Reviewer #2:

Review of “Impacts of Secondary Ice Production on the Microphysics and Dynamics of Deep Convective Clouds in Different Environments”, by Waman and Colleagues.

Verdict: I recommend the paper be published subject to major modifications to the text and analysis. Improved model validation and analysis is needed.

Response: We appreciate the reviewer’s efforts in reviewing the manuscript. The comments and suggestions given by the reviewer would indeed help us to improve the quality of the manuscript. Below are the point-to-point responses to the reviewer’s comments.

We would like to draw the reviewer’s attention to several important changes in the model setup adopted in the revised analysis.

First, we re-performed simulations for all cases of deep convective clouds (DCCs) using an updated treatment of the rime-fraction (ψ) in secondary ice production (SIP) during ice-ice collisions (IIC). In contrast to the previous fixed rime-fraction ($\psi = 0.4$) assumption, the revised simulations use a particle size-dependent rime-fraction parameterization following Gautam et al. (2025, Table 2 therein).

Second, careful assessment of our revised simulations shows that, across all DCC cases, together with other SIP processes, fragmentation of freezing raindrops (RDF) using the Phillips et al. (2018; PHIL18) scheme exhibit better agreement with the observed filtered ice number concentrations (INC) than the Sullivan et al. (2018; SULL18) RDF scheme. Therefore, the revised control simulations adopt RDF based on PHIL18.

Third, our revised analysis of INP concentrations indicates that the Hande et al. (2015) treatment of immersion-freezing INP activation underestimates the observed INP concentrations over the selected study regions. Accordingly, in the revised simulations, the immersion-freezing INP activation was scaled to match the observed INP concentrations for each simulated case. This INP scaling was applied consistently to both the “control” and “No-SIP” simulations.

Based on these updates, the revised manuscript redefines the control simulations using the variable rime-fraction approach together with RDF from PHIL18, while all simulations additionally include the revised INP scaling. All subsequent analyses and comparisons are presented based on these updated simulations.

Additionally, during the validation of the filtered INC for the simulated DCMEX convection, the observed INC measured by the 2DS probe had previously been plotted using incorrect units (cm^{-3} due to a labeling error, whereas the data were intended to be presented in L^{-1}. This has now been corrected, and the observed INC is consistently shown in L^{-1}) throughout the revised manuscript, including Fig. 7f and Figs. S2d, S3b, and S4(d–i) in the supplement.

In the earlier version of the manuscript, the process rates were incorrectly reported in units of kg^{-1} s^{-1}, whereas the analysis was actually performed using units of kg^{-1}. In the revised manuscript, this issue has been corrected, and the process rates are now consistently reported in the appropriate units of kg^{-1} s^{-1}.

A detailed description of these revisions is provided below. Please note that we have marked the reviewer's comments in **red** and corresponding responses from the authors are in **black**.

Major Comments

Reviewer: This paper is commendable in its overall aim to gauge holistically the effect on deep convection in contrasting cases, covering dynamics (latent heating, ascent), radiative effects and the microphysics. Yet there are some issues:

- Inconsistent model–observation comparisons (filtered vs total ice; medians vs means)
- Lack of key validation metrics (INP concentrations, vertical velocity statistics, precipitation time series)
- Missing process-level diagnostics (domain-integrated SIP budget)
- Overinterpretation of dynamical impacts (stratiform vs convective ascent)

Response: We thank the reviewer for these constructive comments and suggestions. In the revised manuscript, we have addressed several of the concerns raised. Specifically, for all simulated DCCs, we now compare the simulated size-filtered INC with the corresponding aircraft observations to ensure consistency between simulated and observed INCs. We have also added comparisons of modeled and observed INP concentrations, as well as vertical velocity statistics, in the revised manuscript.

However, we disagree that the manuscript lacks key validation metrics. The manuscript already includes extensive evaluation of the simulated DCC properties against multiple observational datasets (Figs. 6-9), including radar reflectivity CFADs, INC, LWC, IWC, CDNC, and accumulated surface precipitation (for marine DCCs in which SIP impact is found to be more pronounced) across both continental and marine cases. These diagnostics were selected to evaluate the simulated cloud microphysical structure and the associated dynamical response relevant to the present SIP analysis.

Regarding the SIP process budget, the revised manuscript retains the vertical profiles of the corrected mean SIP process rates to quantify the relative contributions of individual SIP mechanisms. We chose this representation to preserve the vertical structure of the SIP processes within simulated DCCs, which is central to the interpretation of the results in this study.

Reviewer: Specifically, the model validation against radar reflectivity is fine. But against the aircraft data, the comparison between modeled and observed ice concentrations is difficult to interpret because the model total ice concentration is compared with size filtered observations. A consistent comparison (i.e., applying the same size threshold to model output) would be necessary to assess agreement. Also, none of the sample means of observations are shown, only medians, on the validation plots for aircraft data. It is the sample mean that is the estimator of the population mean.

Response: We appreciate the reviewer's point about the need of consistent comparison between modeled and aircraft-observed total ice number concentration (INC). In the revised manuscript, we clarify that the aircraft measurements represent size-filtered INC for each simulated deep convective clouds (DCCs) case (NI_{400} for CAIPEEX, NI_{100} for DCMEX, NI_{200} for MC3E). Accordingly, we now apply the corresponding size thresholds to the simulated INC to ensure a like-for-like comparison between the modeled and observed INCs (Figs. 6-9 in the revised manuscript).

In the revised manuscript (Figs. 6-9) and Fig. R1 below, we now show the sample means. The revised analysis demonstrates that the simulated INCs generally agree well with the corresponding observations after applying the consistent size filtering. The same is true for other simulated properties (LWC, TIWC, CDNC), as shown in Fig R2 (below for LWC) and Figs. 6-9 in the revised manuscript.

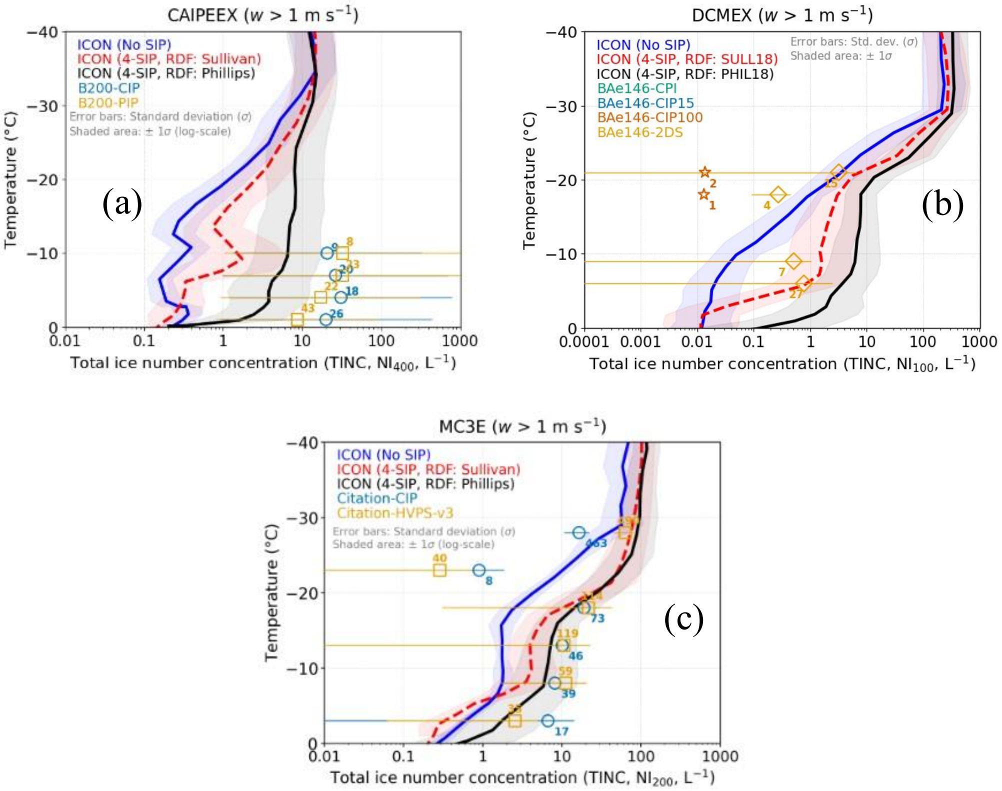

Figure R1: *Comparison of the simulated mean filtered ice number concentrations against the observations from optical probes mounted on the aircrafts, during (a) CAIPEEX (NI$_{400}$), (b) DCMEX (NI$_{400}$), (c) MC3E (NI$_{200}$), in the cloudy convective updraft regions. Note that for this comparison, ICON grid points are collocated in space and time with the track of the aircrafts operational during these campaigns.*

