RESPONSE TO REVIEWER 1:

We sincerely thank Reviewer 1 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.

The paper uses (idealized) numerical simulations of trajectories during DCMEX to study effects of entrainment on ice multiplication activity in deep convective clouds (DCCs). The study investigates different secondary ice production processes: rime-splintering, spherical droplet shattering (mode 1), droplet shattering upon collision with larger ice particle (mode 2) and breakup upon ice-ice collision, using University of Manchester bin microphysics parcel model. The study claims the first systematic attempt to investigate the impact of homogeneous, and inhomogeneous entrainment on SIP. Multiple perturbation simulations, from 15 representative DCMEX cases, have been performed to analyze the impact of entrainment on cloud properties such as LWC, effective particle size, CDNC, and ice number concentrations. Comparison of the simulated microphysical properties with three observed days of DCMEX is presented. I found the topic timely and relevant, aligned with current community efforts to better represent and quantify the impact of SIP in atmospheric models. It also sufficiently discusses the recent developments and uncertainties in SIP research. However, I believe the manuscript could benefit from further clarification, and the major/minor comments below are intended to help the authors improve the overall clarity and organization.

RC: Line 31: Recent experiments Seidel et al. should be Recent experiment by Seidel et al.

AC: Changed.

RC: Lines 34-36: I found these lines quite misleading as if other SIP processes are “perhaps as uncertain”, what this study adds beyond considering other SIP processes over RS to explain ice enhancement in DCCs? These lines need to be re-written as it currently weakens the motivation of the study.

AC: We thank the reviewer for this comment. The discussion of uncertainties has been removed from this paragraph. The paragraph has been revised to more clearly emphasise the importance of investigating different SIP mechanisms beyond RS. The revised text is as follows:

Other potential SIP mechanisms therefore need to be considered to better understand ice enhancement in deep convective clouds.

RC: Line 34: change Ice-ice to ice-ice?

AC: We thank the reviewer for this correction. This issue was removed when the paragraph was rewritten.

RC: Line 35: parametrisations should be parameterisations

AC: Changed.

RC: Line 42: What is HAIC/HIWC?

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

RC: Line 43: As already defined above, ice-ice collisional breakup can be written as CB.

AC: Changed.

RC: For the description of SIP processes in the introduction, the limitations reported in recent studies are described quite well. However, the physical mechanisms underlying these processes are not well described (also not in Sec. 3.3 Secondary ice parameterisations). What is crucial is to discuss how these operate at the microphysical levels (how fragment production in each SIP process depend on temperature, vertical velocity, particle size and mass, etc.). This is important to improve the scientific clarity of the manuscript.

AC: Brief descriptions of the underlying microphysical mechanisms have been added for each SIP process, as highlighted below.

Rime-Splintering (RS), also known as the Hallett–Mossop process, is currently the most widely implemented SIP mechanism in cloud microphysics models. In this process, secondary ice splinters are produced when supercooled droplets collide with and freeze onto riming ice particles. It is active only within a narrow temperature range of -3 to -8 °C (Hallett and Mossop, 1974a) and typically occurs when droplets smaller than 12μm and larger than 24μm in diameter are both present (Mossop, 1978; Harris-Hobbs and Cooper, 1987). However, the large amounts of ice observed in deep convective clouds cannot always be explained by RS, especially in cases where the increase in ice number is too rapid, the temperatures and crystal habits fall outside the RS window, or the necessary conditions for RS are not satisfied (Rangno and Hobbs, 1991; Lawson et al., 2015; Field et al., 2017). Recent experiment by Seidel et al. (2024) also failed to reproduce the high SIP rates reported in earlier RS studies, thereby questioning the significance of rime splintering under mixed-phase convective conditions. As noted in the review by Korolev et al. (2020), the physical mechanism of RS remains poorly quantified and understood, with the parametrisation still highly uncertain. SIP mechanisms that are less constrained by temperature and droplet spectrum conditions therefore also need to be considered to better understand ice enhancement in deep convective clouds.

