Response

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

Major comments:

1. The subject of this study is very interesting as the mechanisms responsible for the large ICNCs in convective clouds remain poorly understood, and especially those that concern secondary ice production. However, due to lack of detailed description of the sensitivity simulations it is not very easy to derive any robust conclusions. For example, the authors mention that Cooper parameterization is used in the Morrison scheme to treat primary ice production. However, as far as I know, this scheme utilizes different expressions for different freezing mechanisms (condensation, immersion, contact) and Cooper represents only one of them. Is this indeed the case with the particular model? If yes, then it doesn't make much sense to modify the freezing efficiency or onset temperature for only a single freezing parameterization (e.g. while the onset temperature for immersion freezing remains constant at -4 C as indicated in Morrison et al. 2005). Also it wouldn't make much sense to replace the Cooper description with the immersion freezing parameterization by Paukert and Hoose (2014), if the immersion mechanism is represented by Bigg (1953) formula in the scheme. This means that the immersion freezing process would be described twice in the respective simulations.

> **Reply:** The description of the experimental design "The aims of the sensitivity simulations are summarised as follows. RLX examined the effect of active INPs at higher temperatures on secondary ice production. TEN explored the effect of more INPs. RLXTEN combined effects of the above two, while RLX3X100 and RLX2X100 probed the effect of even higher loadings of INPs. The DeMott scheme was examined in runs DMTA, DMTRLX, and DMTRLX2 and the effect of dust in PAUKERT and PAUKERTD. Finally, the effect of multi-thermals on secondary ice production was examined in THERMALS."
>
> has been changed to
>
> "The Morrison scheme has several ice freezing modes, including the immersion freezing, the deposition freezing as a function of supersaturation with respect to ice, 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 the 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 outlined 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 observed (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."

2. Finally, since the main conclusion of the study is based on the THERMAL simulation, it is important to understand the realism of this experimental set-up. While injecting instabilities at the beginning of Large-Eddy or Cloud-Resolving Simulations is necessary, I would expect that the model would eventually be able to develop turbulent motions in a relatively realistic way. So basically I don't understand why a high-resolution model cannot produce multi-thermals in a 15 km x 15 km domain. I would appreciate a discussion on the ability of cloud models and weather prediction models to represent such turbulent structures. This would highlight the importance of this study's findings for atmospheric modeling.

Reply: We added the following in the text in response to the reviewer's comment.

"Figure 5a shows that the cloud model produced several separate thermals over the approximately 4-km width of the cloud system. Only one of them (between 6 -7 km) developed and ascended to 9 km. As discussed by Heus et al. (2009), the inflow of air from the subcloud thermal is assumed to be in balance with detrainment from the cloud into the environment in a mature cloud. In a single cloud simulation, a cloud is triggered by perturbations in temperature as a thermal. Since the lower boundary conditions were prescribed and only random perturbations were added, there were no subsequent thermals to produce profound heat or momentum fluxes from the underlying surface that were strong enough to produce more new cloud droplets near the cloud base. Recently, Heiblum et al. simulated the cumulus field using a LES model and showed a series of in-cloud positively buoyant thermals spanning 5 – 15 min each, in precipitating clouds (their Figure 3). However, there was only one thermal in the non-precipitating cases. Their results indicated that their multi-thermals could be a result of cold pool interaction and subsequent lifting. A series of thermals in convective clouds could be topographically or thermally forced in mountainous region. In principle, NWP or cloud models will be able to describe the appearance of sequential thermals if the boundary layer conditions are realistically represented. This study (and previous studies by Ludlam, Koenig, Mason and Jonas, etc) has highlighted the importance of atmospheric models being able to simulate these entities."

Minor comments:

Line 26: please specify... enhancement of the freezing efficiency... Relaxation of the onset temperature

Reply: It has been changed.

Lines 29-30: in a similar way to other convective clouds observed elsewhere in the world' This is a general statement that is not suitable for the abstract section, which aims to summarize main results. You have not provided references in the text for studies that have shown that multi thermals are important for cloud ice generation in 'other convective clouds elsewhere in the world'

Reply: The reviewer is correct. We have deleted that phrase from the sentence.

