Responses to reviewers

Anonymous Referee #1

Review of Hawkler et al, 2021: Model emulation to understand the joint effects of ice nucleating particles and secondary ice production on deep convective anvil cirrus.
In this study an ensemble of LES are used to investigate the influence of INP concentrations, slope of the INP parametrization and Hallett-Mossop rime splinter production rates on deep convective clouds. The authors conduct a very nice statistical method to show that indeed the concentration of INPs and slope of the INP parametrization used have significant impacts on the anvils of deep convective clouds as well as the ice crystals within them. Additionally, they show that the slope of the INP parametrization can play an important role in convective invigoration and this is aided by the HMP. I would like to congratulate the authors on an exceptionally well written and carefully conducted study. I just have a few minor comments and am in full support for publication in ACP after there incorporation/consideration. I thoroughly enjoyed reading this study!

Response: Thank you very much for your time in reading and commenting on our study, and for your kind words about it. We are very pleased to have received such a constructive and supportive review.

General comments:

What happens to snow particles once they reach a certain size? Are they immediately precipitated to the surface or can they continue to contribute to the rime mass and produce splinters via HMP? This did not seem to be addressed and could have an influence on the NINP-38/INP slope feedback on HMP.
Response: Precipitating snow is treated prognostically. Sweep out rates of snow of all sizes are used for riming and secondary production. The effect of snow fall speeds, size, and riming rates are some of the microphysical variables we would recommend including in future studies like ours.

Change to paper: The following line has been added to Section 4: "Uncertainties in the riming, sedimentation and aggregation rates of snow and graupel particles should also be addressed in the future."

Is it known that if in the extremely active simulations (in terms of INP) if there is actually enough dust to produce these high numbers of ice crystals predicted? Or how is this linked to the dust. There is quite some discussion on this but it is not immediately clear how it works for in the simulations if INP are actually calculated based on available dust.

Response: The N_{INP}^{-38} and λ_{INP} factors were carefully selected to ensure coverage of the observed number concentrations of INP as can be seen in Fig. 5d, as well as coverage of all measured values of λ_{INP} and N_{INP}^{-38} individually, as can be seen in Fig.5 a-c. Our findings and methods are not exclusively linked to dust but INP more generally. Therefore, while we would not expect dust to produce the concentrations of INP simulated in all the mixed phase temperatures affected by the parameterisations (as dust is more efficient below -15C), we believe the INP parameterisations are realistic relative to observations of INP. Additionally, many of the ice crystals produced in the simulations are produced homogeneously, so the high numbers of ice crystals shown in the paper are not only a result of INP.

How dependent are the observed sensitivities purely related to the fact that NINP-38 has the largest variance (more than 6 orders of magnitudes versus just 3 for HMP)?

Response: It is certainly possible that the observed increased sensitivity to N_{INP}^{-38} is due to the larger range over which it operates relative to the HM-eff. However, if that is the case, we would argue that the observed sensitivities are still comparable as the chosen uncertainty ranges were chosen based on literature, laboratory and in-situ observations of N_{INP}^{-38} and Hallett-Mossop efficiencies. It would have been unrealistic to include variations of the HM-eff that exceeded the observed range of values, and enhancing the sensitivity ranges to be as large as that of N_{INP}^{-38} would have likely overestimated the potential effects of HM-eff in real clouds.

How sensitive are these results to the updraft speed within the convective core? Do you expect NINP-38 to still be important when updraft speeds are approaching 50 m/s as quoted in the text? In general, the in-cloud updraft velocities are close to the previously calculated/edge of where heterogeneous nucleation competes with homogeneous freezing (~1 m/s e.g. Korolev 2006) and therefore is it possible that the role of INPs and their slope are more important here than in stronger convective clouds?

Response: The updraft speed shown in the paper are generally averaged over a large region of cloud, including less dynamic regions, and are not representative of maximum updraft speeds. The maximum updraft speeds in the simulations range between 30 and 50 m/s in the strongest period of convection. Therefore we feel our results are relevant for strong convective clouds.