Ice-Ice Collisional Breakup (CB) is the second SIP mechanism investigated in this study. This process refers to the mechanical fragmentation of ice particles during collisions, leading to the formation of new ice crystals. This mechanism has been observed in both aircraft measurements (Schwarzenboeck et al., 2009) and laboratory experiments (Vardiman, 1978; Takahashi and Nagao, 1995; Grzegorczyk et al., 2023). Laboratory studies found that CB is most active near −16 °C (Takahashi and Nagao, 1995), and field observations also showed substantial secondary ice near −15 °C (Mignani et al., 2019; Billault-Roux et al., 2023). In simulations of deep convective clouds during the High Altitude Ice Crystals and High Ice Water Content (HAIC–HIWC) campaign, Grzegorczyk et al. (2025) found that CB was a key contributor to ice enhancement, and that only when combined with RS could the observed high ice crystal concentrations be reproduced. However, the collision efficiency and fragmentation probability of this mechanism remain poorly understood, and future parametrisations will need to be improved accordingly (see the discussion in Korolev et al. 2020).

Finally, we examine fragmentation of freezing drops, also known as drop shattering (DS), in which supercooled droplets fragment during freezing and produce secondary ice particles. To better characterise these processes, Phillips et al. (2018) described DS as comprising two modes, with mode 1 (M1) representing fragmentation during the spherical freezing of droplets, and mode 2 (M2) representing fragmentation during collisions between supercooled droplets and more massive ice particles. According to Phillips et al. (2018), fragment numbers in M1 depend strongly on both temperature and droplet size, exhibiting a pronounced thermal peak near -15 °C and increasing with droplet size. In contrast, M2 shows no thermal peak and instead increases with dimensionless collision energy and supercooling, requiring the presence of large supercooled droplets and more massive ice particles. DS has increasingly been incorporated into numerical studies of SIP in recent years, suggesting that it may represent an important source of secondary ice. For example, James et al. (2023) investigated M2 within the framework of Phillips et al. (2018) in parcel model simulations of idealised shallow convective clouds, and compared it with RS, M1, and ice–ice collisional breakup. Their results suggest that, under conditions where RS is limited, M2 may represent an important source of secondary ice in shallow convective clouds, whereas the contribution from M1 is relatively small. In this study, we

RC: Line 60: While the acronym DCMEX is introduced in the abstract, it is not defined anywhere in the introduction. Abbreviations should be defined at their first occurrence in the main text, as the abstract is considered standalone.

AC: The abbreviation DCMEX has now been defined at its first occurrence in the introduction. We have also checked the manuscript and ensured that all abbreviations are consistently defined at their first occurrence in the main text.

RC: Lines 61-65: These lines can be best fitted in Sec. 2 DCMEX.

AC: The paragraph has been moved to Sec. 2 DCMEX.

RC: Lines 70-71: “While some …”. To my understanding, the activity of SIP processes varies significantly over the lifecycle of DCCs, can the author comment on the growth phase of DCCs they are comparing between these studies.

AC: We thank the reviewer for this helpful comment. We have revised the paragraph and added discussion of the cloud growth phase considered in these studies, as discussed in the revised paragraph at Lines 75–87.

RC: Lines 70-78: It is clear from the introduction that SIP processes are uncertain and that previous numerical studies have produced contrasting results regarding the dominant mechanisms. The study attempts to bring novelty by highlighting "entrainment impact on SIP" in the title, which is a valid and interesting point. However, this closing paragraph fails to deliver a convincing motivation for this specific novelty. The sentences do not connect logically, and the reader may be unable to follow the reasoning from the identified gaps in the literature to the scientific objectives of the present study. More critically, the study investigates three distinct entrainment regimes, adiabatic, homogeneous, and inhomogeneous, yet none of these are introduced or discussed anywhere in the introduction. I think the reader needs to understand what these entrainment types represent physically, how they differ and more importantly, why they are important for studies involving SIP.

AC: We thank the reviewer for this comment. We have revised the closing paragraph of the Introduction to provide a clearer scientific motivation and a more logical progression from the identified literature gap to the objectives of this study, as shown in Lines 75 to 87. We have also introduced the physical meaning of the homogeneous and inhomogeneous entrainment representations in Lines 63 to 74.

