Responses to reviews

"Secondary ice production affects tropical convective clouds under different aerosol conditions"

by Mengyu Sun et al.

We thank the editor and reviewers for their time and constructive comments. All suggestions have been carefully considered and addressed in the revised manuscript. Our point-by-point responses are provided in blue, and the corresponding revised sentences is shown in gray italic for clarity.

Referee #1

This article investigates the influence of contrasted aerosol conditions through variations in cloud condensation nuclei (CCN) concentrations on secondary ice production (SIP) processes and their impacts on cloud microphysics and convection. The authors perform a series of sensitivity experiments by varying CCN concentrations from clean to polluted conditions for the tropical convective cloud system Hector, using the UK Met Office Unified Model coupled with the CASIM microphysics scheme.

The purpose of this study is highly relevant, as only a limited number of studies have investigated the interaction between aerosols and SIP processes. In most previous works, the effects of aerosols and SIP mechanisms are examined separately. The numerical framework, as well as the range of sensitivity experiments and the chosen diagnostics, appear appropriate to address the scientific questions.

However, several aspects of the manuscript could be improved, particularly the clarity and organization of the results. Given the large number of sensitivity experiments, the presentation of the results is sometimes a bit difficult to follow. For example, some figure panels are discussed long after others from the same figure, which complicates the reading. Further efforts to improve the structure and layout of the figures would also greatly improve the readability of the manuscript.

From a scientific perspective, I also note that the study primarily investigates the sensitivity to CCN concentrations rather than to aerosol conditions in a broader sense. This is because the heterogeneous ice nucleation parameterizations used (Cooper, 1986 and Bigg, 1953) do not explicitly account for aerosol number or composition and therefore do not represent polluted environments in terms of INPs. As a result, the interpretation of the simulations as representing "contrasted aerosol conditions" should be treated with caution. Other aspects of the results would also benefit from additional discussion and clarification.

Overall, the manuscript is suitable for publication in ACP, as the scientific concept of the study is relevant. I therefore recommend publication after minor revisions, which should focus on improving the organization and clarity of the results, as well as on providing a clearer discussion of key physical interpretations and some caveats.

We thank the reviewer for the positive assessment of this study. Following the reviewer's suggestions, we have improved the organization of the Results section and revised the figure layout and discussion order to improve readability. We have also clarified that the sensitivity experiments primarily represent CCN perturbations and added corresponding context added in the Discussion.

Comments:

Lines 30–32: Additional references could be added here to support the statements.

We have added references to support these statements *(e.g., Bony et al., 2015; Grabowski, 2015; Morrison et al., 2020)*.

Lines 40–42: The original work of Phillips et al. (2018), which first introduced the partition of drop fragmentation into two modes, could be cited here.

The suggested reference has now been added.

Line 47: The reference to Phillips et al. (2017) is not fully appropriate here, as this publication focuses on the formulation of the ice breakup parameterization. Part II of the study, which presents a modeling application of the new parameterization, would be more suitable (https://doi.org/10.1175/JAS-D-16-0223.1) as it depict its effect on supercooled liquid water.

We agree with the reviewer and have replaced the citation with Phillips et al. (2017a, Part II; DOI: 10.1175/JAS-D-16-0223.1) and revised the text and references accordingly.

Lines 66–67: The discussion focuses exclusively on CCN concentrations. However, more polluted conditions may also imply higher INP concentrations, which could lead to more glaciated clouds with less supercooled liquid water. I therefore suggest restricting this statement explicitly to CCN effects, as stated later in lines 68–71.

We agree and have revised the statement to refer to CCN effects only, consistent with our discussion.

Lines 92–95: Is the subgrid cloud fraction scheme still applied in the present study, despite the 1.5 km horizontal resolution? While convection is not parameterized, is it still relevant to parameterize cloud fraction and liquid condensation? Is the liquid fraction not directly predicted by CASIM?

We retain the standard RAL3.2 setup at 1.5 km, where a diagnostic subgrid cloud fraction is used for coupling to radiation. CASIM prognoses hydrometeor mass and number, but it does not provide a prognostic cloud-fraction variable required by the radiation scheme. We have revised the text to clarify this as follows (lines 96-98): "*Subgrid cloud fraction is diagnosed using the bimodal cloud fraction scheme of van Weverberg et al. (2021) for coupling to radiation in the RAL3.2 configuration, ...*"

Lines 109–110: Since heterogeneous ice nucleation in the model depends only on temperature (Cooper, 1986) and droplet volume (Bigg, 1953), and not on aerosol number or composition, the sensitivity experiments explore variations in CCN concentration only. In reality, contrasted clean and polluted aerosol environments are expected to modify both CCN and INP concentrations (and properties), with a direct impacts on the ice phase through heterogeneous ice nucleation. This coupled CCN-INP effect is not represented in the present framework and therefore constitutes an important limitation that should be clearly acknowledged. As a consequence, references to clean or polluted environments throughout the manuscript should be explicitly restricted to CCN conditions. The title of the paper might also be reconsidered to refer to CCN conditions rather than aerosol conditions more generally.

Thank you and we agree. In our model setup, the sensitivity experiments vary CCN conditions only (implemented as prescribed in-cloud droplet number concentration, Nd), while INP is held fixed. We have clarified this in Sect. 2.1 by adding (lines 122-123): "*In this study, aerosol perturbations are represented by prescribed Nd. INP concentrations are not varied and are not linked to Nd; therefore coupled CCN–INP effects are not represented.*" We have also revised Sect. 2.2 to restrict the terms "clean/moderate/polluted" to low/moderate/high CCN (Nd) conditions (lines 186-187): "*Nd was specified at three representative levels: 200, 400, and 800 cm–3, corresponding to low, moderate, and high CCN conditions (hereafter referred to as 'clean', 'moderate', and 'polluted', respectively).*" We prefer to retain the title and we now clarify that "aerosol conditions" here refer to prescribed CCN (Nd) variations only.

