Response

We thank the reviewer for reviewing our manuscript and the helpful and constructive suggestions. We have considered the comments carefully. Please find below a detailed point-by-point response to all comments.

Specific comments:

1. To increase the readability of the manuscript, I suggest renaming the simulations and use some meaningful names. For example: ‘control’, ‘double-HM’, ‘early onset1’, ‘cooper10x’, ‘early onset1 & cooper10x’, ‘early onset1 & 100xINP’, ‘early onset2 & 100xINP’, ‘Demott’, ‘early Demott ‘, ‘Demott 10xINP, ‘multi-thermals’ etc. It's hard to follow the results and conclusions with current names.

   Reply: Those have been changed throughout the manuscript.

2. One of the major objectives of the paper is to investigate whether multiple thermals in the clouds could explain the observed ice concentration. Therefore, some discussion about previous studies on the conditions favorable for secondary ice production is expected (e.g. graupel fall velocity, updraft speed, broader drop size distribution, etc). (Reference: Cloud Conditions Favoring Secondary Ice Particle Production in Tropical Maritime Convection, Andrew Heymsfield1, and Paul Willis). What are the limitations of the current understanding of these processes? How your study is going to explore these uncertainties.

   Reply: We agree with the reviewer's point and have added the following in the paragraph on thermals in the Introduction.

   “The operation of the HM process needs the coexistence of graupel, large drops (> 25 µm) and small drops (< 12 µm) in the temperature zone of -3 °C – -8 °C. Previous studies have investigated the conditions favourable for secondary ice production: moderate vertical velocities to allow graupel particles falling into the HM zone, availability of both large and small drops for riming (e.g., Huang et al., 2008; Heymsfield and Willis, 2014; Huang et al., 2017). It is important to consider the possibility that multiple thermals will ascend through the HM zone because of the additional source of cloud drops that can rime onto graupel particles. ”

3. The default parameterization for primary ice nucleation used in the Morrison scheme is based on Cooper, 1986. The authors need to discuss the ice nucleation modes, supersaturation ranges w.r.t. water and ice for this scheme. It should be clear that which ice nucleation modes are active with the primary ice nucleation. Whether you have changed the onset temperature for only immersion-freezing mode?

   Reply: The following has been added: “The Morrison scheme has several ice freezing modes, including immersion freezing, deposition freezing as a function of supersaturation with respect to water and ice for this scheme, contact freezing, homogeneous freezing, and the secondary ice production by the HM process. For relaxation and enhancement sensitivity simulations, we only modified the immersion freezing mode."

4. It will be good to have a separate section for the ICE-D Observation (Section 2). The text after line number 91 to 106 from the introduction and current section 2 can be merged in the new section. Include the brief discussion on quality control of ice number concentration from 2DC-data. What is the size range of ice particles from observations mentioned in the paper? Whether the particles smaller than 200 µm were considered? Show that the conditions were favorable in the observed clouds for secondary ice formation. e.g. presence of drop larger than 24 µm size, presence of graupel particles in HM zone, etc in the same section. Figures 12 and 13 and relevant results can be added to this section. It should be clear that the comparison between model-simulated ice particles and observations is for particles of a similar size range.

   Reply: We thank the reviewer for this suggestion and have thought about it carefully. The paragraph (Lines 92-100 in the original version) is about the campaigns in this region and the difference between the ICE-D and the previous campaigns. The next paragraph highlights the high concentration of ice particles. Those paragraphs provide the aims of this study, which is an essential element of the introduction. It will leave a gap in the introduction if the paragraphs move to the next section. For this reason, we think it is better for them to remain in their current places. But, "A suite of instruments on board the UK research aircraft FAAM (Facility for Airborne Atmospheric Measurements) BAe 146 measured the information about cloud microphysics, aerosol particles, and other atmospheric variables (see Price et al., 2018, Liu et al., 2018, and Lloyd et al., 2020 for further descriptions)." has been moved into Section 2.

   Figures 12 and 13 are in the discussion section together with the possible other mechanisms. Besides, the text length is in rough balance in this way. They are better in their current places.