RC: Line 77: dependent should be depend?

AC: We thank the reviewer for this correction. This issue was removed when the paragraph was rewritten.

RC: Line 88: BAE should be BAe?

AC: Changed.

RC: Line 89: I wonder if this “Kite-shaped” pattern was for a specific reason.

AC: The kite-shaped flight track was an operational waypoint-guided pattern used to assist navigation and keep the aircraft close to the Magdalena Mountains.

RC: Line 96: What is κ here?

AC: The hygroscopicity parameter (κ) has now been defined at its first occurrence.

RC: Line 99: DMT?

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

RC: Line 99: In CDP-2, 2 for CDP version?

AC: CDP-2 refers to a second-generation version of the cloud droplet probe.

RC: Line 100: Space between (CDNC) and (Lance et al. 2010b).

AC: Changed.

RC: Line 102: SPEC?

AC: SPEC Inc. refers to Stratton Park Engineering Company. As it is not essential in this context, it has been removed for clarity.

RC: Lines 141-142: The authors should clarify what they mean by "zero of the Köhler equation"? Is this the critical supersaturation, the equilibrium radius at a given supersaturation, or something else?

AC: We thank the reviewer for this comment. The phrase "zero of the Köhler equation" was unclear and has been revised to refer to the equilibrium wet diameter at a prescribed environmental relative humidity, assuming initial Köhler equilibrium.

The revised sentence is as follows:

The initial wet diameter and corresponding liquid water mass of aerosol particles are determined by assuming equilibrium with the prescribed environmental relative humidity, based on κ-Köhler theory. This initial wet state is represented numerically on a mass-based bin structure, with the mass of adjacent bins increasing by a factor of $2^{1/2}$.

RC: It is also not clear how primary ice is activated in the current setup of BMM.

AC: A more detailed description of the cold microphysical processes has been added, including homogeneous and heterogeneous freezing, which clarifies how primary ice is generated in the model.

The cold microphysical processes considered in this study include homogeneous and heterogeneous freezing, vapour depositional growth of ice particles, ice–ice aggregation, riming, and secondary ice production. Homogeneous freezing of supercooled droplets is represented following the formulation of Koop et al. (2000). Heterogeneous freezing in this study is represented using the parameterisation of Daily et al. (2025), a recently developed formulation constrained by observations from the DCMEX campaign. To assess the sensitivity of the results to the representation of ice-nucleating particles, DeMott et al. (2010) is also included for comparison. The differences between the two parameterisations are discussed in Sect. 4.3, with additional details provided in Sect. S2 of the Supplement.

RC: Line 139: The hygroscopicity parameter is kept constant across the bins. Can the authors justify this simplification and discuss how it may affect CCN activation and the resulting droplet size distribution? The mode 2 SIP process can be highly sensitive to the drop size thus any bias introduced in droplet size distribution could affect mode 2 activity.

AC: We thank the reviewer for this helpful comment. We have now added a discussion of the limitations associated with using a constant hygroscopicity parameter across all aerosol size bins in Lines 492–501.

RC: Also, the use of SMPS is mentioned earlier in Sec. 2.1 (also in Fig. S1), I wonder if the modeled size distribution is constrained to the observed size distribution (e.g., with Fig. S1).

AC: We thank the reviewer for this comment. The modelled aerosol size distribution is constrained using SMPS observations. This has now been clarified in the manuscript as follows:

The initial aerosol size distribution is represented by a two-mode lognormal fit derived from SMPS observations (see Fig. S1 and Table. S1).

RC: Sec. 3.1: In general, this section should aim to connect each technical choice back to its scientific implications for the results, rather than presenting the model formulation in isolation.

AC: We thank the reviewer for this comment. Section 3.1 has been substantially revised to provide a more detailed and structured description of the model processes, from droplet activation through warm microphysics to cold processes.

RC: Lines 169, 173, 177-179: , μ d/dt are defined multiple times.

AC: The repeated definitions of μ and d/dt have been removed. Section 3.2 (Entrainment Representation) has also been revised for clarity.

RC: Line 188: 10,s should be 10 s?

AC: Changed.