Line 172: Why is the RS process considered together with M1, M2, and BR, rather than analyzing these SIP processes individually? For instance, interpreting the effect of RSM1 alone may be challenging, as RS effects could dominate and obscure the specific contribution of M1.

Thank you for this helpful comment. We agree that the RS+M1, RS+M2, and RS+BR experiments do not isolate the effects of M1, M2, or BR individually. In our experiments, RS was retained as the reference configuration because it is the default SIP process in CASIM. The purpose of these experiments is therefore to assess how adding Mode 1, Mode 2, or BR modifies the simulated response relative to the RS baseline. We have clarified this in the revised manuscript. Specifically, we revised the Introduction to (lines 86-87): "*In addition, we examine the CCN sensitivity of RS and RS-based configurations with additional SIP mechanisms (Mode 1, Mode 2, and BR), to clarify their relative contributions.*" We also revised Section 2.2 to (lines 192-193): "*… RS was retained as the reference configuration because it is the default SIP process in CASIM.*"

Line 178: How is radar reflectivity calculated in the model? A brief description of the radar operator would be helpful here.

We agree that the original manuscript did not clearly describe how radar reflectivity was diagnosed from the model output. We have now added a brief description in Section 2.3 (lines 198-200): "*In the model, radar reflectivity was diagnosed from the simulated hydrometeor particle size distributions using a forward operator based on a simple Rayleigh scattering assumption, consistent with Field et al. (2023).*" The same 10 dBZ threshold was applied to the simulated reflectivity fields for comparison with the CPOL Statistical Coverage Product.

Lines 212–213: While the presence and location of the cloud system are captured, it is evident that the minimum OLR values are higher in all simulations compared to observations. This discrepancy is important and should be at least mentioned.

We have now added a sentence in Section 3.1 to acknowledge this. We also clarify that this systematic bias does not affect the relative differences between experiments that are the focus of the comparison. The revised text reads (lines 244-245): "*It is noted that the minimum OLR values remain higher than observed in all simulations; this systematic bias does not affect the relative differences between experiments discussed here.*"

Section 3.1: The experiment names could be used directly in the text instead of repeatedly describing each configuration. This would significantly improve readability, as the experiment names already depict both the CCN concentration and whether SIP is active.

Overall, the order in which figures and panels are discussed should be made consistent with their layout. For example, in line 224, Figure 3 is introduced starting with panel (b) but it would be clearer to begin with panel (a).

Section 3.1 is overall descriptive and the physical explanations are provided later in Section 3.3. It would be helpful to inform the readers earlier that the underlying mechanisms are discussed in an upcoming section. The same comment applies to Section 3.2 regarding precipitation.

Thank you. We have revised Sections 3.1 and 3.2 to improve readability and to make the presentation of the Results section more consistent.

First, we now use the experiment names more directly throughout the text, rather than repeatedly restating each configuration in descriptive terms.

Second, we have reordered the discussion of the figures so that it follows the panel layout more closely. In particular, the discussion of Figure 3 now begins with panel (a), which serves as the observational reference, before moving to the corresponding simulation panels; the same approach has also been applied to the precipitation analysis in Figure 5.

Third, because Sections 3.1 and 3.2 are intended to describe the radiation and precipitation responses, we have added brief references to indicate that the underlying microphysical mechanisms are discussed later in Section 3.3.1 (lines 224-226): "*In this section, we first describe the radiation responses across the experiments, and the underlying microphysical mechanisms are discussed in Section 3.3.1.*"

Figures 3 and 5: Panels corresponding to SIP and no-SIP simulations could be arranged side by side, with consistent sizes, to facilitate direct comparison. Figure 8 provides a good example of a clear and effective layout.

We have reorganized Figures 3 and 5 so that, for each aerosol condition, the SIP and no-SIP simulations are placed directly side by side with consistent panel sizes. The figure captions and corresponding references in the main text have been updated accordingly.

Figure 7: The axis labels and legends are difficult to read. I suggest enlarging this figure, for example by using a two-column format with five rows.

Thank you for this suggestion. We have revised Figure 7 accordingly by enlarging the figure and improving the readability of the axis labels and legends.

Line 335: Panel 7c is mentioned before panel 7b, which focuses on raindrop number and is not discussed here. The order should be revised.

We have revised the order of the discussion in Section 3.3.1 to follow the structure of Figure 7 more clearly. In the revised manuscript, we now discuss the raindrop diagnostics first, including raindrop number and size (Figs. 7b, g) together with the related RWC evolution (Fig. S2), before moving to the ice crystal, graupel, IWC, and updraft responses (Figs. 7c–j).

Lines 340–341: This result is somewhat surprising, as SIP typically generates small ice particles. Previous studies (e.g., Dedekind et al., 2021) have shown that SIP tends to produce smaller ice particles with reduced sedimentation. This could be mentioned.

We have revised the text to acknowledge that previous studies (e.g. Dedekind et al., 2021). We now contrast that behaviour with the present case, where the SIP-enhanced crystal population is associated with larger mean-mass radii in allSIP-400 and, to a lesser extent, in allSIP-800. The revised text reads (lines 414-418): "*Previous studies have shown that SIP tends to produce numerous smaller ice particles with reduced sedimentation (e.g. Dedekind et al., 2021). In this case, however, the enhanced crystal population is associated with larger mean-mass radii in allSIP-400 and, to a lesser extent, in allSIP-800. This suggests that part of the SIP-generated crystal population continues to grow after formation, likely through vapor deposition aided by stronger updrafts and greater IWC aloft (Field et al., 2023).*"

Lines 343–344: SIP increases ice particle number by several orders of magnitude, while ice water mass increases by only a factor of about two. Given this discrepancy, how can the mean particle diameter increase?