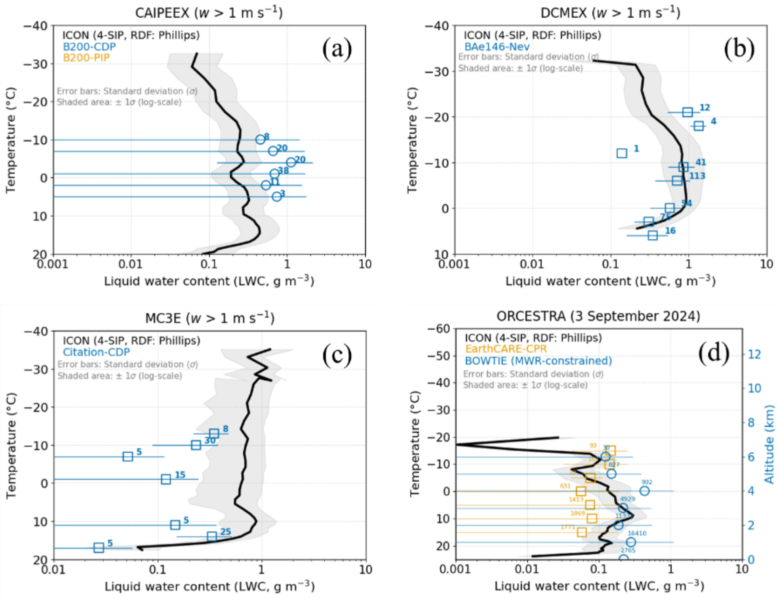

Figure R2: *Comparison of the simulated (control runs) mean liquid water content (LWC) against the observations from optical probes mounted on the aircrafts, during (a) CAIPEEX, (b) DCMEX, (c) MC3E, in the cloudy convective updraft regions. In (d), the simulated LWC is compared with the observations from EarthCARE CPR (yellow squares) and Cloudnet retrievals from RV Meteor ship operational during the BOWTIE campaign in the ORCESTRA study region. Note that for this comparison, ICON grid points are collocated in space and time with the track of the aircrafts operational during these campaigns.*

Reviewer: Thus, the microphysical validation plots are not "apples-to-apples" comparisons. Thus, what appears to be strong disagreement in ice concentration (e.g., Fig. 7d for DCMEX) may partly reflect inconsistencies in the quantities compared rather than true model bias. If the ice concentration were really orders of magnitude too high, then the supercooled liquid water content would be far too low, but that is not apparent for DCMEX, so the disagreement may have been overstated.

Response: We thank the reviewer for this important comment. As discussed above, in the revised manuscript, the validation of microphysical properties (e.g., Fig. 2 above, Figs.6-9 in the revised manuscript) has been updated to ensure consistent comparisons between simulations and aircraft observations.

Also, for DCMEX DCCs, we previously reported that the simulated total INC within the convective regions exceeded the observed NI_{100} by approximately 1-2 orders of magnitude (Fig. 7d in the original manuscript and Fig. R3). In the revised analysis, we further extended the comparison to the stratiform regions ($|w| < 1$ m s^{-1}). This analysis shows that the simulated NI_{100} agrees more closely with the CPI and 2DS-observed INC (Fig. R3).

For DCMEX, we agree with the reviewer that an overestimation of simulated NI_{100} by several orders of magnitude would cause much larger depletion of supercooled liquid water. However, such strong disagreement is not seen in the simulated LWC. Together with the large spread among the observed INC measurements from different probes, this suggests comparatively larger observational uncertainties in the DCMEX INC measurements, which complicates a quantitative assessment of the modeled INC bias.

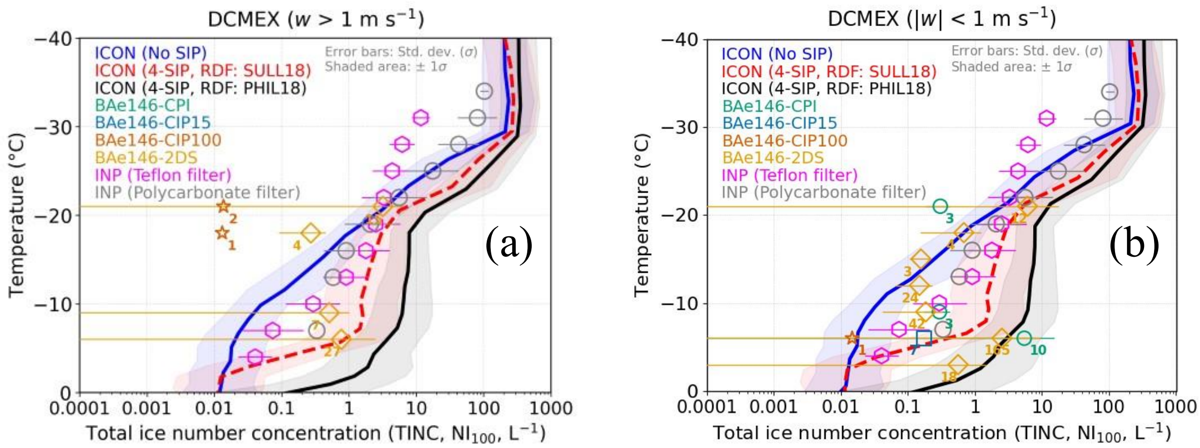

Figure R3: *For DCMEX, NI_{100} from the control (PHIL18, solid black line), SULL18 (dashed red lines), and NO SIP (solid blue lines) simulations is compared against the corresponding observations from CPI, CIP15, CIP100 probes in (a) cloudy convective updraft, and (b) stratiform conditions. Also, in both (a) and (b), observed INP number concentrations from Teflon (magenta circles) and Polycarbonate (gray circles) filters are shown. Error bars and shaded area in (a) and (b) indicate standard deviations.*

Reviewer: Also, there is no comparison of vertical velocity statistics (cumulative frequency histogram) between aircraft and model, so it is difficult to see if the aircraft has properly sampled the deep convection. If the DCMEX aircraft never sampled the convective cores, then that might explain why the measured ice concentrations are so low. There seems no validation of INP active concentrations. In these field campaigns, it seems unlikely that there were no measurements available of INPs, either in the campaign itself or approximately from an adjacent campaign (e.g., similar month, different year). To get the ice concentrations correct and to infer the correct role of the SIP, the primary ice needs to be predicted correctly. And it is unclear how the aerosol chemistry and loadings were initiated for the cases and how aerosol conditions are related to the predicted INP activity.

Response: We thank the reviewer for these important comments. Below are the point-to-point responses to the comments:

1. Validation of Vertical velocity statistics

In the revised manuscript, we have included cumulative frequency distributions (CDFs) of vertical velocity (w) for aircraft-observed continental CAIPEEX, DCMEX, and MC3E cases (Fig. R4). To ensure a physically consistent and spatially representative comparison, ICON grid points are matched with the corresponding aircraft flight tracks in both space and time. This involve sampling ICON output at the geographic locations and altitudes corresponding to each aircraft measurement, using the nearest model output time. The statistics of w are derived by sampling the mixed-phase regions (LWC > 1.e-5 and IWC > 1.e-5 g m^{-3}) and are further restricted to the levels sampled by the aircraft. This approach ensures that both aircraft and model data represent identical dynamical and thermodynamic environments relevant to SIP.

In these simulated continental cases, it is reported that ICON captures the distribution of w quite well. For CAIPEEX, the B200 aircraft adequately sampled mixed-phase convective regions consistent with the model (Fig. R4a), with comparable fractions exceeding $w > 1$ m s^{-}

^{1} (Observed = 40%, ICON = 33%) and $w > 5$ m s^{-1} (Observed = 40%, ICON = 33%). For DCMEX (Fig. R4b), only 5% of observed updrafts are above 1 m s^{-1}, compared to 40% in the model. However, for the strong updrafts ($w > 5$ m s^{-1}), the aircraft and simulated w agrees well, at about 1.4% and 1.6% respectively. This suggest that during DCMEX, Bae146 chiefly sampled stratiform mixed-phase regions, with only occasional passes through intense convective cores. For simulated convection during MC3E (Fig. R4c), a good agreement is seen for weaker updrafts (27% observed and 20% simulated above 1 m s^{-1}), whereas ICON shows more strong updrafts (5%) than the observations (< 1%). This indicates that aircraft mostly sampled stratiform regions.

These comparisons demonstrate that the aircraft generally sampled convective conditions consistent with the simulated mixed-phase conditions in the continental DCCs.

For ORCESTRA, from Fig. R4d it is reported that ICON underestimates the frequency of both moderate (w > 1 m s^{-1}) and stronger (w > 5 m s^{-1}) updrafts, with simulated moderate updraft fraction of only 0.3% compared to observations (23%, Fig. R4d). This discrepancy can be attributed to both weaker simulated convection in this marine case and the limitations of CPR Doppler retrieval of vertical motion in precipitating clouds (Kim et al., 2025).