RC: Sec. 3.3: The current description of SIP parameterizations in the BMM is insufficient. The manuscript repeatedly refers the reader to previous studies for the mathematical representation of the SIP processes considered, but this is not acceptable as a substitute for a self-contained description in the present paper. Although the literature describing derivations of these SIP processes cited here, the authors should, at minimum, present the key equations governing each SIP mechanism, along with a clear explanation of how each process is implemented in the BMM. This includes their bin representation, the favorable conditions, the fragmentation rates, the temperature dependencies, rime-fraction (for CB from Phillips et al. 2017a), and any threshold parameters used. Without this, it is impossible for the reader to critically evaluate the model results or assess whether the implementation is physically reasonable. A paper whose central scientific contribution is the comparison of SIP parameterizations under different entrainment conditions must provide a transparent and complete description of those parameterizations within the manuscript itself. The same holds true for heterogeneous ice nucleation parameterizations.

AC: We thank the reviewer for this important comment. We have added a detailed description of the SIP parameterisations used in the BMM to Section S1 of the Supplement, including the key equations and implementation details for each mechanism. We have also added the descriptions and comparison of the two heterogeneous ice nucleation parameterisations to Section S2 of the Supplement.

RC: Line 203-208: The authors treat mode 2 as a fourth independent SIP mechanism, but in Phillips et al. (2018), mode 1 and mode 2 are both represent a single process called fragmentation of freezing drops (or drop shattering, DS). The authors should justify why mode 2 is classified as a separate mechanism rather than as a part of DS, or restructure their classification accordingly to remain consistent with the original Phillips et al. (2018) framework.

AC: We thank the reviewer for this comment. In this study, we follow the framework of Phillips et al. (2018), in which fragmentation of freezing drops (DS) is represented by two modes (M1 and M2). As these two modes involve distinct physical mechanisms of ice production, they are analysed separately in this work for clarity. The description of M1 and M2 in the Introduction has been revised accordingly, as shown in Lines 49 to 62.

RC: Lines 113-114: is defined multiple times earlier in the manuscript.

AC: Changed.

RC: From Table 1, There is an inconsistency between Section 2.2, where only three flight cases (C300, C303, C309) are stated to be considered in this study, and the table presented here, whose caption suggests that all 15 listed flights are considered. The authors should clarify which flight cases are actually used in the simulations and ensure consistency between the text, table, and caption throughout the manuscript.

AC: We thank the reviewer for pointing this out. We agree that the original wording in Sect. 2.2 was ambiguous. All 15 flight cases listed in Table~1 were used in the simulations, while

C300, C303, and C309 were selected as representative cases for detailed discussion in the main text. The revised text in Sect. 2.2 is given below:

In this study, 15 flight cases were simulated, as listed in Table 1. Three representative cases, 22 July (C300), 25 July (C303), and 01 August (C309), are selected for detailed analysis based on the evolution of the airmass characteristics described above.

RC: Line 234: D_{eff} is not defined earlier?

AC: Added.

RC: Lines 235-236: Just a thought. Ice enhancement can also be defined as N_{ICE}/N_{INP} which I believe can be a more standard way of defining it (See also Hobbs et al. 1980, Waman et al. 2022, Han et al. 2024, etc).

AC: We thank the reviewer for this helpful suggestion. In the revised manuscript, ice enhancement has been redefined as N_{ice} minus N_{inp}.

RC: Lines 243, 245: contributions of individual SIP mechanisms to what?

AC: The sentences have been revised to: "To quantify the effect of SIP on ice enhancement within the parcel" and "Section 4.4 analyses how individual SIP mechanisms contribute to ice enhancement."

RC: Line 246: I guess LWC, CDNC are defined earlier?

AC: We thank the reviewer for this comment. LWC and CDNC are defined in Sect. 2.1 and are briefly restated at the beginning of the Results section for clarity.

RC: Lines 249-250: This sentence seems incomplete as it is not clear which simulation refer to these values of LWC?

AC: We thank the reviewer for pointing this out. This sentence has been revised to clarify that the reported LWC values refer to the non-adiabatic simulations for C300, C303, and C309. The revised sentence is given below.