We thank the reviewer for this comment. The key point is that these quantities are not directly equivalent in our diagnostics. Figure 7c and Figure 7h refer to the number concentration and mean-mass radius of ice crystals, whereas Figure 7e shows total IWC, defined as the sum of ice crystals, graupel, and snow. Therefore, the increase in total IWC does not scale directly with the increase in ice crystal number alone. In addition, the larger mean-mass radius does not imply that all SIP-generated crystals are larger. SIP initially generates numerous additional crystals, many of which are likely small, but in the Nd = 400 cm–3 case, and to a lesser extent in the Nd = 800 cm–3 case, stronger updrafts and greater IWC aloft can support continued post-formation growth of part of the crystal population, likely through vapor deposition. We have revised the text to clarify this (lines 415-418): "*… In this case, however, the enhanced crystal population is associated with larger mean-mass radii in allSIP-400 and, to a lesser extent, in allSIP-800. This suggests that part of the SIP-generated crystal population continues to grow after formation, likely through vapour deposition aided by stronger updrafts and greater IWC aloft (Field et al., 2023).*"

Lines 353–356: While enhanced vapor deposition for ice particles has been reported in previous studies (e.g., Dedekind et al., 2021), why is this effect apparent for riming but not for vapor deposition here? Why is the impact stronger on graupel than on snow? If ice mixing ratio and particle size increase, why does snow not respond similarly?

We have revised the text to clarify that the contrasting responses likely reflect different growth pathways in this case. The larger mean-mass radii of ice crystals may reflect continued growth of part of the SIP-enhanced crystal population, likely through vapor deposition under stronger updrafts and greater IWC aloft. By contrast, the graupel response appears to be controlled more directly by riming: SIP increases graupel number concentration, but the available supercooled liquid is distributed among more graupel particles, therefore reducing graupel radius. We also clarify that the snow category shows only a weak response, which may reflect the stronger influence of aggregation and sedimentation on diagnosed snow in this case, consistent with the CASIM framework and with our previous Hector simulation (Field et al., 2023; Sun et al., 2025). The revised text now reads (lines 417-418; 427-431): "*This suggests that part of the SIP-generated crystal population continues to grow after formation, likely through vapor deposition aided by stronger updrafts and greater IWC aloft (Field et al., 2023).*"

"*This suggests that SIP increases the number of graupel particles, while the available supercooled liquid is distributed among more particles, limiting the growth of individual graupel through riming. By contrast, the snow category shows only a weaker response, which may reflect the stronger influence of aggregation and sedimentation on diagnosed snow in this case (Field et al., 2023). This is also consistent with a previous Hector simulation with CASIM (e.g., Sun et al., 2025).*"

Lines 363–364: What about the role of water vapor availability for deposition?

Thank you. Under low-CCN conditions, larger cloud droplets and earlier rainfall suggest a stronger warm-rain contribution, which likely reduces the available supercooled liquid content for subsequent ice growth. In addition, this case is characterized by a relatively dry mid-tropospheric environment and a weak updraft response, which are less favorable for sustained vapor deposition. We have added this explanation to the revised manuscript. The revised text reads (see lines 439-442): "*Under low-CCN conditions, larger cloud droplets and earlier rainfall suggest a stronger warm-rain contribution, which likely reduces available supercooled liquid content. At the same time, the relatively dry environment and weak updrafts are less favorable for sustained vapor deposition, limiting further ice-mass growth.*"

Line 393: Figure 7b is referenced here, even though Figure 7 began to be discussed much earlier. This is a bit confusing for the readers. The section can be reorganized to group all discussions of Figure 7 together.

We thank the reviewer for this suggestion. We have reorganized Section 3.3.1 and the discussion of Figure 7 is presented in a more structured way, which improves the flow of the section and makes it easier for readers to follow.

Lines 404–406: This explanation is a bit difficult to follow. Referring directly to the experiment names, rather than to "Nd = …" and "SIP/noSIP", would improve the clarity of this sentence. This approach could also be applied elsewhere in the manuscript (e.g., lines 451–453).

We have revised the relevant sentences to refer directly to the experiment names. Specifically, in Section 3.3.1, we now write (lines 388-391): "*In particular, allSIP-200 shows a smaller radius than noSIP-200 at 5–7 km, with allSIP-200 reaching <160 µm compared with ~200 µm in noSIP-200. … In contrast, allSIP-400 and allSIP-800 produce larger raindrops than noSIP-400 and noSIP-800 by ~30–100 µm above 5 km, …*" In Section 3.3.2, we similarly revised to (line 501):

"…, the largest Wmax occurs in RS-400 and RS-800, RSM1-200, RSM2-800, and RSBR-400." We have also applied this naming style elsewhere.

Line 410: Can you specify which figure is discussed?

We have now specified the relevant figure at the start of the sentence. The revised text now reads: "Time–height sections of rainwater content (RWC) further support this pathway (Figure S2 in the Supplement)." We then refer to Figure S2b when describing the RWC enhancement.

Line 423: When multiple SIP processes are active (e.g., RSM1), it is difficult to disentangle the contribution of each process. An alternative approach could be to analyze differences such as RS–RSM1, but I leave this up to the authors consideration.

We agree that diagnostics such as RSM1−RS could help isolate the contribution of an added SIP mechanism. In this study, we retain the current analysis approach because RS is the default SIP process in CASIM, and the purpose of the RS+M1, RS+M2, and RS+BR experiments is to evaluate how the newly implemented SIP mechanisms modify the simulated deep convective cloud responses under different CCN conditions. Using noSIP as a common reference also provides a consistent baseline for comparison across all SIP configurations.