This is now discussed in the revised manuscript (Figs. 6-8, lines 362-366, 387-389, 410-412, 428-432).

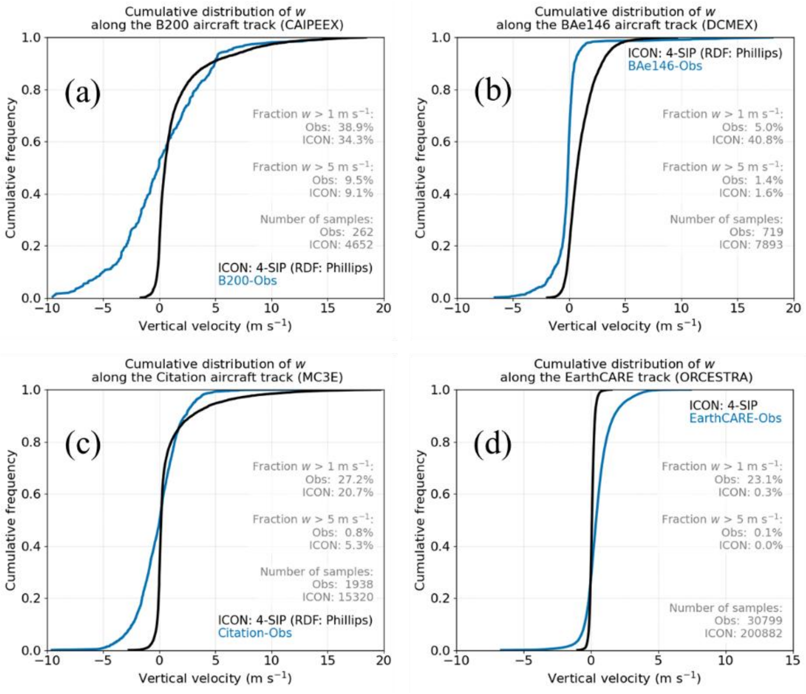

Figure R4: *Comparison of cumulative frequency distribution (CDF) of the simulated (black lines) and aircraft-observed (blue lines) vertical velocities in (a) CAIPEEX, (b) DCMEX, (c) MC3E DCCs. In (d) simulated vertical velocity (black line) is compared with EarthCARE-CPR retrievals (blue line). Note that the ICON grid points are collocated in space and time with the corresponding aircraft and EarthCARE tracks for all continental, and marine DCCs respectively.*

2. Validation of INP concentrations

We thank the reviewer for raising this important point and for the constructive suggestion to use observations from adjacent years where direct INP measurements are unavailable. We have now added a validation of the INP parameterization against the best available observations for each simulated case (Fig. R5 below).

To clarify, our simulations use the Hande et al. (2015) INP activation scheme, which activates INPs through immersion (between −12 and −36°C) and deposition (at levels colder than −20°C) modes as empirical functions of temperature. This treatment of INP activation therefore does not prescribe explicit aerosol chemistry or aerosol loadings.

We compared original Hande et al. (2015) immersion mode parameterization against available INP observations. Our analysis found that the original immersion scheme (Fig. R5, dashed gray lines) significantly underestimates the observed INP concentrations in all simulated cases. We therefore derived the temperature-based scaling factor to match the original scheme of immersion mode, between temperatures −12 and −36°C, with the corresponding observations. The observational datasets used for each case are as follows: For CAIPEEX, we utilized monthly mean profile of INP observations from Patade et al. (2026). For DCMEX, observed INPs are directly available from the campaign. For MC3E, observations are made available from DeMott et al. (2016) and Waman et al. (2022). For ORCESTRA, marine INP observations from collected during Ice in Clouds Experiment-Dust (ICE-D) field campaign (Price et al. 2018) with the FAAM-BAe146 aircraft in a region (Santiago, Praia) close to the study domain are used.

This scaling approach implicitly accounts for the local aerosol conditions over selected study regions and therefore ensures that heterogeneous ice nucleation, which crucially quantifies the impact of SIP, is observationally well-constrained for each simulated case.

The impact of this INP scaling on the simulated cloud properties (INC and heterogeneous ice process rates) is shown in Fig. R5(e–h) for the DCMEX and MC3E convection cases. Scaling the original Hande's INP spectra to match observations increases the total INC by only 10% in DCMEX, and up to 40% in MC3E. The enhanced INP concentrations also lead to up to 50% greater ice-crystal production through heterogeneous ice nucleation. Similar changes are found in the other simulated cases (CAIPEEX and ORCESTRA; not shown).

This extra set of validation and description is now added in the revised manuscript for each simulated case (Figs. 6-9).

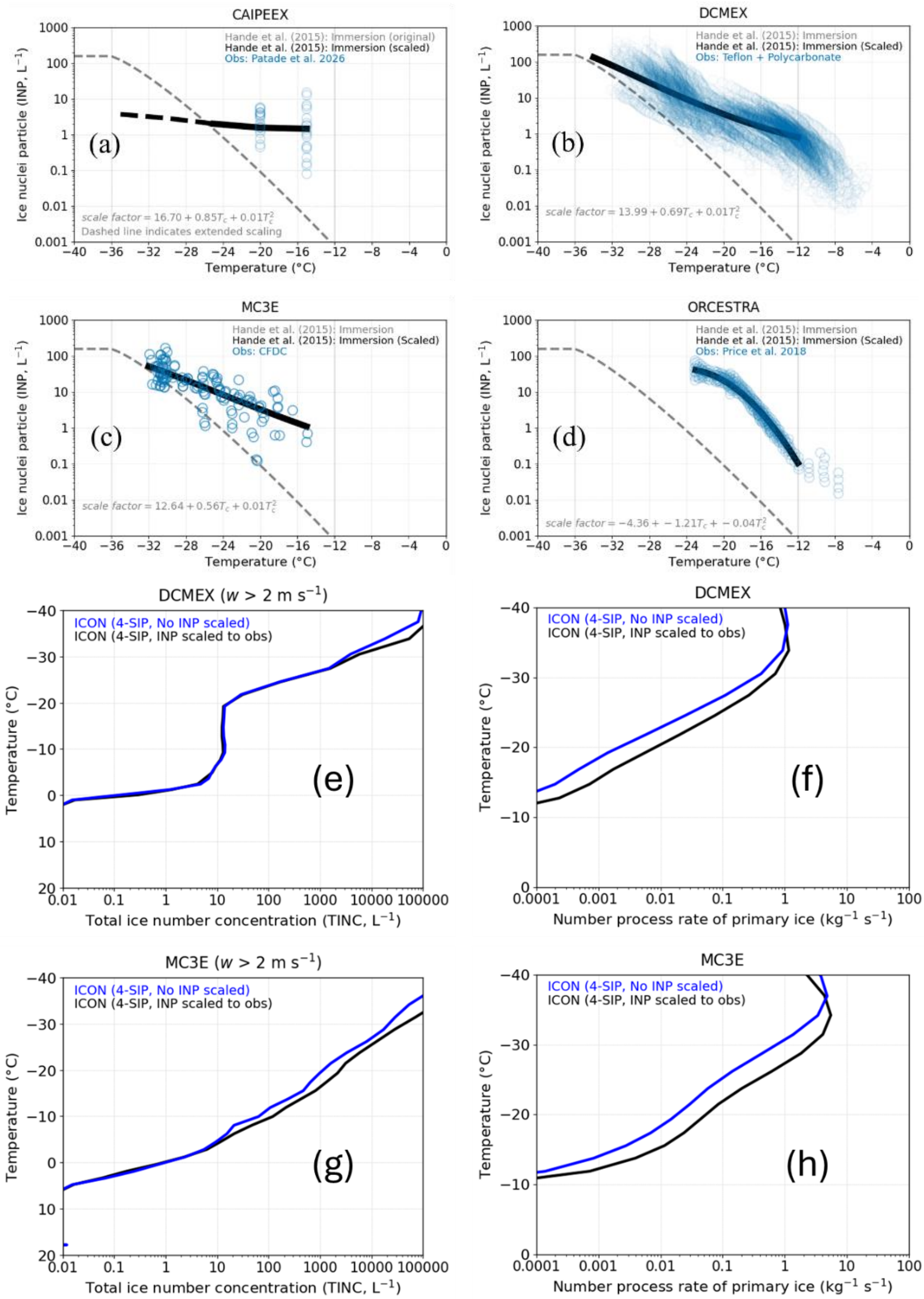

Figure R5: *Comparison of the original (dashed gray lines) Hande et al. (2015) immersion freezing scheme of INP activation against the field observations for the simulated cases from (a) CAIPEEX, (b) DCMEX, (c) MC3E, and (d) ORCESTRA. In (a-d), thick black lines indicate scaled Hande et al. (2015), which is derived by applying temperature-based scaling adjustment to original Hande et al. (2015) immersion freezing scheme to represent the observed INP distribution for each case. Also, In (e, g) total INCs from simulations without (blue lines) and with (black lines) INP scaling to the observations are shown for cloudy convective updrafts (w > 2 m s^{-1}). In (f, h) the process rates of primary ice are shown for these simulations.*

3. Clarity on the aerosol chemistry and loadings

As discussed above, the heterogeneous ice nucleation scheme of Hande et al. (2015) used in the present study is purely empirical, INP concentrations are parameterized as a function of temperature alone, and therefore no prescribed aerosol chemistry or aerosol loading information is required as input (revised manuscript, lines 252-254).