5. Line 88: Authors mentioned previous work by Blyth and Latham (1997) on the role of multi-thermal in HM process. Mention in which aspects your simulations are different than theirs. What are the novel approaches of your study? Does your study agree/disagree with their simulations? How realistic are the simulated thermal in the model? The authors need to add some discussion on this.

   Reply: The following has been added at the end of Section 4.5: "The results are consistent with the findings of Blyth and Latham (1997) in that multi-thermals can significantly enhance the secondary ice production. A conceptual representation of the kinematics was used in the detailed microphysics model described by Blyth and Latham (1997), whilst the present study employed a three-dimensional cloud model with detailed cloud microphysical processes. There have been a few studies of thermals in shallow convective clouds (e.g., Heus et al., 2009; Heiblum et al., 2016). It is impossible to make a direct comparison of thermals between the deep convective cloud in this paper and those shallow clouds, but similar features were found, such as enhanced vertical velocities and cloud mass associated with the thermals.”

6. In the conclusions, the authors mentioned that the multiple thermal still cannot reproduce the observed highest concentration of ice particles. There should be more discussion about it.

   Other secondary ice mechanisms may not be fully absent at temperatures warmer than -10^{o} Observations indicated the presence of fragments of frozen drops. The parametrization of secondary ice particles from frozen drops by Phillips et al. 2017 indicates that at those warmer temperatures this process might have a considerable contribution to the secondary ice formation.

   Reply: The following has been added in the text: “Recent development of the parametrization of secondary ice particles from frozen drops by Phillips et al. (2017) indicates that this process might have a considerable contribution to the secondary ice formation at temperatures greater than -10 °C. Future research will undoubtedly include this parametrization. It should also be noted that research continues on mechanisms that can cause an enhancement of ice particles (e.g. James et al., 2021).”

   ”

Minor/technical comments:

Line 76: Cite recent review articles on secondary ice formation e.g. Review of experimental studies of secondary ice production: A Korolev, T Leisner Atmospheric Chemistry and Physics 20 (20), 11767-11797; A new look at the environmental conditions favorable to secondary ice production: A Korolev, I Heckman, M Wolde, AS Ackerman, AM Fridlind, LA Ladino, Atmospheric Chemistry and Physics 20 (3), 1391-1429.

Reply: Those papers have been added in the text.

Line 91: Define ICE-D in the text also even if it is defined in the abstract.

Reply: It has been added in the text.

Line 94: Rephrase/remove the sentence ‘However….

Reply: As suggested by another reviewer, this sentence has been deleted.

Line 101: Is FAAM defined earlier?

Reply: It has been added here.

Line 119: Full stop is missing after … and aerosol

Reply: It has been added.

Line 134: Correct the figure number. It is Figure 1a. Also add the axes titles (Lat, Long) on the plot. Correct the figure numbers in the rest part of the paragraph e.g. line number 135, 138, 141.

Reply: The figure numbers have been corrected, and the axes titles have been added.

Figure 3: Check the legends for 2DC data. Add the axes title in X-axes (Ice particle number concentration). Mention the size range of ice particles considered here.

Reply: It has been changed The caption has been changed to "Figure 3, Time series of the concentration of ice particles (L^{-1}) in the size range of 50 -1280 μm measured with 2D-S Stereo *Probe* and vertical velocity (ms^{-1}) between 15:48:05 UTC and 15:49:30 UTC on 21 August 2015. "

Figure 4: Add axes titles.

Reply: The axis titles have been added..Please note that a new figure has been added before this figure which is now Figure 5 in the revised version.

Line 150: Mention the lowest diameter of ice particles considered in estimating their concentrations. Are you considering particles smaller than 200 um from the observations?

Reply: The concentration of ice particles in the figure only include those with irregular shapes (non-spherical) measured with the 2DS. The sentence has been changed to "The maximum concentrations of ice particles (i.e., non-spherical in shape) in the size range of 50 -1280 μm were ..."

Line 151: Change -6,8 ^{o}C to -6.8 ^{o}C.

Reply: It has been changed.

Line 163: Add reference for Hallet Mossop process

Reply: The original paper of Hallett and Mossop (1974) has been added. Also added is the paper by Cotton et al. (1986) in which the treatment of the HM process has been used.by CM1.

Line 209: change 14C to -14^{0}C

Reply: It has been changed.

Line 213: Change the graupe to the graupel

Reply: It has been changed.