Section 4: This section provides a clear and useful summary of the results, clarifying the main findings presented in the previous sections.

We thank the reviewer for this positive comment.

Lines 526–527: These results should be balanced with other studies that have shown SIP can reduce particle size and precipitation efficiency (e.g., Dedekind et al., 2021).

We have revised this part of the Discussion to better balance our findings with previous studies. We now clarify that different precipitation responses to SIP have been reported. The revised text now reads (lines 582-585): "*Previous studies also suggest that SIP impacts on precipitation can depend strongly on the parameterization used, and may increase ice particle number while suppressing localized or heavy precipitation (e.g., Dedekind et al., 2021; Grzegorczyk et al., 2025b).*"

Lines 560–561: This statement is not universally valid and may hold only for this specific case, which is particularly important to state given that the following sentence mention a study reporting opposite effects.

We agree and have revised the text to clarify that this statement is specific to this case. The revised text now reads (lines 612-615): "*SIP generates additional ice particles that likely promote riming and subsequent melting, which may provide an extra source of surface rainfall. This may help explain why SIP increases precipitation in the 6 February 2006 case, whereas the pre-monsoon case showed the opposite response.*"

Lines 588–589: I do not see a clear link between the cited papers and the purpose of this sentence.

Thank you for pointing this out. The cited papers were originally included to indicate the physical requirements of the droplet-shattering mechanisms. The main purpose of this sentence, however, is to summarize the non-linear CCN dependence diagnosed from our simulations. We therefore removed these citations to avoid a mismatch between the cited papers and the purpose of the sentence.

Lines 628–631: In my view, this suggests that snow amounts might also increase, as higher ice number, size, and mass should favor conversion to snow.

We agree that increases in ice particle number, size, and total ice mass could potentially favor conversion to snow. First, we have revised Section 3.3.1 to clarify that, in this case, the diagnosed snow response to SIP remains weak (lines 429-431): "*By contrast, the snow category shows only a weaker response, which may reflect the stronger influence of aggregation and sedimentation on diagnosed snow in this case (Field et al., 2023). This is also consistent with a previous Hector simulation with CASIM (e.g., Sun et al., 2025). We therefore focus on crystals and graupel here.*" Second, we have also clarified in the Summary section by adding (lines 687-688): "*The diagnosed snow response to SIP remains weak in this case, even though these changes could potentially favor conversion to snow.*"

Lines 638–639: It should be noted that the simulations still differ significantly from observations.

We agree and have therefore added a sentence to clarify this point in the Summary (lines 704-706): "*It should be noted that, although allSIP-400 shows the best agreement among the simulations, differences from the observations remain in both radiation and precipitation.*"

Conclusions: In addition to uncertainties related to the microphysical assumptions of the model, it should be emphasized that the results strongly depend on the SIP parameterizations used. The physical understanding of SIP mechanisms and therefore their parameterizations remains subject to large uncertainties and ongoing debate. This important limitation should be clearly acknowledged in the conclusions.

We thank the reviewer for this comment. We agree that the conclusions should more clearly acknowledge the limitations associated with SIP parameterizations and the current uncertainty in the physical understanding of SIP processes. We have therefore revised the Conclusions section to state that the CCN sensitivity of individual SIP mechanisms depends on the adopted parameterizations, microphysical assumptions in the model, and environmental conditions, and that the representation of SIP processes remains uncertain and under ongoing debate. The revised text now reads (lines 716-717): "*In addition, the physical understanding and representations of SIP processes are still subject to substantial uncertainty and ongoing debate.*"

Referee #2

Major Comments

The study is stimulating to read. There are some fascinating findings about SIP impacts on deep convection, especially with the apparent effect on latent heating and vertical motions. The updrafts seem to be invigorated by SIP in this deep convection.

Yet, regarding realism, the paper is not very convincing because there is no validation of the ice concentration, which is the topic of the paper, nor of the LWC. It is the ice concentration that the SIP determines, and all the effects on the cloud from SIP occur via the ice concentration.

Yes, I guess there may have been little sampling of the convective cores by the aircraft in ACTIVE. But validation of the convective outflow is still possible in the weak ascent.

Yes, before 2011, there were no tips on the aircraft probes. But it is still possible to validate the filtered ice concentration for ice particles larger than a certain size (e.g. about 0.4 mm), after correcting the data with the Field et al. (2006) inter-arrival time method (see Figure 5 of Korolev et al. 2011). Moreover, the supercooled LWC is coupled with the ice concentration in the mixed-phase region, and the LWC can be validated easily.

So the question arises, how do we know the predicted ice concentrations shown in the sensitivity tests are realistic?

For this reason I recommend major modifications to the paper, with addition of validation of cloud properties such as LWC and ice concentrations against any coincident aircraft data from flights on the simulated day in ACTIVE. And similarly, predicted active IN concentrations, if in situ measurements exist, should be validated too.

Also, it is unclear how raindrop-freezing is treated and whether it is related to the IN conditions. This is pertinent as the study funds a large role for raindrop-freezing fragmentation.

We thank the reviewer for the evaluation of this study. We agree that the simulated SIP response should be better evaluated against observations. In the revised manuscript, we have added the available aircraft observations in Section 2.3 and included an in-situ comparison of upper-level ice properties in the Supplement. We have also clarified the cloud-base conditions from the sounding, added a clearer statement on the uncertainty in primary ice representation, and revised the text to explain how raindrop freezing is treated in the model. Detailed responses to these points are given below.