Change to paper: The following sentence has been added to the description of the convective and anvil cloud stages in Sect. 2.1: "Maximum updraft speeds in the convective cloud period range from 30 to 50 m s^{-1}."

When discussing the role of INP parametrization slope, how sensitive is this to no scavenging? For example in a real world situation, the slope of the INP parametrization would influence the amount of INP remaining at colder temperatures. However in this case, since no scavenging is occurring, the cumulative concentration in the cloud is significantly higher for a less steep slope than a steep one even though both slopes have the same NINP-38 value. Is this really a fair comparison then? Rather, should the cumulative INP concentration across the slopes for a given NINP-38 be constant, since no scavenging is occurring?

Response: The INP parameterisation takes into account the number of ice crystals already in the grid box as well as the INP concentration. This means that ICNC formed at lower levels and advected to upper levels will prevent new heterogeneous nucleation taking place unless there is ice-nucleating aerosol additional to the ICNC available. In a very strong convective core, as is the case here, we believe this acts as a 'proxy' scavenging control, preventing more ice crystals than there is dust available being formed as you would expect dust to be transported upwards in the convective core alongside the ice crystals it has formed heterogeneously. Therefore, we believe the lack of scavenging will not be having too large an effect on the results of the paper, however of course it would be great to test this in a future study. It is not possible to include scavenging in the simulations presented here because the boundary conditions are cyclical so scavenging would result in a rapid depletion of all available aerosol.

Change to paper: The following change (in bold) has been made in Sect. 2.1 in discussing the INP parameterisation: "The INP parameterisations resulting from the perturbations described in Sect. 2.2.1 and 2.2.2 inspect the conditions (temperature, cloud droplet number, ICNC) and aerosol concentrations within a gridbox and use that information to predict an ice production rate via heterogeneous freezing. **The supercooled droplets are depleted by the freezing parameterisation, but scavenging of INPs is not represented. As stated above, inclusion of scavenging was not possible as due to the cyclical boundary conditions of the simulation, scavenging processes would result in the rapid depletion of aerosol from the domain. The consideration of the number of ice crystals already present as well as the number of INP available when calculating the rate of heterogeneous freezing in a grid box acts as a control on the number of heterogeneous ice crystals forming in the absence of scavenging.** Homogeneous freezing of cloud droplets is parameterised according to Jeffery and Austin (1997)."

Minor comments:

Line 157-158: If dust is not scavenged, does this mean that the dust concentration always remains the same throughout the simulations? Does this mean that the new ice formed due to primary nucleation in each time step is the cumulative sum of INP up to the temperature of the cloud parcel? What impacts does this have on the importance of INP slope and overall number?

Response: Yes, the aerosol concentrations remain the same throughout the simulation. However, the INP parameterisation takes into account the number of ice crystals already in the grid box as well as the INP concentration. This means that ICNC formed at lower levels and advected to upper levels will prevent new heterogeneous nucleation taking place unless there is ice-nucleating aerosol additional to the ICNC available. In a very strong convective core, as is the case here, we believe this acts as a 'proxy' scavenging control, preventing more ice crystals than there is dust available being formed as you would expect dust to be transported upwards in the convective core alongside the ice crystals it has formed heterogeneously.

Figure 3: The values of precipitation seem very low. Is this calculated over the entire domain or just underneath the convective cell?

Response: The precipitation values are calculated from all cloudy regions. These cloudy regions include anvil, and non-raining areas. It should be noted that the output diagnostic values are averaged over 5 minutes and instantaneous rain rates will be larger than that shown.

Change to paper: The caption has been altered as follows: "Evolution of surface precipitation **in all cloudy regions** (a)"

Line 265-266: But the INP profile in Fig. 4b does not show that the concentration of INPs is constant with height, which indicates that the dust is not typically advected to all heights or is this outside of a convective core? Regardless, the NINP-38 is then set to the minimum between 1 cm-3 and the ns value, if 1 cm-3 is used, it is way above the available dust.

