RESPONSE TO REVIEWER 2:

We sincerely thank Reviewer 2 for their careful reading of our manuscript and for the constructive and helpful comments. We have revised the manuscript accordingly and provide a point-by-point response below. Reviewer comments are shown in red, and our responses are shown in black. Page and line numbers in our responses refer to the revised manuscript.

This study investigates the importance of different secondary ice production (SIP) processes, including rime splintering (RS), ice–ice collisions (CB), fragmentation during spherical freezing of drops (M1), and fragmentation resulting from collisions between supercooled droplets and larger ice particles (M2), using both model simulations and aircraft campaign observations. The University of Manchester bin-microphysics parcel model (BMM) was employed to conduct the simulations, together with in situ cloud microphysics and aerosol measurements collected during the Deep Convective Microphysics EXperiment (DCMEX) field campaign. One focus of the study is the impact of different entrainment-mixing scenarios on cloud droplet formation and the subsequent influence on SIP processes. The results show that droplet size distributions are better simulated when entrainment is included. In most cases, homogeneous mixing combined with aerosol entrainment provides the best agreement with observed ice-cloud microphysical properties, and no evidence was found that inhomogeneous mixing dominates in the simulated cases. Regarding the relative importance of the different SIP mechanisms, M2 appears to be the dominant process. However, uncertainties remain in the simulation of the other SIP processes due to the limitations of the idealized parcel-model framework. Overall, this study is timely and provides a valuable contribution to the cloud microphysics community, and I have the following suggestions:

General:

RC: In the results shown in Figures 5–7, the simulated ice properties are comparable to the observations, but only for the upper portion of the clouds. The authors suggest that the absence of simulated ice in the lower part of the cloud is due to the lack of vertical transport processes in the parcel model. However, could other processes, such as lateral mixing or turbulent mixing, also contribute to this discrepancy? More importantly, it would be helpful to clarify how the absence of sedimentation and vertical transport in the parcel model may affect the simulated microphysical properties in the upper portion of the cloud.

AC: We thank the reviewer for this helpful comment. We have clarified in the revised Discussion the limitations of the one-dimensional parcel model, including its inability to explicitly represent lateral mixing, turbulent mixing, sedimentation and vertical transport. This discussion has been added in Lines 502--509.

RC: The authors discuss the uncertainties associated with the parameterizations of the different SIP processes, as well as the limitations arising from the use of a parcel model in representing these processes. The results suggest that DS M2 is the most active SIP mechanism for the DCMEX cases. However, given the large uncertainties associated with all investigated SIP processes, the conclusion may need to be stated more cautiously.

We thank the reviewer for this helpful comment. We agree that the conclusion should be stated more cautiously given the uncertainties in the SIP parameterisations and the limitations of the parcel-model framework. We have therefore revised the conclusion as follows:

“When the four SIP mechanisms (RS, CB, M1 and M2) are examined separately, our results suggest that M2, as currently understood and represented in the model, is the most important contributor to ice enhancement in the DCMEX deep convective cloud cases.”

RC: The description of the SIP processes in Section 3.3 could be expanded, particularly for CB and DS. In addition, M1 and M2 are treated here as two separate mechanisms, whereas they are initially introduced as two modes of the DS mechanism. This distinction should be clarified for consistency.

AC: We thank the reviewer for this important comment. We have substantially revised the description of the SIP parameterisations and moved the detailed formulation of all SIP mechanisms to Sect. S1 of the Supplement. The description of the BMM model framework has also been revised to make the implementation of these processes clearer. We have also clarified that M1 and M2 are two modes of the DS mechanism, but are treated separately in the simulations because they represent distinct physical pathways and are parameterised separately. This clarification has been added to the Introduction in Lines 49--62.

Specific:

RC: L20-21: Please also include this paper as one of the references: Ladino, L. A., Korolev, A., Heckman, I., Wolde, M., Fridlind, A. M., and Ackerman, A. S.: On the role of ice-nucleating aerosol in the formation of ice particles in tropical mesoscale convective systems, Geophysical research letters, 44, 1574–1582, https://doi.org/10.1002/2016GL072455, 2017.