Line 221: Check an empty bracket after velocity

Reply: It has been deleted.

Line 254: Do you mean 'starting freezing temperature'.

Reply: It has been corrected.

Line 255: How do you know the ice particles observed during aircraft observations are only originating from secondary ice processes.

Reply: It has been changed to “the primary and the secondary ice production”.

Section 4.3: Be specific about simulations involving changes in freezing efficiency. Mention clearly what are the changes made for this.

Reply: We have added the following in the Experimental design section. “The Morrison scheme has several ice freezing modes, including immersion freezing, deposition freezing as a function of supersaturation with respect to water and ice for this scheme, contact freezing, homogeneous freezing, and the secondary ice production by the HM process. For relaxation and enhancement sensitivity simulations, we only modified the immersion freezing mode. The aims of the sensitivity simulations are summarised as follows. *early onset1* examined the effect of active INPs at higher temperatures on secondary ice production when the onset temperature was increased to -3 °C. *Cooper10x* explored the effect of more INPs (i.e., the freezing efficiency was multiplied by 10). *early ohnset1 & Cooper10x* combined effects of the above two, while *early onset1* & *100xINP* and *early onset2* & *100xINP* probed the effect of even higher loadings of INPs. The DeMott scheme (2010) was examined in runs *Demott*, *early Demott*, and *Demott 10xINP*. To investigate the effect of the dust as INP, the Bigg (1953) scheme was replaced by the Paukert and Hoose scheme (2014) since the Bigg scheme is for general INP types, but the Paukert and Hoose scheme considers different INP types. The *Paukert* run used the mineral dust parameters in the Paukert and Hoose scheme. The *Paukert-dust* run was same as the *Paukert* run except that the INP numbers were increase by a factor 3.3 in the layer between 2 – 3 km where the dust layer was presented (Figure 1e). Finally, the effect of multi-thermals on secondary ice production was examined in *multi-thermals* when a second bubble of 2 °C was added after 20 min into the simulation.”

Line 266: Correct ‘TLXTEN’

Reply: It has been corrected. Please note names of all simulations in Table 1 and in the text in response to the other reviewer.

Line 394: I think the Morrison microphysics scheme was applied for all the runs. It was modified for other sensitivity tests. Make this point clear in the text.

Reply: “The Morrison microphysics scheme was applied for the control run” has been changed to “The Morrison microphysics scheme was applied for the simulations.”

Line 420: Add some of the challenges in measuring full spectra of INP and CCN.

Reply: It has been added.

Figure 2: Check the X-axes title. What is HI? Is it defined in the text? Also, mention the Temperature on the color bar in the box. Also, change the title of X axes to Altitude (above MSL??) (m). Check its position w.r.t plot.

Reply: It has been changed. “The colour bar represents temperature.” has been added in the caption.”

Figure 7: Check the spelling of ‘ratio’ in the figure title.

Reply: They have been changed.

Table 1: RLX3X100 is the experiment where you relax the onset temperature in the **ice nuclei number** concentration and not the ice concentration. Similar to RLX2X100.

Reply: They have been changed to “the ice nuclei number concentration”.

References

James, R. L., Phillips, V. T. J., and Connolly, P. J.: Secondary ice production during the break-up of freezing water drops on impact with ice particles, Atmos. Chem. Phys. Discuss. [preprint], https://doi.org/10.5194/acp-2021-557, in review, 2021.

Heymsfield, A. and Willis, P.: Cloud conditions favoring secondary ice particle production in tropical maritime convection, J. Atmos. Sci., 71, 4500–4526, https://doi.org/10.1175/JAS-D-14-0093.1, 2014.

Huang, Y., Blyth, A., Brown, P., Choularton, T., Connolly, P., Gadian, A., Jones, H., Latham, J., Cui, Z., and Carslaw, K.: The development of ice in a cumulus cloud over southwest England, New J. Phys., 10, 105021, https://doi.org/10.1088/1367-2630/10/10/105021, 2008

Huang, Y., Blyth, A. M., Brown, P. R. A., Choularton, T. W., and Cui, Z.: Factors controlling secondary ice production in cumulus clouds, Q. J. Roy. Meteor. Soc., 143, 1021–1031, 2017.