The left panel of Figure 3 presents the evolution of LWC for the three selected cases, showing only small differences among the non-adiabatic simulations because the parcel model entrains the same total mass of environmental air in each case. These simulations closely reproduce the observed peak LWC values, with maxima of approximately 1.0, 1.25, and 1.25 g m^{-3} for the 22 July, 25 July, and 1 August cases, respectively, near the parcel maximum height.

RC: Lines 251-252: Interesting, can this (300% more LWC) be also highlighted in the abstract?

AC: We thank the reviewer for this comment. We agree that this result is worth highlighting in the abstract. The following sentence has therefore been added:

In addition, the peak liquid water content values in the adiabatic simulations were approximately 300~% of the corresponding peak values obtained in the simulations with entrainment.

RC: Line 261: ADIA simulations instead of adiabatic (ADIA) simulations?

AC: Changed.

RC: Lines 261-285: The description in these paragraphs is difficult to follow without clear references to the relevant figures. The authors should ensure that Fig. 1 and any other supporting figures are explicitly cited at appropriate points throughout these paragraphs to guide the reader. This applies to almost all the paragraphs in the result section.

AC: We thank the reviewer for this comment. The Results section has been systematically revised to include explicit references to the relevant figures throughout. In Sect. 4.1, we have added: *The evolution of CDNC within the parcel is shown in the middle-left panel of Figure 3* and *the evolution of* D_{eff} *within the parcel for the different simulations is shown in the middle-right panel of Figure 3.* Similar revisions have been made across all subsequent sections.

RC: Lines 262-264: The authors refer to a decrease in CDNC but it is not clear what this decrease is relative to.

AC: This sentence has been revised as follows:

CDNC then decreased by approximately 20–40% from the peak values near the model cloud base towards the cloud top, mainly due to collision–coalescence, which remained active in the ADIA simulations.

RC: Lines 274-275: Terms likes RA and EA are repeatedly defined.

AC: Removed.

RC: Lines 285: At this point, the reader may lose track of the flow of information as multiple cases are being discussed simultaneously. Can the authors explicitly attribute the peak Deff to each of the three cases.

AC: This paragraph has been revised as follows:

The evolution of D_{eff} within the parcel for the different simulations is shown in the middle-right panel of Figure 3. Across all simulations, D_{eff} generally increased with decreasing temperature as the cloud developed, as a result of continued droplet growth by collision–coalescence, and reached maximum values near the cloud top. For the ADIA simulations, maximum D_{eff} values reached approximately 22, 29, and 23 μm for the 22 July, 25 July, and 1 August cases, respectively. In contrast, the HOM+EA simulations provided the best agreement with the observations, closely following the upper envelope (yellow region), with peak D_{eff} values of approximately 18, 21, and 19 μm for the three cases. It is worth noting that, in the 25 July case, a pronounced increase in large droplets was observed near cloud top at temperatures of approximately −15 to −20 °C, and a similar feature was reproduced by the HOM+EA simulations. Overall, the inhomogeneous mixing simulations produced larger D_{eff} values than both the homogeneous mixing and ADIA simulations. This effect was particularly evident under inhomogeneous mixing without any entrained aerosol or aerosol recycling, where D_{eff} became significantly larger, likely due to the removal of smaller droplets during inhomogeneous mixing, which shifted the droplet size distribution towards fewer but larger droplets.

RC: Line 287: representative cases to representative DCMEX cases?

AC: Changed.

RC: Lines 291-292: For dispersion, proper figure should be cited here.

AC: This issue was removed when the paragraph was rewritten. This paragraph has been revised as follows:

Figures S11–S13 show the observed and simulated DSDs for three representative DCMEX cases, 22 July, 25 July, and 1 August, at nine selected temperature levels from cloud base to cloud top. In all three cases, the observed DSDs showed clear broadening with height, and bimodal, or even trimodal, structures were found at some temperature levels. This broadening of the DSD is also reflected in the dispersion, as shown in the right panel of Figure 3. The observed dispersion was generally smaller under higher-LWC conditions, which approximately correspond to cloud-core regions, and larger under lower-LWC conditions, shown in blue, which are more representative of cloud-edge regions.