AC: We thank the reviewer for this suggestion. The reference to Ladino et al. (2017) has been added to the revised manuscript.

RC: L42: HAIC/HIWC is not explained.

AC: The abbreviation HAIC–HIWC has now been defined at its first occurrence as “High Altitude Ice Crystals and High Ice Water Content”.

RC: L71: Qu et al., 2022 instead of 2020.

AC: We thank the reviewer for pointing this out. The sentence has been revised, and the citation has been corrected to Qu et al. (2022).

RC: L96: κ is not explained here which appears later in L139.

AC: We thank the reviewer for pointing this out. We have now defined κ as the aerosol hygroscopicity parameter at its first occurrence in the revised manuscript.

RC: L99: "DMT" is not explained.

AC: DMT refers to Droplet Measurement Technologies. As it is not essential in this context, it has been removed for clarity.

RC: L188: "10,s" should be 10 s.

AC: Changed.

RC: Table 1: Please clarify what is meant by ‘neighbouring case’. Does this refer to cases with similar atmospheric conditions that are close in time or geographical location?

AC: We thank the reviewer for pointing this out. We agree that the term “neighbouring case” was ambiguous. We have revised the table note as follows: “Values marked with an asterisk (*) were taken from the closest available DCMEX case with similar atmospheric conditions because case-specific data were not available.”

RC: Figure S5: The color coding appears inconsistent with that used in Fig. S2. In Fig. S2, the ‘blue’ used for the simulations appears closer to purple, whereas in Fig. S5 both solid and dashed blue lines are shown, and the color is similar to that used for the observational dots in Fig. S2. Please ensure that a consistent color scheme is used across all figures.

AC: We thank the reviewer for pointing this out. We have revised the colour scheme so that the simulation colours are consistent across Figs. S3–S14. The observational scatter points have also been changed to light blue to distinguish them more clearly from the simulation lines. In addition, we found that the solid green line for INHOM+EA and the dashed orange line for INHOM+RA had been interchanged in Figs. S3–S5. This has now been corrected in both the Supplement and the corresponding figure in the main manuscript.

RC: Figure 3: The observational data shown as dots overlap substantially, making it difficult to discern the distribution from these plots. It may be more effective to present the results using boxplots, both for all cases and for the subset corresponding to the convective core. In the caption of Fig. 3, the order of CDNC and LWC appears to be reversed. In addition, the caption states that ‘Blue dots indicate observation,’ but the observational points appear in multiple colors. Please adjust this. Finally, many observational data points have values of zero. Could these be screened out if they correspond to clear-sky conditions?

AC: We thank the reviewer for pointing out these issues. We have revised Fig. 3 and its caption. The order of CDNC and LWC in the caption has been corrected, and the description of the observational points has been changed to “Coloured dots indicate observations, with the colour scale defined by the corresponding colour bar in each panel." We have also increased the font size to improve readability. Observational points with LWC below 0.01 g/m^3 have been removed, as these values may correspond to clear-sky or near-clear-sky conditions.

We considered the suggestion of using boxplots, but retained the scatter-plot format because the colour-coded LWC values provide useful information for distinguishing cloud-core-like and cloud-edge-like observations. In this study, higher-LWC points are more representative of cloud-core conditions, whereas lower-LWC points are more representative of cloud-edge or more diluted conditions. This distinction is important for our analysis of entrainment.

RC: L271-272: "average observed CDNC", again, difficult to tell based on the overlapped dots.

AC: We thank the reviewer for pointing this out. The revised sentence now states that *the inhomogeneous mixing simulations were in reasonable agreement with the observed CDNC values associated with relatively low LWC, as indicated by the blue and green dots.*

RC: Figure 3 & 4: The font is a little bit too small. Please increase it.

AC: We thank the reviewer for pointing this out. The font sizes in Figs. 3 and 4 have been increased in the revised manuscript to improve the readability of the axis labels, tick labels, and figure annotations.

RC: L320: "ICNC" is not defined.