Line 52: are you sure about the -14 C threshold? I think Wilson et al (2015) showed that marine organics in sea-spray aerosols can nucleate at temperatures up to -10 C in the immersion mode.

Reply: It has been changed to -10 °C.

Line 63: The INP concentrations are highly variable at a temperature. This is a very general statement. You mean variable at a given temperature depending on aerosol, dynamic and humidity conditions?

> **Reply:** It has been changed to "variable at a particular temperature that depends on the dynamics and the aerosol properties and humidity conditions."

Line 73: Lawson et al (2015) developed a parameterization for secondary ice. Lawson et al. (2017) developed an expression that predicts the level in the updraft core, where liquid water gets depleted.

> **Reply:** We added "Lawson et al (2015) developed a parameterization for the drop-freezing secondary ice production process and subsequent riming. Lawson et al. (2017) developed an expression that predicts the level in the updraft core, where liquid water becomes depleted."

Line 78-79: you might want to rephrase this. There are actually models that are fully coupled with a chemistry component. Maybe say that this is not the case for most models?

> **Reply:** It has been changed to "Many numerical models, including some cloud models, do not explicitly include the information about CCN and INP, but rather use parameterization"

Lines 91-95: 'It occurred before...recommendation'. This sentence is redundant. It is common to analyze older campaign data to address different scientific questions.

> **Reply:** The following has been deleted: "It occurred before the recommendation was made by Field et al (2017) to 'carry out integrated field programs involving in-situ sampling, remote sensing, and modelling studies'. However, the analysis of data from the project are still ongoing and hence addresses the recommendation. "

Line 95: 'previous field campaigns relevant to ice nucleation'. Maybe say 'campaigns investigating or focusing on ice nucleation'? Anyway, this needs to be rephrased.

> **Reply:** It has been changed to "campaigns that have investigated ice nucleation".

Line 105: 'Secondly, the first ice particles appeared at temperatures greater than -8 °C.' Why is this mentioned here as a surprising finding? As you state in the introduction, there are several dust types that nucleate at temperatures higher than -10 C. Is this because such aerosol types are not expected in the studied region? Please explain.

> **Reply:** Although the some dust INPs can nucleate at temperatures greater than -10 °C, we did not know whether it was the case for the clouds in this region on this day. The reasons for including those sentences are that the focus of the paper is the concentration of ice particles in convective clouds in the ICE-D region, and the observation provided us the facts of the cloud microphysics for our simulations. We added "Secondly, the first ice particles were believed to have formed at a temperature greater than about -5 °C (Lloyd et al, 2020)"

Line 125: 'to measure... retrieval of aerosol optical properties' Please rephrase, it does not make any sense. You mean 'to retrieve aerosol optical properties'?

> **Reply:** It has been changed to "and to retrieve aerosol optical properties"

Line 145: 'Only the outside-cloud temperatures were used to avoid the bias caused by wetting'... were used for what?

Reply: It has been changed to "in-cloud temperatures could not be obtained because of wetting problems"

Lines 152-153: 'Firstly, the ice particles appeared at temperatures greater than -10 C'. Please expand the discussion why this is listed as a main point of the study. Ice presence at such temperatures has been observed many times in dust-dominated environments. Even if we exclude nucleation, seeding from higher levels is also a possibility

Reply: "the ice particles appeared at temperatures greater than -10 C" is not a new finding. It is a fact based on the observation. We have changed the sentences: "Two points can be drawn based on the figure. Firstly, the ice particles appeared at temperatures greater than -10 C. Secondly, the ice concentrations were much higher than the predicted by the primary ice production. Therefore, secondary ice production definitely occurred in the clouds." to "The observation indicates that the ice concentrations were a few tens per litre at derived temperatures in cloud between 0 and −2 ˚C (Lloyd et al., 2020). Therefore, secondary ice production most likely occurred."