Response: The INP profile shown in Fig.4b is the profile the simulation is initiated with, not the profile that occurs throughout the simulation. The aerosol fields were checked and the dust is lifted by the convective updraft to all cloud levels. 1cm^{-3} is used in Fig. 4a to approximately match the peak value of the aerosol layer in Fig. 4b and for simplicity of reporting. The applied parameterisations (Fig. 5d) span all observed INP values (including non-dust INP) to ensure coverage of all realistic parameter space. In response to Reviewer 2, we have also changed references to 'dust' in the methods to 'INP' to make it clear that the study is focused on INP parameterisations rather than only dust parameterisations.

Change to paper: The following text was added to the caption of Fig. 4: "Fig. 4b shows the profile that the simulation was initiated with. The aerosols can be advected around and the peak values shown in Fig. 4b are lifted to all cloud levels by the convective updraft (not shown)."

Line 277: Is it really appropriate to assume that the active site density continues to have the same slope at temperatures warmer than -5 C? Due to such low efficiencies, it probably does not make much difference, but basically dust is completely inactive at such warm temperatures. Why wasn't -5 used as the warmest temperature where ns was calculated?

Response: While we used the Niemand et al. (2012) parameterisation as the basis for the parameterisations tested in this study, we wanted to ensure that the parameterisations tested covered all observations of INP, including non-dust INP, as is shown in Fig. 5d. Some of these other INP types (e.g. marine organics etc.) are very active at warmer temperatures and therefore we felt it was important to have representation of INP down to -3C, even though, as the reviewer correctly mentions, this likely makes very little difference. It is worth noting that the use of the word 'dust' in lieu of INP or N_{INP}^{-38} was not meant to indicate that we believe N_{INP}^{-38} to be also representative of dust. Rather this terminology stems from the construction of the MONC model whereby the aerosol mode that can act as INP is termed the 'coarse dust' mode. The only purpose of this coarse dust mode in the model, apart from a very minor contribution to aerosol activation, is to act as INP. Therefore, although the aerosol mode in the model is referred to as the 'coarse dust' mode, it would be more accurate to think of it as an 'INP aerosol mode'. The equivalence of N_{INP}^{-38} and coarse dust is not meant to be an exact representation of the interaction between dust and INP, and of course in the real, non-idealised world, the INP population would be made up of a subset of dust and other aerosols with ice-nucleating ability.

Change to paper: We have changed references in the paper from 'dust' to 'INP' where relevant and amended section 2.2 to be clearer about the use of the coarse dust mode to carry INP, for example, in the following paragraph "The profile shown in Fig. 4b is the mean daily aerosol concentration, assumed to be predominately dust, in Cape Verde extracted from a 2015 Global Model of Aerosol Processes (GLOMAP-mode; Mann et al., 2010) model simulation scaled to be approximately equal to the mean daily K-feldspar INP concentration from the same simulation (Vergara-Temprado et al., 2017). **This is applied as the INP particle number concentration profile in the base case MONC-CASIM simulation. In the MONC model, INP is represented using the coarse mode dust aerosol.**"

Line 285/Equation3: This is a bit confusing. So it is either NINP-38 or ns(-38), whichever is smaller? Perhaps this can be clarified. Also, is the (T) missing after ns?

Response: It is either N_{INP}^{-38} or ns(T).S, at this point we are not only thinking about the temperature as being -38°C. This equation is referring to the plateau that occurs in typical INP parameterisations when the calculated INP concentration from the intercept and the λ_{INP} lead to a higher value than there are INP available, in which case the number of INP available is used instead. It is not very relevant for our paper as at temperature higher than -38°C, the calculated INP number concentration is always lower than N_{INP}^{-38} to allow the λ_{INP} to be a determining factor of INP number concentrations at all mixed-phase temperatures. It was necessary to clarify that the minimum of the two numbers would be used for completeness despite the fact that the minimum function is not used in our paper.

