Review: Sun et al.
Influence of Secondary Ice Formation on Tropical Deep Convective Clouds Simulated by Unified Model

The paper numerically investigates the possible role of secondary ice production (SIP) in forming overall ice number concentration, and its influence on cloud properties in a convective case observed during ACTIVE campaign in early December 2005. For this, the authors incorporated three SIP processes (Hallett-Mossop rime-splintering, ice-ice collision, breakup of freezing raindrop) in UK Met Office Unified Model's 2-moment CASIM microphysics scheme. The study finds that, through increased ice number and mass in the upper region, SIP can modify the anvil structure in the simulated thunderstorm, and changes the precipitation formation, especially associated with the convective core. The ensemble simulations are also performed that illustrates the robustness of the presented results. The introduction is comprehensive, and sufficiently discusses the recent advancements and challenges in SIP research, and the methodology is sound. The simulated properties, such as the radar reflectivity, OLR, and precipitation are compared and validated against the observations. While the overall presentation is good, the paper could benefit from a more detailed discussion and comparison of how the simulated microphysical properties agrees well with the observations. Nevertheless, the study addresses a timely and important topic and may be considered for publication after satisfactorily addressing the following concerns.

General comments

More details of radiation and microphysics scheme used are needed. The presented validation is reasonable but could have benefited from additional comparison, such as observed liquid/ice properties, if such observations (from satellite or other platforms) are available. Also, many findings presented in the manuscript can be supported by some previous studies, and can also be acknowledged in the introduction section to strengthen the proposed research questions.

Specific comments

Abstract

Line 14: Since the study mainly quantifies the impacts of SIP on the simulated clouds without attempting to modify/improve the existing SIP parameterizations (beyond the use of revised Φ from James et al. 2021), I would suggest rephrasing this sentence, as the focus is not on quantifying and reducing uncertainties in the modelled SIP processes.

Line 16: Mode 1 and 2 of drop shattering are scheme-specific terms and are not widely recognized. These are proposed by Phillips et al. (2018) to represent drop shattering in collision between rain/drizzle drop with ice particle. Other schemes of SIP in drop shattering (e.g., Sullivan et al. 2018) only consider shattering of raindrop during freezing, initiated due to immersed INP, without separating mode 1 and 2. Better to omit using mode 1 and mode 2 and say only 'drop fragmentation'.

Line 25: change <1 to < 1.

Line 26: Not sure about the context of this sentence. On which process/property ice-ice collisional breakup has negligible impact? On ice concentration or dynamics?

Introduction

Lines 34-35: Citing more recent studies of observational evidences of SIP would be beneficial (e.g., Korolev et al. 2022).

Lines 35-43: Where the term SIP is introduced, please mention the region (mixed-phase) where it mainly occurs in clouds.

Lines 39-41: Cite previous literature (e.g., Lohmann et al.; Kudzotsa et al. 2016; Han et al. 2024; Waman et al. 2025) supporting this.

Lines 41-43: I do not see that the manuscript attempt to improve the representation of SIP processes in numerical model. Rather, the effect of SIP processes is quantified in deep convective clouds using existing parameterizations. Please rephrase.

Line 50: Can the authors comment and acknowledge findings of recent study by Seidel et al. 2024, which see no experimental evidence of rime-splintering, especially in convective conditions. Considering the findings of Seidel et al. 2024, what is the relevance rime-splintering process and its existing parameterization in representing SIP at such warmer subzero levels?

Line 57: Please cite relevant previous studies that used Unified framework to study SIP.

Line 64: Waman et al. 2022 do not explicitly quantify the impact of SIP on the mentioned ice growth processes. Please correct/clarify more.

Methodology

Line 113: ‘are’ instead of ‘is’?

How are cloud droplets activated in CASIM? Does the scheme explicitly account for the activity of soluble aerosols as CCN, or is the CCN spectrum prescribed from observations? What is the nature of aerosols (continental/marine)?

Line 115: I believe with Cooper, only immersion mode of heterogeneous ice nucleation is represented. Can the authors clarify how other heterogeneous ice nucleation modes (e.g., deposition), that can be crucial at colder temperatures, are treated in CASIM? Additionally, is homogeneous freezing of aqueous aerosols represented separately from homogeneous droplet freezing, and if not, what are the implications for ice formation at cirrus temperatures?

Line 116: ‘rime-splintering’ instead of ‘riming splintering’?

Line 118: ‘other newly implemented SIP processes’ instead?