RC: A suggestion: Can the authors describe briefly the term “dispersion” in the methodology?

AC: A brief description of the term “dispersion” has now been added in Sect. 2.1, as follows:

The cloud droplet probe (CDP-2) was used to measure the droplet size distribution (DSD) over the size range 2 to 50 μm and to derive cloud droplet number concentration (CDNC) (Lance et al., 2010b). The relative dispersion of the DSD was also calculated as the ratio of the standard deviation of droplet diameter to the mean droplet diameter. Liquid water content (LWC) was obtained from a Nevzorov hot-wire probe following the Met Office processing method (Korolev et al., 1998; Abel et al., 2014).

RC: Lines 292-293: The term “instrumental effects” introduced suddenly without prior discussion and dropped without any follow-up. Also, instrumental effects “by” Lance et al. (2010a).

AC: This sentence has been revised as follows:

It should also be noted that some broadening in the observed DSDs may arise from measurement uncertainties, as discussed by Lance et al. (2010a).

RC: The phrase "three cases" is used ambiguously throughout the manuscript. At times it refers to the three selected DCMEX flight cases (C300, C303, C309), and at other times it appears to refer to the three entrainment scenarios used in the ice-phase simulations (ADIA, HOM+EA, and INHOM+EA+RA). This inconsistency makes it difficult to follow the text without repeatedly cross-referencing earlier sections. Can the authors clarify this in the text?

AC: We thank the reviewer for pointing this out. In the revised manuscript, “three cases” is used only for the three selected DCMEX flight cases, C300, C303, and C309. When referring to the simulations, we now avoid this expression and instead explicitly refer to the three entrainment scenarios.

RC: Lines 302-304: The claim that HOM+EA cannot explain the observed dispersion "during the early stages of cloud development" is unclear (if it is in the context of Figure 3, which shows vertical profiles against temperature rather than temporal evolution). Can the authors clarify what they mean by early stages, and if necessary, support this claim with a time series or clearer reference to the relevant figure panels?

AC: We thank the reviewer for this comment. We agree that the phrase “during the early stages of cloud development” was not appropriate in the context of Fig. 3, which shows vertical profiles against temperature rather than temporal evolution. Our original intention was to refer to the earlier stage of parcel ascent in the BMM, for which lower altitudes correspond to earlier model evolution. However, this wording could be misinterpreted as referring to the temporal development of the observed cloud. We have therefore revised the text accordingly and now refer more explicitly to lower levels of the cloud. The sentence has been revised to:

The results show that homogeneous mixing (HOM+EA) alone cannot explain the observed droplet dispersion, particularly at lower levels of the cloud.

RC: Lines 65, 98, 306 (Wu et al. 2025): This article appears to be listed in the reference list as "in preparation" and does not appear to be available as a preprint. Citing unpublished and unavailable manuscripts makes it impossible for the reader to verify the claims supported by this reference.

AC: We thank the reviewer for pointing this out. The previous citation to “Wu et al. 2025” as an unpublished manuscript has now been replaced throughout the manuscript with the corresponding published paper, and the reference list has been updated accordingly.

RC: Line 319-321: Again, no clear mathematical formulation is given in the manuscript to describe heterogeneous freezing by Daily et al. (2025) and DeMott et al. (2010).

AC: We thank the reviewer for this comment. The mathematical formulations for heterogeneous freezing following Daily et al. (2025) and DeMott et al. (2010) have now been added to Section S2 of the Supplement. We have also included a figure comparing the temperature dependence of the two INP parametrisations.

RC: Line 320: ICNC is used without defining it earlier (the same at other places).

AC: We thank the reviewer for pointing this out. In the revised manuscript, ICNC has been replaced with N_{INP} for clarity and consistency. Here, N_{INP} refers to the ice crystal number concentration produced by primary ice nucleation only, without contributions from SIP.

RC: Lines 319-322: I do not see which figure is actually referred here (Fig. 8?).

AC: We have revised the description of the INP curve reported by Daily et al. (2025) and its correspondence to N_{INP}, as described in Lines 341 to 345.