Detailed Comments

1. Introduction

Line 11: you could also mention that tropical deep convection redistributes many other tracers, like aerosol species and trace gases. It governs the vertical gradient of temperature and humidity in the troposphere, and determines where the tropopause is, by the radiative-convection equilibrium.

An atmosphere without deep convection would be almost impossible to imagine.

Thank you. We have revised the Introduction to better reflect the role of tropical deep convection. The revised text now reads (lines 31-34): "*It also redistributes aerosols and trace gases, shapes the vertical structure of temperature and humidity in the troposphere, and contributes to setting the tropical tropopause through radiative–convective equilibrium (e.g., Houze, 2004; Fueglistaler et al., 2009; Sherwood et al., 2014).*"

Line 44: It might be a good idea to insert "as reviewed by" before the references given since the studies by Field et al., 2017; Korolev and Leisner, 2020; Huang et al., 2022 are not observational studies per se and are merely functioning as review papers effectively here.

We agree that these references are cited in a review context and we have revised the sentence to make this clearer. The revised text now reads (lines 47-48): "*while SIP signatures have also been reported in both mid-latitude and tropical convection, as reviewed by Field et al. (2017), Korolev and Leisner (2020), and Huang et al. (2022).*"

Line 75: Where it is written "Huang et al. (2025) linked CCN impacts to changes in SIP and subsequent storm electrification", the two-part paper by Phillips and Patade (2022) did this as well.

Thank you. We have included the study by Phillips and Patade (2022), in addition to Huang et al. (2025), as relevant works linking CCN impacts to SIP and subsequent storm electrification.

2. Method

2.1 Model Description

Line 105: How do you treat ice-ice aggregation? Is the treatment consistent with Connolly et al. (2012) observations of the aggregation efficiency and its temperature dependence? How do you treat sticking efficiency for aggregation?

In CASIM, collection processes include both aggregation, defined as self-collection of a given hydrometeor type, and accretion between different hydrometeor species (Field et al., 2023). The collection efficiencies are prescribed as temperature-dependent functions:

$E_{si} = E_{gi} = E_{gs} = 0.2\exp(0.08T)$,

and $E_{ss} = 0.1\exp(0.08T)$ for self-collection of snow, where T is temperature in degrees Celsius.

By contrast, Connolly et al. (2013) noted that the Morrison et al. (2005) scheme used a fixed ice-aggregation efficiency, $E_{agg} = 0.1$, at all temperatures, and suggested that this value may be too low for the Hector anvil. Thus, the CASIM treatment used here differs from that earlier fixed-efficiency assumption by representing ice-phase collection efficiencies with temperature dependence. We have added the following sentence (lines 121-124): "*In CASIM, aggregation and accretion among ice-phase hydrometeors are represented through parameterizations for self-collection and inter-species collection processes involving ice, snow, and graupel. The associated collection efficiencies are prescribed in the model and are temperature dependent.*"

Line 112: Is raindrop-freezing in collisions between supercooled raindrops and ice crystals represented ? Chisnell and Latham (1976) and others subsequently predicted that it must be essential. Blyth et al. (1997) and Hallett et al. (1978) observed supercooled rain in association with ice multiplication subsequently, consistent with the Chisnell and Latham prediction.

Amazingly, some models in the past have completely omitted collisional raindrop freezing (E.g. RAMS).

Thank you for this comment. In CASIM, raindrop freezing is represented through a bulk heterogeneous freezing parameterization following Bigg (1953), in which freezing raindrops are converted to graupel. In addition, rain–ice interactions are represented through accretion/collection processes in CASIM, and rain collecting ice can contribute to snow or graupel formation depending on the hydrometeor state and model configuration (Field et al., 2023). We have revised the text accordingly (lines 119-121): "*In addition, rain–ice interactions are represented through accretion/collection processes in CASIM, and rain collecting ice can contribute to snow or graupel formation depending on the hydrometeor state and model configuration.*"

Line 114: I am guessing there might be a typo here: "Homogeneous freezing of cloud droplets occurs below –38 °C". In fact, homogeneous freezing of cloud-liquid happens over a narrow range of temperatures, depending on droplet size, with most cloud droplets (about 20 microns) freezing at about -37 or -36 degC. See the plot in Pruppacher and Klett (1997). Do you mean "below the -38 degC level "?

We agree that the original wording was intended to describe the model parameterization. We have revised the text to clarify this is the treatment used in CASIM. The revised sentence now reads (line 119): "*Homogeneous freezing of cloud droplets is parameterized to occur below –38 ℃ (Field et al., 2023).*"

2.2 Model set-up and design of the simulations

Line 160: Where it is written "with a model time step of 75 seconds", I wonder if there might be a typo here. The model referred to here seems to be the cloud-resolving model of resolution 1.5 km. But that time-step seems unusually coarse.

Let us assume there is no typo. The horizontal Courant number would be 30 m/s (peak horizontal velocity, such as around the gust front) divided by the numerical solution maximum speed of 1500/75 = 20 m/s. So the Courant number would exceed 1. But I guess the semi-Lagrangian scheme with ENDGame (semi-implicit for some physics) and numerical limiters (van Leer limiter?) avoids the CFL condition for numerical stability. Parcels in the model can go by more than one grid-spacing per time-step with Lagrangian treatment.

So, even though the CFL condition for stability does not apply to such schemes, accuracy with such long timesteps must be an issue. Microphysical processes in reality happen on faster time-scales than 75 sec (e.g. precipitation fallout, vapour diffusion).

With the vertical motions, updraft speeds of 20 or 30 m/s and a fine vertical resolution (e.g. 200 m) would imply vertical Courant numbers of more than 10.

Is there robustness of the cloud statistics predicted with respect to the choice of timestep?