Change to paper: The (T) was missing after ns, thank you for noticing. The following text has been added after Eq. 3: "The minimum function shown in Eq. 3 is not used in this paper at temperatures other than -38°C due to the parameterisation change described below."

Equation 5: Should -38P be +38P or the -38 in the exponent be +38?

Response: I don't believe so. We want to isolate i in Eq. 4. T is -38°C, so:

$e^{PT+i} = N_{INP}^{-38}/S$

Take the natural log of both:

$\ln(e^{PT+i}) = \ln(N_{INP}^{-38}/S)$

From log rules, $\ln(e^x) = x$, so:

$PT+i = \ln(N_{INP}^{-38}/S)$

T=-38, so:

$P(-38)+i = \ln(N_{INP}^{-38}/S)$

Now, isolate the i:

$i = \ln(N_{INP}^{-38}/S) – P(-38)$

Multiplying minus by minus:

$i = \ln(N_{INP}^{-38}/S) + 38P$

Change to paper: In checking the above equation, I noticed that there was an error in Eq.4. N_{INP}^{-T} should have been N_{INP}^{T} and this has been rectified.

Line 408-414/Figure 6: Why is there such a reduction in homogenous freezing at 11.5 km but then a large increase again above 12 km? I see the two order of magnitude scale difference but I wonder why there is a reduction towards 11.5 km? Is this a time issue?

Response: I presume this comment refers to the reduction in homogeneous freezing at 11.5 km in Fig. 6l but the increase in anvil ice crystal number concentration above 12 km in Fig. 6d. Panel d shows the ICNC at the anvil cloud stage (150-240 mins) and panel l shows the ice particle production in the convective cloud stage (60 and 180 mins) so there is a time difference as the reviewer suggests. Panel d also shows the ICNC from the anvil, the isolation of which is described in Table 1, whereas panel l includes all in-cloud values (also described in

Table 1) so as to include the convective core responsible for many of the cloud and cloud anvil properties. The difference in the homogeneous ice particle production and ICNC profiles are due to the rapid transport of homogeneous formed ice crystals in the convective updraft to the top of the cloud where ice crystals being formed in l are transported up towards the top of the cloud, and likely lateral transport in the outflow region, where they remain in the cloud anvil and are shown in panel d. The strong warm bubble and the resultant updraft strengths mean that the ice crystals formed by homogeneous and heterogeneous freezing are quickly transported to upper levels where they can remain in the cloud anvil.

Figure 6: should it be column integrated (between 5.5 and 7 km) like in Figure 9?

Response: Figure 6 shows the total column integrated heterogenous ice particle production, whereas Figure 9 includes only heterogeneous freezing occurring between 5 and 7.5 km. Therefore Figure 6 includes all heterogeneous freezing occurring in the cloudy columns, whereas Figure 9 shows only that occurring in the Hallett-Mossop region.

Change to paper: We have added the following text to the caption of Figure 9: "Note that panels (d-f) differ from Fig. 6 (e-g) because of the different altitudes: Fig. 6 shows the total column integrated heterogeneous ice particle production, while Fig. 9 (here) shows only the heterogeneous ice particle production occurring in the Hallett-Mossop region (5-7.5 km)."

Line 461-462: Again, isn't this threshold for homogeneous freezing rate highly dependent on the simulated updraft?

Response: Apologies, I'm not sure what threshold this comment is referring to exactly but homogenous freezing is parameterised according to Jeffrey and Austin (1997) and is dependent solely on temperature. In this work, we found that the homogeneous freezing ice production was primarily dependent on the heterogeneous freezing rate because the more heterogeneous freezing occurring the less liquid and droplets available for homogeneous freezing.

Line 511-512: Is this result not the opposite of what was found in Hawkler et al, (2021)?