RC: I noticed a recurring structural issue throughout the results section that paragraphs tend to open with a main finding or conclusion without first pointing to the figure that supports it (e.g., lines 323-330). This makes it harder to follow the argument, as readers encounter the conclusion before understanding what evidence it is based on. I would encourage the authors to pay closer attention to this throughout the manuscript, ideally introducing the relevant figure and briefly describing its contents before stating the result drawn from it.

AC: We thank the reviewer for this helpful suggestion. In the revised manuscript, Sects. 4.3 and 4.4 have been rewritten, and the relevant figures are now explicitly introduced at each stage of the description.

RC: Line 331: (As said earlier) kindly refer to a figure supporting this statement.

AC: In the revised manuscript, the relevant figure is now explicitly cited to support this statement.

RC: Figs. 5-7: The ice number concentrations derived from the CPI are consistently between 1–100 L^{-1}. The FAAM BAe-146 is also equipped with other probes such as the CIP and 2D-S that made observations of ice particles. By plotting data from all three probes, can the authors confirm whether these probes reported similar ice number concentrations, or whether there are discrepancies between the instruments? If differences in the measurement, which probe is reliable and why? Also, what conditions are imposed on the simulated and observed ice number concentration shown in these plots. In the current plots, how the CPI data is filtered, for example, to avoid possible bias from artificial shattering of ice particle upon impact?

AC: We thank the reviewer for this valuable comment. We agree that including measurements from multiple instruments would be useful for assessing observational uncertainty in ice number concentrations. In the present study, the comparison in Fig. 5 was based on the processed CPI ice concentration variable, *concIce*, from the DCMEX/FAAM CPI dataset.

Although processed 2D-S and CIP products are available, they were not incorporated into the present analysis framework. A robust inter-probe comparison would require a harmonised treatment of particle size ranges, sample volumes, ice-particle identification, shattering artefact removal, quality control and uncertainty estimates. This additional analysis is beyond the scope of the present study, and we therefore do not make a quantitative assessment of which probe is most reliable here. However, we agree that such a comparison would be valuable and intend to address it in a separate study focused on ice particle observations from the DCMEX campaign (in preparation).

In the observed ice number concentration boxplots shown in Fig. 5, the CPI *concIce* values were used directly and matched to temperature by interpolating the FAAM core temperature measurements to the CPI sampling times. Only *concIce* values greater than 1~L^{-1} were retained, in order to exclude very low-concentration samples and to focus on samples with a clearer ice signal. The simulated ice number concentrations shown in these figures are the corresponding modelled ice number concentrations from the parcel simulations, without applying an additional instrument-specific size-range or probe-response filter.

The CPI *concIce* values used in this study were taken directly from the processed CPI product, rather than being recalculated from the raw CPI imagery. Therefore, no additional image-level filtering or inter-arrival-time filtering was applied by us. Potential shattering artefacts in airborne ice-particle probe measurements remain a source of observational uncertainty. In general, shattering bias can be reduced either through probe-tip design or through post-processing methods, such as inter-arrival-time filtering. We have clarified the filtering applied to the CPI observations, the treatment of the modelled ice number concentrations, and the limitations associated with the lack of a CPI, 2D-S and CIP cross-comparison in Lines 469--478.

RC: Figs. 5-7: In these figures, particularly Figs 6-7, the general claim is that including SIP in the simulations explains the observed ice number concentrations. However, I do not see this true for most of the vertical levels, particularly levels warmer than -20°C (for C303 flight) and -10°C (for C309 flight). Although RS is included, which is active at much warmer subzero levels, for what reasons the observed ice concentrations is consistently higher than simulated? Also, the current x-axes range in all these plots somewhat hinders the information of ice numbers from simulations for lower values.

AC: We thank the reviewer for this important comment. We have clarified the limitations of the parcel model, particularly its inability to fully represent the warmer parts of the cloud and the effects of vertical transport and sedimentation, as discussed in Lines 502–509. In addition, the x-axis range in the figures has been revised from $10^{-1}$to 10^{2}.