One would expect the variability inside the storm to be underestimated by the model, with peak heating too weak and peak updrafts too slow, with such Courant numbers.

Thank you for this comment. The 75 s time step is not a typo. As shown in Fig. R1 of Field et al. (2023), the UM semi-Lagrangian time-step structure is not a single explicit update: two Atmospheric Physics calls are separated by the advection step, and the semi-Lagrangian advection scheme uses an iterative step around Atmospheric Physics 2 to compute the departure points.

Field et al. (2023) also describe the hydrometeor sedimentation treatment used for relatively large time steps. Because hydrometeors could otherwise cross several vertical levels within one time step, sedimentation is handled using the existing UM approach based on Rotstayn (1997). The increment for mass mixing ratio qx or number mixing ratio (replace qx by nx) due to flux divergence as hydrometeors fall is

$$P_{\mathrm{sedx}} = \frac{R_{\mathrm{f}}}{\rho \Delta z} - q_x \frac{\bar{V}_x}{\Delta z},$$

which when integrated analytically gives

$$q_x(t + \Delta t) = q_x(t)[\exp(-\alpha)] + \frac{R_{\mathrm{f}} \Delta t}{\rho \Delta z}[1 - \exp(-\alpha)],$$

where $\alpha = \bar{V}_x \Delta t / \Delta z$ is the Courant number, $\bar{V}_x$ is the mean mass-weighted or number-weighted fall speed, depending on whether mass or number is being sedimented, and Δt and Δz are the time step and layer thickness respectively. R_{f} is the flux from above ($\rho q_x V_x$ or $\rho n_x V_x$ from the layer above). This approach of using an exponential formulation when Courant numbers are large provides stability for the model (Field et al., 2023).

To clarify this point, we have added the following sentence in Section 2.2 (lines 173-176): "*In this configuration, the semi-Lagrangian dynamical core permits relatively large time steps. To avoid hydrometeors crossing multiple vertical levels within a single time step, sedimentation is treated using an approach based on Rotstayn (1997), as implemented in the UM (Wilson and Ballard, 1999; Field et al., 2023).*" In addition, all SIP sensitivity experiments in this study were performed with the same numerical configuration, so that the reported differences can be attributed to the relative responses to different SIP mechanisms rather than to differing numerical settings.

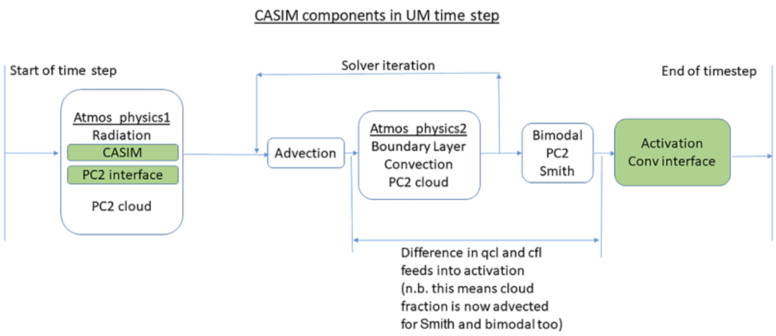

Figure R1. Schematic of Unified Model time step indicating in green boxes where CASIM-related changes are made.

2.3 Observational data

Bizarre that there is no mention of aircraft flights. There were two planes in ACTIVE, which must have sampled layer-cloud properties even if they did not sample the convective cores above the freezing level. Most of the ice particles in the convectively generated layer-cloud are probably generated in the convective cores.

Some validation against aircraft data needs to be shown for the simulated cloud properties for the range of vertical velocities observed.

Why include aircraft in a field campaign when the modellers do not use the aircraft data to compare their models with? How do we know the ice concentrations are accurate?

And if SIP is being modelled, why is there no comparison of the predicted IN concentrations with any in situ CFDC observations ? Or was there no CFDC deployed in ACTIVE? How do we know Cooper (1986) is applicable to that day and location?

Imagine that for whatever reason, the primary ice is drastically under-predicted: then the SIP would likely be over-predicted, since the ice multiplication tends to continue until a maximum ice concentration is reached near the onset of water saturation.

Need to make note of the cloud-base temperature. What does that imply about the balance between warm-rain and ice-crystal processes in the contributions to surface precipitation? Gupta et al. (2023) found that the warm rain process prevails (80%) in simulations of Brazilian tropical deep convection.

Thank you for this comment. We address this point in the following three parts.

(i) In-situ validation of cloud properties

Aircraft observations from the ACTIVE campaign have now been considered. Two aircraft were relevant to Hector: the NERC Dornier, which sampled the lower troposphere up to 4 km, and the Egrett aircraft, which sampled the anvil outflow at 12–14 km (Vaughan et al., 2008). However, the available in-situ measurements of ice water content (IWC) and ice particle number concentration were limited to upper-level cloud and anvil sampling, and did not provide sufficient coverage of the mixed-phase convective region where SIP is expected to be most active. We therefore use these aircraft observations as a limited consistency check on upper-level ice properties. The comparison shows that the simulated upper-level Nice and IWC are broadly consistent with the available aircraft observations (Figure R2). We have added this comparison to the Supplement as Figure S1. We have also added the following text in Section 2.3 (lines 208-213): "*Aircraft observations from the ACTIVE campaign were also considered. The NERC Dornier sampled the lower troposphere up to 4 km, whereas the Egrett sampled the anvil outflow at 12–14 km (Vaughan et al., 2008). Available in-situ measurements of IWC and ice particle number concentration were limited to upper-level cloud, and did not adequately sample the mixed-phase convective region where SIP is expected to be most active. A comparison of these upper-level ice properties is provided in Figure S1 in the Supplement.*"