Reviewer: There is also no validation of surface precipitation as a function of time for all cases, which makes it difficult to visualize the time evolution of each simulated storm.
Response: We appreciate reviewer's suggestion. However, we would like to highlight that the primary objective of the present study is to investigate the impact of SIP on cloud microphysics and dynamics across contrasting convective and aerosol environments, rather than to provide an exhaustive storm-by-storm validation.

We would like to highlight that we have already followed an extensive approach of validation presented in the previous version of the manuscript, which includes LWC, IWC, CDNC, filtered INC, and radar reflectivity CFADs, and newly added comparison of w CDFs and INP concentrations.

We acknowledge that precipitation time series are not shown for the selected continental (CAIPEEX, DCMEX, MC3E) cases. However, we want to draw reviewer's attention to the CFAD analysis of radar reflectivity, which provides statistical representation for all four cases. A CFAD comparison that shows good agreement between simulated and observed reflectivity distributions adequately validates the intensity and vertical extent of the observed convection with that simulated with ICON.

We want to note this adequate statistical agreement would not be achieved if the simulated storm timing and evolution are substantially wrong. We therefore consider the combination of radar reflectivity CFADs for all simulated cases, and the GPM precipitation comparison for ORCESTRA sufficient to establish that ICON adequately simulates the convective evolution and extent across all four cases, which is consistent with the primary focus of this study on quantifying impacts of SIP rather than detailed storm validation.

We acknowledge the reviewer's concern about precipitation time series evolution. However, we would like to clarify that the purpose of the validation in the present study is not to reconstruct the full storm lifecycle but to confirm that ICON accurately captures the observed microphysical and dynamical properties of all simulated DCC cases during period when aircraft or satellite and ground-based measurements were available. The space (±0.03° tolerance) and time co-located comparison of simulated properties (LWC, IWC, CDNC, INC, radar reflectivity, w-statistics) shows that ICON adequately reproduces the observed convective properties. This is sufficient to support the reported impact of SIP on simulated properties. For a case of marine convection observed during ORCESTRA, where SIP impacts on microphysics, dynamics and surface precipitation are most significant, a comparison of simulated accumulated precipitation with GPM observations is already shown (Fig. 9e, supplementary Fig. S5(d, e)).

We therefore believe that adding additional validation metric of precipitation time series for continental cases would only provide additional descriptive context about storm evolution, but not strengthen the validation period directly relevant to the SIP analysis presented in this study.

Reviewer: Regarding the analysis of impact of SIP on microphysics, there needs to be a simple budget of ice initiation (a count of all ice particles initiated) in the entire domain for the 4 SIP mechanisms and primary ice (not including homogeneous freezing), displayed as a pie chart. This will allow the present simulation of MC3E to be compared with the pie chart shown by Waman et al. (2022) for the same SIP mechanisms. The same pie chart can be shown for the PHIL18 run too, to compare with SULL18.

Response: We disagree that including a domain-integrated pie chart of ice initiation budgets is necessary, nor does it provide additional scientific insight compared to the existing mean vertical profiles of process rates for each SIP process and for heterogeneous ice (without homogeneous ice).

We want to highlight that the primary analysis in the revised manuscript (Fig. 10) is based on mean vertical profiles of the process rates, which provide substantially more meaningful information than a domain-integrated pie chart. The vertical profiles quantify not only the relative magnitude of each SIP process, but also their level dependence throughout the simulation period. This information is essential for understanding how SIP activity changes with altitude and which SIP process dominate at different levels in simulated clouds. In contrast, a domain-integrated pie chart would collapse the entire spatial and temporal variability into a single integrated value for each process, therefore obscuring the vertical structure of the activity of each ice initiation process that is central to the present study.

To address the reviewer's request of pie charts, we nevertheless plotted the domain-integrated ice initiation budgets for all simulated cases. For example, for simulated MC3E, Fig. R6a below shows the integrated budget from the present ICON simulation, while Fig. R6b shows the integrated budget from Waman et al. (2022). With this pie chart analysis, the ICON simulation indicates that SIP in IIC dominates the total ice production, whereas the remaining SIP processes each contribute less than 1% to the total initiated ice particles.

Also, for simulated MC3E convection, comparison of the present ice initiation budget with Waman et al. (2022, Fig. R6b) needs careful interpretation. Although both studies analyze the same convection from MC3E, they are based on fundamentally different modelling frameworks and microphysical schemes. Waman et al. (2022) used the WRF-based Aerosol-Cloud (AC) model with a bin microphysics scheme ((Phillips et al., 2008, 2013, 2017a, b, 2018, 2020; Patade et al., 2021, 2022), whereas the present study employs the ICON-NWP model with a two-moment bulk microphysics scheme (Siefert and Beheng 2006). Consequently, differences in the ice initiation budget primarily reflect differences in model structure, microphysical assumptions in implementation of SIP processes, different schemes of heterogeneous ice nucleation (AC uses aerosol-aware Phillips et al. 2013, Patade et al. 2022 scheme, while this study used temperature-based Hande et al. 2015 scheme), rather than providing a direct physical comparison of SIP behavior in MC3E.

Therefore, while a direct comparison of SIP contributions between the two studies could be of interest, such an analysis would require a dedicated evaluation by carefully accounting for differences in model structure, microphysical parameterizations, and the implementation of SIP processes. The present study does not aim to perform a model intercomparison between AC and ICON. Instead, the MC3E case was selected because of its extensive observational dataset, which provides a suitable basis for evaluating SIP behavior within the ICON-NWP framework.

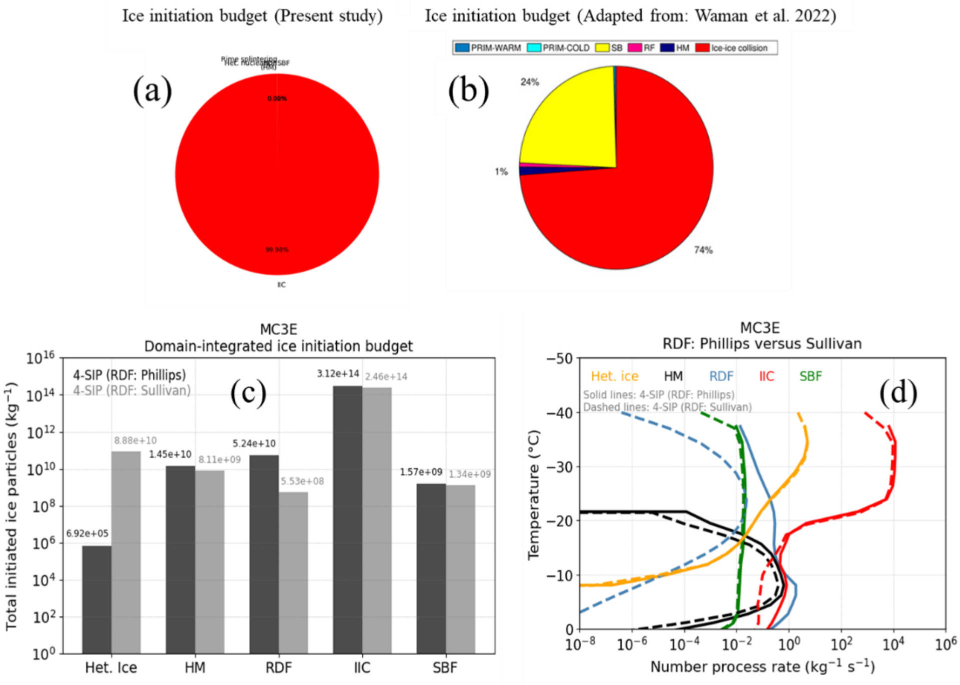

Figure R6: *(a) pie chart comparing the contributions from different SIP processes to total secondary ice formation in (a) control (PHIL18) run of the present study, and (b) similar analysis by Waman et al. (2022) for the simulated MC3E convection. Also, in (c) number of ice particles initiated throughout the simulation period in the control (PHIL18, black bars) and SULL18 (gray bars) simulations is shown. In (d) comparison of number of particles initiated per unit time from PHIL18 (solid lines) and SULL18 (dashed lines) at different vertical levels is shown. In (d) red line represent SIP in ice-ice collisions (IIC), Hallett-Mossop process of rime-splintering (HM, black lines), shattering of freezing raindrop (RDF, blue lines) and sublimation breakup (SBF, green lines), and primary ice (Het. Ice, yellow lines).*

Furthermore, Fig. R7(a, b) below reveals that the separate pie charts for PHIL18 and SULL18 show no differences because the domain-integrated ice initiation budget is strongly dominated by SIP during IIC. To provide a clearer comparison of total ice particle initiation between SULL18 and PHIL18, along with the vertical profiles, we followed grouped-bar chart approach (Fig. R6c above). From this analysis, it is seen that PHIL18 produces about two orders of magnitude more ice particles than SULL18, while changes in other SIP processes remain relatively small. We therefore retain the vertical profile analysis of the process rates (Fig. R6d above, and Fig. 10 in the revised manuscript) as the primary diagnostic in the revised manuscript, and add bar charts and vertical profiles comparing SULL18 and PHIL18 in the revised supplement (Fig. S8).