Response: This relates to this sentence "Ice particle production by heterogeneous freezing is insensitive to changing λ_{INP} values except at the extremes of the $N_{INP}$$^{-38}$ perturbations." In Hawker et al. (2021) we found that the steepest INP parameterisations had the lowest homogeneous freezing rates. However, in Hawker et al. (2021) the use of a computationally expensive numerical weather model meant we could not conduct enough simulations t decouple the effect of the N_{INP}^{-38} and the λ_{INP}. In the previous paper, the steepest parameterisations also had the highest N_{INP}^{-38} meaning the two results (from this paper and the previous) are not necessarily in conflict as in both, the highest concentrations of INP at cold temperatures (due to a steep parameterisation in Hawker et al. (2021) and due to a high N_{INP}^{-38} here) lead to a reduction in homogeneous freezing rates.

Change to paper: The following sentence has been added to the discussion of the differences between hawker et al. (2021) and this study in Sect. 5: "This study has enhanced our understanding of aspects of cloud microphysical behaviours that was not fully explained in Hawker et al. (2021). For example, in both studies a reduction in homogeneous freezing rates occurs where there are high INP number concentrations at low temperatures and the results from this study indicate that it is a high N_{INP}^{-38} rather than a steep λ_{INP} that is the main driver of this effect, a distinction that we were unable to make in the previous study."

Line 515-519: are a bit repetitive as this had already been discussed above

Change to paper: The section in question has been shortened from "Anvil ICNC is sensitive to λ_{INP} values at high N_{INP}^{-38} (>10 cm^{-3}) where anvil ICNCs decline as λ_{INP} becomes more shallow (Fig. 8i). This is because at high N_{INP}^{-38} values, as previously identified, heterogeneous freezing becomes the dominant mechanism of primary ice production. A shallow λ_{INP} can limit the number of cloud droplets available for low temperature heterogeneous freezing (the main temperature region where ice crystals are formed in in simulations with a high N_{INP}^{-38}) and therefore reduce the overall anvil ICNC."

To

"Anvil ICNC is sensitive to λ_{INP} values at high N_{INP}^{-38} (>10 cm^{-3}) where anvil ICNCs decline as λ_{INP} becomes more shallow (Fig. 8i) because shallow λ_{INP} can limit the number of cloud droplets available for low temperature heterogeneous freezing (the main temperature region where ice crystals are formed in in simulations with a high N_{INP}^{-38})."

Line 521-523: Since aerosols are not scavenged, can new CCN be activated if the water saturation is high enough? Again, this process is highly sensitive to updraft velocity

Response: Yes, new CCN can be activated if the water saturation is high enough. However, the activation takes into account the number of droplets already present in a grid box in its calculation.

Line 552-560: How is the ice that falls back through the HMP region influencing secondary ice production? Does the ice that forms higher up not offset the influence of the slope of the INP parametrization through this region or is there not enough time for the ice to fall into this altitude in the simulations? With this in mind, there is some precipitation, so if the ice doesn't fall through this layer is it all due to warm rain processes?

Response: The ice can sediment through the Hallett-Mossop region through the sweep out rate (collection of liquid by snow, or liquid by graupel). However, in the convective core where most ice-crystals from heterogeneous and homogeneous freezing are formed, the updrafts are strong enough to prevent ice sedimentation. We found, as is seen in the results, an effect on the Hallett Mossop process from the λ_{INP}, however it is of course possible that this effect could be reduced with changes to the ice sedimentation.

Edits:

Line 66: add "the" after "This determines"

Change to paper: As suggested.

Line 272: Should it be "NINP" here?

Change to paper: As suggested.

Line 321: "properties"

Change to paper: As suggested.

Line 361: "that" --> "than"

Change to paper: As suggested.

Line 508: What does this mean: "λINP of 10 cm-3."? Should "of" be "at"

Change to paper: Changed to "at a N_{INP}^{-38} of 10 cm^{-3}".