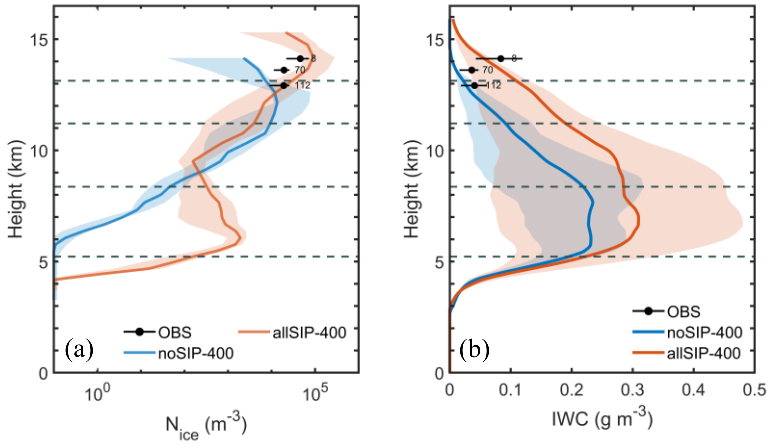

Figure R2. Vertical mean profiles of (a) ice number concentration (N_{ice}) and (b) ice water content (IWC) from the allSIP-400 and noSIP-400 simulations, compared with upper-level aircraft in-situ observations. Both the simulated and observed profiles are calculated over regions where IWC > 0.01 g m^{-3} during 02:30–08:30 UTC on 6 February 2006. Observational samples are grouped into 0.5 km height bins. Black dots show the bin-mean values, black horizontal bars show the first and third quartiles, and numbers indicate the sample count in each bin. Shaded areas indicate the model interquartile spread. Gray dashed lines indicate the 0, −20, −40, and −60 °C isotherms. The simulated upper-level Nice and IWC are broadly consistent with the available aircraft observations. It should be noted that this comparison does not include mixed-phase convective region because the corresponding aircraft measurements were only available in upper-level anvil clouds.

(ii) CFDC observations and primary-ice uncertainty

We agree that uncertainty in primary ice is relevant when interpreting the simulated SIP contribution. In the present study, heterogeneous ice nucleation follows Cooper (1986). The ACTIVE aircraft primarily sampled low-level inflow aerosol and upper-level anvil outflow properties. In particular, the campaign description indicates sampling by the Dornier in the lower troposphere and by the Egrett in the anvil outflow, but there were no in-situ INP measurements available for this case. To make this limitation clearer, we have added the following statement in Section 4.2 (lines 669-672): "*A further uncertainty concerns the representation of primary ice. In the present study, heterogeneous ice nucleation follows Cooper (1986), and this may affect the relative enhancement attributed to SIP. The magnitude of the simulated SIP contribution should therefore be interpreted with caution.*"

(iii) Cloud-base temperature

We agree that the cloud-base thermodynamic environment should be stated more clearly, because it helps interpret the likely balance between warm-rain and ice-phase contributions to surface precipitation. The sounding in Figure 1b indicates a warm and moist lower troposphere, suggesting a relatively low and warm cloud base together with a relatively deep warm-cloud layer. Such a structure is likely to favor early warm-rain development before hydrometeors enter the mixed-phase region. We have added the following sentence alongside the sounding description in Section 2.2 (lines 156-158): "*The sounding also suggests a warm cloud base and a relatively deep warm-cloud layer, which likely favor warm-rain formation during the early stage of storm development.*"

3 Results

3.1 SIP impacts on radiation under varying CCN conditions

Line 196: These simulation names are introduced for the first time without any explanation. What is the difference between allSIP-200 and allSIP-400? Is it the aerosol concentration ?

Thank you for this comment. We have revised this sentence to make the meaning of the experiment names clearer. As the simulation set-up and prescribed Nd values were already introduced in Section 2.2, we here clarify that allSIP-200, allSIP-400, and allSIP-800 refer to the allSIP simulations under clean, moderate, and polluted CCN conditions. The revised text now reads (lines 220-222): "*Figure 2 shows the spatial distribution of observed MTSAT-derived equivalent outgoing longwave radiation (OLR), together with simulated OLR from three allSIP simulations under clean, moderate, and polluted CCN conditions (allSIP-200, allSIP-400, and allSIP-800).*"

Line 217: weaker enhancement of what?

We agree and have revised the sentence to specify that the weaker enhancement refers to a weaker reduction in OLR in allSIP-200 relative to noSIP-200. The revised text now reads (lines 246-247): "*For Nd = 200 cm–3, allSIP-200 shows a weaker OLR reduction relative to noSIP-200 (Figures 2e, S2b), with a minimum OLR of ~165 W m–2 and a smaller increase in the low-OLR coverage (~10–15%).*"

Line 224: "broader vertical extent" would be better written as "deeper vertical extent". Similarly, "a broader yet optically thinner distribution" should be "a deeper yet optically thinner distribution". "Broad" has connotations of horizontal coverage, whereas I think you are talking about the vertical aspects of distributions.

We agree that the term "broader" may imply horizontal extent, whereas the intended meaning here is the vertical development. We have revised the wording accordingly.

Line 231: It is written that "Overall, Nd = 400 cm–3 shows a better agreement with the observation". Surely, it is possible to say if aircraft data from the case are consistent with this droplet number ? Need some validation of cloud properties in situ.

We have addressed this point by adding the available aircraft observations in Section 3.2 and an in-situ comparison of upper-level cloud properties in the Supplement (Fig. S1). Our statement that Nd = 400 cm^{-3} shows better agreement remains based mainly on the radar and satellite comparisons.