Line 155: 'Cloud ... designed for idealised simulation of convective clouds using relatively less memory' Could you provide information on the adapted simplifications to understand the level of idealization in the simulations? This would help assess the realism of the results, especially in terms of the simulated dynamics

Reply: The application of CM1 has increased significantly in recent years, and papers using CM1 have been published in prestigious journals, such as Nature, ACP, JGR, JAS, MWR, JAMES, QJRMS, etc. Those sentences have been changed to "The Cloud Model 1 (CM1) was used for simulations in this study. More details on the model can be found in Bryan et al. (2003) and Bryan and Morrison (2012). The model uses conserved mass and energy conservation numerical schemes in 3-dimensions and has a rich choice of microphysics schemes. "

Line 158: maybe say 'which allows for quick simulations'

Reply: It has been changed to " which allows for quick simulations"

Section 3.2: Please provide a detailed description of the individual parameterizations and sensitivity set-ups. The DeMott expression and simulation description is only stated in the Table. Also please provide details about the Paukert and Hoose (2014) parameterization. For example, it is not mentioned until the Results section that a colder onset temperature is used in this description. Also later you mention that you modified the INP number to drop number ratio in this scheme, without giving any information on the default values or the implemented modifications. Describing the set-up of each sensitivity test in detail is critical for the interpretation and reproduction of the results.

Reply: Please see the reply to the Main Point 1.

Line 174: 'noted that the factor was 4'. Please explain what is meant by this phrase. Do you mean that this is the factor that provides the best agreement with observations?

Reply: We have changed "However, Garimella et al. (2017) noted that the factor was 4 using the Spectrometer for Ice Nuclei, and it varied in the range from about 1 to 10, which indicates that this is one of the major problems in quantifying the formation of ice in numerical models." to

"However, Garimella et al. (2017) noted that the calibration varied from 1.5 to 9.5 because of the lower relative humidity with respect to water than the intended values if aerosol deviated from the laminar flow, which indicates that this is one of the major problems in quantifying the formation of ice in numerical models."

Line 180: 'DeMott et al. (2010) based on all available data' . This statement is wrong. This parameterization takes into account several datasets from different regions, but definitely not all available INP data.

Reply: It has been changed to “DeMott et al. (2010) based on several datasets from different regions”

Line 186: Explain PAUKERTD set-up. How ice concentration is enhanced in the dust layer? (actually all tests should be described in detail).

Reply: Please see the reply to the Main point 1.

Line 194-197: Please add a figure that shows the profiles used to initiate the simulations. Documenting the initial thermodynamic conditions is important for comparison with other modeling studies or for other modellers to simulate the same case

Reply: It has been added as Figure 4 in the revised version.

Line 198: 'where another bubble was added 20 min from the starting time'. I am familiar with the fact that a bubble is needed to initiate instability in cloud resolving simulations, but after this I thought that the models can develop turbulence and simulate multi thermals within a 15x 15 km region. What is the physical explanation for the second bubble? What are the model limitations in representing boundary-layer dynamics? Does grid spacing has any impact on this? Also is choice of injection time random or does it have any physical basis (e.g. updraft lifetime). And do results show sensitivity to this choice?

Reply: Please see the reply to the Main point 1 for most of the comment. We did not test the impact of the grid spacing. We think the spacing does not make big difference because decreasing the spacing will represent more smaller eddies but not the thermals from the sub-cloud layer.

We add the following in the text.

“The injection time of 20 min was chosen when the updraught was about to decay (Figure 11). An earlier injection time (e.g., 10 min) of the second bubble only slightly increased the first main updraught and did not change the result significantly.“

Line 216: 'The cloud top continued to descend was about 8.5 km (T ~ - 18 C) by 40 min' Please rephrase, it does not make any sense

Reply: It has been changed to “'The cloud top continued to descend to about 8.5 km (T ~ - 18 °C) by 40 min”

Line 217: you refer to the ice crystal concentrations? (graupel is also ice)

Reply: It has been changed to “The ice crystal concentration”.

Line 221: please fill the empty brackets

Reply: The brackets have been deleted.

Line 251: What do you mean 'without the dust layer being considered'? Is the parameterization applied only up to a certain level?