Line 508: “INP” should be “NINP-38” for consistency I think.

Change to paper: As suggested.

Line 569: Should it be “red outline”?

Change to paper: As suggested.

Line 666: add “.” After citation

Change to paper: As suggested.

Line 801: “ti” --> “to”

Change to paper: As suggested.

Reviewer 2: Xiaohong Liu (Referee)

This modeling study quantifies the effects of INP number concentration, the temperature dependence of the INP concentration, and the Hallett-Mossop splinter production efficiency on the anvil of an idealized deep convective cloud using a Latin hypercube sampling method and statistical emulation. It is found that anvil ice crystal number concentration (ICNC) is determined predominately by INP number concentration, while anvil ice crystal size is determined predominately by the temperature dependence of ice-nucleating aerosol activity. The slope of temperature dependence of INP concentrations plays a key role for the secondary ice production (SIP) (via Hallett Mossop splinter). Generally, the research topics of INP and SIP effects on anvil properties and convection development are interesting and the results are novel. The manuscript is well-written and clearly to follow although some improvements are needed (see comments). I recommend that the manuscript may be accepted for publication after addressing my comments.

Response: Thank you very much for your time in reading and providing feedback on our paper. We found the comments to be very helpful and constructive, and are very pleased that you would like to recommend the paper for publication.

Main comments

The manuscript is a bit too long. I would suggest that the authors condense some of sections, e.g., by moving the content and figures to the supplementary. For example, section 2.4 regarding the statistical emulation can be simplified by referring to previous studies.

Response: We agree that the manuscript is quite long. We have shortened the section mentioned slightly and moved the former Figure 12 to the Appendix. However, we hesitate to shorten Section 2.4 more and to rely predominately on referral to previous studies as we would like those reading the paper who are unfamiliar with statistical emulation to be able to get a basic understanding of the process from our methods (as many will not want to read previous studies in addition to the journal article they have selected). We hope that the clear numbering of sections will allow for easy navigation of an admittedly lengthy study.

Change to paper: We have shortened the section mentioned slightly and moved the former Figure 12 to the Appendix.

At times in the manuscript, there are mixed use between INP and dust aerosol, e.g., in section 2.2. To my understanding, INPs are a subset of dust (and other) aerosols that are capable of nucleating ice. Even at -38°C (the lowest temperature for mixed-phase heterogeneous nucleation), their concentrations are not necessarily the same, although a positive correlation between them is expected.

Response: We understand the confusion referred to above. The use of dust in lieu of N_{INP}^{-38} is not meant to indicate that we believe N_{INP}^{-38} to be also representative of dust. Rather this terminology stems from the construction of the MONC model whereby the aerosol mode that can act as INP is termed the 'coarse dust' mode. The only purpose of this coarse dust mode in the model, apart from a very minor contribution to aerosol activation, is to act as INP. Therefore, although the aerosol mode in the model is referred to as the 'coarse dust' mode, it would be more accurate to think of it as an 'INP aerosol mode'. The equivalence of N_{INP}^{-38} and coarse dust is not meant to be an exact representation of the interaction between dust and INP, and of coarse in the real, non-idealised world, the INP population would be made up of a subset of dust and other aerosols with ice-nucleating ability.

Change to paper: We have changed references in the paper from 'dust' to 'INP' where relevant and amended section 2.2 to be clearer about the use of the coarse dust mode to carry INP, for example, in the following paragraph "The profile shown in Fig. 4b is the mean daily aerosol concentration, assumed to be predominately dust, in Cape Verde extracted from a 2015 Global Model of Aerosol Processes (GLOMAP-mode; Mann et al., 2010) model simulation scaled to be approximately equal to the mean daily K-feldspar INP concentration from the same simulation (Vergara-Temprado et al., 2017). **This is applied as the INP particle number concentration profile in the base case MONC-CASIM simulation. In the MONC model, INP is represented using the coarse mode dust aerosol.**"