RC: Lines 345-346: No figure is cited to support this ice enhancement from RS (the same is true for other occurrences). Also, as ice enhancement is used to define SIP activity (also lines 353-354), which is the main focus of this study, I would suggest to bring these supplementary figures into the main text and discuss them thoroughly.

AC: We thank the reviewer for this comment. In the revised manuscript, Sect. 4.4, Analysis of Individual SIP Mechanisms, has been substantially rewritten, and the relevant figures are now explicitly cited when discussing ice enhancement from each mechanism. In addition, the supplementary figures originally shown in Figs. S6 to S10, which present the individual SIP mechanism analysis for all 15 cases, have been reorganised, and the results for the three selected cases are now included in Fig. 7 of the main text.

RC: Fig. 5 (right panel) is not discussed in the main text?

AC: We thank the reviewer for pointing this out. In the revised manuscript, the right panel of Fig. 5 is now explicitly discussed in the main text.

RC: Line 355: Please mention temperature -22°C in the main text. Additionally, the motivation for presenting the CPI imagery alongside the ice concentration profiles is not clearly stated, are the two panels intended to be interpreted together, or is the CPI imagery provided purely for illustrative purposes? If purely for illustrative purpose, I would recommend to show these imageries in the case description section for all flights (right panels of Figs. 5, 6, 7) and refer them in the main text whenever needed. Also, the authors claimed that the hexagonal shape visible in CPI imagery in Fig. 5 (at -22°C) is likely from ice-ice collision.

AC: We thank the reviewer for this helpful suggestion. In the revised manuscript, the CPI imagery has been moved to the Supplement, and it is now cited in Sects. 4.3 and 4.4 during the discussion of each individual case.

RC: Lines 356-357: Conceptual clarification: the phrase “fragmentation between supercooled droplets and more massive ice particles” should be revised to “fragmentation during the freezing of supercooled droplets upon collision with a more massive ice particle,” which more accurately reflects the physical mechanism involved. Additionally, as said before, the particle images contain substantial microphysical information and therefore should be described in detail within the case description section.

AC: We thank the reviewer for this comment. In the revised manuscript, the description has been revised accordingly. In addition, the CPI imagery is now discussed in Sect. 4.3.

RC: Line 358: Again, a statement is made without referring to proper figure. As well, kindly consider my previous comment on ice enhancement figures (moving them to and citing them properly in the main text).

AC: The relevant figure is now explicitly cited.

RC: Lines 364: Following this, the wording at line 9 in the current version of the abstract can be misleading as it can simply mean three entrainment conditions. But the main text considers two entrainment representations. Please correct the text for better clarity and consistency. I am unsure whether the authors also consider adiabatic case as a type of entrainment?

AC: We thank the reviewer for this comment. The description in the abstract has been revised for better clarity and consistency.

RC: Lines 368-369: What are the shallower cases here?

AC: We thank the reviewer for pointing this out. The shallower case refer to C309. In the revised manuscript, this has been clarified by revising the text to: whereas CB contributed to ice enhancement in some cases with warm cloud-top temperatures (e.g. C309; Fig.~S8).

RC: Lines 369-370: I wonder whether the CPI imagery alone is sufficient to support this conclusion. While the images demonstrate the presence and morphology of hydrometeors, it is not clear how they can be used to infer that collisions between large supercooled droplets and small ice crystals were rare. This statement, in its current form, appears to go beyond what can be directly inferred from the particle images. Clarification on how this conclusion was derived would be helpful.

AC: We thank the reviewer for this helpful comment. We have added a discussion of the strengths and limitations of CPI imagery in the present study in Lines 469–478, and we have also revised the wording of the description related to the CPI images and their implications for M1.

RC: Line 391: LES?

AC: We thank the reviewer for pointing this out. We have now defined LES as large-eddy simulation at its first occurrence in the revised manuscript.

RC: Line 395: secondary ice production can be written as SIP?

AC: Changed.

RC: Line 396: C306 and C312 are suddenly introduced here without supporting figure/discussion in the main text/supplement.

AC: Changed.

RC: Line 427: What is ICE-T? Similarly, HAIC/HIWC at other places in the manuscript?

AC: Changed.