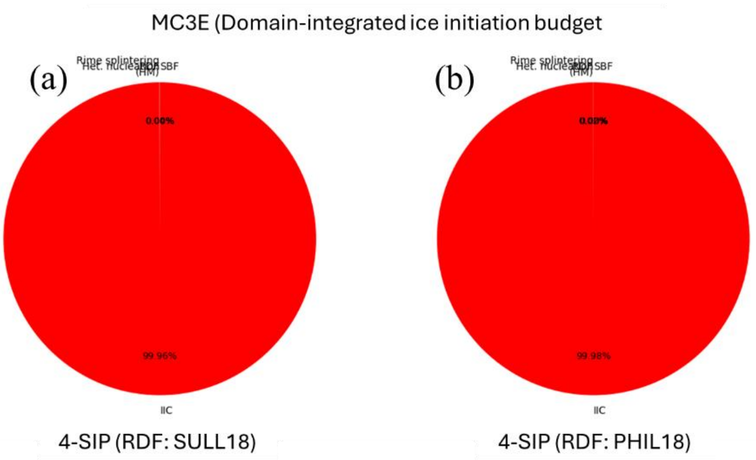

Figure R7: *Pie charts for the contribution from individual SIP process in the (a) control (PHIL18), (b) SULL18 runs.*

Reviewer: Inspection of the plots shows that the dynamical impacts reported actually relate to the stratiform region (the only ascent shown to be sensitive is stratiform in magnitude), not the convective cores, yet the opposite is suggested in the text. This claim "in the simulated DCCs, the exclusion of SIP significantly …weakens the strength of convection through decreased vertical motion" is not supported by the results shown.

Response: We thank the reviewer for pointing out this critical distinction. In the original manuscript, the vertical velocity (w) analysis presented was estimated for the entire simulation domain, without applying a specific convective updraft condition. Consequently, the domain-wide signal was chiefly dominated by the stratiform and weaker convective ascent, thereby obscuring the actual microphysical-dynamical feedbacks due to SIP within intense convective cores. Therefore, we acknowledge this approach was not sufficient to fully support our original claims regarding the impact of SIP on convective strength.

To robustly analyze the impact of SIP on convective strength, we have revised our analysis. In the updated manuscript, we show 90th percentile of vertical velocity in both stratiform ($0 < w < 1$ m s^{-1}, Fig. R8 middle panel) and convective ($w > 1$ m s^{-1}, Fig. R8 right panel) ascent. Also, to isolate thermodynamical and condensate loading impact due to SIP on these ascents, we analyze buoyancy perturbations ($B = g\,\frac{\theta_v - \theta_{v,mean}}{\theta_{v,mean}}$), conditioned these w regimes, both with (black lines) and without SIP (gray lines).

Across all simulated cases, Fig. R8 (middle panel) reveals that, at levels colder than −10°C, the presence of SIP (black lines) leads to a systematic increase in stratiform ascent compared to without SIP (gray lines). This is driven by increase in latent heat due to enhanced depositional growth of extra ice crystals generated by SIP (Sec. 3.5 in the revised manuscript), which creates a net positive buoyancy, which facilitates upward motion of stratiform cloud layer.

In contrast, in strong convective regions of these clouds (Fig. R8, right panel), the rapid growth in crystal numbers leads to higher glaciation and increased total condensate in the mixed-phase region (0 to −20°C). While this releases substantial latent heat through deposition (Sec. 3.5), the associated increase in suspended condensate contributes additional downward drag. This results in a reduction in local buoyancy with SIP than to without SIP (Fig. R8 left panel), thereby reducing convective strength by up to 20%.

Unlike the other cases, in continental DCCs of CAIPEEX, the presence of SIP causes a 10% increase in convective ascent persistently at all subzero levels. This is due to higher buoyancy in convective regions with SIP compared to without SIP (Fig. R8), suggesting that in the relatively less intense convection during CAIPEEX, the latent heating enhancement from SIP dominates over condensate loading effects, maintaining relatively stronger convective cores throughout the depth of the troposphere.

In the stratiform regions of CAIPEEX, DCMEX and MC3E clouds, this analysis further shows a slight phase lag between local buoyancy and vertical velocity at levels colder than −30°C. At these levels, while buoyancy decreases slightly with SIP, the vertical velocity remains elevated compared to without SIP. This may be attributed to vertical momentum advection, where upward acceleration at warmer levels allows parcels to carry their momentum to higher levels through inertia, maintaining elevated vertical velocities despite reduced buoyancy.

The manuscript text and conclusions have been revised accordingly (lines 605-623, lines 686-690) to accurately reflect these case-dependent convective-scale dynamical responses, and the overgeneralized claim about SIP strengthening convection through increased vertical motion has been removed and replaced with a nuanced case-by-case discussion.