This study focuses on three parameters related to ice formation (i.e., INP number concentration, the temperature dependence of the INP concentration, and the HallettMossop splinter production efficiency). It should be noted however, that there are many other uncertain microphysics parameters that can impact anvil cloud properties. In addition, MONC-CASIM is configured to be a two-moment scheme and uses multiple ice categories (cloud ice, graupel, snow) with fixed parameters for bulk physical properties. That can leads to large biases in ice properties, and more physical treatment of ice is now available for a continuous evolution of physical properties (e.g.,density) of ice (e.g. Hashino and Tripoli 2007; Morrison and Milbrandt 2015; Jensen al. 2017).

Response: We acknowledge that the three chosen variables represent only a fraction of the uncertainties in deep convective and anvil ice processes. We try to address the most significant of these in Section 4, but of course the significance of uncertainties is quite subjective, and the issues the reviewer mentions are material as well as those already detailed.

Change to paper: The following paragraph in Section 4 has been amended as follows: "The chosen uncertain input parameters are just three of a multitude of microphysical parameters that contribute uncertainty to convective cloud processes which should be considered in future work. For example, uncertainty in ice crystal number and mass concentrations were strongly affected by assumed ice crystal shape in simulations of a continental deep convective cloud simulated using the 3D MAC3 model (Johnson et al., 2015). Uncertainty in environmental conditions that may affect the cloud properties, for example, the size and temperature perturbation value of the warm bubble initiating our deep convective cloud, or the potential temperature profile (Wellmann et al., 2018), have also not been addressed here. Uncertainties in the initial conditions of our simulations have also not been tested and should be explored in the future (Miltenberger et al., 2018a; Miltenberger and Field, 2020). **Additionally, MONC-CASIM is configured to be a two-moment scheme in this work and uses multiple ice categories with fixed parameters for bulk physical properties. The representation of ice properties using a continuous spectrum of physical properties could be explored in the future.**"

Minor comments

Line 18: "ice crystal size…". Please add "anvil" before this phrase.

Change to paper: As suggested.

Line 77: “the occurrence and intensity of convection”. Wet removal and dry deposition are two critical processes for dust transport from sources to remote regions. This should be added here.

Change to paper: As suggested.

Line 99: “Variation in ice nucleation active site densities…” Please explain the reason for the large variation in ice nucleation active site densities.

Change to paper: The following sentence has been added: “The factors governing active site location and densities are not fully understood but are theorised to be related to features on an INP surface such as surface pits (Holden et al., 2019), hydrophilic sites (Freedman, 2015), or lattice mismatches (Kulkarni et al., 2015).”

Line 122: “non-linear interactions between the two freezing mechanisms”. Can you explain what the non-linear interactions are?

Change to paper: The following clarification has been added: “due to non-linear interactions between the two freezing mechanisms whereby very high heterogeneous freezing rates affect the availability and efficiency of secondary ice production, and vice versa”

Line 166: “Cloud droplet activation”. It is more accurate to say “Cloud droplet nucleation” or “Aerosol activation”.

Change to paper: Changed to “Cloud droplet nucleation”.

Lines 176-179: **(1)** “The INP parameterizations…, but scavenging of INPs is not represented”. For “aerosol concentrations within a grid box”, are you talking about aerosol concentrations within the cloud droplets since here it is heterogeneous freezing of cloud droplets induced by INPs.

(2) “The supercooled droplets are depleted”. Are the frozen droplets converted to ice crystals?

(3) What is the implication of not considering the wet scavenging of INPs? Will this overestimate the INP effects?

Response: **(1)** The INP parameterisations use the interstitial aerosol concentrations, not the concentration of aerosols within cloud droplets to determine heterogeneous freezing rates.

This is necessary because the model has cyclical boundary conditions, meaning no new aerosol apart from that present in the initial conditions is advected into the model domain. Therefore, including aerosol scavenging into cloud droplets and therefore heterogeneous freezing based on activated aerosol rather than interstitial aerosol would result in rapid depletion of the aerosol in the simulation.