Figure 2: the caption seems wrong. Are all the panels showing OLR, with (a)-(c) being observations and the rest the model ? You have not used visible imagery at all here because the entire figure is about the longwave imagery. Delete "visible".

Thank you for pointing this out. We have revised the Figure 2 caption to clarify that panels (a)–(c) show the observed MTSAT-derived equivalent outgoing longwave radiation (OLR). The revised caption now reads (lines 234-235): "*Figure 2. Spatial distribution of MTSAT-derived equivalent outgoing longwave radiation (OLR; W m–2) at (a) 07:30, (b) 08:30 and (c) 09:30 UTC on 6 February 2006.*"

3.2 SIP impacts on precipitation under varying CCN conditions

Line 286: It is written that "SIP accelerates glaciation, shifting water mass from the liquid to the ice phase during the early storm stage, reducing the efficiency of warm-rain production and suppressing peak rates." More detail here would be good. Is the idea that supercooled liquid is accreted onto extra ice precipitation (boosted by SIP) that falls relatively slowly and melts to form smaller drops that are more likely to evaporate before reaching the surface? Or is it that the extra crystals from SIP cause the supercooled liquid to evaporate in regions of weak ascent, so that less liquid can be accreted onto warm rain?

We have revised the text to focus on the interpretation directly supported by the present analysis. Our intention is to indicate that SIP promotes earlier glaciation and shifts part of the condensate from the warm-rain pathway to the ice phase, likely reducing the liquid water available for collision–coalescence during the early storm stage. To make this interpretation more precise, we have therefore revised the text as follows (lines 316-320): "*In clean conditions, early precipitation formation likely involves a stronger contribution from warm-rain processes. SIP accelerates glaciation during the early storm stage, shifting part of the condensate from the liquid to the ice phase and likely reducing the liquid water available for warm-rain collision–coalescence. This is consistent with the lower peak precipitation rates under clean conditions.*"

3.3 SIP impacts on cloud microphysics under varying CCN conditions

Line 363: It is written that "Because warm-rain processes dominate in clean air". But how do you know this to be true? There is no tagging tracer for the component of precipitation from the warm rain process.

We agree and have revised the manuscript to make this interpretation more cautious and better supported by the simulated diagnostics. Specifically, in Section 3.2 we now write (lines 316-320): "*In clean conditions, early precipitation formation likely involves a stronger contribution from warm-rain processes. SIP accelerates glaciation during the early storm stage, shifting part of the condensate from liquid to ice phase and likely reducing the liquid water available for warm-rain collision–coalescence. This is consistent with the lower peak precipitation rates under clean conditions.*"

In Section 3.3.1, we further clarify this interpretation using the microphysical diagnostics (lines 437-440): "*Under low-CCN conditions, larger cloud droplets and earlier rainfall suggest a stronger warm-rain contribution, which likely reduces available supercooled liquid content. At the same time, the relatively dry environment and weak updrafts are less favorable for sustained vapor deposition, limiting further ice-mass growth.*"

4 Conclusions

Line 607: You could compare the relative abundances of SIP mechanisms with Deepak Waman's papers from 2022 to 2024. He shows some budgets from using tagging tracers for components of ice concentration from various SIP mechanisms.

Thank you for this suggestion. We have now added a comparison with previous studies that used tagging tracers to diagnose the relative contributions of individual SIP mechanisms. The text now reads (lines 658-660): "*Previous tagging-tracer analyses in continental deep convection also found a dominant contribution from BR to total ice number, whereas droplet shattering contributed comparatively less (e.g., Waman et al., 2022).*"

It is worth saying that the tropical cloud-base, coupled with an optimal aerosol concentration, can cause many supercooled raindrops to be upwelled aloft, which then supports raindrop-freezing fragmentation. What is the order of magnitude of the supercooled raindrops above the freezing level ? If there are hundreds per Litre, that would explain why raindrop-freezing fragmentation is so prolific here.

We agree that the abundance of raindrops above the freezing level is relevant for interpreting the strong droplet-shattering response in this case. The rain diagnostics confirm that simulated raindrops are present above the freezing level, but the rain number concentration in this layer is much lower than hundreds per litre (Figures 7b, S5). Therefore, we suggest that the strongest response at Nd = 400 cm−3 reflects a more favorable co-existence of raindrops with larger rimed ice particles and stronger liquid–ice overlap in the mixed-phase layer, which allows the droplet-shattering mechanisms to operate more effectively.

It is unclear whether the model resolves heterogeneous raindrop-freezing and collisional raindrop-freezing separately. If it does, does the model relate the heterogeneous raindrop-freezing to the IN activity (e.g. by modifying the Bigg scheme)?

In the configuration used here, raindrop freezing is represented through immersion freezing following Bigg (1953), in which rain mass and number are converted to graupel. In addition, rain–ice interactions are represented through accretion/collection processes in CASIM, and rain collecting ice can contribute to snow or graupel formation depending on the hydrometeor state and model configuration. Thus, heterogeneous raindrop-freezing and rain–ice collisional interactions are both represented in the model, and the latter is treated within the accretion/collection framework. The heterogeneous raindrop-freezing treatment is not linked to IN activity in the present experiments, because aerosol perturbations are represented only by prescribed Nd, while INP concentrations are not varied. We have clarified the manuscript wording accordingly in Section 2.1 by adding (lines 119-121): "*In addition, rain–ice interactions are represented through accretion/collection processes in CASIM, and rain collecting ice can contribute to snow or graupel formation depending on the hydrometeor state and model configuration.*" We also clarify that (lines 122-123): "*In this study, aerosol perturbations are represented by prescribed Nd. INP concentrations are not varied and are not linked to Nd; therefore coupled CCN–INP effects are not represented.*"

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