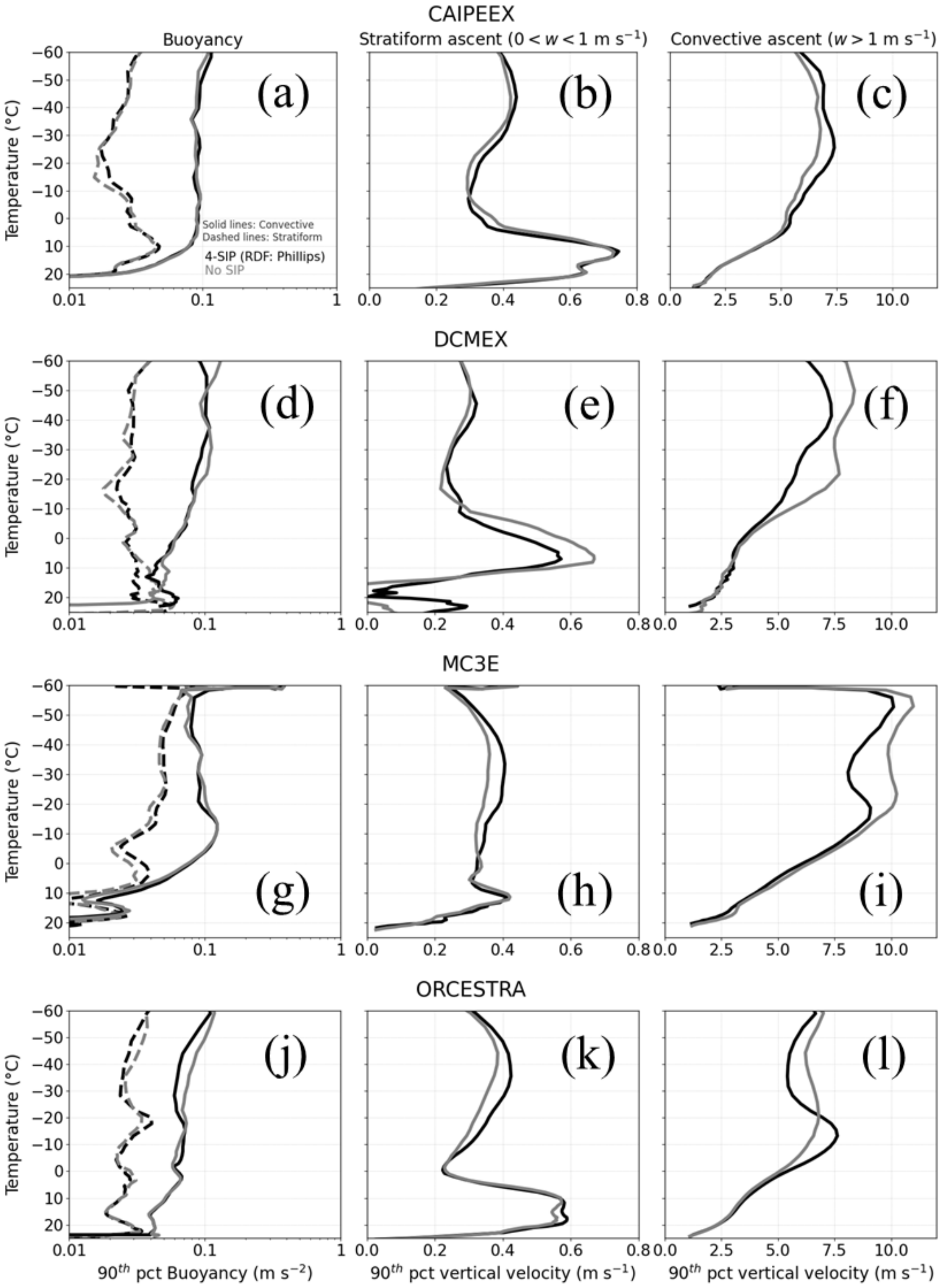

Figure R8: *Domain-averaged profiles of the buoyancy* ($B = g\,\frac{\theta_v - \theta_{v,mean}}{\theta_{v,mean}}$) *(left panel), stratiform* ($0 < w < 1$ m s^{-1}, *middle panel) and convective* ($w > 1$ m s^{-1}, *right panel) ascents during convective periods from the control (black lines) and No SIP (gray lines) of the DCCs during (a-c) CAIPEEX, (d-f) DCMEX, (g-i) MC3E, and (j-l) ORCESTRA. In the left panel, the thick and thin lines denote buoyancy in convective and stratiform ascents, respectively.* θ_v *is the virtual potential temperature.*

Reviewer: On balance, there are some intriguing results about the thermodynamical and radiative effects from SIP. Yet there are some sweeping or misleading statements in the analysis of simulations and the model validation is unconvincing as regards the aircraft data. The manuscript attributes changes in convection strength to SIP; however, the vertical velocity

magnitudes shown (~10 cm s^{-1}) are characteristic of stratiform ascent rather than convective updrafts. This distinction should be made explicit in the interpretation. This distinction is important, and the conclusions should be revised or additional diagnostics of convective updrafts (e.g., conditional statistics for $w > 1$ m s^{-1}) should be provided.
Response: We thank the reviewer for this critical and constructive observation. As discussed above, we have substantially revised our analysis in the manuscript, separately for both stratiform and convective regions.

Detailed comments:

Reviewer: Line 36: The statement that "on a global scale, the majority of surface precipitation is from the ice-crystal ('cold rain') process" requires more careful qualification. The cited studies (Lau and Wu, 2003; Field and Heymsfield, 2015) rely on indirect inference methods that may overattribute precipitation to ice processes—for example, by assuming that the presence of ice aloft implies an ice-phase origin of surface precipitation. This assumption is not generally sufficient to establish causal microphysical pathways.
Response: We thank the reviewer for this important qualification. The revised text in the updated manuscript (lines 37-39) now reads "on a global scale, a significant fraction of surface precipitation is thought to originate from the 'ice-crystal' process, however the dominant pathway varies significantly by region and convective conditions (Lau and Wu, 2003; Field and Heymsfield, 2015; Gupta et al. 2023)."

Reviewer: More recent work (e.g., Gupta et al., 2023) uses tracer-based diagnostics in cloud resolving simulations to explicitly track precipitation origin and demonstrates that the dominant pathway can vary substantially by case. In particular, Gupta et al. show that while ice-phase processes dominate in some mesoscale convective systems, warm rain processes can dominate in other environments (e.g., tropical systems), even when ice is present aloft. These results suggest that stronger caution is needed when making general statements about the global dominance of ice-phase precipitation.
Response: We thank the reviewer for highlighting the work of Gupta et al. (2023), which used tagging tracer approach to provide a more rigorous attribution of precipitation origin. We have now revised the statement accordingly to reflect this case and region dependent behavior of surface precipitation, and have added Gupta et al. (2023) to the citation (line 39).

Reviewer: I recommend citing Gupta et al. (2023) and revising the statement to reflect this uncertainty and case dependence.
Response: We have followed the reviewer's recommendation and cited Gupta et al. (2023) in the revised manuscript (line 39).

Reviewer: Additionally, the term "cold rain process" is potentially misleading. In many cases, the ice-crystal process produces solid precipitation (snow) that may or may not melt before reaching the surface, depending on thermodynamic conditions. Referring to this pathway as a "rain" process can obscure the underlying microphysics, particularly in cold environments where no liquid phase is involved at the surface. A more precise term such as "ice-phase precipitation process" or "ice-crystal process" would improve clarity.
Response: We thank the reviewer for this insightful comment and suggestion. In the revised manuscript, we have replaced "cold rain process" with "ice-crystal process" throughout, and

have revised the statement at Line 37 to reflect the environment-dependent nature of the dominant precipitation pathway, citing Gupta et al. (2023) as recommended.

Reviewer: Line 56: Where it is written "The HM process has been recognized as important for rapid ice enhancement in the early growth of DCCs" the language does not seem very scientific. Recognised by whom? "Importance" can be subjective. Maybe the authors can try to be more precise in the citing of these studies: do you mean that these studies predicted that the HM process was active, and if so was it in the early stage of the growth of the cloud? Which of these studies were validated with acuity (for ice concentration and liquid water content and surface precipitation or radar reflectivity)? What was the evidence?
Response: We thank the reviewer for this valid criticism. In the revised manuscript, we have changed this with a more specific statement clarifying that the cited studies are numerical modelling studies that found active HM process during early growth of convective clouds, and also noted which of these studies included validation against observations.

The modified text in the manuscript now reads as (lines 64-67):
"Previous modelling studies found that the HM process contributes significantly to the overall INC (Patade et al. 2016; Lasher-Trapp et al. 2021), especially during the early growth stage of DCCs (Waman et al. 2022; Han et al. 2024). Although a recent laboratory experiment by Seidel et al. (2024) mimicking convective conditions found no clear splinter production through the HM process, its relevance in natural clouds remains uncertain."

Also, some of these cited studies (Patade et al. 2016; Waman et al. 2022) include detailed validation of simulated microphysical properties against aircraft observations. For example, Patade et al. (2016) validated LWC, IWC, CDNC, INC and radar reflectivity with the available aircraft and ground-based radar observations from the CAIPEEX campaign. For the simulated MC3E case, Waman et al. (2021) compared all simulated microphysical properties against the observations from aircraft, as well as surface precipitation, radar reflectivity, TOA radiative fluxes with satellite observations.

However, a detailed discussion of the validation approach of each individual study is beyond the scope of the present manuscript.

Reviewer: The problem is that this paragraph poses a dilemma that any reader will be intrigued by: is the HM process real or not? It would be helpful to see if there is any field campaign evidence of the HM process. Bower et al. (1996) published a vertical profile of observed ice concentration with a huge peak right in the centre of the HM generation region, in flights through deep stratiform cloud. Has there been anything like this more recently with modern technology for optical probes in stratiform clouds?
Response: We thank the reviewer for this constructive suggestion. We agree that citing observational evidence alongside modelling studies would help to resolve the apparent dilemma about whether the HM process is physically real and active in natural clouds. We have added text in the revised manuscript to the recent field campaign observations that provide evidence of HM process activity in observed clouds. Specifically, in mixed-phase clouds over the Southern Ocean, Huang et al. (2021) and Järvinen et al. (2022) observed ice number concentrations exceeding INP concentrations by several orders of magnitude within the HM

temperature regime (-3 to -8°C). Similarly, Lasher-Trapp et al. (2021) and Lasher-Trapp et al. (2022) documented active HM rime splintering in Southern Ocean cumuli and tropical maritime cumuli respectively. We have also added Bower et al. (1996) to the revised manuscript. Together, these observational studies confirm that the HM process is active across a range of cloud types and environments, while its activity depends on specific dynamical and microphysical conditions.

The revised text in the manuscript now reads as (lines 59-62);
"Previous field observations provide direct evidence for the presence of the HM process across diverse cloud environments. For example, aircraft measurements in convective Bower et al. 1996 and marine boundary layer (Huang et al. 2021 and Jarvinen et al. 2022) clouds observed that the measured INCs exceed INPs by several orders of magnitude within the HM regime. Radar measurements of tropical marine cumuli by Lasher-Trapp et al. 2016 also observed INCs consistent with active rime-splintering, supported by radar evidence of graupel presence within the HM regime."

Reviewer: Line 51: With raindrop-freezing fragmentation, it would be helpful to cite the early experimental studies, such as Takahashi and Yamashita (1977) or Johnson and Hallett earlier.
Response: These references (Takahashi and Yamashita, 1977; Johnson and Hallett, 1968) are now added in the revised manuscript (line 53-54).