Line 135: What is the value of rime fraction used to represent ice-ice collision in Eq. 3? Also, can the authors comment on how the re-fitted values of the parameters in Eq. 3, given by Grzegorczyk et al. 2023 (Table 3) would influence the predictions from ice-ice collisional breakup?

Eq. 6: N_T should be N_{M1T}?

General comment: Overall, the considered SIP processes are described adequately. However, the study does not appear to consider SIP during sublimation of ice particles in subsaturated cloudy environments (e.g., Deshmukh et al., 2022; also see Korolev and Leisner 2020 for limitations). While I understand this mechanism is still under active investigation, it could be relevant in tropical anvil outflow/downdraft regions (Waman et al. 2022). Could the authors briefly comment on the reason for excluding this process and its possible implications for the presented results? Also, adding a table of symbols used would be helpful.

Line 217-218: I do not understand what really makes Hector as an ideal case for studying SIP.

Line 221: Would be nice to mention cloud-base (LCL) and cloud top from Fig. 2.

Results

Line 291: space between '(CFAD)' and 'of'.

Line 306: 'SIP' instead of 'secondary ice production'?

Figure 4: For better comparison, is it possible to show isotherms also in (a)? Also show in the form of text '0°C', '-20°C', '-40°C', '-60°C' (The same can be followed for Figs. 9 and 10).
Figure 5: Also mention date (1 December 2005) in the caption.

Lines 351-356: Can the authors comment on how well the model captures the observed surface precipitation? Overall, I see that the model significantly underpredicts the surface precipitation, both in all-SIP and no-SIP experiments.

Line 356: 'diffused' instead of 'diffuse'?

Line 357: 'The convective core is less pronounced…' This is not clear. How is it interpreted? Additional analysis would be helpful to support this.

Line 358-359: This is also not quite clear. How all-SIP shows a more localized and organized precipitation? Also, I do not agree that the all-SIP experiment resembles the overall observed convective core as the simulations substantially fails to capture the observed precipitation features.

Figure 6d: is this all-SIP minus no-SIP? Mention clearly in the caption.

Line 366: Cite Figure 6d.

Line 367: I do not see these features; can the authors describe this more? How exactly no-SIP shows more evenly distributed precipitation than all-SIP?

Line 369: ‘realistic reproduction’: Both all-SIP and no-SIP rather captures more localized precipitation events in the simulated domain and not the overall precipitation. Please rewrite as precipitation differ significantly between the simulations and observation.

Line 371: What does ‘focus’ mean here? How convective rain(fall) is identified?

Line 377: ‘increase’ instead of ‘increases’?

Line 378: Why the mode 2 results in more pronounced convective core?

Lines 366-405: Can the authors explain briefly in the manuscript what possible factors SIP alters that result in the predicted change in precipitation? Although paragraph (lines 405-413) explains the overall influence of a combination of various SIP processes, the exact cause in each case (in Fig. 7) is not discussed.

Lines 457-460: Although mode 2 is less efficient and more confined than mode 1, for what possible reasons does RS+M2 produces more precipitation (Fig. 7e)?

Line 470: ‘increase’ instead of ‘increases’?

Line 473-474: A suggestion: Time-height maps of total ice concentrations in all-SIP and no-SIP, and a similar difference plot (all-SIP minus no-SIP) would be helpful to visualize increased extensiveness over longer period in all-SIP case.

Line 532: Previous work by Qu et al. (2022) and Grzegorczyk et al. (2025) can be cited here.

Line 569-570: This needs more clarification, as both warm and cold rain processes can happen simultaneously at subzero levels.

References

Korolev, A., DeMott, P.J., Heckman, I., Wolde, M., Williams, E., Smalley, D.J. and Donovan, M.F., 2022. Observation of secondary ice production in clouds at low temperatures. *Atmospheric Chemistry and Physics*, *22*(19), pp.13103-13113.

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Kudzotsa, I., Phillips, V.T. and Dobbie, S., 2016. Aerosol indirect effects on glaciated clouds. Part 2: Sensitivity tests using solute aerosols. *Quarterly Journal of the Royal Meteorological Society*, *142*(698), pp.1970-1981.

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Grzegorczyk, P., Yadav, S., Zanger, F., Theis, A., Mitra, S.K., Borrmann, S. and Szakáll, M., 2023. Fragmentation of ice particles: laboratory experiments on graupel–graupel and graupel–snowflake collisions. *Atmospheric Chemistry and Physics*, *23*(20), pp.13505-13521.

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