Reply: It has been changed to “when the Bigg (1953) scheme was replaced by the Paukert and Hoose (2014) scheme (*Paukert*).” Some deatails have been added at the end of Section 3.2, i.e., Experimental design, “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).”

Line 261: replace 'next' and 'later' with 'in Section…'

Reply: It has been changed to “in Sections 4.3 – 4.5.

Line 263: ' due to different microphysical schemes' , you use the same microphysical scheme (Morrison). Please rephrase

Reply: It has been changed to “To investigate the causes of the change in secondary ice production sensitivity tests were used to introduce variations to onset freezing temperature and freezing efficiency”

Line 295: maybe specify that you refer to the ice crystal concentration

Reply: It has been changed to “the ice crystal number concentration”.

Line 306: 'enhancing the efficiency' of what?

Reply: It has been changed to “enhancing the freezing efficiency”.

Line 314: 'increased approximately above 8 km and decrease' , ' There was less riming', sometimes past tense is used to describe results, and some other times present tense. Please be consistent throughout the whole manuscript

Reply: We have checked the text and used the past tense for the simulation throughout the text.

Line 319-320: further clarifications on the implemented modifications are needed

Reply: “To account for the dust layer between 2 and 3 km (Figure 1), we modified the INP number to drop number ratio in the Paukert scheme.” has been changed to “To account for the dust layer between 2 and 3 km (Figure 1e), we modified the INP number to drop number ratio by a factor of 3.3 in the Paukert scheme.” Please see the reply to the Main point 1 for more explanation.

Line 324: 'the different microphysical schemes affect ', you use Morrison scheme in all runs.

Reply: It has been changed to “The above results indicate that the freezing rate and onset temperature affect the secondary ice production. However, none of them...”

Line 326: ' It would be incorrect not ...'. Remove this sentence or rephrase (e.g. 'it is important to consider... since both play a critical role in ice production', sth like this)

Reply: It has been changed to “It is important to consider both the dynamics and microphysics and their interactions since both play a critical role in ice production”.

Line 328: maximum values of what parameter

Reply: It has been changed to “maximum values of the vertical velocity, the raindrop concentration, the graupel particle concentration, and the ice crystal number concentration.”

Line 335: ...lasted for a longer time, approximately 30 min (rephrase)

Reply: It has been changed to “ lasted for approximately 10 min longer than the first updraft”.

Line 340: 'but there left some' , please rephrase

Reply: It has been changed to “ there were few raindrops remaining near cloud top”.

Lines 259-360: ' in the zone to 121 L-1', please rephrase

Reply: It has been changed to "one maximum being 84.9 L^{-1} at 58 min and z = 6.9 km and the other maximum being 121 L^{-1} at 69 min and z = 6.75 km."

Lines 363-371: I suggest to add a third panel with temperature timeseries in Figure 12 and include it in the discussion here.

Reply: We have not included the temperature time series because the probe became wet in cloud and the values cannot be used

Line 384: 'The shattering mechanism may be most efficient between -10 C and -15 C', yes but ice fragments generated by this mechanism at higher altitudes can be transferred at the lower cloud levels, examined here, right?

Reply: "The shattering mechanism may be most efficient between -10 °C and -15 °C." has been changed to "The shattering mechanism may be most efficient between -10 °C and -15 °C, and ice fragments generated by shattering may be transferred to lower or higher altitudes due to the updraughts and downdraughts. "

Line 385: 'Although the Knight mechanism operates at the similar temperature range, there is no parameterization of the process' Why the Phillips et al. (2017) parameterization does not account for this mechanism? They have developed a scheme that takes into account different ice types and habits. The one referred as 'planar' ice category in their study includes needles.

Reply: " Although the Knight mechanism operates at the similar temperature range, there is no parameterization of the process." has been changed to "The Knight mechanism operates at the similar temperature range, and future studies can investigate the relative importance of this mechanism using new parameterisations accounting for the ice–ice collision processes (e.g. Phillips et al., 2017)"

Line 368-369: 'Fragments of frozen drops were found, but not in a great amount (figures not shown)'. This is important evidence and should be mentioned above, where the possible contribution from other SIP mechanisms is discussed. You cannot assess the contribution of the drop-shattering process only from images, so your conclusion that this mechanism is insignificant is not necessarily accurate.