Reviewer: Line 62: It would be helpful to cite Phillips et al. (2017b, Part II) who were first to predict that breakup in ice-ice collisions prevails in the overall ice production of a simulated mesoscale convective system.
Response: Corrected in the revised manuscript (line 69).

Reviewer: Line 75: It is written that "While laboratory experiments (Oraltay and Hallett, 1989; Bacon et al., 1998; Dong et al., 1994) demonstrated fragmentation of sublimating dendrites and rimed particles under relative humidity with respect to ice (RHi) 50–90%, field observations (Korolev and Issac, 2004) and modeling studies (Waman et al., 2022; Yang et al., 2024) found minimal contribution of SBF to total ice formation in clouds." But Figure 10 of Waman et al. (2022) shows that sublimational breakup in the convective descent and stratiform regions of a MC3E storm (MCS) is about the second most important SIP mechanism in terms of ice concentrations from tagging tracers. The tagging tracer result is definitive as it shows the true concentration in the control simulation. For simulations of GOAmazon, Deshmukh in his PhD Thesis found in the ice initiation budget (after accounting for losses from sublimation of SB-fragments) the sublimational breakup is as important as ice-ice collisional breakup.

Response: We thank the reviewer for careful reading of Waman et al. (2022). We have revised the text in the updated manuscript to correctly address the SBF contribution to total INCs.

It now reads in the revised manuscript as (lines 89-91);
"Waman et al. (2022) further showed that while SBF forms little ice in convective updraft regions, it can account for the majority of SIP in downdraft and stratiform regions following IIC."

Regarding the citation of Deshmukh's PhD thesis, we would like to note that the thesis itself has not undergone peer review, and some of the results discussed therein are not yet available in the peer-reviewed literature. We therefore do not consider the thesis an appropriate basis for revising conclusions that are supported by published modelling and observational studies.

In addition, the present study does not include simulations of the GOAmazon case, and therefore a direct comparison with results presented for that case is not straightforward. SIP process contributions are known to depend strongly on the meteorological environment, aerosol conditions, thermodynamic structure, and the microphysical framework employed. Consequently, results obtained for a specific Amazonian convective case cannot be directly generalized to the contrasting continental and marine environments examined in the present study.

We also want to highlight that the present manuscript already accounts for the survival fraction (20% of the total fragments survives the subsaturation) of sublimation-breakup fragments under subsaturated conditions, following the approach of Waman et al. (2022). This is now clearly mentioned in the revised manuscript (lines 22-23). The same approach of Waman et al. (2022) is followed in Deshmukh (2023) PhD thesis.

It is also important to note that this PhD thesis does not provide a detailed validation of key simulated microphysical and dynamical properties (LWC, INC, IWC, *w* statistics), particularly for the GOAmazon against collocated observations. Therefore, it is difficult to assess the reliability of the simulated SIP contributions (e.g., 49% of ice initiation each from SBF and IIC) presented in that work or to use them as a quantitative basis for revising the conclusions of the present study.

Finally, our objective here is not to perform a case-by-case intercomparison across different model configurations and environments, but rather to evaluate SIP behavior within the ICON-NWP framework for the selected cases analyzed in this study.

Finally, while implementing tagging tracers for dendritic snow tracking in ICON's 2-moment bulk scheme is a nice idea, it is not feasible within the scope of a revision. The tagging tracer approach in Waman et al. (2022) was a dedicated implementation in the AC bin-microphysics model conducted over an extended period at a different institute. Doing the same in ICON requires extensive modelling work, which is beyond the scope of this paper.

Also, the vertical profiles of SIP process rates already presented in the current paper provide a physically meaningful and quantitative characterization of SBF activity without requiring tagging tracers.

Hence, there is no need to have a dedicated tagging tracer to track dendritic snow of SBF of dendritic particles.

Reviewer: Did Yang et al. tag the concentrations rigorously? It would be helpful to know.
Response: No. Yang et al. (2024) did not tag concentrations (of ice fragments from SBF), nor did they implement a dedicated tracer for dendritic crystals. Additionally, their SBF implementation does not account for the loss of ice fragments through sublimation (Waman et

al. 2022) in the subsaturated conditions in which they form, a significant sink term that would substantially reduce the net contribution of SBF to the total ice population.

Reviewer: The citation to Korolev and Isaac (2004) does not appear to support the statement regarding sublimational breakup, in the quote above. The paper focuses on shattering processes generally and does not discuss sublimation at all. A more appropriate reference should be used.
Response: We thank the reviewer for carefully checking our references. The citation Korolev and Isaac (2004) is correct and directly relevant (as reviewed by Korolev and Leisner, 2020) to the statement on SBF, as this paper is titled "Observations of sublimating ice particles in clouds" and provides field observational evidence for ice particle sublimation. The incorrect paper title in the reference list has been corrected in the revised manuscript and subsequent change is made in the main text (line 87).

Reviewer: Line 71: It would be helpful to state that Lawson observed tropical convection… the very warm cloud-base creates a strong warm rain process. Also, they did not "show" that raindrop-freezing fragmentation dominates, since they did not model their case comprehensively. They inferred this with various assumptions, including the notion that the HM process is unimportant. Their aircraft chased very young ascending turrets, and parcel simulations of these cloud-top regions by Phillips et al. (2018) showed that both raindrop-freezing fragmentation and the HM process occurred. Over longer time-scales other SIP mechanisms would likely become prolific too.
Response: We thank the reviewer for pointing this out. In the revised manuscript, this now reads as "Lawson et al. (2015), using aircraft observations and parcel model simulations of tropical marine convection with warm cloud base (17°C), inferred that, RDF dominates SIP between -4 and -15°C, and may facilitate rapid cloud glaciation." at lines 78-80.

Reviewer: Line 72: It is written that "Furthermore, recent modelling studies of DCCs (Huang et al., 2021; Waman et al., 2022; Patade et al., 2025) reported that RDF dominates overall SIP in their early stages." But this is an exaggeration, at least for Waman et al. 2022. See Figure 21 of Waman et al. (2022): HM process is much more prolific than RDF during the first 10 mins, which is very similar to sublimational breakup, in the budget of numbers of particles initiated. Also see Figure 18 of Waman et al., which shows this too.
Response: We thank the reviewer for the careful read of Waman et al. (2022).
We agree that the original statement overstated the role of RDF for Waman et al. (2022) and have revised the text accordingly. However, Fig. 18(b) of Waman et al. (2022) shows that while the HM process dominates ice enhancement at the earliest growth stage when cloud tops are warmer than −8°C, RDF can contribute to total INCs noticeably as cloud tops reach the −10 to −20°C range during early growth stage of DCCs.

This is now addressed in the revised manuscript (lines 79-82) with clarity as
"Furthermore, recent modelling studies of DCCs reported that RDF contributes noticeably to overall SIP (Huang et al., 2021; Patade et al., 2025), particularly as cloud tops reach the −10 to −20 ◦C during early growth stages. Also, Waman et al. (2022) found that while the HM process dominates in clouds with warmer tops (> −10 °C), RDF may become increasingly important as the cloud deepens."

Reviewer: Line 77: Patade et al. 2016 were not the first to discover that the HM process occurs in deep convection. There is a long literature before. It would be helpful to cite earlier studies such as Hallett et al (1978), Bower et al. (1996) and Latham and Blyth (1993) and Phillips et al. (2007).
Response: We thank the reviewer for pointing this. However, Patade et al. (2016) was cited at line 57 in the original manuscript and only shown as an example of modelling of the HM process in deep convection. It is now corrected in the revised manuscript (line 65).

Reviewer: Table 1: It seems unlikely that all four cases would have the same cloud-top, even if it near the tropopause. Perhaps call it "maximum cloud-top". If the latitudes are completely different among the cases, then the tropopause temperature will differ too among them.
Response: Corrected with "maximum cloud-top" in the revised manuscript (Table 1).

Reviewer: Line 228: In Equation 2, this is the wrong temperature range for the HM process. It should be -3 to -8 degC. Perhaps there is a typo? If not, then one cannot call this process the "HM process".
Response: We thank the reviewer for pointing this typo. It is now corrected in the revised manuscript (Eq. 2).

Reviewer: Line 275: Better to track the dendritic snow with a tagging tracer and not limit the sublimational breakup of it to the dendritic temperature regime. Waman et al. (2022) did that.
Response: We thank the reviewer for this insightful suggestion. We understand that tracking dendritic snow with a dedicated tracer, as done by Waman et al. (2022), can be a more appropriate way to represent SBF. However, there is a fundamental structural difference between the models used in Waman et al. 2022 (AC bin microphysics model) and in the present study (ICON 2-mom bulk microphysics scheme). As ICON lacks native tagging tracer infrastructure, implementing such a framework in ICON would require substantial structural model development rather than a minor addition. These efforts are therefore is well beyond the scope of this paper.