(2) Yes the frozen droplets are converted to ice crystals. **(3)** We don't believe not including scavenging will have a substantial effect on the results. Firstly, as stated above, due to the necessity of using an LES model to allow the large number of simulations presented here, it was not possible to include scavenging effects due to the cyclical boundary conditions. Secondly, the use of a strong warm bubble to drive the convection in the simulated simulations mean that we don't anticipate wet deposition having a large effect on the aerosol budget, at least in the most active convective core.

Change to paper: We have added the following text in bold in lines 161 "The aerosol can be advected around but in the simulations presented here we choose to switch off scavenging processes**, and is therefore not incorporated into the cloud droplets when activate.**"

And 183-186: "**As stated above, inclusion of scavenging was not possible as due to the cyclical boundary conditions of the simulation, scavenging processes would result in the rapid depletion of aerosol from the domain.**"

Line 272: "NINP-38". Should this be NINP not NINP -38 in equation (1)?

Change to paper: As suggested.

Line 279: "NINP-38 of 1 cm-3". Should this be dust concentration since Niemand et al. uses dust concentration as input?

Change to paper: As suggested.

Line 361: "that". Change to "than".

Change to paper: As suggested.

Line 569: "black". Should be "red" outline.

Change to paper: As suggested.

Line 606: “Figure 11b” should be “Figure 12b”.

Change to paper: As suggested.

Line 630: “Figure 13a,d,g”. here d,g should be e,i?

Change to paper: As suggested.

Line 690: “10%”. Should this be “90%”?

Response: I believe it is correct as 10% because in Figure A1 we see that only 7 simulations are coloured anything other than yellow. Yellow indicates 90+% of ice crystals in the HM regions were produced by the Hallett-Mossop process and therefore less than 10% were produced by heterogeneous freezing, so the green/blue colours mean 10%+ were produced by heterogeneous freezing.

Line 725: “Fig.15d” should be “Fig.15e”, and “Fig.15e” should be “Fig.15d”.

Change to paper: As suggested, and order of snow, graupel in text reversed to be in order.

Line 735: “Fig.16a” should be “Fig.16b”, and “Fig.16b” should be “Fig.16c”.
As suggested, and also changed to the referencing Figures 16e and 16f were necessary.
Line 744: “Figure 16a” should be “Fig.16b”.

Change to paper: As suggested.

Line 767: “as parameterizations become available”. The parameterizations for other SIP processes rather than H-M have been available and implemented in models (e.g., Zhao X., et al., ACP, 2021 among others).

Change to paper: “as parameterisations become available” has been changed to “in the future”.

Line 783: “the INP number concentrations at -38C was fixed to be equal to the coarse model dust number concentration”. Again they should not be equal. INP is a subset of coarse dust even at -38C.

Response: As stated above, the equivalence of N_{INP}^{-38} and coarse dust is not meant to be an exact representation of the interaction between dust and INP. The only purpose of coarse dust in the model, apart from a very minor contribution to aerosol activation, is to act as INP. Therefore, although the aerosol mode in the model is referred to as the 'coarse dust' mode, it would be more accurate to think of it as an 'INP aerosol mode'.

Change to paper: The sentence has been changed to "In order to effectively decouple the N_{INP}^{-38} and λ_{INP}, **for the purposes of simulation,** N_{INP}^{-38} was fixed to be equal to the coarse-mode dust number concentration **(in reality** N_{INP}^{-38} **would be subset of dust and other aerosols with ice-nucleating abilities)** and the calculation of the intercept of the parameterisation at 0°C from N_{INP}^{-38} and λ_{INP} ensures that INP number concentrations decline constantly between -38 and 0°C."

Line 801: change "ti" to "to".

Change to paper: As suggested.

Line 896. "Varble et al., 2020" is missing in the reference list.

Response: Varble et al. 2020 is included in the reference list.