Reply: The sentence has been modified to "Fragments of frozen drops were found, but not in a great amount, compared with the amount of columnar crystals, although the exact number concentration needed to be determined."

We also changed " it is likely that multi-thermals play an important role in producing very high concentrations of secondary ice particles in some tropical clouds", in the last sentence of the abstract, to "It is possible of course that several mechanisms, some of them only recently being discovered, may be responsible for producing the ice particles in clouds. This study highlights the fact that the dynamics of the clouds likely play an important role in producing high concentrations of secondary ice particles in clouds."

Lines 370-371: 'However, there is no causal evidence and we cannot rule out other mechanisms of secondary ice production'. Maybe refer to Qu et al. (2020) who showed that several SIP mechanisms can operate within a convective cloud.

Reply: The paper by Qu et al (2020) has been cited. We have changed the sentence "However, there is no causal evidence and we cannot rule out other mechanisms of secondary ice production." to "However, there is no causal evidence of other mechanisms of secondary ice production. Qu et al. (2020), for example, showed that several SIP mechanisms can operate within a convective cloud."

Line 394: 'The Morrison microphysics scheme was applied scheme was applied to all runs. Maybe just clarify 'the defaultfor the control runMorrison scheme'

Reply: It has been changed.

Technical corrections:
Line 117: involved an (not in)
Line 124: to measure (not measures)
Line 125: remove 'so that'
Line 128: to measure instead of to measurement
Lines 134-141: it is Figure 1 you are referring to, not Figure 2
Line 139: I don't understand to which panel of Figure 1 you are referring to
Line 287: correct 'increasei7'
Line 312: PAUKERT and CTL
Line 321: to produce secondary ice concentrations similar...
Line 325: observations
Line 344: concentrations
Line 406: 'resulted in even lower' instead of 'had'

Reply: All the above technical corrections have been made.

References:

Qu, Y., Khain, A., Phillips, V., Ilotoviz, E., Shpund, J., Patade, S., and Chen, B.: The role of ice splintering on microphysics of deep convective clouds forming under different aerosol conditions: Simulations using the model with spectral bin microphysics. J. Geophys. Res. Atmos., 125, e2019JD031312. https://doi.org/10.1029/2019JD031312, 2020

Lawson, R. P., Woods, S., and Morrison, H.: The Microphysics of Ice and Precipitation Development in Tropical Cumulus Clouds, J. Atmos. Sci., 72, 2429–2445, https://doi.org/10.1175/JAS-D-14-0274.1, 2015

Lawson, P., Gurganus, C., Woods, S., and Bruintjes, R.: Aircraft Observations of Cumulus Microphysics Ranging from the Tropics to Midlatitudes: Implications for a "New" Secondary Ice Process, J. Atmos. Sci., 74, 2899–2920, https://doi.org/10.1175/JAS-D-17-0033.1, 2017.

Heiblum, R. H., et al. (2016), Characterization of cumulus cloud fields using trajectories in the center of gravity versus water mass phase space: 1. Cloud tracking and phase space description, *J. Geophys. Res. Atmos.*, 121, 6336– 6355, doi:10.1002/2015JD024186.

Heus, T., Jonker, H. J. J., den Akker, H. E. A. V., Griffith, E. J., Koutek, M., and Post, F. H.: A statistical approach to the life cycle analysis of cumulus clouds selected in a virtual reality environment, J. Geophys. Res.-Atmos., 114, D06208, doi:10.1029/2008JD010917, 2009.

Blyth, A. M. and Latham, J.: A multi-thermal model of cumulus glaciation via the Hallett-Mossop process, Q. J. Roy. Meteorol. Soc., 123, 1185–1198, 1997.

CM1 webpage: https://www2.mmm.ucar.edu/people/bryan/cm1/.