Therefore, limiting SBF to the dendritic temperature regime serves as a physically motivated and defensible approximation that aligns with how bulk schemes traditionally parameterize such processes.

Reviewer: Line 279: d is the diameter of the sublimating parent ice particle, and it is not generally a crystal. Rather it is either graupel or snow here.
Response: Corrected in the revised manuscript (line 324).

Reviewer: Figures 6–9: The validation plots for ice concentration are difficult to interpret due to inconsistencies in the quantities being compared. The filtered ice concentration from the observations appears to be compared against the total ice concentration from the model. For a consistent ("apples-to-apples") comparison, the model output should be processed using the same filtering criteria as applied to the observations.
Response: We thank the reviewer for this suggestion and agree that the comparison can be meaningful when means of simulated properties are compared against the corresponding means

of observations at each level. As discussed earlier, we have corrected this analysis in the revised manuscript (Figs. 6-9 and Sec. 3.1).

Reviewer: If the objective is to evaluate mean cloud properties, the comparison should be based on consistent statistics—i.e., model mean versus observed sample mean. In that case, the observational mean should be accompanied by an estimate of sampling uncertainty (e.g., standard error or confidence intervals), particularly given the limited and correlated nature of aircraft measurements.
Response: Thanks for this suggestion. In the revised manuscript (Figs. 6-9), we now only show the mean of the observed properties along with their standard deviation in each temperature bin.

Reviewer: The current box-and-whisker representation primarily conveys the distribution (median and percentiles) of the observations and does not directly quantify agreement in the mean. This is useful for assessing variability, but it does not by itself support conclusions about agreement in mean values
Response: We thank the reviewer for this suggestion. In the revised manuscript (Figs. 6-9), we now only show the mean of the observed quantities in each temperature bin, without box-and-whisker representation to better compare model against the collocating observations at different levels.

Reviewer: If sample means are presented, it would also be helpful to indicate the effective sample size or sampling duration (e.g., time or distance of aircraft segments), given the incomplete and potentially autocorrelated sampling of clouds.
Response: We thank the reviewer for this suggestion. We now show sample size of observed datapoints in each temperature bin in the revised manuscript (Figs. 6-9).

Reviewer: Finally, the inclusion of model extrema (minima/maxima) is of limited interpretive value, as these depend strongly on model resolution and sampling and are not directly comparable to observational extrema.
Response: Thanks for this suggestion. In the revised manuscript (Figs. 6-9), we have removed the minima and maxima lines in revised manuscript and instead show ±1 standard deviation to show model variability.

Reviewer: Also it is not always helpful to plot all the raw data on the validation plots, as it is impossible to interpret when innumerable points just create a wall of opaque shading.
Response: Thanks for this suggestion. We have removed the raw data from all plots in the revised manuscript (Figs. 6-9).

Reviewer: Line 520: This statement seems to suggest an impact on the convective updrafts from SIP: “Excluding SIP in the simulated DCCs causes noticeable changes in the 90th percentile vertical velocities (Fig. 18), thereby influencing the overall convective strength.” But when one looks at Figure 18, these are not convective-scale rates of ascent. Rather, the order of magnitude is 10 cm/sec, which is stratiform.
Response: We thank the reviewer for this suggestion. As discussed earlier (Fig. R8), we have now presented a dedicated analysis to quantify the impact of SIP on both stratiform and

convective ascent. With this analysis, we found that the presence of SIP in simulated DCCs affect stratiform ascent through increased latent heat release from deposition, and reduces convective ascent due to increased condensate loading. This is now discussed in the revised manuscript (lines 605-623, Fig. 18).

Reviewer: It would be helpful to show a cumulative frequency histogram of convective ascent and define the zero-th percentile of ascent to be 1 m/sec. Then the 90th percentile of convective ascent will be a few metres per second at least.
Response: As discussed earlier, we have followed the reviewer's suggestion and estimated vertical velocity (w) conditioned on both cloudy stratiform ($0 < w < 1$ m s^{-1}) and convective ascent regions ($w > 1$ m s^{-1}), effectively defining 1 m s^{-1} as the lower bound of convective ascent. However, rather than a cumulative frequency histogram, we present the 90th percentile of this conditioned distribution as a vertical profile for each case. This allows physically meaningful height-dependent information while confirming that the analysis captures both stratiform and convective-scale updraft motions, with 90th percentile convective w ranging from 2 to 12 m s^{-1} across the simulated cases.

These results are discussed earlier (Fig. R8) and in the updated manuscript (lines 605-623, Fig. 18).

Reviewer: It would be helpful to ascertain if there is any appreciable sensitivity of convective ascent at all in any of the control simulations.
Response: Yes, the 90th percentile convective ascent (Fig. 18 (right panel)) demonstrates appreciable sensitivity to the presence of SIP in all four simulated cases. As discussed earlier, In CAIPEEX, 4-SIP produces stronger convective ascent above −20°C, while in DCMEX and MC3E, No-SIP produces stronger convective ascent throughout the mixed-phase column. ORCESTRA shows a level-dependent response. The physical mechanisms driving these case-dependent responses are also discussed earlier (Fig. R8). This information is now added in the revised manuscript (Fig. 18, lines 605-623).

Reviewer: Without any such re-working of the data, it would be helpful to be open about the fact that the sensitivity of dynamics reported is actually stratiform weak ascent and not the convective cores.
Response: We agree with the reviewer that the original analysis was dominated by stratiform weak ascent and that this distinction was not made sufficiently clear in the manuscript earlier. However, as described in our responses above, we have now explicitly separated the analysis into stratiform ($0 < w < 1$ m s^{-1}) and convective ($w > 1$ m s^{-1}) ascent regimes, and revised the manuscript text (lines 605-623) and conclusions (lines 686-690) accordingly to accurately quantify the dynamical impacts of SIP in each regime.

Reviewer: Line 549: This sentence has a typo "both SULL18 and PHIL18 schemes predict about 1.5 times higher mean INC than SULL18." In this section, it would be helpful to show the process rate profile for PHIL18 in a similar fashion to Fig. 10.
Response: We thank the reviewer for spotting this typo. It is now corrected in the revised manuscript. Also, we now show the process rate profiles of each SIP mechanism to compare SULL18 and PHIL18 (lines 639-652, supplementary Figs. S8) in the revised manuscript.

Reviewer: One suspects that the relative difference in raindrop-freezing fragmentation rates between both schemes must be large (e.g. an order of magnitude?) in most of the cases, because for CAIPEX, the total ice concentration between 0 and -10 degC is changed by up to half an order of magnitude at levels where there are likely far more HM (or IIC) splinters than raindrop-freezing splinters.
Response: The reviewer is right as the updated analysis of process rates exhibits about an order of magnitude more particle formation in PHIL18 than in SULL18. This is now discussed in the revised manuscript (lines 639-652) with updated plots of process rates in the supplementary material (Fig. S8).

Reviewer: Line 590: Point 5 suggests an alternative explanation to that given in point 4: if the cloud-top is much lower without SIP (due to latent heating changes), then the rate of LW emission to space is very much more than with SIP by Stefan's Law. So that explains the reduction in LW heating rate when SIP is excluded.
Response: We thank the reviewer for highlighting this point. We have now revised the conclusion accordingly (lines 683-685).

Reviewer: Line 593: It would be helpful to give the range of percentage change of vertical velocity. 10% seems to be an exaggeration for DCMEX. For one or two cases the response is more like 1%.
Response: For the simulated cases, the updated analysis of stratiform and convective ascent show that the presence of SIP decreases (increases) convective (stratiform) ascent by up to 15%. The text is revised accordingly in the updated manuscript (line 687).

Reviewer: Line 597: It is misleading to state that "In all simulated DCCs, both SULL18 and PHIL18 schemes of SIP through RDF predict similar INC and dynamical properties." In fact, only the total ice concentration in simulations using both RDF schemes is shown. Nowhere is the actual component of ice concentration from each scheme shown or their budgets of ice initiated. It seems almost inevitable that simulations with both schemes give similar total ice concentrations in long-term averaged profiles, since breakup in ice-ice collisions is by far the leading SIP mechanism at most levels in general.
Response: We acknowledge the reviewer's concern and agree that the original statement was misleading.

In the revised manuscript, we have replaced this statement with a more precise discussion supported by the newly added vertical profiles of individual SIP process rates for both SULL18 and PHIL18 (Fig. 20) and discussed at lines 639-652 and 697-699. These profiles show that while IIC dominates total ice production at most levels in both schemes, making similar total INC an expected outcome, the production of ice crystals in these two RDF schemes differ substantially, particularly in the −3 to −10°C range where PHIL18 produces nearly one to two orders of magnitude more fragments than SULL18. This scheme-dependent difference in RDF activity is obscured when only total INC is examined, further demonstrating the value of process-level diagnostics over bulk integrated quantities in evaluating RDF parameterization performance.

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