AC: We thank the reviewer for pointing this out. We have revised the sentence and no longer use the term ICNC in this part of the manuscript. Instead, the SIP-off and SIP-on simulations are now described explicitly as N_{INP} and N_{ICE}, respectively,

RC: L325: Do you mean Figure 8 here instead of Figure 9?

AC: We thank the reviewer for pointing this out. The original reference to Fig. 9 was incorrect. In the revised manuscript, we have rewritten the paragraph describing the ice simulations to improve clarity and have added the correct reference to Fig. 5.

RC: Figure 5: The orange solid line is difficult to distinguish from the red line, as the colors appear too similar.

AC: We thank the reviewer for pointing this out. We have changed the orange solid line to a bright yellow line in Fig. 5 to improve the visual distinction from the red line.

RC: L350: In "INHOM and INHOM+EA", Should INHOM+EA be INHOM+RA?

AC: We thank the reviewer for raising this point. In our simulations, the cases without aerosol release are INHOM and INHOM+EA, where EA denotes entrained aerosol but does not include aerosol release. However, this issue no longer applies because the paragraph has now been rewritten in the revised manuscript, and the original wording has been removed.

RC: L352: In “INHOM+EA+RA and INHOM+RA”, should INHOM+RA be INHOM+EA?

AC: We thank the reviewer for raising this point. In our simulations, the cases including aerosol release are INHOM+EA+RA and INHOM+RA. Therefore, INHOM+RA was the intended notation in the original sentence, rather than INHOM+EA. However, this issue no longer applies because the paragraph has now been rewritten in the revised manuscript, and the original wording has been removed.

RC: L354-357: Could the authors provide additional cases to support this claim? The current example shows only one frozen drop with an irregular shape. More evidence would be needed to substantiate the conclusion.

AC: We thank the reviewer for this helpful comment. We agree that the original statement was too strong, as the CPI image alone cannot substantiate a firm conclusion that M2 occurred. This feature was only clearly identified in the 25 July case and was selected as a representative example of a relatively distinctive CPI observation. The feature is now described as being consistent with possible fragmentation during the freezing of supercooled droplets upon collision with more massive ice particles, rather than as direct evidence that M2 occurred. The conclusion has been softened accordingly.

RC: L369-370: “the collision of large supercooled drops and small ice particles are rare” Could the author provide more evidence to support this? The observation of small ice particles is largely uncertain. Are there any fractured frozen drops in the CPI images? Could the large irregular ice particle in Fig. 5 the remnant of a fractured large frozen drop?

AC: We thank the reviewer for this important comment. We acknowledge that the observation of small ice particles is highly uncertain. Therefore, the interpretation of CPI imagery should be treated cautiously, particularly because the detection of small ice particles is uncertain and CPI has a relatively limited sample volume. We have therefore added a discussion of the strengths and limitations of the CPI imagery in Lines 469–478, including this possible interpretation.

In the available CPI images, clearly fractured frozen drops were not commonly identified. However, we cannot exclude the possibility that the large irregular ice particle in Fig. 5 could be the remnant of a fractured large frozen drop. We have therefore treated the CPI imagery only as supporting observational context and have avoided making a definitive attribution based on this particle. A more detailed assessment of the observed ice particle habits using multiple probes, including CPI, 2D-S, and CIP, would be useful in future work (in preparation).

RC: L414: It seems HM and BR were not explained at this point.

AC: We thank the reviewer for pointing this out. We have revised this sentence by replacing HM with RS and BR with CB, so that the terminology is consistent with the definitions used earlier in the manuscript.

RC: Figure S11-S13: Could the authors explain why a cut-off minimum droplet diameter appears in the ADIA case?

AC: We thank the reviewer for asking us to clarify this point. In the ADIA case, no entrainment, dilution, or aerosol recycling is included. Therefore, after the initially activated droplets grow, there is no subsequent source of newly activated small droplets. During ascent, the existing droplets continue to grow by condensation, while collision-coalescence further depletes the smaller droplet bins. As a result, the lower end of the droplet size distribution shifts towards larger diameters, producing an apparent lower-size limit in the ADIA case. This feature appears particularly sharp because the droplet diameter is plotted on a logarithmic axis, which visually compresses the small-diameter end of the spectrum.