagnostics) of the ice crystal number concentration. We thus cannot directly verify the ice formation rates but we can check whether the prediction of the ice crystal number concentrations concurs with the values measured during the two cases. For ISDAC, we mentioned in the text that the predicted concentration corresponds reasonably well with that measured in **McFarquhar˙2011**. For M-PACE however, the information was missing in the paper. The observed ice crystals number concentration throughout the cloud layer – whose temperature roughly varies from -15 to -10 ^{o}C – ranges between 10^{-1} and 10 L^{-1} with most values of around 1 L^{-1} [**McFarquhar˙2007**]. Our ice crystal estimates in this temperature range using the DeMott parameterization ranges between 0.1 and 2 L^{-1}, depending on the prescribed background aerosols concentration (in the perturbed parameter ensemble experiment). Of course, substantial work remains to be done to properly parameterize ice nu-

cleation in our cloud scheme, but we can at least ensure that the predicted ice crystal number concentration is reasonable. We have added the following paragraph in Sect. 2.2.1:

The observed ice crystals number concentration throughout the cloud layer ranges between 0.1 and 10 L^{-1} with most values of around 1 L^{-1} [**McFarquhar˙2007**]. The INP parameterization used in our cloud scheme with a default value of 1 scm^{-3} provides an ice number concentration roughly between 0.1 and 0.6 L^{-1} for the temperature range within the M-PACE cloud layer, namely between ≈ -15 and -10°C. Those values are thus realistic but in the lower part of the measured range. Simulations with higher ice crystals number concentration will be investigated by varying N_{aero5}.

4. Regarding the case studies selected (section 2.2), a major difference is that ISDAC included continuous nudging of temperature and water vapor whereas M-PACE applied fixed large-scale advective flux divergence profiles. When applying nudging to LES and SCM at every time step, the model thermodynamic profiles cannot diverge as much from one another; in other words, if divergence grows more in one model, it is more offset. Please clarify for readers in the text whether the authors apply nudging in the ISDAC case and whether that differs from the M-PACE case.

We agree this is an important difference that we should clarify in the manuscript. For M-PACE, we now specify:

The advective forcings of the SCM are prescribed and consist in vertical profiles of temperature and humidity horizontal advection terms as well as that of vertical velocity that are constant in time

and for ISDAC:

An important difference in the set-up compared to M-PACE is that the horizontal wind components, temperature and moisture profiles are nudged towards the initial profiles and nudging coefficients are specified to have the height dependency (see Appendix of [**Ovchinnikov˙2014**])